

## Human Security Report Project Responses to Les Roberts'

### 'Appendix A: General comments From Les Roberts on the "Shrinking Costs of War"'

14 April 2010

**LR:** The HSR attacks "Bridge for Peace" efforts, mortality surveys in general, and IRC's DRC [Democratic Republic of the Congo] mortality surveys in particular based on poor scholarship and limited evidence, while providing no constructive alternatives.

**HSRP:** We do *not* "attack" the WHO's "Bridge for Peace" initiatives, we simply note that, while they have clear health benefits, "their *security* benefits have yet to be compellingly demonstrated."

We do *not* attack mortality surveys. On the contrary we note in Chapter 4 of the 'Shrinking Costs of War' that they are "critically important sources of data for war-affected countries where there are rarely any reliable governmental statistics." We are critical only of their use for generating excess death toll estimates.

We *do* suggest a "constructive alternative" to seeking to determine excess deaths—namely that mortality rates in war-affected areas be compared to regional average mortality rates. This is an approach that the IRC itself adopted in a 2009 article on mortality in the DRC published in *Disaster Medicine and Public Health Preparedness*. The other obvious "constructive alternative" is to use the various emergency and crisis thresholds as comparators. This is already standard humanitarian practice.

**LR:** It is strange that a report on recent trends in war is based on a sample of countries which, as depicted in graphic images, exemplifies Guinea-Bissau, Mauritania, and Zimbabwe, but not Afghanistan and Iraq.

**HSRP:** This is not in the least bit strange. We made it clear that Figure 2.1 was a review of *sub-Saharan African countries* in conflict. But exactly the same trends are seen in the rest of the world. Not even in Afghanistan and Iraq do the "best estimate" trends for under-five mortality rates increase during periods of conflict—they are essentially flat. Moreover, the findings in our survey on trends in child mortality in wartime are confirmed by a major World Bank study published in 2008 that included global data on infant and adult mortality.

In the published version of this report we analyze annual changes in under-five mortality for all countries that suffered from major armed conflict between 1970 and 2008. We include only the conflicts with at least 1,000 battle deaths a year—a higher threshold than in our sub-Saharan Africa review. The results are instructive. Child mortality rates increased in roughly 5 percent of the years in which countries were embroiled in warfare. This pattern is also evident at the

country level, with only about 15 percent of all countries experiencing an increase in child mortality during war.

**LR:** The authors seem to be purporting indirect mortality estimate methods (like those employed by Uppsala University in the dataset [PRIO] on which the Human Security Reports have been based) over direct (surveillance and surveys) methods.

This logic is never applied to malnutrition, immunization coverage, or median income estimates. While indirect methods exist for each of these three measures, that are probably better than the indirect methods for mortality, we expect people to go out and take a sample of children before declaring a crisis of malnutrition or poor immunization coverage. The logic should not be different for mortality just because the political stakes are higher.

**HSRP:** It is not clear what point is being made in these two paragraphs. *We think it implies that the HSRP is hostile to population health surveys. Again we point out that our report repeatedly makes clear that we think such surveys are critically important.*

**LR:** IRC went out and spent months sampling in the field in the peak of a raging war to collect this data...data fraught with limitations which the reports attempt to describe. This effort involved at least four events of IRC staff being held by armed groups, a motorcycle accident, and countless harrowing encounters with armed rebels and soldiers. In spite of the extreme security and logistic constraints, this crude and risky effort has since been generally confirmed by:

[a] A series of mortality surveys by MSF and an overlapping survey by Merlin (Kalima, 2001) with similar findings.

**HSRP:** We do not dispute the claim that conditions in the Eastern DRC were extremely dangerous. And the MSF and Merlin surveys may well show findings similar to those of the IRC. However, that does not overcome the fundamental problem with the IRC's first two surveys (i.e., those led by Dr. Roberts), namely that the survey areas were not chosen to be representative of the eastern region of the DRC. The IRC admits this. It follows that *no region-wide extrapolations should ever have been made from these data.*

[b] A half-million dollar assessment undertaken by WHO to examine IRC's claims following the 2001 survey...which in non-quantifiable terms confirmed the crisis IRC had reported.

**HSRP:** Again, we do not dispute the fact that there was a humanitarian crisis in the DRC. But it is the quantitative data that are the source of contention.

