Human Security Report Project Response to

'IRC and the Burnet Institute Strongly Affirm Congo Mortality Study Findings'

14 April 2010 – updated 7 May 2010*

In their joint response to the Human Security Report Project's (HSRP) "Shrinking Costs of War" (which appears at the end of this document) the International Rescue Committee's (IRC) and the Burnet Institute fail to address key aspects of the HSRP's critique.[1]

In particular, the IRC continues to insist that its research is based on "standard and scientifically grounded methodology", a claim that is directly contradicted by a statement in the report on its first survey. Here the IRC states that the sample from which it extrapolated to obtain a region-wide excess death toll estimate for the war-wracked eastern part of the DRC was *not* representative of the region-wide population. This is a major breach of standard survey practice and was the primary reason that the HSRP argued that the findings of the first two surveys should be rejected.

Subsequent to the publication of the first report, the team leader of the first two surveys has stated that they were based on "kind of bad science,"[2] and that they ignored "standard academic protocols"[3] in order to generate estimates that would have an impact on donors.

The findings of these two surveys accounted for 2.5 of the 5.4 million of the IRC's excess death estimate.

In its response to the "Shrinking Costs of War," the IRC repeatedly invokes authority—the text is replete with references to "extensive peer review", "widely-cited", "scientifically-grounded", "accepted by independent experts", "authoritative", etc. Yet it fails to note that peer reviews of

[1] For purposes of convenience, and to avoid possible confusion, the joint response will hereafter be referred to as the IRC response.

[2] Les Roberts, "Iraqi Civilian Deaths" (presentation, University of Minnesota, Minneapolis, MN, November 7, 2005) http://www.ephblog.com/wp-content/uploads/2008/07/roberts_at_university_of_minnesota.pdf.

[3] Jim Giles, "Death Tolls May Loom Too Large in the Fog of War", *NewScientist*, January 20, 2010, http://www.newscientist.com/article/mg20527443.100-death-tolls-may-loom-too-large-in-the-fog-of-war.html?DCMP=OTC-rss&nsref=online-news.

* The excess death toll projection for the DRC in the text of page 15 was revised on 7 May 2010.

its research commissioned by the WHO-affiliated Health and Nutrition Tracking Service (HNTS) raised many of the same concerns as the HSRP—particularly with reference to the critical issue of the baseline mortality. If, as the HSRP and HNTS reviewers agree, the baseline mortality is too low, the excess death toll will be too high.

In what follows we focus on the two main issues in dispute. First, we examine whether the methodology employed by the IRC in the first two surveys is, as the HSRP argues, so flawed that the findings for this period should be rejected completely. Second, we discuss whether the IRC's choice of the sub-Saharan African average mortality rate as the baseline mortality rate for the DRC is an appropriate one, as the IRC continues to maintain despite evidence to the contrary. (Note that in the following discussions much of the technical discussion has been relegated to footnotes)

Finally, we provide examples of where the IRC's claims are untrue or deeply misleading, and note some of the implications for humanitarian policy of getting death toll estimates wrong.

The Major Contested Issues

The following discussion reviews the two major areas of contestation between the IRC and the HSRP. The HSRP has other disagreements with IRC's response, but they are not as central.[4]

Methodological Errors in the First Two Surveys

The IRC's claim that its research consistently followed scientific protocols is contradicted by its very first survey report that states that, "…the 1.2 million people within the sampling universe of the five IRC studies *are not representative of the approximately 20 million people in eastern*

[4] One example is the claim that the report "incongruously lumps together in one classification a relatively minor conflict, such as the occasional fighting in Senegal, which occurred in the context of overall growth and prosperity in one of Africa's most peaceful countries, with devastating national wars such as those in Somalia, Liberia, and Congo." There is absolutely nothing incongruous about this. Our review was of countries in sub-Saharan Africa that had suffered intermediate or high intensity conflict. We expected that death tolls in the less deadly conflicts would have little impact on pre-war mortality trends (which were declining), but surprisingly this was also true of countries with "devastating national wars" including Liberia, Angola, even Somalia and the Congo according to the survey data.

DRC."[5] As we point out in Chapter 3 of the "Shrinking Costs of War", this admission means that the survey data should never have been used to extrapolate an excess death toll to the region as a whole—doing so was a fundamental violation of basic survey principles.

Defending its decision to rely on unrepresentative population samples in the first two surveys the IRC notes that its research was:

> …conducted during a raging war and therefore several large areas could not be sampled due to extreme insecurity. *It was therefore necessary to apply results from surveyed areas in eastern Congo to regions that could not be sampled and to extrapolate these rates to determine the best estimate of excess mortality in the region.*[6]

It wasn't *necessary* to do any such thing. The IRC chose to do so. Why? Dr. Les Roberts, the lead researcher for the first two surveys, made it very clear in a recent interview with *New Scientist*, that the IRC wanted an excess death figure for the war-affected eastern DRC for advocacy purposes, and this took precedence over adhering to "standard academic protocols" when conducting the surveys.[7]

The IRC also claims that excess death calculations were made "following consultations with experts."[8] But there is no way that any experts could overcome the fundamental problem that the survey data the IRC had collected were not representative of the wider population whose excess death toll they sought to estimate. Extrapolating from unrepresentative sample populations violates the most basic principles of this type of survey research. The only other nationwide mortality data that covers the same period as the IRC surveys is from a Demographic and Health Survey (DHS) survey that shows no increase in mortality following the outbreak of war and

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[5] Les Roberts et al., "Mortality in Eastern DRC: Results from Five Mortality Surveys" (New York: International Rescue Committee, May 2000), 12. Emphasis added.

[6] The IRC and Burnet Institute response to which we refer, "IRC and the Burnet Institute Strongly Affirm Congo Mortality Study Findings", is available online from a link on the blog *Turtle Bay:* http://turtlebay.foreignpolicy.com/posts/2010/01/20/the_battle_over_the_cost_of_war. Emphasis added.

[7] Giles.

[8] IRC and Burnet Institute response, 1.

which records a child mortality rate for 2006-2007 that was approximately half that recorded by the IRC.

