

Does Ethnic Diversity Have a Negative Effect on Attitudes towards the Community? A Longitudinal Analysis of the Causal Claims within the Ethnic Diversity and Social Cohesion Debate

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Abstract

Studies demonstrate a negative association between community ethnic diversity and indicators of social cohesion (especially attitudes towards neighbours and the community), suggesting diversity causes a decline in social cohesion. However, to date, the evidence for this claim is based solely on cross-sectional research. This article performs the first longitudinal test of the impact of diversity, applying fixed-effects modelling methods to three waves of panel data from the British Household Panel Survey, spanning a period of 18 years. Using an indicator of affective attachment, the findings suggest that changes in community diversity do lead to changes in attitudes towards the community. However, this effect differs by whether the change in diversity stems from a community increasing in diversity around individuals who do not move (*stayers*) or individuals moving into more or less diverse communities (*movers*). Increasing diversity undermines attitudes among *stayers*. Individuals who *move* from a diverse to a homogeneous community report improved attitudes. However, there is no effect among individuals who *move* from a homogeneous to a diverse community. This article provides strong evidence that the effect of community diversity is likely causal, but that prior preferences for/against out-group neighbours may condition diversity's impact. It also demonstrates that multiple causal processes are in operation at the *individual-level*, occurring among both *stayers* and *movers*, which collectively contribute to the emergence of average cross-sectional differences in attitudes between communities. Unique insights into the causal impact of community disadvantage also emerge.

Introduction

With immigration at historically high levels across many European countries, research suggesting ethnic diversity negatively impacts social cohesion has engendered alarm. Although concerns have long existed in the literature, research by Putnam (2007) showing evidence of social withdrawal in diverse US communities has

generated anxiety across public-political spheres. Despite a substantial volume of research, debate continues as to the veracity of Putnam's findings (Morales, 2013). However, a significant omission is that, to the best of our knowledge, research has only examined diversity's effect using cross-sectional data. Assuming a causal effect (or lack thereof) from cross-sectional

findings can be problematic, especially in neighbourhood studies where selection bias is a problem. This article remedies this omission, examining the effect of community ethnic diversity on a key dimension of social cohesion: community attachment. Using three waves of panel data for individuals in England and Wales, spanning a period of 18 years, we test the causal assumptions of the effect of ethnic diversity on cohesion.

Background and Analytic Framework

Theory and Evidence

We first outline the theoretical framework applied in the ethnic diversity/social cohesion literature (*for fuller discussions see van der Meer and Tolsma, 2014*). Under the threat hypothesis, as proximate out-groups increase in size, superordinate groups become more hostile owing to actual/perceived competition to their economic/social privilege (Blalock, 1967). This competition, driven by contextual exposure, has a psychological impact on individuals, translating into feelings of threat, fomenting prejudice, and reducing cohesion. Individuals may also perceive ‘liabilities... associate[d] with integrated neighborhoods, such as crime, deterioration, and the decline of property values’ (Krysan *et al.*, 2009: 531). At higher diversity, social withdrawal may occur in response to these perceptions while flight/avoidance may further disrupt cohesion (Greif, 2009). Increasing ethnic diversity, alongside ‘linguistic diversity... , diversity in social norms’ and a lack of shared experiences may also ‘induce feelings of anomie’ (van der Meer and Tolsma, 2014: 464). Disparities in social norms, and attendant ‘blocked’ communication, may, in turn, engender feelings of social exclusion, fear, and distrust. Similarly, concepts of ‘homophily’ imply ties are more likely to form *within* ethnic groups, undermining networks in diverse areas. In sum, whether the mechanisms are threat/prejudice, perceived liabilities, innate tendencies to ingroup-trust/connectivity, or anomie, these theories suggest diversity reduces cohesion.

Conversely, the contact hypothesis posits that exposure to out-groups foments cohesion as diversity increases inter-ethnic interaction. Contact should promote positive inter-group attitudes, eroding prejudice and perceived threat, undermining stereotypes, and generating out-group trust (Allport, 1954). Thus, as contextual diversity rises, opportunities for out-group interaction increase, generating cohesion (or having no impact owing to any ‘buffering’ inter-ethnic interaction provides from negative diversity effects (Stolle, Soroka and Johnston, 2008; Laurence, 2011)).

Research has tested this framework using a range of social cohesion indicators,¹ including neighbour-specific and generalized-trust, local/non-local networks, and civic/political participation. Given the reviews available, we only briefly summarize the findings (*see Morales, 2013; van der Meer and Tolsma, 2014*). Outside the United States, studies have largely produced mixed results. Some argue that this inconclusiveness stems from ‘American Exceptionalism’, while others suggest disadvantage is the main culprit (Sturgis *et al.*, 2011). Yet, despite this lack of general corroboration, reviews do highlight consistency across one set of tests: ‘intra-neighbourhood cohesion is quite consistently eroded by the level of ethnic heterogeneity in neighborhoods’, especially attitudes towards the community (Morales, 2013; van der Meer and Tolsma, 2014: 471).

A key feature of such intra-community cohesion is neighbourhood attachment/belonging, characterized as the ‘affective’, ‘emotional bond or connection that people develop toward specific places... via repeated positive interactions’ (Dallago *et al.*, 2009: 148). Studies of intra-community cohesion often include indicators of attachment/belonging alongside measures of neighbour-trust, -reciprocity, and -connectivity, and argue that attachment forms an important element of social capital (Letki, 2008; Fieldhouse and Cutts, 2010). Elsewhere, attachment is seen as a distinct dimension of social cohesion, existing alongside social capital with feedback between the two, or as a psychological prerequisite to social capital (Perkins, Hughey and Speer, 2002).

