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Trends in social inequality in self-reported health in the Netherlands; does infant mortality in year of birth as a cohort indicator matter?

Christiaan W.S. Monden*, Gerbert Kraaykamp, Nan Dirk De Graaf

Department of Sociology/ICS, Nijmegen University, PO Box 9104, 6500 HE Nijmegen, Netherlands

Abstract

In this article, we study trends in self-reported health (general health and chronic conditions) and health inequality in the Netherlands between 1974 and 1998 using an age-period-cohort framework. We answer two questions: (1) to what extent can trends in self-reported health be explained by the current macro-context (period effect) and by infant mortality in year of birth (cohort effect)? And (2) do the effects of period and cohort differ for educational groups? Health indicators are self-reported poor health and chronic conditions. The use of 26 Dutch cross-sectional surveys makes it possible to estimate largely unbiased effects of period and cohort simultaneously (controlled for age effects) and thus to adequately describe trends in social inequality in health. Our results give rise to four conclusions. First, for men poor health has been more or less stable, for women there has been an increase. The prevalence of chronic conditions has increased for both sexes. Second, adding cohort specific experiences to a model including age and period effects is only relevant for women's poor health. Decreasing infant mortality in year of birth leads to better health and consequently the period effect initially found for women appears to be slightly underestimated. Third, we found no trends in social inequalities in self-reported health due to period effects. Fourth, our analyses do show socially unequal trends in health as a result of cohort specific experiences. Contrary to our hypothesis, we found that decreased infant mortality in year of birth makes for a stronger impact of educational differences on self-reported poor health. Concerning chronic conditions no trends for educational groups were found.

Keywords: Social inequality; Self-reported health; Trends; Cohort effects; Infant mortality; Educational inequality; The Netherlands

Introduction

Trends in social inequality in self-reported health have been a frequent subject of study in the last decade (see, for instance, Anitua & Esnaola, 2000; Lahelma, Arber, Rahkonen, & Silventoinen, 2000; Whitehead, Evandrou, Haglund, & Diderichsen, 1997). Concerning trends in self-reported health in the Netherlands, a country with relatively low social health inequalities, research shows mixed results. Some studies report no trend in health inequality (SCP, 1992; Van Baal, 1997), whereas others find a slight increase (Joosten, 1995; Kunst & Mackenbach, 1997; Mackenbach & Verkleij, 1997). Most authors of the above-mentioned studies assume that observed changes over survey years are due to period effects after including age in the analyses. Since age, period, and cohort effects are linearly dependent (cohort = period − age) (Glenn, 1977), they implicitly assume that cohort effects do not exist. In this article, we will investigate if cohort is relevant for health and health inequalities. Moreover, we would like to go beyond the question whether period or cohort effects exist and specify what macro-social circumstances during upbringing are responsible for these cohort differences.

There are apparent reasons to consider cohort in studying health and social inequality in health. Firstly, several recent studies showed that pre-adolescence

*Corresponding author. Tel.: +31-24-3612008; fax: +31-24-3612399.

E-mail address: c.monden@maw.kun.nl (C.W.S. Monden).

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childhood environment is important for health in later life (Blane, 1999; Davey Smith, Hart, Blane, Gilles, & Hawthorne, 1997; Van de Mheen, Stronks, Van den Bos, & Mackenbach, 1997; Wadsworth, 1997, 1999). Since members of birth cohorts differ in their childhood environment it may be expected that cohort effects occur. Secondly, interest in macro-social determinants of morbidity is growing (e.g., ESF Scientific Programme 'Social Variations in Health Expectancy in Europe'). We here will test explicitly the expectation that cohort differences in infant mortality explain trends in health and health inequality over time. This indicator of macro-social circumstances increased over time, which implies cohort differences in childhood experiences. Inequality here refers to differences in health between educational groups. We will answer two questions. The first question reads: *To what extent can trends in self-reported health be explained by the current macro-context (period effect) and by infant mortality in year of birth (cohort effect)?* Our second question is: *Do the effects of the current macro-context (period effect) and infant mortality in year of birth (cohort effect) differ for educational groups?* We will answer these questions for self-reported poor health and chronic conditions. For this purpose we employ repeated cross-sectional Dutch data of 26 surveys (1974–1998). Such a powerful design provides us with the opportunity to estimate largely unbiased effects of period and cohort simultaneously (controlled for age effects).

Theory and hypotheses

Social inequality

In this study, social inequality in health refers to differences between educational groups in self-reported health. Education is one of the most important predictors for a variety of life chances (Hyman, Wright, & Reed, 1976; Ross & Wu, 1995). Everybody is assigned an educational level and (after the age of 25) this is a rather stable attribute over the life-course. Moreover, education pertains to the cognitive abilities and opportunities of people to adapt to certain circumstances in life and to knowledge about health. The link between education and health has often been described. Higher educated people, in general, report preferable heath conditions compared to their lower educated counterparts. We assume this positive association and its empirical support to be evident, and we refer to previous studies for a further interpretation of the association (Ross & Wu, 1995; Stronks, Van de Mheen, Looman, & Mackenbach, 1996). This article addresses the question whether differences between educational groups in reported health are dependent on macro-social circumstances (period and cohort effects).

Specifying effects of age, period and cohort

If we would like to know how macro-social circumstances (i.e. formative and current context-effects) might influence health inequality, we will have to deal with the identification problem of age, period and cohort effects (Menard, 1991; Robertson & Boyle, 1998a, b; Rodgers, 1982). An elegant and theoretical preferable solution to this problem is to specify variables for which age, period and cohort are only indirect indicators (De Graaf, 1999; Firebaugh, 1997; Rodgers, 1982).[1] In this context, we should ask questions like: (a) what might affect one's health in a particular year (period); (b) what macro-circumstances during one's childhood might have remaining effects on health (cohort)?; and (c) what might affect one's health when one grows older (aging). Because our main interest lies in period and cohort effects, we will specify these two questions first and then turn to the effects of age and some confounders.

Effects of period and cohort specific experiences on health

There are several answers to the question: what might affect one's health in a particular year? Economic prosperity, the quality of public health care, welfare programs or even weather conditions might influence an individual's health conditions. For instance, Wilkinson (1999) hypotheses that income inequality within a country affects the general health condition of the population. Ideas like this may be useful because macro-social circumstances that explain differences between countries at one point in time might be relevant for individual differences over time within a single country.

Our primary interest, however, concerns the period effect per se. We do not specify period effects theoretically. We simply hold the assumption that the effect of macro-social circumstance at the time of measurement leads to a linear trend.[2] Specifying the

[1] The standard way of dealing with identification problems is a methodological one. See Robertson and Boyle (1998a, b) for procedures to model age-period-cohort effects.

