The importance of community education for individual mortality: a fixed-effects analysis of longitudinal multilevel data on 1.7 million Norwegian women and men

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ABSTRACT
Background Earlier investigations have shown mortality effects of community socio-economic resources. However, the sex differences have not been clear, and the estimates may well have been biased because of inadequate control for community factors affecting both the socio-economic resources and mortality. The objective of this study was to see whether any effects appeared when time-invariant community characteristics were controlled by including community dummies (fixed effects) and whether there were any differences between women and men.

Methods Discrete-time hazard models for all-cause mortality were estimated for 1981—2002 for all Norwegians aged 60—89, using register data. There were 730 000 deaths among 1.7 million people observed during 19 million person-years. Average education was measured for 433 municipalities for each of the 22 years.

Results According to the simplest models, a high average education in the municipality is associated with increased mortality. Control for population size (time-averaged) reversed the effects. Inclusion of municipality dummies instead of population size, to control also for additional unobserved time-invariant municipality characteristics, gave very different results: the effects were even stronger for men, while those for women were no longer significant. The results were quite robust to alternative specifications, including the use of a lagged average-education variable.

Conclusion The study supports the idea that community socio-economic resources may affect mortality and suggests that sex differentials may deserve more attention. It also illustrates the importance of controlling for time-invariant community factors. Unless these can be easily measured, in future investigations one may consider establishing longitudinal data and using a fixed-effects approach such as that used here.

Several studies have suggested that a person’s mortality is influenced by the socio-economic resources in the community, given his or her own resources.1 2 Such effects have been seen even in egalitarian Nordic countries with a public healthcare system.5—10 However, the effects tend to be rather weak, and there is much uncertainty about their variation. In particular, some authors have found no difference between the sexes,5 7 11—13 while others have found that women’s health is most affected14—16 or that men’s is most affected.17

One potential problem with all such studies is that the community socio-economic resources may be determined partly by community characteristics that also affect individual mortality and that are omitted from the model. While the researcher may know, for example, whether the place is urban or rural, and include that in the model, there are a number of other factors, for example of environmental, political or cultural character, that may be difficult to measure or not available. However, if the data have a longitudinal design, in the sense that the community factor in focus is measured more than once, one can control for at least the time-invariant components of the unobserved community factors by adding 0/1-dummies for each community. These dummies would typically be referred to as ‘fixed-effects’ and the model characterised as a ‘fixed-effects model’ in economics and other social sciences, where various types of such models are often used to take unobserved factors into account.18—22 In epidemiological research on effects of community resources, regional dummies have been included in purely ecological models or to pick up unobserved factors at a higher level than the community variables in focus,23—27 but rarely to control for unobserved factors at the same level as the key community variables. An exception is a recent study on how psychological distress was affected by social capital measured in 58 US Metropolitan Statistical Areas in three successive years.28 Dummies representing that level of aggregation were included, which gave very different results, and the authors pointed out that this was a novel approach. In contrast, so-called ‘random-effects’ models are popular in social epidemiology, but these are based on an assumption that the unobserved community factors are uncorrelated with the socio-economic or other community covariates of interest (see below).

In the present study, an extraordinarily large Norwegian data set that includes individual migration histories, and therefore allows fixed-effects analysis, was used. The two main objectives were to see whether the fixed-effects analysis gave results that were markedly different from those obtained with a more standard analysis (ie, without community dummies), and whether there were any sex differentials in the effect of community socio-economic resources according to either approach. The municipality, which is the lowest political-administrative unit in the country and the lowest unit defined in the data, was the level of aggregation. The focus was on mortality at age 60—89, when most deaths occur: currently, only 7% die earlier.29
Education was used as the only indicator of socio-economic resources. It has attracted much attention as an individual-level health determinant,30–32 and has also been included as a community-level variable in multilevel research—alone or in combination with other community variables.6 33–36 or as one of several factors in a socio-economic index.11 37 38—though community income measures have been more often used. Education and income are of course closely correlated, and especially at the aggregate level,39 so the estimated effects of education also reflect income effects. More precisely, community education affects mortality partly through community income and partly through various non-economic pathways (see Discussion). In addition, community education is to some extent determined by community income.

