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Did Socioeconomic Inequalities in Morbidity and Mortality Change in the United States over the Course of the Twentieth Century?*

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Abstract

In this article we present two sets of empirical analyses that consider the extent to which socioeconomic gradients in self-assessed health and child mortality changed since the beginning of the twentieth century in the United States. This empirical issue has important and wide-ranging research and policy implications. In particular, our results speak to the value of considering the role of broader social, economic, and political inequalities in generating and maintaining socioeconomic disparities in morbidity and mortality. Despite dramatic declines in morbidity and mortality rates in the United States across the twentieth century, we find that socioeconomic-status gradients in morbidity and mortality declined only modestly (if at all) during that period.

For nearly 200 years, researchers have recognized inverse relationships between socioeconomic status (SES) and aggregate morbidity and mortality rates (Antonovsky 1967; Chaplin 1924; Coombs 1941; Kawachi and Kennedy 2002; Link and Phelan 2004; Robert and House 2003; Villerme 1840; Virchow 1848). Over those 200 years, the United States and other developed countries have also witnessed dramatic declines in overall morbidity and mortality rates. In this article, we ask whether the overall declines in morbidity and mortality rates in the United States across the twentieth century were accompanied by reductions in the SES gradients in these outcomes. As we describe below, there is relatively little evidence to suggest that SES gradients in morbidity and mortality declined in the United States across the twentieth century. This straightforward empirical question has important and wide-ranging research and policy implications.

BACKGROUND

Biomedical and public health research has typically sought to explain socioeconomic disparities in morbidity and mortality rates by looking closely at the downstream or proximate factors that link socioeconomic position to these outcomes, including biomedical, psychosocial, behavioral, and physiological mechanisms (Berkman and Macintyre 1997; Deaton 2002; Marmot, Kogevinas, and Elston 1987; McKinley and Marceau 2000; Robert and House 2003). These efforts have clearly led to a better understanding of the ways in

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which the effects of social, economic, behavioral, and psychological factors are mediated by biological processes and behaviors to create health inequalities (McKinlay and McKinlay 1977; Robert and House 2003).

In recent years, some research has moved toward contextualizing these downstream or proximate mechanisms within the framework of upstream or macro-social factors, such as education, occupation, income, and other environmental factors (Hayward et al. 2000; Link and Phelan 1995; Lutfey and Freese 2005; Mackenbach et al. 2003; Marmot et al. 1987; Robert and House 2003; Wilson 2001). The result is a growing interest in conceptualizing SES as a "basic cause" (Lieberson 1985) of morbidity and mortality and a reassessment of the long-term efficacy of exclusively focusing on the proximate mechanisms that link SES to morbidity and mortality. Downstream approaches are not inherently opposed to perspectives that focus on upstream factors; both are located on a spectrum of theoretical and practical approaches to understanding health inequalities (Berkman and Macintyre 1997; McKinley and Marceau 2000; Robert and House 2003). Understanding this spectrum of mechanisms associated with health inequalities has important implications for research, policy, and public health interventions.

Perhaps the most prominent example of an approach to understanding the spectrum of upstream and downstream mechanisms associated with SES disparities in morbidity and mortality is Link and Phelan's (1995, 1996) theory of "fundamental social causes." This theory begins with the observation that the mechanisms or risk factors that account for socioeconomic gradients in morbidity and mortality rates change over time. In the early twentieth century, issues like sanitation, water quality, and food safety may have been the key mechanisms linking SES and health, but now researchers focus more on factors like smoking, obesity, and access to health insurance. Despite changes over time in the intervening mechanisms, these authors contend that the associations between SES and aggregate morbidity and mortality rates have nonetheless persisted. Link and Phelan (2000) note that resources associated with SES help individuals avoid exposure to deleterious health effects:

> [T]he reason SES has been so consistently associated with disease is that it embodies resources like knowledge, money, power, and prestige that can be used in different ways in different situations to avoid risks for disease and death (Link and Phelan, 1995, 1996). People who are relatively better off use their advantage to avoid risks and to adopt protective strategies that enhance health and well-being no matter what the risk and protective factors happen to be at a given point in time. (P. 39)

Lutfey and Freese (2005) explain further:

> [Link and Phelan's] fundamental cause concept implies not a theory of the *specific proximate mechanisms* responsible for a persistent association, but rather that some *metamechanism(s)* is responsible for how specific and varied mechanisms are continuously generated over historical time in such a way that the direction of the enduring association is preserved…|. If an explanatory variable is a fundamental cause of an outcome, then the association cannot be successfully reduced to a set of

> more proximate, intervening causes because the association *persists* even while the relative influence of various proximate mechanisms *changes*. (P. 1327–28)

As exemplified by the two quotations above, research that argues for contextualizing the more proximate mechanisms that link SES to health within the framework of broader social and economic inequalities is frequently motivated by a striking empirical claim: Despite dramatic long-term improvements in public health, changes in the types of diseases that are most prevalent, and changes in health risk factors, the magnitude of socioeconomic disparities in morbidity and mortality rates has persisted for decades or longer. Indeed, Link and Phelan (1996) orient their theory to account for "enduring associations between socio-demographic factors and disease" (p. 472).

In fact, as we review below, there is relatively little systematic evidence to support (or refute) the claim that the magnitude of socioeconomic disparities in morbidity and mortality rates in the United States has persisted over the long term. Consequently, in this article we ask how socioeconomic gradients in morbidity and mortality changed in the United States over the course of the twentieth century. Our goal is to provide empirical evidence that documents long-term SES gradients in health inequalities. We then discuss the role of traditional downstream efforts to improve morbidity and mortality within the context of this empirical evidence.

What relevance does this empirical question have for research and practice? We argue that if socioeconomic inequalities in morbidity and mortality rates did not decline in the United States over the last century, then there is greater motivation for contextualizing downstream proximate mechanisms that link SES to health within the framework of upstream socioeconomic inequalities. Although traditional public health and biomedical research has made major advances in reducing overall morbidity and mortality rates, it may have had little or no impact on SES inequalities in those rates. Indeed, reducing socioeconomic inequalities in those rates has not typically been an explicit goal of such research. On the other hand, if SES inequalities in morbidity and mortality rates are on the wane, then there is less urgency for researchers to contextualize their research within the framework of broader social and economic inequalities. We intend our research to help shape future research and policies related to health inequalities.

