The long shadow of local decline: Birthplace economic adversity and long-term individual outcomes in the UK

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ARTICLE INFO

ABSTRACT

Does growing up in a high-economic adversity area matter for individual economic, cultural, and political views? Despite a significant focus upon the effect of birthplace on economic outcomes, there is less evidence on how local economic conditions at birth shape individual attitudes over the long-term. This paper links the British Household Panel Survey (BHPS) from English and Welsh respondents with historic localised information on unemployment, our measure of economic adversity. Our results, which control for composition effects, family background, and sorting of people across places, show that being born into a high-unemployment Local Authority has a significant, long-term impact on individuals. Birthplace matters beyond economic outcomes, as being born into a Local Authority of high unemployment makes individuals believe in more government intervention in jobs, less progressive on gender issues, and less likely to support the Conservative Party.

1. Introduction

There is widespread concern about high levels of spatial inequality in income and employment across the advanced world (Evenhuis et al., 2021; McCann, 2017). Many rich countries have experienced divergence in employment patterns and income differentials, with economic growth and jobs increasingly concentrated in a few 'successful areas' where workers can benefit from agglomeration economies and better opportunities. Yet, despite the prevalent focus upon how 'place' correlates with income differentials (Baum-Snow & Pavan, 2012; D'Costa & Overman, 2014; Glaeser, 2012; Iammarino et al., 2019), individual attitudes (Abreu & Jones, 2021; Kenny & Luca, 2021), and voting preferences (Colantone & Stanig, 2018, 2019; Dijkstra et al., 2020; Lee et al., 2018), less is known on the extent to which growing up in an area of high economic adversity impacts individuals' views and attitudes over the long term.

Drawing on the literatures on neighbourhood effects and on political socialisation, which show that early-life experiences can significantly shape political preferences for an individual's whole life (Grasso et al., 2019; Holbein, 2017; Neundorf & Soroka, 2018; O'Grady, 2019), we contribute to a developing body of work that shows how birthplace affects individual outcomes in adulthood. This has been described by the

Social Mobility Commission in the UK as the 'long shadow of deprivation' (Carneiro et al., 2020). For example, in the US, using rich administrative data, Chetty et al. (2014) show pronounced differences in children's later-in-life social mobility across US Commuting Zones, in part determined by local factors such as ethnic segregation. Similarly, in the UK, Bosquet and Overman (2019) show how those born in large cities experience higher earnings than those born in other locations, such as smaller cities, towns, or the countryside. We go beyond the existing evidence, by showing that being born in an area of economic adversity – which we empirically measure as Local Authorities with high unemployment rates – has a long-term effect not only on earnings, but also on individual economic, cultural, and political outlooks.

We focus on the UK, which has one of the highest levels of regional inequality of any developed country (McCann, 2017). Our empirics combine two datasets: the British Household Panel Survey (BHPS, University of Essex Institute for Social and Economic Research, 2022) and the Vision of Britain (VoB).[1] The high-quality individual-level panel

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[1] This work is based on data provided through www.VisionofBritain.org.uk and uses historical material which has been re-districted by the Linking Censuses through Time system, created as part of ESRC Award H507255151 by Danny Dorling, David Martin and Richard Mitchell.

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data from the BHPS allows us to track a large sample of British individuals from 1991 to 2008. This strategy allows us to control for individual sorting (Combes et al., 2008), and to disentangle 'compositional' from 'contextual' effects (Maxwell, 2019). Importantly, we have information on birthplace, which we combine with the VoB's historical census-based unemployment data using time-consistent boundaries for 348 Local Authorities across England and Wales. We complement this analysis by instrumenting individuals' Local Authority birthplace unemployment rate with a set of shift-share instruments. Here, we exploit various specifications to argue that our shift-share provides a plausibly exogenous shock to birthplace economic conditions. This is again made possible through the VoB's historical Local Authority industry composition data.

Our results highlight how the effects of place are engrained in the formative years of childhood, proxied by place of birth. A high level of economic adversity in one's birthplace decreases adulthood earnings and makes one more 'economically left-wing' – that is, having a stronger belief in an obligation for the government to provide jobs – and to a lesser extent, less progressive on post-materialist cultural issues related to family life and individual freedoms. Potentially because of these mechanisms, survey respondents who grew up in economically adverse areas are less likely to support Britain's centre-right Conservative Party. In short, high levels of local economic adversity at birth has a long-term impact over the life course.