It is not obvious which age group one should choose when calculating the average education in the municipality. Some effects of community education may involve people of the same age as the individuals under investigation, while others may involve older people or those in working ages. This issue has received very little attention in the literature. In the present study, two alternatives were tried. In addition, it was experimented with lagged variables, since it may take some time before a social environment leaves a visible imprint on mortality. For example, a person’s chance of dying in a particular year is likely to depend on his or her lifestyle in earlier years, which may be influenced by the resources of other individuals with whom the person interacted at that time. Some models included the average education 10 or 20 years earlier, in the municipality where the person lived at that time, rather than the current average-education exposure. Lagged community variables have been considered only in a few earlier studies.40 41

MATERIAL AND METHODS
Data
With approval from the Data Inspectorate and the Regional Ethical Committee, data covering the period up through 2002 were extracted from the Norwegian population register, which includes everyone who has ever lived in Norway after 1960, and from Statistics Norway’s education files based on schools’ reporting since 1980 and earlier censuses. For each person, there is information on the highest educational level achieved as of 1 October every year from 1980 and as of 1970, municipality of residence every month since 1964, dates of any in- and out-migration, and date of death. For confidentiality reasons, the municipalities of residence could not be identified but were defined by an internally consistent set of codes. Using these codes, the average education was computed for each year and each municipality from the individual data, and then added to the individual records, giving a full history of (current or lagged) average-education exposure for each person. Population size was computed in a similar way (see below).

Norway currently has 431 municipalities, but there were 435 at the time when these data were compiled. The size of the municipalities varies greatly. Oslo, the capital, has about half a million inhabitants, and there are four other large urban municipalities with a population of 100 000–250 000. Among the other municipalities, the average population size is about 7000, with a variation from 200 to 75 000.

Estimation of discrete-time hazard models
For each person, a series of 1-year observations was created, starting January the year the person turned 60 or, if born before 1921, January 1981 (except in the analyses where average education was lagged 20 years; see below). The end of follow-up was in 2002, at age 89, or at the time of death or last emigration, whichever came first. These 1-year observations included various variables defined at the start of the year, and the outcome variable was whether the person died within the year. All observations were pooled, and sex-specific logistic models were estimated from the resulting data set, using SAS software (SAS Institute, Cary, North Carolina).

Two types of models were estimated. One was of the form:

1. Equation \((p_{ij}/(1-p_{ij}))=\beta_1X_{ij}+\beta_2Z_{ij}+\beta_3T_{ij}\),

where \(p_{ij}\) is the probability that person \(i\) alive in municipality \(j\) at the beginning of year \(t\) dies within that year, \(X_{ij}\) is a vector of individual time-varying characteristics (education, which in practice varies very little over time, and age), \(Z_{ij}\) is a vector of municipality characteristics (average education and in some models population size, the latter defined as time-invariant within \(j\), though \(j\) of course varies with \(t\) for an individual), \(T_{ij}\) is a vector of dummies representing 1-year periods, and \(\beta_1, \beta_2, \text{ and } \beta_3\) are effect vectors. One-year observations starting when the person under consideration was temporarily abroad or before first immigration were ignored. In total, 917 000 women experienced 544 709 deaths during the 10.9 million person-years of follow-up, while the corresponding numbers for men were 807 000, 359 140, and 8.6 million.

The other type of model was similar, except that a vector of municipality dummies \(F_j\) (one dummy for each municipality except one arbitrarily chosen as a reference) was included to take into account that unobserved time-invariant municipality factors may affect both average education and individual mortality:

2. Equation \((p_{ij}/(1-p_{ij}))=\beta_1X_{ij}+\beta_2Z_{ij}+\beta_3T_{ij}+\beta_4F_j\),

Obviously, the \(Z\) vector cannot then include time-invariant variables, so the population size must be left out. Using this fixed-effects approach, the overall level (time-averages) of average education in the municipalities is absorbed into the municipality dummies, and the effect of average education is identified from the changes in average education and mortality within each municipality. As an example that may give some intuitive understanding of the approach, consider two persons with the same individual characteristics living in a given municipality in different years. If the difference in mortality between the person living there in the latest year and the person living there in the earliest year is most positive in the municipalities that have seen the largest increase in average education over that period, the estimate of the effect of average education will be positive (see simple discussion of the method elsewhere42). It may also be helpful to keep in mind that, according to the model, a man who experiences an immediate (and very hypothetical) one-unit increase in average education over that period, the estimate of the effect of average education will be positive (see simple discussion of the method elsewhere42). It may also be helpful to keep in mind that, according to the model, a man who experiences an immediate (and very hypothetical) one-unit change in average education in the municipality where he lives, with no individual changes taking place at the same time, will experience a change of \(\beta_1\) in his log-odds of dying. This change in mortality will also be experienced if he moves to another municipality with a one-unit higher average education and the same level of time-invariant unobserved characteristics.

