

The Influence of Local Context on Group-Centric Policy Attitudes

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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 theorize and test an alternative 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.

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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; Sears et al., 2000; Hurwitz and Peffley, 2005). 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”, proposing that what sets these issues apart is the presence of “linking information” connecting policies and groups in the minds of citizens.

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 experiential 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.

The idea that citizens’ attitudes respond to visible outgroups in the local context has a long

pedigree, and is typically examined in studies pitting contact theory against theories of group threat (Key, 1949; Allport, 1954; Oliver and Wong, 2003; Branton and Jones, 2005; Bowyer, 2009; Giles and Buckner, 2009). Beyond intergroup attitudes, this study also connects to the broader literature linking local ethnic diversity to political attitudes (Hopkins, 2009, 2010, 2011; Enos, 2015). Relative to these studies, the key contribution here is that the outcome of interest is not social or political attitudes toward ethnic outgroups per se, but rather the degree to which those attitudes are linked with attitudes toward ostensibly unrelated political issues.

I also contribute with a comprehensive empirical test of theoretical predictions, 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 attributable 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.

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.

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 Tøgeby (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 and Sears, 2010; Tesler, 2012). Altogether, these studies tend to find that political rhetoric ‘playing the race card’ is effective at priming group-based antagonisms (Nelson and Kinder, 1996; Mendelberg, 2001; Hurwitz and Peffley, 2005; Tesler, 2015).

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 (Romer et al., 1998; Dixon and Linz, 2000).

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” (Hainmueller and Hiscox, 2007; Ceobanu, 2010). 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.

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 (Wong et al., 2012; Dinesen and Sonderskov, 2015). 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.6.1 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. At the individual level, this correlation almost vanishes completely when correcting for background 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).

In a recent study, Weber et al. (2014) outline a similar argument. 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" (75).

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. 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.

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, 2015). 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.

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 1 plots the trends in ethnic diversity within municipalities and zip codes, the two levels of measurement used here.

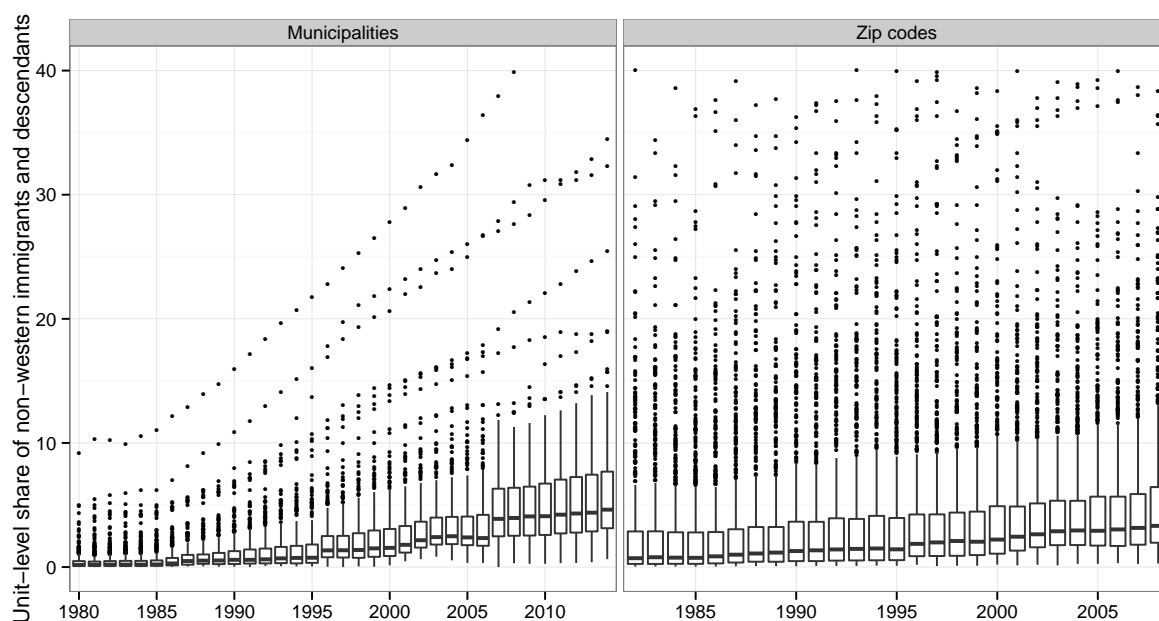


Figure 1: Box plots of distributions of shares of non-western immigrants and descendants at the zip code and municipality level by year. The y-axis is censored at 40 percent in order to more clearly show variation at the bottom of the scale. In the municipality data, the jump in 2007 is partly attributable to a reform which amalgamated municipalities into larger units. Source: Statistics Denmark

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 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 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. Stein et al., 2000; Branton and Jones, 2005; Hopkins, 2010) and U.S. zip codes (e.g., Oliver and Mendelberg, 2000).

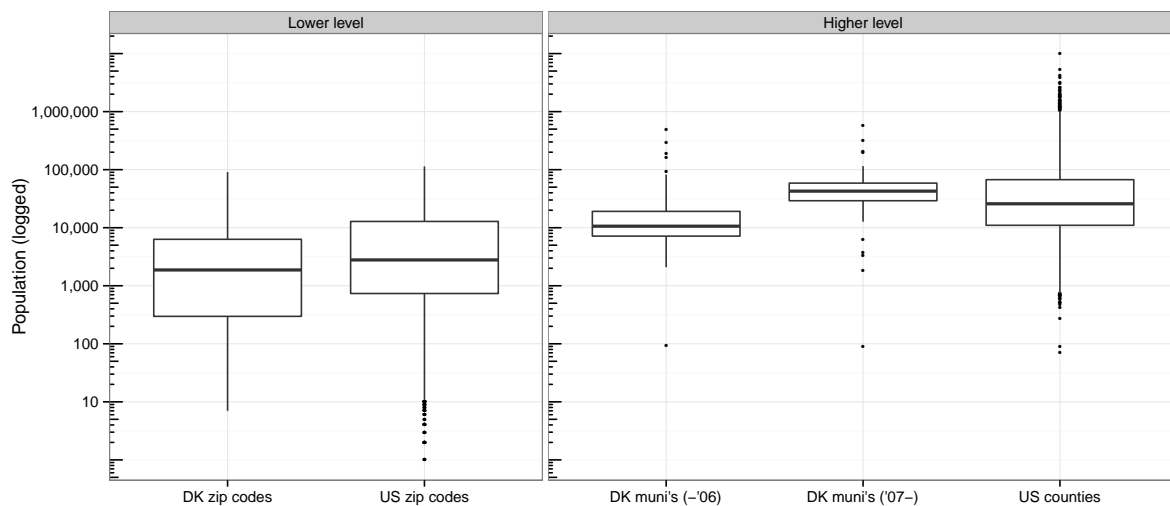


Figure 2: Distributions of sizes of contextual units. Source: Statistics Denmark, United States Census Bureau

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.

4 Data and model

In order to test the hypothesis presented in section 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.¹

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.² 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 (but see Hopkins (2011), who argues that levels may be more relevant in European contexts). However, since I observe local immigration from a baseline of almost total ethnic homogeneity, levels are virtually synonymous with long-run changes in

¹Replication data will be made available upon publication at <https://dataverse.harvard.edu/dataverse/BJPo1S>.

²'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.

the Danish context. Figure A.6.2 in Appendix A.6 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, in order to avoid dropping observations, I opt for a measure of local levels.