The IRC continues to stress that the surveys it carried out to produce excess war death estimates relied on "sound scientific methods".[9] It is therefore worth noting how Dr. Roberts describes the method that he and his team used to select the areas they sampled in the first two surveys:

> …we had thirty clusters of ten houses from five different areas, and then we did something that *scientifically I hope will make you cringe*. We said, "Well you know, we went to this one area in Orientale province, and so our best guess is probably Orientale [province] is like that. And we went to these three places in the Kivus, and we think the Kivus probably are on average like that. And we went to this one place in Katanga province, and so we think the areas controlled by the rebels are like that." And we came up with an estimate: 1.7 million dead.
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> Realize we did not sample; we did a robust sample of about five areas containing about a million people, and we extrapolated our conclusions out to about seventeen million people. *That's kind of bad science*, but …[we were operating in] the middle of a raging war—it's the most deadly war anywhere in the world since World War II—and it seemed sort of prudent.
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> The next year, we went back, and we sort of repeated that same process, going to six places, three of them old, three of them new, again extrapolating from the areas we visited out to areas we thought were similar. In one area, the rebels had lost control of part of Katanga province, so we said, "Well we think maybe in that area, the death rate is halfway between the baseline and where we measured up in northern Katanga. And we came to the conclusion that 2.5 million people were dead."[10]

As described, this sounds like a team of people trying the best they could in very difficult circumstances to collect data on population health. Such data may well have been useful—especially since IRC humanitarian missions had already been set up in two of the areas surveyed and the organization planned to set up a mission in a third area that was surveyed. But the key

[9] Ibid., 3.

[10] Roberts, 2005, 1-2. Emphasis added.

problem is that the IRC had no way of knowing whether the mortality rates in the areas they chose were in fact representative of the province as a whole. They guessed—and the evidence suggests that they not only guessed wrongly, but that their estimates inflated the true death toll.

We do not for a moment underestimate the huge difficulties of conducting surveys in conditions like those in the DRC at that time. And the data collected by Dr. Roberts and his colleagues surely provided valuable insights into the terrible health conditions in the eastern region of the DRC. But they should never have been used to generate excess death tolls for the region as a whole.

WHO epidemiologist Francesco Checchi comes to a similar conclusion in his review of the IRC's first two surveys where he describes the IRC's estimates for the period as "…speculative at best".[11]

Extrapolating from unrepresentative data *could* lead to underestimates of the excess death toll. But the "Shrinking Costs of War" points to a number of factors that suggest the 2.5 million excess death toll for the period of the first two surveys is in fact too high. Here we note two additional reasons for believing that the latter is the case.

In its 2002 report, the IRC indicated that there were problems with the first two surveys. In discussing limitations of the survey, it notes that, "In 2000 and 2001, however, *one or two "hot" places were inadvertently surveyed*."[12]

It is not clear what the implications of inadvertently surveying "hot" places are, but the phrase would seem to suggest that the IRC had realized that its (non-random) choice of survey locales had led to a selection of survey areas with very high mortality rates. Findings from individual survey locations can have a big impact on the excess mortality rates when relatively few survey

[11] Health and Nutrition Tracking Service, "Peer Review Report: Re-Examining Mortality from the Conflict in the Democratic Republic of Congo, 1998-2006", http://www.who.int/hac/techguidance/hnts/hnts_drc_re_examinig_mortality_1998_2006.pdf. We recognize, and explicitly note in our report, that the subsequent three reports *did* follow standard survey practice in selecting the areas to be surveyed.

[12] Les Roberts et al., "Mortality in the Democratic Republic of Congo: Results from A Nationwide Survey" (New York: International Rescue Committee, April 2003), 7. Emphasis added.

locations have been chosen, as was the case with the IRC's first survey. As Chapter 3 of the "Shrinking Cost of War" notes, the mortality rate for just one survey area in Katanga province drives most of the mortality toll for the whole region.

But there is another, more compelling, reason for believing that the excess death toll that derived from the IRC's flawed methodology is too high.

For the period covered by the first two surveys the IRC assumes that the pre-war mortality rate for the DRC is 1.5 deaths per 1,000 per month—the sub-Saharan African (SSA) average rate– while its survey-derived data indicate that the average mortality rate for this period for the war-wracked eastern region of the country is 5.4 deaths per 1,000 per month.