[2] Although some theoretical notions are available, we choose not to specify the period effect. First, it is hard to imagine that a macro-social circumstance (for instance infant mortality, life expectancy or economic growth) in a particular year is effective on health in the same year. It is more plausible that the macro-social circumstances of the previous year, or of two, five or ten years ago will affect a person's current health. There are hardly arguments to chose among these 'lags'. Moreover, these 'lags' will most likely differ for macro-social circumstances and for age groups. Maybe the problem is even more complicated and are changes in recent environmental circumstances what we should model for the period effect. Second, as in almost every study on trends we have to deal with a small number of survey years, which may be referred to as the 'degrees of freedom problem'. This makes it difficult to test theory-based hypotheses. Although there are many respondents, there is only a

period effect in this way has the disadvantage that we cannot test theory-based hypotheses on the effects of income inequality, level of medical care, growing diffusion of medical knowledge in the general population (proto-professionalisation), and other factors (on health and health inequality). There are some general expectations, however, that might help formulate descriptive hypotheses. On the one hand, the increasing number of people surviving bad health, and the process of proto-professionalisation might have led to the reporting of more chronic conditions and poor health. On the other hand, the improved curative and preventive medical care may have caused a decrease in the reported number of chronic conditions and poor health. These two processes may both be important and sign each other out. Consequently, we can formulate two competing hypotheses on period effects. *Over time more persons report poor health and chronic conditions (due to changes in macro-social circumstances), controlling for age and cohort effects* (hypothesis 1a). The competing hypothesis is: *Over time less persons report poor health and chronic conditions (due to the changes in macro-social circumstances), controlling for age and cohort effects* (hypothesis 1b). Our measurement of self-reported chronic conditions is primarily objective, whereas self-reported poor health is more subjective and might partly be a relative measure. People will compare their own health status to that of others. Due to this subjective and relative aspect of self-reported health, some people in a society will always feel worse off compared to others, independent of the average objective health of the population. This is not the case for chronic conditions. Therefore, chronic conditions might be more sensitive to changes in macro-social circumstances over time.

A person's health condition at a certain moment does not come out of the blue. Nor can it be solemnly attributed to current behaviour and current (individual and macro-social) circumstances. Health at the time of interview can be considered as the result of exposure to circumstances and behaviour over the life-course. In this study, we concentrate on exposure in the formative years, so-called cohort effects. To specify these kinds of cohort effects we have to answer the question: what circumstances during one's childhood have effects on health later in life? One way to take exposure into account is to consider the macro-social circumstances during one's youth. The circumstances in which successive birth cohorts grew up differ substantially. Over the twentieth century the level of health care and general wealth have increased enormously. Members of more recent cohorts have benefited more from the macro-social circumstances and therefore are expected to report better health outcomes (irrespective of their age) compared to members of older cohorts.

The level of public health and wealth for individuals belonging to a cohort, in our study, is indicated by infant mortality in year of birth (per 1000 live births). As members of the oldest cohort were born in 1905, and members of the youngest cohort in 1973, there is a substantial variance in infant mortality. Infant mortality indicates the state of the early life circumstances of cohorts (see, for instance, Caselli and Capocaccia (1989) for a similar use of infant mortality as a cohort indicator). We believe decreasing infant mortality indicates the improved general public health and wealth, such as better housing conditions, progress in nutrition, and the fact that all kinds of tasks in the household and in jobs have become physically less straining (for a more detailed description of the mortality decline in the Netherlands and a discussion of its determinants see, for instance, Wolleswinkel-van den Bosch, Van Poppel, Tabeau, & Mackenbach, 1998). Hence, our hypothesis reads: *The lower infant mortality in a person's year of birth, the smaller his/her chance to report poor health or chronic conditions, controlling for age and period effects* (hypothesis 2).

Effects of period and cohort specific experiences on social health inequality

Period and cohort effects on health may differ for educational groups. Hypotheses for these differences can be formulated on both theoretical grounds and empirical ones. Intuitively one might expect social inequality in health to decrease as our society has become more equal in many spheres of life, such as gender differences and intergenerational mobility. The elaborate welfare system that has been established also supports this expectation. However, because gender and social background have become less important, educational inequality has become relatively more important for the distribution of life chances. So, it also is plausible to expect increased social inequality in health.

Previous research, in general, suggests that either a (slight) increase of inequality or a stable pattern exists. For instance, Joosten (1995) concluded that health differences by socio-economic status have increased between 1974 and 1983.[3] Also, Kunst and Mackenbach (1997) reporting on the change in health inequality

(*footnote continued*)
limited number points in time for estimating period effects. The rather strong assumption of linearity implies that we do not maximise period effects. This would imply a dummy for each survey year. To be sure that our results are not biased by our choice of specifying period effects in this way, we ran additional analyses applying other specifications. Our conclusions are not changed by applying 5 years categories or an indicator contrast with dummies for each survey year (results available from the authors).

[3] This analysis only concerns (male and female) heads of households.

between the early eighties (1983–85) and early nineties (1992–93), concluded that inequality has increased over time.[4] Furthermore, according to Mackenbach and Verkleij (1997) social inequality in health has increased according to two health indicators, namely temporary limitation of activity and chronic conditions. In contrast, analyses of a number of other health indicators do not support a trend towards increasing health inequalities (Mackenbach & Verkleij, 1997). A study by the Social and Cultural Planning Office (Sociaal en Cultureel Planbureau, 1992) also shows no evident increase in social health inequality in the period between 1974 and 1989. Van Baal (1997) reports the same conclusion for the period 1981–1996.

To sum, for the Netherlands, the results on the trends in social inequality in health are mixed, but most studies point to a slight increase in inequality. Therefore, we pose our hypothesis as follows: *Over time lower and higher educated persons increasingly differ in their reporting of poor health and chronic conditions (due to changes in macro-social circumstances), controlling for age and cohort effects* (hypothesis 3).

If we turn to the consequences of infant mortality in year of birth for social inequality in health later in life, we think that lower social groups have benefited relatively more from the rise in public health than the higher ones. At least for basic care the higher educated face a 'ceiling effect' compared to the lower educated who, as we presume, have caught up with respect to primary care. Many, if not all, institutions of the welfare states are especially aimed at reaching the lower social strata. Thus, we argue that: *Lower educated persons experience stronger positive effects of decreasing infant mortality in year of birth than higher educated persons on their reporting of poor health and chronic conditions, controlling for age and period effects* (hypothesis 4). This hypothesis implies that the differences between educational groups in health decrease over birth cohorts.

Age and control variables

In our case, the question 'what might affect one's health when one grows (one year) older' has a rather straightforward answer: physiological aging is what affects individual health. In social medicine this might seem very obvious, yet in most fields in social science physiological aging itself is hardly important. What matters is what happens to you at several stages of life. For instance, answering this question in sociology could invoke answers about daily activity, position on the labour market, characteristics of the friendship network, having children and religiosity. Effects of physiological aging are not ruled out, but the overall association with age is assumed to be caused by circumstances in the life cycle rather than physiological age itself (De Graaf, 1999; Sampson & Laub, 1993). In health studies, however, there is a direct aging effect, that is a person's physiological condition will decline over time irrespective of events in the life course. Nevertheless, having children, being married, participation on the labour market and other 'events' in the life cycle are important for a person's health as well (see, for instance, Macintyre, 1992 for family issues). A clear picture of physiological age effects requires the inclusion of such life cycle measures.