The other key variables are measured somewhat differently in the municipality and the zip code data sets. The remainder of this section describes how. Appendix A.2 presents summary statistics for all variables in the municipality and zip code data sets.

4.1 The municipality data

The municipality data gathers survey responses from ten nationally representative surveys from 1990 to 2011, presented in section A.1 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.³

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.1.2 in the Appendix.⁴ 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

³The 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 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.

⁴The 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 the important advantage of having been asked relatively consistently across all surveys.

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.

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.⁵

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.⁶

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.

⁵A 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 ($t = 2.68, p < .01$)

⁶For results treating the scale as ordinal, see below. Appendix A.7 presents plots of the time trends of both variables.

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 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:

$$Crime_{ij} = f(\beta_1 \times Imm_{ij} + \beta_2 \times ED_j + \beta_3 \times Imm_{ij} \times ED_j + \mathbf{X}_{ij} \times \gamma) \quad (1)$$

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, 2015). The fixed-effects estimator has the important property of controlling out time-invariant confounders, the advantages of which I discuss in further detail below.

5 Results

5.1 Regression estimates

Tables 1–2 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 (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 Tables 1–2, 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 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

Table 1: Models using municipality data

	Prefer stricter punishments for violent crime			
	(1)	(2)	(3)	(4)
Anti-immigration (Imm)	1.19*** (0.08)	1.08*** (0.08)	1.39*** (0.08)	1.27*** (0.07)
Ethnic diversity (ED)	0.68 (3.05)	0.83 (2.99)	−3.46 (1.77)	−3.18 (1.80)
Imm × ED	5.57*** (1.06)	5.09*** (1.02)	4.78*** (0.93)	4.40*** (0.90)
<i>Context-level controls:</i>				
Muni. education level	−1.08 (1.57)	−1.44 (1.57)	−0.32 (1.68)	−0.04 (1.70)
Muni population	−115.49*** (32.61)	−113.66*** (32.32)	−9.10 (15.87)	−9.42 (15.59)
<i>Individual-level controls:</i>				
Gender (f)	0.02 (0.03)	0.03 (0.03)	0.03 (0.03)	0.05 (0.03)
Age	−0.01*** (0.001)	−0.01*** (0.001)	−0.02*** (0.001)	−0.02*** (0.001)
Education level	−0.72*** (0.07)	−0.73*** (0.07)	−0.88*** (0.07)	−0.90*** (0.07)
Household income	−0.09 (0.06)	−0.20** (0.06)	−0.14* (0.06)	−0.23*** (0.07)
Student	−0.05 (0.06)	−0.09 (0.06)	−0.12 (0.06)	−0.16* (0.06)
Pensioner	0.08 (0.05)	0.06 (0.05)	0.08 (0.06)	0.06 (0.06)
Left-wing voter		−0.31*** (0.04)		−0.31*** (0.04)
Left/right self-placement		0.43*** (0.08)		0.41*** (0.09)
Intercept	6.41*** (1.64)	6.45*** (1.61)	2.17** (0.71)	2.08** (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 2: Models using zip code data

	Concern about crime			
	(1)	(2)	(3)	(4)
Immigration concern (Imm)	0.20*** (0.01)	0.19*** (0.01)	0.20*** (0.01)	0.19*** (0.01)
Ethnic diversity (ED)	-0.69*** (0.17)	-0.68*** (0.16)	-0.44*** (0.13)	-0.47*** (0.13)
Imm × ED	1.23*** (0.12)	1.25*** (0.12)	1.21*** (0.12)	1.22*** (0.12)
<i>Context-level controls:</i>				
Zip avg. income	-0.12*** (0.02)	-0.12*** (0.02)	-0.03 (0.03)	-0.05 (0.03)
Zip avg. education	-0.11*** (0.02)	-0.09*** (0.02)	-0.08** (0.03)	-0.07** (0.03)
Zip population	-0.83*** (0.22)	-0.80*** (0.21)	-0.72*** (0.18)	-0.68*** (0.18)
<i>Individual-level controls:</i>				
Gender (f)		0.08*** (0.003)		0.09*** (0.003)
Age		0.03*** (0.005)		0.03*** (0.005)
Age ²		-0.003*** (0.001)		-0.003*** (0.001)
Education		-0.16*** (0.01)		-0.16*** (0.01)
Intercept	2.04*** (0.55)	1.81** (0.56)	1.51** (0.59)	1.38* (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

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 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 1 remain robustly significant.

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 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 in a model with no interactions. As the immigration attitude measure, the education measure is scaled 0 to 1, so the coefficients can be interpreted as the predicted change in crime attitude associated with moving across the full range of the variable. The line for the coefficient on education thus 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.

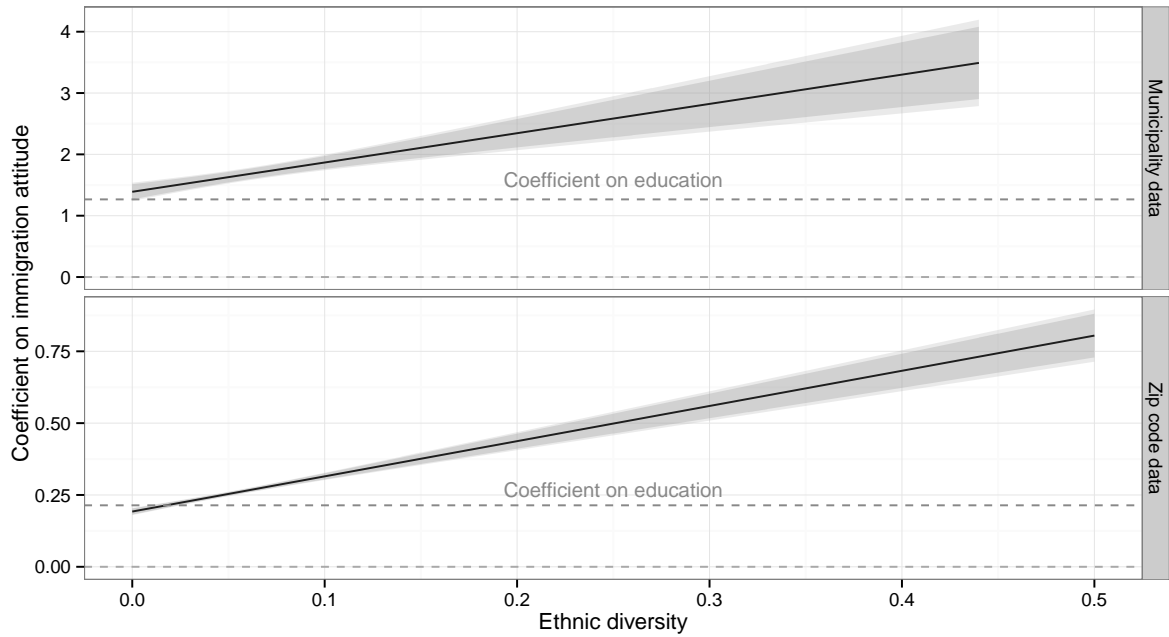


Figure 3: Correlations between anti-immigration attitudes and crime attitudes at varying levels of contextual diversity in municipality and zip code data. Shaded areas represent 90 and 95 percent confidence intervals. The dotted line shows the coefficient on level of education in a model predicting crime 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 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.

As made apparent in Figures 3-4, 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.

