

PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (<http://bmjopen.bmj.com/site/about/resources/checklist.pdf>) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

ARTICLE DETAILS

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| TITLE (PROVISIONAL) | Increase in Assisted Suicide in Switzerland: Did the socioeconomic predictors change? Results from the Swiss National Cohort |
| AUTHORS | Steck, Nicole; Junker, Christoph; Zwahlen, Marcel |

VERSION 1 – REVIEW

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| REVIEWER | Kenneth Chambaere Vrije Universiteit Brussel (VUB) & Ghent University, Belgium |
| REVIEW RETURNED | 15-Dec-2017 |

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| GENERAL COMMENTS | <p>Thank you and congratulations to the authors for a highly intriguing contribution. Very important analysis as a monitoring tool for a highly contentious issue in the UK and worldwide. Nonetheless, I have to report a number of issues here, mostly related to clarity of presentation and excessiveness of results. My comments per section are the following:</p> <p>Abstract: not the clearest abstract. results section could benefit from more figures and %s + vulnerability assertion in conclusion is confusing</p> <p>Introduction:</p> <ul style="list-style-type: none">- please provide a descriptive definition for assisted suicide and euthanasia, for readers not well acquainted with the terminology.- supplementary table 1: is this necessary? if already published somewhere you can just reference that publication- did the authors have specific hypotheses about shifts and which would indicate potential slippery slope effects (i.e. an empirical operationalisation of slippery slope)? <p>Methods:</p> <ul style="list-style-type: none">- use of some variables from 2000 e.g. religion as a fixed characteristic of a person has its problems. This is a considerable limitation given that these variables are the core of your research focus...- identification of assisted suicides: seems like a not so straightforward affair. Are the authors confident they have avoided false positives and false negatives? If so, could this confidence be conveyed in the text?- supplementary table 2 is also not necessary in my opinion- the split in age groups is not argued. Why stratify in two age groups? The choice of time periods are explained, but not the choice of age groups- why the choice for the crude rate and not simply the %s? crude rate is a bit unusual in this regard in my view- are there any potentially relevant socio-economic factors the |
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| | <p>researchers do not have at their disposal? (given the slippery slope concern...)</p> <p>Results:</p> <ul style="list-style-type: none"> - overall: too many supplementary tables; the authors had best pick the most pertinent information to show in supplementary material. - figure 1: the logarithmic scale makes the time trends seem less pronounced. Also why a split between men and women all of a sudden? Not really a relevant distinction given similar trends. Gender differences given in the results section text are not apparent from the figure. - table 2: what does "No" signify at the end of the table? No condition listed/missing? - as a reader I would prefer the results be presented in supplementary table 4 (%s within clinical groups) as opposed to table 2 (% within all assisted deaths). Also here the split between age groups and gender is blurring the main message of your study and conflating tables and figures. - I point to figure 2 in this regard: there is too much information in this figure, and also it takes time to visually see/find the results you mention in the text. It would also help if you described the results in the order in which you present them in the figure (i.e. by age group, if you choose to keep it this way). The description could be boiled down to the most pertinent results. - was it not possible to do year-by-year analysis instead of in periods? Could be more useful for presentation and clarity I feel. - the title speaks of socioeconomic factors, yet the clinical factor of cause of death is also prominent in your analysis? - multivariable analysis: did the authors check for multicollinearity issues? I may have overlooked but did not find a discussion of this, though the issue is likely. - why both Cox and logistic? I also did not find a rationale for this. Would it not be better to choose one technique and report only in the text about differences with the other technique, as a sort of sensitivity analysis? <p>Discussion:</p> <ul style="list-style-type: none"> - overall, there is just too much to discuss and explain: socioeconomic gradients in themselves, changes in those gradients over time, differences between sexes and ages, differences between analysis techniques. I'm afraid there is just too much compressed in one paper. - one main finding is that influential socioeconomic factors before 2008 were still influential after 2008, but which have become more pronounced, and which less? Do we see trends in their (relative) "importance"? The factors themselves and shifts in them are not discussed, while it is the question in your title. Instead, the second paragraph is merely a discussion of the general increase in number of cases. - intriguing that having children gives lower odds of assisted suicide. This has been found in the Netherlands as well (Snijdewind et al JAMA Intern Med 2015). Do the authors have a hypothesis for this? - is "votations" an existing word? - the strengths and limitations section is comprehensive and well balanced. <p>References: reference 15 can be updated, there is new data available for 2015 in the Netherlands (Onwuteaka-Philipsen NEJM 2017)</p> |
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| REVIEWER | Samia Hurst Institute for Ethics, History, and the Humanities Faculty of Medicine, University of Geneva Switzerland |
| REVIEW RETURNED | 27-Dec-2017 |