As mentioned above, it is common when analysing mortality effects of community characteristics (using models such as (1)) to add a community-level random term that is intended to reflect time-invariant unobserved characteristics at that level, and that—as opposed to the municipality fixed effects in (2)—is assumed to be uncorrelated with the observed variables. In the multilevel epidemiological literature, this is usually called a ‘random-’ or ‘mixed-effects model’ (while many researchers in
this area would refer not only to model (2), but also model (1) as a ‘fixed-effects model’ since all terms are ‘fixed’. Inclusion of such a random term increases the standard errors of the community-level effects, while they in principle should have little impact on the point estimates unless there is much unexplained between-community variation. Random effects models are not estimated as part of this investigation. They are of little relevance given the focus on the possibility that certain models are not estimated as part of this investigation. They are observations as here.

**Variables**

**Individual**

For the years 1981–2002, the education variables referred to the highest level attained as of 1 October the previous year. Five educational levels were defined: (1) compulsory (10 years), (2) lower-secondary (11–12 years), (3) upper-secondary (13 years), (4) some college (14–17) and (5) higher education (18 years or more). Unknown (2%) was a separate category. For earlier years (only relevant for models including lagged variables), education referred to the level attained by the 1970 census, which for the youngest may be lower than the current level. Age was included with 5-year categories. A finer categorisation gave the same results.

**Municipality variables**

Unless otherwise stated, education was averaged over all women and men of age 60–89 in the municipality in which the person lived at the beginning of the 1-year observation. It was calculated by summing over individual observations, using a continuous education variable that was set to 10, 11, 13, 15 and 18 years in the five categories. (Using 16 and 19 instead of 15 and 18 gave almost the same results.) However, some models included the average over the age group 30–89 instead, or the average 10 or 20 years earlier in the municipality where the person lived then. Adjustment for differences in sex and age distribution was attempted but gave the same results.

There was substantial variation in average education within and between municipalities, reflecting differences in the opportunities to take education (some time earlier) and in how attractive places are to persons at different educational levels. Within the entire sample, the minimum and maximum values were 10.1 and 15.1 years of education, the SD was 0.49 years, and the mean was 11.0 years. About 20% of the variance was within municipalities. To elaborate on the latter component, which is essential in the fixed-effects modelling, the average education in a municipality increased by 0.51 years over the period 1981–2002 (assuming a linear trend) as a national average. The SD of this trend (across municipalities) was 0.18 years of education, the minimum was −0.07, and the maximum was 1.28.

Also, the population size of the municipality, averaged over the years 1981–2002, was included in some models as a 10-level categorical variable. It was the only available indicator of how urban the municipality is, which is a factor that may affect both education and health, and which it seems reasonable to consider time-invariant during the period under analysis.

**RESULTS**

A significant negative effect of the current average education in the current municipality of residence appeared for both sexes in models including also individual age and education, period, and population size, but not municipality dummies (Model A, table 1). The control for population size was important, and more so for men than for women: in some large municipalities, there was both a generally high average education and a high mortality, and when this was not controlled for, an adverse effect of high average education appeared (Model B, table 1).

In the next step, municipality dummies were included in place of population size to pick up even more of the potentially confounding time-invariant factors. Once again, the estimate was pushed most markedly towards the negative for men, and a very sharp effect appeared (Model K, table 1). For women, the effect was no longer significant at the 5% level.

If these estimates are taken to reflect causal effects, the implication would be that a man who, for example, experiences a 0.5-year increase in the average-education exposure while all other determinants of mortality remain constant would experience a mortality reduction of 17% (1 − 0.686**0.50**). The half-year increase is chosen quite arbitrarily as an example, but it may be noted that it is close to the increase that on average has taken place within municipalities over the 22-year period, and also close to the between-municipality SD of average education. In comparison, if the estimates from the simpler model with control for only population size (Model A) are taken to reflect causal effects, the implication would be that a 0.5-year increase in average education, everything else fixed, would reduce a man’s mortality by only 2% (1 − 0.955**0.50**).

It did not matter much whether the average education was calculated for age 30–89 or age 60–89, neither in models of type 1 (compare models A and C in table 1) nor in models of type 2 (compare models K and L in table 1).