Our research makes three main contributions. First, we contribute to the literature on neighbourhood effects. We contribute to the literature by providing new findings from the United Kingdom, and by going beyond economic outcomes to consider socio-cultural attitudes and voting behaviour. Furthermore, most of the recent literature that has developed from the work of Chetty & Hendren (2018) tends to exploit a 'moving across neighbourhoods' research design. While this type of research design allows disentangling the causal effect of place, exclusively focusing on 'movers' may lead to selection bias if people moving across areas are different from 'non-movers'. In our empirical design we consider both groups. Second, we add to the debate on the implications of rising spatial inequality. The wave of political disenchantment experienced by many rich countries since the mid-2010s has been explained as a 'geography of discontent' (McCann, 2020) or as a 'revenge of places that don't matter' any longer (Rodríguez-Pose, 2018). Our analysis contributes to this debate and shows that the effects of living in 'places that don't matter' go beyond one's current residence, extending to birthplace. As far as we are aware, this is the first study to show that local unemployment at time of birth influences long-term economic, cultural, and political attitudes. Third, we contribute to the literature in political science and sociology on political socialisation by adding birthplace-based effects to a field which emphasises the importance of formative years, for example through cohort effects (Grasso et al., 2019) and the welfare regime one grew up in (Neundorf & Soroka, 2018).

The paper is structured as follows. The next section briefly presents the bodies of literatures to which this paper speaks, and then develops our hypotheses. In section 3, we discuss the case selection for this work, the UK. Section 4 outlines the data used, followed by the methodological approach in section 5. We then present our main findings in section 6, supported with a section of robustness checks. Finally, our concluding discussion outlines the implications of these findings and presents avenues for further research.

2. Birthplace economic adversity and life outcomes: hypotheses

The extent to which the local economic context impacts individual outcomes and political attitudes has become a rich area of research. Economic studies have long shown that local conditions influence individual economic outcomes, even when accounting for unobservable individual characteristics. The literature on city size argues that large cities can influence outcomes.[2] For example, Bosquet and

Overman (2019) show that birthplace population size is associated with higher adulthood earnings. But other local factors also seem to matter. Most famously, Chetty et al. (2014) and Chetty and Hendren (2018), amongst others, show that local economic conditions at birth can have a major influence on life outcomes, even when accounting for parental background.

Political scientists have also argued that local context matters. Social interactions overwhelmingly occur at a local scale. So, if local economies decline, individuals may observe friends or families lose their jobs or homes. Studies have tested the hypothesis that individuals express resentment because of poor or declining local, rather than national, conditions (Reeves & Gimpel, 2012). Local deprivation has been linked to status anxiety and area-contextual grievances (de Botton, 2005). 'Bad' socio-economic conditions, such as high unemployment rates, can lead to the feeling of 'one being next' inline to 'lose out' (Salomo, 2019). This anxiety is often expressed through ethnocentric attitudes and vented via political discontent (Sobolewska & Ford, 2020).The effects of place may come from childhood when lifelong attitudes and preferences are formed.[3] Drawing on the economic and political science literature, we can identify several mechanisms through which birthplace economic adversity, which we proxy by local unemployment, may affect adulthood outcomes and attitudes.

First, high birthplace unemployment may impact adulthood outcomes through intergenerational transmission of characteristics. Sorting based on cost means parents with lower socio-economic status are likely to move to high unemployment areas. There is a correlation between parental and child income, and parental social origins and child attitudes (Blanden et al., 2004; Dinas, 2014; Jaime-Castillo & Marqués-Perales, 2019). Parents in high unemployment areas are more likely to be unemployed themselves, although we can control for parental background.[4]

Second, the impact of seeing those nearby struggling financially may lead to a sense of affinity or empathy (Liu et al., 2020; Lupu & Pontusson, 2011). These attitudes may be held through adulthood, making individuals more likely to favour redistributive economic policies. Similarly, individuals in deprived areas are less likely to develop post-materialistic cultural attitudes and values (Inglehart, 1971; Norris & Inglehart, 2019). They are rationally more concerned about economic precarity, and the idea of 'being next' (Salomo, 2019). We also expect economic precarity to be channelled through attitudes towards gender roles. In areas where jobs are scarce, economic 'threat' may lead to traditional male 'breadwinner' attitudes, beliefs which may persist into adulthood and potentially reduce women's adult earnings.

Third, areas with high levels of unemployment are also likely to have worse public services and 'social infrastructure'. Formal education may be worse, and there may be fewer role models.

Fourth, local unemployment may change perceptions of the importance of luck compared to effort (Piketty, 1995). High unemployment may make people think that luck, not effort, matters and so lead them to reduce effort. Similarly, these individuals would rationally demand more redistribution as they demand insurance against bad luck.

Finally, a large proportion of individuals are immobile. 32% of individuals within our sample always lived in the same district as they were born (authors' calculations). It may be that those born in districts with high unemployment face limited prospects as adults. Moreover, physically immobile individuals tend to have different views than their mobile counterparts, including a higher tendency to vote for Brexit (Lee et al., 2018).

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[2] For a review see Puga (2010) and Rosenthal & Strange (2004).

[3] Due to data limitations, we examine birthplace rather than more general childhood residence. However, in Europe, where mobility is low, birthplace and childhood residence will often be the same.

[4] See Appendix Table 31 for an assessment of how not including parental characteristics increases the magnitude of our findings.

From this framework we formulate our hypotheses. Given these expectations, we would expect individuals born in high unemployment districts to:

H1. earn less;

H2. be more 'economically left-wing', that is, with a stronger belief that the government should provide jobs;

H3. be less progressive on post-materialist cultural issues related to family life and individual freedoms;

H4. be less likely to vote for the Conservative Party than individuals born in low unemployment districts.

