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Misery Loves Company? A Meta-Regression Examining Aggregate Unemployment Rates and the Unemployment-Mortality Association

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Abstract

PURPOSE—Individual-level unemployment has been consistently linked to poor health and higher mortality, but some scholars have suggested that the negative effect of job loss may be lower during times and in places where aggregate unemployment rates are high. We review three logics associated with this moderation hypothesis: health selection, social isolation, and unemployment stigma. We then test whether aggregate unemployment rates moderate the individual-level association between unemployment and all-cause mortality.

METHODS—We use 6 meta-regression models (each utilizing a different measure of the aggregate unemployment rate) based on 62 relative all-cause mortality risk estimates from 36 studies (from 15 nations).

RESULTS—We find that the magnitude of the individual-level unemployment-mortality association is approximately the same during periods of high and low aggregate-level unemployment. Model coefficients (exponentiated) were 1.01 for the crude unemployment rate ($p = 0.27$), 0.94 for the change in unemployment rate from the previous year ($p = 0.46$), 1.01 for the deviation of the unemployment rate from the 5-year running average ($p = 0.87$), 1.01 for the deviation of the unemployment rate from the 10-year running average ($p = 0.73$), 1.01 for the deviation of the unemployment rate from the overall average (measured as a continuous variable; $p = 0.61$), and showed no variation across unemployment levels when the deviation of the unemployment rate from the overall average was measured categorically. Heterogeneity between studies was significant ($p < .001$), supporting the use of the random effects model.

CONCLUSIONS—We found no strong evidence to suggest that unemployment experiences change when macro-economic conditions change. Efforts to ameliorate the negative social and

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economic consequences of unemployment should continue to focus on the individual and should be maintained regardless of periodic changes in macro-economic conditions.

MeSH KEYWORDS

Unemployment; Mortality; Meta-analysis

INTRODUCTION

Individual-level unemployment has been consistently linked to poor health and higher mortality [1–5]. However, some scholars have suggested that this relationship may be moderated by the aggregate unemployment rates in a given place. More specifically, scholars have proposed that (when compared to their employed contemporaries) persons who become unemployed when the unemployment rate is high will have a lower relative risk for adverse health outcomes than persons who become unemployed when the unemployment rate is low [see 6, 7–15]. In other words, the economic context in which a person becomes unemployed may influence the severity of the effects of being unemployed. Being unemployed during a period when many others are also unemployed may be fundamentally different than becoming unemployed during an economic boom.

Multiple logics have been offered for why we might expect the unemployment-mortality association to weaken when aggregate unemployment is high. First, the unemployment-mortality association might be confounded by health selection factors. During periods when unemployment rates are low, it may be that the people who become unemployed are primarily those with pre-existing health problems. However, when unemployment rates are high, a substantial number of healthy people may also become unemployed. The increased numbers of healthy unemployed persons may consequently push down the mortality rate for the unemployed group as a whole, rendering it closer to the (lower) mortality rate for employed persons.

Second, it may be that levels of social isolation are reduced during hard economic times because there are more unemployed persons around with whom an unemployed person can exchange social support. This mechanism can work in two ways. First, interaction among unemployed persons can arise out of social ties created subsequent to becoming unemployed. For example, a substantial number of unemployed persons use public libraries and employment centers to find employment, particularly among populations with limited home internet access [16]. In these locations, the unemployed have a chance to meet others who share their status. The information sharing and social ties created in this way, temporary and weak as they may be, may help to reduce feelings of isolation and self-blame. The larger pool of people visiting libraries and employment centers during periods of high unemployment may increase the odds of tie initiation and thus potentially offset some of the negative effects of unemployment. Second, interaction among unemployed persons may be based on social connections that existed prior to unemployment. For example, a person who becomes through a mass layoff would have social connections with their former coworkers [13]. Through these connections, workers may be able to frame their unemployment as beyond their control and therefore experience fewer negative psychological effects from

their unemployment. In a similar fashion, existing residential connections between neighbors make it reasonable to expect that high neighborhood unemployment rates might reduce the negative impacts of individual unemployment [17]. Despite a higher incidence of some types of social problems, higher rates of resource sharing and other similar exchanges of support have indeed been observed in lower income neighborhoods with high unemployment rates [18].

Finally, it may be that the general public becomes more likely to view unemployment as something beyond the individual's control during periods of high unemployment, reducing the stigma (and thus stress) often associated with losing a job. As Clark [7, p. 346] argues, "unemployment always hurts, but it hurts less when there are more unemployed people around." While Martikainen and Valkonen [12] note that it is unlikely that societal attitudes about individual responsibility for becoming unemployed would change over relatively short periods of time, one may expect that if national economic conditions remain bad for an extended period less blame would be placed on unemployed individuals for their plight.

While each of these explanations is feasible, one must note that the mere premise that the unemployment-mortality association weakens when unemployment rates are high is still questionable, and that the search for mechanisms may therefore be premature. In other words, we do not yet have conclusive evidence that aggregate unemployment rates *systematically* affect the unemployment experience. The purpose of the present study is to test whether the aggregate unemployment rate in a nation is associated with any change in the magnitude of association between mortality and job loss.

Existing research in this area is limited, often confined to the comparison of only two time periods within a single nation, and reported effects are often inconsistent. Some of these studies indeed support the claim that aggregate unemployment rates have an important moderating effect. For example, in a study of working-age Finnish men and women, Martikainen and Valkonen [12] found that those who became unemployed for the first time during a period of low unemployment rates had a higher relative mortality ratio than those who became unemployed for the first time during a period of high unemployment rates. Similarly, in a study of the young working-age population in Australia, Scanlan and Bundy [19] found that the health of unemployed persons was worse during a time of low unemployment. Similar supporting evidence has been reported by Martikainen, Maki, and Janttila [13] and by Henriksson et al. [9]. However, other studies found no effect of the aggregate unemployment rate on the magnitude of the relative risk [6, 10, 11, 20, 21].

In the present study, we use meta-regression methods to examine the effect of aggregate unemployment rates on the individual-level association between unemployment and all-cause mortality on a cross-national level. Our study follows in the footsteps of another recent study of the unemployment-mortality association [1], which sought to determine the mean level of risk. In this previous study, the authors did not examine the potential moderating effect of national economic conditions. In the present study we seek to fill this lacuna.

MATERIAL AND METHODS

The present study is part of a larger effort to examine the associations between various types of negative, stressful life events (e.g., unemployment, divorce/separation, widowhood, war zone exposure, etc.) and all-cause mortality. For the parent study, we identified candidate papers using electronic keyword searches (June 2005 and again in July 2008) using Medline, EMBASE, CINAHL, and Web of Science (see Figure 1; see Section 1 of Appendix for the full search algorithm used for Medline; information on the remaining search algorithms is available from the authors upon request). We identified additional candidate papers through iterative searches of bibliographies and citations. A study was included if the outcome variable was all-cause mortality, the experience of a stressful life event was measured at the individual level (rather than at the neighborhood level), and a clear comparison was made between a group of people who experienced a particular stressful event and another group who did not (see Section 2 of Appendix for details regarding coding procedures and variables for which data were sought). No restrictions were placed on the year a study was published, in what language it was originally written, or the type of outlet in which the paper appeared. In all, we examined 729 studies in detail (see Figure 1). Of these, 262 contained data that could be coded into the parent study database and selected from for the examination of particular stressful live events such as unemployment.