PREVIOUS EVIDENCE

Researchers in the United States consistently find that associations between *income* and mortality rates have either remained steady (Duleep 1989) or have increased since the 1960s (Duncan 1996; Pappas et al. 1993), particularly for men. Likewise, since at least 1960 *educational* gradients in aggregate mortality rates have either held steady (Duleep 1989) or increased (Crimmins and Saito 2001; Lauderdale 2001; Lynch 2003; Pappas et al. 1993). Although most U.S. research operationalizes SES in terms of income or educational attainment, there are exceptions. Steenland, Hu, and Walker (2004), for example, found that mortality differences by *occupation* increased for men between 1984 and 1997.

One limitation of most research on U.S. trends in SES inequalities in aggregate mortality or morbidity rates is the limited time horizon. Most research builds on the Matched Record

Study of 1960 (Kitagawa and Hauser 1973), which matched death certificates registered between May and August of 1960 to records from the 1960 U.S. Census. While much of this research includes individuals who were born across the span of the twentieth century, their analyses are typically not organized in such a way as to model changes across birth cohorts in age-group-specific SES inequalities in mortality or morbidity rates (e.g., Pappas et al. 1993; Preston and Elo 1995), and their findings are thus restricted to changes in health disparities since 1960. Two exceptions include the work of Lauderdale (2001) and Lynch (2003), who each draw conclusions about how age-group-specific educational inequalities in morbidity and mortality rates changed over the course of the twentieth century; both authors found increasing health disparities by education across birth cohorts. However, as discussed below, it is worth wondering whether these findings may conflate changes across the twentieth century in the extent to which education is an important component of socioeconomic status with real changes across birth cohorts in socioeconomic disparities in morbidity and mortality rates.

While research on SES gradients in aggregate morbidity and mortality rates in the United States typically operationalizes SES in terms of income or education, such research in Great Britain typically operationalizes SES in terms of the Register-General's Social Class scheme (which is fundamentally an occupational classification; Marmot et al. 1987). British researchers using this social class scheme have almost uniformly concluded that SES gradients in mortality have remained stable or increased since at least World War II (Antonovsky 1967; Black et al. 1982; Marang-van de Mheen et al. 1998; Wilkinson 1986; Williams 1990). Mackenbach et al. (2003) provide similar evidence for England and Wales, Italy, Finland, Sweden, Norway, and Denmark in the 1980s. Although some researchers have considered British data from as early as the 1920s (Koskinen 1985; Pamuk 1985), pre–World War II trends in Western European SES gradients in mortality or morbidity are not well understood. In the end, the bulk of evidence from Western Europe is subject to the same limited time horizon as most of the U.S. evidence.

CONCEPTUAL ISSUES IN MEASURING SOCIOECONOMIC STATUS

Beyond the limited time horizon of most research on trends in socioeconomic disparities in morbidity and mortality rates, much of this research suffers from inadequate conceptualizations of SES. To be sure, a number of observers have carefully explicated the theoretical and practical issues involved in operationalizing and measuring SES for use in health research (Oakes and Rossi 2003), sociological research (Hauser and Warren 1997), and elsewhere. Beyond the overarching need to conceptualize and measure SES with the same care that typically goes into the measurement of morbidity and mortality, research in this area also faces three additional challenges.

First, at a minimum researchers need to measure both SES and mortality or morbidity in a strictly consistent manner over time. The basic structures of the U.S. educational or occupational systems have not changed dramatically in the last several decades (although the distribution of people within those structures has changed); nor has the basic racial classification system changed. Therefore, at first glance it seems possible to use consistently measured indicators of education, income, occupation, or race/ethnicity as proxies for SES in studying long-term trends in the association between SES and aggregate mortality or morbidity rates in the United States.

Second, even if researchers measure SES in a technically consistent manner over time, the social meaning and consequences of some components of SES have likely changed in important ways over time. For example, in the early twentieth century people without formal educational credentials could still be socially and economically successful through agriculture, skilled blue-collar work, or other means. In the late twentieth century, people without formal educational credentials face severely dimmer prospects for socioeconomic success. This change over time in the extent to which educational attainment reliably signals socioeconomic success raises real questions about how to interpret findings that educational disparities in morbidity and mortality rates increased in the United States across the twentieth century (Lauderdale 2001; Lynch 2003). It may be that socioeconomic disparities in morbidity and mortality remained constant and that education became a better indicator of socioeconomic status over time.

A third measurement challenge faced by researchers seeking to operationalize SES in analyses of trends over time in SES-health relationships stems from a debate about whether absolute or relative measures of SES have greater causal impact on morbidity and mortality. Essentially, this debate concerns whether absolute material deprivation (for instance, not having enough money to purchase adequate food or medical care) or relative deprivation (for instance, having an income that falls well below the median income in a society) matters most for morbidity and mortality. We recognize that there are both theoretical and practical implications of this debate (Deaton 2002; Lynch et al. 2004; Marmot 2003; Wagstaff and Doorslaer 2000; Wilkinson 1997), but the aim of this research is not to resolve the debate between absolute and relative measures. We seek to understand how both absolute and relative deprivation are associated with morbidity.

Our approach to dealing with these measurement issues is to use a variety of indicators of SES in our analyses. All of our SES indicators are measured in a technically consistent manner over time. While some of them might have changed in their conceptual meaning over time, other measures that we use arguably mean the same thing over time. Finally, we utilize two relative measures and two absolute measures of SES in our analyses.

We proceed with our empirical analyses in two parts. In Part I we model the impact of four measures of SES on self-reported overall health using data that include respondents between the ages of 18 and 89 who were born between 1883 and 1986. In Part II we estimate and compare occupation group–specific child-mortality rates in 1910 and in the 1990s. Together, the two parts provide evidence about whether and in what way SES gradients in morbidity and mortality changed in the United States during the twentieth century. As described below, both parts of our analyses are limited in certain respects; neither analysis is based on ideal data or measures. Nonetheless, we contend that, taken together, the two sections of our analyses provide valuable empirical information that speaks to broader policy and research issues.