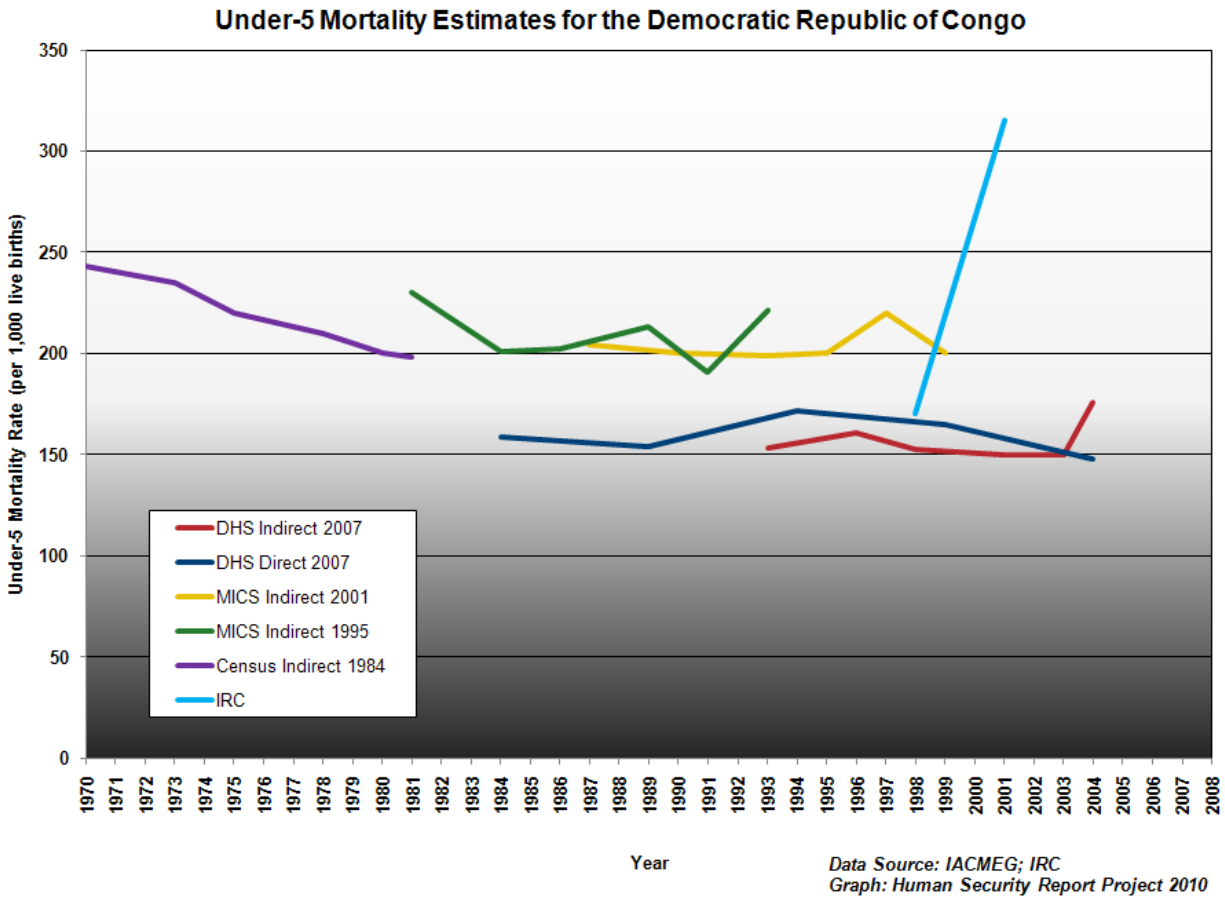
If we, like the IRC (see discussion below), assume that the west of the country had no excess deaths during the 33-month period covered by the first two surveys, then the nationwide average mortality rate would be more than twice the IRC's assumed baseline mortality rate.[13] This is an unprecedentedly large increase over such a relatively short period of time.

We can see this more clearly by examining the child mortality data, which is generally more reliable than other mortality data.

The radical nature of the IRC's claim becomes evident when we compare the IRC's estimate of the change in the child mortality rate following the onset of the war in August 1998 with the child mortality trend data for the DRC from 1970 to 2005 that are provided by the Inter-Agency

[13] The IRC's population source is Zaire: Bureau of Statistics, Bukavu, 1996 (Roberts et al., 2000, 13.) for which they cite a population of 19.9 million in the five provinces of the war-afflicted east. The HSRP was unable to obtain a copy of the original population source in order to ascertain either the total estimated population of the DRC at that time or the share of the total population represented by the 19.9 million in the east. We were, however, able to find an independent source that also referenced the same population data (Christian P. Scherrer, 2002. *Genocide and Crisis in Central Africa: Conflict Roots, Mass Violence, and Regional War*, Praeger: Westport, CT., 2002, footnote 20, 360.) Scherrer notes that this 19.9 million comprised 45% of the total population of the DRC at the time. Assuming a (continued) 55:45 split in population between the east and the west of the DRC, and applying the IRC's assumed baseline of 1.5 deaths per 1,000 per month to the west, and the survey-derived estimate of 5.4 deaths per 1,000 per month to the east, we can calculate the estimated crude death rate for the whole country as of April 2000 (i.e., the end of the first set of surveys). The calculation is the population-weighted average of the two crude deaths rates, or (5.4*.45)+(1.5*.55)=3.255.

Child Mortality Estimation Group (IACMEG) dataset.[14] The trend data shown in the figure below make it clear just how radically different the IRC's estimates are from those of the DHS survey.[15]



The bright blue line is the IRC's child mortality data trend. It reveals a dramatic increase in mortality over a relatively short period of time. *This is greater than any other credible increase in the child mortality rate since 1970 in wartime in the IACMEG child mortality dataset.*

[14] The IRC itself does not publish under-five mortality estimates. But to make the figures comparable with other surveys, under-five mortality estimates can be derived from the IRC's survey findings by making the same assumptions that the IRC employed for its own estimate procedure for excess deaths. A detailed explanation of how the HSRP calculated the IRC's under-five mortality rate for the period of the first two surveys can be obtained from the HSRP by emailing a request to hsrp@sfu.ca.

[15] Inter-Agency Child Mortality Estimation Group (IACMEG), "Child Mortality Estimates Info", http://www.childmortality.org (Accessed 23 September 2009).

The DHS mortality data, on the other hand—represented by the dark blue and red lines—show no increase at all. Unless the DHS data are *hugely* wrong—which no one has suggested—it is inconceivable that a jump in mortality as sudden and dramatic as that recorded by the IRC could have gone undetected.

Figure 1 also shows the trend line data for the 1984 census (purple trend line) and for two UNICEF surveys (green and yellow trend lines). These taken together with the DHS data show a trend that suggests that the high nationwide mortality rate in the DRC owes more to the long-term socio-economic and governance crisis in the country than to the effects of the war in the eastern region. This possibility is discussed in Jon Pedersen's review of the IRC's estimates for the HNTS.

It is, of course, theoretically possible that the IRC's data are correct and those of the DHS are wrong. But the DHS data were collected using standard survey methodology; the IRC's data were not—as the IRC has admitted. Given this, given the unprecedented nature of the increase in mortality during the IRC's first two surveys, and given the other methodological problems noted in the "Shrinking Costs of War", the evidence strongly suggests that it is the IRC's estimate that is wrong.

We conclude that it is unlikely that the huge increase in mortality that the IRC records ever took place—a conclusion shared by other critics.

As Jon Pedersen of Norway's FAFO Institute notes in his review of the IRC's methodology for the HNTS, "the use of the sub-Saharan baseline CDR [Crude Death Rate] for estimating excess deaths is problematic, *as is the implicit assumption of a very rapid increase at the outbreak of the war*."[16]

Pierre Salignon, Project Director of the HNTS, makes essentially the same point when he notes that, "It is unlikely that the war led to a sudden increase [in the mortality rate]." [17]

[16] Health and Nutrition Tracking Service, 9. Emphasis added.

