Can the salmon bias effect explain the migrant mortality advantage? An investigation of major foreign-born populations living in England and Wales.

Introduction

The migrant mortality advantage (or “MMA”) describes empirically observed low mortality among immigrants relative to the majority population in the destination country (Razum, 2008). Despite the regularity with which this phenomenon is observed, it remains unclear exactly what generates it. Oft-discussed explanations include selection (those who move are healthier than those who do not move), that foreign-born retain behavioural traits from the origin which positively influence their health, and censoring and selection biases attributed to remigration. It is crucial to understand the role remigration plays, because while the first two explanations account for a real MMA, remigration reduces it to an artefact, generated by an inability to track foreign-born populations over time and produce reliable estimates of their mortality.

For remigration, *censoring bias* originates from underreporting in that immigrants are more likely to register their entry to a country than their exit. This is problematic because the population at risk becomes inflated and reports of deaths occur in another country (Andersson and Drefahl, 2017). *Selection bias* may then arise if remigrants are disproportionately drawn from a subset of the foreign-born population with a higher mortality risk (a *salmon bias effect*) (Palloni and Ewbank, 2004). While studies have examined censoring bias over a wide range of origin-destination contexts, studies into the salmon bias effect have largely been limited to Hispanics in the U.S., with little consensus over its impact or existence. Thus, we still know little about the salmon bias, its effect on foreign-born mortality rates and whether it should be considered a general mode of bias in all foreign-born populations or limited to specific origin groups.

To rectify this, we use the largest longitudinal resource in the U.K. – the ONS Longitudinal Study (LS) – to investigate the migrant mortality advantage and salmon bias effect among the major foreign-born populations living in England and Wales. The LS is well-suited to investigating the salmon bias because it contains linked individual-level information on mortality and migration from civil/health service registers and health status from census. Our aims are to determine which foreign-born populations experience a migrant mortality advantage, which experience a salmon bias effect and, for cases in which both are observed, whether the bias introduced by the salmon bias can explain lower mortality among foreign-born.

We make several contributions to the literature. We contribute to ongoing debate over the causes of the migrant mortality advantage and whether mortality differences between foreign-born and native-born are real. To that end, and perhaps most importantly, our study is one of the first to indirectly correct foreign-born mortality rates for salmon bias. Additionally, we provide findings from a new context on a diverse range of foreign-born populations (rather than just a single origin-destination pair) which will bestow greater generalizability to the findings.

Background

Censoring bias

The estimation of mortality among foreign-born populations is subject to a number of data issues inherent to the essence of such a mobile population: one which is constantly changing, as individuals enter and leave the country over different time periods, and can be difficult to capture in data sources (Anson, 2004, Guillot et al., 2016). Central to this issue is remigration, which involves foreign-born moving onwards to a third country or back to their origin country (Dustmann and Weiss, 2007). Remigration, irrespective of whether it is motivated by poor health, distorts death rates because if remigrations are not registered individuals continue to age in destination country databases despite having left. Remigrants are included in mortality calculations, despite being unable to die, until they are flagged as having attrited (Turra and Elo, 2008).

Censoring bias has been studied in many destinations (Khlat and Courbage, 1996, Weitoft et al., 1999, Razum et al., 2000, Darmon and Khlat, 2001, Anson, 2004, Kibele et al., 2008, Kohls, 2010). Consensus from these studies is that censoring bias accentuates the MMA to varying degrees (depending on the data source and context) but does not generate mortality differences between foreign- and native-born. Censoring bias has also been investigated in the U.K. Wallace and Kulu (2014a) projected different remigration dates to determine its effect on the MMA. Their estimates were robust to changes in the size of the risk population, so the authors ruled out censoring bias as the primary explanation of the MMA (Wallace and Kulu, 2014a).

However, one problem with studies which only account for censoring bias is summarized by Palloni and Arias (2004); correcting censoring bias works if the characteristics of foreign-born leavers do not differ from stayers. But if the remigrants are more likely to be in poor health, and therefore exposed to a higher mortality risk, then mortality rates will remain downwardly biased even after correcting censoring bias. Therefore it is important to consider not only the level of unrecorded remigration but the *health* of those who leave. Consequently, we turn our attention to the salmon bias effect which has received little attention outside of a Hispanic-U.S. context, limiting the ability to generalise findings to other origin-destination pairs.

Salmon bias effect

The original definition of the salmon bias effect proposes that immigrants who start to suffer from a long-term illness return to their origin country (Valles, 2016). This reflects a desire to return to their birthplace and die in familiar surroundings under the care of relatives (Razum and Twardella, 2002, Abraido-Lanza et al., 2005). Some expand this definition out to include returns after retirement or temporary employment (Abraido-Lanza et al., 1999), and those who do not cope as well socially or economically as other people do (Razum et al., 1998). If remigrants are disproportionately selected from a subset of foreign-born which are affected by poor health, and such a process is operating on a large-enough scale, then this could well generate an MMA even in the absence of other processes (in-selection, retention of healthy traits).

Evidence for the existence of a salmon bias effect is mixed. First, we consider findings based upon Hispanic returns from the US given that this is where nearly all of the research has taken place. Abraido-Lanza et al. (1999) conclude that a salmon bias effect cannot explain lower mortality among Latinos. They highlight two groups; Cubans, who experience an MMA, and whose immigrants rarely remigrate, and Puerto Ricans, who experience an MMA despite the deaths of remigrants in Puerto Rico being captured in US registration systems. Palloni and Arias (2004) find that the MMA among Mexicans can be attributed to a salmon bias effect, but the phenomenon could not account for the MMA observed among other Hispanic groups. Using social security data, Turra and Elo (2008) confirm the existence of a salmon bias effect at ages 65+, but concluded it was of too small a magnitude to fully explain the MMA among Hispanics.

Riosmena et al. (2013) observed evidence consistent with a salmon bias in hypertension, self-rated health, and height (a general indicator of nutrition in childhood that may translate to better adult health) among remigrants with less than 15 years of experience in the US relative to immigrants with similar levels of U.S. experience. Wilson et al. (2014) observe evidence of a salmon bias effect, but only among unauthorised remigrants to Mexico. They reason that unauthorised immigrants may arrive with an MMA, but experience a different lifestyle and difficult working conditions in the US, combined with poor health care access, which may reverse it. Arenas et al. (2015) find evidence of higher probabilities of return migration for Mexicans in poor health as well as lower probabilities of return for those with improving health.

Outside of a US context few studies have been conducted. One study in Denmark conversely found that foreign-born were more likely to remain in Denmark after having reported a severe disease (Norredam et al., 2015). In Belgium, Vandenheede et al. (2015) estimated the impact of salmon bias effect by calculating the number of remigrations per group, as well as the level of mortality required among remigrants to offset the migrant mortality advantage in 25-54 year-olds. To offset lower mortality among the foreign-born, age-standardised mortality ratios needed to be extremely high: 1362/100 000 in Western and 3307/100 000 in non-Western migrants.

Three other European studies have focused on long-distance internal migration to study salmon bias. The first, in the UK, did not find evidence of a salmon bias in limiting long-term illness among returnees from Scotland to England or vice versa. Odds for male remigrants were higher but not different from natives, while odds for females were markedly lower and marginally significant relative to natives (Wallace and Kulu, 2014b). The second, which used historical data in the Netherlands, observed no significant mortality differences between "natives" living in Rotterdam and migrants who were returning to their municipality of birth (Puschmann et al., 2017). In contrast Andersson and Drefahl (2017) observed clear evidence of a salmon bias in the elevated mortality of return migrants from South to North Sweden.