PART I

Data and Methods

We begin with analyses of data from the 1972 through 2004 General Social Surveys (GSS). The GSS is administered by the National Opinion Research Center (NORC) at the University of Chicago. This multipurpose survey of members of households across the United States has been conducted annually between 1972 and 1994 (except in 1979, 1981, and 1992) and biennially since then (Davis, Smith, and Marsden 2005). Each survey year, NORC selects a fresh cross-sectional random sample of Americans living in households. Prior to 1994, the GSS was conducted with about 1,500 respondents each year; beginning in 1994, NORC began interviewing approximately 3,000 people biennially. Respondents vary in age from 18 to 89. The content of the interviews ranges broadly, covering such areas as attitudes about a variety of social issues, questions about socioeconomic and demographic characteristics, and a variety of behavioral measures. Core questions—including those at the heart of our analyses—have been asked the same way in each year in order to facilitate comparisons over time (Davis et al. 2005). Table 1 includes descriptive statistics for our measures of SES, self-assessed overall health, age, and year of birth. This table also indicates the years in which particular survey items were administered.

In all survey years, NORC ascertained each respondent's age at the time of their interview. Using this information, we have computed each respondent's year of birth. Because we have a wide age range in each survey year and because we have more than three decades of cross-sectional survey data, respondents' years of birth range from 1883 to 1986. Respondents' ages range from 18 to 89.

Our measure of self-assessed overall health—the dependent variable for this portion of our analyses—is derived from a survey item that asks, "Would you say your own health, in general, is excellent, good, fair, or poor?" Entirely subjective measures of self-assessed overall health are closely linked to more objective measures of morbidity and mortality (Idler and Benyamini 1997; Idler and Kasl 1991; Wilson 2001); what is more, the available evidence suggests that the association between this measure and mortality has remained more or less constant over time (Idler and Benyamini 1997).

In our analyses of GSS data, we employ four distinct measures of SES, two of which are "absolute" measures of SES, two of which are "relative" measures, two of which pertain to SES in childhood, and two of which pertain to SES in adulthood. First, relative childhood family income is derived from a survey item that asks, "Thinking about the time when you were 16 years old, compared with American families in general then, would you say your family income was far below average, below average, average, above average, or far above average?" Second, in each GSS survey, respondents have been asked about the occupation that their father (or "father substitute") held while they were growing up (an absolute measure of SES in childhood). For our purposes we have collapsed this detailed occupational information into a three-category scheme: white collar, skilled blue collar, and unskilled (which includes unskilled blue collar, farm/forestry/ fishing occupations, and service work). Third, in each GSS survey respondents were asked about their educational attainment (an absolute measure of SES in adulthood). For our purposes, we have divided the sample into those who (1) did not complete high school, (2) completed high school but no post-secondary education, or (3) completed at least some post-secondary education.[1] Finally, relative adult family income is derived from a survey question that asks, "Compared with American families in general, would you say your family income is far below average, below average, average, above average, or far above average?"

Each of our measures of health and SES has its own weaknesses for our purposes. We would certainly prefer a more objective measure of health. In addition, the validity and reliability of retrospective reports of relative childhood family income have not been well established; there is a fair amount of missing data on the father's occupation measure;[2] and the education measure may have conceptual weaknesses, as described in the above discussion of education as an indicator of SES. Consequently, we caution against placing too much faith in the results of models using any one of these SES measures. However, the fact that the results of our analyses are substantively similar across SES measures and that the results from the analyses of the GSS data parallel those from Part II of our analyses give us confidence in the broader validity of our empirical findings.

Descriptive Results

Table 2 reports the percentage of individuals who report their health as excellent or good, separately by 10-year age groups, decade of birth, and relative childhood family income. We choose this SES measure simply as an example, and we hasten to note that we do not collapse categories of the SES measures in our multivariate analyses.[3] Table 2 is designed to demonstrate that we do not observe members of all age groups in all birth cohorts, and we do not observe members of all birth cohorts in all age groups. The empty cells in the upper-left portion of Table 2 are a function of the GSS not beginning until 1972; people born in the 1890s could only be in the 20 to 29 age group if they were interviewed in the 1910s. The empty cells in the lower right portion of the table cannot be filled in yet; individuals born in the 1980s will not be 80 to 89 years old until the 2060s.

Table 2 provides assurance of the quality of the GSS data and of our SES and self-assessed overall health measures. First, as expected, we observe that respondents' self-reported overall health declines with age within every birth cohort. Second, also as expected, we observe that respondents' self-reported overall health improves across birth cohorts within specific age groups. Third, as expected, we observe SES gradients in self-assessed overall health.

[1]We have repeated all analyses using a continuous measure of years of schooling completed. The results, not presented here, are substantively the same.

[2]There is very little missing data (less than 5 percent of cases) on all measures used in our analyses except for father's occupation. About one in six respondents did not grow up with their fathers and are missing on the father's occupation measure. All analyses presented below simply use listwise deletion to handle missing data. We experimented with more elegant techniques for handling missing data on father's occupation— for example, including a new "missing" category of father's occupation—but our results remain substantially the same.

[3]We do, however, dichotomize the self-assessed overall health measure in our multivariate analyses. This eases interpretation, but it also helps us (as described below) to avoid violating methodological assumptions associated with regression models for ordinal dependent variables.

Multivariate Results

In Table 3 we present fit statistics from a series of logistic regression models in which self-assessed overall health ("excellent/good" = 1, "fair/poor" = 0) is modeled as a function of SES, year of birth, and age. We have also estimated a series of ordered logistic regression models that make use of the full range of variability in the self-assessed overall health measure. Although the results of those models are substantively quite similar, we prefer the results that we present because the proportional odds assumption is clearly violated in the ordered logistic regression models. We estimate a separate series of models for each of the SES measures.

The baseline model (model 1) for each SES measure simply includes the SES measure (as a series of dummy variables indicating the SES category to which respondents belong), a continuous measure of age, and a continuous measure of year of birth. To simplify the process of computing predicted values, we have subtracted 18 (the minimum age of sample members) from each respondent's age and 1883 (the earliest year of birth among those included in the sample) from each respondent's year of birth.

Models 2 through 6 add a series of interaction terms to the baseline model. Our strategy is to test hypotheses about whether particular interaction terms in the full model add to the predictive power of the reduced model (which does not include those interaction terms). Because our sample sizes are large—more than 25,000 for each series of models—we base our decisions about improvement in model fit on the Bayesian information criterion (BIC; Raftery 1995). Whereas traditional χ^2 comparisons are especially likely to show improvement in model fit with large sample sizes, BIC provides a better-calibrated assessment of improvement in model fit that accounts for sample size. Reductions in BIC of 10 or greater are associated with strong evidence of improved model fit.