[17] Ibid, 4.

If the trend in the DHS child mortality data and the conclusions of the two reviewers of the IRC's methodology are correct, then it follows that the true death toll in the DRC for this period is far lower than the 2.5 million deaths claimed by the IRC.

Given the series of methodological errors the HSRP research team uncovered in the first survey, given the IRC's admission that the populations it sampled were not representative of the population of the wider eastern region, and given Dr. Roberts' unequivocal statements about "violating standard academic protocols"[18] and the "bad science"[19], we simply do not understand how the IRC can continue to claim that its research has relied on "standard and scientifically-grounded methodology."[20]

The IRC's Problematic Baseline Rate

Determining the baseline mortality rate accurately is critically important in any exercise to estimate excess deaths. Getting it wrong can have a dramatic impact on death toll estimates. In the case of the DRC's third, fourth and fifth surveys, the HSRP analysis reveals how a relatively modest increase in the baseline mortality rate—by one third—decreases the excess death toll by almost 70 percent.

We agree with the IRC that baseline mortality rates are very difficult to determine in conflict zones. Indeed the central argument of Chapter 4 of the "Shrinking Costs of War" is that accurately determining pre-war mortality is only possible in rare circumstances.

The IRC claims that its estimate of 1.5 deaths per 1,000 per month (the SSA average) is "conservative"—i.e., it is higher than previous estimates drawn from the census data and the UNICEF surveys. But the IRC never explains why it believes that the SSA average is an appropriate measure of the prewar mortality rate for a country that is far from average in sub-Saharan Africa.

The DRC languishes at the bottom of most development indicators for SSA. It suffered a devastating 20-year economic decline from the mid-1970s that reduced its GDP per capita from

[18] Giles.

[19] Roberts, 2005, 1.

[20] IRC and Burnet Institute response, 1.

more than US$300 to just a third of that figure by 1998. Foreign aid was withdrawn almost completely in the early 1990s, and Mobutu's hopelessly inept and corrupt government had collapsed in total disarray by 1997.

The experts who reviewed the IRC's DRC research for the HNTS have all expressed skepticism about the choice of the SSA average as an appropriate baseline. Harvard's Kenneth Hill, for example, notes that, "…the IRC counterfactual is not appropriate. [The] DRC almost certainly has had above average mortality by SSA standards for decades."[21]

[21] Kenneth Hill, "Comments on IRC Estimates of Mortality in the DRC and on Estimates by Lambert and Lohlé-Tart" (Undated Review of IRC research on mortality in the DRC undertaken for the WHO-affiliated Health and Nutrition Tracking Service.) The WHO's Francesco Checchi notes that, "… it is plausible to assume a higher CMR [Crude Mortality Rate] for DRC than for the rest of Sub-Saharan Africa, even in the absence of a war." And Jon Pedersen from Norway's FAFO Institute writes that, "the average Sub-Saharan rate for comparison is also problematic…[the] DRC in 1998 was a country that had for many years undergone a profound crisis of governance, which may well have affected mortality patterns." (See Health and Nutrition Tracking Service, 8-9, 30 respectively for Checchi and Pedersen comments).

These are not the only grounds for skepticism about the IRC's choice of baseline. The IRC stresses that the SSA average mortality rate of 1.5 deaths per 1,000 per month is "conservative" because it is higher than UNICEF-reported rates of 1.2 deaths per 1,000 per month in 1996 and 1.25 in 1998 (Dr. Roberts refers, in his response to "The Shrinking Costs of War", to the rate as 1.3 deaths per 1,000 per month in the year 1997; we suspect he is referring to the 1.25 figure for 1998). These lower figures are intended to lend credibility to the use of the 1.5 SSA figure as a baseline for the DRC at the beginning of the war. But are the figures reported by UNICEF credible? There are certainly reasonable grounds to challenge them. First, they both indicate that the DRC's prewar mortality rate is *lower* than the SSA average rate. This is despite the fact that all reviewers believe that the SSA average is too low to be used as the baseline rate for the DRC. Second, with reference to these UNICEF-reported rates, the IRC acknowledges the "limitations of such data, including reservations concerning its validity and ability to capture regional differences in a country as vast as DR Congo." See: Benjamin Coghlan et al., ―Mortality in the Democratic Republic of Congo: An Ongoing Crisis" (New York: International Rescue Committee, 2008), http://www.theirc.org/resource-file/irc-congo-mortality-survey-2007, 17. This is presumably why the IRC chose not to use either of these figures as its baseline. Third, both of these rates are given as single figures. In fact there is no way that a mortality rate that is derived from a survey can be determined with the degree of precision that a single figure suggests. The usual way of indicating the extent of uncertainty is with confidence intervals; in this case they would likely have been large. Whether or not a confidence interval was reported in the original data, there would

But there is an additional reason for concern about the reliability, not just of the UNICEF figure, but all survey-based estimates, including those from the IRC. Data from the child mortality surveys collated by IACMEG are an excellent guide to long-term trends, but between individual surveys there can be substantial variations in child mortality rates over the same time periods. The differences can be very large:

In Angola in 1998, for example, UNICEF recorded a child mortality rate of 271, while the DHS figure over the same period was 179.

In Zimbabwe in 1996, UNICEF recorded an estimate of 108.6; the DHS figure was 54 over the same period.

In Somalia in 1997, UNICEF recorded estimates of 238 and 156 over the same period.

The sharply divergent mortality estimates by the two major institutions that conduct child mortality surveys suggests that little confidence can be placed in *any* particular survey. This applies to the mortality rate that was derived from the UNICEF survey and used to legitimize the IRC's choice of the SSA average as its pre-war baseline point estimate for the DRC in 1998.

The issue here is a simple one. The huge divergences in mortality rates between the mainstream surveys taken over the same period—and the many more, lesser but still significant, differences––over the same periods of time indicate just how imprecise the process of deriving mortality rates from surveys can be.