Of the 262 studies in the parent database, 42 examined the association of unemployment with all-cause mortality. We excluded six of these 42 because they contained redundant data. The analysis presented below is thus based on 62 relative mortality risk estimates from 36 studies (see forest plot in Figure 2) obtained from samples of the working-age population (ages 15–65). Multiple relative risk estimates were taken from a single study/data source solely when they were based on non-overlapping sub-samples (i.e., represent independent risk estimates). Statistical methods varied from study to study, necessitating the conversion of all non-hazard-ratio point estimates into hazard ratio format (the most frequently reported type; see Section 3 of Appendix). In cases where the death rate information required for this conversion was not available in the published study (19 of the 62 risk estimates), we calculated the death rate (matched by nation, age, and year) using information from the *Human Mortality Database* [53] and (for the case of Costa Rica) the World Health Organization's Department of Health Statistics and Informatics [54].

Study quality was assessed using the Newcastle-Ottawa scale for nonrandomized trials [55]. Analyses were conducted in STATA using version 1.3 of the “robumeta” package provided by Fisher and Tipton [see 56]. The possibility of selection and publication bias was examined using a funnel plot of the log hazard ratios (HR) against sample size, Egger's test [57], and Peters' test [58, 59]. *Q*-tests and examinations of the unexplained heterogeneity variance component were used to assess the presence and magnitude of heterogeneity in the data.

We test whether the aggregate unemployment rate moderates the unemployment-mortality association using a random effects meta-regression model with robust standard errors (to account for possible intra-study correlations between effect sizes; we assumed an intra-study effect size correlation of $\rho = .80$). We used the log of the hazard ratio (unemployed vs.

employed) as the dependent variable. When the resulting regression coefficients are exponentiated, the results take the form of a comparison of the magnitudes of the hazard ratios under varying study conditions.

The inverse of the variance of the log-hazard was used as the weighting variable. When variances or standard errors were not directly reported, they were calculated using (1) confidence intervals, (2) t statistics, (3) χ^2 statistics, or (4) p -values. When upper-limit p -values were the only estimate of statistical significance available (e.g. in cases where we knew only that the p -value lay somewhere between 0.01 and 0.05), the midpoint of the upper and lower limits was used to estimate the p -value. For 3 of the 62 relative mortality estimates no standard error was available from which to calculate the inverse variance weight. For these three cases, the standard error was estimated using multiple regression (with predictor variables selected based solely on whether or not a variable had few/no missing values). Significant predictors of the standard error were sample size (log transformed), mean age at baseline, follow-up duration, the magnitude of the hazard ratio, and publication date (Multiple $R = .72$). The mortality measure used was all-cause mortality.

Our focal independent variable was the national unemployment rate at time t (baseline year), a measure of the business cycle conditions (and hence labor demand) present within a given nation at a given time. To ensure robust results, we calculated six alternative specifications for use in separate models. The most basic model used the crude unemployment rate. Because people likely assess their own situation by making comparisons with localized conditions from the recent past, we examined various aggregate unemployment measures meant to capture how individuals would have perceived their own unemployment context. Specifically, we calculated the change in unemployment rate from the previous year, the deviation of the unemployment rate from the 5-year running average, and the deviation of the unemployment rate from the 10-year running average.

In addition, we calculated the deviation of the unemployment rate from the overall average. The advantage of making comparisons to the overall average is that we can assess whether or not unemployment was objectively high or low in a given place and time. We argue that this operationalization of relative unemployment most directly corresponds to those used by previous examinations of this hypothesis [see again 6, 7–15]. Finally, we calculated dummy variables based on the quartiles for the deviation of the unemployment rate from the overall average. The use of a categorical specification for the unemployment rate, rather than treating the relative unemployment rate only as a continuous variable, helps to examine whether a non-linear effect might be present. This allows us to examine the possibility suggested in some recent studies that the unemployment-health association only fundamentally changes during economic crises [60, 61].

We collected data on the national unemployment rate in the civilian population aged 15 or greater primarily from *Organization for Economic Cooperation and Development* (OECD) reports, with supplemental data obtained from the World Bank, the *International Monetary Fund* (IMF), the *International Labor Organization* (ILO), and the national statistical bureaus of Israel and Taiwan. Comparable data on the aggregate unemployment rate were available between 1960–2004 for Belgium, Denmark, Finland, Italy, New Zealand, Switzerland, and

the United States; 1961–2004 for Japan and the United Kingdom; 1963–2004 for Sweden; 1964–2004 for Australia and Spain; 1978–2008 for Taiwan; 1984–2004 for Israel; and 1985–2004 for Costa Rica. Aggregate unemployment rates were matched to mortality risk estimates according to the baseline year in which data collection began, the country in which the study was conducted, and both the age distribution and the gender distribution of the sample being studied.

Other control variables in the meta-regression models were included based on both data availability and theoretical importance. They included: (1) the proportion of respondents who were male (to control for gender differences in the mortality risk associated with unemployment); (2) the mean age of sample at baseline (to control for age differences in the underlying death rate); (3) the age of the study (i.e. years elapsed since the beginning of baseline; included to control for unmeasured changes in research methodology), divided by 10; (4) the time elapsed between the end of baseline and the beginning of follow-up; (5) the maximum follow-up duration; (6) the type of comparison group (to control for differences caused by comparing to employed persons only vs. the general population); (7) whether unemployment included students, early retirees, etc. (included to control for differences caused by including anyone other than the involuntarily unemployed in the numerator group); (8) the geographic region in which the study was conducted (to roughly control for differences in behavioral norms and government policies at the nation-state level); (9) a series of variables indicating whether studies controlled for sex, age, socioeconomic status, and health (to examine differences between studies that reduced confounding by including key control variables vs. those that did not); (10) the study's sample size, log transformed (to control for any selection bias present in the data); (11) whether the standard error was estimated (yes or no; an indicator variable was created so analyses could be conducted both with and without data points where the standard error was estimated); and (12) the Newcastle-Ottawa Scale rating (range, 0–9; included to control for differences in study quality).

The series of indicator variables with respect to whether a study controlled for sex, age, socioeconomic status, or health (number 9 in the list above) is particularly important, given what is known about the possibility of selection effects. For example, if health selection is at work, then one would expect those who become unemployed to already be less health than those who retain their jobs. In addition, health selection is likely to be particularly strong during periods of healthy economic growth but less strong when unemployment is high. Either way, if selection effects are at work, one would expect the observed unemployment-mortality association to be weaker (or non-existent) in studies that control for health when compared to studies that do not. The indicator variable for whether a study controlled for health provides a test of this selection hypothesis. Socioeconomic selection effects, age selection effects, and sex selection effects are likewise tested through their corresponding indicator variables.