Although some conceptual differences exist, when attachment is measured as a distinct outcome in studies examining the effect of diversity, it behaves similarly to other indicators of intra-community cohesion: at higher diversity, attachment is lower (Wilson and Baldassare, 1996; van Ham and Feijten, 2008; Greif, 2009; Feijten and van Ham, 2009; Bailey, Kearns & Livingston, 2012; Górný and Toruńczyk-Ruiz, 2014). In explaining this finding, studies use the same theoretical framework as the general diversity/cohesion literature, including threat/contact (Górný and Toruńczyk-Ruiz, 2014), perceived liabilities (e.g. safety; Greif, 2009; Krysan *et al.*, 2009), and anomie (Taylor *et al.*, 1985). A high degree of theoretical/measurement similarity therefore exists between the neighbourhood attachment and community cohesion literatures, as well as the research on how attachment, and cohesion more generally, react to diverse environments.

In sum, given the increasing diversity of many countries, findings that cohesion is lower in diverse communities are troubling. However, to the best of our knowledge, all research into diversity’s impact has relied

on cross-sectional data. This is problematic, as associations observed under cross-sectional designs could be driven by selection, omitted variable bias, or temporal disordering. The proceeding section outlines a series of alternative possible explanations for the diversity/cohesion cross-sectional association.

Non- and Conditional-Causal Explanations

The first problem in cross-sectional analyses is that associations between variables may be driven by omitted variables. In relation to diversity, owing to historical, cultural, and economic factors, many immigrants' community choices are constrained to more disadvantaged, urban areas, which are also associated with lower cohesion. Putnam (2007) suggests that diverse communities may also possess deficits of public amenities (e.g. community centres) stemming from political inequalities, which may account for lower cohesion. Sturgis *et al.* (2011) show that controlling for a broad range of community characteristics substantially reduces the association between diversity and neighbour-trust. Omitted variables could therefore be driving cross-sectional associations.

Another problem is selection bias, and whether any area differences in outcomes are a product of neighbourhood factors or the differential selection of certain individuals into particular neighbourhoods. Putnam (2007) suggests such explanations are implausible, as minorities would have to choose to select into neighbourhoods where cohesion is at its lowest, or more trusting/sociable individuals would select out of/not move into diverse communities. While selection based solely on prior tendencies towards trust/interaction may be unlikely (*although see* Laurence, 2013), it is not implausible that minorities are more likely to move into communities already exhibiting lower cohesion.

Firstly, patterns of diversity in the UK partly reflect historical processes of White migration (Lupton and Power, 2004). Out-migration of middle-class Whites from inner cities created a surplus of reduced-price housing, while also disrupting cohesion in such areas (similar to US suburbanization (Frey, 1979)). Non-Whites were more likely to move into these cheaper, inner-city communities in which cohesion was likely already lower owing to 'common patterns of disinvestment and political resignation by the incumbent Whites, together with avoidance by prospective White in-movers' (de Souza Briggs, 2008: 222). Secondly, cohesive communities may historically have been able to exclude non-White in-movers (e.g. through co-ordinated discriminatory actions), whereas communities with lower cohesion could not. In sum, cross-sectional associations may be a

consequence of diversity having increased in areas *already* characterized by lower cohesion.

Another issue is that while diversity itself could be driving the cross-sectional association, this would not negate selection processes being in operation; there could be a 'cause and selection-effect' explanation. As discussed, it is implausible that individuals' trust/sociability alone drives selection choices into/out-of diversity, i.e. selection on the outcome variable is unlikely. However, individuals' residential choices can be affected by prior preferences for in-/out-group cohabitation, which drives relocation decisions (van Ham and Clark, 2009). Such biases are an important mediator of the association between diversity and cohesion (Stolle, Soroka and Johnston, 2008). Observed differences in cohesion across communities may therefore be driven by ethnic cohabitation preferences already present among residents (not processes of threat, anomie, etc.), which can trigger spatial sorting, e.g. higher cohesion observed in homogeneous communities may not be a consequence of lower threat but of individuals with prior biases for co-ethnic cohabitation selecting into homogeneous communities. Under such conditions, instead of diversity affecting attachment for all residents, 'treatment-effect heterogeneity' may exist, conditional on such prior biases (Brand and Thomas, 2013).

In sum, the literature explains the cross-sectional association between diversity and cohesion by suggesting that: firstly, living at higher levels of ethnic diversity causes lower cohesion among residents; and secondly, this results from processes of threat, anomie, etc. We have outlined alternative explanations that could account for the cross-sectional association. Given these, we aim to more rigorously test the *causal* claim that ethnic diversity harms cohesion.

Testing the Causal Effect of Diversity

Examining relationships temporally is a critical step towards establishing causality. If higher *levels* of community diversity cause lower individual *levels* of attachment, then a *change* in the *level* of diversity should elicit a *change* in the *level* of attachment (although we discuss limitations below). To examine the causal assumptions implied from the cross-sectional literature we apply fixed-effects panel data models to analyse changes *within* individuals over time. These are appropriate for testing such assumptions, as they remove bias owing to unobservable time-invariant characteristics.

The current cross-sectional analysis effectively treats homogeneous communities as earlier time-points and diverse communities as later-points, implying a temporal relationship: as diversity increases, cohesion among

residents decreases. How cohesion differs across residents of communities at one time-point is therefore taken to infer how *changes* in community diversity will affect residents. However, communities are dynamic environments. When examining this relationship across two time-points ($t-1$ and t), residents of a community can be divided into: *stayers* (who remain in the neighbourhood), *mobile-entry* (who move into the neighbourhood), and *mobile-exit* (who leave). Changes over time in cohesion across communities will therefore be the sum of processes occurring at the *individual-level* among *stayers* and *movers*.

To test the posited individual-level causal processes underlying the cross-sectional associations, we need to account for this dynamism. The community-level hypothesis (changes in community diversity leads to changes in residents' social cohesion) needs to be reframed to reflect the idea that a change in diversity between $t-1$ and t for an *individual* could be the product of a community increasing in diversity around *individuals* who did not move and individuals moving between communities with different levels of diversity. Given *movers* change communities (which may be influenced by community ethnic composition), we subdivide *movers* into individuals moving from 'homogeneous to diverse communities' and individuals moving from 'diverse to homogeneous communities'.