In cases like those above, at least one of the estimates must be wrong. The problem is that we have no way of knowing which one. Little confidence can therefore be placed in any single survey-derived estimate. This includes the UNICEF data that the IRC used to legitimize its decision to use the SSA average mortality rate as a baseline for the DRC in 1998.

The large divergences in mortality rates from different surveys taken over the same time periods place a huge question mark over the ability of surveys to provide reliable estimates of baseline

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necessarily have been uncertainty around that figure. But the IRC gives no indication that this figure would necessarily have been subject to considerable uncertainty.

mortality rates. Without reliable baseline trend data, estimating excess deaths accurately is impossible. This is the main conclusion of Chapter 4 of the "Shrinking Costs of War".

The Dramatic Impact of Alternative Baseline Measures on Excess Death Estimates

In the last three surveys undertaken by the IRC the areas to be surveyed were selected appropriately—i.e., so that they were representative of the country as a whole. But the use of the sub-Saharan African average mortality rate as a baseline places a major question mark over the accuracy of the excess death toll estimate.

To demonstrate the impact that a relatively moderate change in the baseline mortality rate—in this case an increase of just one third—can make to excess death estimates, the HSRP re-ran the excess mortality calculations for the IRC's last three surveys using a baseline rate of 2.0 deaths per 1,000 per month instead of 1.5. The difference was startling. Using the IRC's baseline mortality rate of 1.5 deaths per 1,000 per month, the excess death toll was 2.83 million. Replacing the IRC's figure with the modestly higher rate of 2.0, the death toll shrank to less than 900,000.

Given the consensus among reviewers that the IRC's baseline estimate was too low, any alternative baseline estimate would have to be higher. The rationale for believing that 2.0 deaths per 1,000 per month is a plausible pre-war mortality rate for the DRC is based on a number of logical inferences from the IRC's own data and is linked to the IRC's findings about excess deaths in the west of the country during the period of the first two surveys.

The survey-derived overall mortality rate recorded by the IRC in what it describes as the "non-conflict" western region of the DRC in its third survey was 2.0 deaths per 1,000 per month. During this period the IRC recorded neither violent deaths, nor any other "excess deaths" in the west.

The IRC's findings also suggest that there were no excess deaths in the west of the country for the period of the first two surveys:

- First, the IRC recorded zero excess deaths in the west for the period of the first two surveys in the chart on page 13 of its final report[22]; and
- Second, the IRC's estimate of 5.4 million excess deaths in the DRC between 1998 and 2007 is obtained by adding the 2.5 million estimated excess deaths for the eastern region during the first two surveys, to the nationwide estimate for the periods covered by the final three surveys. Given that the nation-wide excess death toll estimate for the period 1998 to 2007 does not include an estimate for excess deaths in the west of the country during the period of the first two surveys, we conclude that the IRC assumes that there were no excess deaths in the west during those periods.

We have no idea whether the IRC's assumptions about excess mortality in the west during the period of the first two surveys are correct, but they are not implausible. They are consistent with what we know about the patterns of violence in the DRC and the lack of connectedness between the east and the west of the country. From the start of the war in August 1998, the violence was concentrated in the eastern region. Much of the western region, which was controlled by the government, is half a continent away from the war-stricken east. Communication between east and west is minimal and livelihoods throughout the region are based mostly on subsistence agriculture. As such, they are less likely to be disrupted by distant armed violence—with the attendant risk of increased mortality—than would have been the case in a more economically interdependent country.

Given that according to the IRC's findings, there were no excess deaths in the west during the periods of the first, second or third surveys, other things being equal, the average mortality rate for the region during the period of the first two surveys should be the same as that recorded in the third survey—i.e., 2.0 deaths per 1,000 per month.

Finally, other things again being equal, the mortality rate for the whole of the DRC immediately prior to the war should the same as for the non-conflict west of the country. It follows that the 2.0 deaths per 1,000 per month is a plausible pre-war mortality rate for the DRC.

[22] Coghlan et al., 2008.

Like all baseline mortality estimates this figure is open to question, but the HSRP is not alone in believing that the 2.0 deaths per 1,000 per month is appropriate. The WHO's Francesco Checchi notes in his review of the IRC's research for the HNTS that his approach to the baseline issue would be "…to use the east to west CMR [Crude Mortality Rate] rate ratio, which in practice (though not in theory) means adopting the CMR in the west as the baseline for the entire country."[23]

We also note that in its sensitivity analysis for its report on the third survey, the 2.0 deaths per 1,000 per month rate is one that the IRC itself uses to demonstrate the effect of changing the baseline mortality rate. From this we assume that, while the IRC's preferred baseline mortality rate is 1.5, it believes that the 2.0 rate is not implausible.

The point of this exercise was not to produce a "correct" excess death toll estimate—for the reasons noted here and in Chapter 4 of the report, we are skeptical that this is possible—but rather to show how using a modestly higher baseline mortality rate—one we believe is more plausible than the IRC's—can produce a radically lower excess death estimate. We should have made this clearer in the discussion of this issue in the "Shrinking Costs of War".

Finally, we note one more possible consequence of continuing to rely on the IRC's low baseline estimate. By 2007 according to the IRC, just 0.4 percent of excess deaths at the national level were being caused by wartime violence[24]—99.6 percent were the result of the same non-violent causes, mostly disease malnutrition, that also cause most deaths in peacetime.

What would the excess death toll be in 2017 if we assumed that for a further ten years the average mortality rate for the whole country would be some 2.0 deaths per 1,000 per month? This rate is *lower* than the IRC's survey-derived overall mortality rate for the 2006-2007 survey

[23] Health and Nutrition Tracking Service, 7, 39. Note that the Checchi paper comes after the paper by Jon Pedersen in this file.

[24] According to the IRC, only 0.6% of all deaths in the east were the result of violence, while only 0.3% of all deaths in the west were due to violence. (Benjamin Coghlan et al., "Mortality in the Democratic Republic of Congo: An Ongoing Crisis" (New York: International Rescue Committee, 2008), http://www.theirc.org/sites/default/files/migrated/resources/2007/2006-7_congomortalitysurvey.pdf, 7).

period, however, it reflects the trend in the mortality in the DRC since 2002[25]. With a 1.4 deaths per 1,000 per month baseline mortality rate, there would be *an additional 5.0 million excess war deaths*, bringing the 1998 to 2017 excess death toll up to more than 11 million.[26] (Note: as the IRC points out the SSA average mortality of 1.5 deaths per 1,000 per month was revised down to 1.4 in the period covered by the last survey.)

