Ethnicization in Welfare State Politics

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This dissertation is submitted for the degree of
Doctor of Philosophy

January 2016

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Summary

A class of countries, so-called universal welfare states, distinguish themselves by having developed encompassing welfare states with high levels of economic redistribution. In recent decades, these countries have also experienced considerable immigration from non-Western countries and, accordingly, rising levels of ethnic diversity. Since higher levels of ethnic diversity are globally associated with lower levels of economic redistribution, scholars have hypothesized that rising ethnic diversity will put downwards pressure on redistribution levels in universal welfare states.

In the literature on this question, the case of the United States has become a near-universal analytical template for how to think about the effects of diversity on redistribution. Americans' attitudes toward welfare are widely considered 'racialized', i.e. in part based on attitudes toward racial outgroups. By the same token, we can think of political attitudes in universal welfare states as potentially 'ethnicized', i.e. in part based on attitudes toward ethnic outgroups.

In this dissertation, I examine when and how ethnicization occurs. The dissertation's frame, chapters 1–3, presents my argument and ties the dissertation's papers together. I begin by outlining empirical patterns which challenge the predictions of prevailing theoretical approaches. Contrary to typical predictions, changes in ethnic diversity are not robustly associated with changes in welfare spending. At the same time, citizens in universal welfare states readily subscribe to anti-immigrant attitudes.

I argue that this confusion stems in part from insufficient attention to citizen psychology. I outline a framework based on evolutionary psychology which accounts for why citizens' policy attitudes can be ethnicized, but also why some issues are more likely to be ethnicized than others. In short, attitudes are ethnicized when citizens are exposed to group cues, from local contexts or mass media, that provide a meaningful link between the policy and stereotypes about an ethnic outgroup. By this criterion, welfare is not likely to be ethnicized, but other issues – e.g., European integration and crime – are. The existing literature, often too mechanically applying the American experience onto universal welfare states, has tended to miss this point.

The dissertation includes four academic papers, presented in chapters 4–7. In paper A, 'Who benefits', I demonstrate the role of stereotypes in opposition to European cross-border welfare rights, often denoted 'welfare chauvinism'. In an original large-scale survey experiment, respondents' evaluations of the policy are sensitive to cues about recipients' country of origin and family size.

In paper B, 'European integration', I argue that political salience of immigration can ethnicize attitudes toward European integration. I first compare two euro referendums, showing that only where immigration was salient did ethnic prejudice predict vote choice and a subset of voters explain their vote in terms of identity. I then demonstrate a similar pattern in cross-national time-series data, showing that immigration attitudes and support for European integration are more closely associated when immigration is politically salient.

In paper C, 'Immigration debate', I analyze media coverage of immigration in Danish news media across 25 years. Many accounts characterize coverage as having grown increasingly negative over time. Analyzing the full text of a sample of 68,000 newspaper articles, I provide evidence against the posited negative trend. I also show that the most negative newspapers, tabloids, disproportionately cover immigration through stories about crime.

In paper D, 'Local contexts', I propose that exposure to rising ethnic diversity in the local context can in itself give rise to group-centric attitudes. Using two large data sets on citizen attitudes and local ethnic diversity, I show that crime and immigration attitudes are more closely associated in ethnically diverse localities. The finding challenges prevailing explanations of group-centric attitudes, which have tended to emphasize the role of elites.

Altogether, these papers illustrate the influence of group identities in political cognition. They suggest that compared to predictions in the existing literature, ethnicization is at once more limited (in that it occurs for some issues, but not the widely studied case of welfare) and more pervasive (in that in can arise from local contexts as well as from media). It is an important mechanism by which immigration can influence political life, even when the agenda ostensibly revolves around something else.

Dansksproget resumé

Lande karakteriseret ved såkaldt universelle velfærdsstater, herunder Danmark, udmærker sig i global samenhæng ved høje velfærdsydelser. I de seneste årtier har universelle velfærdsstater samtidig oplevet betydelig ikke-vestlig indvandring og heraf følgende øget etnisk diversitet. På tværs af lande hænger etnisk diversitet generelt sammen med lavere velfærdsydelser. Det har fået forskere til at diskutere, om etnisk diversitet i universelle velfærdsstater vil lægge et nedadgående pres på velfærdsydelserne.

Litteraturen om dette spørgsmål har typisk taget udgangspunkt i erfaringerne fra USA. I forskningen i politisk holdningsdannelse er det anerkendt, at amerikaneres holdninger til velfærdsydelser er 'racialiserede', altså præget af deres holdninger til racemæssige minoriteter. Efter samme logik kan man tænke på borgeres holdninger til et politisk emne som 'etnificerede', når holdningerne er præget af holdninger til etniske minoriteter.

I denne afhandling undersøger jeg hvornår og hvordan politiske holdninger etnificeres. Afhandlingens ramme, kapitel 1–3, præsenterer mit argument og binder afhandlingens artikler sammen. Jeg opridser først nogle empiriske træk, som trodser fremherskende teoretiske tilganges forudsigelser. Først og fremmest hænger ændringer i etnisk diversitet faktisk ikke konsistent sammen med ændringer velfærdsydelser. Men samtidig udviser borgere i universelle velfærdsstater tydelig uvilje mod indvandrere.

Jeg argumenterer for, at en del af forvirringen skyldes en for svag teoretisk kobling til politisk psykologi. Jeg opridser en teoretisk tilgang baseret på evolutionær psykologi, som forklarer hvorfor politiske holdninger kan etnificeres, men også hvorfor det er mere sandsynligt for nogle politiske emner end for andre. Kort fortalt etnificeres politiske holdninger, når borgere møder information, enten fra lokale erfaringer eller fra medier, som meningsfuldt kobler et politisk emne med en fremherskende gruppestereotyp. Efter dette kriterie er etnificering af holdninger til velfærdsydelser i universelle velfærdsstater ikke sandsynlig. Det er det til gengæld for andre emner, såsom kriminalitet eller forholdet til EU. Den eksisterende litteratur, som ofte mekanisk overfører de amerikanske erfaringer til universelle velfærdsstater, har savnet blik for denne sondring.

Afhandlingen omfatter fire videnskabelige artikler, præsenteret i kapitlerne 4–7. I artikel A, 'Who benefits', demonstrerer jeg betydningen af stereotyper for modstand mod europæiserede velfærdsrettigheder, såkaldt 'velfærdschauvinisme'. Jeg finder i et surveyeksperiment, at respondenters støtte til europæiserede velfærdsrettigheder afhænger af deres mentale billeder af modtagerens oprindelsesland og familiestørrelse.

I artikel B, 'European integration', argumenterer jeg for at holdninger til EU kan blive etnificerede. Først sammenligner jeg de danske og svenske euroafstemninger, og viser at kun blandt danskere kan modstand mod indvandring forudsige stemmevalg, og kun blandt danskere begrunder mange vælgere deres stemme i national identitet. Dernæst viser jeg et lignende mønster på tværs af lande, hvor holdninger til indvandring og holdninger til EU hænger tættere sammen, når indvandring er højere på den politiske dagsorden.

I artikel C, 'Immigration debate', analyserer jeg dækning af indvandringsspørgsmålet i danske avisartikler gennem 25 år. Mange diskussioner tegner et billede af en dækning, der bliver mere negativ over tid. I analyser af teksten fra mere end 68,000 avisartikler finder jeg, at dækningen ikke er blevet mere negativ over tid. Jeg finder også at de mest negative aviser, tabloidaviser, fokuserer særligt meget på indvandring i relation til kriminalitet.

I artikel D, 'Local contexts', argumenterer jeg for at oplevelse af indvandring i lokalområdet i sig selv kan fremme stereotyper om indvandrere. Med afsæt i to store datasæt om danskeres holdninger og lokal etnisk diversitet viser jeg, at holdninger til indvandring og holdninger til kriminalitet hænger tættere sammen i områder med høj etnisk diversitet. Resultatet udfordrer fremherskende perspektiver på etniske stereotyper, som lægger vægt på eliters indflydelse.

Samlet set illustrerer artiklerne gruppeidentiteters betydning i politisk holdningsdannelse. Sammenholdt med den eksisterende litteratur indikerer de at etnificering på én gang er mere begrænset (idet det kan forekomme for nogle emner, men ikke det hyppigst omtalte emne, velfærdsydelser) og mere omfangsrig (idet det kan fremmes både af lokale erfaringer og gennem medier). Etnificering af holdninger er en måde hvorpå indvandring kan præge det politiske liv, selv når emnet tilsyneladende har forladt den politiske dagsorden.

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Chapter 1

Introduction

Conflict organized along ethnic or racial divisions, whether violent or symbolic, is a feature of political life in many contemporary societies. In the grand scheme of things, this is unexceptional. Across the history of the human species, interethnic conflict has been the rule rather than the exception. Ethnicity, as Horowitz writes, "has fought and bled and burned its way into public and scholarly consciousness" (1985, xv).

Yet even in the absence of violent conflict, ethnic loyalties can be detrimental to political life. Politics based on ethnic or racial loyalties are difficult to reconcile with the principles of equality under the law that are foundational to modern, liberal democracies. And the problem is not only a theoretical one. Across all countries, ethnic diversity is robustly associated with weakened provision of public goods, suggesting ethnic diversity erodes large-scale cooperation (Alesina and Glaeser, 2006; Habyarimana et al., 2007).

The cross-country evidence is corroborated by lessons drawn from one particular, distinctly high-profile case: the politics of welfare in the US. A large literature is dedicated to explaining why US citizens 'hate welfare' (Gilens, 2000). Nearly all of this literature assigns a critical role to the issue of race. In short, it is at this point widely accepted that welfare attitudes in the US are 'racialized', i.e. support for welfare spending among white Americans in large part reflects attitudes toward racial minorities. The case of racialized welfare attitudes in the US has guided scholarly thinking far beyond US borders. As Banting (2005) notes, the US experience has become a "master narrative", a kind of universal analytical template for how to think about the effects of racial/ethnic diversity on support for redistribution. In societies where ethnic diversity is a historically recent phenomenon, these empirical patterns have given rise to concerns about the potential emergence of political fractures along ethnic lines (Soroka et al., 2006).

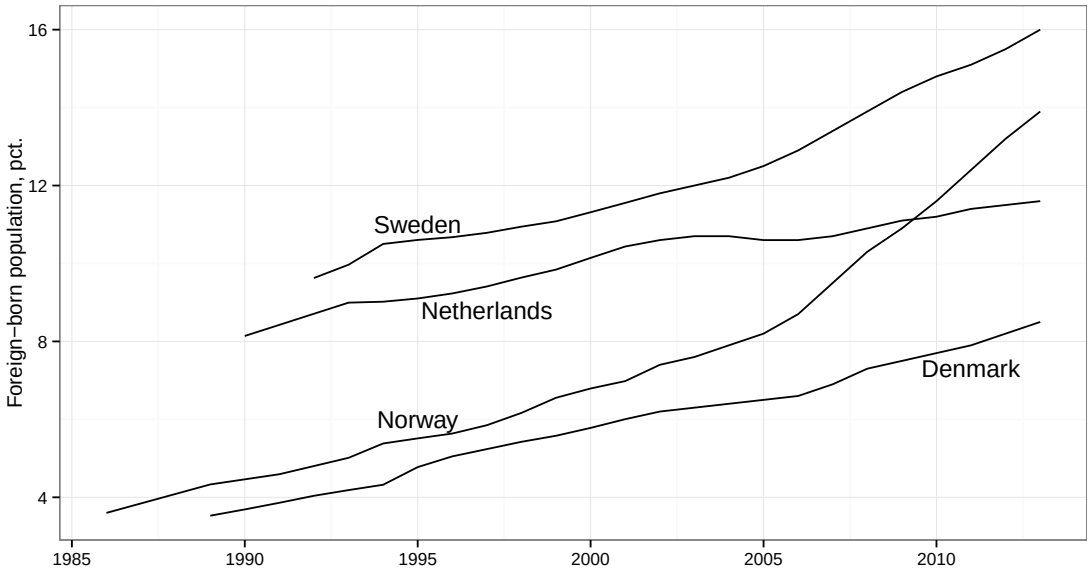

Figure 1.1 Foreign-born population as pct. of total population in Denmark, Norway, Sweden, and the Netherlands, 1986-2013. The countries are included based on typical classifications of universal welfare states, e.g. Esping-Andersen (1990). Source: OECD (2015).

Consider Figure 1.1, which shows the foreign-born population of a group of countries typically labeled 'universal welfare states' (Esping-Andersen, 1990). Although some foreign-born inhabitants do not contribute to a rise in ethnic diversity, the trends in Figure 1.1 reflect steadily rising ethnic diversity in all five countries. All of them historically ethnically homogeneous and characterized by high levels of economic redistribution and provision of public services, these countries are especially likely to undergo political change if rising ethnic diversity undercuts support for an expansive welfare state.

This scenario is the motivating issue behind this dissertation. Specifically, I will address the research question:

> *RQ1: Does rising ethnic diversity undercut support for redistribution in universal welfare states?*

As RQ1 suggests, the hypothesized detrimental effect of ethnic diversity on the size and scope of universal welfare states is assumed to operate by way of a particular mechanism: diminished public support for redistribution. This is a widely shared assumption. In fact, as I will argue in the next chapter, all arguments about the impact (or lack thereof) of immigration on welfare state generosity involve public support for redistribution as a more or less explicit transmission mechanism connecting the two. In other words, though this literature is *prima*

facie concerned with the causal relationship between two macro-level phenomena, specific theories of this relationship all take a position on how individuals react to immigration. In doing so, they inevitably subscribe to some model of political psychology.

Yet in many cases, these implicit models receive too little attention. Immigration is often believed to undercut support for redistribution 'mechanically' through trivially simple mechanisms. In this dissertation, I argue that these assumed mechanisms are *too* simple. The political-psychological models underpinning the purported link between ethnic diversity and redistribution deserve more attention. Answering RQ1 requires a principled understanding of the conditions under which group identities can come to shape policy attitudes. Such an understanding can then in turn inform our expectations as to whether ethnic diversity is likely to undermine support for the welfare state. Hence, I will propose an answer to the following research question:

> *RQ2: When and how do policy attitudes in contemporary universal welfare states become linked with ethnic outgroup attitudes?*

As I will argue, answering RQ2 gets us a good bit of the way towards answering RQ1. Conversely, some of the inconsistencies in existing empirical approaches to RQ1 stem from lacking a consistent theory of RQ2. As this linkage between RQ1 and RQ2 suggests, this dissertation inscribes itself into the comparative politics literature as well as the political psychology literature.

RQ2 also highlights three themes that recur throughout the dissertation. First of all, it draws an explicit distinction between *policy attitudes*, i.e. individuals' level of support for particular matters of public policy, and *ethnic outgroup attitudes*, i.e. individuals' dispositional attitudes toward ethnic outgroups. Second, implicit in this distinction is the standard, but non-trivial assumption that of these two types of attitude, outgroup attitudes are more psychologically fundamental, and so may in some circumstances serve as a the foundation on which citizens base policy attitudes. In the interest of convenience, I will refer to this process as *ethnicization*. I define it as follows: *ethnicization is the process by which citizens come to evaluate policy issues in part based on their attitudes toward ethnic outgroups*. The dissertation focuses on *when* and *how* ethnicization occurs.

Lastly, the research question defines the empirical scope of the dissertation as *contemporary universal welfare states*. This demarcation is largely pragmatic. The main empirical cases covered in the four papers are Denmark and Sweden, both paradigmatic examples of the so-called 'universalistic' class of welfare state regimes (Esping-Andersen, 1990). While

directly relevant to the research design in one of the dissertation's papers ('Who benefits', cf. section 1.1), the label mostly serves to succinctly capture the class of Northern European, historically ethnically homogenous societies characterized by rising levels of ethnic diversity in recent decades. Though the findings may have relevance in other contexts, this class of countries is sufficiently similar to the empirical cases covered to plausibly warrant generalization.

1.1 Overview of the dissertation

Chapters 1–3 in this dissertation constitute the frame. In addition to the frame, the dissertation consists of four papers listed in Table 1.1 and presented in chapters 4–7. The papers are single-authored.

Table 1.1 Overview of papers in dissertation.

	Title	Chapter	Shorthand	Publication status
[A]	"Who benefits? Welfare chauvinism and national stereotypes"	4	'Who benefits'	Forthcoming, *European Union Politics* (Hjorth, 2015)
[B]	"Ethnicization of support for European integration"	5	'European integration'	Under review, *Political Behavior* (further revisions made since submission)
[C]	"Tone and content in the Danish immigration debate"	6	'Immigration debate'	Working paper
[D]	"Group-centric policy attitudes and the role of local contexts"	7	'Local contexts'	Under review, *Journal of Politics*

From each their own perspective, the papers shed light on the process of ethnicization in welfare state politics. Figure 1.2 illustrates one way to think about this relationship. The horizontal line in Figure 1.2 connecting group attitudes to policy attitudes is dashed, which signifies that the psychological link between the two may be stronger or weaker in any given context. Ethnicization can be thought of as the process by which this link is forged or strengthened.

Another important detail in Figure 1.2 is that no paper is dedicated to group attitudes per se, represented by the box on the left. Instead of trying to empirically explain group attitudes, this dissertation takes them as given, asking instead when and how they shape policy attitudes.

As shown, paper A, 'Who benefits', covers the outcome of interest, policy attitudes that in part reflect group attitudes — *in casu*, cross-border welfare rights in the European Union.

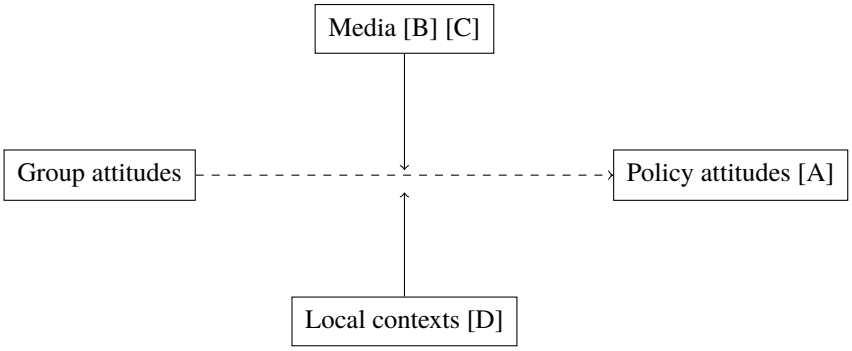

Figure 1.2 Overview of how the four papers relate to the dissertation's research question.

The remaining papers cover different sources of ethnicization of attitudes. Papers B and C focus on macro-level influences, summarized as 'Media', which encompass both political salience in whole or part strategically employed by elites (in B, 'European integration') and the media environment viewed in its entirety (in C, 'Immigration debate'). Lastly, paper D, 'Local contexts', covers a micro-level influence om ethnicized attitudes, namely individuals' experiences in their local contexts.

While Figure 1.2 summarizes in the crudest possible terms how the four papers in this dissertation speak to one another, the aim of this frame is to give a coherent and empirically substantiated account of my theoretical argument. The frame is written so as to be readable independent of the papers. Because of this, some of the evidence I present will be repeated in the papers, though most of the content in this frame is original.

I have also written this frame with an emphasis on argument, giving less priority to description and methodological questions. These are to a large extent covered in the four papers and their appendices (A.1-A.4). Instead, I focus here on outlining answers to my research question from the existing literature and how my account sets itself apart from them.

I proceed from here as follows. In chapter 2, I outline two classes of theories, coarsely labeled the *'universal outgroup aversion'* approach and the *'regimes matter'* approach, which give roughly opposite answers to RQ1. I also present some empirical patterns which challenge these approaches. Then, in chapter 3, I present the theoretical perspective from which I work, showing how this perspective explains some of the aforementioned empirical patterns. Though nominally focused on theory, I present empirical evidence throughout these chapters.

The last part of chapter 3, section 3.3, summarizes what my argument implies for RQ1 and RQ2.

Chapter 2

Existing literature

The literature with which this dissertation engages most directly is the literature on *the impact of immigration on welfare state generosity*, a sprawling literature mostly within the realm of comparative politics, though as argued above, existing approaches typically assign voters a key mediating role, thus more or less explicitly subscribing to some model of political psychology. In chapter 3, I will argue how applying insights from political psychology can help resolve some of the empirical puzzles present in the existing literature.

In this chapter, I situate my research within the broader literature on how ethnic diversity affects public support for the welfare state. I do so by classifying existing studies into two broad categories, which I label respectively the *'universal outgroup aversion'* approach and the *'regimes matter'* approach.

I describe each approach in turn below, but the distinction can be briefly summarized as follows: studies within the 'universal outgroup aversion' approach assume that irrespective of country-level context, individual-level racial/ethnic prejudice reduces preferences for redistribution in the face of rising societal diversity. In contrast, studies within the 'regimes matter' approach argue that although this dynamic may exist for countries in general, specific country-level features of universal welfare states mitigate this effect, rendering them much less susceptible. In other words, the two approaches diverge in the power they ascribe to specific country-level features to dampen the detrimental effects of ethnic diversity on redistribution.

As a way of illustrating the fundamental source of disagreement between the two approaches, consider Figure 2.1. The plot reproduces Alesina and Glaeser's (2006) correlation of country-level ethnic fractionalization and economic redistribution. Three (groups of) countries are highlighted: countries typically labeled as 'universal welfare states' (Denmark,

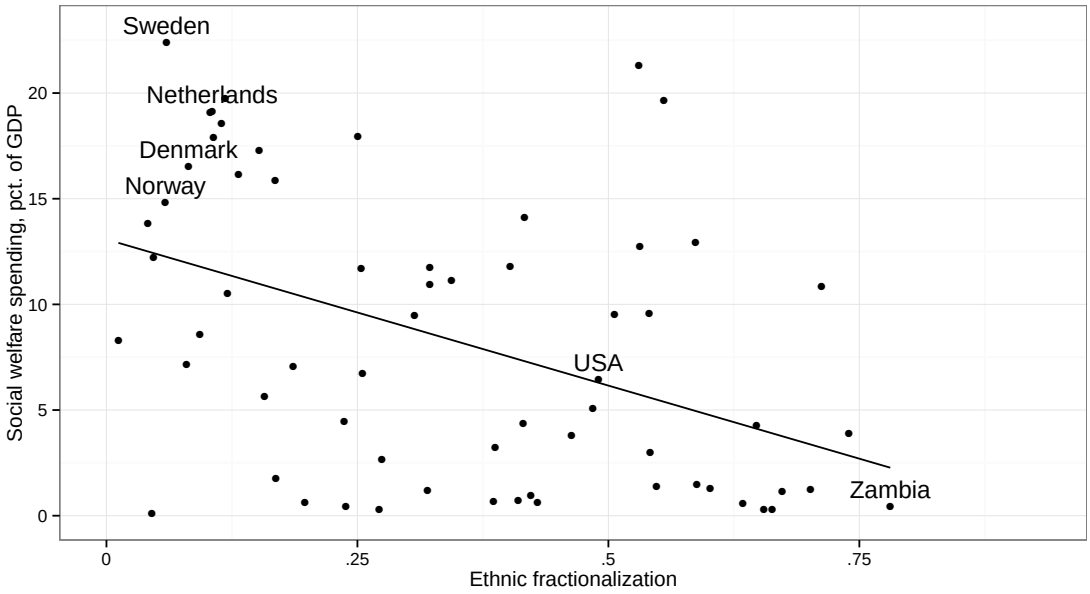

Figure 2.1 Country-level ethnic fractionalization and social welfare spending, reproduced using data from Alesina and Glaeser (2006) and Persson and Tabellini (2004). The plot highlights historically homogenous, universal welfare states (the Scandinavian countries and the Netherlands), a widely studied case (USA), and, at the opposite end of the scale, a highly fractionalized, low-redistribution country (Zambia). The two outliers in the top right are Belgium and Luxembourg. The variables are correlated at $r = .44, p < .01$. The line represents an OLS best fit. The result is robust to excluding outliers and to using a measure of racial rather than ethnic fractionalization.

Norway, the Netherlands, Sweden), a highly influential single case (the US), and the country with the highest measured level of ethnic fractionalization and accordingly low level of redistribution (Zambia).

As shown, the two are negatively correlated: as ethnic fractionalization increases, the share of national income countries dedicate to social spending goes down. This association is quite strong, and Alesina and Glaeser put a lot of stock into this fact. In fact, in a volume dedicated to explaining the gap in welfare spending between the US and Europe, they estimate that around half this gap can be explained by Europe's comparatively high level of racial/ethnic homogeneity alone.

One way to illustrate the distinction between the 'universal outgroup aversion' and 'regimes matter' approaches is in terms of how they predict the future trajectory of the group of universal welfare states in the upper left corner of Figure 2.1. According to the 'universal outgroup aversion' approach, as universal welfare states experience rising levels of ethnic diversity, voter aversion to redistributing wealth to outgroups should cause those countries to 'slide down the scale' of the regression line, ending up with lower levels of redistribution. In contrast, the 'regimes matter' approach, which emphasizes unique country-level features of

universal welfare states, predicts that ethnic diversity will have at most a negligible impact on the social spending levels of these countries. As a result, universal welfare states will merely drift horizontally to the right.

This summary captures how the predictions the two approaches diverge, but also glosses over some important nuances within each of them. I now turn to discussing specific examples of each type of approach in turn. After describing them on their own terms in sections 2.1 and 2.2, I will in section 2.3 present some empirical patterns which challenge the predictions of each type of approach.

2.1 The ‘universal outgroup aversion’ approach

Though they have key theoretical tenets in common, specific studies within the ‘universal outgroup aversion’ approach come in various stripes and from numerous social science subdisciplines. Here, I present two distinct traditions.

One can be found in the empirical literature on ethnic diversity and redistribution, which cuts across subfields. In addition to the association shown in Figure 2.1 above, studies have shown ethnic diversity to be negatively associated with a host of desirable social outcomes, including provision of public services (Alesina et al., 1997; Banerjee et al., 2005), compliance with the rule of law (Li, 2010; Vigdor, 2004), and social trust (Dinesen and Sonderskov, 2015; Putnam, 2007). In fact, the empirical association between ethnic diversity and generally undesirable social outcomes is so strong that Banerjee et al. (2005) label it “one of the most powerful hypotheses in political economy”.

The strength of this relationship has led some researchers to cautiously suggest possible underpinning mechanisms. The earliest theories simply posit that individuals have a “taste for discrimination” against outgroups (Becker, 1957), leading individuals to require an economic premium to offset the intrinsic disutility they derive from cooperating with outgroup members. In a similar argument in a relatively recent study, Luttmer (2001) proposes that individuals exhibit “racial group loyalty”, i.e. a pure, intrinsic preference for higher welfare spending on members of one’s own racial group.

Other studies in this vein propose that ethnic groups disagree over the preferred composition of public spending, leaving them unable to settle on a specific bundle of public goods (Alesina et al., 1997). Still others argue that even with converging preferences, individuals may be unable to impose social sanctions on outgroup members, rendering cooperation unsustainable (e.g., Miguel and Gugerty, 2005). These latter two arguments are what Habyarimana et al. (2009) respectively label the ‘preferences’ and ‘technology’ accounts. As a

third alternative, these authors propose an alternative 'strategy selection' account whereby individuals engage in high-reciprocity relationships (implying both more sharing of resources and tougher sanctioning) only with members of the ethnic ingroup.

Symptomatic of this tradition, Habyarimana et al. (2009) focus on empirical strategies for distinguishing between these three mechanisms, finding the strongest support for strategy selection (but see Enos and Gidron, 2014). They make little effort to explain why strategy selection occurs, except to note that it has emerged "perhaps through some evolutionary process" (29). In this dissertation, I subscribe explicitly to this idea, and flesh it out in further detail in section 3.1.1 below.

A second tradition within the 'universal outgroup aversion' approach, largely rooted in political science, consists of cross-country studies of public opinion directly testing the effect of outgroup affiliation on support for redistribution. For example, Soroka et al. (2013) conduct identical survey experiments on citizens in the US, Canada, and the UK, testing the effect of dark skin complexion on preferred benefit levels for individual recipients. This class of approaches thus applies cross-country tests to the well-established finding from studies in the US that cueing a black recipient tends to diminish white Americans' support for welfare benefits (DeSante, 2013; Gilens, 1996; Winter, 2006a). While the authors do allow for country level-features such as welfare regimes to potentially moderate the effect, the baseline assumption remains that outgroup affiliation universally depresses support for redistribution, though they are agnostic as to whether this reflects symbolic outgroup aversion (Kinder and Sears, 1981) or economic conflict (Sears et al., 2000).

In a similar design, Iyengar et al. (2013) run an identical survey experiment in seven countries, testing the effect of Muslim country origin on admitting an individual immigrant. Here, too, the design reflects the assumption that outgroup aversion manifests itself roughly equally across countries.

In sum, 'universal outgroup aversion' approaches, while originating in different disciplinary traditions, are unified by their focus on individual-level aversion to outgroup members. Though the basic tenets are empirically well-supported, I will in section 2.3 present some evidence which challenges their predictions. Here, I turn to their major alternative.

2.2 The 'regimes matter' approach

The clearest articulation of a 'regimes matter' approach to the effect of ethnic diversity on welfare state support comes from Crepaz and Damron (2009). Here, the authors connect

the welfare regime literature to the historical capacity of welfare states to ameliorate class divisions. This capacity, they argue, can also be brought to bear on ethnic divisions in present-day welfare states. The canonical historical example of the use of state power to bridge ethnic and class divisions by institutional means is Otto von Bismarck’s introduction of social insurance schemes in the newly united Germany in the early 1880’s. Bismarck did so with the express purpose of fostering loyalty among the new German populace to the central state rather than to class or subnational ethnicity (Hennock, 2007). Crepaz and Damron argue that present-day welfare states can similarly foster an overarching civic identity, thereby ameliorating divisions between natives and immigrants.

Yet Crepaz and Damron also argue that this capacity is conditioned by welfare regime. Specifically, the non-targeted social programs typical of universal welfare states foster a unifying “moral logic of inclusion”; in contrast, the means-tested programs typical of corporatist and residual regimes foster a “moral logic of exclusion” which singles out and stigmatizes benefit recipients. Paraphrasing a similar argument made by Rothstein and Stolle (2003), the authors sum up the case for universal systems:

> “In such systems almost everybody contributes and almost everybody receives, making it more difficult to stigmatize receivers of government support as ‘other’. In universal systems, debate centers on ‘general fairness’, not on highlighting differences. Such institutional arrangements are ‘undivisive, encompassing, and inclusionary in character’.” (449)

The argument that this inclusionary character of policy design promotes more positive attitudes toward recipients of welfare benefits is a staple of the welfare regime literature (e.g., Kumlin, 2004; Larsen, 2007; Rothstein, 1998), and indeed is often referred to simply as the ‘regime argument’. Building on this idea, Crepaz and Damron make the slightly stronger argument that universal regimes also engender more inclusionary attitudes *in general*, including tolerance of ethnic outgroups. As support of the argument, the authors show that citizens in universal welfare systems are on average less willing to endorse the statement that ‘immigrants take jobs away’, a typical measure of welfare chauvinism (on welfare chauvinism, see also ‘Who benefits’).

Crepaz and Damron root this line of thinking in the theory of social categorization (Tajfel and Turner, 1979; Tajfel and Wilkes, 1963), arguing that means-tested programs promote the categorization of ingroups and outgroups whereas universal programs inhibit it. Interestingly, both the ‘universal outgroup aversion’ and ‘regimes matter’ approaches thus consider themselves outgrowths of some version social identity theory, although they arrive

at nearly exactly opposite conclusions. Whereas the former sees social categorization as a spontaneous process triggered by exposure to outgroup members, the latter views social categorization as a generalized disposition which can be either promoted or inhibited by citizens' interactions with the state and the ways in which state policy defines categories of citizenship.

In its fairly unabashedly normative formulation, the regime argument resembles Lijphart's (1998) conclusion that consensus systems are "kinder, gentler democracies". Conversely, the regime argument's enthusiasm on behalf of universal systems is in some ways mirrored by the far less sanguine literature on 'policy feedback' in the US. Here, policy feedback scholars argue that the 'residual' US welfare state disguises government service provision (Mettler, 2011) and demobilizes recipients (Soss and Schram, 2007).

Other studies within the 'regimes matter'approach echo this contrasting of antagonistic, divisive experience in the US system with the kinder, gentler politics of universal systems, but focus on *media* instead of policy design. For example, Gilens (2000) argues that media portrayals of welfare recipients as predominantly black (which far overstates the actual prevalence of blacks among US welfare recipients) is the key cause of 'racialized' policy attitudes among white Americans.

In a replication of Gilens' study in Danish and Swedish newspapers, Larsen and Dejgaard (2013) show that media portrayals of the poor are predominantly white in Danish and Swedish newspapers. In fact, the share of poor people portrayed as non-white in these countries closely tracks non-whites' actual prevalence among welfare recipients. The authors interpret this as evidence in support of the regime argument, claiming that more positive media portrayals of minorities is one of the mediators through which welfare regimes affect attitudes.

In sum, whether assigning policy design or media coverage the primary causal role, studies within the 'regimes matter' approach are unified in their contention that stable country-level features moderate the effect of ethnic diversity on support for redistribution. By implication, though ethnic diversity may depress support for redistribution elsewhere, the softer, more inclusionary politics of universal systems should cushion against this effect.

2.3 Empirical challenges

As the above presentation has indicated, the 'universal outgroup aversion' and 'regimes matter' approaches each explain important stylized facts about ethnic diversity and redistribution. In keeping with the 'universal outgroup aversion' approach, individuals are indeed typically

slightly less supportive of redistribution to outgroup members. And consistent with the 'regimes matter' approach, attitudes toward welfare recipients and ethnic minorities' burden on welfare systems are on average less negative in universal welfare systems.

However, these stylized facts coexist with empirical patterns which each of these approaches struggle to explain. I present these patterns in this section, describing in chapter 3 a framework which helps make sense of these seeming inconsistencies.

2.3.1 Challenges to the 'universal outgroup aversion' approach

Country-level changes in ethnic diversity are not associated with lower welfare spending

Perhaps the key empirical challenge to the 'universal outgroup aversion'approach is that in recent decades, countries' actual levels of welfare spending have not followed the trajectory indicated by the Figure 2.1. Rather than 'slide down the scale', countries have in fact increased social spending during the very same time span in which the bulk of historically recent immigration took place. While these parallel trends neither prove an effect or the absence thereof, they do suggest that rising levels of ethnic diversity is fully compatible with high and indeed rising levels of social spending.

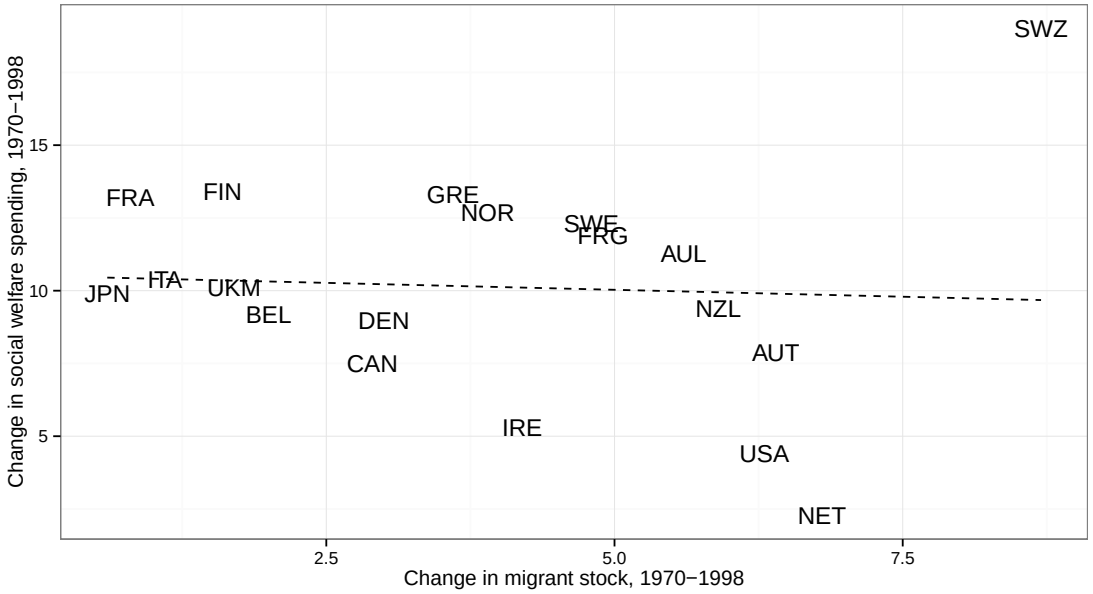

Figure 2.2 Country-level changes in migrant stock and changes in welfare spending, 1970-1998. Data from Soroka et al. (2006), who rely on UN estimates (for migrant stock) and the OECD Social Expenditures (SOCX) database (for welfare spending). The dashed line represents an OLS best fit (which is statistically insignificant). Soroka et al. (2006) fit the line by excluding Switzerland (upper right corner).

The same pattern holds if instead of absolute levels, we compare *changes* at the country-level. This is the empirical strategy of Soroka et al. (2006), who examine whether larger changes in migrant stock are associated with smaller increases in welfare spending. Consider Figure 2.2, which plots Soroka et al.'s data on this relationship.

There is no overall relationship between the two variables, i.e. immigration is not consistently associated with smaller increases in welfare spending. Soroka et al. in fact claim that there *is* a negative relationship, which reflects that they choose to omit the most consequential outlier, Switzerland, from their analysis due to technical changes over time in how OECD calculates Swiss social expenditures. However, even allowing the omission of this particular outlier, the relationship remains sensitive to the extreme cases the US and the Netherlands, the exclusion of either of which would once again render the relationship insignificant.

To be sure, the observation of the absence of a cross-country relationship does not prove the absence of a causal relationship. As the authors themselves note, immigration may be driven by policy choices endogenous to the size of the welfare state, or both may be associated with unobserved country-level heterogeneity. But it does not make sense to consider Alesina and Glaeser's plot in Figure 2.1 evidence of a causal relationship all the while dismissing Figure 2.2 as uninformative. Whatever evidentiary status one assigns to cross-sectional analyses of country-level data should apply equally to the two. And judging by Soroka et al.'s data, the relationship is not as mechanical as the 'universal outgroup aversion' approach would suggest. It is not possible to arrive at a significant, negative relationship without engaging in a good bit of post hoc case selection and elaborate curve-fitting. In sum, the cross-country relationship between ethnic diversity and welfare spending is effectively zero, and at most only very slightly negative.

One might reasonably object that Figure 2.2 that the observed dependent variable is a measure of policy rather than attitudes. This distinction matters because, owing to institutional inertia, policy may be more slow-moving than attitudes. Welfare spending in particular may produce entrenched, concentrated interests which impede the rollback of costly welfare programs (Pierson, 1994). However, the weak to non-existent relationship in Figure 2.2 is mirrored when looking directly at attitudes: across a similar group of countries, the link between immigration and individual support for welfare spending is just as weak (Brady and Finnigan, 2013). Nor is the weakness of the relationship likely a measurement artefact of using country-level data. Looking at plausibly exogenous variation in ethnic diversity across municipalities, Dahlberg et al. (2012) claim to find an effect on support for redistribution, but the finding is disputed (Nekby and Pettersson-Lidbom, 2015).

Welfare recipients are not ex ante perceived as ethnically different

At the individual level, an important empirical challenge to the 'universal outgroup aversion' approach is that individuals do not appear to mentally link immigration to welfare spending. In other words, although immigrants in universal welfare states are in fact more likely than native citizens to receive welfare benefits, the typical citizens' intuitive mental representation of a recipient appears to be that of a fellow native citizen. For an illustration, consider Figure 2.3, which relies on data from the so-called 'ceiling experiment' in Sniderman et al. (2014).

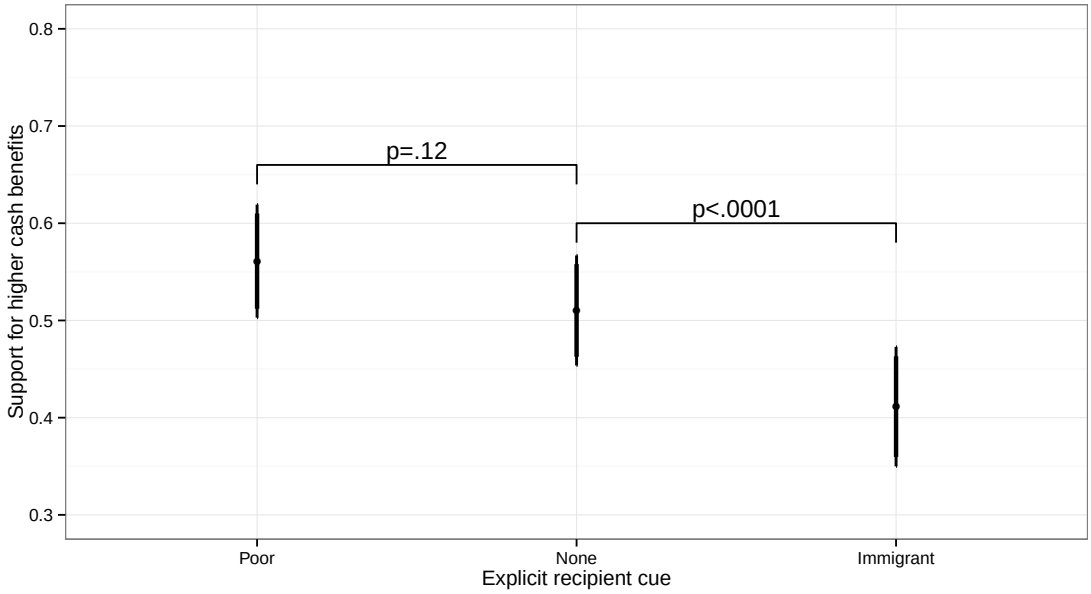

Figure 2.3 Effects of explicit recipient cues (immigrants and the poor) on support for higher cash benefits. Results from a regression model with explicit recipient cues and standard demographic variables (gender, age, education). Vertical lines represent confidence intervals at 90 pct. (thick lines) and 95 pct. (thin lines). Analyzed using the replication data for Sniderman et al. (2014).

In the ceiling experiment, conducted on a sample of voting-age Danish citizens in the spring of 2006, Sniderman et al. asked respondents whether they approved or disapproved of a ceiling on how much an individual may receive in welfare benefits, which was in place at the time. In one experimental condition, respondents were provided no additional information about the ceiling. Two other conditions added explicit cues about the types of people affected by the ceiling: one condition emphasized its effect on the poorest members of society, another that the ceiling affects immigrants. (Other conditions add party cues; in order to focus only on the effects of recipient cues, I ignore those conditions here).

As shown in Figure 2.3, the two experimental conditions had very different effects. There was no discernible effect of the cue reminding respondents that the ceiling particularly affects poor people. Support for the ceiling does not change substantively or significantly between the two conditions. In contrast, when exposed to a cue that the ceiling affects immigrants, support drops substantively and significantly, from scrapping the ceiling being favored by a small majority (51 pct.) to a distinct minority (41 pct.).

As Sniderman et al. note, these results suggest that the 'Poor' condition comes closer to citizens' intuitive mental representations of welfare recipients than does the 'Immigrant' condition. As the authors note:

> "Opposition to a cap on welfare benefits is grounded in a commitment to the values of the welfare state (...) so deeply internalized that it is not necessary to call them to the conscious attention of Danes in order to evoke a supportive response toward those on welfare." (80)

The result challenges the 'universal outgroup aversion'approach because it suggests that citizens in universal welfare states have far from internalized the premise that many co-citizens on welfare benefits are members of ethnic outgroups. And there is no theoretical account of the effect of ethnic diversity on welfare spending that does not run through individual citizens' perceptions of to whom welfare spending accrues.

This disconnect is exactly what motivates the argument in the introduction that answering RQ1 (about the effect of ethnic diversity on welfare spending) requires attention to RQ2 (about how citizens learn to think about policies in terms of particular groups). Inattention to the political psychology underpinning public opinion can lead scholars to overlook empirically important preconditions for opinion shifts.

Welfare states can (and do) implement dualized welfare rights systems

Lastly, studies taking the 'universal outgroup aversion'approach have tended to overlook an important difference between the paradigmatic case of the United States and immigrant-receiving countries: the legal status of the major racial/ethnic outgroup. Because immigrants in the latter group of countries are typically not citizens (and immigrants' access to citizenship is itself politically determined), governments can fully legally discriminate between natives and immigrants in terms of access to welfare services. In other words, they can implement policies that define very low levels of welfare benefits for immigrants all the while maintaining an overall high level of generosity.

This 'dualization' of welfare systems, whereby rights are retrenched for minorities but benefit levels for natives are kept intact, is indeed a clear trend in contemporary European welfare states (Emmenegger, 2012; Rueda, 2014).