We use several individual characteristics that are associated with a person's position in the life cycle as control variables: having children at home, marital status and household income. Analogous to the healthy worker effect, one can speak of a healthy mother effect. Women who are in good health are more likely to have children than women who are in bad health. For men this selection effect is presumed to be smaller or non-existent. Thus, we expect a positive effect of having children at home for women and no effect for men.

Previous research has shown that marital status is important for health through material circumstances and individual riskfull behaviour (Joung et al., 1997). Marital status can be regarded as another indicator of the position in the life cycle as well as a partly independent factor influencing health. We expect to find the typical pattern of married people reporting better health than singles and widows. Divorced persons, especially men, are expected to be relatively unhealthy.

Furthermore, we expect people with a higher income to be in better health than people with a low income. This positive effect of income has been confirmed frequently. Explanations of the income effect among others pertain to preventive medical care, healthy food, and material circumstances. However, it should be noted that income in this study is income at the time of survey. As we use income purely as a control variable we do not consider the causality problem very relevant here. Income usually increases with age, but again this has no physiological grounds. By including these control variables we are better able to estimate period and cohort effects and we believe our age effect comes closer to the real physiological aging effect.

Data and variables

Data sources

Investigating period and cohort effects requires information on respondents from a sufficient number of periods and cohorts. We think we have obtained a powerful data set by combining the two main data

[4] The results reported by Kunst and Mackenbach (1997) may be due to the use of particular survey years. Comparing the NHIS 1983–85 to 1991–92 or 1993–94 instead of 1992–93 (which was used) shows no significant increase.

sources available for studying social inequality in self-reported health in the Netherlands: the Netherlands Health Interview Survey (NHIS, annually since 1983) and the Living Conditions Survey/Continuous Living Conditions Survey (LCS/CLCS, irregularly from 1974 to 1996). We were able to obtain data from 26 surveys (see the appendix) collected by Statistics Netherlands (CBS). We choose to leave out surveys collected by other institutions or with different procedures. Our stacked data file covers the longest possible research period for the Netherlands. Not all surveys asked for chronic conditions, so, after deleting respondents with missing data the file contains 70,382 respondents reporting on chronic conditions and 114,280 respondents reporting on general subjective health. In the appendix we present basic descriptive information about the data sets. The data are weighted according to the annual distribution of age, sex and marital status as reported by Statistics Netherlands.[5]

We analyse individuals between 25 and 74 years of age. The lower limit is chosen to make sure the vast majority has finished education and attained their highest diploma. Previous Dutch research (Kunst & Mackenbach, 1997; Van Baal, 1997) has used age 16 as the lower limit, but at that age a final educational level cannot be established for the majority of respondents. As a consequence, health inequality in these studies may be biased. We set the upper age limit at 74 to have a sufficient number of cases in the oldest age group. Respondents born before 1905 are left out of the analyses, because their cohorts contain very few respondents.

Dependent variables

As dependent variables we employ two *health indicators*. Although the surveys allow for a series of health indicators, we choose the two most commonly used in social epidemiological research: a single question on general health and the number of chronic conditions. Both of these measures are reliable measurements of health status (Ferraro & Farmer, 2000). Self-reported general health was asked in the same manner in all surveys:[6] "How is your health in general? Very good, good, fair/not bad, bad, very bad." By convention we dichotomise the answers in good health (0) and less than good or *poor health* (1).

The number of chronic conditions presented to the respondents is not equal in all surveys. The NHIS volumes up to 1989 include questions on chronic conditions that cannot be used in over-time comparison. The way the questions were posed in the remaining surveys varies slightly. At least 13 conditions, out of maximum of 24, were presented to the respondents.[7] We counted how many of these 13 chronic conditions each respondent reports. This measure correlates highly with a count of the maximally available conditions (0.89) and also correlates highly (0.88) with a standardised score per survey. Our dependent variable indicates whether respondents report no (0) or one or more (1) *chronic conditions*.[8]

Independent variables

Next, we describe the construction of our independent variables. For reasons of parsimony we model a linear *period* effect. To obtain odds ratios that can be interpreted easily we use 5 years as units for the scale and we centre the variable.[9]

We employ one variable that measures *cohort specific experiences*: infant mortality in year of birth. This measure reflects the number of deaths before the age of one per 1000 live births. This information is obtained form official statistics by Statistics Netherlands for each birth year of the respondents in the analyses. Between 1905 and 1973 infant mortality per 1000 live births decreased from 137.2 to 11.5 (see the figure in the appendix). Again, for reasons of interpretation, this variable is centred and divided by ten.

Education of respondents is measured as the highest level attained. As we set the lower age limit to 25, education is completed by almost everyone. Four levels of education are distinguished: primary education (or

[5] Distributions of age, sex, and marital status for the weights were obtained from Statistics Netherlands online reference (http://www.cbs.nl). We used the pweights option in Stata to obtain accurate confidence intervals.

[6] In some surveys the first and third answer category differed slightly. 'Good' in the first two categories is preceded by one of two equivalents of 'very good' ('zeer goed' and 'heel goed'). In some surveys the third answer category reads 'fair', in others 'sometimes good and sometimes bad' is used. We assume these small differences do not affect response patterns. Moreover, we find it does not affect lower and higher educated in different ways. A dummy indicating difference in question formulation did not have a significant effect in our analysis.

[7] Respondents were asked if they suffered from asthma, sinusitis, serious heart disease or heart attack, hypertension, stroke or effects of stroke, stomach ulcer/duodenal ulcer, cystitis, prolapse (for women only), diabetes mellitus, inflamation of thyroid, serious back problems, epilepsy or other diseases of the nervous system, and any form of cancer.

[8] We dichotomise the number of chronic conditions for three reasons. First, the distribution is very shrew. Only 9% of the respondents report more than two conditions and 0.1% reports five or more conditions. The most important difference lies between having no or one or more conditions. Second, we want to present odds ratios which necessitates dichotomisation. Third, it is rather standard to dichotomise on none versus one or more and we want to follow this standard. We also estimated models for an alternative dichotomisation: none or one versus two or more. This analysis did not yield other conclusions.

[9] We checked all the models with a dummy for every single year, and for five 5-year periods instead of a linear function as well. This did not change our conclusions.

less), lower secondary education, higher secondary education and tertiary education (professional vocational training and university). Following the standard practice we define tertiary education as the reference group in our analyses.