But with an overall nationwide mortality rate of 2.0 deaths per 1,000 per month *and* a baseline mortality rate of 2.0, there would be *zero* excess deaths. In this latter case there would still be very large numbers of people dying from disease and malnutrition, as was the case before the war. But the drivers of disease and hunger would be the nationwide economic crisis, endemic poverty, corruption and bad governance that have plagued the DRC for decades, rather than warfare.

Again the point of this hypothetical example is *not* to demonstrate that the 2.0 baseline figure is correct, but simply to note what a huge difference relatively modest changes in the baseline mortality rate can make to excess death toll estimates.

Untrue and Misleading Statements by the IRC

The HSRP "Attempts to Discredit Large-Scale Mortality Surveys"

The IRC claims that the HSRP "attempts to discredit large-scale mortality surveys." This is completely untrue. We believe—and say in Chapter 4—precisely the opposite, namely that, "…such surveys are critically important sources of data for war-affected countries where there are rarely any reliable governmental statistics."

[25] What is the case for assuming an average mortality rate of 2.0 deaths per 1,000 per month for the ten years following the IRC's last survey in 2007? The nationwide mortality rate has been declining steadily throughout the new millennium so it is not at all unreasonable to assume that for the ten years after 2007, the nationwide death rate could continue to decline from 2.2 deaths per 1,000 per month—which is the survey-measured nationwide crude mortality rate in the DRC as of 2007 (Coghlan et al., 2008, ii)—to an average of 2.0. Note that it is not being argued that this figure is correct, simply that it is plausible.

[26] When this response was initially posted, the excess death toll was given as 5.8 million. However, a computational error was later discovered.

Indeed we argue that population surveys should be mandated for every new UN peacekeeping mission in order to provide an evidence-base for peacebuilding policies where none currently exists, with follow-on surveys for the purpose of impact evaluation. Our concern, as the text makes very clear, is not with surveys *per se*, but rather with the use of surveys to estimate nationwide excess death tolls in war-affected countries.

The Methodological Problems the HSRP Raises "Have Already Been Acknowledged"

The IRC claims that the methodological problems that the HSRP has identified "…have already been acknowledged by the IRC and widely discussed in the process of scientific peer review and at academic conferences."

Two points are relevant here. First, it is quite true that some of the problems we point to have also been noted by reviewers of the IRC's research on the DRC for the WHO's HNTS. But these criticisms—about the baseline mortality rate and the IRC's failure to follow standard survey practice in the first two surveys—continue to be rejected by the IRC.

Second, some of the most critical issues the HSRP raises have *never* been discussed publicly before. The HSRP's review of the first survey, for example, exposes a series of major errors that, when addressed, reduce one of the IRC's estimates of the excess deaths for this period from 1.6 million to less than 700,000. The IRC ignores this major error—and no other reviewers have addressed it. Nor has there been any serious discussion—in the literature, or by the IRC—of the critical issues raised in Chapter 4 of the "Shrinking Costs of War." These include the huge challenges of trying to estimate excess mortality in cases where the prewar mortality rate has been changing. Finally, no other review has pointed out that the IRC's survey-derived under-five mortality rate estimate is twice that of the DHS over the same time period—and that both estimates cannot be true.

The IRC Implies that its Surveys were Intended to Inform Humanitarian Policy

The "Shrinking Costs of War" argued that nationwide excess death tolls were of little value for humanitarian workers on the ground in war-affected countries. They need to know who is dying, where, and from what causes, not how many people have died since the war began. This goal, we argued, is met by relatively small-scale needs assessment surveys in war-affected areas. The

IRC states in its critique that it "firmly disagrees" and that "excess mortality estimates derived from retrospective surveys can be invaluable for guiding humanitarian programming and policy…"

In fact, both Dr. Les Roberts, who was the IRC's team leader on the first two surveys, and Dr. Ben Coghlan who led the most recent surveys, have argued that the surveys were pursued primarily for advocacy purposes.

In a recent interview with the *New Scientist*, Dr. Les Roberts claimed that, "*…it would have been irresponsible not to produce a national figure, as aid agencies would not have grasped the scale of the conflict.*"[27] This advocacy goal was deemed important enough to disregard standard survey practice.

Dr. Coghlan is on record saying that the IRC saw that the primary function of the more recent surveys as serving "…advocacy purposes rather than to inform directly the immediate humanitarian operations."[28]

While estimates of huge nationwide excess death tolls may make media headlines, they are not only prone to major inaccuracies, they are wholly unnecessary for the determination of humanitarian need in war-affected countries. A far more reliable procedure, one that is used in nearly all needs assessment surveys and that completely avoids the huge challenges of estimating baseline mortality, is simply to note where, and by how much, survey-derived mortality rates exceed the humanitarian emergency threshold.[29]

An alternative approach, equally appropriate and one sometimes used by the IRC, would be to *compare* mortality rates in the war-affected areas to the average mortality rate for the region. Saying that the mortality rate in the eastern DRC is—say—five times the SSA average would be

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[27] Giles. Emphasis added.

[28] Benjamin Coghlan et al, *Review of Publicly Available Surveys, North Kivu, DRC, 2006-2008,* Health and Nutrition Tracking Service, 2009, http://www.who.int/hac/techguidance/hnts/hnts_review_of_publicly_available_surveys.pdf., 10.

[29] London School of Hygiene and Tropical Medicine, "Existing Recommendations". *The Use of Epidemiological Tools in Conflict-Affected Populations: Open-Access Educational Resources for Policy-Makers*. http://www.lshtm.ac.uk/hpu/conflict/epidemiology/page_112.htm.

both meaningful to the media and other non-specialists and would completely avoid the many pitfalls associated with measuring nationwide excess death tolls. (Note that using the regional mortality rate as a *comparator* is very different from using it as a baseline for measuring nationwide excess deaths.)