RESULTS

In Table 1 we report descriptive statistics for the 62 hazard ratios (HRs) included in the analysis. Among all nations and years, the aggregate unemployment rate ranged from 0.7%

(Denmark in 1970) to 29.6% (Spain in 1996), with an overall average of 6.6%. All but 11 of the 62 aggregate unemployment rates were below 10%, and only 5 were above 15%. Neither the lowest nor the highest aggregate unemployment rates appeared to be associated with any single nation or region. When examined relative to the within-nation average unemployment rate, the deviation of the unemployment rate ranged from 4.81 percentage points below the mean to 13.80 points above the mean. We refer the reader to Table 1 for the descriptive statistics on the control variables used in the analysis.

The mean HR across all studies in the analysis was 1.62 (95% confidence interval, 1.45–1.80), indicating that the mortality risk for unemployed persons was 62% higher, on average, than the mortality risk for employed persons. Caution must be used when interpreting this result, however, because it does not take into account any of the substantial heterogeneity between studies. In Table 2 we report exponentiated regression coefficients from six meta-regression models, with each exponentiated coefficient representing a ratio comparison of two HRs. For example, the exponentiated coefficient corresponding to the Scandinavian region represents the ratio of the mean HR for Scandinavia to the mean HR for the comparison group of nations. Among the continuous measures of the unemployment rate (Models 1 through 5), neither the crude unemployment rate ($p = 0.27$), the change in unemployment rate from the previous year ($p = 0.46$), the deviation from the 5-year running average ($p = 0.87$), the deviation from the 10-year running average ($p = 0.73$), nor the deviation from the overall average ($p = 0.61$) were significant predictors of HR magnitude. In addition, we found no significant effect when we used a categorical measure of the deviation from the overall average (based on quartiles; Model 6). Put together, our findings do not support the hypothesis that the individual unemployment–mortality relationship is moderated by the aggregate unemployment rate.

Across the 6 models, there was a consistently significant difference in HR magnitude associated with differences in the gender composition. Studies examining only men reported HRs that ranged from 35% to 39% higher than studies examining only women. The mean age of the study sample was also a significant predictor. Although no difference was found in the mean HR between studies with a mean age less than 40 (reference group) and studies with a mean age from 40–49.9, studies with a mean age from 50–65 had HRs that ranged from 29% to 31% lower than the reference age group. Studies comparing unemployed persons to the general population had HRs that ranged from 41% to 51% lower than studies that made comparisons directly to employed persons.

The mean HR ranged from 32% to 36% lower among studies that controlled for health behaviors such as smoking and drinking, when compared to studies that did not control for these factors. This suggests that health selection is very much effecting the unemployment–mortality association. It is important to note, however, that some level of association remains even after health selection is taken into account. Thus, it remains valid to talk of a direct unemployment–mortality linkage.

Non-significant predictors included the age of the study; the measures of follow-up structure and duration; whether a study included the voluntarily unemployed and/or disabled persons with involuntarily unemployed persons; the region where the study was conducted; whether

a study controlled for sex, age, and/or socioeconomic status; the sample size of the study (logged); whether an estimated standard error was used to calculate the inverse variance weight; and the measure of study quality.

Robustness checks

Cochrane's Q -test for data heterogeneity indicated low levels of residual heterogeneity in the model. Yet, random-effects models remained necessary in order to manage unobserved sources of heterogeneity (as indicated by the significant association between sample size and HR magnitude and by the significance of the unexplained heterogeneity variance component). Some sampling variability was visible in the funnel plot of the log hazard ratios versus sample size (see Figure 3) and funnel plot asymmetry was confirmed using Eggers' test ($p < 0.001$). However, Peters' test for funnel plot asymmetry in heterogeneous data showed that heterogeneity was not likely a major problem in the final analyses ($p = 0.41$).

We checked the robustness of the model to variable over-specification. It is normal in regression to limit the ratio of cases to independent variables (norms vary, but a maximum of between 5 and 10 cases per independent variables is common). With 62 cases in the analysis, this would imply a limit of between 6 to 12 independent variables. The results shown in models 1 through 6, however, are based on 19 independent variables. We therefore ran a two-stage parsimonious model (results not shown in table) in order to compare our reported results against a model with fewer variables. We first identified a parsimonious set of covariates (examining all variables except for those measuring unemployment rate) using forward selection ($p < .10$ to enter). Next, we sequentially examined each unemployment rate measure. None of the unemployment measures were significant at the $p < .10$ level. The significant covariates in the parsimonious model were the proportion of the sample that was male, the mean age of the study sample at baseline, the indicator for whether the comparison group was the general population, the indicator for whether the unemployed group included those not in the labor force, the indicator for whether a study controlled for health behaviors, and the sample size of the study (logged). Both the pattern of significance and the magnitudes of the regression coefficients was consistent with the results shown in Table 2.

DISCUSSION

Our study offered the first large-scale cross-national test for the hypothesis that unemployment may be relatively less harmful during periods of high unemployment rates. Our findings do not provide support for this hypothesis. We tested six alternative specifications for the unemployment rate (continuous and categorical) and none of them was statistically significant at a p -value below 0.05 (p -values ranged from 0.27 to 0.97).

In Model 1 (Table 2) we examined the potential effect of the crude unemployment rate. Because most of the variation in unemployment rates is between nations, rather than between years within nations, the results from this model do not tell us much about the within-nation effect. They do show, however, no evidence that the individual-level unemployment-mortality association differs between countries. This is remarkable given the large differences between the aggregate unemployment rates of the countries included in the analysis (ranging from 0.7% in Denmark in 1970 to 29.6% in Spain in 1996). This finding is

consistent with studies that have found no relationship between subjective well-being and other aspects of the economic support structure of nations [see 62]. Caution must be used when interpreting the result from Model 1, however, because it reflects both between and within nation variability. A more conservative interpretation would be that the crude unemployment rate fails to significantly account for the combination of these two aspects of data heterogeneity.

However, one might argue cross-national comparisons are not the right ones. That is because people are probably less likely to make comparisons with people in other nations and more likely to make comparisons with localized conditions from the recent past. In Models 2 through 4, we examined various aggregate unemployment measures meant to capture how individuals would have perceived their own unemployment context. This included (1) the change in unemployment rate from the previous year; (2) the deviation of the unemployment rate from the 5-year running average; and (3) the deviation of the unemployment rate from the 10-year running average. For each model, change and deviation scores were calculated on a nation-by-nation basis. Each of these three alternative specifications depends only on economic information that would have been available to study subjects and each alternative specification focuses only on within-nation variations in the unemployment context. Therefore, the coefficients for the unemployment rate variables represent very direct tests of the central hypothesis examined in this paper. The relative unemployment rate measures for all three models were not significant at the 0.10 level (the lowest p -value of any of the alternate measures in models 2 through 4 was $p = 0.46$). The fact that none of these alternative within-nation measures was significant suggests that unemployed persons assess their situation from a very personal vantage point, giving little consideration to broader group and societal trends. This too is consistent with existing psychological research on the perception of unemployment. For example, Walker and Mann [63] showed that levels of stress related to unemployment were explained primarily by the gap between people's personal expectations and their actual attainment, rather than by the relative standing of the group(s) to which they belonged.

In Model 5, we examined aggregate unemployment measures relative to the overall average unemployment rate in each nation (1960–2004 for Belgium, Denmark, Finland, Italy, New Zealand, Switzerland, and the United States; 1961–2004 for Japan and the United Kingdom; 1963–2004 for Sweden; 1964–2004 for Australia and Spain; 1978–2008 for Taiwan; 1984–2004 for Israel; and 1985–2004 for Costa Rica). As such, we calculated deviation scores using information that was not necessarily available to the individuals in the original studies (i.e., using past, present, and *future* unemployment rates). The advantage of making comparisons to the overall averages, again, is that we can assess whether or not unemployment was objectively high or low in a given place and time. Once again, the relative unemployment rate was not significant.

Finally, in Model 6, we further examined the potential effect of aggregate unemployment rates relative to overall averages. As indicated earlier, the categories for the relative unemployment rate were defined by quartile and used to determine whether a non-linear effect might be present. No single quartile emerged as significantly different from the lowest quartile, and there was also no significant pattern in the coefficients (linear or otherwise).

The lack of a significant difference, even for the highest quartile of unemployment rates, indicates a lack of support for the hypothesis that the unemployment-mortality association will differ if an economic crisis is happening.

Taken altogether, none of our various attempts to operationalize aggregate unemployment yielded a significant result. In other words, our extensive cross-national and cross-period analyses lend no support to the premise that aggregate unemployment rates moderate the relationship between individual unemployment and mortality. The various theoretical explanations suggested for such a moderation effect therefore seem premature.

Limitations

We cannot rule out the possibility that aggregate unemployment rates remain an important moderator with respect to outcomes other than all-cause mortality. Indeed, recent research endeavors reported evidence of a moderating effect for outcomes such as suicide [64] and self-rated health [19]. Still, we should note that the existence and direction of the moderation effect in these cases remains somewhat questionable. While some studies have found that self-rated health is worse (rather than better) among the unemployed when aggregate unemployment rates are high [65], others have found that both the short-term unemployed and those who remain employed report worse health during recessions [15]. In addition, similarly to the present study, some studies have found no relationship between unemployment rates and self-reported health [14, 66]. Moreover, our results suggest that any moderating effect for aggregate unemployment rates for these “lower level” health outcomes does not necessarily translate into a moderating relationship with respect to more severe health outcomes such as mortality.

We must also point out that the national unemployment rate is a more appropriate measure of macro-economic conditions for some studies than for others. Many of the studies used in the analysis gathered data from a nationally representative sample; for these the national unemployment measures are directly applicable. For a subset of studies, however, the geographic area examined was more restricted. National unemployment data in these instances is only a proxy measure of local economic conditions. To the extent that conditions in a particular locality are decoupled from national conditions, the results would fail to fully test this paper’s main hypothesis. Furthermore, research has suggested that people are more likely to be affected by unemployment in their local area rather than the nation as a whole and that national unemployment statistics sometime mask important regional variations [17].

Relatedly, an assumption underlying the study design is that the key difference between time periods is the unemployment level, and that the unemployment rate is therefore the only key moderator to be accounted for when examining the individual-level unemployment-mortality association. This is unlikely to be true. A multitude of other factors may also differ between time periods, including changes in medical treatments and technologies, highway safety, and social welfare systems. The inclusion of study age as a control variable helps to account for linear trends in unobserved factors, but this is not the ideal approach. Including fixed effects based on time would be the preferred method. Unfortunately, the limited number of studies precludes the use of this method.

Our meta-regression model was also limited by the follow-up durations of the original studies. While we found that the aggregate unemployment rate was not associated with HR magnitude, this lack of association may derive partly from the fact that the majority of our relative risk estimates were from studies with follow-up durations of greater than 5 years. Assuming that the individual-level effects of unemployment and mortality are largely concentrated in the period immediately following the loss of employment, data derived solely from studies with short follow-up durations may have produced different results with respect to the aggregate-level unemployment measure. Specifically, if the negative effects of unemployment are transitory, then the power to detect this transitory effect decreases as the follow-up period increases. However, in both our current and previous analyses [1] we showed that follow-up duration was not a significant predictor of the magnitude of association between unemployment and mortality, suggesting that the effect of becoming unemployed persists into later years.

One must also keep in mind that data limitations prevented us from looking for differences based on the number of times a person has been employed or for differences based on the duration of unemployment. From a psychological perspective, one might expect the reactions of a person who becomes unemployed for the first time to differ substantially from a person who has experienced repeated unemployment. From a social perspective, one might also expect differences in how these two opposite types of unemployed persons might be treated by others (including prospective employers). Relatedly, the lack of life-course information with respect to the actual duration of unemployment did not allow us to measure the unemployment rate over a time period rather than a single point in time. Our choice to use the unemployment rate in the year a study's baseline data collection began as a measure of the macro-economic conditions is thus only an approximation (though we would argue the only method available).

The limitations discussed in the previous study on this dataset [1], most of which are common issues with meta-regression analysis, are also applicable to the present study. In brief, there is an unknown degree of non-reporting in the literature, we cannot completely rule out the presence of selection bias, the studies included in the meta-regression are observational, and there remains the possibility that selection effects account for some portion of the observed unemployment-mortality association. There is some evidence for health selection specifically, as Models 1 through 6 in Table 2 show that studies that controlled for health behaviors reported HRs that were between 34% to 36% lower than studies that did not control for health behaviors.

Conclusions

In the introduction to the paper, we argued that the search for explanations as to why we might expect the unemployment-mortality association to weaken when aggregate unemployment rates are high may be premature, as research had yet to robustly establish whether or not aggregate unemployment rates systematically affected individual-level associations. Using 6 separate meta-regression models, we found no strong evidence that the aggregate unemployment rate modified the direction or magnitude of the individual-level unemployment-mortality association. The overall impression of these results is that efforts

to ameliorate the negative social and economic consequences of unemployment should continue to focus on the individual and should be maintained regardless of periodic changes in macro-economic conditions.

While we found that the aggregate unemployment rate does not affect the unemployment experience, it remains plausible that a mediating effect for mass unemployment still exists when the group of unemployed people already shares some social connection with one another. For example, one might reasonably expect that a person who is unemployed along with many others from the same workplace (e.g., a mass layoff) would have a difference experience than a person who is unemployed alone. This is, in fact, one of the secondary findings of Martikainen, Maki, and Jantti [13]. This type of hypothesis is also consistent with the literature on posttraumatic stress, which has found that experience trauma in a group is less harmful than experiencing it alone. In a similar fashion, existing residential connections between neighbors make it reasonable to expect that high neighborhood unemployment rates might reduce the negative impacts of individual unemployment as well, as suggested by Milner et al. [17]. The current study primarily examines unemployment in contexts where inter-personal connections are unlikely. In order to fully understand the effect of unemployment levels on the unemployment experience, more research is needed at the workplace and neighborhood level.

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ABBREVIATIONS

HR	Hazard Ratio
OECD	Organization for Economic Cooperation and Development
IMF	International Monetary Fund
ILO	International Labor Organization

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Appendix

Section 1: Full search algorithms for Medline

1. exp stress, psychological/mo
2. exp Stress, Psychological/
3. exp mortality/
4. mo.fs.
5. (death\$ or mortalit\$ or fatal\$).tw.
6. or/3–5
7. 2 and 6
8. 1 or 7
9. stress\$.tw.
10. exp caregivers/
11. caregiv\$.tw.
12. (care giver\$ or care giving).tw.
13. exp family/
14. exp siblings/
15. exp divorce/
16. exp marriage/
17. (marital adj (strife or discord)).tw.
18. widow\$.tw.
19. (marriage or married).tw.
20. divorce\$.tw.
21. famil\$.tw.
22. (son or sons).tw.
23. daughter\$.tw.
24. (spous\$ or partner\$ or husband\$ or wife or wives).tw.
25. (mother\$ or father\$ or sibling\$ or sister\$ or brother\$).tw.

26. exp dissent/ and disputes.mp. [mp=title, original title, abstract, name of substance word, subject heading word]
27. exp domestic violence/
28. domestic violence.tw.
29. ((child\$ or partner\$ or spous\$ or elder\$ or wife or wives) adj5 (violen\$ or abuse\$ or beat\$ or cruelty or assault\$ or batter\$)).tw.
30. ((mental\$ or physical\$ or verbal or sexual\$) adj2 (violen\$ or abuse\$ or cruelty)).tw.
31. exp PEDOPHILIA/
32. (pedophil\$ or paedophil\$).tw.
33. exp social class/
34. exp socioeconomic factors/
35. (socioeconomic\$ or socio economic\$).tw.
36. ((financ\$ or money or economic) adj (stress\$ or problem\$ or hardship\$ or burden\$)).tw.
37. exp poverty/
38. (poverty or poor or depriv\$).tw.
39. exp residence characteristics/
40. ((neighbo?rhood or resident\$) adj (characteristic\$ or factor\$)).tw.
41. (crowd\$ or overcrowd\$).tw.
42. exp prejudice/
43. (prejudic\$ or racis\$ or discriminat\$).tw.
44. exp social isolation/
45. exp social support/
46. (social adj (isolat\$ or support\$ or connect\$ or depriv\$ or function\$ or influen\$ or interact\$ or relationship\$ or separat\$ or ties)).tw.
47. exp friends/
48. (acquaintance\$ or companion\$ or friend\$).tw.
49. neighbo?r\$.tw.
50. exp interpersonal relations/
51. (social adj network\$).tw.
52. exp social behavior/
53. (social\$ adj activ\$).tw.
54. exp work/

55. exp employment/
56. exp job satisfaction/
57. exp work schedule/
58. exp occupational disease/
59. exp occupational health/
60. exp workplace/
61. (job or jobs).ti,ab.
62. employ\$.ti,ab.
63. unemploy\$.ti,ab.
64. (shiftwork\$ or (work adj2 shift\$)).ti,ab.
65. karasek\$.ti,ab.
66. overwork\$.ti,ab.
67. ((job or work or employ\$ or occupation\$) adj (satisf\$ or condition\$ or discontent or stress\$)).ti,ab.
68. exp ACCULTURATION/
69. acculturat\$.ti,ab.
70. (migrant\$ or immigrant\$ or guest work\$).ti,ab.
71. exp Life Change Events/
72. ((trauma\$ or life) adj (change or event\$ or stress\$)).ti,ab.
73. exp natural disasters/
74. (natural disaster\$ or earthquake\$ or hurricane\$ or volcan\$ or typhoon\$ or tsunami\$ or avalanche\$ or fire\$ or flood\$).ti,ab.
75. exp FIRES/
76. exp STRESS DISORDERS, POST-TRAUMATIC/ or exp OXIDATIVE STRESS/ or exp ECHOCARDIOGRAPHY, STRESS/ or exp HEAT STRESS DISORDERS/ or exp DENTAL STRESS ANALYSIS/ or exp STRESS, MECHANICAL/ or exp STRESS FIBERS/ or exp URINARY INCONTINENCE, STRESS/ or exp FRACTURES, STRESS/ or stress disorders, traumatic, acute/ or exp exercise test/
77. ((stress or exercise) adj test\$.sh,tw.
78. exp Accidents, Occupational/
79. (occupation\$ adj (hazard\$ or accident\$)).tw.
80. or/76–79
81. 2 or 9

- 82. or/10–75
- 83. or/76–79
- 84. 82 not 83
- 85. and/6,81,84
- 86. 8 or 85
- 87. exp Cohort Studies/
- 88. Controlled Clinical Trials/
- 89. controlled clinical trial.pt.
- 90. ((incidence or concurrent) adj (study or studies)).tw.
- 91. comparative study.sh.
- 92. evaluation studies.sh.
- 93. follow-up studies.sh.
- 94. prospective studies.sh.
- 95. control\$.tw.
- 96. prospectiv\$.tw.
- 97. volunteer\$.tw.
- 98. or/87–97
- 99. 86 and 98
- 100.limit 99 to humans

Section 2: Coding procedures and variables for which data were sought

Two authors trained in systematic review coding procedures determined publication eligibility and extracted the data from the articles. Prior to coding, both authors jointly reviewed the titles and abstracts of potential publications to determine whether a given work warranted a full examination for coding purposes. Each of these publications was read independently, with each author forming an opinion on final publication eligibility, assigning a tentative subjective quality rating, and highlighting the data to be coded (see below). The two authors then met in conference to discuss each publication. A study was included if the outcome variable was all-cause mortality, the experience of a stressful life event was measured at the individual level (rather than at the neighborhood level), and a clear comparison was made between a group of people who experienced a particular stressful event and another group who did not. No restrictions were placed on the year a study was published, in what language it was originally written, or the type of outlet in which the paper appeared. Data was entered into a spreadsheet only after agreement had been reached on final publication eligibility, the number of relative risk estimates available for extraction, the values to be assigned for the study design variables (e.g. age range, baseline date) corresponding to each relative risk, and consensus had been established with

respect to the final subjective quality rating. In some cases, the data entry involved calculating relative risk estimates from raw death rates or from raw count data. For publications reporting multiple analyses of a single sample, data was sought from a statistically-unadjusted model, a model adjusted for age alone, and from the most statistically-adjusted multivariate model. Data were entered on basic spreadsheets (the data spreadsheet being later imported into SPSS for analysis). The variables we sought to obtain from publications were: 1) Author names; 2) author genders; 3) publication date; 4) publication title; 5) place of publication; 6) characteristics of high stress group (e.g. unemployed); 7) characteristics of low stress group (e.g. employed); 8) characteristics shared by both high and low stress groups; 9) percent of the sample that was male; 10) minimum age; 11) maximum age; 12) mean age; 13) ethnicity; name of data source used; 14) geographic location of study sample; 15) baseline start date (day, month, year); 16) baseline end date (day, month, year); 17) follow-up end date (day month, year); 18) maximum follow-up duration; 19) average follow-up duration; 20) information on timing of stress relative to baseline start date; 21) information on the structure of the follow-up period (e.g. were there any gaps between the end of baseline and the beginning of follow-up?); 22) statistical technique used; 23) total number of persons analyzed in the publication; 24) total number of persons analyzed for the specific effect size; 25) number of persons in the high stress group; 26) number of deaths in the high stress group; 27) number of persons in the low stress group; 28) number of deaths in the low stress group; 29) death rate in the high stress group; 30) death rate in the low stress group; 31) effect size; 32) confidence interval; 33) standard error; 34) t-statistic; 35) Chi-square statistic; 36) minimum value for p-value; 37) maximum value for p-value; 38) full list of control variables used; 39) date of data extraction; 40) subjective quality rating; 41) number of citations received by publication according to Web of Science; 42) number of citations received according to Google Scholar; 43) 5-year impact factor for place of publication.

Section 3: Additional information on the conversion of odds ratios and relative risks to hazard ratios

All non-hazard-ratio point estimates were converted to hazard ratios (the most frequently reported type) using one or both of the following equations [67]: $RR = \frac{OR}{(1-r) + (r * OR)}$ and $HR = \frac{\ln(1 - RR * r)}{\ln(1 - r)}$, where RR is the relative risk, OR is the odds ratio, HR is the hazard ratio, and r is the death rate for the reference (i.e. employed) group.

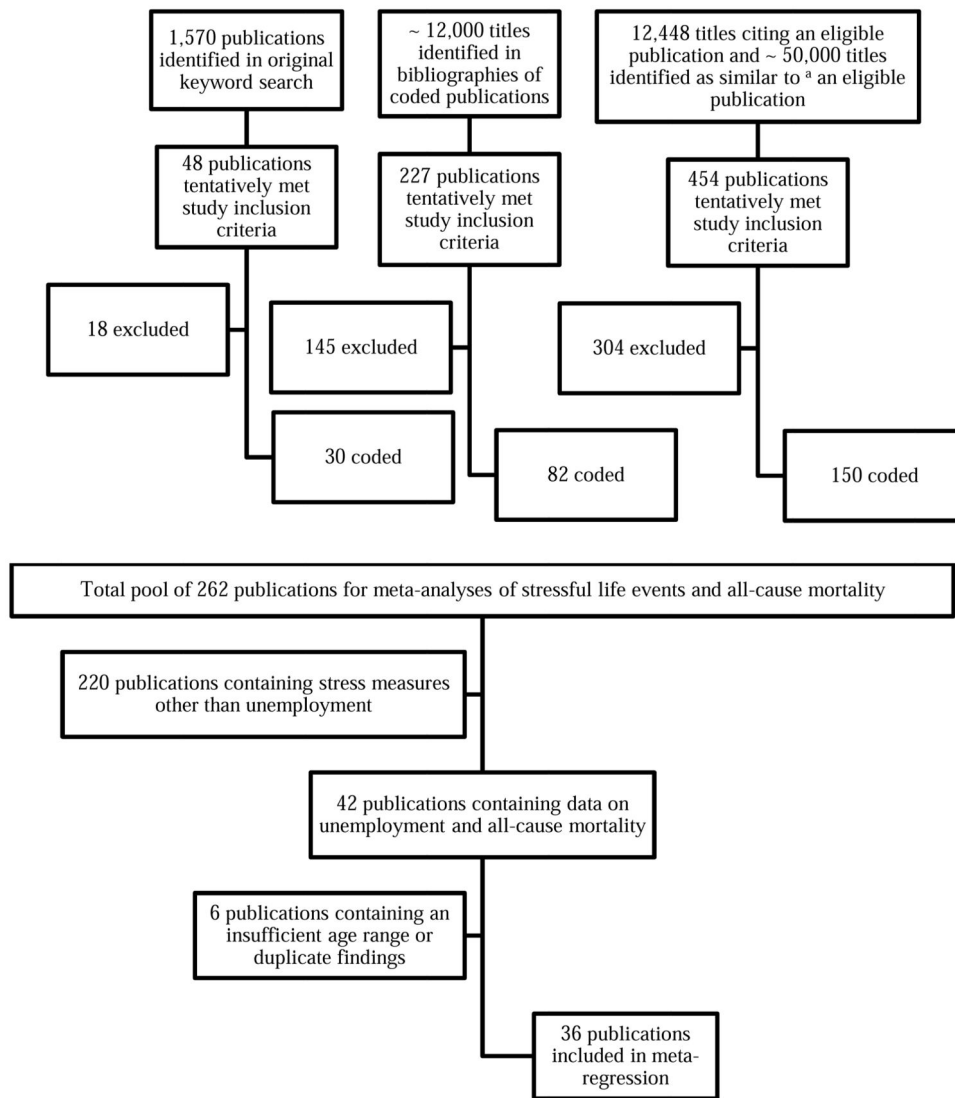


Figure 1. Flow diagram for literature search

^aHere, “similar to” refers to the definitions for article similarity used by Google Scholar and ISI Web of Science

