

A longitudinal investigation of integration/ multiculturalism policies and attitudes towards immigrants in European countries

Bartram, David; Jarochova, Erika

Veröffentlichungsversion / Published Version

Zeitschriftenartikel / journal article

Empfohlene Zitierung / Suggested Citation:

Bartram, D., & Jarochova, E. (2021). A longitudinal investigation of integration/multiculturalism policies and attitudes towards immigrants in European countries. *Journal of Ethnic and Migration Studies*, 48(1), 153-172. <https://doi.org/10.1080/1369183X.2021.1922273>

Nutzungsbedingungen:

Dieser Text wird unter einer CC BY-NC-ND Lizenz (Namensnennung-Nicht-kommerziell-Keine Bearbeitung) zur Verfügung gestellt. Nähere Auskünfte zu den CC-Lizenzen finden Sie hier:

<https://creativecommons.org/licenses/by-nc-nd/4.0/deed.de>

Terms of use:

This document is made available under a CC BY-NC-ND Licence (Attribution-Non Commercial-NoDerivatives). For more information see:

<https://creativecommons.org/licenses/by-nc-nd/4.0>



A longitudinal investigation of integration/multiculturalism policies and attitudes towards immigrants in European countries

David Bartram & Erika Jarochova

To cite this article: David Bartram & Erika Jarochova (2022) A longitudinal investigation of integration/multiculturalism policies and attitudes towards immigrants in European countries, Journal of Ethnic and Migration Studies, 48:1, 153-172, DOI: [10.1080/1369183X.2021.1922273](https://doi.org/10.1080/1369183X.2021.1922273)

To link to this article: <https://doi.org/10.1080/1369183X.2021.1922273>



© 2021 The Author(s). Published by Informa UK Limited, trading as Taylor & Francis Group



[View supplementary material](#)



Published online: 06 May 2021.



[Submit your article to this journal](#)



Article views: 1175




[View related articles](#)



[View Crossmark data](#)

A longitudinal investigation of integration/multiculturalism policies and attitudes towards immigrants in European countries

David Bartram  and Erika Jarochova

Department of Sociology, University of Leicester, Leicester, UK

ABSTRACT

A number of recent studies find that integration and multiculturalism policies help soften anti-immigrant attitudes among the broader population. These findings, however, emerge from cross-sectional analyses and are potentially vulnerable to omitted variable bias. The analysis in this paper overcomes that limitation by adopting a longitudinal approach. This approach uses data from repeated cross-sections drawn from the European Social Survey and the European Values Survey. These data can be treated as panels in a longitudinal framework once it is recognised that the relevant variables (including the attitudes variables) can be handled effectively as country-level averages. Multi-level modelling (the default approach in existing research) is not necessary; in particular, there is no need to use individual-level control variables. In a fixed-effects analysis of country-level data, adoption of more open/accommodating integration and/or multiculturalism policies does not lead to a reduction in anti-immigration sentiment. The findings of the cross-sectional studies evidently suffer from significant omitted variable bias.

ARTICLE HISTORY

Received 27 January 2021

Accepted 23 April 2021


KEYWORDS


Xenophobia; integration policy; multiculturalism; panel data; control variables

Introduction

Recent research in migration studies includes a number of articles offering support for an attractive finding: policies facilitating the integration of immigrants appear to have a salutary effect on attitudes towards immigrants among the broader population. Integration policies don't just enhance immigrant integration – they reduce the extent of 'threat perception' or hostility towards immigrants more broadly (Schlueter, Meuleman, and Davidov 2013; Schlueter, Masso, and Davidov 2020; Callens and Meuleman 2017; Green et al. 2020; Hooghe and de Vroome 2015).

This paper reconsiders that finding, using two related methodological reflections that will likely be relevant to a broader set of research questions relevant to migration studies. The findings noted above (and discussed further below) are grounded in a common analytical approach: the researchers take cross-sectional survey data drawn from

CONTACT David Bartram  d.bartram@le.ac.uk

 Supplemental data for this article can be accessed <https://doi.org/10.1080/1369183X.2021.1922273>.

© 2021 The Author(s). Published by Informa UK Limited, trading as Taylor & Francis Group

This is an Open Access article distributed under the terms of the Creative Commons Attribution-NonCommercial-NoDerivatives License (<http://creativecommons.org/licenses/by-nc-nd/4.0/>), which permits non-commercial re-use, distribution, and reproduction in any medium, provided the original work is properly cited, and is not altered, transformed, or built upon in any way.

individuals, match it to country-level data on policies, and conduct multi-level modelling (MLM) to estimate the impact of the policies on attitudes. The use of MLM appears necessary because the data indeed involve different levels. We will argue, however, that this approach entails an unnecessary constraint, rooted in a tendency to take for granted what sort of control variables are necessary for a question along these lines. To estimate the impact of policies (a ‘level-2’ variable), it is likely not necessary to include ‘level-1’ control variables. If that position is accepted, we can then treat data from repeated cross-sections as country-level panel data giving averages of attitude measures. If these repeated measures (giving information on change in attitudes over time) can be matched to repeated measures of policies, a longitudinal analysis is then possible, reducing the risks (evident in a cross-sectional analysis) of omitted variable bias (compare Jæger 2013 and Olivera 2015).

The paper thus offers an analysis considering the way adoption of integration and multicultural policies might *affect* attitudes towards immigrants (policies→attitudes is a useful notation for this question). The research we already have, giving cross-sectional associations, tells us whether attitudes are less xenophobic in countries where there are policy frameworks that are more accommodating to immigrants. A longitudinal approach helps us evaluate whether these between-country differences are more than ‘associations’: we can ask, if attitudes are less xenophobic in countries with certain policy frameworks, are the attitudes less xenophobic *because* of those frameworks? Do attitudes change over time as a consequence of policy change? To answer questions of that sort, a ‘within’ analysis is more enlightening (as against a cross-sectional ‘between’ analysis).

For an empirical analysis, data on attitudes are drawn from the European Social Survey (ESS) and the European Values Survey (EVS). Data on the ESS are combined with the ‘Migrant Integration Policy Index’ (MIPEX; Niessen 2007). Data from the EVS are combined with the ‘Multicultural Policies Index’ (MCP, available at www.queensu.ca/mcp/). We first review existing research, with attention to the use of MLM and in particular the question of whether ‘level-1’ controls are required. A replication of Schlueter, Masso, and Davidov (2020) (presented in the next section) suggests that these controls are not necessary for investigations of this sort. The usual sections describing the data, presenting the findings, and summarising the key conclusions then follow.

Previous research

The widely noted increase in migration to Europe over recent decades forms the context for sustained research interest in attitudes towards immigrants and immigration. Among the reasons to consider attitudes, a political angle is especially salient: populist political movements in a wide range of European countries have sought to gain votes in part by demonising immigrants – and these efforts have achieved significant success (e.g. Mudde 2013; Yilmaz 2012). The consequent political and social turmoil has sometimes become violent; even where violence is not part of the picture, the populist turn has arguably distorted national politics and governance across much of Europe.

To interpret these patterns, researchers draw on and develop two perspectives: group threat theory and inter-group contact theory. Group threat theories hold that attitudes towards immigrants become more negative when there is competition over resources, which are inevitably limited (Davidov and Semyonov 2017; McLaren 2003;

Scheepers et al. 2002; Quillian 1995). Members of the majority population are said to rationally assess the (mostly economic) competition between them and immigrants, especially for jobs. Attitudes towards immigrants become more negative in countries with worsening economic conditions (e.g. rising unemployment rates). Inter-group relations are thus more important source of negative attitudes towards outgroups than individual psychological conditions (Blumer 1958; Sherif, White, and Harvey 1955). But the inter-group contact theory proposes that more frequent contact across group boundaries can overcome these tendencies, fostering more positive attitudes and reducing prejudice (Allport 1954; Pettigrew and Tropp 2006; McLaren 2003; Schneider 2007). Even when the proportion of immigrants is rising, negative attitudes are not inevitable as long as segregation is mitigated.