Model 2 begins with model 1 and adds an interaction term for age by year of birth. For two of the four measures for SES, BIC declines by at least 10. This means that the effect of age on self-reported overall health varies as a function of year of birth. Specifically, people of a particular age are healthier in more recent cohorts than same-aged people in less recent cohorts.

Model 3 begins with model 2 and adds an interaction term for SES and age. The BIC statistic never indicates improved model fit. Model 4 returns to model 2 and adds an interaction term for SES and birth cohort; again, the addition of this interaction never improves the fit of the model. Finally, model 5 again begins with model 2 and adds interaction terms for both SES and age and SES and birth cohort. Once again, according to BIC, model 5 never fits the data significantly better than model 2. Consequently, our preferred model (shown in bold in Table 3) includes only the interaction between age and year of birth when SES is measured as childhood relative family income or father's occupation; it includes none of these interactions when SES is measured as adult relative family income or education. These technical differences in model specification are substantively important, as they provide evidence about whether SES differences in self-assessed overall health grow, diminish, or remain unchanged over time. If the association between SES and self-assessed overall health changed across birth cohorts, then we would have expected to find significant interaction effects involving the SES and year of birth covariates.[4]

Table 4 reports parameter estimates and standard errors for our preferred model for each measure of SES. The patterns of results are similar across SES measures. As expected, health (1) declines with age for people in the same birth cohort, (2) improves across birth cohorts for people of the same age, and (3) is always better for people in higher SES categories. Most importantly for our purposes, the best-fitting models specify stability in age-specific SES inequalities across cohorts.

PART II

Data and Measures

In the second part of our analyses, we use harmonized SES and child mortality information from the 1910 U.S. Census and the June Fertility and Marital History Supplements to the 1985, 1990, and 1995 Current Population Surveys (CPS). As in our analyses of GSS data, our central research question is whether the association between SES and mortality (here measured as parental occupation and child mortality) changed across the twentieth century. We utilize the 1910 Census Public Use Microdata Sample provided by the Minnesota Population Center (Ruggles and Sobek 1995); this 1-in-250 national random sample of the population includes approximately 89,000 households and 366,000 individuals. The CPS has been conducted since 1948 by the Bureau of Labor Statistics under the auspices of the Bureau of the Census; more than 50,000 nationally representative households are interviewed monthly. Individuals in the CPS are broadly representative of the civilian, non-institutionalized population of the United States. In addition to the basic demographic and labor force questions that are included in each monthly CPS, questions on selected topics are included in most months. The 1985, 1990, and 1995 June CPS files (like the 1910 U.S. Census) include survey items sufficient to produce indirect estimates of child mortality using the Brass (1975; Brass and Coale 1968) method and its extensions (Trussell 1975; United Nations 1983). The key difference in sampling procedures between the Census and CPS concerns the treatment of institutionalized individuals: Whereas the 1910 Census represents the full population, the CPS samples exclude institutionalized individuals. While rates of institutionalization were certainly higher in the latter part of the twentieth century, evidence from 1990 Census data indicate that only about 3 percent of individuals were institutionalized in 1990. While this difference in sample coverage may have some bearing on our empirical results, we doubt that our results are seriously biased by these differences.

Specifically, our child mortality estimates are derived from the children ever born, children surviving (CEBCS) method, which is based entirely on information about the number of children ever born to each woman, the number of those children who were still surviving at the time of interview, and women's mean age at childbearing. In the 1910 Census, ever-married women were asked to report the number of children they had ever given birth to and the number of those children who were still surviving. They were instructed to include

[4]We also estimate a model with all two-way interactions and a three-way interaction. As expected, this model does not fit significantly better than model 2.

children born in previous marriages, but not to include their current spouse's children born in previous marriages. In the 1985, 1990, and 1995 June CPS, all women older than 14 were asked how many children they had ever given birth to (excluding stillbirths). They were then asked about the current place of residence of the first five children to which they gave birth; among the response options was "deceased." Although we are forced to restrict the CPS analyses to the first five babies born to each woman, this restriction has a minimal impact; only 0.3 percent of women in these data had more than 5 children.

The CEBCS method indirectly estimates child mortality using data on the average number of children ever born and average number of children surviving, calculated by the five-year age group of mother.[5] Brass (1975) demonstrated that the probability of dying between birth and age *a*, denoted as *q(a)*, can be estimated by:

$$q(a)=M(x,5) \times D(x,5)$$

where $D(x,5)$ refers to the proportion of children dead among women in age group $(x, x + 5)$ and $M(x,5)$ is an age-specific multiplier that depends on indices of the age pattern of fertility. Under this system, the proportion of children dead among women in five-year age groups ranging from 15–19 to 45–49 are used to calculate $q(1)$, $q(2)$, $q(3)$, $q(5)$, $q(10)$, $q(15)$, and $q(20)$, respectively. All of the results presented below are from Coale-Demeny West models with Trussell equations.

We begin by selecting all women between the ages of 15 and 49 (for reasons outlined above). We then measure SES using information about the occupation of the head of household of these women. In some cases the woman herself is the head of household, but in the majority of cases the head of household is the woman's husband. That is, our child mortality measure is based on the woman's number of children ever born and surviving, while our SES measure is based on the occupation of her head of household. Data from the 1985, 1990, and 1995 censuses were coded to the standards of the 1980 U.S. Census Occupational Classification. The Minnesota Population Center's release of Public Use Microdata from the 1910 Census also includes a recode of all respondents' occupations to the standards of the 1980 U.S. Census Occupational Classification. Consequently, all of these data from the 1910 U.S. Census and from the late-twentieth-century June CPS surveys are consistently coded to the same standards. In these analyses we have collapsed detailed occupational information into a four-category scheme: white-collar, skilled-blue-collar, service, and unskilled (which includes unskilled blue-collar and farm/forestry/ fishing occupations). The difference between this occupational classification and the one used in the GSS analyses is that we have made service occupations a distinct category. Because $q(1)$ and $q(2)$ are best identified for younger women (ages 15–19 and 20–24, respectively), and because so few women in these young age groups have heads of household who are in white-collar or skilled-blue-collar occupations, our analyses are restricted to deriving estimates of $q(3)$ and $q(5)$.

[5]The children ever born, children surviving method also requires information about mean age of mothers at childbearing. We have used the application FERTCB in MORTPAK 4.0 to estimate mean age of mothers at childbearing. FERTCB produces estimates of age-specific fertility rates and mean age at childbearing from data on children ever born tabulated by age of mother.