In focusing on individuals as the driver of change across communities, we posit that if ethnic diversity does have a causal effect on individuals' cohesion, owing to threat, anomie, etc., then, under fixed-effects panel models, a change in the level of diversity, among both *movers* and *stayers*, should elicit a change in an individual's attachment. However, although a robust test of causality, two problems exist with this approach.

Firstly, observing changes in cohesion alongside changes in diversity may not provide evidence that diversity itself is driving the change. In contextual-effects studies there are two potential sources of bias: individual- and community-level unobserved heterogeneity. Fixed-effects models account for individual (time-invariant and asynchronous) unobserved heterogeneity.² When analysing the impact of diversity on *stayers* alone, they also account for community-level unobserved heterogeneity (alongside selection bias). Insofar as any omitted community characteristics are stable over the shorter term (e.g. housing stock, community amenities), or changes are not synchronous with changing diversity, their impact among *stayers* is removed. However, given *movers* switch communities, our models cannot account for unobserved community-level heterogeneity. Changing attachment alongside changing diversity

among *movers* could therefore be driven by omitted variables.

Evidence for one explanation over another may be garnered by comparing diversity's effect among *movers* and *stayers*. If effects among *movers* are a consequence of omitted variables, we would expect changes in diversity to have no effect on *stayers* but to have an effect on *movers*, and for *movers* into diversity to report decreasing cohesion and *movers* into homogeneity increasing cohesion (as they will experience greater/lesser exposure to the omitted variable). However, detecting similar effects of diversity among *movers* and *stayers* would suggest changes in cohesion among *movers* are driven by diversity itself.

The second issue is that evidence of a causal effect of diversity may not validate the theoretical framework of threat, anomie, etc. (especially among *movers*). Instead, processes of self-selection may be in operation alongside causal effects, driven by prior-cohabitation preferences. Changing diversity may therefore elicit a change in attachment; however, changes could be concentrated among certain individuals, and result from prior-cohabitation preferences, not threat, anomie, etc. i.e. 'treatment-effect heterogeneity' (Brand and Thomas, 2013).

Evidence for which mechanisms underpin any causal effects may again be garnered by comparing *movers* and *stayers*. If prior preferences exist, and individuals relocate based on these, then we would expect (assuming diversity is a factor in relocating) the following: *movers to homogeneity* to report increasing attachment, as their community's composition becomes more aligned to their neighbour-ethnic preferences; and *movers to diversity* to experience no change, as the ethnic mix of their community is unlikely to matter.³ For *stayers*, increasing diversity should have no effect, as they would select-out if uncomfortable with diversity. However, as relocation decisions may take time, and opportunities to relocate may be restricted, increasing diversity could have a negative effect (albeit weaker than *movers* to homogeneity).⁴ Observing differences in diversity's effect among both *mover*-types and *stayers* can provide insights into the mechanisms driving any causal effects.

Summary

We aim to robustly test the claim that the negative cross-sectional association between community ethnic diversity and cohesion is causal, and examine whether the evidence validates the theoretical framework of threat, anomie, etc. Given the consistent association between community ethnic diversity and *within*-community cohesion indicators, we focus on this

within-community relationship, examining community affective attachment. As discussed, even under the application of fixed-effects panel models, inferring causality and attributing it to the theories outlined is problematic (especially for *movers*). However, in comparing diversity's effect among *movers* and *stayers*, greater insights into these questions may be found.

Data and Methods

Data

Individual-level data comes from the England and Wales sample of the 1991–2009 British Household Panel Survey (BHPS). *Wave (w) 1* was a nationally representative two-stage stratified sample of 10,264 adults (aged 16+ years), in 5,505 households (response rate 74%).⁵ Adults who left their original households were followed and re-interviewed on a yearly basis, allowing us to study both *movers* and *stayers*. Our aim is to explore how changes in community diversity affect attachment. Accurate data on contextual diversity (at small enough scales to be considered the community) is only collected every 10 years via the 1991, 2001, and 2011 censuses. We are therefore restricted to three waves of panel data: individuals that were present in $w1 = 1991$, $w11 = 2001$, and $w18 = 2009$.

The sample is subject to attrition due to losing contact and non-cooperation. Considering full interviews, of the 9,912 respondents in $w1$, 6,002 were re-interviewed in $w11$. Of those present in both $w1$ and $w11$, 4,412 were interviewed again in $w18$. This provides $n = 13,236$ person-year observations. To test for attrition biases, we experiment with BHPS longitudinal weights.

Community characteristics are taken from the 1991, 2001, and 2011 UK censuses. While $w1$ and $w11$ of the BHPS are synchronous with the censuses, $w18$ (the final BHPS wave) was conducted 2–3 years before the 2011 census.⁶ A number of small-area geographies are suitable for neighbourhood-level analysis in the UK, including the output area (OA; mean population: 300) and lower super-output area (LSOA; mean population: 1,500). OAs are census geographies designed to standardize population sizes, geographical shape, and dwelling-type/housing-tenure homogeneity. However, for our study, it is crucial that community boundaries remain stable across all three waves. Unfortunately, 1991 census data are not available at Output-area levels (these geographies were introduced from 2001). The closest approximation to the community for which we can create stable community-level boundaries across all

censuses is the administrative 1991 census ward (mean population: 5,300) (Supplementary Analysis SA.1).

Wards are larger than individuals' conceptions of their neighbourhood. Cognitive testing suggests individuals conceive of their communities as the collection of streets surrounding their own (HOCS, 2003). Accordingly, studies demonstrate that the smaller the spatial scale at which community characteristics are examined, the more robust the estimates (Dinesen and Sonderskov, 2015). Using cross-sectional BHPS data from 2001, we can compare the association between diversity and attachment using Ward- and LSOA-level geographies. While slightly stronger effects emerge for LSOAs, Ward diversity remains significant and similarly strong, suggesting Wards are viable neighbourhood proxies (although effects may be understated).