Finally, a study examining self-rated health among migrants in China observed that those with poorer health were more likely to return to, or move closer, to the place of origin (Lu and Qin, 2014).

Critics of the salmon bias effect question the motivation of foreign-born to return to the origin country in cases when healthcare is of better quality, and is more accessible, in the destination (Razum et al., 1998, Norredam et al., 2015). Additionally, if the decision to return is based on the strength of ties to the origin country (Turra and Elo, 2008), and families of foreign-born individuals have settled with them in the new country, this may negate any desire to return (Razum et al., 2005, Norredam et al., 2015). Finally, the actual physical ability of severely ill immigrants to undertake and survive the journey home is also questioned (Khlat and Darmon, 2003).

Other explanations of the MMA

Beyond remigration as an explanation for an artificial MMA, the phenomenon could be real and explained by the selection of atypically healthy people from the origin country (Palloni and Arias, 2004) or the retention of beliefs, attitudes, and behaviours related to the societal and cultural norms which prevail in the origin country (Abraido-Lanza et al., 1999). In truth, empirical evidence for both is lacking and it is hard to disentangle the two (i.e. is that foreign-born come from countries where beliefs, attitudes, and behaviours which generate a higher level of health are more normative, or that individuals with favourable beliefs, attitudes, and behaviours are more likely to emigrate?). Nonetheless it is these two explanations which, if the salmon bias could be ruled out as a primary explanation, would mostly likely explain the MMA.

Summary

Based on our literature review we offer the following summary. First, one consequence of remigration (censoring bias) has been investigated more than the other (salmon bias effect). Censoring bias is largely accepted to accentuate, but not explain, the MMA. Second, while the salmon bias effect has been investigated before, studies have almost exclusively focused on Hispanics in the US and little consensus has been reached with regards to its existence or its effect on mortality rates. Finally, with respect to England and Wales, despite Wallace and Kulu (2014a) having accounted for censoring bias in their foreign-born mortality rates, we still know nothing about the health of foreign-born who remigrate from England and Wales. So, it is possible that foreign-born mortality remain downwardly biased by the salmon bias effect.

Data

The Office for National Statistics Longitudinal Study (LS)

The LS, or Longitudinal Study, links census data with life event data for a representative 1% sample of the resident population in England and Wales. The LS is the biggest longitudinal resource in England and Wales. It has linked records at every census since the 1971 Census, for individuals born on 1 of 4 selected and anonymous dates in a calendar year. These same 4 dates were/are used to refresh the sample at each census. Life event data has also been linked for LS members since 1971 and includes births to sample mothers, deaths and cancer registrations. This life event data is provided by the National Health Service (NHS) and civil registers. New LS members can enter the study through birth and immigration (if they are born on 1 of the 4 selected dates of birth). The LS contains records on over 500,000 people usually resident in England and Wales at each census and has amassed data on around one million sample members over a 40+ year period. The LS data is only accessible from a secure setting.

Outcomes

Remigration

The first of our two outcomes is remigration. A remigration is registered in the LS data when people inform the NHS they are leaving England and Wales. Unfortunately, this data is not comprehensive (as it is not a legal requirement) and there are considerable gaps in coverage. Indeed, if we were to rely solely upon registered remigrations, we would encounter problems with low statistical power. Thus, in order to "boost" the number of remigration events, we combine these registered remigrations with foreign-born who are lost to follow-up. We do this because ONS reason unrecorded emigration largely explains the level of loss to follow-up in the LS data (ONS, 2017). Additionally, the lack of data on remigration means that the use of loss to follow-up as a proxy indicator of remigration is considered acceptable international practice for researchers (Borjas, 1989, Razum, 2006, Van Hook and Zhang, 2011, Solignac, 2016).

However, to avoid including people lost for reasons other than remigration, we adopt a two-step definition of unrecorded remigration. First, we use the present at census indicators as is common practice i.e. an individual who appears at one census but not the next is considered lost to follow-up. Then, we additionally use life event information to distinguish those who do not experience any life events after their final census and those who do experience life events after final census. It is only the first group (those who do not experience any events) which we consider to have made unrecorded remigrations and the second which we consider loss to follow-up for other reasons. This differentiation is shown in Figure 1, alongside other possible outcomes (death and censoring). The wide range of life events[1] in the LS should provide good

[1] Births [live and still] to LS sample members, widowhoods, cancer registrations, (r)emigrations, (re)entries, deaths, and enlistments to the army.

coverage over the life course and help capture and exclude those lost through other modes of attrition.

We performed some robustness checks on this definition. The first check fitted two logistic regression models. The outcome of the first model was registered remigration; the outcome of the second model was unregistered remigration. Both models, adjusted for age, sex, and country of birth by health status (an interaction). We did this to determine whether we would find similar enough trends in outcomes and across explanatory variables to justify combining the two. Despite the lack of power for recorded remigrations, the patterns we observed were similar in terms of likelihood of remigration by sex, age, and, most importantly, country of birth by health. The second check compared the two different periods because of the slight differences in definition depending upon when individuals were lost to follow-up in terms of census checks and time in which a life event is not recorded. Nonetheless, the patterns in the outcome and across the explanatory variables remained similar across the two different time periods.

We recognise that while such a definition of remigration represents an improvement on many previous studies, it still makes certain assumptions and may incorrectly capture people lost to follow-up for other reasons. Individuals can still be lost if they were living in England and Wales but were not counted or failed to respond at census, through inconsistencies in linkage information, or if they moved to another part of the UK (Lynch et al., 2013). In our definition, they would also not have been able to record any life events in the time period. Additionally, inconsistences in linkage information would have to remain uncorrected for at least 14-years. Further, because ONS samples on birth dates it is unlikely there is any systematic bias from the oversampling of areas with high ethnic density or non-response. Finally, even if an LS member did not inform the NHS they were moving to another part of the UK, a remigration will still be recorded if they later register with the NHS in Scotland or Northern Ireland (ONS, 2014).

Finally, we acknowledge that, like many other studies, we do not have information on the destination of leavers, even among those who have informed the NHS that they are leaving. Consequently, our outcome includes foreign-born who move onwards or return to the origin country. As far as we are aware, there have been no studies comparing the health of onward and return migrants. The closest is by Norredam et al. (2015) which differentiated remigration to the country of origin and to "any country" in a study of remigration in Sweden. For both outcomes, a tendency was observed towards fewer remigrations of migrants with a chronic disease compared to immigrants without one. Additionally, a study investigating the salmon bias effect in internal migration using historical data in the Netherlands found no significant differences in the mortality risk of returnees to their municipality of birth (relative to natives in Rotterdam), but a much lower mortality risk among onward migrants (Puschmann et al., 2017).

Mortality

The second of our two outcomes is mortality. We model mortality, as well as remigration, for two reasons. First, we want to know which of the foreign-born groups experience an MMA over the native-born and obtain information on the magnitude of any advantage. Second, we model it to gauge differences in the mortality risk of foreign-born stayers by health status. In short, ideally for a salmon bias study, we would know whether foreign-born who remigrate after having reported poor health die after leaving the destination country. Of course, we do not have this kind of information (very few, if any, studies do). However, we are going to use information on the mortality risk of foreign-born stayers by health status to indirectly correct foreign-born mortality rates for the salmon bias effect. We will do this by assuming that the mortality among foreign-born leavers *with* a limiting long-term illness (LLTI) is identical to stayers *with* an LLTI and that the mortality of leavers *without* an LLTI is identical to stayers *without* an LLTI. We expand more upon this idea more in methods. In general, the quality of the mortality data in the LS data is very high as the registration of deaths is required by law; virtually all of the deaths taking place in England and Wales are recorded. However, delays in certification can occur if an inquest is required or if the individual died whilst abroad (ONS, 2017).