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

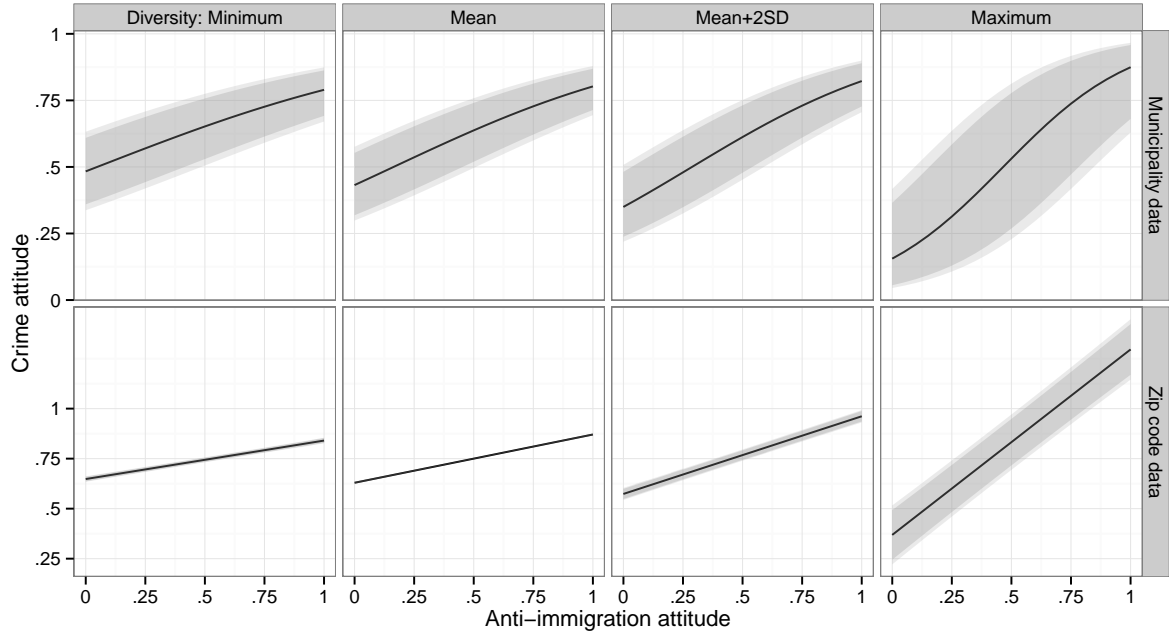


Figure 4: Predicted associations between anti-immigration and crime attitudes at various levels of ethnic diversity, in municipality and zip code data sets. Shaded areas represent 90 and 95 percent confidence intervals. In both data sets, anti-immigration attitude predicts crime attitudes more strongly as local ethnic diversity increases.

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 5 plots the two against each other.

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.⁷ 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 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

⁷Across the full range of the data, the two correlate at $r = .45$, $p < .001$.

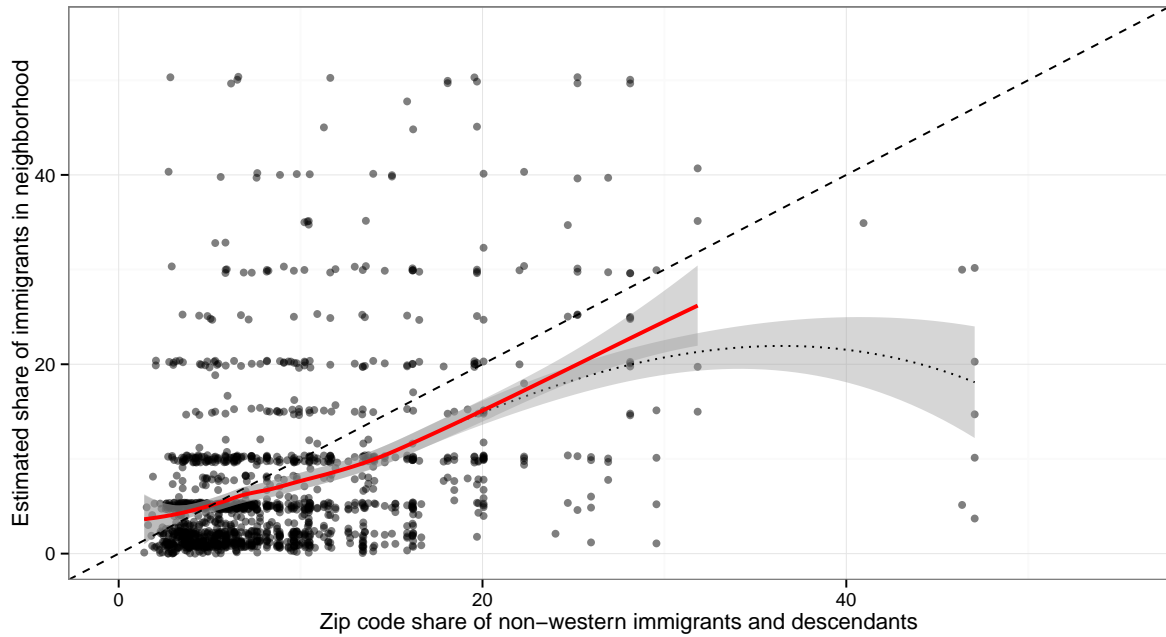


Figure 5: Manipulation check: actual vs. respondent-estimated levels of neighborhood ethnic diversity. The dotted line is a loess fit. The thick line is a loess fit excluding the 10 observations at the highest levels of neighborhood ethnic diversity (above 40 percent).

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.

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 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 (Inglehart, 1971; Borre, 1995). 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.3.1–A.3.2 in the Appendix present placebo tests of the main results, re-estimating the models from Tables 1–2 using a different post-materialist issue, concern for the environment, as the dependent variable. I

summarize the placebo tests in Figure 6, plotting the placebo interaction coefficients alongside the coefficients from the original models.

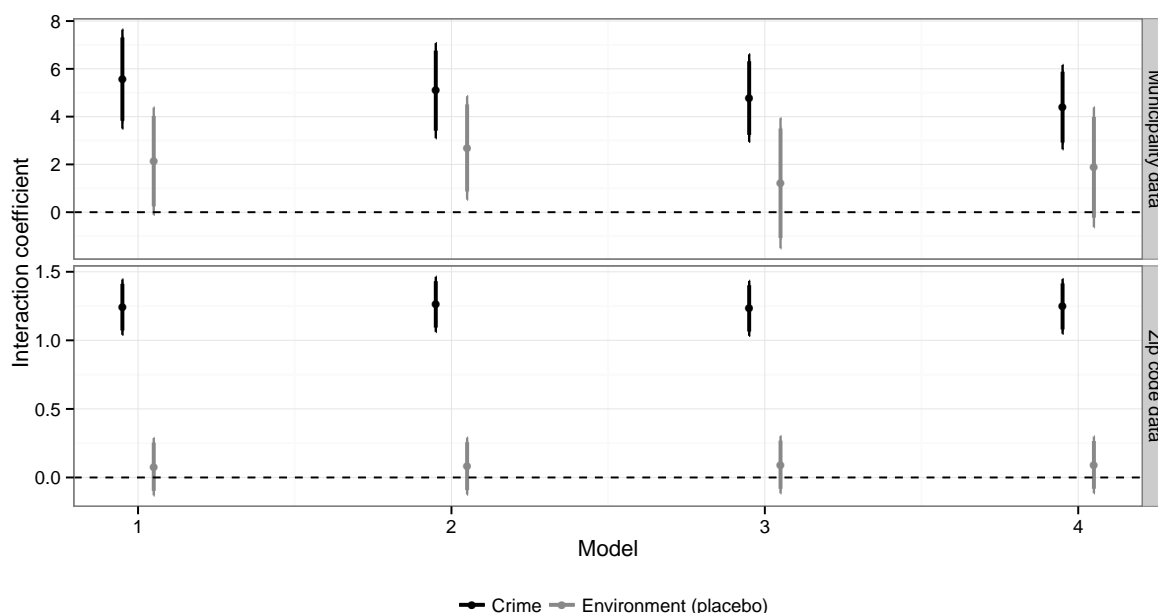


Figure 6: Interaction coefficients in original and placebo models for each of the four presented specifications, municipality and zip code data.

