Author's response to reviews

Title: Comparing complementary data sources improves prevalence estimates validity of chronic medical conditions

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Author's response to reviews: see over
Answers to the Reviewers

Reviewer 1: Joke Korevaar

The objective of this study was to determine the feasibility of combining information from complementary data sources. This is an interesting topic as the data source plays an important role in presented prevalence rates.

Major comments:

The authors studied prevalence of 4 conditions from three different databases resulting in 12 comparisons. The authors conclude that combining provide more valid estimates. Yet, I’m not convinced that this is indeed the conclusion that can be drawn based upon the presented results.

We agree with reviewer 1 that the focus of the paper is the comparison and not the combination of different data sources. The main purpose was to show that data source and definition matter regarding prevalence estimates validity. Therefore, we have changed the title of the paper to “Prevalence of Chronic Medical Conditions in Switzerland: exploring estimates validity by comparing complementary data sources”. Correspondingly, we adapted the text as needed:

Abstract-Background “Using complementary data sources, we assessed estimates validity of prevalence rates of four common chronic medical conditions with high impact on cardiovascular health (diabetes mellitus, hypertension, dyslipidemia, obesity)”, Abstract-Conclusion “Complementary data sources can provide different prevalence estimates of chronic medical conditions in the general population. However, a careful harmonization of the definition of each chronic medical condition permits a high degree of concordance. Definitions based on use of drugs alone appear to give the most reliable results”, Introduction last paragraph “Therefore, we explored similarities and differences between Swiss health statistics (Swiss Health Survey [SHS], primary care data [FIRE], and hospital discharge statistics [MEDSTAT]), regarding four common chronic conditions with high impact on cardiovascular health: hypertension, diabetes mellitus, dyslipidemia, and obesity. We hypothesized that with a careful definition of each chronic condition and each data source we will be able to harmonize the estimates despite different sampling techniques (self-report, physician report, drug prescription, measurement). We also expected to find hints concerning over- or under-reporting of specific chronic medical
We conclude that complementary data from different data sources might generate different prevalence estimates of chronic medical conditions in the general population. However, careful harmonization of the definitions of chronic health conditions will provide strikingly similar age-and gender-specific distributions, even in data sources based on different settings and assessment methods.

The pattern of all three data sources resembles each other (per condition). Yet, the absolute values differ, and the absolute values differ between conditions. I miss information why the FIRE database should not be complete for the conditions studied. High BP is often treated by the GP, and if not treated by the GP, the GP should know that the patient suffers from high BP. So, why should this be missing in the database? Is it a lack of communication between medical specialists and GPs, or what?

We agree with the reviewer that one would expect that a primary care database (FIRE) should provide the most valid estimates of a condition such as hypertension. However, not every patient who had a drug for hypertension had a diagnosis of hypertension, and not every patient who had a blood pressure ≥140/90mmHg on two occasions (definition of hypertension) had a diagnosis of hypertension. Therefore, even the primary care data were far from perfect and might have underestimated the true prevalence of hypertension. On the other hand, a selection bias of primary care patients who might be sicker than the general population might have overestimated the true prevalence of hypertension. Therefore, it is informative to know that we found a higher prevalence of hypertension in a health survey and a lower prevalence of hypertension in hospital discharge statistics. We have expanded on these comments in the limitation of the discussion: FIRE on the other side, has a limited number of participating practices (81), which are not necessarily representative of the study region. Furthermore, FIRE captures the actual prevalence of chronic conditions among persons attending general practices, and not necessarily the prevalence in the general population, thus favoring overestimation due to selection bias of sicker patients. FIRE is based on voluntary participation and might have incomplete data.

The same holds true for DM and dyslipidemia. So why should one add results from SHS or from the hospital? In addition, I don’t think that it is remarkable that prevalence based on hospital data is lower. Not all patients with hypertension are treated in the hospital (not even for ambulatory care). The manuscript would become of more interest if the authors could explain
why it is to be expected that the FIRE database is not complete. And it would become of more value if the authors could indicate the under- or overestimation that would have occurred if just one database was used. And if they could give what the new estimates (bases on all sources) are, so what we gain by using three sources of data.

Again, we agree with the reviewer that primary care data should be most valuable for prevalence estimates of DM and dyslipidemia. However, we acknowledge that even primary care data might be far from perfect due to several reasons (see last comment). Therefore, a comparison with other data sources (which might be even less perfect) remains valuable. We cannot provide new estimates based on all three sources because formal pooling of these different estimates is worthless. The comparison with the discussion of possible reasons for differences seems more important. We have changed the title and other parts of the paper de-emphasizing the combination and emphasizing the comparison of the different data sources (comments concerning the first remark of the reviewer).