Explanatory variables

Migrant status: we define foreign-born by country of birth (reported at census) into groups: India, Pakistan and Bangladesh, Caribbean, sub-Saharan Africa, Europe (EU member 1991), Europe (non-EU member 1991), USA, Canada, Australia, and New Zealand, and Rest of the World.

Limiting long-term illness: we use LLTI to measure health status. In 1991 the question asked: "does the person have any long-term illness, health problem or handicap which limits their daily activities or the work they can do?" In 2001 it was revised to "do you have any long-term illness, health problem, or disability which limits your daily activities or the work you can do?" Respondents could answer 'yes' or 'no'. The change in the question's wording has been linked to an increased reporting of LLTIs in the 2001 Census relative to the 1991 Census (Smith and Grundy, 2011). While the question relies on self-assessment of health, LLTI has previously been found to compare well with all-cause mortality (Bentham, 1998, Boyle et al., 2002, Rees et al., 2009) and is said to be reflective of health in minority populations (Kaplan and Comacho, 1993, Chandola and Jenkinson, 2000, Newbold, 2005). The variable is already dichotomous (0 = I do not have any limiting health problem; 1 = I do have a limiting health problem).

Age, *sex*: both variables are reported at census. Age is coded into 5-year groups from 20 to 85+.

Marital status: reported at census and coded as being one of: single, married, divorced, or widowed.

Qualification level: Education level is coded to "Degree level +" and "less than degree". The inability to provide more detailed information is limited by the way in which the question was worded in the 1991 Census ("Have you obtained any qualifications *after reaching the age of 18*?").

Carstairs deprivation index: Carstairs is a socio-spatial index of deprivation which measures material deprivation in small areas. The scores are an unweight combination of four census variables: unemployment, overcrowding, car ownership and low social class. While Carstairs represents an average value for all people living in a ward, which would contain households or individuals who have varying deprivation, it has been shown to perform well in explaining variations in health measures and is frequently used to illustrate health inequalities (Morgan and Baker, 2007). We divide Carstairs into quintiles from the least (Q1) to most the deprived (Q5).

Sample

Our observation period runs from the 1991 Census (21st April) to the most recent Census in 2011 (27^{th} March) (while Figure 1 shows that we monitor life events to the end of 2015, we do not use the period 2011-2015 in our analysis as there is no indicator of presence in 2015 with which to censor individuals). We only study adults (from age 20 up to the open-ended 85+) because we want to be certain that adults have made their own decision to remigrate, rather than infants accompanying their parents. We exclude students (using economic activity indicators at census) because of the temporary nature of their remigration. We also exclude individuals for whom country of birth or LLTI (or both) has been imputed at the 2001 Census, because these are the two most important explanatory variables in our models. Additionally, we remove individuals who have been identified by the ONS as having moved to Scotland. Finally, we can only study traced LS members (an individual who has been found on the NHS registration systems and whose life events we can monitor). We exclude these untraced LS members because we cannot determine what category they should belong to in our outcome variables. Table 1 displays the distribution of selected covariates by outcome by region of birth.

Methods

To provide insight into the scale and pattern of remigration among foreign-born by age, we calculate proportions remigrated relative to the total resident population by 5-year age bands (i.e. *proportion left* ${}_{n}age_{x}$ / *total resident population* ${}_{n}age_{x}$). This will show us which foreign-born groups have the highest levels of remigration and at what ages. Evidently, groups with high levels of remigration at older (65+) ages, where risk of mortality is greater and limiting long-term illness are much more prevalent will be the most susceptible to biases inherent in remigration.

Then, we run two discrete-time survival analyses to investigate, first, the migrant mortality advantage and, second, the salmon bias effect among the foreign-born living in England and Wales. In general, survival analysis analyses data where the outcome is time until an event of interest occurs. We fit discrete-time survival models, as opposed to continuous-time models, because for remigration we do not have precise information on the date of remigration (only decade), so we treat time as being divided into discrete units. For mortality, we have year of death.

In discrete time models, we refer to the conditional probability of experiencing an event given "survival" up to that point. Specifically, we refer to the conditional probability of foreign-born experiencing mortality (in Model 1) or remigration (in Model 2) relative to being censored. We model remigration and mortality separately (and not as competing risks) for the following reasons. First, while we lack detailed information on the year of remigration, we have this information for mortality. Resultantly, we can specify a more accurate time unit for mortality (year) than we can for remigration (decade). This will, in turn, allow us to produce much more accurate mortality estimates. Second, the risk population for the two models is different. For the mortality model, we include the England and Wales-born so we can quantify the size of the mortality advantage. In the remigration model, however, we do not include the England and Wales-born because their loss to follow-up is not remigration (i.e. the process we want to study).

We fit these discrete-time hazard models by running a logistic regression on a set of pseudo-observations. For death, for example, suppose person **i** dies or is censored at time point $\mathbf{t_{j(i)}}$. We generate death indicator $\mathbf{d_{ij}}$ = 1 if a person **i** dies at time **j** and $\mathbf{d_{ij}}$ = 0 if otherwise. We do this for each time point, creating twenty, one for each year between 1991 ($\mathbf{t_1}$) and 2010 $\mathbf{t_{j(i)}}$. To the time indicators we associate a copy of the covariate vector $\mathbf{x_i}$ and a label **j**, identifying the time point. For remigration, we follow the same procedure, but we only create two time points as our unit of time for remigration is decade, not year. In the logistic regression models we then adjust, alongside our vector of covariates, year (for death) and decade (for remigration). The values of covariate vector $\mathbf{x_i}$ can time-vary in 2001 if an individual's response at census changes.

The model can be expressed as:

$$logit\lambda(t_j|x_i) = \alpha_j + xb,$$

in which $\boldsymbol{logit}\lambda_0(\boldsymbol{t_j})$ is the logit of the baseline hazard and $\boldsymbol{x_i'\beta}$ represents the effect of the covariates on the baseline hazard. The model essentially treats time as a discrete factor by introducing a single parameter $\boldsymbol{\alpha_j}$ for each possible time of the event $\mathbf{t_j}$. Interpretation of the parameters $\boldsymbol{\beta}$ associated with the other covariates follows along the same lines as in logistic regression. In the baseline Model 1/2a, the covariate vector $\mathbf{x_i}$ is age, time unit (year for death; decade for remigration), country of birth, and limiting long-term illness. In the next Model 1/2b, we add marital status, education level and Carstairs deprivation index. In the final Models 1/2c, we then interact country of birth with long-term illness. All models stratify by gender.

Then, having observed the mortality difference between foreign-born stayers according to health status in Model 1c, we correct foreign-born mortality for salmon bias at the aggregate-level. First, by sex we sum the number of deaths ($\mathbf{{}_nD_{xij}}$) and population counts ($\mathbf{{}_nP_{xij}}$) in which ***n*** equals the width of the age group, ***x*** equals the starting age of the age group, ***i*** equals leaver (=***1***) / stayer (=***0***) and ***j*** equals yes LLTI (=***1***) / no LLTI (=***0***). We do this only for groups in which a salmon bias is observed. Of course for leavers we can only sum exposure. Then, for stayers, we calculate age-specific death rates ($\mathbf{{}_nM_{x0j} = {}_nD_{x0j} / {}_nP_{x0j}}$). We assume, in lieu of death information on leavers, that their age-specific mortality is *identical* to stayers by health status i.e. $\mathbf{{}_nM_{x0j} = {}_nM_{x1j}}$. Thus for leavers we can also produce expected deaths by $\mathbf{{}_nD_{1x} = {}_nM_{0x}}$ x $\mathbf{{}_nP_{1x}}$. Next, we use population age structures of each sub-group to create proportion weights for each age group, in which the sum equals 1 (i.e. $\sum \mathbf{{}_nW_{xij} = 1}$). Corrected $\mathbf{{}_nM_x}$ for a foreign-born population is given by multiplying rates by weights and summing the values at each age group. We then repeat this process for foreign-born stayers only to obtain the uncorrected $\mathbf{{}_nM_x}$ rates.