We categorise *age* in ten 5-year groups, with the youngest respondents (25–29) as reference. This way the possible non-linearity in the aging effect is easily reflected in the tables. A dummy variable indicates the presence of *children in the household* (0 = no, 1 = yes). All surveys asked respondents for household *income*. We standardise the income variable within each survey. About 20% of the respondents did not report household income. Their income is estimated based on the regression equation of income on age, education, gender, marital status and labour market participation. These estimations are imputed for the missing values. In all equations, we add a dummy indicating whether income was imputed. *Urbanisation*, as a control variable, is split in three categories from low to high, with low urbanisation serving as reference. Finally, we use a dummy indicating the *type of survey* (NHIS or LCS/CLCS) to control for possible survey effects.

Analyses

Simple trend figures

We start with a description of the trends in self-reported poor health. Figs. 1 and 2 report the development for the four educational groups in the Netherlands between 1977 and 1998 for men and women, respectively. The trend is adjusted for age, urbanisation and marital status separately in each year. The percentage of respondents in the lower educated group reporting poor health is more than twice as high as in the higher educated group. For men this difference seems even larger. If we turn to trends we observe that these are all but smooth. Despite the large sample size and comparability of questions and survey procedures, we find quite some fluctuations. They are mostly within a 5% point range and can best be described as trendless fluctuation. In general, for men a slight decrease in poor health is observed and for women a small increase.

Figs. 3 and 4 show the developments in self-reported chronic conditions. The overall percentage of respondents reporting a negative health condition is higher than the percentage for poor health. Almost one in three respondents reports one or more chronic condition. Poor health is reported by one out of four respondents. The differences between the educational groups appear to be smaller for chronic conditions than for poor health. For men the increase in chronic conditions over time seems somewhat stronger than for women. Nevertheless, the percentage of higher educated women reporting chronic conditions appears to have grown spectacularly in the last 3 years. They are now on the same level as the lowest educated women. In general, men report less chronic conditions than women.

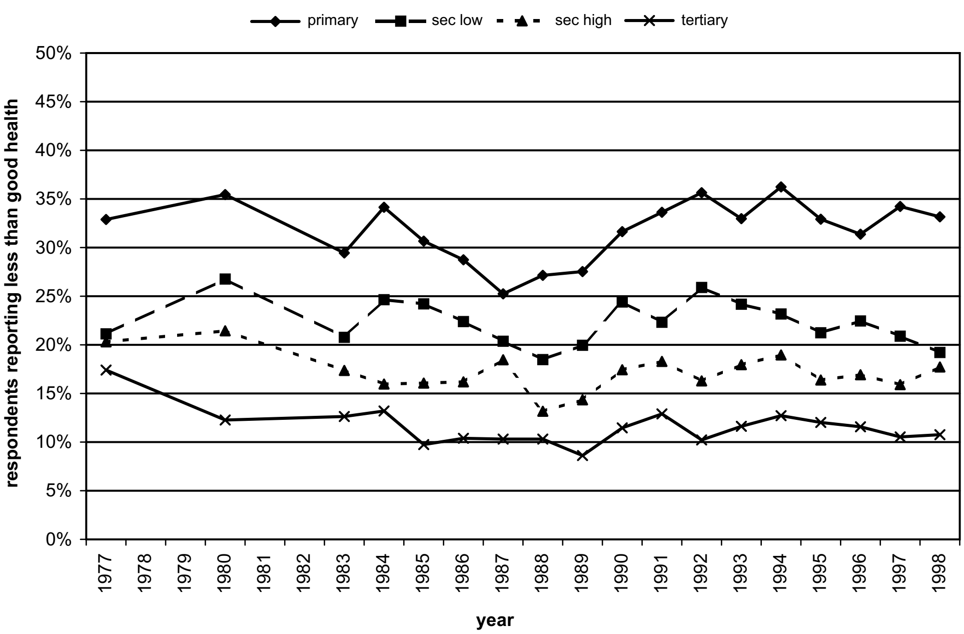

Fig. 1. Percentage of men reporting poor health by educational level, 1977–1998 (controlled for age, marital status and urbanisation).

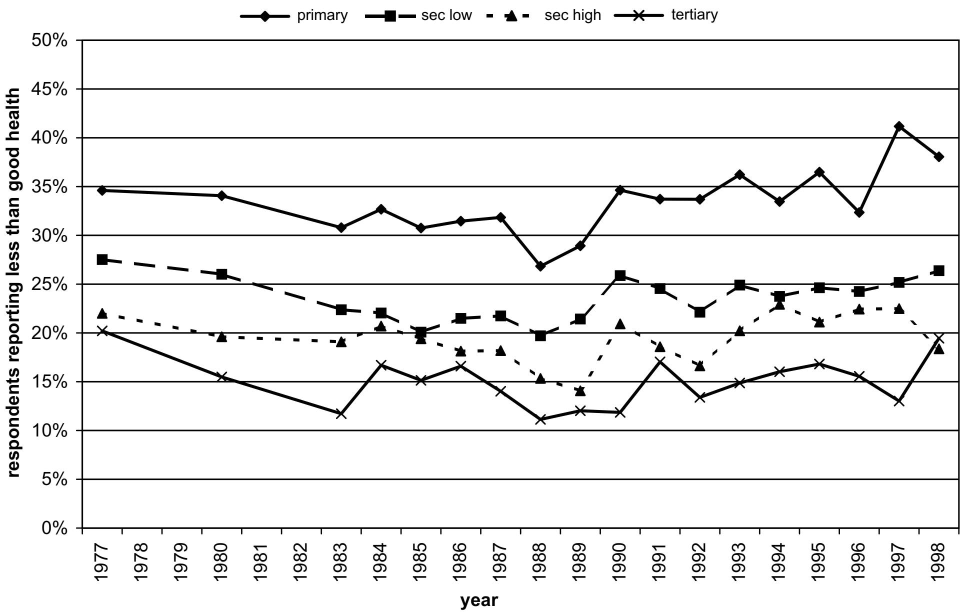

Fig. 2. Percentage of women reporting poor health by educational level, 1977–1998 (controlled for age, marital status and urbanisation).

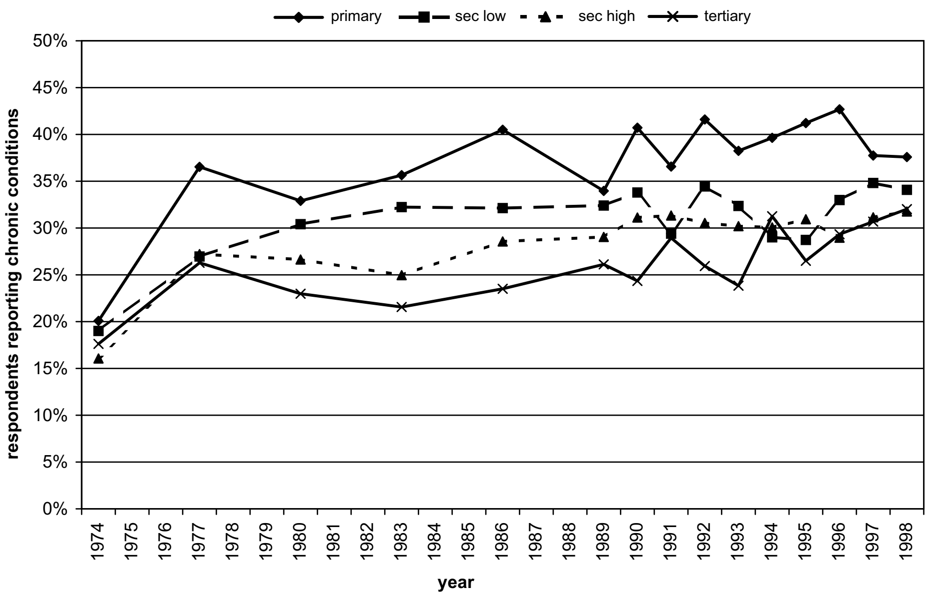

Fig. 3. Percentage of men reporting one or more chronic conditions by educational level, 1974–1998 (controlled for age, marital status and urbanisation).

