Author's response to reviews

Title: Risk Factors for Suicide in Hungary: a Case-control Study

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Author's response to reviews:

REVIEWER 1

Major Compulsory Revisions

Comment 1: The major methodological issue in the current study is the mixed proxy-based and directly obtained nature of controls’ assessment. While the psychological autopsy method is well validated, the method is subject to differing sensitivities and biases, which the authors indirectly acknowledge when discussing the reference time used in controls. In other words, it is problematic that only 34% of controls were assessed using the proxy-based technique with which the cases were assessed. To remedy the situation, I suggest that the authors offer the reader an additional level of information: providing information and analyses restricted to the sample ascertained using proxy-based interviews in addition to the analyses currently presented in the manuscript. Consistent results emerging from both analytical approaches will lead to greater confidence in the findings.

Authors’ response: To address this comment we conducted a sensitivity analysis that we now report in a new table (Table 4). We restricted the data set to the 65 controls (and their 65 matched cases) with third party informants. To avoid making comparisons based on sparse data and resulting unstable regression estimates, we have not included any models in the sensitivity analysis that were based on a cell value of 5 or less. As the patterns of association were generally similar (although effect sizes tended to be somewhat larger in the restricted data set), these extra models indicate the robustness of our findings. The sensitivity
analysis is described in the last paragraph of the Results section (on page 10) and in the Discussion (pages 12-13), and Table 4 is presented on page 22.

Minor Revisions

Comment 2: In the limitations, the authors provide a reference to national statistics to which they compare their sample on the basis of demographics and methods used. Ideally, a comparison would be provided between potential cases retained and not retained in the final sample. In the event that this is not possible are statistics available specifically for Budapest?

Authors’ response: Due to ethical constraints we were not permitted to collect any information for subjects whose informants did not consent to take part (please see 2nd paragraph, page 13). As the reviewer points out, ideally we would have compared our observed cases to all suicides in Budapest and Pest County during the study period. However, as these routinely local data were unavailable, we made comparisons with national data instead.

Comment 3: Please provide information relating to the delay between time of death and when the informants’ interviews took place.

Authors’ response: Please see sub-section ‘Assessment, on page 6: “For almost all subjects, interviews with informants were conducted with three weeks of death (or index date for controls).”

Comment 4: Does lifetime history of psychiatric illness refer to a history of illness prior to those present during the reference date? If so, this should be specified in the text to avoid confusion (i.e. 43.5% lifetime psychiatric diagnoses vs. 69.1% any diagnosis during the reference period).

Authors’ response: Yes, the variable ‘lifetime history of psychiatric illness’ refers to a history of illness prior to those present at the reference date. We now explain the difference between these two psychiatric illness measures under the ‘Assessment’ subheading of the Methods section (last paragraph of page 5). In this respect we also provide a clearer description of the Table 2 risk factors (in the 2nd paragraph on page 9).

Comment 5: It would be more appropriate to refer to affective disorders in the discussion where it is currently referred to as major depressive disorder, since the variable employed reflects more than just major depressive disorder.

Authors’ response: The term ‘major depression’ has been replaced with ‘affective disorder’ throughout the revised manuscript, since this category includes bipolar disorder as well as major depression. A footnote has been added to Table 2 to explain the composition of this category.

Discretionary Revisions
Comment 6: The information available on informants gives the authors an interesting opportunity to contribute to the literature on the validity of the psychological autopsy method. I would recommend that the authors provide some data with respect to the influence of informants’ age, gender and relationship to the deceased, as well as time delay prior to the interview on the rates of variables of interest. I believe this would be a worthwhile addition and strengthen the manuscript.

Authors’ response: In the revised manuscript we have presented an extra Table and placed this in an Appendix (on page 23). This shows the prevalence of most of the potential risk factors (from Tables 1 & 2) among the suicide cases only, according to gender, age (<50 vs. 50+ years) and by type of informant - i.e. close relative (cohabiting partner, parent/step-parent, sibling) vs. ‘other’ type (2nd degree relative, non-cohabiting or ex-partner, close friend, etc.). We do not think there are any striking patterns in the observed differences between these informant categories. In addition there may be difficulties with interpretation in that any observed differences may reflect real variation in exposure prevalence rather than a reporting bias. Therefore our preference would be to omit this table from the revised manuscript, but we have presented it nonetheless to enable the editor / reviewer to decide on this issue.

REVIEWER 2

Minor Essential Revisions

Comment 7: The authors link their results to societal changes in post-communism era. However, the identified risk factors are also present in other countries. The study design does not allow to establish a causal link between societal change and suicide.

Authors’ response: We have added this important caveat to our discussion of the main findings (in the penultimate sentence of page 11).

Comment 8: The authors should state the selection criteria of the 262 suicide cases that were investigated.

Authors’ response: Aside from pseudo-random selection according to day of the week (which was implemented purely for logistical reasons), no other selection criteria were applied at the outset to identify these 262 suicides. Thus, they were a representative sample of a complete population-based case series for Budapest city (1.6 million population) and the surrounding Pest county (0.5 million) during March 2002 to March 2004. This point is now further clarified at the very start of the Materials & Methods section (page 3).

Comment 9: The authors state that suicide rates are elevated in Northern
Europe. This is only true for some northern countries (Finland, Baltic countries and parts of Russia).

Authors’ response: Using the latest available WHO data, i.e. most recently published national rates as of 2008 (http://www.who.int/mental_health/prevention/suicide_rates/en/index.html), we have altered the first sentence of the manuscript to list some of the countries with the highest suicide rates in Europe. These are all in northern or eastern European (i.e. the Baltic countries, Belarus, Croatia, Finland, Hungary, the Russian Federation, Slovenia and the Ukraine).

Comment 10: There are several spelling mistakes, which should be corrected.

Authors’ response: We have identified and corrected some spelling mistakes. These are shown in the manuscript using ‘tracked changes’ (please see pages 5, 6, 10, 11 & 14). As far as we are aware, no spelling mistakes remain.