Moreover, dualization is by all accounts politically popular. Consider Figure 2.4, drawn from the appendix for 'Who benefits'. Here, I asked survey respondents whether they support cross-border welfare rights for other EU citizens, but also whether they support cuts to welfare benefits in general. Figure 2.4 plots the two response distributions against each other, with circle sizes proportional to the number of respondents in each cell.

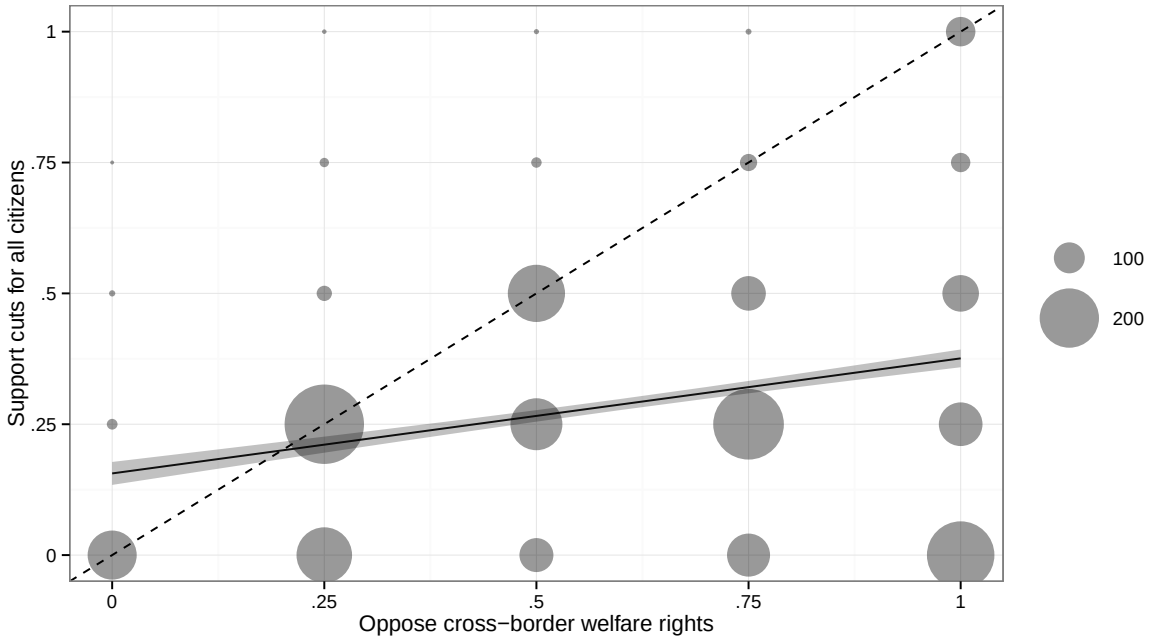

Figure 2.4 Opposition to cross-border welfare rights plotted against support for cutting welfare benefits generally. Circle sizes are proportional to the number of respondents in each cell. Grey line shows regression line with 95 pct. confidence interval ($\beta = .22, p < .001$). 31 pct. of respondents are opposed to welfare benefits for other EU nationals, but also opposed to cuts to benefits for Swedish citizens. In comparison just 9 pct. support welfare cuts for both natives and other EU nationals.

As shown, respondents are weakly opposed to cross-border welfare rights but strongly supportive of existing benefit levels for Swedish citizens. 31 pct. of respondents take the welfare chauvinist position of being opposed to welfare benefits for other EU nationals, but also opposed to cuts to benefits for Swedish citizens (represented by the circles in the bottom right of the figure). In comparison, the 'principled conservative' position in favor of welfare cuts for both natives and other EU nationals is endorsed by just 9 pct. of respondents (represented by the circles in the top right).

In other words, when welfare chauvinist policies are a meaningful policy option, citizens can be strongly opposed to benefits for outgroups while maintaining support for welfare for the (native) majority. While not at odds with the basic theoretical premise of universal outgroup aversion, this suggests the effects of ethnic diversity on overall welfare state generosity may still be limited.

2.3.2 Challenges to the 'regimes matter' approach

Counterintuitive results in cross-country surveys

Drawing on the 'regimes matter' idea that effects of ethnicity on support for welfare benefits may differ systematically between countries, some scholars have in fact carried out identical studies in several countries with the hope of demonstrating theoretically meaningful between-country differences. However, the results from these studies have often turned out to work precisely against predictions.

For example, Soroka et al. (2013) conduct identical survey experiments in the US, the UK, and Canada, randomizing the skin color of a recipient of welfare benefits and asking respondents to state how high a benefit they think the recipient should get. True to expectations, the preferred benefit level is indeed lower when the recipient has darker skin color. Yet this effect is absent among US respondents, and only significant in the two other cases. This cross-country pattern is precisely the opposite of what the 'regimes matter' approach would predict.

Similarly, Larsen (2011) reports results from a survey that asks respondents in the UK, Denmark, and Sweden about perceptions of ethnic minorities. Larsen asks respondents in the three countries questions identical to those asked of US respondents about blacks in the General Social Survey, only with immigrants substituted for blacks. The approach thus exemplifies the use of the US experience as a "master narrative" for understanding European native-immigrant relations. However, respondents in the three European countries were *more* likely to express aversion to living close to the outgroup, and as likely to characterize outgroup members as 'lazy'. The latter comparison is especially telling, since stereotypes about black laziness have typically been portrayed as having particular resonance in American race relations (Gilens, 2000; Winter, 2008). Here, too, survey responses do not conform to the pattern predicted by the 'regimes matter'approach.

Individuals in universal welfare states readily subscribe to other anti-immigrant attitudes

Another challenge to the 'regimes matter'approach is that while citizens in universal welfare states are indeed more muted in their expression of some forms of 'welfare chauvinism' – as was the main finding in Crepaz and Damron (2009) – they readily endorse other anti-immigrant attitudes. This runs against Crepaz and Damron's argument that the inclusionary nature of universal welfare state institutions 'constructs tolerance' by sustaining encompassing civic identities.

For an illustration, consider Figure 2.5. The figure shows individuals' endorsement of stereotypes about immigrants averaged by country. Data is drawn from the most recent round of the European Social Survey (round 7, 2014), which has a special module on immigration attitudes.

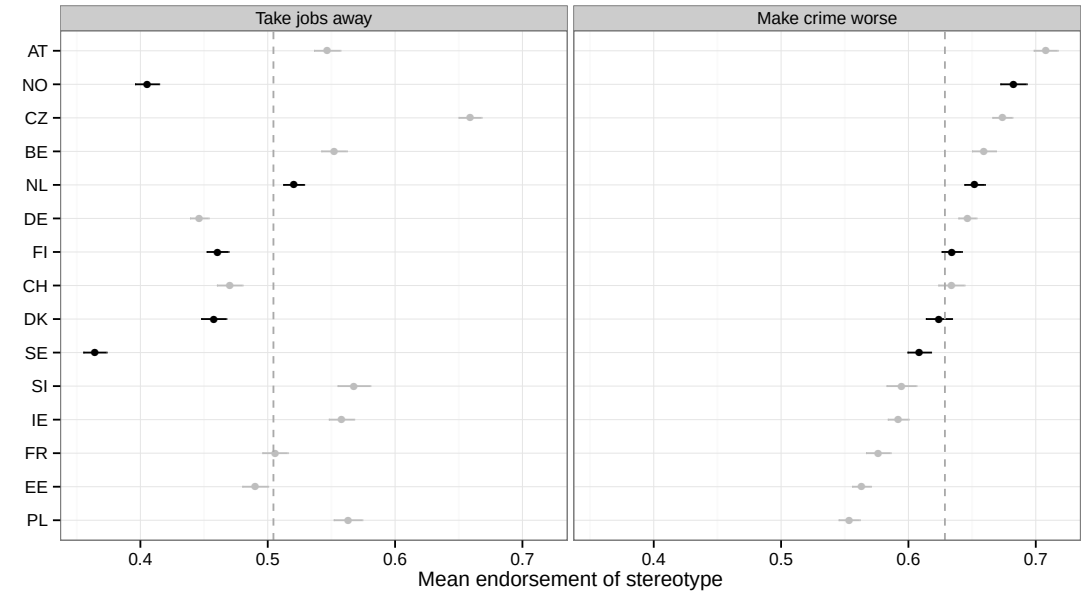

Figure 2.5 Average endorsement of immigrant stereotypes by country. Points reflect country-level average score on an 11-point scale from 0 to 1 where 1 is full agreement. The two statements are whether immigrants "take jobs away" and whether they "make crime problems worse". The universal welfare states shown in Figure 1.1 are highlighted. Horizontal lines represent 95 pct. confidence intervals. Dashed vertical lines show the overall average level. Source: European Social Survey, round 7 (2014).

The left panel of Figure 2.5 shows average agreement with the statement that immigrants 'take jobs away', the measure of welfare chauvinism used by Crepaz and Damron. Although the authors relied on data from previous years and another survey (the ISSP), their result can be roughly replicated using the ESS data: citizens in universal welfare states are indeed on average less likely to endorse this sentiment, with all countries except one below average, and the bottom two positions occupied by Sweden and Norway respectively.

However, the pattern is different when looking at the right panel, which shows average endorsement of the belief that immigrants 'make crime problems worse'. Here, there is no indication that citizens in universal welfare states hold kinder, gentler attitudes toward immigrants. Three of the five countries are above the cross-country average, and the remaining two are just below the average.

While this data challenges Crepaz and Damron (2009)'s claim that universal systems promote inclusive attitudes in general, they do not undermine the standard regime argument, namely that support for redistribution is buttressed by universal systems of welfare provision. But this in turn suggests that some issues may lend themselves more easily to ethnicization than others. For more on the particular ways in which the issue of crime is ethnicized, see 'Local contexts'.

2.4 Summary

In sum, the 'universal outgroup aversion'and 'regimes matter' approaches can both point to substantial bodies of evidence in their favor. In support of the 'universal outgroup aversion' approach, individuals are indeed on average more reluctant to share resources with members of ethnic outgroups. In support of the 'regimes matter' approach, this does not seem to translate mechanically into lower public support for redistribution as ethnic diversity rises.

However, I have also presented empirical evidence that seems to cut against each. Specifically, contra the former, individuals in welfare states seem reluctant to mentally link immigration and welfare issues. Contra the latter approaches, they seem entirely willing to link immigration with other issues.

I will argue in the following that this set of findings, contradictory as it may seem, in fact paints a reasonably coherent picture. The reason the existing literature has struggled to put this picture together is that it has been too narrowly inspired by the US literature on race and welfare. Making theoretical sense of the evidence requires conceding slightly more to the historical and political particularities that set universal welfare states apart from the US experience than previous approaches have tended to be willing to do. A theoretical framework can make such concessions without throwing the proverbial baby out with the bathwater by abandoning the ambition of a general model of political cognition.

In other words, compared to existing approaches, a framework for understanding how ethnic diversity affects politics in universal welfare states needs to be at once *more general*

(in the sense of relying on a theoretically unifying basic model) *and more specific* (in the sense of being theoretically sensitive to the ways in which native-immigrant relations in universal welfare states differ from US race relations). Chapter 3 is dedicated to drawing up the contours of such a framework.

Chapter 3

Theory

In this chapter, I introduce an overall framework for thinking about the question posed in RQ2, i.e. when and how policy attitudes come to be linked with ethnic outgroup attitudes. My ambition is to make clear how this framework helps resolve the empirical challenges presented in the preceding chapter. In doing so, the framework should also clarify how answering RQ2 helps address RQ1.

Section 3.1 presents the framework's theoretical foundation, based on an evolutionary psychology perspective on group cognition. Then, section 3.2 addresses the two aspects of group-policy attitude linkages highlighted in RQ2: the 'when' (i.e., what types of issues can be linked) and the 'how' (i.e., the potential sources of such linkages). In section 3.2 I also present key results from papers A-D which illustrate how the dissertation helps answer RQ2.

In the last part of this chapter, section 3.3, I summarize how this framework speaks to RQ1.

3.1 Understanding the power of group affiliation

A useful starting point for thinking about group-policy linkages is to ask why political attitudes ever reflect group loyalties to begin with. After all, public policy in liberal democracies tends by design to be blind to citizen group affiliations. In spite of this notional indifference to groups, group loyalties pervade public opinion. Beginning with Converse (1964), public opinion scholars have noted that attitudes relating to salient social groups are consistently more stable and more intense.

Later studies have shown that support for a policy is highly contingent on identification with its main beneficiaries (Nelson and Kinder, 1996a; Schneider and Ingram, 1993, see

also 'Who benefits'). Recently, studies on partisan motivated reasoning have demonstrated individuals' sometimes remarkably intense attitudinal partisan loyalties (Bisgaard, 2015; Iyengar and Westwood, 2014; Slothuus and de Vreese, 2010). Linkages between issues and group identities can even cause highly scientifically literate individuals to reject scientific facts in the interest of group identity-protection (Kahan, 2015).

Yet while these studies persuasively demonstrate the cognitive potency of group loyalties, they rarely attempt to explain it. Why are individuals so strongly motivated to act and believe in group-conformant ways?

3.1.1 Coalitional psychology and the role of stereotyping

Here, I will outline a framework which forms a basis for understanding the power of group-centric attitudes. I draw on the concept of *coalitional psychology* (Pietraszewski, 2013; Tooby and Cosmides, 2010). Growing out of evolutionary psychology, the bedrock of the theory of coalitional psychology is a set of assumptions about the nature of human sociality in the ancestral environment, with particular attention to the ways in which it is conditioned by intergroup conflict. Evidence of conflict in the form of warfare is pervasive across the human and proto-human historical record: evidence of intergroup conflict among chimpanzees suggests our ancestors have been practicing war for at least six million years (Wrangham and Peterson, 1996). Reviewing archeological and ethnographic evidence of ancestral warfare among humans, Bowles (2009) estimates that in the Late Pleistocene, i.e. the roughly 115,000 years preceding the Neolithic revolution, around one seventh of total adult mortality can be attributed to warfare. As Sidanius and Pratto (2001) note, all human societies are characterized by group-based dominance hierarchies. These hierarchies tend to be based on age, gender, and arbitrary markers of group affiliation.

All in all, this evidence suggests that intergroup conflict has exerted a significant selection pressure on the human species. Through natural selection, this selection pressure has in turn given rise to a set of psychological systems specifically designed to regulate within-coalition cooperation and between-coalition conflict. It is this set of psychological systems that is collectively referred to as coalitional psychology (Tooby and Cosmides, 2010). The framework I outline here focuses on a particular aspect of coalitional psychology, namely the cognitive systems dedicated to categorizing others as rival outgroups (i.e., social categorization) and constructing category-based representations of outgroup members (i.e., stereotyping).

Though race is the group identifier is at the heart of the vast majority of studies of group-centric attitudes, there is no reason to think humans should be particularly strongly

predisposed to categorizing others by race. Empirical studies of the archaeological record and contemporary hunter-gatherers suggest that our hunter-gatherer ancestors rarely moved residences farther than around 40 miles, which is far too short a distance to be exposed to racially different human populations (Kelly, 1995). As a consequence, the human mind should not be equipped with systems dedicated to categorizing others by race (Kurzban et al., 2001).

Where race is nevertheless a highly salient coalitional marker, as is the case in settings such as contemporary US politics, it is because citizens are predisposed to automatically encode race as a marker for tribal affiliation, much like our hunter-gatherer ancestors would encode foreign clothing, dialects, or manners as marks of a rival tribe. Race is thus mistakenly taken as an indicator of coalitional affiliation because the mind's coalitional psychology executes the ancestrally relevant task of categorizing others as members of one's own or rival tribes. By implication, in settings where much clearer cues about coalitional affiliation are available, race should lose its power as informational proxy.

Studies testing this specific idea seem to support it. People cease to categorize others by race when exposed to cross-cutting coalitional cues such as an arbitrary group marker (Pietraszewski et al., 2014) or party affiliation (Pietraszewski et al., 2015). This is not the case for age or sex, properties for which the mind should have evolved dedicated dimensions of social categorization. Neither is it the case for accent, a cue of cultural foreignness to which our hunter-gatherers would likely have been exposed (Pietraszewski, 2013).

The lesson usually drawn from this argument is that "race can be erased", i.e. that social categorization by race could potentially be overridden by other, cross-cutting group loyalties. In societies marred by racial divisions, this is naturally a highly desirable prospect. Yet the reverse must also hold, namely that visual characteristics that correlate with group membership, racial or otherwise, can come to function as a politically salient and divisive coalitional marker. Specifically, in contemporary, originally ethnically homogeneous societies, ethnically foreign immigrants, who are in many cases visually distinct, should become encoded as coalitionally foreign in the minds of members of the original majority. As individuals experience rising ethnic diversity, through media as well as daily life, these systems should increasingly pick up cues about coalitional divisions and process information about others in terms of their coalitional alignment.

This process of social categorization of outgroups in turns gives rise to stereotypes, i.e. categorical representations of individuals based on group membership (Macrae and Bodenhausen, 2000). In this social-cognitive perspective, stereotypes arise out of a need to efficiently predict the behavior of others based on available information. In other words,

stereotyping is an evolved mechanism for storing category-level information (Petersen, 2009). This perspective on stereotypes stands in contrast to older, motivationally oriented perspectives which construe stereotypes as arising from a need to maintain positive self-esteem (e.g., Tajfel and Turner, 1979). The social-cognitive perspective makes sense of key facts about stereotypes that are not consistent with the motivational perspective, such as the (imperfect) tendency of stereotypes to be accurate (Lee et al., 1995), and the existence of positive stereotypes (Schneider, 1996).

3.1.2 Theoretical implications

From this framework, two important implications for how to think about group-centric attitudes follow. For one, linking group-centric attitudes with coalitional psychology moves us beyond 'content-blind' models of political cognition that construe attitude formation simply as a process of averaging across the most immediately salient considerations, whatever they may be (e.g., Zaller, 1992). Group affiliation is not merely one consideration among others: prompting an individual to think in terms of group identity activates systems dedicated for thinking in terms of social categories (Cosmides et al., 2003). In other words, individuals are functionally predisposed to be particularly attuned to (amongst others) cues that signal the relevance of coalitional alignment. This distinction helps explain the finding, going back to Converse (1964), that considerations related to group affiliation appear significantly more motivationally potent than others. In contrast, content-blind models such as Zaller's RAS model cannot explain why the mind should privilege some types of considerations based on their substantive meaning.

The other important implication comes from coalitional psychology's embeddedness in the greater framework of evolutionary psychology. Coalitional alignment is an important human motive, but nevertheless merely one among others. The evolutionary adaptedness of reliance on group affiliation rests precisely on its activation only when group affiliation is perceived to be relevant to the situation at hand. At any given time, environmental cues may guide individuals' attention to motives other than group affiliation. Because of this domain-specificity, it is pointless to ask whether political attitudes are 'fundamentally' group-based. The answer is that it depends: if citizens are exposed to cues that activate coalitional systems when considering that policy, attitudes are very likely to be group-centric. If no cues signal the relevance of coalitional affiliation, other types of social motives are likely to dominate. Consequentially, explaining group-centric attitudes is a question of identifying the cues that cause citizens to evaluate an issue in terms of coalitional affiliation.

As highlighted in the introduction, I focus in this dissertation on a particular instance of group-centric attitudes: namely policy attitudes based on attitudes toward members of ethnic outgroups. I call the process by which these attitudes are linked *ethnicization*. But as I have argued here, this particular instantiation nonetheless represents a humanly universal psychological mechanism.

In the next section, I flesh out what this overall theoretical framework implies for when we should expect ethnicization to occur. As I will argue, the universal basis of ethnicization does not imply that we can simply abstract away the political context. But before doing so, I discuss two recent additions to the debate over how immigration affects support for the welfare state. Both of these contrast with my approach in important ways.

3.1.3 Ethnicization contra 'immigrationalization'

In a recent study, Garand et al. (2015) argue that American public opinion on welfare is 'immigrationalized', i.e. Americans' feelings toward welfare recipients and support for welfare spending reflect their attitudes toward immigrants. As in my use of the word ethnicization, the label 'immigrationalization' reflects the idea that the phenomenon is in important ways comparable to the racialization of welfare attitudes. In support of their argument, the authors show that among Americans, immigration attitudes predict welfare attitudes, even when controlling for demographics, political ideology, and other group attitudes.

The empirical approach taken by Garand et al. comes with an important threat to inference. Simply put, the observed correlation between immigration attitudes and welfare attitudes might merely reflect ideological constraint among voters. In standard models of public opinion, citizens exhibit (some) ideological constraint across issue attitudes (Ansolabehere et al., 2008), in whole or part because they take cues from elites about what ideologically conformant issue positions are (Lupia and McCubbins, 1998; Zaller, 1992). As a consequence, individual citizens' issue attitudes will tend to correlate, but not because of any causal relationship between any pair of issue attitudes.

Garand et al. do try to account for this potential confounder by including controls for ideology and partisanship. However, if the inclusion of these controls does not fully parse out the confounding role of ideology – say, because of measurement error attenuating the coefficient on ideology – the analysis risks showing a direct link between immigration attitudes and welfare attitudes where there are none. Because of this problem, I take a different approach in this dissertation. Instead of focusing on the existence of a (partial) correlation between immigration attitudes and policy attitudes per se, I focus on whether the magnitude of this

correlation itself varies in ways consistent with ethnicization. For example, I show that the correlation is stronger in times and places where immigration is politically salient ('European integration') and in localities with higher levels of ethnic diversity ('Local contexts'). Because these patterns cannot be explained by the confounding role of ideology, they provide stronger evidence of a direct link between immigration attitudes and policy attitudes.

This difference in empirical approaches aside, I also take issue with Garand et al.'s approach on a conceptual level. The authors take a theoretically agnostic approach to the process they identify, labeling it 'immigrationalization' to signify the specific link between immigration and welfare attitudes. In doing so, they do not take an explicit theoretical stance on the psychological mechanism shared by racialization and 'immigrationalization'. The labeling is, to borrow a phrase from Sartori (1970), too far down the ladder of abstraction: by portraying their finding as reflecting something specific about attitudes toward immigrants, they fail to identify what characterizes the general process by which policy attitudes come to be linked with visibly distinct ethnic outgroups.

The distinction is not just a question of labeling. For an illustration of the problem in the 'immigrationalization' framework, consider the result in Harell et al. (2014), who show in a survey experiment that Canadians' support for welfare benefits is lower when the recipient is portrayed as Aboriginal rather than white (a design and result similar to 'Who benefits'). The 'immigrationalization' framework is of little help here, since it would be plainly absurd to consider Aboriginals immigrants. In contrast, the framework I present here explains how, due to evolved coalitional psychology, similar dynamics can arise in disparate contexts as long as they have visibly distinct racial/ethnic minorities – but irrespective of their status as immigrants per se.

3.1.4 Ethnicization contra 'politics of resentment'

Another recent argument which challenges my approach comes from Sniderman et al. (2014). In short, the authors argue that anti-immigration attitudes in large part are not about immigrants per se, but rather reflect working class anxieties about global economic integration and political elites' inability (or unwillingness) to shield workers from the ensuing international competition. Other scholars have referred to varying articulations of this argument as 'losers of modernization' (Betz, 1993), 'the grievance model' (Ivarsflaten, 2008), or 'the politics of resentment' (Norris, 2005). For convenience, I will adopt the latter term here.

In support of the argument, Sniderman et al. note, using survey data from Denmark, that anti-immigration attitudes are particularly fervent among voters who are culturally

conservative *and* economically leftist. Holding libertarian-authoritarian ideology constant, anti-immigrant attitudes are more fervent among economically leftist voters than among the economically conservative. This left-authoritarian ideological basis of anti-immigration attitudes, they argue, suggests that anti-immigration attitudes really reflect working class longing for an era of economically sheltered nation states and protective welfare state institutions, and not the cultural threat posed by immigration as such. In the politics of resentment argument, working class opposition to immigration is a kind of metaphor, using a vivid outgroup only as a means of giving voice to a more diffuse economic anxiety.

One implication of the working class resentment argument is that, in a counterfactual world with no immigration, working class resentment would merely have found another expression, and the political outcome would not have been much different. Sniderman et al. make this point explicitly, noting that citizens opposed to immigration "express their feelings of vulnerability by calling up a threat in their minds that Muslims pose (...) But we suspect that they would feel the same way even in the absence of Muslim immigrants" (101).

In contrast to my approach, the politics of resentment argument thus implies that anti-immigration attitudes are not in and of themselves politically consequential. For example, in 'European integration', I argue that political elites use the issue of immigration to make voters evaluate European integration in terms of ethnic ingroup/outgroup-distinctions. According to the politics of resentment argument, the real causal operator in this process is in fact not ethnic group identity, but economic anxieties.

I do not take issue with the overall gist of the politics of resentment argument. But it risks overstating the role of economic grievances and understating the importance of group identities. Two empirical patterns support this notion. First of all, the association between anti-immigrant sentiment and economic deprivation is in fact not particularly strong. Studies testing the association have found that the country-level correlation between unemployment rates and anti-immigration attitudes is negligible (and in fact slightly positive) (Sides and Citrin, 2007), and that individual-level unemployment does not consistently predict support for anti-immigrant parties (Goodwin et al., 2014; Oesch, 2008).

Second, across contexts, political parties articulating anti-immigration sentiments do not coalesce around the 'off-diagonal' left-authoritatian position Sniderman et al. identify. Studying the political platform of right-wing populist parties, Ivarsflaten (2008) finds that they differ considerably in terms of their economic platforms and (correspondingly) their voter bases' economic attitudes. In contrast, Ivarsflaten finds that immigration is the one issue over which right-wing populist parties consistently mobilize grievances.

Neither of these two patterns imply that the politics of resentment argument is without merit. But they do suggest that the argument risks giving too short shrift to the *sui generis* role of ethnic group conflict. Immigration is a classically 'easy', symbolic, emotionally engaging issue (Carmines and Stimson, 1980). This 'easy' quality is precisely what allows 'hard' issues to be ethnicized, and it lends immigration a motivational potency virtually no other issue has. The consistency with which populist parties mobilize against immigration – far more consistently than mobilization around economic causes – reflects this quality. And as a consequence, it is at least only partially right to understand merely as an arbitrary expression of economic frustrations.

3.2 When and how policies and group identities become linked

These two theoretical implications speak directly to the question posed in RQ2. On the one hand, they suggest that policy attitudes are likely to be linked to group attitudes, given that individuals are functionally predisposed to think about issues in terms of group affiliation. On the other hand, this linkage is conditional on cues that meaningfully signal the relevance of group affiliation. Here, I elaborate on what this implies for when and how group-policy linkages occur.

3.2.1 When: the importance of stereotype fit

The notion that group-policy linkages occur only when cues signal the relevance of group affiliation highlights an important conditionality. But on its own terms, it says little else. What remains to be accounted for is what types of cues provide such a signal.

In this dissertation, I argue for the critical importance of *stereotype fit*. Stereotype fit refers to the congruence between the target of an attitude on the one hand (such as a policy issue) and stereotyped beliefs about a group on the other. If the substantive content of the target fits the content of the stereotype, stereotype fit is high, and the target is more likely to be evaluated in terms of the stereotype.

The concept of group stereotypes per se is not wholly uncontroversial in political psychology. For example, Kinder and Kam (2009) argue that opposition to immigration reflects 'ethnocentrism', a generalized, undifferentiated antipathy against outgroups. However, in social psychology it is widely accepted that stereotypes exist in the form differentiated traits

associated with social groups (Macrae and Bodenhausen, 2000). Contra Kinder and Kam, the notion of stereotypes implies that outgroup aversion consists not simply of negative affect, but of discrete categorical beliefs about outgroups.

Stereotype fit lies at the heart of the argument in 'European integration', paper B in this dissertation. Consider Table 3.1, reproduced from the paper. I argue there that the the political salience of immigration in the campaign leading up to Denmark's 2000 euro adoption referendum led citizens to think about European integration in terms of immigration. It did so by framing euro adoption as a foreign threat, a framing with high stereotype fit to citizens' mental images of immigrants (in the paper I refer to these mental images as *group schema*, borrowing from Winter (2008)).

Table 3.1 Structure of racialization and ethnicization.

	Racialization	**Ethnicization**
Group attitude	Racial resentment	Ethnic prejudice
In-group	White Americans	Natives
Out-group	African-Americans	Immigrants
In-group stereotype	Hard-working	Familiar, reliable
Out-group stereotype	Lazy	Foreign, threatening
Potentially implicated policy	Welfare	European integration
Analogous policy feature	Unjust rewards	Intrusion

As I argue in 'European integration', framing European integration in terms of foreignness allows elites to link the issue to immigration with a high degree of stereotype fit. This framing in turn ethnicizes attitudes toward euro adoption. Yet this linkage is only possible because a substantive feature of the policy issue is congruent with an outgroup stereotype. Another issue with a high degree of stereotype fit, crime, is the subject of paper D, 'Local contexts'. Here, attitudes about punishment of violent crime links up with stereotypes about immigrants as violent.

Compared to the issues of European integration and crime, the stereotype fit between immigration and welfare is relatively tenuous. We saw this in the discussion of Figure 2.3, which suggests that Danish citizens do not spontaneously think of an immigrant when prompted to think of a welfare recipient. Why would they not make this connection? Recall that Larsen and Dejgaard (2013) point to the role of less ethnically coded media portrayals of welfare recipients. 'Immigration debate', paper C in this dissertation, provides additional evidence on this point. Consider Figure 3.1, reproduced from the paper.

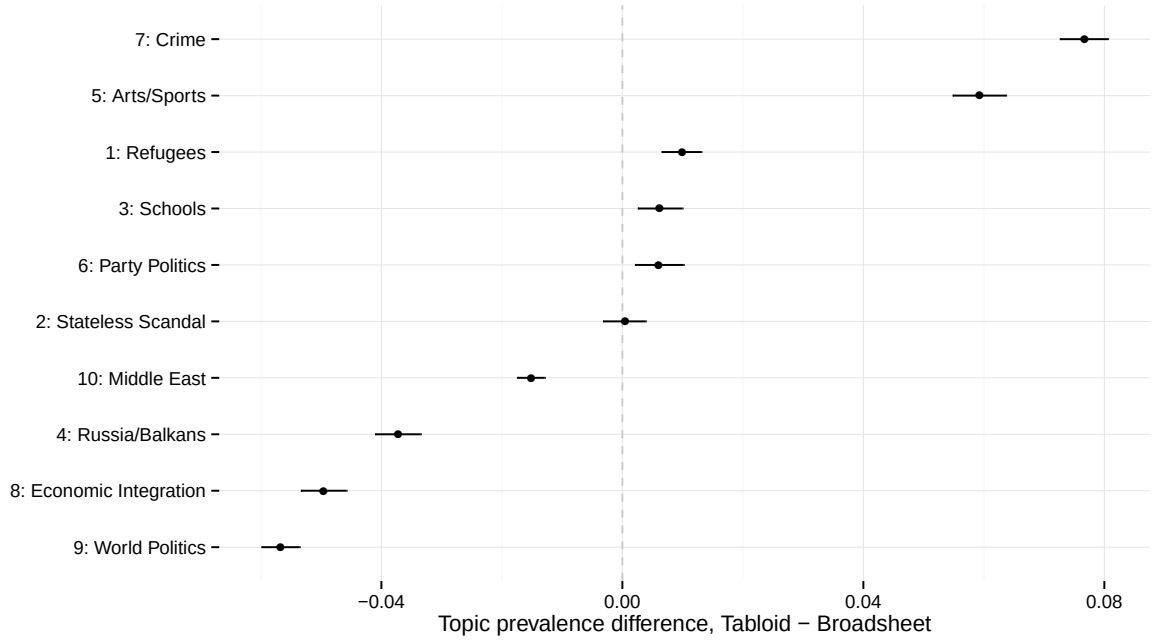

Figure 3.1 Differences in topic prevalence for tabloids vs. broadsheets for each of the 10 topics in the STM. The immigration coverage topic which tabloids place the highest relative emphasis on is crime (topic 7), which is also the only unambiguously negative one. In contrast, immigrants' economic integration into Danish society (topic 8) is predominantly covered by broadsheets. Error bars represent 95 pct. confidence intervals.

Figure 3.1 reproduces a key result from a structural topic model of coverage of immigration across 68,000 newspaper articles (on structural topic models, see Roberts et al. (2014) and 'Immigration debate'). Specifically, the paper shows how the prevalence of each of the 10 most important topics in coverage of immigration differs between tabloid and broadsheet newspapers. This distinction is consequential, since as 'Immigration debate'demonstrates, tabloid newspapers are consistently more negative in their portrayals of immigration.

As shown in Figure 3.1, the issue covered by tabloids with the highest relative prevalence is *crime*. In contrast, the topic 'Economic integration', which covers immigrants' integration into the labor market as well as the fiscal costs of immigrants, is strongly predominantly covered by broadsheet newspapers. Since broadsheet coverage is substantially more positive, this coverage is much less likely to be evocative of immigrant stereotypes.

This piece of evidence provides part of the reason why evidence of ethnicized welfare attitudes is scarce. In contrast to other issues (such as crime), the media in universal welfare states rarely provide cues that link welfare with stereotypes about immigrants (for additional evidence, see Larsen and Dejgaard (2013)). In the absence of such cues, ethnicized welfare attitudes are unlikely to arise.

This hints at a more general weakness in 'universal outgroup aversion' approaches: by ignoring the ignoring the importance of stereotype fit, 'universal outgroup aversion' approaches overlook meaningful variation in how easily issues lend themselves to ethnicization. Perhaps owing to influence from the US 'master narrative', these approaches have focused their attention on the issue of welfare (where stereotype fit is low) at the expense of other issues (for some of which stereotype fit is high).

In sum, the concept of stereotype fit provides part of the answer to RQ2: *policy attitudes become linked with group attitudes citizens are exposed to cues that highlight stereotype fit*. The other part of RQ2 is *how* citizens receive these cues.

3.2.2 How: the diverse sources of group cues

The examples of cues with high stereotype fit in the preceding section are in one way very representative of the literature at large: they highlight the role of *political elites* in promoting group-centric attitudes, often doing so strategically in the interest of gaining political advantage. This perspective is present in this dissertation in the paper 'European integration', where elites strategically frame euro adoption in terms analogous to immigration in order to drum up opposition. Summarizing a similar idea, Charnysh (2015) writes:

> "The linkage between [group] attitudes and political behavior (...) fluctuates over time. *Latent predispositions rooted in the distant past come to matter in the present if and when instrumentalized by political entrepreneurs*. As suggested by the scholarship on racialization in the United States, group-centric sentiments can influence preferences on a broad range of ostensibly unrelated issues, when political elites emphasize linkages between an out-group and a particular policy." (1740, emphasis added)

Another variant of this elite-centric perspective focuses on the role media in promoting group-centric attitudes. In this dissertation, this perspective is partly represented in paper C, 'Immigration debate', although I take a skeptical view of the level and trend of the typically purported degree of negativity in media coverage of immigration. However, the role of the media is thoroughly explored elsewhere. Several accounts argue that stereotypes about immigrants and crime are driven by an 'ethnic blame discourse' in media coverage, which overrepresents and overdramatizes crime perpetrated by immigrants (Dixon and Linz, 2000; Romer et al., 1998).

In ‘Local contexts’, paper D in this dissertation, I argue that this perspective is incomplete. Though ‘top-down’ influences on group-centric attitudes by way of strategic elite rhetoric and stereotyped media coverage are real, they are complemented by ‘bottom-up’, unmediated, experiential processes. Casual observation in the local context can give rise to group-centric attitudes through social categorization processes which link a visible social group with a politically relevant phenomenon.

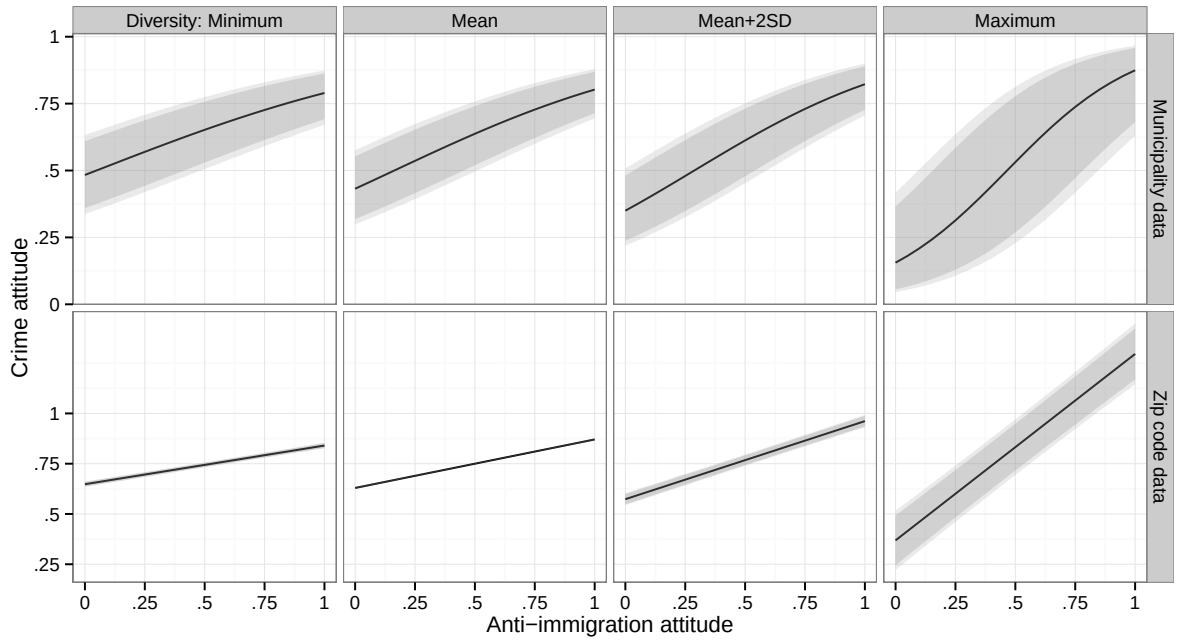

Figure 3.2 Predicted associations between anti-immigration and crime attitudes at various levels of ethnic diversity, in municipality and zip code data sets. Shaded areas represent 90 and 95 percent confidence intervals. In both data sets, anti-immigration attitude predicts crime attitudes more strongly as local ethnic diversity increases.

I provide evidence of this from the link between local ethnic diversity and the association of anti-immigration and crime policy attitudes. Consider Figure 3.2, which reproduces a key result from ‘Local contexts’. Across two different data sets, anti-immigration attitudes predict crime attitudes more strongly in settings characterized by higher levels of ethnic diversity.

I argue in ‘Local contexts’that the varying strength in this relationship reflects variation ethnic stereotypes about crime: the more citizens associate immigrants with crime, the more likely they are to draw on their feelings toward immigrants when asked to provide their attitude toward crime policy. The pattern shown in Figure 3.2 indicates citizens in ethnically diverse localities are more likely to do so.

The broader implication of this result is that cues that promote group-centric attitudes can originate from a diverse set of sources. This insight aligns neatly with the theory of coalitional

psychology, where a basic tenet is that coalitional information is retrieved indiscriminately. In the words of Pietraszewski et al. (2014), social coalitions are "encoded promiscuously but retrieved selectively". This 'promiscuous encoding' implies that citizens make use of any available information to register coalitional affiliations (though only fitness-relevant information is stored). From this perspective, there is no reason for individuals to privilege the symbolic, mediated information over concrete, experiential information as a source of group cues.

3.3 Summary: why not welfare?

The two preceding sections jointly represent my proposed answer to RQ2. Very briefly summarized *policy attitudes become linked with ethnic outgroup attitudes in the presence of cues with a high degree of stereotype fit. These cues may come from media as well as local contexts*. Compared to the existing literature, this perspective is at once narrower and broader. It is narrower in that it identifies stereotype fit as a crucial precondition for ethnicization. At the same time, it is broader in that it highlights a novel source, local contexts, from which citizens may be exposed to group cues.

Table 3.2 illustrates what this perspective implies for selected policy issues.

Table 3.2 Issues by presence of group-relevant cues.

		Media cues	
		−	+
Local context cues	+		Crime
	−	Welfare	European integration

The table is structured based on the presence or absence of group cues from the two sources discussed above and illustrated in Figure 1.2: media and local contexts. In the top right cell is the heavily ethnicized issue of crime, an issue for which citizens receive group cues from the media (cf. paper C, 'Immigration debate') as well as local contexts (cf. paper D, 'Local contexts'). In the bottom right cell is the issue of European integration, an issue with no physically observable aspects (and accordingly not present in local contexts), but often ethnicized through media coverage (cf. papers A and B, 'Who benefits'and 'European integration').

The top left cell of Table 3.2 is empty, signifying that no issues expose citizens to group cues in the local context, but not through the media. This is an empirical absence, not a logical one. For example, if media portrayals of crime strictly refused to mention the perpetrators' ethnic origin, the issue of crime might plausibly belong in the top left cell.

Lastly, the bottom left cell of Table 3.2 has the issue of welfare, only very rarely linked to immigration in media portrayals (cf. paper C, 'Immigration debate'), and largely unobservable in the local context. Once again, this is merely an empirical classification. If media coverage were to cover welfare benefits in highly ethnicized terms, welfare may transition to the bottom right cell. Yet the evidence presented in this dissertation suggests this is far from the case.

Returning to RQ1, this proposed answer to RQ2 offers some measure of relief, narrowly speaking. In universal welfare states, rising ethnic diversity is not likely to undercut support for redistribution. Unexposed to ethnicizing cues about welfare, citizens in universal welfare states do not mentally associate the two. This lack of an association effectively 'short-circuits' the oft-hypothesized link between ethnic diversity and support for redistribution.

As shown throughout the remainder of this dissertation, this does not imply that ethnicization as a phenomenon is a mirage. On the contrary, evidence of ethnicization is apparent for issues such as European integration and crime. Navigating the rough waters of ethnicized politics remains a challenge for universal welfare states.

Chapter 4

Who benefits? Welfare chauvinism and national stereotypes

Abstract
Cross-border welfare rights for citizens of EU member states are intensely contested, yet there is limited research into voter opposition to such rights, sometimes denoted 'welfare chauvinism'. I highlight an overlooked aspect in scholarly work: the role of stereotypes about beneficiaries of cross-border welfare. I present results from an original large-scale survey experiment (N=2,525) among Swedish voters, randomizing exposure to cues about recipients' country of origin and family size. Consistent with a model emphasizing the role of stereotypes, respondents react to cues about recipient identity. These effects are strongest among respondents high in ethnic prejudice and economic conservatism. The findings imply that stereotypes about who benefits from cross-border welfare rights condition public support for those rights.

4.1 Introduction

Under current EU law, the principle of freedom of movement entitles EU citizens to receive welfare benefits in other member states. These so-called cross-border welfare rights have led to concerns about so-called 'benefit tourism' whereby citizens of take residence in member states with relatively generous welfare systems in order to receive benefits. In April 2013, the governments of the United Kingdom, Germany, Austria and the Netherlands voiced such concerns in a joint letter to the European Commission, asserting that 'a significant number of

new immigrants draw social assistance in the host countries, frequently without a genuine entitlement, burdening the host countries' social welfare systems' (Dominiczak, 2013).

The governments' joint letter likely in part reflected electoral considerations in the face of widespread voter skepticism about cross-border welfare rights, often referred to as 'welfare chauvinism' (de Koster et al., 2012; Lubbers and Jaspers, 2011; Mewes and Mau, 2012). The intensity of popular resistance stands in contrast to the observed fiscal impact of intra-European welfare migration on recipient countries. Empirical evidence suggests that this impact is at worst minimally negative and possibly positive (De Giorgi and Pellizzari, 2009; Ruist, 2014). Yet while a considerable literature is devoted to the origins and nature of the legal framework of cross-border welfare rights (e.g., Blauberger and Schmidt, 2014; Ferrera, 2014), there is little social scientific inquiry into public perceptions thereof. Studies that do grapple with the issue tend to focus on characteristics of voters in host countries, such as ideology or labor market self-interest, rather than characteristics of those eligible for benefits. Hence, our knowledge of the causes of welfare chauvinism is incomplete. This stands in contrast to a broadly shared understanding that welfare chauvinism constitutes a significant constraint on the implementation of cross-border welfare rights, and in a broader sense European integration per se (Martinsen and Vollaard, 2014).

In this article, I contribute to a clearer understanding of the dynamics of public support for cross-border welfare rights. I do so by providing evidence for an aspect overlooked in scholarly work on welfare chauvinism: citizens are indeed on average strongly opposed to welfare rights for EU migrants, but this opposition is responsive to cues about the specific types of migrants benefiting from cross-border welfare rights. In other words, notions of 'who benefits' shape public opinion on cross-border welfare rights above and beyond the basic distinction between co-nationals and foreigners implicit to the notion of welfare chauvinism. Moreover, I show that the effects of such cues are more pronounced among citizens already ideologically opposed to cross-border welfare rights.