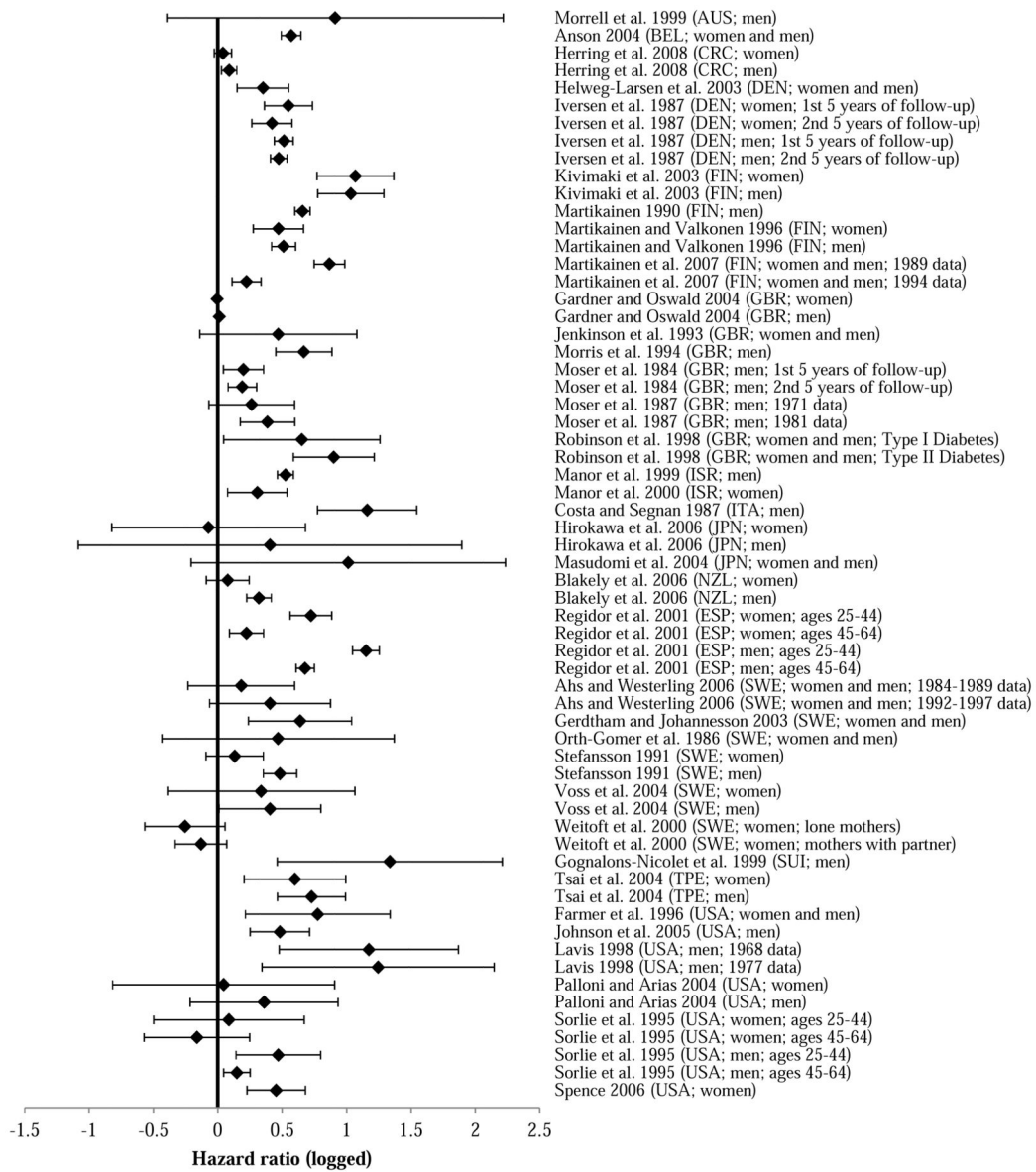


Figure 2. Forest plot of the 62 log hazard ratios (unemployed vs. employed) included in analysis, sorted by nation^a

^aAUS=Australia; BEL=Belgium; CRC=Costa Rica; DEN=Denmark; ESP=Spain; FIN=Finland; GBR=United Kingdom; ISR=Israel; ITA=Italy; JPA=Japan; NZL=New Zealand; SWE=Sweden; SUI=Switzerland; TPE=Taiwan (Taipei); USA=United States

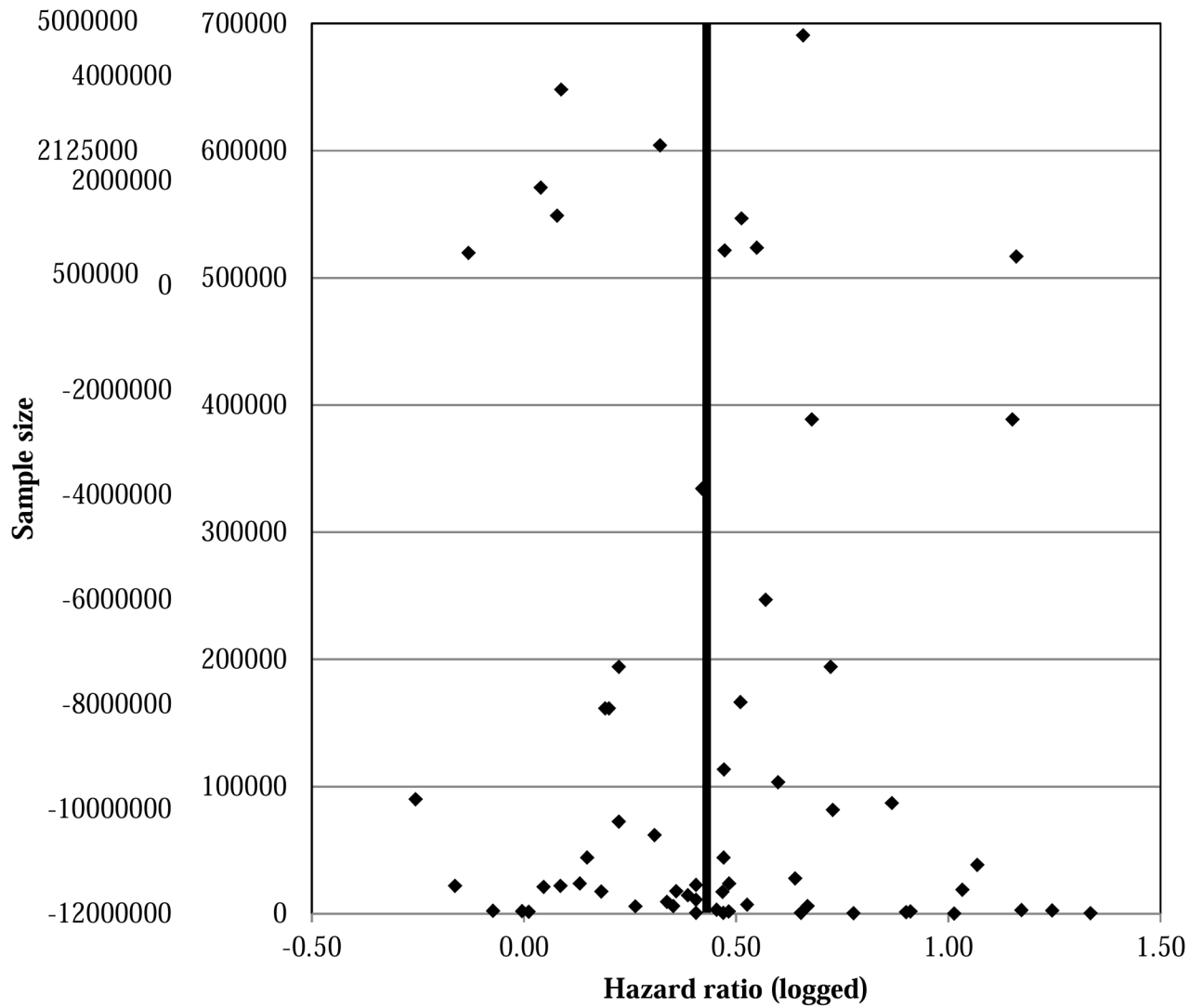


Figure 3. Funnel plot of 62 hazard ratios (logged) included in analysis vs. sample size^a
^aVertical line denotes the mean log hazard ratio of 0.43. To better show the dispersion of points, the y-axis scale is less condensed from 0 to 500 000 and more condensed from 500 000 to 5 000 000.

Table 1

Descriptive Statistics for 62 Hazard Ratios Included in Analysis

	Range	Median/Mean	Number of hazard ratios
Aggregate unemployment rate	0.7% to 29.6%		
Unemployment rate deviation ^a	-4.81 to +13.80		
Gender	Women only		20
	Mixed genders		13
	Men only		29
Mean age of study sample at baseline	<40		19
	40–49.9		30
	50–65		13
Publication year	1967 to 2001	1985 (mean)	
Comparison group	General population		4
	Employed only		58
Unemployment measure	Any non-working		13
	Unemployed only		49
	United States		11
Region	Scandinavia ^b		22
	Other nations ^c		29
			12
Controlled for sex			49
Controlled for age			7
	Educ. or income only		36
Controlled for socioeconomic status			10
	2+ controls		
Controlled for health behaviors			
Sample size	378 to 4,779,535	23,851 (median)	
Death rate estimated			19
Standard error estimated			3
Newcastle-Ottawa quality rating (range: 0 to 9)	4.0 – 9.0	7.5 (mean)	