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| GENERAL COMMENTS | <p>This is a very detailed study of trends in the demographic characteristics of patients who died by suicide assistance in Switzerland between 2003 and 2014. These are important data as concerns regarding the risk of a slippery slope regularly surround assisted death and could be substantiated by an increase in the cases of suicide assistance for particularly vulnerable persons. Although the manuscript requires English-language editing, it is otherwise well-written, clear, and interesting, with informative tables. My main comments are as follows:</p> <p>1) The authors describe this concern regarding a so-called "slippery slope" in the introduction as part of the justification for their study. I was disappointed to see no discussion of whether or not they believe their results to confirm or dis-confirm this concern.</p> <p>2) The authors compared their data to that provided by the Swiss Right-to-Die associations.</p> <p>2.1) This is mentioned on page 5 of 30 with no reference: are these unpublished data or should there be a reference?</p> <p>2.2) On page 6 of 30, the authors report that 95.2% of the suicide assistance cases they identified were also reported by the Right-to-Die associations. Is this due to a) almost all cases are indeed performed by these associations or b) cases not performed by these associations are not being reported as well as those that are? I would welcome discussion of these two possibilities. It is plausible that suicide assistance cases performed by individual doctors not associated with Right-to-Die associations are indeed being less well reported. Other data suggest that these doctors sometimes wrongly sign off on these cases as if they were natural deaths. They may mistakenly believe that since no violence occurred, or because the cause of death is known, this is allowed when in fact it is not. They may be loath to call police to a deathbed scene at home with family members present. They may also of course also possibly act in a self-protective manner if they are uncertain as to whether their own actions were performed in a permissible manner. Whatever their reasons, the possibility that suicide assistance may not be reported well outside the scope of Right-to-Die associations is also the possibility of a serious limitation to the data presented here. This possibility and its likely impact on the findings presented in this paper need to be discussed.</p> <p>3) During the study period, some of the criteria for acceptance of suicide assistance changed. Most notably, the Swiss Academy of Medical Sciences changed its wording on the acceptability of medical participation in suicide assistance in its 2004 directive. This point is worth making at some point of the discussion as it may have affected practices.</p> <p>4) How might changes in the age pyramid and consecutive changes in the prevalence of disease have affected the evolutions presented here?</p> <p>5) On a more minor note, on page 9 of 30 the authors state that having no Swiss passport was associated with a higher rate of</p> |
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| | suicide assistance. Based on the rest of the paper as well as the tables, it seems the association was the reverse. |
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| REVIEWER | Claudia Gamondi Palliative and Supportive Care Clinic Oncology Institute of Southern Switzerland Bellinzona Switzerland |
| REVIEW RETURNED | 30-Dec-2017 |

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| GENERAL COMMENTS | <p>I thank the editor for having given me the opportunity to revise this interesting piece of research. This paper describes the epidemiology of assisted suicide in the last 10 years in Switzerland and whether any vulnerable group of patients can be identified. Authors concluded that there is no observable shift in socio economic factors associated with assisted suicide, but there is a strong increase in the elderly population.</p> <p>It is an emerging need to explore and understand how assisted dying is happening in the society and to have a picture of the Swiss situation in comparison with other countries that allow forms of assisted dying. It is a topic of interest for the readers BMJ Open. The methodology of the study and the results are consistent and well described. Tables are clear and well readable. There are few suggestions to improve to overall good quality and value of the paper.</p> <ol style="list-style-type: none"> 1. The discussion would benefit of a reflection between the associated factors observed in the study and the literature evidencing factors correlated with the patient's wish for hasten death. 2. A direct comparison with the Dutch situation is always relevant whereas in The Netherlands and Benelux countries it is euthanasia happening more frequently than assisted suicide (Gamondi et al 2014), evidencing that there could be different patients obtaining assisted suicide or euthanasia. The transferability of the Swiss results with the Oregon situation can be more pertinent and would give the paper more international relevance. <p>In conclusion it is a paper reporting on a very relevant and actual topic. I found the article interesting, globally clear in its messages and producing new evidence.</p> |
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| REVIEWER | Georg Bosshard Clinic for Geriatric Medicine, Zurich University Hospital, Zurich, Switzerland |
| REVIEW RETURNED | 04-Jan-2018 |