From these, we calculate age standardized mortality ratios (ASMRs) for the uncorrected and corrected $\mathbf{{}_nM_x}$ using England and Wales as the standard. ASMRs are calculated over the entire

20-year period from 1991 to 2011 due to low death and exposure counts for foreign-born by sex and age. Therefore any differences in the scale of mortality differences between foreign- and native-born are the result of this more rudimentary setup relative to the more nuanced setup we used to observe mortality in the individual-level models. Results are shown in Table 4.

Results

Figure 2 shows the proportion of remigration relative to the resident population by age and sex for our foreign-born groups with the population pyramid in the background of each panel. In general, for females and males remigration is highest at young adult ages and decreases over age (steeply for U.S., Canada, Australia and New Zealand and Europe [non EU 1991]; gradually for Europe [EU 1991] and sub-Saharan Africa). Conversely, for males from India and Pakistan and Bangladesh, remigration levels off around age 40 and increases after 55. For females, remigration actually increases over age (more sharply among females from Pakistan and Bangladesh than among females from India). For Caribbeans, remigration levels off from age 40 for males and 30 for females. The patterns by sex (India and Pakistan and Bangladesh aside) are largely consistent. However, we note an increase in remigration in some groups at older ages for females which is not observed for males (see Caribbean, Europe [non EU 1991] and the Rest of the World). Overall, it is populations such as Caribbean males and Pakistani females, which have higher proportions of remigration at older ages which would be the most susceptible to the salmon bias effect, should one be observed in the survival models for these groups.

Model 1a (Table 2 for males; 3 for females) investigates mortality among the foreign-born, adjusting age (20-24 to 85+), time period (which is year, from 1991-2010), limiting long-term illness status and region of birth (with England and Wales-born as the reference). Its purpose is to observe which foreign-born groups experience a migrant mortality advantage over the native-born. In short, nearly all groups experience a migrant mortality advantage (mortality is between 10 to 25% lower than the England and Wales-born across groups). Only males and females from the U.S., Canada, Australia and New Zealand, and males from Europe (non-EU) do not; the mortality of these three groups is close to the baseline level and not significantly different.

In Model 1b (Table 2 for males; 3 for females) we additionally adjust for socio-economic covariates (education level, marital status, and Carstairs deprivation index) to observe whether mortality differences in Model 1a can be explained by socio-economic differences between groups. The above mortality differences persist and for two groups become more pronounced (Pakistan and Bangladesh and the Caribbean). Expectedly, across these two models mortality increases with age, decreases across time period, is higher in those with a limiting long-term illness, those without a degree, those in higher deprivation quintiles and those who are not married.

Model 2a (Table 2 for males and 3 for females) investigates remigration among the foreign-born, adjusting age (20-24 to 85+), time period (which is now decade i.e. 1991-2000; 2001-2010), limiting long-term illness, and region of origin (the reference for which is now India). Its purpose is to observe how likely different foreign-born groups are to remigrate and to see if there is overall evidence of salmon bias among the foreign-born. For both sexes, relative to foreign-born from India, all other groups are more likely to remigrate with the exception of Pakistan and Bangladesh (who have similar odds to India). Remigration is most likely among foreign-born from the U.S., Canada, Australia and New Zealand (3 times more likely), Europe (EU) (1.7 times more likely) and the Caribbean (2 times more likely among males, 1.4 among females). Importantly, while there is no evidence of a salmon bias effect among males, among females we observe a higher likelihood (25%) of having remigrated with a limiting long-term illness.

In Model 2b (Table 2 for males; 3 for females) we adjust additional socio-economic variables (education level, marital status and Carstairs) to see whether differences in remigration levels in Model 2a can be explained by socio-economic differences between groups. Likelihood of remigration falls but overall patterns remain similar to Model 2a. Additionally, the evidence of a salmon bias effect persists in females and remains absent in males. For the explanatory variables, remigration is highest at young adult ages, decreases with age, then increases from age 60 (reflecting the general trends noted in Figure 2), is most likely among highly educated individuals, those belonging to the highest deprivation quintile and individuals who are not married.

Model 1c (Figure 3 for males and 4 for females) takes Model 1b and interacts limiting long-term illness with region of birth to observe how mortality differs among foreign-born stayers by health status. It is insightful for us to observe the size of differences in mortality risk at the individual-level so we can get a good idea of the difference in risk we will apply to leavers by LLTI status at the aggregate-level when correcting rates. Expectedly, across regions mortality is higher among individuals who have a limiting long-term illness, though the difference is more pronounced in some groups (e.g. Caribbean) than others (e.g. Europe [non-EU]). For females there is much more of a consistency in the odds ratios across region groups, but they are still pronounced among those from Europe (EU) and the U.S., Canada, Australia, and New Zealand.

Model 2c (Figure 3 for males and 4 for females) takes Model 2b and interacts limiting long-term illness with region of origin to see if we can observe the salmon bias effect in specific foreign-born populations. For males, evidence for a salmon bias is evident in foreign-born from India, Pakistan and Bangladesh and the Caribbean (between 1.25 and 1.5 times more likely to remigrate with an LLTI). Conversely, all other foreign-born groups have lower odds of remigration having reported a limiting long-term illness which is statistically significant for males from Europe (EU 1991), the U.S., Canada, Australia and New Zealand, and Rest of the World.

For females, a salmon bias effect is similarly observed among foreign-born from Pakistan and Bangladesh, India, and the Caribbean (latter marginally significant). The size of this effect in the first two groups is stronger than males 1.75 to 2 times more likely). Females from U.S., Canada, Australia and New Zealand are the only group which is statistically significantly less likely to remigrate after reporting an LLTI. The rest of the female foreign-born groups (sub-Saharan Africa, Europe (EU and non-EU) have odds around the reference category i.e. these groups are neither more likely nor less likely to remigrate after reporting a limiting long-term illness.

When we consider the results from the above models together, particularly Model 1b and Model 2c, we can identify which groups experience both an MMA *and* a salmon bias effect. We observe both among males and females from India, Pakistan and Bangladesh, and the Caribbean. It is these three populations for which we will correct mortality at the aggregate level.

Table 4 displays both uncorrected and corrected age-standardised mortality ratios (ASMRs) for Indians, Pakistani and Bangladeshis, and Caribbeans relative to the England and Wales-born. Consistent with the individual models, Caribbean, Indian and Pakistani and Bangladeshi males and females all have lower ASMRs than the England and Wales-born (albeit with some minor variation in the scale of advantage). When the ASMRs are corrected (in effect, when we assume mortality of leavers by LLTI status is the identical to stayers and weight the age-specific mortality rates which are required to produce the ASMRs) we observe an increase in the ASMRs for 4 of 6 populations (except males from India and females from the Caribbean). However such an increase is negligible and cannot explain away the mortality advantage of these populations. To that end, we increase the age-specific mortality ratios among leavers incrementally until their mortality advantage can be explained. At the least, mortality would need to be between 1.3 (in Caribbeans males) and, at most, 4 times higher (in Indian males). We consider such an increase unrealistic, particularly among foreign-born leavers without an LLTI.