The Peer Review Question

To support its contention that its findings are scientifically sound, the IRC stresses that its methodology has been peer reviewed. Our response to this is simple. We support peer review unequivocally and we think it plays a critical role in advancing scientific knowledge. However, as a recent article in the *Journal of the Royal Society of Medicine* (*JSRM*) has demonstrated, peer review appears to be remarkably ineffective at detecting major methodological errors in medical journals.[30]

The *JRSM* article noted above examined the findings of a study by a team of researchers who had asked 607 *British Medical Journal* peer reviewers to review three articles, each of which contained nine deliberate major errors. *On average the reviewers failed to detect 67 percent of the major errors*.[31] Two previous studies had come to similar conclusions.

The authors of the article, who included the current editor of the *British Medical Journal*, noted that. "…journal editors should not assume that their reviewers will detect most major flaws in manuscripts. The study paints a rather bleak picture of the effectiveness of peer review."[32]

Our point here is simple: the fact that the IRC's research has been peer reviewed by journals cannot be taken as evidence that it is free of major errors.

As noted earlier, peer reviews of the IRC's research on the DRC have also been commissioned by the HNTS. Like the HSRP, these reviewers note the appropriateness of much of the IRC's research methodology, particularly with respect to the last three surveys. But—again like the HSRP—they also note methodological problems. These include skepticism that the IRC's choice

[30] Sara Schroter et al., "What Errors do Peer Reviewers Detect, and Does Training Improve their Ability to Detect them?" *Journal of the Royal Society of Medicine* 101 (2008), http://jrsm.rsmjournals.com/cgi/reprint/101/10/507.

[31] Ibid.

[32] Ibid.

of the SSA mortality rate is an appropriate baseline mortality rate for the DRC, skepticism about the dramatic jump in the mortality rate immediately after the war started, and a conclusion that the findings of the first two surveys are "speculative at best".[33]

Why Getting It Wrong Matters

There is a broader context to this debate that is important to bear in mind. As Ian Smillie and Larry Minear point out in their landmark 2003 study, *The Charity of Nations: Humanitarian Action in a Calculating World*, the institutional survival of humanitarian NGOs is dependent on donor funding. But the level of funding they receive is directly related to assessments of humanitarian need—assessments that they themselves are usually responsible for generating. This, Minear and Smillie argue, creates perverse incentives:

> It is also common practice…for those seeking funds to exaggerate need…In a highly competitive environment—made competitive by great needs and inadequate funding—exaggeration not only pays, it is sometimes the only thing that will dislodge funding from donors who themselves have too few resources and too many supplicants.[34]

This is not the only challenge. Sometimes lack of experience in survey design and implementation, especially in the case of small NGOs, is the cause of mistakes. As UNHCR's Paul Spiegel has pointed out:

> In the response to humanitarian emergencies to date, there have often been poor quality surveys, insufficient coordination, political interference and inadequate funding for the provision of reliable and timely survey information…It is likely that policies, programmes and hundreds of millions of dollars in resource allocation have been decided upon, at least in part, using bad information.[35]

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[33] Health and Nutrition Tracking Service, 30.

[34] Ian Smillie and Larry Minear, *The Charity of Nations: Humanitarian Action in a Calculating World*, Bloomfield CT., Kumarian Press, 2004, 207.

[35] Paul B. Spiegel, "Who Should be Undertaking Population-based Surveys in Humanitarian Emergencies?" *Emerging Themes in Epidemiology* 4:12, 2007, http://www.ete-online.com/content/4/1/12.

But whatever the reason, the effect has been the same—the creation of mutual suspicion between donors and NGOs and humanitarian agencies, a development that puts at risk the effective implementation of humanitarian policy.

One way to address this challenge would be to make assessments of the health consequences of armed conflicts independent of the organizations responsible for on-the-ground implementation of humanitarian assistance. This idea, canvassed by Dr. Spiegel, among others, would mean that needs-assessment surveys would be carried out by teams led by independent, professional epidemiologists.[36]

Such an initiative would improve the often very uneven quality of data from the field, while addressing donor concerns that NGOs inflate the seriousness of crises to secure more assistance. NGOs, on the other hand, would be able to point to independent assessments of need when making the case that more assistance is warranted. Accurate needs assessments are also a necessary—though not sufficient—condition for the equitable allocation of humanitarian assistance according to need.[37]

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IRC and the Burnet Institute Strongly Affirm Congo Mortality Study Findings[38]

The International Rescue Committee's authoritative surveys in the Democratic Republic of Congo, carried out between 2000 and 2007 by a team of epidemiologists, some of the foremost experts in the field, concluded that there were millions of excess deaths attributable to the war in Congo. The last two surveys, in 2004 and 2007, were jointly carried out by the IRC and the Burnet Institute of Australia. The surveys, based on standard and scientifically-grounded methodology, helped reveal the true scale of suffering in one of Africa's largest countries. The results were widely cited, accepted by independent

[36] Francesco Checchi and Les Roberts put forward a similar proposal in their "Documenting Mortality in Crises: What Keeps Us from Doing Better?" PLoS Medicine 5, no. 7 (2008), http://conference.cedat.be/sites/default/files/Checchi%20&%20Roberts%202008.pdf, 6.

[37] The primary reason for skewed allocations of humanitarian aid has, however, been donor politics.

[38] Available on the blog *Turtle Bay:* http://turtlebay.foreignpolicy.com/posts/2010/01/20/the_battle_over_the_cost_of_war.

experts and published in three established medical journals after extensive peer-review. The findings remain the best estimates available of conflict-related mortality in Congo.

The Human Security Report Project has issued a new paper, *The Shrinking Costs of War,* that attempts to discredit large-scale mortality surveys in general and the IRC's Congo studies in particular. The methodological limitations they criticize have already been acknowledged by the IRC and widely discussed in the process of scientific peer review and at academic conferences, with broad agreement that they do not invalidate our findings. There is, in fact, little that is new in the authors' observations, and overall, their arguments are undermined by inconsistencies, conflicting evidence and poor scholarship.