3. Data

To test these hypotheses, we combine individual level data from the British Household Panel Survey (BHPS) with aggregate Local Authority level information from the Vision of Britain (VoB) dataset.[1] The BHPS is an annual panel survey of British households starting in 1991 and running for a total of 18 waves. The BHPS is a nationally representative sample survey with all adult household members (aged 16 +) being interviewed annually. As some panel members leave the sample (either through death, emigration, or other forms of attrition) new panel members were incorporated through the survey period. Our models include both these individuals who were involved at the BHPS's origination and new entrants. Whilst we track individuals through the waves of the BHPS, there is some limited attrition and respondents may not answer every wave, or every question within a wave. Our sample includes all those individuals born and residing in England or Wales. We discard Scotland and Northern Ireland because we are unable to link individuals to Local Authority-level information with sufficient accuracy.

We identify four key dependent variables, namely future earnings, economic attitudes, cultural attitudes, and voting preferences, and operationalise them as follows. First, we observe individuals' gross pay in every wave (individual annual pre-tax income in 1000s of British pounds sterling, deflated to 2005 levels).[5]

Our main dependent variable for 'economic values' is captured through respondents' views on the survey statement "Government has an obligation to provide jobs", which is answered on a 5-point Likert scale from 1 "Strongly Agree" to 5 "Strongly Disagree". We choose this as our measure of economic preferences for two reasons: first, it is closely aligned to our key independent variable, birthplace unemployment conditions. Second, and most importantly, attitudes towards welfare state support are conventionally treated as integral to Left/Right ideological divides. Furthermore, the other potential economic questions are somewhat dated and/or relate to social rather than economic values.[6]

We then measure post-materialist cultural attitudes through respondents' opposition to homosexuality and support for traditional gender roles (in a similar fashion to Langsæther et al., 2021). Both variables are again measured on a 5-point Likert scale. The questionnaire wording is respectively "Homosexual relationships are wrong", and "Husband should earn, wife stay at home". The homosexuality question is available in six rounds, and the gender question in nine rounds. To ease comparison, we re-order the scale of all value variables (i.e., some scales are flipped) so that 1 is the most 'left-wing' / 'tolerant' outcome.

Finally, we measure voting preferences as the party one supports. This is available in every round apart from wave 2. This is then coded as a binary variable equal to 1 for 'Conservative', the major centre-right party, versus any other party.[7] We use a binary outcome for clarity of analysis, and choose the Conservatives as baseline category as they were the dominant party in 1991 when the BHPS started.

As it will be explained in detail in the methodology section below, in the analysis we also include an array of individual-level controls available from the BHPS. Specifically, we consider age, age squared, BHPS wave, occupation (NSSEC-8 categories), educational attainment (ISCED 6 categories), parental background measured by father's occupation,[8] gender, year of birth, and ethnicity.[9] The full sample within the BHPS consists of 32,380 individuals observed on average in 7.4 waves. However, after including only those for which we have information on birthplace, current residence, our full list of time-varying and time-variant controls, and which participate in at least two waves we are left with a smaller sample.[10] Our dependent variables are available in different rounds, and hence our sample size varies for each respective dependent variable. As an example, in the case of earnings, our main birthplace findings are based on 7,074 individuals observed an average of 9 times. For reasons of space, full descriptive statistics, including a snapshot from Wave 17 of the dependent variables by birthplace, are presented in Appendix Tables 23 and 24.[11]

4. Measuring local unemployment over time

Our key independent variable is birthplace unemployment. We use data from the VoB project, which reconstructed historical census data to be consistent with modern district boundaries. We use data for England and Wales, and, in total, we have information for individuals in 348 Local Authority districts, which are the most meaningful tier of local government across the two nations. We consider each individual Local Authority as a separate spatial unit.[12]

In spatial economics literature, it is common to measure the effect of place on individual earnings by analysing functional spatial units, such as Britain's Travel-to-work areas (e.g. Bosquet & Overman, 2019). It is important to stress however that, in our empirical setting, Local Authorities represent a spatial unit which is more consistent over time. For instance, TTWAs significantly change across censuses depending on changing commuting patterns and attempts to overlap birthplace unemployment information on time-varying TTWAs would lead to distortions affecting the accuracy of our treatment measure.

VoB has unemployment data for every district at each census (which are 10 years apart) going back to 1931. (There is no available data for 1941.) Since we do not have reliable birthplace unemployment data prior to 1931, we exclude from the analysis those individuals born before 1926, in-line with our procedure in matching to other censuses.

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[5] We use the variable *fivr* which includes all labour and non-labour income. We include only those with positive income, excluding those whose response is "proxy/missing" (6% of respondents) and "zero" (4%). We exclude those reporting zero income because we cannot tell for sure if theirs is a response bias or a genuine response. That said, our results differ only marginally when we include zero income responses. Results are available on request.