The environmental issue is useful 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 1–2. On the 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 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.

5.5 Additional tests

As in all observational studies, there is a relevant concern that the results may be model-dependent King and Zeng (2006). To address this concern, Appendix A.4 presents results from various alternative models. First of all, I use fixed effects regressions in the main results above, which typically minimize bias. However, there are relatively few individual-level observations for at least some contextual units in the data, in which case a random effects model may potentially reduce variance enough to offset a minor increase in bias (Clark and Linzer, 2015). In Tables A.4.1–A.4.2 I show that random effects models produce similar estimates. Second, the assumption that the 4-point scale for the independent and dependent attitude variables in the zip code data can be treated as interval scaled may be too strong. In Table A.4.3 and Figure A.4.2, I show that the results are robust to treating both scales as ordered categorical using ordinal logit models. Third, testing the main hypothesis using a multiplicative interaction model allows only the attitudinal independent variable to vary by local context and sets the coefficients on all other variables as fixed. Tables A.4.4–A.4.5 relax this constraint by splitting the local ethnic diversity variable at the median and fitting a simpler, additive on each half of the data, thus allowing the coefficients on all variables to vary by local ethnic diversity. Consistent with the hypothesis, the coefficient on anti-immigration attitudes is larger in the high ethnic diversity sample across all specifications in both data sets, and the difference between coefficients is statistically significant in every case. Hence, the result is not an artifact of the interaction model specification.

Lastly, as in other studies involving effects of features of local contexts, the possibly confounding role of residential self-selection is an important concern (King, 1996; Gallego et al., 2016). In the context of this study, the results could be driven by self-selection if individuals with less group-centric attitudes were systematically more likely to move out of areas with high levels of ethnic diversity. Such a selection pattern could result in changes in local population composition masking as effects of local ethnic diversity. In studies that purport to find adverse effects of local ethnic diversity, this concern about self-selection is typically addressed on theoretical grounds, noting that it is *prima facie* unlikely that the individuals least hostile to ethnic outgroups would be systematically more likely to move out of ethnically diverse areas (e.g., Putnam, 2007). In addition to this theoretical argument, the self-selection concern can also be tested empirically. I do so by following the approach of Dinesen and Sonderskov (2015) and interact a measure of ethnic crime stereotypes with local ethnic diversity in a model where the dependent variable is a binary indicator for whether the respondent moved in the three years subsequent to the survey. I utilize the fact that the European Social Survey round 1 has a reasonable measure of ethnic crime stereotypes (agreement with the statement that “crime problems [are] made worse” by immigrants) and can be linked to information in Danish public registers about respondents’ local contexts and moving behavior subsequent to taking the survey.

I present the result of this analysis in Appendix A.5. Note that if individuals with less group-centric attitudes were systematically more likely to move out of ethnically diverse areas,

the interaction between local ethnic diversity and the attitude measure should be negative. Yet as shown in Table A.5.1, the interaction term is statistically indistinguishable from zero. Moreover, the point estimate of the interaction term is positive, suggesting that if anything, individuals with more group-centric crime attitudes are more likely to move out of ethnically diverse contexts. In sum, although self-selection cannot fully be ruled out, observable moving patterns suggest the study's results are not likely to be attributable to self-selection.

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 in 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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A.1 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 and Slothuus (2013)

Table A.1.1: Overview of included surveys.

Survey no.	Item	Scale
<i>Anti-immigration</i>		
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
<i>Crime</i>		
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.1.2: Overview of items used to measure anti-immigration and crime attitudes.

A.2 Summary statistics

Table A.2.1: Summary statistics, municipality data

Statistic	N	Mean	St. Dev.	Min	Max
<i>Main variables:</i>					
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
<i>Context-level controls:</i>					
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
<i>Individual-level controls:</i>					
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.2.2: Summary statistics, zip code data

Statistic	N	Mean	St. Dev.	Min	Max
<i>Main variables:</i>					
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
<i>Context-level controls:</i>					
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
<i>Individual-level controls:</i>					
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

A.3 Placebo tests

Table A.3.1: Placebo models using municipality data

	Prefer more concern for the environment			
	(1)	(2)	(3)	(4)
Anti-immigration (Imm)	−0.62*** (0.08)	−0.51*** (0.08)	−0.68*** (0.09)	−0.50*** (0.09)
Ethnic diversity (ED)	1.91 (4.18)	1.60 (4.30)	−0.70 (2.09)	−1.14 (2.18)
Imm × ED	2.14 (1.15)	2.69* (1.10)	1.21 (1.39)	1.88 (1.28)
<i>Context-level controls:</i>				
Muni. education level	10.57** (3.45)	11.30** (3.55)	−1.85 (1.80)	−2.05 (1.86)
Muni population	−358.09*** (104.43)	−365.93*** (106.84)	−11.99 (17.04)	−11.20 (17.75)
<i>Individual-level controls:</i>				
Gender (f)	0.03 (0.04)	0.01 (0.03)	0.01 (0.04)	−0.001 (0.04)
Age	0.001 (0.002)	0.0004 (0.002)	−0.002 (0.002)	−0.003 (0.002)
Education level	0.42*** (0.07)	0.42*** (0.07)	0.15* (0.07)	0.15* (0.07)
Household income	−0.08 (0.09)	0.07 (0.09)	−0.06 (0.07)	0.14 (0.08)
Student	−0.08 (0.07)	−0.04 (0.07)	−0.17** (0.07)	−0.12 (0.07)
Pensioner	0.15* (0.06)	0.18** (0.06)	0.18** (0.06)	0.22*** (0.06)
Left-wing voter		0.20*** (0.03)		0.31*** (0.04)
Left/right self-placement		−0.82*** (0.09)		−1.18*** (0.11)
Intercept	14.09** (4.76)	14.49** (4.86)	2.96** (1.08)	3.40** (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.3.2: Placebo models using zip code data

	Concern about pollution			
	(1)	(2)	(3)	(4)
Immigration concern (Imm)	0.09*** (0.01)	0.09*** (0.01)	0.08*** (0.01)	0.09*** (0.01)
Ethnic diversity (ED)	-0.45** (0.17)	-0.42* (0.17)	0.18 (0.15)	0.18 (0.15)
Imm × ED	0.06 (0.13)	0.05 (0.13)	0.09 (0.13)	0.08 (0.13)
<i>Context-level controls:</i>				
Zip avg. income	-0.17*** (0.02)	-0.16*** (0.02)	0.11** (0.04)	0.11** (0.03)
Zip avg. education	-0.14*** (0.02)	-0.15*** (0.02)	-0.001 (0.02)	-0.02 (0.02)
Zip population	0.24 (0.19)	0.17 (0.18)	-0.14 (0.18)	-0.19 (0.17)
<i>Individual-level controls:</i>				
Gender (f)		0.09*** (0.003)		0.09*** (0.003)
Age		0.06*** (0.005)		0.06*** (0.005)
Age ²		-0.01*** (0.001)		-0.01*** (0.001)
Education		0.07*** (0.01)		0.07*** (0.01)
Intercept	2.57*** (0.50)	2.48*** (0.49)	0.02 (0.40)	0.06 (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