In contexts of this sort, researchers have asked: what are the possibilities for policy-based efforts to mitigate the xenophobia? Several contributions perceive a basis for optimism. Callens and Meuleman (2017) find that 'respondents living in a country with more-inclusive integration policies ... display lower perceptions of economic threat [from immigrants]' (367). That finding emerges from research using data from the 2008 European Values Survey (EVS), matched to country-level data from the 2007 version of the MIPEX. The paper reports however that higher MIPEX values are not associated with lower perceptions of 'cultural threat'. Another study (Schlueter, Meuleman, and Davidov 2013) using the same data aggregates these different forms of threat and finds that 'immigrant integration policies that are more permissive are associated with decreased perceptions of group threat from immigrants' (670). For the authors, the policy implications are clear: 'these findings suggest that immigrant integration policies are of key importance in improving majority members' attitudes regarding immigrants ...' (Schlueter, Meuleman, and Davidov 2013, 670).

Schlueter, Masso, and Davidov (2020) investigate attitudes in European countries specifically in relation to Muslims (many of whom would be immigrants or have recent immigration family background). They find that 'more liberal immigrant integration policies ... are associated with lower levels of majority members' negative attitudes towards Muslim immigration ...' (649). Their analysis uses data from Round 7 of the European Social Survey (2015), matched to MIPEX data from the same year. A similar pattern is found also in work by Green et al. (2020), who use the same data (ESS7 and MIPEX 2014): a higher MIPEX score is associated with lower perceptions of 'symbolic' and 'realistic' threat. Additionally, a more tolerant policy frame work (i.e. a higher MIPEX score) enhances the impact of everyday contact with immigrants in reducing threat perceptions.

A fifth investigation along these lines (Hooghe and de Vroome 2015) uses the MIPEX measure and the MCP index, matched to data from Round 5 of the ESS. The MCP Index is not associated with anti-immigrant sentiment. The MIPEX measure, however, 'is significantly, negatively related to anti-immigrant sentiments ... : the more a country offers inclusive integration policies, the lower the levels of anti-immigrant sentiment are' (Hooghe and de Vroome 2015, 757).

What we see, then, is a set of studies all supporting the conclusion that more 'liberal' or 'tolerant' integration policies have important impacts on attitudes towards immigrants. Integration policies in the first instance affect immigrants, and substantial research efforts have been directed to the impact of policies on the immigrants themselves (e.g.

Bloemraad and Wright 2014). But those policies also appear to mitigate xenophobia among the broader population. In addition to the shared substantive findings, these papers also have some key methodological features in common. In particular, they all involve MLM of cross-sectional data. This decision appears to emerge from the view that estimating policies→attitudes requires controlling for a broad range of individual-level variables. We now turn to a close consideration of this approach, raising two key questions. One pertains to the selection of control variables in general, on which a great deal seems to be taken for granted. The other pertains to the cross-sectional nature of the analysis, which leaves findings of this sort open to some significant vulnerabilities.

MLM and the selection of control variables

As noted, all five papers reviewed above include a range of individual-level control variables. They all include age, sex, and education. Three of the four include whether an individual is unemployed; a different set of three includes variables for religiosity (intensity or denomination), and the third set of three includes ‘subjective income’/‘deprivation’ (both drawn from the ESS variable where respondents indicate whether they are finding it hard to get by on their current income). Other variables common across two studies include life satisfaction and political ideology. Certain controls appear in only one study (e.g. having a partner, or living in a diverse neighbourhood).

The lack of consensus about controls across these studies is striking. This lack of consensus is arguably in line with what appears to be a lack of clarity in quantitative research quite generally about what statistical control is meant to do for us in models intended to identify the impact of one variable (X) on another (Y) (Pearl and Mackenzie 2018; Gangl 2010). When discussed at all, the criterion identified for selection of controls is common: control for ‘other determinants’ of the *dependent* variable (Y). In other words, what matters, ostensibly, is the relationship between the controls and Y.

This criterion is ineffective and potentially misleading. What matters about controls (here labelled W) is not only their relationship with Y. Having identified ‘other determinants’ of Y, we then need to ask: what is the relationship between those (potential) controls and X? In the first instance, there are three possibilities (Bartram 2021). (1) The potential control is an antecedent of X ($W \rightarrow X$); here we say that W is a ‘confounder’ and must be controlled. (2) X is an antecedent of the potential control ($X \rightarrow W$); here W is an ‘intervening variable’. (3) There is no relationship between X and W. Only in scenario #1 is it necessary to include W as a control. In scenario #3, inclusion of W will not cause bias in the estimate of $X \rightarrow Y$, but W is in fact irrelevant. In scenario #2, inclusion of W will likely cause bias in the estimate of $X \rightarrow Y$ (Pearl and Mackenzie 2018); including it might actually harm our estimates.

Controlling for confounders (defined narrowly as in the previous paragraph) helps ensure that our estimates of $X \rightarrow Y$ do not give us a quantity that actually reflects the influence of some other variable on both X and Y. Confounders must be controlled if we are to minimise bias. But if X is an antecedent of W and we control for W, the estimate of $X \rightarrow Y$ gives us a result that hides part of the impact of X. Our estimate is now biased; adding this type of control exacerbates bias rather than mitigating it. For example, if we wanted to know the impact of unemployment on life satisfaction, controlling for income

would hide part of that impact. Becoming unemployed usually decreases one’s income, and the decrease in income will then result in lower life satisfaction. If we control for income, our estimate excludes this ‘indirect effect’ of unemployment on life satisfaction. It is essential to have clarity on the relationship between X and W. In some instances the nature of that relationship might not be as clear as it is in this example; perhaps the relationship is closer to $X \leftrightarrow W$. But that possibility should not lead us to a practice of ignoring how X and W are related.

In the present context, where X is a policy (or set of policies), the question of what controls are needed appears in a different light. In an MLM framework, policy (e.g. MIPEX or MCP) is a ‘level-2’ variable. When X is a level-2 variable, do we need any individual-level (‘level-1’) controls? It seems far from obvious that we do, especially in this context. The idea that we do need level-1 controls is sometimes expressed with reference to ‘compositional effects’ – the possibility that higher-level units (e.g. countries) might differ in the composition of their populations, e.g. age structure, sex balance, educational attainment, religiosity, etc. For some questions, compositional effects might be important. What matters, however, is not just whether individual-level factors affect the dependent variable – for compositional effects to be important, individual-level factors must also affect the focal independent variable. In this context, it seems unlikely: whether a society/government adopts a more liberal/tolerant integration policy seems very unlikely to depend on how old individuals are, or what their sex is, or even how educated they are. By the same token, living in a country with a higher MIPEX score is very unlikely to affect the selected individual-level variables. Among the three scenarios articulated above, we are likely to find that the level-1 controls are irrelevant.

Table 1 confirms that intuition. The table functions first as a replication of the analysis in Schlueter, Masso, and Davidov (2020). The first column presents their results as

Table 1. Replication of Schlueter, Masso, and Davidov (2020).