Key Dependent and Independent Variables

As the most consistent association in the literature is between community diversity and *within*-community cohesion, we focus on this *within*-community relationship. However, our intention to use three waves of the BHPS, while strengthening the robustness of findings, limits the measures available. The single item available across all three waves is 'Overall, do you like living in this neighbourhood? (yes/no/don't know)'.⁷ This has been used to measure place attachment (Sampson and Graif, 2009), social capital (De Donder *et al.*, 2012), or a psychological precursor of social capital (Perkins, Hughey and Speer, 2002), and social cohesion (Silk *et al.*, 2004). Others include it alongside indicators of social capital (e.g. neighbour-trust) in latent social capital measures (Letki, 2008). Our own analysis of the 1998 BHPS, which contains this measure alongside a battery of oft-used social cohesion indicators, suggests that it is related to a dimension of local attachment/belonging rather than forming part of a single latent construct of cohesion (Table 1).

We therefore use this as an indicator of place attachment, related to (and one of the key correlates of) local cohesion (Sampson and Graif, 2009). Using a single measure as a proxy for a broader latent concept may be problematic. From a measurement perspective, error is included that would have been removed from a factor model. Furthermore, while empirically associated with, and theoretically closely aligned to, concepts of attachment/belonging, difficulties remain in extrapolating from this measure to the broader concept (although testing increases our confidence in its efficacy: Supplementary Analysis SA.2).

To measure Ward-level ethnic diversity, we apply Simpson's Index of Diversity. Ranging from 0 to 1, this

Table 1. Factor analysis of standard local social cohesion measures using 1998–1999 BHPS

Survey question	Factor 1	Factor 2
Overall, do you like living in this neighbourhood?	0.131	0.596
If you could choose, would you stay here in your present home or would you prefer to move somewhere else?	−0.083	−0.626
I plan to remain a resident of this neighbourhood for a number of years	0.3	0.667
I feel like I belong to this neighbourhood	0.487	0.626
I like to think of myself as similar to the people who live in this neighbourhood	0.466	0.578
The friendships and associations I have with other people in my neighbourhood mean a lot to me	0.706	0.294
I regularly stop and talk with people in my neighbourhood	0.609	0.258
If I needed advice about something I could go to someone in my neighbourhood	0.685	0.231
I would be willing to work together with others on something to improve my neighbourhood	0.418	0.11
I borrow things and exchange favours with my neighbours	0.546	0.089
Eigen value	2.38	1.93

Notes: Promax (Oblique) Rotation (Kaiser On); only factors with Eigen Values above 1 reported.

measures the likelihood that two randomly selected individuals within a community will belong to different ethnic groups. To create a comparable measure of diversity across all censuses, we condense ethnic categories into a 10-group typology: White (White GB, Irish, Other); Indian; Pakistani; Bangladeshi; Other Asian; Black Caribbean; Black African; Other Black (Mixed White-Black African, Mixed White-Black Caribbean); Chinese; Other (Other, Other-Mixed, White-Asian Mixed).

$$\text{Ethnic Diversity}_j = 1 - \sum_k S_{kj}^2$$

where j is the neighbourhood area and k the ethnic group.

Covariates

We adjust for a range of community characteristics. An index of material disadvantage is generated, including % female lone parent households, % unemployed, % non-car-owning households, and % households social renting. Although indicators used in disadvantage indices differ between studies, this measure is created for consistency over the 1991–2008 period. A structural equation modelling (SEM) approach was taken using a longitudinal measurement invariance model (Widaman, Ferrer and Conger, 2010). Exploratory SEM included a larger set of indicators, which were found to have poor fit. Therefore the four-indicator, one-factor model was developed (Supplementary Analysis SB.1).

We also include a measure of % without degrees. This forms a distinct aspect of disadvantage, moderately correlated with material disadvantage ($r=0.42$), and akin to social-status disadvantage⁸ (Laurence, 2013). Measures of the % of the community aged 65+ years, % working in agriculture and number of persons per

hectare, capture age structure, urbanity/rurality and population density respectively.⁹ Minor differences in measurement exist across censuses (Supplementary Analysis SC.1). At the individual level, we include age (quadratic), tenure, children in the household, household income, employment status, qualifications, ethnicity, year, region, gender, and marital status.

Methodology

We apply a fixed-effects approach, which models *within*-individual change in the dependent and independent variables, allowing us to partial out time-invariant characteristics (Allison, 2009). As our dependent variable is binary, we estimate a conditional logit model. This model can be expressed as:

$$\log\left(\frac{P_{it}}{1 - P_{it}}\right) = \mu_t + \beta x_{it} + \gamma z_i + a_i$$

where, i represents individuals and t measurement occasions. P_{it} is the probability that an individual is attached to their neighbourhood. β and γ are vectors of coefficients for the time-varying and time-invariant variables x_{it} and z_i . a_i are individual intercepts, which remain stable over time, and μ_t is an intercept, which is allowed to vary with time. In a fixed-effects framework, non-varying parameters are excluded from the model because they cannot explain within-person variability¹⁰ (Supplementary Analysis SD.1).

The data are clustered at three levels: individuals, households, and communities. This can deflate standard errors due to the violation of the assumption of independent, identically distributed random sample, particularly in analyses using our full sample owing to the high degree of clustering. However, the clustering within our fixed-effects models is substantially smaller owing to the

exclusion of non-varying cases. Analyses of design-effects suggest the remaining clustering is below the point at which accounting for this is required (Muthen and Satorra, 1995). Furthermore, tests using clustered standard errors, including two-way clustering, both at the household- and community-level, revealed little difference in our models. Issues exist in performing such tests for *movers*, given they are nested in multiple higher-level units, e.g. more than one ward; however, sensitivity testing suggested the remaining clustering among *movers* does not bias our models (Supplementary Analysis SD.2 for full discussion). For consistency, and in line with similar fixed-effects studies, we report standard fixed-effects models (Lauen and Gaddis, 2013).