A.4 Alternative models and specifications

Table A.4.1: Random effects models using municipality data

	Prefer stricter punishments for violent crime			
	(1)	(2)	(3)	(4)
Anti-immigration (Imm)	1.17*** (0.06)	1.06*** (0.06)	1.35*** (0.06)	1.23*** (0.06)
Ethnic diversity (ED)	-1.92*** (0.67)	-1.67** (0.71)	-2.47*** (0.56)	-2.15*** (0.57)
Imm × ED	5.69*** (0.81)	5.21*** (0.81)	5.03*** (0.82)	4.66*** (0.83)
<i>Context-level controls:</i>				
Muni. education level	-1.83*** (0.36)	-2.08*** (0.38)	-1.35*** (0.28)	-1.49*** (0.29)
Muni population	-26.81*** (4.71)	-26.61*** (7.95)	-0.49 (2.61)	0.50 (2.95)
<i>Individual-level controls:</i>				
Gender (f)	0.02 (0.03)	0.03 (0.03)	0.03 (0.03)	0.04 (0.03)
Age	-0.01*** (0.001)	-0.01*** (0.001)	-0.02*** (0.001)	-0.02*** (0.001)
Education level	-0.68*** (0.05)	-0.69*** (0.05)	-0.86*** (0.06)	-0.87*** (0.06)
Household income	-0.10 (0.06)	-0.20*** (0.06)	-0.13** (0.06)	-0.22*** (0.06)
Student	-0.05 (0.05)	-0.09* (0.05)	-0.12** (0.06)	-0.16*** (0.06)
Pensioner	0.06 (0.04)	0.04 (0.05)	0.08* (0.05)	0.06 (0.05)
Left-wing voter		-0.31*** (0.03)		-0.30*** (0.03)
Left/right self-placement		0.42*** (0.06)		0.40*** (0.07)
Intercept	1.45*** (0.10)	1.47*** (0.11)	1.33*** (0.22)	1.32*** (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.4.2: Random effects models using zip code data

	Concern about crime			
	(1)	(2)	(3)	(4)
Immigration concern (Imm)	0.20*** (0.01)	0.19*** (0.01)	0.20*** (0.01)	0.19*** (0.01)
Ethnic diversity (ED)	-1.04*** (0.08)	-1.02*** (0.08)	-0.78*** (0.08)	-0.76*** (0.08)
Imm × ED	1.24*** (0.10)	1.26*** (0.10)	1.22*** (0.10)	1.23*** (0.10)
<i>Context-level controls:</i>				
Zip avg. income	-0.21*** (0.01)	-0.21*** (0.01)	-0.02 (0.02)	-0.04 (0.02)
Zip avg. education	-0.01* (0.01)	-0.004 (0.01)	-0.03*** (0.01)	-0.02*** (0.01)
Zip population	-0.001 (0.01)	0.0003 (0.01)	0.001 (0.01)	0.003 (0.01)
<i>Individual-level controls:</i>				
Gender (f)		0.08*** (0.003)		0.08*** (0.003)
Age		0.03*** (0.005)		0.03*** (0.004)
Age ²		-0.003*** (0.0005)		-0.003*** (0.0005)
Education		-0.16*** (0.01)		-0.16*** (0.01)
Intercept	1.06*** (0.06)	0.91*** (0.06)	1.05*** (0.05)	0.90*** (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

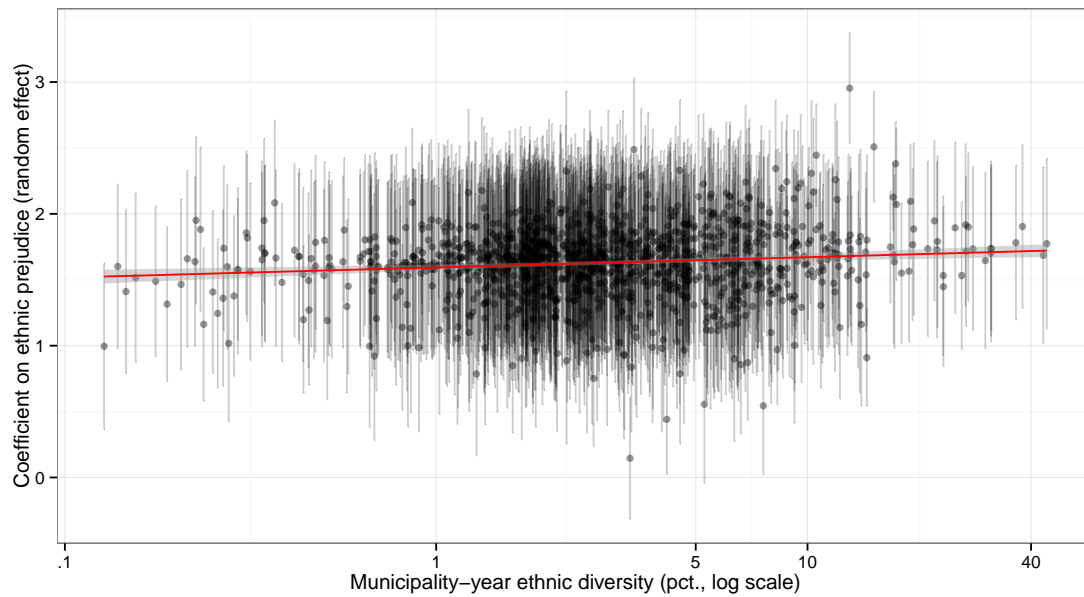


Figure A.4.1: Municipality-year level ethnic diversity plotted against random slopes from a random effects model allowing the coefficient of ethnic prejudice on crime attitudes to vary by municipality-year. Consistent with expectations, the two are more strongly associated in more ethnically diverse municipality-years. The increase in slopes is significant ($\beta = .12, t = 4.2, p < .001$).

