

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Mortality inequalities by occupational status and type of job in men and women. Results from the Rome Longitudinal Study
AUTHORS	Paglione, Lorenzo; Angelici, Laura; Davoli, Marina; Agabiti, Nera; Cesaroni, Giulia

VERSION 1 – REVIEW

REVIEWER	Anton Kunst Department of Public Health Amsterdam UMC University of Amsterdam
REVIEW RETURNED	24-Sep-2019

GENERAL COMMENTS	<p>The study has the potential to be of interest to an international audience, as it describes socioeconomic inequalities in mortality in the capital of Italy. The study is based on a large longitudinal registry-based study. Most of the analysis is sound. However, in the selection and presentation of the results, there are some problems which I think substantially limit the interest to an international audience.</p> <p>1. The introduction to this paper does not review previous Italian studies on socioeconomic inequalities in mortality, and it does not make clear the added value of the current study. Amongst others, it is unclear what this study adds to a previous study on the same data basis that used educational level as socioeconomic indicator. Similarly, it would be useful to specify how this study complements the recent national “atlas” study presented in Epidemiol Prev. 2019 Jan-Feb;43(1S1):1-120.</p> <p>2. Much of the results are about mortality in relationship to “sector of employment”. For studies on socioeconomic inequalities in mortality, this is not an appropriate indicator of socioeconomic studies. It’s rarely used for this purpose. The inclusion of so many results for this measure weakens the focus of the paper. Is this really about socioeconomic inequalities in mortality, or perhaps about mortality in relationship to occupational exposures?</p> <p>3. It is unclear why the authors control for educational level. Commonly, when describing socioeconomic inequalities in mortality, educational level or occupational class are used as ALTERNATIVE indicators of the same concept, and they are used one-by-one, but not simultaneously. They are complementary, not confounders, to each other.</p> <p>4. In the Discussion section, there is no systematic attempt to identify and explain key findings emerging from the analysis.</p>
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	<p>Instead, the discussion touches upon a large number of findings, each which is discussed only briefly. Explanations are commonly limited to one or two sentences per finding, thus lacking thoroughness. This is bit unsatisfactory to the reader.</p> <p>5. I feel that important findings have not been discussed in Discussion, and sometimes not even identified in the paper. For example, the smaller inequalities in mortality among women (as compared to men) seems attributable to much smaller inequalities in mortality from cancer (instead of other causes). Such a finding, when seen in the perspective of other studies, may have been used to better understand the male-female differences observed in this study.</p>
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REVIEWER	G Menvielle Inserm IPLESP, Paris, France
REVIEW RETURNED	14-Oct-2019