Our analytic strategy involves, firstly, estimating a pooled multi-level cross-sectional model (using all three waves) to examine the cross-sectional association. We then apply fixed-effect models to partial-out unobserved heterogeneity.¹¹ To address our key hypotheses in more detail, we then divide the data into subsamples that allow us to differentiate between two distinct sources of community change over time: changing diversity stemming from individuals *moving* between communities, and change occurring around individuals who *stay*.

One approach to partial out these different sources of change is to split the sample into subsamples of *individuals* who ‘never moved ward’ over the entire period (present in the same ward at each wave) and those who ‘moved ward at least once’ (who, between two waves, were found in different wards) (e.g. Sandy *et al.*, 2013). While allowing us to study the independent effect of changing diversity for individuals who stayed, this method cannot isolate independent effects of changes that arise from a move between communities, as 72% of individuals who ‘moved ward at least once’ also experienced ‘staying’ between other waves. Therefore, diversity effects among *movers* would conflate the two types of change.

An alternative approach is to subdivide the sample by observations *within* individuals (Longhi, 2013). If an individual is in the same community in two or more consecutive waves these *observations* are classified as a ‘staying’ period. If an individual is in a different community in two or more consecutive waves, these *observations* are classified as a ‘moving’ period. Individuals can therefore appear as both, *movers* in one model but *stayers* in another.¹² While the minimum *n* of observations for each individual is 2, in the ‘staying’ sample, 65% of individuals have three waves of data. Given that most people who move wards only do so once between waves, 38% of individuals have three waves in the ‘moving’ sample.¹³ This method generates more accurate

estimates for changes in diversity stemming from moving and staying.¹⁴

Results

We first test the cross-sectional association between ethnic diversity and attachment. Model 1 (Table 2) shows a multi-level random intercept logistic regression (observations nested in individuals nested in households nested in wards), using three waves of pooled data, and full individual-/community-level covariates. In line with the literature, diversity exhibits a significant, negative association with attachment. Community disadvantage and rural indicators also have significant associations. Previous studies conclude from this that living in diverse environments *causes* a decline in attachment. However, these results may be biased by unobserved heterogeneity. We therefore turn to fixed-effects panel models.

As fixed-effects logistic regressions only use within-individual variation, cases where attachment does not change between waves dropout of the conditional likelihood function. Attachment is relatively stable across all three waves, with only 14% of individuals reporting a change. Thus, the *n* of observations is lower in our fixed-effects models, potentially resulting in type II errors.

Model 2 shows the fixed-effects analysis on the full sample. Community diversity’s coefficient is significant and negative, indicating that a *change* in diversity is associated with a *change* in attachment. Changing community material/status disadvantage also significantly predict changes in attachment, although the rural indicator is no longer significant. It is also notable that relative to the other indicators, diversity has become stronger.¹⁵ These findings therefore also suggest caution in assuming a causal effect of rurality (with potential unobserved heterogeneity accounting for the cross-sectional associations). However, the reduced sample size in the fixed-effects model, while strengthening our confidence in the effect of diversity, implies the possibility of type II errors for some predictors.

These results provide some of the first evidence that diversity’s cross-sectional association is causal.¹⁶ However, as outlined above, a *change* in community characteristics may result from changes occurring around individuals who remain in the same community or individuals moving between communities. We therefore subdivide our observations into periods of ‘staying’ and ‘moving’. Important differences exist for our key variables between samples. Among *stayers*, only 9% changed their level of attachment, while diversity-change ranges from -0.02 to $+0.37$. Thus, change in

Table 2. Pooled cross-sectional and fixed-effects modelling of full sample

Modelling method	Model 1 HLM pooled cross-sectional	Model 2 Fixed-effects
Sample type	All individuals	All individuals
% aged 65 + years	2.447 (1.758)	2.675 (1.825)
% in agricultural work	6.566* (3.283)	5.130 (3.810)
Material disadvantage	-0.349** (0.131)	-0.244+ (0.132)
% without degrees	-5.234*** (1.272)	-3.963** (1.280)
Density	-0.003 (0.004)	-0.001 (0.004)
Ethnic diversity	-2.338** (0.719)	-3.923*** (0.942)
Constant	8.894*** (1.449)	
Random-effects parameters		
Ward intercept	0.238 (0.102)	
Household intercept	0.655 (0.086)	
Person intercept	-0.139 (0.88)	
N (observations)	12,371	1,777

Notes: + $P < 0.10$; * $P < 0.05$; ** $P < 0.01$; *** $P < 0.001$; controlling for full individual-level covariates (although not shown).

diversity is essentially the effect of *increasing* diversity. Among *movers*, however, 16% changed their level of attachment, while diversity-change ranges from -0.66 to +0.67.

Model 1 (Table 3) replicates the fixed-effects analysis of Model 2 (Table 2) but among *stayers*. We observe that as a community becomes more diverse around an individual, they are likely to become *less* attached to their community. This is a strict test of the causal impact of diversity, minimizing unobserved heterogeneity and eliminating selection bias. Importantly, neither indicator of disadvantage is significantly associated with attachment.¹⁷

Model 2 (Table 3) shows the same analysis among *movers*. Diversity is again significant and negative, suggesting that individuals who move from more diverse to less diverse communities are likely to become more attached (and vice versa).¹⁸ In comparison with *stayers*, moving into communities with higher/lower disadvantage does have a significant negative/positive effect. The coefficients of all non-diversity community

characteristics also increase. Non-diversity effects therefore appear concentrated among *movers*. While the smaller n of the subsamples in Models 1 and 2 may increase type II errors, the continued significance of diversity, and ‘% without degrees’, indicates greater robustness.

The effect of a change in diversity for all individuals (Model 2, Table 2) therefore appears to be a product of negative effects among both *movers* and *stayers*. However, as suggested, an individual’s move into a more/less diverse community may reflect prior preferences for in-/out-group cohabitation. If present, we would expect different outcomes for those moving into more diverse communities (predicted to have no diversity bias) compared with those moving into less diverse communities (predicted to have negative bias). Models 3 and 4 (Table 3) subdivide *movers* into ‘moved into a less-’ or ‘more-diverse community’.