	Models of anti-Muslim prejudice		
	Schlueter	Replication	No level-1
Intercept	3.400	3.119	3.763
Sex	-0.009	0.009	
Age	0.006	0.006	
Education	-0.026	-0.025	
Unemployed	-0.032	-0.013	
Economic deprivation	0.133	0.135	
Friendships with immigrants	-0.252	-0.248	
Catholic	0.068	0.810	
Protestant	0.036	0.035	
Eastern Orthodox	0.003	-0.015	
Other Christian	-0.085	-0.082	
Jewish	-0.166	-0.129	
Eastern religions	-0.092	-0.109	
Other non-Christian	-0.175	-0.187	
TV exposure	0.041	0.038	
Muslim population	-0.089	-0.089	-0.115
Immigrant integration policies (MIPEX)	-0.009	-0.009	-0.010
State support of religion	-0.018	-0.022	-0.030
Immigration/Muslim-related news	0.130	0.147	0.175
N	30,042	30,068	30,068

Level-1 data are taken from Wave 7 of the European Social Survey (ESS-ERIC 2015); data on level-2 variables are taken from Appendix B in Schlueter et al.’s paper. Details about the countries and variables used, including any recoding, are available in their paper and are not duplicated here.

reported in their own paper. The second column is the replication. The key result of interest is the coefficient for the ‘immigrant integration policies’ (MIPEX) variable; the value is identical in the first two columns (confirming the overall success of the replication). In the third column, the model removes all of the level-1 controls. Here the MIPEX coefficient is very slightly different (0.010, up from 0.009) – but not to the extent that the substantive conclusions would be different. The level-1 controls are indeed irrelevant. The same pattern is apparent in replications of two other papers (Schlueter, Meuleman, and Davidov 2013; Callens and Meuleman 2017), available on request. The issue receives additional attention further below, in the results section.

Cross-sectional analysis and ‘omitted variables’

Individual-level controls are very likely not relevant to the estimation of $X \rightarrow Y$ when X is a level-2 variable. A research question in this context is effectively framed as a property of societies (not individuals): what are the consequences of more liberal/tolerant policies on attitudes towards immigrants (where policies are a level-2 variable)? Note that the dependent variable for such a question can be understood as a level-2 variable as well: if policies have an impact on attitudes, that impact will be apparent at the level of *societal averages* of individual attitudes. From this angle, it is not apparent that MLM is the best approach to use: for the dependent variable, we can simply construct country-level averages of attitudes and dispense with ‘level 1’ altogether.

But especially with a cross-sectional analysis, we must pursue further the question of what control variables are required to estimate $X \rightarrow Y$ with a minimum of bias. With ‘level 1’ out of the picture, that question now pertains to other country-level variables. Here as well we see a range of ‘level-2’ controls in the studies reviewed above. Schlueter, Meuleman, and Davidov (2013) include immigrant group size, unemployment rate, conservatism, and social expenditure. Notably, controlling for those variables has no impact at all on the MIPEX result (a point evident via comparison of models 2 through 6 in their Table 2). Callens and Meuleman (2017) include the unemployment rate and the net migration rate; the results do not include models showing the impact of including (vs. excluding) those variables. Hooghe and de Vroome (2015) include minority population size.

We will revisit in the next section the question of what country-level controls are in fact needed. What needs emphasis here is the potential consequences of having a cross-sectional analysis (as with all four papers reviewed here). The point of controls, again, is to minimise bias; if we omit needed controls (antecedents of X), our results are potentially biased. In fairness, the studies discussed here generally recognise the risks of using cross-sectional data and offer the appropriate caveats. The question is whether it is possible to do better, to overcome the limitations that come with cross-sectional analysis. If we could do a longitudinal analysis of panel data, a fixed-effects specification would obviate the need to control for any time-constant properties of societies (see e.g. Brüderl and Ludwig 2015).

Moving forward

Having considered these two methodological points in combination, this paper offers a contribution taking us beyond the limitations of existing research on this topic, by

constructing a longitudinal analysis of country-level data to estimate the impact of policies on attitudes towards immigrants (policies→attitudes). If we do not need level 1, we can take data from repeated cross-sections and treat the country-level averages of attitudes as panel data. These data can then be matched to a measure of policies, which again is a country-level entity. An analysis along these lines promises to address the question of bias in a way that is not available in a cross-sectional approach. It is also more effective in addressing the question of how things change over time: as countries adopt a more open/accommodating set of policies, do anti-immigrant attitudes subside (as the cross-sectional investigations seem to suggest they would)?

Data and analysis

What is needed for an effective analysis of policies→attitudes is panel data where the appropriate attitudes questions are asked at multiple points in time, and where there are repeated measures of policies available for the same set of countries over the same period. The analysis below uses two pairings: (1) data from Rounds 4 through 9 of the European Social Survey (Jowell 2007) are combined with the MIPEX to construct an analysis covering the period 2008 to 2018; and (2) data from the second, third, and fourth rounds of the European Values Survey (EVS 2011) are combined with the MCP to construct an analysis covering 1990 to 2008.

For the ESS/MIPEX analysis, we construct a dependent variable from questions in the ESS asking about attitudes towards immigrants in three respects: whether immigrants are good/bad for the country's economy (what Callens and Meuleman (2017) call 'economic threat'), whether immigrants undermine or enrich the country's cultural life ('cultural threat'), and whether immigrants make the country a better/worse place to live. Each variable is asked on an 11-point scale (0 to 10). These values are averaged to a single measure, reverse-coded so that higher values represent a greater level of anti-immigrant sentiment, and then aggregated to country-level averages in each ESS round.

The Migrant Integration Policy Index (MIPEX, see mipex.eu) is a summary measure of laws and policies affecting the ability of immigrants to participate on equal terms in core institutions in their society of residence. Higher scores indicate a more developed set of policy measures intended to facilitate integration. The full index derives from expert/scholar evaluations (using specified official documents) and covers eight policy areas: labour market mobility, family reunification, education, political participation, long-term residence rights, access to nationality, anti-discrimination, and health. The full index is not available for earlier years, so the analysis uses a version that covers six of these eight policy areas (excluding health and education).

The countries covered by the ESS/MIPEX analysis are: Belgium, Switzerland, Czech Republic, Germany, Denmark, Estonia, Spain, Finland, France, Britain, Hungary, Ireland, Latvia, Netherlands, Norway, Poland, Portugal, Sweden, and Slovenia. Values taken from each round of the ESS are matched to values taken from the MIPEX for the previous year. Using the ESS it is possible to exclude data from respondents who are themselves immigrants (using variables that pertain to being born in the country as well as holding citizenship of that country).

The EVS was conducted in 1990, 1999, and 2008 – so, for the second analysis, we will draw values from the MCP from 1989, 1998, and 2007. The combination of the EVS and the MCP facilitates an investigation covering a longer time-frame (relative to what is available via use of data from the ESS). This pairing accommodates the possibility that it might take more time for policies to affect attitudes. The set of countries where data are available on both measures for at least two of those year-pairs is: Austria, Belgium, Germany, Denmark, Spain, Finland, France, the UK, Greece, Ireland, Italy, Netherlands, Norway, Portugal, and Sweden.¹ For this analysis, it is not possible to exclude immigrants from the sample.

The EVS/MCP analysis considers four dependent variables that give information about respondents' attitudes towards immigrants. These variables do not have the consistent structure of the three ESS questions, so they are not aggregated. For all three waves of the EVS, respondents are asked whether they would not want to have 'immigrants/foreign workers' as neighbours. The response for individuals is dichotomous, but again the responses will be aggregated to country-level means, a measure that will be treated as a continuous dependent variable (with higher levels denoting more xenophobic attitudes). In the same three rounds, respondents are also asked whether employers should give priority to nationals over immigrants when jobs are scarce. Respondents can agree or disagree; if they spontaneously say 'neither', we assign an intermediate value of 0.5 (in between 1 for agree and 0 for disagree). As before, the aggregation to country-level averages produces a continuous dependent variable.

For the second and third waves (1990 and 1999), two additional questions are posed. Respondents are asked about their preferred policy towards immigration, with four categories for response: (1) let anyone come who wants to; (2) come as long as jobs are available; (3) strict limits on the number of foreigners; and (4) prohibit people coming here from other countries. Respondents are also asked about the extent to which they feel concerned about immigrants, with five options for response: very much, much, to a certain extent, not so much, and not at all. For this variable, responses are reversed so that higher numbers represent more concern, in line with the patterns for the other dependent variables.