GENERAL COMMENTS	<p>The manuscript deals with differences in mortality by occupational status and employment group in Rome. The analysis is based on a large sample (the Rome Longitudinal Study) including about 1,5 million subjects followed-up from 2001 until 2015. The methodology is sound and the paper is well written. However, it is not clear what the paper adds to the current literature.</p> <p>First, the authors state that 'occupational status has been less used as an indicator of socioeconomic position' and that many studies restricted the analyses to male population. This is correct. However, there has been quite a large number of studies on social differences in mortality using occupational status as an indicator of socioeconomic position. There have also been studies investigating the association between occupational status (employed/not employed) and mortality. The main conclusions on differences in mortality by education and occupational class are roughly similar. If we want to provide new information in this area, a more refined modeling of occupational status (e.g. combining different aspects) should be used.</p> <p>Second, the main findings of the study: a steeped social gradient in men than in women in all cause and cause specific mortality, except for cardiovascular diseases, and a higher mortality among non working women when compared to employed women, were already known.</p> <p>Minor remark: In Table 4, why do the authors present only one CI? Why does the name for the category 'looking for their first job' differ between men and women?</p> <p>Examples of papers presenting differences in mortality by occupational status/class de Gelder R, Menvielle G, Costa G, Kovács K, Martikainen P, Strand BH, Mackenbach JP. Long-term trends of inequalities in mortality in 6 European countries. <i>Int J Public Health</i>. 2017 Jan;62(1):127-141. doi:10.1007/s00038-016-0922-9. Mackenbach JP, Kulhánová I, Artnik B, Bopp M, Borrell C, Clemens T, Costa G, Dikken C, Kalediene R, Lundberg O, Martikainen P, Menvielle G, Östergren O, Prochorskas R, Rodríguez-Sanz M, Strand BH, Looman CW, de Gelder R. Changes in mortality inequalities over two decades: register based study of European countries. <i>BMJ</i>. 2016 Apr 11;353:i1732. doi: 10.1136/bmj.i1732.</p>
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	<p>Mackenbach JP, Stirbu I, Roskam AJ, Schaap MM, Menvielle G, Leinsalu M, Kunst AE; European Union Working Group on Socioeconomic Inequalities in Health. Socioeconomic inequalities in health in 22 European countries. <i>N Engl J Med</i>. 2008 Jun 5;358(23):2468-81. doi: 10.1056/NEJMsa0707519.</p> <p>Menvielle G, Leclerc A, Chastang JF, Melchior M, Luce D; Evolution Des Inégalités Sociales Par Causes Médicales de Décès (Trend in Social Inequalities by Cause of Death) group. Changes in socioeconomic inequalities in cancer mortality rates among French men between 1968 and 1996. <i>Am J Public Health</i>. 2007 Nov;97(11):2082-7.</p> <p>Leclerc A, Chastang JF, Menvielle G, Luce D. Socioeconomic inequalities in premature mortality in France: have they widened in recent decades? <i>Soc Sci Med</i>. 2006 Apr;62(8):2035-45.</p> <p>Mackenbach JP, Bos V, Andersen O, Cardano M, Costa G, Harding S, Reid A, Hemström O, Valkonen T, Kunst AE. Widening socioeconomic inequalities in mortality in six Western European countries. <i>Int J Epidemiol</i>. 2003 Oct;32(5):830-7.</p>
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REVIEWER	<p>Esther de Vries and Natalia Godoy Department of Clinical Epidemiology and Biostatistics, Faculty of Medicine, Pontificia Universidad Javeriana, Bogotá Colombia</p>
REVIEW RETURNED	05-Nov-2019