[6] In the Appendix Table 30 we provide results replacing our selected question with the other three available in the BHPS to capture economic variables (Heath et al., 1994). Overall, economic variables are available in 7 rounds of the BHPS (Waves 1, 3, 5, 7, 10, 14, and 17).

[7] In Appendix Table 28 we replace this coding, replacing Conservatives with Labour versus any other party. The binary party operationalisation allows simple interpretation using a linear probability model.

[8] Alternatively, in the appendix (Table 1) we provide results when replacing the measure of father's occupation with parental educational attainment. Also available in the appendix (Table 29) is a version with parental occupation, using the dominance approach i.e., the highest status of either mother or father. Results are substantively similar but there is more missing information for mother's occupation compared to father's occupation.

[9] In the appendix, we include a version with income as a control, when it is not the dependent variable (cf. Appendix Table 21). Results are very similar.

[10] We restrict our first-step to those individuals for which we also have parental background (required for the second-step), our results are robust to relaxing this restraint.

[11] For some birthplaces there are relatively few individuals. We check for this in the appendix by including only those birth LAs with more than 20 respondents. Our results remain robust (see Appendix Table 22).

[12] Appendix Table 25 includes a version of our main findings where we treat London as one single district. The results are similar.

Table 1
Summary of unemployment rates, %, by local
authority, 1931-2011.

Census Year	Mean	Std.	Min	Max
1931	9.61	5.08	3.35	33.32
1951	1.84	1.01	0.51	6.91
1961	1.33	0.62	0.51	4.22
1971	3.66	1.29	1.80	10.14
1981	7.79	3.11	3.21	22.17
1991	9.46	3.78	3.04	24.73
2001	4.63	1.97	1.47	11.45
2011	5.76	1.94	1.35	12.00

NOTE: Based on 348 Local Authorities in Eng-
land and Wales. Source: VoB.

Importantly, the BHPS provides, for each respondent, their current residence (by Census Area Statistics Wards, which we map onto Local Authorities) as well as their place of birth.[13] We are hence able to input both the unemployment rate for one's current place of residence and birthplace. We use the unemployment rate from the closest census available in VoB. For example, to an individual born in Tonbridge and Malling in 1957, we use the unemployment rate for that locality from the 1961 census.[14]

The VoB provides us with a relatively long-term perspective on UK regional disparities. Local unemployment rates have varied over time. The 1950s and 1960s were decades of virtually full employment, when the average unemployment rate averaged just 1.6% (Crafts, 1995). Moreover, the range of unemployment rates across the Local Authorities within England and Wales was small. At the 1961 Census, the highest rate of unemployment in any Local Authority was 4.22% and only 10 Local Authorities had unemployment rates of over 3%. By contrast, in 1971 the mean unemployment rate across the Local Authorities was 3.66%, with Liverpool experiencing an unemployment rate of over 10%. The situation further worsened in 1981 and 1991 (although, for our empirics, the sample size of individuals born in these latter cohorts is much smaller – as they would be too young for wave 1 of the BHPS). Table 1 provides a summary of these unemployment statistics.

The existing literature highlights that the underlying macro environment may be important for political socialisation (Alesina & Fuchs-Schündeln, 2007; Grasso et al., 2019). Neundorf and Soroka (2018) show that redistributive preferences are influenced not only by the economic backdrop during childhood but also by the national welfare-policy context at the time (see also Hansen & Stutzer, 2021). In our case, the effect of unemployment may differ depending on the macro situation. Our main empirical findings include periods, specifically the years close to the 1961 census, when Britain experienced nearly full employment and low regional inequality. In our main empirical section, we hence account for this by cross-sectionally controlling for birth year.

5. Empirical strategy

5.1. Addressing individual sorting

A key empirical challenge is to distinguish between composition and contextual effects. For example, Maxwell (2019, 2020) argues that differences in cosmopolitan attitudes across urban and rural areas are largely explained by the type of people that live in cities. Studies have

also considered the extent to which composition effects are amplified by demographic sorting, either because of the increased concentration of high-skill jobs in core areas (Keuschnigg et al., 2019) – in turn attracting younger, more educated, and in general more progressive individuals – or because of 'political homophily', that is, voters' likelihood of moving to areas with a higher presence of people sharing similar political beliefs (Bishop & Cushing, 2009; Gimpel, 1999).

We follow the urban economics literature (Combes & Gobillon, 2015) and use a two-step approach, which allows us to estimate the impact of both current residence and birthplace on our outcomes, while – importantly – controlling for composition effects based on observables *as well as* unobservable individual characteristics by means of individual fixed-effects.[15]