Results

Because the separate 1985, 1990, and 1995 June CPS samples are too small to be analyzed separately, we combine those three samples into one that we will henceforth refer to as "the 1990s." Table 5 reports the mean number of children ever born, the mean number of children surviving, and sample sizes, by the age group of mother and the major occupation group of the head of household in 1910 and in the 1990s. As expected, the table makes clear that both fertility rates and child mortality rates declined between 1910 and the 1990s across each of the SES groups. The data in this table are all that is required to produce estimates of $q(3)$ and $q(5)$ using the CEBCS method.

Figures 1 and 2 report estimates of $q(3)$ and $q(5)$, respectively, by SES and separately for 1910 and the 1990s. For ease of presentation, probabilities of dying by age x have been multiplied by 1,000 so that the resulting number can be interpreted as the expected number of deceased children per 1,000 births. The top panels in both figures reflect dramatic declines in child mortality across the twentieth century and reproduce evidence about SES inequalities in child mortality: At each time point, and for both $q(3)$ and $q(5)$, child mortality rates are lowest among those with heads of household who have white-collar or service occupations.

However, the real focus of our investigation is on the bottom panels of Figures 1 and 2. There we compute the relative risk of dying before age x for the skilled-blue-collar, service, and unskilled-blue-collar/ farm groups as compared to the white-collar group. For example, the bottom panel of Figure 1 shows that in 1910 the risk of a child dying before age 3 was 52 percent higher in the unskilled-blue-collar/farm group as compared to the white-collar group. The most relevant thing to note about this figure is that, in general, these relative risks declined only modestly by the 1990s—for example, to 1.33 for the unskilled-blue-collar/ farm group. The same general pattern is observed in Figure 2 for $q(5)$. The only result that does not conform with this general pattern has to do with service occupations in the 1990s. Here, $q(5)$ is lower in the 1990s than for white-collar occupations. This may have to do with fundamental changes in the nature of service work across the twentieth century. Whereas there has been more stability in the social meaning of the other occupational categories, service occupations more closely resembled unskilled-blue-collar work in 1910 but more closely resembled white-collar work by the 1990s. In any case, the general pattern of results in the second part of our analysis reflects modest declines in SES disparities in child mortality across the twentieth century in the United States. While it is encouraging that these inequalities have declined, it is worth reiterating that these declines are generally fairly modest and that substantial inequalities remain in the 1990s.

DISCUSSION

In this article, we presented two sets of empirical analyses that considered the extent to which SES gradients in self-assessed overall health (Part I) and child mortality (Part II) changed in the United States since the beginning of the twentieth century. In the first part of our analyses we observed no changes in age-specific SES inequalities in self-reported overall health across birth cohorts, regardless of our operationalization of SES. In the second part of our analyses we found SES inequalities in child mortality in both 1910 and the

1990s, and we generally observed only modest declines in those inequalities across those two time points. In general, our results suggest that SES gradients in morbidity and mortality remained stable or declined only modestly in the United States across the twentieth century.

As described above, prior research that has addressed our empirical question has typically been limited by restricted time horizons: Research in the United States generally only goes back as far as 1960, whereas research in Europe goes back only to World War II. What is more, we take the measurement of SES quite seriously in our analyses. We employ multiple absolute and relative indicators of SES that are measured in a technically consistent manner over time, including some that arguably have the same conceptual meaning over time. Our aim was not to distinguish between the salience of relative or absolute inequalities (e.g., Wagstaff and van Doorslaer 2000), but our results demonstrate that both are relevant measures in our individual-level analysis of long-term trends in health inequalities.

With this said, both parts of our analyses are limited in a number of important ways. The analyses of GSS data are limited in at least three respects. First, survivorship biases may be limiting our ability to observe cross-cohort changes in age-specific socioeconomic gradients in morbidity among older Americans. The skeptical reader may choose to ignore our results for people beyond midlife; we would note, however, that our results are generally consistent across age groups. Second, for methodological reasons we are forced to rely on a dichotomous measure of self-assessed overall health (although models that utilize the full variability of the original self-assessed overall health measure, which violate the proportional odds assumption, yield substantively similar results). Third, for reasons described above, none of the SES measures used in our analyses of GSS data are ideal. Although we take comfort from the fact that our diverse SES measures—two of which are absolute measures, two of which are relative measures, two of which are measures of childhood SES, and two of which are measures of adult SES—generally yield the same substantive findings, we recognize that each of these SES measures is imperfect. Finally, the basic structure of the GSS data—repeated cross-sections of the U.S. population—is less than ideal for our purposes.

Our analyses of 1910 U.S. Census data and 1985–1995 CPS data are also limited in a number of respects. First, on a technical note, the sampling schemes changed between 1910 and the 1990s. The 1910 sample includes ever-married women, whereas the 1990s data include all women. There is no way to reconcile this difference between these samples, but because rates of non-marital childbearing were low in the early twentieth century, we do not expect that this difference severely biases our results. Second, we would have preferred to have multiple measures of SES (some relative and some absolute) at our disposal in this part of our analyses. Third, the relatively small sample sizes in the CPS forced us to aggregate the 1985, 1990, and 1995 data; even after this aggregation, there are an insufficient number of child deaths to employ a more detailed occupational classification in our analyses. Finally, our choice of methodology allows no formal test of the statistical significance of changes between 1910 and the 1990s in occupation-group-specific probabilities of dying by ages three or five. Point estimates of the relative risks of dying by ages three and five generally declined modestly between 1910 and the 1990s, but we do not know whether these declines can simply be attributed to sampling variability.

The results of the second half of our analyses indicate that SES disparities in child mortality generally remained constant or declined modestly across the twentieth century. We cannot say whether any reduction in disparities was due to downstream proximate interventions. As in the first half of our analyses, these results document persisting SES inequalities in an important health outcome in the face of substantial reductions in the incidence rate of that health problem. All of these results provide further motivation to consider the ways in which upstream macro-social factors help perpetuate persistent SES inequalities in health. While traditional downstream proximate interventions dramatically reduced mortality and morbidity rates across the twentieth century, they did much less to reduce SES inequalities in those rates.

CONCLUSION

Our results and others' (Lutfey and Freese 2005; Lynch et al. 2004) support the contention that research and policy efforts to reduce health inequalities would benefit from considering broader social, economic, and political inequalities alongside more downstream proximate factors that give rise to health inequalities. The U.S. public health infrastructure is in part designed and funded to alleviate health inequalities; indeed, the second objective of the Department of Health and Human Services' *Healthy People 2010* initiative is to eliminate health disparities in this decade. It is clear that there is important work to be done to tackle the proximate causes of health disparities, such as inequalities in access to care, differentials in health risk behaviors, and inequalities in environmental exposures. We argue that researchers doing this work should be cognizant of the upstream macro-social factors that influence the distributions of individuals at risk of downstream proximate causes of morbidity and mortality.

Beyond the need to contextualize traditional public health research, we suggest that more research and policy attention should be paid to the effects of macro-social inequalities on health disparities. There is evidence that country-level variability in health inequalities is related to country-level variability in socioeconomic inequalities (e.g., Mackenbach and Howden-Chapman 2003). This evidence supports the notion that reducing broader social, economic, and political inequalities in the United States might have the consequence of reducing health inequalities. It may be—as Link and Phelan (1995) and others argue—that in order to alleviate socioeconomic gradients in health we need to focus our research and policy efforts more squarely on reducing broader social and economic inequalities. The extent to which this is true—and the extent to which alleviating macro-level social and economic inequalities might be more or less effective in reducing health disparities than traditional approaches—is an open empirical question that is deserving of further investigation.

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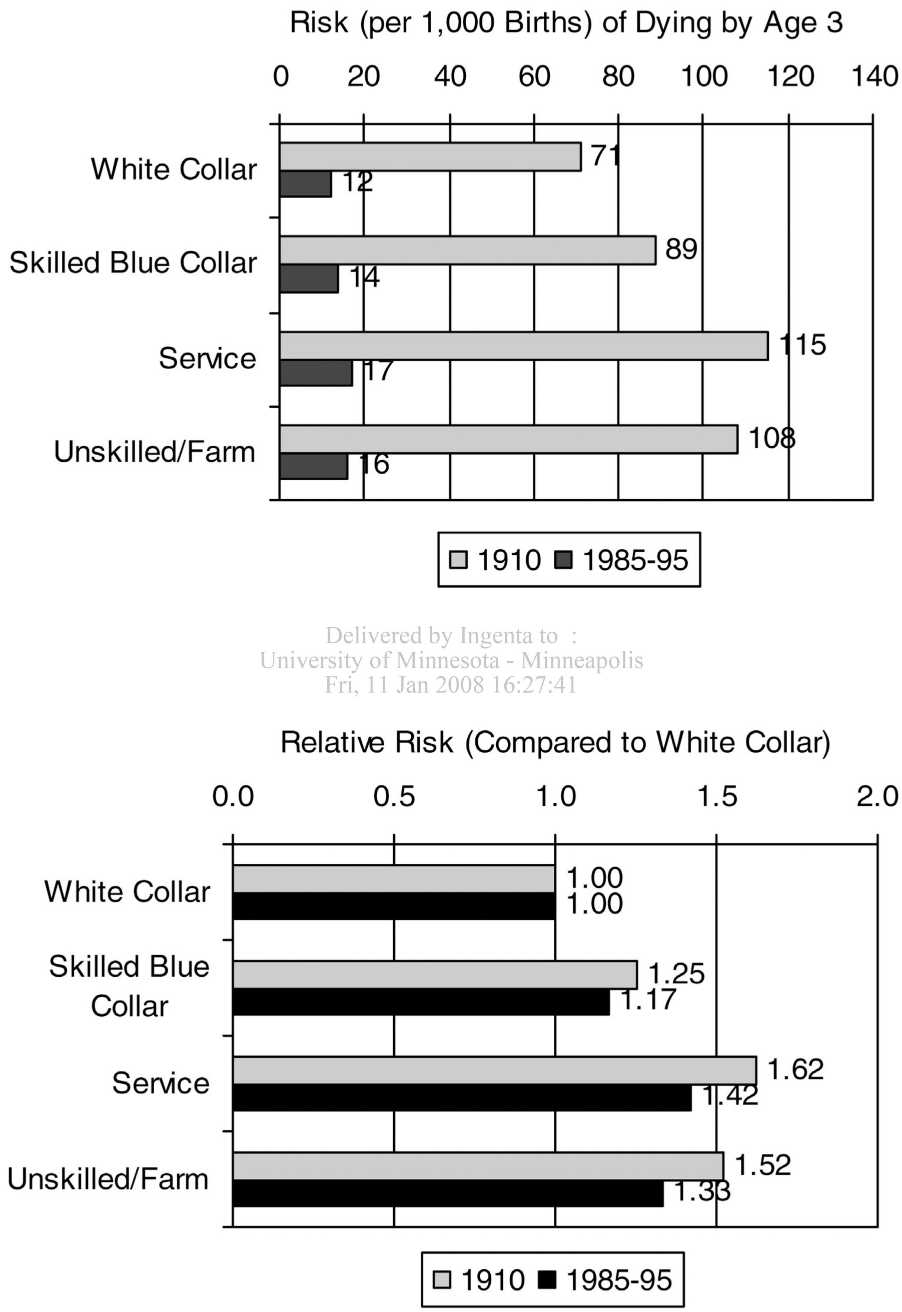

FIGURE 1. Probability of Dying Before Age 3 (*q*3) by Major Occupation Group of Head of Household, 1910 and 1985–95

*Notes: q*3 is derived from "Children Ever Born, Children Surviving" indirect life table estimates. The reported results are from the Coale-Demeny West model (with Trussell equations). See text for details regarding sample and variable construction.