GENERAL COMMENTS	<p>Article: "Mortality inequalities by occupational status and employment group in men and women. Results from the Rome Longitudinal Study"</p> <p>Key message of the manuscript:</p> <p>This is an observational study based on a large administrative cohort that aims to investigate the association between job characteristics and mortality in men and women over 14 years using a survival analysis. The title mentions "mortality inequalities" but it seems to be more an analysis of survival rather than mortality as few mortality rates are presented. The work in itself has been nicely done and there is detailed information by employment group. Major limitation is the lack of information on duration of employment and potential changes in job type and employment status over time, but this limitation does not seem to be able to be solved because of the study design.</p> <p>Our main comments are listed below.</p> <p>Major comments:</p> <p>Introduction</p> <p>- Whereas the authors refer to all socioeconomic differences in health as inequitable and without "biological substrate", this is not always true for work-related differences in health. Whereas they may be unfair, they may have a biological or mechanical substrate. Think of fishermen who have a higher than average risk of drowning, even if their boats comply with all safety standards – heavy weather at sea cannot always be avoided. Similarly, drivers have a higher risk of traffic accidents which may have serious health effects or, even worse, deadly consequences. Therefore, we feel that some additional exploration of work-related mortality and their inequalities may be provided. Of course, it is true that "occupational status and type of job are strongly related to living</p>
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	<p>standards and material resources. They can reflect social standing and access to better care, represent social network and stress control, and determine exposure to professional hazard” but these professional hazards are not necessarily sign of inequities (although many times they are).</p> <p>- Also, the statement describing work conditions for women in Europe (lines 52-55 page 5) is quite strong and needs to be contextualized in order to not generalize. The reference used to support this statement is a study that aimed to analyze gender differences in the impact of long work hours on a variety of health outcomes using data from the 2002 Catalan Health Survey in Spain.</p> <p>Study design and population</p> <p>- The study design and population are well described.</p> <p>- Work-related variables are clearly described and although the subtitle of this section mentions possible confounders in the text they are not mentioned.</p> <p>Statistical analyses</p> <p>- Whilst the authors mention that they “used Cox proportional hazard models to investigate the association between the three occupational variables (occupational status, type of job and sector of employment) and mortality (all-cause and cause-specific) in men and women”, they report the evaluation of the proportional hazards assumption “plotting Kaplan Meier curves and Schoenfeld residuals for the work-related variables, place of birth and level of education”. This leads the reader to think that they used Cox PH models to investigate the association between the occupational variables, place of birth and level of education? Could the authors please explain if the occupational variables in the first phrase of this paragraph are the same as the “work-related variables” mentioned in the phrase regarding the proportionality assumption?</p> <p>- In order to evaluate better the survival experience, it would be highly useful to include Kaplan-Meier results in addition to the Cox PH Hazard Ratios. Moreover, it would be important to contemplate if the censoring (which is unfortunately not reported) is informed or non-informed. Migrants may be particularly either healthier and migrating because of job opportunities, for example, or unhealthy and moving back to their place of origin (of particular concern in Rome, with its large proportion of migrants). If this is the case, the assumptions underlying the Kaplan-Meier and Cox PH hazards are not met and in the discussion section, a discussion on the likely bias that this introduces is warranted. The authors could, for example, evaluate if the distinct migrant groups migrate at young ages, if they are censored in the same proportion as the non-migrants, etc, to get an idea if these effects were likely to have occurred.</p> <p>- Was it not possible to link with cause-of-death / vital statistics data from the rest of Europe to help circumvent partially this large amount of censored people because of emigration?</p> <p>- This study depends entirely on the quality of the census information and, perhaps more importantly, on the completeness and quality of vital statistics and cause-of-death registration in the</p>
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	<p>Rome area. The quality of these data is completely lacking and must be included.</p> <ul style="list-style-type: none"> - The authors mention in the STROBE checklist that there were no subjects with missing data. Is that true? Were all subjects completely characterized in terms of marital status, age, occupational status, etc.? If so, please describe this in the methods section. <p>Results:</p> <ul style="list-style-type: none"> - A study flow diagram could be included describing the selection of the subjects included in the study, including filtering based on data quality, data availability and linkage. - A description of lost to follow-up is lacking until one reaches the "limitations" section in the discussion. The numbers of censored must be mentioned in either methods or results section and, as mentioned above, evaluated in more detail in the discussion section as to the potential consequences of over- or underestimating survival and hazards. As the authors mention, Rome is a city of migrants, and migrants may come to the city but also leave it more easily than non-migrants. Migrants may also have a different disease and mortality pattern. A sensitivity analysis or including a "national migrant", "international migrant" and "Rome born" variable may give valuable insight into this mechanism. If many migrants were censored because of migration, this may be a reason to exclude the migrants from the analyses. - Please be more clear, when referring to HR in the text, if it is an age-adjusted or fully adjusted model. <p>Discussion</p> <ul style="list-style-type: none"> - Authors mention "strong inequalities" and "much higher mortality risks". Firstly, the study presents Hazard Ratios, not mortality risks... which is a slight difference in interpretation. Secondly, without absolute differences it is very difficult for the reader to interpret if a hazard ratio of 1.15 for housewives vs working women for all-cause mortality as reported is "much higher" ... - Although the authors mention the selection bias caused by the censoring for emigration, this should be further detailed. Also, more information should be provided on the fact of not having information on possible changes in the status or in the type of job during the 14 years follow-up and what consequences this can have on the results. <p>Table 1: one could include the % censored in each group in this table.</p> <p>Table 4: consider including in the title the fact that this table pertains only to the working part of the population.</p> <p>Figure 1: include a clear description of the x-axis (presumably mortality rate-ratios, were these adjusted for age or some other variable)?</p> <p>Minor comments:</p> <ul style="list-style-type: none"> - Throughout the text, some typos occurred, as well as errors in the tenses etc. A good revision of the style would help. Please include the reference number before the dot at the end of the sentence – a reference is part of the sentence and must be included in the phrases. - Some examples of small textual errors:
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	<ul style="list-style-type: none"> o Results section, on table 2: “11.5% of men and only 2.6% OF WOMEN were highly qualified manual workers”. o Please use past tense in the results section rather than present tense. o Table 2: Agriculture must be agriculture and hunting must be hunting
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VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Reviewer Name: Anton Kunst

The study has the potential to be of interest to an international audience, as it describes socioeconomic inequalities in mortality in the capital of Italy. The study is based on a large longitudinal registry-based study. Most of the analysis is sound. However, in the selection and presentation of the results, there are some problems which I think substantially limit the interest to an international audience.

We thank prof. Kunst for his useful advises.

1. The introduction to this paper does not review previous Italian studies on socioeconomic inequalities in mortality, and it does not make clear the added value of the current study. Amongst others, it is unclear what this study adds to a previous study on the same data basis that used educational level as socioeconomic indicator. Similarly, it would be useful to specify how this study complements the recent national “atlas” study presented in Epidemiol Prev. 2019 Jan-Feb;43(1S1):1-120.