Logistic regression: period and cohort effects

To test our hypotheses we perform logistic regression analysis. Model A contains all individual characteristics at the time of survey but no contextual time-dependent effects (period and cohort). This baseline model shows the overall educational differences in health. To test the hypothesis on the main effects of the time-dependent variables, in Model B period is added and in Model C infant mortality in year of birth. Finally, Model D is the full-blown model with period as well as cohort effects, and the interactions between these time-dependent variables and educational level. This model provides a test for specific developments in social health inequality (convergence versus divergence).

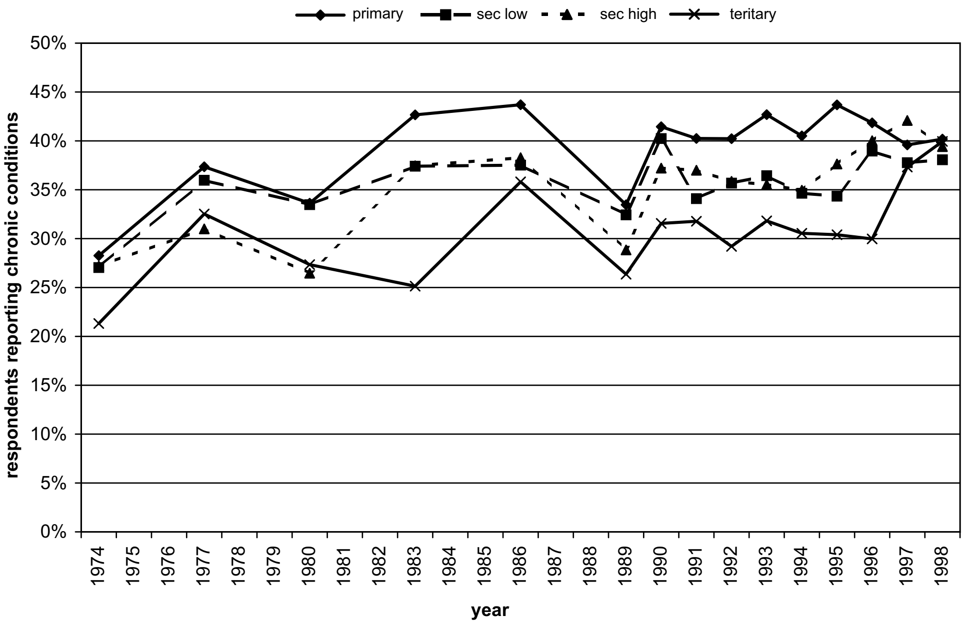

Fig. 4. Percentage of women reporting one or more chronic conditions by educational level, 1974–1998 (controlled for age, marital status and urbanisation).

Tables 1 and 2 present the results for Model A for less than good health and chronic conditions, respectively. We observe the usual patterns for the dependent variables. Most importantly, educational differences are reproduced as expected. In Model A, the odds for reporting less than good health are more than twice as high as for respondents who attained a primary school diploma compared to those with a tertiary diploma. The odds ratio decreases with higher educational attainment. Also, we can observe that the social inequality in health is somewhat more pronounced for men than for women for both health indicators. For both men and women, educational differences are stronger for reporting poor health (Table 1) than they are for chronic conditions (Table 2).

If we look closer at the pattern over the age categories, we observe that there is an almost perfect linear association for general health; respondents aged 25–29 are in better health than any other age group. The pattern for men and women is similar, yet Table 2 suggests age effects are stronger for men than for women. Overall, women are more likely to report less than good health and chronic conditions (not shown in the tables). Marital status has a stronger effect for women than for men. Divorced respondents have a higher chance of reporting bad health for both genders. Women and men who have children at home report better health than respondents without children at home. As expected, higher household income increases the chances on good health.

In Table 3, we report the results of Model B for both health indicators. For reasons of presentation, we left out the control variables of Model A. In general, the odds ratios for the controls only change marginally, they do not change in significance or pattern. Model B provides a test for main effects of period on trends in health. In the left-hand panels of Table 3, the parameter estimates show a non-significant trend towards better health for men and a significant trend towards poorer health for women. So, of our two competing hypotheses on trends, the one predicting that people will report more health problems over time is supported for women. Concerning chronic conditions we observe that, controlled for age, persons increasingly report one or more conditions as time goes by. Both men and women experience this negative trend due to period effects. The odds of reporting at least one chronic condition are about 1.29 (men) and 1.45 (women) times higher in 1998 compared to 1974.

Next, we add to our model infant mortality in year of birth[10] (Table 4). The results of Model C for self-

[10] The correlation between period and infant mortality in year of birth is 0.36. Age and infant mortality in year of birth correlate 0.81 and 0.82 for men and women respectively. Although these correlations are not problematic due to the

Table 1
Odds ratios (and 95% CI) for the effect of educational level, age and control variables on self-reported poor health, for men and women: Model A[a]

	Men	Women
Primary education	**2.40** (2.20–2.61)	**2.29** (2.11–2.49)
Secondary low	**1.72** (1.58–1.86)	**1.52** (1.40–1.65)
Secondary high	**1.35** (1.25–1.45)	**1.28** (1.18–1.38)
Tertiary education (ref)	**1.00**	**1.00**
Age 25–29 (ref)	**1.00**	**1.00**
Age 30–34	**1.45** (1.29–1.63)	**1.28** (1.17–1.42)
Age 35–39	**1.94** (1.73–2.18)	**1.57** (1.42–1.73)
Age 40–44	**2.55** (2.27–2.87)	**2.15** (1.95–2.37)
Age 45–49	**3.60** (3.21–4.04)	**2.68** (2.43–2.96)
Age 50–54	**4.79** (4.27–5.38)	**3.03** (2.75–3.35)
Age 55–59	**5.69** (5.07–6.39)	**3.01** (2.72–3.33)
Age 60–64	**5.75** (5.12–6.47)	**3.44** (3.11–3.81)
Age 65–69	**4.87** (4.30–5.52)	**3.65** (3.29–4.06)
Age 70–74	**5.26** (4.61–6.00)	**3.95** (3.53–4.42)
Married (ref)	**1.00**	**1.00**
Widow	0.96 (0.84–1.10)	0.93 (0.86–1.00)
Divorced	**1.16** (1.04–1.29)	**1.42** (1.30–1.54)
Single	0.94 (0.87–1.02)	**1.23** (1.14–1.32)
Household income	**0.68** (0.66–0.71)	**0.80** (0.78–0.82)
Children at home	**0.94** (0.84–0.99)	**0.86** (0.82–0.91)
N	55,562	58,718

[a] Also in this model: urbanisation in three categories, a dummy for imputed income and a dummy indicating survey type (NHIS vs. LCS/CLCS). Data are weighted for age, gender, and marital status. Significant effects are printed bold.

