Of War and Love

How Cross-Class Romantic Partnerships, Mandatory Military Service, and Civil War Victimization Shape Attitudes towards Inequality and the Police



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Abstract

The first two papers of this dissertation examine how contact between people from different social classes affects support for policies aiming to reduce inequality. The scarcity of research addressing this question, as well as the shadow of self-selection looming over most of it, calls for more research on the relationship between inter-class contact and political attitudes. I contribute to this field by examining two overlooked settings in the existing literature: compulsory military service and cross-class romantic partnerships. For the first paper I employ a diff-in-diff approach, focusing on Spanish cohorts close to the removal of conscription and with women as the control group. I find that men born in conscripted cohorts are significantly more likely to support redistribution. For the second paper, I draw on longitudinal data from the British Household Panel Survey and find that those who marry/begin to cohabitate with someone from a different class move their economic attitudes to the left—*vis-à-vis* those doing so with someone from the same social class. The third paper explores the connection between legacies of political violence during a civil war and attitudes towards the police, focusing on the case of Spain. I argue that civilian victimization could have heightened risk aversion and fear of conflict recurrence, increasing popular support for institutions perceived to be in charge of keeping order and stability. Looking at three different geographical levels and employing three different surveys, I find that individuals in areas which experienced larger levels of Francoist repression during the Civil War and its aftermath display more positive attitudes towards the police nowadays. An analysis of the Guatemalan Civil War confirms a positive relationship between civilian victimization and trust in the police beyond the case of Spain.

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Introduction

Economic inequality might be the most pressing concern for this generation of citizens and scholars. Whereas others before may have focused their efforts on the fight for civil rights and freedoms, the importance of inequality to understand the world today seems undeniable. The push for economic equality which followed World War II seems now a very distant and strange phenomenon, one buried after decades of increasing levels of financialization and neoliberal policies—embodied in the policies of leaders such as Margaret Thatcher and Ronald Reagan. These changes in the distribution of power across groups in society has come hand in hand with a discourse which advocates for economic inequality as the engine of societal progress, always lubricated by a meritocratic narrative preventing any questioning of the existing status quo. In more Marxist terms, the ideal of meritocracy has come to occupy a dominant role in the *superstructure* of modern societies, whose levels of inequality might otherwise pose a serious threat to the survival of their economic structure (*base*).

Recent events—such as the resurgence of radical right parties across Europe, Brexit, and the rise of figures like Donald Trump—might have set us back by decades, to a time when basic freedoms were not guaranteed. However, these events do nothing but underscore the importance of inequality to understand the most significant events of our time. All these phenomena are rooted in a context of rising economic inequality and a lack of policies compensating the working classes as the losers of processes of globalization and automation taking place around the globe in the last decades (e.g., Colantone and Stanig, 2018; Fetzer, 2019; Gingrich, 2019a).

Not only have some scholars rejected inequality—or excessive levels of it—on a normative basis, but they have also highlighted the many dangers that a highly unequal distribution of resources can generate. Political scientists (e.g., Ansell and Gingrich, 2024; Bartels, 2008; Gilens, 2012) have noted how economic inequality may hinder political equality, making the preferences of the wealthiest to be overrepresented in parliaments. Sociologists have highlighted how individuals' class of origin largely determines their class of destination (Erikson and Goldthorpe, 2002), such that it is in more unequal societies where lower levels of equality of opportunity can be found (Corak, 2013). Even economists, who tend to be more acceptant of inequality due to its believed incentivizing effects, have noted the negative externalities of inequality in areas such as reduced investment in human capital (Galor and Zeira, 1993) and, concerning this dissertation, crime (e.g., Becker, 1968; Ehrlich, 1973). The positive relationship between inequality and crime—along with a positive effect on health issues and psychological distress—has also been pointed out by epidemiologists such as Richard Wilkinson and Kate Pickett (2009, 2018).

Becker (1968) proposes a rational-choice approach to the study of crime. With this canonical model in mind, authors such as Ehrlich (1973) and Bourguignon (2001) have proposed and adapted theoretical models predicting a positive relationship between inequality and criminal behavior. The core intuition behind these models is that crime becomes more attractive for the poor in more unequal societies, as the relative gains of criminal activity (e.g., the potential wealth to be stolen from richer individuals if committing property crimes) exceed its (opportunity) costs (e.g., the foregone wage if caught and imprisoned).[1] This positive relationship between inequality and crime was first tested by Ehrlich (1973), who found that those American states in which the poorer were more distant from the median income were

[1] Following a similar logic, the model predicts that the poor are the most likely to engage in criminal activities, rather than the middle class or the rich.

those presenting larger levels of criminality. More recently, Fajnzylber et al. (2002) employed a panel of almost forty countries to test the inequality-crime connection. Paying special attention to issues of omitted variable bias and reverse causality, they find a strong causal effect of inequality on crime, but still are unable to identify the underlying mechanisms.

If we accept the argument that inequality boosts crime, then inequality may have a damaging effect not only for the poorer but also for the richer, as in increased fear of property and violent crimes. A key question, then, is how states may respond to the negative externalities produced by inequality. Two obvious responses arise (see e.g., Benoit and Osborne 1995; Eaton and White 1991; İmrohoroğlu et al. 2000). The first relies on reducing the underlying problem—inequality—as a way of reducing its negative consequences. This mostly concerns redistributive measures aiming to reduce income differences in society. Indeed, transfers and other social safety net mechanisms can be understood as the price that the ruling class has to pay for buying social peace, by increasing the opportunity cost of engaging in crime for the poor (Sala-i-Martín, 1997). However, states can aim to maintain existing levels of inequality by tackling the consequences of inequality—crime—rather than the cause itself. They can do so by investing in security, namely through public spending on policing and incarceration, therefore increasing the expected cost of engaging in crime.

Rueda and Stegmueller (2016) recognize indeed this tension and study how the rich—who should not directly benefit from redistribution—are more likely to support redistribution in more unequal areas through heightened fear of crime. They show that individuals in more unequal regions are indeed more afraid of crime, a phenomenon which leads them to demand more redistribution as a way of reducing this negative externality of inequality.[2]

[2] Their model relies on the assumption that redistribution is more cost-effective than increasing spending on security, as supported by authors such as Donohue and Siegelman (1998).

While the connection—or the lack of it—between trust in political institutions and preferences for redistribution has been examined by many political scientists and sociologists (e.g., Goubin and Kumlin, 2022; Peyton, 2020; Svallfors, 2013), we do not know so much about how trust in the police may influence citizens' preferences for investments in security over redistributive policies. One of my dissertation papers seeks to contribute to this line of research by focusing on the determinants of trust in the police. A better understanding in this initial stage in the causal chain can inform future research on how trust in the police may shape the broader policing-redistributive trade-off as a response to the negative externalities of inequality. The other two dissertation papers will examine one phenomenon explaining varying levels of support for redistribution: contact between individuals from different social classes.

Preferences for redistribution

Scholars in various social science disciplines have addressed very different factors to understand why public support for redistribution varies across and within societies. Most of the approaches in comparative political economy build on the model developed by Meltzer and Richard (1981) to emphasize the importance of material gains and losses to define support for redistribution. One of the main implications of this model is that increases in inequality—understood in this model as deviations between the median and the mean income—should boost support for redistribution among the general public. However, the empirical support for this claim is far from being conclusive. Cavaillé and Trump (2015) try to explain this by splitting the concept of redistributive preferences in two: redistribution from the *haves*, as driven by processes of income maximization, and redistribution to the *have-nots*, as a process of other-oriented social affinity (see e.g., Dimick et al., 2018). Therefore, while rising inequality would lead us to expect increased public support for redistribution—if looking at

the former concept—growing ethnic and racial heterogeneity may have counteracted this effect, according to the latter concept (e.g., Luttmer, 2001; Rueda and Stegmueller, 2019).[3]

Scholars have examined very different factors to explain support for redistributive policies as a function of the gains and losses that individuals may extract from them. For example, Americans become more supportive of welfare spending after experiencing episodes of economic hardship such as a job loss (Margalit, 2013). However, not only do citizens consider the present but also the future when thinking about redistribution. The often-stated idea that the poor do not fully support redistribution because they expect their offspring to climb up the social ladder was formalized by Bénabou and Ok (2001). Prospects of upward mobility of course do not require the existence of future generations. Lifetime income also affects support for redistribution, as citizens may create expectations about future income based on their age (work experience) and level of education (Rueda and Stegmueller, 2016). Moreover, the recognition of varying levels of income along the lifetime span has implications not only if considering redistribution vertically, as in Meltzer and Richard (1981). Redistribution can also be conceptualized as an insurance mechanism (Moene and Wallerstein, 2001). The rich may accept a reduction in their disposable income now if they think that, at some point in their lives, they may experience a negative economic shock and benefit from this insurance mechanism. Workers with general skills are indeed less prone to support redistribution than those with specific skills (Iversen and Soskice, 2001). Whereas the former might easily transfer their skills to a new job, the latter may face a high level of uncertainty if made redundant and support social policies protecting them against potential losses. This relation may be moderated by employment protection legislation, which can mitigate the effect

[3] Other authors have considered electoral institutions and incentives of leftist parties (e.g., Iversen and Soskice, 2006; Pontusson and Rueda, 2010) in order to offer explanations that look at the supply side of redistributive policies, aiming to understand why increases in inequality are not necessarily followed by this type of policies.

of individual risk factors on social policy preferences (Gingrich and Ansell, 2012). The specificity of skills may also help explain why liberal market economies promote the acquisition of general skills and provide low levels of insurance, whereas coordinated market economies encourage the acquisition of specific skills and offer more generous protection (Hall and Soskice, 2001). The insurance argument can be applied also to understand other phenomena, such as why workers in occupations more vulnerable to automation demand more redistribution (Thewissen and Rueda, 2017).

These and many other pocketbook factors define, according to Cavaillé and Trump (2015), redistribution from the *haves*. However, it is phenomena of a different nature that primarily determine redistribution to the *have-nots*. Affinity between the rich and the net beneficiaries of redistribution play an important role to understand varying demand for redistribution across societies. Lupu and Pontusson (2011) employ income differentials between different groups in society as a proxy for social distance, finding that redistribution is larger when middle-income voters are closer to the poor and lower when they are closer to the rich. Shayo (2009) proposes another influential model to understand other-regarding concerns in the study of redistribution (Dimick et al., 2018). Shayo (2009)'s theory proposes that social identity and preferences are endogenously determined. The poor will demand more redistribution when they identify with their social class and less when they do so with their nation.[4] In turn, their class identification will be larger in cases in which the status of the poor is relatively higher. This gives rise to two distinct equilibria: one characterized by low levels of redistribution and the poor identifying with the nation, and another by high levels of redistribution and the poor identifying with their class. Affinity between the rich and recipients

[4] Liberal nationalists, however, argue that national identification can serve as an engine for promoting solidarity towards co-nationals, grounded in a shared sense of nationhood that holds societies together (Miller, 1995; Moore, 2001).

of welfare is at the heart of studies on racial and ethnic diversity too (e.g., Rueda and Stegmueller, 2019).

Another stream of research has examined how certain beliefs, values and perceptions shape preferences for redistribution. Given its relevance to the focus of this dissertation, this literature merits a more in-depth examination. Literacy about the distribution of resources in society and citizens' standing within that are factors that scholars have examined to understand reduced support for redistribution, a pattern that contradicts the findings expected by Meltzer and Richard (1981) for unequal societies. Numerous studies across countries in different continents have shown that individuals systematically misperceive their own position on the income ladder (Engelhardt and Wagener, 2018; Karadja et al., 2017). Individuals tend to exhibit a bias to conceive themselves as middle class, such that the poor overestimate their ranking in society while the rich underestimate it (Bublitz, 2022; Cruces et al., 2013). This has prompted many scholars to examine how informing individuals about their real position in the income distribution affects their demand for redistribution. Respondents who overestimated their position in society demand more redistribution when informed that they were indeed not as rich as they thought (Cruces et al., 2013; Fernández-Albertos and Kuo, 2018).[5]

However, individuals not only misperceive their own position on the income ladder but also aggregate levels of inequality (see Hauser and Norton, 2017, for a review).[6] Kuziemko et al. (2015) find that informing Americans about real levels of inequality change their views about inequality but has only modest effects on most redistributive policies. Fetscher and Rueda (2023) examine the heterogenous effects of informing about actual levels of inequality on the preferences of the rich and the poor. Although their results are also mixed, they find an

[5] See Condon and Wichowsky (2020) for how priming upward comparisons can increase demand for redistribution even without the necessity to provide information about the distribution of resources.

[6] Individuals' tendency to place themselves in the middle of the distribution might indeed be a sign of their systematic underestimation of inequality.

increase in the demand for redistribution among poor individuals with *a priory* low guesses of inequality.

Jacobs et al. (2021) addresses biases in media coverage of economic phenomena for explaining why citizens are not more concerned about inequality in times in which the distribution of resources in society becomes less equal. My dissertation looks also at biases in individuals' perceptions of inequality, yet not provoked by media but by socioeconomically segregated networks (McPherson et al., 2001; Schulz et al. 2022; Thal 2017). As people make inferences about inequality by sampling their own immediate environments (Dawtry et al. 2019; Galesic et al. 2018), socioeconomically segregated social environments will deceive them to think that the population as a whole is as "equal" as it is their surrounding environment. This leads them to confuse segregation—individuals observe homogeneity around themselves because the other classes occupy different geographical and social spaces—with equality—the resources of a society are equally distributed. Then, as inequality keeps hidden, so does the demand for policies aiming to reduce it. It is in this context of increasing invisibility of privileges around the globe (Azpitarte et al., 2021; Mijs and Roe, 2021; Musterd et al., 2017) that contact between social classes may contribute to reverse this phenomenon.

Perceptions of the causes of inequality also have important implications for the study of redistribution. Fairness beliefs—about whether inequality results from luck, or conversely, from merit—might shape the way in which citizens form their preferences for redistribution (Alesina and Angeletos, 2005; Bénabou and Tirole, 2006; Gualtieri et al., 2019; Piketty, 1995).[7] As inequality is perceived to be the outcome of fair competition and hard work, concerns about setting remedies to it fade out. Almås et al. (2020) show that citizens in countries as different as Norway and the United States are less willing to accept inequality when primed to attribute its origin to luck rather than merit. These results are confirmed by Cohn et al. (2023), who find

[7] On the determinants of the perceived fairness of a redistributive policy, see Rodon and Sanjaume-Calvet (2020).

that disparities in redistribution preferences between the wealthy and the general public are at least partly explained by distinctive fairness beliefs. The wealthy tend to attribute economic success to dispositional factors—hard work and intelligence—while emphasizing these factors to be driven in turn by individual choice rather than genetic endowment (Suhay et al., 2021).

Mijs (2018) develops a theoretical framework to examine fairness beliefs which emphasizes the role of socializing institutions as *inferential spaces* where children and young people learn about the origins of inequality. According to his theory, homogeneous institutions expose individuals to a range of outcomes but not to variation in social backgrounds. In contrast, heterogenous institutions expose individuals to both diverse outcomes and diverse backgrounds. As a result, individuals in the latter setting are more likely to attribute success to structural factors than those in the former setting, whose exposure to social diversity is limited.

My dissertation engages with the comparative political economy literature on support for redistribution, adopting a sociological lens that emphasizes how individuals learn about inequality (Mijs, 2018). I argue that contact between people from different classes can reduce meritocratic beliefs by highlighting systemic features shaping the material lives of individuals, as well as upwardly update perceived levels of aggregate inequality. Furthermore, drawing on a social class reading of contact theory, I argue that contact with members of other classes can foster greater empathy and perspective-taking towards disadvantaged communities (Mo and Conn, 2018), leading individuals to internalize others' economic hardship and support policies aimed at reducing inequality (Newman, 2014). Moreover, I aim to introduce a more causal approach into a line of research that, with a few notable exceptions (Gingrich, 2019b; Mo and Conn, 2018; Newman and Vickrey, 2017), has employed cross-sectional data and focused on the study of neighbourhoods (Bailey et al. 2013; Franko and Livingston 2020; Minkoff and Lyons 2019).

The first paper of this dissertation explores mandatory military service as a milieu reducing segregation between social classes. This setting has been overlooked in the literature, something surprising given its potential for inter-class contact and its methodological attractiveness (exogeneity). Conscription was abolished in Spain at the beginning of the 21st century. Until that moment, people from all walks of life were brought together for around a year into a common space which eased the creation of inter-class friendships which, otherwise, would have been very difficult to arise. Paralleling some works which look at how heightened inter-regional contact favored by Spanish conscription increased sympathy towards those from other regions (Bagues and Roth, 2020; Cáceres-Delpiano et al., 2021), I claim that contact at mandatory military service with people from other classes affected the way in which some generations of Spanish men perceived inequality and formed attitudes towards redistribution.

Methodologically, I employ a differences-in-differences approach, focusing on Spanish cohorts close to the removal of conscription and with women as the control group. I find that men born in conscripted cohorts are significantly more likely to support economic redistribution and perform various robustness checks to deal with the low levels of compliance that may have existed for the last conscripted cohorts. Moreover, I present placebo tests to discard that these findings merely reflect a differing trend between genders in a broader set of political attitudes (e.g., pro-environmental attitudes, anti-immigrant attitudes), and rule out alternative explanations (e.g., worsened economic conditions among conscripted males, sympathy towards the party abolishing conscription among non-conscripted cohorts, reduced institutional trust).

For the second paper of this dissertation, I address another setting previously unexplored in the literature: romantic partnerships between people from different social classes. Surprisingly, though diversity in social networks has been addressed by some scholars to explain economic attitudes (e.g., Liu, Kuo and Fernández-Albertos, 2020; Newman, 2014;

Newman and Vickrey, 2017), the role of arguably the most central person in one's life has been ignored in this literature. I claim that entering a partnership with someone from a different social class may result in a better understanding of material differences in society, so aligning with views and policies aware of this problem and willing to address it.

I use longitudinal data from the British Household Panel Survey to overcome self-selection concerns (e.g., left-leaning individuals may be more likely to marry down), finding that those who marry/start to cohabitate with someone from a different social class experience a movement to the left in their economic attitudes *vis-à-vis* those who do so with someone from the same class. This effect is particularly strong for those doing so with someone from a higher class, maybe suggesting that attributions of wealth—the factors that individuals point to explain varying levels of wealth—are more easily activated than attributions of poverty. I discard alternative explanations (i.e., partner's persuasion, alterations in material conditions) and present again various placebo tests showing that cross-class partnerships have no effect on political attitudes unrelated to the economic domain. Furthermore, I find the effect of cross-class partnerships to be more pronounced among those less likely to learn about inequality through means others than personal experience—those not interested in politics and those with lower levels of educational attainment.

Attitudes towards the police

Traditionally, the study of attitudes towards the police has been mostly carried out by scholars in the field of criminology (Brown and Benedict, 2002; Liu and Crank, 2010). Some criminologists argue that perceptions of the police are simply the by-product of socialization processes during adolescence which shape the way in which individuals think not only about the police but also about other authority figures in their lives, such as teachers or parents (Amorso and Ware, 1983). Other authors have emphasized the distinctiveness of the police and have allowed greater room for the quality of interactions between citizens and officers to shape

public attitudes (Bradford and Jackson, 2018; Bradford et al., 2009). Contextual factors have also been examined in this literature (Cao et al., 1996), with certain structural characteristics of neighbourhoods—such as concentration disadvantage, high crime and disorder—found to negatively affect attitudes towards the police (Sampson and Bartusch, 1998; Weitzer, 2000). Socio-demographic characteristics such as age, gender or immigration status are also important determinants in the literature (e.g., Brown and Benedict, 2002; Bradford and Jackson, 2018).

Attitudes towards the police, however, have been less extensively studied by scholars outside of criminology. The third paper of this dissertation seeks to integrate this primarily criminological concern into the study of political violence and the historical legacies framework. Numerous political scientists and sociologists have examined how political violence during civil wars shapes post-conflict political attitudes (for reviews, see Bauer et al., 2016; Davenport et al., 2019; Price and Yaylacı, 2021). Still, the sign of its effect on many political outcomes is under dispute. For example, while Blattman (2009) finds a positive impact of violence on voting in Uganda, Zhukov and Talibova (2018) find a negative one for the case of Russia. Lupu and Peisakhin (2017) find that violence experienced by Crimean Tatars fosters hostility towards Russia, and that this sentiment is transmitted across generations. A penalty for the perpetrator of violence is, however, not universal (Rodon and Tormos, 2023; Wang, 2021; Young, 2019), and might depend on factors such as the credibility of retribution (Rozenas and Zhukov, 2019).

In recent years, scholars have increasingly turned their attention to the political legacies of the Spanish Civil War. Balcells (2012) finds that violence leads to the rejection of the political identity of the perpetrator. Francoist victimization is associated with an increased probability of identifying with the left, whereas Republican victimization is associated with a decreased likelihood. Rodon (2022) confirms this pattern at the electoral level: municipalities which received more Francoist repression during the civil war and its aftermath voted more for

the left in the first free elections. Domènech and Sánchez-Cuenca (2022) find Francoist repression to be a mediator in the observed relationship between agrarian inequality and leftist vote, extending the analysis beyond the first elections. Villamil (2021a) finds the effect of wartime victimization to be conditional on the absence of clandestine left-wing networks during the regime. The positive effect of Francoist repression on leftist vote is stronger in those places where underground opposition organizations were active during the dictatorship.

Though the Spanish Civil War mainly unfolded along the left-right cleavage, the importance of the center-periphery cleavage should not be underestimated. Martínez (2024) argues that not all types of violence have the same effect on political identities. Whereas indiscriminate violence—fascist aerial strikes—fostered stronger Basque identification, selective violence is associated with a weakening of Basque identity. Rodon and Tormos (2023) find that Catalans in municipalities which experienced more intense Francoist repression during the Civil War and the post-war period are less likely to support independence. Similar to the argument presented in this thesis, they emphasize that these citizens reject disruptions of the status quo, which may be perceived as threats to social harmony and a risk to personal integrity.

The third paper of my dissertation proposes a novel explanation for what I term the "Paradox of Spain": the coexistence of critical assessments of the Spanish police by international bodies (e.g., the Commissioner for Human Rights of the Council of Europe) with consistently high levels of public approval within the country.

I argue that this puzzle can be explained, at least partly, through the burden of historic processes in the country. The Spanish Civil War was a particularly harsh conflict, in which around 200,000 people were murdered as victims of political repression (Preston, 2012). The cruelty of the war caused that even natural supporters of the Republican faction wished a quick victory of the Francoist bloc just to end the carnage (Cazorla-Sánchez, 2005). The traumatic

experience of the war remained with the Spanish public even after the War ended with the victory of the Francoist forces and the establishment of a forty-years dictatorship. Spaniards were eager for peace and afraid of the war repeating itself, a phenomenon which the regime also fueled by presenting Franco as the only figure capable to guarantee peace in a country which had just left behind a civil war and was surrounded by a world-war context (Cazorla-Sánchez, 2005). I claim that this situation made Spaniards more open to trade freedom for security, increasing positive views towards institutions—such as the police—perceived to be in charge of keeping order and stability, "prerequisites" for the chaos and violence of the civil war not repeating themselves. I also argue that, in part thanks to these same phenomena, the efforts of the Francoist regime to indoctrinate Spaniards in the necessity of creating a more *vertical* and hierarchical society (Richards, 1998; Vincent, 2007) were successful across a large section of the population. Fear of repression had a paralyzing effect (Calvo Vicente, 1995; Del Arco Blanco, 2009; Molinero and Ysàs, 1998; Sevillano Calero, 2003; Young, 2019) which eased this *verticalizing* indoctrination, making people more risk averse (Young, 2019) and intertwining with the memory of the Civil War itself (Molinero and Ysàs, 1998).

I test this argument by exploiting sub-national variation in the levels of repression across Spanish regions, as the intensity of these factors should be larger in those places which experienced more repression during the Civil War and the post-war period. Following conventional practice in studies of historical legacies, I claim that attitudes towards the police were transmitted intergenerationally either within the family or throughout communal networks (e.g., Cremaschi and Masullo, 2023; Zhukov and Talibova, 2018). Therefore, I link attitudes towards the police of individuals living in a specific region nowadays with the levels of repression that specific area experienced decades earlier—during the Civil War and the post-war period. I begin by testing the relationship at the level of municipalities in Catalonia, where I find that Francoist repression positively correlates with positive evaluations of police

performance. Consistent with previous findings showing that the intensity of legacies of violence weakens for more distant cohorts (e.g., Lupu and Peisakhin, 2017; Wang, 2021), this effect is stronger for generations who experienced the repression of those years themselves, but it fades out for generations of Catalans born under democracy. I then extend the analysis to the level of provinces, finding that previous findings generalize to other regions in Spain. Spaniards in provinces which experienced larger levels of Francoist repression rate more positively the police profession. Finally, I employ a special module of the European Social Survey to confirm the relationship between repression and positive views of the police on numerous dimensions at the NUTS-2 level. I rule out several alternative explanations and find a positive relationship also with a range of attitudes that are theorized to mediate this association.

Finally, I perform a preliminary analysis for the case of the Guatemalan Civil War. Some phenomena in this conflict resonate with the ones analysed for the case of Spain, such as the fear of a return to the civil war, the socialisation in fear of civilians and their reception to the military's propaganda (Bateson, 2017; Green, 1999). The empirical analysis shows that individuals in municipalities that experienced higher levels of state-led violence during the civil war tend to be more trusting of the police. These results underline the generalizability of the proposed thesis beyond the specific case of the Spanish Civil War.

This paper aims to contribute to the literature on attitudes towards the police by incorporating historical factors into the analysis, rather than focusing on factors typically examined by criminologists (e.g., Brown and Benedict, 2002). To the best of my knowledge, only two papers have examined the impact of wartime violence on perceptions of the police (Blair and Morse, 2021; Deglow and Sundberg, 2021). This paper, however, is distinct in at least three ways: first, it explores violence against civilians, rather than aggregate indicators of violence conflating multiple forms of conflict; second, it is able to assess the long-term legacies

of civilian victimization, given the temporal distance from the conflict; and third, it examines an advanced democracy—Spain—while also enhancing the external validity of its findings through an extension to the case of Guatemala.

Chapter 1: Draft of the first paper

Mandatory Military Service and Support for Redistribution

A Tale of Inter-Class Contact

Abstract: Does contact between people from different social classes increase support for policies aiming to reduce inequality? Limited evidence and endogeneity issues call for further investigation into the political effects of inter-class contact. I contribute to this field by examining an unexplored institution in the existing literature: compulsory military service. A diff-in-diff approach, focusing on Spanish cohorts close to the removal of conscription and with females as the control group, reveals that males born in conscripted cohorts are significantly more likely to support redistribution. I present placebo tests discarding that these findings merely reflect a differing trend between genders in a broader set of political attitudes. Furthermore, I rule out plausible alternative explanations as well as present two manipulation checks indicating that the removal of conscription did indeed reduce inter-class contact among males.

And by making the rich kids go to school with everyone else, they grow up with those other kids as friends. And when they become wealthy adults, they have to think twice before they screw them over.

Where to invade next (Michael Moore, 2015)

Introduction

Once upon a time, there was a town called *Unequaland*, made up of two neighbourhoods: *Out of sight*, in the north, and *Out of mind*, in the South. The rich lived in *Out of sight*, where they also worked and enjoyed their leisure time. Even if willing, they could not commute to the South, where the poor likewise spent their lives. A wall geographically—and socially—separated the two neighbourhoods. So neither the northerns knew much about the lives of the southerns, nor the latter about the lives of the former.

When asked about the level of inequality in their town, people from *Unequaland* had a look around them, and reported to live in a society where resources were very equally distributed. After all, that was not surprising according to what they did perceive. Although the neighbourhoods were completely different from one another, they were internally quite homogeneous.

Prince was born in *Out of sight*. He grew up there, went to the school there, and made all his friends therein. So everyone expected that he would find his loved one in *Out of sight*. However, one day, when going for a walk, he noticed a little hole in the wall that separated the two districts. Curiosity led him to have a look through. As soon as his eyelashes touched the cold surface of the wall, he fell in love with the most beautiful smile he had ever seen. Sick of love, he made a big hole in the wall, and crossed the border to encounter *Pauper*. Apart from living happily ever after, *Prince* and *Pauper* were able to know different places to those where they had been reared. It was then when they realized that *Unequaland* was not as equal as they used to think. Furthermore, *Prince* had the possibility to meet many *Out of mind*

inhabitants, which people in *Out of sight* hardly used to talk about. If anything, they drew them as lazy beings with a severe lack of effort. Once he built strong ties with them and put himself in their shoes, *Prince* changed all his prejudices, and wondered if the Council of *Unequaland* should enhance the living conditions of the poorest at the expense of some resources of the richest.

This is a study about the impact of institutions and experiences which break down the metaphorical wall dividing societies between social classes. In the real world, this takes the form of isolation of classes and lack of contact between their members. And as the simplistic fable I start this text with aims to highlight, this lack of contact may be connected to three phenomena which are known to undermine support for redistribution: underestimations of the actual level of inequality in society (Kuziemko et al., 2015), reduced possibilities for empathy processes towards outgroups (people from other social classes) (Mo and Conn, 2018; Newman, 2014), and more meritocratic and less structuralist understandings of inequality (Gualtieri et al., 2019; Larsen, 2016; Mijs, 2019). Although the empathy mechanism should affect mostly those from privileged backgrounds, the other two mechanisms should influence both the richer and the poorer. Then, I expect the effect of inter-class contact to be positive for both the more and the less privileged.

I address the effect of inter-class contact through the study of a setting which has allowed for exceptionally high levels of it in Spanish society: mandatory military service. Obviously, removal of conscription did not reduce to zero contact between classes, turning Spanish society into the *Unequaland* of the introductory tale. However, it reduced substantively inter-class contact, as some empirical analyses at the end of the paper suggest. Because I have no information on individual's military attendance, I rely on his year of birth

to deduce risk of conscription.[1] Drawing on Spanish cross-sectional data from the European Social Survey (ESS) and following a difference-in-differences methodology with women as the control group, I find that being born in a conscripted cohort around the cut-off point of removal of conscription in Spain significantly increases the probability of supporting redistribution among males, relative to females.

The results, which are robust to many different specifications, successfully pass a placebo analysis in which I specify different (false) dates for the suspension of conscription, as well as placebo tests on a variety of unrelated political attitudes. Furthermore, I discard several competing explanations to the one proposed about inter-class contact.

Finally, I provide two analyses suggesting that removal of conscription did reduce inter-class contact among males. First, males in conscripted cohorts have a better understanding of their own material position in society, compared to their counterpart females and to males in non-conscripted cohorts. This, I argue, may be caused by more socioeconomically diverse networks of males in conscripted cohorts, who get more accurate information about the actual distribution of resources in society. Second, I find a positive diff-in-diff effect on the likelihood of having a partner from a different social position, a more direct indicator of inter-class contact.

This paper aims to extend the very scant research on inter-class contact and preferences for redistribution in a twofold way. First, by proposing a different way to look at contact between classes beyond the geographical setting of existing studies (e.g., Bailey et al., 2013; Minkoff and Lyons, 2019; Franko and Livingston, 2020). Second, by trying to overcome the self-selection concern which may be present in that type of analysis, whose results could be explained by the sorting of individuals into neighbourhoods according to their political preferences (Christensen et al., 2024; Gallego et al., 2016). Finally, it also

[1] Other surveys which do contain information on Spaniards' military attendance do not measure, however, support for redistribution.

contributes to the literature on corrections of inequality misperceptions (e.g., Hvidberg et al., 2023; Kuziemko et al., 2015) through a setting which overcomes the external validity concerns often present in survey experiments (Barabas and Jerit, 2010).

Inter-class contact and support for redistribution: A social class reading of contact theory

Understanding why some people support economic redistribution while others oppose it has been at the core of political economy for decades. Departing from the workhorse model in this field, many authors have expanded Meltzer and Richard (1981)'s, addressing different determinants of preferences for redistribution which allude to the self-interest motive (e.g., Benabou and Ok, 2001; Beramendi and Rehm, 2016; Rueda and Stegmueller, 2019; Thewissen and Rueda, 2019). Other scholars, however, have emphasized *ideological* (or less materialistic) explanations to address varying levels of support for redistribution (e.g., Luttmer, 2001). For the latter, the focus is not so much on the personal benefits (or costs) of redistribution but on individuals' perceptions of inequality and deservingness of welfare recipients (Gualtieri et al., 2019; Mijs, 2019; Suhay et al., 2021).

Regarding the impact of inter-class contact on preferences for redistribution, it is surprising the scarcity of research addressing this issue given the vast number of studies on contact theory (on intergroup contact in the military, see Carrell et al., 2015; Finseraas and Kotsadam, 2017; more broadly, see Allport, 1954; Eger et al., 2022; Pettigrew, 1998). The contact hypothesis states that contact with individuals from other groups has the capacity to reduce prejudices towards the outgroup. Although this thesis has been addressed extensively around racial and ethnic lines (e.g., Davies et al., 2011; Finseraas and Kotsadam, 2017; Pettigrew, 1998), publications looking exclusively at contact across socioeconomic lines are very rare.

Minkoff and Lyons (2019) largely resort to this hypothesis to explain why people living in more income-diverse neighbourhoods of New York City are more aware of large income differences, as well as more supportive of policies to reduce the gap. They argue that those exposed to more income diversity in their neighbourhoods have more chances to observe other material realities and change prior negative beliefs about the lower classes, turning inequality into a closer and more tangible concept. Franko and Livingston (2020), changing the operationalization of economic segregation, expand the analysis to nearly the entire US population. Bailey et al. (2013) construct a similar argument to explain why people living in more deprived areas are more supportive of redistribution in England. They argue that what they call *knowledge accumulation* is the relevant explanatory mechanism, whereby "greater proximity to low-income groups [heightens] knowledge or awareness of the scale of that group or of the conditions in which they live" (Bailey et al., 2013: 2158). Unfortunately, the cross-sectional nature of these studies makes them susceptible to criticisms on the basis of self-selection: it could be the case that those who hold more redistributive preferences are more willing to move to more income-diverse neighbourhoods (see e.g., Christensen et al., 2024). Aiming to overcome these methodological issues, Mo and Conn (2018) examine the case of Teach for America through a regression discontinuity design. The results indicate that this national service program—which offers top college graduates the opportunity to teach in low-income schools—promotes greater empathy and perspective-taking towards disadvantaged groups. Also within the field of education, Gingrich (2019b) compares mixed and stratified schools in a context of low standardization. Using matching techniques, the author finds that stratification shapes beliefs about fairness and voting but has a limited effect on attitudes towards redistribution.

Although not strictly looking at contact between social classes, some other papers offer important insights on this topic (e.g., Liu et al., 2020). Newman (2014) finds that those

who have friends struggling to make ends meet financially are more supportive of redistribution. Expanding on previous work, Newman and Vickrey (2017) make use of longitudinal data to reduce endogeneity concerns and find that individuals whose close network has suffered an increase in unemployment experience a boost in preferences for redistribution. Cognitive and emotive empathy (Cuff et al., 2016; Pettigrew, 1998; Stephan and Finlay, 1999), that is, the capacity to imagine another person's circumstances and experience an emotional reaction, is one of the mechanisms which Newman (2014) and Newman and Vickrey (2017) resort to explain their findings. These psychological processes generate a vicarious experience of economic distress which facilitates the transmission of attitudes and beliefs concerning government efforts to support those facing economic hardship.

A couple of experimental studies (Sands, 2017; Sands and de Kadt, 2020) could be *apparently*—or at first sight—considered to address inter-class contact and support for redistribution. Sands (2017) finds that passers-by in a wealthy public space are less likely to sign a petition for a millionaire tax in the presence of poverty-stricken people (confederates of the experiment). In a mirrored-context experiment, Sands and de Kadt (2020) find that individuals in a deprived area are more prone to support a rise in taxes on the rich in front of a high-status car. These results may suggest that contact with people from other classes awake self-interest processes, driving an effect whose direction is conditional on income: the preferences of the rich will lower while those of the poor will rise. Although one could think that these findings go against my theoretical expectations about inter-class contact, it should be highlighted that these two experiments just capture fleeting exposure to inequality rather than real contact with people from other classes. "A crucial determinant of whether diversity will result in positive or negative attitudes towards […] out-groups is the degree of meaningful social contact and interaction between residents", Sturgis et al. (2014: 1290)

claim. Actually, the recognition of diverging effects of intergroup contact conditional on the type of contact established was already noted by Allport more than half a century ago: "[Casual contact] does not dispel prejudice; it seems more likely to increase it" (1954: 263). Whereas superficial interactions may make outgroups more visible as mere competitors for material resources, deep contact may have the capacity to change beliefs about the nature and extent of inequality as well as allow empathy processes towards those with material difficulties.

Inferring the extent and causes of inequality

Although the literature addressing inter-class contact is not extensive, there are some fruitful subfields in the study of preferences for redistribution which are worth addressing. As Bailey et al. (2013) and Minkoff and Lyons (2019) argue too, contact between people from different social classes may change how we perceive inequality and how much of it we observe. Therefore, it is very important to have a look at the existing literature connecting political preferences with the perceived extent and nature of inequality. This means looking at research on two mechanisms theorized to mediate the relationship between inter-class contact and preferences for redistribution.

Let us start addressing the level of inequality—and social conditions too—that individuals perceive.[2,3] Some academics have looked at the impact of correcting inequality misperceptions—see Hauser and Norton (2017) for a review. Kuziemko et al. (2015) find that displaying real levels of US inequality heightens concerns about it and move up some tax policy preferences. Also relevant are the results of Pellicer et al. (2019)'s experiment. South-Africans who are shown how exceptionally unequal their country is better understand that

[2] Although inequality is a social condition, with the latter term I refer to other social conditions which are not relational in nature. This distinction is relevant as I will explain below.

[3] Not surprisingly, authors such as Gimpelson and Treisman (2018) show that it is perceived—not actual—inequality what strongly correlates with demand for redistribution.

inequality is not as inevitable as they used to think and upwardly adjust their preferences for redistribution. In this vein, displaying what the income gaps are regarding ascribed characteristics such as race, sex or family background produces similar results (Becker, 2019).[4]

A critical reader may argue that contact with poorer people could also make rich individuals to upwardly adjust their previously underestimated position in society and lower support for redistribution. However, I anticipate the positive effects of updates in information about the general level of inequality and meritocracy, as well as of empathy processes, to dominate over that. As discussed above when reviewing Sands (2017) and Sands and de Kadt (2020), deep (not casual) contact should produce positive attitudes towards outgroups. Furthermore, Cruces et al. (2013) and Fernández-Albertos (2018) find that those who learn they are richer than thought do not become less supportive of progressive taxation.[5]

Yet why are inferred levels of inequality connected to inter-class contact? People sample from their own immediate social environments to make inferences about the general population (Dawtry et al., 2015; Galesic et al., 2012; Schulz et al., 2022; Thal, 2017).[6] Given that individuals are socially and geographically segregated (Massey and Fischer, 2003; McPherson et al., 2001), the samples they use to extrapolate to the larger population are not representative. Dawtry et al. (2015) find that the negative correlation between income and support for redistribution is not just an artifact of self-interest processes, but also of the rich extrapolating the general level of wealth from their immediate environments, which makes

[4] Becker (2019) can be framed in terms of both the extent *and* the nature of inequality. It provides information on *how much inequality* exists regarding *ascribed characteristics* (a type of inequality almost unequivocally perceived as unfair).

[5] I show in the Appendix (Section A.11) a positive effect of conscription not only for the poorer but also for the richer.

[6] Clearly, this is not the only source of information about inequality available for citizens. However, it is plausible that its effect on political preferences is greater than that of other sources like media consumption (Diermeier et al., 2017), which lack, e.g., the emotional component of deep personal contact.

them less critical with the system and the current distribution of material resources. Likewise, Thal (2017) finds that the wealthy extrapolate from the observed levels of social conditions–such as job opportunities—in their affluent neighbourhoods to make inferences of the larger population. Therefore, affluent individuals in less segregated areas hold more negative—and accurate—perceptions of social conditions, which makes them more likely to have engaged in forms of participation which specifically address harmful social conditions.

Whereas the sign of the bias from extrapolation for social conditions other than inequality is contingent on income—the rich will overestimate them but the poor underestimate it—the relational nature of inequality makes it a totally different story. If people infer general levels of inequality from what they observe in their immediate environments—and what they observe is economic homogeneity—then *both* the rich and the poor will underestimate the level of inequality. Therefore, although in the case of other social conditions the impact of the bias on the aggregate level of perceptions might be ambiguous, in the case of inequality that will be clearly negative. Regardless of whether one is wealthy or impoverished, segregation means that everyone will make underestimated predictions of inequality, which may translate into lower aggregate levels of support for redistribution and criticism with the system.[7] This is consistent with the prediction made by the social networks model developed by Schulz et al. (2022): economic homophily isolates individuals with others alike, severely limiting the representativeness of their social networks and producing systematic underestimations of inequality. Low levels of inter-class contact can make

[7] One could argue that the effect of correctly updating the levels of perceived inequality and other social conditions among the poor is not unambiguously positive in theoretical terms. Since they should be underestimating the levels of other than inequality social conditions when segregated, they could become less critical with the system when desegregated. However, even if that was true, I consider that the effect on preferences for redistribution of inequality updates should be more pronounced than that of other social conditions, since redistribution is more about reducing inequality rather than improving overall social conditions.

inequality invisible, such that individuals look around them and think *what an egalitarian society they live in.*

Furthermore, people make inferences not only about the levels of inequality in society, but also about the processes generating those unequal outcomes. As many academics have already noted, meritocratic understandings of inequality are very relevant in the process of redistribution preferences formation (Alesina and Angeletos 2005; Gualtieri et al. 2019; Suhay et al., 2021) and for inequality concerns (Larsen, 2016; Mijs, 2019). As the unequal distribution of resources is perceived to be generated through fair competition, concerns about the problem and its possible remedies considerably fade out.

To understand why citizens end up holding more individualistic (meritocratic) or more structuralist beliefs about inequality—that is, whether they think that it is the actions of individuals or, by the contrary, structural factors beyond their control (e.g., social class of origin) what determines their economic destiny—Mijs (2018) advocates for studying socializing institutions as *inferential spaces* where children and young people learn about these processes. Because homogeneous institutions (i.e., segregated neighbourhoods or schools) reduce "a person's exposure to explanatory variables other than a person's attitudes and actions" (Mijs 2018: 8), those mostly exposed to this type of institutions overestimate the importance of individuals' actions and underestimate that of factors which remain hidden to them, like family resources. However, in heterogeneous socializing institutions—which bring together people from very different socioeconomic realities, as it was the case of Spanish conscription—people do observe more variation in both structural factors and economic outcomes, having it easier to grasp the relevance of the former to explain the latter. In other words, homogeneous institutions expose people to a very limited range of information but not to its counterfactual, leading to what Edmiston (2018) calls *the poor sociological imagination of the rich*, that is, the failure of many rich people in segregated spaces to acknowledge

systemic sources of inequality. I argue that conscription enriches *the sociological imagination* of individuals, heightening support for egalitarian policies.

Whereas the mechanism involving empathy towards disadvantaged individuals can be straightforwardly classified as emotional, the other two mechanisms—updates in perceived inequality and in fairness beliefs—are not purely informational but also carry an emotional dimension. This follows from the way inequality is conveyed through inter-class contact: not as abstract figures or statistics showing that inequality is greater than expected (e.g., Fetscher and Rueda, 2023; Kuziemko et al., 2015), but as lived realities whose tangible consequences are internalized through deep interactions with real people. In addition, the mechanism concerning meritocratic beliefs can also be understood in terms of socialization, since it challenges an acquired worldview in which merit is seen as the primary determinant of individuals' fate.

It is worth making it clear that I am *not* directly testing the mediating mechanisms discussed above, they are mostly tools to construct the theoretical basis for the impact of inter-class contact on support for redistribution. I cannot see whether it is upward updates on the level of perceived inequality, less meritocratic beliefs or simply empathy processes (or a combination of these and other non-considered factors) what mediates the relationship between conscription and support for redistribution. Still, I predict that whereas the empathy mechanism applies primarily to those from privileged backgrounds coming into contact with poorer people, the other two mechanism—updates in perceived inequality and in fairness beliefs—concerns both the rich and the poor.[8]

[8] However, assuming a larger effect of being born in a conscripted cohort for the rich is not straightforward if levels of compliance depend on income. I discuss this issue in the section of results.

The socio-political importance of mandatory military service

This is not the first research suggesting a link between conscription and inter-class contact. In their analysis of (marriage) endogamy across history, Van Leeuwen and Maas claim that this institution broadened the marriage horizons of many young men, very likely translating into heightened marriages between people from different social classes (2005: 10). Likewise, Stier and Shavit point to the presence of this idea in the public imagination: "[mandatory military service] is often said to be an integrating milieu where people from different walks of life meet and mix" (2003: 317).

A large body of literature in economics has studied the impact of conscription on different labour and educational outcomes (e.g., Angrist, 1990), finding mixed results about whether it in fact improves or worsens economic outcomes in the labour market. Yet more interesting in substantive terms for the present paper is a smaller but very interesting line of research connecting mandatory military service with political preferences. Fize and Louis-Sidois (2020) find a positive effect on voter turnout, but a null one on preferences for different political parties in France. Both Bagues and Roth (2020) and Cáceres-Delpiano et al. (2021) find very similar effects in their studies about conscription. For those coming from Spanish regions with peripheral nationalism, being assigned to do the service in a different location increases Spanish over regional self-identification. Critically, they argue that the underlying mechanism is contact between people from different regions; giving intergroup contact credit by showing that movers are more sympathetic towards, and have more friends from, other regions than nonmovers, as well as confer outgroups higher levels of trust.

That military service is an ideal place for positive intergroup contact also lies at the heart of Finseraas and Kotsadam (2017)'s experiment on anti-immigrant sentiments, in which some soldiers of the Norwegian Army were randomly assigned to roommates of ethnic minorities during the basic training period. As raised by Finseraas and Kotsadam (2017), I

want to highlight that the expectation of a benign effect of military service on attitudes and sentiments towards outgroups is very well grounded on a theoretical basis. Not only does military service accomplish the four conditions that Allport (1954) proposed for optimal intergroup contact—namely, equal status in the specific context, the share of common goals, a cooperative setting, and the relations occurring under some sort of authority—but also attains a fifth one more recently proposed, and which eases the generation of affective ties: friendship potential (Pettigrew, 1998). Not unimportantly too, conscription takes place at the *impressionable years* of early adulthood (Dinas, 2013; Eger et al., 2022).

Mandatory military service in Spain

At the turn of the 21st century, conscription was abolished in Spain, giving way to a fully professionalized army.[9] The 1982 cohort was the last one subject to conscription in Spain, so those born in 1983 and after were exempted (Law 17/1999, 1999). For most of the cohorts analysed here, the functioning of conscription was the following. Men were called at the age of 19 to do a service of 9 months of duration (those who served before 1991 did it for a longer period—either 12, 18 or 24 months). The first three months comprised the training period of bootcamp ("campamento"). There was not socioeconomic segregation of soldiers at all in that first period. For the following 6 months, soldiers could apply for a preferred destination. That depended mostly on the interest to go to a specific town and on the training received in the bootcamp. Although there could be some specialized companies ("compañías") full of, for example, engineers, the vast majority of the companies may have been highly diverse, including people with very different skills. Furthermore, a group of companies formed the battalions ("batallones"), which in turn formed the regiments

[9] The main source of information for the functioning of Spanish conscription and its mix of social classes has been a professor expert in the history of military service in Spain, whose name must remain anonymous during the review process.

("regimientos"). The regiments (around 8,000 soldiers) conformed the unit of living at which many activities were made and again may have shown very high levels of diversity in the vast majority of cases. Apart from the contact arising from natural cohabitation, inter-class contact was also present in activities like the literacy training provided by other conscripts to many illiterate recruits from lower classes and less developed regions. One may argue that in these activities—which constituted a non-negligible part of the conscription experience—status between groups would not be equal (Allport, 1954), given the authority of those teaching relative to those being taught. However, the pursuit of a common goal in this cooperative setting fostered equal status within the situation—a condition emphasized by Allport, as opposed to general status. A similar argument is presented by Mo and Conn (2018) about equal status between teachers and students in the case of Teach for America.

The exceptionally high levels of contact between social classes that conscription allowed for in this specific country are present in the Spanish collective view of the institution. Anecdotally, it is not uncommon to hear this argument on the lips of former conscripted people around. Felipe González, former leader of the Spanish Socialist Workers' Party (PSOE) and Prime Minister of Spain during four terms, defended compulsory military service arguing that it equalized all the citizens (e.g., Díez and Aizpeolea, 1996). However, wise explanations about the potential for inter-class contact of this institution do not necessarily have to come from relevant political actors but can also arise from ordinary citizens. The following is a fantastic example from an individual posting on a forum that an online newspaper publicly opened for sharing memories about Spanish mandatory military service:

> *"The mere fact of the sharing of struggles, pains, jokes, good and bad times between a lawyer from Barcelona, a shepherd from Jaén and a car mechanic from Valencia at the barracks in Santander, all skint and wearing the same*

uniform, was enough to give meaning to mandatory military service. If on top of this we recognize that the lawyer taught the shepherd and the car mechanic to read, the shepherd taught the others to enjoy and respect nature, and the car mechanic fixed others' breakdowns, this is the pinnacle of unity among Spanish people. Nowadays none of the three gets to know the others, so the difference between social classes is exacerbated. In military service all were skint guys with a uniform. Is there any institution fulfilling that purpose nowadays?"

[Comment of user Valhalla on internet blog 2011 (my translation from Spanish)]

The main threat to my identification strategy concerns the low levels of compliance among the last conscripted cohorts (see Section A.2 in the Appendix), an issue which I deal with through different robustness checks below (e.g., excluding last cohorts, excluding highly educated individuals). Military statistics show the most common circumstances adduced to defer conscription—and eventually elude it for the last cohorts.[10] The most common one was academic studies. A much smaller proportion deferred due to other reasons—such as being elected by vote to a public position—or were considered unfit for the service for medical reasons. For the last cohorts, some opted for an alternative civil service ("servicio social sustitutorio"), but the nature of that (often working with NGOs or local administrations on issues related to social labour) made contact with other classes very common in that alternative service too.

Many men obtained a deferral to join the service for pursuing tertiary education. For the last conscripted cohorts, that could eventually mean elude it. Still, this should have not

[10] Unfortunately, neither the Spanish Military Statistical Yearbooks nor the Ministry of Defense (which I have contacted) have any information on year of birth of conscripted males. This precludes the use of the statistics in these yearbooks for any interesting analysis in my paper (e.g., for calculating the proportion of conscripted males by cohort), as these yearbooks are organized by year of entrance into the military, which not only includes those supposed to enter the military that year given their age, but also those who are entering later than expected because having obtained a deferral in the past.

precluded high levels of inter-class contact in conscription. Even in the extreme scenario of no individuals with tertiary education (ISCED 5-6) joining the service for the last cohorts, the level of socioeconomic (educational) diversity (ISCED 0-4) in mandatory military service during those years would still be quite exceptional.

Another option for those highly educated was doing the service in the form of university militias ("Milicias Universitarias").[11] They could do a different (segregated) training and then, holding a higher military rank, join the other conscripts. This, in principle, would not be positive for optimal intergroup contact as it violates the condition about equal status in the specific context. However, those doing university militias were often in charge of organizing the social life of the rest of the soldiers too. Therefore, contact between them was not limited to military situations in which the former held a higher level of authority than the latter, but also included social activities. Organizing the social life of soldiers was not a minor issue in those years, as there was a concern in society about the spare time of soldiers due to *bad habits*, such as drug abuse. Furthermore, not many university students joined these militias, as they required doing a larger military service than normal, with more responsibilities and less spare time.

Data and methodology

I draw on Spanish cross-sectional data from the first nine waves of the ESS, one of the most high-quality and used sources of attitudinal data. This spans the 2002-2018 period, with interviews carried every even year. To capture support for redistribution, I rely on an item frequently used in the literature: "The government should take measures to reduce differences in income levels". Respondents indicate if they strongly agree, agree, neither agree nor

[11] "Milicias Universitarias" was the unofficial name of this programme, which had different official names during the second half of the 20th century: "Instrucción Premilitar Superior", "Instrucción Militar para la Escala de Complemento", and "Servicio para la Formación de Cuadros de Mando".

disagree, disagree, or strongly disagree with this statement. The distribution of this variable shows that around 80% of respondents indicate some level of agreement both across the full sample of the ESS and across the different subsamples of cohorts employed in the empirical analysis. The number of individuals indicating a neutral response ("neither agree nor disagree") is above 10%, with a small fraction of people indicating explicit disagreement. In light of these numbers, it seems appropriate to consider neutral responses as a less overt and more subtle expression of opposition to redistribution (as in Rueda, 2018: 230). Therefore, I create a dummy variable taking value 1 if the respondent either "agrees" or "strongly agrees", and 0 if "neither agrees nor disagrees", "disagrees" or "strongly disagrees".[12]

The consequences of compulsory military service (and the abolition of it) have been broadly studied among economists. Although most papers address its effect on labour, education and health outcomes, their methodological approaches are very relevant here. To deal with the problems arising when studying the impact of conscription, several studies exploit the fact that in most countries women were exempted. This allows for a difference-in-differences specification with women as the control group. This is particularly important in settings like mine, where it cannot be observed if a particular individual actually attended mandatory military service or not, and year of birth must be employed instead to deduce risk of conscription.

Torun and Tumen (2016) apply this strategy when addressing the impact of a law which created a paid exemption from conscription for Turkish males born on or before 1972. By comparing cohorts of men and women born around that year, the authors address the policy intention-to-treat effect on educational attainment and wages. This is also the methodological approach of authors such as Di Pietro (2013) or Maurin and Xenogiani

[12] The results using the non-dichotomous operationalization of the dependent variable (Table A.11 in the Appendix) suggest that the observed shift reflects a change from opposition to acceptance of redistribution, rather than a change in the intensity of support.

(2007). Following this line of reasoning, I define the model to study the effect of compulsory military service on political preferences as:

$$y_i = \alpha + \beta_1 T_i + \beta_2 Male_i + \beta_3 T_i Male_i + \delta Yob_i + \vartheta' Year_i + \theta' X_i + \varepsilon_i \quad (1)$$

where y is a binary dependent variable which takes value 1 for those who either agree or strongly agree with redistribution, α is the constant (coefficient not reported), *T* is a dummy taking value 1 if the respondent is born in a conscripted cohort (i.e., 1982 or before) and 0 if not (i.e., 1983 or after), *Male* is a gender dummy, δYob (Year of birth) captures a linear year-of-birth trend, *Year* is a set of dummies for the different ESS waves, and *X* is a vector of socioeconomic controls containing a dummy for those unemployed, five dummies for the level of educational attainment (ranging from "less than lower secondary education" to "tertiary education completed"), and a measure of relative income. Then, I am comparing the differences in preferences for redistribution between conscripted and non-conscripted cohorts for men and women. β_3 is the diff-in-diff estimator.

I show in the Appendix (Tables A.4-A.5) parallel analyses excluding income, education and unemployment status, which may be considered as post-treatment variables or bad controls (Angrist and Pischke, 2009). Standard errors are clustered at the cohort (i.e., year of birth)-region level, which seems justified given the different contexts (in legal and cultural terms) of the Spanish regions (*Comunidades Autónomas*), as well as the economic differences between them. In any case, I show in the Appendix (Section A.6) that my results are essentially equivalent regardless of the level of clustering of the standard errors. I perform linear probability models (LPM) throughout the analysis to ease interpretation and comparability. Probit regressions in the Appendix (Table A.3) yield similar results.

In line with Torun and Tumen (2016), I restrict the cohorts analysed to those around the cut-off point of 1982. This is paramount to increase confidence in conscripted and non-conscripted cohorts being highly comparable. Otherwise, comparing cohorts very distant in

time would make it difficult to argue that the only difference between them is the abolition of conscription. The more distant in time cohorts are, the less comparable.

I begin by comparing preferences for redistribution of those born in 1982 with preferences of those born in 1983, for gradually opening the window of observations up to ±5 cohorts more (see Figure 1.1). In any case, I check in the Appendix (Table A.6) the sensitivity of the results to much wider windows of cohorts.

Figure 1.1: Different windows of cohorts employed in the analysis

Size-1						1982	1983					
Size-2					1981	1982	1983	1984				
Size-3				1980	1981	1982	1983	1984	1985			
Size-4			1979	1980	1981	1982	1983	1984	1985	1986		
Size-5		1978	1979	1980	1981	1982	1983	1984	1985	1986	1987	
Size-6	1977	1978	1979	1980	1981	1982	1983	1984	1985	1986	1987	1988
	Conscripted cohorts						Non-conscripted cohorts					

The low levels of compliance abovementioned should exert a downward bias in the estimate of β_3 in (1), making it more difficult to find a positive effect. However, one could argue that this may make my findings below to be unrelated to conscription at all, being just the result of a different policy or phenomena. I perform below several robustness checks to deal with this problem (excluding last conscripted cohorts from the analysis; excluding those more likely to have avoided conscription, i.e., those with tertiary education; a placebo which sets false dates for the end of conscription) as well as show in the Appendix (Section A.2) a parallel analysis in which I substitute the born-in-a-conscripted-cohort dummy for a more nuanced (but unreliable) measure of risk of conscription: the (estimated) proportion of conscripted males by cohort.

However, my preferred specification considers the born-in-a-conscripted-cohort dummy rather than the proportion of conscripted males for both practical and substantive reasons. The former (which I discuss in Section A.2 in the Appendix) have to do with

problems to construct a reliable measure of the number of conscripted males. But the more interesting rationale is rather substantive. People tend to disproportionately associate with those from the same gender (Fuhse and Gondal, 2022; McPherson et al., 2001: 422) and the same age (particularly from the same year of birth or school grade level, which in this case is essentially equivalent, as Spanish schools group children together according to their *calendar* year of birth) (McFarland et al., 2014; McPherson et al., 2001: 424).[13] Therefore, this dummy variable might capture not only a direct (*intention-to-treat*) effect of attending conscription, but additionally an indirect effect of more socioeconomically diverse social networks of *males in conscripted cohorts* regardless of actual attendance.

Because networks of males born in conscripted cohorts will be disproportionately crowded with males born in the same year (as a matter of year-of-birth and gender homophily), it is very plausible that heightened levels of inter-class contact disproportionately reached cohorts of males where only a small proportion effectively attended conscription. Take a group of youngsters who attended high school together. Even after leaving high school, most of them will still be friends with others in that class, as they spent many years together—this will be particularly pronounced among those from the same gender due to (choice) gender homophily. Therefore, even if only a few males in that class had attended conscription, the cross-class friendships of these could easily reach those males who did not attend conscription through those who did.[14] This may explain why I observe

[13] In McPherson and Smith-Lovin (1987)'s terms, there may exist here age homophily due to *induced homophily* and gender homophily due to *choice homophily*, i.e., males disproportionately associate with others born in the same year mostly because they are grouped together at school and other social activities (e.g., sports) according to that characteristic, and with other males rather than females mostly as a matter of seek of similarity.

[14] It is very plausible to expect a non-linear effect of the proportion of conscripted males on the socioeconomic diversity of networks by cohort. The unknown functional form of this also speaks in favour of using a born-in-a-conscripted-cohort dummy rather than the proportion of conscripted males.

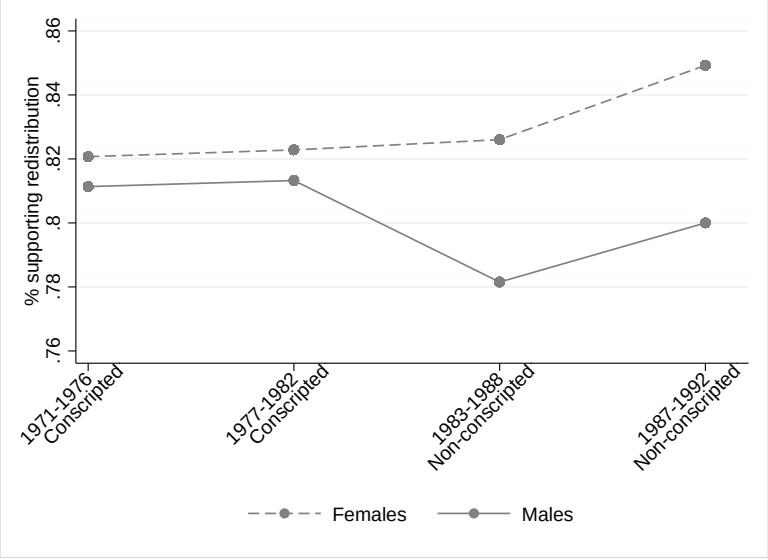
heightened levels of support for redistribution and inter-class contact (the latter in the manipulation checks) even for cohorts with apparently low numbers of conscripted males.