We enriched the introduction with an update of the literature, explaining how our work can integrate the existing published articles and the Atlas published on E&P.

2. Much of the results are about mortality in relationship to “sector of employment”. For studies on socioeconomic inequalities in mortality, this is not an appropriate indicator of socioeconomic studies. It’s rarely used for this purpose. The inclusion of so many results for this measure weakens the focus of the paper. Is this really about socioeconomic inequalities in mortality, or perhaps about mortality in relationship to occupational exposures?

We eliminated sector of employment from the paper and we think the work resulted fluent.

3. It is unclear why the authors control for educational level. Commonly, when describing socioeconomic inequalities in mortality, educational level or occupational class are used as ALTERNATIVE indicators of the same concept, and they are used one-by-one, but not simultaneously. They are complementary, not confounders, to each other.

We eliminated educational level from the evaluation in Table 3.

4. In the Discussion section, there is no systematic attempt to identify and explain key findings emerging from the analysis. Instead, the discussion touches upon a large number of findings, each which is discussed only briefly. Explanations are commonly limited to one or two sentences per finding, thus lacking thoroughness. This is bit unsatisfactory to the reader.

We changed the discussion focusing on few results. We hope it became more satisfactory to the reader

5. I feel that important findings have not been discussed in Discussion, and sometimes not even identified in the paper. For example, the smaller inequalities in mortality among women (as compared to men) seems attributable to much smaller inequalities in mortality from cancer (instead of other causes). Such a finding, when seen in the perspective of other studies, may have been used to better understand the male-female differences observed in this study.

We focused the discussion on main findings, including sex differences in specific outcomes. We enriched the discussion section also explaining some different findings from other more recent Italian studies.

Reviewer: 2

The manuscript deals with differences in mortality by occupational status and employment group in Rome. The analysis is based on a large sample (the Rome Longitudinal Study) including about 1,5 million subjects followed-up from 2001 until 2015. The methodology is sound and the paper is well written.

However, it is not clear what the paper adds to the current literature.

First, the authors state that 'occupational status has been less used as an indicator of socioeconomic position' and that many studies restricted the analyses to male population. This is correct. However, there has been quite a large number of studies on social differences in mortality using occupational status as an indicator of socioeconomic position. There have also been studies investigating the association between occupational status (employed/not employed) and mortality. The main conclusions on differences in mortality by education and occupational class are roughly similar. If we want to provide new information in this area, a more refined modeling of occupational status (e.g. combining different aspects) should be used.

We thank Dr. Menvielle for her precise comments. As our first attempt to investigate occupation inequalities in mortality in Rome, we preferred to use a standard approach in order to be able to compare the magnitude of association with national and international studies. We found some interesting aspect that differ from other studies conducted in Italy and Europe (see Discussion Section)

Second, the main findings of the study: a steeped social gradient in men than in women in all cause and cause specific mortality, except for cardiovascular diseases, and a higher mortality among non working women when compared to employed women, were already known.

You are right, but in our model we explored other aspects related to job characteristics including several categories of non-manual and manual work. While non working women's higher risk has been reported in other countries, in Italy recent data are scanty.

Minor remark: In Table 4, why do the authors present only one CI? Why does the name for the category 'looking for their first job' differ between men and women?

We deleted from the table the second HR, controlled for educational level, in order to respond to reviewer 1.

Examples of papers presenting differences in mortality by occupational status/class

de Gelder R, Menvielle G, Costa G, Kovács K, Martikainen P, Strand BH, Mackenbach JP. Long-term trends of inequalities in mortality in 6 European countries. *Int J Public Health*. 2017 Jan;62(1):127-141. doi:10.1007/s00038-016-0922-9.

Mackenbach JP, Kulhánová I, Artnik B, Bopp M, Borrell C, Clemens T, Costa G, Dibben C, Kalediene R, Lundberg O, Martikainen P, Menvielle G, Östergren O, Prochorskas R, Rodríguez-Sanz M, Strand BH, Looman CW, de Gelder R. Changes in mortality inequalities over two decades: register based study of European countries. *BMJ*. 2016 Apr 11;353:i1732. doi: 10.1136/bmj.i1732.