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| GENERAL COMMENTS | This is an elaborated paper by an experienced research group based on a sound study project concerning an important and topical subject. |
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VERSION 1 – AUTHOR RESPONSE

Reviewer(s)' Comments to Author:

Reviewer: 1

Reviewer Name: Kenneth Chambaere

Institution and Country: Vrije Universiteit Brussel (VUB) & Ghent University, Belgium Please state any competing interests: none declared

Please leave your comments for the authors below Thank you and congratulations to the authors for a highly intriguing contribution. Very important analysis as a monitoring tool for a highly contentious issue in the UK and worldwide. Nonetheless, I have to report a number of issues here, mostly related to clarity of presentation and excessiveness of results. My comments per section are the following:

Abstract: not the clearest abstract. results section could benefit from more figures and %s + vulnerability assertion in conclusion is confusing;

A: To improve the abstract we added some numbers in the result section and tightened the conclusion section.

Introduction:

- please provide a descriptive definition for assisted suicide and euthanasia, for readers not well acquainted with the terminology.

A: We inserted a section at the end of the first paragraph:

While in Physician-Assisted-Suicides the role of the physician is limited to prescribing a lethal drug, in Euthanasia the physician injects the lethal drug when requested by the patient. Euthanasia is prohibited in Switzerland.

- supplementary table 1: is this necessary? if already published somewhere you can just reference that publication

A: The numbers in the columns 2-4 are separately published in the corresponding annual reports of the right-to-die-organisations, but nowhere in this summarized form (we reference the corresponding websites in the text). Columns 6 and 7 show the relation of numbers of reported assisted suicide to the numbers of assisted suicide identified in our data. We would therefore keep supplementary table 1.

- did the authors have specific hypotheses about shifts and which would indicate potential slippery slope effects (i.e. an empirical operationalisation of slippery slope)?

A: We wanted to test if there is an indication for the 'slippery slope' hypothesis. We specified in the last paragraph of the introduction:

Thus we wanted to test the 'slippery slope' hypothesis, if there is a trend towards more vulnerable patients dying assisted: less educated people, people living in a neighbourhood with a lower socio-economic position, but also people living alone and having no children.

Methods:

- use of some variables from 2000 e.g. religion as a fixed characteristic of a person has its problems. This is a considerable limitation given that these variables are the core of your research focus...

A: We agree that the use of characteristics collected in 2000 is a limitation which is discussed in the first paragraph of the strengths and limitation section. To make this clearer we changed the corresponding paragraph in the discussion:

Information obtained at the census 2000 is not completely up to date in 2014 for all individuals. In particular type of household, but also urbanity, the socioeconomic neighbourhood-index, language region, the existence of children, self-declared religion and nationality might have changed within the maximal 14 years of follow-up, whereby changes over time are more likely in the younger than in the older age group. We were able to account for the fact that marital status might change, and more accurately analyse the association of widowhood or divorce with assisted suicide.

- identification of assisted suicides: seems like a not so straightforward affair. Are the authors confident they have avoided false positives and false negatives? If so, could this confidence be conveyed in the text?

A: We agree that the lack of a central register represents a challenge in identifying the assisted suicides. Despite the effort of the Federal statistical office we suspect some false negatives. False positives, on the other hand, are not very likely, as the Federal statistical office contacts the responsible physician in suspected cases. To clarify this, we adapted the second paragraph of the Strength and limitation section in the discussion:

In suspected cases, the Federal statistical office consults the responsible physician. Therefore the risk to classify deaths wrongly as assisted is small. However, as there is no obligation to report the assisted suicides to a central registry, identification of assisted deaths is likely not complete. In particular suicides assisted without the involvement of one of the three main organisations may be missed. In recent years smaller right-to-organisations got active, LifeCircle in the region of Basel and LLExit in the Italian speaking part of Switzerland. It is possible that differences in the process and reporting may increase the proportion of assisted suicides that are not identified by the FSO and therefore at least partly explain the lower increase of assisted suicides in the Italian speaking region of Switzerland. A Belgian study showed that differences found in end-of-life-practices between the language regions Flanders and Wallonia were not only caused by differences in acceptance, but also by differences in the process and the reporting compliance [1].