Results

The parallel trend assumption

As a diff-in-diff methodology is employed, I begin by looking at the plausibility of the parallel trend assumption in which this methodology relies on. I plot in Figure 1.2 the means of support for redistribution for different cohorts of males and females. Similar trends can be observed across genders if comparing different groups of conscripted cohorts or if comparing different groups of non-conscripted cohorts, supporting the plausibility of the parallel trend assumption; but not if comparing conscripted with non-conscripted cohorts, as expected by the theory. Moreover, I offer support for the parallel trend assumption through regression analyses in the Appendix (Table A.15).

Figure 1.2: Comparison of means across groups



Main results and robustness checks

I compare in the first column of Table 1.1 the last cohort subject to conscription in Spain (1982) with the next one (1983), for gradually opening in following columns the window of

cohorts employed. The widest one (column 6) compares the preferences of those born in 1977-1982 with the preferences of those born in 1983-1988.

Table 1.1: Main results

Cohorts	1982-1983	1981-1984	1980-1985	1979-1986	1978-1987	1977-1988
	(1)	(2)	(3)	(4)	(5)	(6)
T (Born in a	.004	.045	.044	.025	.037	.072*
conscripted cohort)	(.058)	(.06)	(.048)	(.042)	(.038)	(.037)
Male	-.117**	-.107***	-.106***	-.102***	-.084***	-.081***
	(.055)	(.034)	(.027)	(.024)	(.023)	(.021)
T*Male	.156**	.129***	.122***	.125***	.108***	.092***
	(.069)	(.046)	(.036)	(.033)	(.03)	(.028)
Year of birth	-	.017	.015	.007	.009	.015***
		(.028)	(.013)	(.008)	(.006)	(.005)
Income	-.07*	-.026	-.05**	-.049***	-.048***	-.062***
	(.035)	(.029)	(.019)	(.017)	(.015)	(.015)
Unemployed	.074*	.056*	.058**	.06**	.049**	.016
	(.04)	(.03)	(.027)	(.023)	(.021)	(.023)
Observations	369	749	1138	1550	1912	2283
R^2	.073	.07	.047	.045	.034	.031

Linear probability models. The dependent variable measures support for redistribution. Robust standard errors clustered at the cohort-region level in parentheses. Wave dummies included. Dummies for five levels of educational attainment included. "Born in a conscripted cohort" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. The diff-in-diff estimator is captured by the "T*Male" coefficient. First column does not include cohort trend due to perfect collinearity.
**** p<0.01, ** p<0.05, * p<0.1.*

The diff-in diff estimator is positive and statistically significant across every column of Table 1.1 at least at the 5%.[15] Being born in a conscripted cohort significantly increases the probability of supporting redistribution among males, relative to females, in a range of between 9 and 16 percentage points. This is also a substantial effect in magnitude. When examining standardized coefficients, the diff-in-diff coefficient is the largest predictor in four of the six windows, and the second largest in the remaining two. Moreover, it is around or exceeds the gap in support for redistribution between voters of Spain's two main parties (0.098), between those with and without a university degree (0.054), between the Spanish regions with the largest and the lowest level of support (0.112), and between the country (Turkey) in the ESS with the highest level of support and Spain (0.079). It also amounts to approximately 23%-41% of one standard deviation of the dependent variable (0.391), which

[15] See Table A.3 for probit results.

is similar to the effect found by Mo and Conn (2018) in the case of Teach for America (one quarter of the standard deviation).[16]

The diff-in-diff effect holds if controls that may be considered as post-treatment variables—income, educational attainment, and unemployment status—are excluded from the analyses and the same pool of individuals is used (Table A.4).[17] The results are not sensitive to open the window more years or even to use the whole sample (Table A.6), as well as to exclude these controls or include new ones. Crucially, the significance of the diff-in-diff coefficient still holds if the last cohorts are excluded due to their apparently low levels of compliance, or if the first non-conscripted ones are dropped (Table A.7). Likewise, it holds if looking at cohorts more distant in time but with higher levels of compliance. For example, if comparing preferences of those born in the period 1973-1978 with those of people born in 1983-1988 (Table A.7).

Because the main reason to postpone—and for the last cohorts, eventually elude—conscription was pursuing tertiary education, I repeat the analysis excluding these highly educated respondents from the sample (Table A.8).[18] This reduces the gap between eligibility (being born in a conscripted cohort) and treatment status (attendance).

Furthermore, the positive effect of being born in a conscripted cohort persists (it is stronger indeed) if abandoning the diff-in-diff set-up, restricting the sample to males (Table A.9); but not if focusing only on females (Table A.10), as one would expect. Also, Tables A.12-A.14 display similar findings when clustering the standard errors at different levels.

[16] As I recognize above, these effects could be considered excessively large given the low levels of compliance among the last conscripted cohorts.

[17] Table A.5 aims to account for some pre-treatment socioeconomic characteristic by controlling for father's level of educational attainment.

[18] Note also that this exclusion would not preclude inter-class contact. Even in the extreme scenario of no single individual with tertiary education (ISCED 5-6) attending conscription for the last cohorts, there would still exist an exceptional level of (educational) socioeconomic diversity (ISCED 0-4) in mandatory military service during those years.

Given the impossibility of simultaneously controlling for age, year of birth and year of the survey, I show alternative specifications of the Age-Period-Cohort (APC) model in the Appendix (Figure A.2), following advice by Dinas and Stoker (2014) and yielding similar results. Another robustness check concerns setting *T=0* for all women regardless of whether they were born in a conscripted cohort or not. By doing that, cohort fixed effects can be brought into the equation. Again, the findings remain unchanged (Table A.16).

Figure 1.3: Placebo analysis with false conscription removal dates



Placebo analysis. Point estimates and 95% confidence intervals for the diff-in-diff coefficient in models paralleling those from Table 1.1 (size-6 windows). The x-axis refers to *f*, namely, the cohort that I artificially set as the last forced to do conscription in this placebo analysis (1982 is the correct specification).

I now perform a placebo analysis in which I specify different (wrong) dates for the removal of conscription (see e.g., Fize and Louis-Sidois, 2020). I set *f* as the last cohort forced to do conscription, where $f \in \{1977, 1978, 1979, 1980, 1981, 1982, 1983, 1984, 1985, 1986, 1987\}$. One would expect to observe the larger diff-in-diff effect for the correct specification (*f*=1982), since that is the only specification which treats all "treated" cohorts as treated and all "non-treated" as non-treated. This peak at *f*=1982 is indeed what can be seen in Figure 1.3, in which I display point estimates and 95% confidence intervals for the diff-in-diff

coefficient in models paralleling those from Table 1.1 (size-6 windows). Note also that only the correct specification yields a significant effect at the 5% level.

Before proceeding to the next section, it is worth briefly discussing how my findings relate to the mechanisms proposed above. If we consider heterogeneity by income, one might expect the empathy mechanism to apply mainly to the rich, while the other two mechanisms—updates in perceived inequality and in fairness beliefs—to apply to both rich and poor. This would suggest a larger effect of conscription among the rich. In Section A.11 in the Appendix, however, I show that the main findings of the paper are quite stable across income deciles. At first glance, this could be read as evidence against the empathy mechanism, which should primarily affect the rich. Yet such an interpretation neglects differences in compliance across social classes. More educated individuals—who also tend to be richer—were less likely to serve, complicating inference: lower compliance among the rich could cancel out the larger treatment we might expect from empathy. As a result, heterogeneity analyses by income offer limited capacity for distinguishing between mechanisms. However, Table 1.2 shows that males in conscripted cohorts have a more accurate perception of their own position in society, which I argue is connected to a better understanding of the distribution of resources (inequality), and therefore could be considered as in indirect support of the mechanism about updates in perceived levels of inequality.

Placebo tests and alternative explanations

One could argue that the observed effect on redistribution preferences is just an artifact of differing gender trends in a broader set of political attitudes hardly imputable to the influence of conscription. I perform here various placebo tests which may help to discard this possibility.[19] If conscription affected preferences for redistribution through inter-class

[19] I perform analyses paralleling those from Table 1.1 above, in which I look at the interaction between gender and a dummy of being born in a conscripted cohort—plus all the controls in Table 1.1. For models predicting

contact, it seems plausible to think that I should observe no effect on other unrelated political attitudes. That is indeed the case with respect to environmental attitudes (Figure 1.4) and two different variables measuring anti-immigrant attitudes. The first measures perceptions of immigrants' contribution to the country's economy, to the cultural life and to the country overall (Figure 1.5). The second, the level of support for restrictive policies to immigration (Figure 1.6). I show point estimates and 95% confidence intervals for the diff-in-diff coefficients.

Figure 1.4: Pro-environmental attitudes

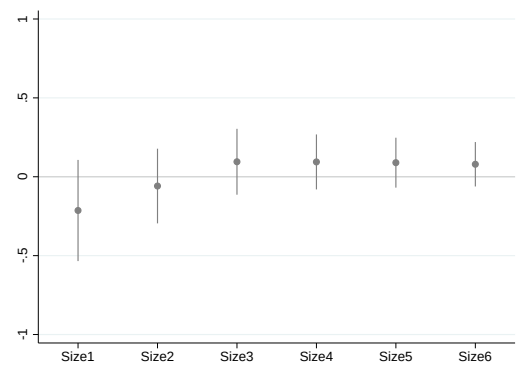


Figure 1.5: Attitudes towards immigrants

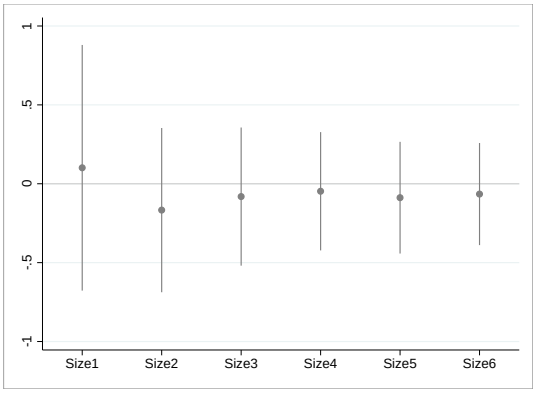


Figure 1.6: Support for restrictions to immigration

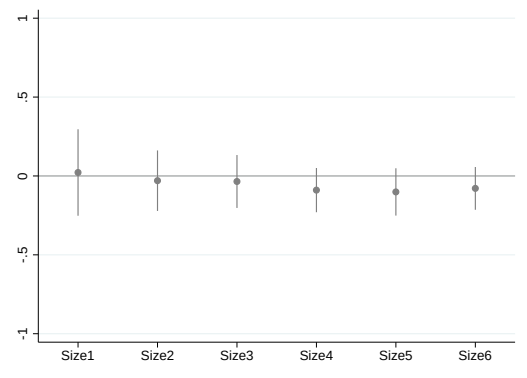
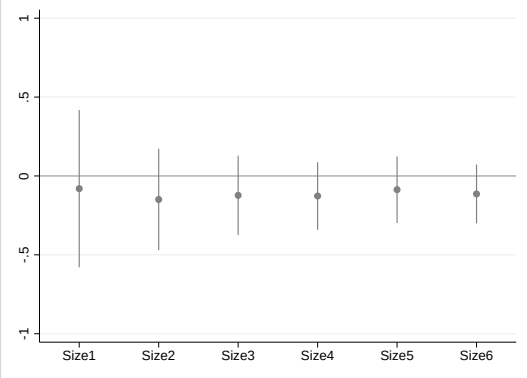


Figure 1.7: Desire for a strong government ensuring safety



Point estimates and 95% confidence intervals for the diff-in-diff coefficient in OLS models paralleling those from Table 1.1 (plus a control about being born in the country in Figures 1.5 and 1.6). See the Appendix (section A.4) for an explanation of dependent variables, as well as for an additional fifth placebo test. Dependent variable ranges 1-6 in Figures 1.4-1.7, 0-10 in Figure 1.5, and 1-4 in Figure 1.6.

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attitudes towards immigrants, I also include a born-in-the-country dummy. In any case, this does not change much the diff-in-diff coefficients on attitudes towards immigrants. More importantly, controlling for that dummy does not change the effect on preferences for redistribution either.

A fourth placebo test looks at the effect on support for a strong government ensuring safety against all threats. This is of especial importance, as could be considered too a competing explanation to the one I propose about inter-class contact. If, given the mindset of the army, conscription increased support for a strong government in all aspects of life, then support for redistribution could be just a consequence of that, a desire for a strong government in nothing but another dimension of it: the economic one. However, as can be seen in Figure 1.7, I find a non-significant—and not even positive—diff-in-diff effect on the desire for a strong government. In summary, no single diff-in-diff estimator of the placebo tests in Figures 1.4-1.7 is statistically significant.[20, 21]

Another competing explanation concerns the self-interest motive. Several authors address a plausible effect of conscription on earnings, educational attainment, and unemployment status. Then, the results above could be channelled by those attending conscription—or being born in a conscripted cohort—having a worse economic situation, which could make them more supportive of policies that would benefit them. However, I control in the core models for income, unemployment status and educational attainment. Furthermore, I approach this issue in the Appendix through moderated-mediation analyses (Table A.36), confirming that the effect of being born in a conscripted cohort does not operate through any of the material variables that might be affected by conscription. Also, I have looked at other plausible explanations which allude to a worse economic situation. First, I have introduced a dummy for those with a fixed-term employment contract. The findings are not affected (Tables A.17-A.18). Second, I have used an indicator of *ever* been unemployed instead of one of present unemployment status, yielding similar results (Table A.19). Third, I have introduced as an additional control the size of the cohort. If the

[20] Not even at the 90%.

[21] I show in the Appendix (Table A.2) that the main results on support for redistribution are unchanged if controlling for the ideological items employed in these placebo tests.

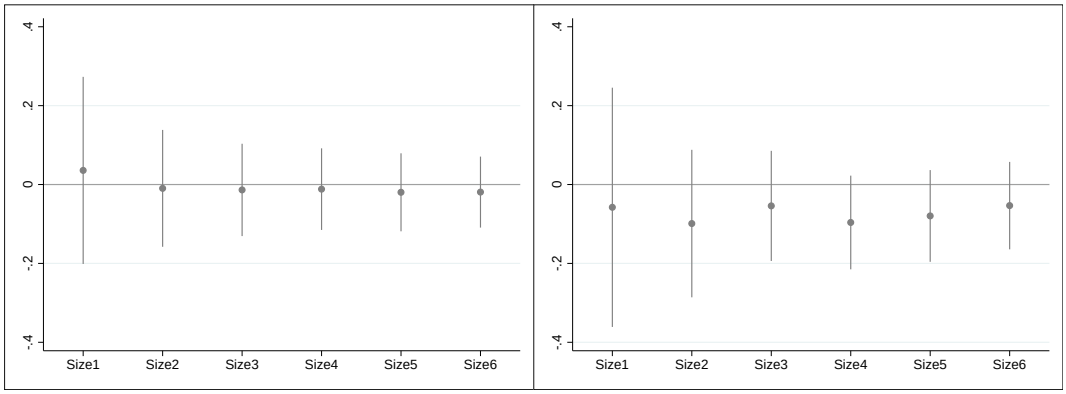
proportion of males drafted is correlated with birth-cohort size, then my findings (especially in Table A.1) could be just picking up an indirect effect of cohorts' earnings. This robustness check is also employed by Imbens and van der Klaauw (1995) when looking at the effect of conscription on earnings. As shown by authors as Brunello (2010), the size of the cohort has a negative effect on average earnings.[22] However, neither controlling for the total size of the cohort nor doing it for the male or the female size alters my results (Tables A.20-A.24). Fourth, as most of my sample is in a typical age to marry/cohabitate and/or become parents—which may affect economic circumstances—I have controlled for living with the spouse/partner and for having children living at home, yielding similar results (Table A.25).

Another plausible explanation concerns the possibility of being drafted into the military rather than actual attendance as a source of political attitudes. Erikson and Stoker (2011) find that those who, according to the Vietnam draft lottery, had more probabilities of being called up for duty developed more anti-war and liberal attitudes. Furthermore, this event turned them against the champion of the new draft policy—Nixon—and made them more Democratic in their voting, regardless of actual participation in the war. The authors argue that this was caused by a disruptive and traumatic event in the impressionable years of several Americans: the possibility of fighting in a war. Obviously, the psychological and economic costs associated with the possibility of participating in a war cannot be equated with those of simply attending mandatory military service during peace time. Still, first generations of Spanish males who were not subject to conscription could have developed sympathy towards the parties involved in the removal of conscription. This policy change was the result of the negotiations, known as *Pacte del Majestic*, between *Partido Popular* (PP)—the dominant right-wing party in Spain—and *Convergència i Unió* (CiU)—a right-wing Catalan nationalist coalition that pushed for the end of conscription (González Ibáñez,

[22] Brunello (2010) finds indeed that earnings are particularly sensitive to cohort size in Southern European countries.

1996). These negotiations resulted in Jose María Aznar becoming Prime Minister of Spain. A heightened sympathy towards PP or CiU could have moved these freed-from-conscription males towards more rightist positions in economic issues such as redistribution.[23] However, the fact that I find a null effect on voting to/feeling closer to PP/CiU (Figure 1.8) easily discards this hypothesis.[24]

Figure 1.8: Effect on voting/feeling closer to PP or CiU



Point estimates and 95% confidence intervals for the diff-in-diff coefficient in linear probability models paralleling those from Table 1.1. In the left (right) model, the dependent variable takes value 1 if voted (feels closer) to PP or CiU.

The results could also be channelled by reduced levels of institutional trust among conscripted cohorts (Bove et al., 2024). Nonetheless, the positive diff-in-diff effect persists even after controlling for respondent's level of trust in the legal system/parliament/parties/politicians, which correlates negatively with support for redistribution (Tables A.26-A.30, also A.36).

Finally, one may worry that some gendered policy implemented around the end of conscription is acting as a confounder. Bove et al. (2024) search for educational, fiscal and

[23] Alternatively, one could argue that PP voters freed from conscription may have reacted negatively to the end of conscription if they supported this institution and were eager to serve. However, that explanation would not pose a threat to my thesis, as it is not observationally equivalent.

[24] If examining independently each of the two parties, I find again a null diff-in-diff effect on voting/feeling closer to PP, but a slightly negative one on feeling closer to CiU. However, the main results of the paper hold if removing from the sample Catalonia, suggesting that this phenomenon does not explain my results.

labor market reforms of this type—gendered and happening around conscription removal—in different European countries, finding that, in the case of Spain, a deduction for women in salaried or self-employed work with children under three years of age was introduced in 2002. However, for this policy to be relevant to explain my results, I should at least observe a greater effect on women's preferences than on men's, which is indeed the opposite to what can be noted in Tables A.9-A.10. Another potential confounder may be the implementation of a new educational law (LOGSE) in Spain in the early nineties. Even if the application of this policy does not have a peak around the 1982-1983 cutoff, it affected many of the cohorts analysed here, so it is worth having a look at it. Felgueroso et al. (2014) find that this law increased dropout rates among males and decreased them among females. This runs counter my results, as this law worsened the socioeconomic situation of males, relative to females. In any case, I have shown that my results are not mediated through the level of education attained, and that is not the case either if the number of years of education is considered instead (Table A.31). Another factor that helps mitigate concerns that my results are solely driven by the educational reform—and possibly by reductions in inter-class contact it may have triggered—is the observed peak in shift preferences around the 1982-1983 cutoff (Figure 1.3).[25]

Manipulation checks

In the previous section, I have tried to discard alternative explanations to the one proposed about inter-class contact. In this section, I present empirical evidence supporting the idea that removal of conscription did indeed reduce inter-class contact.

I claim that if individuals in a society are very socially segregated, it is not very plausible that they know the distribution of economic resources in society and consequently

[25] Still, the gradual implementation of that reform (Felgueroso et al., 2014) complicates a complete dismissal of this alternative explanation (Gingrich, 2019b).

their own position within that. By the contrary, if they have contact with people from other social positions, it is more plausible that the rich (poor) are aware of their privileged (unprivileged) situation. Therefore, in an integrated scenario the rich will place themselves at the top of society and the poor at the bottom with a greater accuracy than in a segregated scenario, where the isolation of classes may indeed lead to a larger middle-class bias (Schulz et al., 2022), resulting in a low correlation between actual and subjective position.

The 6th wave of the ESS contains one question which asks individuals to place themselves on a societal scale ranging from 0 (bottom) to 10 (top). Table 1.2 shows the results of regressing subjective social position on income (decile), gender, the interaction of both, and a linear cohort trend. I show how these explanatory variables correlate with subjective position for conscripted and for non-conscripted cohorts. Also, through the interaction between income and gender, I test whether the correlation between actual and subjective position is different across genders. Then, income (decile) here is basically a proxy for the *actual* position of the individual in society.

If removal of conscription decreased literacy about the distribution of material resources in Spanish society, I would expect income to better predict subjective position in the case of males in conscripted cohorts. This is basically to say that I expect actual economic position to be a better predictor of subjective social position for those more likely to have socioeconomically diverse networks. Given that here I only have one wave of the ESS and therefore a low number of observations if selecting a very narrow window of cohorts, I look at the largest window of Figure 1.1 (size 6).

As can be seen in the first column of Table 1.2, income is a very significant predictor of subjective position for men born between 1977 and 1982 (see the row *Effect of income for males,* calculated as the sum of the effect of income and of its interaction with the male dummy). That is not the case for women. Moreover, the impact of income is different across

genders (see the coefficient of the interaction term). However, if looking at those born between 1983 and 1988, income is not a significant predictor of subjective position either for males or for females, and the difference is not significant either. Note also the different magnitudes of the coefficients, namely, these results are not just a matter of different sample sizes.

Table 1.2: Effect of income on subjective social position

Cohorts	Conscripted (1977-1982)	Non-consc. (1983-1988)
	(1)	(2)
Income decile	.067	.018
	(.048)	(.053)
Male	-.927**	.035
	(.422)	(.499)
Income decile*Male	.133*	.051
	(.07)	(.086)
Year of birth	-.036	-.006
	(.055)	(.072)
Effect of income for males	0.199 ***	0.069 n.s.
Observations	202	116
R^2	.093	.024

OLS models. The dependent variable is the position the individual assigns her/himself on a societal scale from 0 ("Bottom of our society") to 10 ("Top of our society"). Robust (unclustered) standard errors in parentheses. Data from the 6th wave of the ESS.
**** p<0.01, ** p<0.05, * p<0.1*
n.s. non-significant

These findings seem to give credit to the idea that males in conscripted cohorts are highly conscious of the position they hold in society, something which I argue is caused by the exceptionally high levels of inter-class contact that compulsory military service allowed for. Other way of looking at whether removal of conscription decreased inter-class contact concerns looking directly at the socioeconomic diversity of individuals' networks in different cohorts. The most immediate consequence one may expect is those in conscripted cohorts to have more friends from different social classes than those in non-conscripted cohorts. Unfortunately, this is something I cannot test here. What I can check is the socioeconomic diversity of partners. I can test if heterogamous relationships are more prevalent among men

in conscripted cohorts, which should be considered as a very stringent test of whether mandatory military service increased inter-class mixing.[26] The idea is that conscription may have heightened friendships between people from all walks of life—which I cannot test—and that diversity of friends made in the military would have enabled meetings with potential partners of other social positions—e.g., through friends of friends.[27, 28] In Blau (1994)'s words, conscription may have increased *the contact opportunities* with people from other social positions—a necessary condition for social relations to occur—so boosting the probability of a heterogamous match.

Every wave of the ESS includes an item measuring the level of educational attainment of the respondent's partner.[29] I create a binary variable taking value 1 if the level of educational attainment of the respondent—again measured in five levels—differs to that of his/her spouse, and 0 if the respondent and his/her spouse hold the same level. Then, I regress that dummy on gender, a conscripted-cohort dummy, the interaction of both variables (following again a diff-in-diff approach), a linear year-of-birth trend, and dummies for the nine waves of the ESS.

Figure 1.9 displays point estimates and 95% confidence intervals for the diff-in-diff coefficient. This is positive across every model, as well as significant at least at the 95% in three of them. In the Appendix (Section A.9), I find a much stronger diff-in-diff effect when opening the window of cohorts more (Table A.32), as well as successfully pass a placebo

[26] Conscription could also affect heterogamous marriages by postponing marriages to the ages of university attendance (Stier and Shavit, 2003: 317). However, that mechanism would point to a reduction in heterogamy, not to an increase, as I expect in the case of inter-class contact.

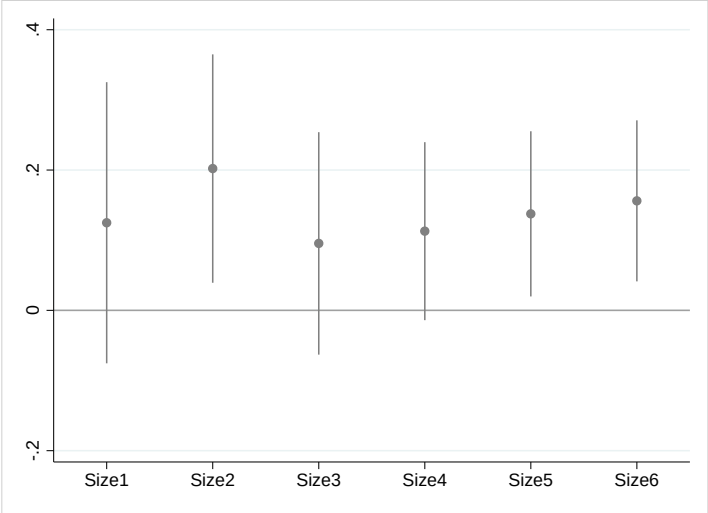
[27] Note also that unless there exists a large prevalence of inter-class marriages between males and females born in the same year, this does not invalidate women as a good control group for the core diff-in-diff analysis on redistribution preferences.

[28] A direct path may also be possible in the case of homosexual males, who may have met in the military.

[29] Looking at occupational class is problematic due to huge missingness. Also, education is one of the most important factors in studies of assortative mating (Blossfeld and Timm, 2003; Schwartz, 2013).

analysis similar to that in Figure 1.3 in which I specify false dates for removal of conscription (Figure A.5). The findings in Figure 1.9 are pretty similar if I additionally control for respondent's age and educational attainment, as well as for educational expansion in each year, which may affect the levels of educational homogamy (Blossfeld and Timm, 2003: 6), as shown in Tables A.33-A.34. These results seem to point in the right direction, namely that conscription did increase contact between different social classes.

Figure 1.9: Effect on the likelihood of having a partner with a different level of educational attainment



Point estimates and 95% confidence intervals for the diff-in-diff coefficient in linear probability models in which the dependent variable takes value 1 in the case of a heterogamous match, and 0 if the respondent holds the same level of educational attainment than his/her partner. The models additionally include a year-of-birth linear trend and wave dummies.

Conclusion

Employing a difference-in-differences approach and focusing on observations close to removal of conscription in Spain to gain in comparability, I show that being born in a conscripted cohort has a positive effect on the likelihood of supporting redistribution among males, relative to females. Furthermore, various placebo tests discard the possibility of these findings being just an artifact of differing gender trends in a broader set of political attitudes.

Finally, I discard competing explanations and provide evidence in support of conscription positively affecting inter-class contact. However, I cannot directly test the mediating role of the different mechanisms proposed (i.e., empathic processes, updates in perceived levels of inequality, and updates in meritocratic beliefs). Also, I cannot observe whether an individual actually attended compulsory military service but only the year he was born on.[30]

All this considered, this paper extends research on the impact of inter-class contact on preferences for redistribution beyond the geographical/neighbourhood scope (Bailey et al., 2013; Minkoff and Lyons, 2019; Franko and Livingston, 2020), proposing a new setting to study inter-class contact and aiming to overcome self-selection concerns. This is something which Mo and Conn (2018) also do by employing a regression discontinuity design in the context of Teach for America (TFA), a programme which integrates top college graduates into low-income areas to teach at schools for two years. However, unlike Mo and Conn (2018), the present paper looks not only at the attitudes of the *advantaged* but also at those of the *disadvantaged*. Furthermore, it addresses some external validity concerns raised by Mo and Conn (2018) themselves: whether inter-class contact may be effective too in contexts in which inequality is not explicit (the TFA organization explicitly aims to reduce educational inequality), in scenarios in which the *disadvantaged* are not children (who may be less likely to be blamed for their socioeconomic circumstances), and if looking at the general population (not just at the group of applicants self-selecting into the application process).

If my reading of the results is correct, such that inter-class contact shapes the way in which people think and learn about inequality and outgroups and, ultimately, moulds preferences for redistribution, we may be facing a vicious circle nowadays, for which increases in inequality will not only *not* increase demand for redistribution—as predicted by classical models in political economy—but indeed will lower it (see also Schulz et al., 2022).

[30] But also note the abovementioned advantages of employing the born-in-a-conscripted-cohort dummy (see *Methodology*).

Considering that inequality heightens geographical segregation and social distance between different social groups (Massey and Fischer, 2003; Mijs and Roe, 2021; Reardon and Bischoff, 2011), this will, in turn, decrease inter-class contact and reduce support for measures aiming to reduce inequality, which very likely will translate into more inequality. It is so in the present context of rising socioeconomic segregation around the globe (Azpitarte et al., 2021; Mijs and Roe, 2021; Musterd et al., 2017) that heterogeneous socializing institutions may play a key role to reverse this cycle.

Whereas research in the field of support for redistribution has produced very relevant and brilliant works for understanding socio-political preferences, few of them possess an implicit potential to change reality. However, heterogeneous socializing institutions are not just important devices to explain society but also critical tools with the ability to transform it. I consider that the introduction of heterogeneous socializing institutions into this line of research is one of the main contributions of this study, due to their practical implications for social policy. Creating institutions and developing policies which bring together people from all walks of life—especially in their formative years—might be a non-negligible tool for those pursuing redistributive policies. For example, by reducing socioeconomic segregation at the school or the neighbourhood level.[31] Borrowing some terminology employed by Hochschild in her book *Strangers in their own land* (2018), the major aim of this article has been to investigate whether deep contact with people from other classes is a meaningful tool to help people climb the *empathy wall* that hinders many redistributive policies.

[31] The comeback of conscription might be quite controversial and have unintended consequences (Bethmann and Cho, 2023; Bove et al., 2024).

Chapter 2: Draft of the second paper

I take you, for richer, for poorer

Cross-class Romantic Partnerships and Attitudes towards the Economy

Abstract: Does having a partner from a different social class shape the way in which the economy is thought about? Drawing on longitudinal data from the British Household Panel Survey for ameliorating self-selection concerns, I find that *both* those who marry/begin to cohabitate with someone from a lower class and those who do so with someone from a higher class *move* their economic attitudes to the left—*vis-à-vis* those doing so with someone from the same social class. This is found to be caused neither by a shift in material conditions nor by individuals copying their partners' attitudes but, I argue, by contact between social classes, which has the capacity to unveil existing privileges and material differences between groups. In line with this thesis, I find a null effect on political attitudes unrelated to the economy. Heterogenous analyses show a more pronounced effect among individuals not interested in politics.

"She is led by love. The world moves for love. It kneels before it in awe"

The Village (M. Night Shyamalan, 2004)

Introduction

How does having a partner from a different social class affect the way in which individuals perceive the economy and form opinions of how it should operate? Although the role of social networks' diversity for explaining economic attitudes has been addressed by some scholars (e.g., Liu, Kuo and Fernández-Albertos, 2020; Newman, 2014; Newman and Vickrey, 2017), it is surprising that the role of arguably the most central person in one's life has been ignored in this literature.

Previous research on contact between social classes has shown that socioeconomic segregation at the geographical level limits support for redistribution (Bailey et al., 2013; Franko and Livingston, 2020; Minkoff and Lyons, 2019). I argue that the increased contact between classes brought by cross-class partnerships also has the capacity to unveil the privileges and material differences between groups (e.g., Gingrich, 2019b; Minkoff and Lyons, 2019; Mo and Conn, 2018), which remain largely hidden in most modern societies, characterized by high levels of socioeconomic segregation in what refers to friendship networks, romantic partnerships, and neighbourhoods (Azpitarte et al., 2021; Massey, 1996; Massey and Fischer, 2003; McPherson et al., 2001; Mijs and Roe, 2021; Musterd et al., 2017). Therefore, I expect both those starting a relationship with someone from a lower class and those starting a relationship with someone from a higher class to gain a better understanding of material differences in society, thus aligning with views and policies aware of this problem and willing to address it. Furthermore, for the former group I expect an additional effect caused by increased empathy towards the material difficulties experienced by the lower classes.

By employing longitudinal data from the British Household Panel Survey (BHPS) in order to reduce issues of self-selection, I find that those who marry/begin to cohabitate with someone from a different social class move to the left *vis-à-vis* those who do so with someone from the same class.[1] In the methodological section, I discuss the plausible limitations of looking only at marriage/cohabitating relationships (as a matter of data availability), and why this issue is not so concerning here.

Furthermore, I discard alternative explanations to the one proposed about inter-class contact, such as partner's persuasion (controlling for the partner's attitudes) or alterations in material conditions (e.g., controlling for changes in income). Reassuringly, I show that cross-class domestic partnerships (i.e., marriage or cohabitation) do not alter political attitudes unrelated to the economic domain, such as views on gender equality and tolerance towards homosexual relationships.

Finally, I show that individuals do not self-select into type of domestic partnership according to their economic attitudes (i.e., pre-partnership attitudes of those entering a cross-class domestic partnership and of those entering a within-class one do not differ beforehand) if looking at the whole sample. However, a different story arises when distinguishing by political interest. Those interested in politics *do* self-select into type of partnership according to their pre-partnership attitudes (i.e., those with more leftist attitudes beforehand are more likely to form a cross-class domestic partnership). However, after domestic partnership formation, they *do not* experience a significant change conditional on the type of partnership joined. It is exactly the opposite story for those not interested in politics. They *do not* self-select into type of partnership according to their economic attitudes. However, among this group of people, those who enter a cross-class domestic partnership *do* experience a pronounced shift to the left after partnership formation. As they have limited opportunities to learn about inequality through

[1] I show in Figure 2.1 that these results are robust to the choice of alternative reference groups.

means other than personal experience, they might be more easily *activated* when confronted with inequality.

This paper contributes to the literature on inter-class contact and economic attitudes by proposing a novel setting—romantic relationships—to approach this phenomenon. It also employs longitudinal data in order to ameliorate self-selection concerns present in most papers (e.g., Bailey et al., 2013; Franko and Livingston, 2020; Minkoff and Lyons, 2019). Namely, any positive correlation between leftist preferences and diverse networks (e.g., cross-class partnerships) found in cross-sectional data could be just an artifact of leftist people being more likely to form diverse networks. It is indeed for that reason that employing longitudinal data is critical for better distinguishing sorting from contextual explanations in this line of research.

Furthermore, this paper aims to capture the effect of deep contact between classes, rather than the brief exposure to socioeconomic diversity examined in past studies, such as the ones conducted by Sands (2017) and Sands and de Kadt (2020). It is not about individuals being shown on paper that inequality is larger than previously thought (e.g., Kuziemko et al., 2015; Trump, 2018), or about coldly observing potential competitors for material resources (Sands, 2017; Sands and de Kadt, 2020). It is about thick and repeated contact with a different socioeconomic reality than one's own, such that inequality is converted into a more tangible and less abstract concept. Finally, this paper also contributes to the scholarship on the relationship between partners' political attitudes, by suggesting a potential new source of convergence among individuals in heterogamous relationships.

Political preferences in the bedroom

A relatively developed field of research has devoted its efforts to try to understand the relationship between spouses' political preferences—to cite just a few, De Graaf and Heath, 1992; Huber and Malhotra, 2017; Kan and Heath, 2006. Another—much less developed and much more recent—strand in political economy has looked at how contact between people

from different social classes affects economic attitudes (e.g., Bailey et al., 2013; Minkoff and Lyons, 2019). The present paper is, up to my knowledge, the first one trying to reconcile these two lines of research.

The existing connections between one's political preferences and those of the spouse have called the attention of many sociologists and political scientists. Most of this scholarship begins with an observed and unsurprising fact: that the level of concordance between spouses' political preferences is larger than one would expect to observe by pure chance in any pair of individuals in society. In other words, there exists a positive correlation between the political preferences of people who form a partnership, either that is party voted for or positioning on social and economic issues. What is not clear, however, is where this relationship stems from.

A first broad set of explanations concerns concordance from the very moment of meeting. Namely, individuals form relationships with others whose *prior political attitudes* are similar. This *ex-ante* political resemblance within the couple can, in turn, be explained by three different phenomena. First, it may be the case that individuals seek similarity in political preferences (e.g., Alford et al., 2011; Huber and Malhotra, 2017). Maybe to minimize conflict in the future (Gerber et al., 2012) or as a matter of attraction towards *birds of the same feather* (McPherson et al., 2001), people may prefer others with alike views about politics. A second explanation concerns self-selection, not directly on political attitudes but on socioeconomic characteristics that may be correlated with the former (e.g., Coffé and Need, 2010). If people seek others with, let us say, similar levels of education or occupations, it is highly likely that the two partners will also hold similar political attitudes. The third explanation revolves around constrained marriage markets (e.g., Kalmijn, 1998). If people are socially and geographically segregated according to socioeconomic characteristics which correlate with political views, the pool of individuals from which one can choose a partner will be skewed towards the ideology

of the individual. Whereas the first two explanations concern *choice homophily*, the last one is what McPherson and Smith-Lovin (1987) denominate *induced homophily*.

The second broad set of explanations involves temporal variation in attitudes, so I will refer to them as *ex post*. Regardless of whether partners resemble each other politically at the moment of meeting, they may become more and more politically similar after sharing time together (e.g., Coffé and Need, 2010; Kan and Heath, 2006; Stoker and Jennings, 2005; Zuckerman et al., 2005). This may be caused by mutual influence of partners—or by only one of them exerting it—either through persuasion or as a way to avoid conflict and minimize tensions in the relationship. However, convergence in preferences as time passes by could also be explained by shared conditions and environments during marriage. For instance, once living together partners share resources at the household level, so their material interests become quite alike (Strøm, 2014). Also, they may spend time together in the same kind of environments, which may in turn shape their political views in a similar manner. "Then, after marriage, each partner is exposed to an enormous number of common experiences, all the way from living in the same neighbourhood and sharing the same children to being exposed to the same media entering the household. These similarities of exposure would be expected to push the partners closer together, other things being equal", Stoker and Jennings (2005: 53) claim. The argument I propose about inter-class contact is, in fact, the reverse of the latter: it is not about *similarities* but about *dissimilarities of exposure*. Having a partner from a different social class exposes the individual to a different background than their own. It is not, therefore, the influence exerted by the environment, but rather the clash between one's socioeconomic environment and that of the partner which changes the attitudes of those in heterogamous partnerships.

Before discussing in more detail why exposure to a new socioeconomic reality may alter attitudes towards the economy, let us first summarize some important *empirical* findings

about the causes of concordance in spouses' political preferences, as a good understanding of those will be very helpful for a correct construction of the methodological setting of the paper.

When it comes to explain similarity of political preferences within the bedroom, the role of self-selection on political attitudes has been automatically neglected in many cases. "Political preferences do not form a component of marital homophily", Zuckerman et al. (2005: 76) claim. Coffé and Need explicitly state that "[they] may assume that most people will not consider party preference as a salient factor in selecting a spouse or partner" (2020: 260) in order to attribute any similarity originated *ex ante* to selection on correlated socioeconomic characteristics. Although Stoker and Jennings' reading of their findings is more about mutual socialization and reciprocal influence, they do not dismiss that self-selection on correlates of political preferences *but not directly on political preferences* may play a role to explain observed concordance between spouses: "while Americans seldom use politics as a criterion for mate selection, assortative mating based on factors related to politics (such as class, religion and race) will inevitably produce some initial convergence and pave the way for further convergence" (2005: 53).

Employing an instrumental-variables approach, Coffé and Need (2010) find mixed results in the attribution of spouses' voting alignment to *ex-ante* or *ex-post* explanations: both similarity in socioeconomic characteristics and the mutual influence of spouses seem to explain political concordance for Dutch marriages. By finding that the correlation between spouses' political attitudes grows as they spend more years together, Stoker and Jennings (2005) point mostly to *ex-post* explanations such as partner's persuasion or shared environments (see also Zuckerman et al., 2005).[2] Other indirect evidence in support of the thesis about spouses' influence concerns Kan and Heath (2006), who find that partner's ideology is more influential

[2] However, Alford et al. (2011) note that the increases observed by Stoker and Jennings (2005) are quite modest. Therefore, *ex-ante* explanations should not be dismissed at all.

to determine the preferred party of an individual in cases in which the former is interested in politics but the latter is not.

In the last decade, however, the findings of Alford et al. (2011) and Huber and Malhotra (2017) have arisen to support the importance of self-selection processes when forming relationships, a mechanism mostly neglected by previous scholarship. Alford et al. (2011), employing data from a Virginia study of twins and their relatives, make a striking finding: similarity across romantic partners is very strong when looking at political attitudes, but much weaker with respect to personality and physical traits. They are able to rule out explanations about self-selection on socioeconomic correlates and constrained marriage markets, as well as about persuasion and shared environments, arguing that the large majority of similarity observed must come from the seeking of a politically similar partner. Huber and Malhotra (2017) take this line of research one step forward by combining an experimental with an observational study. In the former, they randomly manipulate the political description of online dating profiles presented to participants, finding that shared ideology (i.e., concordance in labels of conservative, liberal, or moderate) leads to more positive evaluations of the candidates. For the latter, they look at real data from an online dating site. Controlling for the geographical availability of potential partners and for socioeconomic correlates, they find positive sorting on ideology, partisanship, political engagement, and policy positions. Thanks to the combination of internal and external validity that the experimental and the observational studies respectively confer, Huber and Malhotra (2017) provide strong evidence for the idea that politics is very far from being meaningless when it comes to the formation of a relationship, urging for taking into account seriously self-selection on political attitudes.

Inter-class contact and attitudes towards the economy

The hypothesis of this paper is that cross-class romantic partnerships have the potential to move economic attitudes to the left due to increased inter-class contact—this contact occurring by

definition but also through the partner's kith and kin.[3] In this section, I summarize the findings of the scarce line of research about contact between social classes and attitudes towards the economy, while discussing potential mechanisms underlying this relationship.[4]

Some scholars have focused on the geographical space aiming to understand how sharing neighbourhood with people from different social classes affects the way in which inequality and remedies to it are conceived (Bailey et al., 2013; Franko and Livingston, 2020; Minkoff and Lyons, 2019). Minkoff and Lyons (2019) find that those surrounded by more economic diversity in their neighbourhoods of New York City perceive larger income differences in society. This finding relates to the role of immediate social environments for acquiring information. Modern societies are, to a greater or lesser extent, geographically and socially segregated (Massey, 1996; Massey and Fischer, 2003; McPherson et al., 2001). Therefore, what one can observe when looking around is social homogeneity, namely mostly people from a similar social position. And as people make inferences about the general population by sampling from their own immediate social environments (Dawtry et al., 2019; Galesic et al., 2018), they will think that the population as a whole is as "equal" as it is their surrounding environment (Schulz et al., 2022; Thal, 2017). In other words, they are confounding the homogeneity surrounding them (segregation) with an equal distribution of resources in society (equality). Therefore, both the poor and the rich living in homogeneous environments will make underestimated predictions of actual levels of inequality in society. Whereas segregation keeps privileges and material differences between social groups hidden, inter-class contact has the capacity to unveil them.

[3] Although a partner and her social circle must not necessarily belong to similar social classes, they will systematically do (for homophily by social class, see e.g., McPherson et al., 2001).

[4] This line of research has mostly focused on preferences for redistribution, as well as on attitudes towards inequality. I focus in this paper on attitudes towards the economy in a broader sense, as the BHPS does not contain an item explicitly addressing support for redistribution.

Minkoff and Lyons (2019) find not only that those in more diverse settings perceive more inequality but also that they support more that the gap *should* be smaller—not only in what respects to their city but also to the country as a whole. The step from a *positive* perception of the situation to a *normative* evaluation of it seems quite natural. The smaller the problem—inequality—seems to be, the less it should be done about it (e.g., redistribution; Becker, 2019; Kuziemko et al., 2015; Pellicer et al., 2019). Similar findings arise in the USA at the country level (Franko and Livingston, 2020) and in England (Bailey et al., 2013): those in more socioeconomically diverse neighbourhoods are more likely to support redistribution than those living in more homogeneous spaces.

It is important to note that this line of research often finds inter-class contact to exert a positive effect on leftist economic attitudes among both the poorer and the richer (e.g., Bailey et al., 2013; Franko and Livingston, 2020; Minkoff and Lyons, 2019; but also see Christensen et al., 2024). Namely, being surrounded by socioeconomic diversity not only makes inequality a less distant and abstract issue for the richer, but also a more visible one for the poorer, thus increasing critical views of it among both the more and the less advantaged.[5] Likewise, I expect those starting a relationship with someone from a different social class—regardless of whether they do it with someone from a lower or from a higher class—to become more aware of

[5] A couple of experiments (Sands, 2017; Sands and de Kadt, 2020) might contradict this benign effect of inter-class contact for both the richer and the poorer. Sands (2017) and Sands and de Kadt (2020) find that the poor are more likely to support redistribution in the presence of a high-status car, whereas the rich are less prone to do so when a poverty-stricken person is around. Therefore, these results together might suggest that the sign of the effect of inter-class contact is contingent on income. However, these experiments do not really address inter-class contact but just fleeting exposure. And as Allport stated more than six decades ago, "[Casual contact] does not dispel prejudice; it seems more likely to increase it" (1979[1954]: 263). Furthermore, survey experiments such as those of Cruces et al. (2013) and Fernández-Albertos and Kuo (2018) find that individuals learning they are richer than thought do not reduce support for redistribution. Likewise, I do not expect to observe the adverse effects predicted by the inequality-begets-inequality literature (e.g., Trump, 2018), as I address deep and repeated contact.

economic differences in society, thus aligning with policies recognizing and addressing these material differences.[6]

Not only do people look at their surrounding environment for extrapolating the level of inequality in society, but also for inferring its causes. A person may have, broadly speaking, either a more individualistic/meritocratic understanding of inequality—where outcomes are mostly determined by individuals' actions, such as effort or hard work—or a more structuralist view of the problem—where structural factors beyond individuals' control, such as coming from a wealthy family, do play a key role in the distribution of resources. Mijs (2018) proposes studying socializing institutions as spaces where children and young adults learn about inequality. "For a person to experience the salience of an individual or structural factor, he or she must be able to observe variation in both the factor and in the relevant outcome", Mijs (2018: 8) claim. Therefore, a child in a homogeneous socializing institution (i.e., segregated school) will observe variation in factors such as effort but not in others like class of origin, inferring that it is the former type of factors alone what determines social outcomes. However, a child attending a more diverse school, in which she can observe not only different levels of effort exerted but also diverse socioeconomic backgrounds, will attribute more weight to factors such as class of origin for deciding the fate of individuals than a child in a segregated school will do. Consistent with this thesis, Gingrich (2019b) finds that adults who attended stratified affluent schools are less likely to perceive economic unfairness than those educated in mixed schools. Socioeconomic mixing may have so the power to improve, in Edmiston (2018)'s words, *the poor sociological imagination* of many individuals to acknowledge systemic features shaping the material lives of individuals. Regarding the present analysis, I

[6] Entering a cross-class domestic partnership obviously affects income at the household level, very likely moving economic preferences through a self-interest mechanism too. My thesis is that, net of that attitudinal change caused by a shift in material conditions, both those with a partner from a lower class and those with a partner from a higher class will move to the left.

expect that, on the one hand, structural attributions of poverty will increase for those starting a romantic partnership with someone from a lower class (i.e., they will blame less the poor for being poor and will give less credit to agency-based explanations of poverty). On the other hand, structural attributions of wealth will increase for those starting a romantic partnership with someone from a higher class, who will understand wealth less as a function of hard work and more as the result of inherited assets.

The phenomena explaining a positive effect of inter-class contact on economic attitudes are not limited, however, to the objective recognition of the levels and causes of material differences in society.[7] Some scholars have made a social class reading of contact theory to explain why those exposed to more socioeconomic diversity hold more leftist views of the economy (Franko and Livingston, 2020; Minkoff and Lyons, 2019; Mo and Conn, 2018; Newman, 2014; Newman and Vickrey, 2017). Contact theory states that intergroup contact has the potential to reduce prejudices towards the outgroup (Allport, 1979[1954]; Pettigrew, 1998). Although most research in this area has revolved around ethnic and racial lines (e.g., Finseraas and Kotsadam, 2017), its potential to analyze interactions between the rich and the poor is evident. Newman (2014) and Newman and Vickrey (2017) resort to explanations concerning empathy towards those experiencing material difficulties to explain why those whose social networks experience unemployment and economic distress are more supportive of redistribution. Mo and Conn (2018) look at the Teach for America (TFA) case, a national service program which places top college graduates to teach in low-income schools for two years. The results of a regression discontinuity analysis show that participants are less likely to justify the system and hold meritocratic beliefs, as well as more likely to support policies

[7] Moreover, I claim that these two mechanisms are not purely informational but also carry an emotional dimension. It is not only a matter of learning that inequality is larger than expected through figures and statistics (e.g., Kuziemko et al., 2015), but also of internalizing its tangible, human consequences.

reducing inequality. The authors argue that contact with lower classes leads to increased empathy and perspective-taking among participants in the program, who come mostly from privileged backgrounds.

I claim that *both* those starting a romantic partnership *with someone from a lower class* and those doing so *with someone from a higher class* will move their economic attitudes to the left, as both groups will become more aware of the degree of material differences in society as well as will explain poverty and wealth in structural rather than agency-based terms. However, I expect a larger effect for those starting a romantic partnership with someone from a lower class, who may *additionally* develop an empathic response towards the material difficulties experienced by the lower classes. Moreover, I argue that this effect is independent of changes in economic attitudes resulting from shifts in material conditions or from the influence of a partner's attitudes. I test this proposition by controlling for changes in household income and for the partner's attitudes, respectively.

Although very illustrative and insightful, most papers in this line of research employ cross-sectional data, leaving the door open to criticisms on the basis of self-selection (see e.g., Christensen et al., 2024). It may be the case that those living in more diverse neighbourhoods (Bailey et al., 2013; Franko and Livingston, 2020; Minkoff and Lyons, 2019) or having more economically distressed friends (Newman, 2014) hold *a priori* more leftist attitudes. Three notable exceptions in this regard are Newman and Vickrey (2017), who address shifts in preferences, Mo and Conn (2018), who employ a regression discontinuity design to infer causality, and Gingrich (2019b), who uses matching techniques to compare individuals educated in mixed and stratified schools in England. The present paper follows a similar approach to Newman and Vickrey (2017), by using longitudinal data and looking at attitudinal *changes*. Unlike them, however, I focus on the unexplored setting of cross-class romantic relationships, therefore more explicitly addressing contact between classes rather than shifts in

the material conditions of one's network (vicarious experiences of unemployment in the case of Newman and Vickrey, 2017). Furthermore, I address a few limitations of Mo and Conn (2018), some of them raised by the authors themselves. For example, does inter-class contact have positive effects also in situations in which the disadvantaged are not children, who are less likely to be blamed for their material situation? And most importantly, whereas Mo and Conn (2018) study the attitudes of the *advantaged* but not those of the *disadvantaged*, I address here the attitudes of both groups, as Gingrich (2019b) also does. Then, I can answer the question of how the preferences of less privileged people are moulded after meeting more privileged individuals.

This theoretical framework, as the literature analyzed, largely builds on income differentials, a concept which more easily conveys the intuitions. Still, these are implicitly arguments also about social class, a more encompassing concept than income. Indeed, as explained in the following section, I am operationalizing social class in occupational terms, as it is often the case for sociologists when addressing issues of socialization, such as the one in this paper. Furthermore, occupation—which strongly correlates with income and other material benefits—is a much more stable measure than income, therefore delivering better predictions at any given point in time of the real position of individuals in the societal distribution of resources given its lower volatility across waves, and relatedly, better describing lifetime earnings profiles (e.g., Goldthorpe and McKnight, 2006).

Data

This article addresses how economic preferences change (or not) after marrying/beginning to cohabitate with someone from a different social class. For avoiding the constant repetition of these two terms, hereinafter I will refer to domestic partnerships for referring *both* to *living with one's spouse* and to *cohabitating with one's partner* without being married. I bundle together in the empirical analyses those marrying and those starting to cohabitate as I do not

theoretically expect different effects for these two groups.[8] Indeed, I show in the Appendix (Section B.21) that both those getting married and those starting to cohabitate with someone from a different class move their economic attitudes to the left.[9]

As has been discussed above, people select into relationships in a non-random way in what refers to socioeconomic characteristics (e.g., occupation) and even political attitudes (Alford et al., 2011; Huber and Malhotra, 2017).[10] Therefore, focusing on changes is important to tackle self-selection concerns, for which it is obviously a necessary condition to employ longitudinal data. If cross-sectional data was used instead, any positive finding could be just the artifact of people with *a priori* different economic preferences holding different probabilities of forming a partnership with someone from a different social class. For example, it could be the case that those who hold more leftist economic attitudes present a higher likelihood of starting a relationship with someone from a lower social class than those with more conservative preferences; or as stated by Coffé and Need (2010: 267), it "seems reasonable to expect that those upholding more traditional principles will be more conventional and thus tend more to follow cultural norms of homogeneity", forming a partnership with someone from a similar social position.

Longitudinal datasets asking the same individuals repeatedly about economic attitudes while including information on partner's social class are not common. One of the few cases that accomplishes this necessary condition for the type of analysis of this paper is the BHPS. Although carried every year between 1991 and 2008, this survey did not include the items measuring economic preferences in every round. Therefore, the analysis below relies on the

[8] I cannot look at other types of romantic relationships in which partners do not live together as this is a household survey, so I do not have information about those, as I discuss in next section.

[9] If anything, the coefficient for those getting married is slightly larger than the one for those starting to cohabitate.

[10] For a review of the causes and consequences of assortative mating—not looking at political similarity—see Schwartz (2013).

seven rounds that did so: 1991, 1993, 1995, 1997, 2000, 2004, and 2007. For measuring economic attitudes, I rely on a *laissez faire*/socialist scale developed and validated by Heath et al. (1994) and widely employed in British research by authors such as Evans and Neundorf (2020) or Langsæther et al. (2022).[11] This additive index looks at the level of agreement in a 5-point scale with each of the following six items: "Major public services and industries ought to be in state ownership", "Private enterprise is the best way to solve Britain's economic problems" (reversed), "It is the government's responsibility to provide a job for everyone who wants one", "Strong trade unions are needed to protect the working conditions and wages of employees", "There is one law for the rich and one law for the poor", and "Ordinary people get their fair share of the nation's wealth" (reversed).[12, 13] As I employ the average of the responses to these six items, the dependent variable ranges from 1 to 5 (the mean being 3.37 in my sample, and the standard deviation, 0.54), with higher values indicating more economically leftist attitudes, that is, a higher level of agreement with these statements—except for the two reversed, for which disagreement indicates a more leftist attitude. In Section B.10 in the Appendix, I show disaggregated results for each of the six items forming the index. Furthermore, I disaggregate the index into two in Section B.19 in the Appendix: an economic policy mood index (first four items) and a perceived class bias one (last two items).[14]

Whether a partnership is heterogamous or homogamous can be defined with respect to many different socioeconomic traits. Here, I follow a classical sociological approach and define social class in terms of occupation (e.g., De Graaf et al., 1995; Kitschelt and Rehm, 2014;

[11] The Cronbach's alpha in my sample is 0.6, indicating a fair level of internal consistency

[12] The possible responses to each of the items are "strongly agree", "agree", "neither agree nor disagree", "disagree" and "strongly disagree".

[13] I show in Section B.18 in the Appendix a positive effect also on an item about considering unfair that wealth buys medical priority. This item is not part of the index by Heath et al. (1994) as it is unavailable in some of the BHPS waves.

[14] These results suggest a stronger effect for attitudes concerning perceived class bias.

Weeden and Grusky, 2005). More specifically, I look at the Registrar-General's Social Class (RGSC).[15] Formerly the official socio-economic classification in the United Kingdom, the RGSC has not been so commonly employed in the last years as it was in the past (for a critical discussion of the RGSC, see e.g., Bland, 1979; Brewer, 1986; Rose and Pevalin, 2002; on the origins of the RGSC, see Szreter, 1984; for articles that make use of the RGSC in the field of politics and sociology, see e.g., Clifford and Heath, 1993; Strøm, 2014).[16] However, this schema accomplishes a condition that is paramount for my analysis (especially when distinguishing between those forming a partnership with someone from a lower class and those doing so with someone from a higher class) and which other more popular schemas (e.g., the one developed by Goldthorpe and his affiliates) lack: it is strictly hierarchical.[17]

The RGSC is a skill-based classification which divides occupations into six ordered classes: "professional", "managerial and technical", "skilled non-manual", "skilled manual", "partly skilled", and "unskilled" (descending order). I treat as in a cross-class domestic partnership those individuals whose partner's occupation belongs to a class different than his/her own according this six-group schema and, for analyses which distinguish between those starting a relationship with someone from a lower class and those doing so with someone from a higher class, I consider in the former (latter) group those whose class is above (below) that of their partner.[18] Of the 1,472 cases employed in the main specification below, 30.3% are within-class partnerships and 69.7% are cross-class partnerships—with almost the same number of upward and downward partnerships. Among the cross-class partnerships, the vast

[15] I look at the most recent occupation of individuals.

[16] The RGSC is still very often employed in demographic studies. It was born in the context of the debate between advocates of environmentalist and of eugenist explanations of varying fertility rates across the social strata (Szreter, 1984).

[17] For a review of different occupational-based social classifications, see Connelly et al. (2016).

[18] The BHPS contains a seventh distinct category: Armed Forces. However, no individual in my sample of those who just formed a domestic partnership belongs to this category.

majority are between individuals whose occupations are no more than three RGSC classes apart.

Moreover, I show in Section B.7 in the Appendix similar results when employing various versions of the Goldthorpe schema. These robustness checks show that my results are not contingent on the use of the RGSC, therefore reducing concerns about a schema criticized by authors such as Brewer (1986) or Rose and Pevalin (2002). Still, results in the main text employ the RGSC for its hierarchical structure.

Methodology

Aiming to see how political attitudes change after entering a cross-class partnership, this analysis restricts the sample to those individuals who have just started a domestic partnership between a given wave (period *t*) and the previous one (period *t-1*) for their first time in the survey. Then, β_2 in the equation below denotes the change in economic attitudes experienced after entering a cross-class domestic partnership *vis-à-vis* the change experienced after entering a within-class domestic partnership—the latter being the reference group, as they do not experience contact with a new socioeconomic reality through their partner.[19] Obviously, this reduces the generalizability of the results to the whole population, as those not entering a domestic partnership or those already in one before entering the BHPS are excluded. However, I consider those entering a within-class domestic partnership as the natural control group for those entering a cross-class one, as both groups are going through a similar process and stage of life (i.e., entering a domestic partnership, with all the emotional and material implications which that entails), and therefore are highly comparable.[20]

For the main analysis, I estimate the following equation:

[19] The act of entering a domestic partnership may have, of course, an effect itself on economic attitudes. However, by focusing only on those doing so I am keeping that effect constant.

[20] See also Sections B.17 and B.22 in the Appendix for a discussion of this matter.

$$y_{i,t} = \beta_0 + \beta_1 y_{i,t-1} + \beta_2\ Entering\ a\ cross\ class\ partnership_{i,t}(vs.\ entering\ a\ within\ class\ partnership) + \vartheta' Year_{i,t} + \theta' X_{i,t-1} + \varepsilon_{i,t}$$

subject to the individual having entered a domestic partnership for the first time in the sample between the present and the previous wave[21, 22]

where *i* denotes individuals and *t* denotes periods (*t* or *t-1*), y_t is the *laissez faire*/socialist scale developed by Heath et al. (1994), y_{t-1} is the value of the dependent variable in the previous wave, *Entering a cross-class partnership* is a dummy which takes value 1 if respondent's social class is different to that of his/her partner according to the six classes of the RGSC and 0 if it is the same (in some models this dummy is split up into those in a domestic partnership with someone from a lower class and those in a domestic partnership with someone from a higher class), *Year* is a set of dummies for the different BHPS waves, and $X_{i,t-1}$ is a set of usual controls in the literature, containing a gender dummy, dummies for the different levels of educational attainment ("degree/diploma", "vocational qualification", "higher school level", "low school level or below"), age, a dummy taking value 1 for those in unemployment, and change in household's income. For descriptive statistics of these and other variables, see Table B.5 in Section B.4 in the Appendix.

The rationale to control for covariates such as gender is to make sure that results are not given by some group of people (e.g., females) holding a higher likelihood of entering a cross-class partnership and at the same time of moving to the left after partnership formation regardless of doing so. In any case, the inclusion or exclusion of these controls do not change substantively my findings, as I will show below.

[21] See Section B.16 in the Appendix for a description of the process I follow to detect those entering a domestic partnership.

[22] Note that the estimates of the variables of interest are identical to those of a model in which the dependent variable takes the form of a first difference instead, i.e.,

$$y_{i,t} - y_{i,t-1} = \beta_0 + \beta_1 y_{i,t-1} + \beta_2\ Entering\ a\ cross\ class\ partnership_{i,t} + \vartheta' Year_{i,t} + \theta' X_{i,t-1} + \varepsilon_{i,t}$$

Except for income, all covariates in $X_{i,t-1}$ are pre-treatment measures, namely they are measured at *t-1*, given that factors such as education could be affected by whom one starts a partnership with. Income is, however, included as a first difference (change between *t* and *t-1*) to control for the fact that entering a cross-class domestic partnership very likely brings a change in household income that of course may affect economic preferences.[23]

Studying attitudinal changes by controlling for the lag of the explained variable is, as well, the approach followed by Margalit (2013) when studying the effect of economic shocks on welfare preferences for alleviating issues of selection (see also Newman and Vickrey, 2017). Still, I show in section B.6 in the Appendix the results of using instead a first-difference approach, by subtracting for all variables the value at *t-1* (the wave previous to domestic partnership formation) from the value at *t* (the wave after domestic partnership formation). That setting allows me to get rid of all time-constant unobserved heterogeneity and is equivalent to a fixed effects model when having two periods. However, for the main analysis I prefer to stick to the specification above (as in Margalit, 2013) as it allows me to control for the partner's attitudes to make sure that the findings are not the mere result of individuals being persuaded by their partners.

A critical reader might wonder why addressing attitudinal changes happening after marrying/starting to cohabitate and not after start dating…and she would be right. Ideally, I would like to observe the latter. Contact with a new socioeconomic reality—not only through the partner herself but also through the friends and relatives of that—may begin mostly after start dating rather than after beginning to cohabitate or marrying. However, due to data limitations, I can only observe the moment at which domestic parentships begin, not the moment at which individuals start a romantic relationship.

[23] Similar results if introducing educational attainment and unemployment status as a first difference, as they could also be affected by entering a cross-class partnership (Section B.15 in the Appendix).

Still, it is plausible that for many individuals I am indeed also capturing the latter. The existing time span between the two moments I employ for measuring attitudinal changes for each individual (the wave previous to domestic partnership formation, *t-1*, and the one after, *t*) is of 2 to 4 years.[24] Therefore, for many individuals—those who do not date for a very long period before moving together—*t-1* will not only represent a moment in time previous to domestic partnership formation (this will be the case by construction) but also a moment in time previous to the beginning of the romantic relationship. Reassuringly, I show in Section B.5 in the Appendix that my results are pretty similar if rather than looking at changes between *t* and *t-1* I address changes between *t* and *t-2*, increasing the inter-wave time span to between 4 and 7 years.

Results

The main findings of the paper are presented in Table 2.1. I begin by showing in column (1) the results of a model in which I do not control for the lag of the dependent variable (LDV), therefore not accounting for plausible self-selection processes. As can be seen, among those who have just entered a domestic partnership, those who have done so with someone from a different social class hold, *ceteris paribus*, more left-wing economic attitudes than those who have done so with someone from the same social class. This could be caused by some contextual phenomena happening after partnership formation but also be the mere result of individuals' sorting: it may be the case that those open to form a relationship with someone from a different social class hold *a priori* a less conservative view of the economy.

To address this issue, I introduce in following columns the lag of the dependent variable. Controlling for it allows me to hold economic attitudes before partnership formation constant, interpreting these findings as changes in preferences. The coefficient for cross-class

[24] See Table B.4 in the Appendix.

domestic partnerships ("entering a cross-class partnership") remains statistically significant at the 5% level after controlling for the LDV (columns 2-4): those who enter a domestic partnership with someone from a different social class *experience a change* to the left in their preferences which is statistically larger than that experienced by those entering a within-class relationship.[25]

Table 2.1: Main results

	Index of attitudes towards the economy							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lag of index	-	0.592***	0.59***	0.541***	-	0.591***	0.59***	0.539***
		(0.023)	(0.023)	(0.024)		(0.023)	(0.023)	(0.024)
Male	-0.066**	-	-.0041*	-0.062***	-0.068**	-	-0.043*	-0.067***
	(0.028)		(0.023)	(0.023)	(0.029)		(0.023)	(0.023)
HH income (Δ)	-1.498***	-	-0.328**	-0.16	-1.489***	-	-0.337**	-0.179
	(0.268)		(0.145)	(0.142)	(0.268)		(0.147)	(0.144)
Age	-0.002	-	0.000	0.000	-0.002	-	0.000	0.000
	(0.002)		(0.001)	(0.001)	(0.002)		(0.001)	(0.001)
Unemployed	0.038	-	0.072	0.064	0.037	-	0.071	0.063
	(0.062)		(0.051)	(0.049)	(0.062)		(0.05)	(0.049)
Partner's attitudes	-	-	-	0.212***	-	-	-	0.214***
				(0.022)				(0.022)
Entering a cross-class partnership	0.099***	0.086***	0.072***	0.058**	-	-	-	-
	(0.03)	(0.025)	(0.025)	(0.024)				
Entering a cross-class partnership (down)	-	-	-	-	0.085**	0.073**	0.061**	0.034
					(0.034)	(0.028)	(0.029)	(0.028)
Entering a cross-class partnership (up)	-	-	-	-	0.114***	0.099***	0.083***	0.082***
					(0.035)	(0.028)	(0.028)	(0.028)
Entering a within-class partnership (Reference)	-	-	-	-	-	-	-	-
Observations	1472	1472	1472	1420	1472	1472	1472	1420
Education and BHPS dummies	YES	NO	YES	YES	YES	NO	YES	YES
R-squared	.065	.348	.367	.411	.065	.348	.367	.412

Robust standard errors in parentheses. The dependent variable is an index of attitudes towards the economy, with larger values indicating more left-wing attitudes. Dummies for respondent's level of education and for BHPS wave included except in columns (2) and (6). Covariates are measured at t-1. HH income in columns (1) and (5), ⊿ in HH income in columns (2)-(4) and (6)-(8). The sample includes those who have married or begun to cohabitate with his/her partner for the first time in their time in the BHPS between the present and the previous wave.
**** p<0.01, ** p<0.05, * p<0.1*

[25] Given *t* and *t-1* are only 2 to 4 years distant, one could consider that these attitudinal changes occur surprisingly quickly after meeting. However, this may be not so unexpected for at least two reasons. First, my results are consistent with those of previous papers addressing the effect of inter-class contact on economic attitudes, which find even faster effects (e.g., Mo and Conn, 2018; Newman and Vickrey, 2017). Second, the time span which I am addressing includes the honeymoon phase of the relationship, which could be adding some strength to the impact of a new reality (Bradbury and Karney, 2004; Lorber et al., 2015).

The positive effect of entering a cross-class partnership is found regardless of not controlling (column 2) or controlling (column 3) for a battery of variables often employed in this line of research.[26] Furthermore, as I control for the change in household income experienced after partnership formation, these results should not be the mere product of those entering a domestic partnership with someone from a lower class experiencing a boost in *socialist* attitudes as a consequence of becoming relatively poorer at the household level—a mere self-interest process.

To put in context the magnitude of the coefficients presented, the effect of entering a cross-class partnership is larger than that of becoming unemployed (0.05) or dissolving the partnership (0.01). Also, it is equivalent to between a tenth and a fifth of the average difference in the economic index between supporters of the Conservative and of the Labour Party, or between a professional and an unskilled worker, differences which indeed correspond to the standard deviation of the dependent variable (0.5).

Some scholars have argued that partners influence each other in what refers to political attitudes, leading many times to a convergence in preferences, and in others to an adaptation of one partner to the other's preferences (e.g., Coffé and Need, 2010; Kan and Heath, 2006; Stoker and Jennings, 2005; Zuckerman et al., 2005). Could the results here be explained by some similar mechanism? To deal with this issue, I control for the economic attitudes of the partner (column 4). Though ideally I would like to control for partner's attitudes at time *t-1*, the fact that this is a household survey makes that impossible, as I do not have information on partner's opinions at a time in which she/he had not joined the survey yet (as she/he did not form part of the household at time *t-1*).[27] Then, as a second best, I employ the attitudes of the

[26] Although in the main models I do not control for individual's social class due to high multicollinearity, I show results when doing so in Section B.8 in the Appendix, as well as for father's social class in Section B.9.

[27] This is also the reason why the partner is not included as another observation in the analysis, as for him/her I cannot observe attitudes at period *t-1* and measure changes in preferences.

partner at time *t*. It is important to note that this is not only a stringent but also a very conservative test, as controlling for the attitudes of the partner at time *t* rather than at time *t-1* exerts a downward bias in the estimate of β_2, making it even more difficult to find an effect of entering a cross-class partnership on economic preferences.[28]

As can be seen in column (4) of Table 2.1, the coefficient for entering a cross-class partnership slightly goes down after controlling for partner's attitudes. However, it is still significant at the 5% level even in such a conservative test. The results also suggest that respondents may copy the economic attitudes of their partners—for example, by discussing economy-related topics—and/or select partners with similar views of the economy, yet these processes cannot fully explain why those entering a cross-class partnership become more left-wing oriented than those entering a within-class one. I argue that this happens because of contact with new socioeconomic realities. As discussed in the theoretical section, once freed from the socioeconomically segregated worlds that characterize most modern societies, people gain a better understanding of the unequal distribution of resources and of the processes bringing these outcomes, therefore becoming less conservative in their economic attitudes.

In previous analyses, I have grouped together those entering a domestic partnership with someone from a higher class and those doing so with someone from a lower class. Let us now treat differently these two groups. The first reason for doing so is that, as explained earlier, I expect a larger impact for the latter group. But separating these groups may also illustrate that my findings are not the result of some unmeasured material process. One could argue that the material variables (e.g., Δ in HH income) which I include in the analysis are not enough to effectively account for self-interest processes. If that was the case, observing that those entering a domestic partnership with someone from a lower class become more left-wing oriented could

[28] I explain in Section B.17 in the Appendix why the bias is downwards.

be simply the consequence of them joining someone poorer and becoming poorer at the household level.

However, the results in this section show that this is not the whole story, as an attitudinal shift to the left is observed too for those entering a relationship with someone from a higher class, *not only* for those doing so with someone from a lower class (columns 6-7 in Table 2.1). If changes after partnership formation were just the product of self-interest processes, we should observe changes of opposite sign: those with a partner from a lower class should move to the left while those with a partner from a higher class should move to the right. That is not what can be observed here at all. *Both* those who enter a domestic partnership with someone from a lower class and those doing so with someone from a higher class move to the left *vis-à-vis* those entering a within-class domestic partnership. This is consistent with previous results finding a positive effect of inter-class contact for both the richer and the poorer (e.g., Franko and Livingston, 2020; Minkoff and Lyons, 2019).

Furthermore, if one tries to frame romantic relationships between classes in social mobility terms (e.g., as if those entering a partnership with someone from a higher class were experiencing upward mobility), no theory in that literature can be reconciled with the fact that *both* those entering a partnership with someone from a lower class and those doing so with someone from a higher class move to the left. Neither the *economic hypothesis* (adaptation to the attitudes of the class of destination) nor the *status maximization hypothesis* (adaptation to the attitudes of the highest status class, namely origin or destination) can explain the uniform move to the left observed for those in cross-class domestic partnerships (see e.g., De Graaf et al., 1995: 999-1001; Nieuwbeerta et al., 2000: 329-330).[29]

[29] None of them can explain the effect observed for those with a partner from a higher class. The economic hypothesis could, if anything, explain the effect observed for those with a partner from a lower class.

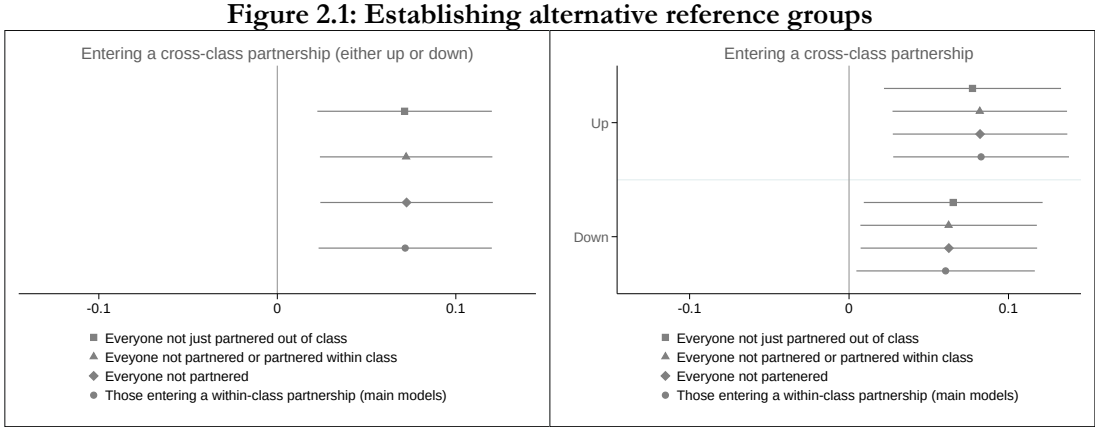
Contrary to my expectations, however, those entering a partnership with someone from a lower class do not experience a larger change to the left than those doing so with someone from a higher class. As the former might not only gain a better understanding of material differences in society but might also develop processes of empathy towards the lower classes, I expected to observe a larger effect in their case. That is not what I find. Indeed, once the partner's attitudes are introduced into the equation, only the coefficient for those entering a partnership with someone from a higher class retains its statistical significance (column 8). The lack of significance for the coefficient for those with a partner from a lower class might be explained by the fact that partners from lower classes tend to hold more left-wing economic positions, which added to the conservativeness of the test (note the abovementioned subsequent bias when controlling for partner's attitudes at *t* rather than at *t*-1) results in a defused net effect of entering a partnership with someone from a lower class. Alternatively, this unexpected finding could also be explained by attributions of wealth (affecting with more intensity those entering a partnership with someone from a higher class) being for some reason more easily activated than attributions of poverty (affecting with more intensity those entering a partnership with someone from a lower class). As shown by Hunt (2004), beliefs about the causes of wealth need not match those about the causes of poverty.

Before proceeding to the next section, a brief note about the choice of the reference group employed throughout the paper. I focus on individuals who have just entered a cross-class partnership between *t* and *t-1* and compare them to those entering a within-class partnership. In Sections B.17 and B.22 in the Appendix, I justify why I consider this the best course of action. I claim that those entering a within-class partnership are the natural control group for those entering a cross-class partnership, as these two groups are going through a similar process and stage of life (i.e., marrying/starting to cohabitate), and thus are highly

comparable. Also, this specification allows for a non-ambiguous interpretation of the coefficients.

However, I show below in Figure 2.1 the results of alternative specifications in which I include all respondents (also those who never enter a domestic partnership during their time in the survey) and all observations (also those before *t-1* and after *t*), and set plausible alternative reference groups: everyone who did not just enter a cross-class partnership (Model A), everyone not partnered or partnered within the same class (Model B), everyone not partnered (Model C), and those who just entered a within-class partnership (Model D), namely the specification employed throughout the paper.[30] As can be seen, the results are very similar regardless of the reference group employed.[31] Still, I continue using the preferred specification (Model D) for the reasons already discussed.

Figure 2.1: Establishing alternative reference groups



Point estimates and 95% confidence intervals for the coefficients of entering a cross-class partnership, in models with alternative reference groups—see main text and section B.22 in the Appendix.

Furthermore, I show in Section B.23 in the Appendix the results of an alternative estimation strategy in which I estimate the difference in the dependent variable between those entering a cross-class partnership and those who remain single throughout the panel. Then, I compare that

[30] These correspond to columns 3 and 7 of Table 2.1. Namely, models which do not control for partner's attitude. I proceed this way for consistency, as in the other models of Figure 2.1 I include single people for which this variable is obviously not defined.

[31] See Section B.22 in the Appendix for a more detailed explanation.

difference to the difference between those entering a within-class partnership and those who remain single throughout the panel. I find again a positive and significant effect of entering a partnership with someone from a different class.

Placebo tests

Is contact with new socioeconomic realities what moves to the left the attitudes of those entering a cross-class partnership? The fact of tracking the same individuals over time instead of relying on the snapshot that cross-sectional data offers helps to reduce issues of self-selection, as I look at changes in preferences and not at levels. Also, controlling for the material situation at the household level helps to rule out self-interest explanations, as well as it does the fact that *both* those with a partner from a lower class and those with a partner from a higher class change their preferences in the same direction. Furthermore, controlling for the partner's attitudes reduces concerns about the findings being the mere result of respondents copying their partners' preferences.

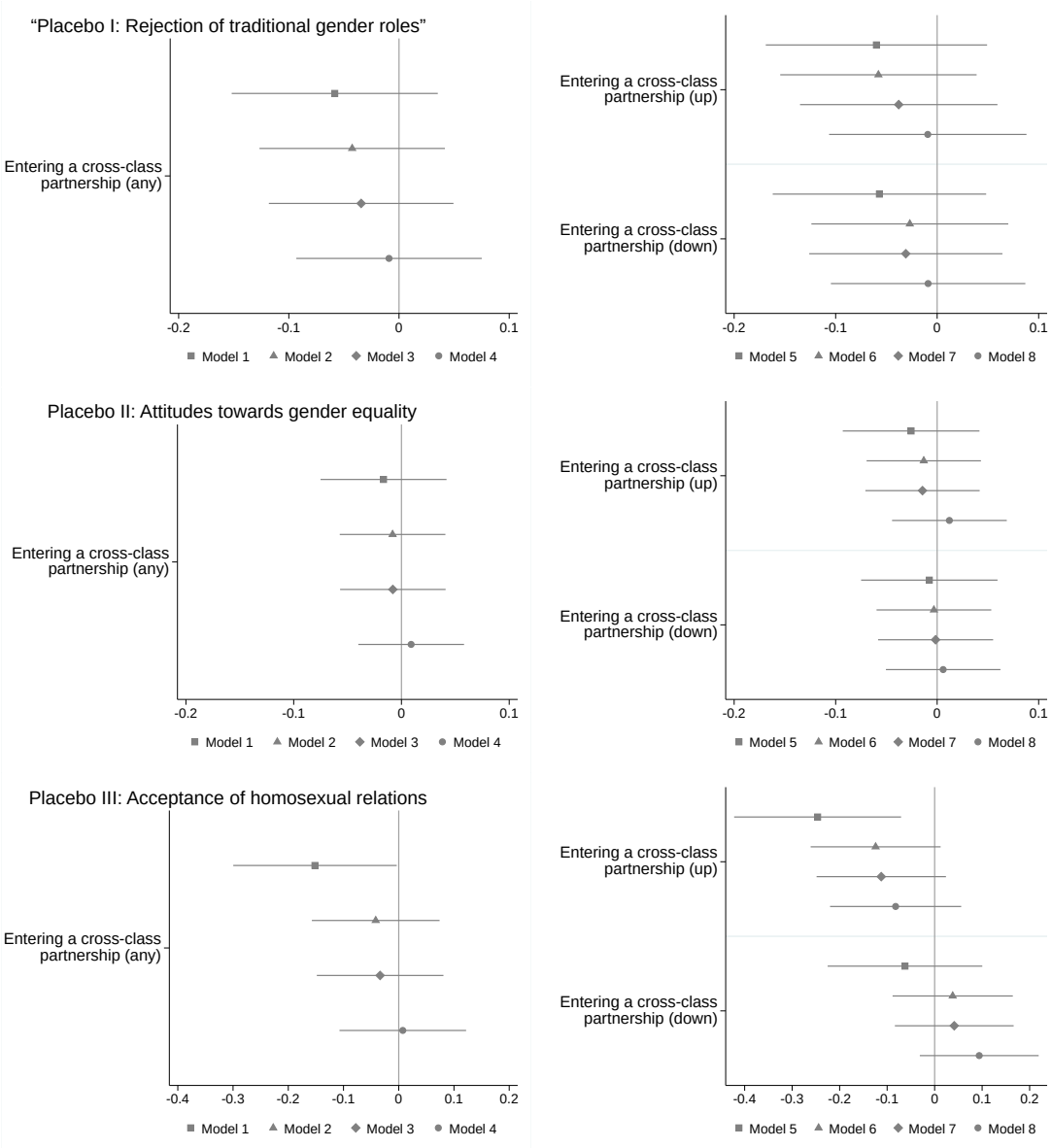
Still, there could exist some other process unrelated to the perception of the economy broadly affecting political attitudes in general and not only economic ones. Observing a change in other political attitudes unrelated to the economic domain would cast some doubt on the thesis about contact with new socioeconomic realities driving my results.

In order to test this possibility and further increase confidence in the thesis of the paper, I perform a series of placebo tests. Do those entering a cross-class partnership present different trends of changes in non-economic preferences than those entering a within-class partnership?

Aiming to answer this question, I look at three different measures of social conservatism: a measure of acceptance of traditional gender roles (e.g., Campbell et al., 2010; Langsæther et al., 2022), an index of attitudes towards gender equality, and a measure of acceptance of homosexual relationships (e.g., Langsæther et al., 2022; La Roi and

Mandemakers, 2018).[32] I replicate the eight models of Table 2.1 for each of these placebos, displaying in Figure 2.2 the point estimates and 95 per cent confidence intervals of the dummies for entering a cross-class partnerships (the reference group being in all models those entering a within-class partnership). Higher values represent more progressive values.

Figure 2.2: Placebo tests



Point estimates and 95% confidence intervals for the coefficients of entering a cross-class partnership, in models analogous to those estimated in Table 2.1, but with the dependent variable being three different measures of social conservatism (see Section B.12 in the Appendix for more details). Higher scores represent more progressive values. The reference group is in all models those entering a within-class domestic partnership. Models 1-8 corresponds to models analogous to those in columns (1)-(8) of Table 2.1.

[32] See Section B.12 in the Appendix for an explanation of these measures and for the tables of results associated to Figure 2.

The picture portrayed in these analyses seems clear. Whereas entering a cross-class partnership moves attitudes towards the economy to the left (Table 2.1), it does not produce relevant changes on unrelated measures of social conservatism (Figure 2). Furthermore, the (often) insignificant changes observed in these placebo analyses run in the opposite direction to those observed for economic attitudes: those entering a cross-class partnership do not move to the left but towards social conservatism, increasing confidence that my findings do not simply reflect a change in a broader set of political attitudes.[33]

Heterogeneous analyses

If contact with new socioeconomic realities through a cross-class partnership shapes the way in which people think about the economy, then one would expect to observe a larger effect among those who have fewer chances to learn about inequality through means other than personal experience (e.g., Minkoff and Lyons, 2019). One factor that may be relevant in this regard is political information. It seems plausible to consider those politically informed as more likely to have a better knowledge of the economy and inequality. Then, I expect the impact of entering a cross-class partnership to be limited for those politically informed. However, for those politically uninformed, personal contact with a new socioeconomic reality may have more room for providing new information and change economic attitudes.[34, 35]

[33] The large effect in model (5) of the third placebo is found before controlling for the LDV, namely it does not measure a change in preferences.

[34] I also look in Section B.1 in the Appendix at heterogeneity by educational attainment. In line with Minkoff and Lyons (2019), I find a more pronounced effect for those not highly educated, who may have fewer chances to learn about inequality through means other than personal experience.

[35] I also look in Section B.24 in the Appendix at heterogeneity by gender. The effect of entering a partnership with someone from a higher class is significant for men but not women. It is the opposite in the case of entering a partnership with someone from a lower class. This phenomenon could be explained by men and women reacting in different ways to the different mechanisms (e.g., informational, emotional, etc.), which merits further inspection in the future. Men in my sample are significantly more likely to marry up, while women are significantly more likely to marry down.

Unfortunately, the BHPS does not contain any measure of political knowledge. However, it does include a question on political interest that can be taken as a very good proxy of being politically informed (Rapeli, 2022). When studying the validity of various possible survey proxies for political knowledge, Rapeli (2022) finds that political interest is indeed the most valid one, superior to accuracy of party placements in an ideological dimension, internal political efficacy, political participation, and even self-assessed knowledge, which may suffer from social desirability bias.

The BHPS asks respondents to indicate whether they are "very interested", "fairly interested", "not very interested" or "not at all interested" in politics.[36,37] I split the sample into two: those who report to be "very interested" or "fairly interested" in politics, and those who report to be "not very interested" or "not interested at all" (as e.g., Fraile and Sanchez-Vítores, 2020; Kan and Heath, 2006). I estimate separately for these two groups models analogous to those in columns (3) and (7) of Table 2.1—I exclude the others for clarity of presentation.[38] I show the point estimates and 95 per cent confidence intervals of the dummies for entering a cross-class partnership—the reference group in all models being again those entering a within-class partnership.

As can be observed in Figure 2.3, those interested in politics do not suffer any significant change when entering a cross-class domestic partnership.[39] However, those without an interest in politics, who might be less likely to learn about inequality through means other than personal experience, shift their economic attitudes to the left when entering a cross-class

[36] This question is not included in waves 7 and 10 of the BHPS.

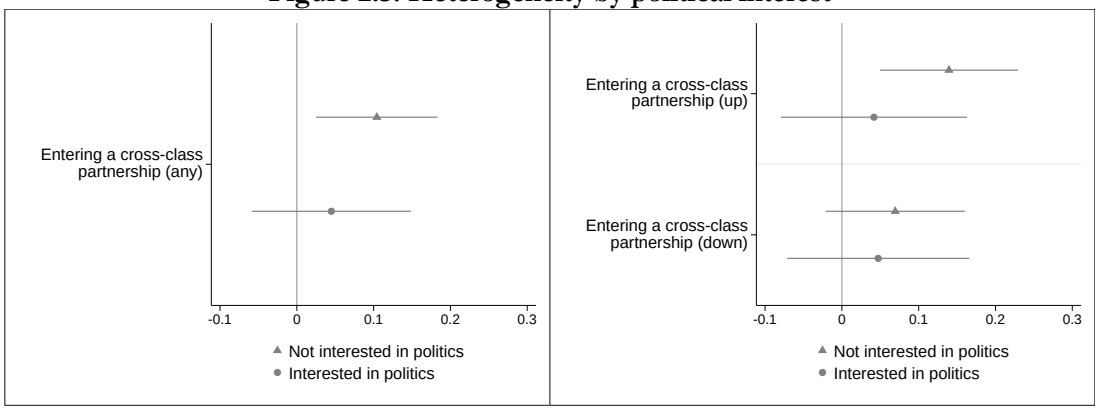
[37] There is not any non-respondent to this item in my sample.

[38] Similar findings if grouping all respondents together and including an interaction with political interest.

[39] See Section B.13 in the Appendix for the table of results associated to Figure 2.3.

partnership.[40,41] The effects observed for those not interested in politics are around twice those observed in the main analyses of Table 2.1, and represent around a third of the average difference in economic attitudes between supporters of the Conservative and of the Labour Party.

Figure 2.3: Heterogeneity by political interest



Point estimates and 95% confidence intervals for the coefficients of entering a cross-class partnership. The reference group is in all models those entering a within-class domestic partnership. The figure on the left (right) corresponds to a model analogous to the one in column 3 (7) of Table 2.1. See Table B.21 in the Appendix for the controls included in each model. The dependent variable is an index of attitudes towards the economy, with larger values indicating more left-wing attitudes.

Reassuringly too, no similar pattern of heterogeneity arises in the distinction between those interested and those not interested in politics for any of the non-economic attitudes employed in the placebos of Figure 2.2 (results not shown), giving more credit to the inter-class contact hypothesis.

Considering the magnitude of the distance between partners' social class

I perform in Section B.20 in the Appendix an analysis examining the possibility of non-linear effects of the absolute distance between partners' social class positions in the six-class schema employed, rather than making the dichotomous distinction between those in a cross-class partnership and those in a within-class partnership. In other words, does it have the same impact

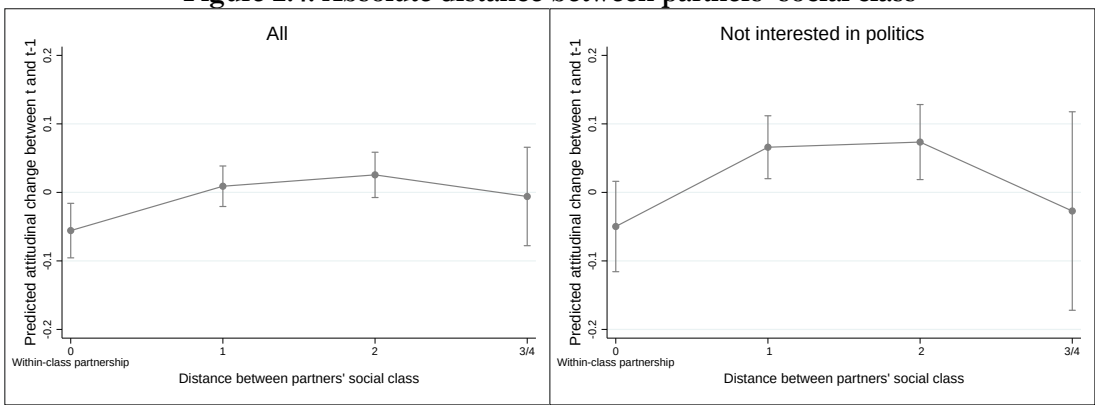
[40] Note that although the sample sizes of those interested and of those not interested in politics are very different, the magnitudes of the coefficients are very distinct too (i.e., the different findings are not just a matter of different levels of statistical power).

[41] Section B.2 in the Appendix discusses alternative explanations for the heterogeneous findings.

for a partly skilled worker to form a partnership with a skilled manual worker rather than with a professional?

I conduct analyses in which the main explanatory variable is the *absolute* number of class steps separating the partners (e.g., distance=1 for adjacent classes) and the square of that value. Figure 2.4 shows predicted attitudinal changes between *t* and *t-1* in the index of economic attitudes, for different absolute distances between partners' social class, both for the full sample and for those not interested in politics.[42] As can be seen, those entering a within-class partnership (distance=0) do not change their attitudes or do it to the right, while those entering partnerships distant 1 to 2 classes move to the left (especially for the subsample of those not interested in politics). However, that effect weakens for those in more distant pairings.

Figure 2.4: Absolute distance between partners' social class



Predicted values and 95% confidence intervals for the attitudinal change in the index of economic attitudes between *t* and *t-1*, for different absolute distances between partners' social class. See Section B.20 in the Appendix.

On self-selection into cross-class partnerships and reverse causality

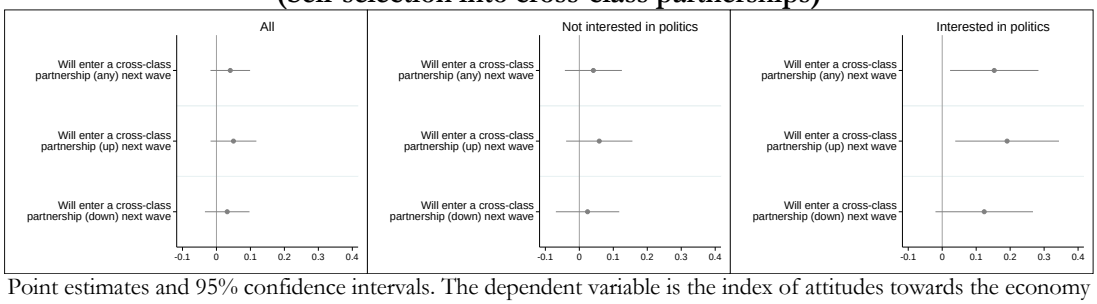
Now I present a simple test of self-selection in which I look at economic attitudes before partnership formation (i.e., I predict attitudes at time *t-1* rather than *t*) to check whether those

[42] For the analyses in Figure 2.4, I group together those partnerships whose distance is 3 and 4, given the low number of observations in the latter group. There are no partnerships 5-classes distant in my sample (i.e., between individuals on opposite extremes of the six-class schema, namely a professional and an unskilled worker).

who *will* form a cross-class partnership have *a priori* more leftist attitudes. The results, which are displayed in Figure 2.5, are very illustrative.[43]

Whereas at the aggregate level (left graph of Figure 2.5) I do not find any proof of self-selection into type of domestic partnership (i.e., cross-class or within-class) according to economic attitudes, the story is very different if making the distinction between those who are interested in politics and those who are not.[44] For those not interested, I find again no proof of self-selection. This means that although these people do not self-select into cross-class partnerships by their *levels* of economic attitudes (middle graph of Figure 2.5), they *do* change their preferences afterwards when forming a domestic partnership with someone from a different class (Figure 2.3).

Figure 2.5: Predicting attitudes before partnership formation (Self-selection into cross-class partnerships)



Point estimates and 95% confidence intervals. The dependent variable is the index of attitudes towards the economy at period *t-1*. First line of each graph from a model comparing those who will enter a cross-class domestic partnership with those who will enter a within-class domestic partnership (reference group). Second and third lines from a model comparing those who will enter a domestic partnership with someone from a higher class and those who will enter a domestic partnership with someone from a lower class with those who will enter a within-class partnership (reference group). See Table B.2 in the Appendix for more details.

For those interested in politics, it is the opposite story. They *do* self-select into cross-class partnerships conditional on their *levels* of economic attitudes (right graph of Figure 2.5), namely those with *a priori* more leftist attitudes are more likely to form a cross-class partnership. However, they do not change their preferences afterwards (Figure 2.3). This is consistent with a world in which people who do not care too much about politics enter cross-

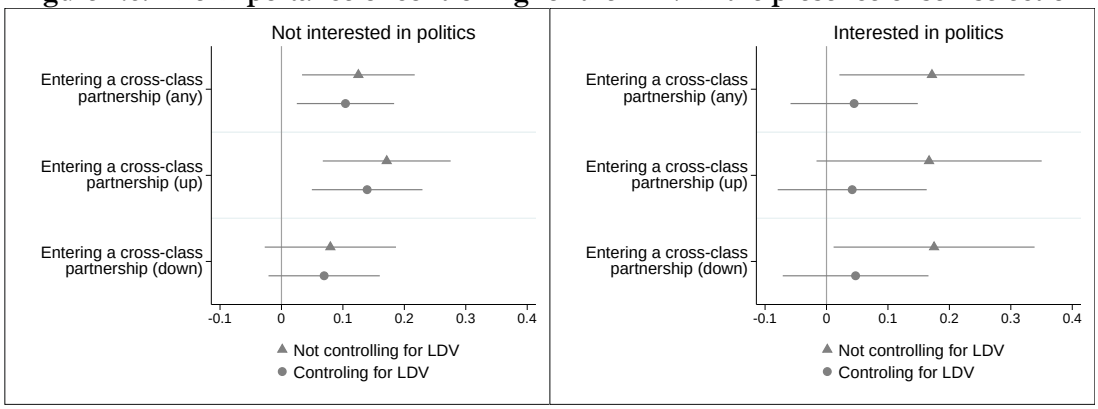
[43] See Table B.2 in the Appendix for more detailed results. See also Section B.2 in the Appendix for a discussion of alternative explanations for the heterogeneous findings.

[44] The self-selection findings are robust to controlling for the economic attitudes of the partner at time *t*.

class partnerships regardless of their ideology as well as have more malleable preferences, whereas for those for whom politics is a key issue, ideology does play a role in determining cross-class partnerships while they also have stickier preferences (i.e., conservative individuals with an interest in politics may aim to avoid people from other classes to follow conservative cultural norms of homogeneity).

These findings can also be illustrated through Figure 2.6. Consistent with the heterogeneous findings of Figure 2.3, I show how controlling for the LDV change the estimated effect of entering a cross-class partnership for those interested in politics (they do self-select) but not for those not interested (they do not self-select).[45]

Figure 2.6: The importance of controlling for the LDV in the presence of self-selection



Point estimates and 95% confidence intervals of models estimated separately for those not very interested or not interested at all in politics ("not interested in politics") and for those very interested or fairly interested in politics ("interested in politics") at time *t-1*, both when controlling and when not controlling for the lag of the dependent variable (LDV). See Table B.22 for full results. First line of each graph from a model comparing those entering a cross-class domestic partnership with those entering a within-class domestic partnership (reference group). Second and third lines from a model comparing those entering a domestic partnership with someone from a higher class and those entering a domestic partnership with someone from a lower class with those entering a within-class partnership (reference group). The dependent variable is the index of attitudes towards the economy at time *t*, with larger values indicating more left-wing attitudes.

The test in Figure 2.5 was one of self-selection on *levels* of economic attitudes prior to partnership formation, something not very worrisome as I control for that in the main analyses, yet which still highlights the necessity of controlling for attitudes in earlier periods (e.g., *t-1*).

[45] See Section B.14 in the Appendix for the tables of results associated to Figure 2.6.

More troubling for the thesis of the paper would be the possibility of reverse causality, which could be described as an explanation of self-selection on attitudinal *changes* rather than *levels*.

What if those who enter a cross-class partnership do not move to the left as a consequence of that but the other way round? Namely, what if those in a positive trend of leftist economic attitudes are more likely to form a cross-class partnership because of indeed being in that trend? In light of the results of the placebos and heterogeneity analyses above, as well as an analysis showing that those in a leftist trend between *t-2* and *t-1* are not more likely to form a cross-class partnership at time *t* (Table B.3 in the Appendix), I conclude that reverse causality is not a plausible explanation for my results.

Discussion

The present research proposes a novel way to measure contact between social classes in the literature studying inter-class contact and political attitudes, which almost solely looks at the neighbourhood (e.g., Bailey et al., 2013; Franko and Livingston, 2020; Minkoff and Lyons, 2019). Moreover, the fact that I look at attitudinal changes rather than levels helps ameliorating the self-selection concerns often present in this literature (Christensen et al., 2024).

I have shown that forming a domestic partnership with someone from a different social class moves to the left the way in which the economy is perceived. The fact that both those who do so with someone from a lower position and those who do so with someone from a higher position move in the same direction, as well as the inclusion of a control accounting for changes in material circumstances, discard that observed shifts in economic attitudes are just a product of changes in economic conditions. Moreover, I draw on research about spouses' political preferences and show that these changes are not the product of individuals copying their partners' attitudes either. I argue that these changes are caused by contact with a new socioeconomic reality, which has the ability to make inequality and its black box visible, while allowing for empathy processes towards disadvantaged groups. Consistent with this thesis, I

find that political attitudes in domains unrelated to the economy are not changed. Finally, I find heterogeneous results by political interest. The effect of cross-class partnerships is more pronounced for those without an interest in politics, who may rely more on personal experiences to acquire information about inequality and the economy.

Given the nature of the data (a household survey), I have only information on the time at which marriage/cohabitation rather than the relationship itself begins. However, as I am measuring changes in preferences between points in time that are 2 to 4 years distant (the wave previous to marriage/cohabitation, *t-1*, and the one after, *t*), for many individuals *t-1* will represent also a moment in time previous to the start of the relationship. Reassuringly, Section B.5 in the Appendix shows that measuring instead attitudinal changes between *t* and *t-2* (so obtaining inter-wave time spans of 4 to 7 years) produces similar findings.[46] Still, future research capturing with more precision the moment of meeting a new partner, as well as extending this analysis not only to romantic relationships but also to friendship ones, would be very beneficial for better understanding the relationship between inter-class contact and economic attitudes.

In a context of rising socioeconomic segregation around the world (Azpitarte et al., 2021; Mijs and Roe, 2021; Musterd et al., 2017), the results of this paper suggest that reductions in inter-class contact may tilt the attitudes of citizens towards economic conservatism, lowering the demand for policies aiming to reduce inequality. As inequality, in turn, boosts segregation and widens social distance between groups (Massey and Fischer, 2003; Mijs and Roe, 2021; Reardon and Bischoff, 2011), we may be facing nowadays a vicious circle which asks for the creation of institutions and the development of policies bringing together people from different

[46] Note that addressing changes between *t* and *t-2* reduces substantively the sample size, which means losing some statistical power and somewhat increasing the p-values. However, the magnitudes of the coefficients are very similar to those in the analyses addressing changes between *t* and *t-1*.

social classes. For example, by enhancing socioeconomic diversity at schools and neighbourhoods which, in turn, would plausibly increase the number of romantic partnerships between social classes.

Chapter 3: Draft of the third paper

Franco's (Lasting) Labyrinth

Civilian Victimization during a Civil War and Attitudes towards the Police

Abstract: The Spanish police has been reiteratedly criticized in European reports for ill-treatment and lack of accountability. Indeed, according to the Political Terror Scale measuring state-sanctioned violence (e.g., police brutality), Spain is one of the most repressive countries in Europe. However, all this contrasts with a very positive perception of the police by the Spanish society, relative to other countries. I argue that this paradox might be explained by the *verticalization* of society brought by a long Francoist dictatorship which followed a bloody Civil War. Aiming to quantitatively approach this idea, I exploit sub-national variation in levels of Francoist repression during the Civil War and the post-war period, as regime's indoctrination in the necessity of strong hierarchies to preserve order and stability might have been easier in those places which suffered more repression, due to a combination of heightened risk aversion and fear of a new conflict. Looking at three different geographical levels (municipalities, provinces and autonomous communities) and employing three different surveys, I find that individuals in areas which experienced more Francoist repression during the Civil War and its aftermath value the police more positively nowadays. Consistent with previous research on the legacies of violence, the effect of repression weakens for younger generations. Furthermore, I discard several alternative explanations to the one I propose, such as accounts concerning the sorting of individuals (i.e., exile) or the influence of many pre-war phenomena, and show that Francoist repression correlates also with different attitudes argued to mediate the relationship with attitudes towards the police. A preliminary analysis of the Guatemalan Civil War confirms a positive relationship between civilian victimization and trust in the police beyond the case of Spain.

"To obey, just like that, for the sake of obeying, that is something only people like you do, Captain"
Pan's Labyrinth (Guillermo del Toro, 2006)

"The principles of authority, order and hierarchy will be necessary to be restored with all severity"
(Franco, 1939)

Introduction

In the Oscar awarded film Pan's Labyrinth (Guillermo del Toro, 2006), a 13-year-old girl named Ofelia has to navigate some trials ordered by a faun in order to succeed to the throne of a fantasy realm. What might seem at first glance a bedtime story is indeed a harsh fairy tale for adults about the moral necessity to disobey taking place in the context of post-Civil War Spain.

The film begins with Ofelia and her mother moving to a small village, where the new spouse of that, a captain of Franco's army, is in charge of eliminating the last Republicans hiding in the mountains (the so-called *maquis*). This ruthlessly Francoist villain aims to impose unquestioned obedience on those around him—or more specifically, below him—through repression and death. In this way, the concepts of hierarchy and blind obedience thematically connect the real-world of 1944 Spain with the fantastic kingdom underneath it.

In the climax of the plot, Ofelia chooses to disobey both the faun and the captain. Once this sort of *Alice* has learnt the virtue of disobedience, she is finally worthy of the throne of her particular *Wonderland*. However, the thesis of the film is made explicit earlier in the upper cruel world of adults when a medical doctor—who works for the fascist captain but helps the *maquis* in secret—refuses to follow the merciless order of the villain to keep conscious and alive a tortured prisoner for the purpose of extracting information out of him. Stunned by the refusal of an order, the captain asks the doctor why he disobeyed him, to which the latter

responds with the opening quote of this text, signing his own death sentence: *To obey, just like that, for the sake of obeying, that is something only people like you do, Captain*.

As I discuss in more detail in next section, the Spanish police has been reiteratedly criticized by European agencies such as the Commissioner for Human Rights of the Council of Europe (CHR) or the European Committee for the Prevention of Torture and Inhuman or Degrading Treatment or Punishment (CPT): "Reports indicating excessive use of force by law enforcement authorities in the course of anti-austerity demonstrations in 2011 and 2012 brought to light a number of long-standing, serious human rights issues concerning the actions of Spanish law enforcement agencies" (CHR, 2013: 19). Moreover, according to the Political Terror Scale (PTS)—a measure of state-sanctioned violence (e.g., police brutality)—Spain is one of the most repressive countries in Europe. However, despite all this, Spain is one of the countries in which the police are better perceived according to data from the European Social Survey (ESS).

What might explain this paradox? Why a country with so much repression has so positive attitudes towards the police?[1] I argue that this is caused—at least partly—by the *verticalization* of Spanish society brought by four decades of Francoism following a bloody Civil War. One of the issues in which this regime aimed to exert an influence on was the creation of a more vertical and hierarchical society (Richards, 1998; Vincent, 2007), which, I argue, may have produced more favorable evaluations of institutions such as the police.[2] The

[1] As I show in next section, evaluations of the police are a decreasing function of the level of repression across European countries, with Spain as an outstanding exception where high levels of repression coexist with positive views of the police.

[2] That the police is a very vertical institution is particularly obvious in a country such as Spain, where policing follows a bureaucratic/professional rather than a public service model (Bradford et al., 2016). Namely, it follows a reactive model which emphasizes the role of the police in enforcing law and order rather than focusing on the underlying causes of crime. Also, it does not rely on the proximity between the police and the citizens, which would be the case in more horizontal models where serving the community is a core function of policing—the

regime's willing of conveying a mindset of extreme discipline and blind obedience can be observed, for example, in school textbooks (Pinto, 2004) or in the public discourses of Franco himself (Franco, 1939). Spanish society embraced this narrative due to the fear of a new war, as order and discipline were perceived as necessary for avoiding the chaos that "provoked" the Civil War (Cazorla-Sánchez, 2005; Sevillano Calero, 2003). As claimed by Bateson, "Through the mechanism of fear (…), a critical mass of [Guatemalan] civilians emerged from the war either acquiescing to or fully embracing (the) narrative (that) repression is a necessary and effective way to provide security (2017: 635).

I empirically test a logical implication of my thesis by exploiting variation of Francoist repression within Spain. Vertical indoctrination by the Franco regime might have been easier in those places where there existed more Francoist repression during the Civil War and the post-war period. Fear of repression had a paralyzing effect (Young, 2019) during the dictatorship (Calvo Vicente, 1995; Molinero and Ysàs, 1998; Del Arco Blanco, 2009; Sevillano Calero, 2003). Moreover, fear heightened risk aversion (Young, 2019). This made paralyzed Spaniards in more repressed areas to better embrace institutions like the police, which, by increasing order and stability, were perceived to reduce the risk of violence of the Civil War repeating itself (Blair, 2022).[3]

I conduct three analyses in which I predict individuals' attitudes towards the police with Francoist repression measured at the regional level. For improving data triangulation, I test my hypothesis at three different levels of geographical aggregation of the variable measuring repression. First, employing data from the Catalan *Institut de Ciències Polítiques i Socials*

latter is often present in the Anglophone world. In countries like Spain, "legitimacy is granted to police by dint of their location within broader structures of authority" (Bradford et al., 2016: 403).

[3] This connection between repression and regime's acceptance is supported, for example, by the fact that there are more streets commemorating Franco's dictatorship in places where there was more repression (Tur-Prats and Valencia Caicedo, 2020).

(ICPS), I find that respondents living in Catalan municipalities which experienced more Francoist repression during the Civil War hold more positive evaluations of police performance nowadays. I find this effect to be more pronounced across older generations of Catalans, who more directly experienced the violence. This is a nuanced analysis at the finest level of detail, with repression being measured at the most disaggregated geographical level, namely that of municipalities.

However, looking only at Catalan municipalities may rise concerns about the generalizability of the results to the rest of Spain. Therefore, I conduct a second analysis which covers not only Catalonia but also all the other Spanish regions. For that, I resort to surveys from Spain's Sociological Research Centre (CIS, or *Centro de Investigaciones Sociológicas*), which has information on the province (NUTS 3) of residence of respondents. I find that individuals living in provinces which experienced more Francoist repression during the Civil War rate more positively the police profession nowadays.

Lastly, I carry out an exploratory analysis employing the special module of the 5th round of the ESS about the justice system. Although this dataset contains information on the region of residence of respondents at a level of disaggregation which is not optimal (autonomous communities/NUTS 2), it contains a rich battery of items measuring attitudes towards the police. This allows me to show that Francoist repression positively correlates with positive views of the police on numerous dimensions.

Throughout the different analyses, I discard several alternative explanations to the one I propose. Among other, these findings cannot be explained by individuals' sorting (i.e., people with negative attitudes towards the police fleeing more repressed municipalities), by several pre-war phenomena (e.g., individuals in provinces with a more developed civil society before the war holding negative perceptions of the police), or by lower levels of support for Catalan secession in repressed areas (Rodon and Tormos, 2023). Furthermore, the results cannot be

explained either by a link between political violence by any warring faction and authoritarian values (e.g., Davis and Silver, 2004; Dyrstad, 2013; Hobfoll et al., 2006), as I find a null effect of Republican repression.

Consistent with previous findings in the literature showing that the intensity of the legacies of violence decreases for more distant cohorts as the strength of intergenerational mechanisms of transmission weakens (e.g., Lupu and Peisakhin, 2017; Wang, 2021), I find the effect of repression to decrease for younger generations of Spaniards. Still, the effect of Francoism may have persisted many decades after the death of the dictator in 1975—what some have called *sociological Francoism*—thanks partly to a Transition to democracy which, unlike in the case of other countries such as Portugal, did not bring an inversion of hierarchies in society (Fishman, 1990, 2011; Fishman and Lizardo, 2013).

Furthermore, I find that Francoist repression also correlates with different attitudes argued to mediate the relationship with attitudes towards the police, such as demand for obedience and security in society, more materialist rather than post-materialist values, aversion to risk and a higher desire for stability in life, reduced risky behaviours (i.e., participation in demonstrations), and lower tolerance of acts perceived to endanger the political status quo.

Approaching the research question at three different geographical levels (municipalities, NUTS 3, NUTS 2) makes it possible to combine the benefits of different analyses. For example, the first (third) analysis presents a high (low) level of geographical disaggregation but has just one (many) item/s measuring police attitudes and covers only Catalonia (the totality of the country). Also, this strategy allows me to include in the empirical analysis covariates which are not always available at all levels of geographical aggregation. For clarity, I summarize the empirical approach followed throughout the paper in Figure 3.1 below.

The finding of similar results at three different geographical levels and using three different surveys demonstrate the strength of the relationship between Francoist repression and positive views of the police. Obviously, this does not preclude the existence of endogeneity. Repression during the Civil War was of course not random. However, controlling for plausible determinants of repression in the Spanish Civil War (e.g., Balcells, 2011, 2017; Herreros and Criado, 2009) makes the relationship even stronger.[4] Actually, most omitted variables that one could think of in this analysis might be biasing my results not upwardly but towards zero.[5] Likewise, I employ numerous individual-level controls often employed in the literature on attitudes towards the police.

Figure 3.1: Summary of the empirical approach followed

Level of aggregation at which the variable of Francoist repression is defined	**Dataset**	**Dependent variable(s)**	**Controls**	**Alternative explanations discarded**
Municipalities (Catalonia)	ICPS	Evaluation of police performance	- Individual-level controls. - Regional predictors of repression (vote for the left and electoral competition in 1936, trade unions presence, elevation of the municipality, etc.)	- Effect of repression by *any* warring faction - Sorting of individuals (i.e., individuals with negative attitudes towards the police fleeing more repressed municipalities) - Support for secession from Spain
Spanish Provinces (NUTS 3)	CIS	Rating of the police profession, evaluations of some characteristics of the police	- Individual-level controls - Regional predictors of repression (vote for the left and electoral competition in 1936, agrarian inequality in XIX century, illiteracy rate, etc.)	- Larger recruitment of policemen in repressed areas - Different police practices across regions - Level of development of civil society in XIX century - Effect of repression by any warring faction - National pride/Identification with Spain - See in text for others (e.g., criminological accounts)
Spanish autonomous communities (NUTS 2) Exploratory analysis showing that repression correlates with several attitudes towards the police	ESS	Several attitudes towards the police from a special module on the justice system	Individual-level controls	

Finally, I perform a parallel preliminary analysis for the Guatemalan Civil War. The work of authors such as Regina Bateson (2017) and Linda Green (1999) highlights the interrelationship between fear in the socialisation processes of Guatemalans during the Civil War, their fear to

[4] This is not surprising given that leftist places received more Francoist repression, and leftist people tend to hold more negative views of institutions as the police.

[5] With the exception of the explanation about people with negative views of the police fleeing more repressed areas, which I address by controlling for the level of displacement as in Balcells and Steele (2016).

a return to the war and their reception to the military's propaganda, characteristics which resonate with the theory I propose for the Spanish Civil War. Looking at municipalities in Guatemala, I find a positive correlation between state-led repression during the civil war and trust in the police nowadays.

To the best of my knowledge, only two quantitative analysis have systematically assessed the effect of violence during civil wars on public attitudes towards the police. Deglow and Sundberg (2021) argue that, by taking on counterinsurgency-related roles during a conflict, the capacity of the police to address ordinary crime is undermined, affecting perceptions of police effectiveness. Furthermore, actions to fight insurgency may result in practices eroding perceived procedural justice. Examining the case of the ongoing civil war in Afghanistan, the authors find local conflict intensity to be *negatively* associated with perceived effectiveness, procedural justice and trust in the police. Blair and Morse (2021) show that violence perpetrated by the rebels during the civil wars in Liberia *positively* correlates with trust in the police and citizens' reliance on state over non-state actors for security. This paper is distinct from Deglow and Sundberg (2021) and Blair and Morse (2021) in at least three key respects. First, I focus on violence against civilians in the context of a civil war, rather than on aggregate indicators of violence conflating multiple forms of conflict, such as battle-related deaths. This allows for a more nuanced examination of the relationship between civilian victimization and public police perceptions. Moreover, Blair and Morse (2021) measure exposure to violence through self-reported indicators, raising concerns of endogeneity. Second, the temporal proximity to the ongoing conflict in Afghanistan and the Liberian civil wars limits the ability of Deglow and Sundberg (2021) and Blair and Morse (2021) to assess whether legacies of violence persist over generations. By contrast, this paper draws on a conflict more than eight decades past, contributing to a growing literature on the long-term legacies of conflict (Cremaschi and Masullo, 2023; Zhukov and Talibova, 2018). Third, this is the first study to assess the

relationship between civil-war violence and police perceptions in the context of an advanced democracy—Spain—and extend the analysis to more than one country by incorporating evidence from Guatemala, thereby enhancing the external validity of the findings.

While these findings diverge from studies of the Spanish Civil War documenting a penalty for the regime by way of more votes for the left in more repressed areas during the democratic period following the dictatorship (Domènech and Sánchez-Cuenca, 2022; Rodon, 2022; Villamil, 2021a), they align with the results of Rodon and Tormos (2023), who find that individuals in Catalan municipalities more repressed by Franco are less likely to support secession from Spain. According to these authors, given that secession means a rupture of the status quo, those who experienced more closely political violence decrease their support for independence, as their attitudes are more responsive to an increased perceived risk of the break of social harmony. In a similar vein, I argue that repression increased tolerance towards institutions perceived to be in charge of maintaining order and stability, such as the police, as a way of avoiding future political conflicts. These results are also consistent with Blair (2022), who finds wartime violence in Liberia to be associated with demand for state-provided security and greater fear of civil war recurrence.

The Spanish puzzle of attitudes towards the police

Several decades after the death of Franco and the subsequent transition to democracy, the imprint of a forty-years dictatorship can still be felt in some Spanish social phenomena. One of them is policing. The significant lack of accountability of law enforcement agencies in a democratic country such as Spain is striking (Brandariz García and Faraldo Cabana, 2015). Reports from the Commissioner for Human Rights of the Council of Europe (CHR) or the European Committee for the Prevention of Torture and Inhuman or Degrading Treatment or Punishment (CPT) pointing to police brutality and mistreatment by the police in Spain are not infrequent: “Ill-treatment by and impunity of members of law enforcement agencies is a very

serious, long-standing human rights issue in Spain" (CHR, 2013: 3). For instance, the visit of the CHR in June 2013 rises the concern of disproportionate use of force during demonstrations, as in the case of ill-treatment towards peaceful demonstrators, passers-by or even journalists (CHR, 2013: 23). Of serious concern too for this Commissioner is "the Spanish government (…) long-standing practice (…) of granting pardons to members of law enforcement agencies involved in serious human rights violations, including torture" (2013: 3), which "sends the entirely unacceptable signal that abuse committed by law enforcement officers may be tolerated and may remain" (2013: 27). Indeed, as pointed by Brandariz García and Faraldo Cabana (2015: 209), 47 police officers convicted of torture obtained that exceptional pardon in the period 1998-2012. The visit of the CPT in the Spring of 2011 calls, among other things, for the investigation of allegations of coercion and ill-treatment, which are systematically ignored (CPT, 2011: 15). Also worrying is the discriminatory practice of racial profiling, which has called the attention of different international bodies (e.g., European Commission against Racism and Intolerance, 2011; CHR, 2013).

The theme of torture in Spain inevitably springs the memory of Francoist police to mind. The terms Political-Social Brigade (the secret police in charge of persecuting and repressing opposition movements) or *los grises* (the grey ones, for the color of their uniforms) still remain in the Spanish collective imaginary. Obviously, police corps have evolved greatly since the end of the dictatorship in Spain. However, some of their features seem closer to a black-and-white Spain rather than a 21st century democracy. This should not come as a total surprise. The Amnesty Law promulgated in 1977 guaranteed impunity for perpetrators of Francoist atrocities. Police officers of repressive agencies as the Political-Social Brigade, even those who had tortured civilians, were not purged but instead remained in the policing apparatus (Alcántara, 2022). This was part of a Transition to democracy which meant neither the total rupture with the previous regime nor the partial inversion of social hierarchies present

in cases as Portugal (Fishman, 1990, 2011; Fishman and Lizardo, 2013; Bernat and Whyte, 2020).[6] Therefore, even if the *old school* police officers from the dictatorship may have been generationally replaced at this point in time, their practices may have been kept alive in the structure of the system.[7] This imprint of the previous regime can be clearly observed in the tension between policing and the right to demonstrate, which has led the CHR to urge Spanish authorities "to ensure that the activities of law enforcement agencies in the context of demonstrations do not infringe on the rights to freedom of expression and freedom of peaceful assembly" (2013: 29). Emblematic of this issue too is the prohibition of demonstrating in front of the Spanish Parliament (Fishman, 2011: 249).

Not only did the state of the issue seem not to improve in the last decade but indeed got worse with the passing of the so-called *Ley Mordaza* ("Gag Law") in 2015, which an article of the New York Times referred to as an "ominous law (…) harken(ing) back to the dark days of the Franco regime" (2015). The present law of public safety has been broadly criticized by international organizations such as the United Nations (El Huffington Post, 2015), given the backlash it entails for the exercise of freedom of expression. This law has been very controversial in many of its points, such as the skyrocketing fines for protesting in front of the Parliament and other government buildings. Furthermore, the prohibition of "unauthorized" use of images of the police (making it more difficult to document police abuses) and the strong

[6] I argue that this may have also contributed to the persistence of police views across time (see section C.1 in the Appendix).