Mackenbach JP, Stirbu I, Roskam AJ, Schaap MM, Menvielle G, Leinsalu M, Kunst AE; European Union Working Group on Socioeconomic Inequalities in Health. Socioeconomic inequalities in health in 22 European countries. *N Engl J Med*. 2008 Jun 5;358(23):2468-81. doi: 10.1056/NEJMs0707519.

Menvielle G, Leclerc A, Chastang JF, Melchior M, Luce D; Evolution Des Inégalités Sociales Par Causes Médicales de Décès (Trend in Social Inequalities by Cause of Death) group. Changes in socioeconomic inequalities in cancer mortality rates among French men between 1968 and 1996. *Am J Public Health*. 2007 Nov;97(11):2082-7.

Leclerc A, Chastang JF, Menvielle G, Luce D. Socioeconomic inequalities in premature mortality in France: have they widened in recent decades? *Soc Sci Med*. 2006 Apr;62(8):2035-45.

Mackenbach JP, Bos V, Andersen O, Cardano M, Costa G, Harding S, Reid A, Hemström O, Valkonen T, Kunst AE. Widening socioeconomic inequalities in mortality in six Western European countries. *Int J Epidemiol*. 2003 Oct;32(5):830-7.

Thank you. We read the papers and included them in our references.

Reviewer: 3

Key message of the manuscript:

This is an observational study based on a large administrative cohort that aims to investigate the association between job characteristics and mortality in men and women over 14 years using a survival analysis. The title mentions “mortality inequalities” but it seems to be more an analysis of survival rather than mortality as few mortality rates are presented. The work in itself has been nicely done and there is detailed information by employment group. Major limitation is the lack of information on duration of employment and potential changes in job type and employment status over time, but this limitation does not seem to be able to be solved because of the study design.

We thank prof. Esther de Vries and Dr. Natalia Godoy for their comments. Since we analyzed cause-specific mortality as outcomes, although it was a survival analysis and we did not present mortality rates, we would like to maintain the title as it is.

Our main comments are listed below.

Major comments:

Introduction

- Whereas the authors refer to all socioeconomic differences in health as inequitable and without “biological substrate”, this is not always true for work-related differences in health. Whereas they may be unfair, they may have a biological or mechanical substrate. Think of fishermen who have a higher than average risk of drowning, even if their boats comply with all safety standards – heavy weather at sea cannot always be avoided. Similarly, drivers have a higher risk of traffic accidents which may have serious health effects or, even worse, deadly consequences. Therefore, we feel that some additional exploration of work-related mortality and their inequalities may be provided. Of course, it is

true that “occupational status and type of job are strongly related to living standards and material resources. They can reflect social standing and access to better care, represent social network and stress control, and determine exposure to professional hazard” but these professional hazards are not necessarily sign of inequities (although many times they are).

Thank you for your comment. We slightly modified the part on inequalities in the introduction section. To respond to the first reviewer, we deleted from the work the analyses on job sectors.

- Also, the statement describing work conditions for women in Europe (lines 52-55 page 5) is quite strong and needs to be contextualized in order to not generalize. The reference used to support this statement is a study that aimed to analyze gender differences in the impact of long work hours on a variety of health outcomes using data from the 2002 Catalan Health Survey in Spain.

We agree. We rephrased the sentence.

Study design and population

- The study design and population are well described.

Thank you

- Work-related variables are clearly described and although the subtitle of this section mentions possible confounders in the text they are not mentioned.

We changed the subtitle.

Statistical analyses

- Whilst the authors mention that they “used Cox proportional hazard models to investigate the association between the three occupational variables (occupational status, type of job and sector of employment) and mortality (all-cause and cause-specific) in men and women”, they report the evaluation of the proportional hazards assumption “plotting Kaplan Meier curves and Schoenfeld residuals for the work-related variables, place of birth and level of education”. This leads the reader to think that they used Cox PH models to investigate the association between the occupational variables, place of birth and level of education? Could the authors please explain if the occupational variables in the first phrase of this paragraph are the same as the “work-related variables” mentioned in the phrase regarding the proportionality assumption?

We changed the text accordingly to your suggestions, explaining better the analysis and modifying the text.