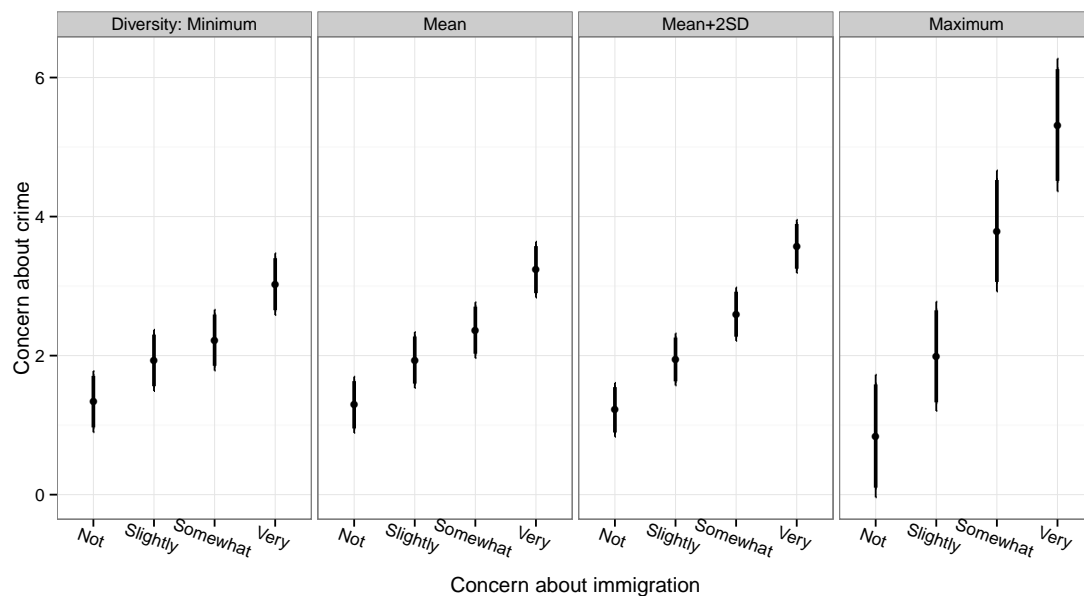


Figure A.4.2: Predicted associations between anti-immigration and crime attitudes at various levels of ethnic diversity, using ordinal logit models. Thick and thin error bars represent 90 and 95 percent confidence intervals respectively.

Table A.4.3: Ordinal logit models using zip code data

	Concern about crime (ordinal)			
	(1)	(2)	(3)	(4)
y > 'Slightly'	2.36 (2.30)	2.18 (2.30)	1.02 (2.52)	1.06 (2.48)
y > 'Somewhat'	0.76 (2.30)	0.56 (2.30)	-0.59 (2.52)	-0.57 (2.48)
y > 'Very'	-0.58 (2.30)	-0.82 (2.30)	-1.95 (2.52)	-1.97 (2.48)
Imm > 'Slightly'	0.58*** (0.03)	0.61*** (0.03)	0.64*** (0.03)	0.67*** (0.03)
Imm > 'Somewhat'	1.04*** (0.03)	1.03*** (0.03)	1.07*** (0.03)	1.06*** (0.03)
Imm > 'Very'	1.91*** (0.04)	1.88*** (0.04)	1.94*** (0.04)	1.91*** (0.04)
Ethnic diversity (ED)	-0.12* (0.05)	-0.13* (0.05)	-0.04 (0.04)	-0.04 (0.04)
ED × Imm > 'Slightly'	0.07** (0.03)	0.08** (0.03)	0.04 (0.03)	0.05 (0.03)
ED × Imm > 'Somewhat'	0.17*** (0.03)	0.17*** (0.03)	0.15*** (0.03)	0.15*** (0.03)
ED × Imm > 'Very'	0.21*** (0.03)	0.22*** (0.04)	0.20*** (0.03)	0.21*** (0.04)
<i>Context-level controls:</i>				
Zip avg. income	-0.21*** (0.05)	-0.22*** (0.05)	-0.01 (0.06)	-0.04 (0.06)
Zip avg. education	-0.46*** (0.11)	-0.41*** (0.11)	-0.29* (0.12)	-0.26* (0.11)
Zip population	-1.02** (0.32)	-1.00** (0.32)	-0.87*** (0.26)	-0.83** (0.27)
<i>Individual-level controls:</i>				
Gender (f)		0.63*** (0.02)		0.63*** (0.02)
Age		0.39*** (0.06)		0.40*** (0.06)
Age ²		-0.32*** (0.06)		-0.33*** (0.06)
Education		-0.20*** (0.01)		-0.20*** (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

Table A.4.4: Ethnic diversity low/high split, municipality data

	Low ED	High ED	Prefer stricter punishments for violent crime					
			Low	High	Low	High	Low	High
Anti-immigration (Imm)	1.41*** (0.07)	1.71*** (0.09)	1.31*** (0.07)	1.51*** (0.09)	1.53*** (0.07)	1.82*** (0.07)	1.43*** (0.07)	1.63*** (0.07)
<i>Context-level controls:</i>								
Muni. education level	5.43** (1.68)	−4.02 (2.29)	4.89** (1.68)	−4.26 (2.27)	1.27 (2.81)	−2.50 (2.24)	1.11 (2.78)	−1.92 (2.25)
Muni population	−97.28* (39.67)	−80.67 (44.99)	−98.65* (40.00)	−79.59 (44.34)	10.35 (39.22)	20.31 (27.02)	12.91 (38.97)	17.19 (26.55)
<i>Individual-level controls:</i>								
Gender (f)	0.07 (0.04)	−0.01 (0.05)	0.08 (0.05)	0.01 (0.05)	0.07 (0.05)	−0.004 (0.04)	0.08 (0.05)	0.01 (0.05)
Age	−0.01*** (0.002)	−0.01*** (0.002)	−0.01*** (0.002)	−0.01*** (0.002)	−0.02*** (0.002)	−0.02*** (0.002)	−0.02*** (0.002)	−0.02*** (0.002)
Education level	−0.70*** (0.08)	−0.85*** (0.12)	−0.70*** (0.08)	−0.86*** (0.12)	−0.80*** (0.08)	−0.99*** (0.10)	−0.80*** (0.08)	−1.02*** (0.11)
Household income	−0.21* (0.09)	0.05 (0.08)	−0.30** (0.09)	−0.08 (0.08)	−0.27** (0.10)	−0.04 (0.08)	−0.35*** (0.10)	−0.15 (0.09)
Student	−0.03 (0.09)	−0.06 (0.09)	−0.06 (0.09)	−0.10 (0.09)	−0.11 (0.09)	−0.12 (0.09)	−0.14 (0.09)	−0.16 (0.09)
Pensioner	0.06 (0.08)	0.13 (0.08)	0.05 (0.08)	0.09 (0.08)	0.005 (0.08)	0.15 (0.08)	−0.01 (0.08)	0.11 (0.08)
Left-wing voter			−0.21*** (0.05)	−0.39*** (0.05)			−0.21*** (0.05)	−0.40*** (0.05)
Left/right self-placement			0.45*** (0.11)	0.43*** (0.12)			0.48*** (0.11)	0.36** (0.13)
Intercept	−0.61 (0.55)	5.80* (2.30)	−0.52 (0.55)	5.91** (2.25)	1.61 (1.05)	0.94 (1.26)	1.51 (1.03)	1.01 (1.24)
N	11,507	11,849	11,507	11,849	11,507	11,849	11,507	11,849
Municipality fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects					✓	✓	✓	✓
R ²	0.18	0.18	0.19	0.19	0.22	0.22	0.23	0.23