I provide this evidence using an original large-scale survey experiment among Swedish voters. Since Swedish voters hold comparatively very lenient immigration attitudes, the results provide plausible lower bound estimates of the effects of interest. In a broader perspective, the results support the notion, largely implicitly assumed in the literature, that the cultural and economic diversity of the European Union brought on by successive enlargements has constrained popular support for Europeanized welfare rights. While European 'boundaries of welfare' may have been redrawn legally speaking, pan-European identities do not appear to be able to override salient mental categories of national citizenship.

4.2 Making sense of welfare chauvinism

Political resistance notwithstanding, cross-border welfare rights are an enduring, and still more influential, feature of contemporary European Union law and politics. This permanence is primarily due the fact that cross-border welfare rights are legally based in one of the founding principles of the European Union, the principle of free movement. Current EU law prohibits legislation discriminating against citizens of other EU member states. Crucially, this includes the at least partial prohibition of 'territorial principles' such as residence requirements for social benefits (Martinsen, 2005). Originally covering only qualified workers, the regulation enshrining cross-border welfare rights expanded in 2004 to cover non-workers as well.

Political scientists have long since noted the potentially far-reaching implications of this emergent legal regime. Remarking on the changing nature of EU citizenship, Charles Tilly (1997) argues that '[t]he European Union is (...) complicating categories of citizenship by establishing rights that transfer from state to state, and by creating some sets of rights connecting whole categories of Europeans not to agents of the state within which they reside, but to agents of the Union itself' (quoted in Wind, 2008). As for its impact on welfare states as such, Martinsen (2007) argues that '[a]s a process which moves into the core of the welfare state, [the emergence of cross-border welfare rights] has challenged and essentially redefined the constitutive link between the state and its citizens — as well as the territorial boundaries of welfare'. By decoupling social protection from national political communities, cross-border welfare rights disrupt the century-long process of 'bounding' and 'bonding' within European welfare states, i.e. the anchoring of social insurance systems and shared ties of solidarity within territorial nation states (Ferrera, 2005, 2014).

Popular opposition to cross-border welfare rights, whether in- or outside of an EU context, is typically referred to as 'welfare chauvinism'. This denotes preferences for more generous welfare entitlements for native citizens relative to immigrants (Kitschelt, 2007). Prominent accounts have noted that a 'resurgence of nationalist and welfare chauvinist sentiments' (Hemerijck, 2013) has 'underlined the limits of solidarity within the EU' (Martinsen and Vollaard, 2014). These accounts highlight an important consequence of welfare chauvinism in the EU: by limiting support for cross-border welfare rights, welfare chauvinism creates a 'free movement-welfare cleavage' between old and new member states. This cleavage structures intergovernmental political conflict over social rights at the EU level.

Yet because these accounts view the issue from the perspective of government policy-making, they shed little light on how welfare chauvinist sentiments are triggered at the level of public opinion. The relatively few existing studies of predictors of welfare chauvinism tend

to oscillate between emphasizing economic scarcity vis-a-vis cultural threat, often suggesting a role for both (Mewes and Mau, 2012; Reeskens and van Oorschot, 2012; van der Waal et al., 2010). In doing so, they have tended to focus on the characteristics of respondents that predict welfare chauvinism, ignoring variation driven by stereotypes about the beneficiaries of cross-border welfare rights. Some studies have explored the question of national stereotypes in the context of the European Union, but here the focus has been on generalized trust, an attitudinal disposition clearly distinct from welfare chauvinism (Delhey, 2007; Gerritsen and Lubbers, 2010).

Though welfare chauvinism is empirically a matter of public opinion, its importance is not limited solely to the study of public opinion. Scholars of EU politics should care about welfare chauvinism with respect to cross-border welfare rights, because experience suggests that public opinion tends to ultimately constrain government policy, even in the realm of EU politics (Hobolt and Klemmensen, 2005; Page and Shapiro, 1983).

The issue of cross-border welfare rights is in fact emblematic of this long-run constraint. In discussing the post-Maastricht shift from a 'permissive consensus' to a 'constraining dissensus', Hooghe and Marks (2009) argue that European multi-level governance is challenged by the fact that 'the functional need for human co-operation rarely coincides with the territorial scope of community'. This tension between functional needs of political-economic integration and territorially bounded scopes of community is exceedingly apparent in the current political contestation over cross-border welfare rights. Hence, a theoretically richer understanding of welfare chauvinism helps illuminate the mechanics of the constraining dissensus over European integration.

I add to the literature by addressing precisely these questions, borrowing insights from the political psychology literature on attitudes toward immigration. It may seem theoretically reductive to construe attitudes toward cross-border welfare rights as analogous to immigration attitudes, seeing as the issue also connects to the question of European integration per se as well as classic redistributive politics. Yet drawing on previous work's emphasis on the importance of identity (e.g., Hooghe and Marks, 2009), I expect voters to heuristically make sense of cross-border welfare rights analogous to how they think of the issue of immigration.

I arrive at this expectation for theoretical as well as empirical reasons. Theoretically speaking, there is ample reason to believe that voters' thinking about political issues draws on known, related issues: the starting point in the comprehensive literature on framing effects is exactly the tendency of voters to understand novel issues in terms of preexisting considerations (Chong and Druckman, 2007a; Zaller, 1992). Empirically, I can show that the respondents

studied here prefer different benefit levels for Swedes and EU migrants respectively, indicating that they distinguish quite sharply between the two.[1]

Building on this, I present a framework for thinking about welfare chauvinism in the context of cross-border welfare rights. I draw on two key theoretical debates in the political psychology literature on immigration attitudes. The first of these debates concerns the role of labor market self-interest vs. sociotropic concerns. The other concerns the role of specific outgroup stereotypes vs. generalized outgroup hostility. These two debates guide the research design and theoretically inform the interpretation of subsequent findings.

4.2.1 Sociotropic concerns vs. self-interest

An influential tradition in the study of immigration attitudes posits that voters base their attitudes on their personal interest in immigration from the perspective of labor market competition. This perspective is the predominant account of how self-interest can shape attitudes toward immigration (though in principle, individuals may be for or against increased immigration for self-interested reasons unrelated to labor market competition). The classical theoretical point of reference is the Heckscher-Ohlin model of trade, which predicts that voters oppose immigrants with skill levels similar to their own in order to guard against downward wage pressure (O'Rourke, 2003). In line with the Heckscher-Ohlin model, high-skill workers do in fact tend to be relatively favorable to (predominantly low-skill) immigration (Mayda, 2006; Scheve and Slaughter, 2001).

However, as shown by Hainmueller and Hiscox (2007, 2010), the empirical evidence tends not to support a corresponding prediction of labor market competition models, namely that high-skill voters should oppose high-skill immigration. Highly educated voters should be opposed to highly skilled immigrants, with whom they would be in direct labor market competition. Yet regardless of immigrants' skill level, highly educated voters hold relative positive attitudes toward immigration. This finding reflects a general pattern: models positing self-interested immigration attitudes generally do not fare well (see Hainmueller and Hopkins, 2014). Adding to this pattern, Tingley (2013) finds that, although high-income voters are more exposed to the potential fiscal burden of immigration, they are no less likely to support it. Instead, the link between skill level and immigration support is more likely to reflect highly educated voters' stronger commitment to cosmopolitan values.

In general, theories emphasizing economic concerns tend to also assume that attitudes are based on self-interest, possibly because both assumptions are standard in economic research.

[1] Figure A.7 in the appendix provides evidence for this divergence.

However, accepting that immigration attitudes are sociotropic rather than self-interested does not rule out economic concerns. For example, though the 'fiscal burden' model is generally equated with self-interested concerns about exposure to the public costs of immigration, voter skepticism about immigration can also arise from principled beliefs about government expenditures on immigrants independent of self-interest, a class of explanations I will denote *economic threat*. In a widely cited study, Citrin et al. (1997) show that negative evaluations of the national economy robustly predict anti-immigration attitudes. Presaging the current consensus, the authors emphasize that personal concerns do not appear influential. Yet while seeing some role for economic concerns, Citrin et al. (1997) along with later authors still ascribe more significance to what may be broadly labeled *cultural threat*, a class of explanatory variables encompassing various forms of ethnocentrism and racial/ethnic prejudice. Existing studies of correlates of welfare chauvinism mirror this debate about the relative contributions of economic vs. cultural beliefs (Sniderman et al., 2004).

In sum, there is an emerging consensus in the study of immigration attitudes that ideology dominates self-interest, though within the realm of ideology the relative weight of economic versus cultural threat is less clear. The consensus builds on studies comparing the power of self-interest measures vis-a-vis ideology in predicting welfare chauvinist and anti-immigration attitudes. This consensus notwithstanding, a note of caution is in order about the causal conclusions that can reasonably be drawn from such studies. Inferences based on these studies are complicated by a methodological and conceptual issue sometimes overlooked in studies of immigration attitudes. Although ideology measures, particularly ethnic prejudice, are as expected far stronger predictors than labor market self-interest, one cannot infer from that comparison that the former are more *causally* important. It may be the case that the causal link between skill level and opposition to cross-border welfare rights is partially mediated by ideology. In that case, including both variables in the model amounts to introducing post-treatment adjustment bias (Rosenbaum, 1984). As argued by Tingley (2013), 'confusion over proximate versus less proximate causes can lead scholars to think that variables like 'ethnocentrism' explain immigration attitudes better than more distal variables that capture material self-interest. This might be the case statistically, but only because the more proximate variables are themselves driven by the more distal ones'.

4.2.2 Ethnocentrism vs. the role of stereotypes

Perhaps driven by the frustration of untangling causation from attitudes' partial correlations with other attitudes, some studies have examined the impact of randomly assigned cues

about the identities of immigrants. In an experiment similar to the one presented in this article, Sniderman et al. (2004) find that cues about immigrants' cultural foreignness are more influential than cues about their economic cost to the host society. Other studies use citizens' reactions to immigrants countries of origin to gauge responsiveness to stereotypes, typically finding substantial effects (Hainmueller and Hangartner, 2013, though see Dinesen et al., 2014).

In addition to the methodological advantage of using experiments rather than relying only on cross-sectional attitudinal data, these studies highlight the potential influence that cues about beneficiaries' skill level and cultural foreignness may have on welfare chauvinism. If such cues affect respondents' expressed welfare chauvinism, this suggests that stereotypes about who benefits from cross-border welfare rights are influential in shaping public opinion.

In this context, I thus conceive of stereotypes as simplified mental representations of characteristics of groups (the groups here being types of beneficiaries of cross-border welfare rights) (Macrae et al., 1994). The notion that stereotypes matter follows a long-standing idea in the public opinion literature that the support for public policies depends to a large part on stereotypes of target populations (Schneider and Ingram, 1993).

Some scholars contest the substantive significance of stereotypes, arguing that distinctions among immigrants are inconsequential. Kinder and Kam (2009) contend that a generalized predisposition to think in terms of in-groups and out-groups, ethnocentrism, is the main causal driver of immigration attitudes, leaving little room for group-specific attitudes. In other words, according to the ethnocentrism model, specific characteristics of out-groups matter little, and specific cues should thus be have little effect on attitudes.

I engage with this debate over the role of stereotypes by exposing respondents to experimentally manipulated cues about cross-border welfare beneficiaries' cultural foreignness. First of all, by employing a nationality cue, I study the effects of cues about beneficiaries being either Dutch or Bulgarian (on the choice of these specific nationalities, see below). The first hypothesis captures the theoretical intuition about the effect of the nationality cue.

> *H1*: Cues about a Bulgarian beneficiary relative to a Dutch one increase expressed welfare chauvinism.

Besides allowing for testing for for the importance of the role of stereotypes in general, the chosen conditions have the advantage of being able to disentangle effects that are conflated in many similar designs. European citizens may exhibit negative reactions to immigrants of, say, Turkish origin as in Hainmueller and Hangartner (2013), yet this reaction by itself

could reflect either old-fashioned racism, religious discrimination, some subtler form of ethnic prejudice or assumptions about culturally foreign immigrants' inability to assimilate culturally. By comparing respondent reactions to cues about a population that is different culturally, but not racially or religiously, I can be quite confident that the reaction reflects neither racial nor religious prejudice.

In order to further test the role of stereotypes, the experiment employs an additional cue about beneficiaries: their number of children. Since the survey is nominally about the right of other EU citizens to receive the Swedish child benefit, mentioning the number of children of a hypothetical beneficiary is inconspicuous, yet I expect the cue to evoke stereotypes about their cultural foreignness and cost to public finances. The second hypothesis captures this expectation.

> *H2:* Cues about a beneficiary with a higher number of children increase expressed welfare chauvinism.

Lastly, while the effects of cues posited in hypotheses 1 and 2 are estimated as averages across all respondents, some respondents may be more responsive to such cues than others. This expectation combines insights from the ethnocentrism framework advocated by Kinder and Kam (2009) with the stereotype framework advocated by Sniderman et al. (2004) and others. While some cues about beneficiary characteristics should evoke stronger reactions than others, it is also likely the case that some respondents are particularly predisposed to such reactions. The third hypothesis captures this individual-level heterogeneity in the effects of cues.

> *H3:* Measures of ideology that predict welfare chauvinism are also associated with stronger reactions to cues.

4.2.3 The economic/cultural threat distinction and the role of individuating information

As argued above, comparing reactions to nations of origin within the EU has the advantage of ruling out some mechanisms that may in other cases confound measures of cultural threat, namely racial and religious prejudice. Adverse reactions to a Bulgarian as opposed to a Dutch beneficiary are not likely to reflect either of those. However, the comparison does not distinguish between two broader types of mechanisms: those of cultural vs. economic threat, a long-standing theoretical distinction in the literature on immigration attitudes (McLaren,

2002). In short, welfare chauvinism may reflect either cultural prejudice against a Bulgarian beneficiary or concerns that (s)he, due to an assumed lower skill level, will impose a larger fiscal burden on the host society.

This design of this study does not allow for distinguishing between these two mechanisms. I implement this design not because disentangling the role of economic vs. cultural threat is uninteresting, but because the standard approach to doing so would likely introduce a novel source of bias. In this standard approach to distinguishing the effects of cultural vs. economic threat, a survey elicits reactions to hypothetical, elaborately described specific individuals whose specific cultural and economic threat levels are randomized (e.g., Turper et al., 2014). However, other studies show that the observed effect of cultural threat tends to be diminished in studies of attitudes toward *individual migrants* (e.g. Hainmueller and Hopkins, 2015). When deciding whether to accept or reject specific individuals, voters appear both more positive on average and less responsive to cultural cues, an effect known as the *person-positivity bias* (Iyengar et al., 2013; Sears, 1983). This conforms to social-psychological findings that exposure to individuating information tends to crowd out the role of stereotypes (Kunda and Sherman-Williams, 1993).

The person-positivity bias implies that studies of decisions about specific individuals do not accurately capture attitudes about policy design. As a result, I cannot disentangle the role of cultural threat from that of economic threat by presenting respondents with highly individuated information separating the two. This study deliberately exposes respondents to coarse, abstract cues about beneficiaries of cross-border welfare rights in order to capture reactions to groups affected by the policy rather than individuals. This comes at the expense of providing less information about the exact drivers of these reactions.

4.3 Experimental design

I presented each survey respondent with a variation of the following informational probe:

> *It is sometimes discussed whether the EU should allow citizens of EU member states to receive benefits in other EU countries. For example, under current EU law, a [Dutchman/Bulgarian] moving to Sweden [with his children] would immediately be entitled to receive Swedish child benefit[, including if the children remain in their home country]. If the [Dutchman/Bulgarian] has [1 child-5 children], he or she would be entitled to [1,050-8,114] kroner per month.*

As indicated by the bracketed sections of the probe, each respondent was given an example of an EU citizen's right to receive the Swedish child benefit with characteristics of the example randomly varied.[2] The randomized components are summarized in Table 4.1. The table also presents the probe's benefit levels converted to euros.

Table 4.1 Overview of randomized components

Variable	Outcomes
Nationality	Bulgarian Dutch
Number of children (benefit)	1 (1,050 kronor) 2 (2,250 kr.) 3 (3,754 kr.) 4 (5,814 kr.) 5 (8,114 kr.)
Children can stay home	Mentioned Not mentioned

As shown, the *Nationality* treatment takes two forms: the hypothetical recipient is described as either a Dutchman or a Bulgarian. These two nationalities were chosen so as to maximize variation in cultural distance. However, in order to minimize the possibly confounding role of geographical proximity, Netherlands was chosen as a culturally similar, but non-neighbouring country. Bulgaria takes the role of the culturally foreign country. I opted for Bulgaria over the possibly more culturally foreign Romania for two reasons: for one, the cultural foreignness of Romanians may be partly conflated with Romas, an ethnic group subject to particularly strong, negative stereotypes (Sigona, 2005). Furthermore, at the time of the survey, Swedish media covered a scandal involving police registration of Roma families. In order to avoid the confounding influence of both factors, I chose Bulgarian as the culturally distant country.

The *Number of children* treatment has five conditions, each of which presents the number of children with the corresponding child benefit in Swedish kronor bracketed. Alternatively, the design could have consisted of just the extremes of this range, i.e. one and five children. But I opted for a separate condition for each number in order to be able to assess potential nonlinearities in the effects. As I will show in the results section, however, there do not appear to be nonlinearities in the effects.

[2]The informational probe is presented here in its entirety. It is thus implied, but not explicitly stated, that the migrant is legally residing in Sweden, thus meeting the legal requirement for economic self-sufficiency. For reasons described in the preceding section, the probe is deliberately kept informationally sparse.

Lastly, I include the treatment *Children can stay home* for whether it is mentioned that the child benefit depends only on the parents' country of residence, and that the children could thus potentially remain in the home country. I do not focus on this treatment in this article, but included it in the experimental design because it was often referenced in the political debate over cross-border welfare rights. Out of a suspicion that respondent sensitivity to cues was dependent on this fact, I randomized the mention of it. Respondent sensitivity to cues turns out not to be dependent on this treatment, so I will not elaborate on it further, though I will describe its main effect in the results section.

In addition to the elements shown in Table 4.1, one more aspect of the survey was randomized: the timing of the scale measuring respondents' level of ethnic prejudice (cf. below). One concern about administering a scale of ethnic prejudice is that the questions may themselves prime respondents to think about policy issues in group-centric terms. Such an effect could bias the design in favor of a cultural threat interpretation by artificially inflating respondents' reliance on ethnic prejudice. In order to account for this, the experiment randomized the sequence of the ethnic prejudice scale and the informational probe. Table A.4 in the appendix provides results testing the importance of this sequence by interacting the sequence with the ethnic prejudice scale as well as each of the experimental treatments. Neither the scale nor the treatments of interest here are significantly affected by the order of presentation of the ethnic prejudice scale and the informational probe.

4.3.1 Empirical setting: the Swedish child benefit

The present study elicits voter responses to the right of intra-EU migrants to receive the Swedish child benefit. Throughout the experiment, respondents are presented with only factually correct information. This removes problems associated with presenting respondents with deceptive or hypothetical information, such as ethical issues, respondents second-guessing the purpose of the study, or respondents knowing the information to be false (Dickson, 2011).

Furthermore, the case has a number of suitable features for examining public opinion about cross-border welfare rights. First of all, the Swedish child benefit is universal, i.e. Swedish citizens with residence in Sweden are entitled to the benefit regardless of income or labor market insurance. This is precisely the type of benefit affected by EU law on cross-border welfare rights, since as an EU member, Sweden cannot legally discriminate between Swedish citizens and other EU citizens. Hence, a citizen of any EU country with residence in Sweden is entitled to the benefit.

Second, the benefit is comparatively generous. A family with one child entitled to the child benefit receives a tax-exempt monthly cash transfer of 1050 Swedish kronor, or about

111 euros. The size of the benefit increases progressively with the number of children such that a family of five receives a monthly transfer of 8114 kronor, or about 860 dollars. By describing a real, comparatively generous benefit, the test is *ceteris paribus* more likely to elicit deliberate, effortful responses.

Third, Swedish voters are on average favorable to immigration in general. For evidence, consider Figure 4.1, which shows average respondent attitudes toward three dimensions of immigration among the 21 participating EU countries in the sixth round of the European Social Survey in 2012. Swedish respondents express the strongest average agreement that immigration 'makes their country a better place', the second strongest average agreement that immigrants 'enrich the cultural life of their country', and the second strongest agreement that 'immigration is good for the economy'. The on average highly inclusive attitudes among Swedes work against the expectation that voters should react to cues about beneficiary characteristics. If Swedish respondents are sensitive to such cues, citizens of other countries are likely to be as well.

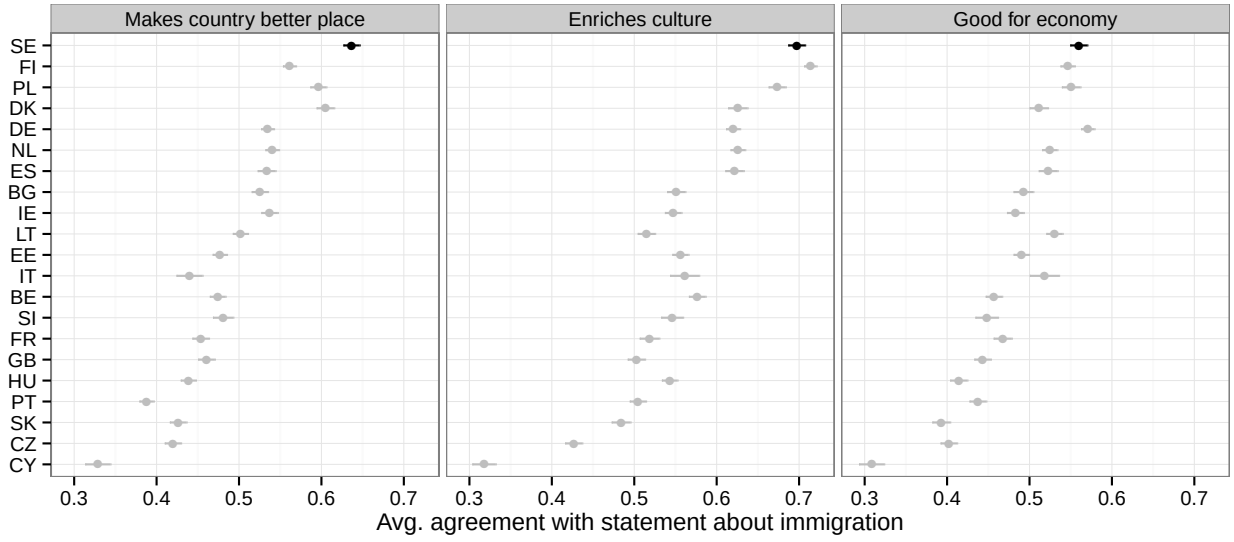

Figure 4.1 Average response to the questions about immigration, European Social Survey round 6 (ESS, 2012). *Note:* The 10-point response scale is rescaled to range from 0 to 1 (higher values equal more positive attitudes). Sweden's average is highlighted (dark color). Bars are 95 pct. confidence intervals.

Lastly, but importantly, the benefit's dependence on number of children allows the test to contain a subtle, realistic cultural cue. Since family size is likely to be perceived as a cue about cultural foreignness, increasing the number of children in the example is likely to signal to respondents that beneficiaries will be culturally foreign. Yet since the cost of the benefit also increases with the number of children, respondents can express opposition under the

guise of reacting to the cost of the benefit, and avoid violating societal norms of equality (Mendelberg, 2001).

4.3.2 Measurement of key variables

After reading the text, the survey asks respondents about their level of support for cross-border welfare rights on a five-point scale ranging from 'strongly approve' to 'strongly disapprove'. I use this response as a measure of respondent welfare chauvinism. This will function as the dependent variable in all hypothesis tests. I rescale the variable to range from zero to one, where one represents strongest opposition to cross-border welfare rights. I estimate the models using linear regression. Results are robust to using a median-split, dichotomized dependent variable and logistic regression (see Table A.5 in the appendix).

In addition to their response to this question, respondents are asked about standard demographic items. These include education and labor market status, which in line with previous studies I will use as proxy measures of *self-interest*. Education is measured as a nine-point interval scale ranging from 'not completed elementary school' to 'Ph.D.', though in order to guard against non-linear effects I include it in regressions as a dummy variable for some level of tertiary education. Labor market self-interest is included as a dummy for whether the respondent is employed.

Measures of *ideology* consist of two batteries of ideological attitude items: one, comprising a scale labelled *economic conservatism*, consists of items about classically left-right economic issues in contemporary Swedish politics. The items are used in similar standard measures in Swedish election studies (Oscarsson and Holmberg, 2013). The scale exhibits high reliability (Cronbach's α=.86). I will use economic conservatism as a measure of each individual's sensitivity to economic threat, drawing on the intuition that individuals opposed to higher levels of taxation should be more critical of the increased tax burden imposed by immigration.

The other ideological scale, here labelled *ethnic prejudice*, taps negative affect against ethnic outgroups. The items are drawn from the 'modern racial prejudice' scale developed and validated on a Swedish sample by Akrami et al. (2000). The scale is a theoretical and methodological extension of the symbolic racism scale, widely used in studies of racial prejudice and policy attitudes in the United States (Kinder and Sanders, 1996; Kinder and Sears, 1981). The scale exhibits high reliability (α=.87). The items used in each ideological scale are presented in Table A.3 in the appendix.

One potential concern with using the modern racial prejudice scale is that some of the items ask respondents about government transfers to immigrants. The scale includes these items in an effort to minimize social desirability bias, but some items could be considered so similar to the dependent variable as to render an observed correlation tautological. In order to account for this, I rerun key analyses using a reduced scale excluding three potentially tautological items. Reliability, though slightly lower, remains acceptable (α=.77). The results, shown in Table A.6 in the appendix, are robust to using the reduced scale.

4.3.3 Sample demographics

The survey experiment was conducted in late 2013 as part of the Citizen Panel administered by the University of Gothenburg (Martinsson et al., 2013). The Citizen Panel consists of a a mixed, but mainly self-recruited, sample. 70 percent of the gross sample is opt-in, while 30 percent is probability based recruitment from population samples. While the entire Citizen Panel has 14,494 active members, the present survey was administered to 4,354 members of whom 2,525 filled out the survey, yielding a Net Participation Rate of 58 percent.

The demographics of the sample skew male and highly educated relative to the population at large. Specifically, the sample average age is similar to the population average (47.8 years vs. 49.4 years), but the sample contains relatively few females (35.1 percent). Most strikingly, 75.6 percent of respondents have some tertiary education, which is only true for 25.3 percent of the population in general. To the extent that these differences are correlated with the variables of interest, this lack of representativeness will bias estimates of the average population levels of those variables. Yet while estimates of *levels* are biased, studies generally find estimates of *effects* to be robust to skewed sample compositions (Druckman and Kam, 2011; Leeper and Mullinix, 2014).

4.4 Triggers and moderators of welfare chauvinism

Table 4.2 presents the results from estimating various models of respondents' expressed welfare chauvinism. Model 1 uses only individual-level variables measuring respondent demographics, labor market self-interest, and ideology, as discussed above. Models 2-4 add variables for the experimental components of the study.

Table 4.2 Models of welfare chauvinism

	Opposed to cross-border welfare rights			
	(1)	(2)	(3)	(4)
Intercept	0.22^{***}	0.07^{*}	0.13^{***}	0.13^{***}
	(0.03)	(0.03)	(0.04)	(0.04)
Gender (f)	0.06^{***}	0.06^{***}	0.06^{***}	0.06^{***}
	(0.01)	(0.01)	(0.01)	(0.01)
Age	0.002^{***}	0.002^{***}	0.002^{***}	0.002^{***}
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Education (some uni.)	-0.02	-0.02	-0.02	-0.02
	(0.01)	(0.01)	(0.01)	(0.01)
Employed	0.05^{***}	0.05^{***}	0.04^{***}	0.04^{***}
	(0.01)	(0.01)	(0.01)	(0.01)
Ethnic prejudice (EP)	0.64^{***}	0.63^{***}	0.47^{***}	0.64^{***}
	(0.03)	(0.03)	(0.06)	(0.03)
Economic conservatism (EC)	0.09^{**}	0.09^{**}	0.09^{***}	-0.09
	(0.03)	(0.03)	(0.03)	(0.06)
Children stay		0.19^{***}	0.19^{***}	0.19^{***}
		(0.01)	(0.01)	(0.01)
Nationality: Bulgarian		0.06^{***}	0.02	-0.01
		(0.01)	(0.02)	(0.02)
No. of children (NC)		0.01^{*}	-0.01	-0.001
		(0.004)	(0.01)	(0.01)
EP×Nationality: Bulgarian			0.10^{*}	
			(0.05)	
EP×NC			0.04^{*}	
			(0.02)	
EC×Nationality: Bulgarian				0.19^{***}
				(0.04)
EC×NC				0.02
				(0.02)
N	2370	2370	2370	2370
R^2	0.26	0.35	0.36	0.36
Adjusted R^2	0.26	0.35	0.35	0.36

$^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

4.4.1 Cues about recipient characteristics

An informative result from comparing Model 1 in Table 4.2 with the remaining models is that including experimental treatments clearly improves model fit, from an adjusted R^2 of around .26 to one of around .35. Though this comparison is not informative of the substantive magnitude of each of the treatment effects, it does indicate that the treatments in total explain a substantial part of the observed variation in welfare chauvinism. This in turn suggests that beliefs about who benefits, induced by the experimental cues, are indeed consequential.

Looking at each model in turn, Model 2 includes terms for the information about children remaining and the two cues about recipient characteristics, nationality and number of children. Notably, the 'Children stay home' treatment has a large and strongly significant effect: this cue alone causes a movement of nearly a fifth of the scale of the dependent variable in the direction of increased welfare chauvinism. The other two models in Table 4.2 add interactions between these two cues and the two measures of ideology, ethnic prejudice and economic conservatism. Figure 4.2 illustrates the main experimental results, plotting the predicted marginal effects of each condition described in hypotheses 1 and 2.

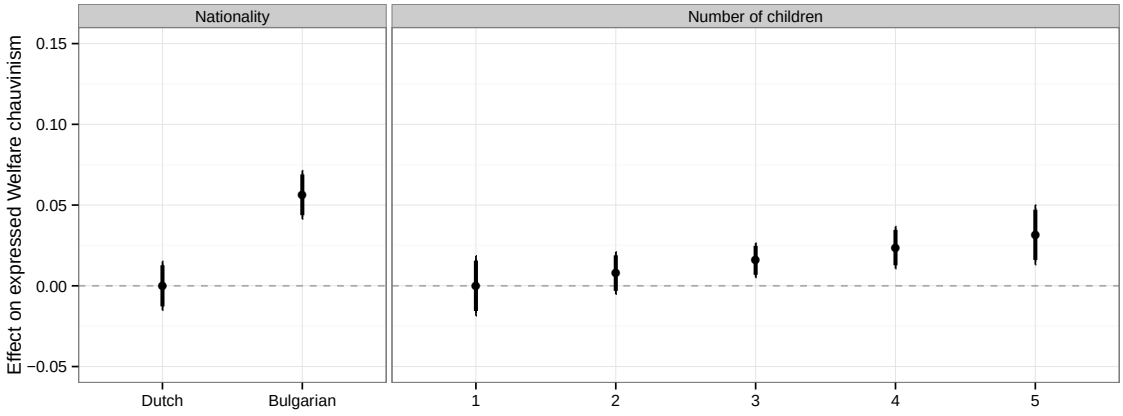

Figure 4.2 Predicted effects of nation of origin (left panel) and number of children (right panel).
Note: Based on model 2 in Table 4.2. Effects are plotted relative to reference categories (respectively Dutch nationality and 1 child). Error bars represent confidence intervals of 90 percent (thick lines) and 95 percent (thin lines).

As attested by the estimates in Model 2, respondents do in fact react strongly to the nationality cue. The mere mention of a Bulgarian beneficiary in an example significantly increases welfare chauvinism relative to mentioning a Dutch one. The effect is not only statistically but substantively significant: the nationality cue increases opposition by about six

percentage points of the range of the dependent variable. That corresponds to moving about halfway across the observed range of economic conservatism. The data thus supports *H1*.

A similar picture emerges from the other cue about the beneficiary's number of children. Moving across the range of the variable is associated with an increase in welfare chauvinism of about three percentage points. The shift is thus statistically significant and in the expected direction. Hence, the data supports *H2*.

One potential concern could be that the experimental treatment conditions interact, such that for example the effects of the nationality cues are only present under the 'five children' condition. To test for the possibility of interactive effects, Table A.7 in the appendix presents a full factorial specification of the results, interacting each 'number of children' treatment with the nationality cue for each condition of the 'children stay home' treatment separately. Figure A.9 in the appendix plots the results of the full factorial models. Though the nationality cue appears somewhat weaker in the 'five children' condition, the effects of the treatment conditions are generally additive with no clear interactive patterns. All statistical tests of interactive effects between treatment conditions turn up insignificant. The appendix also shows results from full factorial ANOVA models of the experimental treatments, one model with 'number of children' treated as an interval scale variable (Table A.8), one where it is treated as a nominal scale variable (Table A.9). In the full factorial ANOVA models, the 'number of children' treatment is significant only when treated as an interval scale variable, as is the case in Table 4.2.

In sum, these findings support the overall theoretical expectation that beneficiary characteristics matter. The effect is present for the nationality cue as well as the number of children cue: for the average respondent, cues about Bulgarian nationality and more children independently increase expressed welfare chauvinism. However, cues may not affect everyone equally; in particular, they might elicit a stronger response among those already ideologically opposed to cross-border welfare rights.

4.4.2 Interactions with respondent ideology

Testing the moderating influence of respondent ideology effects of cues, models 3 and 4 in Table 4.2 include interactions between the ideology measures and the cues about beneficiary characteristics. In order to make sense of the estimates, I also plot estimated effects of cues at the highest and lowest levels of ethnic prejudice and economic conservatism respectively.

Overall, the results strongly indicate that ideology moderates the effect of cues about beneficiary characteristics. As was the case with the unconditioned experimental effects, the

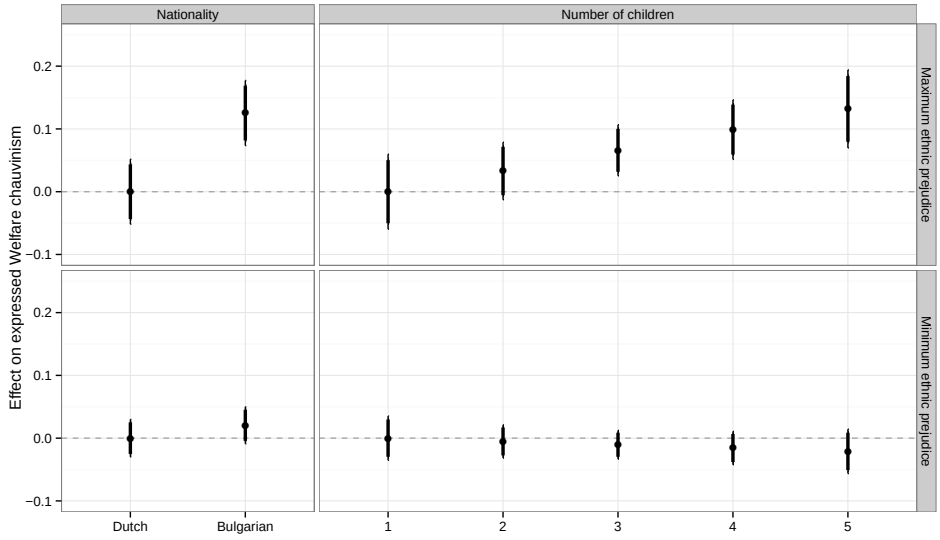

Figure 4.3 Predicted effects of nation of origin (left panels) and number of children cues (right panels) for minimum and maximum levels of respondent ethnic prejudice. *Note:* Based on model 3 in Table 4.2. Effects are plotted relative to reference categories (respectively Dutch nationality and 1 child). Error bars represent confidence intervals of 90 pct. (thick lines) and 95 pct. (thin lines).

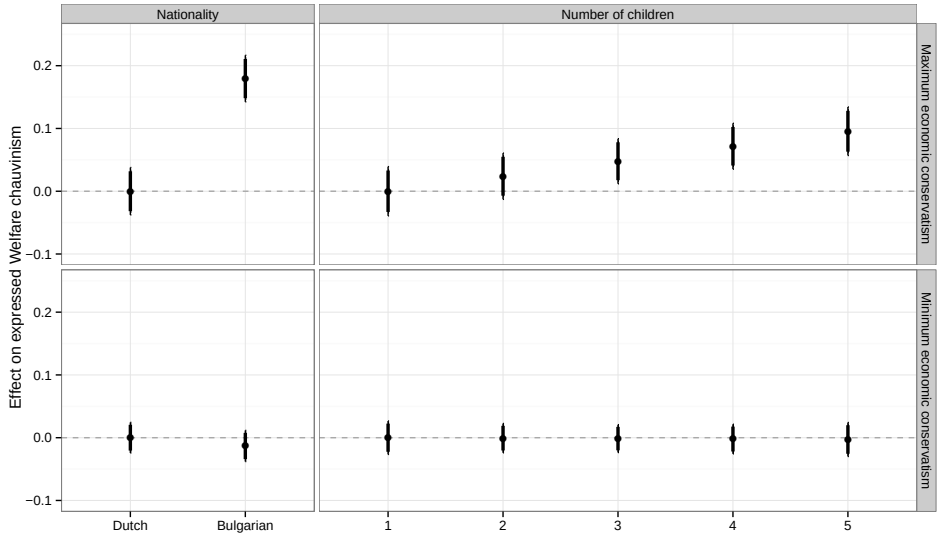

Figure 4.4 Predicted effects of nation of origin (left panels) and number of children cues (right panels) for minimum and maximum levels of respondent economic conservatism. *Note:* Based on model 4 in Table 4.2. Effects are plotted relative to reference categories (respectively Dutch nationality and 1 child). Error bars represent confidence intervals of 90 pct. (thick lines) and 95 pct. (thin lines).

evidence is clearest for the nationality cue. For those most ideologically prone to welfare chauvinism, the nationality cue has a large effect of around one sixth of the range of the dependent variable. In contrast, among those at the opposite end of the ideological spectrum, there is no significant effect of the nationality cue. The pattern is consistent across the two measures of ideology.

For the number of children cue, the pattern is substantively similar, though slightly less clear. The effect of the cue interacts with ethnic prejudice, such that those high in ethnic prejudice are significantly more responsive. The interaction is not significant for economic conservatism, though the effect of the cue is significant for those highest in economic conservatism. In other words, though the interaction term itself falls just short of significance at conventional levels, the pattern is the same for the number of children cue as for the nationality cue. Both cues have substantive, significant effects, but those effects are concentrated among those high in ethnic prejudice and economic conservatism. In the appendix Tables A.10 and A.11, I show that the estimates are similar when obtained using a full factorial ANOVA model.

Notably, this effect is not isolated to a small, ideologically extreme subset of respondents. Figure A.8 in the appendix illustrates this for the number of children cue: the effect reaches significance at around the median value of each ideology measure.

In sum, the data supports *H3*. Cues about beneficiary characteristics do not affect all respondents equally. They elicit stronger reactions among those ideologically opposed to cross-border welfare rights to begin with.

4.5 Conclusion

This study set out to study determinants of popular opposition to cross-border welfare rights, often denoted 'welfare chauvinism'. In order to test the effect of stereotypes on welfare chauvinism, I implemented randomized cues about beneficiary characteristics in a large survey about attitudes toward cross-border welfare rights.

The experimental results generally supported the expectation that respondents react to cues about who benefits from cross-border welfare rights. The nationality cue and the number of children cue both increase expressed welfare chauvinism in the predicted direction. Furthermore, those ideologically prone to oppose cross-border welfare rights reacted strongly to both cues.

The experimental findings address an ongoing debate about the role of stereotypes vs. ethnocentrism in shaping attitudes. The findings lend support to both accounts, suggesting they

are not mutually exclusive: consistent with the former account, voters are indeed responsive to cues about out-group characteristics. Consistent with the latter, this responsiveness is significantly higher among those ideologically predisposed to welfare chauvinist attitudes.

The findings also highlight the role of stereotypes in welfare chauvinism. While previous research has tended to emphasize individual characteristics, this study demonstrates the importance of predominant mental images of the beneficiaries of cross-border welfare rights. Voters prompted to think of a beneficiary of cross-border welfare rights as a Dutch individual with one child were substantially less likely to express welfare chauvinistic sentiments compared to one prompted to think of a Bulgarian with five children. Hence, stereotypes about who benefits are likely to be consequential in shaping political debate over cross-border welfare rights. Future research might examine the nature and origins of the actively held stereotypes of European mass publics.

As for the scope of inferences to be drawn from this study, two caveats are worth keeping in mind. For one, the patterns observed here may play out differently for insurance-based welfare benefits, for which recipiency is conditional on labor market participation. This should, ceteris paribus, allay citizen concerns about the fiscal impact of cross-border welfare rights in insurance-based systems. Second, the experiment presented here remains a single-country study. As argued, Sweden is at least in some important respects a least-likely case, with the ensuing inferential benefits. But it is hardly a typical one. Future research would do well to explore public opinion dynamics of welfare chauvinism in other political contexts.

Keeping these caveats in mind, two broader implications for public policy would appear to follow from the results. First, the results suggest that policy-makers' efforts to counter public opposition by informing voters about the limited (or in fact positive) fiscal impact of intra-EU labor migrants address only a subset of voters' concerns. It may well be the case that intra-EU labor migrants are net contributors to the public coffers of host countries. Yet if cultural concerns loom at the very least as large as economic ones in the minds of voters, such an argument may miss the target and ultimately be of little persuasive power.

Second, the results imply that although cross-border welfare rights may legally speaking blur the boundaries of citizenship within the European Union, these boundaries remain highly salient in the minds of voters. Voters are on average sceptical of cross-border welfare rights, and this scepticism increases in the face of cues about beneficiaries' cultural foreignness. By implication, the expansion of the European Union into a more culturally diverse political community is likely to have diminished public support for cross-border welfare rights. As illustrated by the case of cross-border welfare rights, public opinion remains uncommitted even to policies which reflect the foundational principles of the Union.

Chapter 5

Ethnicization of support for European integration

Abstract

By presenting political issues in ways that evoke prominent group stereotypes, political elites can cause citizens to evaluate the issue based on their attitude towards that group. In this article, I argue that such a process operates with respect to immigration and the issue of European integration. I call this process 'ethnicization' of support for European integration. I provide evidence for ethnicization in three steps. In Study 1, I rely on evidence from two referenda on euro adoption in highly similar political systems, Denmark's referendum in 2000 and Sweden's in 2003, where political elites linked immigration and European integration only in the former case. Consistent with ethnicization, ethnic prejudice and vote choice were more strongly associated among Danish voters. In Study 2, relying on open-ended survey responses, I show that Danish voters were also more likely to explain their vote choice in terms of identity. In Study 3, I demonstrate a similar pattern in cross-national data, showing that European integration attitudes are more closely linked with immigration attitudes when immigration is politically salient. The study conceptualizes and provides evidence of an important mechanism by which policy issues can become subsumed by group identities in the minds of voters.

5.1 Introduction

Arriving at opinions on political issues is a civic norm for members of democratic societies, yet for most citizens it is one fraught with ambiguity, lack of motivation and insufficient factual

knowledge. The task is complicated by the fact that on many issues, not only is it disputable what the right position is; it is unclear what the issue is really about. The literature on political issue framing convincingly shows that frames broadly construed can affect public opinion in powerful ways (Chong and Druckman, 2007b). Yet for rhetorical framings of issues, what Chong and Druckman label "frames in communication", to shape citizens' reasoning about issues, i.e. "frames in thought", they need to resonate with citizens' existing predispositions.

A long-running but recently reinvigorated strand of framing research examines the power of frames that appeal to group identities (Nelson and Kinder, 1996b; Sides, 2013; Winter, 2008). By appealing to identities, group cues can short-circuit voters' systems of higher-order information processing: they allow voters to substitute a 'hard' issue for an 'easy' one (Carmines and Stimson, 1980). A well-known special case of group-based cognition in American politics, racialization, is the focus of a substantial body of work linking racial identity to policy attitudes about welfare (Gilens, 1996), crime (Peffley and Hurwitz, 2002), and health care (Tesler, 2012). In fact, the vast majority of studies of group-centric framing focus on racial politics in the United States and the specific subprocess of racialization of attitudes.