The independent variable here is the MCP Index. This index indicates whether a country has policies fostering multiculturalism for immigrants in eight respects: (1) constitutional/legislative/parliamentary affirmation of multiculturalism (together with an institutional basis for implementation); (2) adoption of multiculturalism in the school curriculum; (3) inclusion of ethnic representation/sensitivity in mandates for public media; (4) exemptions from dress codes; (5) accommodation of dual citizenship; (6) public/state funding for ethnic-group organizations/activities; (7) public/state funding for bilingual/mother-tongue instruction; and (8) affirmative action for disadvantaged immigrant groups. Values for each can be 0 for absent, 1 for present and effective, and 0.5 for present but limited or only partially effective. The 'total' value of the index is then additive.

The analyses below consist of 'fixed-effects' models exploiting the panel structure of the data. (The term 'fixed effects' can seem opaque; it is likely more enlightening to refer to these models as 'within' models, evoking a contrast to 'between' models drawing on information between countries in a mode akin to cross-sectional analysis. Bell, Fairbrother, and Jones (2019) are effective on this point.) The coefficients derive

from change over time in departures from each country's mean values on the dependent and independent variables. The coefficients tell us whether anti-immigrant attitudes increase or decrease (or neither) as integration or multicultural policies are adopted (or rescinded). In contrast to cross-sectional analyses, however, that question is answered in a way that controls for any time-constant differences between countries (Brüderl and Ludwig 2015).

An analysis of repeated cross-sectional data in this mode departs in certain respects from a conventional longitudinal analysis of panel data where the individual respondents are constant across waves. The main difference is that the population of the countries changes over time, via the basic demographic processes of birth, death, and migration. The samples selected for each round of the ESS/EVS can be taken as representative of the corresponding populations at each point in time, but the populations themselves do change. We can consider: do the 'replacement' processes amount to confounders in this context, such that it is necessary to adjust our estimates for those processes in some way? That question is addressed below subsequent to the main EVS/MCP analysis.

We now return to the question of what (time-varying) country-level control variables are needed to give an unbiased estimate of the impact of policies on attitudes. There is no need to consider time-constant differences across societies (e.g. pertaining to geography/location or fixed aspects of a country's history). In effect, we need to identify the time-varying determinants of X (i.e. $W \rightarrow X$): why do some countries adopt integration or multicultural policies (while others do not)? From answers to that question, we can then consider: do any of those factors also affect people's attitudes towards immigrants? If there are variables meeting these conditions, then they must be included as controls. But we should also ensure that we don't control for determinants of attitudes that are likely *affected by* the policies themselves ($X \rightarrow W$).

One possibility is per-capita GDP (Quillian 1995; Schneider 2007): perhaps in wealthier countries people feel more secure about immigrants ($W \rightarrow Y$) and governments are thus more willing to adopt policies that accommodate immigrants in general and their differences in a multicultural mode ($W \rightarrow X$). It seems unlikely that the relationship works the other way around ($X \rightarrow W$): more immigration might enhance GDP growth (e.g. Portes 2019), but it is less obvious that integration or multicultural policies specifically have the same impact. A variable for per-capita GDP (in constant dollars, adjusted for purchasing-power parity), in thousands, is thus included in models below (the data are taken from the 'Gapminder' website, which draws on the World Bank and the Penn World Tables – see gapminder.org, the documentation for V26).

Another potential control pertains to the proportion of immigrants in a country's population (Schneider 2007; Semyonov, Rajman, and Gorodzeisky 2006). The impact of such a variable on attitudes towards immigrants could go in either direction. Intuitively, it might seem that people are likely to be more concerned about immigration when there are more immigrants. On the other hand, the 'contact hypothesis' (Allport 1954; McLaren 2003) suggests the reverse possibility: once people encounter actual immigrants (as opposed to the bogeyman of hypothetical immigrants presented by populist politicians), they see real human beings and perhaps develop a sense of empathy. An increased presence of immigrants could also lead countries to adopt more accommodating policies ($W \rightarrow X$); here as well it seems less likely that more

accommodating policies would lead to increased immigration (though that possibility should probably not be dismissed entirely). A variable giving the percentage of immigrants in the population is included, drawing on data from the OECD (see Salt 2001).

Table 2 gives univariate information on the variables used in the analysis, by year/wave. In addition to an overall standard deviation, the table gives ‘within’ standard deviations for each variable, offering an indication of the extent to which each variable changed (within countries) over time. Relative to overall variation, within-country change over time is smaller; there is some degree of stability for all variables, including attitudes towards immigrants (in line with Berning and Schlueter 2016; Kustov, Kaaker, and Reller 2020). The implications of these patterns are discussed in the conclusion.

Table 2. Descriptive/univariate values.

		Mean	Overall St. dev.	Within St. dev.	Minimum	Maximum
ESS/MIPEX						
Anti-immigrant sentiment	2008	4.80	0.63	0.31	3.79	5.89
	2010	4.92	0.67		3.45	6.01
	2012	4.75	0.64		3.71	5.88
	2014	4.84	0.65		3.44	6.23
	2016	4.73	0.80		3.67	6.58
	2018	4.50	0.77		3.63	6.22
MIPEX	2007	59.0	16.0	3.21	34	88
	2009	58.1	16.6		33	88
	2011	59.2	14.7		36	86
	2013	58.6	14.2		36	86
	2015	58.3	14.6		36	86
	2017	58.7	14.1		37	86
Per-capita GDP (US\$ 1000s)	2008	37.9	11.7	2.92	20.4	64.6
	2010	36.5	11.6		21.1	62.4
	2012	37.1	11.4		22.6	62.1
	2014	38.0	11.4		24.1	63.4
	2016	40.0	12.1		25.7	64.2
	2018	42.1	12.4		28.2	70.4
Immigrants as % of population	2008	11.4	5.3	1.41	3.9	25.8
	2010	11.2	5.1		4.1	26.1
	2012	11.3	5.6		1.6	37.0
	2014	12.0	6.3		1.6	27.9
	2016	12.1	6.0		1.6	28.8
	2018	12.6	6.2		1.8	29.5
EVS/MCP						
No immigrants as neighbours	1990	0.119	0.050	0.036	0.046	0.203
	1999	0.109	0.045		0.026	0.165
	2008	0.112	0.057		0.042	0.232
Nationals get job priority	1990	0.667	0.156	0.072	0.356	0.887
	1999	0.609	0.193		0.167	0.828
	2008	0.567	0.181		0.227	0.771
Immigration policy preference	1999	2.46	0.21	0.069	2.08	2.70
	2008	2.51	0.13		2.29	2.75
Concern re immigration	1999	2.81	0.25	0.143	2.48	3.15
	2008	2.83	0.30		2.36	3.25
Multiculturalism policy index	1989	1.33	1.53	1.112	0.00	5.00
	1998	1.97	1.58		0.00	5.00
	2007	3.20	1.89		0.00	7.00
Per-capita GDP (US\$ 1000s)	1989	28.50	6.05	6.076	19.30	42.50
	1998	34.30	7.57		23.40	55.90
	2007	41.90	8.62		38.10	65.10
Immigrants as % of population	1989	3.63	2.61	3.302	0.40	8.90
	1998	6.81	3.53		1.50	12.20
	2007	10.40	3.44		3.80	15.70

Results: ESS/MIPEX

Some comments about interpretation are required before proceeding to the ESS/MIPEX results given in Table 3. The regression tables include standard errors despite misgivings (some readers might want this information to gauge the ‘precision’ of the coefficients as estimates), and the use of asterisks to denote thresholds of statistical significance is not contemplated. (It is tempting to suggest that some social researchers who do quantitative work have an ‘asterisk fetish’ that would be worth overcoming.) Statistical significance is sometimes used as a basis for determining whether an effect is ‘significant’ in some broader sense (call it ‘substantive’ significance) – but it is better to say that this is a *misuse*.