Table 2
Odds ratios (and 95% CI) for the effect of educational level, age and control variables on self-reported chronic conditions, for men and women: Model A[a]

	Men	Women
Primary education	**1.53** (1.40–1.67)	**1.30** (1.19–1.42)
Secondary low	**1.18** (1.09–1.28)	**1.14** (1.05–1.23)
Secondary high	**1.12** (1.04–1.20)	**1.20** (1.11–1.30)
Tertiary education (ref)	**1.00**	**1.00**
Age 25–29 (ref)	**1.00**	**1.00**
Age 30–34	**1.28** (1.14–1.43)	**1.28** (1.16–1.40)
Age 35–39	**1.60** (1.43–1.79)	**1.36** (1.24–1.50)
Age 40–44	**1.84** (1.64–2.06)	**1.48** (1.34–1.64)
Age 45–49	**2.26** (2.02–2.54)	**1.94** (1.75–2.14)
Age 50–54	**2.63** (2.34–2.96)	**2.18** (1.97–2.42)
Age 55–59	**3.51** (3.12–3.96)	**2.53** (2.29–2.83)
Age 60–64	**3.62** (3.21–4.08)	**2.96** (2.65–3.31)
Age 65–69	**3.89** (4.42–5.43)	**3.34** (2.98–3.76)
Age 70–74	**4.29** (3.72–4.95)	**4.36** (3.83–4.96)
Married (ref)	**1.00**	**1.00**
Widow	1.05 (0.89–1.24)	1.01 (0.92–1.11)
Divorced	**1.12** (1.00–1.26)	**1.26** (1.14–1.38)
Single	**0.86** (0.79–0.93)	0.98 (0.90–1.06)
Household income	**0.92** (0.90–0.95)	**0.91** (0.88–0.94)
Children at home	**0.92** (0.86–0.97)	**0.94** (0.89–0.99)
N	34,178	36,204

[a] Also in this model: urbanisation in three categories, a dummy for imputed income and a dummy indicating survey type (NHIS or LCS/CLCS). Data are weighted for age, gender, and marital status. Significant effects are printed bold.

reported poor health show that, for males, there is no trend due to either period or cohort effects. We expected a positive effect of decreasing infant mortality in year of birth on health. For women we indeed find a trend towards better health due to the cohort effect: lower infant mortality in year of birth makes for better health. Comparing Models B and C shows that the period effect on poor health is underestimated for women if infant mortality is not controlled for. Infant mortality in year of birth does not affect a person's chronic conditions. Adding the cohort indicator hardly changes the conclusions from Model B concerning the period effect. Again, the period effect for women was slightly underestimated in Model B.

Subsequently, we turn to the analyses on the differences in the effects of period and infant mortality for the four educational groups. This is the test for hypotheses on trends in social inequality in health. Table 5 presents the odds ratios for the main effects of period and infant mortality and the interaction effects of period and infant mortality with the educational groups (the highest group is the reference category). Again, for reasons of presentation, we do not report the baseline variables of Model A. We have predicted a (slight) increase in the social inequality in health measured by an interaction of a person's educational group with period. Table 5 shows that there is no support for our hypothesis. Respondents in all educational groups are equally affected by period for both health indicators.

(*footnote continued*)
large number of cases, we checked possible multi-collinearity in two ways. We examined the results for random sub-samples and sub-samples randomly leaving out birth cohorts. Subsequently, we followed Belsley's (1991) recommendation to re-estimate the model after adding small perturbations to suspected variables. We used the SPSS Macro PERTURB (http://www.sls.wau.nl/bk/bedrijfskunde/jhendrickx/spss/perturb/perturb.html, more information J. Hendrickx, Management Studies Group, Wageningen UR, Hollandseweg 1, 6706 KN Wageningen, the Netherlands) to re-estimate our model one thousand times with perturbations in age, infant mortality in year of birth and period. We obtained stable coefficients.

Table 3
Odds ratios (and 95% CI) for the effect of educational level and period on self-reported poor health and chronic conditions, controlled for individual characteristics, for men and women: Model B[a]

	Poor health		Chronic conditions	
	Men	Women	Men	Women
Primary education	**2.38** (2.19–2.59)	**2.33** (2.14–2.53)	**1.55** (1.42–1.70)	**1.33** (1.22–1.46)
Secondary low	**1.71** (1.57–1.85)	**1.54** (1.42–1.66)	**1.20** (1.11–1.30)	**1.16** (1.07–1.25)
Secondary high	**1.34** (1.25–1.45)	**1.28** (1.19–1.38)	**1.11** (1.04–1.20)	**1.20** (1.11–1.30)
Tertiary education	1.00	1.00	1.00	1.00
Period	0.98 (0.96–1.00)	**1.04** (1.02–1.06)	**1.07** (1.04–1.10)	**1.10** (1.07–1.13)

[a] All variables from Model A are included in the model. Data are weighted for age, gender and marital status. Significant effects are printed bold.

Table 4
Odds ratios (and 95% CI) for the effect of educational level, period and life expectancy at birth on self-reported poor health and chronic conditions, controlled for individual characteristics, for men and women: Model C[a]

	Poor health		Chronic conditions	
	Men	Women	Men	Women
Primary education	**2.39** (2.19–2.60)	**2.37** (2.19–2.59)	**1.55** (1.42–1.70)	**1.33** (1.22–1.46)
Secondary low	**1.70** (1.57–1.85)	**1.56** (1.44–1.69)	**1.20** (1.11–1.30)	**1.16** (1.07–1.26)
Secondary high	**1.34** (1.24–1.44)	**1.30** (1.20–1.41)	**1.11** (1.04–1.20)	**1.20** (1.11–1.30)
Tertiary education	1.00	1.00	1.00	1.00
Period	0.99 (0.97–1.02)	**1.06** (1.04–1.09)	**1.07** (1.04–1.10)	**1.12** (1.08–1.16)
Infant mortality in year of birth	1.02 (1.00–1.04)	**1.03** (1.01–1.05)	1.00 (0.97–1.02)	1.02 (1.00–1.04)

[a] All variables from Model A are included in the model. Data are weighted for age, gender and marital status. Significant effects are printed bold.