Discussion

In this article, we have advanced our understanding of the salmon bias effect as a cause of the MMA by studying mortality and remigration among major foreign-born populations living in England and Wales using a dataset, the LS, which links census and life event information for individuals. We set out with three main aims. Aim 1 was to ascertain which foreign-born populations experienced a *migrant mortality advantage* in England and Wales. Aim 2 was to determine whether a *salmon bias effect* was operating among these populations. Aim 3 was then to observe, among foreign-born populations which experienced both a *migrant mortality advantage* and a *salmon bias effect*, whether the latter could "explain away" the former. To achieve these three aims, we fitted separate logistic regressions for remigration and mortality to determine whether either phenomenon could be observed and to obtain information on the likelihood of these two outcomes contingent upon the health status of each of the foreign-born populations. Then, we produced age-standardized mortality ratios, "correcting" for the salmon bias by assuming that the mortality risk of leavers was identical to stayers according to LLTI status.

In relation to aim one; nearly all foreign-born populations experienced an MMA. This MMA persisted, and in some populations increased, after adjusting for additional socio-demographic variables. In relation to aim two; evidence for the salmon bias effect was observed in three of the foreign-born populations only: India, Pakistan and Bangladesh, and the Caribbean. For the other five populations then, it is telling that an MMA can be generated without a salmon bias effect and in some cases even despite the opposite (remigration of those *less likely* to report a limiting long-term illness). Some other process must generate the MMA. In relation to aim three; correcting the mortality rates of foreign born from India, Pakistan and Bangladesh, and Caribbean had almost no effect on the size of mortality differences relative to the England and Wales-born. We calculated the increase in mortality required to "explain away" the MMA and considered such levels to be unrealistic, particularly among foreign-born leavers without an LLTI.

That said, there are limitations in this study which offer opportunities for further research. We have already given due consideration to the limitations of our remigration variable, so we only outline them here. Remigration was a combination of registered remigration and loss-to-follow-up. We attempted to minimise the inclusion of those who attrited for other reasons, but some may have been captured in our outcome. Then, among remigrants, we do not know if people return to the origin country or make onward moves. Ideally, for a study on the salmon bias we would only study return migrants. Of course, with this data and many other data such a differentiation is not possible. Onward and return movers may have different motives and the sociodemographic characteristics of the two groups may differ. Finally, to correct mortality rates of foreign-born stayers, we included leavers in calculations by assuming their mortality risk was identical to stayers by health status. This, of course, may not be the case. To counter this, we performed a check to see the level of mortality required to explain the

MMA. In truth, it is likely that the mortality risk of leavers lies somewhere in-between the two.

In summary, our study has advanced understanding of what generates the MMA by showing that the MMA can exist in the *absence* of a salmon bias effect and that even when a salmon bias effect is found to be operating it cannot "explain" away the MMA. More importantly, taken with the negligible impact censoring bias had on foreign-born mortality in a study using the LS (Wallace and Kulu, 2014a), it becomes clear that the biases inherent in remigration can be ruled out as an explanation of the MMA. Subsequent research should now direct focus to the positive selection of healthy individuals and cultural factors as explanations of a "real" MMA.

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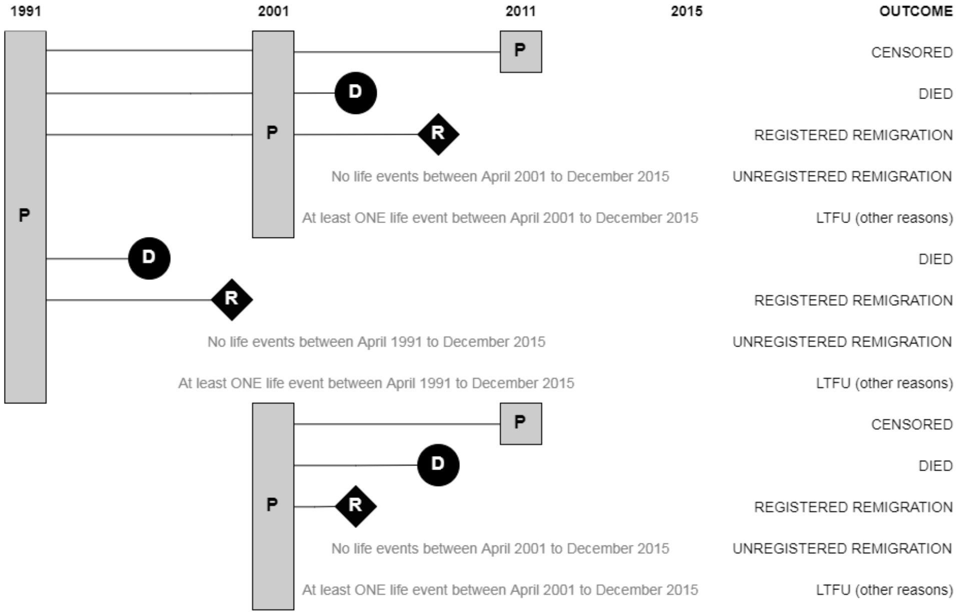

Figure 1. All possible trajectories for LS members and their outcomes.
Notes: P = present, D = death, R = remigration