In this aticle, I propose that a structurally similar but substantively different subprocess, *ethnicization*, operates with respect to the politics of immigration in European societies. Focusing on the issue of European integration, I argue that group-centric framing can promote ethnicized attitudes toward European integration by forging implicit links to attitudes toward immigration. Specifically, political elites ethnicize European integration by making salient basic mental categories of foreignness vs. familiarity. This in turn makes citizens more prone to evaluate the the issue based on how they feel about immigration.

I present evidence in support of this idea in three steps. In Study 1, I show that in a survey from Denmark's 2000 referendum on euro adoption, immigration attitudes predicted vote choice. In contrast, in Sweden's 2003 referendum on the same issue – an environment in which immigration had very low salience – there was no such correlation. In Study 2, relying on open-ended survey responses from the same set of surveys, I provide evidence for the implied mechanism by showing that Danish voters were also more likely to explain their vote in terms of identity. In Study 3, I present cross-national evidence from 23 European countries across 12 years showing a similar pattern: in country-years where immigration is politically salient, attitudes toward immigration and European integration are more strongly associated.

This article contributes to the extant literature both empirically and theoretically. Empirically, it provides evidence for group-centric framing of a novel and substantively important

issue, European integration. Theoretically, the study shows that frames can implicate group-based attitudes in citizens' thinking about issues even in the absence of any meaningful connection between the two. This stands in contrast to most existing accounts of group-centric attitudes, which posit that group-centric frames work by the group and policy issue in question. The results in this article suggest this is not a necessary condition for group-centric framing to be effective. Both the empirical and the theoretical contributions thus imply a broader scope of geographical and political contexts in which policy issues can become subsumed by group identities in the minds of voters.

5.2 Theory

The core of my theoretical argument is that by making immigrant-native distinctions politically salient, political elites can cause citizens to evaluate European integration in terms of their attitudes toward immigration. Here, I elaborate on this argument and situate it in the existing literature.

5.2.1 Group implication theory and racialized issue attitudes

The argument draws on *group implication theory* (Winter, 2008), a framework for how frames that symbolically evoke group identities can cause voters to understand policy issues in terms of those identities. In the most general sense, group implication is "the process through which ideas about social groups (...) can be applied to political issues that do not involve [them] directly" (Winter, 2008, 19). The theory thus posits that policies can be implicitly framed so as to be structurally similar to widely shared cognitive schema about group relations, leading voters to draw analogies between the two. The idea borrows from Converse's proposition that group-centric attitudes require the presence of "interstitial linking information" connecting a familiar intergroup relation with salient features of the policy in question (Converse, 1964).

Applying insights from group implication theory, the theoretical account presented here departs from the extant literature on racialized policy attitudes in two important ways. First and foremost, racialization as typically construed is unlikely to occur in European societies, for the simple reason that these countries do not have a racial minority with the political significance of African-Americans in the United States. However, they do have *ethnic* minority groups whose rights and obligations are a matter of intense, often salient political contestation, namely non-Western immigrants, whose size as a share of native populations has increased

considerably in recent decades (Zick et al., 2008). Since the process revolves around a native-immigrant immigrant group distinction rather than a racial one, I refer to it as *ethnicization*. By providing a theoretical 'translation' from racialization to ethnicization, the account presented here allows for perceiving a broader spectrum of group-implicating rhetoric beyond the canonical U.S. examples.

Second, drawing on group implication theory, my theoretical account challenges a key assumption found in many previous studies of group-centric framing. Specifically, these studies have assumed that linking information works by accentuating real group-relevant features of a policy. For example, racialized attitudes about welfare spending can be triggered by portraying welfare recipients as black (Nelson and Kinder, 1996b), and racialized health care attitudes by emphasizing Barack Obama's political sponsorship of health care reform (Tesler, 2012). However, I argue that such group-relevant features are not required for group-centric framing to be effective. Instead, a policy can be linked to a group solely by virtue of rhetoric symbolically linking the two, despite the group being in no meaningful sense a target population of the policy (for a similar argument with respect to race, see Winter, 2008). The next section describes in more detail how I theorize this linkage in the context of ethnicization.

5.2.2 Ethnicized support for European integration

Table 5.1 summarizes the theoretical argument. The left column summarizes the standard conceptualization of racialized attitudes, based on the argument presented in Winter (2008). The right column summarizes constituent elements of ethnicization of the issue of European integration. In the latter case, the notion of *intrusion* is the linking information connecting the group attitude and the policy issue.

Table 5.1 Structure of racialization and ethnicization.

	Racialization	**Ethnicization**
Group attitude	Racial resentment	Ethnic prejudice
In-group	White Americans	Natives
Out-group	African-Americans	Immigrants
In-group stereotype	Hard-working	Familiar, reliable
Out-group stereotype	Lazy	Foreign, threatening
Potentially implicated policy	Welfare	European integration
Analogous policy feature	Unjust rewards	Intrusion

As summarized in the right column of Table 1, by framing European integration as the intrusion of something foreign threatening an object of symbolic in-group attachment, European integration can potentially become ethnicized. This symbolic linkage does not occur at random, but as the result of a particular political calculus. Political entrepreneurs who oppose further European integration can campaign against it by strategically framing it in terms of a foreign intrusion.

This framing in turn makes the issue schematically analogous to a group relation many voters find very pertinent: that of natives and immigrants, which similarly involves an elite-driven, foreign intrusion into a familiar experience. This is likely to be a resonant frame: qualitative studies find that metaphorical representations of immigration in European political discourse are typically spatially structured around outside-inside distinctions (Charteris-Black, 2006). As a consequence, voters struggling to make sense of the complex issue of European integration are likely to adopt the emotionally resonant frame as a cognitive heuristic. Voters receptive to the frame should come to symbolically associate the European integration with the threatening intrusion of something foreign into a familiar realm.

In contrast to racialization, which is typically linked to racial resentment, I conceptualize *ethnic prejudice* as the operative intergroup attitude in ethnicization. Using the concept of ethnic prejudice resolves the awkwardness of speaking of racial attitudes in a context where intergroup relations are ethnic rather than racial. Nevertheless, racial resentment and ethnic prejudice should be understood as localized expressions of the same basic process. This is supported by the fact that the two are in important ways empirically similar: for example, both measures contain classical as well as modern, subtler components (Pettigrew and Meertens, 1995). In other words, though substantively different in important respects, ethnicization and racialization are structurally similar.

Note that the link per se between immigration attitudes and support for European integration is well-established. For example, de Vreese and Boomgaarden (2005) and Hooghe (2005) both find that measures of anti-immigration attitudes tend to strongly predict opposition to European integration. This finding is important on its own terms, yet leaves unexplained between-country variation in the strength of this association. In contrast, I theorize and test the conditioning effect of country-level informational context on exactly this relationship. This not only helps explain between-country variation in identity-based voting on European integration. It also provides a more explicit model of the underlying psychological process that links the issue with citizens' group identities.

5.2.3 Hypotheses

A key implication of the argument above is that the accessibility of the native-immigrant distinction will moderate the likelihood with which support for European integration is ethnicized. In other words, *support for European integration is more reflective of, and thus more strongly correlated with, immigration attitudes when immigration is politically salient*. This is the key hypothesis tested in this article.

This idea hews closely to previous studies showing how racialized messaging can cause vote choice to rely more on racial resentment (Tesler and Sears, 2010; Valentino et al., 2002). In these studies, respondents either experimentally or observationally exposed to racializing messages exhibit stronger correlations between racial resentment and support for the policy in question. By the same token, ethnicization should manifest itself in stronger correlations between ethnic group attitudes and policy preferences.

In the remainder of this article, I test the hypothesis in various ways. I do so in three parts. In Study 1 (section 5.3), I show that ethnic prejudice and vote choice were more strongly correlated in Denmark's euro adoption referendum, where immigration was more politically salient, compared to Sweden, where the political salience of immigration was low. In Study 2 (section 5.4), I show that voters in Denmark's referendum were also more likely to explain their vote in terms of identity. Lastly, in Study 3 (section 5.5), I show in a series of cross-national surveys in EU member countries that immigration attitudes and support for European integration are generally more strongly correlated in country-years in which immigration is politically salient.

5.3 Study 1: Ethnicized voting on euro adoption

In Study 1, I test the hypothesis using data from Denmark's 2000 referendum, where immigration was highly politically salient, ethnic prejudice predicted support for euro adoption. Needless to say, the correlation alone makes little sense in isolation. The empirical argument thus requires a reasonable baseline with which to compare the Danish case. To this end, I use Sweden's 2003 euro referendum as a control case. Before describing the data, I explain why the two cases are useful for analyzing comparatively.

5.3.1 Case: the Danish and Swedish euro referendums

By comparing survey data from Denmark's 2000 referendum and Sweden's 2003 referendum on euro adoption, the research design is effectively a *most similar systems design* (MSSD) comparative case study (Przeworski and Teune, 1970). Employing comparative case study designs using Denmark and Sweden is a widely used empirical strategy (see, e.g. Daugbjerg, 1998; Green-Pedersen and Odmalm, 2008; Iversen, 1996; Jupille and Leblang, 2007; Swenson, 1991). Studies comparing the two countries point to a host of commonalities: both are relatively small, historically homogeneous, universal welfare states with multi-party, parliamentary systems of government historically dominated by social-democratic parties.

In other studies, scholars have compared specifically the Danish and Swedish euro referendums. These studies often emphasize that not only are the cases in many ways similar, they constitute the only two known instances in which the question of abandonment of a national currency has been put to a popular vote (Hobolt, 2007; Jonung, 2004; Jupille and Leblang, 2007).

As with all country-level comparisons, the MSSD is an imperfect design (Przeworski, 2009). Ideally, the two cases would be fully identical in all respects save exactly one, which is not reasonable to assume. In other words, I cannot rule out that the observed between-country differences are caused by other country-level confounders. Yet by comparing broadly similar cases, the MSSD substantially narrows the set of country-level variables that can plausibly account for observed differences in the outcome of interest. Here, I argue for the importance of a specific difference between the two: the fact that immigration was politically salient leading up to Denmark's 2000 referendum, and virtually absent in Sweden's 2003 referendum.

This difference is borne out by contemporary, qualitative accounts of the two campaigns. For example, in his retelling of the Danish referendum campaign, Bille (2001) mentions that while economic considerations dominated the debate, "appeals from the 'no' side to the general conservative and nationalistic sentiments of voters gained ground during the campaign" (p. 287).

Campaign materials from the Danish referendum provide additional, direct evidence of messaging linking the euro with the issue of immigration. Figure 1 shows four pages from a referendum campaign booklet by the Danish People's Party. With taglines such as "Should we Danes make the decisions in Denmark?", the campaign messages frame the euro issue in a way strongly evocative of the immigration issue. Similarly, the tagline "Danish welfare is threatened when the floodgates between countries are opened" specifically evokes the metaphor of intrusion.

(a) "Keep the krone - vote Danish!"

(b) "Should we Danes make the decisions in Denmark?"

(c) "Danish welfare is theatened when the floodgates between countries are opened"

(d) "For the krone and the fatherland"

Figure 5.1 Danish People's Party flyer during the 2000 euro referendum.

In contrast, contemporary accounts of the Swedish campaign mention no appeals to Swedish voters' sense of national identity. Widfeldt (2004), in his retelling of the campaign, explains that

> "[t]he campaign centered on two main themes: economy and influence. On the former theme, the 'Yes' side claimed that the euro would have positive effects for business and employment. (...) The 'No' side argued that there is no clear relationship between economic performance and membership in the eurozone (...). On the influence/democracy theme, the 'Yes' side used the slogan 'Should we be part or stay outside?' (...). The 'No' side criticised the European Central Bank (ECB) for a lack of openness and democratic accountability (...)" (Widfeldt, 2004, 1146)

In other words, economic and political considerations dominated the Swedish campaign. The reason for the difference in campaign environments is quite straightforward: In 2003, Sweden had no established equivalent to the Danish People's Party, which largely drove the nationalistic messaging in the Danish campaign. In other words, party system dynamics produced significantly different issue environments across the two countries, with national identity being a relatively salient issue in Denmark and largely absent in Sweden.

The contrast identified in contemporary qualitative accounts is corroborated by quantitative data on the political agendas in elections surrounding each referendum. Figure 5.2 shows the share of party manifestos devoted to national identity issues based on data from the

Comparative Manifesto Project (CMP) (Volkens, 2013). CMP data codes the attention parties devote to issues by coding the number of 'quasi-sentences' across 56 categories. The figure presents the average share of quasi-sentences parties devote to the category 'Multiculturalism'. This is the CMP category to which statements about immigration are assigned, and which previous studies have used as a cross-country measure of the political salience of immigration (e.g., Sonderskov and Thomsen, 2014).

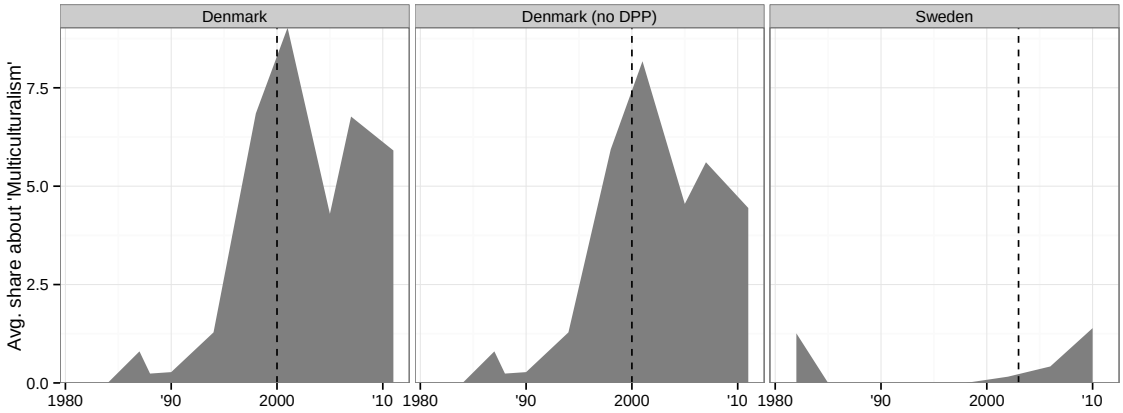

Figure 5.2 Average share of (quasi)sentences in party manifestos devoted to "multiculturalism" (categories 607 and 608) by party in Denmark and Sweden, 1981-2011. Both positive and negative sentences are included. Dashed lines show euro referendum years.

As the leftmost panel in the figure shows, immigration emerges on the party agenda in Denmark around the mid-1990's and comprises a substantial share of party manifestos since then, with a noticeable peak in 2001. In contrast, the issue is essentially absent from the Swedish agenda (rightmost panel), albeit with a slight uptick in the late 2000's, too late to affect the agenda for the 2003 referendum. The middle panel reproduces the results from Denmark, but omits manifesto data from the right-wing populist Danish People's Party. As the panel shows, the contrast between the two cases is not merely attributable to the presence of the Danish People's Party, but a feature of the broader political environment.

Explaining this difference in national agendas in turn is beyond the scope of this study, but other studies attribute the difference to differing strategic incentives for issue-competing center-right parties (Green-Pedersen and Krogstrup, 2008). In the case of the euro referendums, one specific reason deserves mention: for Sweden, adopting the euro would have entailed switching from a floating to a fixed exchange-rate regime. This policy feature itself contributed to economic concerns dominating the campaign agenda (Jupille and Leblang, 2007).

5.3.2 Data and measures

In order to test the hypotheses, I turn to nationally representative surveys conducted around the time of each country's referendum. Denmark's 2000 referendum survey, "EURO-afstemningen, 2000" (Worre and Nielsen, 2003), is a post-election survey with N=1,000. Sweden's 2003 referendum survey, "Folkomröstningsundersøkning 2003" (Holmberg et al., 2003) consists of equal-sized pre- and postelection surveys with total N=2,947.

Measuring ethnic prejudice

Tables A1 and A2 in the appendix present translations of the specific question wordings used to measure each of the key variables used in the models. Of these, ethnic prejudice is the main independent variable and a challenging concept to measure validly and reliably in survey-based research. Hence, my measurement strategy deserves some further elaboration.

Most importantly, the items are chosen so as to best approximate previously used measures of ethnic prejudice, given the constraints of data availability. Akrami et al. (2000) develop scales of classical and modern racial prejudice which (although the name suggests otherwise) are designed to elicit prejudicial attitudes toward immigrants in a Scandinavian context. The scales consider items such as "A multicultural Sweden would be good" and "I favor full integration of Swedes and immigrants" which closely mirror the items used in this study. Hence, the measurement of ethnic prejudice used in this study matches earlier approaches.

In the Danish data, ethnic prejudice is measured using a dichotomous measure of agreement with the statement "immigrants and refugees [are] a threat to Danish culture". The measure is thus fairly coarse, but closely captures the theoretical attitude of interest. A particularly attractive feature of the item is that it reflects a pure intergroup attitude rather than being a statement about the respondent's policy preference. The downside of the measure is to the coarseness introduces considerable measurement error, which will attenuate observed associations.

In the Swedish data, ethnic prejudice is based on an additive index, adding 5-point scale agreement that "[Sweden should] accept fewer refugees" with 11-point scale agreement that "[Sweden should a]im for a multicultural society with great tolerance toward people from foreign countries with different religions and ways of life". The two items exhibit acceptable reliability (α=.76). One significant advantage of adding the two is that the latter question was asked only in the pre-election survey; hence, using an average of the two items allows for using both pre- and postelection surveys, nearly doubling the available sample size (from

N=827 to N=1,513). Maximizing the number of cases is particularly important in the Swedish case, since increasing the statistical power strengthens the evidence that the true association is in fact negligible (Rainey, 2014). However, as shown in table A5 in the appendix, the results are robust to using an ethnic prejudice measure based on only the latter item.

A second concern in the Swedish case could be that the latter item, whose question wording is quite laden with social desirability bias, would leave no room for respondent disagreement. As shown in figure A1 in the appendix, though the distribution indeed is skewed toward the lower end of the scale, the measure still exhibits variation across the entire scale.

Control variables

In addition to testing the bivariate association between ethnic prejudice and vote choice, I estimate models adding a standard set of controls. This includes demographic controls for gender, age, education, and income, measured using standard survey items. In a third set of models, I add controls for *political interest*, measured using items asking respondents about rate their level of interest in matters regarding European Union on 5- and 4-point scales in Denmark and Sweden respectively.

I include these controls to ensure the observed correlation is not driven by variation in personal ressources (i.e., income, human capital and/or political knowledge) which might correlate with ethnic prejudice as well as referendum vote choice. All variables except for age are rescaled to range from 0 to 1 in order to simplify comparison across variables. Tables A.14 and A.15 in the appendix provide summary statistics for each of the variables in the two surveys.

5.3.3 Results

I regress euro referendum vote choice on ethnic prejudice in each of the two countries. In both cases, I report results from bivariate logit regressions as well as models adding controls for standard demographic variables and political interest. The full regression results are presented in tables A.16 and A.17 in the appendix. Figure 5.3 presents the key quantity of interest from these regressions, the estimated marginal effects of ethnic prejudice on vote choice. As marginal effects in logit models are non-constant, I report average marginal effects (AMEs) of ethnic prejudice for each model. Since the measure of ethnic prejudice in the Danish data is dichotomous, I dichotomize the variable in the Swedish data in order to ensure roughly comparable levels of measurement error.

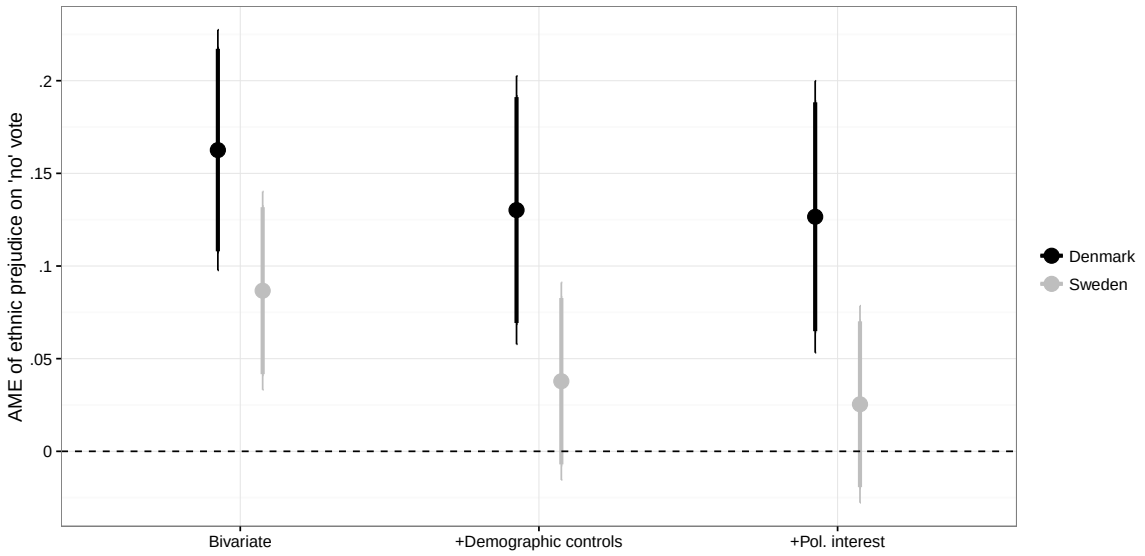

Figure 5.3 Average Marginal Effects (AMEs) for logit regressions of euro referendum vote choice on ethnic prejudice and various sets of controls, Denmark and Sweden. Thick lines represent 90 pct. confidence intervals, thin lines 95 pct. confidence intervals. The full model results are shown in tables A.16 and A.17 in the appendix.

As shown, the AME of ethnic prejudice on vote choice is positive and significant in all specifications in the Danish data. In contrast, the AMEs in the Swedish data are smaller in every case and not consistently statistically significant. The differences between AMEs are in all models statistically significant at the .1 level or less ($p = .08$, .04, and .03 respectively, two-tailed). Hence, the data suggests ethnic prejudice is in fact more strongly related to vote choice among Danish voters, consistent with the hypothesis.

5.4 Study 2: Is it really about identity? Evidence from open-ended responses

While supportive of the hypothesis, the difference in correlations observed in Study 1 comes well short of constituting conclusive evidence of ethnicized attitudes. Most importantly, the correlations may be confounded by some other attitudinal dimension correlated with ethnic prejudice rather than reflecting vote choice actually being motivated by ethnic prejudice.

In order to get a closer look at actual motivations, I therefore turn to open-ended survey responses. If the difference observed in Study 1 really reflects ethnicized attitudes in the Danish referendum, respondents' own explanations for their vote should mirror this pattern. Specifically, voters in Denmark's referendum should be significantly more likely to explain

their vote in terms of ethnic group affiliation. Study 2 tests if identity-based responses are more common among voters in Denmark's referendum.

5.4.1 Data and measures

Data comes from the same surveys described in Study 1. In order to assess whether respondents vote based on their group identity, I exploit the fact that both surveys asked voters a simple, open-ended question: *why did you vote the way you did?* Although open-ended questions were regularly fielded in the early years of social science survey research, the responses have rarely been utilized (though see e.g. Brewer and Gross, 2005).

Measuring vote motivation using open-ended survey responses

The advantage of using open-ended questions to study voter motivations is that they convey voters' spontaneous, unstructured explanations for their vote choice (Bradburn, 1983). This is an advantage of added flexibility, not necessarily of added authenticity: the open-ended nature does not imply that the responses are in any sense truer or closer to respondents' real motivations than responses to closed-ended questions. Some scholars argue that survey responses are mere rationalizations of processes hidden from the surveyer and perhaps even from the respondents' own conscious cognition (Lodge and Taber, 2013; Nisbett and Wilson, 1977). Still, the open-ended responses offer a much more diverse set of stated voter motivations relative to closed-form responses. In a research setting where the hypothesized mechanism is the adoption of the aforementioned "frames in communication" into citizens' political thinking, open-ended responses arguably constitute the most direct available measure of the "frames in thought" guiding citizen decision-making (Chong and Druckman, 2007b).

For the purpose of analyzing responses quantitatively, using open-ended responses is a double-edged sword: while each individual response can be informative about the voters' motivation, systematic comparison requires some method of categorization. Since my data consists of relatively short responses across two different languages, which would be difficult for automated methods to parse, I opt for human coding of the responses. In order to avoid confirmation bias, I hired student coders naïve to the study's hypothesis to code the responses from both surveys. The students were Danish, but sufficiently familiar with Swedish that they could understand the content of the responses.

After failing to reach acceptably high inter-coder reliability with a fine-grained overall coding scheme, I adopted a two-stage coding scheme. First, a student coder categorized the

responses according to a simple, dichotomous scheme: whether the voter appears to have voted based on their identity (coded 1) or not (coded 0). In order to assess reliability, a second coder was asked to code a randomly drawn subsample of 100 responses from each country. The coding suggested high inter-coder reliability (Krippendorf's $\alpha = .83$). In the following, I will mainly rely on this dichotomous categorization scheme. However, closer inspection of the identity-coded responses suggests that they emphasize meaningfully different dimensions of identity. Hence, as a second stage, I also had a separate set of coders classify identity-based responses according to whether they reflected specifically national identity as opposed to other dimensions (i.e., attachment to Europe or the welfare state). Applying this subtler distinction, reliability was lower, though acceptable (Krippendorf's α=.63). In Figure 5.4 in the results section below, I show that results obtained for identity-based responses are robust to relying instead on this narrower definition.

In order to ensure a focus on purely identity-based concerns, coders were instructed that ambiguous responses seemingly about identity but possibly reflecting political beliefs about delegation of power (e.g. references to "national sovereignty") should be coded as zero. This strategy likely yields a number of 'false negatives' in that some of these zero-coded responses possibly do in fact reflect identity concerns (in classification terminology, this implies prioritizing precision over recall). The strategy was selected in order to arrive at conservative estimates of levels of identity-based voting. This conservative bias should not affect observed between-country differences, but should be kept in mind when interpreting absolute levels.

The open-ended questions in the two surveys differ in one significant regard: whereas Danish voters are asked to explain their vote only once, Swedish voters are prompted an additional two times after their first response whether they can think of additional reasons for their vote. 42 percent of those giving a reason on the first prompt provide an additional reason on the second prompt. 13 percent provide an additional, third reason. In the analysis that follows, Swedish voters' responses are counted as identity-based if one or more of their responses were coded as identity-based. This inclusive coding rule thus works against the hypothesis.

5.4.2 Results

Figure 5.4 presents the results from the coding of identity-based responses, showing the estimated proportion of identity responses for each country, both according to the broader coding criterion and the narrower criterion coding only 'national identity' responses. The

figure also shows the share of identity-based responses among Swedish voters' first responses, which are presumably the most causally important. Furthermore, so as to provide a sense of the content of the responses, the figure plots a random sample of identity-coded responses for each country.

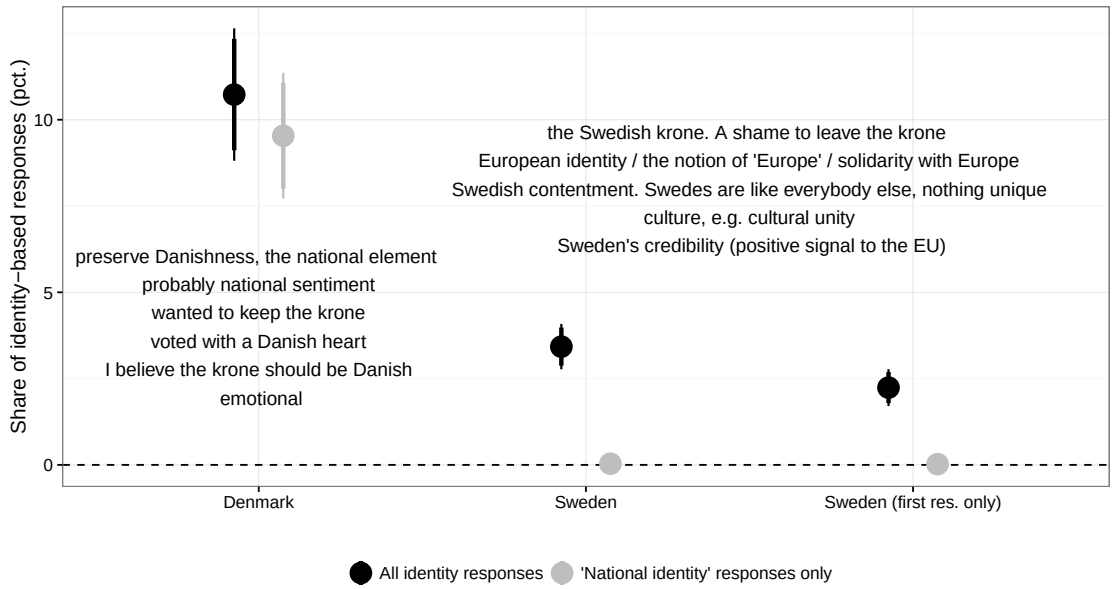

Figure 5.4 Share of identity-based stated responses (all identity responses and 'national identity' responses only) for voting in Denmark and Sweden. Thick lines represent 90 pct. confidence intervals, thin lines 95 pct. confidence intervals. Word clouds provide examples of identity-coded responses.

Besides providing evidence of the coding's face validity, the sampled responses show that some voters appeared to base their vote choice purely on concerns about national identity. For example, the response *"preserve Danishness, the national element"* expresses a clearly nationalistic sentiment. Other responses in the full data set (not shown in Figure 5.4) are even more explicit: *"no more foreigners"* or *"do not want foreign influence in Denmark"* are among the most striking examples. One respondent replies *"opening the floodgates"*, directly echoing the campaign message described in section 5.3.1 above.

Consistent with expectations, identity-based responses are far more common among Danish respondents. Given that the euro referendum concerns the ostensibly purely economic issue of joining the eurozone, a share of around 11 percent is arguably a substantial share of voters giving identity-based reasons for their vote. In contrast, the Swedish share of identity-based responses, even across all three response prompts, is a comparatively low 3 percent. Using the narrower criterion of only specifically 'national identity' responses, the

disparity is even starker. 9.5 percent of Danish responses are classified as such, whereas the same is true for only a single Swedish response (.03 percent).

Study 2 thus substantiates the expectation that the correlation between ethnic prejudice and vote choice observed in Study 1 reflects an actual psychological link. Consistent with this idea, identity-based responses are significantly more common among Danish voters. In the appendix (Tables A.18 and A.19 and Figure A.10), I provide additional validation of the identity-based responses, showing that Danish respondents who give identity-based responses are also significantly higher in ethnic prejudice.

5.5 Study 3: Cross-national evidence

As with all comparative designs, the inferential strength of Study 1 and Study 2 depend on the typical comparative case study assumption that the observed difference between Danish and Swedish voters does not reflect other, unobserved differences between the two cases. As evidence against such country-level heterogeneity, I here present cross-country evidence showing the same pattern. When immigration is politically salient, attitudes toward European integration more closely reflect attitudes toward immigration. In this data, the hypothesis implies an interaction: immigration salience should moderate the association between ethnic prejudice and support for European integration such that the association is stronger when salience is higher.

5.5.1 Data and measures

For Study 3, I rely on two data sources. For data on individuals, I rely on waves 2-4 and 6-7 of the European Social Survey (ESS). These particular waves ask both the attitudinal independent variable I need and the dependent variable. I measure the attitudinal independent variable, *ethnic prejudice*, as respondents' answer to the question "Is [country] made a worse or a better place to live by people coming to live here from other countries?", measured on a 10-point scale where higher corresponds to stronger agreement with "worse place to live". I measure the dependent variable, *opposition to EU integration*, as respondents' answer to the question "some say European unification should go further. Others say it has already gone too far. (...) What number best describes your position?", measured on a 10-point scale where higher corresponds to stronger agreement with "unification already gone too far". I code the dependent variable to measure opposition rather than support to make the interaction coefficient more easily interpretable.

The moderator, *immigration salience*, is measured using data from the Comparative Manifesto Project (CMP) (Volkens, 2013). Using CMP data, I measure immigration salience for each respondent as the attention paid by political parties to immigration in the most recent national election. As in Figure 5.2 above, I use the category 'Multiculturalism' to measure attention to immigration. This is the same approach to measuring immigration salience as in Sonderskov and Thomsen (2014). As with all CMP data, parties' political attention is measured as the proportion of manifesto quasi-sentences dedicated to each issue. I merge the CMP data with the ESS data by assigning to each respondent the immigration salience of the most recent national election relative to the survey interview date.

The immigration salience variable is strongly right-skewed: 70 percent of respondents are surveyed in a year where fewer than 2 percent of manifesto quasi-sentences cover immigration. The highest observed level is Denmark's national election in 2001, where salience is measured at 9 percent. In order to reduce the influence of outliers, I log-transform the salience variable, though the results are robust to using the untransformed measure. Figure A.12 in the appendix shows distributions of the original and logged versions of the immigration salience measure.

The ESS covers several non-EU countries, such as Israel, Russia, and Ukraine, where attitudes toward European integration likely take on a very different meaning. To keep the interpretation consistent, I restrict the sample to respondents in countries that are EU members at the time of the survey. The resulting data set covers 23 EU member countries and national elections in each year from 2001 to 2013.

5.5.2 Results

Table 5.2 presents the results. The estimates for the hypothesized interaction between immigration salience and ethnic prejudice, IS$\times$EP, are shown in the third row from the top.

Column 1 presents the basic results, controlling at the individual level only for standard demographics and (as in all models) country fixed effects. In all models, standard errors are clustered at the country-year level. Column 2 adds controls for variables ideology and political interest. Note that including ideology, which is potentially post-treatment to ethnic prejudice, may bias the estimate downwards. Column 3 adds year fixed effects. Column 4 reestimates the model in column 3 but excludes observations from Denmark and Sweden. I apply this latter restriction to show that the results are not merely a rediscovery of the pattern found for those two countries in Study 1 and Study 2.

As seen in Table 5.2, the hypothesized interaction is positive and significant in all interactions. The significant interaction implies that in country-years in the data where immigration

Table 5.2 Models predicting opposition to European integration

	Agree 'unification gone too far'			
	Basic	+political vars	+year FE	w/o DK and SE
	(1)	(2)	(3)	(4)
Imm. salience (IS)	$-.026^{***}$	$-.028^{***}$	$-.035^{***}$	$-.035^{***}$
	(.008)	(.007)	(.007)	(.009)
Ethnic prejudice (EP)	$.340^{***}$	$.331^{***}$	$.332^{***}$	$.337^{***}$
	(.013)	(.013)	(.013)	(.014)
IS × EP	$.043^{**}$	$.048^{***}$	$.054^{***}$	$.053^{***}$
	(.014)	(.013)	(.013)	(.016)
Controls				
Gender (f)	$.016^{***}$	$.012^{***}$	$.012^{***}$	$.010^{***}$
	(.002)	(.002)	(.002)	(.002)
Age	$.001^{***}$	$.001^{***}$	$.001^{***}$	$.001^{***}$
	(.0001)	(.0001)	(.0001)	(.0001)
Education (years)	$-.001^{***}$	$-.001^{***}$	$-.001^{***}$	$-.001^{***}$
	(.0001)	(.0001)	(.0001)	(.0001)
Ideology		$-.041^{***}$	$-.041^{***}$	$-.026^{*}$
		(.010)	(.010)	(.010)
Pol. interest		$.054^{***}$	$.053^{***}$	$.057^{***}$
		(.004)	(.005)	(.005)
Constant	$.366^{***}$	$.360^{***}$	$.350^{***}$	$.336^{***}$
	(.024)	(.024)	(.015)	(.016)
N	141,125	127,373	127,373	111,940
Country FE	✓	✓	✓	✓
Year FE			✓	✓
R^2	.142	.144	.149	.151
Adjusted R^2	.141	.144	.148	.150

$^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

was politically salient in the most recent national election, ethnic prejudice tends to be more strongly correlated with opposition to European integration.

In the appendix, I test the robustness of the result to alternative analytical choices. In Table A.20 I show the results using the unlogged measure of immigration salience. In Table A.21 I show the results restricting the data to observations where the most recent national election happened within the past 2 years. I do so because observations with more temporally distant national elections may be very imprecise measures of the salience of immigration at the time of the interview. In Table A.22 I show the results using standard errors clustered at the country level rather than the country-year level. Though the immigration salience measure varies by country-year, clustering by country-year may understate the level of within-country correlation and thus overstate the precision of the estimate (Arceneaux and Nickerson, 2009). Clustering at the country levels yields higher standard errors and thus a more conservative test. As shown in the appendix tables, the result is robust across all of these tests.

As an additional test of the result, Table A.23 in the appendix reports results from a 'placebo test' where the dependent variable is support for gay rights rather than European integration. The dependent variable measures respondents' disagreement with the statement that "Gays and lesbians free to live life as they wish" on a five-point scale, and is available in the same ESS waves as support for European integration. If the main result were caused by a rise in ideological constraint due to unobserved factors, we should expect these attitudes to correlate more strongly as well. Yet as seen in Table A.23, the interactions are insignificant. Since the test has high statistical power, we can be confident that the result of the placebo test is in fact negligible.

Another important consequence of the statistical power of the test is that even strongly significant results can mask substantively small effects. However, the results presented in Table 5.2 imply a substantial interaction effect. As an illustration of this, consider Figure 5.5, which shows the predicted marginal effect of anti-immigration attitude on opposition to European integration across the observed range of levels of immigration salience.

As seen in Figure 5.5, moving from minimum to maximum immigration salience in the most recent national election roughly doubles the magnitude of the association between anti-immigration attitude and opposition to European integration. Since both attitude variables are scaled from 0 to 1, the predicted values imply that at the lowest level of immigration salience, moving across the range of ethnic prejudice is associated with a 21 percentage point change in opposition to European integration, whereas at the highest level of immigration salience, the predicted change more than doubles to 45 percentage points. In sum, the results from Study 3 suggest that the pattern found in Study 1 and Study 2 generalizes to other European countries.

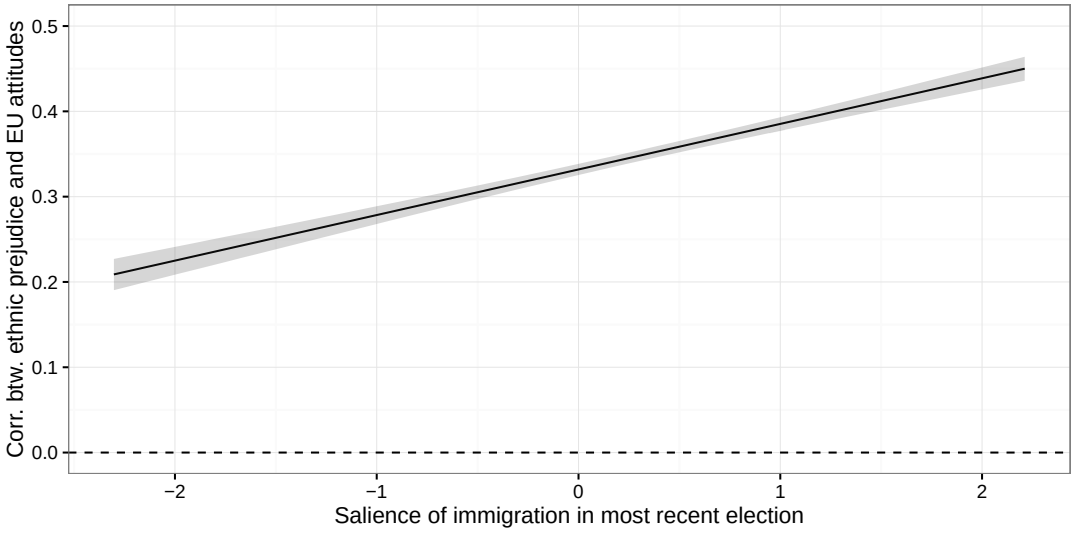

Figure 5.5 Predicted marginal effect of anti-immigration attitude on opposition to European integration across the observed range of levels of immigration salience. The prediction is based on the interaction coefficient in model 3 in Table 5.2. The grey band corresponds to a 95 percent confidence interval. Moving across the range of immigration salience roughly doubles the magnitude of the association between ethnic prejudice and opposition to European integration.

5.6 Discussion and Conclusion

As political issues go, immigration is a typical example of an 'easy' issue, symbolically laden and with no requirement of technical knowledge (Carmines and Stimson, 1980). European integration, on the other hand, is a 'hard' issue, placing high demands on the political knowledge of citizens. In this article, I have argued that this distinction has real consequences in that under the right circumstances, citizens will jump at the chance to make sense of the hard issue in terms of the easy one.

I have presented a theoretical account of how group implicating rhetoric can promote ethnicized attitudes by implicitly linking the ostensibly disparate issues of immigration and European integration. As evidence, Study 1 showed that in Denmark, where immigration dominated the political agenda, ethnic prejudice was robustly predictive of actual referendum vote choice. In Sweden, where immigration was largely absent from the agenda, ethnic prejudice did not consistently predict vote choice. Study 2 showed that consistent with the ethnicization argument, voters in the Danish referendum were also significantly more likely to give identity-based reasons for their vote choice. Lastly, Study 3 showed that the same pattern holds in a sample covering several countries and survey years: across EU member countries, ethnic prejudice is more strongly associated support European integration when immigration is politically salient.

The main caveat of this study is that both the comparative case study design and cross-sectional time-series data are imperfect bases for causal inference. The evidence presented here thus does not show conclusively that the observed attitudinal differences can be attributed specifically to differences in the political salience of the issue of immigration.

From a theoretical perspective, another important caveat is that the argument presented here presumes that the linkage between immigration and European integration is purely symbolic. In other words, it assumes that citizens evaluating European integration based on their attitude toward immigrants does not reflect an actual policy link between European integration and immigration. As migration policy is increasingly europeanized (Menz, 2011), it cannot be straightforwardly assumed that citizens link immigration and European integration only for symbolic reasons.

Future work in this area can shed light on both these unresolved questions. For example, to get a stronger claim to causal inference, survey experiments could randomly vary the implicit linkages to the issue of immigration in a message about European integration. The theory presented here would predict that stronger linkages to immigration would increase the association between ethnic prejudice and policy preference. Such a design could also probe to what extent the observed effects reflect subjects' considerations of actual policy links. The correspondent weakness of such a design is that a single manipulated frame has a weaker claim to ecological validity.

To address this shortcoming, future studies might also look to exploit naturally occurring shocks to the political salience of immigration. For example, incidents of terrorism or global migration waves might raise the political salience of immigration for reasons exogenous to the opinion dynamics of a given country. Here, too, European integration attitudes should be more strongly associated with ethnic prejudice. Both of these approaches would be valuable complements to the observational tack taken here. It is one way in which future research can enrich our understanding of the conditions under which policy attitudes come to be reflective of group identities.

Chapter 6

Tone and content in the Danish immigration debate

Abstract
In recent decades, Western European societies have seen the rise of immigration as a political issue accompanied by widespread popular anti-immigration sentiment. Many popular and scholarly accounts attribute this development to increasingly negative media coverage of immigration. But persuasive evidence of this remains scarce. Focusing on the case of Denmark, I conduct an unprecedentedly comprehensive test of this proposition by characterizing the tone and content of newspaper coverage of immigration over a time period of 25 years. I do so by analyzing the full text of a stratified sample of 68,000 newspaper articles using automated content analysis, allowing for description at greater scope and granularity than previous studies. I take a two-step approach, characterizing first trends in tone (using a supervised method), and then trends in content (using a topic model). The results indicate that contrary to several existing accounts, negativity has in fact not increased over time, but instead peaked in the mid-1990's and has declined since then. However, results do suggest increasing polarization over time, with respect to newspaper coverage of immigration as well as the issue's integration into party political conflict.

6.1 Introduction

In recent decades, immigration has changed the population makeup of most Western European societies. This societal change has pushed the issue of immigration onto the political agenda of most host societies. In short order, immigration has transitioned from a peripheral issue to

one that structures electoral cleavages and, in turn, transforms party systems. In particular, the emergence of immigration as a political issue has given rise to populist right-wing parties channelling popular anti-immigration sentiment.

But how has immigration achieved such sustained prominence as a political issue? Scholarship typically points to the role of media coverage. Many accounts, both popular and scholarly, argue that negative media coverage of immigration has fueled anti-immigrant sentiment in mass publics. Discussing the case of Denmark, Hervik (2011) exemplifies this position:

> "If you have lived in Denmark for the last decade, you can hardly fail to have noticed the development of a particularly strong and powerful 'us/them' division in the media and in the popular consciousness. There is much talk about 'we', the Danes, the hosts, who are born and raised in Denmark, represented positively in news articles and interviews, and 'the others', the guests who 'do not belong' properly and are described in negative terms (...). Not surprisingly, the media's coverage has shaped the popular consciousness (...)" (8)

The quotation above consists of two distinct claims. One is that media coverage of immigration in Denmark grew increasingly negative during the 1990's and 2000's. The other is that this negativity caused public opinion to turn correspondingly more negative. In this paper, I test the first of these two claims, revisiting in the conclusion what the findings imply for the second claim.