[7] Furthermore, even in the presence of intergenerational replacement and the absence of structural factors, there might be a skew towards far right/authoritarian ideologies among individuals in institutions which played a key role during the Francoist dictatorship (i.e., through self-selection of individuals). Indeed, disproportionate support for far-right parties among individuals in military occupations and police forces in Spain has been demonstrated by Villamil et al. (2024). It is not hard to imagine this to be particularly pronounced among anti-riot squads, a phenomenon which could help explain the high levels of police brutality in Spain.

presumption of trustworthiness of police reports have "weaken(ed) the position of the defendants vis-à-vis the state" (European Commission for Democracy through Law, 2021: 23).

Figure 3.2: Support for how the police usually act

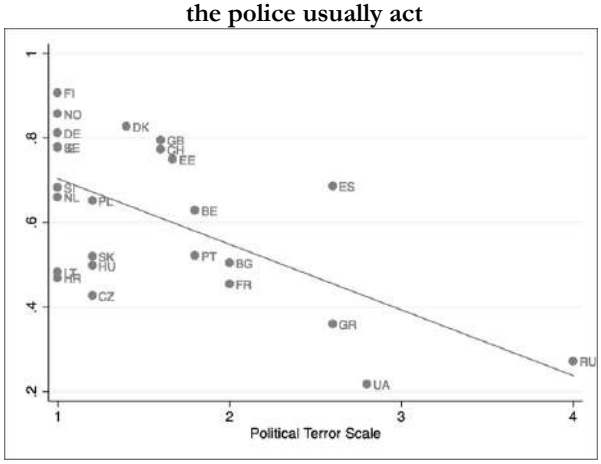


Figure 3.3: The police stand up for values important for people like them

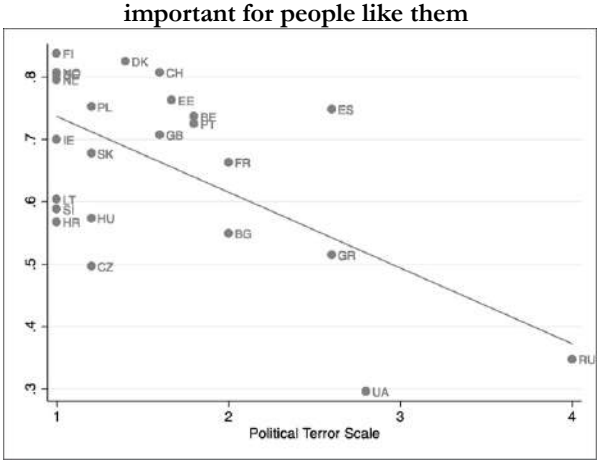


Figure 3.4: The police have the same sense of right and wrong as them

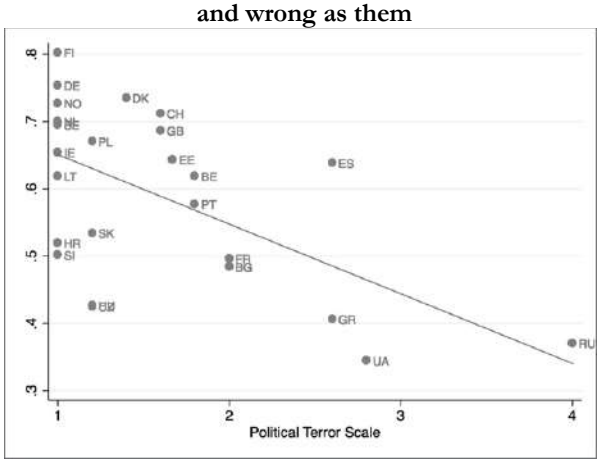
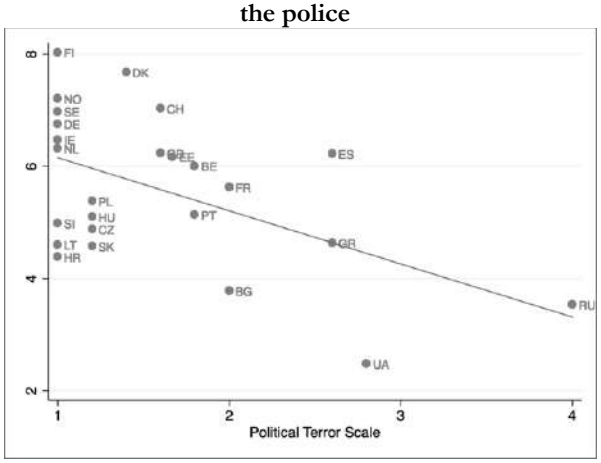


Figure 3.5: Trust in the police



Country comparison of attitudes towards the police and state-sanctioned violence (e.g., police brutality). All figures show in the x-axis the average score in the Political Terror Scale (Amnesty International's scores) for the period 2007-2011. Figures 3.2-3.4 show in the y-axis the proportion of respondents who either agree or strongly agree with three different statements about the police. Figure 3.5 displays in the y-axis the average level of trust in the police in a 0-10 scale. Attitudinal data from the 5th round of the ESS (2010-2011).

Given the repressive role that the police play in Spain, one would expect a not very welcoming reception of this institution on the part of the majority of the citizenship. Looking at the 5th round of the ESS, which includes a special module measuring attitudes towards the police, it can be noted that 69% of Spanish respondents generally support how the police usually act.[8] Is

[8] Namely, they agree or strongly agree with the statement "I generally support how the police usually act".

that a high or a low number? Aiming to respond this question, let us compare this with the figures of other European countries.

Figure 3.2 displays in the vertical axis the proportion of individuals in a given country who support how the police usually act. In the horizontal, it is shown a measure of state-sanctioned violence (e.g., police brutality): the Political Terror Scale (PTS).[9] The scale ranges from 1 to 5, with larger values indicating more repression. Then, this graph makes an international comparison of empirical and normative legitimacy of the police (Hough et al., 2013).

As can be seen, support for the police is unsurprisingly a decreasing function of the level of repression. Because Spain (ES in the graph) appears as one of the most repressive states in the sample—only Ukraine and Russia rank higher—one would expect Spanish society to hold very negative views of the police. But that is not the case at all. Spain is an outstanding exception in the sample, one in which large levels of repression translate into positive assessments of the police. In Greece, which presents the same level of repression as Spain, only 36% of people support how the police usually act, which is almost half of the Spanish number.

This phenomenon is not limited to the item in Figure 3.2. If looking at measures of moral alignment with the police, the issue is even more striking. Specially in Figure 3.3, where it can be seen that three out of four Spanish respondents consider that the police stand up for values that are important to people like them. This places Spain close to the attitudes of Nordic countries, which present very low levels of repression. Figure 3.4 shows the proportion of respondents who consider that the police have the same sense of right and wrong as them. Finally, Figure 3.5 shows average levels of trust in the police. Again, trust is a decreasing function of state-sanctioned repression, with Spain as an outstanding exception. What might

[9] I show the average of the Political Terror Scale (Amnesty International's scores) for the five years preceding the 5th round of the ESS (2007-2011).

explain the exceptionality of Spain? Why does a country with so reduced normative legitimacy have so heightened empirical legitimacy? I propose a response to that puzzle in the next section.

The *verticalization* of Spanish society

The Spanish Civil War (1936-1939) was the result of a failed coup organized by senior members of the armed forces in July 1936 against the democratically elected left-wing government that won the elections of February 1936 in the context of the Second Republic (1931-1936). This mostly *conventional* civil war took place between two main blocs mainly divided among the left-right cleavage. The Nationalist rebel forces, commanded by General Francisco Franco, confronted the Republican forces, formed by army officers loyal to the Republic as well as by militias of political parties and trade unions. At the international level, the Nationalists had the support of fascist Germany and Italy, whereas the Republicans received support from the International Brigades formed of foreign volunteers. The war ended with the victory of the Nationalists in April 1939 and the establishment of a dictatorship which lasted for almost forty years.

The bloody character of the Spanish Civil War can be felt in the figures of men and women executed far from the battlefields. Around 200,000 civilians were murdered during the Civil War and its aftermath as victims of political repression, with the numbers of lethally repressed by the Nationalists being about three times those of the Republicans (Preston, 2012). However, Francoist repression did not end with the war. The goal of eliminating any residual of political opposition continued during Franco's regime.[10] The Francoist project had the ultimate goal of *purifying* Spain from the anti-Spain *diseases*, which were the main objectives

[10] It has even been speculated that Franco could have intentionally prolonged the war with the objective of eliminating his enemies (e.g., Preston, 1994)

of Francoist repression.[11] These included but were not limited to leftists, ethnic minorities such as Basques and Catalans, and of course any minority which did not fit in the Franco agenda (e.g., queers). This project aimed a return to the purity of *the glory old days* of the Spanish fatherhood and its "traditional values of Catholicism, family and order" (Pinto, 2004: 650).[12] An order which had to be maintained through the *verticalization* of society, for in this way overcoming *the chaos and anarchy of the Second Republic*.

Richards (1998) and Vincent (2007) argue that one of the defining features of the Franco regime was the quest of a strongly hierarchical organization of society. Vincent (2007) claims that Francoism was a project directed to *the good Spaniards*, in other words, "las personas de orden" (the people of order). Indeed, order, discipline, hierarchy, harmony and unity were the watchwords of the new state (5). These were claimed requisites for keeping the peace that the Second Republic had allegedly destroyed thorough a partial inversion of hierarchies and an egalitarian understanding of social relations.

The *Crusade* against the anti-Spain had not finished with the end of the Civil War, as internal and external enemies were still lurking. Then, Franco became the sentinel of Catholicism and conservatism against the egalitarianism present in post-war Europe. In the regime's view, horizontal liberal capitalism—and, of course, Marxism—had contaminated the purity and the unity of Spain (Richards, 1998: 72). Order had been bastardised by the Republic. But also by decades of liberalism, with the notable exception of the dictatorship of General Primo de Rivera, which acted as a temporary brake on the chaos (75). That order needed to be restored at all costs for a return to the *true* Spain, to the path of *positive progress*, free of Marxism, Masonry or the modernity, to name a few enemies of Spain, according to the regime

[11] The use of medicine vocabulary was very common in the Nationalist discourse to talk about the cleansing of Spain during and after the Civil War (Richards, 1998).

[12] The vocabulary of the Nationalists revolved too around the Crusades and the Catholic *Reconquista* of Spain from the Moors and the Jews (Richards, 2002).

(Del Arco Blanco, 2009). For that business to be successful, verticality had to be present in all aspects of society, as for example in the sole legal trade union known as the Vertical Syndicate, a corporatist and harmonicist approach to the relationship between labour and capital (Foweraker, 1989).

The aim to indoctrinate Spanish society in the blessings of strong verticality was of course not limited to adults. Pinto (2004)'s discourse analysis of an elementary school textbook of the Francoist period highlights the regime's obsession with order and hierarchies. With regard to the *form* of transmission of the message, these books employed an authoritative discourse by which "a monologue from above (…) imposes itself on the subordinate" (653). This is a one-way transmission which not only does not need a response, but which actually does not allow one. The imposition of a "Fascist rigid ideology" (649) was present of course also in the *content* of the textbook. Authority, obligation and duty were key ideologemes of these textbooks, with abundant references to a vertical, ordered and disciplined organization of the country (663-664). The textbook under analysis is full of numerous examples of slogans underlining a "repetitive image of hierarchy" (658): "We, the subordinate ones, do not have any mission except obeying. We must obey without arguing", "I promise to be a disciplined child, orderly and studious. I promise to yield myself completely, integrally, to the orders of my teacher, my parents and the Authorities", "Let's obey him [Franco] in order to make Spain happy", or "Let's carry out faithfully what has been ordered". Pinto (2004) claims that this political discourse was ultimately the voice of Franco himself, who explicitly expressed these ideas in some of his discourses too: "(T)he principles of authority, order and hierarchy will be necessary to be restored with all severity" (Franco, 1939: 264).

I argue that the Francoist effort to indoctrinate in the benefits of strong hierarchies may have had an impact on attitudes towards highly vertical institutions such as the police, often

perceived to be the guarantors of order and stability.[13] In the context of the Francoist period they were seen as the ultimate barrier against the anti-Spain diseases and so too against a new war. Namely, police forces were perceived to act as guards against the *chaos* brought by the Republic, which had, in the regime's discourse, irretrievably culminated in a bloody and painful Civil War.

But why was the regime successful in *verticalizing* society, namely in making Spaniards hold a more vertical conception of society? For responding this question, it is important to understand the reaction of the Spanish society during the dictatorship. These were long decades of deep silence for Spain (Richards, 1998, 2002). Calvo Vicente (1995) claims that the level of consensus during the Franco regime was much greater than usually thought. It was maybe not a fervent and active acceptance of the regime among the broad population, typically a characteristic of more traditional fascist regimes such as those of Italy or Germany (Del Arco Blanco, 2009). However, it was a resigned acceptance—if anything, a silent rejection which did not translate into opposition to the regime—achieved of course largely through the threat of repression but also through other means (Cazorla-Sánchez, 2005; Del Arco Blanco, 2009; Sevillano Calero, 2003).

One of the most significant factors for explaining this broad consensus—or at least the lack of opposition—was the threat of a new conflict. Spaniards accepted—or at least did not broadly oppose—the regime not only because of the fear of political repression, but also, and relatedly, because they were afraid of the war repeating itself (Cazorla-Sánchez, 2005; Vincent, 2007). In that context, Franco became the "guarantor of both peace and stability" (Cazorla-Sánchez, 2005: 504). The memory of a bloody civil war and all the pain and death it had caused

[13] Note also that police forces have a hierarchical functioning—internally but also externally in the treatment of citizens—which might be more easily accepted and taken as natural by people who have a more vertical view of the world.

were very alive in the collective worldview (Molinero and Ysàs, 1998). The war had been framed by the regime as an inescapable consequence of the chaos brought by the Republic (Sevillano Calero, 2003), a fratricidal war between Spaniards that would never have to be repeated.[14] The desire for peace in society at any cost was very strong, so much that even during the war many individuals who were not natural supporters of the Nationalists wished a quick victory of the bloc seemingly most likely to win, just to end the carnage (Cazorla-Sánchez, 2005: 507). As noted by Richards, there was "an understandable preference for stability, and many accepted that this would be achieved in part coercively" (2013: 105). The phenomenon of a broad desire for peace was noted too by the US ambassador (Cazorla-Sánchez, 2005: 514) and the Francoist information services: "[Spaniards] just want order and a sense of authority" (512). Even if a great deal of the desire for peace could have been genuine, the regime fueled the idea of a peace-guarantor Franco through, for example, the comparison of a peaceful Spain with the images of a devastated European continent in the newsreels (515). Then, Spaniards broadly adhered to the regime's discourse on the necessity of order and stability for avoiding more tragic events, an order that only strong and vertical institutions could achieve.

This theoretical argument I present does not imply, however, that Francoist indoctrination was necessarily successful at every level. For example, recent findings suggest that repression was not fruitful in moving citizens towards right-wing rather than left-wing parties (Balcells, 2012; Domènech and Sánchez-Cuenca, 2022; Martínez, 2024; Rodon, 2022; Villamil, 2021a).[15] Many citizens could have felt that right-wing policies (in a broad sense) were not strictly necessary while embracing the specific narrative about a more vertical society being *sine qua non* for sustaining what Spanish people had longed for: peace. A similar

[14] For the regime, this discourse was also a way to get rid of the responsibility of the war.

[15] Actually, this is not so surprising once one takes into account that the regime aimed to indoctrinate too in the rejection of the party system, which was blamed to fuel the fights and hostilities between good Spaniards from all social classes (Cazorla-Sánchez, 2005: 515; Franco, 1939: 223)

argument about the avoidance of conflict and the yearning for stability is made by Rodon and Tormos (2023). These authors find that support for secession in Catalonia is a decreasing function of municipality's Francoist repression. As secession implies a rupture of the status quo and a likely break of social harmony, those who experienced more closely the pain of the Civil War prefer to stick to positions that minimize the risk of conflict and maximize stability in society. Blair (2022) observes, for the case of Liberia, that wartime violence prompts fears of civil war recurrence and heightens demand for state-led security provision. Likewise, and as I develop in next section, I claim that those in places which experienced more Francoist repression will better tolerate institutions in charge of maintaining societal order and harmony, such as the police.

Exploiting variation in Francoist repression across Spanish regions

Bateson claims that "living in a state of constant fear made individuals more receptive to the [Guatemalan] military's propaganda, as they were desperate for some narrative to make sense of the chaotic, unpredictable violence surrounding them" (2017, 643). Likewise, I claim that those Spaniards who experienced more fear better accepted the regime's discourse and were socialized more easily in obedience to authority. Potential dissidents who were neither killed nor exiled were paralyzed by terror (Del Arco Blanco, 2009). As claimed by Calvo Vicente, "violence allowed for the neutralization of the enemies (…) and opened the door to a wide use of new methods of propaganda" (1995: 150). Coercion acted so as an inhibitor of dissenting behaviours, even as a booster of consensus (Calvo Vicente, 1995: 149).

I propose looking at regional variance in Francoist repression during the Civil War in order to quantitatively approach the thesis of this paper. Even if the verticalizing efforts of the regime may have been relatively constant across Spanish regions, I claim that its success in socializing citizens in obedience to authority was contingent on the levels of victimization during the war and its subsequent fear of continued repression. Fear of repression had a

paralyzing effect (Calvo Vicente, 1995; Del Arco Blanco, 2009; Molinero and Ysàs, 1998; Sevillano Calero, 2003; Young, 2019) that eased indoctrination in the necessity of hierarchies as a way of reducing the probability of the violence repeating itself (Blair, 2022). Moreover, fear of repression made people more risk averse (Young, 2019), and that intertwined with the memory of the Civil War itself (Molinero and Ysàs, 1998). In other words, this is not simply to say that fear had a paralyzing effect making those in more repressed areas more receptive to any aspect of the regime's discourse—otherwise people in more repressed areas would be more rightist today, contradicting established findings in the literature (e.g., Domènech and Sánchez-Cuenca, 2022; Rodon, 2022; Villamil, 2021a). Fear of repression was successful at making Spaniards more agreeable to institutions such as the police because, by making them more risk averse (Young, 2019), they better accepted institutions perceived to bring order and decrease the risk of the violence experienced during the Civil War repeating itself. Moreover, I claim that having experienced repression not only made individuals more risk averse per se (i.e., less reluctant to accept risk), but also made them more aware of the negative consequences of the bad outcome: a new war. Namely, they were not only less tolerant of the risk of violence repeating itself, but also perceived that, if that happened, the consequences would be worse compared to the consequences perceived by those in less repressed areas.

Consistent with these ideas, I will show in Figures 3.11-3.14 that individuals in more repressed areas have a larger desire for obedience and security in society, hold materialist rather than post-materialist values, and prefer a more stable and less risky life.

As summarized in the path diagram (Figure 3.6), I claim that indoctrination in the necessity of strong hierarchies and obedience to authority permeated much better regions which experienced higher levels of Francoist political repression, as in those places fear of repression,

heightened risk aversion and fear of a new conflict intertwined provoking a better acceptance of institutions in charge of maintaining order and stability.[16]

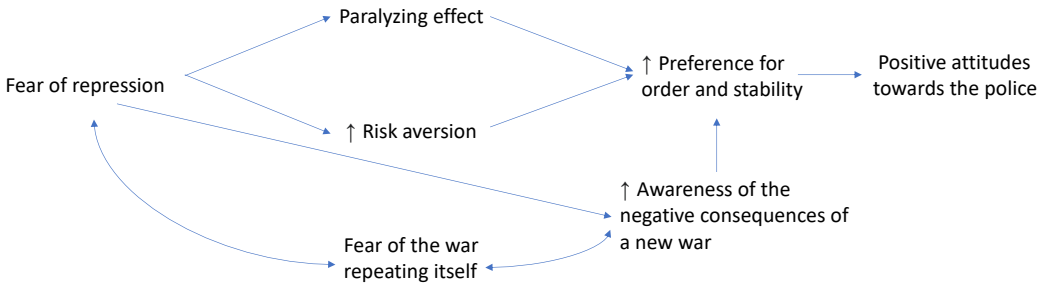


Figure 3.6: Path diagram illustrating the relationship between fear of repression, threat of a new conflict and attitudes towards the police.

Before proceeding to the empirical section, it is worth briefly discussing the scope conditions of the theory I propose, as this may also help clarify the mechanisms at play. I expect civilian victimization to foster positive attitudes towards the police *only* in the context of civil wars—specifically, long and bloody conflicts in which civilians were surrounded by pervasive levels of chaos and violence, such that fear played a major role in their socialization. Beyond Spain, cases such as Guatemala (analyzed below), Nicaragua, and El Salvador may fit this pattern. In these settings, fear of a return to war made citizens more willing to trade liberties for security and more likely to view order-keeping institutions positively.

By contrast, I do not expect victimization to produce positive attitudes—indeed, they could be negative—in contexts outside civil war, such as after a successful coup that did not develop into a war. Cases like Argentina or Chile may approximate this pattern. In these cases, repression by the state may not have been perceived as necessary to stop a war, and the police forces may instead have been seen simply as perpetrators of the violence rather than guarantors

[16] The connection between repression and subjugation to the regime's discourse is supported in the literature, for example, by the findings of Tur-Prats and Valencia Caicedo (2020): streets commemorating Franco's dictatorship are more common in places which suffered more repression during the war.

of stability. Even if the military sought to justify their actions invoking supposed social chaos, the overall levels of violence were far lower, undermining the credibility of that rhetoric.

Empirics

I conduct three different analyses in which I predict individuals' attitudes towards the police with Francoist repression. Each of them defines repression at a different level of geographical aggregation and employs a different dataset, aiming to improve data triangulation. The first is a fine-grained analysis at the municipality level, employing a set of opinion polls from the Catalan ICPS which include an item asking respondents to evaluate police performance.[17]

However, addressing only Catalan municipalities may rise concerns about the generalizability of the results to the rest of Spain. Therefore, employing data from CIS, I conduct a second analysis covering all regions in Spain and not just Catalonia. These surveys include an item measuring respondents' rating of the police profession and have information on the province (NUTS 3) of residence of respondents.[18, 19] Moreover, this analysis allows me to control for variables which are not available at the municipality level (e.g., development of the civil society in the XIX century).

Finally, I carry out an exploratory analysis employing data from the 5th round of the ESS, which contains a rich battery of attitudes towards the police. Although the region of residence of respondents is measured in this dataset at a level of disaggregation which is not optimal (autonomous communities/NUTS 2), this analysis aims to test whether there exists a

[17] Until 2007, the survey asked separately about the two existing national police forces: the Police and the *Guardia Civil*, which has a more rural focus. From 2008 onwards, they were unified under the term National Security Forces. Then, for responses up to 2007 I take the average of the evaluations of the Police and the *Guardia Civil*.

[18] Spain is divided in 50 provinces.

[19] I have selected those CIS surveys including respondents' rating of the police profession, which corresponds with years 1997, 1998, 1999, 2000, 2002, 2005, 2007, 2009, 2011, 2013, 2015, and 2017.

relationship between Francoist repression and a broad set of attitudes towards the police (Jackson et al., 2011).[20]

Data on repression

For measuring Francoist repression, I resort to two different sources. For municipal figures of repression in Catalonia, I resort to data from Solé i Sabaté and Villarroya i Font (1989) and "The list of juridical repair of victims of Franco's regime" (Generalitat de Catalunya, 2017) employed in Rodon and Tormos (2023), which includes the number of people repressed during the Civil War and the post-war period, that is, the number of executions as well as people imprisoned up until 1945 and other repressive measures such as sanctions. For the NUTS-3 and NUTS-2 analyses, I use data from Ledesma and Espinosa (2020), which includes the number of killings during the Civil War and the post-war period.[21] The main explanatory variable in all analyses is defined as the number of repressed individuals as a percentage of the pre-Civil War population.[22,23]

Individual-level controls

I include in all the analyses several individual-level controls often appearing in the criminological literature explaining attitudes towards the police (e.g., Bradford and Jackson, 2018; Brown and Benedict, 2002; Gerber and Jackson, 2017; Liu and Crank, 2010; Medina,

[20] Spain is divided in 17 autonomous communities.

[21] I use the updated figures employed by Domènech and Sánchez-Cuenca (2022).

[22] Analyses on Catalan municipalities take the population figures from the 1936 census. NUTS-2 and NUTS-3, from the 1930 province census.

[23] For a discussion on the measurement of political violence either through direct records of victimization or through proxies such as mass graves, see Villamil (2021b). Taking data from Spain, the author highlights the biases that the use of the latter would introduce.

2003; Nix et al., 2015). These include measures of age, gender, education, income and left-right self-placement.[24,25]

Regional-level controls and the endogeneity of repression

Regional levels of repression during the Civil War were of course not random (e.g., Balcells, 2011, 2017; Herreros and Criado, 2009). Therefore, the effects found below could be argued to be the result of some variable correlating with Francoist repression and attitudes towards the police. I aim to overcome this weakness in at least two ways. First, by showing that the relationship between Francoist repression and police views holds for very different levels of aggregation of the variable measuring repression, such as municipalities, provinces and autonomous communities. Even if that obviously does not fully solve the problem, it makes more difficult to argue that some contextual variable working at very different regional levels is driving the results.

Second, I control for several factors addressed in the literature. Scholars such as Balcells (2011, 2017) and Herreros and Criado (2009) have aimed to respond the question about the factors which explained varying levels of repression across Spain during the Civil War. Within the broad literature on the determinants of violence against civilians in civil wars (e.g., Kalyvas, 2006), the conventional nature of the Spanish Civil War sets the response to this question far from issues of military competition. In other words, the incentives of armed groups in conventional civil wars—in which the warring factions have hegemonic control on their territories behind the frontline—are different to those in non-conventional civil wars, in which control is fragmented and there is thus competition to gain territory.

[24] For the municipality analyses, I do not control for income, as the ICPS does not have a consistent measure of it.

[25] Ideological placement can be plausibly considered as a post-treatment variable. For that reason, I have checked through mediation analyses that the effect of repression does not work through ideology, which may have been affected by repression (e.g., Balcells, 2012).

Balcells (2011, 2017) finds that places where there existed more electoral parity before the onset of the war (i.e., the left and the right were closer to a 50-50% distribution) experienced higher levels of direct violence. In these places, violence could easily alter the local political balance, such that civilians aligning with the armed group ruling the territory had more incentives to collaborate with it in the elimination of strong supporters of the competing faction—rather than restrain it from the use of violence. Whereas Balcells (2011, 2017) applies this rationale to explain repression of both warring factions, Herreros and Criado (2009) claim that repression followed different logics in the Nationalist and the Republican rearguards. While Francoist repression was pre-emptive and had the strategic goal of eliminating political opponents and minimize potential future rebellions, violence in the Republican side did not follow a terrorist strategy but was the consequence of the collapse of the state. Then, according to Herreros and Criado (2009), Francoist repression was larger in provinces where the left obtained more votes in the pre-war elections.[26]

Domènech and Sánchez-Cuenca (2022) connect historical agrarian cleavages with leftist vote nowadays. Provinces that were more unequal in the XIX century display today higher levels of support for the left. One of the mediators found to explain this relationship is Francoist repression. Namely, regions with higher agrarian inequality experienced larger levels of repression which, in turn, moved preferences of voters in those places to the left.

With the findings of these papers in mind I control for measures of pre-war political competition and leftist vote, as well as of agrarian inequality in 1860—the latter only in

[26] Herreros and Criado (2009) find popular opposition to the coup d'état to be a relevant factor to explain varying levels of repression too. They construct a categorical variable indicating whether the coup triumphed without opposition, triumphed with strong opposition, or failed. As this variable is missing for some provinces, I have checked for the robustness of the results to the inclusion of this variable, but instead I am showing results when controlling for a dummy indicating whether the province remained loyal to the Republican government (Domènech and Sánchez-Cuenca, 2022).

analyses of provinces, as this variable is not available at the municipality level for Catalonia.[27] Additionally, for measuring pre-war grievances, I control for the intensity of anti-clerical violence (Domènech and Sánchez-Cuenca, 2022).[28] The inclusion or exclusion of these controls in the different analyses are a matter of availability at a given geographical level.

Aiming to capture pre-war horizontality

Regardless of the ideological composition of territories, it could be the case that those regions with a more horizontal—as opposed to vertical—view of society experienced less Francoist repression.[29] If that was the case, the positive relationship between political violence and perceptions of the police could be just an artifact of a pre-war phenomenon, namely that individuals in more horizontal societies may hold more negative attitudes towards hierarchical institutions such as the police. Aiming to rule out this explanation, I propose two variables aiming to capture how horizontal regions were before the war. For analyses of municipalities, I look at the per capita number of clerics at the beginning of the XX century, as in Aguilar et al. (2024). This aims to capture variation in the presence of a very hierarchical institution—the Catholic Church—in the municipality. For analyses of provinces, I proxy the level of development of civil society in 1867 by the per capita number of cafes (Arnabat Mata, 2019).[30]

Models

I run OLS models with robust standard errors clustered at the level of geographical aggregation of the repression variable (i.e., in analyses of municipalities, clustered at the municipality level). When the analysis includes more than one survey round—as in CIS and ICPS analyses—I include round fixed effects.

[27] Electoral data from Álvarez Tardío and Villa García (2017). Data on agrarian inequality from Domènech and Sánchez-Cuenca (2022).

[28] Clergymen were one of the main targets of leftist violence in this period.

[29] For example, if people in those places collaborated less with the regime in the exercise of repression.

[30] Data at the province level from the National Statistics Institute of Spain (INE).

For analyses of municipalities, ICPS has information on whether respondents were born in Catalonia or in a different part of Spain. Then, I exclude those individuals born out of Catalonia, as in Rodon and Tormos (2023), in order to address a better level of correspondence between the region of residence of respondents and the region where their ancestors spent the Civil War and the post-War period. I do not have a similar item in the other two datasets employed.

As I discuss in depth in the Appendix, another strategy for increasing correspondence between the region of residence of respondents and the region of their ancestors entails focusing on those in rural areas. This is especially relevant from the point of view of a mechanism of intergenerational transmission of attitudes within the family (e.g., Lupu and Peisakhin, 2017; Wang, 2021). Moreover, the repressive atmosphere during the war was particularly stifling and traumatizing in rural settings, where the small size of communities favored a more "prisonlike atmosphere" (Richards, 2013: 106), one in which political, social and personal conflicts intertwined strongly (Arnabat Mata, 2013; Richards, 2002, 2013). Consistent with these ideas, I show in section C.3 in the Appendix a stronger impact of repression for those in rural areas.

Results: Repression defined at the municipality level (ICPS data)

Results to be presented now measure repression at the most fine-grained possible level, that of municipalities, therefore overcoming concerns about over-aggregation. Employing survey data from the Catalan *Institut de Ciències Polítiques i Socials* (ICPS), I test whether respondents in Catalan municipalities which experienced more Francoist repression during the Civil War and the post-war period hold nowadays better views of the police.[31]

[31] I cannot perform this analysis for other Spanish regions due to data availability. Note that for ESS and CIS analyses I have no information on respondent's municipality.

Although this analysis does not cover all Spanish regions but only Catalonia, it presents two main benefits. Theoretically, focusing on municipalities makes the results much more credible than if looking at more aggregated geographical levels. Empirically, by looking at municipalities I can obtain a fair number of clusters for calculating the standard errors: around 450.

Table 3.1: Repression defined at the municipality level

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Francoist repression	.073**	.074**	.08***	.078***	.134***	.078***	.068**	.06**
	(.032)	(.03)	(.03)	(.029)	(.034)	(.029)	(.029)	(.028)
Individual-level controls								
Age	.011***	.011***	.011***	.011***	.01***	.011***	.011***	.011***
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
Left-right scale	.471***	.472***	.471***	.472***	.476***	.471***	.472***	.403***
	(.027)	(.027)	(.027)	(.027)	(.031)	(.027)	(.027)	(.026)
Male	-.233***	-.233***	-.233***	-.233***	-.227***	-.229***	-.234***	-.223***
	(.033)	(.033)	(.033)	(.033)	(.037)	(.033)	(.033)	(.032)
University degree	-.335***	-.33***	-.33***	-.33***	-.303***	-.328***	-.33***	-.284***
	(.049)	(.048)	(.048)	(.048)	(.05)	(.048)	(.048)	(.044)
Support for independence	-	-	-	-	-	-	-	-1.018***
								(.036)
Municipality-level controls								
Vote for the left in 1936	-	.663***	-	.506**	.522*	.475*	.583**	.378
		(.22)		(.251)	(.312)	(.251)	(.252)	(.233)
Electoral competition in 1936	-	-	-.648***	-.354	.153	-.321	-.342	-.215
			(.219)	(.256)	(.318)	(.26)	(.257)	(.247)
Unions (CNT/UGT) during the 1930s	-	-	-	-.051	.006	-.046	-.069	-.056
				(.072)	(.081)	(.073)	(.069)	(.066)
Exiled (%)	-	-	-	-	-.003	-	-	-
					(.004)			
Clerics	-	-	-	-	-	-2.007	-	-
						(5.453)		
Leftist repression	-	-	-	-	-	-	.125	-
							(.102)	
Province and year fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Standard deviation of elevation	NO	YES	YES	YES	YES	YES	YES	YES
Observations	20,793	20,785	20,785	20,785	17,015	20,669	20,785	20,785
Municipalities	441	440	440	440	311	436	440	440
R-squared	.129	.13	.13	.13	.128	.129	.13	.164

OLS models. The dependent variable in all models is evaluation of police performance, ranging from 0 to 10. Robust standard errors clustered at the municipality level in parentheses. Constant not reported. Time (round) dummies included. Province dummies included. Dummies for size of municipalities included. Restricted to respondents born in Catalonia.
**** p<.01, ** p<.05, * p<.1*

ICPS data covers the period 1991-2019 and is representative of the Catalan voting-age population.[32] Municipalities in the sample employed cover more than 95% of the population in Catalonia. The indicator of attitudes towards the police in this survey asks respondents to evaluate police performance in a scale from 0 to 10.

Moreover, ICPS polls include a question on the region of birth of respondents. Therefore, I restrict the analyses to those who were born in Catalonia for better approaching the intensity of repression experienced by the individuals or their ancestors, as in Rodon and Tormos (2023).

First column of Table 3.1 shows the results when controlling for individual-level variables as well as for province and year (ICPS round) fixed effects. Respondents born in Catalonia who live nowadays in a municipality which experienced more Francoist repression during the Civil War and the post-war period evaluate more positively the police. In columns 2-4, I add some regional variables that might explain varying levels of Francoist repression.[33] These are factors often addressed in the literature, such as vote for the left in the elections preceding the Civil War (Herreros and Criado, 2009) or electoral competition between the left and the right (Balcells, 2011, 2017). I additionally include a dummy indicating municipal pre-war existence of affiliates to one of the two main labour unions—namely, the anarchist *Confederación Nacional del Trabajo*, CNT, and the socialist *Unión General de Trabajadores*, UGT—as well as the standard deviation of elevation as a measure of ruggedness of terrain (inaccessibility), associated with the logistical costs of repression (Zhukov and Talibova, 2018). As can be noted, the relationship between Francoist repression and positive evaluations of the police keeps robust to the introduction of several covariates. This is also a non-negligible

[32] Years 2003 and 2004 are not included in the analysis as I do not have information on region of birth of respondents for those two rounds in the anonymized dataset shared by ICPS.

[33] Most of these covariates from Villamil (2021a).

effect in magnitude. When examining standardized coefficients, the effect of Francoist repression (0.026) amounts to approximately a third of the effect of one of the most relevant predictors of attitudes towards the police at the individual level (age, 0.081) and is very similar to the effect of vote for the left in 1936 (0.022). Moreover, respondents in municipalities that experienced the largest levels of repression hold attitudes towards the police around 17% higher than those in municipalities with the lowest levels of repression, a figure somewhat below the one observed by Blair (2022) for the case of Liberia (29%).

In the last columns of Table 3.1, I aim to discard some plausible alternative explanations to the one I propose. Francoist repression was not only larger in municipalities electorally tilted to the left (e.g., Herreros and Criado, 2009), but also triggered exile in those municipalities (Balcells and Steele, 2016). In other words, Catalan municipalities which voted more leftist in the pre-war elections and those with a larger proportion of affiliated to the anarchist union CNT observed a higher displacement of individuals.[34] As this displacement mostly affected leftist individuals (Balcells and Steele, 2016), my results could be explained by individuals with negative attitudes towards the police fleeing more repressed municipalities. For addressing this issue, I control in column 5 for the level of displacement/exile in each municipality, as estimated by Balcells and Steele (2016) for Catalan municipalities.[35] The positive effect of Francoist repression keeps robust to the introduction of this measure of exile—also of a variable measuring population changes between 1940 and 1970—suggesting that my results cannot be merely explained by the sorting of individuals as a consequence of a war

[34] Balcells and Steele (2016) focus on the so-called Exile of 1939, which took place towards the end of the war, when the frontline was moving, and the Nationalists were conquering Catalonia. This mostly affected supporters of leftist parties and trade unions.

[35] This is calculated as "the difference in the local population censuses of 1940 and 1936, from which all those people who disappeared for reasons other than migration/exile and natural death are subtracted" (Balcells and Steele, 2016: 18). The smaller sample size in column 5 is due to this variable being missing for some municipalities.

phenomenon.[36] In column 6, I control for the number of clerics in the municipality at the onset of the XX century, aiming to capture the pre-war presence of a very hierarchical institution: the Catholic Church.

Other plausible competing explanation concerns the literature connecting political violence and threat with authoritarian values. Authors such as Dyrstad (2013) find that exposure to violence during a civil war has the potential to create more authoritarian individuals. Paralleling Inglehart's modernization theory (1977), individuals exposed to violence feel their personal security threatened, being more willing "to trade off civil liberties for greater personal safety and security" (Davis and Silver, 2004: 28). That is the thesis of Davis and Silver (2004) when studying the consequences of 9/11 terrorist attacks on Americans' attitudes towards democracy. A similar argument can be found for the Israelian case in Hobfoll et al. (2006). Violence has the capacity to raise authoritarian values [which often include issues of obedience and respect for authority (Hobfoll et al., 2006: 211)], a relationship partly mediated through heightened levels of post-traumatic stress disorder (Dyrstad, 2013; Hobfoll et al. 2006). Therefore, it is worth considering the possibility that my results are explained by this phenomenon.

For testing whether it is just political violence what is causing heightened positive views of the police in areas more intensely repressed by Franco, I add Republican repression to the equation. If repression by both warring factions exerted a positive effect on views of the police,

[36] A gloomy version of this explanation is that those with more negative views of the police were killed. However, I consider very unlikely that this phenomenon can fully explain my results for at least two reasons. First, one would expect a positive correlation between exile and killings, such that controlling for the former is somehow taking the latter into account. Second, even if Francoist repression far from the battlefields was of course harsh, it seems unlikely that it was as prevalent as to generate such an attitudinal change directly through the execution of more anti-police individuals. Interestingly, however, this explanation is, among the ones discussed in this paper– along with the one expecting a positive effect of Republican repression and the one I propose—the only which could also explain the exceptionality of Spaniards' attitudes towards the police observed in Figures 3.2-3.5.

my argument would lose credibility in favour of a more classical argument in the literature: political violence triggering authoritarian values via post-traumatic stress disorder (Davis and Silver, 2004; Dyrstad, 2013; Hobfoll et al., 2006). As can be seen in column 6, however, Republican repression is not significantly correlated with attitudes towards the police. Still, the effect of Francoist repression keeps robust to the introduction of that variable into the model. Then, political violence—by any warring faction—cannot explain by itself varying levels of attitudes towards the police across Catalan municipalities.

Finally, Rodon and Tormos (2023) construct a similar argument to the one I propose about avoidance of conflict and yearning for stability, finding that Catalans in more repressed municipalities are less likely to support secession from Spain. Then, my results could be explained by pro-independence individuals having more negative views of the national police.[37] For addressing this possibility, I introduce a dummy indicating support for secession at the individual level. Still, even after such a stringent test, I find that Francoist repression has an effect on attitudes towards the police which is independent of the effect of supporting secession.

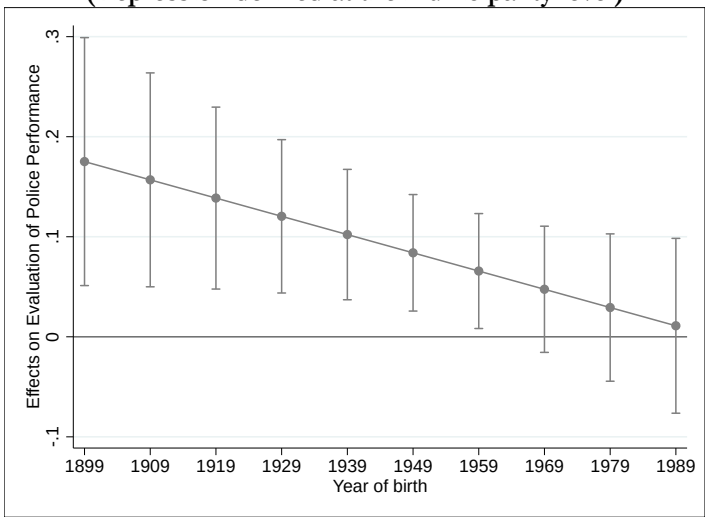
Does year of birth moderate the relationship between Francoist repression and positive attitudes towards the police?

I argue that attitudes towards the police were transmitted by generations who experienced the Civil War and its aftermath to younger generations trough socialization processes (Balcells, 2012; Lupu and Peisakhin, 2017; Oto-Peralías, 2015; Tur-Prats and Valencia Caicedo, 2020; Wang, 2021). This is indeed a common assumption in studies of historical legacies, which claim that political attitudes are transmitted intergenerationally either within the family or throughout communal networks (e.g., Cremaschi and Masullo, 2023; Zhukov and Talibova,

[37] See footnote 16 for an explanation of the dependent variable in ICPS analyses.

2018). However, even if the legacies of violence may last across generations, it is often found that its intensity weakens for more distant cohorts, thanks maybe to the impact of competing sources of socialization (e.g., Lupu and Peisakhin, 2017; Wang, 2021).

Figure 3.7: The moderator effect of cohort
(Repression defined at the municipality level)



Point estimates and 95% confidence intervals for the coefficient of Francoist repression on evaluation of police performance, for different cohorts.

For empirically addressing this issue, I check now if the effect of Francoist repression across Catalan municipalities is consistent for different cohorts or if, on the contrary, that weakens for younger generations. Figure 3.7 shows the estimated average marginal effects of repression for different cohorts.[38] The effect is particularly strong for those generations who experienced repression in first person as adults or children, as well as for their offspring. However, the effect fades out for the last generations, already born under democracy.[39,40]

[38] Results from a model equivalent to the one in column 4 of Table 3.1, in which I add as explanatory variables year of birth and the interaction of that with repression—I drop the age variable to avoid perfect multicollinearity.

[39] The effect is still significant at the 10% for those born between 1969 and 1978, a group of people raised already during the first years of democracy.

[40] Note that this descending effect could also be explained by a migration phenomenon. If younger generations of Catalans are more likely to migrate out of their municipalities, these results could be explained by lower levels of correspondence between region of residence of respondents and region of ancestors among the younger cohorts, which could bias the coefficients towards zero.

Results: Repression defined at the province level (CIS data)

I move now to the second geographical analysis of the paper, employing data from the surveys of the Sociological Research Centre of Spain (CIS). The main purpose of this analysis is trying to overcome plausible concerns about the generalizability of the findings in the previous section, as CIS surveys cover all regions of Spain and not just Catalonia, as well as to control for plausible correlates of repression which are not defined at the municipality level. CIS surveys indicate the province (NUTS 3) of residence of respondents. The dependent variable in all models of this section indicates respondent's rating of the police profession in a scale from 0 to 10.[41]

First column of Table 3.2 shows the results when controlling for individual-level variables and year fixed effects. Respondents living in provinces which experienced more Francoist repression rate more positively the police profession nowadays. In subsequent columns, I add some variables defined at the municipality level that might explain varying levels of Francoist repression, such as vote for the left in the elections preceding the Civil War (Herreros and Criado, 2009) or electoral competition between the left and the right (Balcells, 2011, 2017). In column 4, I add some variables I borrow from Domènech and Sánchez-Cuenca (2022), which may be related to Francoist repression, such as agrarian inequality in 1860 and a dummy indicating whether the province remained loyal to the Republic after the coup (Domènech and Sánchez-Cuenca, 2022). I also include the intensity of anti-clerical violence as a measure of pre-war grievances. For controlling for the level of development of regions, the

[41] Other surveys of CIS contain an item measuring trust in the police. However, I consider ratings of the police profession a more interesting indicator as it should be less affected by contextual factors such as levels of crime, better capturing broad and more abstract attitudes towards the police. Still, I do also find a positive relationship between Francoist repression and trust in the police employing CIS data. This effect is, though, weaker (but still very strong for those in rural areas), maybe because the indicator of trust is present in more recent rounds of CIS than that of rating of the profession, therefore containing fewer individuals from older cohorts who more directly experienced repression.

illiteracy rate before the war is added in column 6. As can be observed, the positive correlation between respondents' rating of the police profession and Francoist repression is very robust to the introduction of several individual and province-level controls.[42]

Table 3.2: Repression defined at the province level

	(1)	(2)	(3)	(4)	(5)	(6)
Francoist repression	.312***	.291***	.351***	.313***	.308***	.168*
	(.094)	(.1)	(.087)	(.088)	(.075)	(.09)
Individual-level controls						
Age	.004**	.005***	.005***	.005***	.005***	.006***
	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)
Left-right scale	.188***	.193***	.189***	.193***	.186***	.183***
	(.018)	(.018)	(.018)	(.018)	(.016)	(.016)
Income	-.002	-.004	.001	-.002	.003	.014
	(.014)	(.014)	(.015)	(.014)	(.013)	(.012)
Male	-.354***	-.349***	-.353***	-.349***	-.35***	-.354***
	(.029)	(.029)	(.029)	(.029)	(.03)	(.03)
Living in rural area	-.02	.067	-.006	.065	.089	.071
	(.062)	(.06)	(.063)	(.059)	(.062)	(.062)
Province-level controls						
Vote for the left in 1936	-	.017***	-	.015**	.015**	.019***
		(.006)		(.006)	(.007)	(.007)
Electoral competition in 1936	-	-	1.243*	.652	.74	.242
			(.641)	(.657)	(.578)	(.595)
Agrarian inequality in 1860	-	-	-	-	.002	0
					(.006)	(.006)
Anti-clerical violence	-	-	-	-	.3***	.209**
					(.097)	(.103)
Province loyal to the Republic in the coup	-	-	-	-	-.182	-.154
					(.136)	(.129)
Illiteracy rate in 1920	-	-	-	-	-	.01***
						(.003)
Observations	15,484	15,484	15,484	15,484	15,484	15,484
R-squared	.071	.078	.074	.079	.081	.084

OLS models. The dependent variable in all models is respondent's rating of the police profession, ranging from 0 to 10. Robust standard errors clustered at the province level (NUTS 3) in parentheses. Constant not reported. Time (round) dummies included. Dummies for five levels of educational attainment included.
**** p<.01, ** p<.05, * p<.1*

In Table 3.3, I address potential alternative explanations to the one proposed in this paper. For example, it could be the case that, regardless of ideological affinity, those places which had *a priori* more horizontal views of society received less repression, as civilians could have restrained rather than collaborated in the exercise of repression. If that was the case, the

[42] The positive effect of leftist vote and electoral competition could be capturing to some degree unmeasured repression, as those places where competition was higher and the left was more successful suffered more repression (Balcells, 2011, 2017; Herreros and Criado, 2009).

observed correlation could be just an artifact of a pre-war phenomenon, namely of places with more negative views of vertical institutions such as the police receiving less repression. Aiming to measure the level of horizontality of regions before the war, I proxy the level of development of civil society by the number of cafes (per capita) in the province in 1867 (Arnabat Mata, 2019). As expected, regions which had a more developed civil society in 1867 hold more negative views of the police nowadays. Still, even after controlling for that, I find an independent effect of repression on ratings of the police profession.

Table 3.3: Competing explanations (repression defined at the province level)

Dependent variable	Rating of the police profession						
Covariate controlled for in each model (Province-level variables except in column 5)	(1) Cafes in 1867	(2) Leftist repression	(3) Relative, close friend or acquaintance in the police	(4) Police charges during the Transition	(5) Proud of being Spanish	(6) Set of regional dummies (see text)	(7) Crime rate
Francoist repression	.199*	.341***	.329***	.262***	.145**	.256***	.343***
	(.1)	(.107)	(.105)	(.091)	(.069)	(.08)	(.089)
Covariate	-.101**	.287	.003	-1.264***	.669***	†	1.08**
	(.048)	(.244)	(.425)	(.336)	(.068)		(.45)
Observations	15,484	13,587	14,947	15,484	15,166	15,484	15,484
R-squared	.074	.071	.072	.084	.121	.093	.074

OLS models. The dependent variable in all models is respondent's rating of the police profession, ranging from 0 to 10. Robust standard errors clustered at the province level (NUTS 3) in parentheses. Constant not reported. Time (round) dummies included. Individual-level controls in all models: age, left-right scale, income, gender, dummies for five levels of educational attainment, living in rural area.

†: *the coefficients for the different regions are -.003(Coast region), -.342***(North region), -1.585***(Basque region), .024(Central region), Reference (Madrid region).*

**** p<.01, ** p<.05, * p<.1*

Furthermore, I find again in column 2 a null effect of Republican repression, the other warring faction in the Spanish Civil War, confirming at the province level that political violence—by any warring faction—cannot fully explain the correlation between Francoist repression and police attitudes.[43]

Other alternative explanation concerns the recruitment of the police. It could be the case that the regime recruited more policemen from regions which experienced more repression. If that was the case, my results could be explained by a larger presence of policemen in the social

[43] The different sample sizes when adding Republican repression into the model are a consequence of the missingness of this variable for 8 Spanish provinces.

networks of individuals in more repressed areas. In other words, if many people in a region have friends or relatives in the police, they may value this institution more positively. For addressing this issue, I construct a variable measuring the percentage of individuals in a province with a relative, close friend or acquaintance in the police forces.[44] The results in column 3 show that the effect of Francoist repression is robust to the introduction of this variable.[45]

What if the practices of the police differ still today across Spanish regions as a consequence of some Civil-War phenomena? Different police practices in the use of force, for instance, could explain varying levels of perceptions of the police (e.g., Brown and Benedict, 2002; Tuch and Weitzer, 1997). Aiming to test this idea, I resort to detailed data from Sánchez-Cuenca (2014) on the number of police charges during the Transition to Democracy, a vivid period in terms of protests and strikes in Spain.[46] Police charges correlate negatively with ratings of the police profession (column 4). However, the effect of Francoist repression is robust to the introduction of this covariate.

In subsequent columns, I include other controls which may correlate with both Francoist repression and ratings of the police profession. I deal with the possibility that those who do not identify with Spain value more negatively the police by controlling in column 5 for reported pride of being Spanish. These results are similar if controlling for reported feelings towards the national flag and the national anthem, for a dummy for those in provinces belonging to Catalonia or the Basque Country (the two Spanish regions with a substantial pro-independence movement), or for a dummy for those who consider themselves nationals of their

[44] Similar results for the repression coefficient if, for creating this variable, I do not consider those individuals who only have an acquaintance rather than a relative or close friend in the police.

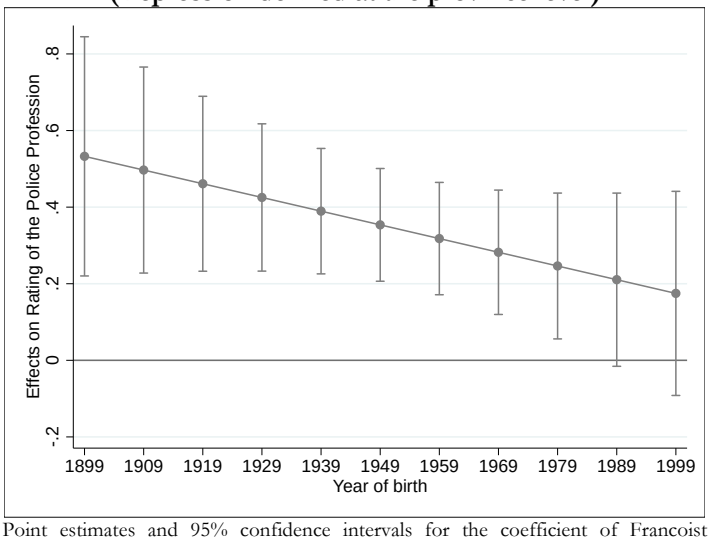
[45] The sample size is smaller in column 3 as the special module in social networks of CIS employed for creating this variable does not cover a few provinces. Also, this special module does not measure respondents' rating of the police profession either. This precludes controlling for this variable at the individual level.

[46] I divide the number of police charges in the region by population in 1971.

autonomous community and not of Spain (the latter variable being unavailable for most rounds of the survey).

Column 6 introduces an alternative set of regional dummies gathering Spanish provinces in 5 different groups (Coast, North, Basque, Central, Madrid), which take into account both a geographical rationale and different patterns of criminality across Spanish territories (Medina, 2003: 8). Finally, column 7 includes the criminality rate in a region in 2016, which may affect evaluations of the police (e.g., Bradford and Jackson, 2018; Brown and Benedict, 2002; Sherman, 2002).[47]

Figure 3.8: The moderator effect of cohort (Repression defined at the province level)



Point estimates and 95% confidence intervals for the coefficient of Francoist repression on respondent's rating of the police profession, for different cohorts.

In Figure 3.8, I show that the relationship between attitudes towards the police and Francoist repression is moderated by year of birth also when repression is measured at the province level.[48] Again, the effect is stronger for those generations who experienced repression in first person as adults or children, as well as for their offspring, while fading out for the last

[47] This is included as the number of total crimes in the province per 100 inhabitants in 2016.

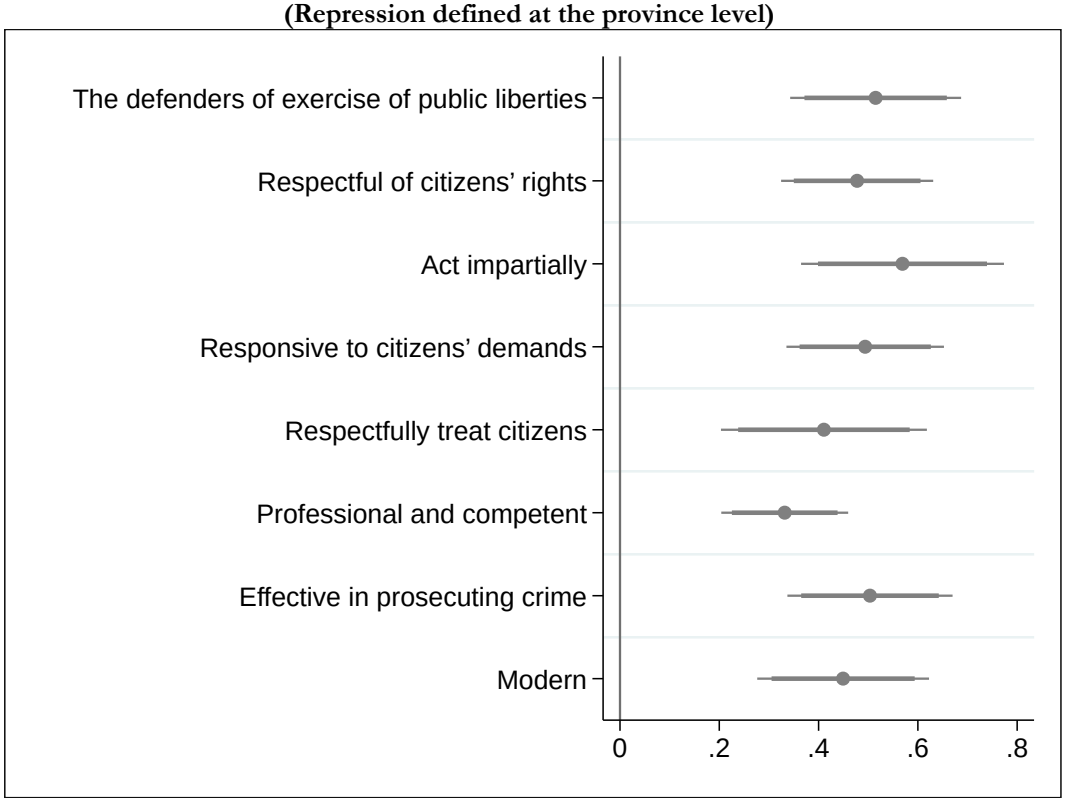
[48] Results from a model equivalent to the one in column 5 of Table 3.2, in which I add as explanatory variables year of birth and the interaction of that with repression—I drop the age variable to avoid perfect multicollinearity.

generations. This finding aligns with many works showing that the intensity of the legacies of violence weakens for more distant cohorts (e.g., Lupu and Peisakhin, 2017; Wang, 2021).

More attitudes towards the police

Employing now a special module about the police carried by CIS in 1995, I show that repression correlates with numerous attitudes other than ratings of the police profession. As can be seen in Figure 3.9, those in provinces which experienced more Francoist repression are more likely to perceive the police as guarantors of liberties and rights, as well as hold more positive evaluations of the police regarding aspects such as impartiality, responsiveness, professionalism, or effectiveness.

Figure 3.9: Francoist repression and the evaluation of different characteristics of the police (Repression defined at the province level)

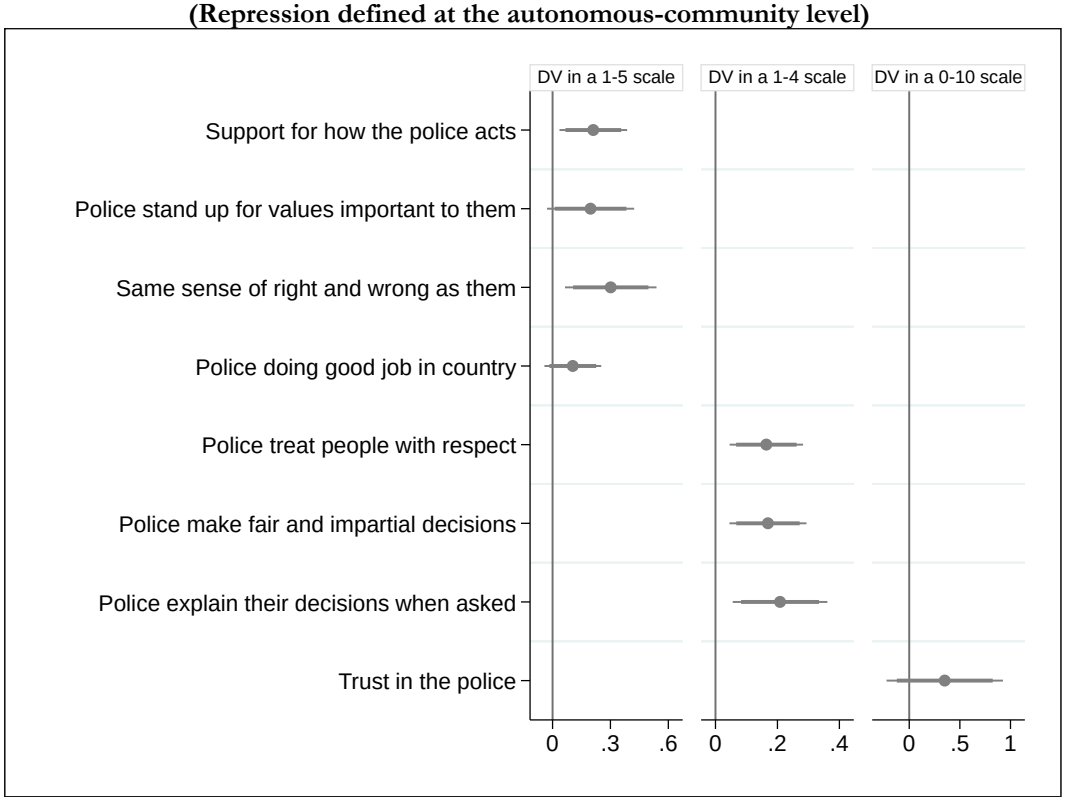


Point estimates with 90% (thick line) 95% (thin line) confidence intervals for the coefficient of Francoist repression. OLS models predicting individuals' attitudes towards the police. All models control for age, left-right scale, economic status, gender, dummies for six levels of educational attainment, living in rural area, vote for the left in 1936, electoral competition in 1936, agrarian inequality in 1860, anti-clerical violence, and province loyal to the Republic in the coup. Robust standard errors clustered at the province level. Dependent variables range from 1 to 10.