[c] Reviews by HTNS and CRED cited by the Human Security Report (HSR) but which the HSR neglected to report had produced summary conclusions broadly in step with the IRC reports.

**HSRP:** On the critical issue of excess deaths, all the HNTS reviews expressed skepticism about the IRC's baseline. It is this too-low baseline that drives the IRC's huge mortality estimates. With respect to the first two surveys, HNTS reviewer, Francesco Checchi of the WHO, suggests that the excess death estimates are 'speculative at best'. This is precisely the point made by the HSRP.

[d] Declining numbers of children found by UNICEF for immunizing across the Eastern Provinces during the peak years of the war.

**HSRP:** There is no disagreement between HSR and IRC regarding the fact the child mortality rates were very high in the eastern provinces.

**LR:** Many of the HSR's conclusions derive from the examples and definitions they choose to select and the examples they choose to ignore. As mentioned above, if "a war" is defined with a higher and more disruptive threshold, for example a conflict that causes 1% of the population to die in a year (e.g. Somalia in 1992, Rwanda in 1994, DRC in 2000) virtually every conclusion in the report would be reversed.

**HSRP:** We used data from the Inter-Agency Child Mortality Estimation Group (IACMEG). These data do *not* show mortality increasing in Somalia or the DRC—nor, Liberia, or Ethiopia. But it is of course true, as we point out, that very high-fatality conflicts will reverse the downward trend—the most striking example is Rwanda. A central thesis of our study is the finding that in the post-Cold War world high-fatality conflicts are far less common than they used to be.

**LR:** Moreover, treating a minor conflict in Senegal with equal weight as the war in Angola is not constructive. The conclusions about war being associated with improved health conditions should be based on data limited to the places and times during the conflict and weighted so that the largest and most intense conflicts are considered the most. Most humanitarian aid flows to a few major crises which do not reflect the conflicts on which this report's conclusions are based.

**HSRP:** Our claim is straightforward and correct—recent wars rarely reverse the downward trend in mortality rates that has been the norm for most of the developing world for more than 30 years. We made it very clear that mortality rates are often highly elevated in conflict zones, but that these tend to be localized. In part for this reason, and in part because death tolls from wartime violence are far lower than they used to be, nationwide mortality rates rarely increase during wartime.

**LR:** The so-called data presented in the report are perplexing for those of us that were in Mozambique in the early 1990's or Rwanda in 1994, the graphs do not match the thousands and thousands of children buried and real-time measures of mortality made by CDC, MSF, and others.

**HSRP:** The “so-called data” are from IACMEG. If Dr. Roberts has an issue with the U5MR for Mozambique and Rwanda then he is taking issue not with us, but with IACMEG’s choice of the “best estimate” trend line. In the case of Rwanda, IACMEG has had a very large number of mortality surveys and the IACMEG best estimate trend line shows mortality increasing well into 1996. But it is indeed possible to read individual surveys’ data as suggesting that mortality decreased in 1995 after the genocide had ended.

**LR:** Crude mortality is perhaps the most critical indicator of humanitarian conditions according to the CDC, WHO, OFDA, and the SPHERE project. Suggesting that mortality surveys are fine but cannot be used to determine excess deaths undermines the entire logic and purpose of defining a normal mortality and the degree of urgency associated with higher levels. To suggest mortality surveys work on a district or refugee camp level but not on a national level, defies the logic of sampling on which much public health progress has been made.

**HSRP:** It is unclear what this paragraph means. But to reiterate: our argument is that retrospective mortality surveys can *in principle* be used to estimate excess deaths accurately; however, this is rarely possible *in practice* for reasons we spell out in detail. This is not least because of the major challenges involved in determining pre-war mortality—challenges that are demonstrated by the IRC’s choice baseline.

**LR:** The HSR authors ask, which child-mortality estimate for DR Congo is wrong, the DHS or the IRC surveys? Bill Taylor showed us long ago that the 5 year birth history method employed by the DHS is certainly the one that is wrong in the setting of Congo. (Taylor WR, Chahnazarian A, Wienman J, Wernette M, Roy J, Prebley AR, Bele O, ma-Disu M. Mortality and Use of Health Services Surveys in Rural Zaire. *Int. J. of Epi.* Vol. 22, suppl. 1, 1993. pp. S15-19.) Stan Becker found similar results in Liberia.