The issue of how to characterize mass media coverage of immigration is subject to considerable scholarly attention, including recent work on the United States (Abrajano et al., 2015) and the United Kingdom (Allen and Blinder, 2013). This is no less true of the Danish case, an often-studied empirical setting, where efforts include interpretive approaches typically based on discourse analysis (Andreassen, 2007; Hervik, 2004; Hussain et al., 1997; Yilmaz, 1999) and, less commonly, systematized hand coding of samples of articles (Green-Pedersen and Krogstrup, 2008; Jacobsen et al., 2013).

I contribute to the extant literature by characterizing media coverage of immigration in Denmark with unprecedented scope and granularity. In total, I analyze more than 68,000 newspaper articles spanning 25 years, an amount of data around two orders of magnitude greater than in those used in previous studies. My approach combines stratified sampling from a known universe of newspaper articles with two separate applications of automated content analysis, one characterizing the texts in terms of tone (ReadMe, Hopkins and King, 2010), and one characterizing the texts in terms of content and its relation to text-level covariates (the structural topic model (STM), Roberts et al., 2014).

6.1.1 Analytical strategy

The methodological contribution of this article is its application of automated content analysis to a subject previously only studied using manual content analysis. The obvious advantage of using automated content analysis is that texts can be analyzed at nearly zero marginal cost, allowing for characterizing vast amounts of text. Hence, the sampling of texts, described in section 6.2 below, is designed make optimal use of this advantage. In particular, this much larger set of texts allows for new insights into how the immigration debate has changed over time.

The advantage of greater scope and speed comes at a cost. Detecting subtle semantic distinctions is nearly impossible using automated methods and better left to closer, qualitative readings. This trade-off between breadth and depth is inescapable in any single study. Accordingly, automated and manual approaches should be seen as complementary, reciprocally informative research programmes. Automated methods cannot replace human judgment, but can augment it in important ways (Grimmer and Stewart, 2013).

This article makes use of both of supervised and unsupervised methods, the two main types of automated content analysis. Though the latter may superficially seem more atheoretical, the importance of human judgment holds equally across the two. The only distinction is that human judgment enters the analysis at an earlier stage in the case of supervised methods.

The specific analytical strategy of this article is to analyze the sample of texts about immigration in two steps. First, I use a supervised method to characterize the immigration debate across a single, theoretically important dimension, *tone*. Second I use an unsupervised method to characterize the *content* of the debate and how content depends on text-level covariates. The logic of this two-step analytical strategy is to use insights from the first (coarser) analysis to inform model specification and interpretation of results from the second (more granular) analysis. By applying two different methods, we also gain confidence that inferences are not model-dependent.

I proceed as follows. Section 6.2 explains my sampling strategy, designed to approximate simple random sampling across the full available time range. Section 6.3 describes findings from the analysis of tone. Section 6.4 describes findings from the analysis of content. Section 6.5 concludes.

6.2 Sampling articles

As discussed above, the cost of diminished interpretive depth of automated content analyses is only worthwhile if it can be offset by a considerable increase in scope. This speaks in favor

of collecting as many texts as possible. In the context of this study, the ideal set of data would be full text transcripts of all Danish media coverage of immigration.

In practice, I have to settle for somewhat less. I rely on the newspaper database *Infomedia*, which provides full texts of articles from Danish newspapers, most starting in 1991. *Infomedia* does not allow for downloading articles in bulk, only for reading articles returned from searches for specific keywords and dates.

I get around this access constraint using stratified sampling of articles. Within each month, starting in January 1991, one Monday, one Tuesday, one Wednesday, and so on is randomly selected. For each of these days, a search is run for articles about immigration in major Danish newspapers.[1,2] From each sampled day, an html file containing all the returned articles is collected. Using a customized scraping program, I collect metadata on each article saved in the html files and save the text of each article separately. The resulting data set has full text and metadata for 68,398 articles. Figure 6.1 shows the number of articles sampled per month.

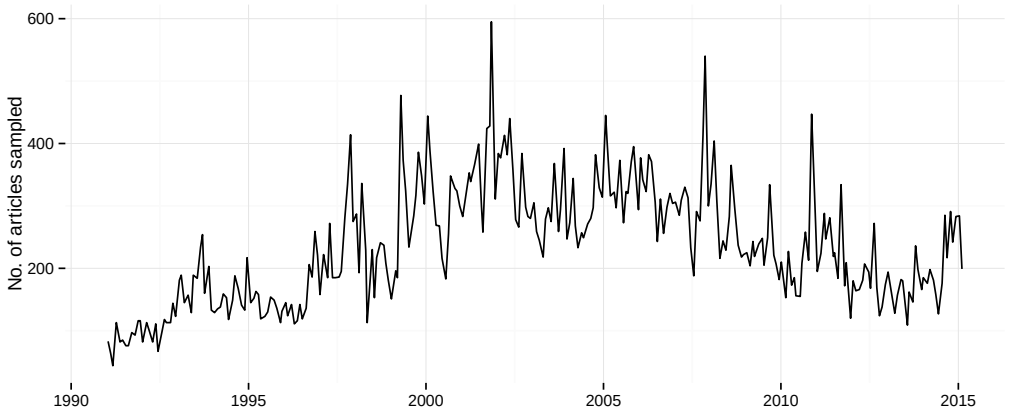

Figure 6.1 Number of articles about immigration sampled by month.

As can be seen in Figure 6.1, the number of articles about immigration varies considerably over time: in March 1991, 44 articles fit the criterion, whereas in November 2001, 595 articles do. Readers with casual knowledge of Danish politics will likely find that this pattern fits reasonably with their intuitions about the salience of the immigration issue over recent years: for example, November 2001 was the month of a national election with the issue of immigration topping the political agenda.

[1]The search uses the terms `indvandr*, flygtn*, immigra*, asyl*, udlænd*`, corresponding to typical terms for immigration, refugees, asylum, and foreigners. The search is restricted to major newspapers *B.T.*, *Berlingske Tidende*, *Ekstra Bladet*, *Information*, *Jyllands-Posten*, *Kristeligt Dagblad*, and *Politiken*.

[2]I thank student Thomas Seremet for carrying out the manual part of the data retrieval as part of his Master's thesis work (Seremet, 2016). Thomas also conducted the hand-coding of articles, cf. section 6.3 below.

In other words, the number of articles sampled over time appears to be a rough measure of the media salience of immigration. As a way of testing this idea formally, Figure 6.2 plots the number of sampled articles per month against an index of segments about immigration in national radio news. The data on radio news segments come from the Danish Policy Agendas Project.[3] The two correlate strongly ($r = .7, p < .01$), suggesting that the sampled articles reasonably accurately reflect the immigration issue's prominence in the broader media environment.

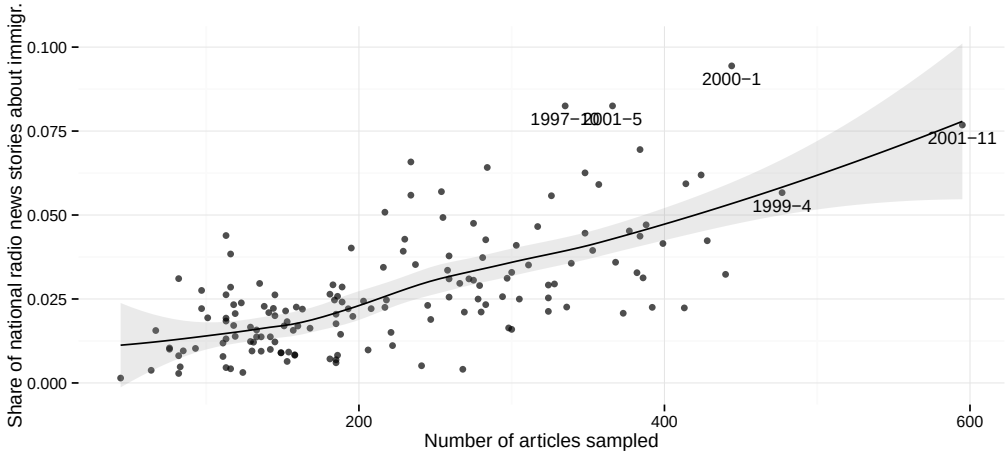

Figure 6.2 Number of articles about immigration sampled by month plotted against share of immigration stories in national radio news.

I use this set of sampled texts as the data material for both of the following analyses. Before analyzing the texts, I clean the articles using standard preprocessing steps, i.e. stemming and removing very common, uninformative words (stopwords), punctuation, and numbers, and transforming Danish characters æ, ø, and å.

6.3 Characterizing tone: Insights from ReadMe

The focus of the first analysis is how the tone, or valence, of the immigration debate has changed over time. As discussed in the introduction, most existing accounts characterize the trend as one of increasing negativity over time.

The approach I use falls under a set of approaches typically referred to as 'supervised learning' (Grimmer and Stewart, 2013). Supervised learning approaches can characterize a

[3]The data in the Danish Policy Agenda Project have been collected by Christoffer Green-Pedersen and Peter Bjerre Mortensen with support from the Danish Social Science Research Council and the Research Foundation at Aarhus University. For further details see www.agendasetting.dk.

large set of texts using information drawn from a (much) smaller set of human-coded texts. In the typical supervised learning process, a researcher hand codes a set of texts, called the 'training set'. The remaining set of uncoded texts is called the 'test set'. The researcher then performs hold-out validation of the method of choice by treating a subset of the training set as unknown and assessing how well the method reproduces the known codings in this subset. If it does so to the researcher's satisfaction, the method is 'let loose' on the full test set.

The canonical example of a supervised learning approach is spam filters in online email clients: once a classifier is trained to recognize the lexical features distinguishing spam from non-spam, it can rapidly and automatically sort spam messages out of the inbox. Crucially, by training a classifier on known cases of spam rather than using more primitive dictionary approaches (i.e., sorting based on specific, pre-defined keywords), the supervised learning approach makes use of all of the textual information available. The supervised learning approach can potentially 'see' distinguishing lexical features that are invisible to the naked human eye.

The specific supervised method I use here, called *ReadMe*, is introduced thoroughly elsewhere (Hopkins et al., 2010; Hopkins and King, 2010). Though not yet widely used, prominent existing applications of ReadMe include King et al. (2013, 2014), who use it to estimate the content of blog posts censored by the Chinese government, and Grimmer et al. (2012), who use it to estimate the proportion of credit-claiming messages by U.S. House members. The key distinguishing feature of ReadMe is that it estimates the distribution of content categories across a set of texts directly, rather than classifying each text individually and aggregating up from there. In this analysis, this feature allows us to directly characterize the distribution of articles about immigration in terms of their tone.

I begin by assigning a student assistant to hand-code articles according to the following 4-category scheme:

1. Positive towards immigration
2. Neutral towards immigration
3. Negative towards immigration
4. Not about immigration

The fourth category is necessary because some articles included in the sample are in fact not relevant to the domestic immigration debate (for example, an article about refugee flows to Australia would be coded 4). The presence of this category also means that articles cannot meaningfully be arrayed along a single dimension, so methods based on unidimensional

scaling (e.g. Wordscores or Wordfish) would likely perform poorly (Hjorth et al., 2015). The categorization scheme is quite crude, and indeed a more fine-grained scheme would have been preferable. However, more fine-grained schemes performed poorly in intercoder reliability tests. In contrast, the intercoder reliability with this coarser scheme is acceptable. Lastly, the coding scheme resembles the schemes used in similar, earlier studies (Green-Pedersen and Krogstrup, 2008; Jacobsen et al., 2013).

It is important to note that the articles were coded in terms of their negativity or positivity with respect to immigration, and not their tonal negativity or positivity per se. Hence, an article that is sharply critical of the Danish People's Party's restrictive stance on immigration is negative in terms of its overall tone, but is coded as positive towards immigration. Similarly, an article that reports on higher unemployment among immigrants may be neutral in its tone, but will be coded as negative towards immigration. While this may make the classification of articles slightly less reliable, I believe this is the more theoretically interesting distinction. The appendix provides excerpts from examples of articles coded respectively positive, neutral, and negative.

By default, ReadMe provides just an estimate of the distribution over categories in the full sample of texts. In itself, this provides little new information, since this distribution is reasonably approximated in the hand-coded sample. In order to get more empirically interesting results, I therefore run ReadMe on various subsets of the data in order to characterize relative shifts in tone.

6.3.1 Trends in tone over time

First, I estimate the distribution of frames over time by running ReadMe on 290 different subsets of the data, once for each month from January 1991 to February 2015. The resulting estimates are shown in Figure 6.3.

Three trends are discernible from the over-time results. First, negative coverage peaks around the late 1990's and early 2000's, and wanes from then on. Second, positive coverage appears to be weakly increasing, at least until the late 2000's. Third, the proportion of neutral coverage is weakly decreasing over time.

The overall balance of frames can be hard to assess from Figure 6.3. As a way of showing this more clearly, Figure 6.4 shows *net negativity*, i.e. the balance of negative vs. positive articles in each month. As shown, negative articles outweigh positive ones until the early 2000's, after which positive articles dominate slightly. In recent years, the overall balance is roughly neutral.

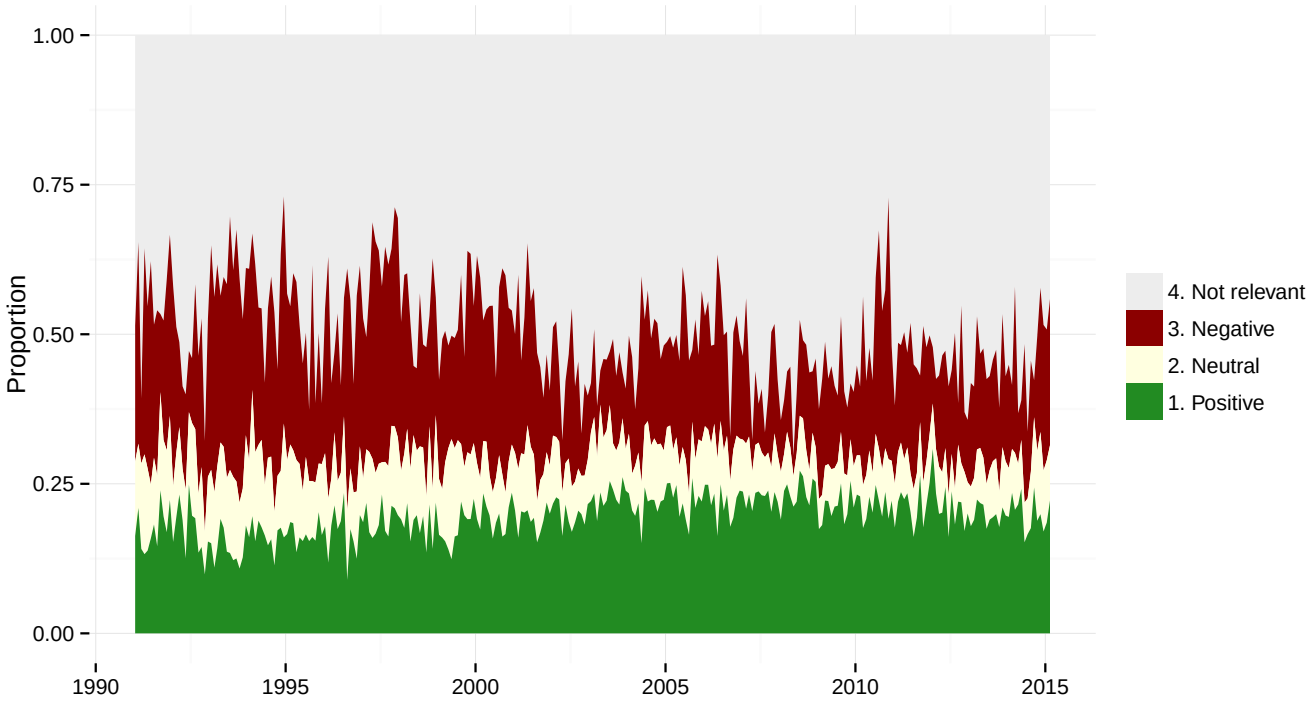

Figure 6.3 Estimated proportions of immigration coverage categories over time. The ordering of the legend corresponds with the ordering of the areas.

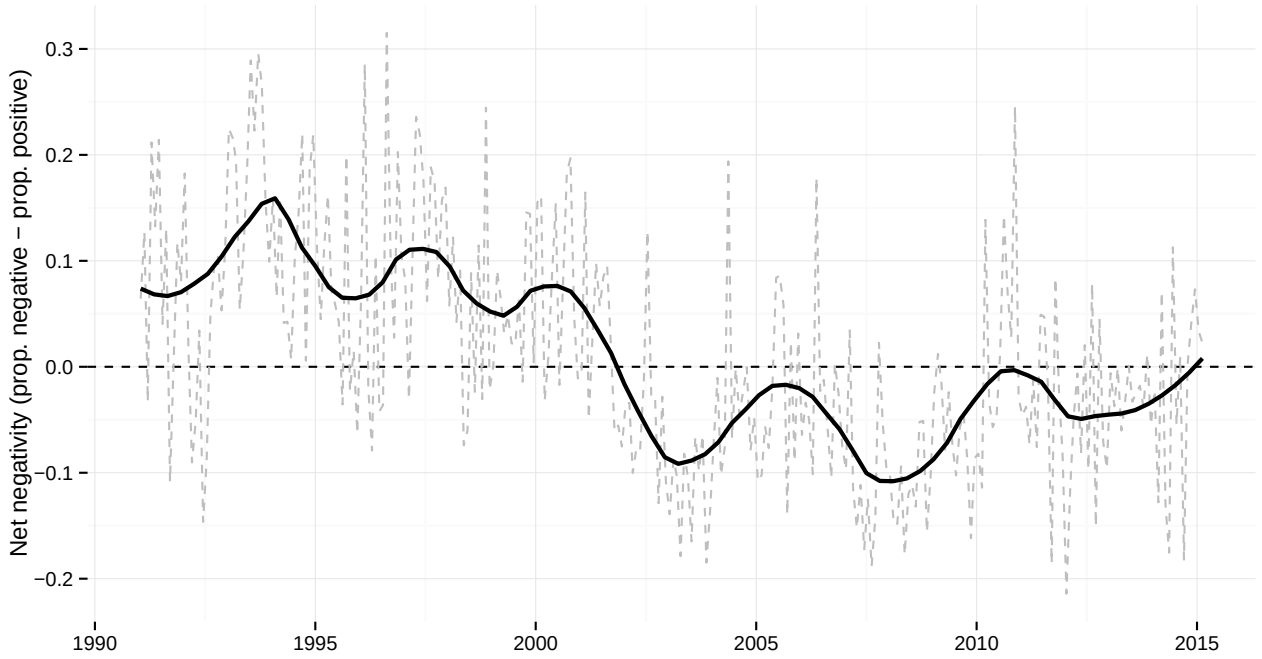

Figure 6.4 Estimated net negativity of immigration coverage over time.

6.3.2 Trends in tone in specific newspapers

Another important source of variation in the data is that between newspapers. I do not have enough data to get monthly estimates for each major newspaper, so Figure 6.5 presents net negativity by newspaper and year.

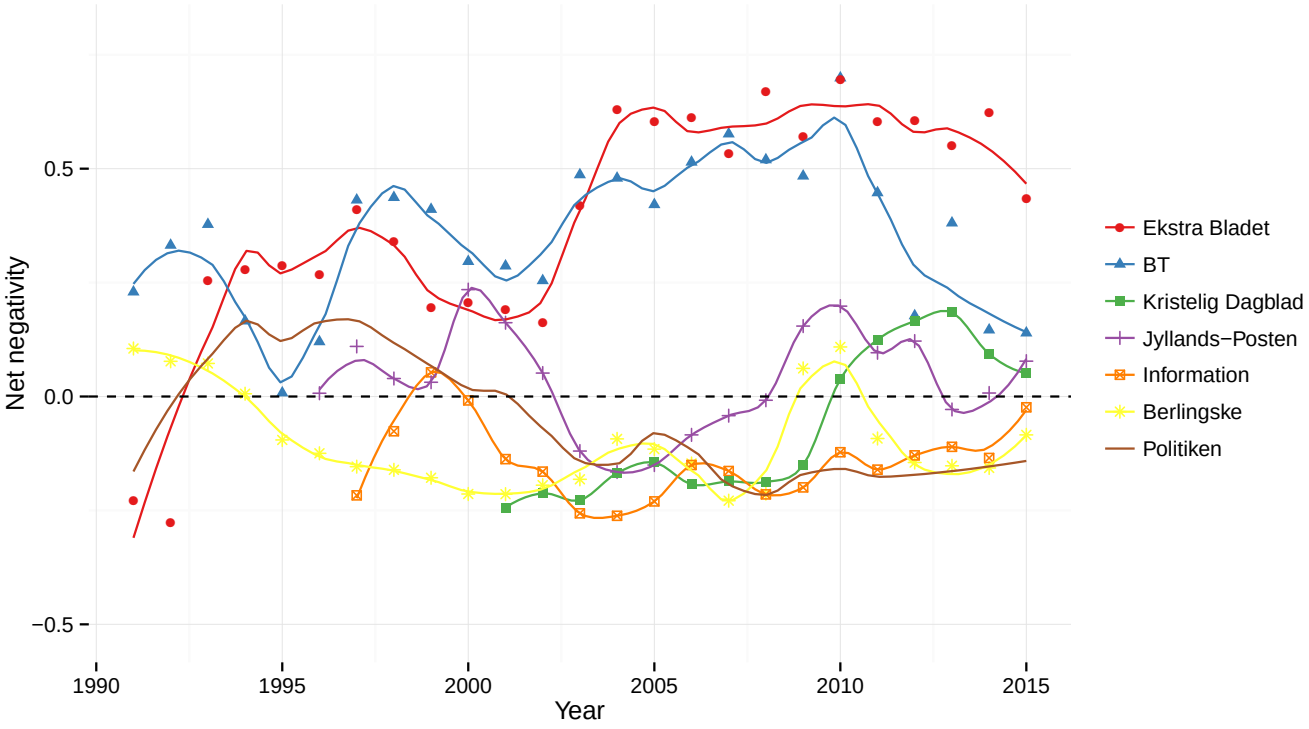

Figure 6.5 Estimated net negativity of immigration coverage over time, by newspaper. The ordering of the legend corresponds to the ordering of the lines at the right end of the graph.

The figure provides an important corrective to the results above. Although aggregate negativity does decrease in the early 2000's, this overall trend masks a clear trend of increasing polarization between newspapers. In the years around year 2000, the two major Danish tabloid newspapers, *Ekstra Bladet* and *B.T.*, turn sharply more negative. In fact, in the mid-to-late 2000's, net negativity is at .5, implying that these two newspapers average around 50 percentage points more negative articles than positive ones. Since there are four categories in total, this large difference puts a very low upper bound on the share of positive articles. In fact, in the late 2000's, the share of positive articles in *Ekstra Bladet* is estimated at around just 7 percent.

For the broadsheet newspapers, the trend is in the opposite direction, though weaker. The major center-left newspaper *Politiken* is a case in point. In the early 1990's, *Politiken*'s

negativity is on par with that of *Ekstra Bladet*. Yet from the mid-1990's onwards, *Politiken* turns distinctly more positive, ending up as the most positive major newspaper in 2015.

In sum, two patterns stand out from the analysis of tone. First, contrary to received wisdom, the evidence suggests that immigration coverage has in fact grown less negative in recent years, peaking in negativity in the late 1990's. Second, there is evidence of polarization of coverage of immigration such that over time, tabloid newspapers become distinctly more negative compared to broadsheet, though this difference appears to have tapered off in recent years.

I now turn to the specific topical content of newspapers.

6.4 Characterizing content: Insights from a structural topic model

The differences in negativity between tabloid and broadsheet newspapers naturally raises the question: what is it about the coverage of immigration in tabloids that makes it distinctly more negative? Answering this question requires looking into the content, rather than the tone, of immigration media coverage. To do so, I turn to a structural topic model (STM) (Roberts et al., 2014).

The STM belongs to a broader class of unsupervised methods labeled 'topic models', all of which are designed to estimate the topic structure in a set of texts without researcher-imposed constraints on the topical content. The only substantive constraint imposed by the researcher is on the number of topics to estimate. One might heuristically think of topic models as analogous to factor analysis in survey research. As factor analysis tells us which variables covary across individuals, topic models tell us which words co-occur across texts. And like in factor analysis, using topic models requires some measure of post-hoc interpretation by the researcher to make sense of the results.

The distinguishing feature of the STM is that it allows for adding covariates for topics, allowing us to characterize how topic prevalence is associated with features that vary across texts. Here, I focus on the two characteristics that predicted negativity in the preceding analysis: time trend and whether the newspaper is a tabloid. To do so, I first fit an STM for 10 topics. The resulting topics are shown in Table 6.1.

For each of the 10 topics in Table 6.1, the column shows the words with the highest FREX score, a measure that summarizes how much a given word uniquely predicts a topic. As shown, the results are characterized by a high level of semantic coherence. To illustrate the

Table 6.1 Summary of extracted topics

	Label	Highest FREX score words
1	Refugees	asylansoeg, flygtning, asyl, opholdstillad, asylsoeg, flygtningehjaelp, kor
2	Stateless Scandal	roen, praest, regl, birth, sag, minist, hornbech
3	Schools	skol, foraeldr, laer, elev, samfund, kultur, etnisk
4	Russia/Balkans	nato, soldat, rusland, russisk, serbisk, militaer, serb
5	Arts/Sports	film, klub, musik, sang, fodbold, spil, skuespil
6	Party Politics	fogh, nyrup, socialdemokrat, statsminist, parti, socialdemokrati, venstr
7	Crime	politi, anhold, betjent, faengsel, faengsl, overfald, mand
8	Economic Integration	kommun, virksom, arbejdskraft, arbejdsmarked, pct, job, mia
9	World Politics	tysk, frankr, europa, kina, fransk, kinesisk, europaeisk
10	Middle East	israel, palaestinensisk, israelsk, palaestinens, islam, muslim, gaza

Note: Column 2 shows the words for each topic assigned the highest FREX score by the structural topic model. The FREX (frequency-exclusivity) score for a given word is the harmonic mean of the probability of appearance under a topic and its exclusivity to that topic, and summarizes which words are most uniquely predictive of that topic. Column 1 shows manually added summary labels of each topic.

coherence of the results for non-Danish speakers, the words for topic 7 are *police, arrest, officer, prison, imprison, assault,* and *man*, a topic clearly reflecting coverage of crime and law enforcement. Similarly clearly, the words for topic 8 are *municipality, business, labor, labor market, percent, job*, and *billion*, reflecting economic integration of immigrants. One slightly more ambiguous topic is topic 3, whose highest FREX score words are *school, parents, teach, pupil, society, culture*, and *ethnic*. This topic is labeled 'Schools' and clearly partly reflects coverage related to education, but may also more broadly capture coverage of immigrant's cultural integration into Danish society. Overall, then, the STM appears to successfully capture semantically coherent and intuitively meaningful topics.

6.4.1 Content differences of tabloids vs. broadsheets

First, informed by the tabloid-broadsheet gap apparent in Figure 6.5, I estimate how the prevalence of each topic depends on newspaper type. Note that although the presentation of results here resembles that found in studies eliciting causal effect estimates from experimental or observational data, these coefficients cannot meaningfully be thought of as the 'effect' of newspaper type on content. Instead, they show which topics distinguish tabloid from broadsheet coverage descriptively speaking. Figure 6.6 shows tabloids' relative topic prevalence for each topic.

Five topics stand out in Figure 6.6 as having distinctively high or low prevalence in tabloid newspapers. The topic with the highest relative prevalence for newspapers is crime (topic 7), an intuitive difference given the earlier finding that tabloids are more negative

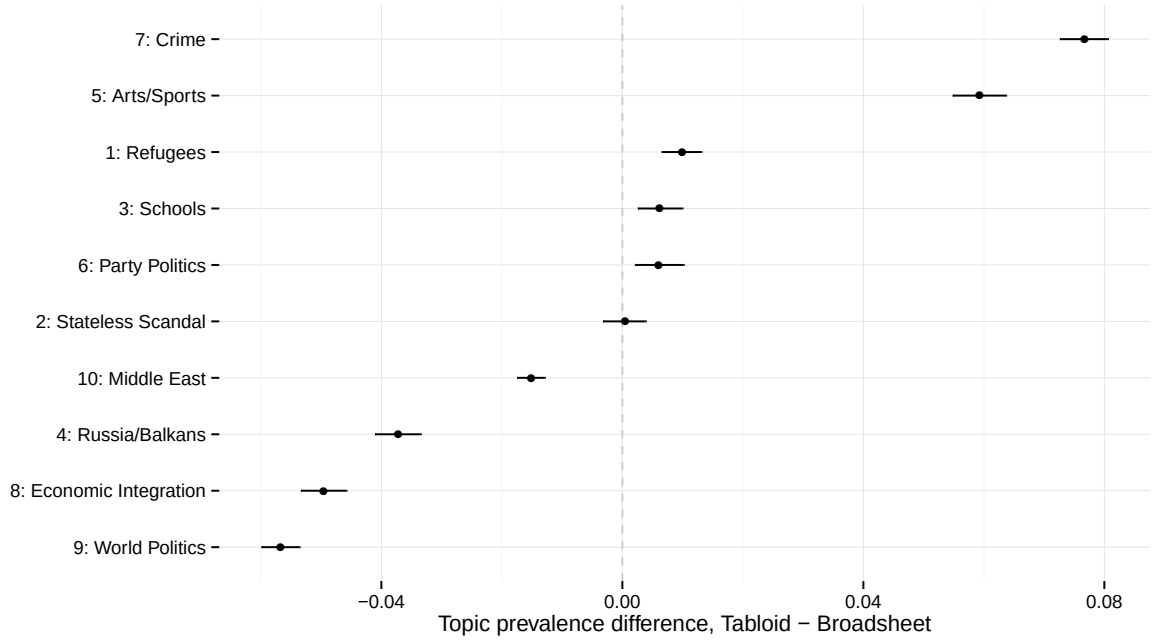

Figure 6.6 Differences in topic prevalence for tabloids vs. broadsheets for each of the 10 topics in the STM. The immigration coverage topic which tabloids place the highest relative emphasis on is crime (topic 7), which is also the only unambiguously negative one. In contrast, immigrants' economic integration into Danish society (topic 8) is predominantly covered by broadsheets. Error bars represent 95 pct. confidence intervals.

on average combined with the fact that crime is arguably the only unambiguously negative topic identified. If a newspaper predominantly covers immigration in the context of criminal incidents involving immigrants, I can reasonably infer that its overall portrayal of immigration is a negative one. The other high-prevalence topic for tabloids is arts and sports (topic 5), a topic capturing 'false positive' stories which mention foreigners and were thus captured by the search string, but are unlikely to have any bearing on immigration as a political issue.

The three topics with distinctively low prevalence in tabloid coverage tell a different, but complementary story. Two of them are world politics (topic 9) and Russia/Balkans (topic 4), topics related to international politics. Most of this coverage is likely irrelevant to the domestic political debate over immigration, though some may link foreign conflicts with ensuing domestic refugee flows. In contrast, coverage of immigrants' economic integration (topic 8) is of apparent relevance to the domestic immigration debate. Economic integration is significantly more relatively prevalent in broadsheet coverage. This may be because, in contrast to coverage of crime, coverage of economic integration lends itself most easily to 'thematic' framing (which focuses on large-scale social trends) rather than 'episodic' (which focuses on singular events and individuals) (Iyengar, 1990).

6.4.2 Trends in content over time

In addition to newspaper type, the STM models topic prevalence as a function of time. How each topic changes over time is unknowable ex ante, so in order to avoid imposing a specific functional form to the trend, the time covariate is entered as a spline. Figure 6.7 plots the results from the model, which hence allows the prevalence of each topic to vary smoothly over time.

Each of the topic trends in Figure 6.7 appear to roughly match intuitions about when triggering events would make each topic salient. For example, the local modes of topic 4, which reflects conflicts in Russia and the Balkans, coincide with the wars in Bosnia (in the early 1990's), Kosovo and Chechnya (around 1999), and Crimea (in 2014). However, although this lends some face validity to the results, confirmation bias can easily lead to overfitting curve peaks and troughs to historical events. Hence, I will focus here only on clear, long-term patterns, which I can be more confident reflect real patterns in the data.

Of these, two of the clearest over-time trends in topic prevalence have a direct bearing on the focus of this paper. For one, coverage of crime exhibits a clear downward-sloping trend throughout most of the period, indicating that coverage of immigration in the context of crime has become less prevalent since peaking around 1993. Specifically, from a peak of around 15 percent of immigration coverage, crime is estimated to account for around 7 percent of immigration coverage in 2015. Seeing as crime is the only unambiguously negative topic, this lends further support to the notion that immigration coverage has not become more negative over time.

The other relevant trend is for coverage of party politics (topic 6), which reflects party political conflict over immigration. The trend line has a local mode in 2001, most likely capturing coverage of the 2001 general election, in which immigration topped the political agenda (Andersen et al., 2003). Yet even more strikingly, the prevalence of the party politics topic increases over time, such that from a starting level of around 5 percent the topic most recently accounts for around 14 percent of coverage. The rise in the party politics topic is consistent with the contemporaneous emergence and consolidation of a party system cleavage largely structured around the issue of immigration (Stubager, 2010). At the same time, the trend supports the notion that while the immigration debate has not grown more negative over time, it has become more politically polarized.

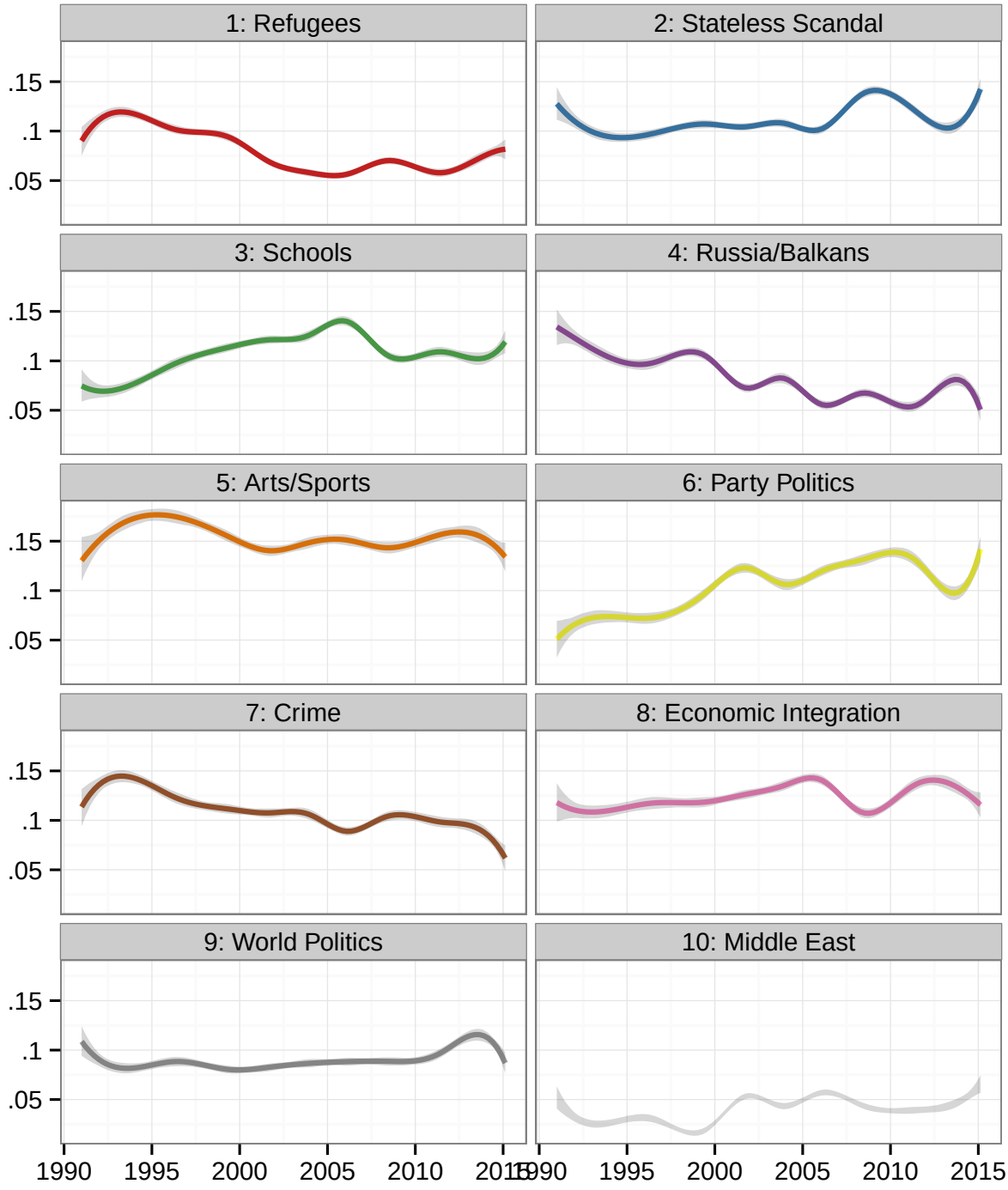

Figure 6.7 Topic prevalence over time for each of the topics presented in Table 6.1. Time is entered into the model as a spline, so as not to impose a particular functional form. Error bands represent 95 pct. confidence intervals.

6.5 Conclusion

In this paper, I have argued that automated content analysis combined with stratified sampling of texts can provide a method for characterizing trends in media coverage over time. I have done so using the case of the Danish debate on immigration, a topic already subject to scholarly attention. Drawing on a time-stratified sample of 68,398 newspaper articles from 1991 to 2015, I characterized the changing composition of positive, neutral, and negative portrayals of immigration.

Two results stand out. First, looking at tone, negativity has not increased over time, but rather reached a peak in the late 1990's and has receded since then. Second, there is a clear trend of polarization since the late 1990's with tabloid newspapers becoming clearly more negative and broadsheets slightly more positive. Results from the analysis of content largely corroborate these two findings. Specifically, coverage of crime, the only unambiguously negative topic, has declined steadily since the mid-1990s'. Furthermore, coverage of party political conflict, which most likely partly reflect increased political polarization, has been on the rise.

The target of inference of this paper has been 'merely descriptive' (Gerring, 2012). The descriptive nature of the findings is perhaps the study's main limitation. Descriptive trends do not translate easily into causal inference, and care is especially called for when inferring causation from parallel trends in theoretically related phenomena, tempting as it may be.

However, although the findings presented here do thus not readily translate into conclusions about causes and effects, they may provide grounds for skepticism about previously conjectured cause-effect relationships. Specifically, previous accounts of immigration media coverage in Denmark have argued that increasingly negative coverage has laid the ground for correspondingly negative immigration attitudes in the public at large (Andreassen, 2007; Hervik, 2004, 2011; Hussain et al., 1997; Yilmaz, 1999). The findings presented here indicate that the premise of this argument, i.e. the trend towards increasingly negative immigration coverage, is inaccurate.

If this is indeed the case, one of two possible conclusions must follow. It may be the case that immigration attitudes are indeed driven by media coverage, but that attitudes have not actually become more negative over time. Indeed, research focusing on public opinion on immigration tends to find attitude stability (Gaasholt and Togeby, 1995; Togeby, 1998). Alternatively, if in contrast to these latter findings attitudes have indeed become more negative over time, they must have done so in response to informational sources other than those considered here. One of those sources could be citizens' everyday exposure to immigration,

which previous studies indicate can diminish social trust (Dinesen and Sonderskov, 2015) and promote stereotypes (see 'Local contexts'). Regardless of the merits of each of these specific explanations, this study's approach to sampling and measurement of media content may inform future tests of theories of media dynamics in other substantive domains.

Chapter 7

Group-centric policy attitudes and the role of local contexts

Abstract
Several studies provide evidence of group-centric policy attitudes, where policies are evaluated based on linkages with visible social groups. The existing literature generally points to the role of media imagery, rhetoric, and prominent political sponsors in driving group-centric attitudes. I propose a theoretically novel source: exposure to rising ethnic diversity in the local context. Focusing on the issue of crime, I first theorize how casual observation in the local context can give rise to ethnic stereotypes. Then, using two large, nationally representative data sets on citizen group and policy attitudes linked with registry data on local ethnic diversity, each spanning 20 years, I show that crime attitudes become more strongly linked with immigration attitudes as local ethnic diversity rises. The results suggest that the typically emphasized 'top-down' influence on group-centric attitudes by elite actors is complemented by 'bottom-up' local processes of experiential learning about group-policy linkages.

7.1 Introduction

In contemporary societies characterized by racial or ethnic divisions, public opinion about specific issues is often group-centric, i.e. citizens' attitudes about those issues are shaped by their feelings toward racial/ethnic groups. A prominent example of group-centrism in public opinion is the racialized nature of public opinion in the United States on issues such as welfare, health care, or crime (Gilens, 1996; Hurwitz and Peffley, 2005; Sears et al., 2000). Less commonly, studies have demonstrated group-centrism with respect to ethnic

or gender identities (Winter, 2006b). By aligning political positions with group identities, group-centrism can create or dismantle powerful political coalitions (Alesina and Glaeser, 2006). Hence, clarifying the conditions that promote or inhibit group-centric attitudes is an important task in the study of politics.

The concept of group-centric attitudes is nearly as old as the modern study of public opinion itself. Observing that the two policy issues where average voters show the highest levels of ideological constraint both have to do with race relations, Converse (1964) remarked that "[i]t seems more than coincidence that [this is] the only pair of items involving the fortunes of a visible population grouping". Converse later suggested that what sets these issues apart is the presence of "linking information" connecting particular policies and groups.

Building on this reasoning, a large body of scholarship in political science is devoted to the question of where this linking information comes from. Simply put, how do citizens learn to think about policies in terms of group identities? The bulk of the existing literature singles out one source in particular: mass communications. Either through news coverage in mass media or strategically deployed rhetoric by political elites, mass communications contribute to group-centric public opinion. Whether focused on news media or political figures, these accounts are 'elite-centric' in that they identify linking information as something transmitted to citizens by elite actors.

I argue that this account, though correct on its own terms, is too narrow. In addition to elite-driven information, citizens receive and process linking information from casual observation in the local context. Citizens take cues from observable group distinctions in the local environment and use them to infer about group-policy linkages. If a given policy appears to map onto stereotypes about a contextually salient out-group, citizens evaluate the policy based on their feelings about that group. Hence, casual observation can by itself engender group-centric attitudes toward public policies. This implies that 'top-down' influence on citizens' attitudes by elite actors is complemented by 'bottom-up' local processes.

I contribute to the literature on group-centric policy attitudes by providing a theory of how casual observation can promote group-centric cognition about political issues. This not only helps provide a more complete account of what linking information can be. It is also a theory more in line with classical and widely accepted social-psychological theories of stereotyping and social categorization.

I also contribute with a comprehensive empirical test of the role of casual observation, focusing on the issue of crime in an ethnically diversifying, modern welfare state. The evidence comes from two large sets of surveys of citizens in Denmark, each spanning around 20 years, linked with detailed registry data on local contexts. The data show that since the

mid-1980's, Danish citizens' attitudes about crime and immigration have become more closely linked in response to local increases in ethnic diversity. Since the change is in response to local increases only, it cannot be due to national-level communication from political elites. Taken together, the theory and evidence suggest that accounts placing responsibility for the dissemination of stereotypes solely with elite actors are incomplete.

7.2 Theory

Here I describe and exemplify the elite-centric approach predominant in the extant literature, including its role in explaining stereotypes about crime. Following that, I develop a theoretical account, drawing on ideas from social psychology, of how casual observation promotes group-centric attitudes.

7.2.1 Elite-centric approaches

From its onset, the study of public opinion has been guided by the assumption that political attitudes are shaped by the impersonal influence of mass communications (Lippmann, 1922). For example, while discussing the limited amount of personal contact with racial outgroups experienced by the average white American, Sigelman and Welch (1993) reason that "[l]acking such firsthand information, whites must base their responses on whatever other information they may have at their disposal. Given the tendency of media coverage to focus on cases of intense, dramatic conflict, the secondhand information whites have about blacks is apt to accentuate the negative".

A key line of reasoning in the media-centric approach, then, is that the informational gap left by limited personal contact with outgroups could only be filled by media imagery. The argument is not exclusive to discussions of racial intergroup contact in an American context. In a study of immigration attitudes in Denmark, the empirical setting of this study, Gaasholt and Togeby (1995) reason that since beliefs about immigrants "[are] only to a very limited extent based on their own experiences (...) these beliefs must arise from elsewhere. (...) At the end of the day, beliefs and attitudes probably come from television or newspapers".