For *movers* into homogeneity (Model 3), changes in diversity represent a change to less diversity only. We see a strong significant, negative effect of diversity, i.e. the more homogeneous their destination (relative to their origin) community the more likely attachment will increase. This coefficient can be alternatively read by reversing the sign and observing that individuals become more attached the greater the increase in community homogeneity. However, a move into a more diverse community appears to have no effect, with the coefficient substantially reduced and non-significant¹⁹ (Model 4). There is thus substantial heterogeneity underlying the negative effect of diversity among *movers*. Other differences also exist between these samples: status disadvantage impacts those moving to more homogeneous communities, while economic disadvantage and percent aged 65 + years impacts *movers* to diversity.

These analyses perform a strict test of the causal claim that diversity undermines community attachment. Alternative modelling specifications and applying a range of sensitivity tests to the samples/results returned consistent findings (Supplementary Analysis SE.1).²⁰

Discussion

This article subjected the claim that community diversity undermines cohesion to stringent causal examination. The most robust test is conducted among *stayers*. For those who remain in the same area for two or more consecutive waves, increasing community diversity is related to a decline in attachment. Fixed-effects methods reduce bias from unobserved heterogeneity, while focusing on *stayers* undermines arguments that findings are (solely) owing to selection. Given assumptions that unobserved

Table 3. Fixed-effects modelling of subsamples: *stayers*, *movers*, and *movers* by diversity of destination

Modelling method	Model 1	Model 2	Model 3	Model 4
Sample type	Fixed-effects Stayers	Fixed-effects Movers	Fixed-effects Movers to homogeneity	Fixed-effects Movers to diversity
% aged 65 + years	-7.663 (4.461)	4.397 ⁺ (2.485)	-3.917 (5.211)	8.849** (3.391)
% in agricultural work	1.288 (6.496)	2.807 (5.698)	13.207 (13.082)	4.852 (7.158)
Material disadvantage	0.138 (0.433)	-0.376 ⁺ (0.191)	0.443 (0.430)	-0.788** (0.261)
% without degrees	-2.265 (2.489)	-4.715* (1.927)	-13.612** (4.918)	-0.177 (2.790)
Density	-0.008 (0.008)	0.004 (0.007)	-0.009 (0.018)	0.007 (0.009)
Ethnic diversity	-3.854* (1.762)	-3.681** (1.341)	-12.323** (3.919)	-0.491 (2.221)
N (observations)	901	749	295	443

Notes: ⁺ $P < 0.10$; * $P < 0.05$; ** $P < 0.01$; *** $P < 0.001$; controlling for full individual-level covariates (although not shown).

heterogeneity at both the individual- and community-level is time-invariant, this provides strong evidence that diversity negatively impacts individuals' community attitudes.

Among *movers*, there is heterogeneity in diversity's effect based on moves into/out of diverse environments. For individuals relocating to less diverse communities, the more homogeneous the destination the more likely their attachment will increase. This could indicate that cohabiting with out-groups causes attachment to decline (via processes of threat, etc.). However, because we cannot partial-out time-invariant unobserved *community*-level heterogeneity, diversity may be correlated with omitted characteristics, which undermine attachment; therefore, moving away from diversity increases attachment. Alternatively, individuals may possess prior in-group cohabitation preferences that manifest themselves as higher attachment when moving to homogeneity. For individuals relocating to more diverse communities, increasing diversity is not associated with changes in attachment, indicating either that diversity has no effect or the absence of prior preferences against out-group cohabitation.

Without further data, we cannot claim greater support for one explanation. However, inferences can be made when the results are taken together. If exposure to diversity undermined attachment for all individuals owing to processes of threat, anomie, etc., then we would expect *movers* into diverse communities to also be affected. If it were an omitted characteristic of diverse communities, we would expect *stayers* to be unaffected and *movers* into diversity to be affected. Instead, the

findings support the idea of prior biases influencing how an individual's attachment reacts to diverse environments: while increasing diversity affects *stayers*, the effect is weaker than it is for *movers* into homogeneous communities, while *movers* into diversity experience no effect.²¹

We infer whether individuals possess a negative (or no) bias towards diversity based on whether they moved out-of (or into) diverse communities. We suggest that subsequent changes in attachment (or lack thereof) is evidence of the existence (or absence) of prior biases. However, alternative explanations exist. Kaufmann and Harris (2015) show that individuals moving into diversity tend to be younger, single, renters without children, while those moving out of diverse areas tend to be homeowners, married, older and with children (Supplementary Analysis SF.1). It is plausible that heterogeneity in diversity's effect among *movers* is driven by certain socio-demographic groups being more/less sensitive to changing diversity,²² e.g. older individuals may be more sensitive, and therefore, only *movers* into homogeneity are affected.

Relocation decisions and neighbourhood choice also occur for other reasons. We posited that omitted variables correlated with (but not caused by) diversity (e.g. services/amenities quality) may drive the change in attachment among *movers*. Yet, if this were the case, *movers* into diverse communities should experience a corollary decline in attachment. However, the effects of omitted variables may be dependent on the socio-demographic characteristics of the different *mover* groups. For example, young, single, childless people may not prioritize the quality of

services (e.g. schools), and thus, moving into more diverse areas has no impact. Older people with children may place a premium on service quality. Thus, a move to homogeneity would increase attachment. This may apply to a range of characteristics (e.g. urban environments), affecting different socio-demographic groups differently. Thus, omitted characteristics could still account for findings among *movers*; however, only certain types of individuals are affected by these.