- In order to evaluate better the survival experience, it would be highly useful to include Kaplan-Meier results in addition to the Cox PH Hazard Ratios. Moreover, it would be important to contemplate if the censoring (which is unfortunately not reported) is informed or non-informed. Migrants may be particularly either healthier and migrating because of job opportunities, for example, or unhealthy and moving back to their place of origin (of particular concern in Rome, with its large proportion of migrants). If this is the case, the assumptions underlying the Kaplan-Meier and Cox PH hazards are not met and in the discussion section, a discussion on the likely bias that this introduces is warranted. The authors could, for example, evaluate if the distinct migrant groups migrate at young ages, if they are censored in the same proportion as the non-migrants, etc, to get an idea if these effects were likely to have occurred.

We added a flow chart to explain censoring and selection of the population, and the Kaplan-Meier curves for all-cause mortality. Reading the text, it was not clear that “migrants” was not

intended as international migration, but as censoring for changing residence outside the municipality of Rome. We explained better this part in the revised text, in the last part of “discussion” section.

- Was it not possible to link with cause-of-death / vital statistics data from the rest of Europe to help circumvent partially this large amount of censored people because of emigration?

Unfortunately, it is not even possible to have Italian vital statistics on those who migrated.

- This study depends entirely on the quality of the census information and, perhaps more importantly, on the completeness and quality of vital statistics and cause-of-death registration in the Rome area. The quality of these data is completely lacking and must be included.

- The authors mention in the STROBE checklist that there were no subjects with missing data. Is that true? Were all subjects completely characterized in terms of marital status, age, occupational status, etc.? If so, please describe this in the methods section.

Our cohort is a result of a national project in managing health data with a population-based approach. Is not a collection of data taken from different sources, but an integration of different sources. We have no missing data in our cohort because it is created by the population census, by law a comprehensive evaluation of the population. We added in the last part of “discussion” section a sentence explaining this.

Results:

- A study flow diagram could be included describing the selection of the subjects included in the study, including filtering based on data quality, data availability and linkage.

We provided a detailed flow chart explaining the selection of the population.

- A description of lost to follow-up is lacking until one reaches the “limitations” section in the discussion. The numbers of censored must be mentioned in either methods or results section and, as mentioned above, evaluated in more detail in the discussion section as to the potential consequences of over- or underestimating survival and hazards. As the authors mention, Rome is a city of migrants, and migrants may come to the city but also leave it more easily than non-migrants. Migrants may also have a different disease and mortality pattern. A sensitivity analysis or including a “national migrant”, “international migrant” and “Rome born” variable may give valuable insight into this mechanism. If many migrants were censored because of migration, this may be a reason to exclude the migrants from the analyses.

Following the first reviewer suggestions, we simplified the analysis considering only age adjusted models. We added the flow chart with selection and migration by employment status. The figure shows no huge differences.

We extended the explanation on migrants in our cohort in the last part of the discussion section.

- Please be more clear, when referring to HR in the text, if it is an age-adjusted or fully adjusted model.

In this version, the HRs are age adjusted only.

Discussion

- Authors mention “strong inequalities” and “much higher mortality risks”. Firstly, the study presents

Hazard Ratios, not mortality risks... which is a slight difference in interpretation. Secondly, without absolute differences it is very difficult for the reader to interpret if a hazard ratio of 1.15 for housewives vs working women for all-cause mortality as reported is “much higher” ...

We rephrased those sentences.

- Although the authors mention the selection bias caused by the censoring for emigration, this should be further detailed. Also, more information should be provided on the fact of not having information on possible changes in the status or in the type of job during the 14 years follow-up and what consequences this can have on the results.

We provided a more detailed explanation for this correct observation in the last part of “discussion” paragraph.

Table 1: one could include the % censored in each group in this table.

We added censoring in the flow chart in order not to burdening tables.

Table 4: consider including in the title the fact that this table pertains only to the working part of the population.

We added this explanation in the table 4 title.

Figure 1: include a clear description of the x-axis (presumably mortality rate-ratios, were these adjusted for age or some other variable)?

We agree, but we eliminated the figure about the sector of employment.

Minor comments:

- Throughout the text, some typos occurred, as well as errors in the tenses etc. A good revision of the style would help. Please include the reference number before the dot at the end of the sentence – a reference is part of the sentence and must be included in the phrases.

The journal requires the reference after the punctuation.

- Some examples of small textual errors:

- o Results section, on table 2: “11.5% of men and only 2.6% OF WOMEN were highly qualified manual workers”.
- o Please use past tense in the results section rather than present tense.

Done.

- o Table 2: Agriculture must be agriculture and hounting must be hunting

Thanks, but we eliminated the sector of employment.