Empirical studies falling under the elite-centric approach tend to rely on one of two designs (or in rare cases both). One consists of observational designs in the form of content analyses of media representations of minorities. These observational studies have tended to find that racial/ethnic minorities, make up a disproportionate share of news media portrayals of recipients of welfare (Gilens, 2000) or criminal offenders (Dixon and Linz, 2000). The

theoretical linchpin of these studies is the idea that news stories represent manifestations of stable semantic structures—labelled 'media packages', 'scripts', 'frames', or 'discourses'—which guide and constrain public stereotypes about the target groups of public policies (Entman and Rojecki, 2000).

The other type of design within the media-centric approach is experimental. Resting on theoretical ground similar to that of the observational studies, these studies experimentally vary the presence of 'linking information' connecting group identity to issues such as crime (Peffley et al., 1997), Social Security (Winter, 2006a), or health care (Tesler, 2012). Relative to observational studies, experimental studies have focused less on news media and more on the strategic deployment of racial cues by political elites, testing the effects of cues inserted into political rhetoric. Other studies in this vein argue that even salient background characteristics of prominent political sponsors can by themselves promote group-centric attitudes (Tesler, 2012; Tesler and Sears, 2010). Altogether, these studies tend to find that political rhetoric 'playing the race card' is effective at priming group-based antagonisms (Hurwitz and Peffley, 2005; Mendelberg, 2001; Nelson and Kinder, 1996a; Tesler, 2014).

7.2.2 Elite-centrism and criminal stereotypes

In this study, I focus on the issue of crime, one on which public opinion scholarship is in many ways emblematic of the elite-centric approach. For example, in a canonical study of racialized crime attitudes, Hurwitz and Peffley (1997) conclude that "these tragic associations [between race and crime] have permeated the public consciousness in some way. This conflation is doubtless exacerbated by the critical role of the mass media". Similarly, Sides and Citrin (2007) argue that "attitudes towards immigrants have become increasingly divorced from social reality (...) people's perceptions of immigration and immigrants come to rely more on vivid events (...) and messages from politicians and media". Other accounts focus on the content of media coverage, arguing that an "ethnic blame discourse" shapes mass beliefs about minority over-representation in criminal behavior (Dixon and Linz, 2000; Romer et al., 1998).

In two relevant aspects, the issue of crime is distinct from other issues. First of all, public opinion about crime is group-centric to an unusually explicit extent. The association of racial/ethnic minorities with criminal behavior is perhaps the most broadly held stereotype about minorities in Western societies. In the United States, racial ideology shapes white Americans' policy attitudes about crime (Peffley et al., 1997). Similarly, Europeans associate immigration with higher levels of crime, expressing widespread agreement that "crime

problems are made worse by people coming to live here from other countries" (Ceobanu, 2010; Hainmueller and Hiscox, 2007). In fact, no other proposed consequence of immigration, negative or positive, is as widely agreed upon among citizens of European countries (Sides and Citrin, 2007). For my purposes, the unusually explicit group-centrism on the issue of crime is analytically useful. Since racial/ethnic stereotyping is so evidently a feature of contemporary attitudes about crime, I can set aside the issue of whether this is the case and focus on the question of how this stereotyping is learned.

Second, crime differs from some other issues of public policy is that it is spatially manifested. Contrary to pure public goods such as national security or climate change, the social costs of most types of crime are highly localized. As a result, citizens are likely to be attuned to local contextual cues about the level and nature of criminal activity. This implies that the issue of crime is, if anything, a 'most-likely' case for the role of casual observation. I return to the implications of this in the concluding section. Now, I present my theoretical argument of how casual observation in the local context can promote group-centric attitudes.

7.2.3 The role of casual observation in stereotype formation

I argue that elite-centric approaches overlook the role of casual observation in the formation of group-centric policy attitudes. In doing so, I draw on the literature on context effects, specifically that part which focuses on individuals' subjective experiences of their local environment (Dinesen and Sonderskov, 2015; Wong et al., 2012). Casual observation is thus not understood as learning from social networks (Huckfeldt and Sprague, 1987) or national-level political discourse (Quillian, 1995), but from mundane, everyday experience. Akin to what Baybeck and McClurg (2005) call "the slow drip of everyday life", casual observation shapes attitudes through the gradual accumulation of individually unremarkable experiences. The notion of casual observation thus breaks the traditional theoretical distinction between influences either impersonal (largely from mass media) or personal (such as close friendships, crime victimization, unemployment or hospitalization) (e.g., Mutz, 1998). Best understood as a third, intermediate category, casual observation facilitates political learning from peripherally perceived cues about the immediate social environment, absent any direct personal involvement. Casual observation is thus neither strictly impersonal or personal, but rather both of these at once.

On the topic of stereotypes about crime, I expect citizens to be attuned to particular neighborhood characteristics indicative of group threat. First and foremost, citizens are likely to infer the likelihood of crime from neighborhood cues of *disorder*, such as graffiti and

other visible traces of vandalism, noisy, brash groups of young people, or public fights or discussions. Most of these cues are far too innocuous to merit the label crime. But casually observed, most citizens will use exactly such cues to arrive at their implicit estimates of neighborhood crime rates. And, crucially, to the extent that cues of disorder covary with the presence of racial/ethnic minorities (either directly or indirectly), they may engender stereotypes about the typical group affiliation of perpetrators of crime.

Empirically speaking, the experience of living in an ethnically diversifying neighborhood is indeed likely to covary with increased levels of social disorder. Consider figure A.16 in the appendix, which shows police reports of crime plotted against municipal-level ethnic diversity in Denmark, the empirical setting studied here. Rates of reported citizen-directed crime (the most visible, personally affecting types, e.g. vandalism, assault, or robbery) increase consistently with higher levels of local ethnic diversity. Statistically, this correlation almost disappears when correcting for individual-level characteristics (such as age and socio-economic status) as well as contextual features (such as population density) that are analytically distinct from ethnic diversity (Andersen and Tranaes, 2011). But citizens observing these concomitant trends 'bivariately' in their own neighborhood are unlikely to be able (or indeed motivated) to parse out these confounding factors. Casual observation cannot make statistical adjustments. What remains for citizens exposed to their local contexts is the experientially salient fact of social disorder rising along with ethnic diversity.

The notion that stereotypes can arise out of observed covariation between social groups and patterns of behavior is a long-standing theme in social psychology. In fact, influential early work argued that stereotypical beliefs can even develop in the absence of group differences (Hamilton and Gifford, 1976). Another class of models attribute stereotype formation to social categorization based on real group differences, though these differences can in turn be either exaggerated (Tajfel and Wilkes, 1963), disproportionately attributed to dispositional factors (Pettigrew, 1979), or nonconsciously detected (Hill et al., 1989).

With respect to this study, the key lesson from the social-psychological literature on stereotype formation is that stereotypes are a result of automatically occurring social categorization processes designed to accentuate between-group differences. As part of this process of social categorization, individuals search for cues that seem indicative of group affiliation. This search is attentive to any type of cue in the informational environment, including those accessible through casual observation in the local context. In local contexts characterized by an increasingly salient majority-minority group distinction (such as ethnic background) and a rare and threatening behavioral pattern (such as crime), this social categorization process

will promote minority-group stereotyping among majority-group individuals. Once encoded, individuals pay increased attention to information that confirms this categorization.

In societies characterized by spatially varying ethnic diversity, the implication of this process is that stereotypes are more accessible to individuals residing in diverse contexts with more salient group distinctions. Notably, though this theory describes how stereotypes can arise from information searches in the local context, it provides no explanation of why individuals are cognitively motivated to conduct this search to begin with. One such explanation, from the perspective of evolutionary psychology, would be that social categorization is the automatic execution of an 'alliance detection system' evolved to track the presence of relevant coalitions in the near environment (Pietraszewski et al., 2014). All else equal, this evolutionary account is more likely given that social disorder is likely to elicit a sense of threat: previous studies indicate that a state of anxiety can shift individual cognition to rely more on evolved response patterns (Arceneaux, 2012).

The study is to the best of my knowledge the first to provide a theoretical argument as to how group-centric attitudes can arise from casual observation in the local context. However, the idea that citizens' attitudes respond to visible outgroups in the local context, i.e. the notion of *group threat*, has a long pedigree (Branton and Jones, 2005; Giles and Buckner, 2009; Key, 1949; Oliver and Wong, 2003). Beyond intergroup attitudes, this study also connects to the broader literature on the effects of local ethnic diversity (Dinesen and Sonderskov, 2015; Enos, 2015; Hopkins, 2009). The key contribution here is to connect this literature to the idea of group-centric policy attitudes.

In a recent study, Weber et al. (2014) present an argument similar to ours. The authors present data from a survey of voters in New York State showing that, among respondents low in self-monitoring, residing in racially diverse contexts is associated with a stronger correlation between racial stereotypes and stereotype-relevant policy preferences. (Self-monitoring captures respondents' sensitivity to social norms). Mirroring the argument presented here, the authors state that "[the] findings underscore the contextualized nature of stereotype expression, suggesting that racial stereotypes have their greatest influence on policy attitudes among whites in diverse zip codes".

I extend the work of Weber et al. (2014) in two important ways. I provide a theoretically detailed account of how casual observation in the local context can promote group-centric attitudes. I also conduct a more comprehensive empirical test of the hypothesis. Furthermore, by amassing two large-N data sets covering surveys across two decades, I provide stronger evidence of how group-issue linkages emerge in gradually diversifying contexts.

7.2.4 Hypothesis

The mechanism of casual observation consists of exposure to mundane social disruption in ethnically diversifying neighborhoods. In turn, as respondents are exposed to higher local levels of ethnic diversity, their attitudes about crime should more strongly reflect how they feel about immigrants. Hence, I expect crime attitudes to be more tightly linked with anti-immigration attitudes in settings with higher proportions of racial/ethnic minorities. This leads to the main hypothesis tested in this paper:

> *As ethnic diversity in the local context increases, crime attitudes become more strongly associated with anti-immigration attitudes.*

The hypothesis thus implicitly assumes that the a stronger association between immigration attitudes and crime attitudes reflects higher levels of group-centrism. This follows standard practice in the racialization literature, where increased correlations with racial predispositions are taken as evidence of increased reliance on those predisposition (Tesler, 2012, 2014). This measurement strategy has the crucial advantage of allowing us to rely on any survey data set that contains measures of immigration and crime attitudes, without requiring direct measures of stereotyping, which are both much scarcer and more prone to social desirability bias.

7.3 Empirical setting

The data used to test the hypothesis are drawn from two large data sets each collecting responses from a number of surveys conducted in Denmark between 1983 and 2011. This choice of empirical setting has a number of advantages. For one, the data capture considerable variation in ethnic diversity, increasing from a very low to a moderately high level. The observed values of contextual ethnic diversity in the data, measured as local share of non-western immigrants and descendants, ranges from zero to around 50 percent. Figure 7.1 plots the trends in ethnic diversity within municipalities and zip codes, the two levels of measurement used here.

The empirical setting is particularly useful for studying the consequences of contextual ethnic diversity in that it allows for comparing citizens in fully ethnically homogeneous contexts with citizens in highly diverse contexts. By observing citizens in contexts across this range, the setting allows for observing conditions under which group-based distinctions become increasingly salient from a baseline of being virtually absent. This setting contrasts

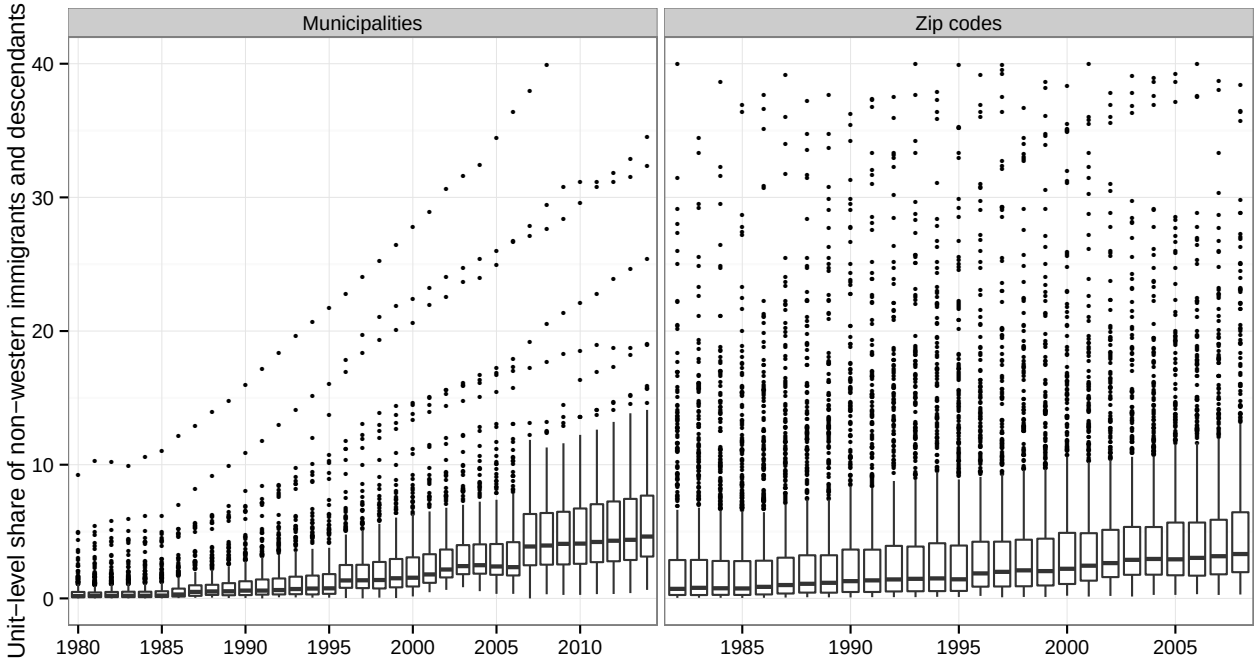

Figure 7.1 Box plots of distributions of shares of non-western immigrants and descendants at the zip code and municipality level by year. The y-axis is censored at 40 percent in order to more clearly show variation at the bottom of the scale. In the municipality data, the jump in 2007 is partly attributable to a reform which amalgamated municipalities into larger units. Source: Statistics Denmark

with most studies of contextual ethnic diversity, which examine already diverse contexts (though see Enos, 2014).

Another important feature of the setting is that citizens' news diet is relatively nationalized. The Danish newspaper market is dominated by three national dailies, and Danes are less likely to watch local TV news than either Americans or Brits (albeit more so than other Scandinavians) (Benson et al., 2012).

Lastly, the empirical setting allows for retrieving highly accurate and relatively localized contextual data drawn from official registries. Consider figure 7.2, which plots the sizes of the contextual units analyzed, zip codes and municipalities (the latter both before and after the 2007 amalgamation reform). For comparison, the figure also plots the distributions of geographical units often used in studies of context effects, U.S. counties (e.g. Branton and Jones, 2005; Hopkins, 2010; Stein et al., 2000) and U.S. zip codes (e.g., Oliver and Mendelberg, 2000).

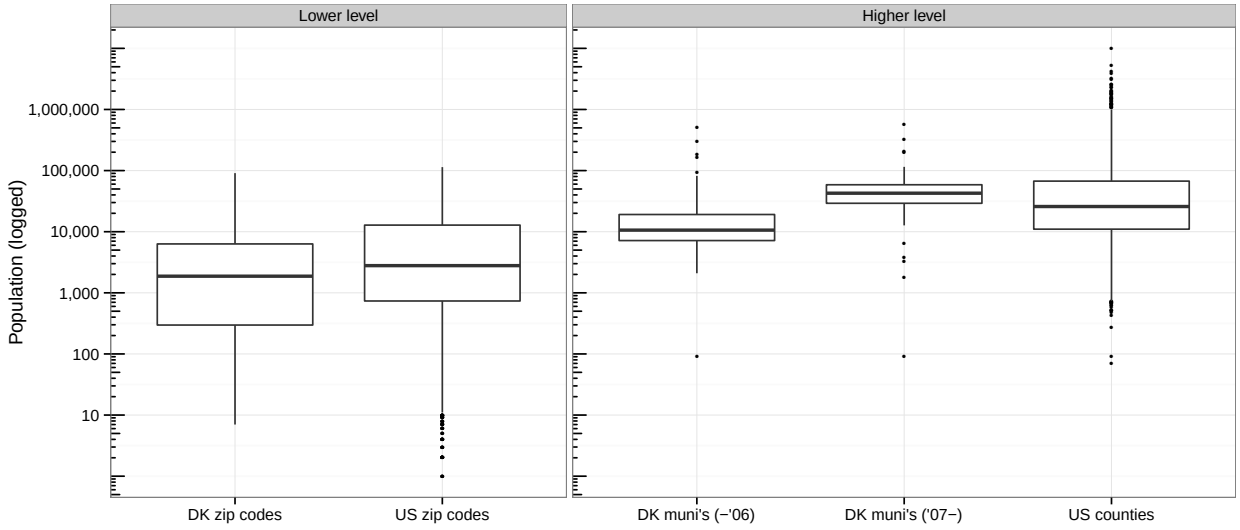

Figure 7.2 Distributions of sizes of contextual units. Source: Statistics Denmark, United States Census Bureau

Pre-reform Danish municipalities, which represent around 69 percent of the municipality data, are substantially smaller than typical U.S. counties. Post-reform, the average municipality is slightly larger. Like pre-reform municipalities, Danish zip codes skew smaller than their U.S. counterparts, with several zip codes containing fewer than 100 inhabitants. Hence, particularly for the zip code data, measured contextual ethnic diversity should capture important variation in respondents' everyday exposure to ethnic minorities. Equally importantly, Danish unit sizes tend to be less variant than is the case for U.S. counties and zip codes. This alleviates a

concern when working with U.S. data, namely that the size of the contextual unit (which is correlated with measurement error) is itself correlated with covariates of interest.

Besides varying measurement error, official geographical units such as municipalities and zip codes are in another, more basic sense problematic measures of individuals' environments of contextual experience. First of all, there is no guarantee that individuals' subjective experiences, their 'pseudoenvironments' (Lippmann, 1922), capture objective features of their environment accurately. In fact, evidence suggests that these pseudoenvironments do not resemble official units in shape or content (Wong et al., 2012). Furthermore, scholars who nevertheless find themselves relying on official units are faced with the 'modifiable area unit problem' (MAUP). Because geographical space is continuous, it can be partitioned in an infinite, arbitrary number of ways. The MAUP is the phenomenon whereby this arbitrarily chosen method of aggregation in itself affects a correlation between variables of interest, even to the point of flipping its sign (Voss, 2009; Wong, 2009).

This study, like most others, is constrained by data availability and so relies on contextual measures from official geographical units. However, in order to alleviate concerns about the MAUP, I follow the recommendation of Wong (2009) and Tam Cho and Baer (2011) and use contextual measures from two different geographical units. As the results will show, the observed association is robust across the two levels of measurement. This robustness makes it less likely that the inference is an artefact of the specific measure of context.

7.4 Data and model

In order to test the hypothesis presented in section 7.2.4, data with at least three types of information is needed: data on individuals' intergroup attitudes, their crime attitudes, and a contextual identifier allowing for merging in data contextual ethnic diversity. Two data sets, each with their own advantages and disadvantages, satisfy this criterion. One is an aggregate of 10 separate, nationally representative surveys, most of which are election surveys, in which respondent context is observed at the municipality level. The other comes from a commercial polling agency which for a number of years conducted regular surveys about political concerns which include data on respondents' zip codes. In the following, I will present results from these two data sets in parallel. As will be clear, despite differences in measurement, results are highly consistent across the two sets of data. For convenience, I will refer to them as *the municipality data* and *the zip code data* respectively.

In both sets of data, one of the main independent variables, ethnic diversity, is constructed as the unit-level share of non-Western immigrants and descendants.[1] Since I theorize stereotypes about ethnic minorities as inferred from visible contextual cues, it makes sense to use a measure concentrating on non-Western immigrants and descendants, who are the most likely to be visibly distinct from the native population. The choice a simple measure of the share of non-Western immigrants and descendants over other, more complex measures such as the Herfindahl index is primarily theoretically motivated: using the Herfindahl index would imply distinguishing between the specific nationalities of individual outgroup members, a distinction citizens engaging in casual observation are unlikely to make. However, the choice of measure of ethnic diversity is not empirically consequential: Dinesen and Sonderskov (2015), who rely on similar data, find that results are robust to relying on a Herfindahl index.

I depart from other studies in using the *level* of local ethnic diversity as opposed to a measure of *change*. For example, both Hopkins (2010) and Newman (2013) use a measure of 10-year change in local ethnic diversity, arguing that changes in the local environment are more psychologically salient than levels. However, since I observe local immigration from a baseline of almost total ethnic homogeneity, levels are virtually synonymous with changes in the Danish context. Figure A.17 in the appendix plots levels versus changes for U.S. counties and Danish municipalities, showing that whereas the two are only weakly correlated in the U.S. (r=.45), they are nearly synonymous in my data (r=.93). Hence, I use a measure of local levels in order to avoid dropping observations.

The other key variables are measured somewhat differently in the municipality and the zip code data sets. The remainder of this section describes how. Section A.4 in the appendix presents summary statistics for all variables in the municipality and zip code data sets.

7.4.1 The municipality data

The municipality data gathers survey responses from ten nationally representative surveys from 1990 to 2011, presented in section A.4 in the appendix. Three of the surveys included do not provide the municipality of residence of the respondent. In order to be able to use these data, I impute the municipalities of respondents in those two surveys by exploiting the fact that several surveys provide information on both municipality and zip code.[2]

[1] 'Non-Western' is a category defined by Statistics Denmark as people from outside EU–15, Iceland, Norway, Switzerland, the European micro-states, North America, Australia and New Zealand.

[2] The specific method is as follows: For each of the 637 zip codes containing a respondent with an unknown municipality, I tabulate the municipalities of all other respondents residing in that zip code. (Zip codes are not perfectly nested within municipalities, and so will in some cases cut across municipal borders). The

The individual-level independent variable, *anti-immigration attitude*, is measured using survey items tapping into the question of immigration as a cultural threat, reported in table A.25 in the appendix.[3] The dependent variable, *crime attitude*, is measured in a slightly cruder though reasonably theoretically valid way. The typical question used is whether the respondent supports "tougher sentences for violent crime", arguably a face valid measure of attitude toward crime. However, in some cases the response options are binary, and in the Likert-scaled items, the responses are highly skewed in favor of supporting tougher sentencing. In order to ensure a balanced measure of crime attitude, I dichotomize the item across all surveys.

The set of statistical controls available is partly constrained by the fact that any variable needs to be present in all 10 surveys. Hence, at the individual level the models rely on standard demographic controls gender, age, and education as well as the household income of the respondent and dummies for whether the respondent is a student or retired. Income is a typical demographic control, but including it introduces the problem of missing data for nearly a third of respondents. Simply ignoring missing data by using listwise deletion can lead to severe bias, so I impute income and other demographics using the multiple imputation approach presented in Honaker and King (2010). At the aggregate level, I follow Hopkins (2010) and include a control for municipality-level average education as well as population. Some models also include various combinations of fixed effects for municipality and year and controls for ideology and partisanship, the inclusion of which is discussed below. All variables except age and ethnic diversity are coded to range 0-1 in order to maximize comparability.

respondent is then assigned to the most common municipality for that zip-code. In order to ensure reasonably high confidence in the imputation, respondents are only assigned if the most common municipality accounts for at least 80 percent of all associated municipalities. This procedure assigns 399 zip codes to municipalities. The municipalities of the remaining, unassigned 238 zip codes are treated as missing data. This method ensures that each respondent residing in an unknown municipality is assigned to where (s)he is most likely to reside given the information available in the surveys. Nonetheless, the method is likely assign some respondents to the wrong municipality. However, the wrongly assigned municipality is exceedingly likely to be adjacent to the correct municipality. Since ethnic diversity is strongly spatially correlated, the ensuing measurement error associated with assigning the wrong municipality is likely to be small.

[3]The theoretical variable of interest is respondents' intergroup predispositions, i.e. how they feel about ethnic outgroups, which these items tap into reasonably well. The ideal item would likely have been a feeling barometer for immigrants, which is only available in a single survey. In that data, the item used here and the immigrants feeling barometer are strongly correlated ($r = .8, p < .001$). This suggests that the item is an acceptably valid measure of interethnic attitudes, and it has has the important advantage of having been asked relatively consistently across all surveys.

7.4.2 The zip code data

The zip code data is drawn from a quarterly survey conducted by the *Institute for Business Cycle Analysis* (IBCA), a private polling agency (Thulstrup, 2008). From 1983 to 2004, the IBCA was contracted by the Danish Ministry of Justice to conduct quarterly, nationally representative surveys of citizens' concerns and worries about various issues. The key attitudinal measures used here are all drawn from this battery. Altogether, the data set collects around 56,000 responses.

As the measure of *anti-immigration attitude*, I use an item measuring respondents' concerns about "immigrants and refugees". As was the case in the municipality data, this item taps respondents' basic feeling toward ethnic outgroups reasonably well. The dependent variable, *crime attitude*, is measured as respondents' level of concern about "violence and crime". This measure is analytically distinct from the one used in the municipality data: whereas the former measured an attitude about policy, this item reflects concerns about personal security. Yet in a broader sense, both items capture respondents' thinking about crime, and so should provide a reasonable test of the hypothesis.[4]

For both measures, the response range is a 4-point scale moving from *very* concerned to *not at all*. Though a 4-point scale is coarser than the conventional minimal standard for interval-scale data, for ease of interpretation I assume both scales to be continuous measures.[5]

The data has only few individual-level control variables. Only standard demographic controls gender, age, and education, which are asked of all respondents, are included here. The aggregate level includes some additional control variables, which are constructed from individual-level registry data and matched with respondent zip code. These include the zip code population, average level of education, and average income. As was the case in the municipality data, I recode all individual-level variables except age to range 0-1 in order to maximize comparability.

7.4.3 Modeling strategy

The data is constructed to test the hypothesis that crime and anti-immigration attitudes are more strongly correlated in ethnically diverse contexts. In other words, the hypothesis states

[4] A small subset of the data provides for some additional convergent validation (Adcock and Collier, 2001), in that respondents are asked about support for the death penalty. 570 respondents are asked, 112 of whom are in favor. In a two-sample t-test, death penalty support correlated with crime concern in the predicted direction, such that supporters express higher levels of concern ($t = 2.68, p < .01$)

[5] Figures A.18 and A.19 in the appendix presents plots of the time trends of both variables. In Table A.32 and Figure A.15 in the appendix, I show that the results are robust to treating the scales as ordered categorial using ordinal logit models.

that the association between anti-immigration and crime attitudes is moderated by contextual ethnic diversity. Hence, I specify a number of interaction models of the basic form:

$$Crime_{ij} = f(\beta_1 \times Imm_{ij} + \beta_2 \times ED_j + \beta_3 \times Imm_{ij} \times ED_j + \mathbf{X_{ij}} \times \gamma) \tag{7.1}$$

Where $Crime_{ij}$ is a measure of attitude toward crime for respondent i in context j, Imm_{ij} is a measure of respondent i's anti-immigration attitude, ED_j is the ethnic diversity in context j, and $\mathbf{X_{ij}} \times \gamma$ is a vector of additional controls and their coefficients. The hypothesis implies that the coefficient on the interaction term, β_3, is positive and significant.

In the municipality data, the measure of $Crime_{ij}$ is binary, so I estimate $f(\cdot)$ using logistic regression. In zip code data, the measure is continuous, so I estimate $f(\cdot)$ using OLS. All models are fixed effects models with standard errors clustered at the level of unit-year (Wooldridge, 2006). Given the limited within-unit variation but high statistical power, I opt for fixed effects models in order to minimize unit-level bias at the expense of inefficiency, which is less of a concern given the large number of units (Clark and Linzer, 2014). The fixed-effects estimator has the important property of controlling out time-invariant confounders, the advantages of which I discuss in further detail below. I show in section A.4 in the appendix that random effects models provide similar results.

7.5 Results

7.5.1 Regression estimates

Tables 7.1-7.2 presents results from various specifications of the model described above for the municipality and zip code data sets respectively. In both tables, I present the main and interaction terms in (7.1) first and then various control variables. The models differ in two respects: the types of fixed effects included in the model and the inclusion of individual-level political covariates.

In both tables 7.1 and 7.2, the last two models differ from the first two only with respect to the inclusion of fixed effects. In models 1-2, fixed effects for municipalities or zip codes are included. These control for all time-invariant unobserved heterogeneity at the contextual level of measurement. Models 3-4 add fixed effects for survey-years. These control for unobserved heterogeneity specific to each survey, such as the contemporaneous political or media agenda. As the tables show, even when including both sets of fixed effects and the full

Table 7.1 Models using municipality data

	Prefer stricter punishments for violent crime			
	(1)	(2)	(3)	(4)
Anti-immigration (Imm)	1.19^{***}	1.08^{***}	1.39^{***}	1.27^{***}
	(0.08)	(0.08)	(0.08)	(0.07)
Ethnic diversity (ED)	0.68	0.83	-3.46	-3.18
	(3.05)	(2.99)	(1.77)	(1.80)
Imm $\times$ ED	5.57^{***}	5.09^{***}	4.78^{***}	4.40^{***}
	(1.06)	(1.02)	(0.93)	(0.90)
Context-level controls:				
Muni. education level	-1.08	-1.44	-0.32	-0.04
	(1.57)	(1.57)	(1.68)	(1.70)
Muni population	-115.49^{***}	-113.66^{***}	-9.10	-9.42
	(32.61)	(32.32)	(15.87)	(15.59)
Individual-level controls:				
Gender (f)	0.02	0.03	0.03	0.05
	(0.03)	(0.03)	(0.03)	(0.03)
Age	-0.01^{***}	-0.01^{***}	-0.02^{***}	-0.02^{***}
	(0.001)	(0.001)	(0.001)	(0.001)
Education level	-0.72^{***}	-0.73^{***}	-0.88^{***}	-0.90^{***}
	(0.07)	(0.07)	(0.07)	(0.07)
Household income	-0.09	-0.20^{**}	-0.14^{*}	-0.23^{***}
	(0.06)	(0.06)	(0.06)	(0.07)
Student	-0.05	-0.09	-0.12	-0.16^{*}
	(0.06)	(0.06)	(0.06)	(0.06)
Pensioner	0.08	0.06	0.08	0.06
	(0.05)	(0.05)	(0.06)	(0.06)
Left-wing voter		-0.31^{***}		-0.31^{***}
		(0.04)		(0.04)
Left/right self-placement		0.43^{***}		0.41^{***}
		(0.08)		(0.09)
Intercept	6.41^{***}	6.45^{***}	2.17^{**}	2.08^{**}
	(1.64)	(1.61)	(0.71)	(0.70)
N	23,356	23,356	23,356	23,356
Municipality fixed effects	✓	✓	✓	✓
Year fixed effects			✓	✓
R^2	0.18	0.19	0.22	0.23

$^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table 7.2 Models using zip code data

	Concern about crime			
	(1)	(2)	(3)	(4)
Immigration concern (Imm)	0.20^{***}	0.19^{***}	0.20^{***}	0.19^{***}
	(0.01)	(0.01)	(0.01)	(0.01)
Ethnic diversity (ED)	-0.69^{***}	-0.68^{***}	-0.44^{***}	-0.47^{***}
	(0.17)	(0.16)	(0.13)	(0.13)
Imm $\times$ ED	1.23^{***}	1.25^{***}	1.21^{***}	1.22^{***}
	(0.12)	(0.12)	(0.12)	(0.12)
Context-level controls:				
Zip avg. income	-0.12^{***}	-0.12^{***}	-0.03	-0.05
	(0.02)	(0.02)	(0.03)	(0.03)
Zip avg. education	-0.11^{***}	-0.09^{***}	-0.08^{**}	-0.07^{**}
	(0.02)	(0.02)	(0.03)	(0.03)
Zip population	-0.83^{***}	-0.80^{***}	-0.72^{***}	-0.68^{***}
	(0.22)	(0.21)	(0.18)	(0.18)
Individual-level controls:				
Gender (f)		0.08^{***}		0.09^{***}
		(0.003)		(0.003)
Age		0.03^{***}		0.03^{***}
		(0.005)		(0.005)
Age^2		-0.003^{***}		-0.003^{***}
		(0.001)		(0.001)
Education		-0.16^{***}		-0.16^{***}
		(0.01)		(0.01)
Intercept	2.04^{***}	1.81^{**}	1.51^{**}	1.38^{*}
	(0.55)	(0.56)	(0.59)	(0.61)
N	40,614	39,838	40,614	39,838
Zip code fixed effects	✓	✓	✓	✓
Year fixed effects			✓	✓
R^2	0.16	0.19	0.18	0.20
Adjusted R^2	0.16	0.19	0.18	0.20
chi^2	$7{,}321.84^{***}$	$8{,}480.51^{***}$	$7{,}988.36^{***}$	$9{,}116.81^{***}$

$^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

set of individual-level controls, the hypothesized interaction effect is substantively robust and strongly statistically significant.

Statistically speaking, the robustness of the coefficient on $Imm \times ED$ across these sets of models is informative, since the inclusion of fixed effects removes bias from unobserved spatial or temporal heterogeneity at the expense of larger standard errors. Even so, the interaction is strongly significant across all specifications. This is more than a mere technical point, especially with respect to the unit-level fixed effects included in all four models. By controlling away time-invariant unobserved heterogeneity between units, the fixed effects strengthen the case that local ethnic diversity is in fact the causally consequential contextual feature. In contrast, researchers using cross-sectional data need to assume that they can observe and adjust for all potential context-level confounders. If not, they risk ascribing effects to contextual diversity that are in fact due to other local characteristics. Hence, the large sets of data I use here are not merely sources of high statistical power. The cross-sectional time-series nature also provides a stronger foundation for the proposed causal mechanism.

In each table, models 2 and 4 differ from models 1 and 3 with respect to inclusion of individual-level controls. In the zip code data, the individual-level controls are standard demographics. As is clear in table 7.2, including these is inconsequential. In the models based on municipality data, the inclusion of additional individual controls is more debatable, in that models 2 and 4 include voters' party choice in the previous election as well as their left-right self-placement. These variables are included in order to account for heterogeneity with respect to voters' general political outlook. For example, if voters of a particular political orientation are more likely to self-select into ethnically diverse localities and simultaneously more likely to think about crime in ethnic terms, the observed interaction will be spuriously inflated. The downside of including these variables is that they may be post-treatment to anti-immigration attitudes, in which case the observed interaction may be underestimated (Rosenbaum, 1984). Regardless of which of these effects dominates, the results in table 7.1 remain robustly significant.

7.5.2 Illustrations of effect sizes

The statistical significance of the results aside, the substantive magnitude of the interaction is difficult to make sense of based on the regression output alone. To help illustrate the interaction, figure 7.3 plots the predicted association between anti-immigration and crime attitudes at various levels of contextual ethnic diversity. In order to help make sense of the predicted effect, the plot includes a line for the coefficient on education, which is the predicted

change in crime attitude associated with moving across the range of education in a model with no interactions. The line provides a baseline for comparing how strongly immigration and crime attitudes are associated at various levels of ethnic diversity.

The observed patterns in each of the two data sets are strikingly similar. At the lowest level of ethnic diversity, anti-immigration attitude is about as informative as level of education in terms of predicting crime attitude. But moving across the range of ethnic diversity, the association increases to a level more than double (in the municipality data) or triple (in the zip code data) that of level of education. The comparison indicates that the interaction is thus not just statistically but substantively significant.

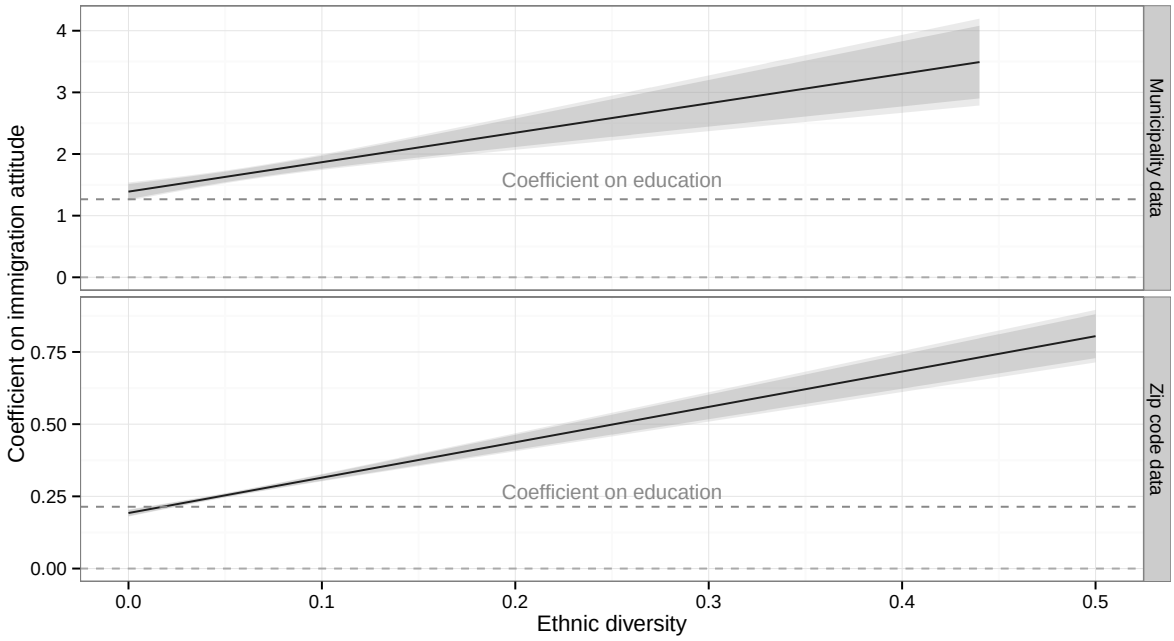

Figure 7.3 Correlations between anti-immigration attitudes and crime attitudes at varying levels of contextual diversity in municipality and zip code data. Shaded areas represent 90 and 95 percent confidence intervals. The dotted line shows the coefficient on level of education in a model predicting crime attitudes. At the lowest level of ethnic diversity, anti-immigration attitude is about as informative as education in terms of predicting crime attitude. But moving across the range of ethnic diversity, the association increases to a level more than double (in the municipality data) or triple (in the zip code data) that of level of education.

Another way of making sense of the substantive magnitude of the interaction is shown in figure 7.4, which plots the predicted associations between immigration and crime attitudes at various observed levels of ethnic diversity. Again, results for the two datasets are similar. At the lowest observed level, moving across the range of anti-immigration attitude corresponds to a change of about a quarter of the dependent variable. At the highest observed level of ethnic diversity, the corresponding predicted change is three quarters of the dependent variable or

more. The figures serve to illustrate how much more closely immigration and crime attitudes are linked in highly diverse contexts.

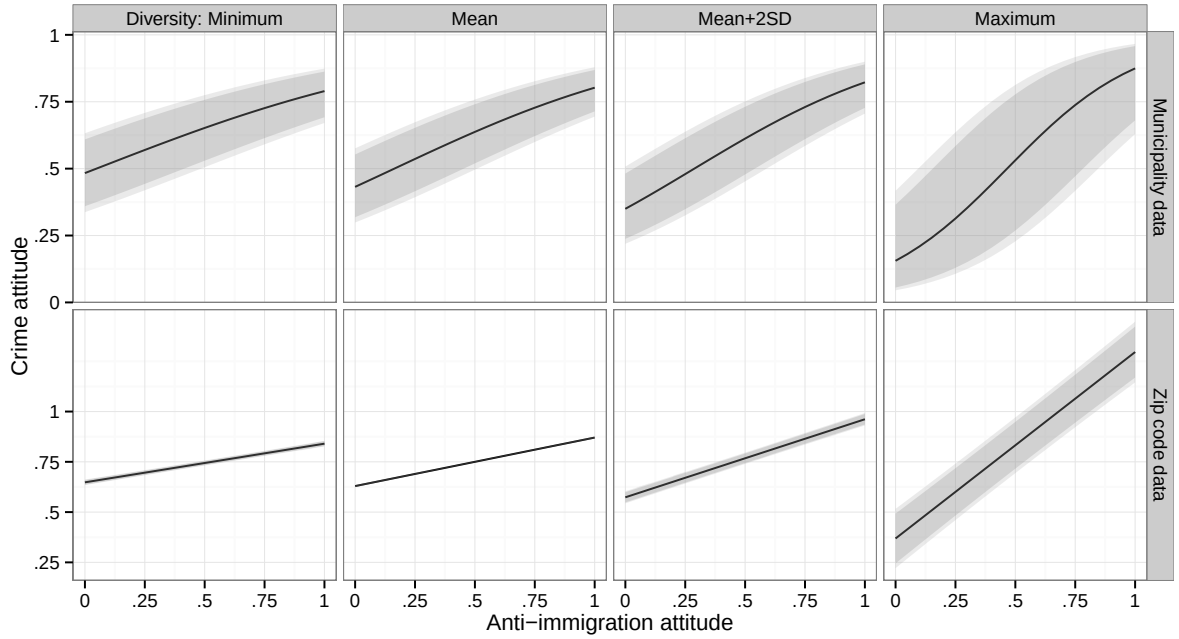

Figure 7.4 Predicted associations between anti-immigration and crime attitudes at various levels of ethnic diversity, in municipality and zip code data sets. Shaded areas represent 90 and 95 percent confidence intervals. In both data sets, anti-immigration attitude predicts crime attitudes more strongly as local ethnic diversity increases.

As made apparent in figures 7.3-7.4, the estimated interaction effect is considerably greater at the zip code level compared to the municipality level. This most likely reflects a smaller amount of attenuation bias in the zip code data, where both the dependent variable (a more fine-grained measure) and the contextual measure (a smaller geographical unit) contain less measurement error.

7.5.3 Manipulation check

This study's hypothesis rests on the assumption that the stronger association between immigration and crime attitudes in more ethnically diverse contexts reflects exposure to ethnic diversity. That is, I assume that individuals in diversifying contexts react to changing neighborhood composition. The hypothesized stronger link between immigration and crime attitudes is a down-stream consequence of this experience. As a way of bolstering the case for this link, I present evidence supporting the case that individuals actually experience neighborhood ethnic diversity. The test thus serves as a 'manipulation check' of the hypothesized treatment,

showing that neighborhood ethnic diversity does in fact enter into respondents' everyday lives.

In a study using U.S. survey data, Newman et al. (2013) conduct a similar check, showing that respondents do in fact 'receive the treatment' in that they can reasonably accurately estimate local levels of immigration and unemployment. Here, I show that citizens in Denmark are equally responsive to local characteristics. I rely on the Danish implementation of the 2009 International Social Survey Programme (ISSP), which includes a few items on how respondents perceive their neighborhood. One question tells respondents "please provide your best guess—approximately what proportion of people living in your neighborhood are immigrants from non-western countries?". Response options range from zero to 100 percent. Since the data also provide respondents' zip codes, I can match respondent estimates to actual zip code shares of non-western immigrants and descendants. Figure 7.5 plots the two against each other.

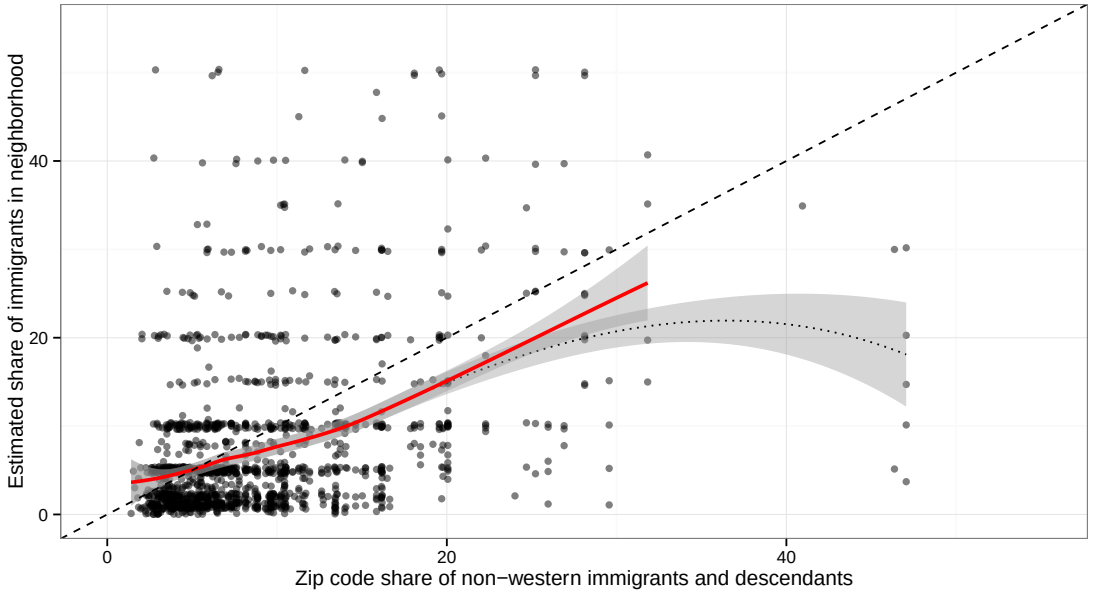

Figure 7.5 Manipulation check: actual vs. respondent-estimated levels of neighborhood ethnic diversity. The dotted line is a loess fit. The thick line is a loess fit excluding the 10 observations at the highest levels of neighborhood ethnic diversity (above 40 percent).

As shown by the loess lines in the figure, respondent estimates of neighborhood ethnic diversity track actual ethnic diversity very closely across almost the entire observable range.[6] In fact, the observed correlation is around three times stronger than that found in a similar study of neighborhood perceptions in the United States (Chiricos et al., 1997). The notable exception

[6] Across the full range of the data, the two correlate at $r = .45$, $p < .001$

is the 10 respondents from zip codes with more than 40 percent non-western immigrants, who seem to *underestimate* neighborhood diversity. This underestimation at the highest end of the range should not affect the main results, since very few respondents in the municipality and zip code data sets (0.02 percent) are observed in contexts that diverse. Furthermore, any bias introduced by underestimation of neighborhood diversity should attenuate the estimated effects, yielding a more conservative test. In sum, I can be fairly confident that as a rule, respondents in more ethnically diverse contexts actually perceive them as such.

7.5.4 Placebo tests

While the main results show a clear interaction, it remains the case that immigration and crime attitudes are significantly correlated even at zero levels of ethnic diversity (see, for example, the leftmost panels in figure 7.4). This should not come as no surprise. Immigration and crime attitudes are both tied into the class of 'post-materialist' issues which emerged as an independent ideological dimension in Western electorates in the postwar era (Borre, 1995; Inglehart, 1971). Hence, at minimum levels of ideological constraint, the two issue positions should correlate simply by virtue of reflecting the same ideological dimension.

This is not in and of itself a problem for the theory proposed here, which is concerned with changes in the correlation conditional on ethnic diversity rather than its baseline level. But it does raise the concern that the results may reflect higher post-materialist ideological constraint among inhabitants of ethnically diverse localities and not an issue-specific change in how those inhabitants think about crime. In order to test this proposition, tables A.30-A.31 in the appendix presents placebo tests of the main results, re-estimating the models from tables 7.1-7.2 using a different post-materialist issue, concern for the environment, as the dependent variable. I summarize the placebo tests in figure 7.6, plotting the placebo interaction coefficients alongside the coefficients from the original models.

The environmental issue is convenient as a placebo test since, being a classical post-materialist issue, it is measured in all of the surveys used in this study. At the same time, it has no meaningful direct connection to anti-immigration attitudes. Since the benefits of environmental policy tend to be diffuse (i.e., environmental quality is a pure public good), it is unlikely to be associated with any specific group. Hence, while environmental attitudes are ideologically aligned with crime attitudes, they should not be easily implicitly linked with visible social groups, immigrants included. If the main results merely reflected higher ideological constraint among post-materialist attitudes in ethnically diverse localities, the models should thus return significant interaction similar to those in tables 7.1-7.2. On the

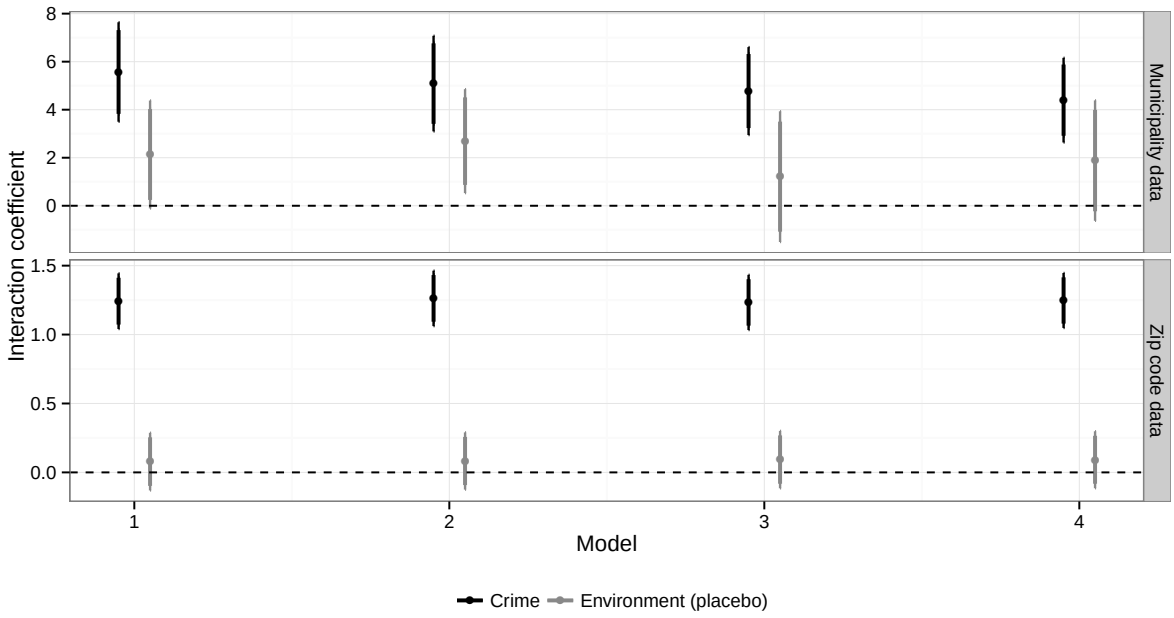

Figure 7.6 Interaction coefficients in original and placebo models for each of the four presented specifications, municipality and zip code data.

other hand, if the effect of ethnic diversity really is specific to ethnic stereotyping of the issue of crime, the interaction should be negligible. The results illustrated in figure 7.6 suggest the latter is the case. In all the placebo models save one, the interaction term is insignificant, and its magnitude is at most half of that found in the main results.

In sum, the placebo test indicates that the main result does not merely reflect a broader tendency toward more ideologically constrained post-materialist attitudes in diverse contexts, but is in fact a specific link between diversity and the issue of crime.

7.6 Conclusion and Discussion

In explaining the group-centric nature of various policy attitudes, scholars have typically turned to elite-centric accounts, emphasizing the role of mass communications and prominent political sponsors in promoting group-policy linkages. In this paper, I have challenged the assumption that only elite influences can engender such associations. Instead, casual observation in the local context contributes in part to citizens' beliefs about the group-linked nature of public policies.

Using data from two large sets of surveys spanning 20 years, I provided evidence consistent with this argument: as ethnic diversity increases is citizens' local contexts, their views on

crime will more closely reflect their feelings about immigrants. In Danish localities, where ethnic diversity has increased significantly in recent decades, moving across the observable range of diversity is associated with this link increasing two- to threefold. Since the effects persist when considering only variation within small geographical units and within years, they cannot be attributable to national-level media discourse. The results highlight the potential role of casual observation in the development of stereotypes about racial/ethnic minorities.

The main limitation of this study lies in the observational, time-series cross-sectional nature of the data. For one, this leaves open the possibility that the observed associations reflect changes in population composition rather than changed attitudes. In theory, the observed data could be explained by less-prejudiced citizens being more likely to move out of ethnically diverse localities for unobserved reasons. Though difficult to obtain for sufficiently long stretches of time, panel data would allow for observing within-individual responses to changing local contexts. Even so, the question of the role played by self-selection in and out of diversifying contexts would remain. A worthwhile avenue for future research would thus be to test the argument presented here using exogenous variation in exposure to local ethnic diversity, either naturally occurring or implemented experimentally.

An additional limitation of this study is its single-issue character, leaving open the question of whether casual observation is particularly important for the issue of crime. Exploring the boundary conditions of the political role of casual observation is an important task for future research.

For those concerned with the detrimental impact of racial/ethnic stereotypes on political discourse and public policy, the study holds somewhat sombre implications. The theory and findings suggest that ridding mass communications of news media distortions and racialized campaign rhetoric would not fully do away with stereotypes about crime. Altering the impressions citizens get from the local context is a greater challenge, involving intractable tasks such as reducing public disorder and preventing ethnic segregation. But to the extent that stereotypes derive not just from media but also from casual observation in the local context, addressing such stereotypes is a question not only of political communication, but also one of public policy.

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Appendices

A.1 Appendix for 'Who benefits'

Tables

Table A.3 Measures of ethnic prejudice and economic conservatism. Items marked with an asterisk are excluded from reduced measure of ethnic prejudice.

Variable	Items	Scale
Ethnic prejudice		1-5
	There have been enough programs designed to create jobs for immigrants.*	
	Racist groups are no longer a threat toward immigrants.	
	It is easy to understand immigrants' demands for equal rights.*	
	Immigrants get too little attention in the media.	
	It is important to invest money in teaching immigrants their mother tongue.	
	Special programs are needed to create jobs for immigrants.*	
Economic conservatism		1-5
	Sell public companies to private buyers.	
	Remove tax deduction for household work.	
	Run more health care on private hands	
	Introduce special tax for most highly taxed houses	
	Reduce social transfers	
	Stock payouts should not be allowed for taxpayer financed health care, schools, and elderly care	

Table A.4 Tests of sequence effects

	Opposed to cross-border welfare rights				
	(1)	(2)	(3)	(4)	(5)
Intercept	.08*	.07*	.09**	.07*	.09*
	(.03)	(.03)	(.03)	(.03)	(.03)
Gender (f)	.06***	.06***	.06***	.06***	.06***
	(.01)	(.01)	(.01)	(.01)	(.01)
Age	.002***	.002***	.002***	.002***	.002***
	(.0004)	(.0004)	(.0004)	(.0004)	(.0004)
Education (some uni.)	−.02	−.02	−.02	−.02	−.02
	(.01)	(.01)	(.01)	(.01)	(.01)
Employed	.04***	.04***	.04***	.04***	.04***
	(.01)	(.01)	(.01)	(.01)	(.01)
Ethnic prejudice (EP)	.63***	.65***	.63***	.63***	.63***
	(.03)	(.04)	(.03)	(.03)	(.03)
Economic conservatism (EC)	.09**	.09**	.09**	.09**	.09**
	(.03)	(.03)	(.03)	(.03)	(.03)
EP scale before probe (Before)	−.01	−.001	−.03*	.002	−.03
	(.01)	(.02)	(.02)	(.02)	(.03)
Children stay home (SH)	.19***	.19***	.17***	.19***	.19***
	(.01)	(.01)	(.02)	(.01)	(.01)
Nationality: Bulgarian	.06***	.06***	.06***	.07***	.06***
	(.01)	(.01)	(.01)	(.02)	(.01)
No. of children (NC)	.01*	.01*	.01*	.01*	.01
	(.004)	(.004)	(.004)	(.004)	(.01)
Before×EP		−.03			
		(.05)			
Before×SH			.04		
			(.02)		
Before×Bulgarian				−.03	
				(.02)	
Before×NC					.004
					(.01)
N	2370	2370	2370	2370	2370
R^2	.35	.35	.35	.35	.35
Adjusted R^2	.35	.35	.35	.35	.35

*p < .05; **p < .01; ***p < .001

Table A.5 Models of welfare chauvinism, logistic regressions

	Opposed to cross-border welfare rights			
	(1)	(2)	(3)	(4)
Intercept	-2.18^{***}	-3.52^{***}	-2.86^{***}	-3.02^{***}
	(0.25)	(0.30)	(0.35)	(0.34)
Gender (f)	0.37^{***}	0.38^{***}	0.38^{***}	0.39^{***}
	(0.10)	(0.11)	(0.11)	(0.11)
Age	0.01^{*}	0.01^{**}	0.01^{**}	0.01^{**}
	(0.003)	(0.004)	(0.004)	(0.004)
Education (some uni.)	-0.15	-0.23	-0.23	-0.23
	(0.11)	(0.12)	(0.12)	(0.12)
Employed	0.22^{*}	0.23^{*}	0.22^{*}	0.21^{*}
	(0.10)	(0.11)	(0.11)	(0.11)
Ethnic prejudice (EP)	4.03^{***}	4.42^{***}	2.50^{***}	4.51^{***}
	(0.29)	(0.31)	(0.62)	(0.31)
Economic conservatism (EC)	0.63^{**}	0.68^{**}	0.67^{**}	-0.85
	(0.23)	(0.24)	(0.24)	(0.54)
Children stay		1.35^{***}	1.35^{***}	1.37^{***}
		(0.10)	(0.10)	(0.10)
Nationality: Bulgarian		0.48^{***}	0.15	-0.001
		(0.10)	(0.19)	(0.17)
No. of children (NC)		0.08^{*}	-0.09	-0.01
		(0.03)	(0.07)	(0.06)
EP×Nationality: Bulgarian			1.04^{*}	
			(0.50)	
EP×NC			0.51^{**}	
			(0.18)	
EC×Bulgarian				1.37^{***}
				(0.40)
EC×NC				0.27
				(0.14)
N	2370	2370	2370	2370
Log Likelihood	-1401.27	-1288.85	-1282.50	-1281.37
AIC	2816.53	2597.70	2589.01	2586.74

$^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table A.6 Models of welfare chauvinism, reduced measure of ethnic prejudice

	Opposed to cross-border welfare rights			
	(1)	(2)	(3)	(4)
Intercept	0.23^{***}	0.09^{**}	0.16^{***}	0.15^{***}
	(0.03)	(0.03)	(0.04)	(0.04)
Gender (f)	0.07^{***}	0.06^{***}	0.06^{***}	0.06^{***}
	(0.01)	(0.01)	(0.01)	(0.01)
Age	0.002^{***}	0.002^{***}	0.002^{***}	0.002^{***}
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Education (some uni.)	-0.03^{*}	-0.04^{**}	-0.04^{**}	-0.04^{**}
	(0.01)	(0.01)	(0.01)	(0.01)
Employed	0.04^{**}	0.04^{**}	0.04^{**}	0.04^{**}
	(0.01)	(0.01)	(0.01)	(0.01)
Ethnic prejudice (EP)	0.55^{***}	0.55^{***}	0.38^{***}	0.55^{***}
	(0.03)	(0.03)	(0.06)	(0.03)
Economic conservatism (EC)	0.11^{***}	0.11^{***}	0.11^{***}	-0.07
	(0.03)	(0.03)	(0.03)	(0.06)
Children stay		0.19^{***}	0.19^{***}	0.20^{***}
		(0.01)	(0.01)	(0.01)
Nationality: Bulgarian		0.06^{***}	0.01	-0.01
		(0.01)	(0.02)	(0.02)
No. of children (NC)		0.01	-0.01	-0.003
		(0.004)	(0.01)	(0.01)
EP×Bulgarian			0.12^{**}	
			(0.04)	
EP×NC			0.04^{*}	
			(0.02)	
EC×Bulgarian				0.18^{***}
				(0.04)
EC×NC				0.03
				(0.02)
N	2387	2387	2387	2387
R^2	0.25	0.34	0.35	0.35
Adjusted R^2	0.24	0.34	0.34	0.35

$^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table A.7 Models of welfare chauvinism, full factorial models

	Opposed to cross-border welfare rights	
	Children stay	Not mentioned
	(1)	(2)
Intercept	0.33***	0.04
	(0.05)	(0.05)
Gender (f)	0.06***	0.06***
	(0.02)	(0.02)
Age	0.002***	0.001*
	(0.001)	(0.001)
Education (some uni.)	−0.03	−0.02
	(0.02)	(0.02)
Employed	0.03	0.06**
	(0.02)	(0.02)
Ethnic prejudice (EP)	0.50***	0.76***
	(0.04)	(0.04)
Economic conservatism (EC)	0.09*	0.09*
	(0.04)	(0.04)
Nationality: Bulgarian	0.01	0.07
	(0.03)	(0.04)
No. of children (NC)=2	−0.03	0.003
	(0.03)	(0.04)
NC=3	−0.03	0.02
	(0.03)	(0.04)
NC=4	0.01	0.02
	(0.03)	(0.04)
NC=5	0.04	0.04
	(0.03)	(0.04)
Bulgarian×NC=2	0.08	0.01
	(0.05)	(0.05)
Bulgarian×NC=3	0.06	−0.003
	(0.05)	(0.05)
Bulgarian×NC=4	0.06	0.01
	(0.05)	(0.05)
Bulgarian×NC=5	−0.001	−0.04
	(0.05)	(0.05)
N	1193	1177
R^2	0.25	0.34
Adjusted R^2	0.24	0.33

*p < .05; **p < .01; ***p < .001

Table A.8 Full factorial ANOVA on all experimental conditions with covariates (number of children as interval scale)

	Df	Sum Sq	Mean Sq	F value	Pr(>F)
Gender (f)	1	0.34	0.34	4.80	0.0285
Age	1	4.83	4.83	69.23	0.0000
Education (some uni.)	1	3.42	3.42	48.98	0.0000
Employed	1	1.19	1.19	17.09	0.0000
Ethnic prejudice (EP)	1	55.01	55.01	788.06	0.0000
Economic conservatism (EC)	1	0.75	0.75	10.72	0.0011
Bulgarian	1	1.90	1.90	27.17	0.0000
No. of children (NC)	1	0.19	0.19	2.72	0.0990
Children stay (CS)	1	21.97	21.97	314.76	0.0000
Bulgarian*NC	1	0.02	0.02	0.25	0.6153
Bulgarian * CS	1	0.03	0.03	0.40	0.5275
NC * Children stay	1	0.03	0.03	0.41	0.5231
Bulgarian * NC * SC	1	0.01	0.01	0.19	0.6632
Residuals	2356	164.45	0.07		

Table A.9 Full factorial ANOVA on all experimental conditions with covariates (number of children as nominal scale)

	Df	Sum Sq	Mean Sq	F value	Pr(>F)
Gender (f)	1	0.34	0.34	4.80	0.0286
Age	1	4.83	4.83	69.10	0.0000
Education (some uni.)	1	3.42	3.42	48.89	0.0000
Employed	1	1.19	1.19	17.06	0.0000
Ethnic prejudice (EP)	1	55.01	55.01	786.55	0.0000
Economic conservatism (EC)	1	0.75	0.75	10.70	0.0011
Bulgarian	1	1.90	1.90	27.11	0.0000
No. of children (NC)	4	0.25	0.06	0.88	0.4772
Children stay (CS)	1	21.95	21.95	313.86	0.0000
Bulgarian*NC	4	0.36	0.09	1.28	0.2741
Bulgarian * CS	1	0.03	0.03	0.36	0.5496
NC * Children stay	4	0.12	0.03	0.44	0.7819
Bulgarian * NC * SC	4	0.07	0.02	0.25	0.9102
Residuals	2344	163.92	0.07		

Table A.10 Full factorial ANOVA, interactions with ethnic prejudice

	Df	Sum Sq	Mean Sq	F value	Pr(>F)
Gender (f)	1	0.34	0.34	4.81	0.0283
Age	1	4.83	4.83	69.37	0.0000
Education (some uni.)	1	3.42	3.42	49.08	0.0000
Employed	1	1.19	1.19	17.13	0.0000
Ethnic prejudice (EP)	1	55.01	55.01	789.63	0.0000
No. of children (NC)	4	0.24	0.06	0.87	0.4811
Bulgarian	1	1.99	1.99	28.52	0.0000
Economic conservatism (EC)	1	0.66	0.66	9.48	0.0021
Children stay (CS)	1	21.95	21.95	315.09	0.0000
EP * NC	4	0.64	0.16	2.31	0.0554
EP * Bulgarian	1	0.35	0.35	5.08	0.0243
Bulgarian * NC	4	0.36	0.09	1.29	0.2701
Bulgarian * CS	1	0.02	0.02	0.32	0.5734
NC * Children stay	4	0.12	0.03	0.43	0.7905
Bulgarian * NC * SC	4	0.06	0.02	0.23	0.9204
Residuals	2339	162.94	0.07		

Table A.11 Full factorial ANOVA, interactions with economic conservatism

	Df	Sum Sq	Mean Sq	F value	Pr(>F)
Gender (f)	1	0.34	0.34	4.84	0.0279
Age	1	4.83	4.83	69.76	0.0000
Education (some uni.)	1	3.42	3.42	49.36	0.0000
Employed	1	1.19	1.19	17.22	0.0000
Ethnic prejudice (EP)	1	55.01	55.01	794.10	0.0000
Economic conservatism (EC)	1	0.75	0.75	10.81	0.0010
No. of children (NC)	4	0.23	0.06	0.81	0.5161
Bulgarian	1	1.92	1.92	27.66	0.0000
Children stay (CS)	1	21.95	21.95	316.87	0.0000
EC * NC	4	0.53	0.13	1.90	0.1076
EC * Bulgarian	1	1.37	1.37	19.85	0.0000
Bulgarian * NC	4	0.36	0.09	1.31	0.2639
Bulgarian * CS	1	0.02	0.02	0.30	0.5822
NC * Children stay	4	0.12	0.03	0.43	0.7887
Bulgarian * NC * SC	4	0.08	0.02	0.28	0.8905
Residuals	2339	162.02	0.07		

Figures

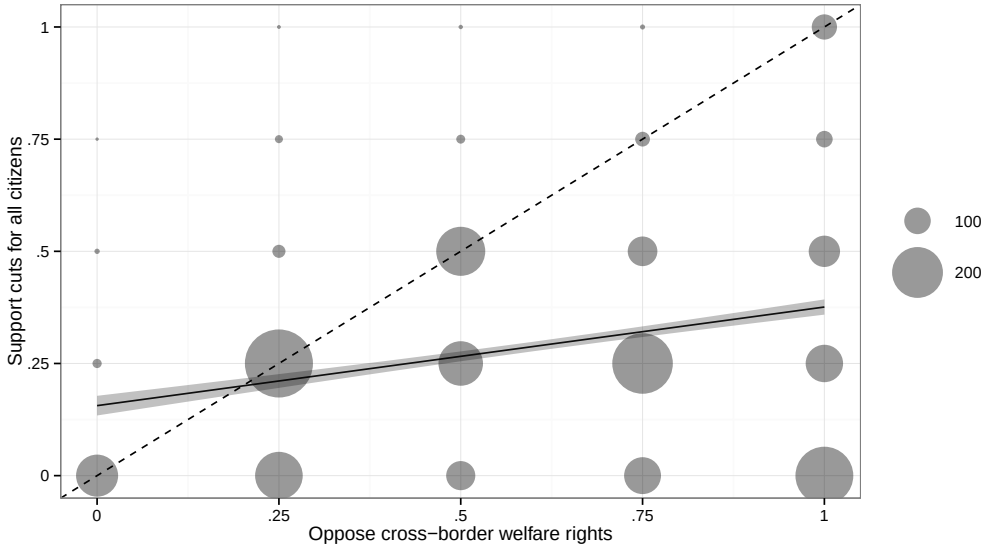


Figure A.7 Opposition to cross-border welfare rights plotted against support for cutting welfare benefits generally. *Note:* Grey line shows regression line with 95 pct. confidence interval ($\beta = .22, p < .001$). In general, respondents are weakly opposed to cross-border welfare rights but strongly supportive of existing benefit levels for Swedish citizens. 49 pct. of respondents take the welfare chauvinist position of being opposed to welfare benefits for other EU nationals, but also opposed to cuts to benefits for Swedish citizens.

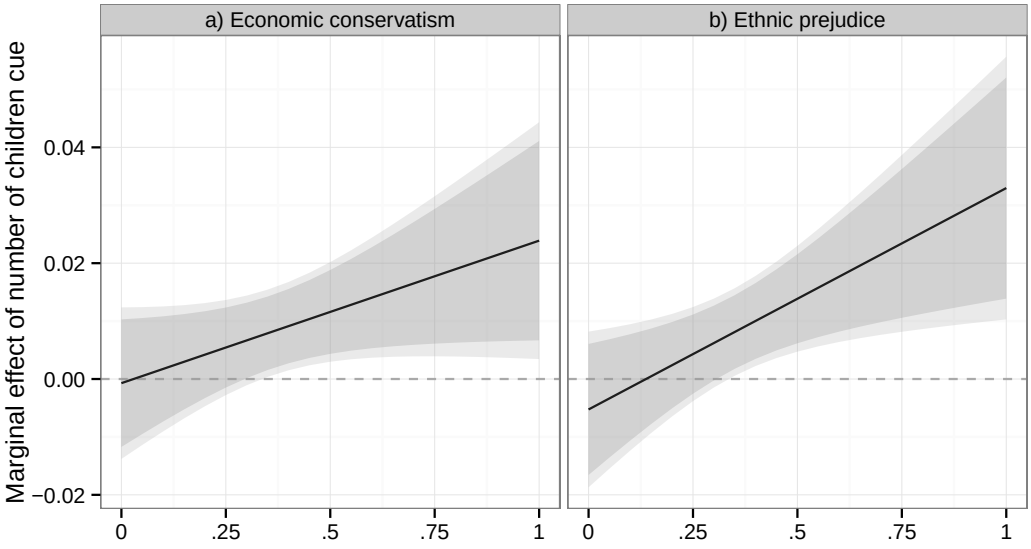


Figure A.8 Predicted marginal effect of number of children cue on welfare chauvinism across the observed range of economic conservatism (panel a) and ethnic prejudice (panel b). Note: Based on models 2 and 3 in Table 4.2.

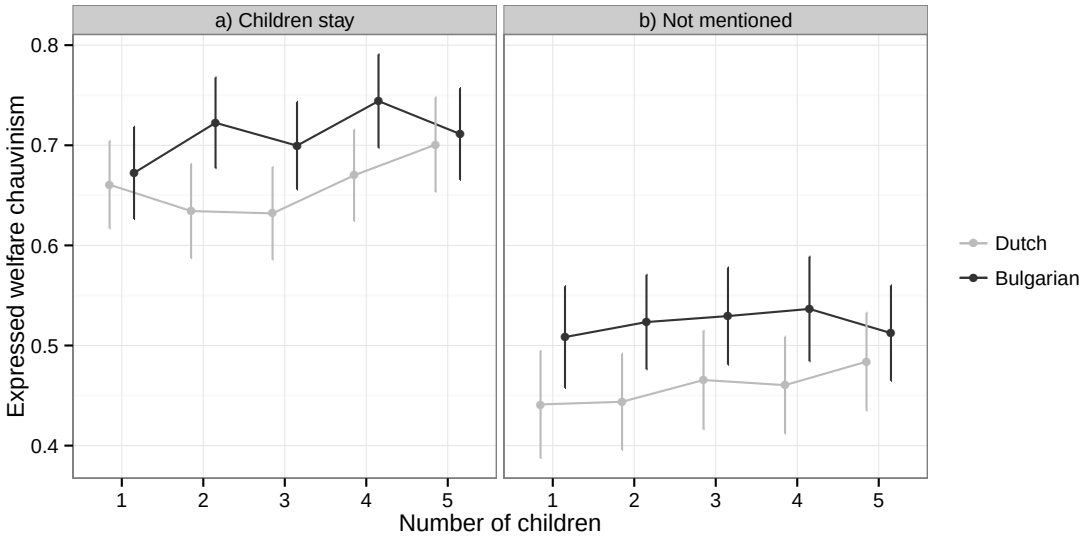


Figure A.9 Predicted levels of expressed welfare chauvinism in full factorial models, one with the 'children stay home' cue (panel a) and one without (panel b). The 'children stay home' cue has a large effect, and the effect of the nationality cue is clear and roughly constant in both panels. The effect of number of children is not as apparent, but approximately linear. There are no clear signs of interactive effects.

A.2 Appendix for 'European integration'

Survey items used

Table A.12 Measures of attitude variables, Denmark

Variable	Items	Scale
Ethnic prejudice	*I will now mention some points of view from general political debate which you may or may not agree with. Here is a card with five options. Please select one of the responses.*	
	Do you consider immigrants and refugees a threat to Danish culture?	1-2
Political engagement	How interested are you in European Union politics, i.e. matters regarding the EU?	1-5
Economic ideology	*Now I have a few questions to which I would like you to respond either yes or no.*	
	Government has too little control over private investment	1-3

Table A.13 Measures of attitude variables, Sweden

Variable	Items	Scale
Ethnic prejudice	*I will now read a number of policies which some people believe should be implemented in Sweden.*	
	Accept fewer refugees in Sweden?	1-5
	I will now read a number of visions of types of societies which some people believe we should aim for in the Sweden of the future.	
	Aim for a multicultural society with great tolerance toward people from foreign countries with different religions and ways of life?	0-10
Political engagement	*Now, a number of questions about the EU.*	
	How interested are you in questions regarding the EU?	1-4
Economic ideology	*I will now read a list of policies which some people believe should be implemented in Sweden.*	
	Reduce the size of the public sector?	1-5
	Lower taxes?	1-5
	Reduce income differences in society?	1-5
	Reduce the influence of financial markets?	1-5
	Run more health care on private hands?	1-5

Summary statistics

Table A.14 Summary statistics, Denmark

Statistic	N	Mean	St. Dev.	Min	Max
Identity-based vote	932	0.11	0.31	0	1
Ethnic prejudice (EP)	939	0.38	0.49	0	1
Political engagement (PE)	10	0.61	0.26	0	1
Gender (female)	10	0.53	0.50	0	1
Age	10	45.98	16.33	18	99
Education	10	0.47	0.32	0	1
Income	812	0.49	0.29	0	1

Table A.15 Summary statistics, Sweden

Statistic	N	Mean	St. Dev.	Min	Max
Identity-based vote	2,947	0.03	0.18	0	1
Ethnic prejudice (EP)	1,571	0.48	0.28	0	1
Political interest	1,882	0.61	0.26	0	1
Gender (female)	2,293	0.51	0.50	0	1
Age	2,293	46.07	16.38	18	80
Education	1,872	0.56	0.35	0	1
Income	1,808	0.21	0.28	0	1

Study 1: Regression results

Table A.16 Models of voting against auro adoption, Denmark

	(1)	(2)	(3)
Ethnic prejudice	0.66***	0.55***	0.54***
	(0.14)	(0.16)	(0.16)
Gender (f)		0.43***	0.41***
		(0.15)	(0.15)
Age		0.002	0.003
		(0.01)	(0.01)
Education		−0.77***	−0.74***
		(0.25)	(0.25)
Income		−1.01***	−1.00***
		(0.27)	(0.27)
Political interest			−0.19
			(0.31)
Constant	−0.28***	0.25	0.34
	(0.08)	(0.32)	(0.36)
N	939	771	771
Log Likelihood	-639.02	-502.25	-502.07
AIC	1,282.05	1,016.50	1,018.13

*p < .1; **p < .05; ***p < .01

Table A.17 Models of voting against auro adoption, Sweden

	(1)	(2)	(3)
Ethnic prejudice	0.35***	0.17	0.12
	(0.11)	(0.12)	(0.13)
Gender (f)		0.64***	0.60***
		(0.12)	(0.12)
Age		−0.01***	−0.01***
		(0.004)	(0.004)
Education		−1.11***	−0.99***
		(0.20)	(0.20)
Income		−1.63***	−1.58***
		(0.24)	(0.24)
Political interest			−1.01***
			(0.25)
Constant	0.01	1.46***	1.99***
	(0.08)	(0.28)	(0.31)
N	1,331	1,281	1,281
Log Likelihood	-911.30	-801.59	-793.42
AIC	1,826.60	1,615.17	1,600.84

*p < .1; **p < .05; ***p < .01

Study 2: Validation of measure of identity-based voting

Results from logit models of identity-based responses within each country are presented in Table A.18 and Table A.19. Figure A.10 plots the associations of interest.

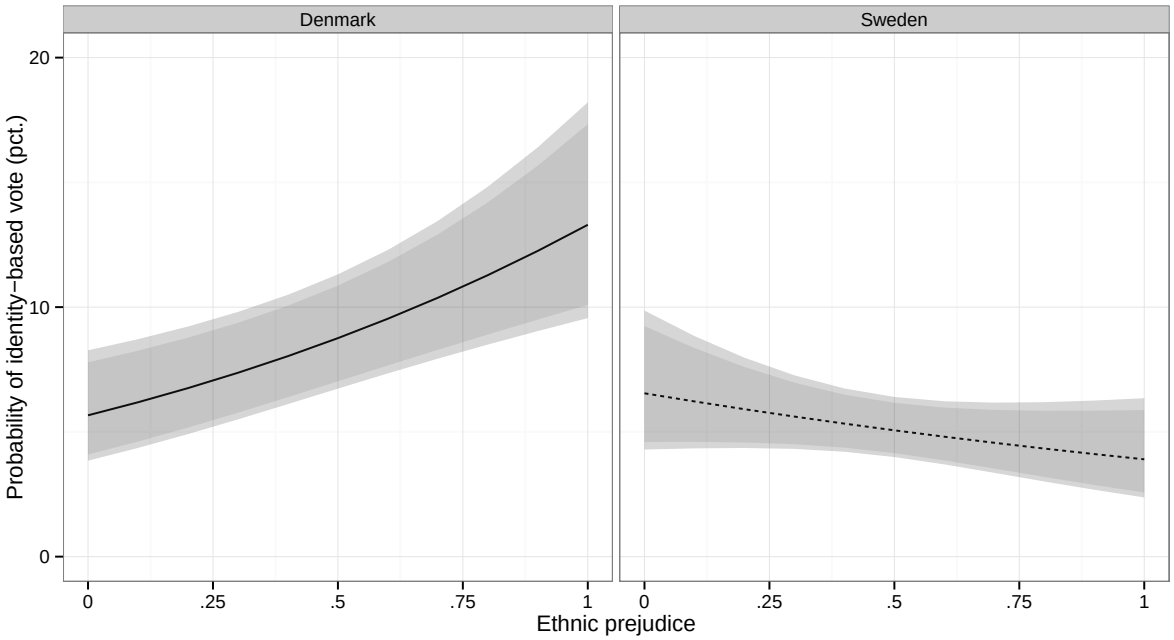


Figure A.10 Ethnic prejudice, political engagement and predicted probability of identity-based referendum vote in Denmark and Sweden. Ethnic prejudice is positively and significantly associated with identity-based voting in Denmark, but weakly and negatively in Sweden. Predicted probabilities are based on model 3 in table A.18 and A.19 respectively. Gray bands represent 90 and 95 pct. confidence intervals. Solid lines signify statistically significant associations (p<.05).

As shown in the left panel of Figure A.10, which plots predicted probabilities from model 3 for each country, ethnic prejudice is indeed significantly associated with identity-based responses among Danish voters. In other words, Danish voters with a negative attitude towards immigration are more likely to refer to national identity when explaining their vote on the euro. The increase is not only statistically, but substantively significant: across the observable range of ethnic prejudice, the predicted probability of an identity-based response increases from around 5 percent to around 14 percent.

In contrast, the relationship among Swedish voters, though significant, is weak and negative. The sample of identity-coded responses in Figure 5.4 provides a hint as to why: consistent with contemporary accounts of the campaign, the small minority of Swedish voters referring to identity in explaining their vote tend to be pro-euro voters referencing a European identity. Consistent with this, the coefficient on education, which is negative among Danish

Table A.18 Logit models of identity-based voting, Denmark

	(1)	(2)	(3)	(4)
Ethnic prejudice	1.07***	1.00***	0.94***	0.91***
	(0.23)	(0.27)	(0.27)	(0.28)
Gender (f)		0.73***	0.68**	0.69**
		(0.27)	(0.27)	(0.29)
Age		−0.002	−0.0003	0.004
		(0.01)	(0.01)	(0.01)
Education		−1.01**	−0.94**	−1.02**
		(0.45)	(0.46)	(0.48)
Income		−0.56	−0.53	−0.28
		(0.47)	(0.48)	(0.50)
Political interest			−0.65	−0.74
			(0.53)	(0.55)
Constant	−2.64***	−2.36***	−2.02***	−2.28***
	(0.17)	(0.55)	(0.61)	(0.66)
N	874	727	727	689
Log Likelihood	-284.52	-216.28	-215.51	-198.90
AIC	573.04	444.57	445.02	411.80

*p < .1; **p < .05; ***p < .01

Table A.19 Logit models of identity-based voting, Sweden

	(1)	(2)	(3)
Ethnic prejudice	−1.08***	−0.69	−0.55
	(0.39)	(0.42)	(0.42)
Gender (f)		−0.27	−0.20
		(0.22)	(0.22)
Age		0.02***	0.02**
		(0.01)	(0.01)
Education		1.15***	0.98***
		(0.36)	(0.36)
Income		0.64*	0.56
		(0.38)	(0.39)
Political interest			1.41***
			(0.47)
Constant	−2.19***	−4.06***	−4.84***
	(0.19)	(0.53)	(0.60)
N	1,571	1,513	1,513
Log Likelihood	-370.84	-334.76	-330.06
AIC	745.68	681.51	674.13

*p < .1; **p < .05; ***p < .01

voters, is positive in the Swedish case. As a result, Swedish voters high in ethnic prejudice are in fact slightly less likely to explain their vote in terms of identity.

Finally, model 4 in Table A.18 examines whether the results are robust to excluding Danish People's Party identifiers. None of the coefficients change substantially; most importantly, ethnic prejudice is still strongly and significantly associated with identity-based voting among voters low in political engagement. While partisan cue-taking is likely an important component in explaining voting on euro adoption in general, the robustness of the result to excluding Danish People's Party identifiers suggest that it cannot explain the observable evidence for group implication.

Study 3: Robustness checks

Table A.20 Models predicting opposition to European integration, unlogged salience measure

	Agree 'unification gone too far'			
	Basic	+political vars	+year FE	w/o DK and SE
	(1)	(2)	(3)	(4)
Imm. salience (IS)	$-.013^{**}$	$-.014^{***}$	$-.018^{***}$	$-.024^{***}$
	(.004)	(.004)	(.005)	(.006)
Ethnic prejudice (EP)	$.306^{***}$	$.296^{***}$	$.297^{***}$	$.288^{***}$
	(.019)	(.020)	(.020)	(.024)
IS × EP	$.025^{**}$	$.027^{***}$	$.027^{***}$	$.038^{***}$
	(.008)	(.008)	(.008)	(.011)
Controls				
Gender (f)	$.016^{***}$	$.012^{***}$	$.012^{***}$	$.011^{***}$
	(.002)	(.002)	(.002)	(.002)
Age	$.001^{***}$	$.001^{***}$	$.001^{***}$	$.001^{***}$
	(.0001)	(.0001)	(.0001)	(.0001)
Education (years)	$-.001^{***}$	$-.001^{***}$	$-.001^{***}$	$-.001^{***}$
	(.0001)	(.0001)	(.0001)	(.0001)
Ideology		$-.041^{***}$	$-.041^{***}$	$-.026^{*}$
		(.010)	(.010)	(.010)
Pol. interest		$.054^{***}$	$.053^{***}$	$.057^{***}$
		(.004)	(.005)	(.005)
Constant	$.379^{***}$	$.375^{***}$	$.373^{***}$	$.366^{***}$
	(.026)	(.026)	(.018)	(.018)
N	141,125	127,373	127,373	111,940
Country FE	✓	✓	✓	✓
Year FE			✓	✓
R^2	.141	.144	.148	.151
Adjusted R^2	.141	.143	.148	.150

$^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table A.21 Models predicting opposition to European integration, only cases with election in past 2 years

	Agree 'unification gone too far'			
	Basic	+political vars	+year FE	w/o DK and SE
	(1)	(2)	(3)	(4)
Imm. salience (IS)	$-.030^{**}$	$-.031^{***}$	$-.038^{***}$	$-.038^{***}$
	(.009)	(.009)	(.008)	(.010)
Ethnic prejudice (EP)	$.335^{***}$	$.326^{***}$	$.328^{***}$	$.332^{***}$
	(.016)	(.015)	(.016)	(.017)
IS × EP	$.042^{**}$	$.049^{***}$	$.054^{***}$	$.055^{**}$
	(.016)	(.015)	(.015)	(.018)
Controls				
Gender (f)	$.018^{***}$	$.014^{***}$	$.014^{***}$	$.012^{***}$
	(.002)	(.002)	(.002)	(.002)
Age	$.001^{***}$	$.001^{***}$	$.001^{***}$	$.001^{***}$
	(.0001)	(.0001)	(.0001)	(.0001)
Education (years)	$-.001^{***}$	$-.001^{***}$	$-.001^{***}$	$-.001^{***}$
	(.0001)	(.0001)	(.0001)	(.0002)
Ideology		$-.041^{***}$	$-.041^{***}$	$-.027^{*}$
		(.012)	(.012)	(.012)
Pol. interest		$.058^{***}$	$.057^{***}$	$.060^{***}$
		(.004)	(.005)	(.005)
Constant	$.360^{***}$	$.348^{***}$	$.340^{***}$	$.325^{***}$
	(.014)	(.015)	(.017)	(.019)
N	103,259	92,589	92,589	82,368
Country FE	✓	✓	✓	✓
Year FE			✓	✓
R^2	.144	.147	.152	.154
Adjusted R^2	.144	.147	.151	.154

$^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table A.22 Models predicting opposition to European integration, standard errors clustered at the country level

	Agree 'unification gone too far'			
	Basic	+political vars	+year FE	w/o DK and SE
	(1)	(2)	(3)	(4)
Imm. salience (IS)	$-.026^{***}$	$-.028^{***}$	$-.035^{***}$	$-.035^{***}$
	(.008)	(.008)	(.007)	(.009)
Ethnic prejudice (EP)	$.340^{***}$	$.331^{***}$	$.332^{***}$	$.337^{***}$
	(.026)	(.025)	(.026)	(.028)
IS $\times$ EP	$.043^{*}$	$.048^{**}$	$.054^{**}$	$.053^{*}$
	(.019)	(.017)	(.017)	(.021)
Controls				
Gender (f)	$.016^{***}$	$.012^{***}$	$.012^{***}$	$.010^{***}$
	(.002)	(.003)	(.003)	(.003)
Age	$.001^{***}$	$.001^{***}$	$.001^{***}$	$.001^{***}$
	(.0001)	(.0001)	(.0001)	(.0001)
Education (years)	$-.001^{***}$	$-.001^{***}$	$-.001^{***}$	$-.001^{***}$
	(.0001)	(.0001)	(.0001)	(.0001)
Ideology		$-.041^{*}$	$-.041^{*}$	$-.026$
		(.020)	(.020)	(.019)
Pol. interest		$.054^{***}$	$.053^{***}$	$.057^{***}$
		(.008)	(.008)	(.008)
Constant	$.366^{***}$	$.360^{***}$	$.350^{***}$	$.336^{***}$
	(.015)	(.020)	(.025)	(.024)
N	141,125	127,373	127,373	111,940
Country FE	✓	✓	✓	✓
Year FE			✓	✓
R^2	.142	.144	.149	.151
Adjusted R^2	.141	.144	.148	.150

$^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table A.23 Placebo test: Models predicting opposition to gay rights

	Disagree 'gays should be free to live their life'			
	Basic	+political vars	+year FE	w/o DK and SE
	(1)	(2)	(3)	(4)
Imm. salience (IS)	−.010	−.010	.002	.002
	(.010)	(.010)	(.006)	(.008)
Ethnic prejudice (EP)	.174***	.167***	.165***	.163***
	(.010)	(.009)	(.009)	(.010)
IS × EP	.0004	−.006	−.015	−.013
	(.010)	(.010)	(.009)	(.012)
Controls				
Gender (f)	−.047***	−.053***	−.053***	−.052***
	(.002)	(.002)	(.002)	(.002)
Age	.003***	.003***	.003***	.003***
	(.0001)	(.0001)	(.0001)	(.0001)
Education (years)	−.001***	−.001***	−.001***	−.001***
	(.0003)	(.0003)	(.0003)	(.0003)
Ideology		.089***	.088***	.095***
		(.013)	(.013)	(.014)
Pol. interest		.056***	.056***	.058***
		(.005)	(.005)	(.005)
Constant	.097***	.034	.044**	.032*
	(.018)	(.017)	(.013)	(.015)
N	146,561	130,406	130,406	114,452
Country FE	✓	✓	✓	✓
Year FE			✓	✓
R^2	.215	.224	.231	.219
Adjusted R^2	.215	.224	.231	.219

*p < .05; **p < .01; ***p < .001

Distributions of variables

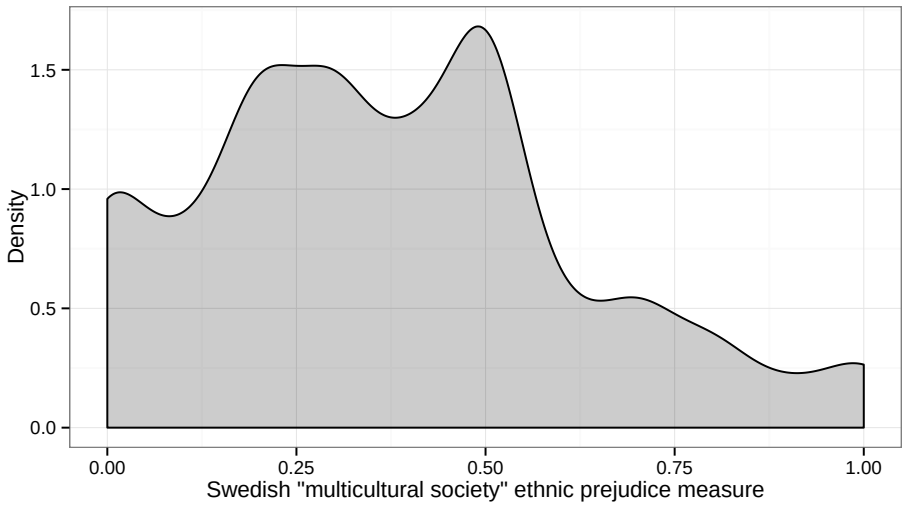


Figure A.11 Distribution of 11-point scale agreement that “[Sweden should a]im for a multicultural society with great tolerance toward people from foreign countries with different religions and ways of life” ($N = 855$)

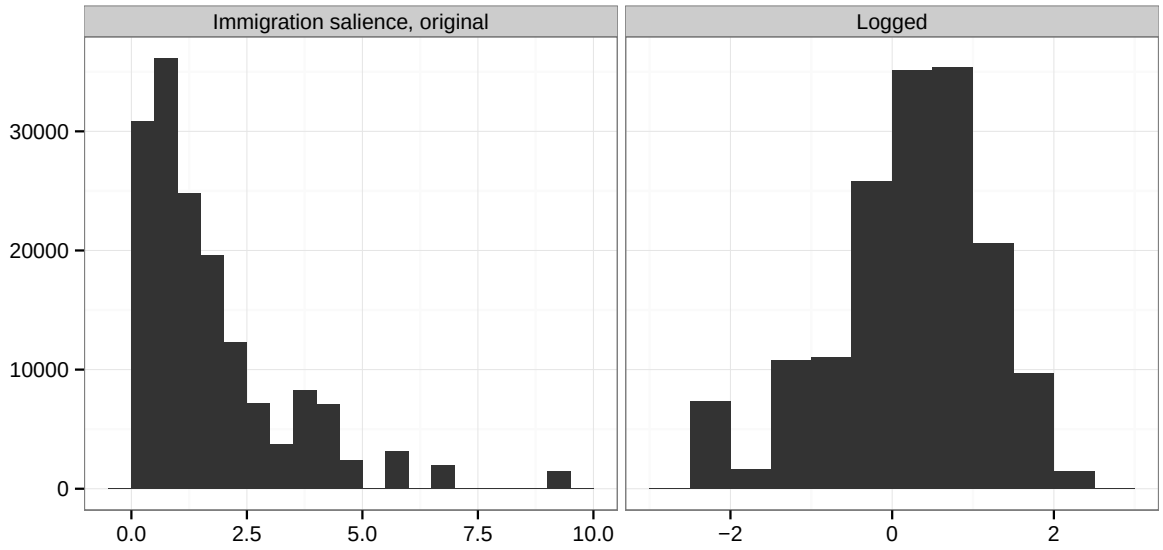


Figure A.12 Distributions of original and logged versions of immigration salience measures. Based on CMP data on percentage mentions of the category 'Multiculturalism' (codes 607 and 608) in national elections in EU member countries. For both variables $N = 158,952$.