Results: Repression defined at the autonomous-community level (ESS data)

I am showing in this section the findings of some exploratory analyses employing data from the 5th round of the ESS, which includes a special module on the justice system (see Jackson et al., 2011). This module of the ESS does not include information on the region of residence of Spanish respondents at a more disaggregated level than that of autonomous communities (NUTS 2). Still, it is worth having a look at this exploratory analysis to see how Francoist repression correlates with other items measuring concepts such as moral alignment with the police, trust in police fairness or trust in police effectiveness (Hough et al., 2013), while also employing a different survey.

Figure 3.10: Francoist repression and different attitudes towards the police (Repression defined at the autonomous-community level)



Point estimates with 90% (thick line) 95% (thin line) confidence intervals for the coefficient of Francoist repression. OLS models predicting individuals' attitudes towards the police. All models control for age, left-right scale, income, gender, rural, and dummies for educational attainment. Robust standard errors clustered at the autonomous-community level (NUTS 2). Dependent variables in the first column range from 1 to 5, in the second column from 1 to 4, and in the third column from 0 to 10. See the Appendix for associated table of results (Table C.1) and definitions of the dependent variables (Figure C.1).

I present point estimates (with 90% and 95% confidence intervals) for the coefficient of Francoist repression in Figure 3.10. Living in a region which experienced larger levels of

Francoist repression during the Civil War is associated positively with several attitudes towards the police. People in more repressed areas are more likely to support nowadays how the police generally acts. Moreover, they are more likely to consider that the police stand up for values important for people like them, have the same sense of right and wrong as them, treat people with respect, make fair and impartial decisions, and explain their decisions and actions when asked.[49] Finally, even if the effect is not statistically significant, those in more repressed areas hold higher levels of trust in the police and are more likely to consider that the police are doing a good job in the country. These results are much stronger—and significant for every item—if focusing only on respondents in rural areas.

Francoist repression, obedience, risk aversion, and post-materialist values

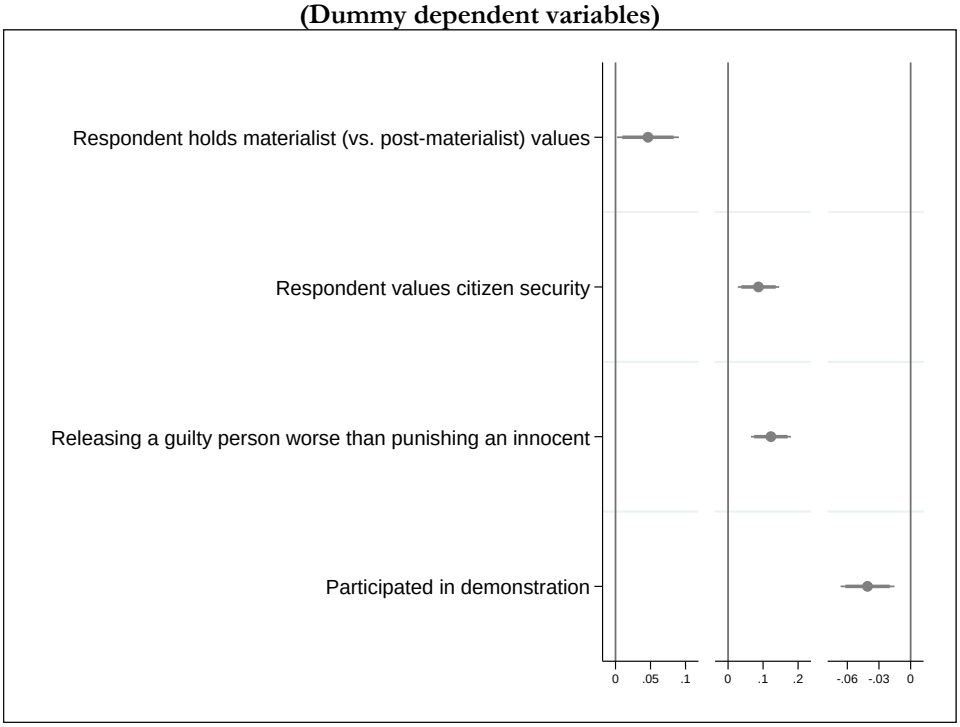
I claim that Francoist repression affected attitudes towards the police through a change in the mindset of Spaniards. This mindset would relate, among other things, to a positive understanding of obedience and heightened risk aversion (see Young, 2019), the latter translating, for instance, in a preference for life stability and the avoidance of risk (see Rodon and Tormos, 2023). Also, given the seek for stability and personal safety, they should align better with materialist rather than post-materialist values.

Aiming to test these ideas, I perform now some exploratory analyses whose results are presented in Figures 3.11-3.14, which include point estimates with 90% (thick line) and 95% (thin line) confidence intervals for the coefficient of Francoist repression on various attitudes and behaviours. In first row of Figure 3.11, I employ data from ICPS' 1993 round, which includes an item asking respondents to choose between materialist (a stable economy/a big

[49] I dichotomize these dependent variables in the Appendix (Table C.2), offering a more intuitive interpretation of the results. One more repressed individual out of a hundred in the region is associated with an increase of 10 to 15 percent in the likelihood of holding a positive view of the police. Moreover, these findings cannot be fully explained by heightened levels of general trust. A similar measure of general trust is not available in CIS and ICPS datasets.

army) and post-materialist aspects (progress toward a more human society/protecting the environment) considered to be important for a country.[50] Then, repression in this analysis is defined at the most fine-grained level: municipalities. Consistent with the argument about Spaniards' eagerness for personal safety, people in more repressed municipalities better align with materialist rather than post-materialist values.[51]

Figure 3.11: Francoist repression and various attitudes and behaviours (Dummy dependent variables)



Point estimates with 90% (thick line) 95% (thin line) confidence intervals for the coefficient of Francoist repression on various attitudes. Francoist repression defined at the municipality level in first row, at the province level in second and third row, and at the autonomous-community (NUTS 2) level in fourth row. Robust standard errors clustered at the level of geographical aggregation of the repression variable See Figures C.3-C.4 in the Appendix for an explanation of the dependent variables and controls included.

Second and third row employ data from CIS, such that repression is defined at the province level. Consistent with the previous finding, Spaniards in provinces more repressed by Franco tend to select citizen security as the most important social value, over individual freedom,

[50] "A big army" could be considered as somewhat endogenous to attitudes toward the police. Still, the results are identical if respondents selecting this aspect are excluded and only those choosing a stable economy form the materialist group—indeed, almost no respondent selected a big army as the most important aspect for a country.

[51] The effect of leftist repression on this variable is null—the coefficients are indeed negative. Again, this speaks against a more typical explanation of my results through political violence by any warring faction.

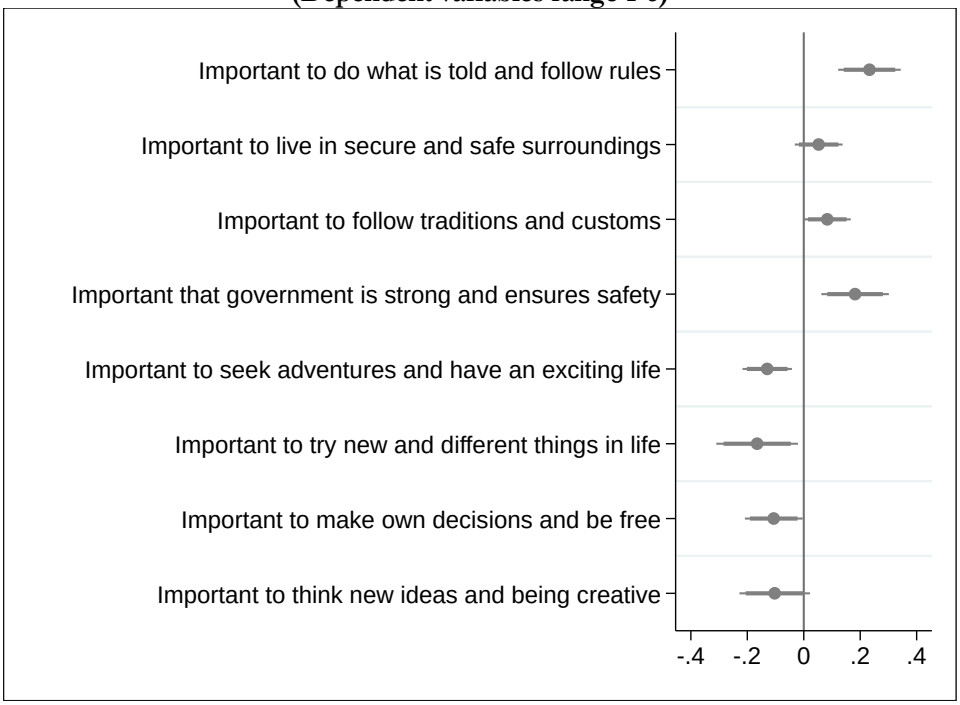
social equality or solidarity. They also seem to hold a more conservative understanding of the justice system, maybe connected to their long for security, as they consider the possibility of a guilty person being released to be worse than the possibility of an innocent person being punished.[52]

The remaining models in Figures 3.11-3.14 employ data from the ESS. As explained earlier, I do not have information in this survey about the region of respondents at a better level of disaggregation than that of autonomous communities, so these results should be taken with caution. Still, this survey contains numerous items on obedience, preference for life stability and other attitudes connected to the thesis I propose. Interestingly, I find that individuals in more repressed autonomous communities are less likely to have attended a public demonstration in the past year (Figure 3.11)—maybe to avoid participating in events perceived to be risky—sadly reproducing what Franco cynically used to say: "Do like me, don't get involved in politics".

Respondents in autonomous communities which received more Francoist repression also consider more important to do what is told and follow the rules, think that schools must teach children obey authority and that the law should always be obeyed (Figures 3.12-3.13). This is consistent with the idea of repression bringing unquestioned and blind obedience, a key point in the indoctrination agenda of the regime (Pinto, 2004; Franco, 1939). Indeed, their eagerness for security makes them more likely to accept a strong government to ensure that safety (Figure 3.12).

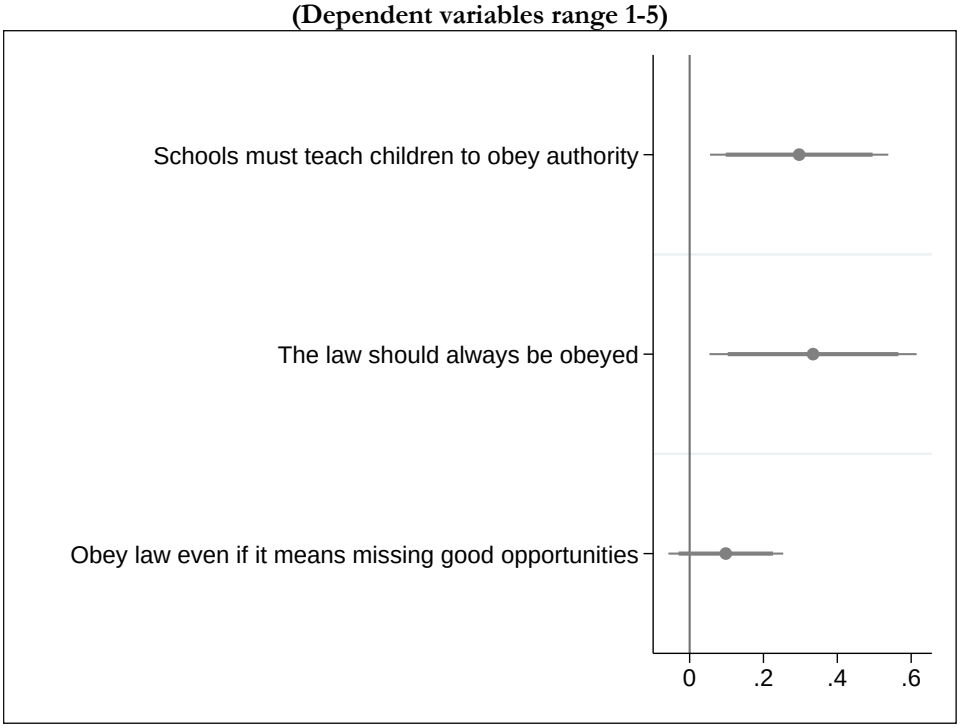
[52] Also, they better approve that a terrorist suspect should be in prison until the police is satisfied. However, by referring to the police, this measure might be endogenous to police attitudes.

Figure 3.12: Francoist repression and various attitudes (Dependent variables range 1-6)



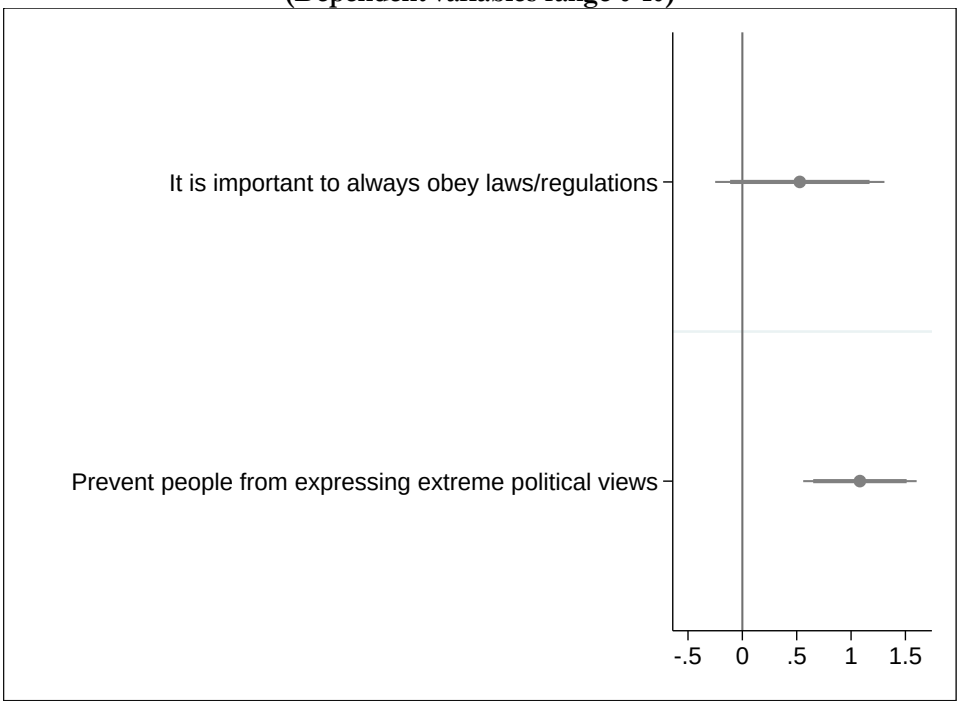
Point estimates with 90% (thick line) 95% (thin line) confidence intervals for the coefficient of Francoist repression on various attitudes. Francoist repression defined at the autonomous-community (NUTS 2) level. Robust standard errors clustered at the NUTS-2 level. See Figures C.3-C.4 in the Appendix for an explanation of the dependent variables and controls included.

Figure 3.13: Francoist repression and various attitudes (Dependent variables range 1-5)



Point estimates with 90% (thick line) 95% (thin line) confidence intervals for the coefficient of Francoist repression on various attitudes. Francoist repression defined at the autonomous-community (NUTS 2) level. Robust standard errors clustered at the NUTS-2 level. See Figures C.3-C.4 in the Appendix for an explanation of the dependent variables and controls included.

Figure 3.14: Francoist repression and various attitudes (Dependent variables range 0-10)



Point estimates with 90% (thick line) 95% (thin line) confidence intervals for the coefficient of Francoist repression on various attitudes. Francoist repression defined at the autonomous-community (NUTS 2) level. Robust standard errors clustered at the NUTS-2 level. See Figures C.3-C.4 in the Appendix for an explanation of the dependent variables and controls included.

Their personal preference for stability can also be noted in a more conservative view of the world in risk-taking terms. Those in more repressed areas are less likely to seek adventures, to try new things in life, and more likely to follow traditions and customs. Moreover, they value less creativity, as well as making their own decisions and being free (Figure 3.12). Also consistent with their preference for political stability and the status quo at the political level (Rodon and Tormos, 2023) is their larger agreement with preventing people from expressing extreme political views (Figure 3.14).

Beyond Spain: A preliminary analysis of the Guatemalan Civil War

Above, I proposed a theory linking repression during civil war to positive views of the police, due to a combination of heightened risk aversion and eagerness for stability, which make individuals exposed to civilian victimization to better accept verticalizing discourses and institutions in charge of keeping stability, such as the police. I argue that this dynamic applies

to long and bloody civil wars, but not to cases where state repression took place outside a civil war context. After examining the Spanish Civil War, I extend the analysis here to another case that approximates the theory: Guatemala. In future work, I aim to examine whether, as predicted, repression does not correlate positively with attitudes towards the police in cases such as Argentina and Chile.

Table 3.4. Civilian victimization during the Guatemalan Civil War and trust in the police

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
State-led repression	1.091***	1.278***	1.256***	1.762***	1.88***	1.888***	1.733***	1.747***
	(.319)	(.399)	(.428)	(.441)	(.488)	(.486)	(.639)	(.659)
Individual-level controls								
Age	-.002***	-.002***	-.002***	-.002***	-.002***	-.002***	-.002***	-.002***
	(0)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
Male	-.015	-.027*	-.027*	-.027*	-.027*	-.027*	-.027*	-.027*
	(.013)	(.016)	(.016)	(.016)	(.016)	(.016)	(.016)	(.016)
Left-right scale	-	.031***	.031***	.03***	.03***	.03***	.03***	.03***
		(.003)	(.003)	(.003)	(.003)	(.003)	(.003)	(.003)
Municipality-level controls								
Share of indigenous population	-	-	-	-	.058	.06	.058	.067
					(.041)	(.041)	(.041)	(.045)
Literacy rate	-	-	-	-	-.003	-.004	-.005	.02
					(.051)	(.051)	(.051)	(.071)
Share of forest cover	-	-	-	-	-	-.02	-.019	-.016
						(.055)	(.055)	(.055)
Rebels' violence	-	-	-	-	-	-	9.042	8.629
							(22.202)	(22.154)
Distance (logged) to Guatemala City	-	-	-	-	-	-	-	-.009
								(.026)
Area (logged)	-	-	-	-	-	-	-	.006
								(.016)
Year fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Departamentos fixed effects	NO	NO	NO	YES	YES	YES	YES	YES
Standard deviation of elevation	NO	NO	YES	YES	YES	YES	YES	YES
Observations	17,429	12,818	12,818	12,818	12,818	12,818	12,818	12,818
Municipalities	314	314	314	314	314	314	314	314
R-squared	.031	.041	.041	.048	.049	.049	.049	.049

OLS models. The dependent variable in all models is trust in the police, ranging from 1 to 4. Robust standard errors clustered at the municipality level in parentheses. Constant not reported. Time (round) dummies included. Dummies for size of municipalities included. Dummies for seven levels of educational attainment included.
**** p<.01, ** p<.05, * p<.1*

For the analysis of Guatemala, I depart from the work of authors such as Regina Bateson (2017) and Linda Green (1999), who highlight the socialisation in fear of Guatemalan civilians during the war, their fears of a return to la violencia (the Civil War) and the reception to the military's propaganda, issues which resonate with the theory I propose for the case of Spain. Then, I

hypothesize again a positive relationship between civilian victimization and acceptance of institutions perceived to keep order and stability. Employing survey data from *Latinobarómetro*, I find that Guatemalans in municipalities which experienced larger levels of violence during the Civil War (records of the Historical Clarification Commission and the International Center for Human Rights Investigations) hold more positive attitudes towards the police nowadays. As can be seen in Table 3.4, these results hold after controlling for several phenomena related to state-led killings at the municipality level (see Villamil, 2022). These controls include important drivers of civilian victimization (e.g., share of indigenous population), geographic factors determining wartime dynamics (e.g., share of forest cover), and geographical isolation (e.g., distance to Guatemala City). State-led repression is measured as the number of state-led killings divided by the population of the municipality in 1973.

Discussion

Despite long-standing issues of ill-treatment (i.e., CHR, 2013; CPT, 2011), the Spanish police, one of the most repressive in Europe, is very positively perceived by Spaniards. Aiming to answer the puzzle about why Spain is an outlier in the cross-country negative relationship between repression and attitudes towards the police, I propose a theory which concerns the historical heritage of the Franco regime and the bloody Civil War that brough the dictator into power. This heritage may persist in part thanks to a reformist transition to democracy which neither fully broke with the previous regime nor brought an inversion of societal hierarchies (Fishman, 1990, 2011; Fishman and Lizardo, 2013).[53]

I claim that Francoist repression prepared a fertile soil for the subsequent indoctrination in a vertical and hierarchical understanding of society (Pinto, 2004; Franco, 1939; Richards, 1998; Vincent, 2007), concepts very aligned with policing agencies in general (Sherman, 2002)

[53] Still, I find the association between repression and attitudes towards the police to decrease across generations.

and with the Spanish police in particular (Bradford et al., 2016). Spaniards embraced this narrative to make sense of the chaos and violence surrounding them. They did so thanks to the combination of two interrelated factors: fear of repression and fear of civil war recurrence (Blair, 2022). In contrast to the so-called anarchic and chaotic democratic regime preceding him, Franco was framed as the guarantor of peace and stability (Cazorla-Sánchez, 2005; Sevillano Calero, 2003). Peace and stability which, of course, needed the restoration of "authority, order and hierarchy" to be effective (Franco, 1939).

Consistent with this idea, it has been shown that Francoist repression during the Spanish Civil War and the post-war period is associated with more positive views of the police, an institution perceived to preserve order and stability. This finding, which is robust to the introduction of several covariates of repression, holds at three different geographical levels, each employing a different dataset and several measures of attitudes towards the police.

This paper contributes to the different literatures which it touches upon in different ways. Regarding research on the determinants of attitudes towards the police, it situates the focus far from the more conventional criminological approaches which focus on characteristics of individuals, their encounters with the police or contextual factors such as crime (see e.g., Brown and Benedict, 2002; Liu and Crank, 2010), bringing into the analysis historical factors. To the best of my knowledge, only Deglow and Sundberg (2021) and Blair and Morse (2021) offer a systematic assessment of the effects of wartime violence on perceptions of the police. This paper contributes to that emerging literature primarily by focusing on a specific form of violence—civilian victimization—rather than relying on aggregate measures of conflict intensity, and also by incorporating evidence from more than one country, thereby enhancing the external validity of the findings.

Concerning the study of historical legacies in political science, the finding of this paper aligns with an increasing line of research showing that the existence of a penalty for the

perpetrator of repression is not universal (e.g., Rodon and Tormos, 2023; Rozenas and Zhukov, 2019; Wang, 2021; Young, 2019; Zhukov and Talibova, 2018; Villamil, 2022).

Finally, most criminological works implicitly assume as socially desirable the existence of positive attitudes towards the police in society for, among other things, citizenship cooperation against crime. However, a blindly positive perception of the police might be problematic and clash with elemental democratic principles. That the Spanish police is neither accountable of their actions in a legal way (Brandariz García and Faraldo Cabana, 2015) nor at the public-opinion level is no good news at all for the health of Spanish democracy, whose citizens are seeing compromised their exercise of freedom of expression (CHR, 2013).

Conclusion

Economic inequality is on the rise (Piketty, 2014), with negative consequences across multiple dimensions of society, including health outcomes and crime rates (Fajnzylber et al., 2002; Wilkinson and Pickett, 2009). Inequality also drives segregation between social classes, distancing both geographically and socially citizens from different backgrounds and limiting opportunities for cross-class interaction (Massey and Fischer 2003; Mijs and Roe 2021; Reardon and Bischoff 2011). The first two papers of this dissertation aim to shed light on how inter-class contact may shape individuals' perceptions of inequality and support for redistribution. I examine two settings overlooked in the literature: mandatory military service and cross-class romantics partnerships.

Mandatory military service provides a unique empirical setting for studying contact between individuals from very different social classes. It is therefore surprising that this context has been overlooked in the literature—especially given its exogenous nature. For the first paper of this dissertation, I look at how conscription may have prompted support for redistribution among Spaniards. Mandatory military service facilitated contact between people from different social classes in an environment that, when viewed through the lens of Allport (1954)'s and Pettigrew (1998)'s optimal conditions for intergroup contact, could be considered ideal.

Employing the removal of conscription in Spain at the beginning of the 21st century and taking into account that women were never subject to conscription, I perform a difference-in-differences analysis that shows that being born in a conscripted cohort significantly increases the likelihood of supporting redistribution among males, relative to females. These results are robust to the inclusion of many different controls, as well as to many different specifications.

Moreover, I present two different placebo tests. The first specifies *false* dates for the suspension of conscription, finding that only the *true* specification presents a significant diff-in-diff effect. The second looks at the effect on different political attitudes, such as anti-immigrant attitudes and pro-environmental attitudes. The objective of these placebos is showing that my findings are not just an artifact of differing gender trends in a broader set of political attitudes. I also discard several competing explanations to the one I propose about inter-class contact in mandatory military service. Erikson and Stoker (2011) find that those more likely to be called for the Vietnam War—according to a lottery—developed more antipathy towards Nixon, the champion of the policy. Making a parallel argument, one could argue that those who were exempted from conscription developed more sympathy towards the right-wing political party (PP) which removed conscription in Spain and consequently moved their economic attitudes to the right. In order to discard this possibility, I show a null diff-in-diff effect on the probability of voting to or feeling closer to this political party. I also address other explanations focusing on reduced levels of institutional trust among conscripted cohorts (Bove et al., 2022), worsened material conditions (e.g., Angrist, 1990), or gendered policies implemented around the time of conscription (Felgueroso et al., 2014), showing that none of them can explain my results. Finally, I present two manipulation checks indicating that the removal of conscription did indeed reduce inter-class contact among men. In the first, I find that men in conscripted cohorts better predict their real position in society compared to women and men in non-conscripted cohorts. I claim that this is caused by a better understanding of the distribution of resources due to inter-class contact at conscription. In the second, I show that inter-class romantic partnerships are more prevalent among men in conscripted cohorts.

Though I present numerous robustness checks and different specifications, I cannot directly test that it was conscription which made men more pro-redistributive. Due to the absence of an indicator of conscription attendance in the European Social Survey, I rely on

year of birth to infer risk of being conscripted. However, many men in the last conscripted cohorts could obtain a deferral and eventually avoid conscription. Though I aim to deal with these issues in different ways throughout the paper, a survey containing an indicator of conscription attendance would increase the internal validity of my results. Up to my knowledge, the only survey with such an indicator does not contain any variable measuring support for redistribution. Therefore, future research could search for other countries with such an available survey, in order to increase both the internal and the external validity of my results.

The second paper of this dissertation proposes another overlooked setting in the literature. Although the diversity of individuals' social networks has been addresses by some scholars before (e.g., Liu, Kuo and Fernández-Albertos 2020; Newman 2014), the role of arguably the most central person in one's life has been previously ignored. I claim that entering a romantic partnership with someone from a different social class has the capacity to unveil the privileges and material differences between groups in society (e.g., Minkoff and Lyons 2019; Mo and Conn 2018), which remain largely hidden in many modern societies (Azpitarte et al. 2021).

For the empirical analysis, I employ panel data from the BHPS. This is useful methodologically to reduce issues of self-selection. If only looking at cross-sectional data, a concern may arise about those in cross-class partnerships being more pro-redistributive even before partnership formation. Namely, it could be argued that those with more leftist preferences are also more open to form a partnership with someone from a lower class. The longitudinal nature of the BHPS, however, allows me to examine changes in preferences. The core models of my analysis therefore examine how the preferences of individuals entering cross-class partnerships change, compared to those entering partnerships within the same social class. I consider the latter the most natural control group as they are going through a similar process and stage of life. I find that those entering a cross-class partnership move their

economic attitudes to the left. Further analyses show that this is the case indeed for both those entering a partnership with someone from a lower and from a higher social class. I present several robustness checks for these results and discard alternative explanations, such as partner's persuasion and changes in material conditions. Moreover, I carry out various placebo tests in which I show that entering a cross-class partnership has no effect on different political attitudes unrelated to the economic domain (e.g., views on homosexual relationships).

Very interesting findings also arise if distinguishing between individuals more and less likely to learn about inequality through means other than personal experience (e.g., Minkoff and Lyons, 2019). One plausible factor for that is political information, which can be proxied by political interest (Rapeli, 2022). Then, I claim that those with an interest in politics may be less easily activated by personal experiences of inequality, given their heightened knowledge about it. Indeed, I find that the distinction by political interest has important implications for the effect of cross-class romantic partnerships but also for sorting phenomena. Those without an interest in politics do not self-select into cross-class partnerships according to their economic attitudes (i.e., pre-partnership economic attitudes do not predict cross-class partnership formation for those not interested in politics). However, once they enter a cross-class partnership they move their economic attitudes to the left. It is the opposite story for those interested in politics. As they may place greater importance on politics in their daily lives, among this group of people it is those with more leftist pre-partnership attitudes who are more likely to form a cross-class partnership. However, once they do so, they do not move their economic attitudes significantly.

Though longitudinal data allows me to reduce concerns about self-selection often present in the study of inter-class contact (e.g., Bailey et al., 2013; Minkoff and Lyons, 2019), this paper is of course not without limitations. For instance, I cannot know the exact moment at which two romantic partners meet, but instead the moment—wave—at which they marry or

begin to cohabitate. Although I address this issue in different ways throughout the paper, future research could aim to define that moment with more precision as well as extend the analyses to other countries with not so class-based societies as the UK. Furthermore, scholars should explore the impact of non-romantic inter-class relationships—such as friendships—on economic attitudes, while paying attention to endogeneity issues maybe through the use of panel data (e.g., Newman and Vickrey, 2017).

The first two papers of my dissertation aim to extend a relatively nascent literature examining how contact between people from different social classes affects the way in which individuals perceive inequality. Most of this research addresses neighbourhood effects and employs cross-sectional data (e.g., Bailey et al. 2013; Minkoff and Lyons 2019; Franko and Livingston 2020), finding that people in more diverse neighbourhoods perceive larger levels of inequality and are more supportive of redistribution. However, these findings could be explained by sorting phenomena (e.g., Christensen et al., 2024; Gallego et al., 2016). The first two papers of this dissertation aim to advance this line of research methodologically by, respectively, studying an exogenous shock—the removal of conscription in Spain—and employing longitudinal data. Only a few studies to date have adopted a more causal approach in this field. Newman and Vickrey (2017), employing longitudinal data, study the effect of changes in unemployment in an individual's social circle. Mo and Conn (2018) examine the case of Tech for America—a national service program thanks to which top college graduate can teach in low-income schools—through a regression discontinuity design. Gingrich (2019b) compares stratified and mixed schools through matching techniques. I propose in this dissertation two settings unexplored in the literature, and unlike Mo and Conn (2018) and Newman and Vickrey (2017), I examine the attitudinal consequences of inter-class contact for the *advantaged* but also for the *disadvantaged*.

Future research could explore contact between social classes by turning attention to settings never examined before. While a rigorous quantitative approach may limit the exploration of new contexts, this should not deter researchers from investigating alternative environments such as integrated workplaces and diverse schools (Gingrich, 2019b) to better understand this politically relevant phenomenon. As inequality drives segregation (Massey and Fischer 2003; Reardon and Bischoff 2011), the results of this dissertation point to a vicious circle linking inequality, segregation and weakened demand for redistributive policies. In this context, the rising levels of socioeconomic segregation worldwide (Azpitarte et al. 2021; Mijs and Roe 2021; Musterd et al. 2017) underscore the urgent need to promote policies and institutions that foster interactions across social lines.

In the third paper of this dissertation, the focus turns to public attitudes towards the police. While previous research in this area mostly focuses on criminological explanations (e.g., Bradford and Jackson, 2018; Brown and Benedict, 2002; Liu and Crank, 2010; Nix et al., 2015), this paper proposes examining the effect of historical legacies. I argue that the climate of violence during the Spanish civil war made Spaniards more tolerant towards institutions perceived as guarantors of order and stability, as a means of avoiding the cruelty of another civil war (Blair, 2022), a phenomenon which intertwined with the regime's efforts to indoctrinate its citizens in the virtues of a vertical and hierarchical society (Richards, 1998; Vincent, 2007).

I test this theory by exploiting variation in Francoist repression across Spanish regions. I link respondents' attitudes towards the police nowadays with levels of repression experienced in their regions during the Civil War and the post-war period. Employing different surveys which define respondents' region of residence at different geographical levels, I perform three different empirical analyses. First, I show that Catalans in municipalities which experienced more repression in the past evaluate more positively police performance nowadays. Consistent

with a literature that finds the intensity of the legacies of violence to weaken across generations (e.g., Lupu and Peisakhin, 2017; Wang, 2021), I find the relationship between Francoist repression and attitudes towards the police to be stronger for older generations. I perform later a second analysis in which I define repression at the province level (NUTS 3). Although this is a less fine-grained analysis, it allows me to test whether the relationship found for Catalan municipalities holds for other regions in Spain. I find a positive relationship between Francoist repression and rate of the police profession, an effect which weakens for younger generations. The third analysis employs data from a special module of the ESS to show that repression—defined at the NUTS-2 level—correlates with views of the police on numerous dimensions. Moreover, I show that Francoist repression also correlates with a set of attitudes claimed to mediate this relationship, such as demand for obedience and security.

Establishing causality in this area of research presents a significant challenge. Therefore, future scholars may explore the relationship between repression and attitudes towards the police while rigorously addressing endogeneity. Tur-Prats and Valencia Caicedo (2020), for example, exploit deviations from the initial military plans of attack of the Francoist forces along with a geographical regression discontinuity design. Although establishing causality is not the main focus of my third dissertation paper, I aim to show that the positive effect I find is robust to the introduction of many variables employed in the literature to explain varying levels of repression during the Spanish Civil War (Balcells, 2011, 2017; Herreros and Criado, 2009). Furthermore, I discard several alternative explanations for my results, such as exile, development of the civil society before the war, varying levels of criminality, regional patterns of police recruitment and police practices, or support for secession of Catalonia (Rodon and Tormos, 2023). The theory of this paper indeed shares some characteristics with that of Rodon and Tormos (2023), who propose that Catalans in municipalities that experienced

more Francoist repression during the Civil War and the post-war period are nowadays less prone to support independence, as a means of preserving the status quo and political stability.

Finally, I examine the case of Guatemala in order to increase the external validity of the analysis. Some phenomena in the Guatemalan Civil War parallel the ones analysed for the case of Spain, such as the fear of civilians to a return to the civil war and the reception to the military's propaganda due to the socialisation in fear (Bateson, 2017; Green, 1999). I find that those Guatemalans living in municipalities which experienced more state-led repression display larger levels of trust in the police.

As noted by Eck et al., it is surprising that "the literatures on policing, specifically, and political violence, more generally, have developed separately from one another" (2021, 1642). Indeed, to my best knowledge, only two quantitative studies have systematically assessed the consequences of civil war on citizens' attitudes towards the police. In the context of the current civil war in Afghanistan, Deglow and Sundberg (2021) find that local conflict intensity negatively correlates with police perceptions. As the police are required to focus not only on ordinary policing but also on counterinsurgency duties, the diversion of resources from the former to the latter may undermine public perceptions of their effectiveness in addressing ordinary crime. Moreover, police-led counterinsurgency activities may themselves negatively affect perceptions of procedural justice. Blair and Morse (2021) show that rebel-perpetrated violence during the civil wars in Liberia is associated with trust in the police and reliance on state over non-state actors for security. The third paper of this dissertation contributes to the study of political violence and historical legacies in at least three distinct respects, distinguishing itself from these two works (Blair and Morse, 2021; Deglow and Sundberg, 2021). First, I examine violence against civilians rather than any type of violence (e.g., battle violence). Despite the plausible relevance of local conflict intensity (Deglow and Sundberg, 2021) and exposure to any type of violence during a civil war (Blair and Morse, 2021) to shape

post-conflict attitudes, violence against civilians may have a distinctive effect given its target of the public. Moreover, the measure of exposure to violence employed by Blair and Morse (2021) is self-reported by respondents, raising concerns about endogeneity. Second, the temporal proximity to the civil wars in Liberia and the ongoing conflict in Afghanistan limits the ability to test whether distinct attitudes resulting from conflict are transmitted across generations (Cremaschi and Masullo, 2023; Zhukov and Talibova, 2018). Third, this study is the first to examine the relationship between violence during a civil war and attitudes towards the police within the context of an advanced democracy—Spain—and to extend this analysis to more than one country by including Guatemala.

These results align with a growing body of research demonstrating that the emergence of a penalty for the perpetrator of repression is not a universal outcome (e.g., Rodon and Tormos, 2023; Rozenas and Zhukov, 2019; Wang, 2021; Young, 2019; Zhukov and Talibova, 2018; Villamil, 2022). They are also consistent with Blair (2022) and Rodon and Tormos (2023), who show that civil-war violence prompts fears of conflict recurrence and reduce support for political projects perceived as threatening to the status quo, such as secessionist movements. Finally, this paper seeks to highlight a situation with serious implications for the proper functioning of democracy: a country—Spain—where the police is not held accountable for their actions, either through legal mechanisms (Brandariz García and Faraldo Cabana, 2015; CHR, 2013) or in the realm of public opinion.

This dissertation, at the intersection of comparative political economy and political sociology, has examined experiences and socialisation processes that leave a long-term footprint in the way individuals think about the economy and forge beliefs about various societal institutions, in contrast to more situational approaches to the study of attitudes formation, whose effects are often less stable across the lifetime.

Appendix A: Supplementary data for Chapter 1

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Section A.1: Measuring income in the ESS

The variable measuring income deserves special attention in the ESS. The first three rounds of this survey contain twelve intervals from which the respondent indicates her own income. However, a different approach was employed beginning in round 4. The new categories were created based on actual deciles, leading to just ten categories instead of twelve.[1] In order to create an income indicator that I can employ for the nine rounds I have proceeded as follows. Observing the ESS showcards for Spain, I assign every individual the midpoint of the interval selected.[2] Later, I divide this number by that round's income mean in Spain.[3] By doing this, I am looking at the relative position of the respondent within the income distribution of that country-year, which is in fact the reasoning behind most models in the field.

Besides operationalizing income as a) [$income_{ir}$/mean $income_{r}$], I have also defined that as:

b) [$income_{ir}$ - mean $income_{r}$], and

c) [log ($income_{ir}$)/log (mean $income_{r}$)],

where *i* stands for individual and *r* for round. Employing a), b), or c) does not alter the effect of conscription on preferences for redistribution. However, the effect of income itself is more sensible to the specific income variable employed: the logarithmic measure shows a weaker effect of income on preferences.

[1] These are nation-specific deciles. In any case, remember that here I am only using data from Spain.

[2] Because the highest interval has no upper limit, I have assigned those individuals the lower limit of that interval plus half the range of the penultimate interval.

[3] Round's income mean from Eurostat. Almost identical results if income mean is calculated from the ESS, being simply the mean of all income responses (as midpoints) of that round.

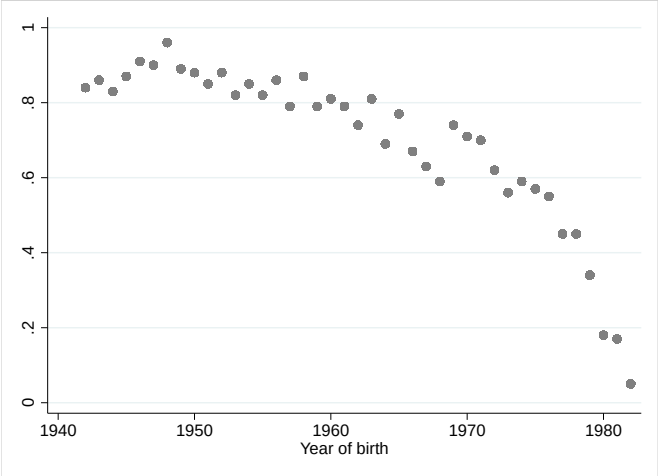
Section A.2: An alternative measure of conscription

A second approach to the difference-in-differences strategy proposes an *apparently* more nuanced indicator of the risk of conscription. Instead of a crude dummy taking value 1 for all conscripted cohorts, some authors have resorted to the proportion of conscripted males by cohort for measuring risk of conscription (Bedard and Deschênes, 2006; Imbens and van der Klaauw, 1995). This strategy also makes more plausible to argue that what is observed is in fact caused by conscription, in line with Rodgers (1982) and de Graaf (1999)'s advocacy for looking at the substantive variables for which generation effects are indirect indicators.

Unfortunately, there exists no official statistics of Spanish conscripts by year of birth. Therefore, I perform through CIS (Centro de Investigaciones Sociológicas) surveys an estimation of that. These Spanish surveys contain information on military attendance and age of the respondents (but not on preferences for redistribution). Unfortunately, this should be considered only as a very imperfect—and maybe unreliable—measure of the real proportion of conscripted males by generation for several reasons. The CIS survey does not contain information on year of birth, which has to be (imperfectly) deduced through respondent's age. Leaving aside additional problems regarding the representativeness of the sample by year of birth (or age), there are a few positive responses to mandatory military service attendance in individuals for whom it is impossible to have attended conscription given their age.[4] Furthermore, the estimated proportion of conscripted males does not seem to be decreasing on educational attainment, as it clearly should, also calling for being very cautious to take this as a reliable measure of risk of conscription. All this said, let us have a look at the results with this variable. The trend seems relatively stable until the 1960 cohorts, when the rate of compliance begins to fall.

[4] It is very likely that these positive responses come from non-attentive respondents who actually have been or were in the military but in a voluntary way—not in the context of compulsory military service, e.g., as in a professional career. These responses have been re-coded as zero for constructing the variable. Note that joining voluntarily the military should have no connection with heightened inter-class contact.

Figure A.1: Proportion of conscripted males by cohort (estimations, not official numbers)



Following the diff-in-diff approach and in line with Imbens and van der Klaauw (1995), I assign both men and women the male service rate of their cohort and then interact that with a gender dummy.[5] Then, through this model I am testing whether the gender gap in preferences for redistribution between cohorts is a function of the proportion of conscripted men. This means estimating the following refinement of the model estimated in Table 1.1:

$$y_i = \alpha + \beta_1 P_i + \beta_2 Male_i + \beta_3 P_i Male_i + \delta Yob_i + \vartheta' Year_i + \theta' X_i + \varepsilon_i$$

where P is the service rate computed for each male cohort, taking the same value for both men and women. The remaining notation goes as in the core model. β_3 is the diff-in-diff estimator here.

Results

Table A.1 shows the results of estimating the effect of conscription on preferences with this alternative specification. The diff-in-diff estimator is given, of course, by the coefficient of the interaction term. Again, the model is re-estimated for different samples of cohorts around the cut-off point of 1982. The picture is not very different to that observed in Table 1.1: the effect of conscription is positive and statistically significant across every column, although apparently estimated with more noise.

[5] Bedard and Deschênes (2006) and Fize and Louis-Sidois (2020) assign a value of zero to all women instead of introducing an interaction term. However, I consider that Imbens and van der Klaauw (1995)'s better approach the idea of difference in differences. In any case, both approaches yield similar results.

Table A.1: Alternative measure of conscription

Cohorts	1982-1983	1981-1984	1980-1985	1979-1986	1978-1987	1977-1988
	(1)	(2)	(3)	(4)	(5)	(6)
Male service rate (P)	.072	-.953**	-.255	-.292	-.124	-.152
	(1.152)	(.458)	(.38)	(.188)	(.127)	(.114)
Male	-.117**	-.078**	-.08***	-.076***	-.058***	-.054***
	(.055)	(.034)	(.027)	(.023)	(.021)	(.02)
P*Male	3.119**	.664**	.571**	.421***	.26***	.17**
	(1.377)	(.321)	(.232)	(.156)	(.089)	(.078)
Year of birth	-	-.063**	-.011	-.014	-.005	-.003
		(.028)	(.016)	(.01)	(.007)	(.006)
Income	-.07*	-.028	-.052**	-.051***	-.05***	-.064***
	(.035)	(.029)	(.02)	(.017)	(.015)	(.015)
Unemployed	.074*	.058*	.059**	.06**	.048**	.015
	(.04)	(.031)	(.028)	(.023)	(.021)	(.024)
Observations	369	749	1138	1550	1912	2283
R^2	.073	.064	.04	.04	.028	.023

Linear probability models. The dependent variable measures support for redistribution. Robust standard errors clustered at the cohort-region level in parentheses. Wave dummies included. Dummies for five levels of educational attainment included. "P" indicates the proportion of males conscripted in each cohort, with the same value assigned for both males and females in the cohort. The diff-in-diff estimator is captured by the "P*Male" coefficient. First column does not include cohort trend due to perfect collinearity. **** p<0.01, ** p<0.05, * p<0.1.*

Unlike the findings employing a dummy for those born in a conscripted cohort, the results with the estimated service rate are more sensitive to the specific window of cohorts employed. If expanding the analysis to more cohorts (paralleling the analysis from Table A.6 below), the diff-in-diff coefficient reaches statistical significance in half of the specifications, being consistently positive across all of them. However, I consider these results should be taken very cautiously for a variety of reasons. First, as I have mentioned before this is a very imperfect measure of the true proportion of conscripted males by cohort. Second, as discussed in the main text, due to year-of-birth and gender homophily, it is very plausible that social networks of males in conscripted cohorts present disproportionate levels of inter-class contact regardless of actual attendance to compulsory military service. One could argue that the larger the proportion of males conscripted by cohort the larger the level of inter-class contact and consequently the larger the preferences for redistribution of that cohort of males should be. Even if that could make sense in principle, it is very plausible that that effect is non-linear, as we are dealing with social networks. Namely, that the impact on a cohort with 50% of conscripted males must not necessarily be double of that on a cohort in which 25% of males were conscripted. Actually, it makes sense to think about saturation effects in which once a certain proportion is reached, the level of inter-class contact in that cohort of males does not increase more. This may explain why even if a consistently positive diff-in-diff effect is found when taking the proportion of males conscripted, some specifications do not reach statistical significance.

Section A.3: Alternative specifications of the APC model

Given that age, year of birth and year of the survey are exact linear functions of each other (Age=Year-Yob), I cannot control simultaneously for the three of them. Dinas and Stoker (2014) propose the use of control groups when addressing cohort effects—an adaptation of the diff-in-diff estimation to the APC framework, as I do here—and provide some ideas to deal with the impossibility of simultaneously controlling for all the time variables, checking for the sensitivity of the results. For that purpose, I proceed as follows. First, I repeat the analyses of Table 1.1 introducing a variable of *age*.[6] For consistency, I also change the ESS rounds' dummies for a *period* variable taking the value of the year in which the survey was taken. This model containing *age* and *period* is one that assumes both parallel age and parallel period linear effects across genders (left model of Figure A.2). Later, for checking for the sensitivity of these findings, I relax each of these assumptions by introducing interactions between the *age*/*period* variables and gender—each of them at a time. The results are almost identical if I relax the parallel age (centre model of Figure A.2) or the parallel period assumption (right model of Figure A.2). I show below point estimates and 95% confidence intervals for the diff-in-diff coefficient.

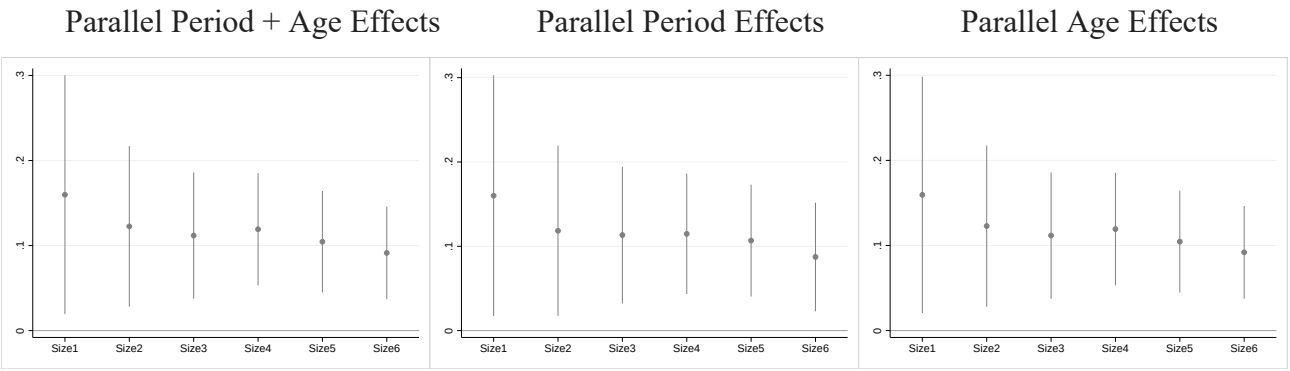


Figure A.2: Alternative APC specifications. Point estimates and 95% confidence intervals for the diff-in-diff coefficient in models paralleling those from Table 1.1, introducing *age*, dropping the linear year-of-birth trend, and changing the ESS rounds' dummies for a linear *period* variable. Left model assumes both parallel period and parallel age effects across genders. Centre model assumes parallel period effects, but not parallel age effects. Right model assumes parallel age effects, but not parallel period effects.

[6] Then, I have to exclude the year-of-birth variable due to perfect collinearity.

Section A.4: On the placebo tests

I define in this section of the Appendix the operationalization of the dependent variables in models linked to Figures 1.4-1.7. Later, I show the results of an additional placebo test on attitudes towards homosexual relationships (Figure A.4), which I do not include in main text for convenience of visual presentation. Finally, I show that the main results on support for redistribution are robust to controlling for these placebos (Table A.2).

Like the question capturing support for redistribution, all the items employed in these placebo tests are present across every round of the ESS, in order to have a similar number of observations and reduce concerns about different levels of statistical power across different regressions—see in Figure A.3 the number of observations for the different analyses and window sizes.

Figure A.3: Number of observations of the placebo tests (Figures 1.4-1.7, A.4) and in the main analysis (Table 1.1)

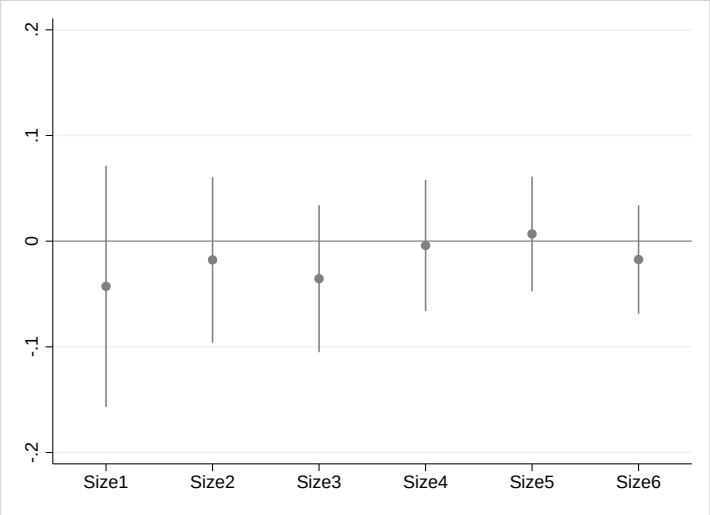
	Size 1	Size 2	Size 3	Size 4	Size 5	Size 6
Figure 1.4	374	757	1148	1560	1923	2296
Figure 1.5	371	751	1143	1556	1920	2293
Figure 1.6	363	735	1123	1518	1877	2239
Figure 1.7	369	750	1137	1546	1906	2276
Figure A.4	372	750	1138	1548	1911	2277
Table 1.1	369	749	1138	1550	1912	2283

- The dependent variable in Figure 1.4 indicates how similar the respondent claims to be to someone described as "She/He strongly believes that people should care for nature. Looking after the environment is important to her/him". Responses range from "very much like me" to "not like me at all", in a six-point scale (the other four responses being "like me", "somewhat like me", "a little like me", and "not like me"). Responses are reversed such that higher values indicate more similarity.
- The dependent variable in Figure 1.5 is the average of the responses from 0 to 10 to three items indicating the opinion of the respondent on whether (i)immigrants make the country a better place to live, (ii) immigration is good for country's economy, and (iii)country's cultural life is enriched by immigrants. Higher responses indicate a more positive perception of immigrants.
- The dependent variable in Figure 1.6 is the average of the responses from 1 ("Allow many to come and live here") to 4 ("Allow none") to three items indicating whether (i)immigrants from the same

race or ethnic group of the majority, (ii)immigrants of different race/ethnic groups of the majority, and (iii)immigrants from poorer countries outside Europe, should be allowed to come and live in Spain (the other responses being "allow some", and "allow a few").

- The dependent variable in Figure 1.7 indicates how similar the respondent claims to be to someone described as "It is important to her/him that the government ensures her/his safety against all threats. She/he wants the state to be strong so it can defend its citizens". Responses range from "very much like me" to "not like me at all", in a six-point scale (the other four responses being "like me", "somewhat like me", "a little like me", and "not like me"). Responses are reversed such that higher values indicate more similarity.
- For the dependent variable in Figure A.4, I have employed an item measuring agreement with the statement "Gays and lesbians should be free to live life as they wish". The possible responses are "strongly agree", "agree", "neither agree nor disagree", "disagree", and "strongly disagree". This is the only of the *placebos* with the same structure of available responses as the variable capturing support for redistribution and maybe the only for which a dichotomous operationalization appears naturally (note the ambiguity of a dichotomous operationalization of any of the dependent variables in Figures 1.4-1.7). I have considered again neutral responses as a less overt expression of opposition. Actually, the distribution of responses is strikingly similar to that about preferences for redistribution, with around 80% indicating some level of agreement and 10% showing a neutral response—the only difference being that strong agreement is much more prevalent for the item about homosexuality than for the item about redistribution. Those who agree or strongly agree with the statement are considered as open, and those who neither agree nor disagree, disagree, or strongly disagree as not open. I show the results of LPM analyses in Figure A.4.

Figure A.4: Effect on openness towards homosexual relationships



Point estimates and 95% confidence intervals for the diff-in-diff coefficient in linear probability models paralleling those from Table 1.1. See above for an explanation of the dependent variable.

Table A.2: Main results when controlling for ideological items (Dependent variable is support for redistribution)

Ideological item	Pro-environmental attitudes	Attitudes towards immigration	Support for restrictions to immigration	Desire for a strong government	Openness towards homosexuals	Left-right scale
	(1)	(2)	(3)	(4)	(5)	(6)
Ideological item	.009	.013***	-.035***	-.005	.106***	-.023***
	(.008)	(.004)	(.009)	(.007)	(.028)	(.004)
T (Born in a conscripted cohort)	.071*	.071*	.079**	.069*	.068*	.075**
	(.036)	(.036)	(.037)	(.038)	(.036)	(.038)
Male	-.08***	-.081***	-.077***	-.081***	-.079***	-.088***
	(.021)	(.022)	(.022)	(.022)	(.021)	(.023)
T*Male	.092***	.088***	.077***	.092***	.096***	.09***
	(.028)	(.028)	(.029)	(.028)	(.028)	(.03)
Year of birth	.015***	.015***	.015***	.015***	.015***	.015***
	(.005)	(.005)	(.005)	(.005)	(.005)	(.005)
Income	-.061***	-.059***	-.059***	-.062***	-.065***	-.06***
	(.015)	(.015)	(.015)	(.015)	(.015)	(.016)
Unemployed	.016	.02	.016	.014	.015	.006
	(.023)	(.024)	(.024)	(.023)	(.023)	(.025)
Observations	2276	2278	2225	2256	2267	2063
R^2	.031	.035	.036	.031	.037	.048

Linear probability models. The dependent variable measures support for redistribution. Robust standard errors clustered at the cohort-region level in parentheses. Wave dummies included. Dummies for five levels of educational attainment included. "Born in a conscripted cohort" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. The diff-in-diff estimator is captured by the "T*Male" coefficient. Analysis includes those born between 1977 and 1988.
**** p<0.01, ** p<0.05, * p<0.1.*

Section A.5: Robustness checks

In the following sections of the Appendix, I perform various robustness checks. Most of them are variations of the model estimated in Table 1.1 of the main text. For space purposes, I often only show the numbers for the diff-in-diff coefficients and for the controls of interest when applicable. In other special cases (e.g., Table A.3 when looking at probit analysis), full results are displayed.