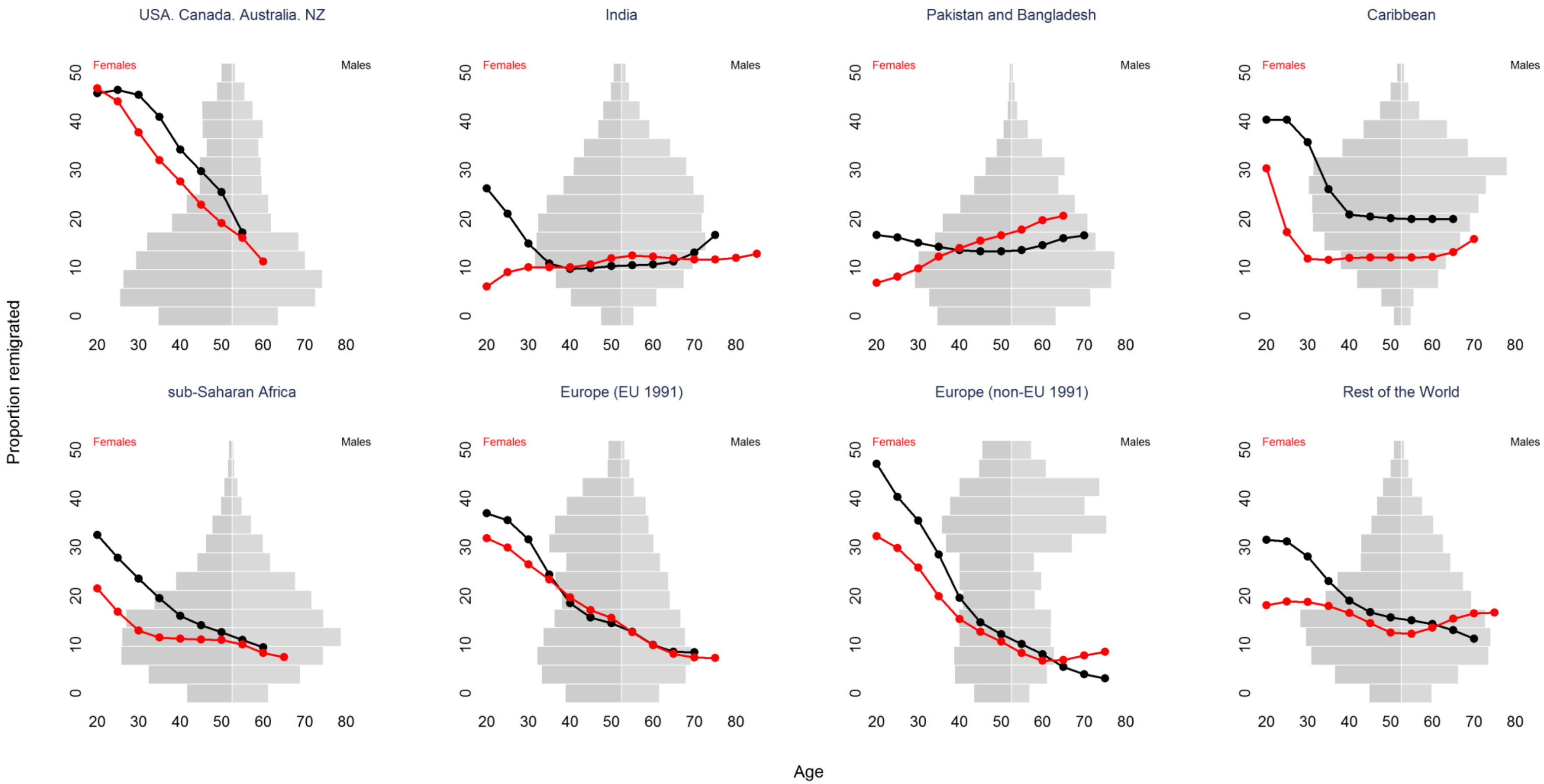

Figure 2. Proportion remigrated by age and sex for each foreign-born population.

<u>Notes</u>: Population pyramids are plotted in the background from ages 20-24 to 85+; the labels signifying the sex of each side of the pyramid also act as a legend for the remigration lines; missing points in the remigration lines at older ages are the result of values not meeting Final Output Clearance requirements of the Office for National Statistics. Authors' calculations based on the ONS LS data.

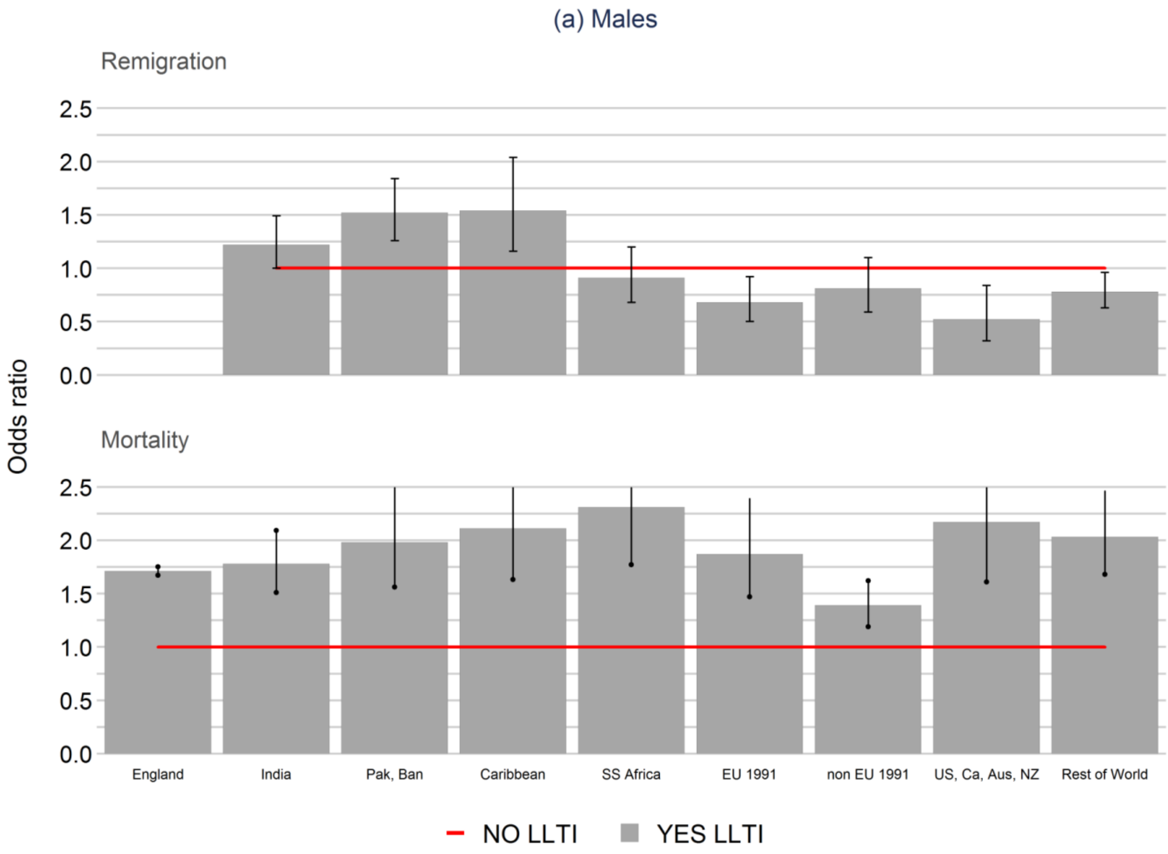

Figure 3. Probability of remigration and mortality by health status, males

<u>Notes:</u> Models adjust for the same covariates as Model 1/2b, with country of birth and LLTI interacted. Models were run with each country of birth # no LLTI as the reference to obtain all odds relative to 1. Authors' calculations based on the ONS LS data.

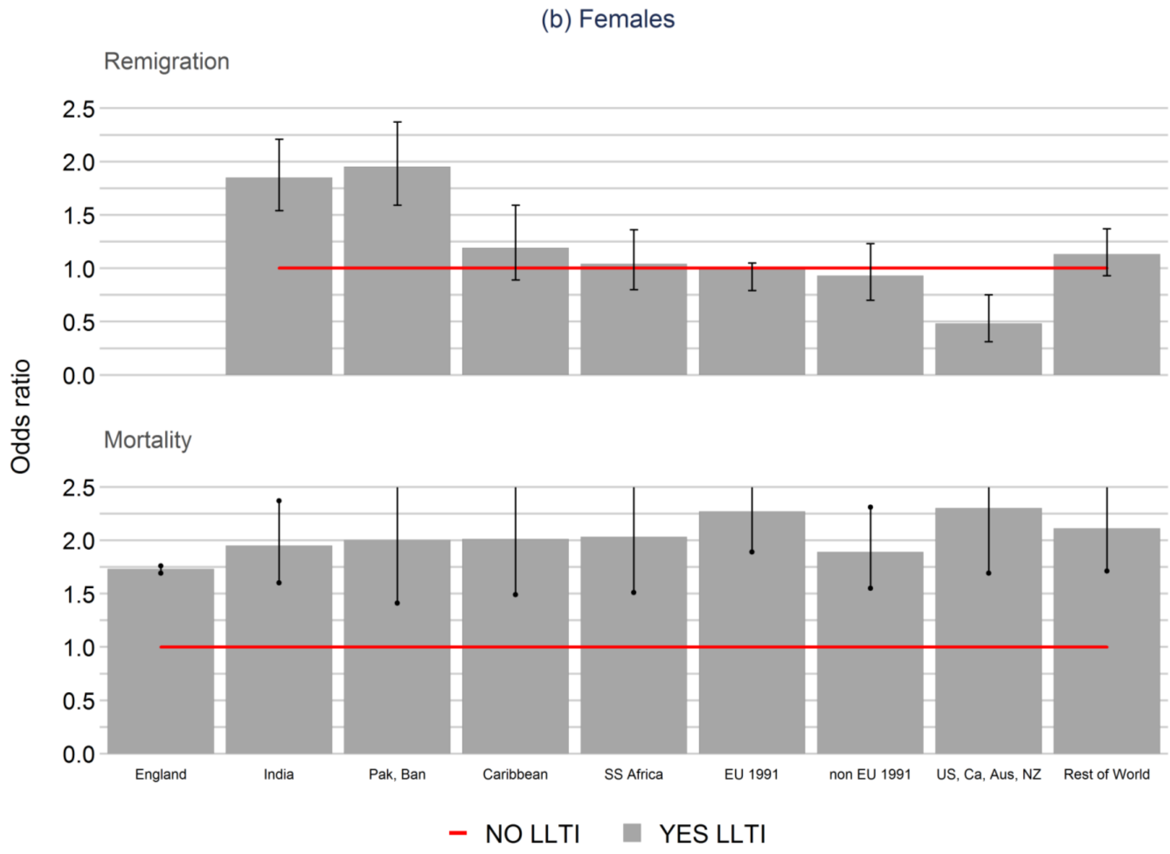

Figure 4. Probability of remigration and mortality by health status, females

<u>Notes:</u> *Models adjust for the same covariates as Model 1/2b, with country of birth and LLTI interacted. Models were run with each country of birth i # no LLTI as the reference to obtain all odds relative to 1. Authors' calculations based on the ONS LS data.*