^a Deviation of aggregate unemployment level at baseline from the within-nation overall mean over the 1960–2008 period (number of observations varies by nation; see main text for additional information)

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^bDenmark, Finland, Sweden

^cAustralia, Belgium, Costa Rica, Israel, Italy, Japan, New Zealand, Spain, Switzerland, Taiwan, United Kingdom

Table 2 Multivariate Meta-Regression Analyses Predicting the Magnitude of the Effect of Unemployment on Mortality^a

Variable	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Crude unemployment rate	1.01 (<i>p</i> = .27)					
Change in unemployment rate from previous year		0.94 (<i>p</i> = .46)				
Deviation of unemployment rate from the 5-year running average			1.01 (<i>p</i> = .87)			
Deviation of unemployment rate from the 10-year running average				1.01 (<i>p</i> = .73)		
Deviation of unemployment rate from the overall average (continuous)					1.01 (<i>p</i> = .61)	
Deviation of unemployment rate from the overall average (categorical)						
4.81% to 1.65% below overall average (Lowest 25%; reference group)						1.00
1.64% to 0.04% below overall average						0.93 (<i>p</i> = .76)
0.03% below to 2.49% above overall average						0.89 (<i>p</i> = .60)
2.50% to 13.80% above overall average (Highest 25%)						1.01 (<i>p</i> = .97)
Proportion of sample that is male (0 to 1)	1.39 (<i>p</i> < .01)	1.37 (<i>p</i> < .01)	1.37 (<i>p</i> = .01)	1.36 (<i>p</i> = .01)	1.38 (<i>p</i> < .01)	1.35 (<i>p</i> = .01)
Mean age of study sample at baseline (Reference group, <40)						
40 to 49.9	1.02 (<i>p</i> = .86)	1.00 (<i>p</i> = .98)	1.02 (<i>p</i> = .88)	1.03 (<i>p</i> = .81)	1.02 (<i>p</i> = .89)	1.00 (<i>p</i> = .99)
50 to 65	0.70 (<i>p</i> = .03)	0.70 (<i>p</i> = .04)	0.71 (<i>p</i> = .04)	0.71 (<i>p</i> = .04)	0.71 (<i>p</i> = .04)	0.69 (<i>p</i> = .05)
Study age (divided by 10)	1.13 (<i>p</i> = .21)	1.08 (<i>p</i> = .45)	1.10 (<i>p</i> = .35)	1.11 (<i>p</i> = .30)	1.12 (<i>p</i> = .29)	1.11 (<i>p</i> = .35)
Years between end of baseline and start of follow-up	0.99 (<i>p</i> = .56)	1.00 (<i>p</i> = .86)	0.99 (<i>p</i> = .77)	0.99 (<i>p</i> = .65)	0.99 (<i>p</i> = .70)	0.99 (<i>p</i> = .72)
Years between end of baseline and end of follow-up	1.01 (<i>p</i> = .58)	1.01 (<i>p</i> = .76)	1.01 (<i>p</i> = .75)	1.01 (<i>p</i> = .67)	1.01 (<i>p</i> = .68)	1.01 (<i>p</i> = .79)
Comparison group (1, General population; 0, Employed persons)	0.51 (<i>p</i> = .01)	0.59 (<i>p</i> = .05)	0.51 (<i>p</i> = .02)	0.50 (<i>p</i> = .02)	0.51 (<i>p</i> = .02)	0.49 (<i>p</i> < .01)
Unemployment measure (1, Any non-working; 0, Unemployed only)	1.16 (<i>p</i> = .23)	1.17 (<i>p</i> = .28)	1.19 (<i>p</i> = .27)	1.20 (<i>p</i> = .26)	1.19 (<i>p</i> = .25)	1.16 (<i>p</i> = .40)
Region (Reference group, Other nations ^b)						
United States	0.83 (<i>p</i> = .53)	0.81 (<i>p</i> = .48)	0.84 (<i>p</i> = .55)	0.83 (<i>p</i> = .54)	0.84 (<i>p</i> = .56)	0.87 (<i>p</i> = .68)
Scandinavia	0.83 (<i>p</i> = .42)	0.86 (<i>p</i> = .55)	0.85 (<i>p</i> = .51)	0.82 (<i>p</i> = .43)	0.84 (<i>p</i> = .48)	0.83 (<i>p</i> = .45)
Controlled for sex (1, Yes; 0, No)	0.92 (<i>p</i> = .62)	0.90 (<i>p</i> = .58)	0.92 (<i>p</i> = .64)	0.93 (<i>p</i> = .64)	0.92 (<i>p</i> = .63)	0.95 (<i>p</i> = .81)
Controlled for age (1, Yes; 0, No)	1.19 (<i>p</i> = .24)	1.26 (<i>p</i> = .15)	1.22 (<i>p</i> = .20)	1.23 (<i>p</i> = .19)	1.20 (<i>p</i> = .22)	1.27 (<i>p</i> = .21)
Controlled for socioeconomic status (Reference group, No controls)						
Controlled for only education or only income (1, Yes; 0, No)	1.11 (<i>p</i> = .65)	1.12 (<i>p</i> = .64)	1.09 (<i>p</i> = .70)	1.07 (<i>p</i> = .75)	1.10 (<i>p</i> = .69)	1.09 (<i>p</i> = .74)

Variable	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Controlled for two or more SES measures (1, Yes; 0, No)	0.98 ($p = .91$)	1.04 ($p = .86$)	1.03 ($p = .89$)	1.01 ($p = .96$)	1.00 ($p = .98$)	1.05 ($p = .82$)
Controlled for health behaviors specifically (1, Yes; 0, No)	0.68 ($p = .02$)	0.64 ($p = .01$)	0.65 ($p = .01$)	0.66 ($p = .02$)	0.66 ($p = .01$)	0.66 ($p = .02$)
Log of sample size	0.95 ($p = .16$)	0.93 ($p = .08$)	0.94 ($p = .12$)	0.94 ($p = .17$)	0.94 ($p = .14$)	0.93 ($p = .16$)
Standard error estimated? (1, Yes; 0, No)	1.15 ($p = .78$)	1.25 ($p = .69$)	1.18 ($p = .75$)	1.16 ($p = .77$)	1.16 ($p = .77$)	1.20 ($p = .75$)
Newcastle-Ottawa quality rating	0.99 ($p = .90$)	1.01 ($p = .95$)	0.99 ($p = .88$)	0.99 ($p = .92$)	0.99 ($p = .86$)	0.99 ($p = .90$)
Constant	1.80	2.28	2.31	2.02	2.09	2.41

^a All regressions calculated by maximum likelihood using a random effects model ($n = 62$ hazard ratios) with robust standard errors (which account for the effect of intra-study correlation; robust standard errors calculated using an assumed intra-study effect size correlation $\rho = .80$). The numbers reported are the exponentiated coefficients, which represent ratios of hazard ratios (i.e., the change in hazard ratio when the independent variable increases by 1 unit). P -values are given in parentheses. All relative unemployment rate measures (models 2 through 6) are based on within-nation calculations

^b Australia, Belgium, Costa Rica, Israel, Italy, Japan, New Zealand, Spain, Switzerland, Taiwan, United Kingdom