Table 5
Odds ratios (and 95% CI) for the effect of educational level, period, life expectancy at birth and interactions on self-reported poor health and chronic conditions, controlled for individual characteristics, for men and women: Model D[a]

	Poor health		Chronic conditions	
	Men	Women	Men	Women
Primary education	**2.50** (2.24–2.79)	**2.43** (2.17–2.72)	**1.62** (1.45–1.82)	**1.38** (1.22–1.56)
Secondary low	**1.74** (1.57–1.93)	**1.59** (1.43–1.78)	**1.22** (1.10–1.35)	**1.23** (1.10–1.38)
Secondary high	**1.36** (1.22–1.50)	**1.30** (1.15–1.45)	**1.14** (1.03–1.27)	**1.21** (1.07–1.36)
Tertiary education	1.00	1.00	1.00	1.00
Period	1.01 (0.95–1.08)	**1.09** (1.01–1.17)	**1.09** (1.03–1.15)	**1.15** (1.07–1.24)
by primary education	0.94 (0.92–1.06)	0.98 (0.91–1.06)	0.97 (0.91–1.04)	0.98 (0.90–1.05)
by secondary low	0.97 (0.90–1.04)	0.97 (0.89–1.04)	0.97 (0.92–1.04)	0.95 (0.88–1.02)
by secondary high	0.98 (0.91–1.05)	1.00 (0.92–1.08)	0.98 (0.92–1.04)	1.01 (0.93–1.09)
by tertiary education	1.00	1.00	1.00	1.00
Infant mortality in year of birth	**1.10** (1.06–1.14)	**1.06** (0.84–1.15)	1.02 (0.98–1.06)	1.01 (0.97–1.05)
by primary education	**0.89** (0.86–0.93)	**0.96** (0.93–0.99)	**0.96** (0.92–0.99)	1.01 (0.97–1.05)
by secondary low	**0.93** (0.89–0.96)	0.97 (0.93–1.00)	0.97 (0.93–1.01)	1.00 (0.96–1.04)
by secondary high	**0.95** (0.92–0.99)	0.98 (0.95–1.02)	1.01 (0.97–1.05)	1.02 (0.98–1.07)
by tertiary education	1.00	1.00	1.00	1.00

[a] All variables from Model A are included in the model. Data are weighted for age, gender and marital status. Significant effects are printed bold.

For self-reported poor health we observe significant interactions of infant mortality in year of birth and educational level. First, note that the main effect of infant mortality in year of birth now should be interpreted as the effect of infant mortality for respondents with a tertiary diploma. The three interaction terms show whether the other educational levels significantly differ from the tertiary group. For men, Table 5 shows that the health status of respondents holding tertiary education has increased over the cohorts. However, for men with only primary education a decrease in infant mortality raises their chance to report poor health. So, contrary to our hypothesis the lowest and highest educated men have grown more apart over the cohorts. We will come back to this in the conclusion and discussion section. For men with lower and higher secondary education, health status increases over the cohort as it does for tertiary men, but it does so more slowly and thus the differences increase over time. The positive development for the higher educated groups and the negative development for the lowest group explain why, in Model C, we did not observe a main effect of infant mortality in year of birth. The results for women show an almost similar pattern; higher educated women have benefited more from decreased infant mortality in year of birth than lower educated women. But unlike men, all women experienced a positive effect of the decreased infant mortality on health. However, this cohort effect on health was much stronger for higher educated women (odds ratio of 1.06) than it was for women of the lowest educational group (odds ratios of 1.02). The middle two groups do not differ significantly from women with tertiary education.

With regard to chronic conditions, we only find a significant interaction between educational level and infant mortality for men. The effect of infant mortality in year of birth is absent for the three highest educational groups, but for men with primary education the chance to report chronic conditions has increased over the cohorts. This is in line with the finding on self-reported health. Higher and lower educated women do not differ in the effect that infant mortality in year of birth has on their chances to report chronic conditions.

Conclusion and discussion

Our aim was to test whether cohort specific experiences affect trends in health and trends in social inequality in health. By introducing the distinction between age, period and cohort effects, we investigated the influence of the macro-context during childhood next to the current macro-context. We specified cohort effects theoretically. By doing so, we were able to go beyond the question whether cohort effects exist or not, and test whether life expectancy at birth is a meaningful explanation for cohort differences.

The results of this paper suggest four more general conclusions. First, we have shown that for men self-reported poor health in the Netherlands has been more or less stable over two decades, suggesting a trendless fluctuation. For women there has been an increase in self-reported poor health. The same holds true for both sexes with regard to chronic conditions. Second, adding cohort specific experiences to a model including just age and period effects was only relevant for women's self-reported poor health. Women who are born in cohorts with a lower infant mortality in year of birth report better health irrespective of their age and survey year compared to women who were born in years with higher infant mortality. The period effect initially found (i.e. in a model without cohort) appears to be slightly underestimated. Third, we found no trends in social inequalities in health due to period effects. Fourth, our results show some trends in social inequality due to cohort specific experiences. Lower infant mortality in year of birth has a positive effect on women's general health. However, this positive effect is much stronger for higher educated women than for lower educated women. Among men, the higher educated experience a positive effect of decreasing infant mortality in year of birth on general health whereas the lowest educated group is negatively affected. Concerning chronic conditions the difference between men with only primary education and higher educated men increased over time because the lowest group experienced a negative effect of decreasing infant mortality whereas the other groups were unaffected. We found no such differences between female educational groups.

Before further elaborating on the results, three limitations of this study should be mentioned. The first limitation is that we did not specify the *period effect* with theoretical indicators. This makes it impossible to assess the effect of different and possibly divergent macro-social developments. However, in the absence of theoretical arguments to divide the period 1974–1998 into sub-periods or to otherwise define the period effect, we chose to model a linear period effect. With respect to period, we are primarily interested in the effect of time passing by and choosing a linear effect is a practical choice; it makes our models more parsimonious.

Secondly, we had to deal with the *comparability* of the surveys. We cannot rule out that slightly different question formulations and answer categories or survey procedures caused small distortions. The important question, however, is whether the changes in question formulation have influenced our results and conclusions. This seems unlikely. In our choice of health indicators and surveys we have been rather conservative. Still, we plea for great care in changing questions or procedures in repetitive surveys.

Thirdly, the results might be biased by *non-response*. The level of non-response has increased over the years (from 34% in the 1980s to 42% in the 1990s). It is generally assumed that there is some response selection over time on independent variables, such as income and educational level. This will not influence our conclusions. In our case, divergent response selection over survey years with respect to health *within* educational levels would cause serious bias. There is no evidence that this kind of selectivity has taken place, nor are there convincing reasons to expect such patterns in non-response selection (for the relationship between health and non-response in a Dutch postal health survey see Mackenbach et al., 1994). Partial non-response has been very stable for most variables. These conclusions can be drawn from the appendix. In cases where there were large changes we were not able to link them to changes in the prevalence rates of health problems in a structural way.