Statistical significance might be useful for gauging whether we can be confident in generalising results from a sample to a larger population. But that usage requires a sample that is representative of a larger population. We do not have data of that sort here. The data are national averages from a group of 19 countries in Europe. We do not have grounds for suggesting that these 19 countries are representative of some larger group of countries. Of course, ‘Europe’ consists of a larger set of countries, but it is far from evident that the countries that are included in the analysis are a representative sample of the larger set that includes countries that are *not* analysed here. Generalisation is therefore not a relevant idea in this context – so, statistical significance is likewise irrelevant. That perspective could inform a view about whether statistical significance is generally relevant to ‘level-2’ variables in analyses that use MLM; in many instances it is likely not relevant, because the level-2 entities are not usually representative of some larger population in the way level-1 respondents in a sample are (Lucas 2014). In any event, the statistical significance will not help us gauge whether the results in Table 3 are ‘significant’ in some other way. Instead, we will consider effect size.²

The analysis in Table 3 indicates that an increase in the MIPEX leads to a slight increase in anti-immigrant sentiment. That conclusion is evident in the fixed-effects

Table 3. Models of anti-immigrant sentiment (ESS).

	Fixed-effects models		Pooled models	
	Model 1	Model 2	Model 3	Model 4
MIPEX	0.020 (0.009)	0.019 (0.010)	-0.016 (0.004)	-0.013 (0.004)
GDP p/c		-0.032 (0.013)		-0.028 (0.006)
Immigrant stocks		-0.001 (0.026)		-0.006 (0.011)
2010	0.104 (0.101)	0.048 (0.107)	0.113 (0.215)	0.059 (0.189)
2012	-0.089 (0.102)	-0.143 (0.109)	-0.041 (0.215)	-0.171 (0.187)
2014	0.007 (0.102)	-0.019 (0.113)	0.034 (0.215)	-0.065 (0.187)
2016	-0.116 (0.103)	-0.077 (0.116)	-0.073 (0.217)	-0.126 (0.189)
2018	-0.338 (0.102)	-0.232 (0.129)	-0.307 (0.215)	-0.291 (0.186)
Constant			5.768 (0.291)	6.775 (0.292)
Observations	112	109	112	109
R ²	0.23	0.29	0.16	0.42

models; the inclusion of the specified controls makes little difference (Model 2, vs. Model 1). The coefficients here are positive (denoting higher levels of anti-immigrant sentiment) – but they are also very small, at 0.020 and 0.019. The ‘within’ standard deviation of the dependent variable here is 0.31. By comparison, an effect of 0.02 is very small – perhaps small enough to be described as negligible. The largest increase (from 2008 to 2018) in the value of the MIPEX in this set of countries is 10 (for Estonia and Poland). Even with that level of change in the MIPEX, the predicted change in anti-immigrant sentiment would be 0.2. The *range* of the dependent variable runs from approximately 3.5 to 6.5 – and a change of 0.2 in that context does not seem at all substantial.³

Table 3 also includes models from a ‘pooled’ specification (Models 3 and 4). This specification is cross-sectional with time addressed only via dummy variables for survey round; in that respect it is closer to what is used in existing research on this topic (especially Green et al. 2020, who also match the MIPEX and the ESS). In these models, higher values of the MIPEX are associated with *lower* levels of anti-immigrant sentiment – in line with the broad shape of existing findings. Comparing the coefficients to their standard errors (in parentheses), we could also anticipate finding that these results are ‘statistically significant’ (though again that idea is not relevant in this context).

But there are clear reasons to prefer the results from the fixed-effects models – in particular, the fact that these results are not biased from omission of time-constant between-country differences. The contrast between the two sets of results leads to what seems like an important insight: existing research, using a cross-sectional approach, gives findings that very likely reflect ‘pre-existing’ differences among countries that in fact need to be controlled as confounders. The FE models, which are more successful in that regard, indicate that the impact of integration policies (as captured by the MIPEX) is the opposite of what is given in existing/previous research (compare Kragten and Rözer (2017) for a similar pattern of cross-sectional vs. longitudinal results, pertaining to the relationship between inequality and health). More conservatively, our core finding is that integration policies do not mitigate anti-immigrant sentiments – a formulation that still stands as a contrast to current understandings.

Similar to the replication of Schlueter, Masso, and Davidov (2020) presented above, we can consider whether individual-level controls are consequential for the analysis in Table 3. In a supplementary analysis (available in an online appendix, Table A2), we explored models using individual-level controls via a ‘within-between’ specification (Bell, Fairbrother, and Jones 2019). Sometimes described as a ‘Mundlak’ model (Mundlak 1978), this approach facilitates inclusion of individual-level variables in a ‘between’ mode, in the context of a panel model where macro-level independent variables (e.g. the independent variables used in Table 3) are decomposed into within and between components. These models enable us to consider whether ‘compositional effects’ are consequential for the estimation of the within estimates that are our main interest here. The answer is that the individual-level factors are not consequential for that purpose: the coefficient for the MIPEX variable does not change at all (at three decimal points) when the individual-level controls are added. This robustness check reinforces the logic of using a purely longitudinal/within approach as in Table 3, which thus forms the basis for our main results.

What about reverse causation (attitudes→policies)? We constructed an exploratory random-intercepts cross-lagged panel model (RI-CLPM, Hamaker, Kuiper, and

Grasman 2015) to evaluate the idea that failure to account for the possibility of reciprocal causal links might be suppressing evidence of the causal effect we are evaluating (policies→attitudes). This model (available via online supplement, Table A3) did not offer any evidence supporting a conclusion different from what is evident in Table 3. This model likely has limited value: with only 19 ‘observations’ (countries) there is not a great deal of leverage for estimating the number of parameters in a typical RI-CLPM. This topic deserves further exploration; it is entirely plausible that ‘attitudes→policies’ captures an important pattern, with xenophobic attitudes perhaps impeding the adoption of more accommodating policies towards immigrants. In any event, our own exploration does not suggest that our conclusions above (about policies→attitudes) are incorrect.

Results: EVS/MCP

An inspection of the bottom portion of Table 2 suggests that the attitudes towards immigrants measured in the EVS, considered via country-level averages, do not change a great deal over the time periods investigated here. The exception pertains to views about whether employers should give preference to nationals over immigrants in hiring decisions: here we see a decrease of one-tenth of a point from the first to the third wave. That decrease is more than half of the aggregated standard deviation of this variable (0.18) and larger than the within standard deviation (0.072). For the other variables, however, any change over time appears to be very small. In contrast, there is a steady increase in the adoption of multicultural policies over the same time period. At first glance, then, it does not appear that these policy developments contribute to change in attitudes.

The models presented in Table 4 help us evaluate whether this surface-level view changes when the relationship is investigated in a more formal way, with results adjusted for the identified confounders. Intuitively, all the coefficients for the MCP in Table 4 seem small. Only for the ‘jobs’ and ‘policy’ analyses does a 1-unit increase in the MCP index result in a change in the dependent variable amounting to more than one one-hundredth of a point (−0.016 and 0.029, respectively, for models where control variables are included). If we compare those numbers to the within standard deviations for the

Table 4. Fixed-effects (‘within’) models of attitudes towards immigrants/immigration (EVS).