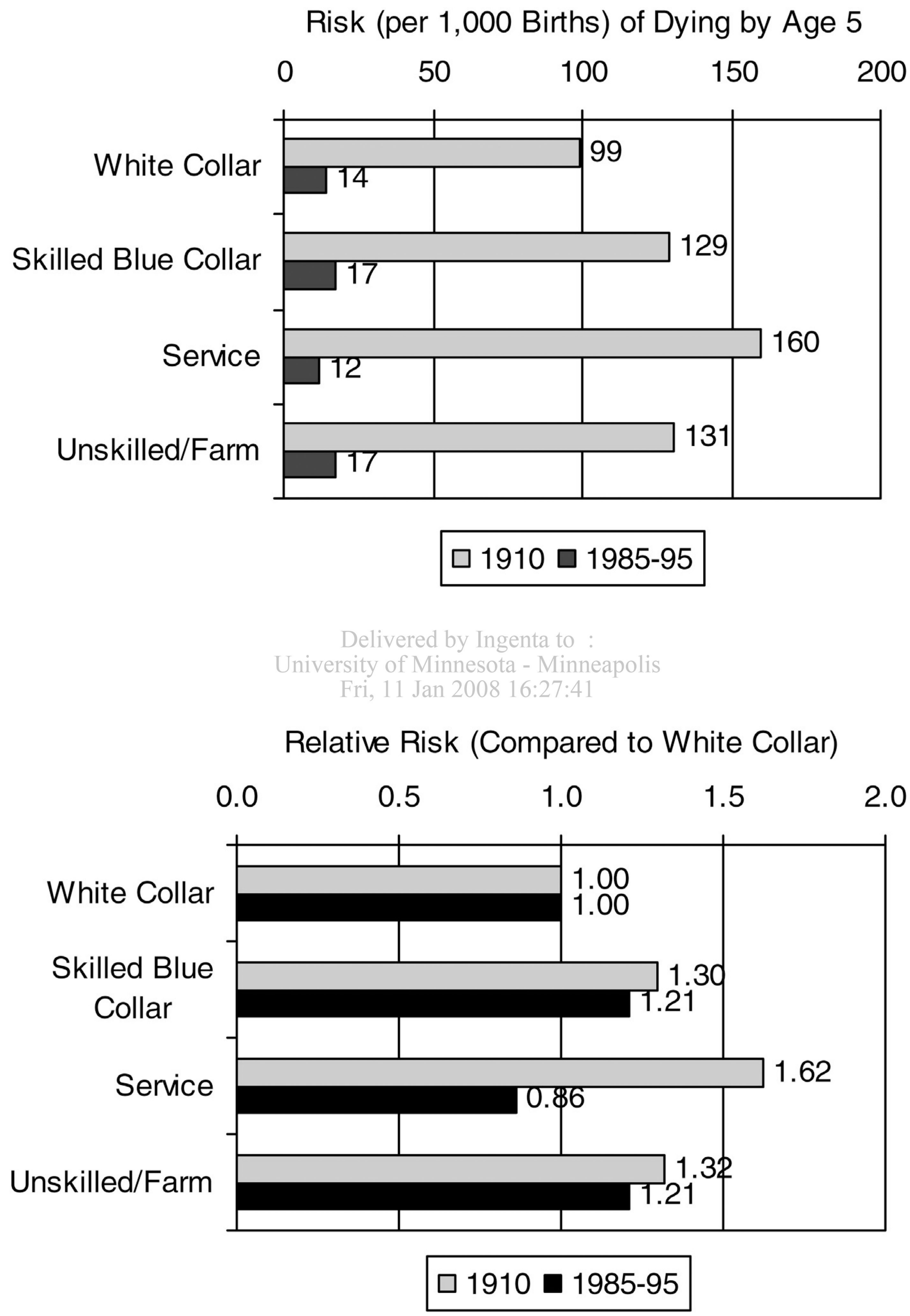

FIGURE 2. Probability of Dying Before Age 5 (*q*5) by Major Occupation Group of Head of Household, 1910 and 1985–95

*Note: q*5 is derived from "Children Ever Born, Children Surviving" indirect life table estimates. The reported results are from the Coale-Demeny West model (with Trussell equations). See text for details regarding sample and variable construction.

TABLE 1

Descriptive Statistics for Variables in the Analyses of GSS Data

	%	n	Years Observed
Self-assessed overall health			
Poor	6%	1,958	1972 to 2004 (except 1978, 1983, and 1986)
Fair	18%	6,425	
Good	45%	15,531	
Excellent	31%	10,943	
Relative childhood family income			
Far below average	8%	2,802	1972 to 2004 (except 1996, 1998, and 2000)
Below average	24%	8,332	
Average	52%	17,947	
Above average	15%	5,096	
Far above average	2%	603	
Father's occupation			
White collar	46%	17,609	1972 to 2004
Skilled blue collar	22%	8,327	
Unskilled blue collar, service, and farm	32%	12,315	
Educational attainment			
Not a high school graduate	25%	11,597	1972 to 2004
High school graduate	32%	14,562	
Some college completed	43%	20,061	
Relative adult family income			
Far below average	5%	2,241	1972 to 2004
Below average	24%	10,138	
Average	51%	21,875	
Above average	18%	7,952	
Far above average	2%	843	
Age group			
18 to 19	2%	814	1972 to 2004
20 to 29	20%	9,486	
30 to 39	22%	10,249	
40 to 49	18%	8,365	
50 to 59	14%	6,571	
60 to 69	12%	5,434	
70 to 79	8%	3,849	
80 to 89	3%	1,576	
Birth cohort			
1883 to 1889	0%	60	1972 to 2004
1890 to 1899	1%	608	
1900 to 1909	4%	2,051	
1910 to 1919	9%	4,068	

	%	n	Years Observed
1920 to 1929	12%	5,489	
1930 to 1939	12%	5,701	
1940 to 1949	18%	8,332	
1950 to 1959	21%	9,958	
1960 to 1969	14%	6,587	
1970 to 1979	6%	2,983	
1980 to 1986	1%	507	

Note: GSS = General Social Survey.

TABLE 2

Percentage of Respondents in Excellent or Good Health, by Decade of Birth, Age Group, and Childhood Family Income, 1972–2004 General Social Surveys (sample sizes given below percentages)

		Decade of Birth										
Age Group	**Relative Family Income at Age 16**	**1880s**	**1890s**	**1900s**	**1910s**	**1920s**	**1930s**	**1940s**	**1950s**	**1960s**	**1970s**	**1980s**
20 to 29	Average or above average							90%	88%	89%	85%	87%
								716	1,708	1,340	407	107
	Below average							83%	80%	84%	81%	78%
								254	508	361	113	32
30 to 39	Average or above average						83%	88%	87%	89%	87%	
							462	1,323	1,465	479	166	
	Below average						79%	77%	82%	81%	88%	
							258	562	573	196	69	
40 to 49	Average or above average					79%	79%	85%	82%	88%		
						454	894	987	452	130		
	Below average					65%	71%	77%	81%	77%		
						234	473	492	189	66		
50 to 59	Average or above average				68%	71%	77%	83%	88%			
					393	846	621	272	115			
	Below average				59%	59%	60%	70%	77%			
					258	593	365	141	70			
60 to 69	Average or above average			56%	61%	68%	77%	80%				
				342	726	582	137	65				
	Below average			43%	50%	57%	69%	84%				
				164	472	476	107	51				
70 to 79	Average or above average		56%	55%	56%	64%	**70%**					
			161	487	474	101	**23**					
	Below average		45%	39%	51%	53%	**88%**					
			96	258	322	66	**17**					
80 to 89	Average or above average	53%	55%	54%	67%	**67%**						
		38	173	218	42	**9**						

Age Group	Relative Family Income at Age 16	Decade of Birth										
		1880s	1890s	1900s	1910s	1920s	1930s	1940s	1950s	1960s	1970s	1980s
	Below average	**53%**	40%	44%	42%	**56%**						
		17	82	100	33	**9**						

Note: Figures in bold are for cells with fewer than 30 observations.