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

Table A.4.5: Ethnic diversity low/high split, zip code data

	Concern about crime							
	Low ED	High ED	Low	High	Low	High	Low	High
Immigration concern (Imm)	0.22*** (0.01)	0.31*** (0.01)	0.21*** (0.01)	0.30*** (0.01)	0.21*** (0.01)	0.30*** (0.01)	0.21*** (0.01)	0.29*** (0.01)
<i>Context-level controls:</i>								
Zip avg. income	0.02 (0.06)	-0.30*** (0.04)	0.02 (0.06)	-0.29*** (0.04)	0.02 (0.08)	-0.08 (0.07)	0.02 (0.08)	-0.09 (0.07)
Zip avg. education	-0.21*** (0.06)	-0.02 (0.03)	-0.20** (0.06)	-0.01 (0.03)	-0.12* (0.05)	-0.07* (0.03)	-0.11* (0.05)	-0.06 (0.04)
Zip population	-0.24 (0.46)	-0.72* (0.29)	-0.29 (0.46)	-0.62* (0.29)	-0.69* (0.30)	-0.70** (0.25)	-0.70* (0.29)	-0.63* (0.25)
<i>Individual-level controls:</i>								
Gender (f)			0.08*** (0.004)	0.09*** (0.004)			0.08*** (0.004)	0.09*** (0.004)
Age			0.05*** (0.01)	0.01 (0.01)			0.05*** (0.01)	0.01 (0.01)
Age ²			-0.005*** (0.001)	-0.001 (0.001)			-0.005*** (0.001)	-0.001 (0.001)
Education			-0.12*** (0.02)	-0.20*** (0.02)			-0.11*** (0.02)	-0.20*** (0.02)
Intercept	3.20*** (0.75)	1.06 (0.56)	2.98*** (0.75)	0.97 (0.59)	1.97** (0.65)	1.47* (0.71)	1.83** (0.64)	1.42 (0.75)
N	20,175	20,439	19,702	20,136	20,175	20,439	19,702	20,136
Zip code fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Year fixed effects					✓	✓	✓	✓
R ²	0.11	0.19	0.14	0.22	0.13	0.20	0.16	0.23
Adjusted R ²	0.11	0.19	0.14	0.21	0.13	0.19	0.15	0.22

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

A.5 Residential self-selection

Table A.5.1 shows results from logit models of moving within 3 years after the interview, based on data from the Danish version of European Social Survey round 1 linked with moving data from Danish public registers.

Table A.5.1: Models predicting moving within 3 years after interview

	Moved within 3 years	
	(1)	(2)
Local non-western imm. share	-0.123 (2.201)	-1.133 (2.540)
‘Imm. mk. crime worse’	-0.661 (0.344)	-0.242 (0.396)
Local non-western imm. share × ‘Imm. mk. crime worse’	3.494 (3.225)	3.316 (3.695)
Gender(male)		-0.088 (0.138)
Age		-0.048*** (0.005)
Education (yrs)		0.019 (0.028)
Income (Mkr)		-5.110*** (1.226)
Unemployed		-0.046 (0.294)
Non-single		-0.772*** (0.152)
Constant	-0.613** (0.238)	2.259*** (0.472)
R-squared		
N	1370	1366

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

If the study’s finding were driven by residential self-selection, respondents with group-centric attitudes about crime should be less likely to move out of ethnically diverse contexts. In that case, the interaction coefficient between local share of non-western immigrants and agreement that ‘immigrants make crime worse’ should be negative. Instead, as shown in Table A.5.1, the interaction is statistically insignificant and positive.

A.6 Ethnic diversity: crime rates and trends vs. levels

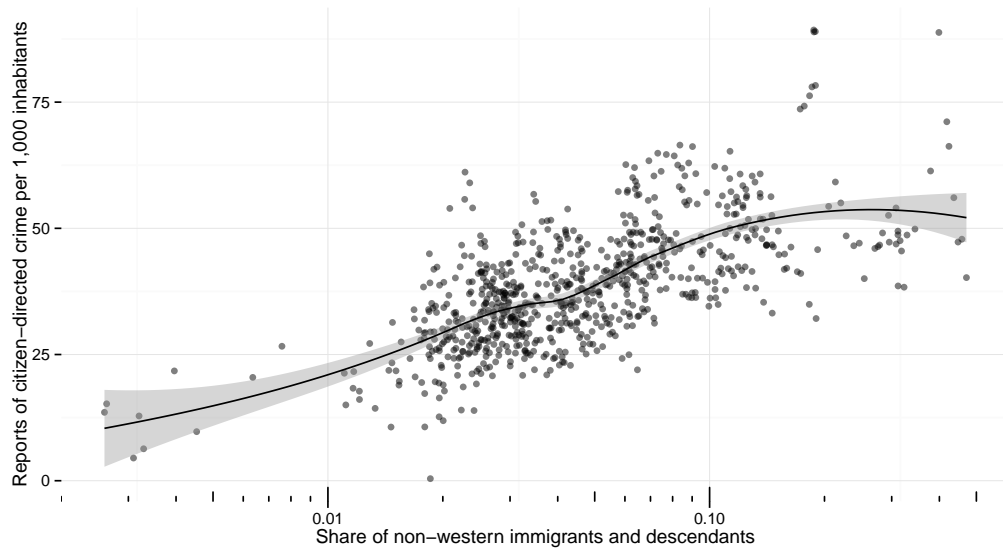


Figure A.6.1: Municipality-level ethnic diversity and rates of citizen-directed crime, 2007-2014. Each dot is a municipality-year. ‘Citizen-directed crime’ encompasses vandalism, break-ins, robbery, sexual assault, and theft. The x-axis is log-transformed to show variation more clearly.

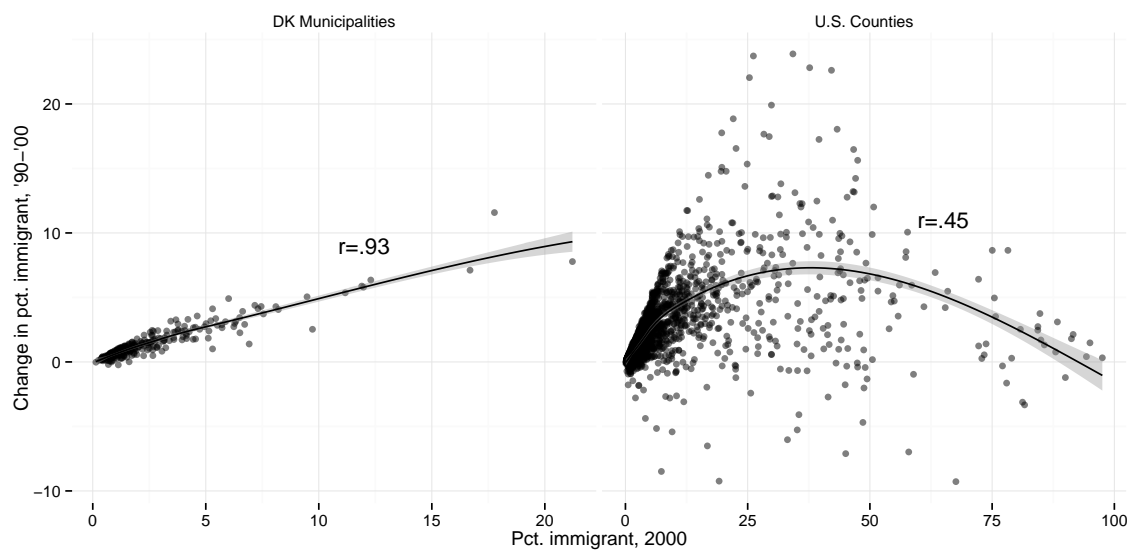


Figure A.6.2: Correlations between trends and levels of immigration in Danish municipalities and U.S. counties.

