Authors' responses to review comments on "Willingness to Pay for Health Risk Reductions: Differences by Type of Illness" (Ref.: Ms. No. JHE2930)

Editor's Comments:

Both I and the referee are enthusiastic about this manuscript.

RESPONSE:

The referee has a number of suggestions for clarification, although I think they all can be accommodated by rewriting. I also had a few comments:

1. On page 23 you deal (or rather don't deal) with the probability of future lost earnings from the illness. These clearly can vary by the degree to which an individual could resume work and his or her disability insurance. To the degree that individuals fail to account for these costs or that there are any second-order effects with income, they are in your numbers.

RESPONSE:

2. You don't, however, deal with any out-of-pocket medical costs from the illnesses. Like lost earnings, it is presumably in your numbers in some fashion. If respondents did not account for such costs (or lost earnings), your estimates are probably conservative.

RESPONSE:

3. You never say when your survey was administered. Both in the text and Table 3 you should give the year that the dollars are in so that any future work can account for inflation.

RESPONSE:

4. A couple minor points: Number the equations and fix the reference to Cameron (2010).

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Reviewer's comments:

The manuscript "Willingness to Pay for Health Risk Reductions: Differences by Type of Illness" is not only very original, but also part of a large scale undertaking that will make an important contribution to the field. As the authors note, the use of VSL estimates in policy making has relied on estimates primarily drawn from wage differential studies, and little work has been done to understand how willingness to pay for risk reductions might vary by the type of disease risk. This study is part of a large effort to generate more nationally representative estimates of

willingness to pay for health risk reductions, using a state-of-the-art stated preference survey design that is unique in its ability to tease out different drivers of individuals preferences. This manuscript and its companions will push the field forward by contributing evidence toward many long-standing issues in WTP/VSL literature.

RESPONSE:

Clearly, there were many methodological challenges, but the provided appendix does an excellent job of describing and explaining the rationale for key design choices, and validity checks. Certainly there are points where reasonable people might disagree about particular choices (for example, I am not convinced it was a good idea to include the "percentage" risk reduction in the vignettes, as these would lead respondents to overvalue risk reductions that are large relative to baseline, but not large in absolute terms) but I found the documentation quite satisfying in both its level of detail and clarity of writing. Nearly every possible concern one might raise, had been considered carefully by the authors.

The main text is also clearly written, and could be published virtually as is. I have some points where I would suggest the author consider some revision for clarity and for greater impact.

MAIN COMMENTS

The authors could do more to organize/synthesize their findings and discuss them. They find what appears on first-read as a mish-mash of 'patterns' (e.g. WTP goes up with age for some diseases and down for others).and the patterns are not all intuitive. The authors might consider consolidating findings more by disease. E.g. for each disease: discuss its autonomous utility, sick year disutility, and lost year disutility, and WTP(ur) together and see if any clearer pattern emerge that could be highlighted. This approach could be used in the discussion for a few key diseases, perhaps.

RESPONSE:

The other main issue is that the reader may have trouble interpreting the finding for particular diseases in this survey (where disease labels were applied to risk and health state time profiles that have no connection--expect for plausibility constraints--to *actual disease risk and health state time profiles*). The authors could be clearer about what they think we learn about how to invest in real disease-reducing programs from this kind of study.

MINOR COMMENTS

(1) The authors could be clearer about why they chose to use WTP(ur) instead of VSL. The suggest it has advantages, but do not say what those advantages are.

RESPONSE:

(2) In section 3.9 of the Appendix, discussing duration of illness, it is not clear why multiple repeated values appear. For example, under 'gender-specific cancers' TODEATH the list is

shows several zeros and several '999's. does this mean that those values had a greater chance of being drawn from the distribution in the simulation? Better explanation in the surrounding paragraphs in the appendix would help.

RESPONSE:

(3) A related issue: it might be helpful to augment Figure 1 in the main text to illustrate exactly how these parameters in the Appendix section 3.4 to 3.14 end up mapping into what the user faces as their 3-way choice. For figure 1, the authors might also include the individual's life expectancy and age, since these parameters are used in determining the parameters that show up in the vignette.

RESPONSE:

(4) I was confused in looking at Figure 1 alone and trying to understand why for program B, the patient, after recovering from colon cancer, dies of something else at 73, whereas with program A the person would continue to have a heart condition but live until 79. I gather that this is due to the "reductions in lifespan due to non-fatal cases" described in Appendix section 3.4. My instinct would have been that this person at their current age is assigned a life expectancy of more than 79 years. If they get heart disease or colon cancer their life expectancy may be reduced (e.g. to 79 years in the case of heart disease in this vignette). Since the colon cancer program seems to lead to a recovery, then I would have thought the life expectancy description would have said "Die at X" where X is >= 79. Or, if colon cancer impacts LE, I would expect it to say "Die of colon cancer related complications at age 73". This was a point of confusion.

RESPONSE:

(5) The authors discuss in Appendix 1.2.1.2 in ex ante risk information that it was their desire to "evaluate ex ante preferences, not their updated preferences after being educated through the survey." When thinking about how the study results might be used for policy, this 'desire' is puzzling. The survey seems to estimates nationally representative age-specific willingness to pay for disease specific risk reduction. But the 'willingness' to pay is based on whatever information the respondents can readily access during the survey regarding those diseases. So diseases that are salient to particular subgroups tend to show higher WTP. But its not clear that these are the values that should be used in policymaking, because they could be driven by beliefs about risk or about the experience of disease-specific morbidities that are incorrect.

RESPONSE:

(6) It was interesting (and encouraging) to see that disease-specific difference were not seen (very much) in the utility of 'lost years', supporting a reasonable assumption that from the point of death, it doesn't matter what you die of. Other than issues of the cause of death affecting ability to have an open-casket funeral, or particular beliefs about the afterlife, it's

hard to see how the disease that causes lost years of life should affect your willingness to pay to avoid the loss.