	Immigrants not wanted as neighbours		Employers should prefer nationals		Restrictive immigration policy preferences		Concern about immigration	
Multiculturalism Policies Index (MCP)	−0.002 (0.009)	−0.004 (0.009)	−0.015 (0.014)	−0.016 (0.014)	0.039 (0.025)	0.029 (0.024)	−0.028 (0.060)	0.008 (0.057)
Per-capita GDP (PPP)		0.003 (0.003)		0.003 (0.005)		0.007 (0.011)		−0.041 (0.027)
Immigrants as % of population		0.000 (0.005)		0.005 (0.008)		0.018 (0.010)		−0.029 (0.025)
1990	−0.012 (0.019)	−0.030 (0.027)	−0.071 (0.030)	−0.103 (0.043)				
1999	−0.005 (0.024)	−0.048 (0.050)	−0.082 (0.038)	−0.151 (0.078)	0.013 (0.042)	−0.095 (0.084)	0.044 (0.104)	0.417 (0.204)
N	43	43	43	43	29	29	29	29
R ²	0.027	0.075	0.434	0.453	0.303	0.517	0.020	0.341

respective variables, on the other hand, the effects for those two variables now appear to be non-trivial. For the jobs variable, the coefficient amounts to slightly more than one-tenth of the standard deviation ($-0.016/0.072 = -0.22$). For the policy variable, the effect is larger ($0.029/0.069 = 0.42$). The jobs result, then, exceeds the conventional threshold associated with Cohen's D for a 'small' effect (0.20); the policy result is not far from the threshold for a 'medium' effect (0.50).

For the other two variables (neighbours and concern), any effect seems negligible. A bivariate model for 'concern' suggests that an increase in the MCP leads to a decline in concern about immigration that is perhaps substantial (-0.028) – but the addition of controls indicates that that apparent decline more plausibly stems from changes in p/c GDP and the relative size of the immigration population. Where effects are not negligible (jobs and policy), they go in opposite directions. An increased adoption of multicultural policies leads to a decrease in agreement with the idea that employers should favour nationals over immigrants in hiring decisions. But it leads more strongly to an increase in preferences for restrictive immigration policies. Those two trends (which are evident in Table 2 as well, without connection to the MCP) are not necessarily in conflict: people can support fair/equal treatment of immigrants already 'here' while also favouring a reduction in (new) immigration.

Even so, it seems reasonable to conclude that there is only weak evidence for the idea that adopting multicultural policies leads to a reduction in hostility to immigrants and immigration. The only result that supports that conclusion pertains to hiring preferences. For the other measures, the effect either forms a contrast (as with the policy measure) or is negligible, at least as gauged via the comparison of the coefficient to standard deviation.

We could press the issue of effect size by asking about the likely impact of a more radical adoption of multicultural policies. The country that travelled the furthest distance in these terms was Finland: in 1989 Finland's index value was 0 and by 2007 it had increased to 6. For other countries, the increase was smaller – and in two instances (Denmark and the Netherlands) the value of the index declined, as previously adopted policies were rescinded. We might then ask (in a counterfactual mode): what if Denmark and Finland had been more like Finland?

That question (like all counterfactual questions) cannot be answered directly via data. In a speculative mode, the answer might appear obvious: if Denmark and the Netherlands had been more like Finland, they would ('obviously?') have seen a similar evolution of attitudes. Via multiplication, the predicted change in Finnish attitudes regarding hiring decisions ('jobs') would be $-0.096 (= -0.016 \times 6)$, and the predicted change in 'policy' preferences would be $0.174 (= 0.029 \times 6)$. (The actual jobs figure in Finland declined from 0.768 to 0.654, a difference almost equal to the predicted change; the policy figure increased from 2.470 to 2.490 – here as well, almost equal to the predicted change.)

Would Denmark and the Netherlands have actually experienced similar evolution of attitudes if they had adopted multiculturalist policies with a zeal similar to Finland's, 'all else equal'? This question leads us back to the risks associated with drawing conclusions via comparison of countries to one another (as against using 'within' information taken from change over time). If Denmark and the Netherlands had been more like Finland, they would have been *less like themselves*; a change of that sort would have involved those countries being/becoming quite different from what they were in reality. It is difficult to know what 'all else equal' would mean, from this angle.

The extent of risk involved in drawing conclusions in this mode can again be gauged by comparing to a ‘pooled’ specification (having more in common with a cross-sectional analysis) of models equivalent to those in Table 4. These results are given in an appendix (Table A1); here the effects are all more ‘negative’, i.e. they offer stronger support for the idea that adoption of multicultural policies leads to less hostility towards immigrants and immigration. This analysis does not adjust for the (unmeasured) ways in which these various countries are different from one another; as suggested above for the ESS/MIPEX analysis, it is less effective (relative to a FE specification) in controlling for those differences and in particular for the way they might lead to different tendencies to adopt multicultural policies. Again, we see that any conclusion suggesting that adoption of multicultural policies generally reduces hostility to immigrants would likely reflect ‘pre-existing’ differences among countries that in fact need to be controlled as confounders.

We now turn to the question of whether failing to account for cohort replacement processes might be distorting the conclusions derived from Table 4. As noted, that possibility pertains to the fact that the repeated cross-sections represent changing populations over time. People who were already relatively old in 1990 are less likely to be present, in the sample and the population alike, via death, refusal, or residence in unsampled units (e.g. care homes). By the same token, people who were relatively young in 2008 would not have been part of the sample (nor the relevant population) in earlier rounds. This replacement process might affect the evolution of attitudes towards immigrants, and an analysis that does not adjust for it might produce results that do not accurately represent the effect of multicultural policies.

Table 5 explores that possibility by presenting results from analyses specified to isolate changes that might be taking place *within* cohorts. As per the row labels, each row starts with an age-specific subsample in 1990 (the first year of measurement used from the EVS); for subsequent rounds, the values for the attitude variables are drawn from the corresponding cohorts at that later point in time. (So: for the first row, which takes respondents in their 20s in 1990, the attitude measures for 1999 are drawn from respondents aged 29–38, and the measures for 2008 are drawn from respondents aged 38–47.

Table 5. Coefficients for MCP effect on attitudes towards immigrants, exploration of cohorts.

	Immigrants not wanted as neighbours		Employers should prefer nationals		Restrictive immigration policy preferences		Concern about immigration	
Age 20–29 in 1990	0.001	0.000	–0.017	–0.018	0.057	0.049	–0.056	–0.024
Controls?	No	Yes	No	Yes	No	Yes	No	Yes
Age 30–39 in 1990	–0.010	–0.012	–0.015	–0.018	0.009	–0.002	–0.034	0.008
Controls?	No	Yes	No	Yes	No	Yes	No	Yes
Age 40–49 in 1990	0.000	–0.002	–0.012	–0.014	0.024	0.016	–0.005	0.031
Controls?	No	Yes	No	Yes	No	Yes	No	Yes
Age 50–59 in 1990	–0.008	–0.010	–0.022	–0.022	0.019	0.006	0.027	0.068
Controls?	No	Yes	No	Yes	No	Yes	No	Yes
Age 60–69 in 1990	–0.010	–0.012	–0.030	–0.032	0.029	0.021	0.036	0.076
Controls?	No	Yes	No	Yes	No	Yes	No	Yes
Average across cohorts	–0.005	–0.007	–0.019	–0.021	0.028	0.018	–0.006	0.032
Controls?	No	Yes	No	Yes	No	Yes	No	Yes
From Table 3	–0.002	–0.004	–0.015	–0.016	0.039	0.029	–0.028	0.008
Controls?	No	Yes	No	Yes	No	Yes	No	Yes

Equivalent selections are made for people who in 1990 were in their 30s, 40s, 50s and 60s.) The cohort-specific coefficients are averaged in the penultimate row (of numbers). The final row (of numbers) in the table duplicates the MCP coefficients given in Table 3, to facilitate comparison.