- supplementary table 2 is also not necessary in my opinion

A: Supplementary table 2 is not necessary to understand our research and the content of the article. For reasons of transparency we would keep supplementary table 2, because it shows the categorization of the ICD-10 codes for the detailed categories. See also our answer to the number of supplementary tables below.

- the split in age groups is not argued. Why stratify in two age groups? The choice of time periods are explained, but not the choice of age groups

A: We are sorry, the explanation for the split of the analysis in two age groups is indeed missing. We therefore added in the first paragraph of the Statistical analysis part in the methods section: Earlier studies showed interactions between age group and other variables[2], therefore the multivariable analyses were not only stratified by time period (2003-2008), but also by age-group (25 to 64 and 65 to 94). The cut-off at 65 years reflects retirement age for men in Switzerland.

- why the choice for the crude rate and not simply the %s? crude rate is a bit unusual in this regard in my view

A: We consider the possibility to publish crude rates as one of the advantages of a population based cohort study. We report also absolute numbers and percentages, but only the crude rates take the denominator of persons at risk into account.

- are there any potentially relevant socio-economic factors the researchers do not have at their disposal? (given the slippery slope concern...)

A: Yes, as we were limited to the information given in census 2000 we miss relevant-socio-economic factors. The most important is the economic well-being, we do not have any direct data on income or asset. However, education is not only an important dimension of the socioeconomic position itself, it also precedes and influences others dimensions as occupational status and personal income[3]. The Swiss neighbourhood index of Socioeconomic position (SEP) covers the rent per square metre as a proxy for income, but not on an individual basis.

We therefore added this sentence at the end of the first paragraph of "Strengths and limitations" in the discussion section:

We do not have any individual data for the economic well-being. However, education is not only an important dimension of the socioeconomic position itself, it also precedes and influences others

dimensions as occupational status and personal income[3]. In addition we have an indicator for the socio-economic standing of the closest neighborhood of each individual [4].

Results:

- overall: too many supplementary tables; the authors had best pick the most pertinent information to show in supplementary material.

A: We deleted supplementary table 3 (old) and replaced supplementary table 4a and 4b with a slenderized supplementary table 3 (previously table 2 in the main text, see also answer below). All information needed to understand our article is integrated in the text or in the tables and figures in the article. However, in the supplementary material we provide more details about the methods and results and raw data of figures. We would therefore prefer to keep the remaining supplementary tables.

- figure 1: the logarithmic scale makes the time trends seem less pronounced. Also why a split between men and women all of a sudden? Not really a relevant distinction given similar trends. Gender differences given in the results section text are not apparent from the figure.

A: We decided to use the logarithmic scale because it allows a clearer description of the increase in rates in the younger age group. In addition, the presentation of the uncertainty is better visible with the logarithmic scale. (see figure with linear scale below) As women have a higher rate than men in the younger age group, but similar rates in the older age group, we show the crude rates by age and time period separately for men and women.

- table 2: what does "No" signify at the end of the table? No condition listed/missing?

A: "No" signifies "No condition listed". We clarified this in supplementary table 4 (new, old table 2).

- as a reader I would prefer the results be presented in supplementary table 4 (%s within clinical groups) as opposed to table 2 (% within all assisted deaths). Also here the split between age groups and gender is blurring the main message of your study and conflating tables and figures.

A: Table 2 (old) summarizes absolute numbers and percentages of the underlying conditions. We agree that the percentage of assisted suicides among all deaths with the same underlying condition might be more conclusive. Therefore we integrated supplementary table 4 in the article (new table 2) and show the % of underlying conditions among all assisted deaths as supplementary material (new supplementary table 3). We show the results by age group, because the distribution of the underlying conditions differs between the age groups.