Table A.3: Probit regressions

Cohorts	1982-1983	1981-1984	1980-1985	1979-1986	1978-1987	1977-1988
	(1)	(2)	(3)	(4)	(5)	(6)
T (Born in a	.035	.257	.231	.129	.164	.294**
conscripted cohort)	(.253)	(.245)	(.201)	(.175)	(.159)	(.148)
Male	-.428**	-.369***	-.385***	-.368***	-.308***	-.305***
	(.214)	(.13)	(.102)	(.093)	(.092)	(.084)
T*Male	.662**	.477**	.445***	.467***	.413***	.35***
	(.305)	(.191)	(.153)	(.138)	(.124)	(.111)
Year of birth	-	.095	.071	.032	.039	.063***
		(.113)	(.052)	(.034)	(.024)	(.021)
Income	-.267**	-.102	-.181***	-.179***	-.18***	-.219***
	(.135)	(.102)	(.066)	(.056)	(.051)	(.05)
Unemployed	.426*	.283*	.287**	.31***	.243**	.089
	(.233)	(.146)	(.136)	(.118)	(.103)	(.101)
Observations	369	749	1138	1550	1912	2283
Pseudo R^2	.085	.072	.05	.049	.037	.033

Probit models. The dependent variable measures support for redistribution. Robust standard errors clustered at the cohort-region level in parentheses. Wave dummies included. Dummies for five levels of educational attainment included. "Born in a conscripted cohort" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. The diff-in-diff estimator is captured by the "T*Male" coefficient. First column does not include cohort trend due to perfect collinearity.
**** p<0.01, ** p<0.05, * p<0.1.*

Table A.4: Excluding post-treatment variables (income, educational attainment, unemployment status)

Cohorts	1982-1983	1981-1984	1980-1985	1979-1986	1978-1987	1977-1988
	(1)	(2)	(3)	(4)	(5)	(6)
T (Born in a	.021	.056	.053	.027	.042	.075**
conscripted cohort)	(.056)	(.062)	(.048)	(.043)	(.038)	(.036)
Male	-.105*	-.098***	-.105***	-.104***	-.087***	-.083***
	(.056)	(.034)	(.027)	(.023)	(.023)	(.021)
T*Male	.14*	.12**	.119***	.127***	.108***	.09***
	(.071)	(.047)	(.037)	(.034)	(.03)	(.028)
Year of birth	-	.021	.017	.008	.01*	.016***
		(.029)	(.013)	(.008)	(.006)	(.005)
Observations	369	749	1138	1550	1912	2283
R^2	.041	.038	.027	.026	.021	.018

Linear probability models. The dependent variable measures support for redistribution. Robust standard errors clustered at the cohort-region level in parentheses. Wave dummies included. "Born in a conscripted cohort" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. The diff-in-diff estimator is captured by the "T*Male" coefficient. First column does not include cohort trend due to perfect collinearity.
**** p<0.01, ** p<0.05, * p<0.1.*

Table A.5: Excluding post-treatment variables but including father's level of educational attainment

Cohorts	1982-1983	1981-1984	1980-1985	1979-1986	1978-1987	1977-1988
	(1)	(2)	(3)	(4)	(5)	(6)
T (Born in a	.014	.04	.046	.032	.051	.084**
conscripted cohort)	(.062)	(.07)	(.051)	(.045)	(.04)	(.039)
Male	-.09*	-.094***	-.108***	-.117***	-.097***	-.089***
	(.053)	(.035)	(.027)	(.023)	(.022)	(.021)
T*Male	.134*	.113**	.106**	.128***	.105***	.082***
	(.072)	(.05)	(.041)	(.034)	(.031)	(.029)
Year of birth	-	.01	.01	.009	.012**	.017***
		(.033)	(.013)	(.008)	(.006)	(.005)
Father's level of educational attainment						
Less than lower secondary (reference)	-	-	-	-	-	-
Lower secondary	-.031	.002	.001	.001	-.013	-.027
	(.055)	(.035)	(.026)	(.023)	(.022)	(.02)
Upper secondary	.047	.022	-.025	-.035	-.077**	-.062**
	(.049)	(.041)	(.039)	(.035)	(.033)	(.031)
Post-secondary	.181***	.053	.043	.031	.015	-.024
non-tertiary	(.042)	(.08)	(.065)	(.055)	(.052)	(.049)
Tertiary	-.051	-.079*	-.098***	-.107***	-.101***	-.092***
	(.053)	(.044)	(.033)	(.029)	(.026)	(.023)
Observations	353	713	1075	1459	1806	2153
R^2	.051	.043	.035	.038	.033	.025

Linear probability models. The dependent variable measures support for redistribution. Robust standard errors clustered at the cohort-region level in parentheses. Wave dummies included. "Born in a conscripted cohort" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. The diff-in-diff estimator is captured by the "T*Male" coefficient. First column does not include cohort trend due to perfect collinearity.
**** p<0.01, ** p<0.05, * p<0.1.*

Table A.6: Let's open the windows more

Cohorts	1976-1989	1975-1990	1974-1991	1973-1992	1972-1993	1971-1994	1970-1995	1969-1996	1968-1997
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
T (Born in a	.075**	.056*	.049*	.043	.043	.033	.026	.028	.022
conscripted cohort)	(.033)	(.031)	(.029)	(.028)	(.028)	(.027)	(.026)	(.025)	(.024)
Male	-.063***	-.051**	-.051***	-.058***	-.057***	-.063***	-.064***	-.06***	-.06***
	(.02)	(.02)	(.019)	(.02)	(.019)	(.018)	(.018)	(.017)	(.017)
T*Male	.05*	.049*	.046*	.049**	.049**	.064***	.066***	.061***	.056***
	(.027)	(.026)	(.025)	(.024)	(.023)	(.022)	(.022)	(.021)	(.021)
Year of birth	.011***	.007**	.006**	.005**	.005**	.005***	.004**	.004***	.003**
	(.004)	(.003)	(.003)	(.002)	(.002)	(.002)	(.002)	(.001)	(.001)
Observations	2650	2999	3305	3669	4008	4318	4629	4923	5243
R^2	.023	.019	.021	.024	.027	.028	.028	.027	.026

Linear probability models. The dependent variable measures support for redistribution. Robust standard errors clustered at the cohort-region level in parentheses. Wave dummies included. Dummies for five levels of educational attainment included, as well as controls for income and unemployment status. "Born in a conscripted cohort" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. The diff-in-diff estimator is captured by the "T*Male" coefficient.
**** p<0.01, ** p<0.05, * p<0.1.*

Table A.6: Let's open the windows more (cont.)

Cohorts	1967-1998	1966-1999	1965-2000	1964-2001	1963-2002	1962-2003	1961-2004	1960-2004	1910-2004 (Full sample)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
T (Born in a	.023	.019	.018	.013	.009	.002	.006	.009	-.01
conscripted cohort)	(.024)	(.023)	(.022)	(.022)	(.022)	(.021)	(.021)	(.021)	(.015)
Male	-.056***	-.059***	-.06***	-.066***	-.065***	-.066***	-.067***	-.067***	-.068***
	(.017)	(.017)	(.017)	(.017)	(.017)	(.017)	(.017)	(.017)	(.017)
T*Male	.048**	.049**	.049**	.053***	.057***	.059***	.056***	.051***	.042**
	(.02)	(.02)	(.02)	(.02)	(.02)	(.02)	(.019)	(.019)	(.018)
Year of birth	.002*	.002*	.002**	.002*	.002*	.001	.001	.001*	0
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(0)
Observations	5534	5802	6068	6311	6529	6761	7004	7245	11825
R^2	.025	.024	.024	.025	.025	.024	.024	.024	.019

Linear probability models. The dependent variable measures support for redistribution. Robust standard errors clustered at the cohort-region level in parentheses. Wave dummies included. Dummies for five levels of educational attainment included, as well as controls for income and unemployment status. "Born in a conscripted cohort" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. The diff-in-diff estimator is captured by the "T*Male" coefficient.
**** p<0.01, ** p<0.05, * p<0.1.*

Table A.7: Other comparisons of cohorts

Cohorts	1977-1988 Excluding 1982	1976-1988 Excluding 1982	1977-1988 Excluding 1980-1982	1974-1988 Excluding 1980-1982	1977-1988 Excluding 1979-1982	1973-1988 Excluding 1979-1982	1971-1988 Excluding 1977-1982	1971-1988 Excluding 1977-1984	1967-1988 Excluding 1973-1982
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
T (Born in a conscripted cohort)	.067	.051	.074	.014	.067	.03	.101*	.089	.007
	(.045)	(.042)	(.053)	(.044)	(.058)	(.052)	(.057)	(.075)	(.078)
Male	-.081***	-.083***	-.082***	-.084***	-.083***	-.087***	-.088***	-.079***	-.088***
	(.021)	(.021)	(.022)	(.021)	(.022)	(.022)	(.022)	(.028)	(.022)
T*Male	.088***	.064**	.087**	.07**	.066*	.056**	.081***	.072**	.082***
	(.029)	(.029)	(.033)	(.029)	(.036)	(.027)	(.028)	(.033)	(.028)
Year of birth	.015**	.009	.016**	.004	.014*	.005	.011**	.01*	.002
	(.006)	(.006)	(.007)	(.004)	(.007)	(.005)	(.004)	(.005)	(.005)
Income	-.06***	-.054***	-.069***	-.063***	-.06***	-.054***	-.058***	-.061***	-.058***
	(.016)	(.015)	(.019)	(.016)	(.021)	(.016)	(.014)	(.015)	(.014)
Unemployed	.013	.012	-.003	.003	-.015	.005	.051***	.047**	.031
	(.025)	(.024)	(.029)	(.024)	(.031)	(.024)	(.019)	(.021)	(.021)
Observations	2095	2334	1648	2359	1420	2395	2416	2064	2433
R^2	.028	.024	.032	.026	.036	.03	.042	.039	.038

Linear probability models. The dependent variable measures support for redistribution. Robust standard errors clustered at the cohort-region level in parentheses. Wave dummies included. Dummies for five levels of educational attainment included. "Born in a conscripted cohort" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. The diff-in-diff estimator is captured by the "T*Male" coefficient.
**** p<0.01, ** p<0.05, * p<0.1.*

Table A.8: Excluding individuals with tertiary education

Cohorts	1982-1983	1981-1984	1980-1985	1979-1986	1978-1987	1977-1988
	(1)	(2)	(3)	(4)	(5)	(6)
T*Male	.101	.121**	.127***	.1**	.096***	.092***
	(.078)	(.051)	(.042)	(.039)	(.036)	(.033)
Observations	263	517	778	1057	1296	1526
R^2	.086	.086	.059	.055	.041	.036

Linear probability models excluding individuals with tertiary education completed. The dependent variable measures support for redistribution. Robust standard errors clustered at the cohort-region level in parentheses. Wave dummies included. Dummies for four levels of educational attainment included, as well as controls for income, unemployment status, year of birth, T and gender. "T" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. The diff-in-diff estimator is captured by the "T*Male" coefficient. First column does not include cohort trend due to perfect collinearity.
*** p<0.01, ** p<0.05, * p<0.1.*

Table A.9: Only males

Cohorts	1982-1983	1981-1984	1980-1985	1979-1986	1978-1987	1977-1988
	(1)	(2)	(3)	(4)	(5)	(6)
Born in a	.144***	.19***	.167***	.144***	.159***	.191***
conscripted cohort	(.036)	(.068)	(.052)	(.047)	(.042)	(.042)
Observations	199	391	593	799	990	1180
R^2	.119	.119	.069	.071	.06	.046

Linear probability models including only males. The dependent variable measures support for redistribution. Robust standard errors clustered at the cohort-region level in parentheses. Wave dummies included. Dummies for five levels of educational attainment included, as well as controls for income, unemployment status, and year of birth. "Born in a conscripted cohort" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. First column does not include cohort trend due to perfect collinearity.
*** p<0.01, ** p<0.05, * p<0.1.*

Table A.10: Only females

Cohorts	1982-1983	1981-1984	1980-1985	1979-1986	1978-1987	1977-1988
	(1)	(2)	(3)	(4)	(5)	(6)
Born in a	.011	.012	.037	.029	.026	.046
conscripted cohort	(.057)	(.09)	(.063)	(.055)	(.049)	(.045)
Observations	170	358	545	751	922	1103
R^2	.076	.063	.049	.036	.025	.034

Linear probability models including only females. The dependent variable measures support for redistribution. Robust standard errors clustered at the cohort-region level in parentheses. Wave dummies included. Dummies for five levels of educational attainment included, as well as controls for income, unemployment status, and year of birth. "Born in a conscripted cohort" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. First column does not include cohort trend due to perfect collinearity.
*** p<0.01, ** p<0.05, * p<0.1.*

Table A.11. Non-dichotomous dependent variable

Cohorts	1982-1983	1981-1984	1980-1985	1979-1986	1978-1987	1977-1988
	(1)	(2)	(3)	(4)	(5)	(6)
T (Born in a	.096	.243	.207*	.124	.101	.188**
conscripted cohort)	(.136)	(.151)	(.116)	(.1)	(.088)	(.087)
Male	-.19	-.174**	-.169***	-.155***	-.144***	-.122***
	(.123)	(.085)	(.06)	(.051)	(.048)	(.045)
T*Male	.245	.187	.155*	.159**	.154**	.105
	(.172)	(.116)	(.089)	(.076)	(.068)	(.064)
Year of birth	-	.087	.056*	.022	.015	.029**
		(.066)	(.031)	(.02)	(.014)	(.013)
Income	-.266**	-.088	-.111**	-.112***	-.124***	-.147***
	(.108)	(.08)	(.05)	(.041)	(.034)	(.035)
Unemployed	.254*	.182**	.215***	.232***	.2***	.132**
	(.141)	(.084)	(.074)	(.06)	(.053)	(.055)
Observations	369	749	1138	1550	1912	2283
R^2	.095	.088	.061	.061	.056	.047

OLS models. The dependent variable measures support for redistribution, ranging from *strongly disagree* (1) *to strongly agree* (5). Robust standard errors clustered at the cohort-region level in parentheses. Wave dummies included. Dummies for five levels of educational attainment included. "Born in a conscripted cohort" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. The diff-in-diff estimator is captured by the "T*Male" coefficient. First column does not include cohort trend due to perfect collinearity.
*** p<0.01, ** p<0.05, * p<0.1.*

Section A.6: Level of clustering of the standard errors

Throughout the main text, I show results when standard errors are clustered at the cohort (i.e., year of birth)-region level. A more conventional approach would have been to cluster simply at the year-of-birth level, since cohorts of nationals may share formative experiences and, for example, common educational laws. However, doing this leaves me with a very low number of clusters. Then, clustering standard errors at the cohort-region level allows me to heighten the number of clusters to a much more reasonable level. In any case, the findings are pretty similar if clustering at the cohort level (Bedard and Deschênes, 2006; Torun and Tumen, 2016), at the cohort-sex level (Fize and Louis-Sidois, 2020), or if simply the standard errors are not clustered at all (see Tables A.12-A.14).

Table A.12: Standard errors clustered by year of birth

Cohorts	1982-1983	1981-1984	1980-1985	1979-1986	1978-1987	1977-1988
	(1)	(2)	(3)	(4)	(5)	(6)
T*Male	.156***	.129***	.122***	.125***	.108***	.092***
	(.000)	(.018)	(.013)	(.012)	(.019)	(.021)
Observations	369	749	1138	1550	1912	2283
R^2	.073	.07	.047	.045	.034	.031

Linear probability models. The dependent variable measures support for redistribution. Robust standard errors clustered at the year-of-birth level in parentheses. Wave dummies included. Dummies for five levels of educational attainment included, as well as controls for income, unemployment status, year of birth, T and gender. "T" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. The diff-in-diff estimator is captured by the "T*Male" coefficient. First column does not include cohort trend due to perfect collinearity.
*** p<0.01, ** p<0.05, * p<0.1.*

Table A.13: Standard errors clustered by year of birth-gender

Cohorts	1982-1983	1981-1984	1980-1985	1979-1986	1978-1987	1977-1988
	(1)	(2)	(3)	(4)	(5)	(6)
T*Male	.156***	.129***	.122***	.125***	.108***	.092***
	(.009)	(.03)	(.025)	(.02)	(.022)	(.026)
Observations	369	749	1138	1550	1912	2283
R^2	.073	.07	.047	.045	.034	.031

Linear probability models. The dependent variable measures support for redistribution. Robust standard errors clustered at the year-of-birth/gender level in parentheses. Wave dummies included. Dummies for five levels of educational attainment included, as well as controls for income, unemployment status, year of birth, T and gender. "T" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. The diff-in-diff estimator is captured by the "T*Male" coefficient. First column does not include cohort trend due to perfect collinearity.
*** p<0.01, ** p<0.05, * p<0.1.*

Table A.14: Unclustered standard errors

Cohorts	1982-1983	1981-1984	1980-1985	1979-1986	1978-1987	1977-1988
	(1)	(2)	(3)	(4)	(5)	(6)
T*Male	.156**	.129**	.122***	.125***	.108***	.092***
	(.077)	(.056)	(.045)	(.039)	(.035)	(.033)
Observations	369	749	1138	1550	1912	2283
R^2	.073	.07	.047	.045	.034	.031

Linear probability models. The dependent variable measures support for redistribution. Unclustered robust standard errors in parentheses. Wave dummies included. Dummies for five levels of educational attainment included, as well as controls for income, unemployment status, year of birth, T and gender. "T" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. The diff-in-diff estimator is captured by the "T*Male" coefficient. First column does not include cohort trend due to perfect collinearity.
*** p<0.01, ** p<0.05, * p<0.1.*

Section A.7: Parallel trend assumption

I offer in this section a regression analysis in support of the parallel trend assumption in which this methodology implicitly relies on. Females can be considered a valid control group if the trends of cohorts unaffected by the treatment do not differ across genders. As can be noted in first column of Table A.15 by looking at the interaction between gender and year of birth, that is the case for those born after conscription was removed. It also holds for those born prior to 1977—which is the oldest cohort I include in the analyses—and for those born before the 1960s—for which the rate of compliance is quite steady across cohorts. As expected, however, the trends differ in a statistically significant way if looking at the period 1977-1988, which includes both conscripted and non-conscripted cohorts.

Table A.15: Test of parallel trend assumption

Cohorts	>1982	<1977	<1960	1977-1988
	(1)	(2)	(3)	(4)
Year of birth (YOB)	.001	.000	-.001	.005
	(.002)	(.000)	(.001)	(.003)
Male	2.097	-1.174	-1.01	17.765**
	(6.973)	(1.01)	(1.928)	(8.366)
YOB*Male	-.001	.001	.001	-.009**
	(.004)	(.001)	(.001)	(.004)
Income	-.056***	-.04***	-.031***	-.064***
	(.016)	(.007)	(.011)	(.015)
Unemployed	.04*	.021	-.005	.015
	(.021)	(.014)	(.028)	(.024)
Observations	1810	8677	4580	2283
R^2	.038	.02	.016	.023

Linear probability models. The dependent variable measures support for redistribution. Robust standard errors clustered at the cohort-region level in parentheses. Wave dummies included. Dummies for five levels of educational attainment included.
**** p<0.01, ** p<0.05, * p<0.1.*

Section A.8: More robustness checks

Table A.16: Year-of-birth fixed effects

Cohorts	1982-1983	1981-1984	1980-1985	1979-1986	1978-1987	1977-1988
	(1)	(2)	(3)	(4)	(5)	(6)
T'	.156**	.13***	.125***	.126***	.109***	.093***
	(.069)	(.046)	(.036)	(.033)	(.03)	(.028)
Observations	369	749	1138	1550	1912	2283
R^2	.073	.071	.049	.048	.036	.035

Linear probability models. The dependent variable measures support for redistribution. Year-of-birth fixed effects included. Robust standard errors clustered at the cohort-region level in parentheses. Wave dummies included. Dummies for five levels of educational attainment included, as well as controls for gender, income and unemployment status. *T'* takes value 1 for males born in conscripted cohorts, and 0 for males born in non-conscripted cohorts and for all females.
**** p<0.01, ** p<0.05, * p<0.1.*

Table A.17: Controlling for having a fixed-term contract (missing values in this variable treated as 0)

Cohorts	1982-1983	1981-1984	1980-1985	1979-1986	1978-1987	1977-1988
	(1)	(2)	(3)	(4)	(5)	(6)
T*Male	.156**	.128***	.121***	.124***	.107***	.092***
	(.069)	(.046)	(.037)	(.033)	(.03)	(.028)
Fixed-term contract	-.002	.012	.022	.014	.007	.004
	(.033)	(.028)	(.023)	(.02)	(.019)	(.017)
Observations	369	749	1138	1550	1912	2283
R^2	.073	.07	.048	.046	.034	.031

Linear probability models. The dependent variable measures support for redistribution. Robust standard errors clustered at the cohort-region level in parentheses. Wave dummies included. Dummies for five levels of educational attainment included, as well as controls for income, unemployment status, year of birth, T and gender. "T" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. The diff-in-diff estimator is captured by the "T*Male" coefficient. First column does not include cohort trend due to perfect collinearity. "Fixed-term contract" takes value 1 for those holding a limited-duration contract, and 0 otherwise (including missing values in that observation due to huge missingness).
**** p<0.01, ** p<0.05, * p<0.1.*

Table A.18: Controlling for having a fixed-term contract (missing values in this variable excluded)

Cohorts	1982-1983	1981-1984	1980-1985	1979-1986	1978-1987	1977-1988
	(1)	(2)	(3)	(4)	(5)	(6)
T*Male	.224**	.152***	.107**	.095**	.075**	.059*
	(.09)	(.053)	(.044)	(.039)	(.036)	(.032)
Fixed-term contract	-.003	.003	.008	-.001	0	-.001
	(.035)	(.028)	(.023)	(.02)	(.02)	(.017)
Observations	299	626	944	1273	1569	1873
R^2	.107	.104	.056	.051	.035	.034

Linear probability models. The dependent variable measures support for redistribution. Robust standard errors clustered at the cohort-region level in parentheses. Wave dummies included. Dummies for five levels of educational attainment included, as well as controls for income, unemployment status, year of birth, T and gender. "T" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. The diff-in-diff estimator is captured by the "T*Male" coefficient. First column does not include cohort trend due to perfect collinearity. "Fixed-term contract" takes value 1 for those holding a limited-duration contract, and 0 otherwise (missing values in that observation excluded). **** p<0.01, ** p<0.05, * p<0.1.*

Table A.19: Controlling for ever been unemployed (rather than present unemployment status)

Cohorts	1982-1983	1981-1984	1980-1985	1979-1986	1978-1987	1977-1988
	(1)	(2)	(3)	(4)	(5)	(6)
T*Male	.158**	.128***	.123***	.129***	.113***	.095***
	(.068)	(.046)	(.037)	(.033)	(.03)	(.028)
Ever unemployed	.102**	.075**	.064***	.077***	.058***	.036**
	(.044)	(.03)	(.021)	(.017)	(.017)	(.016)
Observations	369	746	1135	1546	1907	2277
R^2	.084	.075	.05	.051	.037	.033

Linear probability models. The dependent variable measures support for redistribution. Robust standard errors clustered at the cohort-region level in parentheses. Wave dummies included. Dummies for five levels of educational attainment included, as well as controls for income, year of birth, T and gender. "T" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. The diff-in-diff estimator is captured by the "T*Male" coefficient. First column does not include cohort trend due to perfect collinearity.*** *p<0.01, ** p<0.05, * p<0.1.*

Table A.20: Controlling for total size of the cohort

Cohorts	1981-1984	1980-1985	1979-1986	1978-1987	1977-1988	1976-1989
	(1)	(2)	(3)	(4)	(5)	(6)
T*Male	.13***	.123***	.125***	.108***	.092***	.051*
	(.046)	(.036)	(.033)	(.03)	(.028)	(.027)
Size of the cohort	-.012	.001	0	.001	0	0
	(.01)	(.002)	(.001)	(.001)	(.001)	(0)
Observations	749	1138	1550	1912	2283	2650
R^2	.071	.048	.046	.034	.031	.024

Linear probability models. The dependent variable measures support for redistribution. Robust standard errors clustered at the cohort-region level in parentheses. Wave dummies included. Dummies for five levels of educational attainment included, as well as controls for income, unemployment status, year of birth, T and gender. "T" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. The diff-in-diff estimator is captured by the "T*Male" coefficient. The comparison of cohorts 1981-1982 (Column 1 in Table 1.1) cannot be performed due to perfect collinearity when controlling for size of the cohort. Size of the cohort in thousands. *** *p<0.01, ** p<0.05, * p<0.1.*

Table A.21: Controlling for male size of the cohort

Cohorts	1981-1984	1980-1985	1979-1986	1978-1987	1977-1988	1976-1989
	(1)	(2)	(3)	(4)	(5)	(6)
T*Male	.13***	.123***	.125***	.108***	.092***	.051*
	(.046)	(.036)	(.033)	(.03)	(.028)	(.027)
Male size of the cohort	-.015	.002	.001	.001	0	0
	(.013)	(.004)	(.003)	(.001)	(.001)	(.001)
Observations	749	1138	1550	1912	2283	2650
R^2	.071	.047	.046	.034	.031	.024

Linear probability models. The dependent variable measures support for redistribution. Robust standard errors clustered at the cohort-region level in parentheses. Wave dummies included. Dummies for five levels of educational attainment included, as well as controls for income, unemployment status, year of birth, T and gender. "T" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. The diff-in-diff estimator is captured by the "T*Male" coefficient. The comparison of cohorts 1981-1982 (Column 1 in Table 1.1) cannot be performed due to perfect collinearity when controlling for male size of the cohort. Male size of the cohort in thousands. *** *p<0.01, ** p<0.05, * p<0.1.*

Table A.22: Controlling for female size of the cohort

Cohorts	1981-1984	1980-1985	1979-1986	1978-1987	1977-1988	1976-1989
	(1)	(2)	(3)	(4)	(5)	(6)
T*Male	.13***	.123***	.125***	.108***	.092***	.051*
	(.046)	(.036)	(.033)	(.03)	(.028)	(.027)
Female size of the	-.05	.003	.001	.001	0	.001
cohort	(.042)	(.003)	(.002)	(.001)	(.001)	(.001)
Observations	749	1138	1550	1912	2283	2650
R^2	.071	.048	.046	.034	.031	.024

Linear probability models. The dependent variable measures support for redistribution. Robust standard errors clustered at the cohort-region level in parentheses. Wave dummies included. Dummies for five levels of educational attainment included, as well as controls for income, unemployment status, year of birth, T and gender. "T" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. The diff-in-diff estimator is captured by the "T*Male" coefficient. The comparison of cohorts 1981-1982 (Column 1 in Table 1.1) cannot be performed due to perfect collinearity when controlling for female size of the cohort. Female size of the cohort in thousands. *** *p<0.01, ** p<0.05, * p<0.1.*

Table A.23: Controlling for relative size of the cohort (male size/female size)

Cohorts	1981-1984	1980-1985	1979-1986	1978-1987	1977-1988	1976-1989
	(1)	(2)	(3)	(4)	(5)	(6)
T*Male	.13***	.125***	.126***	.109***	.092***	.051*
	(.046)	(.036)	(.033)	(.03)	(.028)	(.027)
Relative size of the	-5.605	-3.63	-1.197	-1.409	-.198	-1.035
cohort	(4.713)	(2.623)	(1.682)	(1.635)	(1.491)	(1.236)
Observations	749	1138	1550	1912	2283	2650
R^2	.071	.049	.046	.034	.031	.024

Linear probability models. The dependent variable measures support for redistribution. Robust standard errors clustered at the cohort-region level in parentheses. Wave dummies included. Dummies for five levels of educational attainment included, as well as controls for income, unemployment status, year of birth, T and gender. "T" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. The diff-in-diff estimator is captured by the "T*Male" coefficient. The comparison of cohorts 1981-1982 (Column 1 in Table 1.1) cannot be performed due to perfect collinearity when controlling for relative size of the cohort.
**** p<0.01, ** p<0.05, * p<0.1.*

Table A.24: Controlling for gender-specific size of the cohort

Cohorts	1981-1984	1980-1985	1979-1986	1978-1987	1977-1988	1976-1989
	(1)	(2)	(3)	(4)	(5)	(6)
T*Male	.199***	.11***	.122***	.102***	.093***	.049*
	(.071)	(.041)	(.034)	(.031)	(.028)	(.028)
Gender-specific size	-.017	.003	.001	.001	0	0
of the cohort	(.016)	(.004)	(.002)	(.001)	(.001)	(.001)
Observations	749	1138	1550	1912	2283	2650
R^2	.071	.048	.046	.034	.031	.024

Linear probability models. The dependent variable measures support for redistribution. Robust standard errors clustered at the cohort-region level in parentheses. Wave dummies included. Dummies for five levels of educational attainment included, as well as controls for income, unemployment status, year of birth, T and gender. "T" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. The diff-in-diff estimator is captured by the "T*Male" coefficient. "Gender-specific size of the cohort" (in thousands) takes the male-size value of the cohort for males, and the female-size value of the cohort for females. The comparison of cohorts 1981-1982 (Column 1 in Table 1.1) cannot be performed due to perfect collinearity when controlling for gender-specific size of the cohort.
**** p<0.01, ** p<0.05, * p<0.1.*

Table A.25: Controlling for living with spouse/partner and by having children living at home

Cohorts	1982-1983	1981-1984	1980-1985	1979-1986	1978-1987	1977-1988
	(1)	(2)	(3)	(4)	(5)	(6)
T*Male	.149*	.115**	.099**	.115***	.094***	.08***
	(.082)	(.051)	(.039)	(.035)	(.033)	(.03)
Living with	-.082*	-.012	-.008	-.001	.001	-.007
spouse/partner	(.048)	(.03)	(.023)	(.021)	(.019)	(.018)
Children living	.038	.003	-.023	-.013	.004	.015
at home	(.05)	(.036)	(.029)	(.024)	(.022)	(.022)
Observations	324	660	1008	1362	1685	2024
R^2	.074	.066	.047	.043	.032	.03

Linear probability models. The dependent variable measures support for redistribution. Robust standard errors clustered at the cohort-region level in parentheses. Wave dummies included. Dummies for five levels of educational attainment included, as well as controls for income, unemployment status, year of birth, T and gender. "T" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. The diff-in-diff estimator is captured by the "T*Male" coefficient. First column does not include cohort trend due to perfect collinearity.
**** p<0.01, ** p<0.05, * p<0.1.*

Table A.26: Controlling for level of trust in the legal system

Cohorts	1982-1983	1981-1984	1980-1985	1979-1986	1978-1987	1977-1988
	(1)	(2)	(3)	(4)	(5)	(6)
T*Male	.174**	.136***	.128***	.129***	.115***	.097***
	(.066)	(.045)	(.036)	(.033)	(.03)	(.028)
Trust in the	-.005	-.011**	-.009**	-.007*	-.006*	-.009***
legal system	(.008)	(.005)	(.004)	(.004)	(.003)	(.003)
Observations	365	741	1127	1534	1891	2256
R^2	.076	.074	.051	.047	.036	.034

Linear probability models. The dependent variable measures support for redistribution. Robust standard errors clustered at the cohort-region level in parentheses. Wave dummies included. Dummies for five levels of educational attainment included, as well as controls for income, unemployment status, year of birth, T and gender. "T" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. The diff-in-diff estimator is captured by the "T*Male" coefficient. First column does not include cohort trend due to perfect collinearity. "Trust in the legal system" takes a value from 0 (not trust at all) to 10 (complete trust).
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.27: Controlling for level of trust in country's parliament

Cohorts	1982-1983	1981-1984	1980-1985	1979-1986	1978-1987	1977-1988
	(1)	(2)	(3)	(4)	(5)	(6)
T*Male	.163**	.128***	.115***	.117***	.103***	.091***
	(.067)	(.044)	(.035)	(.033)	(.03)	(.028)
Trust in	-.014	-.013**	-.013***	-.01**	-.007*	-.009**
country's parliament	(.009)	(.006)	(.005)	(.004)	(.004)	(.004)
Observations	363	729	1105	1502	1856	2214
R^2	.076	.076	.055	.049	.035	.033

Linear probability models. The dependent variable measures support for redistribution. Robust standard errors clustered at the cohort-region level in parentheses. Wave dummies included. Dummies for five levels of educational attainment included, as well as controls for income, unemployment status, year of birth, T and gender. "T" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. The diff-in-diff estimator is captured by the "T*Male" coefficient. First column does not include cohort trend due to perfect collinearity. "Trust in country's parliament" takes a value from 0 (not trust at all) to 10 (complete trust).
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.28: Controlling for level of trust in political parties

Cohorts	1982-1983	1981-1984	1980-1985	1979-1986	1978-1987	1977-1988
	(1)	(2)	(3)	(4)	(5)	(6)
T*Male	.171**	.136***	.125***	.124***	.109***	.092***
	(.067)	(.045)	(.037)	(.034)	(.031)	(.029)
Trust in	-.003	-.017**	-.017***	-.013***	-.007	-.008**
political parties	(.01)	(.007)	(.006)	(.005)	(.004)	(.004)
Observations	349	710	1076	1470	1812	2166
R^2	.073	.083	.055	.049	.033	.03

Linear probability models. The dependent variable measures support for redistribution. Robust standard errors clustered at the cohort-region level in parentheses. Wave dummies included. Dummies for five levels of educational attainment included, as well as controls for income, unemployment status, year of birth, T and gender. "T" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. The diff-in-diff estimator is captured by the "T*Male" coefficient. First column does not include cohort trend due to perfect collinearity. "Trust in political parties" takes a value from 0 (not trust at all) to 10 (complete trust). *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.29: Controlling for level of trust in politicians

Cohorts	1982-1983	1981-1984	1980-1985	1979-1986	1978-1987	1977-1988
	(1)	(2)	(3)	(4)	(5)	(6)
T*Male	.157**	.126***	.121***	.123***	.109***	.093***
	(.07)	(.046)	(.037)	(.034)	(.031)	(.028)
Trust in	.000	-.008	-.008	-.006	-.004	-.007*
politicians	(.009)	(.007)	(.005)	(.005)	(.004)	(.004)
Observations	368	742	1128	1540	1897	2266
R^2	.073	.072	.048	.046	.034	.032

Linear probability models. The dependent variable measures support for redistribution. Robust standard errors clustered at the cohort-region level in parentheses. Wave dummies included. Dummies for five levels of educational attainment included, as well as controls for income, unemployment status, year of birth, T and gender. "T" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. The diff-in-diff estimator is captured by the "T*Male" coefficient. First column does not include cohort trend due to perfect collinearity. "Trust in politicians" takes a value from 0 (not trust at all) to 10 (complete trust).
*** p<0.01, ** p<0.05, * p<0.1.

Table A.30: Controlling for level of institutional trust

Cohorts	1982-1983	1981-1984	1980-1985	1979-1986	1978-1987	1977-1988
	(1)	(2)	(3)	(4)	(5)	(6)
T*Male	.157**	.13***	.124***	.125***	.109***	.093***
	(.069)	(.045)	(.036)	(.033)	(.03)	(.028)
Institutional trust	-.009	-.018**	-.017***	-.013**	-.009**	-.013***
	(.01)	(.007)	(.006)	(.005)	(.005)	(.004)
Observations	368	746	1134	1546	1908	2278
R^2	.075	.077	.054	.049	.036	.035

Linear probability models. The dependent variable measures support for redistribution. Robust standard errors clustered at the cohort-region level in parentheses. Wave dummies included. Dummies for five levels of educational attainment included, as well as controls for income, unemployment status, year of birth, T and gender. "T" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. The diff-in-diff estimator is captured by the "T*Male" coefficient. First column does not include cohort trend due to perfect collinearity. "Institutional trust" is the average of "Trust in the legal system", "Trust in country's parliament", "Trust in political parties" and "Trust in politicians".
*** p<0.01, ** p<0.05, * p<0.1.

Table A.31: Controlling for years of full-time education completed instead of level of educational attainment

Cohorts	1982-1983	1981-1984	1980-1985	1979-1986	1978-1987	1977-1988
	(1)	(2)	(3)	(4)	(5)	(6)
T*Male	.145**	.119**	.119***	.123***	.107***	.092***
	(.071)	(.047)	(.037)	(.033)	(.03)	(.028)
Years of education	-.004	-.003	-.003	-.004*	-.001	-.002
	(.004)	(.003)	(.002)	(.002)	(.002)	(.002)
Observations	370	748	1135	1545	1905	2271
R^2	.067	.05	.042	.042	.033	.031

Linear probability models. The dependent variable measures support for redistribution. Robust standard errors clustered at the cohort-region level in parentheses. Wave dummies included, as well as controls for income, unemployment status, year of birth, T and gender. "T" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. The diff-in-diff estimator is captured by the "T*Male" coefficient. First column does not include cohort trend due to perfect collinearity.
*** p<0.01, ** p<0.05, * p<0.1.

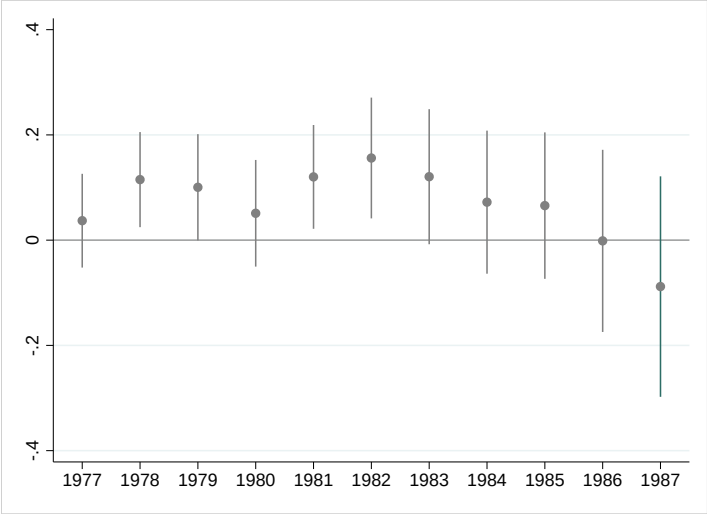
Section A.9: Manipulation check about having a partner with a different level of educational attainment

Table A.32: Effect on the likelihood of having a partner with a different level of educational attainment—Let's open the windows more

Cohorts	1982-1983	1981-1984	1980-1985	1979-1986	1978-1987	1977-1988	1976-1989	1975-1990	1974-1991	1973-1992
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
T (Born in a	.031	-.039	.036	.019	-.028	.014	.012	-.014	-.045	-.035
conscripted cohort)	(.077)	(.111)	(.083)	(.068)	(.06)	(.057)	(.055)	(.049)	(.048)	(.049)
Male	-.175*	-.151**	-.139**	-.172***	-.157***	-.161***	-.155***	-.143***	-.141***	-.137***
	(.087)	(.072)	(.067)	(.055)	(.05)	(.05)	(.048)	(.047)	(.046)	(.044)
T*Male	.125	.202**	.095	.113*	.138**	.156***	.144***	.138**	.144***	.144***
	(.098)	(.081)	(.08)	(.064)	(.06)	(.058)	(.054)	(.055)	(.053)	(.051)
Year of birth	-	-.009	.01	.004	-.007	.006	.006	0	-.006	-.004
		(.048)	(.023)	(.015)	(.011)	(.009)	(.008)	(.005)	(.004)	(.004)
Observations	213	442	648	890	1109	1355	1588	1822	2043	2297
R^2	.063	.033	.018	.02	.014	.01	.008	.007	.009	.008

Linear probability models. The dependent variable takes value 1 in the case of a heterogamous match, and 0 if the respondent holds the same level of educational attainment than his/her partner. Robust standard errors clustered at the cohort-region level in parentheses. Wave dummies included. "Born in a conscripted cohort" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. The diff-in-diff estimator is captured by the "T*Male" coefficient. First column does not include cohort trend due to perfect collinearity.
**** p<0.01, ** p<0.05, * p<0.1.*

Figure A.5: Having a partner with a different level of educational attainment (Placebo analysis with false conscription removal dates)



Placebo analysis. Point estimates and 95% confidence intervals for the diff-in-diff coefficient in models paralleling those from Figure 1.9 (size-6 windows). The x-axis refers to *f*, namely, the cohort that I artificially set as the last forced to do conscription in this placebo analysis—1982 is the correct specification.

Table A.33: Effect on the likelihood of having a partner with a different level of educational attainment—controlling for individual's and year-specific aggregated level of education (I)

Cohorts	1982-1983	1981-1984	1980-1985	1979-1986	1978-1987	1977-1988
	(1)	(2)	(3)	(4)	(5)	(6)
T (Born in a	-.067	-.017	.038	.036	.001	.03
conscripted cohort)	(.105)	(.117)	(.089)	(.074)	(.066)	(.06)
Male	-.192*	-.148*	-.126**	-.167***	-.15***	-.15***
	(.104)	(.076)	(.064)	(.054)	(.051)	(.049)
T*Male	.137	.183*	.075	.1	.116*	.139**
	(.137)	(.095)	(.08)	(.068)	(.062)	(.058)
Year of birth	-	.01	.009	.026	.026	.026
		(.055)	(.034)	(.025)	(.021)	(.017)
Age	.069*	.014	.007	.023	.03	.018
	(.041)	(.028)	(.024)	(.02)	(.018)	(.016)
Proportion with	.681	.35	.407	.172	-.144	.094
tertiary education	(1.932)	(1.244)	(1.03)	(.879)	(.756)	(.679)
Proportion with less	5.676*	.95	.386	1.544	2.143	1.299
than secondary	(3.376)	(2.339)	(1.978)	(1.699)	(1.521)	(1.36)
Observations	213	442	648	890	1109	1355
R^2	.072	.081	.06	.064	.072	.069

Linear probability models. The dependent variable takes value 1 in the case of a heterogamous match, and 0 if the respondent holds the same level of educational attainment than his/her partner. Robust standard errors clustered at the cohort-region level in parentheses. Dummies for five levels of educational attainment included. "Born in a conscripted cohort" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. The diff-in-diff estimator is captured by the "T*Male" coefficient. First column does not include cohort trend due to perfect collinearity. "Proportion with tertiary education" and "Proportion with less than secondary" takes the value of the proportion of individuals in the given year who hold, respectively, tertiary education and less than secondary education.
**** p<0.01, ** p<0.05, * p<0.1.*

Table A.34: Effect on the likelihood of having a partner with a different level of educational attainment—controlling for individual's and year-specific aggregated level of education (II)

Cohorts	1982-1983	1981-1984	1980-1985	1979-1986	1978-1987	1977-1988
	(1)	(2)	(3)	(4)	(5)	(6)
T (Born in a	-.015	-.011	.041	.037	.002	.03
conscripted cohort)	(.095)	(.116)	(.088)	(.074)	(.066)	(.06)
Male	-.181*	-.145*	-.126**	-.167***	-.149***	-.15***
	(.104)	(.075)	(.064)	(.054)	(.051)	(.049)
T*Male	.117	.181*	.075	.1	.115*	.139**
	(.137)	(.095)	(.08)	(.068)	(.062)	(.058)
Year of birth	-	.009	.011	.015	.01	.017
		(.05)	(.028)	(.018)	(.014)	(.011)
Age	.037	.011	.008	.012	.014	.01
	(.024)	(.015)	(.013)	(.011)	(.01)	(.008)
Mean level	-.853	-.169	-.078	-.161	-.304	-.187
of education	(.662)	(.433)	(.36)	(.311)	(.272)	(.242)
Observations	213	442	648	890	1109	1355
R^2	.066	.08	.06	.063	.071	.069

Linear probability models. The dependent variable takes value 1 in the case of a heterogamous match, and 0 if the respondent holds the same level of educational attainment than his/her partner. Robust standard errors clustered at the cohort-region level in parentheses. Dummies for five levels of educational attainment included. "Born in a conscripted cohort" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. The diff-in-diff estimator is captured by the "T*Male" coefficient. First column does not include cohort trend due to perfect collinearity. "Mean level of education" is the mean of all individuals' level of education (ranging from 1 to 5) in the given year.
**** p<0.01, ** p<0.05, * p<0.1.*

Section A.10: Heterogeneity by some socioeconomic characteristics indicating exposure to economic hardship and inequality

If inter-class contact in the military service heightened support for redistribution, one would expect a stronger effect among those who have had fewer chances to learn about inequality and economic hardship through other means (see Minkoff and Lyons, 2019, for a similar conditional hypothesis). Namely, those who have been exposed to material difficulties and unequal settings in other ways throughout their lives should experience a limited impact of conscription.

For each of the three analyses in this section (having a highly educated father, having ever experienced unemployment, and having experienced intergenerational mobility), I split the sample in two (e.g., those who have ever been unemployed and those who have not) and re-estimate the model of Table 1.1. As each of these analyses can include a very different number of observations (i.e., there are many more observations in which the father's individual did not completed tertiary education than observations in which the father did), I show in Figure A.6 the number of observations employed in each of the analyses of Figures A.7-A.9.

Figure A.6: Number of observations in analyses of Figures A.7-A.9

	Subsample	Size 1	Size 2	Size 3	Size 4	Size 5	Size 6
Figure A.7	Non highly edu. father	-	609	917	1241	1543	1845
	Highly educated father	-	104	158	218	263	308
Figure A.8	Ever unemployed	193	380	571	767	939	1130
	Never unemployed	176	366	564	779	968	1147
Figure A.9	Mobiles	97	209	312	413	523	640
	Non-mobiles	256	504	763	1046	1283	1513

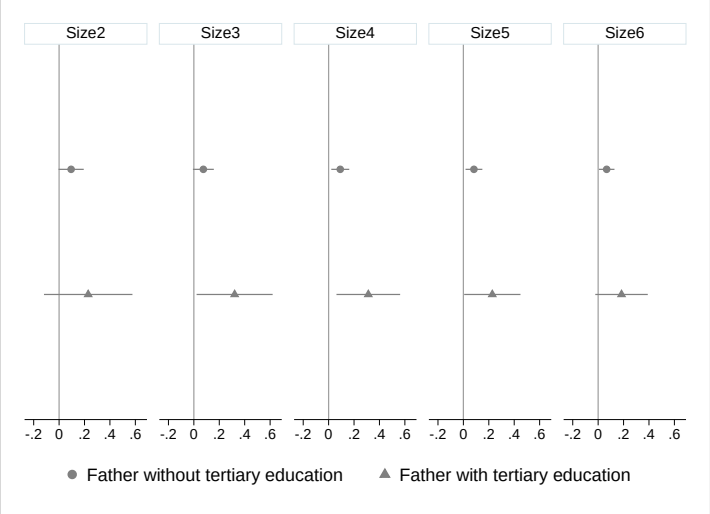
I do not include as part of this section the analysis of income heterogeneity. Even if some authors may have suggested that individuals in different positions of the income distribution may have different chances to learn about inequality (Minkoff and Lyons, 2019), I consider that analysis more prone to be confounded with the argument about other-regarding considerations being less important for the poor as the stakes of redistribution for them are much more dramatic than for the rich (e.g., Rueda and Stegmueller, 2019). Then, as far as possible I aim to look at non-present socioeconomic characteristics. Of course, one could argue that these analyses are not free of this problem. For example, having ever been unemployed is correlated with present unemployment status. Still, I aim to reduce this type of concern in different ways. For example, I

obtain similar results in Figure A.8 if controlling for present unemployment status or discarding from the analysis those who are currently unemployed. Or, in the analysis about heterogeneity by father's level of educational attainment (Figure A.7), I control for present socioeconomic indicators, such as individual's income or level of education—of course that does not fully solve the problem, as those coming from a higher class of origin may present a larger level of inherited wealth.

Heterogeneous effects by father's level of educational attainment

I first look at whether the results are different by class of origin (measured in educational terms). I have split the sample in two: those whose father completed tertiary education and those whose father did not. Then, I re-estimate Table 1.1 separately for each of the subsamples. I show in Figure A.7 the point estimates and 95% confidence intervals for the diff-in-diff coefficient for windows of size 2 to 6. [9] As can be seen, the diff-in-coefficients are larger for those with a highly educated father, though the estimation is less precise, maybe because the sample is much smaller (e.g., in the size-6 window, the number of observations is 1,845 for the subsample of individuals whose father did not complete tertiary education, but only 308 for the subsample of individuals whose father did).

Figure A.7: Heterogeneity by father's level of educational attainment



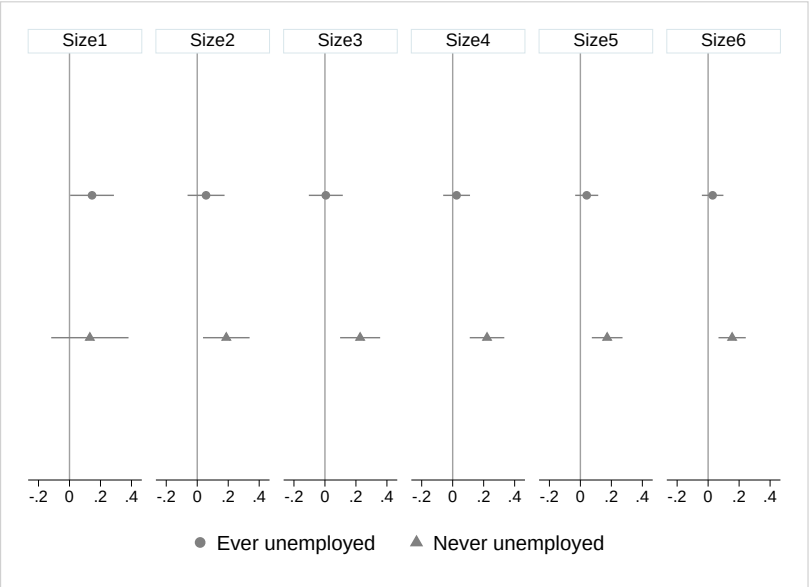
Point estimates and 95% confidence intervals for the diff-in-diff coefficient in linear probability models paralleling those from Table 1.1. For each window, the upper level analyzes those whose father did not complete tertiary education, and the lower level those whose father did.

[9] Size-1 window contains a very low number of individuals whose father completed tertiary education (around 50). The confidence interval is so large compared to the others in the graph that including this window alters the visual representation of the whole figure. Therefore, I have decided to drop the size-1 window from the figure.

Heterogeneous effects by experience of ever been unemployed

I look now at whether the results are different for those who have ever experienced unemployment—ddefined in the ESS as "ever unemployed and seeking work for a period of more than three months—and for those who have not. I re-estimate Table 1.1 for each of the two subsamples separately. [10] Point estimates and 95% confidence intervals for the diff-in-diff coefficient are displayed in Figure A.8. The diff-in-coefficients are larger for those who have never been unemployed in every but the size-1 specification. Except for that one, another pattern arises too: the diff-in-diff effect is statistically significant for those who have never been unemployed but not for those who have (see Figure A.6 to note that this does not seem to be a consequence of different sample sizes). I argue that the rationale for this is that the latter group has had more chances to learn about economic hardship throughout their lives, experiencing a limited effect of conscription.

Figure A.8: Heterogeneity by experienced unemployment



Point estimates and 95% confidence intervals for the diff-in-diff coefficient in linear probability models paralleling those from Table 1.1 (but not controlling for present unemployment). For each window, the upper level analyzes those who have ever been unemployed, and the lower level those who have not.

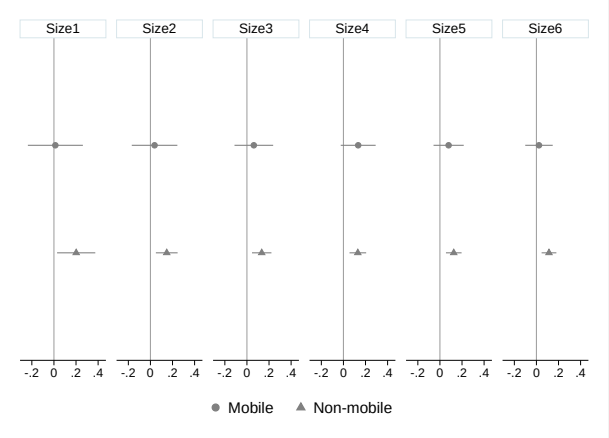
[10] The results below do not control for present unemployment status (that variable takes value 1 for those unemployed in the last 7 days). However, the results are not different if I control for that or if I discard from the sample those currently unemployed. Note also that according to these definitions, there could be individuals currently unemployed but who have never experienced unemployment, as the latter requires three months of unemployment while the former only requires being unemployed during the 7 days prior to the ESS interview. These represent, in any case, an extremely low proportion of respondents.

Heterogeneous effects by experience of intergenerational mobility

The two previous analyses show a more pronounced diff-in-diff effect for those less likely to have been exposed to economic hardship through other means than inter-class contact. However, this does not imply that they are also less likely to have been exposed to inequality, namely to both advantaged and disadvantaged worlds. For that reason, I turn now to a different phenomenon: intergenerational mobility. Those intergenerationally non-mobile should have been exposed to more economic homogeneity throughout their lives, as they have grown up in atmospheres economically similar to the ones surrounding them in their adulthoods. However, those who have moved up or down in the social ladder should have been more exposed to both advantaged and disadvantaged environments in their lives. Therefore, I expect the effect of conscription to be stronger among the former group: the intergenerationally non-mobiles.

I look in the present analysis at educational mobility, distinguishing between completing tertiary education or not (a key determinant of life chances in modern societies). Mobile individuals are those who have attained a different level of education than their father, either because their father completed tertiary education and they have not, or because their father did not complete tertiary education, but they have. As can be noted in Figure A.9, these individuals present lower diff-in-diff effects in all but one window specification, in many cases these being indeed statistically and substantively very close to zero. However, it is a very different story for those non-mobile, for whom the diff-in-diff effect is always significant. It is true too, however, that the estimation for the subsample of those mobile is less precise than that of those non-mobile, as the number of observations is quite lower (see Figure A.6).

Figure A.9: Heterogeneity by intergenerational mobility



Point estimates and 95% confidence intervals for the diff-in-diff coefficient in linear probability models paralleling those from Table 1.1. For each window, the upper level analyzes those who have attained a different level of education (tertiary or not) than their father, and the lower level those who have not.

Section A.11: Heterogeneous effects by individual's income

Authors such as Bailey et al. (2013), Franko and Livingston (2020) or Rueda and Stegmueller (2019) find the effect of contextual variables on preferences for redistribution to be heterogeneous by individuals' income. Whereas the preferences of the richer are much more dependent on contextual variables, those of the poorer are quite stable, as the stakes of redistribution are much more dramatic for the latter than for the former.

Minkoff and Lyons (2019), however, find that the impact of neighbourhood's economic diversity is stronger for both those with low and with high incomes rather than for those in the middle of the distribution. They argue that middle-income people have more interactions with other classes out of the neighbourhood than the rich and the poor, having more options to learn about inequality processes in those settings, making the neighbourhood effect limited for them. According to these authors, that is not the case for those in the extremes of the economic distribution, whose patterns of socialization in the neighbourhood are reproduced out of it.

In this section, I address whether there exists in my data some heterogeneity by income. For simplicity in presenting the results, I will focus on males. I re-estimate the model in Table A.9, adding an interaction between being born in a conscripted cohort and income. First column presents income as defined through the rest of the article (see section A.1). For column 2, however, observations are grouped into group deciles for later graphically present the results of the analysis. As can be seen in Table A.35, the interaction effect is not distinguishable from zero measured in either way.

In Figure A.10, I show the effect of being born in a conscripted cohort for the different income deciles. The effect is positive and statistically significant across every decile. Although I focus here on the size-6 window (1976-1989), the lack of a significant interaction effect between income and being born in a conscripted cohort persists across different windows. The only difference I perceive with respect to other specifications is that, for some windows, the effect of being born in a conscripted cohort lacks statistical significance for the very lowest deciles. In summary, no clear heterogeneous effect by income is observed. If anything, the effect is marginally higher for higher-income individuals, as one could expect from studies as Bailey et al. (2013) and Rueda and Stegmueller (2019).

The finding of a positive effect also for the richer supports the idea that what exists here is *deep* contact between classes, in opposition to the plausible negative effect of *casual* contact (Sands, 2017, Sands and de Kadt, 2020), which could make the richer to lower their support for redistribution after realizing that their actual position in the income distribution is higher than they used to think. It also helps to discard that my findings result from some policy affecting only some income classes of Spanish society.

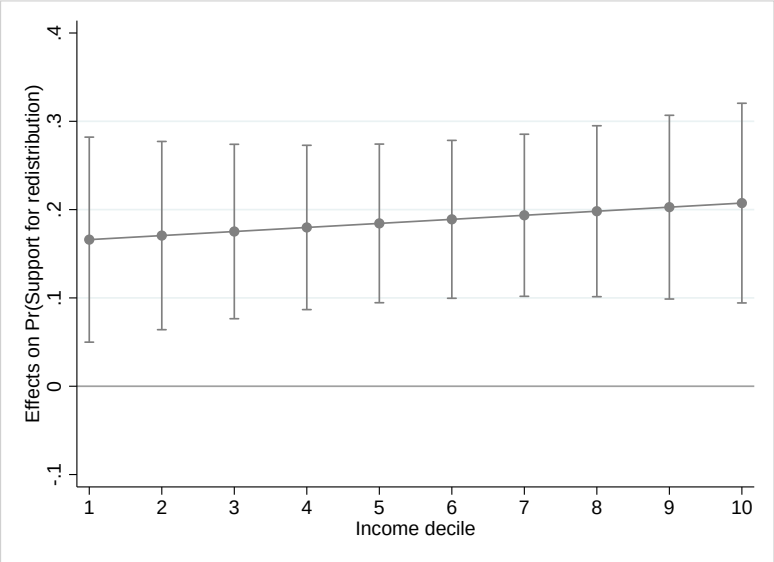
On a different note, one could interpret this stability across income levels against the plausibility of the empathy mechanism—which should affect mostly the richer, unlike the other informational mechanisms, which should affect both the more and the less privileged. However, the fact that more educated people—who tend to be richer—were less likely to join conscription makes that interpretation not straightforward, as diminished levels of compliance with military service among the richer might be cancelling out their larger treatment effects due to the empathy mechanism.

Table A.35: Heterogeneity by income (Only males)

Cohorts	1977-1988	
	(1)	(2)
T (Born in a conscripted	.181***	.173***
Cohort)	(.057)	(.065)
Income/Income decile	-.047	-.01
	(.03)	(.007)
Income/decile*Male	.01	.003
	(.037)	(.008)
Year of birth	.021***	.02***
	(.007)	(.007)
Unemployed	.031	.027
	(.029)	(.03)
Observations	1180	1180
R^2	.046	.045

Linear probability models including only males. The dependent variable measures support for redistribution. Robust standard errors clustered at the cohort-region level in parentheses. Wave dummies included. Dummies for five levels of educational attainment included. "Born in a conscripted cohort" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. Column (1) employs income, column (2) employs income decile (see text). *** p<0.01, ** p<0.05, * p<0.1.

Figure A.10: Conditional marginal effects of being born in a conscripted cohort



Point estimates and 95% confidence intervals of the effect of T in model (2) of Table A.35, for each of the income deciles.

Section A.12: Moderated-mediation analyses

In this section of the Appendix, I address through moderated-mediation analyses if the effect of being born in a conscripted cohort on preferences for redistribution can be explained through an indirect effect on socioeconomic variables—income, educational attainment, present unemployment status, an indicator of ever been unemployed, and an indicator of having a fixed-term contract—or on levels of institutional trust—an index of trust in the legal system/parliament/parties/politicians.

For these analyses, I employ a Structural Equation Modelling (SEM) framework, cluster standard errors at the cohort-region level and focus on size-6 windows of cohorts—namely, those born between 1977 and 1988. I show in the different columns of Table A.36 the direct effect, the indirect effect—namely, the effect operating through the mediator(s)—and the total effect—the sum of the indirect and the direct effect—for males and for females. I let the gender dummy (i.e., the moderator) to moderate both the path of being born in a conscripted cohort on the mediator(s) and the path of the mediator(s) on support for redistribution, as well as to moderate the effect of the control variables.

The results seem quite clear. Regardless of the specification we look at, being born in a conscripted cohort has a strong effect on support for redistribution for males but not for females, and this effect is not mediated by any of the socioeconomic variable(s) considered or by the level of institutional trust. Namely, this is a direct effect, giving credit to the idea that the robust diff-in-diff effect found throughout the paper may be the result of heightened inter-class contact in cohorts of conscripted males, rather than a product of worse socioeconomic conditions or reduced levels of institutional trust.

Table A.36: Effect of being born in a conscripted cohort (Moderated-mediation analyses)

Independent variable: born in a conscripted cohort	Direct effect	Indirect effect	Total effect
Mediators: income, unemployed, education (dummies for the different levels of educational attainment)			
Males	.191***	.003	.194***
	(.042)	(.005)	(.042)
Females	.046	-.002	.044
	(.045)	(.007)	(045)
Mediators: income, unemployed, education (ordered categorical variable)			
Males	.191***	.003	.194***
	(.042)	.005	(.042)
Females	.046	-.002	.044
	(.045)	.006	(.045)
Mediator: income			
Males	.189***	.004	.194***
	(.042)	(.004)	(.042)
Females	.046	-.002	.044
	(.045)	(.006)	(.045)
Mediator: education (in dummies)			
Males	.195***	-.002	.194***
	(.042)	(.003)	(.042)
Females	.045	-.001	.044
	(.045)	(.004)	(.045)
Mediator: education (ordered categorical variable)			
Males	.195***	-.002	.194***
	(.042)	(.003)	(.042)
Females	.045	-.002	.044
	(.045)	(.002)	(.045)
Mediator: unemployed			
Males	.193***	.001	.194***
	(.042)	(.002)	(.042)
Females	.045	-.001	.044
	(.045)	(.002)	(.045)
Mediator: fixed-term contract (missing excluded)			
Males	.193***	.001	.194***
	(.042)	(.002)	(.042)
Females	.044	-.000	.044
	(.045)	(.001)	(.045)
N		2283	
Mediator: fixed-term contract (missing =0)			
Males	.221***	.000	.221***
	(.048)	(.002)	(.048)
Females	.060	-.000	.059
	(.045)	(.002)	(.045)
N		1873	
Mediator: ever unemployed			
Males	.194***	.001	.194***
	(.046)	(.003)	(.046)
Females	.045	-.000	.044
	(.040)	(.003)	(.040)
N		2277	
Mediator: institutional trust			
Males	.200***	-.005	.194***
	(.045)	(.004)	(.045)
Females	.047	-.003	.044
	(.051)	(.003)	(.051)
N		2278	

Standard errors clustered at the cohort-region level in parentheses. The dependent variable measures support for redistribution. Control variables in all models: year of birth and ESS round dummies. "Born in a conscripted cohort" takes value 1 if the respondent is born in 1982 or before, and 0 otherwise. **** p<0.01, ** p<0.05, * p<0.1.*

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Section B.1: Heterogeneity by educational qualification

Following Minkoff and Lyons (2019), I look in this section at heterogeneity by educational attainment, distinguishing between those highly educated and those who are not. Whereas the former have more chances to learn about inequality through other means—especially via formal learning, but also through news consumption or discussion with others, which may have an educational gradient (Anderson and Paskeviciute, 2005; Strömbäck et al., 2013)—the latter may enjoy a larger impact of being personally exposed to unequal settings by meeting people from a different socioeconomic reality.