**HSRP:** Our point about the DHS vs. the IRC findings on child mortality rates—the latter being twice as high as the former—is simply that both can’t be right. (Both could be wrong of course.) It is certainly true that the DHS data have been criticized for underestimating child mortality.<sup>1</sup> But given that the IRC’s child mortality rate is *double* the DHS rate, the difference in the figures cannot be explained by a general DHS bias towards underestimation.

### ***Appendix B: Inconsistencies in the 2009 HSR***

**LR:** HSR is full of internal inconsistencies:

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<sup>1</sup> Sullivan, Jeremiah M., “An Assessment of the Credibility of Child Mortality Declines Estimated from DHS Mortality Rates” (working draft of a report submitted to UNICEF, 2007), [http://www.childinfo.org/files/IGME\\_Overall\\_Results\\_of\\_Analysis.pdf](http://www.childinfo.org/files/IGME_Overall_Results_of_Analysis.pdf).

It suggests baseline mortality should decline in war and criticizes IRC for assuming a 1.5 deaths/1000/mo. baseline when UNICEF said it was 1.3 in 1997 and the last pre-war census suggested an even lower rate.

**HSRP:** The issue of the baseline mortality rate is discussed in some detail in our response to the IRC's critique of the 'Shrinking Costs of War.' We will not duplicate this argument here, except to note that the UNICEF figure is itself problematic. The HSRP's conclusion that the sub-Saharan African average mortality rate is not an appropriate baseline mortality rate for the DRC is supported by Jon Pedersen of FAFO, and Harvard's Kenneth Hill, both of whom reviewed the IRC's survey methodology for the Health and Nutrition Tracking Service.

**LR:** The HSR cites SMART as the experts on what is appropriate recall periods, but then cites the 5 year recall DHS as more credible than the ~1 year IRC surveys.

**HSRP:** Dr. Roberts suggests that we claim that the DHS data are more credible than those of the IRC. Chapter 3 of our report says no such thing. It notes that in 2006-2007 the IRC's estimate of the child mortality rate was almost double that of the DHS. But it goes on to say that, "*our point here is not to determine which of the estimates is correct.* It is simply to note that the IRC's fatality estimates, while not publicly controversial, have not only been challenged, but are much higher than those of other studies." This is self-evidently true.

We do, however, point out in our *Overview to the Debate Generated by the "Shrinking Costs of War"*, that the DHS data *are* more credible than that of the IRC for the period of *the first two surveys*. But this is because the DHS followed standard practice in conducting the survey that covered this period; the IRC did not, as Dr. Roberts has himself admitted.

The reference to the SMART protocols in Chapter 4 of our report had nothing to do with either the IRC or DHS. It was a reference to problems that can arise when determining baseline mortality rates using recall data.

**LR:** The HSR cites the Canadian Government co-funded SMART initiative as an expert voice in this field but rejects their basic strategy for getting agencies to do a better job of measuring mortality and malnutrition by cluster surveys where surveillance is not functioning.

**HSRP:** This simply repeats what has been said previously. To reiterate, our critique is not of the use of cluster surveys in general, but their particular use to estimate excess death tolls.

**LR:** The HSR cites the *New England Journal of Medicine* article on mortality as conflicting with other results ignoring that the results they cited only reported violence and indicated twice as many excess deaths from non-violence.

**HSRP:** The HSR makes no direct mention of "the *New England Journal of Medicine* article on mortality." This article is, however, one of two we referred to as being undertaken for UN agencies—the WHO in this particular case. Even though there are important differences in estimates of *overall* excess deaths (violent and non-violent) in Iraq, we should have made it clear that in this case the most dramatic divergences are between the estimates of violent deaths. The violent death tolls recorded in the WHO study were radically lower than the violent death tolls recorded in a controversial survey on Iraq over a similar time period, whose findings were published in *The Lancet*. Dr Roberts was closely involved with the latter survey.

**LR:** The HSR takes infrequent measures of entire nations that experienced short periods of violence among a small sub-population and deduces that war is associated with lowering mortality. It is critical that back in 2000, IRC's carefully examined a million people at a period of intense war to induce conclusions about elevated mortality in the 17 million around them over that war period. Which of these acts is weaker logic?