- I point to figure 2 in this regard: there is too much information in this figure, and also it takes time to visually see/find the results you mention in the text. It would also help if you described the results in the order in which you present them in the figure (i.e. by age group, if you choose to keep it this way). The description could be boiled down to the most pertinent results.

A: To simplify figure 2 we decided to show only the results of the cox regression models in the figure, to summarize the results of the logistic regression model in the text and show the results of both regression analyses in detail in the supplementary tables 4 (cox regression) and 5 (logistic regression).

- was it not possible to do year-by-year analysis instead of in periods? Could be more useful for presentation and clarity I feel.

A: Here it depends which analysis this statement refers to. Of course, we could describe the increase in rates over the calendar years more finely by calendar years (see figure 1), but we think that the figure would lose clarity instead of gaining clarity. For the analysis of factors associated with the hazard of assisted suicide we had specified a priori that we want to compare the strength of the

association in the years 2003-2008 with the strength of the associations in the period 2009-2014. The argument was that with two time periods we should have a reasonable power in the tests for effect modification. In addition we doubt that testing effect modification with a finer spaced time intervals would result in more clarity in the results. We therefore did not change the two calendar time intervals.

- the title speaks of socioeconomic factors, yet the clinical factor of cause of death is also prominent in your analysis?

A: The aim of our analysis was to analyse time trends in socio-economic predictors associated with assisted suicide. However, we assess the underlying diagnosis as important information in this context and therefore report this too. Nevertheless we shortened the paragraph on the underlying conditions in the older age group in the result section:

In the age group 65 to 94 years the percentage of assisted suicides with cancer was lower (39.3 %) (Supplementary table 3). Diseases of the nervous (11.8 %) and the circulatory system (12.0 %) accounted each for hardly one eighth of assisted deaths. 10.0 % of all assisted suicides were patients with diseases of the musculoskeletal system, 4.7 % with diseases of the respiratory system. Mental and behavioural diseases accounted for 4.2 percent of all assisted suicides, whereby mood disorders contributed most (2.9 %), followed by dementia (0.8 %) and other (0.5%). The percentage of assisted deaths among all deaths (table 2) with the same underlying disease was highest in diseases of the nervous system (1.2 %).

- multivariable analysis: did the authors check for multicollinearity issues? I may have overlooked but did not find a discussion of this, though the issue is likely.

A: We did not formally check for multicollinearity issues. Evidently the distribution of certain factors are somewhat associated with the distribution of other factors as for example civil status and type of household or education and religion. Hazard ratios only change somewhat between crude and multivariable Cox regression models if there is some correlations between included variables. In fact, the existing correlations in the factors did not cause any problems in our multivariable analysis and never caused problems in previous analysis of the Swiss National Cohort [2 5 6].

- why both Cox and logistic? I also did not find a rationale for this. Would it not be better to choose one technique and report only in the text about differences with the other technique, as a sort of sensitivity analysis?

A: We use in our analysis the cohort of the whole census 2000 Swiss population. Therefore we cannot differentiate, if the factors identified in the Cox proportional hazard model are associated with becoming "at risk" for assisted suicide - getting terminally ill, having chronic and uncontrollable pain and suffering - or if the factors are associated with assisted suicide among the persons "at risk". As the results of the logistic regression among all deaths were mostly the same, we conclude that the identified factors are associated with assisted suicide.

To make this clear we adapted the paragraph on Statistical analysis in the methods section:

Because the result of Cox proportional hazard regression might not only identify risk factors for assisted suicide, but also for being terminally ill or being in chronic and uncontrollable pain and suffering, we performed also a logistic regression to identify determinants of assisted suicide among all deaths. We included the same variables and characteristics as in the Cox analysis but also age at death (10 year bands) and underlying diagnosis (broad categories, see above).

As the results of the cox and the logistic regression model were consistent, we removed the results of the logistic regression from figure 2 and only discuss them in the text. (see also above)

Discussion:

- overall, there is just too much to discuss and explain: socioeconomic gradients in themselves, changes in those gradients over time, differences between sexes and ages, differences between analysis techniques. I'm afraid there is just too much compressed in one paper.