Moving/staying decisions are also affected by constraints as much as choice. Weaker effects observed among *stayers* may stem from a combination of those choosing to stay (among whom diversity likely has a weaker effect) and those constrained to stay, e.g. owing to limited resources (for whom diversity likely has a stronger effect). Constraints could also play a role among *movers*, as neighbourhood choice may be constrained by available resources. However, more diverse communities are, in theory, less desirable, given diversity is largely found in disadvantaged, dense, high turnover areas with poorer accommodation; thus, they would be likely destinations for those *movers* with more constraints. Yet, it is *movers* to diversity who experience no effect of increasing diversity. As such, constraints likely play less of a role for our *mover* findings. Heterogeneity in diversity's effect may also depend on the duration a resident has lived in their community. For example, diversity may have a greater impact on *stayers* who have lived in their communities longer, as they may be less able or willing to move. Testing suggests longer-term residents are somewhat more sensitive to community change.²³

Despite the possible explanations, these results remain compelling, as they suggest that underlying the cross-sectional association between diversity and attachment are complex causal processes occurring at the individual level, among *stayers* and *movers*. At the same time, contrary to the literature, changing diversity can have no effect on certain individuals. This questions the generalizability of the theoretical framework (of threat, etc.) used to account for the cross-sectional findings. Therefore, despite finding strong evidence of a causal effect, the results (especially among *movers*) suggest prior preferences likely play a role.

This article also reveals insights into the role of other community characteristics. The pooled cross-sectional analysis demonstrates that disadvantage and rural-living are significantly associated with attachment. In fixed-effects models, these characteristics are only significant for *movers*. Furthermore, coefficient size declines for *stayers* and rises among *movers*. The causal effects inferred from the cross-sectional associations may be primarily driven by *movers*.

Statistical issues may account for these effects being concentrated among *movers*, e.g. for *stayers*, there may be little change in disadvantage within communities over time (Supplementary Analysis SG.1). However, there may be substantive reasons. The mechanisms connecting disadvantage to cohesion may take longer to emerge. For example, the 'petty crime . . . physical decay, and social disorder', which erode cohesion could lag behind contemporaneous changes in their disadvantage antecedents (Oliver and Mendelberg, 2000: 576). The disadvantaged communities that individuals move into/out-of may already exhibit these processes. Hence, effects are concentrated among *movers*. Alternatively, changes in disadvantage for *stayers* may not result in immediate changes in perceptions of deprivation. Studies show the latter is more relevant for attachment (Feijten and van Ham, 2009). Changes in diversity, however, are likely more discernible to residents. If diversity's effects are related to exposure to out-groups, any effect would be relatively synchronous with changes in diversity.

This article has certain limitations. Our measure of attachment is a weaker proxy for the networks element of cohesion. Furthermore, Putnam's (2007) 'hunkering down' thesis should not be inferred from these locally specific findings. There is also a relatively small amount of change in attachment over time. This is partly because our measure is binary (we observe far more temporal change on an index of cohesion), as well as the length of time between waves.

Another issue is that the findings may be conditional on the time span between waves. For example, among *stayers*, individuals might be negatively affected by diversity; however, their attachment may have recovered by the time they are surveyed again 8–10 years later, i.e. accommodation processes may be occurring. Our analysis may focus on those particularly susceptible to diversity, whose attachment remains depressed. With shorter periods between waves, diversity effects could be weaker. Alternatively, over the 8–10 year period, those individuals most adversely affected by diversity, whose attachment declines as community diversity increases, may have moved before they are re-surveyed. This would understate any diversity effect for *stayers* (although increase it for *movers*). Importantly, who is a *stayer* or *mover* may be affected by changes in community diversity; yet, how they are classified will be, in part, a product of the 8–10 year wave span.

Similarly, this time span makes isolating the effect of diversity among *movers* more difficult, as diversity data for the two waves across which a move occurred are absent (see Supplementary Analysis SE.1). However, the current design may be advantageous, given large

changes in diversity occur over longer periods (at least for *stayers*), potentially increasing the likelihood of identifying effects present.

Another limitation is that, while the fixed-effects approach allows us to demonstrate robust evidence of a causal effect of diversity, it limits our ability to validate a tenet of the theoretical framework: that *levels* of diversity affect *levels* of cohesion. Our models examine whether *changes* in diversity elicit *changes* in cohesion. Cross-sectional studies show both the level of diversity and the amount of recent change in diversity can independently impact cohesion (although both effects are explained using the same theoretical framework, e.g. threat (Feijten and van Ham, 2009)). Our models do not disentangle changes in levels versus change itself. Demonstrating that *changes* in levels of diversity elicit *changes* in cohesion produces the strongest evidence yet that, as predicted, levels of diversity do affect cohesion. However, all or part of this effect may be a result of *change* itself, rather than any new *level* of diversity.

Conclusion

This study makes important contributions to the diversity and social cohesion literature. It performs the first panel data analysis of the effect of community diversity on intra-community attitudes, producing evidence that the negative cross-sectional association between diversity and indicators of cohesion (especially those related to attachment) is likely causal. Furthermore, distinguishing between diversity change from *movers* and *stayers* demonstrates that multiple causal processes occur at the individual level (including possible selection processes), providing insights into how the aggregate community-level differences in cohesion observed in the cross-sectional literature may emerge. Yet, changes in community diversity do not impact all individuals equally, casting doubt on the generalizability of the theoretical framework (of threat, etc.) often applied. While evidence for a causal effect of diversity does suggest such processes may be in operation, potential in-/out-group preferences likely play a central role, influencing if and how far diversity impacts cohesion.

Notes

- 1 Given our community focus, the cross-national literature is omitted.
- 2 By analysing *change*, this method also addresses selection on the outcome, i.e. whether cross-sectional associations emerge solely from diversity increasing in communities already exhibiting lower cohesion.