**HSRP:** We don't understand the question.

**LR:** The report discusses excess mortality as if it were a new concept recently employed. In the general press and discourse, the widely cited death estimates from the Holocaust, the American Civil War, WWI, and the rape of the Congo, were mostly from disease and malnutrition. But the HSR authors are now saying that the excess deaths are impossible to measure so we should not bother while simultaneously concluding that the trends of war deaths are declining. Either the HSR can accurately measure the excess deaths (the majority of the deaths we traditionally attribute to war) in Somalia and Yemen and Afghanistan today in real time, or they should acknowledge that they do not know anything about recent trends.

**HSRP:** The HSR does *not* discuss excess deaths as if it is a new concept. We do explain what the term means because the *HSR* seeks to be accessible to non-specialists. We demonstrate that direct deaths from wartime *violence* have declined dramatically worldwide. We argue that there is a positive relationship between direct and indirect deaths—the more the former, the more the latter, but that the relationship is not consistent. We do not think that Dr. Roberts would disagree.

**LR:** Chapter 4 of the last Human Security Report starts out stating, "PART IV Counting the Indirect Costs of War. Battle-death counts are the commonly used indicators of the severity of conflicts. But while important, they measure only a small part of the real human cost of war." This chapter comes to the exact opposite conclusion of this 2009 Human Security Report!

**HSRP:** We wonder if Dr. Roberts has actually read the 'Shrinking Costs of War'. The first page of Chapter 1 contains the following text: "There is general agreement in the research community that *the violence that generates deaths on the*

*battlefield is an important driver of indirect deaths, and that the latter are significantly greater in number than the former.” This makes exactly the same point as that made in the first HSR that Dr. Roberts cites.*

**LR:** Chapter 3 of the last Human Security Report starts out describing how the lack of reliable data has allowed governments to have their vulnerable citizens to be raped and used as child soldiers without consequence. Yet in the PRIO data the HSR utilizes, and the DHS type surveys suggested as a better alternative, giving these same governments authority over documenting killings and deaths as if those are somehow less incendiary for them.

**HSRP:** We do not of course believe, nor have we ever claimed, that “the lack of reliable data has allowed governments to have their vulnerable citizens to be raped and used as child soldiers without consequence.”

The PRIO data are in fact drawn from a huge range of sources—governments are only one of them.

**LR:** The report speculates that smaller wars affecting smaller areas are having less mortality impact but it does not present any data on those issues. Correlations between numbers of soldiers and mortality are needed to draw the conclusion: “Smaller wars mean fewer war deaths and less impact on nationwide mortality rates.” If we consider the most deadly conflicts of the past half century (Cambodia, DR Congo, Biafra...) they did not involve massive armies.

**HSRP:** First, we were very clear that our discussion of the decline in battle deaths referred only to violent deaths, not indirect deaths although we acknowledge that there is a relationship between the two. Second, the number of soldiers is just one factor in determining the number of excess deaths—direct and indirect. The use of heavy weapons, external support for the combatants, and access to humanitarian assistance all make a difference. In Cambodia, for example, the massive carpet-bombing campaigns by the US killed huge numbers of people but involved only a tiny number of American aircrew.

**LR:** The report concludes a trend of less conflict-related mortality due to “increases in the level, scope and effectiveness of humanitarian assistance to war-affected populations” but again presents no evidence of this. General increases in wealth, improved local medical services, the availability of cell phones, all may contribute to such a trend if it exists. Other careful examinations of the effectiveness of humanitarian aid have been less complimentary.

**HSRP:** We agree that levels of economic development as well as health interventions make a difference to reducing peacetime, and wartime, mortality—and say so in the report. With respect to the effectiveness of aid, we cite studies that find that humanitarian assistance has become more effective. This, combined with the fact that the level of assistance per displaced person has trebled since the end of the Cold War, suggests that more lives are being saved by aid than in the Cold War years.

**LR:** The authors describe their own assessment of the IRC mortality surveys in DR Congo as “the most comprehensive analysis to date of the IRC’s methodology” which is debatable upon review of the HNTS or CRED assessments of the DR Congo studies.