A similar pattern in the estimates appeared, in both types of models, when average education was lagged 10 years (compare models A and F and models K and P in table 2). The dummies then referred to the municipality where the person lived at that time. The results were also quite similar when a 20-year lag was used, in which case the observation period had to be reduced to 1991–2002 (compare models G and H with model A, and models Q and R with model K). It should be noted, however, that the effect for women almost attained significance at the 5% level in one of the model specifications, and that some CIs for women overlapped with those from corresponding models for men.

The results also appeared to be quite robust to various other alternative specifications (not shown in the tables). For example, similar effects were seen when the five largest or 100 smallest municipalities were excluded, when using the proportion high-school graduates rather than average years of education (the different scale taken into account), and when focusing on mortality at age 40–59 rather than 60–89.

**DISCUSSION**

**Empirical conclusions**

Estimates from models that included population size but not municipality dummies suggested an inverse relationship between individual mortality and community socio-economic resources, measured here by education. This accords with many other Nordic studies, which have usually focused on somewhat younger persons and have been based on economic community indicators rather than average education. The effect did not vary with sex. Earlier studies have provided mixed evidence about such sex differentials (as well as those in the effect of individual education, and another frequently studied community factor, social cohesion).
Table 1 Effects of average education in the municipality on individual mortality, according to various models, among women and men aged 60—89 in 1981—2002 (ORs with 95% CIs)

<table>
<thead>
<tr>
<th>Models of type 1†</th>
<th>Women</th>
<th>Men</th>
</tr>
</thead>
<tbody>
<tr>
<td>A:</td>
<td>0.940** (0.928 to 0.952)</td>
<td>0.955** (0.943 to 0.966)</td>
</tr>
<tr>
<td>B:</td>
<td>1.039** (1.031 to 1.048)</td>
<td>1.098** (1.090 to 1.107)</td>
</tr>
<tr>
<td>C:</td>
<td>0.927** (0.916 to 0.939)</td>
<td>0.959** (0.948 to 0.970)</td>
</tr>
</tbody>
</table>

Models of type 2

<table>
<thead>
<tr>
<th>Models of type 2</th>
<th>Women</th>
<th>Men</th>
</tr>
</thead>
<tbody>
<tr>
<td>K:</td>
<td>0.946* (0.890 to 1.005)</td>
<td>0.686** (0.648 to 0.725)</td>
</tr>
<tr>
<td>L:</td>
<td>0.942 (0.877 to 1.012)</td>
<td>0.731** (0.682 to 0.783)</td>
</tr>
</tbody>
</table>

*p<0.10; **p<0.01; ***p<0.001.
†Effects of individual education: women: 1, 0.826, 0.782, 0.704, 0.688; men: 1, 0.836, 0.816, 0.693, 0.603.

A more important result, however, is that the estimated effects of the average education in the municipality were strongly dependent on whether time-invariant municipality characteristics were controlled. A reverse relationship appeared only when population size was included. Inclusion of that variable made especially the estimates for men more negative. However, population size may not fully capture the extent to which the municipality has an urban character, and there may be other types of time-invariant factors affecting both average education and mortality, such as distance from major cities, access to natural resources and lifestyle traditions (partly reflecting the physical environment). When all this was picked up through the municipality dummies, the effects for men were further strengthened, while those for women became weaker.

In other words, a number of time-invariant and time-varying factors may affect both the socio-economic resources in the community and individual mortality. It has been shown here that failure to control for the former may lead to severely biased estimates. In future investigations, it may therefore be a good idea to try a longitudinal design that allows control for unobserved time-invariant factors (unless the potentially important time-invariant confounders can be easily measured of course). Further, the possibility of sex differentials seems to deserve more attention.

One might suspect that the results would be sensitive to the choice of age group over which to calculate average education, since some of the mechanisms that are thought to be relevant (see review below) involve the educational level among the relatively young, while others, which perhaps have a different strength, involve that of the older. The results from this study, based on the use of two alternative age groups (30—89 and 60—89), suggest that the choice of age group may not be critical, which would be convenient for researchers in this area. However, there is always a possibility that a check of additional alternatives and a focus on other aggregate variables would have led to another conclusion.

It is also important to note that the same pattern in the estimates appeared regardless of whether the average education was lagged 0, 10 or 20 years, although it would seem reasonable to expect that some of the suggested mechanisms (see below) continued to operate.