VERSION 2 – REVIEW

REVIEWER	Anton Kunst Department of Public and Occupation Health Amsterdam UMC Netherlands
REVIEW RETURNED	11-Feb-2020

GENERAL COMMENTS	<p>This study provides important new descriptive information on socioeconomic inequalities in mortality within Europe. These estimates from Rome complement previous estimates from other parts of Italy (e.g. Turin) and for other socioeconomic indicators (education). The data are detailed and reliable and the statistical techniques are appropriate. The presentation and interpretation of the results is careful and balanced.</p> <p>I have a few suggestions for improvement and clarification.</p> <ol style="list-style-type: none"> 1. While the paper is well written, the text of the Abstract has a clearly lower quality. I suggest the authors to re-edit this text. 2. The authors may need to clarify whether subjects may have been lost to follow-up for other reasons than death and migration, e.g. due to administrative or linkage problems. 3. It would be useful to provide a reference to the job classification. Is this classification based on a sociological theory or scheme? Is it a standard Italian classification? 4. The position of “Director” in this classification is unclear. This is not a common term in social class schemes. What makes it distinct from the “high qualified non-manual”? 5. In the Results section, much of table 1 is not yet discussed, especially the distribution of the population according to the demographic characteristics. 6. The Introduction, at page 8 rows 32 to 47, the authors seem to hypothesize that women who work would have more health problems. The results are however in the opposite direction. I think that the authors should address this discrepancy in the Discussion section. 7. More generally, I would think that the discussion on women’s results, at page 17 row 20 to below, could be reconsidered. I think that there is insufficient empirical support for the statement around row 30. Moreover, the authors should re-consider the small differences in cancer mortality – this may perhaps also relate to small inequalities in alcohol use and lack of inequalities in breast cancer mortality 8. In general, the authors may give more attention to the “healthy worker effect”, which may contribute to the differences in mortality according to employment position (table 3). 9. I would remove the isolated remark on the economic crisis at the very end of the limitations section (page 18, row 40). 10. I found the conclusions quite general and nothing new compared to all recommendations that have often been made
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	<p>regarding inequalities in health. I would be more interested in one or two implications for Italy that the authors would specifically derive from their own results.</p> <p>11. I would think that Figure 1 could be removed and replaced by a bit more information in the main text. This is not a systematic review for which flow diagrams are common.</p> <p>12. What is meant by “single again” in Table 1? Is this “married again”?</p> <p>13. In Table 1, I would use the same classification as used in Table 3, in order to maintain coherence between the tables.</p>
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REVIEWER	Esther de Vries and Natalia Godoy Department of Clinical Epidemiology and Biostatistics, Pontificia Universidad Javeriana, Bogota, Colombia
REVIEW RETURNED	22-Jan-2020

GENERAL COMMENTS	<p>The authors have substantially improved the manuscript, recommendations were incorporated where possible.</p> <p>As a very minor observation: In the discussion section, the paragraph comparing mortality from traffic accidents, CVD and other accidents is a bit confusing, since it is written as one paragraph and the traffic accidents and other accidents are “interrupted” by the CVD. As a reader, one thinks that this is all part of an argument explaining the traffic accidents, whereas actually, we guess it is just a list of observed/expected directions of the results of the study in comparison with existing literature. Perhaps the authors can improve this a bit to avoid confusion, although the contents in itself are correct, it is more a matter of style and organization of this bit of the text.</p>
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REVIEWER	G Menvielle Inserm, France
REVIEW RETURNED	22-Jan-2020

GENERAL COMMENTS	The paper has improved. It is well written and clear.
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VERSION 2 – AUTHOR RESPONSE

Reviewer: 3

We thank prof. Esther de Vries and Dr. Natalia Godoy for this second revision.

As a very minor observation: In the discussion section, the paragraph comparing mortality from traffic accidents, CVD and other accidents is a bit confusing, since it is written as one paragraph and the traffic accidents and other accidents are “interrupted” by the CVD. As a reader, one thinks that this is all part of an argument explaining the traffic accidents, whereas actually, we guess it is just a list of observed/expected directions of the results of the study in comparison with existing literature. Perhaps the authors can improve this a bit to avoid confusion, although the contents in itself are correct, it is more a matter of style and organization of this bit of the text.

We reordered this paragraph according to this suggestion.