In the first-step we regress each of our dependent variables (part of the vector E) for individual i, currently living in area a, at wave t, on our vector of individual time-varying characteristics X, a wave dummy W, the unemployment rate in their current area U, an individual fixed-effect (FE) I – which, importantly, allows us to control for individual heterogeneity based on unobservable traits, and an error term e. We use a fixed value for current residence district unemployment, from the 2001 census (that is, the most recent census before the end of our panel). Thus, the effect of current unemployment is for 'movers' only. We include only those individuals in the regression who have participated in at least two waves, to have estimates of individual fixed-effects for the second-step regression. We regress:

$$E_{it} = \beta' X_{it} + \beta_1 U_{a(i)t} + W_t + I_i + e_{it} \tag{1}$$

This is followed by the second-step regression, where the predicted individual fixed-effects components $\hat{I}$ estimated from Eq. 1 (net of time-variant individual observables, including current place of residence characteristics) are regressed on the array of individual time invariant variables T (parental characteristics, gender, year of birth, and ethnicity), and unemployment at the time of birth in the individual's birthplace Z:

$$\hat{I}_i = \beta'^{T_i} + \beta_2 Z_{ai} + e_i \tag{2}$$

The coefficient β_2 is our main objective of interest, and it can be interpreted as the effect of a one percentage point increase in birthplace unemployment on our dependent variables. It is important to stress that including individual fixed-effects in the first step is essential to disentangle sorting effects based on unobservable characteristics, a process widely established in the urban economics literature (Combes & Gobillon, 2015). In fact, regressing current outcomes on current place of residence and birthplace would lead to biased estimates, since people sort across space depending on unobserved traits. Even if those traits were uncorrelated to place of birth – e.g. conditioning on parental characteristics, the correlation between birthplace and current residence would make the estimation of β_2 inconsistent (Bosquet & Overman, 2019).

Whereas the effect from current Local Authority unemployment rate is derived from 'movers' only (for non-movers, the coefficient is absorbed by the individual fixed effect), conditional on having an estimate $\hat{\beta}_1$, the effect of birthplace is estimated from all individuals. That is, we use the predicted fixed effect for both 'movers and non-movers' from Eq. 1) as our dependent variable in Eq. 2). We provide a robustness check where we exclude non-movers from the analysis.[16] We also provide another specification in the appendix where we match current residence unemployment rate to the nearest census (i.e., 1991, 2001, or 2011).[17] In this case, the coefficient of current unemployment rate

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[13] In some cases, nearby birthplaces are merged in the BHPS – presumably to preserve anonymity. There are also some boundary inconsistencies between birthplace districts and VoB due to boundary changes – details are available on request.

[14] As a robustness check, we also use Norman's (2017) alternative method for constructing consistent-boundary unemployment statistics. Whilst their data are only available back to the 1971 census, results (available on request) are substantively similar.

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[15] Should we run a single regression with both current local authority and birthplace unemployment, the coefficients from our main analysis are substantively similar, and are reported in Appendix Table 26. However, the current residence coefficients increase in magnitude compared to the main results, which is in-line with the compositional reasoning for the two-step approach.

[16] See Appendix 15.

[17] See Appendix 27.

is estimated from both non-movers and movers.[18] We use linear OLS regression for each dependent variable, even for voting, despite its binary nature. We do so, rather than using logistic or conditional logistic specifications, for ease of interpretation.

Addressing endogeneity in unemployment rates and family sorting

The empirical approach discussed in Section 5.1 – and, specifically, the inclusion of individual fixed-effects – plausibly allows us to control for unobservable individual traits. Yet, a second potential source of concern linked to individual sorting when identifying coefficient β_2 relates to how the potential movement of individuals across Local Authorities may affect the local labour supply and, hence, unemployment rates. Besides, local unemployment is likely correlated to other birthplace factors we are unable to control for, potentially leading to omitted variable bias. Third, our two-step strategy allows us to control for individual sorting, but not for confounding unobservable family differences across locations (beyond observable characteristics such as parental education and occupation).

To address these issues, we adopt a two-stage-least square (2SLS) estimator. We follow in an established tradition and use the shift-share approach linked to Bartik (1991), where national changes in industry structure are interacted with the initial share of a specific industry within a Local Authority. Recent literature discusses in depth the instrument's theory and inference properties (Adão et al., 2019). The identification assumption is that shift-share instruments allow isolating exogenous local economic shocks that are orthogonal to other potentially confounding factors. Goldsmith-Pinkham et al. (2020) consider a setting where the conditional exogeneity assumption is interpreted in terms of the initial *shares*. Alternatively, Borusyak et al. (2022) show that the initial exogeneity of the shares is sufficient but not necessary, since 'Bartik' instruments can also rely on *many heterogenous shifts* across different industries. The first strategy reflects the quasi-random local exposure of a specific industry to aggregate shocks. Since it may be often hard to assume the exogeneity of the initial shares, the second approach highlights how the estimator consistency may also come from many heterogeneous shocks. We test both approaches and show that our results are robust across alternative specifications.

Our first strategy relies on manufacturing shares. In line with other industrialised economies, over the Twentieth Century Britain's labour market has gone through dramatic shifts. First and foremost, employment in manufacturing grew during the first part of the century, peaked in 1966 and, since then, has gone through a dramatic decline. By the late 1990s, most of the labour market had moved to service sectors. While at its peak in the late 1960s 8.9 million people worked in manufacturing, accounting for around 30% of total employment, by 2019 industrial Britain employed just 2.7 million workers, that is, only 7.7% of the workforce (Beatty & Fothergill, 2020). While the shift from industrial (and mining) to service sector employment is not unique to the UK – but a feature in common with most other advanced economies, the process of 'destruction of industrial Britain' (ibid.) has gone further and faster than elsewhere. Industrial jobs fell particularly steeply since the late 1970s and early 1980s, during a recessionary period triggered by spiking interest rates and an exchange rate kept intentionally high by the central government to defend the growing British financial sector.