A.7 Attitude trends over time

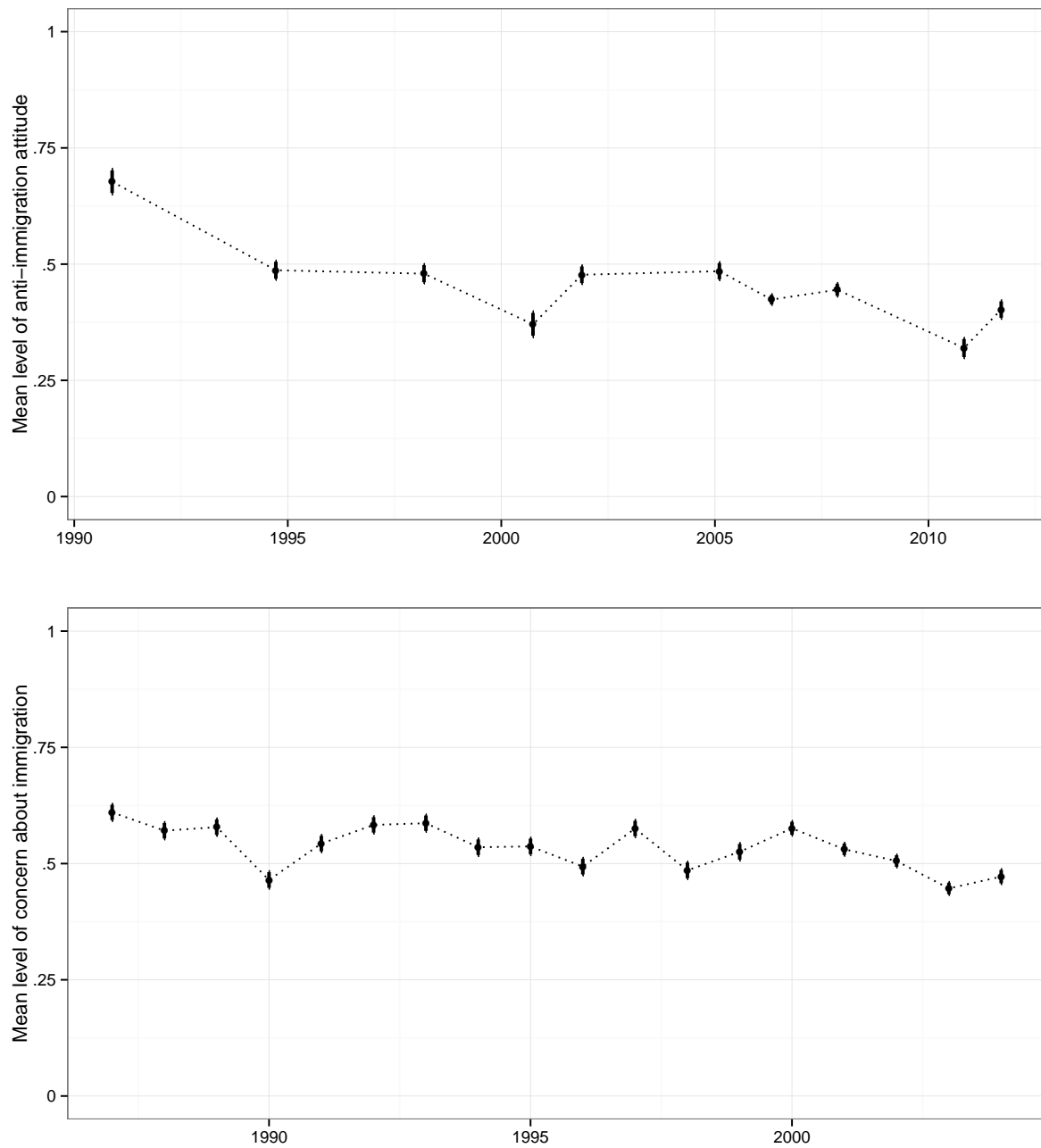


Figure A.7.1: Mean levels of measure of anti-immigration attitudes in municipality data (top) and measure of concern about immigration in zip code data (bottom)

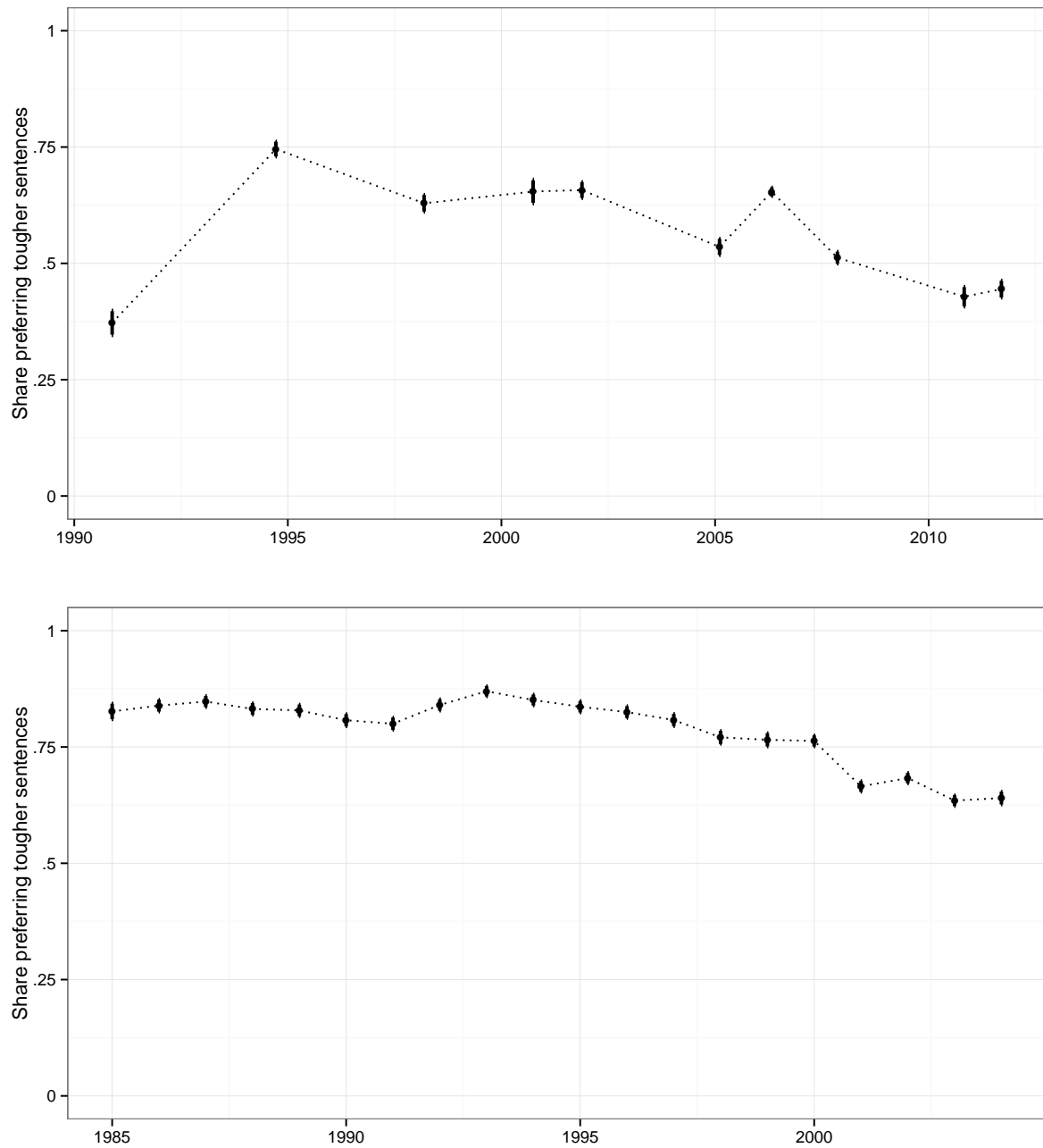


Figure A.7.2: Mean levels of measure of crime attitudes in municipality data (top) and measure of concern about crime in zip code data (bottom).