To address the existence of heterogeneity by educational attainment, I split the sample into two: those who have at least a degree/diploma, and those who have a lower qualification (vocational qualification, higher school level, and low school level or below). I estimate separately for these two groups models analogous to those in columns (3), (4), (7) and (8) of Table 2.1—I exclude the others for clarity of presentation.[1]

As can be observed in Table B.1, those who hold at least a degree/diploma do not enjoy a significant change in their economic preferences after entering a cross-class domestic partnership relative to the change experienced by those entering a within-class one. As they have more possibilities to learn about inequality through means other than personal experience, I argue that they are not "activated" by contact with new socioeconomic realities. However, for those with a lower level of educational attainment, this is a completely different story. As these may lack the large levels of information that formal education allows for, they may be more easily "activated" by the information about inequality personally provided in their lives by entering a domestic partnership with someone from a different social position. For them, I find a significant change towards the left in their economic preferences.[2]

[1] Similar results are obtained if instead of splitting the sample into two, I include an interaction between having a degree/diploma and the dummies for cross-class domestic partnership.

[2] Note that although the sample sizes of those who hold a degree/diploma and of those who do not are very different, the magnitudes of the coefficients are very distinct too.

Table B.1: Heterogeneity by educational qualification

	Index of attitudes towards the economy							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Less than	At least	Less than	At least	Less than	At least	Less than	At least
	Degree/Diploma		Degree/Diploma		Degree/Diploma		Degree/Diploma	
Lag of index	.554***	.666***	.508***	.606***	.554***	.664***	.507***	.605***
	(.027)	(.042)	(.028)	(.043)	(.027)	(.042)	(.028)	(.043)
Male	-.042	-.059	-.051*	-.104**	-.045	-.058	-.06**	-.104**
	(.028)	(.042)	(.027)	(.041)	(.028)	(.042)	(.028)	(.041)
Δ in HH income	-.63***	-.138	-.433*	.018	-.652***	-.14	-.485**	.018
	(.243)	(.172)	(.242)	(.17)	(.246)	(.174)	(.244)	(.171)
Age	0	0	0	0	0	0	0	0
	(.001)	(.003)	(.001)	(.003)	(.001)	(.003)	(.001)	(.003)
Unemployed	.072	.092	.066	.065	.072	.089	.066	.064
	(.053)	(.126)	(.052)	(.117)	(.053)	(.125)	(.052)	(.118)
Partner's attitudes	-	-	.201***	.242***	-	-	.206***	.242***
			(.027)	(.039)			(.027)	(.039)
Cross-class partnership	.087***	.04	.084***	.002	-	-	-	-
	(.03)	(.044)	(.029)	(.042)				
Cross-class partnership (down)	-	-	-	-	.074**	.027	.051	.001
					(.035)	(.05)	(.035)	(.048)
Cross-class partnership (up)	-	-	-	-	.098***	.057	.113***	.003
					(.033)	(.054)	(.032)	(.053)
Within-class partnership (Reference)	-	-	-	-	-	-	-	-
Observations	1046	426	1006	414	1046	426	1006	414
R-squared	.329	.473	.367	.529	.329	.473	.369	.529

Robust standard errors in parentheses. Odd columns display results for individuals with less than a degree/diploma qualification (at time t-1). Even columns, for those with at least a degree/diploma (at time t-1). The dependent variable is an index of attitudes towards the economy, with larger values indicating more left-wing attitudes. Dummies for respondent's level of education and for BHPS wave included. The sample includes those who have married or begun to cohabitate with his/her partner for the first time in their time in the BHPS between the present and the previous wave.
**** p<.01, ** p<.05, * p<.1*

Section B.2: Alternative explanations for the heterogeneous findings

I propose above that heterogenous findings across different groups arise from different opportunities to learn about inequality through means other than personal experience (e.g., Minkoff and Lyons, 2019). However, although that is the most coherent and plausible explanation given some findings below, it is not the only possible one.

The BHPS has information on when someone marries or begins to cohabitate with his/her partner, but not on the moment at which they meet or start dating. Although ideally I would like to observe the latter, it is the former what I see. One could argue that a plausible implication of this should be that I will not observe a change in economic attitudes for individuals who have been dating their partner for a long time, as this attitudinal change may have occurred earlier in time for them. If so, this could exert a downward bias in the estimate of the true effect of start dating someone from a different class, making it more difficult to find a positive effect on economic preferences.

An inspection of the existing time span between the wave after which the individual marries (*t*) and the previous wave (*t-1*) shows an average of 2.94 years for the 1,472 individuals employed in the main analyses, with the vast majority of observations concentrated between 2 and 4 years.[3] Therefore, it is plausible to think that for some individuals the attitudinal change will indeed have occurred earlier than *t-1*.[4]

What if those uninterested in politics or those not-highly educated date shorter? If so, this could explain the heterogeneous findings. Obviously, I cannot check directly for it as I do not have information on time of meeting/start dating. However, one could theorize that those from a higher social position—those interested in politics and those highly educated indeed belong to higher classes in my sample—date for a longer period of time as they might move and/or marry at an older age, given university delays entrance into the labour market, therefore increasing the likelihood of a long-period relationship before marriage/moving together.[5,6]

Something that I can indeed observe is the age at which the individual marry/begins to cohabitate, which should correlate positively with the duration of the relationship before marrying/starting to cohabitate, as an older age of marriage/cohabitation by definition increases the possibility of having a longer dating relationship. Then, let us employ for a moment age at marriage/cohabitation as a (crude) proxy of having dated for a longer time. If I look at my sample of those who have just married/started to cohabitate, I find a positive statistically significant correlation between age at entering domestic partnership and being interested in politics or holding a degree/diploma. Therefore, the heterogenous results could be explained too by those keen on politics moving together later and after a longer relationship, having experienced a left-wing change before,

[3] The full distribution of this variable can be seen in Table B.4.
[4] Section B.5 shows results when looking at changes between *t* and *t-2*, namely increasing the inter-wave time span to between 4 and 7 years.
[5] From a less material perspective, higher classes tend to have more liberal values in what refers to marriage and cohabitation. The effect of this seems ambiguous. On the one hand, more liberal values mean fewer incentives to opt early into marriage due to social pressure. On the other hand, more liberal values ease moving together without being married.
[6] Table B.5 shows how those interested and those not interested in politics differ in some key demographics.

at an unknown moment before *t-1*. Nonetheless, the mean age at which those interested in politics enter a domestic relationship is only 2.5 years higher than the mean age for those not interested in politics (30.52 versus 28.03), which may seem not big enough to explain the observed heterogeneity.

Let us now look at this issue from a different angle, indeed the one discussed in the main text. Table B.2 in Section B.3 makes a simple test of self-selection into type of domestic relationship. I look at the same sample of individuals that I have been employing throughout the paper, namely those who marry/start to cohabitate for the first time in the sample between *t* and *t-1*. However, I see now how economic preferences at *t-1* (not at *t*) can be explained by the type of domestic partnership which individuals will later form. Controls are measured at period *t-1*. As can be seen in Table B.2, preferences at *t-1* are not significantly correlated with the type of domestic partnership individuals will later form. However, a different picture arises if distinguishing between those interested and those not interested in politics. For those who are not interested in politics, later entering a cross-class partnership does not correlate significantly with economic preferences at *t-1* either. This implies that, among individuals not interested in politics, left-wing individuals are not more likely to self-select into a cross-class partnership than right-wing individuals. This seems natural given that politics is not central to these individuals' lives. Yet this finding is providing another important clue. Given that, among individuals not interested in politics, preferences of those who enter a cross-class partnership and of those who enter a within-class partnership are not different at time *t-1* but change later at *t*, it seems reasonable to infer that most not-interested-in-politics individuals do not date for a too long period before moving together, such that I am capturing what I aim to with a high degree of success.

Let us now look at those who report to be very or fairly interested in politics. For this group of people, political preferences at *t-1 do* significantly correlate with the type of domestic partnership that individuals will select into later in time. One can interpret this finding in two completely different ways. First, one could argue that economically conservative individuals who are interested in politics are more likely to follow cultural norms of homogeneity, sorting into same-class domestic partnerships. Then, those interested in politics self-select into types of domestic partnerships according to their view of the economy, but do not change later their attitudes conditional on the kind of partnership formed. However, one could argue too that rather than selecting into domestic partnerships according to their economic attitudes, those interested in politics often date for a longer period, such that the preferences of those entering a cross-class partnership move to the left but do it earlier than at period *t-1*.

Finally, one could argue that even if we assume that those interested and those not interested in politics date for similar periods of time, the fact that the former enter a domestic partnership at an older age may make the difference. According to the impressionable years hypothesis, political preferences of young people tend to be less sticky and more reactive to new information (see e.g., Dinas, 2013).[7] Therefore, it could be the case

[7] Although research on political socialization shows that preferences of young people are more volatile, that could still be explained by two different factors. First, it could be that young people are more open to attitudinal change. Second, it might be simply that people change more their preferences at youth because of the large concentration of significant life events at this stage of life (e.g.,

that those not interested in politics change their political preferences as they enter a domestic partnership earlier, at a moment in life in which their attitudes are more vulnerable to new information. But note again that the mean ages at which those interested and those not interested in politics marry/begin to cohabitate only differ by 2.5 years (see Table B.5).

entering the labour market and/or university, marrying, etc.). By focusing on reactions to the Watergate scandal to hold the second mechanism constant, Dinas (2013) shows that young people do attach more weight than older people to new information in order to form political preferences.

Section B.3: Self-selection into cross-class partnership according to economic attitudes and reverse causality

Table B.2: Test of self-selection

	Index of attitudes towards the economy in wave previous to partnership formation (*t-1*)									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	All		Interested in politics		Interested in politics		Less than	At least	Less than	At least
			No	Yes	No	Yes	Degree/Diploma		Degree/Diploma	
Male	-.065**	-.066**	-.087**	-.063	-.089**	-.063	-.061*	-.082	-.061*	-.08
	(.028)	(.028)	(.043)	(.068)	(.043)	(.068)	(.032)	(.052)	(.033)	(.052)
HH income	-1.715***	-1.709***	-1.714***	-1.705***	-1.702***	-1.706***	-1.783***	-1.65***	-1.784***	-1.626***
	(.298)	(.297)	(.363)	(.509)	(.364)	(.509)	(.367)	(.459)	(.367)	(.448)
Age	-.003*	-.003*	-.001	-.009**	-.001	-.009**	-.002	-.006*	-.002	-.005
	(.002)	(.002)	(.003)	(.004)	(.003)	(.004)	(.002)	(.003)	(.002)	(.003)
Unemployed	-.023	-.023	-.024	.128	-.024	.129	.015	-.189	.015	-.197
	(.06)	(.06)	(.09)	(.164)	(.091)	(.164)	(.062)	(.185)	(.062)	(.185)
Will enter a cross-class partnership next wave	.041	-	.019	.167**	-	-	.024	.072	-	-
	(.03)		(.045)	(.077)			(.035)	(.054)		
Will enter a cross-class partnership (down) next wave	-	.032	-	-	.003	.175**	-	-	.025	.03
		(.033)			(.051)	(.083)			(.04)	(.06)
Will enter a cross-class partnership (up) next wave	-	.051	-	-	.036	.158*	-	-	.024	.129*
		(.034)			(.052)	(.092)			(.039)	(.066)
Will enter a within-class partnership next wave (Reference)	-	-	-	-	-	-	-	-	-	-
Observations	1472	1472	568	337	568	337	1046	426	1046	426
R-squared	.066	.066	.07	.124	.071	.124	.06	.087	.06	.091

Robust standard errors in parentheses. All variables measured at time t-1, namely the wave previous to partnership formation. The dependent variable is an index of attitudes towards the economy, with larger values indicating more left-wing attitudes. Columns (3) and (5) restrict the sample to individuals who report to be not very interested or not at all interested in politics (at time t-1). Columns (4) and (6), to those who report to be very interested or fairly interested in politics (at time t-1). Columns (7) and (9) restricts the sample to individuals with less than a degree/diploma qualification (at time t-1). Columns (8) and (10), to those with at least a degree/diploma (at time t-1). Dummies for respondent's level of educational attainment included. Dummies for waves included.

**** p<.01, ** p<.05, * p<.1*

On reverse causality

Strictly speaking, it is possible that my results are not explained by those who enter a cross-class partnership moving in a different direction than those who enter a within-class partnership. As assignment into type of partnership is not random, reverse causality is a concern in my analysis. Namely, it could be the case that people *moving* to the left between *t* and *t-1* systematically decide to form cross-class relationships at *t*. It would be something like a self-selection mechanism based on attitudinal changes rather than levels. Although I find this a much less plausible explanation for my results than the thesis I propose, I cannot totally rule it out. However, some of the findings throughout the paper seem quite inconsistent with a reverse causality rationale.

First, the heterogeneous findings expressed in the previous paragraph: namely, that those who self-select into cross-class partnerships according to their prior levels of economic attitudes do not change their attitudes later (those interested in politics), and those who do not self-select do change their economic attitudes later (those not interested). Second, the results of the placebo analyses do not seem very consistent either with that explanation, as they imply that those moving to the left in a more general sense (not economically) would not be more likely to form a cross-class partnership. Finally, if the results of the paper were explained by those in an increasing trend of economic attitudes prevalently joining cross-class partnerships, one might expect to observe that relationship also in the trend before *t-1*. In other words, I would expect those in an already increasing trend between *t-2* and *t-1* to be more likely to form a partnership at *t*. In order to test this, I regress a dummy taking value 1 for those in a cross-class domestic partnership at time *t* (0 for those in a within-class partnership) on the difference of economic attitudes between *t-2* and *t-1* (subtracting the former from the latter). Table B.3 shows that those in an increasing trend between *t-2* and *t-1* are not more likely to self-select into a cross-class partnership at time *t*, giving credit to the idea that my results are not an artifact of reverse causality.

Table B.3: Test of reverse causality

	In a cross-class relationship at time t (1 if yes, 0 if not)				
	(1)	(2)	(3)	(4)	(5)
	All	Interested in politics		Less than	At least
		No	Yes	Degree/Diploma	
Δ in eco. attitudes	-.026	-.003	-.018	-.012	-.046
(t-1 minus t-2)	(.035)	(.056)	(.07)	(.043)	(.06)
Male	.02	.072	-.07	-.021	.103*
	(.031)	(.051)	(.071)	(.039)	(.054)
HH income	-.084	-.239	.258	-.326	.129
	(.262)	(.498)	(.418)	(.441)	(.3)
Age	-.002	-.006*	0	-.001	-.002
	(.002)	(.003)	(.004)	(.002)	(.003)
Unemployed	-.02	.028	-.165	-.014	-.051
	(.067)	(.087)	(.159)	(.074)	(.175)
Observations	903	338	202	579	324
R-squared	.034	.05	.052	.021	.049

*Robust standard errors in parentheses. The dependent variable is a dummy taking value 1 if respondent will be in a cross-class partnership at time t, and 0 if she will be in a within-class partnership. All variables measured at time t-1, namely the wave previous to partnership formation. Column (2) restricts the sample to individuals who report to be not very interested or not at all interested in politics (at time t-1). Columns (3), to those who report to be very interested or fairly interested in politics (at time t-1). Column (4) restricts the sample to individuals with less than a degree/diploma qualification (at time t-1). Columns (5), to those with at least a degree/diploma (at time t-1). Dummies for respondent's level of education included. Dummies for waves included. *** p<.01, ** p<.05, * p<.1*

Section B.4: Some descriptive statistics

Table B.4: Number of observations, by number of years between wave of domestic partnership formation (*t*) and previous wave (*t-1*)

	All	Interested in politics		Less than	At least
Years between *t* and *t-1*		No	Yes	Degree/Diploma	
2	588	345	242	442	146
3	530	194	79	350	180
4	309	11	5	216	93
5	6	3	3	4	2
6	11	7	4	10	1
7	15	1	0	12	3
9	5	4	1	4	1
10	2	0	0	2	0
11	4	1	3	4	0
13	2	2	0	2	0
Total	1472	568	337	1046	426
Mean years between *t* and *t-1*	2.94	2.98	2.87	2.95	2.93

Table B.5: Demographics of different subsamples employed in the analysis

	Analysis about attitudes on				
	The economy			Gender	Homosexuality
	Not interested in politics	Interested in politics	All		
Age (mean)	28.03	30.52	29.33	28.89	30.1
Year of birth (mean)	1971	1967	1970.7	1971	1974
Gender (%)					
Males	41.90	55.79	47.49	46.51	47.49
Females	58.10	44.21	52.51	53.49	52.51
Education (%)					
Low school level or below	16.23	7.72	12.39	12.64	10.79
Vocational qualification	41.98	32.05	36.49	38.10	32.79
Higher school level	12.87	12.17	11.91	12.52	12.23
Degree/diploma	28.92	48.07	39.21	36.74	44.19
Unemployed (%)	5.63	5.64	5.23	5.88	4.31
Income—deflated by retail price index but unadjusted for household size (mean)	1,448	1,750	1,573	1,564	1,608
Entering a cross-class partnership (%)	72.54	69.14	69.70	71.61	68.82
Entering a cross-class partnership, down (%)	36.09	37.39	34.78	35.50	34.77
Entering a cross-class partnership, up (%)	36.44	31.75	34.92	36.12	34.05
Entering a within-class partnership (%)	27.46	30.86	30.30	28.39	31.18
Class (most recent occupation) (%)					
Professional	3.70	9.79	5.43	6.12	5.74
Managerial and technical	23.24	36.80	28.67	26.59	31.49
Skilled non-manual	32.39	24.33	28.53	28.14	24.82
Skilled manual	20.95	15.43	19.43	19.54	17.13
Partly skilled	17.08	11.87	15.01	16.08	17.13
Unskilled	2.64	1.78	2.92	3.53	3.69
Partner's class (most recent occupation) (%)					
Professional	4.58	6.82	5.64	5.07	5.64
Managerial and technical	26.58	37.39	31.52	29.25	31.38
Skilled non-manual	25.53	26.71	25.34	25.23	24.31
Skilled manual	24.65	14.24	19.09	21.21	18.67
Partly skilled	14.61	11.57	14.67	15.09	16.10
Unskilled	4.05	3.26	3.74	4.14	3.90
Number of observations	568	337	1,472	1,617	975

Section B.5: Replication of some of the analyses in the paper when controlling for the dependent variable lagged two periods rather than one

Table B.6: Measuring attitudinal changes between *t* and *t-2*

	Index of attitudes towards the economy							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All		All		Interested in politics		Interested in politics	
					No	Yes	No	Yes
Lag of index (t-2)	.56***	.513***	.56***	.512***	.564***	.589***	.562***	.586***
	(.029)	(.03)	(.029)	(.03)	(.052)	(.063)	(.052)	(.063)
Male	-.019	-.046	-.021	-.05*	-.013	-.018	-.021	-.02
	(.029)	(.029)	(.029)	(.029)	(.051)	(.069)	(.051)	(.07)
Δ in HH income	-.313*	-.164	-.329*	-.182	.034	-.154	-.006	-.132
(t minus t-2)	(.187)	(.18)	(.186)	(.178)	(.339)	(.535)	(.331)	(.536)
Age	-.002	-.002	-.002	-.002	-.004	-.003	-.004	-.003
	(.002)	(.002)	(.002)	(.002)	(.004)	(.004)	(.004)	(.004)
Unemployed	.123*	.09	.125**	.092	.121	.121	.134	.123
	(.063)	(.063)	(.063)	(.062)	(.115)	(.169)	(.115)	(.169)
Partner's attitudes	-	.183***	-	.185***	-	-	-	-
		(.029)		(.029)				
Cross-class	.07**	.05	-	-	.096*	.05	-	-
partnership	(.032)	(.032)			(.053)	(.075)		
Cross-class	-	-	.055	.029	-	-	.062	.065
partnership (down)			(.036)	(.036)			(.059)	(.075)
Cross-class	-	-	.085**	.072*	-	-	.134**	.03
partnership (up)			(.037)	(.037)			(.062)	(.098)
Within-class	-	-	-	-	-	-	-	-
partnership								
(Reference)								
Observations	927	891	927	891	342	202	342	202
R-squared	.368	.395	.368	.396	.344	.472	.347	.473

Robust standard errors in parentheses. The dependent variable is an index of attitudes towards the economy, with larger values indicating more left-wing attitudes. Dummies for respondent's level of education and for BHPS wave included. Columns (5) and (7) restrict the sample to individuals who report to be not very interested or not at all interested in politics (at time t-1). Columns (6) and (8), to those who report to be very interested or fairly interested in politics (at time t-1). The sample includes those who have married or begun to cohabitate with his/her partner for the first time in their time in the BHPS between the present and the previous wave.

**** p<.01, ** p<.05, * p<.1*

Table B.7: Number of observations, by number of years between wave of partnership formation (*t*) and two waves before (*t-2*)

	All	Interested in politics	
Years between *t* and *t-2*		No	Yes
4	345	199	139
5	188	0	0
6	12	9	3
7	361	131	58
9	11	1	0
10	4	0	0
11	3	1	1
12	1	0	0
13	2	1	1
Total	927	342	202
Mean years between *t* and *t-2*	5.53	5.26	4.97

Section B.6: First-difference estimator results

Table B.8: Main results with first-difference estimator

	Δ in index of attitudes towards the economy									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	All		Interested in politics		Interested in politics		Less than	At least	Less than	At least
			No	Yes	No	Yes	Degree/Diploma		Degree/Diploma	
Δ in HH income	-.479***	-.484***	-.269	-.516	-.29	-.516	-.869***	-.178	-.888***	-.177
	(.163)	(.164)	(.245)	(.325)	(.245)	(.326)	(.258)	(.187)	(.258)	(.187)
Δ in age	-.012	-.012	0	-.049*	0	-.048*	-.005	-.056**	-.004	-.056*
	(.011)	(.011)	(.02)	(.026)	(.021)	(.026)	(.011)	(.028)	(.011)	(.028)
Δ in unemployed	-.054	-.055	-.025	-.046	-.028	-.045	-.045	-.151	-.046	-.151
	(.047)	(.047)	(.071)	(.112)	(.07)	(.112)	(.053)	(.107)	(.053)	(.107)
Δ in highly	-.054	-.054	.014	-.007	.012	-.006	-	-	-	-
educated	(.046)	(.046)	(.09)	(.085)	(.09)	(.085)				
Δ in cross-class	.052*	-	.079*	.018	-	-	.074**	-.006	-	-
partnership	(.028)		(.046)	(.058)			(.034)	(.05)		
Δ in cross-class	-	.044	-	-	.056	.007	-	-	.059	-.002
partnership (down)		(.032)			(.052)	(.068)			(.039)	(.055)
Δ in cross-class	-	.06*	-	-	.101*	.03	-	-	.087**	-.011
partnership (up)		(.032)			(.052)	(.067)			(.038)	(.059)
Δ in within-class partnership (Reference)	-	-	-	-	-	-	-	-	-	-
Observations	1469	1469	567	337	567	337	1046	426	1046	426
R-squared	.012	.012	.007	.02	.009	.02	.015	.019	.016	.019

Robust standard errors in parentheses. The dependent variable measures the change in an index of attitudes towards the economy, with larger values indicating more left-wing attitudes. First-difference model, namely the value at t-1 (the wave previous to partnership formation) is subtracted from the value at t (the wave of partnership formation) for all variables. Highly educated is a dummy taking value 1 if holding a degree/diploma, and 0 otherwise. Columns (3) and (5) restrict the sample to individuals who report to be not very interested or not at all interested in politics (at time t-1). Columns (4) and (6), to those who report to be very interested or fairly interested in politics (at time t-1). Columns (7) and (9) restricts the sample to individuals with less than a degree/diploma qualification (at time t-1). Columns (8) and (10), to those with at least a degree/diploma (at time t-1).

**** p<.01, ** p<.05, * p<.1*

Section B.7: Results with the Goldthorpe schema

Figure B.1: Different versions of the Goldthorpe class schema

Full version (names as in the BHPS)		Condensed versions employed below as alternatives to the RGSC			
		Version 1 (7-class version)	Version 2 (5-class version)	Version 3 (5-class version)	Version 4 (5-class version)
I	Service class, higher grade	I+II	I+II+IIIa+IIIb	I+II	I+II
II	Service class, lower grade	IIIa+IIIb	IVa+IVb	IIIa+IIIb	IVa+IVb+IVc
IIIa	Routine non-manual employees	IVa+IVb	IVc+VIIb	IVa+IVb+IVc	IIIa+IIIb
IIIb	Personal service workers	IVc	V+VI	V+VI	V+VI
IVa	Small proprietors with employees	V+VI	VIIa	VIIa+VIIb	VIIa+VIIb
IVb	Small proprietors without employees	VIIa			
IVc	Farmers, Smallholders	VIIb			
V	Foreman, Technicians				
VI	Skilled manual workers				
VIIa	Semi/Unskilled manual workers				
VIIb	Agricultural workers				

Note: Full version as well as versions 1 and 2 from Erikson and Goldthorpe (1992: 38-39). Version 3 is a collapsed five-class version previously employed in the literature (e.g., Tolsma et al., 2009). Version 4 is a variation of version 3 in which group III is downgraded under the petty bourgeoisie, following authors such as Houle (2011), who group together the service class with the petty bourgeoisie to define the upper class and what concerns upward and downward mobility (see also De Graaf and Heath, 1992). Higher classes appear in a higher position in the figure.

Table B.9: Main results with version 1 of the Goldthorpe schema

	Index of attitudes towards the economy							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lag of index	-	.598***	.597***	.548***	-	.599***	.598***	.548***
		(.024)	(.024)	(.025)		(.024)	(.024)	(.025)
Male	-.064**	-	-.039*	-.059**	-.066**	-	-.043*	-.065***
	(.029)		(.023)	(.023)	(.029)		(.024)	(.023)
Δ in HH income/	-1.578***	-	-.306**	-.135	-1.575***	-	-.316**	-.149
HH income	(.278)		(.143)	(.142)	(.279)		(.144)	(.143)
Age	-.002	-	0	0	-.002	-	0	0
	(.002)		(.001)	(.001)	(.002)		(.001)	(.001)
Unemployed	.029	-	.063	.057	.028	-	.063	.055
	(.062)		(.05)	(.049)	(.062)		(.05)	(.048)
Partner's attitudes	-	-	-	.209***	-	-	-	.211***
				(.022)				(.022)
Cross-class	.04	.077***	.063**	.045*	-	-	-	-
partnership	(.03)	(.025)	(.025)	(.025)				
Cross-class	-	-	-	-	.036	.066**	.05*	.023
partnership (down)					(.035)	(.029)	(.028)	(.029)
Cross-class	-	-	-	-	.046	.089***	.077***	.067**
partnership (up)					(.035)	(.028)	(.028)	(.028)
Within-class	-	-	-	-	-	-	-	-
partnership								
(Reference)								
Observations	1445	1445	1445	1394	1445	1445	1445	1394
Education and								
BHPS dummies	YES	NO	YES	YES	YES	NO	YES	YES
R-squared	.059	.348	.367	.409	.059	.349	.368	.41

Robust standard errors in parentheses. The dependent variable is an index of attitudes towards the economy, with larger values indicating more left-wing attitudes. Dummies for respondent's level of education and for BHPS wave included except in columns (2) and (6). HH income in columns (1) and (5), ⊿ in HH income in columns (2)-(4) and (6)-(8). The sample includes those who have married or begun to cohabitate with his/her partner for the first time in their time in the BHPS between the present and the previous wave. See Figure B.1 for a definition of the class schema employed.
**** p<.01, ** p<.05, * p<.1*

Table B.10: Main results with version 2 of the Goldthorpe schema

	Index of attitudes towards the economy							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lag of index	-	.594***	.594***	.546***	-	.595***	.595***	.546***
		(.024)	(.024)	(.025)		(.024)	(.024)	(.025)
Male	-.064**	-	-.037	-.058**	-.073**	-	-.052**	-.073***
	(.029)		(.023)	(.023)	(.031)		(.025)	(.024)
Δ in HH income/	-1.559***	-	-.288**	-.138	-1.554***	-	-.301**	-.152
HH income	(.277)		(.144)	(.142)	(.277)		(.145)	(.144)
Age	-.002	-	0	0	-.002	-	0	0
	(.002)		(.001)	(.001)	(.002)		(.001)	(.001)
Unemployed	.025	-	.061	.056	.024	-	.06	.055
	(.062)		(.05)	(.049)	(.062)		(.05)	(.048)
Partner's attitudes	-	-	-	.208***	-	-	-	.208***
				(.022)				(.022)
Cross-class	.086***	.091***	.07***	.038	-	-	-	-
partnership	(.028)	(.023)	(.023)	(.023)				
Cross-class	-	-	-	-	.069**	.069**	.042	.008
partnership (down)					(.034)	(.028)	(.029)	(.029)
Cross-class	-	-	-	-	.105***	.114***	.101***	.07**
partnership (up)					(.036)	(.028)	(.029)	(.029)
Within-class	-	-	-	-	-	-	-	-
partnership								
(Reference)								
Observations	1445	1445	1445	1394	1445	1445	1445	1394
Education and								
BHPS dummies	YES	NO	YES	YES	YES	NO	YES	YES
R-squared	.063	.351	.368	.409	.064	.352	.369	.41

Robust standard errors in parentheses. The dependent variable is an index of attitudes towards the economy, with larger values indicating more left-wing attitudes. Dummies for respondent's level of education and for BHPS wave included except in columns (2) and (6). HH income in columns (1) and (5), ⊿ in HH income in columns (2)-(4) and (6)-(8). The sample includes those who have married or begun to cohabitate with his/her partner for the first time in their time in the BHPS between the present and the previous wave. See Figure B.1 for a definition of the class schema employed.
**** p<.01, ** p<.05, * p<.1*

Table B.11: Main results with version 3 of the Goldthorpe schema

	Index of attitudes towards the economy							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lag of index	-	.598***	.597***	.548***	-	.599***	.598***	.548***
		(.024)	(.024)	(.025)		(.024)	(.024)	(.025)
Male	-.065**	-	-.039*	-.059**	-.066**	-	-.043*	-.065***
	(.029)		(.023)	(.023)	(.029)		(.024)	(.023)
Δ in HH income/	-1.579***	-	-.306**	-.134	-1.577***	-	-.316**	-.148
HH income	(.278)		(.143)	(.142)	(.279)		(.144)	(.143)
Age	-.002	-	0	0	-.002	-	0	0
	(.002)		(.001)	(.001)	(.002)		(.001)	(.001)
Unemployed	.028	-	.063	.057	.028	-	.062	.055
	(.062)		(.05)	(.049)	(.062)		(.05)	(.048)
Partner's attitudes	-	-	-	.209***	-	-	-	.211***
				(.022)				(.022)
Cross-class	.042	.077***	.063**	.046*	-	-	-	-
partnership	(.03)	(.025)	(.025)	(.024)				
Cross-class	-	-	-	-	.038	.065**	.05*	.025
partnership (down)					(.035)	(.029)	(.028)	(.028)
Cross-class	-	-	-	-	.047	.089***	.077***	.067**
partnership (up)					(.035)	(.028)	(.028)	(.028)
Within-class	-	-	-	-	-	-	-	-
partnership								
(Reference)								
Observations	1445	1445	1445	1394	1445	1445	1445	1394
Education and								
BHPS dummies	YES	NO	YES	YES	YES	NO	YES	YES
R-squared	.059	.348	.367	.409	.059	.349	.368	.41

Robust standard errors in parentheses. The dependent variable is an index of attitudes towards the economy, with larger values indicating more left-wing attitudes. Dummies for respondent's level of education and for BHPS wave included except in columns (2) and (6). HH income in columns (1) and (5), ⊿ in HH income in columns (2)-(4) and (6)-(8). The sample includes those who have married or begun to cohabitate with his/her partner for the first time in their time in the BHPS between the present and the previous wave. See Figure B.1 for a definition of the class schema employed.
**** p<.01, ** p<.05, * p<.1*

Table B.12: Main results with version 4 of the Goldthorpe schema

	Index of attitudes towards the economy							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lag of index	-	.598***	.597***	.548***	-	.598***	.597***	.547***
		(.024)	(.024)	(.025)		(.024)	(.024)	(.025)
Male	-.065**	-	-.039*	-.059**	-.065**	-	-.039*	-.061***
	(.029)		(.023)	(.023)	(.029)		(.023)	(.023)
Δ in HH income/	-1.579***	-	-.306**	-.134	-1.577***	-	-.309**	-.142
HH income	(.278)		(.143)	(.142)	(.279)		(.144)	(.143)
Age	-.002	-	0	0	-.002	-	0	0
	(.002)		(.001)	(.001)	(.002)		(.001)	(.001)
Unemployed	.028	-	.063	.057	.028	-	.063	.055
	(.062)		(.05)	(.049)	(.062)		(.05)	(.049)
Partner's attitudes	-	-	-	.209***	-	-	-	.211***
				(.022)				(.022)
Cross-class	.042	.077***	.063**	.046*	-	-	-	-
partnership	(.03)	(.025)	(.025)	(.024)				
Cross-class	-	-	-	-	.038	.07**	.059**	.029
partnership (down)					(.035)	(.029)	(.029)	(.028)
Cross-class	-	-	-	-	.046	.084***	.068**	.062**
partnership (up)					(.034)	(.028)	(.028)	(.028)
Within-class	-	-	-	-	-	-	-	-
partnership								
(Reference)								
Observations	1445	1445	1445	1394	1445	1445	1445	1394
Education and								
BHPS dummies	YES	NO	YES	YES	YES	NO	YES	YES
R-squared	.059	.348	.367	.409	.059	.348	.367	.41

Robust standard errors in parentheses. The dependent variable is an index of attitudes towards the economy, with larger values indicating more left-wing attitudes. Dummies for respondent's level of education and for BHPS wave included except in columns (2) and (6). HH income in columns (1) and (5), ⊿ in HH income in columns (2)-(4) and (6)-(8). The sample includes those who have married or begun to cohabitate with his/her partner for the first time in their time in the BHPS between the present and the previous wave. See Figure B.1 for a definition of the class schema employed.
**** p<.01, ** p<.05, * p<.1*

Section B.8: Results when controlling for respondent's social class

Table B.13: Main results when controlling for respondent's social class

	Index of attitudes towards the economy							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lag of index	-	.592***	.582***	.535***	-	.591***	.581***	.535***
		(.023)	(.024)	(.024)		(.023)	(.024)	(.024)
Male	-.096***	-	-.056**	-.076***	-.094***	-	-.054**	-.077***
	(.031)		(.026)	(.025)	(.031)		(.026)	(.025)
Δ in HH income/	-1.385***	-	-.269*	-.121	-1.37***	-	-.245*	-.132
HH income	(.267)		(.141)	(.14)	(.268)		(.142)	(.142)
Age	-.002	-	0	0	-.002	-	0	0
	(.002)		(.001)	(.001)	(.002)		(.001)	(.001)
Unemployed	.028	-	.061	.056	.027	-	.06	.057
	(.062)		(.051)	(.049)	(.062)		(.051)	(.049)
Partner's attitudes	-	-	-	.208***	-	-	-	.209***
				(.022)				(.022)
Cross-class	.087***	.086***	.063**	.051**	-	-	-	-
partnership	(.031)	(.025)	(.026)	(.025)				
Cross-class	-	-	-	-	.11***	.073**	.077***	.044
partnership (down)					(.035)	(.028)	(.029)	(.029)
Cross-class	-	-	-	-	.056	.099***	.044	.061**
partnership (up)					(.037)	(.028)	(.031)	(.031)
Within-class	-	-	-	-	-	-	-	-
partnership								
(Reference)								
Class								
Professional	-	-	-	-	-	-	-	-
(Reference)								
Managerial and	.13*	-	.06	.052	.142*	-	.068	.047
technical	(.073)		(.055)	(.055)	(.073)		(.056)	(.056)
Skilled	.131*	-	.077	.058	.154**	-	.091	.05
non-manual	(.075)		(.058)	(.058)	(.076)		(.06)	(.061)
Skilled	.271***	-	.152**	.128**	.305***	-	.173***	.117*
manual	(.077)		(.06)	(.06)	(.081)		(.064)	(.065)
Partly skilled	.243***	-	.15**	.112*	.285***	-	.176***	.098
	(.08)		(.062)	(.062)	(.086)		(.067)	(.068)
Unskilled	.245**	-	.137	.102	.302***	-	.171*	.084
	(.101)		(.083)	(.085)	(.109)		(.09)	(.092)
Observations	1472	1472	1472	1420	1472	1472	1472	1420
Education and								
BHPS dummies	YES	NO	YES	YES	YES	NO	YES	YES
R-squared	.08	.348	.373	.414	.081	.348	.373	.414

Robust standard errors in parentheses. The dependent variable is an index of attitudes towards the economy, with larger values indicating more left-wing attitudes. Dummies for respondent's level of education and for BHPS wave included except in columns (2) and (6). HH income in columns (1) and (5), ⊿ in HH income in columns (2)-(4) and (6)-(8). The sample includes those who have married or begun to cohabitate with his/her partner for the first time in their time in the BHPS between the present and the previous wave.

**** p<.01, ** p<.05, * p<.1*

Section B.9: Results when controlling for father's social class

Table B.14: Main results when controlling for father's social class

	Index of attitudes towards the economy							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lag of index	-	.625***	.629***	.572***	-	.625***	.629***	.571***
		(.029)	(.03)	(.031)		(.029)	(.03)	(.032)
Male	-.106**	-	-.034	-.078**	-.105**	-	-.034	-.081**
	(.042)		(.033)	(.033)	(.042)		(.033)	(.033)
Δ in HH income/	-1.678***	-	-.378**	-.252	-1.662***	-	-.403**	-.284*
HH income	(.395)		(.165)	(.165)	(.392)		(.17)	(.171)
Age	-.006***	-	-.002	-.001	-.006***	-	-.002	-.001
	(.002)		(.002)	(.002)	(.002)		(.002)	(.002)
Unemployed	-.037	-	.013	.026	-.039	-	.01	.022
	(.102)		(.078)	(.07)	(.102)		(.078)	(.07)
Partner's attitudes	-	-	-	.182***	-	-	-	.189***
				(.03)				(.03)
Cross-class	.111**	.103***	.091***	.074**	-	-	-	-
partnership	(.044)	(.034)	(.035)	(.035)				
Cross-class	-	-	-	-	.09*	.078**	.061	.028
partnership (down)					(.049)	(.04)	(.04)	(.04)
Cross-class	-	-	-	-	.133***	.127***	.12***	.118***
partnership (up)					(.051)	(.039)	(.04)	(.039)
Within-class	-	-	-	-	-	-	-	-
partnership								
(Reference)								
Father's class								
Professional	-	-	-	-	-	-	-	-
(Reference)								
Managerial and	-.098	-.059	-.091	-.101	-.096	-.058	-.09	-.101
technical	(.091)	(.07)	(.07)	(.07)	(.091)	(.07)	(.07)	(.07)
Skilled	.067	.044	.006	-.004	.066	.042	.005	-.01
non-manual	(.106)	(.08)	(.078)	(.079)	(.106)	(.08)	(.078)	(.079)
Skilled	.144	.097	.071	.058	.145	.096	.072	.056
manual	(.088)	(.068)	(.067)	(.068)	(.088)	(.068)	(.067)	(.068)
Partly skilled	.169*	.08	.073	.038	.168*	.078	.071	.034
	(.096)	(.076)	(.076)	(.078)	(.096)	(.076)	(.076)	(.078)
Unskilled	.283**	.234**	.205*	.132	.283**	.23**	.205*	.126
	(.135)	(.109)	(.11)	(.108)	(.135)	(.108)	(.108)	(.107)
Observations	745	745	745	716	745	745	745	716
Education and								
BHPS dummies	YES	NO	YES	YES	YES	NO	YES	YES
R-squared	.126	.449	.464	.491	.127	.45	.466	.495

Robust standard errors in parentheses. The dependent variable is an index of attitudes towards the economy, with larger values indicating more left-wing attitudes. Dummies for respondent's level of education and for BHPS wave included except in columns (2) and (6). HH income in columns (1) and (5), ⊿ in HH income in columns (2)-(4) and (6)-(8). The sample includes those who have married or begun to cohabitate with his/her partner for the first time in their time in the BHPS between the present and the previous wave.

**** p<.01, ** p<.05, * p<.1*

Section B.10: Disaggregation of the economic index

Table B.15: Replication of results for each of the six items forming the economic index (Only individuals not interested in politics)

	(1) Ordinary people get their fair share of the nation's wealth (reversed)	(2) There is one law for the rich and one law for the poor	(3) Strong trade unions are needed to protect the working conditions and wages of employees	(4) Private enterprise is the best way to solve Britain's economic problems (reversed)	(5) Major public services and industries ought to be in state ownership	(6) It is the governme nt's responsibil ity to provide a job for everyone who wants one	(7) Ordinary people get their fair share of the nation's wealth (reversed)	(8) There is one law for the rich and one law for the poor	(9) Strong trade unions are needed to protect the working conditions and wages of employees	(10) Private enterprise is the best way to solve Britain's economic problems (reversed)	(11) Major public services and industries ought to be in state ownership	(12) It is the governme nt's responsibil ity to provide a job for everyone who wants one
Lag of index	.327***	.4***	.44***	.353***	.195***	.449***	.33***	.399***	.44***	.351***	.192***	.449***
	(.043)	(.043)	(.04)	(.041)	(.049)	(.039)	(.042)	(.042)	(.04)	(.041)	(.049)	(.039)
Male	-.017	-.035	-.187***	-.08	.073	-.049	-.032	-.042	-.192***	-.083	.065	-.045
	(.075)	(.07)	(.072)	(.064)	(.077)	(.081)	(.075)	(.07)	(.072)	(.064)	(.077)	(.082)
Δ in HH income	-.085	.097	-.019	-.325	-.582	1.113**	-.221	.035	-.07	-.349	-.648	1.144***
	(.412)	(.486)	(.42)	(.559)	(.485)	(.435)	(.424)	(.482)	(.421)	(.56)	(.478)	(.435)
Age	.008*	.003	-.005	.002	.005	-.003	.008*	.003	-.005	.002	.005	-.003
	(.005)	(.003)	(.005)	(.004)	(.005)	(.005)	(.004)	(.003)	(.005)	(.004)	(.005)	(.005)
Unemployed	.097	-.142	.207	-.022	.202	-.143	.096	-.143	.206	-.021	.203	-.143
	(.141)	(.144)	(.133)	(.106)	(.133)	(.157)	(.141)	(.143)	(.133)	(.107)	(.132)	(.157)
Cross-class partnership	.093	.126	.171**	-.068	.156**	.083	-	-	-	-	-	-
	(.075)	(.076)	(.077)	(.066)	(.079)	(.09)						
Cross-class partners. (down)	-	-	-	-	-	-	-.02	.07	.124	-.091	.097	.113
							(.087)	(.084)	(.089)	(.075)	(.088)	(.099)
Cross-class partners. (up)	-	-	-	-	-	-	.206**	.182**	.217**	-.044	.215**	.053
							(.085)	(.088)	(.084)	(.074)	(.091)	(.101)
Within-class partnership (Reference)	-	-	-	-	-	-	-	-	-	-	-	-
Observations	558	565	551	522	521	558	558	565	551	522	521	558
R-squared	.153	.239	.293	.194	.077	.24	.165	.242	.295	.195	.08	.241

*Robust standard errors in parentheses. Sample restricted to those who report to be not very interested or not at all interested in politics (at time t-1). The dependent variable measures the level of agreement in a 5-point scale with each of the statements forming the laissez faire/socialist scale developed by Heath et al. (1994), with higher values indicating more agreement, namely more economically leftist attitudes (except for the two items denoting "reversed", for which higher values indicate more disagreement, namely more leftist attitudes). Dummies for respondent's level of education and for BHPS wave included. The sample includes those who have married or begun to cohabitate with his/her partner for the first time in their time in the BHPS between the present and the previous wave. *** p<.01, ** p<.05, * p<.1*

Table B.16: Replication of results for each of the six items forming the economic index (All individuals)

	(1) Ordinary people get their fair share of the nation's wealth (reversed)	(2) There is one law for the rich and one law for the poor	(3) Strong trade unions are needed to protect the working conditions and wages of employees	(4) Private enterprise is the best way to solve Britain's economic problems (reversed)	(5) Major public services and industries ought to be in state ownership	(6) It is the governme nt's responsibil ity to provide a job for everyone who wants one	(7) Ordinary people get their fair share of the nation's wealth (reversed)	(8) There is one law for the rich and one law for the poor	(9) Strong trade unions are needed to protect the working conditions and wages of employees	(10) Private enterprise is the best way to solve Britain's economic problems (reversed)	(11) Major public services and industries ought to be in state ownership	(12) It is the governme nt's responsibil ity to provide a job for everyone who wants one
Lag of index	.352***	.404***	.473***	.391***	.314***	.489***	.353***	.404***	.473***	.391***	.314***	.489***
	(.026)	(.027)	(.026)	(.026)	(.029)	(.025)	(.026)	(.027)	(.026)	(.026)	(.029)	(.025)
Male	.028	.005	-.167***	-.108***	.013	-.098**	.021	.004	-.166***	-.11***	.011	-.096**
	(.043)	(.043)	(.045)	(.039)	(.046)	(.049)	(.044)	(.044)	(.045)	(.039)	(.047)	(.049)
Δ in HH income	-.044	-.037	-.288	-.763***	-.215	-.122	-.087	-.044	-.284	-.777***	-.226	-.112
	(.29)	(.303)	(.276)	(.274)	(.376)	(.33)	(.295)	(.305)	(.277)	(.275)	(.379)	(.329)
Age	.005**	.005**	-.005*	0	0	-.005*	.005**	.005**	-.005*	0	0	-.005*
	(.002)	(.002)	(.003)	(.002)	(.003)	(.003)	(.002)	(.002)	(.003)	(.002)	(.003)	(.003)
Unemployed	.092	.052	.163*	.092	-.003	.012	.088	.052	.163*	.09	-.003	.013
	(.089)	(.096)	(.09)	(.086)	(.091)	(.102)	(.088)	(.096)	(.09)	(.086)	(.091)	(.101)
Cross-class partnership	.03	.164***	.092*	.07	.041	.035	-	-	-	-	-	-
	(.046)	(.048)	(.049)	(.043)	(.049)	(.054)						
Cross-class partners. (down)	-	-	-	-	-	-	-.019	.155***	.096*	.054	.028	.048
							(.053)	(.054)	(.057)	(.049)	(.056)	(.06)
Cross-class partners. (up)	-	-	-	-	-	-	.079	.172***	.088	.086*	.053	.022
							(.053)	(.054)	(.055)	(.049)	(.057)	(.062)
Within-class partnership (Reference)	-	-	-	-	-	-	-	-	-	-	-	-
Observations	1446	1464	1445	1374	1384	1453	1446	1464	1445	1374	1384	1453
R-squared	.155	.227	.279	.208	.113	.268	.157	.227	.279	.209	.113	.268

Robust standard errors in parentheses. The dependent variable measures the level of agreement in a 5-point scale with each of the statements forming the laissez faire/socialist scale developed by Heath et al. (1994), with higher values indicating more agreement, namely more economically leftist attitudes (except for the two items denoting "reversed", for which higher values indicate more disagreement, namely more leftist attitudes). Dummies for respondent's level of education and for BHPS wave included. The sample includes those who have married or begun to cohabitate with his/her partner for the first time in their time in the BHPS between the present and the previous wave.

**** p<.01, ** p<.05, * p<.1*

Section B.11: A matter of *larger openness* to attitudinal change among those *more open* to enter a cross-class partnership?

It has been found that those who enter a cross-class partnership experience a movement to the left in their economic attitudes with respect to the change experienced by those entering a within-class partnership. One could consider that my interpretation of the results is not correct, and that these findings have nothing to do with contact with different social classes, but they are just an artifact of larger levels of openness to change among those willing to enter a cross-class partnership. Namely, those entering a partnership with someone from a different social position might be more open to change—which seems plausible as they are not following cultural norms of homogeneity by entering a cross-class partnership—than those who do not, therefore being more likely to update their political preferences too.[8] For testing the plausibility of this interpretation of the results, I have re-estimated Table 2.1 with a new dependent variable: the *absolute value* of the change in economic attitudes between *t* and *t-1*.[9] If the phenomenon considered here is solely driving my results, I would expect those entering a cross-class partnership to experience a larger change (in absolute-value terms) than those entering a within-class partnership. Table K.1 shows that this is not the case at all. Once measuring the change in preferences in absolute-value terms, the difference experienced between those with a partner from the same class and those with a partner from a different class is statistically and essentially indistinguishable from zero.

Note that results in Table K.1 do not preclude the possibility of those in cross-class partnerships being more open to change than those in within-class partnerships. What it precludes is that my findings are a mere artifact of that. In other words, even if those in cross-class partnerships might be more flexible about their political attitudes, it is needed a mechanism explaining why they systematically change in a certain direction and not in the other, which I propose to be inter-class contact. It is not because they are more open to change that they move their understanding of the economy to the left after coming into contact with a new socioeconomic reality, but it is because of that contact itself—regardless of whether their openness to not follow cultural norms of homogeneity could have in principle allowed them to meet that new reality through their partner.

[8] But note that they do not update their preferences with respect to domains unrelated to the economy.
[9] Remember that my results are identical to those of a model in which the dependent variable takes the form of a first difference instead, namely as in the case of Table B.17 but without taking the absolute value.

Table K.1: Change in economic attitudes (absolute value)

	Absolute value change in the index of attitudes towards the economy							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lag of index	-	.008	.007	.004	-	.008	.007	.004
		(.02)	(.021)	(.023)		(.02)	(.021)	(.023)
Male	.009	-	.006	.002	.008	-	.006	.002
	(.017)		(.017)	(.017)	(.017)		(.017)	(.017)
Δ in HH income/	-.306**	-	.119	.135	-.304**	-	.117	.133
HH income	(.137)		(.116)	(.119)	(.137)		(.117)	(.119)
Age	-.001	-	-.001	-.001	-.001	-	-.001	-.001
	(.001)		(.001)	(.001)	(.001)		(.001)	(.001)
Unemployed	.014	-	.023	.018	.014	-	.023	.017
	(.038)		(.038)	(.038)	(.038)		(.038)	(.038)
Partner's attitudes	-	-	-	.024	-	-	-	.024
				(.018)				(.018)
Cross-class	-.01	-.006	-.007	-.011	-	-	-	-
partnership	(.018)	(.018)	(.018)	(.018)				
Cross-class	-	-	-	-	-.012	-.01	-.009	-.013
partnership (down)					(.021)	(.021)	(.021)	(.021)
Cross-class	-	-	-	-	-.008	-.002	-.005	-.008
partnership (up)					(.02)	(.02)	(.02)	(.021)
Within-class	-	-	-	-	-	-	-	-
partnership								
(Reference)								
Observations	1472	1472	1472	1420	1472	1472	1472	1420
Education and								
BHPS dummies	YES	NO	YES	YES	YES	NO	YES	YES
R-squared	.006	0	.005	.006	.007	0	.005	.006

Robust standard errors in parentheses. The dependent variable is the absolute-value change between t and t-1 in the index of attitudes towards the economy. Dummies for respondent's level of education and for BHPS wave included except in columns (2) and (6). HH income in columns (1) and (5), ⊿ in HH income in columns (2)-(4) and (6)-(8). The sample includes those who have married or begun to cohabitate with his/her partner for the first time in their time in the BHPS between the present and the previous wave.

**** p<.01, ** p<.05, * p<.1*

Section B.12: Placebo tests

In this section, I explain the three measures of social conservatism employed in the placebos in Figure 2.2, as well as show the tables of results associated with those. The first is an item indicating the level of agreement in a 5-point scale with the statement "A husband's job is to earn money; a wife's job is to look after the home and family". This measure has been employed by authors such as Campbell et al. (2010) or Langsæther et al. (2022) to measure attitudes towards gender equality. Higher values indicate stronger disagreement, namely a less traditional view of the role of husbands and wives. For consistency with the previous analyses, I replicate the eight models of Table 2.1 with this new dependent variable. As can be seen in Table B.18, entering a cross-class partnership does not produce a change in preferences towards the role of men and women.

For the second placebo test, I construct an index of attitudes towards gender equality which measures the level of agreement in a 5-point scale with the previous item as well as other five: "A pre-school child is likely to suffer if his or her mother works", "All in all, family life suffers when the woman has a full time job", "A woman and her family would all be happier if she goes out to work" (reversed), "Both the husband and wife should contribute to the household income" (reversed), and "Having a full-time job is the best way for a woman to be an independent person" (reversed). Like the index employed in the main analysis about economic attitudes, this is the average of the responses to these six items, ranging from 1 to 5.[10] Higher values indicate more egalitarian gender attitudes, that is, a higher level of disagreement with the statements—except for those reversed, for which agreement indicates a more gender-equal view. Table B.19 shows that the effect of entering a cross-class partnership on attitudes towards gender equality is statistically and substantively indistinguishable from zero.[11] As expected, starting a domestic partnership with someone from a different socioeconomic reality does not alter individuals' way of thinking about gender.

For the last placebo test, I look at the level of agreement with the statement "Homosexual relationships are wrong", (e.g., Langsæther et al., 2022; La Roi and Mandemakers, 2018). Higher values in this variable indicate stronger disagreement, namely more openness towards homosexual relationships. Results are shown in Table B.20. Only for those entering a partnership with someone from a higher class and when not controlling for any covariate can a significant—at the 90 per cent—shift in attitudes be observed. This is the only case in the placebos in Tables B.18-B.20 that I observe

[10] The six items of this index have a Cronbach's alpha of 0.69, indicating a fair level of internal consistency.

[11] Very similar results are found if also including the following item in the index: "Children need a father to be as closely involved in their upbringing as the mother". Another candidate from the BHPS to be included is "Employers should make special arrangements to help mothers combine jobs and childcare". Although the latter is not a very good placebo as it expresses not only views on gender but also on the role of government in the economy, the results are again very similar if this item forms part of the index about gender attitudes.

a statistically significant change in attitudes after entering a cross-class partnership, which is actually around what I would expect to observe by pure chance.[12] But most importantly, this change is towards social conservatism, making it very difficult to argue that observed changes in economic attitudes are merely reflecting a shift to the left in a broader array of political attitudes.

Finally, note that the items measuring attitudes towards the economy are not present in all the same waves of the BHPS (1,3,5,7,10,14 and 17) than the items measuring gender attitudes (1,3,5,7,9,11,13,15 and 17) and opposition to homosexual relationships (8,10,12,14,16 and 18). Therefore, the samples employed are not exactly the same across the three different analyses. However, Table B.5 shows that they are quite similar on many observables. Furthermore, for the placebos regarding gender attitudes, the number of observations is very similar (even larger) to that in the main analysis—so reducing concerns about a lower level of statistical power in those placebos—and many of the waves indeed overlap.

Table B.18: Placebo I

	Level of disagreement with the statement "A husband's job is to earn money; a wife's job is to look after the home and family"							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lag of attitude	-	.478***	.434***	.412***	-	.478***	.434***	.412***
		(.026)	(.027)	(.028)		(.026)	(.027)	(.028)
Male	-.265***	-	-.127***	-.19***	-.265***	-	-.126***	-.19***
	(.044)		(.042)	(.042)	(.044)		(.042)	(.042)
Δ in HH income/	1.013***	-	-.134	-.226	1.012***	-	-.132	-.226
HH income	(.328)		(.238)	(.224)	(.328)		(.238)	(.224)
Age	-.02***	-	-.012***	-.01***	-.02***	-	-.012***	-.01***
	(.003)		(.003)	(.003)	(.003)		(.003)	(.003)
Unemployed	-.088	-	.013	.069	-.088	-	.013	.069
	(.094)		(.078)	(.077)	(.094)		(.078)	(.077)
Partner's attitudes	-	-	-	.155***	-	-	-	.155***
				(.022)				(.022)
Cross-class	-.058	-.043	-.034	-.009	-	-	-	-
partnership	(.048)	(.043)	(.043)	(.043)				
Cross-class	-	-	-	-	-.057	-.027	-.031	-.009
partnership (down)					(.054)	(.049)	(.049)	(.049)
Cross-class	-	-	-	-	-.06	-.058	-.038	-.009
partnership (up)					(.056)	(.049)	(.05)	(.05)
Within-class	-	-	-	-	-	-	-	-
partnership								
(Reference)								
Observations	1606	1606	1606	1523	1606	1606	1606	1523
Education and								
BHPS dummies	YES	NO	YES	YES	YES	NO	YES	YES
R-squared	.103	.239	.272	.298	.103	.239	.272	.298

Robust standard errors in parentheses. The dependent variable indicates the level of disagreement in a 5-point scale with the statement "A husband's job is to earn money; a wife's job is to look after the home and family". Dummies for respondent's level of education and for BHPS wave included except in columns (2) and (6). HH income in columns (1) and (5), ⊿ in HH income in columns (2)-(4) and (6)-(8). Waves included: 3,5,7,9,11,13,15,17. The sample includes those who have married or begun to cohabitate with his/her partner for the first time in their time in the BHPS between the present and the previous wave.
**** p<.01, ** p<.05, * p<.1*

[12] The large effect in models (1) and (5) of Table B.20 are found before controlling for the LDV. Therefore, they do not measure a change in preferences.

Table B.19: Placebo II

	Index of attitudes towards gender equality							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lag of index	-	.559***	.554***	.519***	-	.559***	.554***	.519***
		(.025)	(.025)	(.024)		(.025)	(.025)	(.024)
Male	-.099***	-	.005	-.028	-.098***	-	.005	-.028
	(.028)		(.024)	(.024)	(.028)		(.024)	(.024)
Δ in HH income/	.313**	-	-.013	-.008	.307**	-	-.009	-.01
HH income	(.153)		(.147)	(.144)	(.153)		(.148)	(.145)
Age	-.01***	-	-.004***	-.003**	-.01***	-	-.004***	-.003**
	(.002)		(.001)	(.001)	(.002)		(.001)	(.001)
Unemployed	.081	-	.127***	.125***	.082	-	.128***	.125***
	(.059)		(.047)	(.045)	(.059)		(.047)	(.045)
Partner's attitudes	-	-	-	.189***	-	-	-	.189***
				(.023)				(.023)
Cross-class	-.017	-.008	-.008	.009	-	-	-	-
partnership	(.03)	(.025)	(.025)	(.025)				
Cross-class	-	-	-	-	-.008	-.003	-.002	.006
partnership (down)					(.034)	(.029)	(.029)	(.029)
Cross-class	-	-	-	-	-.026	-.013	-.014	.012
partnership (up)					(.034)	(.029)	(.029)	(.029)
Within-class	-	-	-	-	-	-	-	-
partnership								
(Reference)								
Observations	1617	1617	1617	1535	1617	1617	1617	1535
Education and								
BHPS dummies	YES	NO	YES	YES	YES	NO	YES	YES
R-squared	.042	.307	.322	.352	.042	.307	.322	.352

Robust standard errors in parentheses. The dependent variable is an index of attitudes towards gender equality, with larger values indicating a less conservative attitude. Dummies for respondent's level of education and for BHPS wave included except in columns (2) and (6). HH income in columns (1) and (5), ⊿ in HH income in columns (2)-(4) and (6)-(8). Waves included: 3,5,7,9,11,13,15,17. The sample includes those who have married or begun to cohabitate with his/her partner for the first time in their time in the BHPS between the present and the previous wave.
**** p<.01, ** p<.05, * p<.1*

Table B.20: Placebo III

	Level of disagreement with the statement "Homosexual relationships are wrong"							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lag of attitude	-	.666***	.634***	.6***	-	.663***	.633***	.597***
		(.028)	(.031)	(.033)		(.028)	(.031)	(.033)
Male	-.598***	-	-.144**	-.251***	-.592***	-	-.141**	-.251***
	(.073)		(.058)	(.063)	(.073)		(.058)	(.063)
Δ in HH income/	.075	-	.256	.212	.007	-	.268	.222
HH income	(.874)		(.302)	(.302)	(.852)		(.304)	(.304)
Age	-.016***	-	-.012***	-.01***	-.016***	-	-.012***	-.01***
	(.004)		(.003)	(.003)	(.004)		(.003)	(.003)
Unemployed	-.157	-	.017	.048	-.161	-	.015	.047
	(.158)		(.135)	(.136)	(.157)		(.135)	(.135)
Partner's attitudes	-	-	-	.142***	-	-	-	.147***
				(.028)				(.028)
Cross-class	-.152**	-.042	-.034	.007	-	-	-	-
partnership	(.075)	(.059)	(.058)	(.058)				
Cross-class	-	-	-	-	-.062	.038	.041	.094
partnership (down)					(.083)	(.064)	(.064)	(.064)
Cross-class	-	-	-	-	-.246***	-.124*	-.112	-.082
partnership (up)					(.09)	(.07)	(.069)	(.07)
Within-class	-	-	-	-	-	-	-	-
partnership								
(Reference)								
Observations	975	975	975	931	975	975	975	931
Education and								
BHPS dummies	YES	NO	YES	YES	YES	NO	YES	YES
R-squared	.124	.464	.482	.499	.128	.467	.486	.502

Robust standard errors in parentheses. The dependent variable indicates the level of disagreement in a 5-point scale with the statement "Homosexual relationships are wrong". Dummies for respondent's level of education and for BHPS wave included except in columns (2) and (6). HH income in columns (1) and (5), ⊿ in HH income in columns (2)-(4) and (6)-(8). Waves included: 10,12,14,16,18. The sample includes those who have married or begun to cohabitate with his/her partner for the first time in their time in the BHPS between the present and the previous wave.