Table 1. Covariates distributions by outcome by region of birth.

Covariates	England and Wales			India			Pak, Bang			Caribbean			SS Africa			Europe (EU)			Europe (nEU)			US, Ca, Aus, NZ			Other		
	C	*R*	*M*	*C*	*R*	*M*	*C*	*R*	*M*	*C*	*R*	*M*	*C*	*R*	*M*	*C*	*R*	*M*	*C*	*R*	*M*	*C*	*R*	*M*	*C*	*R*	*M*
Sex																											
Males	46.9	58.8	47.1	48.5	51.8	56.9	51.0	56.9	66.7	44.6	56.4	54.3	46.1	56.0	54.0	38.8	44.6	36.2	41.4	45.8	60.7	41.4	46.3	50.4	46.3	53.3	53.8
Females	53.1	41.2	52.9	51.5	48.2	43.1	49.0	43.1	33.3	55.4	43.6	45.7	53.9	44.0	46.0	61.2	55.4	63.8	58.6	54.2	39.3	58.6	53.7	49.6	53.7	46.7	46.2
Period																											
1991-2000	51.1	30.7	55.1	48.0	41.7	46.1	39.6	36.5	43.6	52.9	53.1	54.0	40.6	32.7	42.9	46.3	38.2	50.8	47.6	33.0	56.4	45.6	39.9	53.7	43.8	38.8	45.1
2001-2011	48.9	69.3	44.9	52.0	58.3	53.9	60.4	63.5	56.4	47.1	46.9	46.0	59.4	67.3	57.1	53.7	61.8	49.2	52.4	67.0	43.6	54.4	60.1	46.3	56.2	61.2	54.9
LLTI																											
No	85.7	82.9	43.4	81.7	75.5	40.9	80.4	73.4	40.2	78.2	73.7	40.7	88.1	89.5	47.6	87.0	89.4	45.5	79.5	82.4	42.2	91.7	96.0	48.8	87.0	88.2	43.8
Yes	14.3	17.1	56.6	18.3	24.5	59.1	19.6	26.6	59.8	21.8	26.3	59.3	11.9	10.5	52.4	13.0	10.6	54.5	20.5	17.6	57.8	8.3	4.0	51.2	13.0	11.8	56.2
Education																											
< degree	84.8	84.7	95.1	80.8	79.0	90.9	90.4	90.8	94.0	84.0	88.4	91.9	70.5	67.0	84.8	77.3	69.0	93.0	79.0	73.0	95.5	60.2	48.6	85.8	72.2	68.4	88.3
Degree level +	15.2	15.3	4.9	19.2	21.0	9.1	9.6	9.2	6.0	16.0	11.6	8.1	29.5	33.0	15.2	22.7	31.0	7.0	21.0	27.0	4.5	39.8	51.4	14.2	27.8	31.6	11.7
Carstairs																											
Least deprived	14.6	12.9	11.6	5.6	4.7	6.6	2.1	1.6	3.8	3.4	4.1	2.2	10.6	9.1	10.9	15.5	14.9	9.4	10.5	9.1	7.7	18.8	17.1	19.9	11.4	10.5	9.8
Less deprived	17.1	14.8	15.4	7.2	8.1	8.4	3.6	2.8	4.2	5.0	2.7	2.2	9.7	8.2	9.0	16.0	13.5	17.0	12.8	11.1	12.3	17.1	15.6	19.9	12.6	10.7	11.6
Middle	20.0	18.8	19.7	11.0	9.3	12.4	5.4	5.4	4.9	7.7	5.7	5.3	12.6	12.1	13.5	19.4	17.6	20.9	16.5	17.5	16.7	20.1	17.5	19.3	16.5	15.6	13.1
More deprived	23.0	22.8	24.0	18.6	18.2	19.2	12.2	12.4	9.8	16.3	15.0	15.5	19.5	19.1	16.4	21.0	18.7	24.2	21.7	18.0	26.1	19.7	18.6	19.3	20.1	19.8	18.6
Most deprived	24.6	30.0	28.1	56.9	59.3	52.6	76.2	77.0	76.0	66.7	71.7	74.0	47.0	50.6	48.3	27.5	34.8	27.8	37.8	43.6	35.8	23.3	30.7	20.2	38.5	42.6	45.8
Marital status																											
Single	21.6	34.9	9.9	4.3	7.7	4.7	4.9	6.9	2.2	21.0	25.4	14.9	18.0	29.7	10.2	21.6	38.6	6.3	9.1	24.0	9.1	23.0	38.7	9.3	14.9	20.2	9.2
Married	63.9	45.6	48.9	86.5	77.0	61.7	87.5	82.2	79.8	53.9	49.3	50.3	69.9	58.4	58.5	63.0	49.7	55.9	70.9	56.1	51.1	63.9	53.3	53.4	72.5	67.1	57.9
Divorced	9.2	12.9	6.0	3.7	4.7	4.8	3.9	4.8	4.7	18.7	19.1	17.7	8.6	9.1	11.1	9.3	7.7	6.2	9.6	10.2	7.7	9.2	6.4	7.9	9.0	7.8	7.7
Widowed	5.3	6.5	35.3	5.6	10.6	28.8	3.7	6.1	6.7	6.4	6.2	17.1	3.5	2.9	20.1	6.0	4.0	31.7	10.5	9.7	32.1	3.9	1.5	29.4	3.7	5.0	25.2
Total	513,399	35,001	94,838	9,437	1,451	1,113	7,463	1,348	450	2,737	661	457	6,794	1,359	422	5,640	1,553	796	3,320	766	1,186	2,476	1,232	367	9,565	2,426	845

Notes: C = censored , R = remigration, M = mortality. Percentages are column % relative to totals at the bottom. Authors' calculations based on the ONS LS data.

Table 2. Discrete-time survival models for mortality and remigration among foreign-born, 1991-2010, males.