Next, the authors conclude that definitions based on drugs use alone give the most reliable results. Do the authors know for which condition the drug was prescribed? And was this indeed the indication to which the authors ascribe the drug?

We are very confident about identifying diabetes mellitus based on drug use, because there is no anti-diabetic drug that could be used for other conditions than diabetes. It is more difficult for dyslipidemia because use of lipid lowering drugs, especially for secondary prevention, does not imply elevated lipid levels above certain cut-off values. We expand on this in the last part of the limitation section: Both, the definition based on lipid values and the definition based on drug use are fallible, because the lipid cut-off values are based on a variable expert opinion and the indication for medication depends on the patients’ need for secondary prevention (while drug use for primary prevention remains disputed). As a result, none of the definitions is comprehensive and all are prone to over- or underestimating bias. The identification of hypertension based on drug use is even more complicated because antihypertensive drugs can be used for heart disease or renal disease as well. However, many patients with renal disease or heart disease have hypertension. We calculated the prevalence rates for hypertension with and without patients who had renal disease or heart disease and found no significant difference. Therefore, we provide prevalence estimates for hypertension based on all FIRE participants.
There is a difference in the results for obesity compared to the other conditions, this condition is lower in self-reported data and far lower in hospital data, as one could expect. My questions is what the added value of these latter two sources to the FIRE data is for this condition. If I see the results, I think the overall conclusion does not fit for this condition.

We agree with the reviewer that obesity is clearly underrepresented in the survey data and the hospital discharge statistics and this is an important observation of the paper. We do not claim that we have solved the problem of underreporting. Furthermore, we have clearly de-emphasized the idea that combining data adds value in estimating prevalence of chronic conditions (see answers regarding the first comment of the reviewer).

This manuscript clearly shows that the use source plays a role in the observed prevalence rates, yet, I cannot not see that this problem is solved (or diminished) by combining several sources.

We agree with the author that we cannot solve the problem of varying data sources and case definitions by combining data. Therefore, we were limited concerning estimates of absolute prevalence rates. However, the figures clearly demonstrate that prevalence estimates, e.g. the age- and gender-specific prevalence estimates, are essentially concordant between the different data sources.
Reviewer 2: Heike Hansen

Thank you for this manuscript. This article is a good addition to previous studies about prevalence estimates of chronic conditions in different data sources for the area in Switzerland. These analyses based on three large data sources. Please find my comments below:

Major Compulsory Revisions

1. Title: The term “Combining” is misleading to the reader. Maybe it is better if you use “Comparison” because the three data sources were not linked but compared. This also applies to the rest of the manuscript. In the results section you have already used the term “comparison” and in the discussion you used “complementation”.

Thank you for this remark. This was also a major concern of reviewer 1. We have deleted the word “combining” from the title, the abstract, and the entire text and focus now on comparison.

2. Abstract: Background: “Prevalence estimates of chronic medical conditions and their multiples (multimorbidity) in the general population are scarce and often rather speculative.” Did you mean this for Switzerland? If yes, it should be written.

We have included Switzerland as suggested.

3. Introduction: Paragraph 2: “However, measuring prevalence rates of chronic medical conditions poses challenges in countries where information systems are poorly developed, because of varying case definitions…” This sentence is confusing. I do not think that the information systems are poorly developed. On the basis of your argumentation the systems are different.

Thank you. We have dropped the text “in countries where information systems are poorly developed”.

4. Data protection: I miss more information in this section. Who are the other researchers? Please explain the “harmless acknowledgement”.

We thank the Swiss Federal Statistical Office for providing hospital discharge (MEDSTAT) and Swiss Health Survey (SHS) data.

We added information on data protection: SHS and MEDSTAT are administered by the Swiss Federal Statistics Office as a part of its legal mission. The use of fully anonymized individual data from these sources is subject to specific data contracts with the Institute of Social and Preventive Medicine. FIRE data are fully anonymized and stored on a central server. Only the two authors (UZ, VK) had access to the data. According to the current Swiss law on human research (Humanforschungsgesetz, HFG) retrospective analyses of anonymized medical routine data do not requires approval by the regional ethics committee http://www.bag.admin.ch/themen/medizin/00701/00702/07558/.

5. Definitions of chronic conditions:
SHS: The descriptions of the definitions of the chronic conditions are similar to table 1. Maybe the text can be shortened.

We agree with the reviewer that there is repetition regarding the definition of chronic conditions within the text and in table 1. However, it becomes very confusing, when a part of the information is within the text, while the other part has to be extracted from the table. Therefore, some repetition remains even after shortening the text.