TABLE 3

Logistic Regression Models of Excellent or Good Health on SES, Cohort, and Age Group

	Relative Childhood Family Income	Father's Occupation	Education	Relative Adult Family Income
Model 1: Logistic regressions of self-reported overall health on SES, year of birth, and age				
$\chi 2$ (*df*)	2253.8 (6)	2440.7 (4)	**3917.5 (4)**	**3552.7 (6)**
Model 2: Model 1 + year of birth × age interaction term				
Improvement in χ^2 vs. model 1 (*df*)	**15.3 (1)**	**14.4 (1)**	.0 (1)	1.0 (1)
Change in BIC vs. model 1	−5.2	**−4.1**	10.5	9.4
Model 3: Model 2 + SES × age interaction terms				
Improvement in χ^2 vs. model 2 (*df*)	6.2 (4)	2.7 (2)	8.3 (2)	4.8 (4)
Change in BIC vs. model 2	34.4	17.8	12.6	36.8
Model 4: Model 2 + SES × year of birth interaction terms				
Improvement in χ^2 vs. model 2 *(df)*	7.5 (4)	5.6 (2)	13.8 (2)	6.4 (4)
Change in BIC vs. model 2	33.1	14.9	7.1	35.2
Model 5: Model 2 + all two-way interaction terms				
Improvement in χ^2 vs. model 2 *(df)*	11.6 (8)	8.7 (4)	15.3 (4)	15.8 (8)
Change in BIC vs. model 2	69.6	32.4	26.5	67.4

Notes: SES = socioeconomic status. BIC = Bayesian information criterion. Figures in bold indicate the preferred model for each measure of socioeconomic status.

TABLE 4

Results for Best–Fitting Models in Analyses of 1972–2004 General Social Survey Data

	Relative Childhood Family Income		Father's Occupation		Education		Relative Adult Family Income	
	b	**(s.e.)**	**b**	**(s.e.)**	**b**	**(s.e.)**	**b**	**(s.e.)**
Socioeconomic status, year of birth, and age main effects								
Lowest SES category	−.59	(.12)***	−.63	(.04)***	−1.32	(.03)***	−1.47	(.13)***
Second SES category	−.17	(.12)	−.45	(.04)***	−.51	(.03)***	−.99	(.12)***
Third (or highest) SES category	.17	(.12)	Reference group		Reference group		−.36	(.12)***
Fourth SES category	.45	(.12)***	—		—		.32	(.12)**
Highest SES category	Reference group		—		—		Reference group	
Year of birth (× 100)	1.24	(.23)***	.86	(.21)***	.13	(.14)	1.47	(.14)***
Age (× 100)	−2.51	(.30)***	−2.88	(.28)***	−2.57	(.15)***	−2.01	(.16)***
Year of birth by age interaction term								
Year of birth × age (× 1,000)	.18	(.05)***	.17	(.04)***	—		—	
Intercepts								
Constant	.93	(.21)***	1.77	(.16)***	2.42	(.13)***	1.41	(.17)***

* $p < .05$;

** $p < .01$;

*** $p < .001$

TABLE 5

Mean Number of Children Ever Born, Mean Number of Children Surviving, and Sample Sizes, by Age of Mother and Major Occupation Group of Head of Household, 1910 and 1985–95

	White Collar			Skilled Blue Collar			Service			Unskilled Blue Collar/Farm		
	Mean Number of Children …			Mean Number of Children …			Mean Number of Children …			Mean Number of Children …		
	… Ever Born	**… Surviving**	***n***	**… Ever Born**	**… Surviving**	***n***	**… Ever Born**	**… Surviving**	***n***	**… Ever Born**	**… Surviving**	***n***
1910 (U.S. Census)												
Age of mother												
15–19	.086	.081	2,920	.169	.155	2,658	.162	.151	1,023	.267	.251	9,915
20–24	.731	.694	3,328	1.206	1.129	2,611	.901	.803	1,086	1.528	1.392	8,920
25–29	1.677	1.559	3,250	2.398	2.181	2,375	1.699	1.499	1,028	2.861	2.542	7,610
30–34	2.461	2.219	2,842	3.395	2.951	2,140	2.623	2.197	828	3.988	3.457	6,128
35–39	3.116	2.734	2,725	4.236	3.631	2,007	3.373	2.805	770	5.087	4.299	5,684
40–44	3.660	3.137	2,063	4.922	4.025	1,546	4.117	3.237	607	5.706	4.694	4,580
45–49	3.857	3.222	1,574	5.195	4.161	1,252	4.311	3.286	518	6.298	5.063	3,938
1985–1995 (1985, 1990, 1995 June Current Population Surveys)												
Age of mother												
15–19	.070	.068	3,942	.095	.095	84	.064	.064	3,270	.089	.089	751
20–24	.295	.292	8,019	.590	.586	251	.484	.477	2,881	.610	.601	1,189
25–29	.673	.666	9,688	.954	.945	307	1.116	1.099	2,464	1.230	1.211	1,332
30–34	1.195	1.180	10,194	1.351	1.320	353	1.692	1.671	2,313	1.656	1.632	1,424
35–39	1.552	1.532	9,850	1.757	1.713	338	1.975	1.940	2,084	1.948	1.914	1,491
40–44	1.787	1.756	8,762	1.876	1.846	299	2.139	2.097	1,846	2.215	2.160	1,216
45–49	2.013	1.965	7,012	2.181	2.130	216	2.485	2.425	1,531	2.432	2.357	1,052