**HSRP:** We leave this for readers to judge for themselves.

**LR:** The HSR writes as if the last DHS survey in DR Congo was credible regarding mortality but choose another WHO source with very contradictory findings to show improvements in immunization coverage.

**HSRP:** We used the WHO as a source because their data were current.

**LR:** The report finally concludes, “*The Shrinking Costs of War* goes on to argue that estimating ‘excess’ war deaths—which include those from war-exacerbated disease and malnutrition as well as war-related injuries—is a task so fraught with challenges that it can rarely succeed.” This contradicts the past conclusions of most expert bodies on humanitarian monitoring the report cites.

**HSRP:** In Chapter 4 we discuss a series of challenges that arise when attempts are made to estimate excess death tolls using surveys. These challenges we argue, are insuperable except in the case of very short wars. Since, to the best of our knowledge, none of the expert bodies have examined the challenges that we raise, the “past conclusions” that don’t take them into account are hardly pertinent.

### *Appendix C: Errors or misleading aspects of the HSR*

**LR:** The HSR makes great protest about the Moba data being used in 2000 as representing 2/3<sup>rd</sup> of Katanga and that this was the highest health zone measure made among any of the IRC studies. They speculate that this could have dramatically skewed the data upwards. What the HSR did not know, and did not care to ask the authors about, is that IRC went to Moba at the suggestion of the OFDA and OCHA representatives in Eastern Congo at the time as they felt it was the safest and only health zone in all of RCD controlled Katanga Province to be surveyed. This strongly suggests that the Moba data under-estimated the Katanga mortality estimate. The priests living in Moba also agreed with this analysis. Moreover, the HSR conveniently neglects to mention that the health zone visited the next year in Katanga was the second highest mortality rate ever recorded by IRC... confirming the wider representativeness of the Moba data the year before.

**HSRP:** The fact that Moba at the time of the survey may have been the least physically dangerous place in Katanga, does *not* mean that other parts of the province necessarily had higher mortality rates as Dr. Roberts suggests. Most excess deaths are caused by disease and malnutrition, not violence. Moba could well have been relatively safe from physical violence *and* suffer higher rates of death from disease and malnutrition.

But the real problem here is that Moba and the other survey sites were not chosen in such a way as to be representative of the Eastern DRC as a whole. This remains the primary reason for rejecting the excess mortality estimates of the first two surveys.

**LR:** The HSR conveniently ignored the Katanga sample from 2001 showing survivor bias in the sample, meaning that the IRC survey estimates there, including the Moba estimate in 2000, were probably under-estimating mortality.

**HSRP:** These are two related, but distinct, points: i) the possibility of survivor bias; and ii) how representative Moba is of Katanga (or eastern DRC) as a whole.

We acknowledge the possibility of survivor bias, which, if applicable in this situation, would mean that there would potentially be an under-estimation of mortality *in the areas surveyed*. Thus, survivor bias in Moba may be underestimated by a few percentage points, but this does not then mean that mortality throughout the eastern DRC was under-estimated since, as we argue in the report, the IRC has no way of knowing whether Moba's mortality situation is representative of the DRC as a whole. In subsequent nationwide surveys the levels of intra-provincial variance of mortality are much higher than inter-provincial variations in mortality.

**LR:** The authors of the HSR neglect to emphasize that while the first two surveys could not be samples because of the extreme security situation, the IRC reports included a sensitivity analysis to assess the effect of various assumptions (e.g. the assumed baseline) and showed that the assumptions had little influence on the general conclusions.

**HSRP:** If the extreme security situation meant that it was impossible to select areas to be surveyed in a way that ensured that they would be representative of the population of the Eastern region as a whole, then no confidence can be placed in any extrapolated estimates.

The IRC's sensitivity analysis for the first survey involved the use of three different estimation methods to determine excess deaths. But far from ignoring these, the HSRP carefully evaluated them and demonstrated that each was marred by serious errors. Neither Dr. Roberts nor the IRC have challenged our critique of these errors.

**LR:** The IRC report showed several indications that Eastern DRC was in crisis during the peak years of the war (e.g. more deaths than births in many health zones, shrinking fraction of the population <5 years). These are consistent with the high death toll estimates but not mentioned by the HSR.

**HSRP:** No one doubts that there was a humanitarian crisis in the Eastern region of the DRC during the time covered by the first two reports. This is not the issue in contention.