- one main finding is that influential socioeconomic factors before 2008 were still influential after 2008, but which have become more pronounced, and which less? Do we see trends in their (relative) "importance"? The factors themselves and shifts in them are not discussed, while it is the question in your title. Instead, the seconde paragraph is merely a discussion of the general increase in number of cases.

A: The only shift over the two time periods in the socioeconomic factors associated with assisted suicide concerned the Italian speaking part of Switzerland (Figure 2, supplementary tables 4-6). We discuss this in the strength and limitation sector. We added a third paragraph in the discussion, to discuss the factors and if they confirm the slippery slope hypothesis:

The consistency of the socio-economic factors associated with assisted suicide over the study period does not support the slippery slope hypothesis: assisted suicides of less educated people and people living in neighbourhoods with a lower socioeconomic position did not increase disproportional. However, other factors persistently associated with assisted suicide as female gender, having no children and living alone, being widowed or divorced may reflect a social vulnerability. Of note, people who are single and have no children were less likely to get their request for euthanasia granted in a study in a Dutch End-of-Life Clinic [7].

- intriguing that having children gives lower odds of assisted suicide. This has been found in the Netherlands as well (Snijdewind et al JAMA Intern Med 2015). Do the authors have a hypothesis for this?

A: See answer above.

- is "votations" an existing word?

A: For sure it is not a common word, so we replaced it with "vote". Thank you for the hint.

- the strengths and limitations section is comprehensive and well balanced.

A: Thank you, we included some more points you mentioned in the methods-part in this section.

- References: reference 15 can be updated, there is new data available for 2015 in the Netherlands (Onwuteaka-Philipsen NEJM 2017)

A: Thank you. We updated the corresponding reference.

Reviewer: 2

Reviewer Name: Samia Hurst

Institution and Country: Institute for Ethics, History, and the Humanities, Faculty of Medicine, University of Geneva, Switzerland Please state any competing interests: None declared

Please leave your comments for the authors below This is a very detailed study of trends in the demographic characteristics of patients who died by suicide assistance in Switzerland between 2003 and 2014. These are important data as concerns regarding the risk of a slippery slope regularly surround assisted death and could be substantiated by an increase in the cases of suicide assistance for particularly vulnerable persons. Although the manuscript requires English-language editing, it is otherwise well-written, clear, and interesting, with informative tables. My main comments are as follows:

1) The authors describe this concern regarding a so-called "slippery slope" in the introduction as part of the justification for their study. I was disappointed to see no discussion of whether or not they believe their results to confirm or dis-confirm this concern.

A: We added a third paragraph in the discussion, where we discuss our results in regard to the slippery slope hypothesis:

The consistency of the socio-economic factors associated with assisted suicide over the study period does not support the slippery slope hypothesis: assisted suicides of less educated people and people living in neighbourhoods with a lower socioeconomic position did not increase disproportional. However, other factors persistently associated with assisted suicide as female gender, having no children and living alone, being widowed or divorced may reflect a social vulnerability. Of note, people who are single and have no children were less likely to get their request for euthanasia granted in a study in a Dutch End-of-Life Clinic [7].

2) The authors compared their data to that provided by the Swiss Right-to-Die associations.

2.1) This is mentioned on page 5 of 30 with no reference: are these unpublished data or should there be a reference?

A: These data are from the annual reports published by the organisations. We referenced the webpages, where the annual reports are published.

2.2) On page 6 of 30, the authors report that 95.2% of the suicide assistance cases they identified were also reported by the Right-to-Die associations. Is this due to a) almost all cases are indeed performed by these associations or b) cases not performed by these associations are not being reported as well as those that are? I would welcome discussion of these two possibilities. It is plausible that suicide assistance cases performed by individual doctors not associated with Right-to-Die associations are indeed being less well reported. Other data suggest that these doctors sometimes wrongly sign off on these cases as if they were natural deaths. They may mistakenly believe that since no violence occurred, or because the cause of death is known, this is allowed when in fact it is not. They may be loath to call police to a deathbed scene at home with family members present. They may also of course also possibly act in a self-protective manner if they are uncertain as to whether their own actions were performed in a permissible manner. Whatever their reasons, the possibility that suicide assistance may not be reported well outside the scope of Right-to-Die associations is also the possibility of a serious limitation to the data presented here. This possibility and its likely impact on the findings presented in this paper need to be discussed.