Table 2 Effects of average education in the municipality on individual mortality, according to various models with and without lag, among women and men aged 60—89 in 1981—2002 (ORs with 95% CIs)

<table>
<thead>
<tr>
<th>Models of type 1</th>
<th>Women</th>
<th>Men</th>
</tr>
</thead>
<tbody>
<tr>
<td>A:</td>
<td>0.940*** (0.928 to 0.952)</td>
<td>0.955*** (0.943 to 0.968)</td>
</tr>
<tr>
<td>F:</td>
<td>0.924*** (0.910 to 0.938)</td>
<td>0.938*** (0.925 to 0.952)</td>
</tr>
<tr>
<td>G:</td>
<td>0.951*** (0.936 to 0.966)</td>
<td>0.963*** (0.948 to 0.977)</td>
</tr>
<tr>
<td>H:</td>
<td>0.930*** (0.908 to 0.953)</td>
<td>0.935*** (0.914 to 0.957)</td>
</tr>
</tbody>
</table>

Models of type 2

<table>
<thead>
<tr>
<th>Models of type 2</th>
<th>Women</th>
<th>Men</th>
</tr>
</thead>
<tbody>
<tr>
<td>K:</td>
<td>0.946* (0.890 to 1.005)</td>
<td>0.686*** (0.648 to 0.725)</td>
</tr>
<tr>
<td>P:</td>
<td>0.964 (0.911 to 1.019)</td>
<td>0.753*** (0.715 to 0.792)</td>
</tr>
<tr>
<td>Q:</td>
<td>0.886* (0.751 to 1.002)</td>
<td>0.692*** (0.603 to 0.795)</td>
</tr>
<tr>
<td>R:</td>
<td>0.933 (0.810 to 0.975)</td>
<td>0.826** (0.724 to 0.943)</td>
</tr>
</tbody>
</table>

*p<0.10; **p<0.01; ***p<0.001.
Why sex differences?
The individual reason why community education may influence the sex ratio is not described in the report. However, it is discussed that sex differences may be present in the effects of community education and that women may have a lower mortality than men, net of one's own education. Some studies suggest that men may have a higher mortality than women, net of one's own education, though there is a surprisingly large difference between the sexes. The effect is significant only for men, which is consistent with the findings of previous studies. The effect of education on health, both for men and women, is well-established and the results are consistent with previous findings. However, the sex differences in the effects of education on health are not fully understood and further research is needed to clarify these differences.

What is already known on this subject

Earlier investigations have shown health and mortality effects of education that may differ by sex. For example, education may have a greater effect on health and mortality for men than for women. However, the sex differences in the effects of education on health are not fully understood and further research is needed to clarify these differences.

Methodology

In this study, the population size was controlled, and then an additional variable was included to control for community characteristics that may influence the results. The results showed that the sex differences in the effects of education on health are not fully understood and further research is needed to clarify these differences.

What this study adds

In the present study, population size was controlled and then an additional variable was included to control for community characteristics that may influence the results. The results showed that the sex differences in the effects of education on health are not fully understood and further research is needed to clarify these differences.

Weakened and limitations

It should be noted that the sex differences in the effects of education on health are not fully understood and further research is needed to clarify these differences. Additionally, the study includes a small number of observations, which may limit the generalizability of the findings. Further studies with larger sample sizes are needed to confirm these findings and to explore the mechanisms underlying the sex differences in the effects of education on health.
Selective migration may also produce a bias: people who live in the municipality when education is generally high may be different—in terms of unobserved individual characteristics—from those living there when it is low, and not as a result of the high average education, which would simply be a causal pathway, but because some characteristics may increase the chance of moving to or remaining in a place with a high level of education. For example, those who are exposed to a high average level of education may tend to have socially advantaged parents, or may for other reasons have had a strong economy in the past.\textsuperscript{57} They may also tend to have better-educated spouses, which may contribute to a lower mortality.\textsuperscript{54} Spouse’s education was included in some additional models estimated only for the married and was found to explain less than 10\% of the effect of municipality education. In addition to such socio-economic factors, there may be selection with respect to, for example, individual attitudes that are linked to health behaviour.\textsuperscript{537 58}

Interesting questions remain to be answered. In particular, no attempt was made in this study to see whether the effects of the average education in the municipality are non-linear, and it was not checked whether the effects vary across causes of death, as other community effects have been found to do in some studies.\textsuperscript{9} Further, it was not checked whether the effects of community education vary with the level of own education, which would accord with some\textsuperscript{57 58} but not all\textsuperscript{59} earlier investigations. Finally, the data did not allow consideration of smaller geographical units. It surely makes sense to focus on municipalities, where many important political decisions are taken, but there would also be good reasons for considering higher\textsuperscript{10} or lower\textsuperscript{17 15} levels of aggregation, if possible.

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REFERENCES