The question is whether the cohort-specific results depart substantially from the results given in Table 4. For the most part, the differences are not large, especially in the first two pairs of columns (each pair corresponding to models without and with controls). For ‘restrictive immigration policy preferences’ and ‘concern about immigration’, we can perhaps discern differences that merit further attention. For our purposes, what matters is that the cohort-specific results do not lead us *towards* a conclusion that the adoption of multicultural policies reduces xenophobia (i.e. the finding of previous research). On the contrary, the cohort-specific analysis takes us *further away* from that conclusion, especially in connection with the ‘concern’ variable. In the final column, the MCP coefficient in a model with controls is now 0.032, as against 0.008 in the corresponding model from Table 4 (so, the policies appear to reinforce xenophobia in this sense, rather than mitigating it).

There are of course more elaborate methods that could be used to analyse repeated cross-sectional data (Lebo and Weber 2015). But those methods are intended to address issues that do not amount to problems here, including incorrect standard errors, bias from autocorrelation in long time-series, and exploring possibly dynamic effects of level-1 variables. Taking these in turn: (1) standard errors are irrelevant here – the level-2 units (countries) do not form a sample and extrapolation via hypothesis-testing is unnecessary (and indeed inappropriate). (2) The time-series formed by the data used here (considered as the number of measurements/time periods) is relatively short (Lebo and Weber’s technique is directed primarily at situations where $T > 50$). (3) The question of level-1 effects is irrelevant to the estimation of policies→attitudes. In general, when variables are selected as controls (W) for the estimation of $X \rightarrow Y$, the coefficients for the controls cannot be treated as estimates of effects denoted by $W \rightarrow Y$; interpretation along these lines amounts to the ‘Table 2 fallacy’ (Westreich and Greenland 2013).

Conclusion

Adoption of integration and/or multicultural policies, then, does not generally lead to reduction of hostility towards immigrants and immigration in the specific European countries investigated here. The optimistic findings of previous research are not confirmed in a longitudinal investigation; if anything, the findings offered here are more in line with the pessimistic conclusions of Citrin, Levy, and Wright (2014), who find that multiculturalism policies intensify the connection between hostility towards immigration and dissatisfaction with politics. Multicultural policies appear to contribute to a slightly stronger sense of fairness towards immigrants when it comes to hiring decisions. But it leads more strongly to an increased preference for restrictive immigration policies – though even for this measure the apparent impact is still small in the terms discussed above. In the big picture, any effect of adopting integration/multicultural policies on attitudes towards immigrants and immigration (policies→attitudes) seems generally weak.

This conclusion emerges from a methodological position holding that the question is best addressed by considering change over time ‘within’ countries, as against comparison ‘between’ countries at one point in time. As with longitudinal analysis more generally, this approach is more effective at mitigating the extent of omitted variable bias, in particular with regard to time-constant between-country differences (Brüderl and Ludwig 2015).

This research of course has certain limitations. The article considers the potential effects of policy on attitudes only via country-level averages. Effects are likely to differ for individuals with different characteristics; that possibility is clearly established at least for cross-sectional associations (e.g. Ziller 2020, exploring differences rooted in whether people hold egalitarian values; see also Citrin, Levy, and Wright 2014). Most of the cross-sectional research discussed in the ‘Previous research’ section above explores average effects, and this article is addressed in response to findings constructed in that way. But future research should indeed explore heterogeneity, as a way of further demonstrating the value of adopting a longitudinal approach.

Another potential limitation has to do with the fact that the extent of within-country over time change in the variables explored here is reasonably viewed as small (via Table 2); in these circumstances, we might wonder whether a longitudinal analysis is sensible. The answer depends on our research goals. If the goal is simply to identify an association (asking whether attitudes are different in countries with different policy frameworks) then perhaps the answer is no. But the value of identifying an association is not clear. An association is surely more interesting and valuable if it is generated via a causal relationship. In a context where xenophobia has deepened (Mudde 2013; Yilmaz 2012), it is important to know whether the adoption of policies that are more accommodating to immigrants is an effective response. The researchers who conducted the cross-sectional studies discussed above are careful to avoid stating their conclusions in those terms, but the question surely has intrinsic value. The answer offered in this article is that attitudes do not appear to change all that much, even when policies do (compare Kustov, Kaaker, and Reller 2020).

The research presented here suggests in general the desirability of considering policies→attitudes using a longitudinal approach. For future research along these lines, a key message arising from this paper is that a longitudinal approach is not only desirable but it is also sometimes possible – especially if we look beyond the idea that MLM of cross-sectional survey data is the ‘obvious’ way of doing the research. To consider the impact of policies – a country-level variable – it is more productive to ignore ‘level 1’ and focus on other variables conceived at country level, for which repeated measures are sometimes available. That approach is relevant beyond the question of how policies might affect attitudes – it is likely useful for investigation of the way policies might affect any outcome conceived in the first instance at the level of individuals.

Notes

1. Not every country in the list above offers data for every variable. Nothing is available for Norway in 1999 and for Greece in 1990. The implication is that the analysis of the policy and concern variables cannot use Norway (as it would provide data from only one wave).

2. This approach also helps avoid the potential objection that “Type 2 errors” (failure to reject a false null hypothesis) might follow from the way a small sample contributes to larger standard errors.
3. This analysis includes data from Estonia and Latvia – and so a consideration from Gorodzeisky and Leykin (2020) merits attention. In those countries data on “immigrants” include a good many people of Russian origin who did not themselves migrate to Estonia and Latvia – rather, they became “foreigners” after the fall of the USSR. (For many this foreign status was assigned despite their having been born locally.) In a supplementary analysis (available on request), we have substituted what from this angle would be considered more reasonable estimates of the immigrant stocks (%) variable. The impact is minimal; the overall conclusions remain unchanged.

Acknowledgements

We are grateful to Elmar Schlueter for helpful feedback on an earlier draft and to Ben Aviani-Bartram for assistance with data.

Disclosure statement

No potential conflict of interest was reported by the author(s).

Data availability

Data used in this paper are available directly from the sources indicated in the text and references.

Notes on contributors

David Bartram is Associate Professor in Sociology at the University of Leicester. He is co-editor of the Journal of Happiness Studies and President of RC31, the section on the Sociology of Migration of the International Sociological Association. He has held grants from the UK Economic and Social Research Council, Leverhulme, and the Nuffield Foundation. He gained a PhD from the University of Wisconsin–Madison and a BA from Kenyon College.

Erika Jarochova is a PhD student in Sociology at the University of Leicester. She gained undergraduate and master’s degrees from Charles University in the Czech Republic.

ORCID

David Bartram  <http://orcid.org/0000-0002-7278-2270>

References

- Allport, G. 1954. *The Nature of Prejudice*. Cambridge: Addison-Wesley.
- Bartram, D. 2021. “Cross-Sectional Model-Building for Research on Subjective Well-Being: Gaining Clarity on Control Variables.” *Social Indicators Research*, doi:10.1007/s11205-020-02586-3.
- Bell, A., M. Fairbrother, and K. Jones. 2019. “Fixed and Random Effects Models: Making an Informed Choice.” *Quality & Quantity* 53: 1051–1074. doi:10.1007/s11135-018-0802-x.