A: It is well possible that suicides assisted by physicians without the involvement of one of the three main right-to-die organisations may be less well reported. Generally it is believed that the right-to-die organisations are involved in the large majority of assisted suicides in Switzerland. Unfortunately, there are hardly any studies estimating the number of suicides assisted without the involvement of the organisation. In a survey in 2006, 103 out of 1,650 doctors had ever assisted a suicide without involvement of a right-to-die association. We discuss this problem in the second paragraph of the Strengths and limitations section and expanded the paragraph taking into account your considerations:

However, as there is no obligation to report the assisted suicides to a central registry, identification of assisted deaths is likely not complete. In particular suicides assisted without the involvement of one of the three main organisations may be missed. In recent years smaller right-to-die-organisations got active, LifeCircle in the region of Basel and LLExit in the Italian speaking part of Switzerland. It is possible that differences in the process and reporting may increase the proportion of assisted suicides that are not identified by the FSO and therefore at least partly explain the lower increase of assisted suicides in the Italian speaking region of Switzerland. A Belgian study showed that differences found in end-of-life-practices between the language regions Flanders and Wallonia were not only caused by differences in acceptance, but also by differences in the process and the reporting compliance [1]. It is

also possible that physicians not associated with right-to-die-organisations do assist in suicides and do not report these cases appropriately. Even though this is concerned as a marginal problem compared to the numbers of suicides assisted by right-to-die-organisations in Switzerland, there are no numbers because of the lack of an obligatory register. In a survey in 2006, 103 out of 1,650 doctors had ever assisted a suicide without involvement of a right-to-die association. [8]

3) During the study period, some of the criteria for acceptance of suicide assistance changed. Most notably, the Swiss Academy of Medical Sciences changed its wording on the acceptability of medical participation in suicide assistance in its 2004 directive. This point is worth making at some point of the discussion as it may have affected practices.

A: To analyse the effect of the change in the directive of the Swiss Academy of Medical Sciences we would need more data for the time before 2004. Unfortunately there are hardly any data on assisted suicides in Switzerland before 2003, therefore we can not assess if and how the change had an impact on factors associated with assisted suicide.

4) How might changes in the age pyramid and consecutive changes in the prevalence of disease have affected the evolutions presented here?

A: The change in age pyramid and consecutive changes in the prevalence of diseases have affected the absolute numbers and the percentages of assisted suicides and corresponding diagnoses.

However, the multivariable analyses of the hazards and the proportion of assisted suicides among all deaths with the same comorbidity should not be affected by the age pyramid.

5) On a more minor note, on page 9 of 30 the authors state that having no Swiss passport was associated with a higher rate of suicide assistance. Based on the rest of the paper as well as the tables, it seems the association was the reverse.

A: Thank you for this correction. This was an error of wording on our side, and we changed it in the text to "having a Swiss passport".

Reviewer: 3

Reviewer Name: Claudia Gamondi

Institution and Country: Palliative and Supportive Care Clinic, Oncoogy Institute of Southern Switzerland, Bellinzona Switzerland Please state any competing interests: None declared.

Please leave your comments for the authors below

I thank the editor for having given me the opportunity to revise this interesting piece of research. This paper describes the epidemiology of assisted suicide in the last 10 years in Switzerland and whether any vulnerable group of patients can be identified.

Authors concluded that there is no observable shift in socio economic factors associated with assisted suicide, but there is a strong increase in the elderly population.

It is an emerging need to explore and understand how assisted dying is happening in the society and to have a picture of the Swiss situation in comparison with other countries that allow forms of assisted dying. It is a topic of interest for the readers BMJ Open.

The methodology of the study and the results are consistent and well described. Tables are clear and well readable.

There are few suggestions to improve to overall good quality and value of the paper.

1. The discussion would benefit of a reflection between the associated factors observed in the study and the literature evidencing factors correlated with the patient's wish for hasten death.