Reviewer: 2

Thank you Dr. Menvielle for your revision.

Reviewer: 1

This study provides important new descriptive information on socioeconomic inequalities in mortality within Europe. These estimates from Rome complement previous estimates from other parts of Italy (e.g. Turin) and for other socioeconomic indicators (education). The data are detailed and reliable and the statistical techniques are appropriate. The presentation and interpretation of the results is careful and balanced.

Thank you Professor Kunst for your revision.

I have a few suggestions for improvement and clarification.

1. While the paper is well written, the text of the Abstract has a clearly lower quality. I suggest the authors to re-edit this text.

Done.

2. The authors may need to clarify whether subjects may have been lost to follow-up for other reasons than death and migration, e.g. due to administrative or linkage problems.

Each subject had an anonymous identifier which allowed to link census data with vital status, so we do not think that there could be a linkage problem. The migration could be artificial, because a small percentage of population could take the residence outside Rome for fiscal advantages (taxes on the second property house within a family). However, when we used inverse probability weighting to take account of censoring for migration in a previous work (Cacciani et al. 2015), we did not find different results.

3. It would be useful to provide a reference to the job classification. Is this classification based on a sociological theory or scheme? Is it a standard Italian classification?

This classification is based on the 14th Italian General Census of 2001. We attached the individual survey with the original, more complex, definition, from which we had deduced our classification.

4. The position of "Director" in this classification is unclear. This is not a common term in social class schemes. What makes it distinct from the "high qualified non-manual"?

The position of Director is distinct in the General Census survey from the high-qualified non-manual, and defined as "director of complex organizations". It indicates an apical managerial role, we renamed the category as Manager. The "high qualified non manual" category includes professions with technical, intellectual, scientific highly specialized skills (doctors, school and university professors, musicians and actors, lawyers, researchers, etc.)

5. In the Results section, much of table 1 is not yet discussed, especially the distribution of the population according to the demographic characteristics.

We enriched the results section on table 1, providing a more detailed description of contents.

6. The Introduction, at page 8 rows 32 to 47, the authors seem to hypothesize that women who work would have more health problems. The results are however in the opposite direction. I think that the authors should address this discrepancy in the Discussion section.

We decide to delete the statement about women's job in the introduction, and to add some specification in the last part of discussion section.

7. More generally, I would think that the discussion on women's results, at page 17 row 20 to below, could be reconsidered. I think that there is insufficient empirical support for the statement around row 30. Moreover, the authors should re-consider the small differences in cancer mortality – this may perhaps also relate to small inequalities in alcohol use and lack of inequalities in breast cancer mortality

Thank you for this comment. We slightly modified the paragraph, including the suggestion of breast cancer as a possible explanation. Breast cancer incidence is higher in high than in low social classes.

8. In general, the authors may give more attention to the “healthy worker effect”, which may contribute to the differences in mortality according to employment position (table 3).

We added a sentence on the healthy worker effect in the discussion.

9. I would remove the isolated remark on the economic crisis at the very end of the limitations section (page 18, row 40).

Done.

10. I found the conclusions quite general and nothing new compared to all recommendations that have often been made regarding inequalities in health. I would be more interested in one or two implications for Italy that the authors would specifically derive from their own results.

We add three statements about a critical lecture of the data for our context.

11. I would think that Figure 1 could be removed and replaced by a bit more information in the main text. This is not a systematic review for which flow diagrams are common.

Done.

12. What is meant by “single again” in Table 1? Is this “married again”?

We corrected this definition in “Divorced or Separated”.

13. In Table 1, I would use the same classification as used in Table 3, in order to maintain coherence between the tables.

Some groups in table 3 remain different because we grouped together some categories described in table 1. In the methods section we provided the details on how and why we reclassified the categories.

VERSION 3 – REVIEW

REVIEWER	Anton Kunst Department of Public Health Amsterdam UMC University of Amsterdam
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REVIEW RETURNED	01-Apr-2020
GENERAL COMMENTS	<p>The authors have responded adequately to my comments on the previous version.</p> <p>My only concern is that the English language may be improved. Two examples:</p> <ul style="list-style-type: none"> - the word "apical" (not used in the previous version) is, to my knowledge, not common English - the new version of the concluding sentence speaks about "urge" whereas "need" is implied.

VERSION 3 – AUTHOR RESPONSE

Thank you for this comment, we have provided a new and more detailed revision of the language of our manuscript.