- Berning, C. C., and E. Schlueter. 2016. "The Dynamics of Radical Right-Wing Populist Party Preferences and Perceived Group Threat." *Social Science Research* 55: 83–93. doi:10.1016/j.ssresearch.2015.09.003.
- Bloemraad, I., and M. Wright. 2014. "'Utter Failure' or Unity out of Diversity? Debating and Evaluating Policies of Multiculturalism." *International Migration Review* 48 (1_suppl): 292–334. doi:10.1111/imre.12135.
- Blumer, H. 1958. "Race Prejudice as a Sense of Group Position." *The Pacific Sociological Review* 1 (1): 3–7. doi:10.2307/1388607.
- Brüderl, J., and V. Ludwig. 2015. "Fixed-Effects Panel Regression." In *The SAGE Handbook of Regression Analysis and Causal Inference*, edited by H. Best, and C. Wolf, 327–357. Los Angeles: SAGE Reference.
- Callens, M.-S., and B. Meuleman. 2017. "Do Integration Policies Relate to Economic and Cultural Threat Perceptions? A Comparative Study in Europe." *International Journal of Comparative Sociology* 58 (5): 367–391. doi:10.1177/0020715216665437.
- Citrin, J., M. Levy, and M. Wright. 2014. "Multicultural Policy and Political Support in European Democracies." *Comparative Political Studies* 47 (11): 1531–1557. doi:10.1177/0010414013512604.
- Davidov, E., and M. Semyonov. 2017. "Attitudes Toward Immigrants in European Societies." *International Journal of Comparative Sociology* 58 (5): 359–366. doi:10.1177/0020715217732183.
- European Social Survey ERIC (ESS ERIC). 2015. European Social Survey, Round 7 - 2014. Norwegian Centre for Research Data. doi:10.21338/NSD-ESS7-2014.
- EVS. 2011. "European Values Study Longitudinal Data File 1981-2008 (EVS 1981-2008)." *GESIS Data Archive*, doi:10.4232/1.11005.
- Gangl, M. 2010. "Causal Inference in Sociological Research." *Annual Review of Sociology* 36 (1): 21–47. doi:10.1146/annurev.soc.012809.102702.
- Gorodzeisky, A., and I. Leykin. 2020. "When Borders Migrate: Reconstructing the Category of 'International Migrant'." *Sociology* 54 (1): 142–158. doi:10.1177/0038038519860403.
- Green, E. G. T., E. P. Visintin, O. Sarrasin, and M. Hewstone. 2020. "When integration policies shape the impact of intergroup contact on threat perceptions: A multilevel study across 20 European countries." *Journal of Ethnic and Migration Studies* 46 (3): 631–648. <http://doi.org/10.1080/1369183X.2018.1550159>.
- Hamaker, E. L., R. M. Kuiper, and R. P. P. P. Grasman. 2015. "A Critique of the Cross-Lagged Panel Model." *Psychological Methods* 20 (1): 102–116. doi:10.1037/a0038889.
- Hooghe, M., and T. de Vroome. 2015. "How Does the Majority Public React to Multiculturalist Policies? A Comparative Analysis of European Countries." *American Behavioral Scientist* 59 (6): 747–768. doi:10.1177/0002764214566499.
- Jæger, M. M. 2013. "The Effect of Macroeconomic and Social Conditions on the Demand for Redistribution: A Pseudo Panel Approach." *Journal of European Social Policy* 23 (2): 149–163. doi:10.1177/0958928712471225.
- Jowell, R. 2007. *European Social Survey, Technical Report*. London: Centre for Comparative Social Surveys, City University.
- Kragten, N., and J. Rözer. 2017. "The Income Inequality Hypothesis Revisited: Assessing the Hypothesis Using Four Methodological Approaches." *Social Indicators Research* 131 (3): 1015–1033. doi:10.1007/s11205-016-1283-8.
- Kustov, A., D. Kaaker, and C. Reller. 2020. "The Stability of Immigration Attitudes: Evidence and Implications." *Journal of Politics*.
- Lebo, M. J., and C. Weber. 2015. "An Effective Approach to the Repeated Cross-Sectional Design." *American Journal of Political Science* 59 (1): 242–258. doi:10.1111/ajps.12095.
- Lucas, S. R. 2014. "An Inconvenient Dataset: Bias and Inappropriate Inference with the Multilevel Model." *Quality & Quantity* 48 (3): 1619–1649. doi:10.1007/s11135-013-9865-x.
- McLaren, L. M. 2003. "Anti-Immigrant Prejudice in Europe: Contact, Threat Perception, and Preferences for the Exclusion of Migrants." *Social Forces* 81 (3): 909–936. doi:10.1353/sof.2003.0038.

- Mudde, C. 2013. "Three Decades of Populist Radical Right Parties in Western Europe: So What?" *European Journal of Political Research* 52 (1): 1–19. doi:10.1111/j.1475-6765.2012.02065.x.
- Mundlak, Y. 1978. "On the Pooling of Time Series and Cross Section Data." *Econometrica* 46 (1): 69–85.
- Niessen, J., et al. 2007. *Migrant Integration Policy Index*. London: British Council.
- Olivera, J. 2015. "Changes in Inequality and Generalized Trust in Europe." *Social Indicators Research* 124 (1): 21–41. doi:10.1007/s11205-014-0777-5.
- Pearl, J., and D. Mackenzie. 2018. *The Book of Why: The New Science of Cause and Effect*. London: Allen Lane.
- Pettigrew, T. F., and L. R. Tropp. 2006. "A Meta-Analytic Test of Intergroup Contact Theory." *Journal of Personality and Social Psychology* 90 (5): 751–783. doi:10.1037/0022-3514.90.5.751.
- Portes, J. 2019. "The Economics of Migration." *Contexts* 18 (2): 12–17. doi:10.1177/1536504219854712.
- Quillian, L. 1995. "Prejudice as a Response to Perceived Group Threat." *American Sociological Review* 60 (4): 586. doi:10.2307/2096296.
- Salt, J. 2001. *Current Trends in International Migration in Europe*. Council of Europe. https://www.coe.int/t/dg3/migration/archives/Documentation/Migration_management/2001_Salt_report_en.pdf.
- Scheepers, P., M. Gijssels, and M. Coenders 2002. "Ethnic Exclusionism in European Countries: Public Opposition to Civil Rights for Legal Migrants as a Response to Perceived Ethnic Threat." *European Sociological Review* 18 (1): 17–34. doi:10.1093/esr/18.1.17.
- Schlueter, E., A. Masso, and E. Davidov. 2020. "What Factors Explain Anti-Muslim Prejudice?" *Journal of Ethnic and Migration Studies* 46 (3): 649–664. doi:10.1080/1369183X.2018.1550160.
- Schlueter, E., B. Meuleman, and E. Davidov. 2013. "Immigrant Integration Policies and Perceived Group Threat: A Multilevel Study of 27 Western and Eastern European Countries." *Social Science Research* 42 (3): 670–682. doi:10.1016/j.ssresearch.2012.12.001.
- Schneider, S. L. 2007. "Anti-Immigrant Attitudes in Europe: Outgroup Size and Perceived Ethnic Threat." *European Sociological Review* 24 (1): 53–67. doi:10.1093/esr/jcm034.
- Semyonov, M., R. Raijman, and A. Gorodzeisky. 2006. "The Rise of Anti-Foreigner Sentiment in European Societies, 1988–2000." *American Sociological Review* 71 (3): 426–449. doi:10.1177/000312240607100304.
- Sherif, M., B. J. White, and O. J. Harvey. 1955. "Status in Experimentally Produced Groups." *American Journal of Sociology* 60 (4): 370–379. doi:10.1086/221569.
- Westreich, D., and S. Greenland. 2013. "The Table 2 Fallacy: Presenting and Interpreting Confounder and Modifier Coefficients." *American Journal of Epidemiology* 177 (4): 292–298. doi:10.1093/aje/kws412.
- Yilmaz, F. 2012. "Right-wing Hegemony and Immigration." *Current Sociology* 60 (3): 368–381. doi:10.1177/0011392111426192.
- Ziller, C. 2020. "Established and Excluded? Immigrants' Economic Progress, Attitudes Toward Immigrants, and the Conditioning Role of Egalitarianism and Intergroup Contact." *Political Studies*, doi:10.1177/0032321720953561.