A: The patient's wish to hasten death is a very interesting topic very much related to the subject of assisted suicides. The main reasons for the wish to hasten death are the loss of physical function, pain, but also psychological and emotional factors as fear and hopelessness, social factors such as being a burden on others, dependency and the loss of the identity [9]. Unfortunately our data is restricted to information given in the census 2000 or reported in the mortality registry. We do not have any data on the reasons patients wanted to die and do not know anything about the stage and the effect of the disease.

2. A direct comparison with the Dutch situation is always relevant whereas in The Netherlands and Benelux countries it is euthanasia happening more frequently than assisted suicide (Gamondi et al 2014), evidencing that there could be different patients obtaining assisted suicide or euthanasia. The transferability of the Swiss results with the Oregon situation can be more pertinent and would give the paper more international relevance.

A: In the Netherlands and Belgium is indeed evidence for differences between patients obtaining assisted suicide and euthanasia. However, as patients in Switzerland do not have this choice, they can't be categorized this way. We clarified in second paragraph of discussion, that in the Netherlands mainly euthanasia is requested. In the last sentence of the second paragraph in the discussion we compare the increase in assisted suicides with the situation in Oregon and Washington.

In conclusion it is a paper reporting on a very relevant and actual topic. I found the article interesting, globally clear in its messages and producing new evidence.

Reviewer: 4

Reviewer Name: Georg Bosshard

Institution and Country: Clinic for Geriatric Medicine, Zurich University Hospital, Zurich, Switzerland

Please state any competing interests: none declared

Please leave your comments for the authors below This is an elaborated paper by an experienced research group based on a sound study project concerning an important and topical subject.

Literature (for Answers to reviewer comments):

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8. Burkhardt S, Wyss K, La HR. [Assisted suicide in Switzerland: physicians' position]. *Revue Medicale Suisse* 2007;3(137):2861-64
9. Rodriguez-Prat A, Balaguer A, Booth A, et al. Understanding patients' experiences of the wish to hasten death: an updated and expanded systematic review and meta-ethnography. *BMJ open* 2017;7(9):e016659 doi: 10.1136/bmjopen-2017-016659[published Online First: Epub Date].

VERSION 2 – REVIEW

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| REVIEWER | Samia Hurst Institute for Ethics, History, and the Humanities Faculty of Medicine, University of Geneva Switzerland |
| REVIEW RETURNED | 16-Feb-2018 |

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| GENERAL COMMENTS | <p>The authors have answered reviewer comments well and have greatly improved the strength and clarity of the paper. A few things remain to be corrected.</p> <p>1) Although the authors have corrected the association of higher suicide assistance with having a Swiss passport (rather than not having one) in the text this is still inaccurately described in the abstract.</p> <p>2) English language editing is required, especially in the newly added parts.</p> |
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| REVIEWER | Claudia Gamondi Palliative and Supportive Care Clinic, Oncoogy Institute of Southern Switzerland, Bellinzona Switzerland |
| REVIEW RETURNED | 19-Feb-2018 |

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| GENERAL COMMENTS | Thank you very much for the revision of the paper according to the reviewers' suggestions. |
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| REVIEWER | Kenneth Chambaere End-of-Life Care Research Group, Vrije Universiteit Brussel (VUB) & Ghent University, Belgium |
| REVIEW RETURNED | 21-Feb-2018 |

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| GENERAL COMMENTS | <p>Thank you to the authors for their comprehensive rebuttal of my remarks.</p> <p>The changes made have improved the paper considerably, and where the authors did not follow my suggestions for change I feel they were able to clearly explain their rationale behind their decision. I have no further comments.</p> |
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VERSION 2 – AUTHOR RESPONSE

Monday, 12 March 2018

Dear Dr. Bedi

We hereby re-submit the revised manuscript ID bmjopen-2017-020992.R1 entitled "Increase in Assisted Suicide in Switzerland: Did the socioeconomic predictors change? Results from the Swiss National Cohort".

We checked that throughout the text the association of higher suicide assistance with having a Swiss passport is correctly described. The English editor Chris Ritter helped us with the English language editing of the whole text.

In addition we fulfilled the request of the Editorial Office from 9th of March 2018 and added a sub-heading 'Patient and Public Involvement'. Given the design of our study, neither patients nor public were involved in the development of the research question, in the analysis, and in drawing conclusions from the results.

Yours sincerely

Nicole Steck on behalf of the authors