FIRE and MEDSTAT:
I missed a description how the selection of the criteria (e.g. ICD Code or blood values) took place. Please describe the process.

We modified the text in Definition of chronic conditions as follows:

“We used disease-specific drugs, diagnostic codes, or laboratory values to define four chronic medical conditions (Table 1):”

“We used all disease-specific ICD-10 codes to identify diabetes mellitus (E10-14), hypertension (I10-I15), lipid disorder (E78), and obesity (E66).”

6. Analyses:
Please add the numbers (n) of the person who were excluded.

We modified the text accordingly:

“Because all FIRE primary care physicians were located in the German speaking part of Switzerland, we restricted all analyses to residents of that area, thus avoiding bias due to different operating customs as well as cultural and semantic disparities. Since SHS provides only information on individuals 15 years and older, we excluded individuals aged less than 15 years at the last consultation/hospitalization in FIRE (N=7,287) and MEDSTAT (N=89,135). Those aged >95 years were also excluded because of small absolute numbers (N=7 for SHS 2007, N=395 for FIRE 2010-12, N=4,296 for MEDSTAT). Based on these selection criteria, the study population amounted to 13,580 for SHS 2007, 99,441 for FIRE 2010-12, and 883,936 for MEDSTAT 2009-10.”

7. Analyses:
What is about Missing values in the three data sources? How did you handle
We added in the Analyses section:

"Generally, missing values, e.g. for diagnoses, could not be discerned from negative answers and had therefore to be handled as negation. Except BMI in FIRE, missing values were extremely rare (N=184 for BMI and N=18 for all questions concerning diabetes in the SHS 2007, N=7 for age or sex in FIRE 2010-12)"

8. Results:
In the figure you wrote “drug prescribed” for FIRE data. In the text you wrote “drug use alone”. Drug prescription is not the same as drug use. Of course you could hardly control for the real use of drugs in the patients, but you should use the term carefully. Or you explain and discuss the term, respectively. The same problem is in SHS data for the term “Doctor told” in figures vs. “diagnosis of …” in the text. There should be a more clear term in the text e.g. doctors told diagnosis. Or you switch it to “self reported diagnosis”. All in all you have to revise and bring into line the descriptions of the categories.

We reedited the result section using self-reported drug use and self-reported diagnosis for the survey data, and drug prescription with the FIRE data

9. Discussion:
You should integrate subtitles, e.g. Comparison with other studies, Strength and limitations, Conclusion.

Done

10. Discussion: “Therefore, the transfer of prevalence estimates of chronic medical conditions from primary care and hospital settings to the general population seems feasible with the appropriate care to the methodology used. “This issue should be discussed in more detail.

We made the following adjustment: Therefore, prevalence estimates of chronic medical conditions based on primary care data and hospital discharge statistics can be generalized to the population after a careful and appropriate harmonization of the case definitions.

11. Discussion: Line 347
You classify dyslipidemia as a symptom-based chronic condition. I think this is a clear cut-off diagnosis measured with e.g. cholesterol value. The patient noted his high cholesterol initially not based on symptoms but on the blood values. What is your definition of symptom-based chronic condition?

We agree with the reviewer that dyslipidemia is not a symptom-based chronic condition. We rephrased the whole paragraph: In line with others [8-10, 13], concordance between health survey and primary care data was good for clearly defined chronic conditions (diabetes and hypertension), and fair for chronic conditions with a less clear cut-off (dyslipidemia and obesity). In general, chronic conditions with a less clear cut-off were more reflected in the health survey than in the primary care data (dyslipidemia). While for diabetes comprehensive definitions performed best, for hypertension and dyslipidemia building the case definition on drug use alone was more efficient. Hospital data performed well for clearly defined diagnoses (diabetes, hypertension), but underreported severely chronic conditions with a less clear cut-off (dyslipidemia, obesity).

12. Discussion: From Line 391
You should revise the conclusion related to the first comment. And it should be written what this study adds.

We have revised the conclusion as follows: We conclude that complementary data from different data sources might generate different prevalence estimates of chronic medical conditions in the general population. However, careful harmonization of the definitions of chronic health conditions will provide strikingly similar age-and gender-specific distributions, even in data sources based on different settings and assessment methods.

- Minor Essential Revisions
1. Data protection: “All data “WERE” anonymized…”
Done: (see 4. Data protection)

2. Results: Please unify the number of decimal places (one vs. two)
Done
3. Figure legends: The sentence about the “Data sources” is in each legend the same. This can be shortened.

Done