A.3 Appendix for 'Immigration debate'

Description of the ReadMe method

First, assume we have a set of documents $D_i, \ldots, D_N$, where N is so large as to make hand coding of the entire set infeasible. For this full set of documents, we want to know the distribution of documents across J categories. We assume that each document D_i belongs to one category $j = 1, \ldots, J$. Lastly, we represent each document i as a *word stem profile* $\mathbf{S}_i$, i.e. a vector denoting the presence or absence of each of the word stems present in the entire document set.[7] For K word stems present in the full set of documents, each document is represented as one of 2^K possible word stem profiles.

The model assumes that $\mathbf{S}_i$ depends on D_i, i.e. the word profile of a document depends on the category to which it belongs. In the context of this study, this corresponds to assuming that someone writing an article negative towards immigration will use a set of words different from someone writing an article positive about immigration. For any given article, we can then characterize the probability of a specific word stem profile s as follows:

$$P(\mathbf{S} = s) = \sum_{j=1}^{J} P(\mathbf{S} = s | D = j) \times P(D = j) \tag{2}$$

Rewriting (2) as a matrix expression, we get

$$\underset{2^K \times 1}{P(\mathbf{S})} = \underset{2^K \times J}{P(\mathbf{S}|D)} \times \underset{J \times 1}{P(D)} \tag{3}$$

where the key quantity of interest is the vector $P(D)$, i.e. the distribution of documents across the J categories. $P(\mathbf{S})$ can be estimated directly from the observed distribution of word stem profiles in the full set of documents. $P(\mathbf{S}|D)$ cannot be estimated directly, but we estimate it from the relation between document categories and word stem profiles in the hand-coded training set. In other words, we assume that for each category, words are used with the same probabilities in the training set as in the full set of texts. With estimates of $P(\mathbf{S})$ and $P(\mathbf{S}|D)$, *ReadMe* solves for $P(D)$ and provides estimates of the proportion of documents in each category.

[7] For example, assume two documents D_1="immigration is good" and D_2 ="immigration should stop". Here, we have (ignoring stemming) $K = 5$ words {immigration, is, good, should, stop}, and for D_1 the word stem profile is $\mathbf{S}_1 = \{1, 1, 1, 0, 0\}$.

By using the hand-coded training set to estimate the relationship between document categories and word profiles, *ReadMe* thus takes a standard supervised learning approach to content analysis by drawing information from the 'true' classifications from a subset of documents. The distinguishing feature of *ReadMe* is that it estimates $P(D)$ directly, i.e. provides estimates only of the proportions of documents in each category. In contrast, most other approaches try to classify each document individually and then aggregate up to estimate proportions. Skipping the step of individual classification allows for estimating proportions using weaker assumptions. And aggregate proportions are typically the phenomenon of real interest to social scientists. As Hopkins and King (2010) argue, rather than trying to find the needle in the haystack, social scientists are typically interested in characterizing the haystack.

Validation of ReadMe coding

A separate question when developing a training set is how many articles to code. Hopkins et al. (2010) recommend coding around 500 articles, beyond which the advantage of more hand coding begins to level off. In order to test this assumption, we perform hold-out validation, running *ReadMe* on a randomly selected half of coded articles and compare the estimates with the known proportions. The full set of hand coded articles is itself randomly sampled from the full sample of articles. Figure A.13 shows the root mean square error (RMSE) from this validation as we hand code progressively more articles.

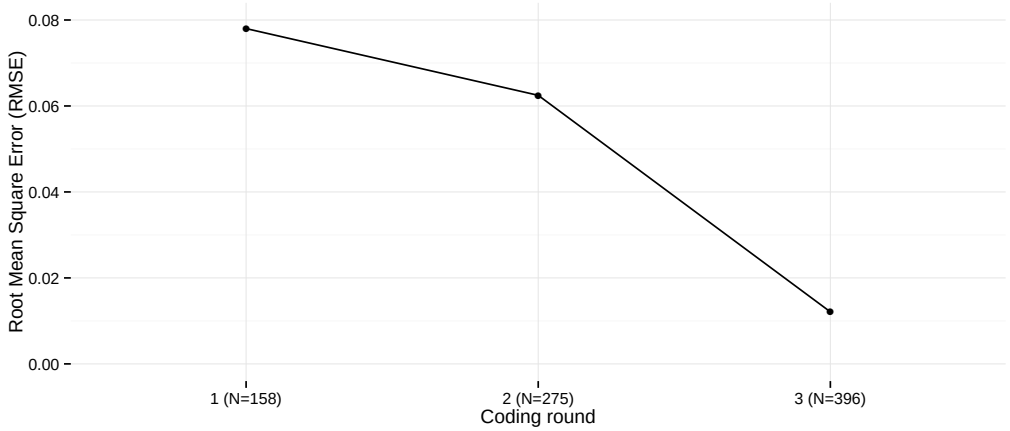


Figure A.13 Classification error decreases as more hand-coded articles are included in the training set.

As shown, the RMSE decreases as we hand code more articles, reflecting that *ReadMe* gets progressively more accurate. At around 400 hand coded articles, the RMSE is around 1 percent, meaning that estimated proportions deviate just 1 percent from true proportions on

average. Using these 400 hand coded articles as a training set, we proceed to estimate the content of the remaining $\sim$ 68,000 articles.

Examples of hand coded newspaper articles

'Positive' example

```
Fredsprocessen på Vesterbro

Unge indvandrere flytter grænser i et enestående forsøg på at nedbryde had og fordomme
i Mellemøsten ved hjælp af teater på Vesterbro i København. af Michael von Bülow
Palæstinensere og jøder i sprængfarligt teaterprojektEn yngre mand træder frem
på scenen.  Han præsenterer sig som palæstinenser. Endnu en mand træder frem.
Han præsenterer sig som jøde. I en symbolsk gestus flytter palæstinenseren arabertørklædet
fra sit eget hoved over på jødens. Sådan begynder et teaterstykke, som fra i aften
kan opleves på Terra Nova teatret på Vesterbrogade i København. Hvem er hvem? De
medvirkende er unge, herboende palæstinensere og jøder, men undervejs i forestillingen
kan man ikke se, hvem der er hvem.  Og det er netop pointen.Stykket foregår på
et tidspunkt, hvor jøder, kristne og muslimer levede i fredelig harmoni med hinanden
i det land, der i dag kaldes Israel. En situation der var virkelighed for mindre
end hundrede år siden. Nu om stunder omgås jøder og palæstinensere ikke med hinanden,
heller ikke i Danmark. Derfor måtte de medvirkende flytte nogle grænser hos sig
selv, før de kunne begynde at flytte andres. »Det er utroligt, hvordan mange års
had har hjernevasket os.  I starten måtte vi undgå politiske diskussioner, for
de tændte øjeblikkeligt.  Vi måtte også finde en abstrakt måde at fortælle vores
fælles historie på,« siger Doron Aviman, som er den ene af stykkets instruktører.
Han er jøde, men spiller rollen som den palæstinensiske bonde Dawoud, der forelsker
sig i pigen Leila. (...)
```

'Neutral' example

```
Finanslov:  Kortere ventetid for asylansøgere

af Rikke Egelund og Kjeld Hybel 45 nye medarbejdere i Udlændingestyrelsen skal
sætte fut under sagsbehandlingen i asylsager, så sagsbehandlingstiden i løbet af
dette år nedbringes til mindre end halvdelen af, hvad den er i dag. Sidste år tog
det i gennemsnit 203 dage at afgøre en asylansøgning - men ved udgangen af i år
skal tiden være ned på 90 dage pr. sag. I finanslovsforslaget er der sat 15 millioner
af til at lønne det nye personale, som allerede er ved at begynde arbejdet i styrelsen.
Der er også sat 700 millioner kroner - 60 millioner mere end sidste år - af til
at indkvartere ansøgerne, mens myndighederne behandler deres sag. Og her satser
regeringen. I prognosen for de kommende tre år skal udgiften nedbringes til en
god tredjedel - og antallet af ansøgere, der venter, fra 8.774 'helårspersoner'
til 3.214.I øjeblikket er næsten 11.000 personer indkvarteret, mens de venter -
```

enten på at få asyl eller på at blive sendt ud af landet. 12.000 søgte sidste år
om asyl i Danmark - lidt flere end 6.000 fik opholdstilladelse. Kontorchef i Integrationsmini
Henrik Kyvsgaard forklarer, at beregningerne i finansloven er lavet ud fra det
nuværende retsgrundlag - at der ikke er beregnet besparelser efter de stramninger,
som regeringen fremsætter i Folketinget i slutningen af næste måned. Besparelserne
i løbet af de kommende år skal ske, alene ved at sagsbehandlingen i Udlændingestyrelsen
kommer op i tempo. (...)

'Negative' example

Flygtninge forfalsker stjålne pas

EKS af Philip Lauritzen Ekstra Bladet afslører en livlig sortbørs-handel i Sandholm-lejren
med danske pas, der for asyl-ansøgere er frikort til hele verden Hvorfor vente
i månedsvis, ja årevis, på at få asyl i Danmark, når man så let som at klø sig
i nakken kan købe det eftertragtede danske pas illegalt? I asyl-centret Sandholm
er originale danske pas til salg hver dag, og igen er det tilsyneladende den lille,
men meget effektive øst-mafia bestående af blandt andet russere, baltere og andre
fra den tidligere østblok, der er leveringsdygtig.Ved siden af deres sortbørsmarked
med alskens stjålen isenkram - alt fra videokameraer og CD'ere til sko og nyt modetøj
som omtalt i Ekstra Bladet forleden - har de også et yderst velfungerende paskontor.
Her kan man dels købe originale danske pas, dels kan man får passet tilpasset med
for eksempel ens eget foto. Man skal blot selv stille med et pasfoto. Få dage
efter leveres et vaskeægte dansk rødbedepas med nyt foto, flot prægestemplet af
Politidirektøren i København - lige til at rejse på. Pris 5.500 kr. Men så ligger
verden og specielt EU også åben, hvis man ikke orker at vente på asyl men gerne
vil slå sig ned i en beskyttet tilværelse i Vesten som 'EU-borger'. Med et dansk
EU-pas er verden og specielt Europa pludselig helt åben, venlig og imødekommende.
KØB ET PAS Måske lyder det som en lidt for god røverhistorie, og på Ekstra Bladet
var vi også noget skeptiske, da vi første gang blev præsenteret for oplysningen
om, at det var pære-nemt at købe et pas i Sandholm-lejren. (...)

A.4 Appendix for 'Local contexts'

Surveys and items used

No.	Name	N	Year	Muni.	Zip	Reference
1	Danish National Election Study 1990	1,008	1991	No	Yes	Andersen et al. (1991)
2	Danish National Election Study 1994	2,021	1994	Yes	No	Andersen and Borre (2002)
3	Danish National Election Study 1998	2,001	1998	No	Yes	Andersen et al. (1999)
4	Euro Referendum Survey	1,056	2000	Yes	No	Worre and Nielsen (2003)
5	Danish National Election Study 2001	2,126	2002	Yes	Yes	Andersen et al. (2003)
6	Danish National Election Study 2005	2,264	2005	No	Yes	Andersen (2007)
7	Political Attitudes in Political Context	6,370	2006	Yes	No	Stubager et al. (2012)
8	Danish National Election Study 2007	4,018	2007	Yes	Yes	Andersen (2012)
9	European Social Survey round 5	1,576	2010	Yes	No	European Social Survey (2010)
10	Danish National Election Study 2011	2,078	2011	Yes	Yes	Stubager et al. (2013)

Table A.24 Overview of included surveys.

Survey no.	Item	Scale
	Anti-immigration	
1	Arab countries are a threat to Danish culture	1-5
2	Immigration is a threat to Danish culture	1-5
3	Immigration is a threat to Danish culture	1-5
4	Immigrants and refugees threaten Danish culture	1-2
5	Immigration is a threat to Danish culture	1-5
6	Immigration is a threat to Danish culture	1-5
7	Immigration is a threat to Danish culture	1-5
8	Immigration is a threat to Danish culture	1-5
9	Country's cultural life undermined or enriched by immigrants	1-4
10	Immigration is a threat to Danish culture	1-5
	Crime	
1	There should be tougher punishments for violent crime	1-5
2	There should be tougher punishments for violent crime	1-5
3	There should be tougher punishments for violent crime	1-5
4	There should be more control with police methods	1-2
5	There should be tougher punishments for violent crime	1-5
6	There should be tougher punishments for violent crime	1-5
7	Violent crime should be punished more severely than today	1-5
8	There should be tougher punishments for violent crime	1-5
9	People who break the law much harsher sentences	1-5
10	There should be tougher punishments for violent crime	1-5

Table A.25 Overview of items used to measure anti-immigration and crime attitudes.

Summary statistics

Table A.26 Summary statistics, municipality data

Statistic	N	Mean	St. Dev.	Min	Max
Main variables:					
Anti-immigration	24,777	0.46	0.39	0	1
Ethnic diversity	24,074	0.06	0.05	0.001	0.44
Crime attitude	24,518	0.58	0.49	0	1
Pro-environment	24,599	0.36	0.48	0	1
Context-level controls:					
Muni. education level	24,069	0.22	0.08	0.08	0.47
Muni. population	24,069	0.01	0.01	0.0002	0.05
Individual-level controls:					
Gender (f)	25,520	0.49	0.50	0	1
Age	25,498	46.07	16.86	13	104
Education level	23,907	0.57	0.31	0	1
Household income	15,792	0.47	0.27	0	1
Employment status: student	25,491	0.09	0.28	0	1
Employment status: pensioner	25,520	0.20	0.40	0	1
Leftist party (prev. elec.)	25,520	0.44	0.50	0	1
Left/right self-placement	24,092	0.56	0.25	0	1.11

Table A.27 Summary statistics, zip code data

Statistic	N	Mean	St. Dev.	Min	Max
Main variables:					
Concern about immigration	49,027	0.53	0.36	0	1
Ethnic diversity	43,001	0.05	0.04	0.001	0.51
Concern about crime	52,545	0.77	0.30	0	1
Concern about pollution	54,307	0.74	0.30	0	1
Context-level controls:					
Zip avg. income	43,001	1.12	0.26	0.49	2.85
Zip avg. education	43,001	11.54	0.61	9.16	16
Zip population	43,001	0.34	0.20	0.0001	0.94
Individual-level controls:					
Zip population	43,001	0.34	0.20	0.0001	0.94
Gender (f)	56,477	0.51	0.50	0	1
Age (10 yrs)	56,444	4.40	1.76	1.70	8
Education level	55,556	0.43	0.15	0	1

Regression tables and robustness checks

Table A.28 Random effects models using municipality data

	Prefer stricter punishments for violent crime			
	(1)	(2)	(3)	(4)
Anti-immigration (Imm)	1.17***	1.06***	1.35***	1.23***
	(0.06)	(0.06)	(0.06)	(0.06)
Ethnic diversity (ED)	−1.92***	−1.67**	−2.47***	−2.15***
	(0.67)	(0.71)	(0.56)	(0.57)
Imm × ED	5.69***	5.21***	5.03***	4.66***
	(0.81)	(0.81)	(0.82)	(0.83)
Context-level controls:				
Muni. education level	−1.83***	−2.08***	−1.35***	−1.49***
	(0.36)	(0.38)	(0.28)	(0.29)
Muni population	−26.81***	−26.61***	−0.49	0.50
	(4.71)	(7.95)	(2.61)	(2.95)
Individual-level controls:				
Gender (f)	0.02	0.03	0.03	0.04
	(0.03)	(0.03)	(0.03)	(0.03)
Age	−0.01***	−0.01***	−0.02***	−0.02***
	(0.001)	(0.001)	(0.001)	(0.001)
Education level	−0.68***	−0.69***	−0.86***	−0.87***
	(0.05)	(0.05)	(0.06)	(0.06)
Household income	−0.10	−0.20***	−0.13**	−0.22***
	(0.06)	(0.06)	(0.06)	(0.06)
Student	−0.05	−0.09*	−0.12**	−0.16***
	(0.05)	(0.05)	(0.06)	(0.06)
Pensioner	0.06	0.04	0.08*	0.06
	(0.04)	(0.05)	(0.05)	(0.05)
Left-wing voter		−0.31***		−0.30***
		(0.03)		(0.03)
Left/right self-placement		0.42***		0.40***
		(0.06)		(0.07)
Intercept	1.45***	1.47***	1.33***	1.32***
	(0.10)	(0.11)	(0.22)	(0.22)
N	23,356	23,356	23,356	23,356
Municipality random intercepts	✓	✓	✓	✓
Year random intercepts			✓	✓
Log Likelihood	−14,511.59	−14,402.20	−14,001.78	−13,904.15
AIC	29,049.19	28,834.40	28,031.55	27,840.29
BIC	29,153.95	28,955.28	28,144.38	27,969.23

*p < .1; **p < .05; ***p < .01

Table A.29 Random effects models using zip code data

	Concern about crime			
	(1)	(2)	(3)	(4)
Immigration concern (Imm)	0.20^{***}	0.19^{***}	0.20^{***}	0.19^{***}
	(0.01)	(0.01)	(0.01)	(0.01)
Ethnic diversity (ED)	-1.04^{***}	-1.02^{***}	-0.78^{***}	-0.76^{***}
	(0.08)	(0.08)	(0.08)	(0.08)
Imm $\times$ ED	1.24^{***}	1.26^{***}	1.22^{***}	1.23^{***}
	(0.10)	(0.10)	(0.10)	(0.10)
Context-level controls:				
Zip avg. income	-0.21^{***}	-0.21^{***}	-0.02	-0.04
	(0.01)	(0.01)	(0.02)	(0.02)
Zip avg. education	-0.01^{*}	-0.004	-0.03^{***}	-0.02^{***}
	(0.01)	(0.01)	(0.01)	(0.01)
Zip population	-0.001	0.0003	0.001	0.003
	(0.01)	(0.01)	(0.01)	(0.01)
Individual-level controls:				
Gender (f)		0.08^{***}		0.08^{***}
		(0.003)		(0.003)
Age		0.03^{***}		0.03^{***}
		(0.005)		(0.004)
Age^2		-0.003^{***}		-0.003^{***}
		(0.0005)		(0.0005)
Education		-0.16^{***}		-0.16^{***}
		(0.01)		(0.01)
Intercept	1.06^{***}	0.91^{***}	1.05^{***}	0.90^{***}
	(0.06)	(0.06)	(0.05)	(0.05)
N	40,614	39,838	40,614	39,838
Municipality random intercepts	✓	✓	✓	✓
Year random intercepts			✓	✓
Log Likelihood	−6,147.26	−5,516.58	−5,816.01	−5,203.16
AIC	12,312.51	11,059.15	11,652.01	10,434.33
BIC	12,390.02	11,170.86	11,738.13	10,554.62

$^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

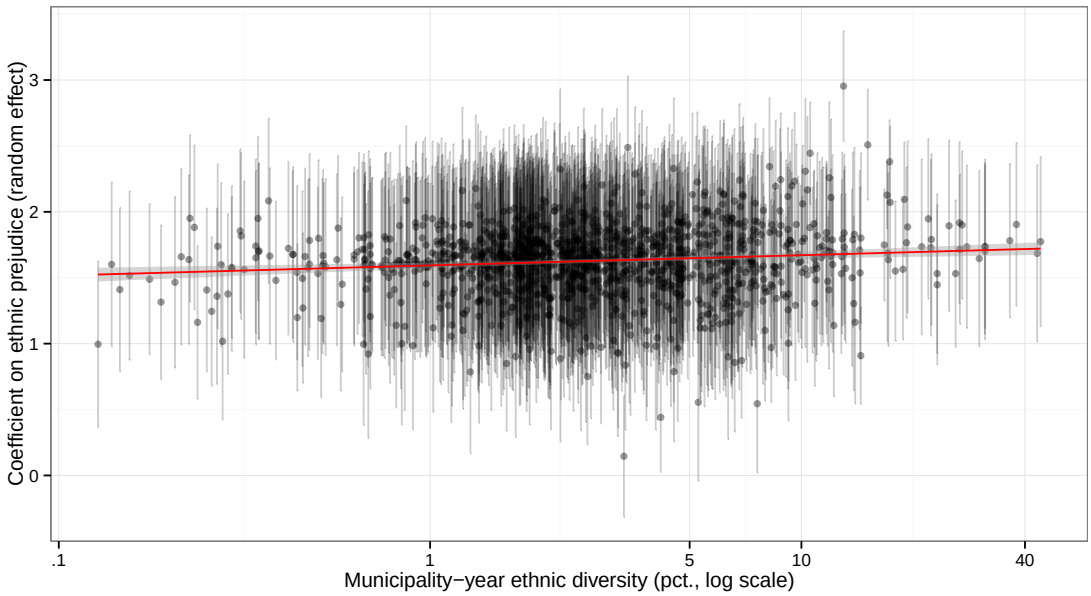


Figure A.14 Municipality-year level ethnic diversity plotted against random slopes from a random effects model allowing the coefficient of ethnic prejudice on crime attitudes to vary by municipality-year. Consistent with expectations, the two are more strongly associated in more ethnically diverse municipality-years. The increase in slopes is significant ($\beta = .12, t = 4.2, p < .001$).

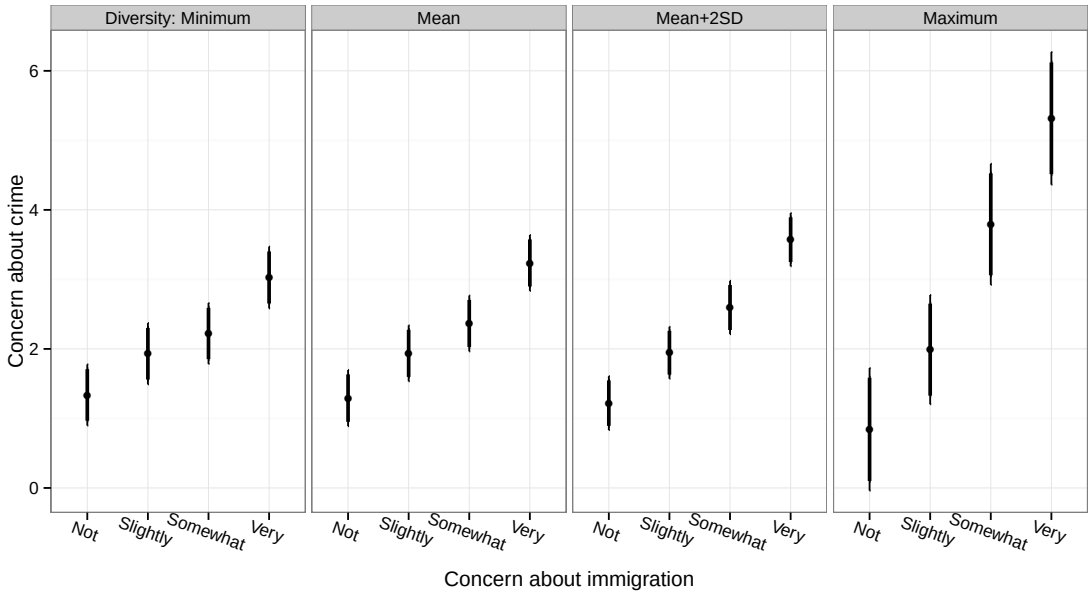


Figure A.15 Predicted associations between anti-immigration and crime attitudes at various levels of ethnic diversity, using ordinal logit models. Thick and thin error bars represent 90 and 95 percent confidence intervals respectively.

Table A.30 Placebo models using municipality data

	Prefer more concern for the environment			
	(1)	(2)	(3)	(4)
Anti-immigration (Imm)	-0.62^{***}	-0.51^{***}	-0.68^{***}	-0.50^{***}
	(0.08)	(0.08)	(0.09)	(0.09)
Ethnic diversity (ED)	1.91	1.60	-0.70	-1.14
	(4.18)	(4.30)	(2.09)	(2.18)
Imm $\times$ ED	2.14	2.69^{*}	1.21	1.88
	(1.15)	(1.10)	(1.39)	(1.28)
Context-level controls:				
Muni. education level	10.57^{**}	11.30^{**}	-1.85	-2.05
	(3.45)	(3.55)	(1.80)	(1.86)
Muni population	-358.09^{***}	-365.93^{***}	-11.99	-11.20
	(104.43)	(106.84)	(17.04)	(17.75)
Individual-level controls:				
Gender (f)	0.03	0.01	0.01	-0.001
	(0.04)	(0.03)	(0.04)	(0.04)
Age	0.001	0.0004	-0.002	-0.003
	(0.002)	(0.002)	(0.002)	(0.002)
Education level	0.42^{***}	0.42^{***}	0.15^{*}	0.15^{*}
	(0.07)	(0.07)	(0.07)	(0.07)
Household income	-0.08	0.07	-0.06	0.14
	(0.09)	(0.09)	(0.07)	(0.08)
Student	-0.08	-0.04	-0.17^{**}	-0.12
	(0.07)	(0.07)	(0.07)	(0.07)
Pensioner	0.15^{*}	0.18^{**}	0.18^{**}	0.22^{***}
	(0.06)	(0.06)	(0.06)	(0.06)
Left-wing voter		0.20^{***}		0.31^{***}
		(0.03)		(0.04)
Left/right self-placement		-0.82^{***}		-1.18^{***}
		(0.09)		(0.11)
Intercept	14.09^{**}	14.49^{**}	2.96^{**}	3.40^{**}
	(4.76)	(4.86)	(1.08)	(1.11)
N	23,472	23,472	23,472	23,472
Municipality fixed effects	✓	✓	✓	✓
Year fixed effects			✓	✓
R^2	0.14	0.15	0.38	0.40

$^{*}p < .05$; $^{**}p < .01$; $^{***}p < .001$

Table A.31 Placebo models using zip code data

	Concern about pollution			
	(1)	(2)	(3)	(4)
Immigration concern (Imm)	0.09***	0.09***	0.08***	0.09***
	(0.01)	(0.01)	(0.01)	(0.01)
Ethnic diversity (ED)	−0.45**	−0.42*	0.18	0.18
	(0.17)	(0.17)	(0.15)	(0.15)
Imm × ED	0.06	0.05	0.09	0.08
	(0.13)	(0.13)	(0.13)	(0.13)
Context-level controls:				
Zip avg. income	−0.17***	−0.16***	0.11**	0.11**
	(0.02)	(0.02)	(0.04)	(0.03)
Zip avg. education	−0.14***	−0.15***	−0.001	−0.02
	(0.02)	(0.02)	(0.02)	(0.02)
Zip population	0.24	0.17	−0.14	−0.19
	(0.19)	(0.18)	(0.18)	(0.17)
Individual-level controls:				
Gender (f)		0.09***		0.09***
		(0.003)		(0.003)
Age		0.06***		0.06***
		(0.005)		(0.005)
Age^2		−0.01***		−0.01***
		(0.001)		(0.001)
Education		0.07***		0.07***
		(0.01)		(0.01)
Intercept	2.57***	2.48***	0.02	0.06
	(0.50)	(0.49)	(0.40)	(0.40)
N	42,170	41,393	42,170	41,393
Zip code fixed effects	✓	✓	✓	✓
Year fixed effects			✓	✓
R^2	0.09	0.13	0.10	0.13
Adjusted R^2	0.09	0.13	0.10	0.13
chi^2	4,090.09***	5,668.91***	4,436.51***	5,985.37***

*p < .05; **p < .01; ***p < .001

Table A.32 Ordinal logit models using zip code data

	Concern about crime (ordinal)			
	(1)	(2)	(3)	(4)
y >'Slightly'	2.36	2.18	1.02	1.06
	(2.30)	(2.30)	(2.52)	(2.48)
y >'Somewhat'	0.76	0.56	−0.59	−0.57
	(2.30)	(2.30)	(2.52)	(2.48)
y >'Very'	−0.58	−0.82	−1.95	−1.97
	(2.30)	(2.30)	(2.52)	(2.48)
Imm >'Slightly'	0.58***	0.61***	0.64***	0.67***
	(0.03)	(0.03)	(0.03)	(0.03)
Imm >'Somewhat'	1.04***	1.03***	1.07***	1.06***
	(0.03)	(0.03)	(0.03)	(0.03)
Imm >'Very'	1.91***	1.88***	1.94***	1.91***
	(0.04)	(0.04)	(0.04)	(0.04)
Ethnic diversity (ED)	−0.12*	−0.13*	−0.04	−0.04
	(0.05)	(0.05)	(0.04)	(0.04)
ED×Imm >'Slightly'	0.07**	0.08**	0.04	0.05
	(0.03)	(0.03)	(0.03)	(0.03)
ED×Imm >'Somewhat'	0.17***	0.17***	0.15***	0.15***
	(0.03)	(0.03)	(0.03)	(0.03)
ED×Imm >'Very'	0.21***	0.22***	0.20***	0.21***
	(0.03)	(0.04)	(0.03)	(0.04)
Context-level controls:				
Zip avg. income	−0.21***	−0.22***	−0.01	−0.04
	(0.05)	(0.05)	(0.06)	(0.06)
Zip avg. education	−0.46***	−0.41***	−0.29*	−0.26*
	(0.11)	(0.11)	(0.12)	(0.11)
Zip population	−1.02**	−1.00**	−0.87***	−0.83**
	(0.32)	(0.32)	(0.26)	(0.27)
Individual-level controls:				
Gender (f)		0.63***		0.63***
		(0.02)		(0.02)
Age		0.39***		0.40***
		(0.06)		(0.06)
Age^2		−0.32***		−0.33***
		(0.06)		(0.06)
Education		−0.20***		−0.20***
		(0.01)		(0.01)
N	40,614	39,838	40,614	39,838
Zip code fixed effects	✓	✓	✓	✓
Year fixed effects			✓	✓
R^2	0.18	0.21	0.19	0.23
chi^2	7,072.92***	8,417.61***	7,745.16***	9,082.08***

*p < .05; **p < .01; ***p < .001

Ethnic diversity: crime rates and trends vs. levels

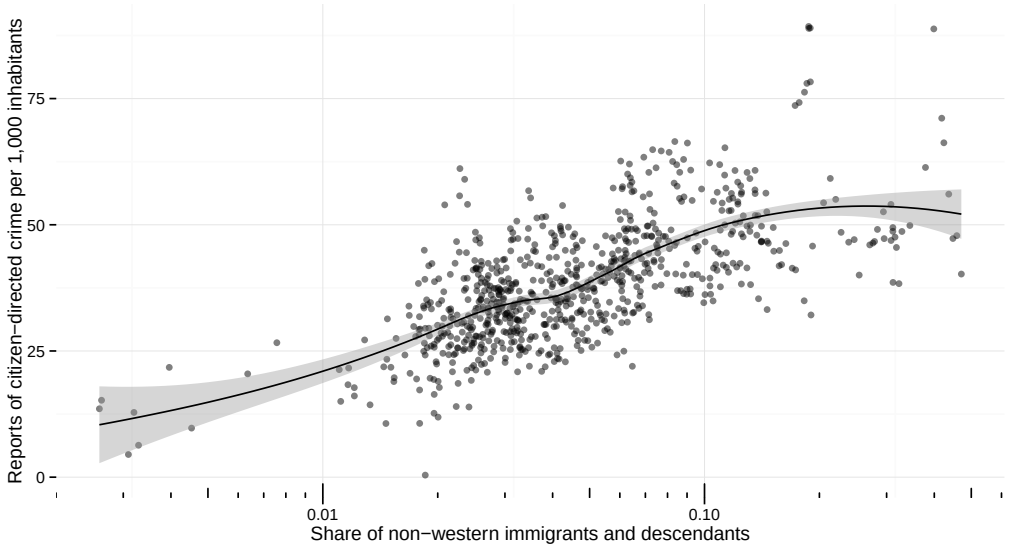


Figure A.16 Municipality-level ethnic diversity and rates of citizen-directed crime, 2007-2014. Each dot is a municipality-year. 'Citizen-directed crime' encompasses vandalism, break-ins, robbery, sexual assault, and theft. The x-axis is log-transformed to show variation more clearly.

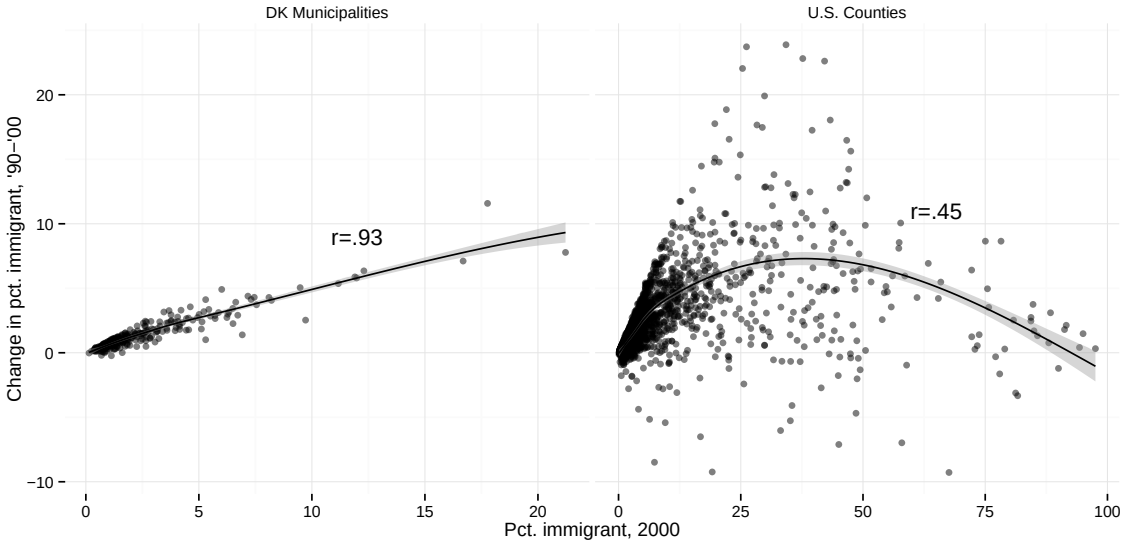
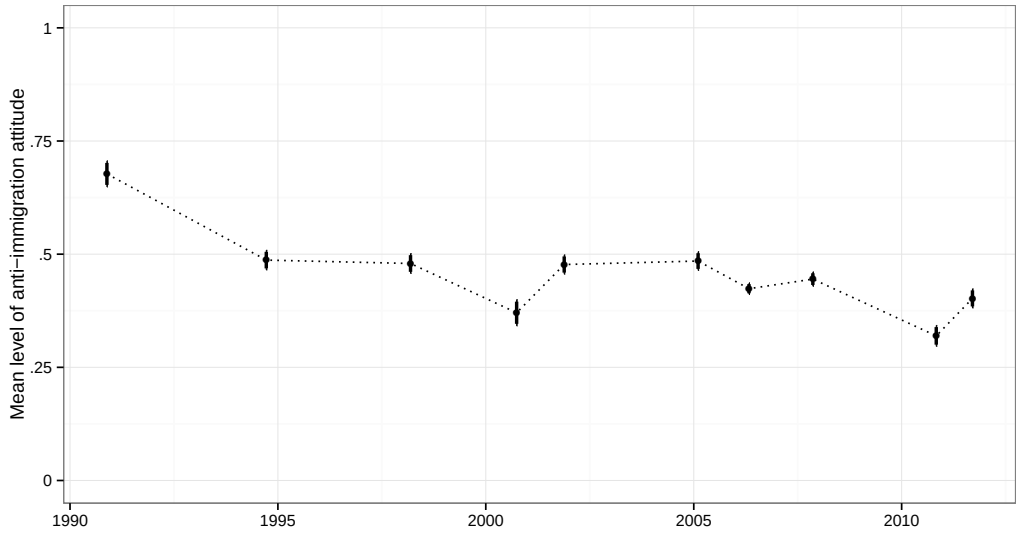
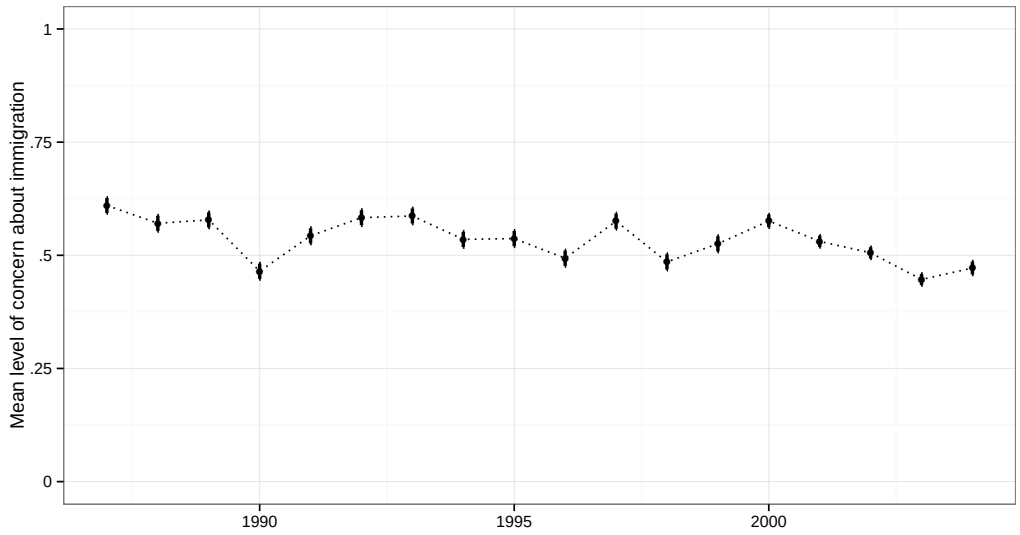


Figure A.17 Correlations between trends and levels of immigration in Danish municipalities and U.S. counties.

Attitude trends over time

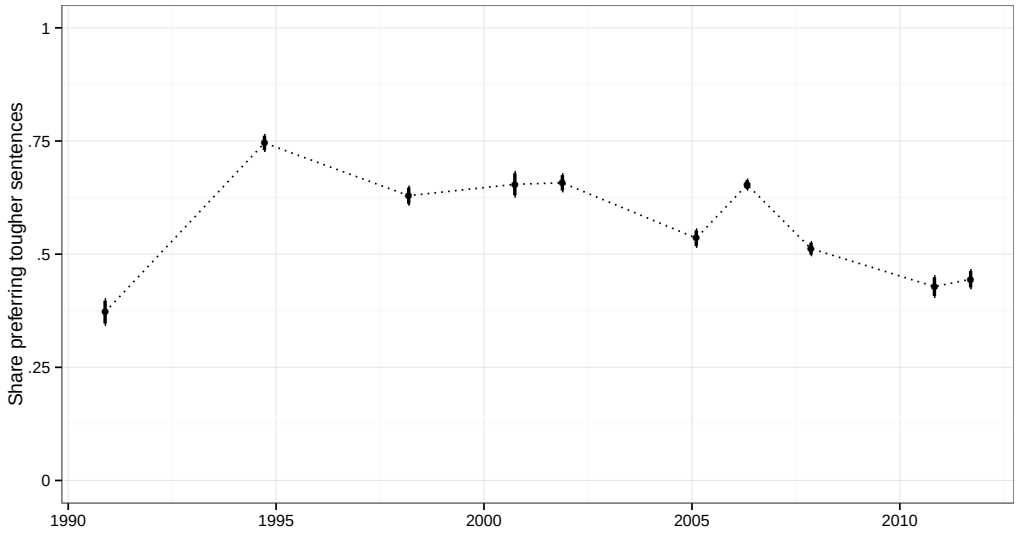


(a)

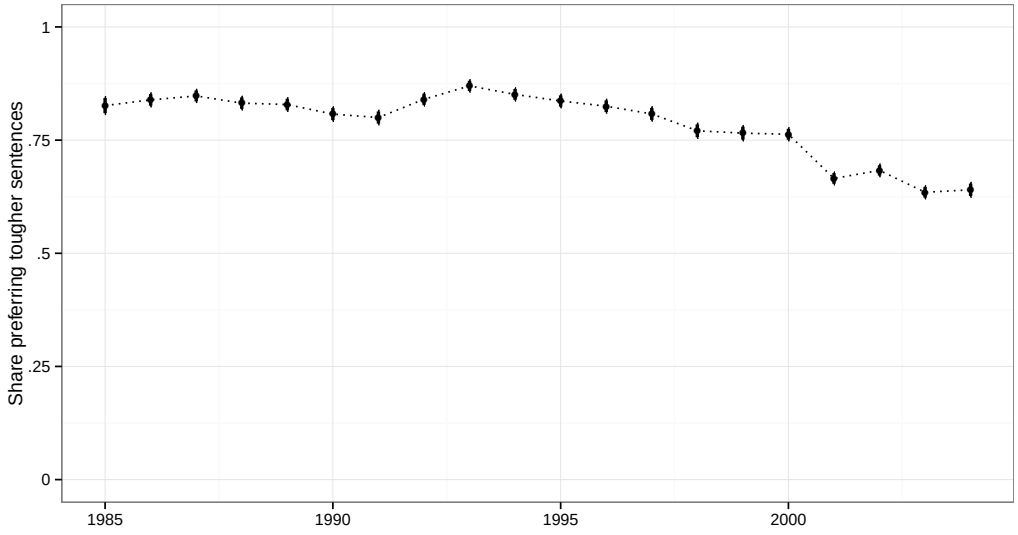


(b)

Figure A.18 Mean levels of (a) measure of anti-immigration attitudes in municipality data, and (b) measure of concern about immigration in zip code data



(a)



(b)

Figure A.19 Mean levels of measure of (a) crime attitudes in municipality data, and (b) measure of concern about crime in zip code data.