The UK's industrial job growth and then losses have been significantly concentrated in specific areas of the country, partly reflecting the overall distribution of manufacturing (traditionally more prevalent in some regions than others) and, partly, the location of specific industries such as steel, heavy engineering, shipbuilding, and textile, all of which represented the backbone of the UK manufacturing tradition, and which also experienced the largest reduction in employment since the 1970s. We again use the VoB data. We construct initial shares of manufacturing in a Local Authority based on a long lag, exploiting information from the

1911 national census. We then use national shift rates between 1911 and each of the subsequent census waves 1931-2001[19] (each subscripted as 'wave t' in the equation below) to form the instrument. As customary, when computing aggregate shifts, we don't include the respective Local Authority, to avoid any concern that a specific LA may drive the national changes:

$$Shiftshare_{wave\,t} = \frac{LA_{1911\,manuf\,emp}}{LA_{1911\,total\,emp}} * \frac{UK_{manuf\,emp\,at\,wave\,t} - UK_{1911\,manuf\,emp}}{UK_{1911\,manuf\,emp}}$$

(3)

In the Appendix we also provide versions where we instrument both birthplace and current unemployment, either using the same instrument based on manufacturing shares, or with different instruments, where current unemployment is instrumented with a second shift-share based on the consumer service industry (using again LA-level shares from 1911). Results are substantively similar.

It may be argued that, while distant in time, manufacturing shares in 1911 may still not be conditionally exogenous e.g., if initial local industrial structure was correlated to specific sociocultural norms and other traits which may be very slow to change, passed on through several generations, and endogenously influence the outcomes. We hence test an expanded time lag and use information from the 1841 census to construct the initial industry shares (the economic geography of Britain underwent important transformations towards the final part of the XIX Century, so this approach should significantly address any remaining concern about the endogeneity of the 1911 shares).[20]

Our final approach, instead, draws on Borusyak et al. (2022), and relies on many exogenous shifts across different industries. With only the manufacturing sector as an instrument, it could be contested that the shocks are unlikely to be mutually uncorrelated.[21] To mitigate this concern, in Appendix 36 we include regional dummies to partial out potentially correlated economic shocks within the 10 government office regions of England and Wales. We then further use our data in two ways to extend our analysis and plausibly argue that the shocks are uncorrelated. First, we include a full set of instruments for each of the seven industries in which historical LA-level employment data is broken down into.[22] Thus, we significantly increase the effective sample size of shocks, and the results are substantively similar (cf. Appendix Table 14). Here, we identify a potential overidentification issue.[23] Thus our second, and most conservative robustness test is to introduce location fixed effects (Appendix 35). Our shift share is calculated at multiple dates and based on constant (1911) shares. Thus, including unit fixed effects purges both time invariant unobservables and time invariant components of the shocks (Borusyak et al., 2022).

6. Results

6.1. Main findings

Table 2 presents the results. Panel A reports the effects of the first-step regressions for current Local Authority unemployment. An increase in 1 percentage point in the level of unemployment where one currently resides is associated with a statistically significant reduction in £298 in

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[18] The only exceptions are those non-movers who only participate in so few waves as to match to only one census date.

[19] We have data for 1991 and 2011, but not 2001. Therefore, to construct our measure for 2001, we average 1991 and 2011.

[20] However, choosing a baseline year from the 1800s reduces the instrument relevance and, with it, the precision of the estimates. Reassuringly, the results continue to point to a similar direction.

[21] We include descriptive statistics of the shift share instruments shocks in Appendix 18, based on Borusyak et al. (2022).

[22] These are: agriculture; mining; utilities, construction, and transportation; manufacturing; consumer services; business services; and public services. While it would be ideal to have more granular industries (and hence shocks), long panels can compensate for few industries (cf. Borusyak et al., (2022).

[23] See the Hansen-J statistic in Appendix 14.

Table 2

Birthplace unemployment and individual life outcomes: Robust OLS and 2SLS estimates.