The IRC and Burnet are compelled to respond to the report's claims and outline discrepancies and key facts:

First, the authors claim that for the first two mortality surveys, "IRC's researchers did not select the areas to be surveyed in a way that ensured that they were representative of the region as a whole." These two surveys were conducted during a raging war and therefore several large areas could not be sampled due to extreme insecurity. It was therefore necessary to apply results from surveyed areas in eastern Congo to regions that could not be sampled and to extrapolate these rates to determine the best estimate of excess mortality in the region. Calculations were made following consultations with experts. Because of the sampling limitations, we made deliberately conservative assumptions and extrapolations—likely leading to underestimates of mortality.

The significant challenges of conducting surveys in conflict zones were widely acknowledged and reported in the surveys. But even before their release, the reports were reviewed by scientists from the Centers for Disease Control and Prevention, Harvard, and Columbia University to ensure academic rigor. In spite of the limitations, our conclusions were widely accepted as valid by independent experts.

Second, the authors of the Human Security Report claim that the baseline rate that we used to determine mortality had there been no war was too low, and as consequence, our estimate of excess deaths too high. As the authors note, a baseline crude mortality rate is difficult to determine for conflict zones. The IRC and partner epidemiologists analyzed all available pre-war mortality statistics for Congo and elected to use a more conservative rate. The 1984 census in Congo documented a rate of 1.3 deaths per 1,000 per month. In 1996, UNICEF reported a rate of 1.2. This reduction in pre-war mortality is actually consistent with the "long-term downward trend in peacetime mortality that has become the norm for most

of the developing world," as described in the Human Security Report study. Nevertheless, we chose to use the higher baseline rate for sub-Saharan Africa of 1.5, again being deliberately cautious in our assumptions to insure we did not overestimate the number of excess deaths.

The Human Security Report study argues that we should have used a rate of 2.0 deaths per 1,000 per month, on the grounds that it was the rate that we documented in western Congo in our 2002 survey. This is surprising, as this rate is inconsistent with the "downward trend" purported by the authors. The likely cause of the 2.0 rate in the west in 2002 was that the region was suffering the effects of lingering insecurity and a large-scale war in the east. We can find no evidence that 2.0 represents an appropriate national pre-war crude mortality rate and the authors of the report do not provide any either. Nevertheless, it is worth noting that if a crude mortality rate of 2.0 was used, estimated excess deaths would be 3.3 million since 1998.

The Human Security Report study also suggests that child mortality rates documented in the IRC and Burnet Institute's 2007 survey are double what they should be. They base this argument on a 2007 Demographic and Health Survey (DHS) which documents an under-5 mortality rate substantially lower than ours.

While DHS surveys are generally well-regarded, their 2007 Congo survey yielded some questionable findings. Among them, it reported that 85% of women received pre-natal care during the previous five years, that 70% of births occurred in a health facility and that 74% of women had a trained health professional present at the time of delivery. Experts and aid workers who know Congo simply find this data implausible.

We also find other data cited as fact by the authors of the Human Security Report to be inconsistent with DHS findings. In particular, the authors claim that immunization coverage in Congo for DPT3 and measles rose from 20% at the start of the war in 1998 to almost 80% in 2007. The DHS survey in 2007 states figures of 45% for DPT3, 63% for measles, and 31% for all vaccines combined. The authors should either withdraw their claims about high vaccination rates or acknowledge limitations in the 2007 DHS report. To quote the authors, "Both cannot be correct."

More importantly, our estimates have been supported by numerous other studies. Several smaller scale mortality surveys, including studies by MSF and Merlin, documented similar rates to the IRC surveys. A comprehensive review by the Centre for Research on the Epidemiology of Disasters (CRED) of all 82

Congo mortality surveys conducted between 2000 and 2006 also showed mortality rates in Congo consistent with our findings. With specific reference to the IRC and Burnet Institute survey in 2004, CRED said, “Our analysis of the distribution of CMR and U5MR across all provinces yielded similar findings.”

The IRC, Burnet and other partner epidemiologists strongly stand by our estimate that millions of people died unnecessarily due to conflict and humanitarian crisis in Congo. At the same time, we acknowledge that conflict epidemiology is an inexact science. Like all surveys, including public opinion polls, our reports have always included margins of error. As noted in the most recent IRC / Burnet Institute report, 5.4 million is our best estimate based on established methodology and conservative assumptions, but the real figure could be as low as 3.0 million or as high as 7.6 million.

Overall, the Human Security Report paper is undermined by its own internal inconsistencies and selective cherry-picking from other studies to support the authors’ thesis. They frequently cite expert references, such as SMART or HNTS, but fail to present findings from those same groups that contradict their own claims.

We find many problems with the report unrelated to its analysis of the Congo mortality surveys. Among them, the report incongruously lumps together in one classification a relatively minor conflict, such as the occasional fighting in Senegal, which occurred in the context of overall growth and prosperity in one of Africa’s most peaceful countries, with devastating national wars such as those in Somalia, Liberia, and Congo.

But more troubling to us is that the authors argue against using retrospective mortality surveys for estimating excess mortality on the grounds that there are too many challenges and that they have little practical utility for humanitarian policy and response. We firmly disagree. The authors’ point ignores the fact that non-credible sources will always speculate on war time death tolls. Indeed, an unsourced New York Times estimate of 100,000 deaths in Congo in 2000 was among the reasons the IRC conducted its first survey. That study’s findings revealed to the world the full scale of Congo’s humanitarian crisis, leading to major changes in humanitarian policy and international political engagement. A review of mortality surveys in Darfur, Sudan, by the US Government Accountability Office was used to counter inflated mortality estimates that were being promulgated by various groups. Similarly, a Kosovo-wide survey in 1999 led to an authoritative estimate of war deaths that helped respond to claims of higher death tolls. The record shows that excess mortality estimates derived from retrospective surveys can be

invaluable for guiding humanitarian programming and policy and for responding to the politicization of human suffering.

The IRC and Burnet used sound scientific methods for estimating the previously unknown cost of war in Congo, showing that millions, rather than thousands, had died as a result of the war and its aftermath. We believe this information is valid and that it has been and continues to be of essential value to public health and political decision-makers.

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