Males	Mortality								Remigration							
	Model 1a				Model 1b				Model 2a				Model 2b			
	OR	SE	Sig	95% CIs	OR	SE	Sig	95% CIs	OR	SE	Sig	95% CIs	OR	SE	Sig	95% CIs
Age	*Adj. 40-45 = reference*								*Adj. 40-45 = reference*							
Time period	*Adj. (timescale = year) 2000-2001 = reference*								*Adj. (timescale = decade) 1991-2000 = reference*							
Country/region of origin																
England and Wales	1				1											
India	0.83	0.03	***	0.77 - 0.90	0.83	0.03	***	0.76 - 0.90	1				1			
Pakistan and Bangladesh	0.76	0.05	***	0.68 - 0.86	0.73	0.04	***	0.65 - 0.82	1.05	0.06		0.94 - 1.17	1.06	0.06		0.95 - 1.19
Caribbean	0.83	0.05	***	0.73 - 0.94	0.72	0.05	***	0.63 - 0.82	2.03	0.15	***	1.76 - 2.33	1.81	0.13	***	1.57 - 2.09
sub-Saharan Africa	0.87	0.06	**	0.76 - 0.99	0.86	0.06	**	0.75 - 0.99	1.30	0.07	***	1.16 - 1.45	1.25	0.07	***	1.11 - 1.40
Europe (EU 1991)	0.85	0.05	***	0.76 - 0.96	0.85	0.05	**	0.75 - 0.96	1.67	0.10	***	1.48 - 1.88	1.64	0.10	***	1.45 - 1.85
Europe (non-EU 1991)	0.97	0.04		0.90 - 1.05	0.94	0.04		0.87 - 1.02	1.46	0.11	***	1.26 - 1.69	1.44	0.11	***	1.25 - 1.67
USA, Can, Aus, NZ	0.95	0.07		0.82 - 1.10	0.99	0.08		0.86 - 1.16	2.85	0.19	***	2.50 - 3.24	2.77	0.19	***	2.42 - 3.18
Rest of the World	0.75	0.04	***	0.69 - 0.83	0.75	0.04	***	0.68 - 0.83	1.61	0.08	***	1.46 - 1.79	1.60	0.08	***	1.44 - 1.77
Has an LLTI																
No	1				1				1				1			
Yes	1.80	0.02	***	1.76 - 1.83	1.72	0.02	***	1.68 - 1.75	1.07	0.05		0.97 - 1.17	1.03	0.05		0.94 - 1.13
Education level																
Degree level +					1								1			
Less than degree					1.31	0.03	***	1.26 - 1.36					0.85	0.03	***	0.80 - 0.92
Carstairs deprivation																
Q1: Least deprived					1								1			
Q2: Next least deprived					1.05	0.02	***	1.01 - 1.08					0.96	0.06		0.85 - 1.08
Q3: Mid point					1.10	0.02	***	1.07 - 1.13					1.02	0.06		0.91 - 1.15
Q4: Moderate deprived					1.16	0.02	***	1.12 - 1.19					1.08	0.06		0.96 - 1.21
Q5: Most deprived					1.24	0.02	***	1.21 - 1.28					1.23	0.06	***	1.11 - 1.37
Marital status																
Married					1								0.71	0.03	***	0.65 - 0.78
Single					1.35	0.02	***	1.31 - 1.40					1			
Divorced					1.31	0.03	***	1.26 - 1.36					1.00	0.07		0.87 - 1.15
Widowed					1.17	0.02	***	1.14 - 1.20					0.90	0.11		0.70 - 1.14

Notes: Age and time period adjusted but not shown. Authors' calculations based on the ONS LS data.

Table 3. Discrete-time survival models for mortality and remigration among foreign-born, 1991-2010, females.

Females	Mortality								Remigration							
	Model 1a				Model 1b				Model 2a				Model 2b			
	OR	SE	Sig	95% CIs	OR	SE	Sig	95% CIs	OR	SE	Sig	95% CIs	OR	SE	Sig	95% CIs
Age	*Adj. 40-45 = reference*								*Adj. 40-45 = reference*							
Time period	*Adj. (timescale = year) 2000-2001 = reference*								*Adj. (timescale = decade) 1991-2000 = reference*							
Country/region of origin																
England and Wales	1				1											
India	0.83	0.04	***	0.76 - 0.92	0.82	0.04	***	0.74 - 0.90	1				1			
Pakistan and Bangladesh	0.79	0.07	***	0.67 - 0.94	0.74	0.06	***	0.62 - 0.87	1.00	0.06		0.88 - 1.12	1.03	0.06		0.91 - 1.16
Caribbean	0.88	0.06	*	0.76 - 1.01	0.81	0.06	***	0.70 - 0.93	1.41	0.11	***	1.22 - 1.64	1.24	0.10	***	1.06 - 1.45
sub-Saharan Africa	0.86	0.06	**	0.74 - 0.99	0.85	0.06	**	0.73 - 0.98	1.07	0.06		0.95 - 1.20	1.00	0.06		0.88 - 1.12
Europe (EU 1991)	0.89	0.04	***	0.81 - 0.97	0.89	0.04	**	0.82 - 0.98	1.70	0.10	***	1.52 - 1.90	1.57	0.09	***	1.40 - 1.76
Europe (non-EU 1991)	0.89	0.04	***	0.81 - 0.98	0.89	0.04	**	0.81 - 0.98	1.44	0.10	***	1.26 - 1.64	1.39	0.10	***	1.21 - 1.59
USA, Can, Aus, NZ	0.95	0.07		0.82 - 1.10	1.00	0.08		0.86 - 1.17	2.99	0.19	***	2.64 - 3.38	2.72	0.18	***	2.39 - 3.10
Rest of the World	0.71	0.04	***	0.64 - 0.79	0.70	0.04	***	0.63 - 0.78	1.50	0.08	***	1.36 - 1.67	1.45	0.08	***	1.30 - 1.61
Has an LLTI																
No	1				1				1				1			
Yes	1.79	0.02	***	1.75 - 1.82	1.74	0.02	***	1.71 - 1.78	1.28	0.06	***	1.17 - 1.40	1.26	0.06	***	1.16 - 1.38
Education level																
Degree level +					1								1			
Less than degree					1.37	0.03	***	1.30 - 1.43					0.82	0.03	***	0.77 - 0.89
Carstairs deprivation																
Q1: Least deprived					1								1			
Q2: Next least deprived					1.02	0.02		0.99 - 1.05					0.99	0.06		0.88 - 1.11
Q3: Mid point					1.06	0.02	***	1.03 - 1.09					0.99	0.06		0.89 - 1.11
Q4: Moderate deprived					1.12	0.02	***	1.09 - 1.15					1.02	0.06		0.92 - 1.14
Q5: Most deprived					1.17	0.02	***	1.14 - 1.21					1.10	0.06	*	0.99 - 1.21
Marital status																
Married					1								0.60	0.03	***	0.55 - 0.65
Single					1.24	0.02	***	1.20 - 1.28					1			
Divorced					1.22	0.03	***	1.17 - 1.27					0.57	0.04	***	0.50 - 0.65
Widowed					1.18	0.01	***	1.16 - 1.21					0.90	0.07		0.78 - 1.04

Notes: Age and time period adjusted but not shown. Authors' calculations based on the ONS LS data

Table 4. Age standardised mortality ratios (20-85+) corrected for remigration, 1991-2010

Region of birth	Original		Corrected	
	ASMR	*95% CIs*	*ASMR*	*95% CIs*
Males				
England and Wales	1890.5	(1873.5 - 1907.4)		
Caribbean	1615.8	(1413.2 - 1818.5)	1626.6	(1443.6 - 1809.6)
India	1440.4	(1325.9 - 1554.8)	1438.6	(1331.0 - 1546.3)
Pakistan, Bangladesh	1359.3	(1206.3 - 1512.4)	1360.2	(1207.1 - 1513.4)
Females				
England and Wales	1456.1	(1443.7 - 1468.6)		
Caribbean	1247.1	(1074.9 - 1419.3)	1244.0	(1084.6 - 1403.5)
India	1132.4	(1022.8 - 1242.1)	1139.5	(1036.8 - 1242.3)
Pakistan, Bangladesh	1201.3	(1011.0 - 1391.7)	1220.5	(1027.2 - 1414.0)

Notes: Authors' calculations based on the ONS LS data