- 3 Although preferences *for* diversity could increase attachment.
- 4 Relocation decisions may reflect other preferences (e.g. home ownership), correlated with, but not driven by, diversity (discussed later).
- 5 Stage one: random selection of Postcode sectors (PCS) using a systematic sampling method. Stage two: random sample of addresses within each PCS.
- 6 Part of the BHPS sample was integrated into the 2011 Understanding Society survey, which replaced the BHPS. Of the participants present in both *w1* and *w11* of the BHPS, the dropout rate between *w11* and the 2011 wave of Understanding Society was 50%, compared to 23% between *w11* and *w18* of the BHPS. This loss outweighs the benefits of data closer to the 2011 census (although using Understanding Society produces similar results).
- 7 Five percent of individuals reported ‘don’t know’. Re-coding them into ‘yes’, ‘no’, or excluding them did not change our findings. Results with ‘don’t know’ coded as ‘missing’ reported.
- 8 ‘% without degrees’ does not load substantially on to our index of economic disadvantage. It correlates highly with % Managerial/Professional occupations ($r=0.9$): both can be used interchangeably.
- 9 A measure of residential turnover only exists for 1991 and 2001. However, in first-difference models using 1991 and 2001 BHPS data, the addition of ‘change in residential turnover’ does not alter the conclusions presented here.
- 10 A conditional maximum likelihood model is estimated owing to the incidental parameters problem.
- 11 Random-effects models were examined. Hausman tests suggest the unique errors are correlated with the regressors, indicating it is biased. We also apply one-way fixed effects models with no time-fixed effects (although including them does not change the findings).
- 12 Given the 8–10 year gap between waves, issues exist with such a sample division, e.g. individuals present in the same ward in two or more consecutive waves may have moved ward *in-between* waves (Supplementary Analysis SE.1).
- 13 Given the lower number of three-wave observations for *movers*, we follow Longhi (2013) in also estimating the effects of changing diversity among movers using first-difference models between the two waves across which an individual moved (*see below*). We also experimented with restricting

ourselves to ‘individuals who never moved’ and ‘individuals who *only* moved’, i.e. individuals who possess three waves of data. We observed consistent findings.

- 14 For robustness, we tested model specifications, which divided our sample by *individuals* who ‘never moved’ and ‘moved at least once’. Substantively similar results are returned (*see below*).
- 15 Direct comparisons between logit models are difficult because model adjustments change the scale against which the estimates are measured. We examined within-model relative differences to help understand how different specifications affect the results.
- 16 Given our diversity measure is ‘colour-blind’, we may expect different results for Whites and non-Whites. Replicating analyses among Whites reveals stronger findings. This is likely driven by the high correlation ($r \geq .95$) between ethnic diversity and percent non-white. However, our non-White sample is too small to run a separate robust analysis.
- 17 This holds when including the disadvantage-index variables individually.
- 18 By examining movers together, we already account for any increase/decrease individuals receive from moving itself.
- 19 Given the coefficient size decreases substantially and the larger n of *movers* into diversity (compared with homogeneity), it is less likely this non-significance is a consequence of model n .
- 20 We re-ran analyses but subdivided our sample by individuals (not observations), i.e. individuals who ‘never moved’ and who ‘moved at least once’ (including ‘moved into a more homogeneous community at least once’ and ‘moved into a more diverse community at least once’) ([Appendix 1](#); Models 1–4). We also ran our analysis on movers using first-difference models ([Appendix 1](#); Models 5–7). These revealed consistent findings.
- 21 *Stayers* may also exhibit certain biases, given they do not move.
- 22 Socio-demographic differences between *movers* could account for why other community characteristics affect *movers* into/out of diversity differently.
- 23 Restricting our *stayers* sample to those who ‘lived in the community for ‘<5 years’ or ‘>5 years’ before the change in diversity occurs reveals stronger effects for the latter (*coef.*: -5.384^{**}) than the former ($-2.462+$); although sample size remains an issue.

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Supplementary Data

Supplementary data are available at *ESR* online.

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Appendix

Appendix 1. Robustness tests of alternative model specifications

<i>Modelling method</i> <i>Sample</i>	Model 1 Fixed-effects Individuals— never moved	Model 2 Fixed-effects Individuals— moved once or more	Model 3 Fixed-effects Individuals— moved to homogeneity once or more	Model 4 Fixed-effects Individuals— moved to diversity once or more	Model 5 First-difference Moved between two waves	Model 6 First-difference Moved to homogeneity between two waves	Model 7 First-difference Moved to diversity between two waves
% aged 65 + years	−8.435 (5.056)	4.402* (2.150)	−0.899 (5.800)	9.092** (3.295)	2.960** (0.977)	2.199 (1.610)	4.068** (1.290)
% in agricultural work	2.060 (7.090)	4.172 (4.917)	23.111 (16.775)	5.757 (6.422)	2.193 (1.724)	3.035 (3.099)	2.592 (2.203)
Material disadvantage	0.553 (0.499)	−0.284 ⁺ (0.150)	0.067 (0.488)	−0.575* (0.237)	−0.152* (0.068)	−0.070 (0.106)	−0.244** (0.092)
% without degrees	−2.076 (2.775)	−4.393** (1.574)	−7.608 ⁺ (3.763)	−1.679 (2.491)	−0.163 (0.758)	−1.216 (1.093)	1.356 (1.094)
Density	−0.000 (0.009)	0.001 (0.005)	−0.007 (0.017)	0.007 (0.008)	0.002 (0.002)	0.001 (0.003)	0.001 (0.003)
Ethnic diversity	−4.644* (1.994)	−3.732** (1.155)	−10.837*** (3.250)	−1.146 (2.102)	−1.615*** (0.470)	−2.761*** (0.669)	0.294 (0.884)
N (observations)	893	1,284	543	746			
N (individuals)					2,537	989	1,748

Notes: ⁺ $P < 0.10$; * $P < 0.05$; ** $P < 0.01$; *** $P < 0.001$; controlling for full individual-level covariates (although not shown); individuals in the 'move to diversity once or more' category never moved to a more homogeneous community; individuals in the 'move to homogeneity once or more' category never moved to a more diverse community; the 5% of individuals who made a move into both a more diverse and more homogeneous community over the three waves are excluded from the results.