Outcome:	Income (£1000s)	Gov. provide jobs	Homosexuality	Gender Roles	Vote
Panel A: first step					
Current LA unemployment	-0.298***	-0.0122	0.00675	0.00210	-0.00136
	(0.0708)	(0.00882)	(0.00909)	(0.00679)	(0.00204)
Time varying controls	Y	Y	Y	Y	Y
Panel B: second step (OLS)					
Birthplace unemployment	-0.0826***	-0.00838**	0.00325	0.0155***	-0.00476**
	(0.0242)	(0.00389)	(0.00412)	(0.00260)	(0.00194)
Time invariant controls	Y	Y	Y	Y	Y
Panel C: second step (2SLS)					
Birthplace unemployment	-0.208***	-0.00727	0.0203**	0.0301***	-0.00543
	(0.0525)	(0.00743)	(0.0102)	(0.00450)	(0.00403)
Time invariant controls	Y	Y	Y	Y	Y
F-Stat First stage 2SLS	316.44	278.19	180.16	293.26	320.53
Observations in first step	63,437	29,309	20,269	30,797	45,767
Number of individuals	7,074	5,441	4,895	5,997	5,901

NOTES: Standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The five dependent variables are: Gross income (measured in £1000s per year); Government should provides jobs (measured on a 5-point Likert scale, where 1 is strongly agree and 5 is strongly disagree; Homosexuality (measured on a 5-point Likert scale, where 1 is not opposed to it, and 5 is strongly opposed); Gender roles (5-point Likert scale, where 1 is in against traditional gender roles, and 5 is strongly in favour); Voting (a binary dummy variable where 1=Conservative, 0=any other party). Current LA unemployment rate is measured by 2001 Local Authority rates. Standard errors are clustered at the birthplace level in the second step. The 2SLS table reports second-stage results of the second-step regression, instrumenting birthplace unemployment with a manufacturing shift-share instrument. F-Stat is from the first-stage of the second step regression.

earnings. However, confirming our priors, according to which any impact of local unemployment on individual attitudes would play through early-life impacts, current residence unemployment does not reach statistical significance at a threshold of $p < 0.05$ for any of our other dependent variables. That is, once we control for an array of individual controls, the level of unemployment within one's residence does not affect economic and cultural views, nor the propensity to vote for the Conservative Party. Supporting this, many of the individual controls (reported in Appendix Tables 32, 33 and 34 for reasons of space) do reach statistical significance in the direction expected.

The second-step results are then presented in Panel B. Birthplace local labour market conditions affect a wide array of life-outcomes. These include earnings, but also economic and cultural attitudes, and political behaviour.

First, a one percentage point increase in the birthplace unemployment rate is associated with a £83 decrease in adulthood earnings ($p < 0.01$). To take an example, the model predicts that an individual born in Liverpool in 1971 (unemployment rate 10.1%) would earn £677 less than that same individual born in East Hertfordshire (unemployment rate 1.9%), accounting for current residence and our other individual level controls.

Importantly, birthplace unemployment also affects economic attitudes, as growing up in a district with higher levels of unemployment is associated with a greater belief in the government's obligation to provide jobs (p=0.03). The effect size is 0.0084 for each one percentage point change in unemployment, on a 5-point scale. To put this effect size in context, the effect size would be approximately half of the difference between 'high' and 'mid' level father's background ('high' occupations are management and professional occupations; 'mid' intermediate-level, self-employed and technical occupations; and 'low' semi-routine, and routine jobs).[24][25]

While the evidence towards cultural views is less clearcut, we uncover again a birthplace effect. While the effect on views towards homosexuality is not significant in panel B, on average, an individual born in an area with high unemployment believes less in gender role equality ($p < 0.001$). In this second case, the effect size of birthplace unemployment compares again, approximately, to the impact of father's occupation.

Finally, we analyse the effect on political party preferences. An additional percentage point in the unemployment rate is associated with a 0.48 percentage point reduction in support for the Conservative party (p=0.02). Again, this effect is substantial and is comparable, in magnitude, to father's occupation.

In summary, our second-step regressions show that increased birthplace unemployment has a negative effect on individuals' life earning outcomes, and it also impacts their attitudes, making these individuals more 'economically left-wing' and culturally more traditional with respect to gender roles. Potentially, these factors mediate their political preferences as well, considering how higher birthplace unemployment is associated with lower levels of support for the Conservative Party.

6.2. Two-stage-least-square findings

As we discuss in section 5.3, we use a shift-shares Two-stage-least-square estimator (2SLS) to exogenously predict levels of unemployment in respondents' birthplace in Eq. 2 (i.e., the second-step of our two-step procedure). In a first specification, we instrument birthplace unemployment with a shift-share based on 1911 manufacturing shares. In the bottom panel of Table 2, we report the 2SLS results (First-stage results are reported in Appendix Table 2, while the first step outputs are the same as our main results presented in Panel A of Table 2). Whilst the coefficient for voting loses significance, the estimates are broadly in line with our main results. The coefficient for one's view on homosexuality is significant under this specification, suggesting that, on average, those individuals born in high levels of unemployment are more likely to express intolerant attitudes.

The results point in a similar direction should we construct our instrument using manufacturing shares from 1841 instead of 1911 even

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[24] See the Appendix 32/33/34 for full regression tables, including father's occupation coefficient.

[25] Alternatively, in Appendix Table 1 we replace father's occupation with a variable capturing parental educational attainments. Results are similar.

though – as expected – the instrument loses relevance and, hence, the second-stage coefficients become larger and less reliable (cf. Appendix Tables 3 and 4). The results are also similar should we instrument unemployment rates in both birthplace and current residence, either using an instrument based on manufacturing shares for both birthplace and current residence (cf. Appendix Tables 5, 6, 7, and 8) or using two different instruments respectively based on manufacturing and consumer services industries (cf. Appendix Tables 9, 10, 11, and 12).