**** p<.01, ** p<.05, * p<.1*

Section B.13: Heterogeneity by political interest

Table B.21: Heterogeneity by political interest

	Index of attitudes towards the economy							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Interested in politics		Interested in politics		Interested in politics		Interested in politics	
	No	Yes	No	Yes	No	Yes	No	Yes
Lag of index	.558***	.695***	.5***	.627***	.556***	.695***	.496***	.627***
	(.037)	(.04)	(.039)	(.043)	(.038)	(.04)	(.039)	(.043)
Male	-.03	-.057	-.043	-.101**	-.034	-.057	-.05	-.101**
	(.039)	(.05)	(.038)	(.048)	(.039)	(.05)	(.038)	(.048)
Δ in HH income	-.093	-.338	.028	-.214	-.132	-.338	-.018	-.213
	(.246)	(.274)	(.227)	(.277)	(.245)	(.275)	(.223)	(.28)
Age	.001	-.001	.002	-.002	.001	-.001	.002	-.002
	(.002)	(.003)	(.002)	(.003)	(.002)	(.003)	(.002)	(.003)
Unemployed	.048	.03	.041	.024	.047	.031	.041	.021
	(.079)	(.108)	(.074)	(.108)	(.079)	(.108)	(.074)	(.109)
Partner's attitudes	-	-	.214***	.219***	-	-	.218***	.221***
			(.034)	(.043)			(.034)	(.043)
Cross-class partnership	.104**	.045	.084**	.032	-	-	-	-
	(.04)	(.053)	(.039)	(.051)				
Cross-class partnership (down)	-	-	-	-	.069	.047	.039	.016
					(.046)	(.06)	(.045)	(.059)
Cross-class partnership (up)	-	-	-	-	.139***	.042	.129***	.054
					(.046)	(.062)	(.045)	(.062)
Within-class partnership (Reference)	-	-	-	-	-	-	-	-
Observations	568	337	546	326	568	337	546	326
R-squared	.319	.535	.361	.573	.322	.535	.367	.574

Robust standard errors in parentheses. Odd columns display results for individuals who report to be not very interested or not at all interested in politics (at time t-1). Even columns, for those who report to be very interested or fairly interested in politics (at time t-1). The dependent variable is an index of attitudes towards the economy, with larger values indicating more left-wing attitudes. Dummies for respondent's level of education and for BHPS wave included. The sample includes those who have married or begun to cohabitate with his/her partner for the first time in their time in the BHPS between the present and the previous wave.
**** p<.01, ** p<.05, * p<.1*

Section B.14: The importance of controlling for the LDV in the presence of self-selection

Table B.22: Controlling and not controlling for the LDV for those interested and for those not interested in politics

	Index of attitudes towards the economy							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Interested in politics				Not interested in politics			
Lag of index	-	.558***	-	.556***	-	.695***	-	.695***
		(.037)		(.038)		(.04)		(.04)
Male	-.095**	-.03	-.101**	-.034	-.127*	-.057	-.127*	-.057
	(.047)	(.039)	(.047)	(.039)	(.068)	(.05)	(.069)	(.05)
Δ in HH income	.036	-.093	-.016	-.132	-.158	-.338	-.158	-.338
	(.313)	(.246)	(.31)	(.245)	(.449)	(.274)	(.449)	(.275)
Age	.001	.001	.001	.001	-.006	-.001	-.006	-.001
	(.003)	(.002)	(.003)	(.002)	(.004)	(.003)	(.004)	(.003)
Unemployed	.065	.048	.065	.047	.168	.03	.168	.031
	(.092)	(.079)	(.092)	(.079)	(.167)	(.108)	(.166)	(.108)
Cross-class partnership	.125***	.104**	-	-	.172**	.045	-	-
	(.047)	(.04)			(.077)	(.053)		
Cross-class partnership (down)	-	-	.08	.069	-	-	.175**	.047
			(.054)	(.046)			(.083)	(.06)
Cross-class partnership (up)	-	-	.171***	.139***	-	-	.167*	.042
			(.053)	(.046)			(.093)	(.062)
Within-class partnership (Reference)	-	-	-	-	-	-	-	-
Observations	568	568	568	568	337	337	337	337
Education and R-squared	.043	.319	.048	.322	.093	.535	.093	.535

Robust standard errors in parentheses. Analysis restricted to individuals who report to be not very interested or not at all interested in politics (at time t-1) in columns (1)-(4). Analysis restricted to individuals who report to be very interested or fairly interested in politics (at time t-1) in columns (5)-(8). The dependent variable is an index of attitudes towards the economy, with larger values indicating more left-wing attitudes. Dummies for respondent's level of education and for BHPS included. The sample includes those who have married or begun to cohabitate with his/her partner for the first time in their time in the BHPS between the present and the previous wave.

**** p<.01, ** p<.05, * p<.1*

Section B.15: Replication of Table 2.1 when education and unemployment status are measured as a first difference

Table B.23: Main results when educational attainment and unemployed as a first difference

	Index of attitudes towards the economy							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lag of index	-	.592***	.594***	.544***	-	.591***	.594***	.542***
		(.023)	(.023)	(.024)		(.023)	(.023)	(.024)
Male	-.066**	-	-.038*	-.06***	-.068**	-	-.041*	-.064***
	(.028)		(.023)	(.023)	(.029)		(.023)	(.023)
HH income (Δ)	-1.498***	-	-.348**	-.181	-1.489***	-	-.36**	-.201
	(.268)		(.146)	(.142)	(.268)		(.148)	(.144)
Education (Δ)	-	-	-.023	-.014	-	-	-.023	-.014
			(.019)	(.019)			(.019)	(.019)
Age	-.002	-	0	0	-.002	-	0	0
	(.002)		(.001)	(.001)	(.002)		(.001)	(.001)
Unemployed (Δ)	.038	-	-.035	-.041	.037	-	-.036	-.043
	(.062)		(.043)	(.041)	(.062)		(.043)	(.041)
Partner's attitudes	-	-	-	.215***	-	-	-	.217***
				(.022)				(.022)
Entering a cross-class partnership	.099***	.086***	.081***	.066***	-	-	-	-
	(.03)	(.025)	(.025)	(.024)				
Entering a cross-class partnership (down)	-	-	-	-	.085**	.073**	.066**	.039
					(.034)	(.028)	(.028)	(.028)
Entering a cross-class partnership (up)	-	-	-	-	.114***	.099***	.095***	.093***
					(.035)	(.028)	(.028)	(.027)
Entering a within-class partnership (Reference)	-	-	-	-	-	-	-	-
Observations	1472	1472	1469	1417	1472	1472	1469	1417
Education and BHPS dummies	YES	NO	YES	YES	YES	NO	YES	YES
R-squared	.065	.348	.364	.408	.065	.348	.364	.41

Robust standard errors in parentheses. The dependent variable is an index of attitudes towards the economy, with larger values indicating more left-wing attitudes. Dummies for BHPS wave included except in columns (2) and (6). Covariates are measured at t-1. HH income in columns (1) and (5), ⊿ in HH income in columns (2)-(4) and (6)-(8). Unemployed in columns (1) and (5), ⊿ in unemployed in columns (2)-(4) and (6)-(8). Educational attainment in columns (1) and (5), ⊿ in educational attainment in columns (2)-(4) and (6)-(8). The sample includes those who have married or begun to cohabitate with his/her partner for the first time in their time in the BHPS between the present and the previous wave.

**** p<0.01, ** p<0.05, * p<0.1*

Section B.16: Process followed for detecting those who just entered a domestic partnership

I proceed in the following way to detect those who just entered a domestic partnership. First, I look at the variable identifying the spouse or partner and compare it with the same variable in the previous wave. I categorize as entering a domestic partnership those for whom this variable is not zero in the present wave but was zero in all the previous ones. Since some individuals enter a domestic partnership more than once during their time at the BHPS, I keep only the first occurrence, for approaching as much as possible the "first marriage" situation.[13] I do so to homogenize as much as possible the changes experienced among individuals in my sample, as it may not be the same entering a domestic partnership for the first than for, say, the third time. Also, if not restricting the sample to one observation per individual I could face situations very difficult to interpret, as for example one person who first has a domestic partner from a lower position and later one from a higher position.

Finally, I exclude from the sample a reduced number of observations for which a deeper inspection shows that even if the partner's identifier was missing in the previous wave (*t-1*)—apparently not living with a spouse or partner at that time—the individual and her partner can be matched as living in the same household at *t-1* through the household identifier. In any case, the inclusion of these few cases does not change substantively the results.

[13] Note that this cannot be attained perfectly as individuals who enter the BHPS as single may have entered a domestic partnership for the first time and later divorced before entering the BHPS. In any case, further analyses which restrict the sample to those entering a domestic partnership in their younger ages—for approaching better the "first marriage" situation—display similar results (not shown).

Section B.17: Some clarifications about the data

What do I mean by wave? Throughout the analysis, wave refers to each of the seven waves employed and not to the entirety of the eighteen rounds of the BHPS. Therefore, if a given wave is 1995, the previous wave is 1993 and not 1994. If an individual was present in the survey in e.g., 1995 and 1991, but not in 1993, previous wave for that specific individual is represented by his 1991 observation. Still, cases like this represent a very low proportion of the totality of observations employed in my analysis.

Why restricting the analysis to those who have just entered a domestic partnership? Different specifications including more waves lead to ambiguity in the interpretation of the results. For example, by including individuals who have a domestic partner from a lower position and later one from a higher position. But especially, by establishing reference groups which are not theoretically and empirically obvious (see Section B.22 below).

Given I only employ one observation per individual in the main analyses, I do not perform a fixed-effects model. However, I do carry out in Section B.6 a first-difference analysis with periods *t* and *t-1*, a method which is equivalent to fixed effects when having two periods. Furthermore, the present setting has another important advantage. By focusing only on those entering a domestic partnership, the effect of that on economic attitudes is kept constant.

Why controlling for the attitude of the partner at time *t* exert a downward bias in the estimate of entering a cross-class partnership (β_2)? The reason why controlling for the attitudes of the partner at time *t* exerts a downward bias in the estimate of β_2 (with respect to the estimate that I would obtain if I could control for the attitudes of the partner at time *t-1*) is the following. I have found that those who enter a cross-class partnership move their preferences to the left—relative to those who enter a within-class partnership. Therefore, it seems natural to assume that the preferences of the partners of the individuals in cross-class partnerships in my sample have also moved to the left in relative terms between *t* and *t-1*, as these people—who are not in my sample—by definition have entered too a partnership with someone from a different class. This would mean that the positive correlation between entering a cross-class partnership and partner's preferences is stronger when preferences are measured at time *t* rather than at time *t-1*. Therefore, the estimation of β_2 will be downwardly biased when controlling for partner's preferences at time *t*—relative to the correct but unfeasible specification of controlling for that at time *t-1*.

Section B.18: Another item measuring attitudes towards the economy

Table B.24: Unfair that wealth buys medical priority

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lag of attitude	-	.444***	.441***	.419***	-	.445***	.442***	.42***
		(.034)	(.034)	(.035)		(.034)	(.034)	(.035)
Male	-.188**	-	-.094	-.126*	-.188**	-	-.095	-.129*
	(.074)		(.067)	(.068)	(.074)		(.067)	(.069)
HH income (Δ)	-1.165*	-	.058	.156	-1.169*	-	.042	.134
	(.609)		(.418)	(.432)	(.612)		(.417)	(.43)
Age	-.005	-	-.002	-.002	-.005	-	-.002	-.002
	(.004)		(.004)	(.004)	(.004)		(.004)	(.004)
Unemployed	.092	-	.04	.052	.095	-	.036	.046
	(.146)		(.123)	(.124)	(.146)		(.123)	(.124)
Partner's attitudes	-	-	-	.085**	-	-	-	.085**
				(.034)				(.034)
Cross-class partnership	.114	.175**	.161**	.162**	-	-	-	-
	(.08)	(.071)	(.071)	(.072)				
Cross-class partnership (down)	-	-	-	-	.129	.149*	.137	.118
					(.092)	(.083)	(.084)	(.085)
Cross-class partnership (up)	-	-	-	-	.1	.199**	.184**	.203**
					(.09)	(.078)	(.078)	(.079)
Within-class partnership (Reference)	-	-	-	-	-	-	-	-
Observations	881	881	881	845	881	881	881	845
Education and BHPS dummies	YES	NO	YES	YES	YES	NO	YES	YES
R-squared	.024	.2	.211	.211	.024	.201	.211	.212

Robust standard errors in parentheses. Larger values in the dependent variable indicate more agreement in a 5-point scale with the statement that it is unfair that wealth buys medical priority. Dummies for respondent's level of education and for BHPS wave included except in columns (2) and (6). HH income in columns (1) and (5), ⊿ in HH income in columns (2)-(4) and (6)-(8). The sample includes those who have married or begun to cohabitate with his/her partner for the first time in their time in the BHPS between the present and the previous wave.

**** p<0.01, ** p<0.05, * p<0.1*

Section B.19: Disaggregating the economic-attitudes index into two

- Items on the economic policy mood index: "Major public services and industries ought to be in state ownership", "Private enterprise is the best way to solve Britain's economic problems" (reversed), "It is the government's responsibility to provide a job for everyone who wants one", and "Strong trade unions are needed to protect the working conditions and wages of employees".
- Items on the perceived class bias index "There is one law for the rich and one law for the poor", and "Ordinary people get their fair share of the nation's wealth" (reversed).

Table B.25: Effects on economic policy mood and perceived class bias

	Perceived class bias				Index of economic policy mood			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lag of index	.457***	.419***	.457***	.417***	.55***	.519***	.55***	.518***
	(.024)	(.024)	(.024)	(.024)	(.026)	(.026)	(.026)	(.026)
Male	.013	-.001	.008	-.008	-.08***	-.105***	-.08***	-.107***
	(.035)	(.035)	(.035)	(.035)	(.027)	(.027)	(.027)	(.027)
HH income (Δ)	-.062	.143	-.087	.109	-.44***	-.288*	-.44***	-.298*
	(.242)	(.241)	(.246)	(.245)	(.164)	(.162)	(.165)	(.163)
Age	.004**	.005**	.004**	.005***	-.002	-.002	-.002	-.002
	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)
Unemployed	.091	.073	.088	.068	.052	.053	.052	.052
	(.072)	(.072)	(.072)	(.071)	(.062)	(.06)	(.062)	(.06)
Partner's attitudes	-	.259***	-	.263***	-	.204***	-	.205***
		(.032)		(.032)		(.025)		(.025)
Cross-class partnership	.095**	.077**	-	-	.066**	.051*	-	-
	(.038)	(.038)			(.029)	(.029)		
Cross-class partnership (down)	-	-	.065	.033	-	-	.066**	.037
			(.044)	(.044)			(.033)	(.033)
Cross-class partnership (up)	-	-	.126***	.121***	-	-	.066**	.065**
			(.043)	(.043)			(.033)	(.033)
Within-class partnership (Reference)	-	-	-	-	-	-	-	-
Observations	1468	1416	1468	1416	1469	1417	1469	1417
Education and BHPS dummies	YES	NO	YES	YES	YES	NO	YES	YES
R-squared	.253	.286	.254	.289	.327	.362	.327	.363

Robust standard errors in parentheses. The dependent variable in columns (1)-(4) is an economic policy mood index, while one of perceived class bias in columns (5)-(8). Larger indicate more left-wing attitudes. Dummies for respondent's level of education and for BHPS wave included. The sample includes those who have married or begun to cohabitate with his/her partner for the first time in their time in the BHPS between the present and the previous wave.
**** p<0.01, ** p<0.05, * p<0.1*

Table B.26: Effects on economic policy mood and perceived class bias (Only not interested in politics)

	Perceived class bias				Index of economic policy mood			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Lag of index	.442***	.401***	.443***	.402***	.489***	.457***	.488***	.456***
	(.038)	(.038)	(.037)	(.037)	(.041)	(.043)	(.041)	(.043)
Male	-.019	-.028	-.029	-.042	-.058	-.073*	-.058	-.075*
	(.058)	(.056)	(.058)	(.056)	(.045)	(.044)	(.045)	(.044)
HH income (Δ)	-.028	.185	-.125	.084	-.074	-.012	-.079	-.028
	(.32)	(.299)	(.317)	(.294)	(.293)	(.27)	(.294)	(.269)
Age	.004	.005*	.004	.005*	0	.001	0	.001
	(.003)	(.003)	(.003)	(.003)	(.003)	(.003)	(.003)	(.003)
Unemployed	.001	-.041	0	-.041	.071	.092	.071	.092
	(.115)	(.108)	(.114)	(.107)	(.094)	(.09)	(.094)	(.09)
Partner's attitudes	-	.29***	-	.297***	-	.181***	-	.183***
		(.048)		(.048)		(.038)		(.039)
Cross-class partnership	.106*	.089	-	-	.1**	.075	-	-
	(.062)	(.061)			(.048)	(.047)		
Cross-class partnership (down)	-	-	.02	-.007	-	-	.095*	.059
			(.069)	(.068)			(.054)	(.054)
Cross-class partnership (up)	-	-	.194***	.185***	-	-	.105**	.091*
			(.07)	(.069)			(.053)	(.052)
Within-class partnership (Reference)	-	-	-	-	-	-	-	-
Observations	566	544	566	544	565	543	565	543
Education and BHPS dummies	YES	NO	YES	YES	YES	NO	YES	YES
R-squared	.25	.296	.26	.307	.262	.292	.262	.292

Robust standard errors in parentheses. Sample restricted to those who report to be not very interested or not at all interested in politics (at time t-1). The dependent variable in columns (1)-(4) is an economic policy mood index, while one of perceived class bias in columns (5)-(8). Larger indicate more left-wing attitudes. Dummies for respondent's level of education and for BHPS wave included. The sample includes those who have married or begun to cohabitate with his/her partner for the first time in their time in the BHPS between the present and the previous wave.

**** p<0.01, ** p<0.05, * p<0.1*

Section B.20: Checking for non-linear effects of partners' class differences

Aiming to address the possibility of non-linear effects of partners' class differences, I have performed some analyses, whose results I show in this section of the Appendix. I have created a variable ("d") taking the (absolute) distance in social classes in the partnership. Theoretically, this ranges from 0 (homogamous partnerships) to 5 (partners on opposite extremes of the social-class schema). In my sample, this variable never takes value 5; indeed only 2% of observations take a value of 4, and only 10% a value of 3.

I have obviously removed the dummy for cross-class partnership and included as regressors "d" and the square of that ("d^2"). The results show indeed a slightly concave relationship between "d" and economic attitudes. The effect of cross-class partnerships is larger for partnerships between people from proximate classes. However, that effect does not decrease too much for more distant partnerships.

Table B.27: A non-linear effect of class differences?

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All				Interested in politics			
Lag of index	-	.592***	.59***	.541***	-	.562***	.558***	.502***
		(.023)	(.023)	(.024)		(.037)	(.037)	(.039)
Male	-.066**	-	-.042*	-.063***	-.07	-	-.031	-.043
	(.028)		(.023)	(.023)	(.047)		(.039)	(.038)
HH income (Δ)	-1.506***	-	-.332**	-.164	-1.297***	-	-.095	.029
	(.268)		(.145)	(.142)	(.43)		(.247)	(.226)
Age	-.002	-	0	0	0	-	.001	.002
	(.002)		(.001)	(.001)	(.003)		(.002)	(.002)
Unemployed	.036	-	.071	.063	.021	-	.045	.04
	(.062)		(.051)	(.049)	(.093)		(.078)	(.074)
Partner's attitudes	-	-	-	.212***	-	-	-	.213***
				(.022)				(.034)
Class difference (d)	.111***	.098***	.08**	.065**	.113*	.104*	.111**	.102*
	(.038)	(.032)	(.032)	(.031)	(.062)	(.054)	(.055)	(.055)
Class difference (d^2)	-.026**	-.024**	-.02*	-.016	-.025	-.026	-.028	-.028
	(.013)	(.011)	(.011)	(.011)	(.022)	(.02)	(.02)	(.02)
Observations	1472	1472	1472	1420	568	568	568	546
Education and BHPS dummies	YES	NO	YES	YES	YES	NO	YES	YES
R-squared	.065	.348	.367	.411	.057	.302	.318	.361

Robust standard errors in parentheses. Class difference in absolute value (d) can range from 0 (those married with someone from the same social class) to 5 (those in opposite extremes of the six-class RGSC schema). All respondents (i.e., regardless of political interest) in columns (1)-(4). Only those not interested in politics in columns (5)-(8). The dependent variable is an index of attitudes towards the economy, with larger values indicating more left-wing attitudes. Dummies for respondent's level of education and for BHPS wave included except in columns (2) and (6). HH income in columns (1) and (5), ⊿ in HH income in columns (2)-(4) and (6)-(8). The sample includes those who have married or begun to cohabitate with his/her partner for the first time in their time in the BHPS between the present and the previous wave.

**** p<0.01, ** p<0.05, * p<0.1*

Section B.21: Separate analyses for those marrying and for those starting to cohabitate

Table B.28: Marriage versus cohabitation

	(1) Marriage	(2) Cohabit.	(3) Marriage	(4) Cohabit.
Lag of index	.615***	.571***	.615***	.57***
	(.039)	(.029)	(.039)	(.029)
Male	-.02	-.049*	-.02	-.052*
	(.042)	(.028)	(.042)	(.028)
HH income (Δ)	.093	-.53***	.095	-.544***
	(.272)	(.177)	(.273)	(.18)
Age	0	-.001	0	-.001
	(.002)	(.002)	(.002)	(.002)
Unemployed	.124	.061	.125	.062
	(.106)	(.057)	(.106)	(.057)
Cross-class partnership	.108**	.057*	-	-
	(.043)	(.03)		
Cross-class partnership (down)	-	-	.109**	.04
			(.048)	(.036)
Cross-class partnership (up)	-	-	.106**	.073**
			(.052)	(.034)
Within-class partnership (Reference)	-	-	-	-
Observations	469	1003	469	1003
R-squared	.426	.349	.426	.35

Robust standard errors in parentheses. The dependent variable is an index of attitudes towards the economy, with larger values indicating more left-wing attitudes. Dummies for respondent's level of education and for BHPS wave included. Odd columns restrict the sample to those marrying. Even columns, to those starting to cohabitate.

**** p<0.01, ** p<0.05, * p<0.1*

Section B.22: Establishing alternative reference groups

In this section of the Appendix, I propose alternative specifications to the main one used throughout the paper, in which I restricted the sample to those entering a domestic partnership between the present and the previous wave, and compare the preferences of those entering a cross-class partnership with the preferences of those entering a within-class partnership (reference group).

Table B.29: Alternative reference groups (entering any cross-class partnership)

Dependent variable	Index of attitudes towards the economy			
Reference group	(A) Everyone not just partnered out of class	(B) Everyone not partnered or partnered within same class	(C) Everyone not partnered	(D) Those just partnered within class
Lag of index	.6476***	.6476***	.6474***	.59***
	(.0043)	(.0043)	(.0043)	(.0234)
Male	-.0318***	-.0318***	-.0306***	-.0415*
	(.0038)	(.0038)	(.0038)	(.0231)
HH income (Δ)	-.0953**	-.0953**	-.0941**	-.3279**
	(.0378)	(.0378)	(.0378)	(.1448)
Age	-.0006***	-.0006***	-.0006***	-.0003
	(.0001)	(.0001)	(.0001)	(.0013)
Unemployed	.0556***	.0556***	.0535***	.0721
	(.0098)	(.0098)	(.0098)	(.0505)
Entering a partnership	-.0491**	-.0491**	-.0586***	-
	(.021)	(.021)	(.0211)	
Partnered out of class	.0009	-	-	-
	(.0038)			
Already partnered out of class	-	.0009	-	-
		(.0038)		
Already partnered	-	-	-.0141***	-
			(.0039)	
Entering a partnership	.0713***	.0722***	.0724***	.0716***
*partnered out of class	(.0249)	(.0246)	(.0246)	(.0247)
Observations	49343	49343	49343	1472
R-squared	.479	.479	.4791	.3671

Robust standard errors clustered at the level of the individual in parentheses. The dependent variable is an index of attitudes towards the economy, with larger values indicating more left-wing attitudes. Dummies for respondent's level of education and for BHPS wave included.
**** p<0.01, ** p<0.05, * p<0.1*

The first row in each column of Tables B.29-B.30 states the reference group employed in that given analysis: everyone not entering a cross-class partnership in first column, everyone not partnered or partnered within the same class in second column, everyone not partnered in third column, and finally, I show again in column 4 the results of the main specification employed throughout the paper.[14] Then,

[14] In models A-C, the reference group is basically every set of observations which is not controlled for. Let us take the third model (C) to explain this. All observations in the sample can be divided into those not partnered, those already partnered (either out or within class), and those entering a partnership (either out or within class). As the reference group in Model C is those not partnered, we should control for every observation which is not included in that definition, namely those already partnered and those entering a partnership (either up or down). Nonetheless, in Model A I need to additionally

the first three models employ all respondents in the BHPS (also those who never enter a domestic partnership during their time in the survey) and all observations (also those before *t-1* and after *t*). That is not the case for the last model, which restricts the sample to those individuals who have just entered a partnership between a given wave (*t*) and the previous one (*t-1*) for their first time in the survey (i.e., it focuses on those who entered a domestic partnership during their time in the survey and takes only one observation per individual).

Table B.30: Alternative reference groups (down/up cross-class partnerships)

Dependent variable	Index of attitudes towards the economy			
Reference group	(A) Everyone not just partnered out of class	(B) Everyone not partnered or partnered within same class	(C) Everyone not partnered	(D) Those just partnered within class
Lag of index	.6475***	.6475***	.6474***	.5896***
	(.0043)	(.0043)	(.0043)	(.0235)
Male	-.0315***	-.0315***	-.0307***	-.0431*
	(.0038)	(.0038)	(.0038)	(.0232)
HH income (Δ)	-.0957**	-.0957**	-.0945**	-.3374**
	(.0378)	(.0378)	(.0378)	(.1465)
Age	-.0006***	-.0006***	-.0006***	-.0003
	(.0001)	(.0001)	(.0001)	(.0013)
Unemployed	.0554***	.0554***	.0535***	.0712
	(.0098)	(.0098)	(.0098)	(.0504)
Entering a partnership	-.0492**	-.0492**	-.0586***	-
	(.021)	(.021)	(.0211)	
Partnered down	-.0029	-	-	-
	(.0047)			
Partnered up	.0046	-	-	-
	(.0045)			
Already partnered down	-	-.0029	-	-
		(.0047)		
Already partnered up	-	.0046	-	-
		(.0045)		
Already partnered	-	-	-.0141***	-
			(.0039)	
Entering a partnership	.0774***	.082***	.0822***	.0828***
*partnered down	(.0283)	(.0279)	(.0279)	(.0281)
Entering a partnership	.0653**	.0624**	.0626**	.0606**
*partnered up	(.0286)	(.0282)	(.0282)	(.0285)
Observations	49343	49343	49343	1472
R-squared	.479	.479	.4791	.3674

Robust standard errors clustered at the level of the individual in parentheses. The dependent variable is an index of attitudes towards the economy, with larger values indicating more left-wing attitudes. Dummies for respondent's level of education and for BHPS wave included.
**** p<0.01, ** p<0.05, * p<0.1*

control for entering a partnership and for "partnered out of class" because otherwise I would not be controlling for the constitutive terms of the interaction (see e.g., Brambor et al., 2006). This is highly problematic due to the conflation of different effects. Namely, if not controlling for the constitutive terms of the interaction here, I would be conflating the effect of entering a cross-class partnership with the effect of entering a partnership, which may affect economic attitudes itself as it implies a change in material circumstances.

Table B.29 shows the effect of entering any cross-class partnership, whereas Table B.30 distinguishes between those with a partner from a higher class and those with a partner from a lower class. In terms of the controls included, I show here the results of analyses analogous to those in models 3 and 7 of Table 2.1, namely including all controls except for the one about partner's attitude at time *t*.[15] The main coefficient of interest here corresponds to the interaction between entering a domestic partnership and out-class partnership (making the up/down distinction in Table B.30): "Entering a partnership*partnered out of class". As models A-C include more than one observation per individual, I cluster standard errors at the level of the individual.

As can be seen, I find very similar results regardless of the reference group employed. Still, I use throughout the paper my preferred specification because it allows for a non-ambiguous interpretation of the results, as it responds to the question about how the preferences of those entering a cross-class partnership move compared to the change experienced by those entering a within-class one. Namely, I claim that those entering a within-class partnership are the natural control group of those entering a cross-class one, as these two groups are going through a similar process and stage of life (i.e., entering a domestic partnership), and therefore are highly comparable.

[15] The difference between the variables "Already partnered out of class" and "Partnered out of class" is that, while the former variable takes value 0 for the observation in which the respondent is entering a cross-class partnership, the latter variable takes value 1 for that observation.

Section B.23: An alternative estimation strategy

I show in this section of the Appendix the results of conducting an alternative estimation strategy. First, I estimate the difference in the dependent variable between those entering a cross-class partnership and those who remain single throughout the panel. That would correspond to β_2 in the following equation:

$$y_{i,t} = \beta_0 + \beta_1 y_{i,t-1} + \beta_2\, Entering\ a\ cross\ class\ partnership_{i,t}\ (vs. remaining\ single\ thorughout\ the\ panel) + \vartheta' Year_{i,t} + \theta' X_{i,t-1} + \varepsilon_{i,t}$$

for i={entering a cross-class partnership, remaining single throughout the panel}

Then, I estimate the difference in the dependent variable between those entering a within-class partnership and those who remain single throughout the panel. That would correspond to α_2 in the following equation:

$$y_{i,t} = \alpha_0 + \alpha_1 y_{i,t-1} + \alpha_2 Entering\ a\ within\ class\ partnership_{i,t}\ (vs. remaining\ single\ thorughout\ the\ panel) + \tau' Year_{i,t} + \delta' X_{i,t-1} + \rho_{i,t}$$

for i={entering a within-class partnership, remaining single throughout the panel}

Finally, I test if the difference between the two differences $(\beta_2 - \alpha_2)$ is statistically significant. This diff-in-diff styled analysis shows that entering a cross-class partnership moves attitudes of individuals to the left beyond the effect of entering a within-class partnership, compared to those who remain single throughout the panel (see Table B.31 below). This is also the case for those individuals not interested in politics at *t-1*.

Table B.31: An alternative estimation strategy

	(1)	(2)	(3)	(4)
	All		Not interested in politics	
Cross-class Partnership (β_2)	.007 (.015)	-	.034 (.023)	-
Within-class Partnership (α_2)	-	-.068*** (.021)	-	-.061* (.035)
Always single (Reference)	-	-	-	-
Lag of index	.593*** (.008)	.593*** (.008)	.543*** (.013)	.539*** (.014)
Male	-.033*** (.008)	-.037*** (.009)	-.041*** (.014)	-.05*** (.014)
HH income (Δ)	-.124* (.071)	-.129* (.071)	-.059 (.141)	-.085 (.141)
Age	-.001*** (0)	-.001*** (0)	-.001*** (0)	-.001*** (0)
Unemployed	.047*** (.018)	.057*** (.018)	.033 (.029)	.057* (.03)
$\beta_2 - \alpha_2$	.074***		.095**	
Observations	12318	11738	4652	4396
R-squared	.401	.404	.332	.334

Robust standard errors in parentheses. All respondents (i.e., regardless of political interest) in columns (1)-(2). Only those not interested in politics in columns (3)-(4). The dependent variable is an index of attitudes towards the economy, with larger values indicating more left-wing attitudes. Dummies for respondent's level of education and for BHPS wave included. The sample includes those entering a cross-class partnership and those always single in columns (1) and (3), while those entering a within-class partnership and those always single in columns (2) and (4).
**** p<0.01, ** p<0.05, * p<0.1*

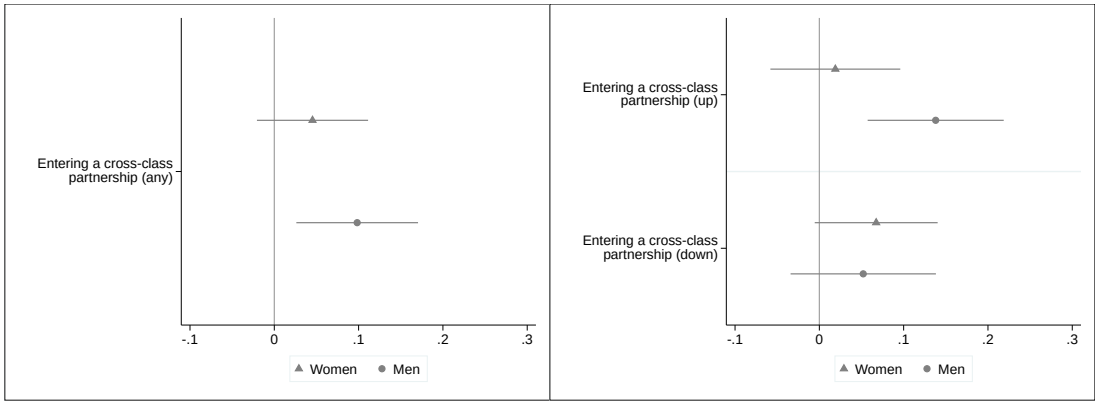
Section B.24: Heterogeneity by gender

Table B.32: Heterogeneity by gender

	Index of attitudes towards the economy							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Women	Men	Women	Men	Women	Men	Women	Men
Lag of index	.51***	.659***	.455***	.607***	.509***	.655***	.455***	.604***
	(.034)	(.033)	(.035)	(.033)	(.034)	(.033)	(.035)	(.034)
Δ in HH income	-.407**	-.194	-.224	-.017	-.382**	-.224	-.224	-.042
	(.186)	(.219)	(.178)	(.217)	(.186)	(.226)	(.179)	(.224)
Age	.000	.000	.001	.000	.000	.000	.001	.000
	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)
Unemployed	.099	.045	.085	.045	.096	.036	.085	.039
	(.074)	(.07)	(.069)	(.068)	(.074)	(.069)	(.069)	(.068)
Partner's attitudes	-	-	.193***	.261***	-	-	.193***	.257***
			(.028)	(.036)			(.028)	(.035)
Cross-class partnership	.045	.098***	.029	.086**	-	-	-	-
	(.034)	(.037)	(.033)	(.036)				
Cross-class partnership (down)	-	-	-	-	.067*	.052	.029	.051
					(.037)	(.044)	(.037)	(.043)
Cross-class partnership (up)	-	-	-	-	.019	.138***	.028	.116***
					(.039)	(.041)	(.039)	(.04)
Within-class partnership (Reference)	-	-	-	-	-	-	-	-
Observations	773	699	734	686	773	699	734	686
R-squared	.298	.427	.348	.472	.299	.431	.348	.474

Robust standard errors in parentheses. Odd columns display results for women. Even columns, for men. The dependent variable is an index of attitudes towards the economy, with larger values indicating more left-wing attitudes. Dummies for respondent's level of education and for BHPS wave included. The sample includes those who have married or begun to cohabitate with his/her partner for the first time in their time in the BHPS between the present and the previous wave.
**** p<.01, ** p<.05, * p<.1*

Figure B.2: Heterogeneity by gender



Point estimates and 95% confidence intervals for the coefficients of entering a cross-class partnership. The reference group is in all models those entering a within-class domestic partnership. The figure on the left (right) corresponds to a model analogous to the one in column 3 (7) of Table B.32. See Table B.32 for full results. The dependent variable is an index of attitudes towards the economy, with larger values indicating more left-wing attitudes.

Appendix C: Supplementary data for Chapter 3

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Section C.1: The transition to democracy in Spain: Not challenging social hierarchies

One could argue that the coming of democracy to Spain after the death of the dictator in 1975 brought a new wave of ideas to society which should have reshaped the attitudes of Spaniards towards vertical institutions such as the police. However, I claim that the nature of the Spanish transition to democracy limited the capacity to overturn much of the attitudinal imprint left by the dictatorship, at least of those socialized during the Civil War and its aftermath.

The work of Robert Fishman is very illustrative when aiming to understand and analyze the Spanish transition to democracy. The comparison with other transitions—specially the Portuguese one—allows Fishman to highlight the distinctive features of this period of time in Spain as well as the implications of that for democratic practice. Whereas in the Portuguese case political change came from actors within the *state* (initiated by a middle-level officers' revolt in the military), the crucial move towards democracy after the death of Franco in Spain was assumed by reformist sectors within the *regime*. Therefore, while in Portugal there existed a *rupture* with the old regime, that was not the case of its neighboring country, which experienced a much more top-down *reform* (Fishman, 1990).

The revolutionary nature of the Portuguese transition brought a partial inversion of social hierarchies that the reformist Spanish one did not (Fishman, 2011). Actually, "resistance to domination" (2011, 253) is a significant theme in the commemoration of the "antihierarchical (…) Portugal's 1974 democratizing coup" (2011, 246), as well as "defense to disobedience" and "fundamental challenge to hierarchies" (2011: 247) are constitutive features of the way in which the origin of democracy is understood among the Portuguese. That is not the case at all in the more reformist and consensus-oriented Spanish case.

Fishman (2011) claims that these different historical pathways to democracy have an impact on several societal outcomes, one of them being cultural consumption. Fishman and Lizardo (2013) focus on cultural omnivorousness due to its link with anti-hierarchical

inclusivism. The authors claim that pedagogical practices followed in Portugal after the transition to democracy had a post-canonical orientation towards aesthetics which contrasts with the more hierarchical and rigid educational institutions of Spain, whose transition to democracy did not overturn hierarchies but instead focused on consensus, as a matter of discouraging polarizing endeavors such as the questioning of hierarchies. Then, I claim that the non-anti-hierarchical nature of the Spanish transition to democracy may have favoured the persistence of attitudes towards institutions such as the police by not questioning existing hierarchies in the country.

Section C.2: Analyses when repression is defined at the autonomous-community level: Explanation of dependent variables, full table of results, and dichotomous models

In this section of the Appendix, I clarify some aspects of the analyses which take ESS data, defining repression at the NUTS-2 level. First, I define in Figure C.1 the dependent variables employed, as well as show the way in which I dichotomize these for the analysis that I will perform later. Table C.1 shows full results associated with Figure 3.10 in the main text. Finally, Table C.2 repeats the analyses dichotomizing these dependent variables for a more intuitive interpretation of the results.

Figure C.1: Explanation of the dependent variables (different attitudes towards the police) employed in Figure 3.10

Phrasing of the question	Range of responses	Dichotomization
I generally support how the police usually act	1-5 (Disagree strongly- Strongly agree)	Positive (1): 4-5 Negative (0) :1-3
The police stand up for values that are important to people like me	1-5 (Disagree strongly- Strongly agree)	Positive (1): 4-5 Negative (0) :1-3
The police generally have the same sense of right and wrong as I do	1-5 (Disagree strongly- Strongly agree)	Positive (1): 4-5 Negative (0) :1-3
Taking into account all the things the police are expected to do, would you say they are doing a good job or a bad job?	1-5 (Very bad job- Very good job)	Positive (1): 4-5 Negative (0) :1-3
Based on what you have heard or your own experience, how often would you say the police generally treat people in Spain with respect?	1-4 (Not at all- Very often)	Positive (1): 3-4 Negative (0) :1-2
About how often would you say that the police make fair, impartial decisions in the cases they deal with?	1-4 (Not at all- Very often)	Positive (1): 3-4 Negative (0) :1-2
When dealing with people in Spain, how often would you say the police generally explain their decisions and actions when asked to do so?	1-4 (Not at all- Very often)	Positive (1): 3-4 Negative (0) :1-2
Trust in the police	0-10	No dichotomization

Table C.1: Full table of results associated with Figure 3.10

	(1)	(2)	(3)	(4)
Dependent variable	Support how the police acts	Police stand up for values important to me	Same sense of right and wrong as me	Police doing good job in country
Francoist repression	.211**	.197*	.302**	.105
	(.083)	(.107)	(.112)	(.069)
Age	.011***	.007***	.008***	.008***
	(.002)	(.002)	(.001)	(.002)
Left-right scale	.058***	.066***	.071***	.05***
	(.012)	(.011)	(.014)	(.01)
Income	.024*	.026***	.023**	.015
	(.013)	(.009)	(.008)	(.013)
Male	.01	-.018	.053	.015
	(.046)	(.041)	(.036)	(.031)
Rural	.014	.035	.007	-.031
	(.064)	(.09)	(.074)	(.051)
Observations	1338	1328	1323	1340
R-squared	.07	.059	.071	.046

OLS models. Definitions of the dependent variables in Figure C.1. Robust standard errors clustered at the autonomous-community level (NUTS 2) in parentheses. Constant not reported. Dummies for five levels of educational attainment included.
**** p<.01, ** p<.05, * p<.1*

Table C.1 (Cont.): Full table of results associated with Figure 3.10

	(5)	(6)	(7)	(8)
Dependent variable	Police treat people with respect	Police make fair and impartial decisions	Police explain their decisions when asked	Trust in the police
Francoist repression	.164***	.169**	.208**	.351
	(.056)	(.059)	(.072)	(.271)
Age	.007***	.006***	.006***	.019***
	(.001)	(.001)	(.001)	(.004)
Left-right scale	.033**	.044***	.043***	.113***
	(.011)	(.009)	(.01)	(.03)
Income	.022*	.01	.018	.041
	(.012)	(.009)	(.01)	(.038)
Male	.006	-.002	.02	-.064
	(.052)	(.03)	(.056)	(.161)
Rural	.033	.016	.053	.107
	(.027)	(.043)	(.051)	(.189)
Observations	1316	1275	1238	1340
R-squared	.053	.068	.047	.03

OLS models. Definitions of the dependent variables in Figure C.1. Robust standard errors clustered at the autonomous-community level (NUTS 2) in parentheses. Constant not reported. Dummies for five levels of educational attainment included.
**** p<.01, ** p<.05, * p<.1*

Table C.2: Dichotomization of the dependent variables

	(1) Support how the police acts	(2) Police stand up for values important to me	(3) Same sense of right and wrong as me	(4) Police doing good job in country	(5) Police treat people with respect	(6) Police make fair and impartial decisions	(7) Police explain their decisions when asked
Francoist repression	.096**	.051	.094	.046	.067***	.081*	.152***
	(.043)	(.049)	(.062)	(.042)	(.023)	(.039)	(.048)
Age	.005***	.004***	.004***	.004***	.003***	.003***	.003***
	(.001)	(.001)	(.001)	(.001)	(.001)	(0)	(.001)
Left-right scale	.022***	.03***	.031***	.021***	.013**	.023***	.026***
	(.005)	(.005)	(.006)	(.005)	(.005)	(.004)	(.006)
Income	.009	.007	.008	.009	.015***	.003	.01
	(.006)	(.005)	(.005)	(.008)	(.004)	(.005)	(.007)
Male	.035	.004	.04**	.039*	-.026	-.022	-.012
	(.03)	(.024)	(.016)	(.022)	(.021)	(.015)	(.031)
Rural	.02	.014	.012	-.036	.007	-.014	.017
	(.032)	(.043)	(.03)	(.027)	(.025)	(.031)	(.037)
Observations	1338	1328	1323	1340	1316	1275	1238
R-squared	.045	.042	.056	.034	.043	.056	.04

Linear probability models. Definitions of the dependent variables in Figure C.1. Robust standard errors clustered at the autonomous-community level (NUTS 2) in parentheses. Constant not reported. Dummies for five levels of educational attainment included.

**** p<.01, ** p<.05, * p<.1*

Section C.3: Main analyses when restricting the sample to respondents in rural areas

Given the implicit mechanism of intergenerational transmission of this and most papers in the study of historical legacies, I propose now a strategy for increasing the degree of correspondence between the region of residence of respondents and the region where the family of that lived decades ago—during the Civil War and the post-war period. That comprises restricting the sample of analysis to those individuals living nowadays in rural areas.

I claim that correspondence between respondent's region of residence and ancestors' region of residence during the Civil War and its aftermath might be much higher for those living nowadays in smaller towns rather than in cities. Indeed, I find strong empirical support for this straightforward claim in the Spanish census of 1991, the oldest one which allows for a cross-tabulation analysis. In that year, 86% of Spaniards living in rural areas were residing in their province of birth, that figure being only 72% for those living in urban areas.[1] This means that, if we randomly picked an individual from the whole Spanish population in 1991, the probability of that individual having been born in the same province as the one of residence was almost 20% larger for those living in rural rather than in urban areas. However, this already large gap only looks at the rural/urban differential of correspondence between region of residence and region of birth, not at the one of interest for my analysis: that is, the one comparing region of residence with region of ancestors, which of course must be strikingly higher than 20%, given the accumulation of differentials over generations and the fact that migrations in Spain were essentially unidirectional from rural to urban areas during most of the twentieth century (Romero Valiente, 2003: 213). Indeed, expanding the abovementioned differential at a constant rate across three further generations leads to a level of correspondence

[1] Rural defined as municipalities of up to 10,000 inhabitants.

between region of residence and region of ancestors for those in rural settings which is more than double that of those in urban areas.[2]

However, this is not the only reason for focusing on respondents in rural areas when addressing the Spanish Civil War. The second rationale concerns the subjective experience of repression. Even if the repressive atmosphere and fear breathable in cities were of course not negligible, these were particularly stifling and traumatizing in rural settings, where the small size of communities favored a more "prisonlike atmosphere" (Richards, 2013: 106), one in which political, social and personal conflicts intertwined strongly (Arnabat Mata, 2013; Richards, 2002, 2013). Moreover, authors such as Arnabat Mata (2013) claim that the largest shares of repression were observed in small municipalities.

Consistent with these arguments, I expect the effect of Francoist repression on attitudes towards the police to be larger if focusing on rural areas. Then, I repeat below the analyses addressing the relationship between Francoist repression and attitudes towards the police by restricting the sample to those individuals living nowadays in rural areas.[3] Table C.3 mirrors Table 3.1 in the main text (repression is defined at the municipality level). Tables C.4 and C.5 mirror Tables 3.2 and 3.3 in the main text (repression is defined at the province level). Finally, Figure C.2 mirrors Figure 3.10 in the main text (repression is defined at the autonomous-community level).[4] As can be observed in these tables and figures, the coefficients for Francoist repression are larger in terms of size and also more statistically significant when focusing on those in rural areas.

[2] Namely, $(0.86^4-0.72^4)/0.72^4 \approx 104\%$.

[3] In ESS analyses, I define as rural those in a "country village" or in a "farm or home in countryside". In CIS analyses, those in municipalities of up to 10,000 inhabitants. In ICPS analyses, municipalities of up to 10,000 inhabitants in 1936.

[4] I do not replicate Figure 3.9 because the 1993 round employed in that analyses hardly includes a hundred respondents living in rural areas.

Table C.3: Repression defined at the municipality level (Only respondents in rural areas)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Francoist repression	.083***	.08***	.085***	.082***	.128***	.084***	.078***	.07**
	(.03)	(.029)	(.03)	(.029)	(.037)	(.029)	(.029)	(.028)
Individual-level controls								
Age	.012***	.012***	.012***	.012***	.011***	.012***	.012***	.012***
	(.001)	(.001)	(.001)	(.001)	(.002)	(.001)	(.001)	(.001)
Left-right scale	.421***	.423***	.421***	.422***	.414***	.42***	.422***	.351***
	(.024)	(.024)	(.024)	(.024)	(.029)	(.024)	(.024)	(.023)
Male	-.335***	-.335***	-.335***	-.335***	-.355***	-.325***	-.335***	-.327***
	(.047)	(.047)	(.047)	(.047)	(.056)	(.047)	(.047)	(.046)
University degree	-.375***	-.376***	-.374***	-.375***	-.361***	-.371***	-.374***	-.325***
	(.06)	(.06)	(.06)	(.06)	(.07)	(.06)	(.06)	(.057)
Support for independence	-	-	-	-	-	-	-	-1.014***
								(.062)
Municipality-level controls								
Vote for the left in 1936	-	.555**	-	.419*	.497*	.443*	.458*	.247
		(.22)		(.239)	(.294)	(.237)	(.233)	(.227)
Electoral competition in 1936	-	-	-.551**	-.336	.324	-.296	-.328	-.283
			(.27)	(.3)	(.346)	(.301)	(.301)	(.268)
Unions (CNT/UGT) during the 1930s	-	-	-	.002	.069	-.015	-.007	-.041
				(.065)	(.074)	(.066)	(.063)	(.062)
Exiled (%)	-	-	-	-	-.004	-	-	-
					(.003)			
Clerics						1.838	-	-
						(4.826)		
Leftist repression	-	-	-	-	-	-	.073	-
							(.112)	
Province and year fixed effects	YES	YES	YES	YES	YES	YES	YES	YES
Standard deviation of elevation	NO	YES	YES	YES	YES	YES	YES	YES
Observations	10214	10206	10206	10206	7638	10,090	10206	10206
Municipalities	420	419	419	419	294	415	419	419
R-squared	.13	.131	.131	.132	.127	.128	.132	.167

OLS models. Restricted to respondents born in Catalonia and in rural area. Rural is defined as municipalities smaller than 10,000 inhabitants in 1936. The dependent variable in all models is evaluation of police performance, ranging from 0 to 10. Robust standard errors clustered at the municipal level in parentheses. Constant not reported. Time (round) dummies included. Province dummies included.
**** p<.01, ** p<.05, * p<.1*

Table C.4: Repression defined at the province level (Only respondents in rural areas)

	(1)	(2)	(3)	(4)	(5)	(6)
Francoist repression	.45***	.416**	.498***	.458***	.472***	.376**
	(.165)	(.179)	(.144)	(.152)	(.139)	(.141)
Individual-level controls						
Age	.005*	.005**	.005**	.005**	.006**	.008***
	(.003)	(.002)	(.002)	(.002)	(.002)	(.002)
Left-right scale	.205***	.214***	.21***	.214***	.209***	.206***
	(.029)	(.03)	(.028)	(.03)	(.028)	(.027)
Income	-.043	-.043	-.038	-.04	-.036	-.009
	(.033)	(.031)	(.034)	(.033)	(.031)	(.028)
Male	-.271***	-.258***	-.256***	-.253***	-.256***	-.267***
	(.059)	(.06)	(.061)	(.061)	(.06)	(.06)
Province-level controls						
Vote for the left in 1936	-	.016**	-	.01	.008	.017
		(.006)		(.009)	(.011)	(.011)
Electoral competition in 1936	-		1.57***	.973	.85	.248
			(.564)	(.877)	(.813)	(.809)
Agrarian inequality in 1860	-	-	-	-	0	-.01
					(.008)	(.008)
Anti-clerical violence	-	-	-	-	.454**	.371**
					(.171)	(.146)
Province loyal to the Republic in the coup	-	-	-	-	.033	-.056
					(.197)	(.203)
Illiteracy rate in 1920	-	-	-	-	-	.016***
						(.005)
Observations	3534	3534	3534	3534	3534	3534
R-squared	.078	.085	.085	.086	.09	.096

OLS models. Restricted to respondents in rural areas. Rural is defined as municipalities of up to10,000 inhabitants. The dependent variable in all models is respondent's rating of the police profession, ranging from 0 to 10. Robust standard errors clustered at the province level (NUTS 3) in parentheses. Constant not reported. Time (round) dummies included. Dummies for five levels of educational attainment included.

**** p<.01, ** p<.05, * p<.1*

Table C.5: Competing explanations (repression defined at the province level, only respondents in rural areas)

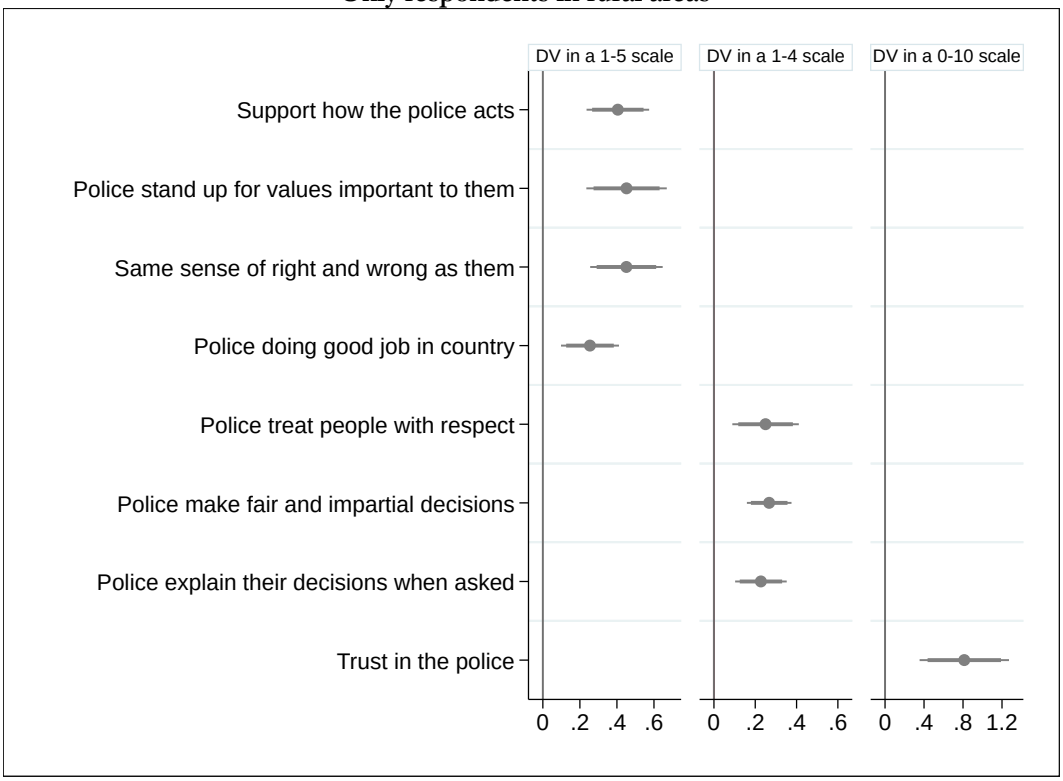
Dependent variable	Rating of the police profession						
Covariate controlled for in each model (Province-level variables except in column 5)	(1) Cafes in 1867	(2) Leftist repression	(3) Relative, close friend or acquaintance in the police	(4) Police charges during the Transition	(5) Proud of being Spanish	(6) Set of regional dummies (see text)	(7) Crime rate
Francoist repression	.379**	.426**	.436**	.465***	.245*	.412**	.534***
	(.17)	(.175)	(.186)	(.156)	(.145)	(.179)	(.155)
Province-level covariate	-.173**	.073	-.0001	-1.235***	.67***	†	1.33***
	(.072)	(.34)	(.309)	(.35)	(.07)		(.492)
Observations	3534	3028	3147	3534	3487	3534	3534
R-squared	.085	.077	.081	.095	.128	.104	.084

OLS models. Restricted to respondents in rural areas. Rural is defined as municipalities of up to10,000 inhabitants. The dependent variable in all models is respondent's rating of the police profession, ranging from 0 to 10. Robust standard errors clustered at the province level (NUTS 3) in parentheses. Constant not reported. Time (round) dummies included. Individual-level controls in all models: age, left-right scale, income, gender, dummies for five levels of educational attainment, living in rural area.

†: *the coefficients for the different regions are .053(Coast region), -.423***(North region), -1.605***(Basque region), -.008(Central region), Reference (Madrid region).*

**** p<.01, ** p<.05, * p<.1*

Figure C.2: Francoist repression and different attitudes towards the police
(Repression defined at the autonomous-community level)
Only respondents in rural areas



Point estimates with 90% (thick line) 95% (thin line) confidence intervals for the coefficient of Francoist repression. Restricted to respondents in rural areas (i.e., those in a "country village" or in a "farm or home in countryside"). OLS models predicting individuals' attitudes towards the police. All models control for age, left-right scale, income, gender, rural and dummies for educational attainment. Robust standard errors clustered at the regional (NUTS 2) level. Dependent variables in the first column range from 1 to 5, in the second column from 1 to 4, and in the third column from 0 to 10.

Section C.4: Explanation of the dependent variables and models employed in analyses of different attitudes and behaviours of Figures 3.11-3.14

Figure C.3: Explanation of the dependent variables and sample size in analyses of Francoist repression and various attitudes and behaviours (Figures 3.11-3.14)

Model	Phrasing of the question	Range of responses	N (Sample)
1	Among the following, what is the most important for a country? A stable economy (1)/A big army (1)/Progress toward a more human society (0)/Protecting the environment (0)	Dichotomous (0-1)	625
2	What is the most important social value? Citizen security (1) /Individual freedom (0)/Social equality (0)/Solidarity (0)	Dichotomous (0-1)	2,847
3	What is more serious? The possibility that a guilty person is released (1)/The possibility that an innocent person is punished (0)	Dichotomous (0-1)	2,642
4	Having taken part in lawful public demonstration in the last 12 months	Dichotomous (0-1)	14,484
5	It is important to do what is told and follow rules	1-6 (Not like me-Very much like me)	1,372
6	It is important to live in secure and safe surroundings	1-6	843
7	It is important to follow traditions and customs	1-6	901
8	It is important that government is strong and ensures safety	1-6	14,283
9	It is important to seek adventures and have an exciting life	1-6	14,439
10	It is important to try new and different things in life	1-6	14,416
11	It is important to make own decisions and be free	1-6	14,407
12	It is important to think new ideas and being creative	1-6	14,413
13	Schools must teach children obey authority	1-5 (Disagree strongly-Strongly agree)	14,377
14	The law should always be obeyed	1-5	14,416
15	You should always obey law even if it means missing good opportunities	1-5	14,335
16	To be a good citizen: how important to always obey laws/regulations	0-10 (Extremely unimportant- Extremely important)	857
17	Important for democracy: prevent people from expressing extreme political views	0-10 (Not at all important-Extremely important)	207

Figure C.4: Models in analyses of Francoist repression and various attitudes and behaviours (Figures 3.11-3.14)

Models	Level of aggregation at which Francoist repression is defined (also level at which robust standard errors are clustered)	Dataset	Controls included
1	Municipalities (Catalonia)	ICPS	Age, left-right scale, male, university degree, Republican repression, vote for the left in 1936, electoral competition in 1936, unions during the 1930s, standard deviation of elevation, province dummies. No year fixed effects, as only one round of the survey contains the item.
2-3	Provinces	CIS	Age, left-right scale, economic status, gender, dummies for six levels of educational attainment, living in rural area, vote for the left in 1936, electoral competition in 1936, agrarian inequality in 1860, anti-clerical violence, province loyal to the Republic in the coup. No year fixed effects, as only one round of the survey contains the item.
4-17	Autonomous communities (NUTS 2)	ESS	Age, left-right scale, gender, dummies for five levels of educational attainment, living in rural area. Income, where the variable measuring income is comparable across the rounds employed in the analysis; feeling about household's income, where the variable measuring income is not comparable across the rounds employed in the analysis. Year fixed effects, where the item is present in more than one round of the survey.

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