Our study is a first indication of the importance of cohort specific experiences. More research is needed to support the significance of our results, both for the Netherlands and other countries. We think theoretical progress has been made in this study by specifying a cohort effect instead of choosing a methodological approach to the identification problem in age-period-cohort analyses. We were able to test a meaningful hypothesis on the cohort effect of decreasing infant mortality in year of birth. If possible the difference between social context and individual exposure needs to be addressed in future research. In our study, respondents from one birth year are all modelled to have been exposed to the same environment. If we link exposure to ever smaller social units, from country (as in this study) via region and neighbourhood to the family of origin, we move from exposure as context effects to individual exposure effects. The question arises to what extent high level contexts still affect health later in life if we control for the differences in individual exposure since the encountered macro-effects might be composition effects. On national or regional level educational chances or income inequality may be important, on the regional or neighbourhood level the provisions and quality of health care may have an effect, and on the family or individual level smoking may be what matters.

The sub-title of this article asked: does infant mortality in year of birth as a context matter? We can answer: yes, infant mortality in year of birth as a context matters for the inequality in self-reported poor health later in life. However, we found an effect contrary to our expectation. Decreased infant mortality in year of birth makes for a stronger impact of educational differences on self-reported poor health. It may well be that there are in fact two developments taking place. On the one hand, there might by convergence between educational groups because of an overall rise in health, and, on the other hand, there might by divergence because the ever smaller group of lower educated becomes more selective. In older cohorts the lowest group was more heterogenous in terms of cognitive abilities and possibly physical abilities as well. As the chances to obtain secondary and tertiary education have grown and have become less dependent on family background, the lower educated group has become a more select and homogenous group of the truly disadvantaged. These two developments have taken place more or less simultaneously. If the latter has had a much stronger impact this may explain why our hypothesis was not supported by the data for self-reported poor health. We invite other scholars to further unravel the social gradient in health by testing new hypotheses on period and cohort effects for the Netherlands and other countries.

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Appendix

Infant mortality (per 1000 live births) by year of birth (1905–1973) is shown in Fig. 5 and used data sets in Table 6.

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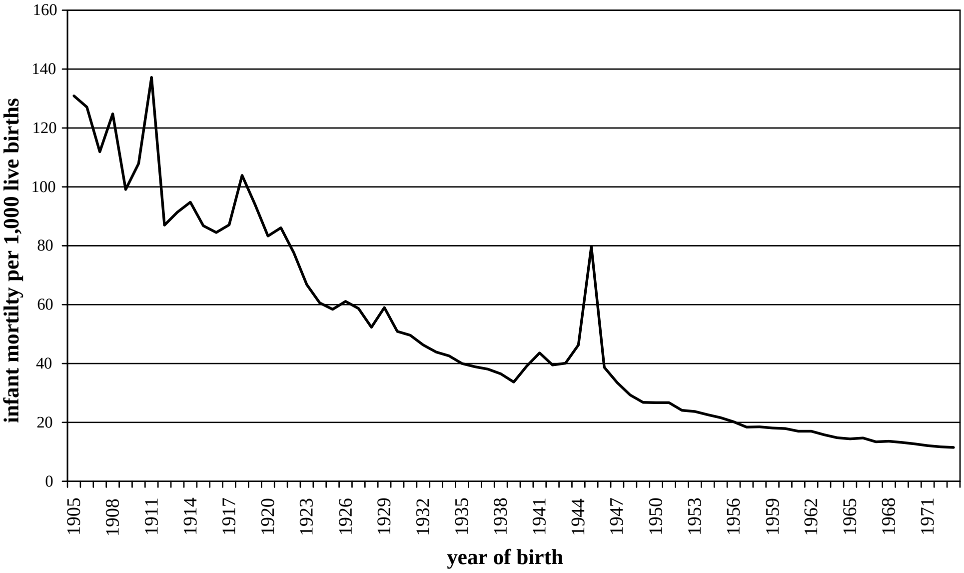

Fig. 5. Infant mortality (per 1000 live births) by year of birth (1905–1973).

Table 6
Used data sets

	Name	Year	A	B	C	D	E	F
1.	Living Conditions Survey	1974	3296	72.0	10.5	—	0.00	2.85
2.	Living Conditions Survey	1977	2801	70.3	12.5	0.50	0.00	2.57
3.	Living Conditions Survey (CBS)	1980	2136	64.0	13.2	0.61	—	5.06
4.	Living Conditions Survey (SCP)	1980	2059	61.0	13.6	0.19	0.00	2.53
5.	Health Interview Survey	1983	4840	64.5	14.1	0.17	0.33	3.22
6.	Living Conditions Survey	1983	2758	58.0	12.6	0.04	—	3.77
7.	Health Interview Survey	1984	5006	63.4	14.4	0.12	—	2.36
8.	Health Interview Survey	1985	4822	63.4	15.3	0.21	—	2.90
9.	Living Conditions Survey	1986	2776	57.0	17.4	2.74	0.29	4.18
10.	Health Interview Survey	1986	4868	63.8	16.0	0.12	—	3.49
11.	Health Interview Survey	1987	4470	59.0	15.7	0.18	—	3.69
12.	Health Interview Survey	1988	4286	58.3	17.4	0.00	—	3.24
13.	Health Interview Survey	1989	4459	58.5	18.1	0.00	0.00	2.33
14.	Health Interview Survey	1990	4070	56.3	19.2	0.05	0.02	2.31
15.	Health Interview Survey	1991	3872	56.7	16.0	0.00	0.03	2.20
16.	Health Interview Survey	1992	4890	56.7	17.4	0.00	0.04	0.20
17.	Continuos Living Conditions Survey	1993	4128	46.0	20.4	0.00	0.00	3.49
18.	Health Interview Survey	1993	4693	55.0	18.6	0.00	0.04	0.19
19.	Continuos Living Conditions Survey	1994	2413	52.0	19.3	0.04	—	3.90
20.	Health Interview Survey	1994	5032	56.1	18.1	0.02	8.15	0.62
21.	Continuos Living Conditions Survey	1995	2906	52.0	21.2	0.03	—	3.99
22.	Health Interview Survey	1995	5246	58.6	19.1	0.06	6.88	0.74
23.	Continuos Living Conditions Survey	1996	2805	52.0	21.1	0.04	—	3.10
24.	Health Interview Survey	1996	5056	56.6	18.2	0.02	11.12	0.59
25.	Health Interview Survey	1997	5928	59.4	19.8	0.02	19.52	2.31
26.	Health Interview Survey	1998	4929	58.1	20.8	0.00	20.59	2.78

A = number of respondents in the analyses. B = response rate (the overall decrease in response rates is to a large extent due to an increase in people who can not be reached or do not speak Dutch, and to a smaller extent due to an increase in refusals). C = percentage respondents with tertiary education. D = percentage missing self-rated poor health. E = percentage missing chronic conditions. F = percentage missing independent variables.

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