When including shift-share instruments for each of the seven industries in which local employment is broken down in the VoB dataset, our results are similar. Whilst this option provides a greater number of shocks, there is a potential overidentification issues. The 2SLS first- and second-stage outputs are respectively reported in Appendix Tables 13 and 14. Being the most conservative approach, with the greatest number of shocks, we include birthplace fixed effects. Again, these results are substantively similar, except for the voting dependent variable, which is not significant and switches direction, see Appendix 35.

7. Robustness checks

7.1. Household vs local area unemployment

In this section we test the robustness of our main results. Our main results control for father's occupation, thus we are including only families in employment. A direct mechanism of being born in an area of high unemployment, however, is that respondents may be more likely to have unemployed parents. In Appendix Table 17 we hence report a set of results which interact dummies for father's employment status (employed or unemployed) when respondents are aged 14 with the birthplace unemployment rate. We show that our birthplace effects remain significant for those with employed fathers. Interestingly, the interaction estimate shows the effect of birthplace unemployment is greater for those with unemployed fathers (although unsurprisingly the interaction is mostly not significant, given the small sample size).

7.2. Birthplace population size, and other birthplace Local Authority confounders

We have argued that economic adversity affects later life chances and attitudes. We have adopted the unemployment rate as our indicator of economic adversity and do not try to distinguish from other potential proxies (e.g., activity rate that could be used to indicate the profile of the area in which one grows up). However, we view this as distinct from work on population sizes, as in Bosquet and Overman (2019). The mechanisms through which birthplace unemployment and birthplace population may affect later life views and attitudes are different. To test this empirically, we include (log) population both in the first- and second-step regressions along with birthplace unemployment rate (see Appendix Table 19 for the second-step details). Interestingly, we also see a separate effect for population size, confirming the results from Bosquet and Overman, whereby increased birthplace population size is associated with higher income. Higher birthplace population is also associated with more tolerance towards homosexuality and greater acceptance of gender equal roles, aspects which could be described as 'cosmopolitan' views. The implications of this 'urban density effect' clearly go beyond the aims of this current paper.

Whilst we are restricted by the data, we further include (in both the first- and second-step regressions) three LA-level variables for which information is available in the VoB. We control for local 'social class structure',[26] local educational attainments,[27] and housing over-

crowding.[28] Our results, presented in Appendix Table 20, remain significant apart from attitudes towards gender roles, which becomes insignificant. This is a particularly strong test, given that it also reduces our sample size as these variables are not available in every census, hence why we do not include them in our main specifications.

8. Conclusion

In our view, the debate as to whether place has a causal or compositional effect on outcomes and preferences fails to engage at the right point in an individual's life. Drawing on individual-level data from the British Household Panel Survey covering England and Wales, we show that birthplaces with large unemployment decrease adulthood earnings, as well as making one more 'left-wing' on economic issues related to the redistributive role of the state, and less likely to vote for the Conservative Party. There is also evidence that high birthplace unemployment is associated with more traditional views about gender roles, although we don't find strong evidence that birthplace conditions are associated with views towards other post-materialist values, such as views towards homosexuality (results are in the expected direction, but not always significant).

Overall, our findings complement a growing body of work interested in understanding the effects of 'place-based socialisation', and led by empirical investigations carried out in the United States by Chetty et al (2014; 2018). In particular, recent research has shown how place of birth and the context where individuals spend their 'impressionable years' – i.e., the period of late adolescence and early adulthood during which people form durable political attitudes (Jeannet & Dražanová, 2019) – have a significant influence in moulding both observable characteristics such as education (Bosquet & Overman, 2019) and unobservable cognitive characteristics and capacities (Rentfrow et al., 2008).

We add to this body of work by showing that the effects of early-life socialisation – which, due to data availability, we proxy with birthplace – extend to different types of social attitudes and to political party support. Furthermore, we broaden the understanding of place beyond city-size or urban-rural typologies, to encompass key local socio-economic conditions. We conclude that where individuals are born and grow up is one of the most important determinants regarding adulthood outcomes and attitudes of any citizen. Theoretically, we offered several explanations as to why birthplace unemployment matters. These included the influence of social networks on attitudes in formative years, the availability of public services and infrastructure in more deprived areas, the importance of perceptions of luck compared to effort, and regional immobility. However, we have not been able to differentiate between these mechanisms. Future work may want to focus on exactly why birthplace matters.

Declaration of Competing Interest

The authors declare there is no conflict of interests.

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jue.2023.103571.

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[26] Proxied by the share of working-age males in social grades A and B (i.e., higher and intermediate managerial/administrative/professional workers, broadly corresponding to those on higher income).

[27] Proxied by the share of individuals with a degree.

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[28] We proxy overcrowding by the count of persons in households. This is evidently a crude proxy but, unfortunately, censuses do not consistently report details on housing overcrowding and amenities.

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