RESPONSE:

(7) The differences in utility for sick-years, make sense. It is an important contribution to the literature that this paper considers latency, sick years (including variable severity of morbidity), and lost years in estimating WTP for risk reductions, because most previous WTP studies only focus on premature death (rather than a more realistic time-dependent disease profile). It appears, from the results in Table 2B, that the experience (disutility) of a 'sick-year' is not the same across diseases. Certainly we know this to be true from more direct approaches to health state valuation. But the patterns that emerge are not immediately clear. The authors do little in their writing to suggest what is going on. Also, I assume it is the case, but it is not completely clear whether the differences in disutility for sick years persist after controlling for the 'severity' factors in the vignettes such as 'moderate pain' or 'severe pain'. It would be interesting, and feasible to do with the data I think, to see if the disutility of a sick year depends on the number of sick years. It would also be interesting to see how these differences compare to more direct measures of health state quality of life for these diseases (perhaps in another paper!).

RESPONSE:

(8) There were also large differences in autonomous utility, suggesting that there are features of these diseases not captured fully in the vignettes' descriptions of sick-years and lost years. I think this warrants more discussion, because it unclear what this means and what to do with it. My reading of section IV-A is that the authors observe (for example) that young women are willing to pay a lot more for an absolute risk reduction due to breast cancer than for the same amount of risk reduction for heart disease, controlling for time profile and severity of morbidity of health states. The authors seem to attribute these differences to the respondents' subjective probabilities that they will get these diseases or subjective beliefs about how bad the disease experience is. I found these results wanting of much more discussion-especially since the pattern (with respect to age) vary by disease in ways that are not intuitive. There was also no discussion of these results in the context of other work on how VSL varies by age.

RESPONSE:

(9) Related to (7). the authors could do more to explain/discuss the role of respondents' subjective beliefs about the diseases, their risks of occurring, time profile, morbidity, and impact on life expectancy, and the stated values of those attributes in the vignettes. In other words, if the respondent is presented with the vignette in Fig 1, are we to understand from the significance of disease specific autonomous utility differences, that the respondents 'information set' includes is some feature of the disease that is overriding (or combining with) the information given by the vignette. In other words, the vignette tells the user his chance of experiencing a case of heart disease (with an expected time profile given by the vignette) is 4% without intervention A, but that with intervention A, that risk would drop to

3.8%. Coincidentally, Program B would also reduce the risk of a different disease and time profile by the same absolute amount 0.2 percentage points. So differences in WTP for A and WTP for B should be driven by disutility of the thing you would increase your chance of avoiding. I gather from the autonomous utility results and the disutility of sick years results, that even if the Symptoms/Treatment and Recovery/Life Expectancy sections of the vignette were the same for both diseases, the respondent (depending on his/her age) might be WTP more for the same bundle of benefit just because it is labeled Colon Cancer instead of Heart Disease, since Colon cancer is just thought to be worse for some unknown/unspecified reason, and because sick years from colon cancer are thought to be worse than the same number of sick years from heart disease. The later makes some sense, and could be tested/compared to actual measures of health state valuation for both diseases. The former is harder to understand. It seems the respondent is basically replacing the risks provided by the vignette with her own subjective belief about her risk of that disease or replacing other vignette information with her own subjective beliefs about time profile, morbidity, hospitalization, or there is some other 'feature' of the disease that does not have to do with any of the things captured in the detailed vignette (as the authors suggest: "These differences go beyond just what can be accounted for by the future pattern of health states represented by the health risk in question (i.e. Latency, sick years, recovered/remission years, and lost life years)". If the respondents, upon seeing the 'disease' heading, proceed to override or subconsciously adjust all the information that follows in the vignette, then the WTP measures are a reflection of respondents subjective beliefs about their own risk of the disease and their expectation about the disease course, which may not be tightly correlated with their actual risk or the actual expected disease course (for example, the authors point out that respondents overestimate their life expectancy by several years).

RESPONSE:

I appreciate the appendix discussion about pilot tests that showed people really wanting to know the name of the disease, and struggling with a generic scenario. The study finds a systemic effect on WTP that goes beyond the objective attributes in the vignette. That is useful to know and especially useful to have that information for many major disease all from the same high quality representative survey. But then it doesn't seem clear how people actually value the objective attributes, since we don't know whether respondents were combining the vignette information with whatever subjective beliefs they have regarding the disease.

RESPONSE:

It is not for this article to resolve, but I was left wondering how we ought to treat these 'disease premiums'. Do we treat them as 'valid' and make policy decisions that acknowledge them, or should they be ignored because they may be driven by misperceptions about risk and health states profiles for those diseases. In other words, if epidemiological data tells us which diseases are the Top 10 in terms of disability-adjusted life years, and a RDD phone survey with a representative sample of the population suggest that people believe in a different Top 10 list (and objectively incorrect one) then should we allocate resources according to the priorities in the subjective list? If the authors feel their study sheds any light

on these issues, it would be great to include that in the discussion to strengthen the linkage of this work to public policy.

RESPONSE:

(9) The finding in Table 4 showing remarkable correlation in ranking between public spending on disease programs and the study's findings regarding WTP for risk reductions from each disease is interesting. The authors cautiously suggest that the private values of the public revealed in their survey may influence the public spending patterns...but the 'autonomous utility' findings leave me asking whether the influence is going in the other direction.. the public spending leads to availability/salience influencing the respondents ex ante beliefs about the diseases which leads to respondents wanting to pay more for those programs that the public spends a lot on. I don't think this direction of influence can be rejected.

RESPONSE:

(10)   There are some typos where several equations are referred to as equation(0)

RESPONSE: