Under-5 Mortality Estimation in Humanitarian Emergencies: A comparison of estimation methodologies using microsimulation

by

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Abstract

Complex humanitarian emergencies are characterized by increases in mortality, mass migration, and collapse of infrastructure. Demographic estimation on under-5 mortality in these settings is generally conducted using household surveys. Indirect methods of estimation, collected using summary birth histories, have clear advantages over complete birth histories, as they are faster and require less training to implement. It is unclear, however, how well the analytic techniques developed for summary birth histories perform when mortality patterns fluctuate. Using the Socsim simulation program, one baseline and four emergency scenarios were developed and each was simulated 100 times. Two methods of indirect estimation for child mortality - the Brass methodology and the IHME methodology - and the direct method of under-5 mortality estimation were applied to assess how quickly each method was able to detect rapid changes in mortality, how well the method was able to estimate the underlying level of mortality, and for how long after the crisis period ended the method was affected by the increase in mortality. In general, none of the indirect methods performed well. The Brass method, though able to detect abrupt changes in mortality is inadequate because of its reliance on a reference period. The IHME methods, though able to estimate mortality for the survey year, were generally not able to accurately estimate the level of mortality change in situations with extreme changes. In situations of fluctuating mortality, all indirect methods smoothed fluctuations, eliminating the ability to estimate excess deaths due to conflict. Although more time-consuming, if under-5 mortality is of primary interest, complete birth histories and direct estimation should be used.

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Chapter 1: Introduction

> *Despite this attempt at bureaucratic accountability, however, the figures for what happened in the Congo are not satisfactory. Stephen records that 'between 12 and 32 million' died there. He has underlined the word 'between' heavily. He has also noted the twentieth century deaths of 800,000 Armenians, 6 million Jews, about 3 million in Bangladesh, some 20 million in the labour camps of the Soviet Union, 2 million in Vietnam, and between 1 and 3 million in Cambodia [...] The words 'between', 'about' and 'some' are all underlined in red ink. [...] 'Between 12 and 32 million killed' is a phrase that cannot exist. [...] What kind of history, what kind of mathematics is this, what has happened to those spare tens of millions? Unnumbered, unburied, will they haunt the earth forever, will they ever find a resting place? Do they not jostle us, do they not stifle us, are we not kept awake at nights by their squeaks and gibbering batlike cries?*
>
> \- Margaret Drabble (1991) (1)

In the twentieth century, great progress was made in improving health on the global scale. Estimates of life expectancy improved, on average, by one-sixth to a quarter of a year, each year; for the past 160 years, female life expectancy has improved an average of three months each year while male life expectancy has improved by approximately two months per year (2). Although estimates of child mortality prior to the mid-twentieth century are difficult to obtain, reductions since 1950 have been rapid; since 1950, under-5 mortality is estimated to have declined from 214 deaths per 1,000 live births to 59 deaths per 1,000 live births (3). In tandem with these improvements came improvements in demographic estimation. Methods for estimating life expectancy, maternal mortality, child mortality and other demographic indicators were developed and/or refined, benefitting from advancements in survey design and implementation, computer technology, and mathematics. The development of super-computing and the ability to store massive amounts of data allow researchers to study demographic processes at a speed and scale previously impossible.

The progress of the twentieth century was not uniform, however. From the Lost Generation arising from World War I to the Holocaust and the Great Leap Forward, from the Rwandan genocide to the ongoing conflicts in Afghanistan and the Democratic Republic of Congo, the twentieth century, and now the twenty-first, were defined as much by conflict, genocide, and famine as by progress. And while demographic methods have improved in many fields, demographic estimation in emergencies continues to lag (4,5). Destruction of records, mass migrations, and unstable conditions challenge researchers in the development of reliable estimates of mortality, while political and economic pressures may lead to estimates that are over- or underestimated for strategic gain (6). Often estimates that arise from countries affected by humanitarian emergencies are deemed too unreliable to include in global estimates of mortality, leaving the accuracy of global estimates in question and leaving deaths resulting from complex emergencies unaccounted for (7).

Problem Statement

In recent years, there has been greater effort to improve our ability to estimate the impact of war and other humanitarian emergencies and research in conflict demography has grown (5). A critical gap that has been identified is the inability to estimate mortality for sub-groups within a population, such as under-5 mortality; in general, estimates from humanitarian emergencies are generated only as crude death rates, occasionally disaggregated by sex (4). Given the disruptions in mortality patterns as a result of conflict or famine, the underlying age and sex-specific distribution of a population will change and extrapolating crude death rates to estimate under-5 mortality rates or excess deaths to children or other age groups is difficult and unreliable (4). Gaining a better understanding of the impact of war on sub-populations, particularly under-5 mortality, is critical so that relief services can be improved, health systems can better meet the needs of populations, and post-emergency reconstruction can provide resources where they are needed most.

How best to generate estimates of under-5 mortality in complex humanitarian emergencies, what questions to ask and analytic techniques to use, is the subject of this dissertation. The majority of mortality data in emergencies is collected via household surveys. There are several techniques that can be used to estimate under-5 mortality with surveys, and while comparisons of these methods have been conducted using data from non-emergency settings, no comparisons have been conducted to specifically test which method will generate the least biased numbers in an emergency. This dissertation uses data from hundreds of simulated datasets, designed to simulate four humanitarian emergencies, to test two methods of indirect estimation, the Brass method and the IHME methods, against the direct method of estimation.

Specifically, this dissertation will explore the following questions:

1) How fluctuations in mortality affect the Brass indirect estimation technique: How quickly will the Brass method detect an increase in mortality and how accurate is that estimate? How long after a crisis period has ended will the Brass method be affected by an increase in mortality?
2) How fluctuations in mortality affect the IHME indirect estimation techniques: How quickly will the IHME methods detect an increase in mortality and how accurate is that estimate? How long after a crisis period has ended will the IHME method be affected by an increase in mortality?
3) Is one of these methods more suitable to use to estimate under-5 mortality during and after a complex humanitarian emergency?

By exploring which methods are best able to capture abrupt changes in mortality in a rapid fashion, how well those methods capture the true extent of mortality change, and how long those methods are affected by past increase in mortality, it may be possible to identify the best way to measure under-5 mortality in emergency settings. Identifying the best methods and subsequently, the best survey questions and analytic techniques to use, serves multiple purposes. First, it alleviates some of the ethical concerns of conducting research in fragile settings; research conducted using unreliable methodology places people in danger for no real purpose. Improving our understanding of which methodologies are reliable in these settings and the best way to implement them does not ease all ethical considerations arising from conducting research, but at least researchers can better weigh the risks and benefits of conducting research. Improving our ability to estimate under-5 mortality in emergency settings can lead to better relief and humanitarian response outcomes, particularly if estimates can be generated quickly. Knowing the burden of mortality on children can aid in the allocation of resources, channeling limited resources to those most in need. More reliable estimates of mortality in emergencies also lead to more reliable estimates of mortality on a national and global scale. As countries continue to pursue the Millennium Development Goals, reliable estimates of mortality arising from the most fragile environments can contribute to policy changes and health system development to improve child health. Finally, we can eliminate such phrases as “between 12 and 32 million killed”, gaining a better understanding of the true costs of war and famine on nations and people.

Chapter 2: Background and Review

In 2000, the United Nations set the ambitious Millennium Development Goals to improve health, education, environment, and wealth throughout the world by 2015. Millennium Development Goal 4 aims for a two-thirds reduction in the under-5 mortality rate, or the probability of child dying between birth and age 5, from 1990 levels. As 2015 approaches there has been increasing demand for timely and accurate estimates of under-5 mortality. Aside from the Millennium Development Goals, the under-5 mortality rate is also used in the development of life expectancy at birth and other summary indicators of mortality (8). Errors made when estimating under-5 mortality may then be compounded when used in other summary mortality measures; thus it is critical to obtain accurate measures of child mortality.

Two groups that have attempted to estimate childhood mortality trends over the past decades, the United Nations Inter-agency Group for Child Mortality Estimation (UN-IGME) and the Institute for Health Metrics and Evaluation at the University of Washington (IHME), have produced reports that largely agree at the global level. Child mortality is decreasing; UN-IGME estimated in 2010 that the number of under-5 deaths worldwide declined from 12 million in 1990 to 7.6 million in 2010, an annual rate of decline of 2.2% per year (9). IHME estimated 7.2 million deaths occurring in 2010 and confirmed the same average rate of decline of 2.2% (10). The groups differ, however, on the extent and rate of the decline at the country level. Among those countries that both groups analyzed, in 2010, 20% differed by more than ten deaths per 1,000 live births and had relative differences greater than 10% (7). For 13% of the countries, the UN-IGME estimates were 30% or higher than the IHME estimates, while in 8% of the countries, the IHME estimates were 30% or higher than the UN-IGME estimates (7). While the estimates of the total number of deaths that occurred in 2010 was not statistically significantly different, at the country level there are significant differences in the level, total number, and rate of decline in child mortality.

The disparities between the two groups are the result of two reasons; the datasets that were included in analysis and the analytic method used. Alkema and colleagues found that a large percentage of the differences between the two groups was a result of using different datasets. UN-IGME included more datasets of arguably lower quality to increase the sample size and precision of estimates while IHME discarded datasets they felt were of highly questionable quality (9,10). The use of different analytic techniques also contributed to the inconsistencies between the groups, although, on average, the differences were not as large. The average difference over all years due to differences in data was 6 deaths per 1,000 births while the average difference due to estimation methods was close to 0. However, the use of differing estimation methods did occasionally result in significant differences; for example, the UN-IGME estimate for Pakistan was lower than the IHME estimate by 20 deaths per 1,000 live births (7).

Some of the largest discrepancies were found in countries that had experienced conflict or civil unrest, such as Afghanistan, Angola, and Somalia, and countries with high HIV prevalence (7). IHME discarded datasets that they felt were highly questionable, a large percentage of which were from countries affected by unrest (10). UN-IGME on the other hand, adjusted their estimates for such countries with expert-based input and external information such as health services coverage (9). The adjustments made were therefore largely subjective, but likely more accurate than results obtained from discarding such data completely (7).

The fact that countries affected by conflict have large inconsistencies in estimates of under-5 mortality underscores a critical problem; countries that have the greatest need to understand the burden of mortality often have the least ability to do so. The destruction of data collection systems and mass migrations associated with humanitarian emergencies make quality data collection and analysis extremely difficult. The challenges of demographic estimation are important to overcome, however. In addition to affecting global estimates and our understanding of progress towards the Millennium Development Goals, estimating the burden of mortality associated with humanitarian emergencies is important in its own right. With a better understanding of the human costs of conflict and civil unrest, humanitarian agencies can deliver better services, policy makers can better understand the economic and social impacts of war, and the international community can better decide what actions, if any, should be taken to prevent future crises. For example, the publication of surveys from the Democratic Republic of Congo, estimating 3.8 million excess deaths since 1998, resulted in a doubling of humanitarian aid (11). Equally important is to prevent the manipulation of mortality figures for political and economic gain. Both humanitarian agencies and political figures can benefit from artificially inflated or reduced mortality and morbidity numbers in an emergency. While the best efforts to collect and analyze unbiased data do not guarantee that they will be published or used appropriately, the continued efforts to document the effects of war can perhaps deter such manipulation over time. Understanding the best way to measure mortality in emergencies is a critical need and one that should be pursued, not in spite of, but because of, its inherent difficulties.

Demography and Complex Emergencies

The term *complex humanitarian emergency* describes a particular type of disaster. Keely (2001) defines it as

> a situation in which a large civilian population is affected by a combination of civil or international war, or a gross attempt to restructure the state or society (such as a genocide), leading to large-scale population displacement with accompanying deterioration of living conditions (such as food, potable water, shelter, and sanitation) creating the *potential for a significant increase in mortality* [emphasis added] typically during some limited period of time, but sometimes lasting much longer. (pg. 1).

Due to the difficulties in estimating population trends during a complex emergency, which will be discussed in more detail shortly, there is little agreement amongst demographers on the demographic impact of conflict. It is understood that mortality rates are affected by complex emergencies, but how mortality in complex emergencies is measured is inconsistent (5,12). When calculating deaths due to war, investigators may count only violent deaths(13), overall death rates (14), or estimate excess deaths, the number of deaths that would not have occurred if the conflict had been avoided (15). Which of these estimates is used can lead to differing conclusions about the overall impact of conflict on a population (12). Even when the same measure is used, different survey methodologies, the point in the conflict when estimates are taken, and the choice of analytic techniques can lead to vastly different findings. For example, among five surveys conducted in Iraq between March 1, 2004 and August 31, 2007, estimates of deaths due to violence varied from 26,000 to 1,033,000 (12). These estimates are crude estimates, only counts, and are not disaggregated by any indicators, such as age or sex, yet understanding the distributions of death across age and sex is critically important to frame humanitarian response.

When attempts are made to estimate mortality by age group, even when age groups are as crude as under- and over-5, discrepancies between surveys may become even larger. Estimates of child mortality in the DRC made by the International Rescue Committee were almost twice as high those found by the Demographic and Health Survey (DHS), but there is no consensus on which estimate is accurate (12). Understanding the age distribution of mortality in an emergency is critically important, however. Children in developing countries are vulnerable to mortality under normal circumstances, but even more so during emergencies (16). If a large portion of the population is under-5 and subject to extremely adverse circumstances, there may appear to be a rapid rise in the crude death rate. The opposite may also be true, however; if children make up a large proportion of the population in the initial stage of a complex emergency but die in excess relative to other ages, the remaining population may appear to have lower mortality over time (4). If so, heavy losses of a vulnerable population early in a crisis, followed by humanitarian assistance, can result in mortality levels amongst survivors that are lower than pre-emergency situations. Without a nuanced understanding of age patterns, this information could be used for political gain, for example, by claiming that the impact of a conflict is lower than otherwise assumed or used to inflate the impact of humanitarian assistance (4,17).

Despite consensus that high quality demographic estimation is important in times of complex emergencies, many estimates of mortality are generated without a critical examination of whether traditional demographic methods are appropriate to apply (11). That is, much of demographic estimation depends on core assumptions of mortality, fertility, and migration patterns that are unlikely to hold true during an emergency. Methods are often applied that do not account for these disruptions (5) and little work has been done to test how methodological limitations, particularly in the under-5 age group, are impacted by complex humanitarian emergencies (7,18,19). Additionally, while different survey and analytic methods exist to measure under-5 mortality, no comparisons have been conducted to see specifically which methods best estimate mortality levels in crisis. Finally, there is the question of how well these methods perform after an emergency has ended, once mortality has returned to approximately baseline levels. Although mortality levels may return to baseline, or slightly elevated levels, relatively quickly, the increase in mortality seen during the crisis period may continue to affect the ability of indirect methods to accurately quantify mortality over time. Much of research is done in post-emergency settings, when the danger to program and research teams is lower and, thus, an understanding of the effect of increases in mortality on indirect estimation techniques after crises has ended, is also important.

These are the question that I will explore through the use of microsimulation. Specifically, I will examine:

1) How fluctuations in mortality affect the Brass indirect estimation technique: How quickly will the Brass method detect an increase in mortality and how accurate is that estimate? How long after a crisis period has ended will the Brass method be affected by an increase in mortality?
2) How fluctuations in mortality affect the IHME indirect estimation techniques: How quickly will the IHME methods detect an increase in mortality and how accurate is that estimate? How long after a crisis period has ended will the IHME method be affected by an increase in mortality?
3) Is one of these methods more suitable to use to estimate under-5 mortality during and after a complex humanitarian emergency?

In this chapter, I will first provide a brief discussion of under-5 mortality estimation techniques in general, data sources, direct versus indirect estimation, and a background on the Brass and IHME methods. I will then discuss how mortality patterns in humanitarian emergencies may affect these estimation techniques and discuss the mortality patterns of four humanitarian emergencies that took place during the late twentieth century. Finally, I will provide some background on microsimulation and how it has been used to answer similar questions in demography before providing a detailed explanation of the simulations and analytic techniques in the methods chapter.

Under-5 Mortality Estimation

Data Sources

Vital Registration

Vital registration systems, which attempt to document all births and deaths in a population as they occur, are the preferred method of collecting information on child mortality because they are able to generate timely estimates and cover entire populations, thus eliminating sampling variance. While vital registration systems are preferred, they are generally expensive and require considerable infrastructure and oversight. As such, the majority of countries worldwide do not have well-functioning or comprehensive vital registration systems. The UN estimates that only 60% of the 230 countries and regions worldwide register at least 90% of births occurring in their borders and only 47% of countries have at least 90% coverage of deaths (20). In total numbers, it was estimated that almost one-third of the 135 million births and over two-thirds of the approximately 57 million deaths worldwide were unregistered and unrecorded in 2010 (21). In many countries, therefore, generating estimates of crude mortality, let alone under-5 mortality, cannot be done through the use of vital registration systems. As a result, cross-sectional population based surveys have become the primary source of data in countries without well-functioning vital registration systems.

Surveys

Household surveys generally employ one of two methods to retrospectively estimate under-5 mortality; a complete birth history (occasionally complete pregnancy history) and a summary birth history. A complete birth history gathers the date of birth and, if applicable, age at death or date of death, for every live birth reported by a female respondent. A complete pregnancy history additionally includes pregnancies that do not end in live birth, including abortion and stillbirth. A summary birth history does not gather specific information for each birth, but instead gathers aggregate data on the total number of live births that a woman has experienced, how many of these children are still alive and how many have died. For analysis of the summary birth history, additional information regarding mother's age or time since first marriage is also needed as a proxy for children's approximate age.

There are advantages and disadvantages to each method. Advantages of a full birth history include relying on fewer assumptions regarding past mortality and fertility patterns, the ability to calculate rates for a specific time period instead of a time period based on proxy measures, and because the exact time to death can be calculated, it is possible to calculate both a mortality rate and a probability of dying by a certain age. Mortality rates are an estimate of the risk of an event (in this case death) occurring in a population of known size and over a given time period, while probabilities express the likelihood of experiencing an event by a certain age or time periods amongst all who are at risk of the event. Both are valuable, but only a complete birth history allows for the estimation of both indicators. However, due to the detailed nature of the questions, complete birth histories require extensive training and supervision during fieldwork, increasing the cost and time of survey rounds. Additionally, complete birth histories rely on a woman's ability to accurately recall dates of birth and age at death for each live born child. Hill identifies an almost universal tendency for women to report age at death for children in exact numbers or convenient fractions of years, such as 12 months or 5 years, which can result in fluctuations in numbers of deaths around these arbitrary milestones (22).

The summary birth history addresses many of the disadvantages of the complete birth history. It is fast, requires less training and supervision in the field, and does not require accuracy in reporting multiple dates and ages, other than a woman's own age or years of marriage. However, it cannot differentiate between neonatal, infant and child deaths, nor can it accurately estimate recent trends in child mortality, such as within the last three years. Rather, the summary birth history generally estimates the probability of child dying before their fifth birthday for a year approximately 5-7 years previous to the survey. Finally, summary birth histories rely on more assumptions than a complete birth history, namely constant or linearly declining fertility and mortality. The assumptions for estimating under-5 mortality vary based on the mathematical method used to derive the estimates, and both the assumptions and the mathematical methods will be discussed in more detail below.

Analytic Methods

Analytic methods for estimating under-5, infant and neonatal mortality are relatively straightforward for complete birth histories, generally referred to as direct estimates. When using direct estimation, no assumptions need to be made regarding the underlying mortality and fertility patterns of the population. The only assumption required is that women are able to accurately report on the birth dates and ages of death of their children. This assumption is important when transforming mortality rates into probabilities (discussed in more detail in the methods chapter), but in general, the complete birth history can be completed with relatively few assumptions made by the researcher.

The analysis of summary birth histories, or indirect estimation, is more complicated. Several analytic methods have been developed, each with its own assumptions and mathematical foundations. This dissertation will focus on two methods, the Brass method and a set of methods developed by IHME, which I will call the IHME methods. The mathematical foundations of the methods will be discussed in greater detail in the methodology section but a brief discussion of the history and assumptions underlying each method is warranted here.

Brass

William Brass pioneered indirect estimation techniques for estimating child mortality using the summary birth history; in fact, the questions that make up the summary birth history are often referred to as the Brass questions. The Brass methodology is based on the idea that under-5 mortality can be estimated by analyzing the proportion of children ever born who have died. Births to groups of women, classified either by age or time since marriage, follow a distribution over time. The proportion of children ever born to a cohort of women then depends on the length of exposure to dying (time since birth) and the mortality risk itself. A higher proportion of children will die in a high mortality context; similarly, a higher proportion of children will likely have died among older mothers relative to young mothers because they will have had a longer exposure time. This proportion will also vary based on the underlying fertility distribution; societies with young childbearing relative to societies with older childbearing may appear to have a higher proportion of children dead as a result of exposure time. After adjusting for the distribution of births in time (the underlying fertility patterns of the population), it is possible to convert the proportion of children who have died to the probability of dying by age 5, the under-5 mortality rate.

To calculate the probability of dying by exact age 5 using the Brass method, the following information is needed:

1. Number of children ever born, classified by sex and by five-year age group of mother
2. The number of children surviving (or the number dead), classified by sex and by five-year age group of women
3. The total number of women (irrespective of marital status), classified by five-year age group. All women, not just ever-married women, must be included.

While the Brass method is the most widely used indirect method to estimate under-5 mortality, there are several limitations to the method. The first is that Brass methodology generally estimates under-5 mortality for a time period between 5 and 7 years prior to the survey. This is because information is discarded for women age 15-24, although these are the women most likely to have children in the recent past and thus contribute to recent under-5 mortality changes. Information is discarded because generally estimates of child mortality for these women are inflated. Women under 25 are more likely to have children who died in the recent past for a combination of two reasons: the first, they are simply more likely to have children under the age of five relative to older women and younger children, particularly infants are at a higher risk of dying than older children (23,24). Secondly, mothers under the age of 25, and particularly those under the age of 20, are more likely to be socioeconomically disadvantaged relative to women who have children of the same age but who began childbearing at older ages. The elevated risk of mortality amongst young mothers has been demonstrated in multiple studies (24,25) . For these reasons, under-5 estimates for younger mothers are biased upwards, inflating estimates for recent history (26). The reliance on estimates from 3 to 6 years prior to a survey can be a challenge for program monitoring and evaluation and to track recent changes and progress towards goals such as the Millennium Development Goals.

A second concern is that Brass methods do not generate estimates of uncertainty. The methodology does not estimate standard error or uncertainly bounds, like those that can be estimated from complete birth histories. Without uncertainly bounds it is difficult to know if differences between estimates over time are true differences or the result of randomness in the estimate due to sampling. This problem is particularly salient when attempting to make estimates amongst smaller populations such as those being serviced by program interventions or at sub-national levels, as small sample sizes may be more prone to random fluctuations in the estimate relative to larger samples (26).

Finally, the Brass method makes several assumptions. The first is that the age pattern of child mortality follows an a priori established pattern, generally that of the Coale-Demeny West level life table, and that mortality has been declining linearly in the recent past (22,27). Secondly, the method assumes that fertility has been roughly constant over time (22). The Brass methodology utilizes two set of multiplying factors, discussed in more detail in the methods chapter, to estimate the probability of dying by age five and these multiplying factors have been derived based on simulations of mortality and fertility decline. The multiplying factors have been tested and modified several times since the development of the Brass technique and various corrections have been applied to adjust for changing fertility, but the factors still depend on these assumptions (18,27–29). In general, violations regarding changing fertility patterns can be corrected for, particularly if data are available from sequential surveys, which can be used to estimate true cohort fertility patterns (22).

Violations in the underlying mortality patterns are of greater concern for this dissertation (30). Hill (1991) argues that violations in the assumption of changing mortality will tend to be smoothed out over time and that in general, violations in the constant mortality assumption will have little impact on estimates. Silva (2012), however, found that these violations can have substantial impacts on the accuracy of estimates, a subject which we will return to shortly.

IHME

In 2010, Rajaratnam and colleagues at the Institute for Health Metrics (IHME) developed a set of analytic methods to address some of the shortcomings of the Brass methodology. Specifically, their methods are intended to estimate under-5 mortality rates for periods closer in time than 3-6 years and to provide uncertainty intervals around estimates to improve the ability to detect statistically significant changes in mortality over time. The group developed four methods, the Maternal Age Cohort-Derived Method (MAC), Time since First Birth Cohort-Derived Method (TFBC), Maternal Age Period-Derived Method (MAP), and the Time since First Birth Period-Derived Method (TFBP) and created a summary measure from the four methods using a weighted combination of the methods.

The methods are considerably more complex than the Brass methodology, involving extensive smoothing of past trends and the incorporation of data from neighboring countries to fill in gaps (26). Data from neighboring countries is used in the development of multiplying factors, similar to those used in the Brass method. Some have questioned the appropriateness of these assumptions, as the multiplying factors rely exclusively on the models developed by the authors and have not been extensively tested and verified by other researchers (31). The introduction of multiplying factors that are reliant on the history of neighboring countries has the potential to introduce new biases; if the history of fertility and mortality decline between countries are not similar, as is likely to be the case if a country has undergone a humanitarian crisis, the multiplying factors may not be appropriate to use. The introduction of inappropriate multiplying factors may then lead to an under- or over-estimate of mortality. However, the authors do not provide any alternatives and the assumption must be made that across regions, countries undergo similar fertility and mortality declines.

Secondly, the IHME methods utilize statistical techniques to smooth fluctuations in data over time. This smoothing can lead to significant biases if there have been dramatic changes in mortality, such as those that are present in a complex emergency (26). Finally, the Maternal Age Period-Derived method is able to estimate child mortality within one year of the survey when using information from women age 15-17, within two years of the survey using data from women age 18-19, and within three years from women age 20-21. However, in many countries, age heaping, wherein women are more likely to report their ages in increments of 5 or 10, is a possible issue. If women age 18-19 differentially report their ages as 20, and these women's children also have a differential risk for mortality, then this will affect the accuracy of estimates that are closest in time to the survey, and the most useful to estimate under-5 mortality during a crisis.

Comparison and Consistency of Estimates

The majority of previous studies that focused on the consistency of direct and indirect estimation techniques, primarily the Brass method, utilized surveys from the World Fertility Surveys of the 1970s and 1980s and from early waves of the DHS program (18). These often reported differences between the direct and indirect estimates which were attributed to violations in the underlying assumptions of constantly or linearly declining mortality and fertility (18,32,33). Recently, Silva used 132 surveys in 49 countries, and after considerable adjustment for changes in fertility patterns, estimates derived from the Brass method were generally consistent with estimates derived from complete birth histories. Silva notes that those countries with the greatest inconsistencies between direct and indirect estimates were countries that had experienced either political or economic upheaval or that had a stalled health transition, such as Niger, where mortality began to fall in the 1960s and 1970s but has since stalled. Interestingly, in those countries where there is only a "short period of excess mortality", the findings are generally consistent between direct and indirect, although neither a "short" period nor the level of excess mortality is defined (pg. 8).

In order to validate their methods, IHME tested the estimates they derived using summary birth histories with the estimates from complete birth history data. They then compared the standard Brass method to the direct results and estimated the relative error of each method using 166 DHS from 69 countries. According to IHME, on average, their methods were 43.7% closer to the direct estimates than the Brass methods (95% CI, 41.2-45.2). When restricted to the five years prior to the survey, the time period in which the Brass method is weakest, the IHME methodology was closer to the direct estimate method than the Brass method by 53.5% (95% CI 51.3-55.2) (26). As with the Brass method, the largest errors occurred in countries that experienced "dramatic" mortality fluctuations, but they do not specify how large the errors are or discuss in which countries these errors occurred. Nor do they provide a discussion of the change in the level of mortality that is associated with a dramatic mortality fluctuation.

Before continuing to a discussion on the additional challenges of demographic estimation in complex humanitarian emergencies, it is briefly worth noting other indirect methods for estimating child mortality than the two discussed above. The first method is to include questions regarding births and survivorship within the previous 12 months. This method is both simple to ask and simple to calculate and can be used to calculate the Infant Mortality Rate (IMR) for the year prior to the survey. However, in order to extrapolate this data to an under-5 mortality rate, more information on the underlying age pattern of mortality and changes in patterns of infant and child mortality over the recent past must be available (22). Most importantly, this method has been shown to substantially underestimate child mortality (22).

A similar method that has been proposed is to ascertain the survival of a previous birth amongst women who are either about to or have just delivered a child. This methodology estimates mortality for a fairly recent time period, within two years, but the same limitations as above apply. No information regarding the age pattern of child mortality is obtained, nor are trends in under-5 mortality over time. Selection bias also plays a larger role in this method than in others, as this information is generally gathered only from women who are giving birth in a health facility. Children of these mothers are likely to be at lower risk in general for death than children of mothers who do not deliver in health facilities. In countries where a large proportion of women do not give birth in health facilities, this method then has the potential to greatly underestimate both infant and child mortality (22). When applied in household surveys rather than at health facilities, it was shown that the method performed worse than the Brass methodology (22).

Finally, the truncated birth history has been proposed. Similar to the complete birth history, dates of birth and age at death are obtained, but the time period for inclusion is generally truncated to some years prior to the survey, generally five. With this method, a better sense of the age pattern of early child mortality can be gathered than through using either Brass or IHME, although the age pattern of mortality can only be observed up to the truncation date and not further. While some surveys that used the truncated birth history found relatively little difference between its results and those of the complete birth history, others have found a tendency for child deaths to be heaped immediately prior to the deadline for truncation, leading to an undercount of child deaths and underestimate of child mortality (22).

Estimation in Complex Humanitarian Emergencies

Above, I described the most common methods to estimate under-5 mortality, including the advantages and disadvantages of each method, and the assumptions that underlie two of the most common analytic methods. Both methods are weakest in countries where there has been large-scale economic and/or political collapse, leading to excess mortality. Despite the weaknesses of each method in estimating mortality during or after an emergency, the unique conditions of these crises often dictate the use of such methods. In the event of large-scale population displacement and the collapse of civil infrastructure, vital registration systems are not likely to exist during humanitarian emergencies. Information on mortality and other demographic indicators is generally therefore collected by survey.

Challenges that may be present regardless of the emergency, such as difficulty in accessing certain populations and mistrust of survey organizations, may be exacerbated, while additional challenges such as danger to surveyors or other staff members may exist (34,35). Mortality risks will most likely vary across region and sub-group and some groups, which may have faced the most distress and highest mortality, may be deemed too difficult to survey. Surveyors may be restricted to a few areas of easier access or be limited to only refugee populations, which are likely generate biased estimates of mortality risks (36). Finally, with elevated mortality, the selection effect of mortality may be significant. For overall mortality estimation, at least one person in the family or an immediate relative must be alive at the time of the survey to be a respondent. In the event that the probabilities of survival of family members are correlated223, such as in genocides, this will introduce bias. If an entire family, parents and children, die during a complex emergency, the deaths of children will not be recorded. Under non-emergency circumstances, such clustering will likely have only a small effect, but when mortality is politically or ethnically motivated, such clustering has the potential for a larger effect (36). In the case of under-5 mortality, this potential for selection bias will impact estimates if children of mothers who die have a different risk of death than children whose mothers do not die.

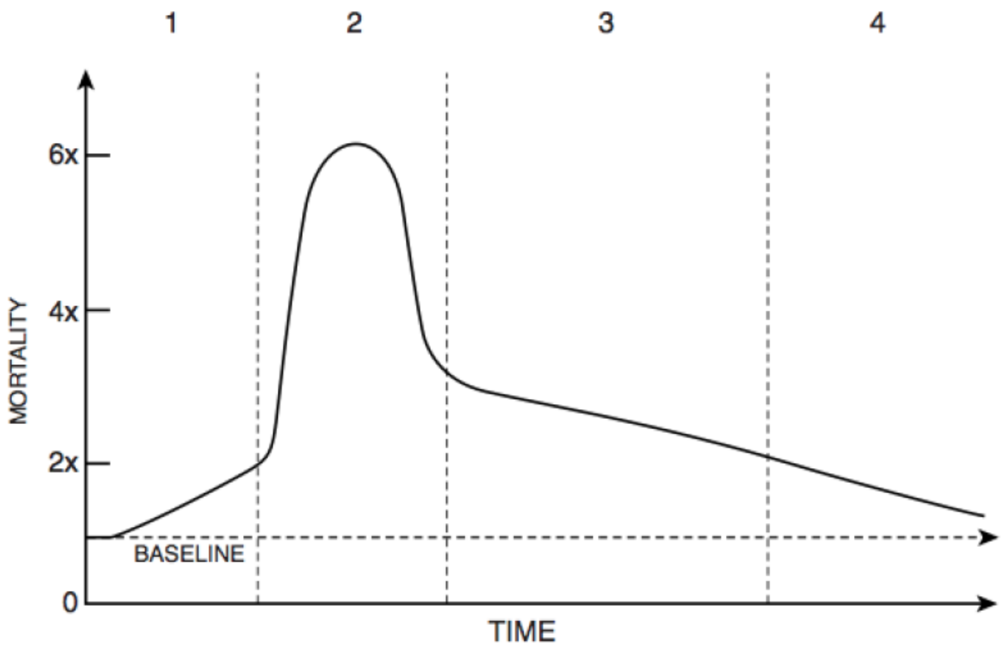
Despite these shortcomings, the sample survey is often the only method to gather information that can be utilized to generate timely estimates during or immediately after a crisis. It is critical therefore to gain a better understanding of the best way to measure mortality using sample surveys, through improving selection, survey design, and analysis. Some issues, such as the inherent risk of selection bias, cannot be easily addressed and must be handled based on the specific emergency and the understanding and knowledge of the investigators. However, what questions to include in a survey, and what analytic techniques are best suited for each situation can be explored further, as this dissertation will do.

Mortality in Complex Humanitarian Emergencies

We can better understand the unique challenges that complex emergencies present for under-5 mortality estimation with a discussion of how mortality patterns are affected by conflict or other emergencies. Before answering the question of which method is best suited to measuring under-5 mortality during complex humanitarian emergencies, we will therefore briefly discuss the pattern of mortality in complex emergencies in general and in four specific emergencies. Before doing so, a brief clarification on mortality rates in emergencies is necessary. During crises, mortality rates are often reported as the daily Crude Mortality Rates (CMR) and measured as the number of deaths per 10,000 people per day. This differs from the standard reporting of annual Crude Death Rates measured as the number of deaths per 1,000 people per year. To convert between the two, the CDR is multiplied by 36.5 to obtain the CMR.

First, there is no one pattern that applies to every humanitarian emergency. Each complex humanitarian emergency is different, motivated by different political, social and economic forces, that result in a unique situation. However, some generalizations may be made (4,37). Figure 1 below depicts how mortality rates typically change over time in an emergency situation (37).

Figure 1: Model of mortality change in a forced migration situation. Source: Reed et al, 1998, Figure 2



Phase 1	Beginning of the crisis
Phase 2	Height of the crisis
Phase 3	Arrival of humanitarian assistance
Phase 4	Stabilization of the crisis

During Phase 1, the beginning of the crisis, mortality rates may increase but remain only slightly above baseline. During Phase 2, when distress is greatest, mortality increases sharply and may begin to decrease again when many of the most vulnerable people in the population, generally the very young and very old, have died. Once humanitarian assistance arrives, in Phase 3, the mortality rate will begin to slowly decline and finally during Phase 4, may return to baseline and in some cases, may drop below baseline.

The levels of mortality, both at baseline and during the crisis, may fluctuate widely between different crises and within a crisis; however, the pattern of an inverted U can generally describe emergencies. Additionally, the length of time for each phase also varies, from one to two months in the case of the Rwandan genocide to the much longer famines experienced in Somalia and North Korea. For displaced persons, the time surrounding migration and arrival are often the period of highest mortality. For example, in 1992, Mozambican refugees who were in Chambuta camp, Zimbabwe for less than one month had a CMR of 8 per 10,000 per day, four times higher than those who had been in the camp for one to three months and 16 times higher than baseline (4). However, over time, as mortality rates stabilize, refugees or internally displaced persons living in camps may have lower mortality than the stable, non-migrating population. Comparing male and female age-specific mortality rates from Cambodian refugee camps in Thailand that had been extant for ten years to the non-displaced population mortality estimates, Keely showed that for both sexes and almost all ages, mortality risk was lower amongst refugees than for the corresponding non-displaced population (4). The only age group that did not see improvements in mortality was infants; for male infants, the relative risk of dying was 80% greater amongst refugees than males in the stable population while for females, the relative risk of death was 20% greater. Reductions in mortality are not likely to be seen in much of a population affected by a humanitarian emergency, but, for those in camps with increased access to humanitarian assistance such as health services, food, and improved water and sanitation facilities, over time mortality may decline to levels below the levels of the population of origin. However, it should be emphasized that these declines may take years, or even decades to achieve.

While there may not be consistency in the levels of mortality or the duration of time during which mortality is raised across complex humanitarian emergencies, what is consistent is disruption. Mortality may rise sharply and decline quickly, it may slowly rise and fall, it may fluctuate over time, but it is unlikely to remain constant. These fluctuations are what motivate this dissertation, as we aim to test how disruptions in the decline of mortality affect two different methods to estimate under-5 mortality.

To do this, I have simulated four different populations that are based loosely on the patterns of mortality seen in four humanitarian emergencies that took place in the latter half of the twentieth century: the Rwandan Genocide of 1994, the Cambodian Genocide of 1975-1979, the North Korean Famine of 1994-1998, and the ongoing conflict in Afghanistan. While the simulations do not replicate either the exact histories of the complex emergencies or the overall level of mortality, each of the complex emergencies displayed a unique mortality pattern that motivates the simulation. I will briefly summarize a simplified history of each conflict and the mortality patterns that were seen before introducing the specifications of the simulations in the methodology chapter below.

Four Humanitarian Emergencies

Rwanda

The Rwandan Genocide was the worst genocide in modern times. Over the course of six weeks, beginning April 6, 1994, 500,000 to 1 million people were killed in an explosion of ethnic violence between the majority Hutu and minority Tutsi. Following years of political and ethnic tension and the assassination of the Hutu president, the country erupted into widespread violence, with Hutu citizens being encouraged by the government to eliminate all Tutsi citizens. Hutus that were moderate or sympathetic to the Tutsis were also killed. Over a three-month period approximately 20% of the population, primarily Tutsis, died (14). Mass migration also took place with as many as 1.75 million people fleeing either genocide, retribution killings, or punishment (38). The majority of refugees fled into Tanzania, Burundi, and Zaire (now Democratic Republic of Congo).

Within five days, July 14 to 18, 1994, between 500,000 and 850,000 people arrived in Goma, Zaire, bordering Rwanda. Initially, mortality rates were extremely high following back-to-back cholera and dysentery outbreaks, in addition to sub-standard environmental conditions (14). Legros and colleagues estimated that between 6 to 10 percent of refugees who arrived in Goma between July 14 and July 18 died within a month of their arrival (14). Daily crude mortality rates were estimated to be as high as 41.3 deaths per 10,000 people per day. In comparison, daily CMRs amongst Somali refugees in Kenya were 7.3 deaths per 10,000 people per day in 1992 and daily CMRs amongst Iraqi refugees in Turkey were estimated at

4.6 (4,39), underscoring the extremely high mortality that was witnessed during this time. These are amongst the highest CMRs estimated in complex humanitarian emergencies outside of Rwanda. However, these extremely elevated daily CMRs lasted for less than a month. By the end of August, CMRs were estimated at 3.0 deaths per 10,000 people per day (14).

Estimates for the total number dead from the genocide range between 500,000 and 1,000,000 people, a difference of 500,000 people or 10% of the country. Much of the discrepancy is explained by whether or not deaths attributable to the genocide include only violent deaths or if they also include deaths due to forced migration and resettlement, such as those due to cholera and dysentery. The exact number of deaths will never be known, but the pattern of mortality is well documented. In the case of the Rwandan genocide, the mortality pattern was one of extremely elevated mortality across all ages and sexes that occurred within a very short time period (between six to ten weeks) and then dropped precipitously.

Cambodia

The mass killings that took place in Cambodia during the 1970s were similar in scope to Rwanda but took place over a much more prolonged timeline. The decade began with the Communist Part of Kampuchea's (CPK) armed opposition to prince Norodom Sihanouk. After Sihanouk's ousting and replacement by his Prime Minister, Lon Nol, Sihanouk allied with North Vietnam and the CPK to challenge the government and win back power. In response, the government targeted the half million Vietnamese living in Cambodia, killing thousands and prompting the mass exodus of 300,000 Vietnamese in 1970 (36). Over the next four years, the Cambodian government continued to wage war with Sihanouk and the CPK. Between 1970 and 1975, it is estimated that 300,000 people died as a result of the civil war, although Sihanouk claimed that 700,000 Cambodians were killed under the Lon Nol government (36,40). The CPK, now referred to as the Democratic Kampuchea (DK) and under the leadership of Pol Pot, took the capitol Phnom Penh in April 1975, beginning the Khmer Rouge era.

The first act of the DK was to relocate approximately two million urban refugees from Phnom Penh into rural areas. The relocation, in combination with food shortages, malaria, and exhaustion led to a dramatic mortality increase among the most vulnerable, young children and the elderly, culminating in a famine in 1979 (36). At the same time, political and ethnically motivated killings became the norm. According to Heuveline, "executions could punish any minor violations of the Khmer Rouges' orders, as well as internal dissension within the party ranks". Although there has been disagreement about the extent of mortality, studies have estimated between 1.3 and 2 million excess deaths between 1975 and 1979, or 21 to 24 percent of the population dying as a direct result of the Khmer Rouge (36,41,42). As many as 1.1 million deaths were due to violence and executions, more than half of all deaths during this time period (41). Famine, too, played a major role in mortality, accounting for about 35% of all reported deaths (43).

The different causes of death had specific age patterns of mortality. Famine and "natural" causes of death had traditional age patterns, with the highest rates occurring amongst the very young and very old. Under-5 deaths due to famine were three times more common than deaths due to violence or natural causes between 1975-1979 (43). Conversely, deaths due to violence demonstrate an age pattern that is the inverse of the traditional J-shaped curve, with a peak in mortality during young adulthood. Male violent mortality exceeded female violent mortality in young adulthood, peaking amongst those in their mid-20s (36). By older ages however, female and male deaths due to violence were equivalent. Though deaths due to violence among children under five were lower than for other ages, Heuveline estimated that males between the ages of 0-4 had a greater than 20% chance of dying due to violence between 1970 and 1979 while females had approximately a 15% chance of dying due to violence (36). Neuport and Prum estimated that 40% of the excess deaths that took place during the Khmer Rouge regime were to children below age 15 (42). The combination of famine and violence resulted in a cohort of children born during the 1970's whose mortality between ages one and five made up a substantial component of under-5 mortality. For children born after the 1970s, most of the mortality was concentrated during the first year of life, which is expected under normal mortality scenarios (11).

The overall pattern of mortality seen in Cambodia during 1975 to 1979 was thus one of elevated mortality over an extended period of time. Additionally, due to the age pattern of mortality associated with both famine and violence, there was a flattening in the J-shaped curve as children older than one and young adults experienced much higher mortality than in normal circumstances.

North Korea

In July 1994, the leader of North Korea, Kim Il Sung, died, passing power of the impoverished nation to his son, Kim Jong Il. Beginning in 1995, the country, already economically unstable since the break-up of the Soviet Union and experiencing food shortfalls since 1991, was hit by a series of natural disasters. In the summer of 1995, severe flooding damaged 400,000 hectares of land, displaced 500,000 people and led to a 30% loss in the 6.5 million tons of grain needed to feed the nation (45). At the same time, the Chinese cut back food shipments, further reducing food availability. In the following year, floods again hit the country, affecting the primary farming regions and further reducing the grain output. In the summer of 1997, North Korea was hit not by floods, but by drought. By late July, water levels were 10 to 20 percent below normal and the regions most severely affected by drought were those that had been affected by floods, resulting in a third straight year of crop losses (45). As a result of three successive years of severe food shortages, the government decreed food rations declined from a pre-crisis level of 700g per person per day to about 100g by 1997 (46). Despite the presence of the World Food Programme, there was evidence of mass famine beginning in 1995 and ending in 1998, but due to North Korea's insulation, it was impossible to know the extent of the famine.

North Korea is one of the most politically, economically, and socially isolated countries in the world. As a result, much of its demographic profile, including the magnitude and impact of the famine that occurred in the mid-1990s remains a mystery. Widely divergent numbers have been suggested, from a low of 220,000 deaths to a high of 3.5 million (45). The government of North Korea released estimates in March 1999 of a CDR in 1995 of 6.5 deaths per 1,000 people and in 1998 of 9.3 deaths per 1,000 people. Goodkind projected that if these rates were correct, than approximately 236,000 famine related deaths occurred in the four-year period. However, these CDRs are almost certainly too low (45–47). Robinson and colleagues estimated a CDR that rose from 22.1 deaths per 1,000 people in 1995 to 51.8 in 1996 before declining to 27 per 1,000 in 1998 (46). In addition, they reported that the excess mortality, though higher amongst the very young and very old, was also elevated for older children and adults; nearly two-thirds of the deaths occurred to people between the ages of 20 and 59 (47). These rates would result in approximately 2.6 million famine related deaths, which would constitute over 10% of the 1993 population (45). In contrast, Goodkind estimated that there were between 600,000 and 1 million famine related deaths in North Korea in the four-year period.

The true extent of mortality that occurred due to the famine is not likely to be revealed without North Korea's cooperation. While the exact levels of mortality are speculative, the mortality pattern is well understood, with a slow increase in mortality from 1991 to 1994 and then a sharper increase from 1995 to 1998. The mortality levels were much higher in these years relative to a non-famine year, but they do not approach the levels of mortality in Rwanda or Cambodia. Consistent with these emergencies, excess mortality was highest amongst the very young and very old, but older children and younger adults also faced an elevated risk of mortality.

Afghanistan

Beginning with the Soviet invasion in 1979, Afghanistan has been almost continually beset by conflict. From December 1979 to February 1989, US-backed Mujahideen engaged in fighting with the Soviet occupation, resulting in as many as 1.8 million deaths and 7.5 million refugees (48). Following the end of the Soviet occupation, Mujahideen groups and local militias began waging war with each other to fill the power vacuum that formed with the ousting of the communist regime. The Taliban secured control of Kabul city in 1996, after intense shelling and rocket attacks, instituting strict sharia laws and dismantling health and education systems (49). The Taliban was toppled in 2001, by the US-led coalition, however, fighting continued with escalating international involvement. As a result of almost continuous fighting, the destruction of infrastructure, and the restrictive policies of the Taliban, Afghanistan had some of the worst health indicators in the world at the turn of the century, including the highest maternal mortality ratio in the world, and amongst the highest infant and under-5 mortality rates (50).

The Karzai Administration, which came to power in 2001, has focused on reconstruction of the health system with assistance from the US, Canada, the European Union, and India (49). Reconstruction efforts have led to an improvement in many regions of the country and reductions in maternal, infant, and child mortality (49). However, progress has been slow and in those areas that remain under Taliban control, it is impossible to know the health status of the population with any certainty. Most surveys do not cover areas that are very unstable, leaving demographers to guess at mortality levels based on areas where conflict is lower and health systems are more stable or from refugees from the area (49,51), both of which are likely to lead to biased estimates.

The mortality pattern in Afghanistan during the past thirty years cannot be as easily estimated or visualized as those in the previous examples. Given the dearth of reliable statistics, the variability in duration, intensity, and location of conflict, and the geographic and social heterogeneity of the population, it is impossible to know what the true underlying pattern of mortality is at the national level. However, given the cyclical nature of the complex emergencies, mortality has likely fluctuated, rising during the Soviet invasion and declining once the invasion had ended, rising again with ongoing conflicts between mujahedeen and Taliban, declining as reconstruction efforts took hold, and potentially increasing again as security has continued to decline in recent years (49,50,52,53).

Table 1 below shows a selection of excess mortality estimates that have been proposed by different organizations or authors for the four humanitarian emergencies described above. Within each emergency, there is wide enough variability to render the true extent of mortality unknown.

Table 1: Estimates of excess deaths for four humanitarian emergencies

Country	Agency/Author	Estimates
Rwanda	Human Rights Watch (54)	500,000
	Hansch, 2001	750,000
	Official Rwandan government estimate (55)	1,000,000 +
Cambodia	Vickery, 1984	740,000
	Kiernan, 1996	1,500,000
	Heuveline, 1998	1,170,000 – 3,420,000
North Korea	Official North Korean government estimate (56)	220,000
	Robinson, 2001	450,000
	Good Friends Center for Peace, Human Rights, and Refugees (56)	3,500,000
Afghanistan	Hansch, 2001	200,000 – 2,000,000

Each of the humanitarian emergencies described had a unique political, economic, and cultural history that impacted the intensity and duration of the particular emergency, resulting in a distinct pattern of mortality. Sadly, there are many other examples of humanitarian emergencies that could be modeled and extensive demographic work that has been done to estimate the impact of these emergencies on human life. I chose these emergencies because each represents a distinct mortality pattern and there is some understanding, limited though it may be, of the extent of mortality and the associated age patterns, with the possible exception of

Afghanistan. Afghanistan, however, has been in a state of tumult for over thirty years. There is great interest and debate regarding the quality of demographic estimates that have arisen in recent years (53,57) and a better understanding of the effect that lingering conflict has on estimation merits an examination of such mortality patterns.

Demography and simulation

If it is possible to obtain mortality estimates in emergencies, why simulate them? Precluding the examination of the effect that disruptions in mortality patterns have on indirect estimation techniques is the fact that there are no publicly available surveys conducted during or immediately after a humanitarian emergency that include both a complete and summary birth history. While several DHS surveys have been conducted in countries where there is regional civil conflict, during the height of conflict these regions are generally avoided for security considerations (e.g. Northern Uganda in 2000, southern Afghanistan in 2010, and Balochistan, Pakistan in 2012). Even if a dataset did exist, the mortality pattern of that emergency would follow a distinct pattern and would not allow for exploration of what effect different mortality patterns have on estimation techniques. Finally, any surveys conducted during times of emergency may be host to a range of potential selection and information biases, as has been discussed.

Simulation solves these problems by generating complete data that is free of any other potential bias. Generating multiple simulations based on the same parameters allows for estimating variation across the Brass indirect estimates, which is not otherwise possible. By simulating datasets that mimic mortality fluctuations, we can isolate the effect that a disruption in the mortality pattern will have on estimation, rather than biases that may be due to selection bias or incomplete data, and can estimate how closely indirect estimates model the "true" mortality estimated using a complete birth history.

Simulations have been used extensively in demography and related fields, such as epidemiology and economics, in order to model what effect changes in individual, population, and social factors have on outcomes like disease progression, population structure, and economic growth. Broadly, there are two kinds of simulations, macrosimulations and microsimulations, with additional variants of each. In macrosimulation, the unit of analysis is the population as a whole, or aggregated groups within those populations such as males and females, rather than the individual (58). In macrosimulation, the question of interest is generally how changes at the population level, such as declining fertility rates or increases in contraceptive prevalence, affect other population level factors, such as population growth rates or GDP. Population projection models such as SPECTRUM are examples of macrosimulations.

In contrast, microsimulations are concerned with the trajectories of simulated individuals. Life histories for individuals within a population are simulated using mathematical algorithms. The researcher inputs probabilities for events of interest (for example, marriage and childbirth) and random draws within the program determine whether and when a specific event will occur. Microsimulation thus produces individual level data that can be analyzed using standard statistical techniques or aggregated to the population level for examination of population-level outcomes (58). Ultimately, the purpose of this dissertation is to model the population-level estimates of under-5 mortality, but to do so, it is necessary to aggregate individual data. Microsimulation is therefore the method of choice.

Microsimulation has been used in the past to model demographic processes and test methodology. Garenne used simulation to compare estimates of maternal mortality generated from indirect methodology to those generated from direct methods (59). He found that the indirect methods had much greater variability across simulations than did the direct methods and called into question their use as an estimation technique for maternal mortality. Fernando used simulated data to test the Feeney method of infant mortality estimation, quantifying the impact of biases that resulted due to differences in the age pattern of mortality (60). Dwyer-Lindgren and colleagues used simulation to compare indirect estimates of child mortality to direct estimates using successively smaller sample sizes. In addition, they explored how the error and bias varied by the underlying true level of mortality in the population (61). In recent years, there has not been extensive use of simulation testing in the field of indirect under-5 mortality estimation because of the relatively large number of surveys that include complete birth histories. However, the ability to quickly generate multiple datasets that are otherwise free of error allows for the unique opportunity to isolate specific biases of interest and test its overall effect on estimation.

Chapter 3: Methods

In order to compare the results of the Brass and IHME methods of under-5 mortality estimation to the results of the direct estimates across multiple scenarios, it was first necessary to generate the populations themselves. To create the populations, I used a simulation program called Socsim, which utilizes birth, death, and marriage probabilities to generate individual life histories of thousands of simulated individuals. I will describe in brief the simulation program before discussing in more detail the demographic rates that were used. Finally, I will discuss each of the methodologies and how they were applied to these simulated populations.

Simulation Models

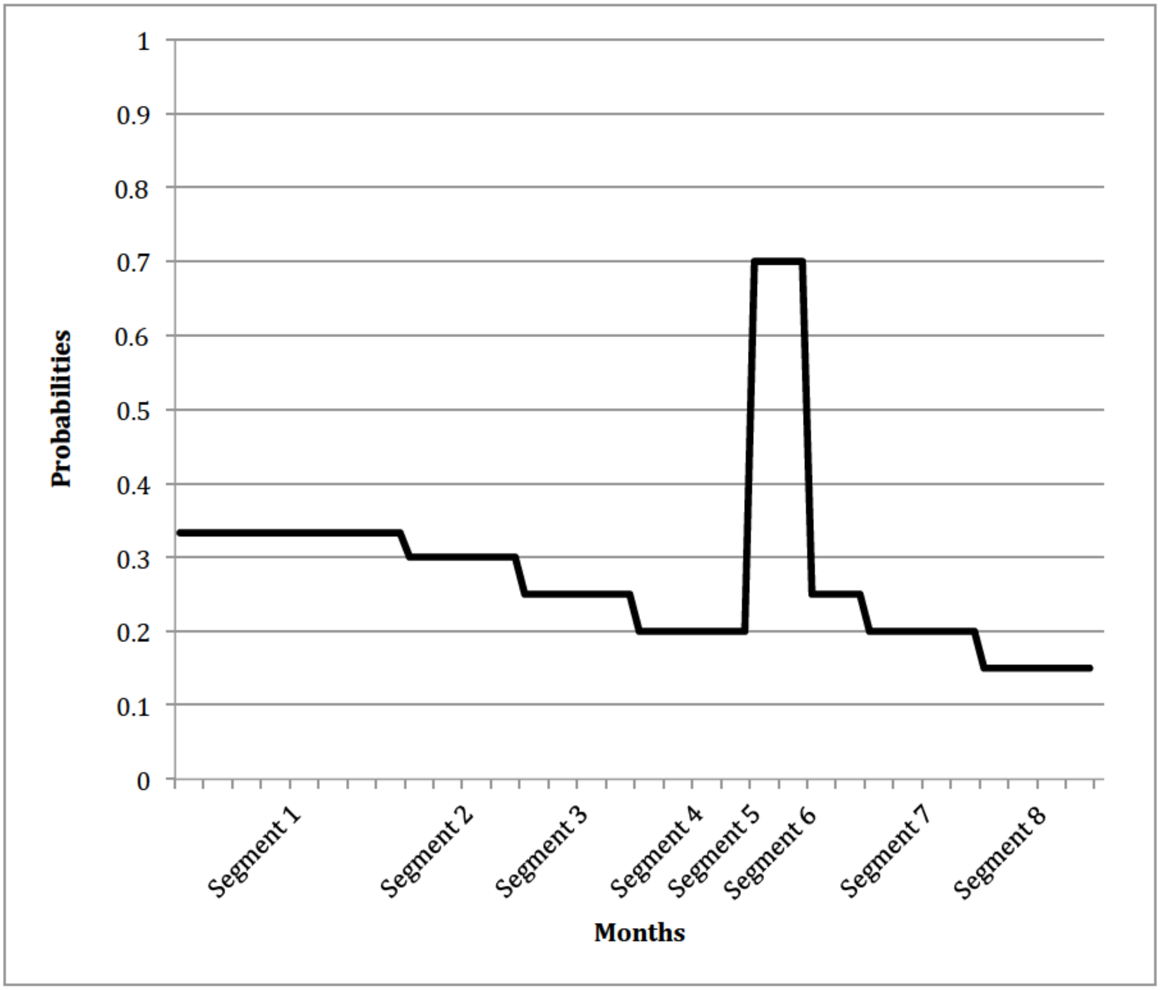
Five separate scenarios were simulated, a baseline simulation of constantly declining mortality and fertility and four scenarios with fluctuations in mortality, each with a separate mortality pattern. Each scenario, with its unique mortality parameters, was run 100 times, generating a total of 500 simulated populations.

The simulations were built using the microsimulation program Socsim, developed by University of California, Berkeley (62). Socsim uses population-level mortality, fertility, and marriage probabilities to generate life histories of individuals. These demographic parameters, inputted by the researcher, are applied over a simulation segment, a period of simulated time defined by the use of the single set of parameters (63). Within each simulation segment, Socsim schedules an event (marriage, birth, or death) for each simulated individual "alive" during the segment. To schedule the event, a random wait time for each event that a person is at risk of having is generated, based on the age-specific probabilities of birth, marriage, and death. Socsim creates a list of all events scheduled for the simulation segment. When the list of scheduled events is completed, Socsim begins executing each event in one-month intervals, drawing a random selection from within the list to begin executing the events. Once one event is executed, Socsim will schedule the next event for the person, in addition to any new events that would arise as a result (for example, a birth to a woman would give rise to a new schedule of events for the person born). When all events for a month have been executed, the simulation will continue into the next month. While the probabilities that are inputted as parameters govern the overall likelihood that an event will happen for any one individual person, the random waiting times and event executions means that each simulation will have a unique distribution of the number of events in time. This distribution allows us to model variation across simulations.

As Socsim operates by scheduling events in one-month intervals, all vital events (births, deaths, and marriage) are inputted as monthly probabilities. For these simulations, mortality, fertility, and marriage probabilities were applied for 12-month durations, meaning each simulation segment is one year. There are two exceptions to the one-year simulation segment; 1) the first simulation segment, which is run of 4200 months to generate a stable population and 2) when the parameters that define the humanitarian emergencies are applied. Each humanitarian emergency scenario had a unique set of monthly mortality probabilities that define the crisis period, applied over a differing amount of time, discussed in more detail below.

A simplified graphic is supplied below (Figure 2) demonstrating the relationship of months and simulation segments. In Segment 1, which runs for approximately 24 months, one set of probabilities is supplied. Once a new set of probabilities is introduced, a new segment is started, each segment running for approximately 12 months. The exception to this is the crisis period of each humanitarian emergency, where the simulation segments are based on the length of the emergency. In the graphic below, the crisis probabilities are applied for six months in Segment 5, before declining in Segment 6. In the simulations I have run, Segment 1 runs for 4200 months instead of 24 months to create a stable population.

Figure 2: Simplified graphic of Socsim progression constructing simulation segments over time



In total, five scenarios were simulated 100 times each. The same fertility and marriage probabilities were used in each scenario (discussed further below and supplied in Appendix I and II, but the mortality probabilities differed widely across each scenario An explanation of how the probabilities of mortality, fertility, and marriage were derived for each scenario follows below.

Mortality

Each simulation begins with the same baseline level of mortality applied for 350 years (4200 months) and maintains the same rate of decline over a 40-year period, with age-specific probabilities of death taken from Coale-Demeny West model life tables, with different increases in mortality programmed in each humanitarian emergency (64). Before discussing what the exact levels of mortality are in each scenario, I will first explain how the monthly probabilities were derived from the annualized probabilities given in the West model life tables.

In the model life tables, for the first year of life, probabilities of death are given separately, reflecting the much higher risk of dying within the first year of life relative to later years. Probabilities of death are then given in five-year increments (with the exception of the four year probability of dying between age one and exact age five among those who survive to age one.) Five-year probabilities for death were first transformed into rates using

Equation 1

$$r = [-\ln(1-q)]/t$$

where r is the mortality rate, q is the probability of dying in the one, four, or five year interval, and t is the time interval (65).

The rates were then transformed into monthly probabilities using the equation:

Equation 2

$$q = 1 - e^{(-rt)}$$

in which t is the inverse of the number of months in the interval (65).

Baseline

Sex-specific mortality probabilities were extracted from life tables with life expectancies ranging from 40 years to 55 years. Life expectancy increased by one-quarter of a year per each 12 month simulation segment, reflecting the global rate of change in life expectancy estimated by Oeppen and Vaupel (2). While the levels of life expectancy may seem low, they simulate a society of a similar mortality profile to the four countries previously described (39,42,46,49). Although there is considerable variation within and across country-level estimates of life expectancy, estimates for Rwanda, Cambodia, Korea, and Afghanistan, indicate that each of the countries had relatively high mortality at the time of their emergencies (Table 2). Notably, North Korea had higher life expectancy than the other countries prior to its famine, although the reliability of these estimates is questionable given the dearth of information regarding North Korea (46).

Table 2: Life expectancy estimates in selected countries and year

Country	Agency/Author	Year	Female	Male
Rwanda	United Nations World Population Prospects 2012	1990-1995	24.8	21.4
	United Nations World Population Prospects 2012	1985-1990	47.5	44.0
	Keely, 2001	1992	45.5	45.5
Cambodia	United Nations World Population Prospects 2012	1960-1965	43.2	48.4
	Keely, 2001	1990	49.5	49.5
	Heuveline, 1998	1960	53	51
North Korea	United Nations World Population Prospects 2012	1995-2000	67.4	59.3
	United Nations World Population Prospects 2012	1990-1995	62.5	65.8
	Robinson, 2001	1991	73	66
Afghanistan	United Nations World Population Prospects 2012	2000-2005	57.0	54.7

Humanitarian Emergencies

All of the humanitarian emergency probabilities were applied after 423 years of simulations, when life expectancy reached 46. Following each emergency period, mortality probabilities corresponding to a life expectancy of 45, slightly below baseline, were applied for one year to simulate a recovery period and then mortality and fertility dropped at the same pace as in the original baseline simulation.

Scenario 1

For the first scenario, loosely based on the Rwandan genocide, ratios were obtained using the age- and sex-specific mortality rates estimated during July 17-August 5, 1994 in Katale Camp, Zaire (16). These rates correspond to a time some weeks after the height of the genocide and thus do not include the spike that is associated with violent deaths; however, the death rates are extremely high and drop suddenly, following the general pattern of mortality seen in Rwanda.

Reflecting the difficulty in obtaining age- and sex-specific estimates in emergency settings, the mortality rates are non-sex specific and have little age gradation. The rates in Table 3 were transformed into annualized mortality rates and then compared to the sex-specific mortality rates corresponding to a life expectancy of 45.5, the estimated life expectancy in Rwanda prior to the genocide given by Keely and colleagues (4).

Table 3: Age-specific death rates of Rwandan refugees in Katale Camp, Zaire, 1994 (per 10,000 per day). Source: Davis, 1996.

Age Group	*ASDR*
<5	7.8
5-15	2.9
15-45	3.4
>45	17.7

The ratios of the emergency to pre-emergency monthly mortality probabilities derived from the rates given in Table 3 were then applied to the sex-specific monthly mortality probabilities extracted from the Coale-Demeny model life table for a life expectancy of 46. The age and sex-specific ratios are shown in Table 6 and

Table 7 (below). The elevated probabilities were applied for two months, generating what Humanitarian Emergency 1 (HE 1). Given that the ratios are extremely elevated, they were compared to those estimated by Keely and colleagues for validation (4). While they are not exactly the same for each age group, the level is similar across all ages.

Scenario 2

The second scenario is motivated by the Cambodian genocide, which had lower mortality rates than Rwanda, but the time in which elevated mortality persisted was much longer.

Table 4: Age-specific death rates Cambodia (per 1, 000 per year). Source: Slewinski, 1995.

Age group	*ASDR*
0-10	23.3
10-20	22.0
20-30	33.1
30-40	36.1
40-50	36.5
50-60	42.0
60+	40.0

Several studies have estimated the total number of deaths due to the Khmer Rouge and attempted to decompose many of these deaths into deaths due to violence, famine, or other causes, but few of the papers provided any age-specific death rates.

Only Slewinski in 1995 estimated age-specific mortality rates (as cited in 40). These rates were transformed into probabilities and compared to the sex-specific mortality probabilities corresponding to female life expectancy of 53 and male life expectancy of 51, which were estimated by Heuveline as the approximate life expectancy of the population in 1962, prior to the Khmer Rouge (42). The ratios of emergency to pre-emergency mortality probabilities, substantially lower than those in Rwanda and shown in Table 6 and Table 7 below, were then applied to the monthly probabilities for five years, to generate the simulation we will call Humanitarian Emergency 2 (HE 2).

Scenario 3

Table 5: Emergency age-specific death rates; North Korea (per 1,000 per year). Source: Robinson, 1999.

Age Interval	*Male ASDRs*	*Female ASDRs*
0-4	23.5	37.3
5-9	24.2	11.9
10-14	12.5	8.3
15-19	4.8	1.9
20-24	3.7	4.4
25-29	8.7	4.4
30-34	24.9	11.9
35-39	34.9	18.4
40-44	32.9	24.7
45-49	49.6	25.6
50-54	66.7	32.8
55-59	82.3	42.0
60-64	134.2	73.9
65+	191.6	128.2

Ratios for Humanitarian Emergency 3 (HE 3) were taken from Robinson and colleagues work (2001). The author calculated age and sex-specific baseline and emergency mortality rates. Baseline was defined as the mortality present in 1993 and the emergency mortality rates are an average of the three-year mortality rates estimated for 1995-1998. The rates of each were transformed into probabilities and the ratios applied.

The ratios are somewhat higher than the Cambodia ratios, particularly in the age groups 5-14 and 30-44, but are still lower than the ratios estimated for the Rwandan genocide. The exception to this is the ratio for age 0-1, which is higher than the estimated ratio from HE 1. The HE 3 ratios are applied over a course of three years to replicate a long-term emergency with high mortality. It should be noted that the ratios that are derived are quite high, but they are also derived from a population that had initially lower mortality than the other three scenarios.

Scenario 4

Of all of the humanitarian emergencies described, the emergency that has unfolded in Afghanistan over the past forty years is the most difficult to model. Due to the extreme difficulty of conducting surveys in Afghanistan, there are very few, if any, reliable mortality estimates and none that model mortality over time. Thus, Humanitarian Emergency 4 (HE 4), though it is built to model intermittent violence, is not based on data from Afghanistan. Rather, HE 4 uses the same ratio of emergency to baseline mortality derived for the HE 2 model, applied at five-year intervals over the course of twenty years. The data from this scenario was chosen because it demonstrates elevated mortality with the highest proportional increases occurring in young adulthood, those ages most affected by violence. This does not, nor is it meant to, realistically model the experience of Afghanistan. It does, however, model a pattern of mortality that fluctuates over time, establishing a non-constant mortality rate, simulating the effect of long-term mortality disruption.

Table 6 and Table 7 show the sex-specific ratios of the probability of dying in an emergency mortality setting compared to baseline mortality in each of the four scenarios described. At older ages, the mortality ratio was often less than 1 because estimates from these emergencies tended to aggregate all mortality over 60 into one rate, masking the difference in mortality between age groups such as 60-65 and 90+. As it is unlikely that mortality at older ages would improve during a complex emergency, whenever the ratio dropped below 1, it was replaced with 1.0.

Table 6: Ratio of emergency to pre-emergency mortality probabilities in four humanitarian emergencies (Females)

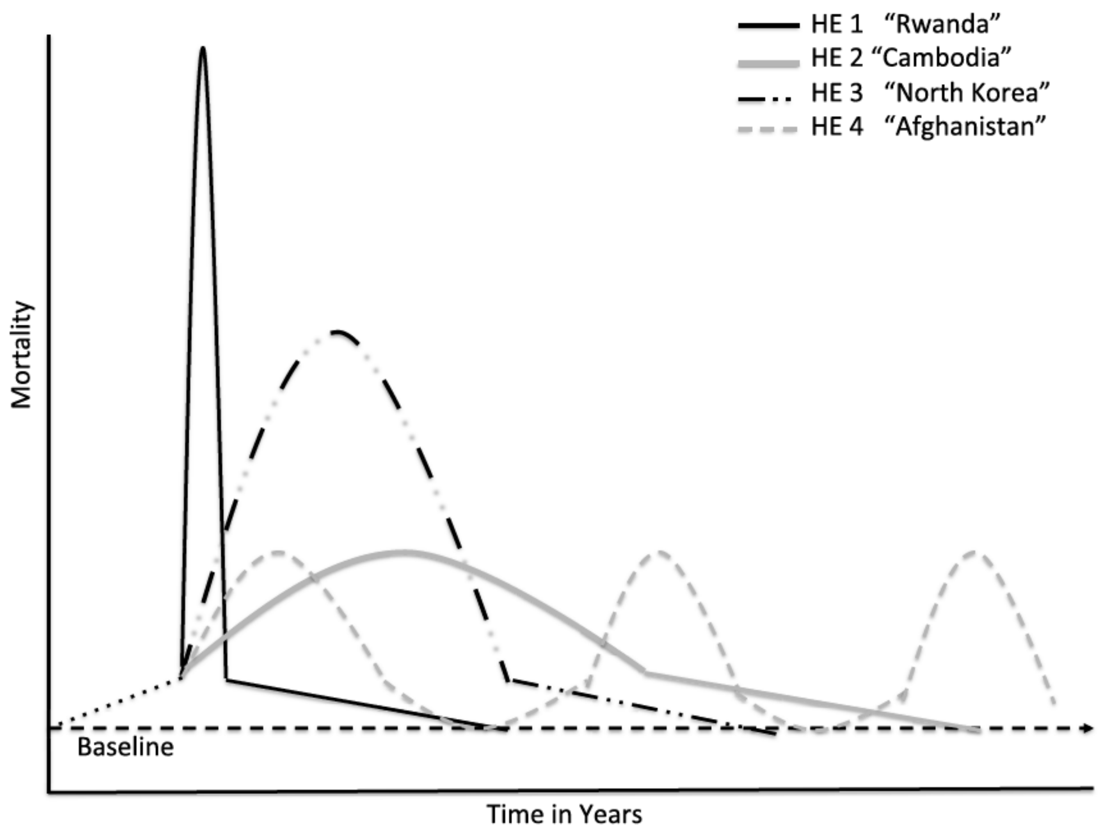
	Emergency and Duration			
	HE 1	***HE 2***	***HE 3***	***HE 4***
Age	Two months	Five Years	Three Years	Intermittent
0-1	4.75	1.54	7.3	1.54
1-4	30.55	1.54	7.3	1.54
5-9	123.64	6.46	23.7	6.46
10-14	157.66	7.87	41.4	7.87
15-19	135.96	5.52	4.8	5.52
20-24	107.06	6.38	7.4	6.38
25-29	95.31	5.64	6.2	5.64
30-34	83.89	5.42	17.0	5.42
35-39	75.22	4.82	26.2	4.82
40-44	67.67	4.94	22.4	4.94
45-49	156.20	4.17	16.0	4.17
50-54	117.4	2.97	12.6	2.97
55-59	88.22	2.18	8.7	2.18
60-64	58.87	1.12	7.3	1.12
65-69	40.94	1.00	2.9	1.00
70-74	26.52	1.00	1.00	1.00
75-79	17.27	1.00	1.00	1.00
80-84	11.34	1.00	1.00	1.00
85-89	7.79	1.00	1.00	1.00
90+	1.00	1.00	1.00	1.00

Table 7: Ratio of emergency to pre-emergency mortality probabilities in four humanitarian emergencies (Males)

	Emergency and Duration			
	HE 1	***HE 2***	***HE 3***	***HE 4***
Duration	Two months	Five Years	Three years	Intermittent
0-1	4.56	1.65	4.2	1.65
1-4	35.98	1.65	4.2	1.65
5-9	145.89	6.88	34.6	6.88
10-14	200.47	8.86	31.3	8.86
15-19	162.72	5.93	7.9	5.93
20-24	113.55	6.23	4.6	6.23
25-29	103.73	5.74	7.9	5.74
30-34	90.62	5.49	19.2	5.49
35-39	75.55	4.54	23.3	4.54
40-44	60.71	4.17	12.2	4.17
45-49	129.48	3.27	12.4	3.27
50-54	96.75	2.30	9.8	2.30
55-59	72.41	1.67	5.7	1.67
60-64	50.43	1.00	4.3	1.00
65-69	35.33	1.00	2.5	1.00
70-74	23.91	1.00	1.00	1.00
75-79	15.73	1.00	1.00	1.00
80-84	10.54	1.00	1.00	1.00
85-89	7.15	1.00	1.00	1.00
90+	1.00	1.00	1.00	1.00

Figure 3 graphically depicts the mortality patterns that are modeled by each simulation. The figure is not drawn to the scale of the ratios or to the exact timeline but is meant to provide a visual representation of what each scenario's mortality pattern might look like.

Figure 3: Mortality patterns of four simulated humanitarian emergencies



While the mortality patterns change in each scenario, fertility and marriage probabilities are unchanged across the five scenarios. There is conflicting evidence regarding the impact of humanitarian emergencies on fertility and very little research has been done regarding its impact on marriage. Hill and colleagues found that, with the exception of famine, there is limited evidence that emergencies and displacement affect fertility and even in famine, fertility rates are not affected substantially in the long-term (66). A review of the literature on the effect of war and conflict on reproductive health found that fertility increased in some populations affected by war and decreased in others (67). Very little work has been done to estimate the impact of conflict on nuptiality rates (68,69). Given the dearth of information on fertility and nuptiality patterns in times of conflict, and in order to isolate the effect of changes only in mortality patterns, fertility and marital probabilities were kept consistent across all simulations.

Fertility

In Socsim, fertility is the only event that is programmed using monthly rates instead of monthly probabilities. Annual age-specific fertility rates were derived using model fertility schedules developed by Schmertmann (70) and transformed into monthly rates. The age-specific rates were derived from graphical parameters based on the age of first birth, peak age of childbearing, and overall TFR.

The initial population was simulated to have a total fertility rate of 6.85, with an age of first birth at 12 and a peak of childbearing at 23. After the 350 years of initial simulation, fertility rates declined gradually until reaching a low of 5.6 at the end of the simulation. After the initial 350 years, the age of first birth increases over time to 14 and peak childbearing increases to age 24. With the exception of North Korea prior to the 1995 famine, all countries had fertility of approximately this level prior to their emergencies (Table 8) (3). This level of fertility, though high, is therefore realistic for the situation. The exception to this is the case of North Korea, which has lower estimated fertility than the other countries in the pre-emergency era. However, these estimates are difficult to corroborate due to the insular nature of the government.

Table 8: Total fertility rate for four pre-emergency settings. Source: UN World Population Prospects, 2012

	Rwanda 1990-1995	***Cambodia 1960-1965***	***North Korea 1990-1995***	***Afghanistan 2000-2005***
TFR	6.55	6.95	2.25	7.4

Nuptiality

As marriage involves two different people, scheduling marital events in Socsim is slightly more complicated than scheduling either births or deaths. Socsim can utilize either sex– and age- specific nuptiality rates or calculate the probability of the event based on the distribution of spousal age differences. There is generally very little information on age-specific nuptiality rates. While model mortality and fertility schedules have been developed and validated, much less work has been done to model nuptiality. Therefore, in these simulations, I used information on the distribution of age differences between spouses.

The distribution of age differences is modeled by specifying four parameters; the ideal age difference between bride and groom, the maximum and minimum age differences allowed, the marriage slope ratio, and the percent married by a certain age. The ideal age difference is the preferred difference in ages, with the assumption that the groom is older. For the purposes of Socsim, it is expressed in months. Unlike fertility and mortality, there are few population estimates of what the ideal age difference is and whether it has changed over time. Given that men tend to marry later than women and generally choose younger brides (71), I have set the ideal age difference at 60 months.

The marriage slope ratio compares the rate of decline of a marriage probability for men compared to the marriage probability for women as they deviate from the ideal age difference. Socsim seeks to select marriages that are as close as possible to the ideal age difference between the spouses (here 5 years, with the groom older than the bride). As the age difference between the bride and groom increases, the probability of a match between two potential spouses decreases. Alternatively, if the bride is older than the groom, the probability of a match decreases. A match wherein the bride is older than the groom will have a lower probability of being scheduled than a match where the groom is older than the bride. The rate at which these probabilities change is 7:1; that is, a marriage between a groom that is seven years older than the bride is as likely to be scheduled as a marriage where the bride is one year older than the groom. A marriage where the groom is 14 years older than the bride is as likely to be scheduled as a marriage where the bride is two years older than the groom. This means that in general, marriages in which the wife is older than the husband are uncommon, which is generally supported by the literature (71).

The maximum and minimum age differences establish a range, beyond which no marriages are allowed. For these simulations, I have created a maximum age difference of 240 months and a minimum age difference of -180 months. That is, no marriages are allowed in which the man is more than 20 years older than the woman or when the woman is more than 15 years older than the man. Finally, it is necessary to specify by what age a certain proportion of the population is married to generate basic rates. For females, marriage can begin as early as 12, though it is unlikely, and by age 25, 75% of females are married. For males, marriage can begin as early as 18 and by age 28, 75% of males are married. While these numbers are somewhat arbitrary, the percent married by age and the singulate mean age at marriage presented in Table 9 below confirm that they are plausible numbers for the simulation.

Table 9: Singulate mean age at marriage and percent ever married by age for four countries. Source: UN World Marriage Data, 2012.

	Rwanda 1995	***Cambodia 1962***	***North Korea 2008***	***Afghanistan 2010***
Females				
Singulate Mean Age at Marriage	23.3	21.3	25.5	21.5
% ever-married age 20-24	55.7	68.4	19.2	66.2
% ever-married age 25-29	81.8	90.7	75.4	90
Males				
Singulate Mean Age at Marriage	24.8	24.4	29	25.3
% ever-married age 20-24	28.4	34.2	1.1	NA
% ever-married age 25-29	65.6	79.5	33.2	88.0

There is an obvious difference between the percent married at certain ages between North Korea and the other countries. It is difficult to know the veracity of North Korean marriage data, given the insular nature of the country. Similarly low percentages of women and men married by age 20-24 and 25-29 were seen in South Korea in 1995 (16.7 and 3.7, and 70.4 and 35.6, respectively), lending credence to these numbers (72). Remarriage rates after divorce and widowhood are twice as high for males as females in the simulation. This is a somewhat arbitrary decision as there is very little information available on remarriage rates for males and females in the developing world. The majority of estimates have been generated using data from the developed world, finding that men are more likely than women to remarry after either divorce or death of a spouse (71,73)

Migration

Migration is not specified in this simulation. This is, of course, an inaccurate assumption to make during a complex emergency, as it is established that large-scale migration is a defining feature of a complex emergency. However, there were several reasons that I decided not to address migration in this simulation. First, there is very little information available on migration rates by age in complex emergencies and even less information on reintegration after the emergency has ended. While it is possible to model some general mortality patterns based on historical scenarios, it is much less clear what the migration patterns of these scenarios are. Secondly, when estimating under-5 mortality, migration would only affect the estimates if there were differential mortality between those who remain in the population and those who migrate. While it is likely that this differential mortality does exist in the majority of emergencies, it is not always clear in which direction this difference will exist. For example, refugees may have very high mortality in the beginning phases of migration but as humanitarian assistance improves, they may over time achieve lower mortality than the non-migrant population (4). This was the case amongst Cambodian refugees displaced to Thailand during the Cambodian genocide, where, other than at the youngest ages, mortality was lower for both male and female refugees than amongst the non-displaced (36). On the other hand, those who stay behind in an emergency may be those who are the poorest of the poor, generally the most vulnerable to mortality, and unable to flee in an emergency (4). The differential mortality that may exist between those who stay and those who leave is difficult to predict and therefore, the effect that it may have on estimating under-5 mortality in an emergency is not possible. Finally, my aim is to isolate the effect of mortality disturbances on estimation. Adding in the mass migration that may occur in these emergencies would add complexity and potentially obscure the effect of mortality versus migration.

Analysis

Socsim provides the month of birth and month of death for each person in the simulated population. To calculate annual under-5 mortality rates, months were grouped into twelve-month intervals. The first 4200 months (350 years) were discarded in the analysis as this time simulation segment was intended only to generate a large population structure.

Direct

The first step was calculating the annual period under-5 mortality rate utilizing the birth and death dates of each individual. Individuals began accumulating person-time at birth and stopped accumulating time either at death or when they reach age 5. Cohort under-5 mortality was not calculated because the majority of surveys collect cross-sectional data and calculate period, rather than cohort, rates.

Annual under-5 mortality rates were calculated using the equation

Equation 3

$$r = d/t$$

where d is the number of deaths to children under age 5 in a year over the person-time, t, accumulated by children under-5 in that year.

It is important to note that the term under-5 mortality rate as it is applied in the literature is in fact a probability. Thus, the under-5 mortality rate calculated here, a true rate, was transformed into a probability using

Equation 4

$$_nq_x = \frac{n \cdot {}_nM_x}{\left(1 + (n - {}_na_x\,)\right) \cdot {}_nM_x}$$

where x is exact age x, n is the number of years in the interval (in this case one), $_nM_x$ is the mortality rate, and $_na_x$ is the mean number of years lived in the interval for persons dying in the interval of *x to x +n.*

Annual under-5 rates and probabilities were calculated for every year and every simulation.

Brass

To calculate the probability of dying by exact age 5 using the Brass method, the following information is needed:

1. Number of children ever born, classified by sex and by five-year age group of mother

2. The number of children surviving (or the number dead), classified by sex and by five-year age group of women
3. The total number of women (irrespective of marital status), classified by five-year age group. All women, not just ever-married women, must be included.

It is not necessary to collect sex-specific information for points 1 and 2 above unless it is the intention of the researcher to estimate sex-specific under-5 mortality rates. In this paper, only under-5 mortality rates for both sexes combined are presented.

It is also possible to use time since first marriage instead of age of the mother, however age is generally preferred. Age is be preferable due to inaccuracies in reporting age at marriage or in the event that marriages are frequently dissolved or interrupted (74). As Socsim does not generate dates of marriage, it is not possible to estimate time since first marriage or the corresponding under-5 mortality rate using this technique.

Computational Procedure

In general, the probability of dying between birth and exact age *x* can be expressed as

Equation 5

$$q(x) = k(i)D(i)$$

where $q(x)$ is the probability of dying by exact x, $D(i)$ is the proportion dead among children ever born to women in five-year age groups i, and $k(i)$ is a multiplier adjusting for the underlying fertility distribution.

The multiplicative factors, $k(i)$, that are used to adjust for the fertility distribution are based on regression equations that were fitted based on simulated data. The most commonly used variants of the multiplicative factors are from Sullivan or Trussell. The values of $k(i)$ have been extensively analyzed and tested over time (27,28) and generally the Trussell multipliers are used. For consistency, I will also use the Trussell variant.

The steps for calculating under-5 mortality are outlined below (74).

Step 1: calculation of average parity per woman.

First average parity per woman among women of age i, $P(i)$, is calculated. $P(i)$ is defined as the average number of children ever born, $CEB(i)$, among all women of age i, $FP(i)$, regardless of marital status. $P(i)$ is calculated for the three youngest age groups. Average parity $P(1)$ refers to age group 15-19, $P(2)$ to age group 20-24, and $P(3)$ to age 25-29.

Equation 6

$$P(i) = CEB(i)/FP(i)$$

Step 2: calculation of proportion of children dead for each age group of mother.

The proportion of children, *D(i),* that have died among women of age group *i*, is the ratio of reported children dead relative to reported children ever born

Equation 7

$$D(i) = CD(i)/CEB(i)$$

where *CD(i)* is the number of children dead reported by women of age group *i* and *CEB(i)* is the same as defined in Equation 6 above.

Step 3: Calculation of multipliers

The values of *k(i)*, the multiplicative factors that adjust for the underlying fertility distribution, are estimated using coefficients derived by Trussell and applied in Equation 8. The coefficients to apply when using the West model life table are provided in

Table 10 below.

Equation 8

$$k(i) = a(i) + b(i)(P(1)/P(2)) + c(i)(P(2)/P(3))$$

Table 10: Coefficients for estimation of child mortality multipliers, Trussell variant. Source: UN Manual X, 1982.

Age group of women	*Index*	*Mortality ratio q(x)/D(i)*	*a(i)*	*b(i)*	*c(i)*
15-19	1	q(1)/D(1)	1.1415	-2.7070	0.7663
20-24	2	q(2)/D(2)	1.2563	-0.5381	-0.2637
25-29	3	q(3)/D(3)	1.1851	0.0633	-0.4177
30-34	4	q(5)/D(4)	1.1720	0.2341	-0.4272
35-39	5	q(10)/D(5)	1.1865	0.3080	-0.4452
40-44	6	q(15)/D(6)	1.1746	0.3314	-0.4537
45-49	7	q(20)/D(7)	1.1639	0.3190	-0.4435

Step 4: Calculation of probability of dying and surviving

Estimates of *q(x)*, the probability of dying by exact age *x*, are obtained per Equation 5. The probability of surviving from birth to age x, *l(x)*, is obtained by subtracting the probability of dying from 1.

Equation 9

$$l(x) = 1.0 - q(x)$$

where *x* is age. Per

Table 10 above, we can see that *q(5)* is directly obtained using the information from women age 30-34. However, as noted previously, this estimate does not apply to the time of the survey but to a reference period that remains to be calculated in Step 5.

Step 5: Calculation of reference period

Similar to the Trussell multipliers, *t(x)*, the reference period derived for each age-specific *q(x)*, can be estimated by applying previously derived coefficients to Equation 10. The coefficients in Table 11 were derived from simulations wherein mortality remained constant or declined in a predictable manner.

Equation 10

$$t(x) = a(i) + b(i)(P(1)/P(2)) + c(i)(P(2)/P(3))$$

Table 11: Coefficients for estimation of the reference period, t(x). Source: UN Manual X, 1983.

Age group	*Index*	*Mortality ratio q(x)/D(i)*	*a(i)*	*b(i)*	*c(i)*
15-19	1	q(1)/D(1)	1.1415	-2.7070	0.7663
20-24	2	q(2)/D(2)	1.2563	-0.5381	-0.2637
25-29	3	q(3)/D(3)	1.1851	0.0633	-0.4177
30-34	4	q(5)/D(4)	1.1720	0.2341	-0.4272
35-39	5	q(10)/D(5)	1.1865	0.3080	-0.4452
40-44	6	q(15)/D(6)	1.1746	0.3314	-0.4537
45-49	7	q(20)/D(7)	1.1639	0.3190	-0.4435

In general, the coefficients used to estimate the reference period are those derived by Feeney (30). They are adapted from the original Brass coefficients to allow for the effects of changing mortality over time. However, they were developed with an assumption of linearly declining mortality and thus may be biased if this assumption is not met.

IHME

As shown above, when applying indirect estimation techniques, it is not sufficient to estimate only under-5 mortality, it is also necessary to estimate the point in time to which the estimates refer. IHME developed two alternatives to the Brass methodology to estimate both under-5 mortality and the period in time to which the estimates pertain; the cohort-derived and period-derived measures. Each of the measures can be estimated by using either the mother's age or the time since her first birth. The same information used for Brass is needed:

1. Age of the mother or time since first birth
2. Total number of children ever born
3. Total number of children that have survived

Cohort Derived Methods

When using either the Maternal Age Cohort Method (MAC) or the Time since First Birth Cohort-derived Method (TFBC), the only information needed is either the age or the time since first birth (to categorize women into appropriate cohorts) and the information from the summary birth history (number of children born and number of children who have died).

Step 1: Estimate the reference period

The equations can be adapted to either the age of the mother or the time since first birth. For simplicity, the explanations and notation in equations will refer to age only, but are the same for the time since first birth method.

The first equation, which estimates the reference time or time period to which the mortality rates apply, is

Equation 11

$$reftime_i = \beta_{0i} + \beta_{1i}\frac{CD_i}{CEB_i} + \beta_{2i}CEB_{ijk} + \beta_{3i}\frac{P(15-19)_{jk}}{P(20-24)_{jk}} + \beta_{4i}\frac{P(20-24)_{jk}}{P(25-29)_{jk}} + \epsilon_{ijk}$$

where *i* is the age group of the woman, *P(i)* is the average number of children ever born per woman in the specified age group, CD_i is the total number of children born who have died for age group *i*, and CEB_i is the total children ever born. This method can be applied to multiple countries, *j*, or rounds of data, *k,* which the notation above indicates. For the purpose of this dissertation however, these notations are irrelevant. The coefficients for Equation 11 estimated by IHME using both age and time since first birth are presented in Table 12 and Table 13 below .

Table 12: Coefficients to estimate reference time using Maternal Age Cohort - Derived Method, IHME. Source: Tran and Rajaratnam, 2013

Age Group	B_o	B_1	B_2	B_3	B_4
15-19	0.94	0.00	0.19	0.75	0.54
20-24	2.53	0.00	0.28	0.89	0.62
25-29	4.30	0.00	0.10	0.29	2.84
30-34	7.27	0.00	-0.31	0.58	4.69
35-39	11.43	0.00	-0.47	1.50	4.44
40-44	15.49	0.00	-0.55	3.03	3.97
45-49	19.90	0.00	-0.55	6.08	0.91

Table 13: Coefficients to estimate reference time using Time Since First Birth Cohort-Derived Method, IHME. Source: Tran and Rajaratnam, 2013.

Age Group	B_o	B_1	B_2	B_3	B_4
0-4	1.37	0.00	0.03	0.60	0.49
5-9	2.36	0.00	-0.11	0.56	3.81
10-14	-0.06	0.00	0.04	3.69	8.65
15-19	1.80	0.00	-0.08	2.09	12.86
20-24	-1.17	0.00	0.02	4.65	18.86
25-29	5.28	0.00	-0.16	-0.91	19.95
30-34	9.56	0.00	-0.22	-17.01	31.70

Step 2: Estimate $_5q_0$

The IHME method differentiates itself from the traditional Brass method by directly estimating $_5q_0$ for each age group (or cohort of mothers). The logit of 5q0, estimated using Equation 12, is back transformed to estimate 5q0 for each group of women, *i,* in country, *j,* and survey round, *k* using Equation 13.

Equation 12

$$\text{logit}(5q0_{ijk}) =$$

$$\beta_{0i} + U_{ij} + \beta_{1i}\text{logit}\left(\frac{CD_{ijk}}{CEB_{ijk}}\right) + \beta_{2i}CEB_{ijk} + \beta_{3i}\frac{P(15-19)_{jk}}{P(20-24)_{jk}} + \beta_{4i}\frac{P(20-24)_{jk}}{P(25-29)_{jk}} + \epsilon_{ijk}$$

Equation 13

$$\text{invlogit} = \frac{e^x}{1+e^x}$$

where x equals logit $5q0_{ijk}$.

The IHME model allows for estimated country-specific variation by introducing the random intercept, U_{ij}. The values for this random intercept were estimated by IHME during initial development of the model. As my data are simulated and do not represent the experience of any one country, no random intercepts are used in this analysis. The coefficients developed by IHME for estimating ${}_5q_0$ are shown in Table 14 and Table 15 below.

Table 14: Coefficients to estimate 5q0 using Maternal Age Cohort-Derived Method, IHME. Source: Tran and Rajaratnam, 2013

Age Group	B_o	B_1	B_2	B_3	B_4
15-19	-0.70	0.56	2.57	-3.92	-0.30
20-24	0.13	0.89	0.21	-0.01	-1.11
25-29	0.02	0.99	0.05	0.07	-0.29
30-34	-0.10	0.97	0.02	0.00	0.00
35-39	-0.09	0.97	0.01	0.22	-0.23
40-44	-0.24	1.00	0.02	0.16	-0.06
45-49	0.04	1.02	0.00	0.86	-0.80

Table 15: Coefficients to estimate 5q0 using Time Since First Birth Cohort-Derived Method, IHME. Source: Tran and Rajaratnam, 2013.

Age Group	B_o	B_1	B_2	B_3	B_4
0-4	0.86	0.86	-0.14	-1.75	0.25
5-9	0.05	0.97	-0.01	-0.81	0.59
10-14	0.31	0.99	-0.02	-0.54	0.08
15-19	0.36	0.96	-0.01	-1.37	0.43
20-24	-0.18	0.98	0.01	-1.39	0.93
25-29	0.81	1.00	-0.03	-2.52	0.73
30-34	2.09	0.81	-0.07	-4.00	-0.02

Period-Derived Methods

As with the Brass method, the cohort method described above uses responses from the youngest mothers to estimate mortality in the recent (<5 years) past, which introduces the potential for bias. Additionally, the cohort-derived methods generate reference times that are in the distant past for older mothers; the average reference time for women age 45-49 is 18.1 years prior to the survey (26).

To address these issues, IHME developed the period-derived methods, which estimate a period-based CD/CEB ratio for each year prior to the survey and that can be applied to either age or time since first birth. The CD/CEB ratios are derived from distributions of child birth dates and death dates for different groups of mothers, stratified by region, age and number of children born and dead. These distributions were generated by IHME and rely on pooling regional data (Asia, Latin

America and the Caribbean, North Africa/Middle East, sub-Saharan Africa South/East, and sub-Saharan Africa North/Central) to estimate regional distributions of births and deaths to women of age *i*. The distributions are used to estimate the expected number of children who were ever born and died in every year prior to the survey (up to 25 years before) for mothers of each particular age, region, and parity and to generate the expected CD/CEB ratio for that year. This ratio is then applied using Equation 14 to estimate logit ($_5q_0$)

Equation 14

$$\text{logit}(5q0_{tjk}) = \beta_t^0 + U_{tj} + \beta_t^1 \text{logit}\left(\frac{CD_{tjk}}{CEB_{tjk}}\right) + \epsilon_{tjk}$$

where t is the time before the survey, j is the country-specific estimate, and k is the survey year, in the event that multiple rounds of data exist within a given country. The CD/CEB ratio and the coefficients vary by year and by country and are estimated by IHME. No random effects were applied in this dissertation. As it is not possible to apply the models without selecting a region, I chose sub-Saharan Africa South/East, as the high fertility patterns simulated in the model persist in many countries in this region (3).

Combined Method

The Combined Method averages the estimates of the cohort- and period-defined IHME methods through a process of inverse weighting. Each of the cohort-defined methods produces seven estimates of 5q0 (one for each age category) while each of the period-defined methods produces 25 estimates of 5q0, one for each year prior to the survey year. Each estimate produced by the cohort-method is then given a weight of 1/7 and each estimate from the period defined method is given a weight of 1/25 in order to weight each method equally. The estimates for the youngest age group using the Maternal Age Cohort (MAC) method are dropped per IHME advice because of the generally small number of women in the category, leading to noisy estimates. The combined method then smoothes the estimates to account for non-linear trends over time (for example, over the 25 year period) using Loess regressions. Using the recommendation provided by IHME, the α, or bandwidth, parameter was set at .5, and weighted using the inverse weights just described. The Combined Estimates were then generated using data from all four of the IHME methods and using only the maternal age methods (MAC and MAP) methods. This was done to examine the increased accuracy, if any, gained by incorporating time since first birth information with maternal age information.

The Loess smoother can also be applied to data from each individual method, rather than a combination of methods, to show the trend that each method predicts. For example, for each year of survey data, the MAC method will generate seven separate 5q0s, each with a reference period. Using these 5q0s, the Loess smoother can approximate what the trend in child mortality has been over time based solely on the MAC method. This process can be done for all methods and can incorporate data from only survey year or from multiple survey years. For each of the methods, I created an individual smoothed estimate using data from all survey years and using data only from specific years to see how trends changed when data from only year were available (Years 0, 1, 5, 10, and 20).

Comparison of Estimates

In total, 500 simulations were run over a time frame of 60 to 80 years each (excluding the original 350 years of simulation and depending on the length of the humanitarian emergency). Thus a pattern of mortality prior to the emergency, during, and after the emergency could be established. For consistency across all scenarios, data are presented showing Year 0 as the year in which the humanitarian emergency started and estimates are provided for a forty year time period, 13 years pre-emergency and 26 years post-emergency.

Within each simulated year and within each simulation, six estimates of under-5 mortality were made: direct, Brass, Maternal Age Cohort (MAC), Time since First Birth Cohort (TFBC), Maternal Age Period (MAP), and Time since First Birth Period (TFBP). In addition, the Combined estimates, both all methods combined and MAC/MAP only, were run using data from all years and run using data from single survey years. Thus in total, there were upwards of 160,000 individual under-5 estimates calculated, across all years, scenarios, and simulations.

While the simulations were programmed with specified levels of infant and child mortality, the element of random selection in Socsim ensures that each simulation has a slightly different mortality profile and thus a slightly different under-5 mortality rate. There is not therefore an underlying true estimate of under-5 mortality being tested across each simulation, although the average of the directly estimated 5q0s within a year is a very close approximation to the programmed parameters. Thus to compare how well each method estimates the true under-5 mortality rate within a year and a simulation, the estimate derived from the direct method is taken as the gold standard and compared to the estimates derived from the indirect estimates within the same year and simulation.

I used three metrics to assess how well estimates from each indirect method compared to the values from the direct method. The first is the average absolute error; within a specific survey year, the absolute differences of each simulation-specific direct estimate and indirect method estimate (Brass, MAC, MAP, TFBC, TFBP, and two Combined estimates) were totaled and divided by the total number of simulations (Equation 15).

Equation 15

$$\frac{\sum_{i=1}^{n}\left|q_{ij}^{Indirect} - q_{ij}^{Direct}\right|}{n}$$

where q is the estimate of ${}_5q_0$, i is the simulation number, j is the survey year, and n is the number of simulations.

Secondly, the mean of these differences, both within a specific year and across years, assesses whether the specified indirect method, on average across the simulations, over or underestimates the direct estimate (Equation 16).

Equation 16

$$\frac{\sum_{i=1}^{n} q_{ij}^{Indirect} - q_{ij}^{Direct}}{n}$$

Finally, the distribution of the differences was examined to assess how much variation there was in the differences across the simulations in a given survey year.

Although this was done across each year, for simplicity, the majority of the metrics are presented only for Year 0, Year 1 (one year since onset of the humanitarian emergency), Year 5, Year 6, Year 10, and Year 20.

Chapter 4: Results

As multiple models were tested in several scenarios and across hundreds of simulations, there are many ways in which this chapter could be organized. In order to focus the results and subsequent discussion, results will be presented in the following manner:

Section 1: First, results from the "baseline" scenario in which there is no disruption in declining mortality are presented. I will first discuss how each of the methods – Brass, the Cohort-Derived methods, the Period-Derived methods, and the Combined methods – compare to the direct estimate of mortality and then show comparisons of the methods in an "ideal" scenario.

Section 2: Based on the results of Section 1, selected methods will be presented to demonstrate how well they perform across humanitarian emergency, focusing specifically on Brass, MAP, MAC and the MAC/MAP combined method. The time since first birth-derived methods will not be discussed in detail as they generally perform poorly across all scenarios, as will be demonstrated in Section 1.

Chapter 4: Results

Clarifications

I refer to under-5 mortality and 5q0 interchangeably in this dissertation; for readability in tables, the probability is multiplied by 1,000 to reflect the number of children who will die before their fifth birthday out of every 1,000 children under 5.

Additionally, I will refer to two different time points in this chapter, the survey year and the reference year. Each simulation is run for over 40 continuous years and thus generates data and point estimates for every year. When I refer to the survey year, I am referring to the year in which the data were generated. All direct estimates are made with data from 12 months prior to the survey period. All indirect estimates, however, have some reference period and a corresponding reference year. Thus, each indirect estimation method will generate an estimate *for* a specific reference year using data *from* a specific survey year. When making comparisons, the estimate for the reference year will be compared to the direct estimate for that same reference year. For example, data from survey year 15 will generate a direct estimate for survey year 15 and a Brass estimate for a reference period of approximately 6 years prior. The Brass estimate, estimating 5q0 in Year 9, will be compared to the direct estimate for Year 9 to gauge accuracy.

Finally, for each simulation, 100 different datasets were generated. Each direct and indirect estimate for a specific year is the average across all of these simulations. In some cases, such as overall reference period and overall deviations between the direct method and the indirect estimate, averages across each simulation are then averaged over the forty-year simulation period, for simple comparison purposes.

Baseline Comparisons

I first examined how Brass and each IHME estimate compare to direct estimate in a simulation that mimics a population experiencing linearly declining mortality and fertility. This simulation presents a baseline for understanding how each method compares in an normal situation before examining how each method performs in a complex emergency.

Brass

Figure 3 shows the average of the estimated 5q0s generated by the Brass method and the average generated by the direct method for each year across a forty-year simulation period. Note that the x-axis begins at the year -13; this is due to the fact that Year 0 denotes the starting point of the humanitarian emergencies in the four emergency simulations and the scale of the x-axis represents time in relation to Year 0. Having applied this to the figures derived from the humanitarian emergency scenarios, for consistency, the figures for the baseline scenario are presented in the same fashion.

Chapter 4: Results

Across the forty-year simulation period, the 5q0 declines from an average of .244 across the 100 simulations to an average of .152; the Brass estimates decline from .244 to .156 over the same period. Across the time period, when the Brass estimate for the reference year is compared to the direct estimate in that reference year, the estimates are very close (Figure 4a), although across all years, the average of the Brass estimates is slightly elevated. There is no significant difference between the accuracy of the Brass estimate at higher, versus lower, levels of mortality. For comparison, I have also included a graph that demonstrates the difference between the direct estimate and the Brass estimate if the Brass estimate is applied to the survey year rather than the reference year (Figure 4b). When misapplied in this way, the under-5 mortality rate is consistently over-estimated in a non-crisis setting. Although this is not standard practice, I have shown this result here to facilitate a comparison to the crisis settings when the adjustment from the survey year to the reference year becomes problematic.

Figure 4: Average across 100 simulations for 5q0 estimates derived from direct and Brass methodology by year - baseline scenario

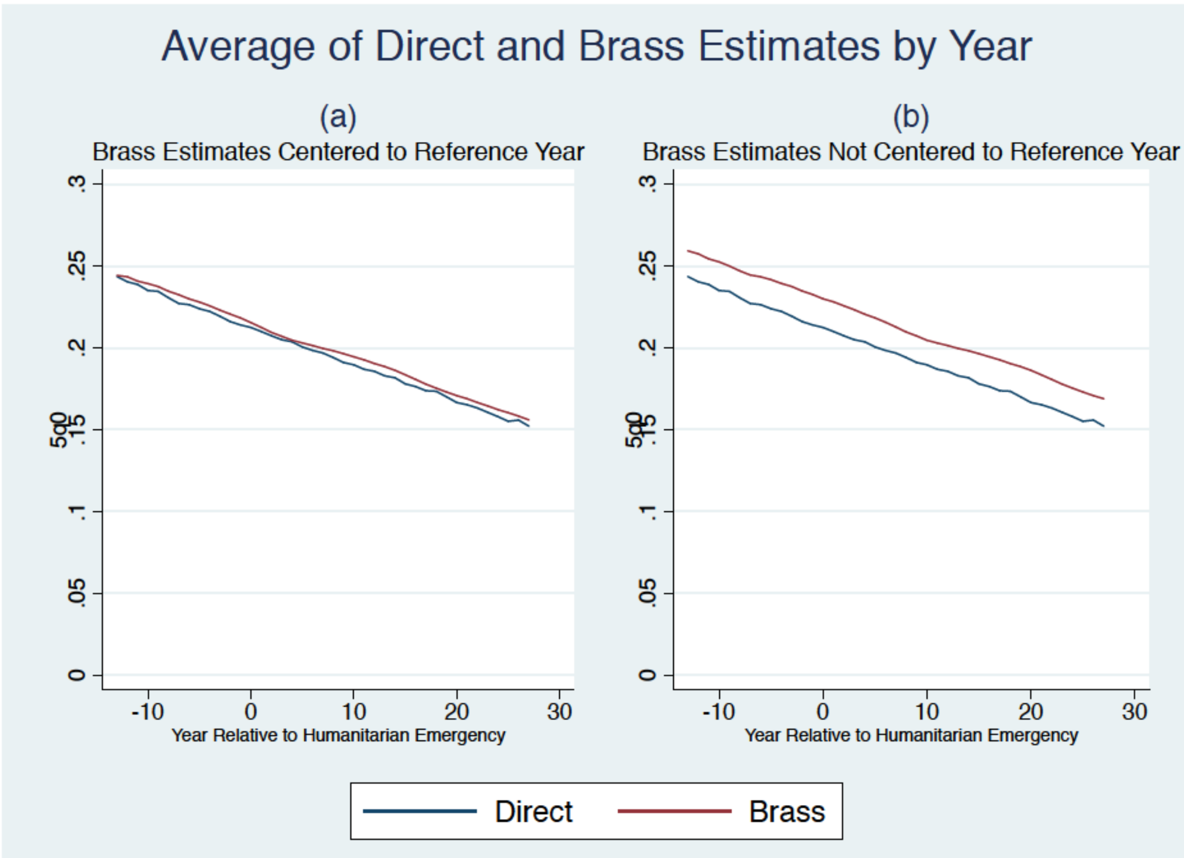


Table 16 below summarizes the average deviation of the Brass estimates from the direct estimates across the 40-year simulation period. It also includes the average reference period derived for the Brass estimates using all data across the forty-year simulation period and the average difference and average absolute difference between the direct and indirect methods across the simulation period.

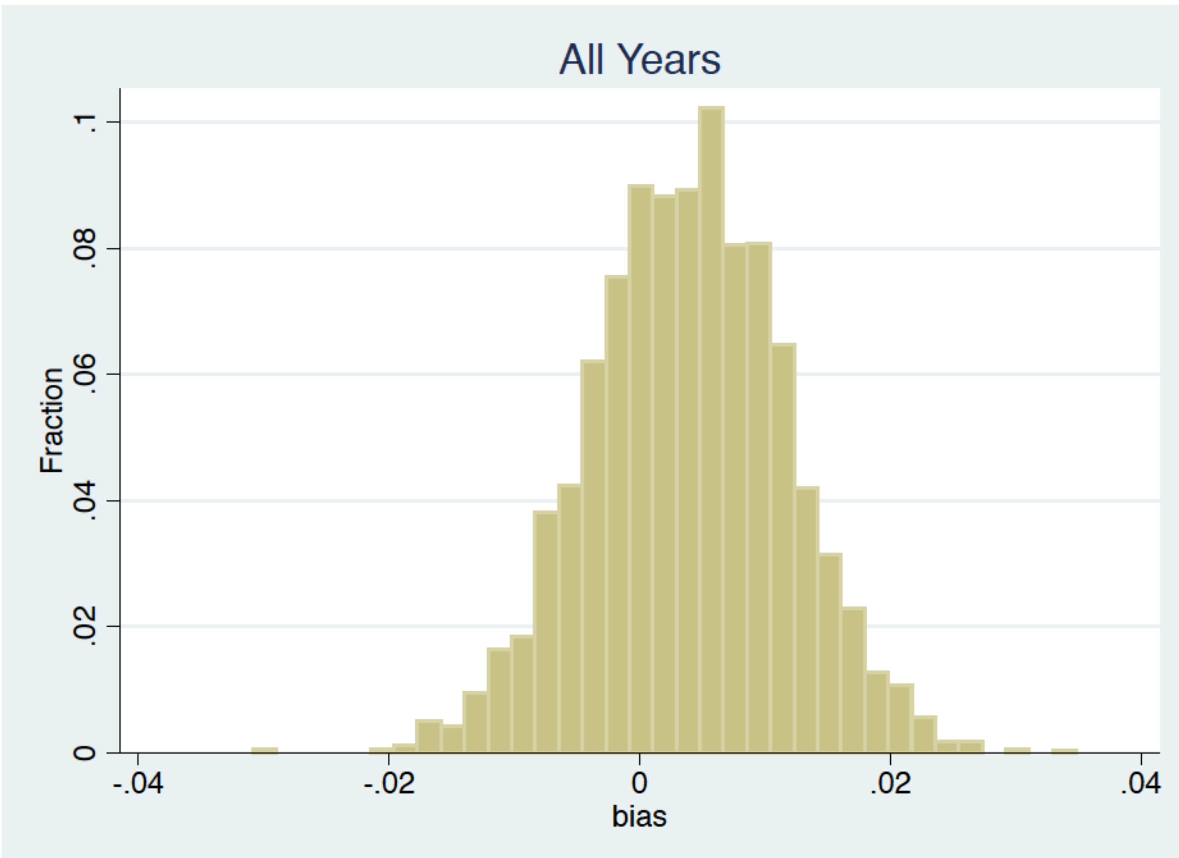
Table 16: Average of direct and Brass estimates (deaths per 1,000 live births) and deviations across all simulations and years - baseline scenario

Average across 40 years	
Direct	197
Brass	200
Mean reference period	6.15
Mean difference (Indirect-direct)	3
Mean absolute difference	7

On average, across all simulations and years, the Brass method overestimates the direct estimate by 3 deaths per 1,000 live births, slightly over 10% of the average direct estimate. The mean absolute difference of 7 deaths per 1,000 live births indicates that the Brass method both over- and underestimated mortality across the simulations.

In terms of absolute differences, Figure 5 below shows the distribution of the difference between the simulation-specific direct estimate and the simulation-specific Brass estimate across the 40-year simulation period. In only 95 out of the 4,000 estimates, the absolute difference was greater than or equal to 10% of the direct estimate, which on average is 19 of the 197 deaths per 1,000 live births. More concretely, in 2% of all simulations, the absolute difference between the direct estimate and the Brass estimate was greater than or equal to 20 deaths per 1,000 live births and in 25% of the simulations, the absolute difference was greater than or equal to 10 deaths per 1,000 live births.

Figure 5: Distribution of differences between the simulation-specific direct and Brass estimates across all simulations and year - baseline scenario



IHME

MAC and TFBC Methods

As the methodology for the Cohort-Derived methods (MAC and TFCB) is the same, I have combined the results in this section to better demonstrate the differences that arise when using maternal age information versus time since first birth to define cohorts of women.

The MAC and TFBC methods estimate a separate 5q0 for each age group or time since first birth group (TFBC group). In comparison, the Brass method estimates 5q0 using data only from the age group 30-34, estimating 1q0, 2q0, 3q0 using data from the younger age groups and 10q0, 15q0, and 20q0 using data from the older age groups. Thus from any one survey, the MAC or TFBC method will supply seven separate 5q0 estimates and seven separate reference periods, while the Brass method will supply one 5q0 estimate and a corresponding reference period. The MAC and TFBC estimates, therefore, generate separate probabilities of dying for women of each age or TFBC group and do not generate one population-level estimate. The point estimates for the reference years can be compared to the direct estimates, as I do below, or the methods can be smoothed using the Loess Smoother. Table 17 below summarizes the average reference period and the average mean difference between the direct estimate and the method-specific estimate using data from all simulations and years.

The MAC method point estimates are generally a better approximation of the direct estimate than the TFBC estimates. The exceptions to this are for the two youngest age groups and most recent TFBC groups[1]. In these groups, the MAC method underestimates the direct method, while the TFBC method overestimates the method by a similar amount. While the MAC method point estimates are on average an underestimate of the direct method in the reference year, the estimates for age group 30-34, 35-39 and 40-44 are all close to the direct estimate over time, with their average means differing from the mean direct estimate by between 5% and less than 1% of the direct estimate (Table 17). The lowest mean difference is found for the age group 40-44, which differs from the overall mean estimate by .2%. However, these estimates refer to time periods that are on average 15 years prior to the year of the survey (Table 17).

The TFBC method overestimates under-5 mortality, particularly amongst women who had their first birth 25-29 and 30-34 years ago; on average and across the time interval, the TFBC overestimates the direct estimates by 106% and 196%, respectively (Table 17). Estimates from younger women appear to be more reliable, particularly among women who gave birth 5-9 years prior to the survey. With a reference period of 3.4 years, the estimates are much closer in time to the survey year than the most accurate estimates generated by the MAC method. Even so, the

[1] When referring to "most recent" TFBC groups, I am referring to the cohort of women who had their first birth either 0-4 years prior to the survey or 5-9 years prior. Conceptually, most recent TFBC group is equivalent to youngest age group and least recent TFBC group is equivalent to oldest age group.

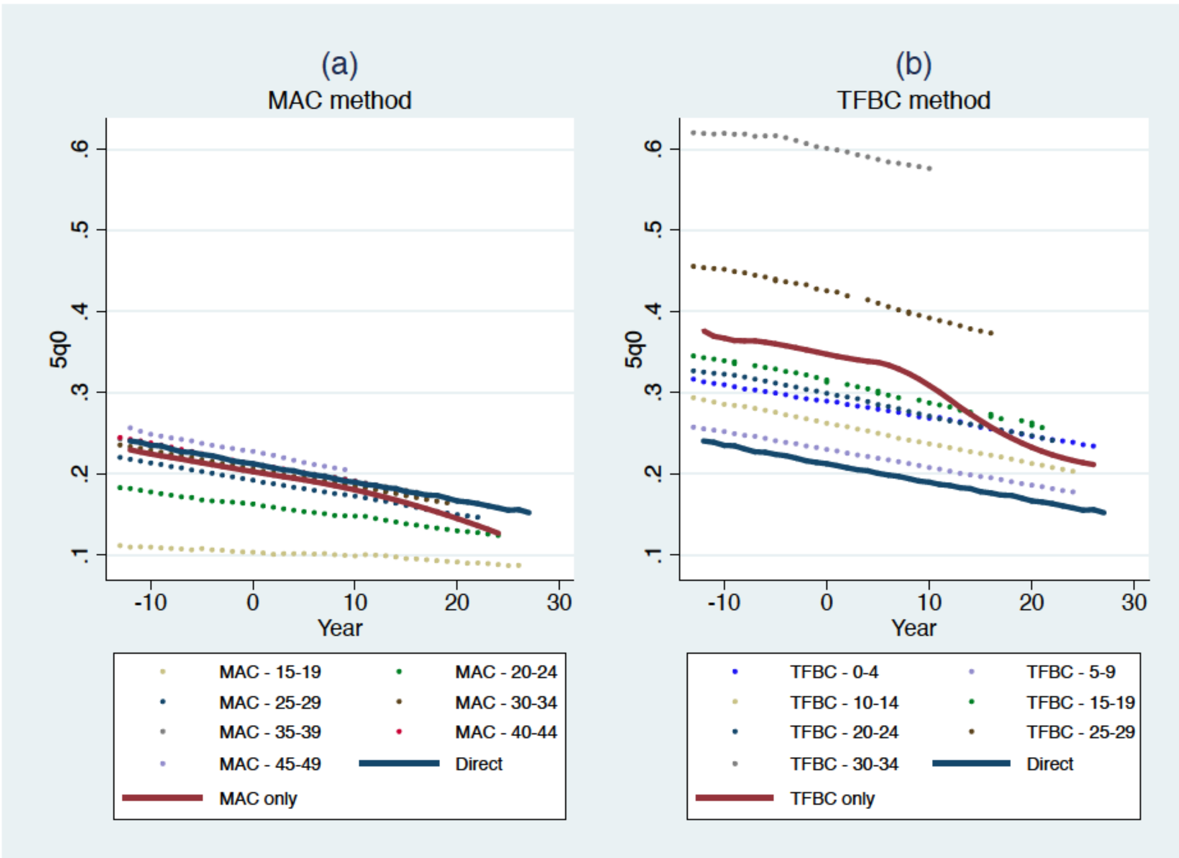
total mean of the TFBC estimate for group 5-9 differs from the total mean direct estimate by approximately 10%.

Table 17: Mean reference period and differences (deaths per 1,000 live births) comparing direct and MAC and TFBC method estimates by age group and time since first birth group across all years and simulations - baseline scenario

MAC Method				***TFBC Method***			
Age	Mean Reference Period	Mean Difference	% of Direct Estimate	Time Since First Birth	Mean Reference Period	Mean Difference	% of Direct Estimate
15-19	1.3	-97	49.3	0-4	1.6	80	40.7
20-24	3.12	-46	22.9	5-9	3.4	20	10.2
25-29	5.8	-19	9.7	10-14	3.4	50	25.4
30-34	8.4	-6	3.0	15-19	6.0	100	50.8
35-39	11.8	.4	.2	20-24	5.8	90	45.7
40-44	15.3	-.2	.1	25-29	11.1	210	106.3
45-49	18.7	13	6.6	30-34	17.7	380	196.3

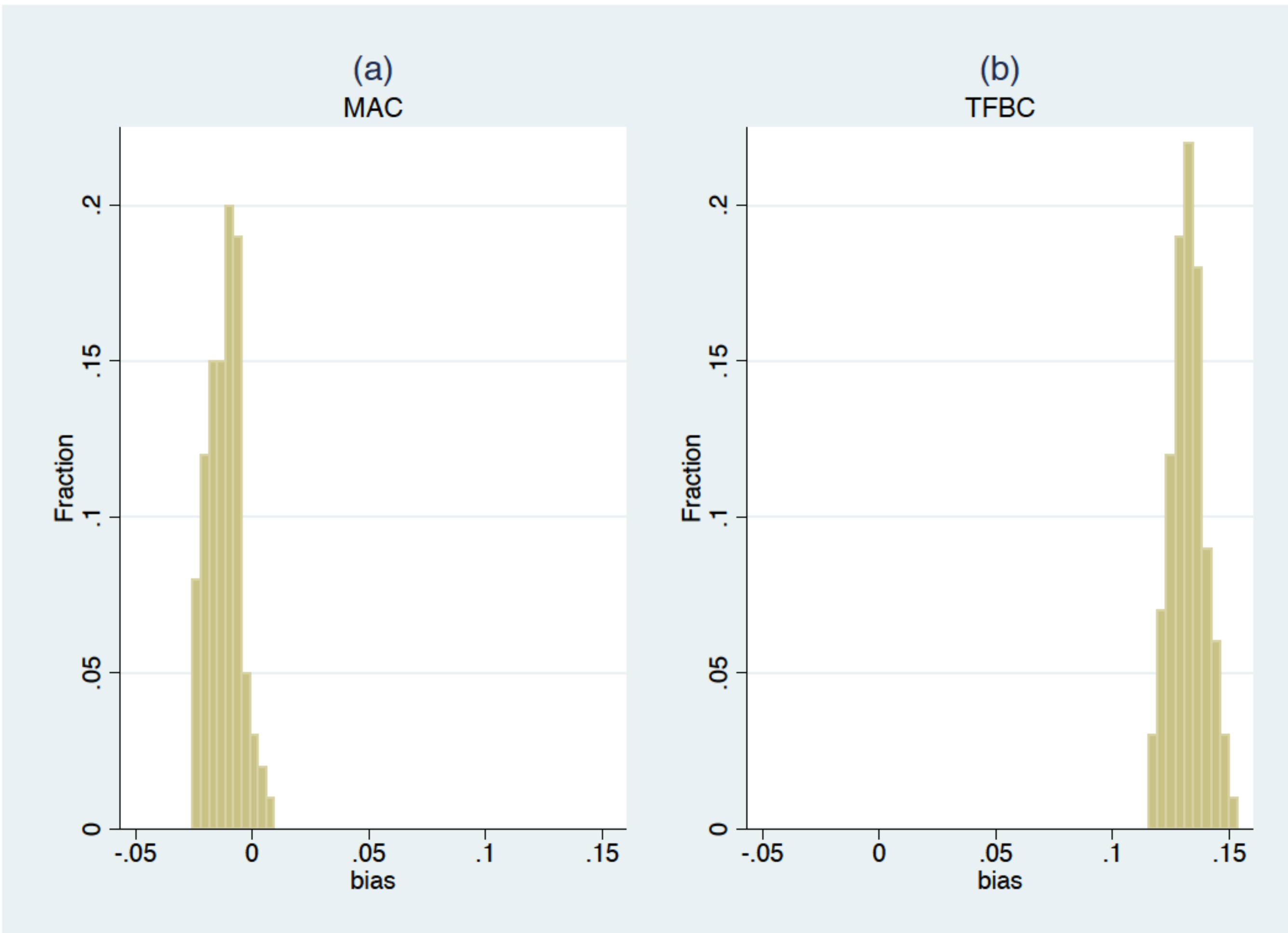
Figure 6 (below) shows the estimates of 5q0 for each group in the corresponding reference year and the corresponding direct estimate of 5q0 for that year. Each figure also has the smoothed method-specific Loess line generated using data from all survey years. In keeping with IHME recommendations, the age group 15-19 was dropped when generating the Loess line for the MAC method.

Figure 6: Average point estimates across 100 simulations using MAC and TFBC methods and average of the Loess smoothed estimates - baseline scenario



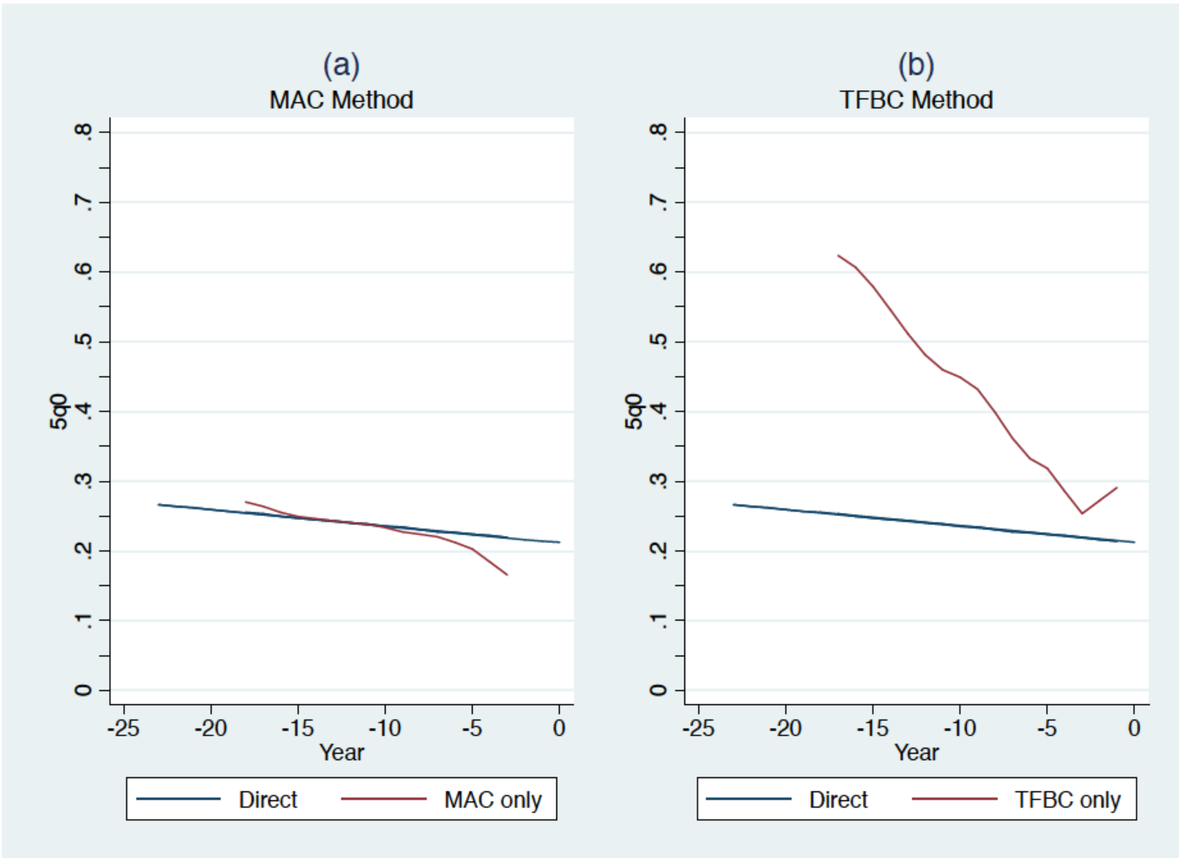
To show a distribution of differences for each simulation across each year is difficult for the MAC and TFBC methods as single point estimates are not generated but trends across time. It is possible to isolate the mean of the predicted value for a specific year however, so I have shown the mean of the predicted value estimated by the smoothed MAC method and the smoothed TFBC method for a single year. In this case, the mean estimates for the predicted value for Year 0 generated from the smoothed MAC and smoothed TFBC are compared to the direct estimate for Year 0 (Figure 7 below). In the majority of simulations, the estimate predicted by the smoothed MAC method for Year 0 underestimates the direct estimate; however the average difference is -.012, or 12 deaths per 1,000 live births. This is less than 10% of the direct estimate. The Time since First Birth Cohort derived method does not perform as well, consistently overestimating the direct estimate by a significant degree. This is due to the extremely overestimated 5q0s estimated for the cohorts that had their first births over 25 years ago. On average, the TFBC method overestimates the direct value in Year 0 by .132, or 132 deaths per 1,000 live births, almost double the direct estimate of .197.

Figure 7: Distribution of differences between direct estimates and smoothed MAC method (a) and smoothed TFBC method (b) across 100 simulations in year 0 - baseline scenario



Data from every year were used in the Loess procedure that generated the smoothed estimates in Figure 6. The inclusion of data from every year leads to consistent decline until approximately year 10, when fewer data points are available and the methods show dramatic declines as a result of a dependence on the lower estimates derived from younger cohorts/ more recent first births. When the Loess smoother incorporates less data, as is done in Figure 8, the trend in child mortality is less stable. In this case, only data from Year 0 is used to generate the MAC and TFBC point estimates which are then smoothed with the weighted Loess. While the smoothed MAC method does a reasonable job of estimating mortality eight to fifteen years previous (as we would expect from Table 17), it underestimates mortality in the five years previous to the survey. Contrastingly, the TFBC method shows a rapid decline in mortality over time from levels that are significantly above baseline and an increase in mortality in the three years previous to the survey. Using data from Year 0, the TFBC method estimates a decline in mortality from .623 to .285 over approximately 15 years and then an increase to .290; however the true decline, estimated from the direct method is a decline from .253 to .214.

Figure 8: Average across 100 simulations of 5q0 estimates derived from direct and Loess smoothed MAC (a) and TFBC (b) using data from Year 0 – baseline scenario

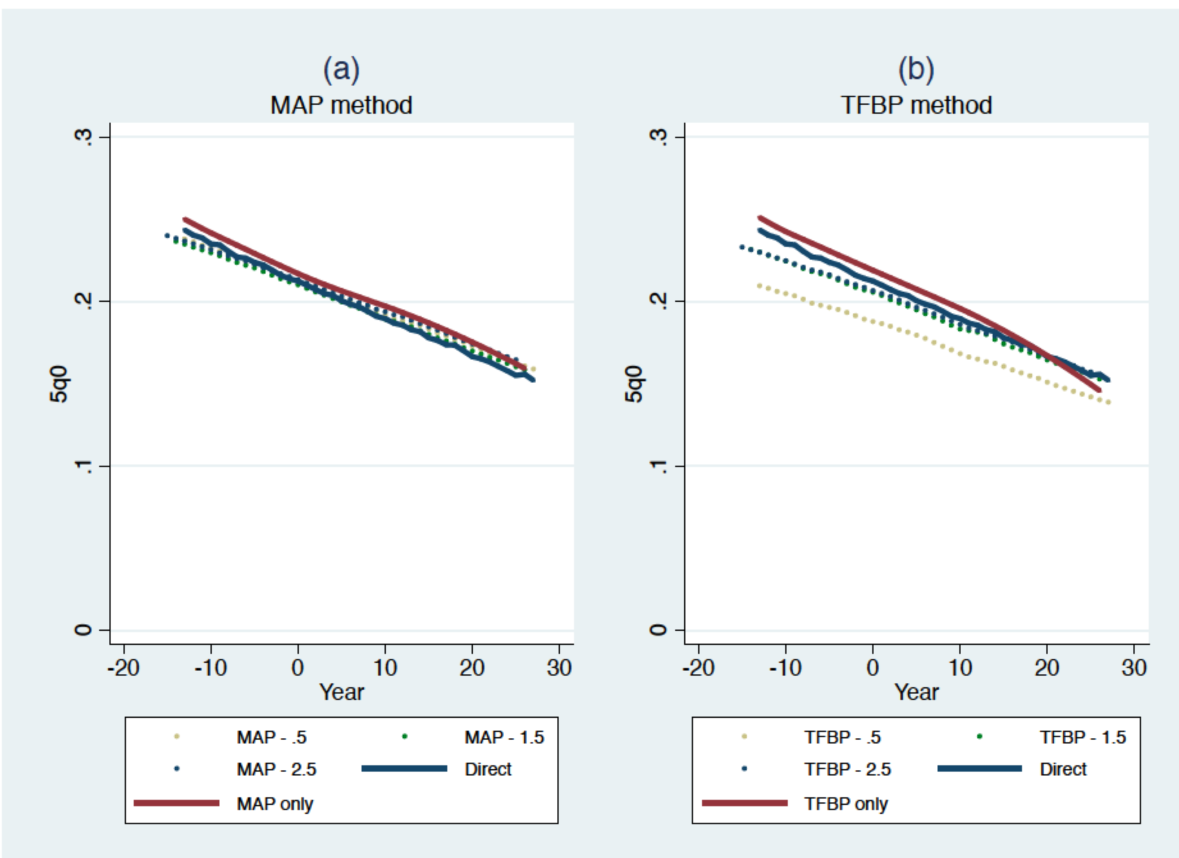


MAP and TFBP Methods

For each survey year, the MAP and TFBP methods each generate 25 separate 5q0 estimates and a corresponding reference year (in intervals of 1 year beginning .5 years prior to the survey year). In the interest of clarity, I have not shown the point estimates for every reference year in Figure 9 (below); instead, I have shown only the estimates for the three most recent reference periods derived using the period-derived methods of .5, 1.5, and 2.5 years. This is because the purported advantage to these methods is their ability to produce recent estimates of under-5 mortality and it is of particular interest to know how well these recent estimates approximate the direct estimate. I have also shown the smoothed Loess trend for the MAP and the TBFP utilizing all point estimates derived from data for every survey year.

When using data from all years, both of the smoothed estimates produce trends that are close to the direct estimate, although slightly elevated. When the weighted Loess reaches Year 20, and there are fewer data points available, the estimates are skewed downwards as a result of the consistent underestimation of mortality in the reference year of .5 years prior to the survey.

Figure 9: Average across 100 simulations of 5q0 estimates derived from direct and Loess smoothed MAP (a) and TFBP (b) using data from all years – baseline scenario



As with the cohort-derived methods, when data from only one year is used in the Loess Smoother there is less accuracy in the trend lines over time. Figure 10 (below) shows the trend in child mortality predicted by each period-derived method when only data from Year 0 are used. The TFBP method underestimates mortality in the two years prior to the survey and overestimates it in the years prior to that, while the smoothed MAP method generates a close prediction for the most distant and most recent time periods and overestimates mortality in the interim.

Figure 10: Average across 100 simulations of 5q0 estimates derived from direct and Loess smoothed MAP (a) and TFBP (b) using data from Year 0 – baseline scenario

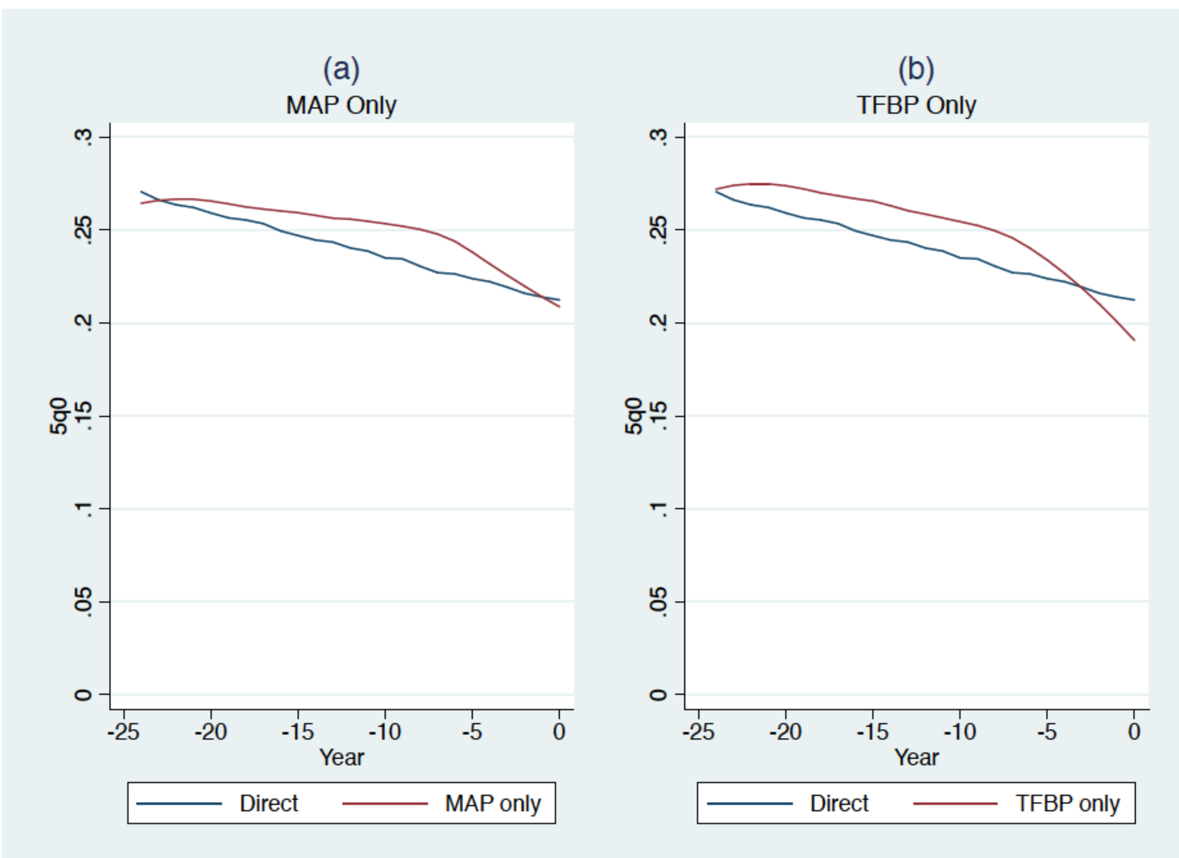


Table 18 shows the average difference between the method specific point estimates derived for each reference year and the corresponding direct estimate for that year, using data only from Year 0. It also shows the predicted mortality estimate for the reference year that is generated using the Loess smoother. Years in which the methods produce estimates that differ by more than 10 deaths per 1,000 live births are highlighted below[2].

Although the point estimates for the MAP method show a mean difference of less than .001 from the direct estimate in Year 0, there is variation across the simulations for both the MAP and TFBP methods, shown in Figure 11 (below). Looking at only the most recent reference periods of .5 and 1.5 years prior to Year 0, there are no simulations in which the direct estimate differs from the MAP estimate by more than 10%. There are 68 simulations (out of 100) in which the TFBP method differs by more than 10% of the direct estimate when using a reference period of .5, but no simulations that differ by more than 10% when using a reference year of 1.5, at year 0.

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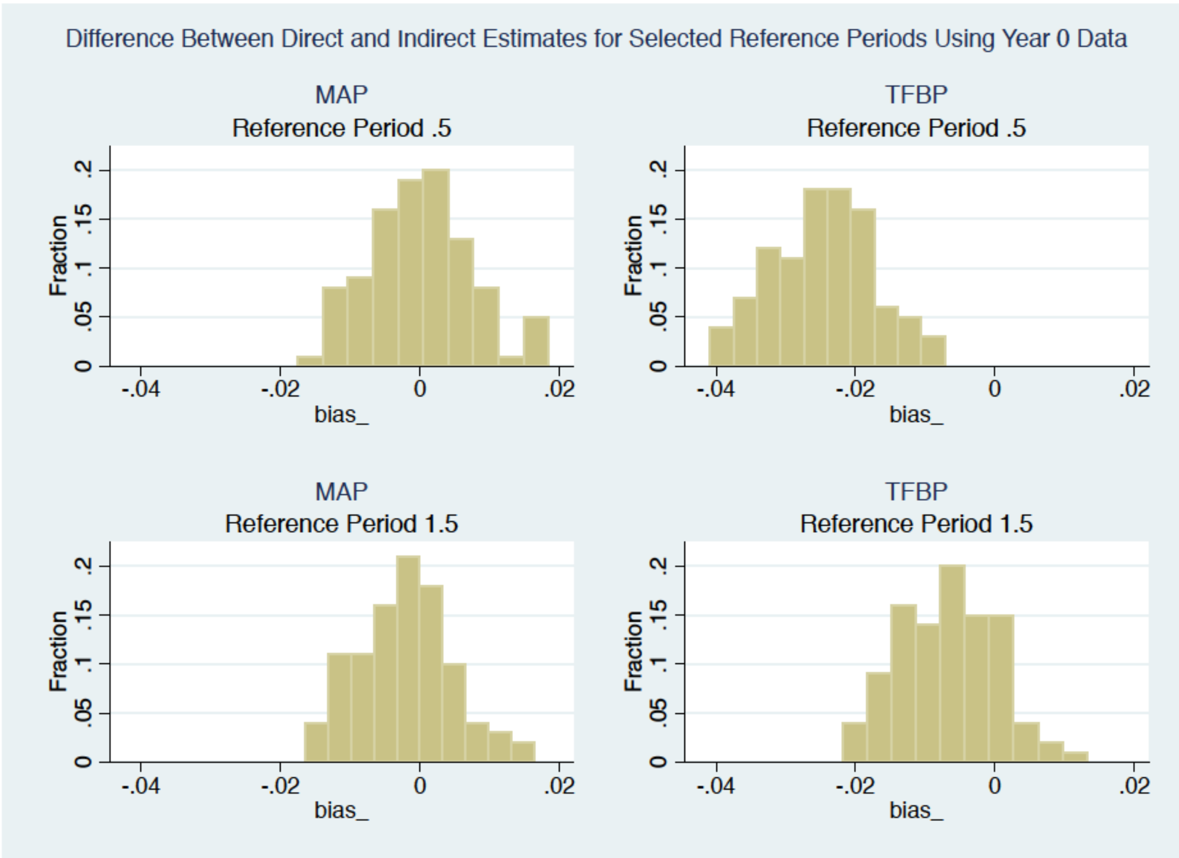
[2] This level was chosen based on the comparison of UN-IGME and IHME estimates developed by Alkema and You (2012).

Table 18: Mean differences of direct estimates from MAP and TFBP unsmoothed and MAP and TFBP Loess-smoothed estimates (deaths per 1,000 live births) for specified reference years using Year 0 data - baseline scenario

Year	***MAP – Period n=100***	***MAP – Loess n=100***	***TFBP – Period n=100***	***TFBP – Loess n=100***
-24	-9	-6	0	1
-23	0	0	7	8
-22	7	3	11	11
-21	7	4	20	13
-20	15	6	26	15
-19	-8	8	-2	16
-18	10	7	18	15
-17	11	8	15	15
-16	11	11	22	17
-15	14	12	20	19
-14	11	13	16	18
-13	8	13	10	17
-12	21	16	26	18
-11	14	16	12	18
-10	24	18	30	20
-9	14	18	9	18
-8	15	20	18	19
-7	23	21	17	19
-6	22	18	25	14
-5	17	14	4	10
-4	5	10	5	4
-3	3	6	-7	-1
-2	1	4	-6	-6
-1	-2	0	-7	-13
0	0	-4	-25	-22

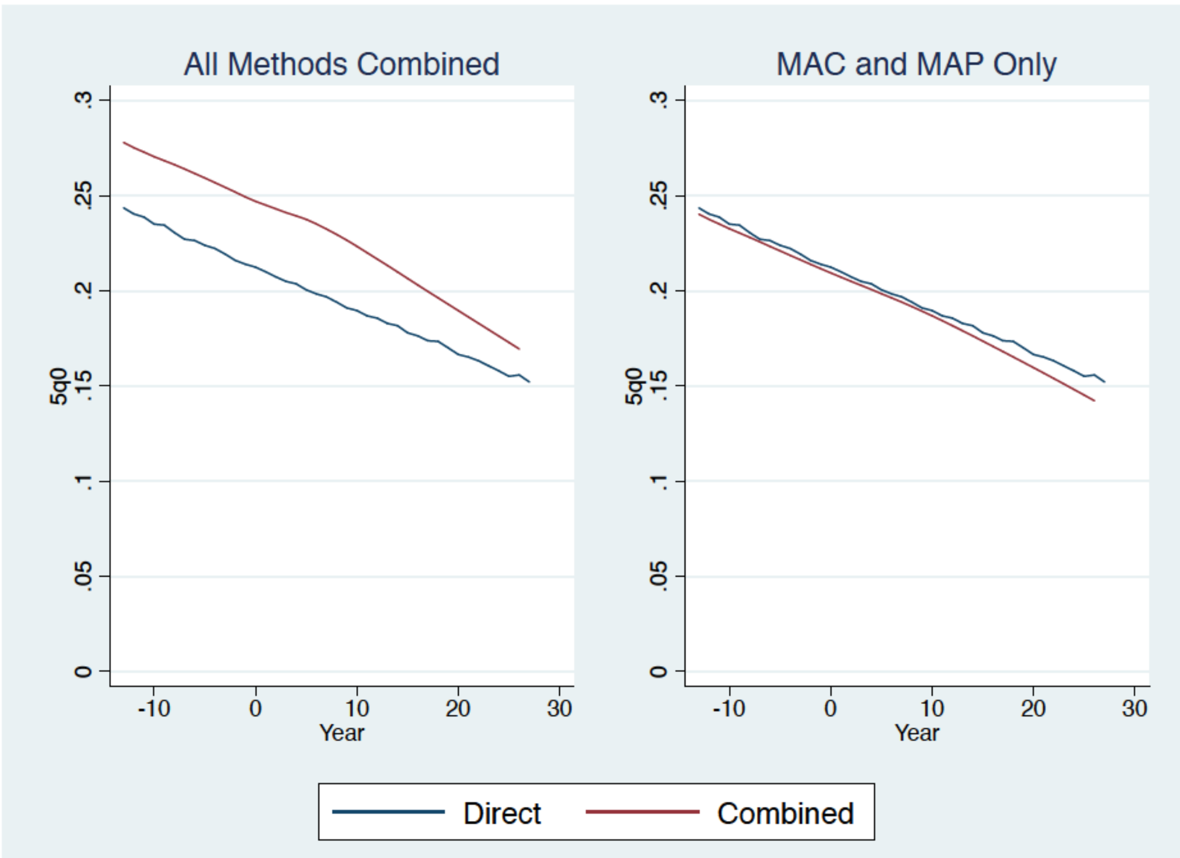
Figure 11: Distribution of differences between simulation-specific direct method and MAP and TFBP method for reference period of .5 and 1.5 years before Year 0 – baseline scenario



Combined Method

Figure 12 (below) shows the estimates when all methods are combined and when only the MAC/MAP methods are combined using data from all years are used; in Figure 12 (a) estimates from the MAC, TFBC, MAP, and TFBP methods are combined and in 12 (b), only MAC and MAP are used. The TFBC and TFBP methods, each independently higher than the direct estimates, lead to an overestimate across all years relative to the direct estimate when used in the combined method. However, when only the MAP and MAC methods are used, the combined method generate estimates close to those of the direct estimate, until approximately 15 years prior to the end of the simulation.

Figure 12: Average estimates across 100 simulations using weighted Loess Smoother; all methods combined and maternal age methods only – baseline scenario



The MAC/MAP Combination above incorporates data from every survey year throughout the 40-year interval, using a total of 1280 points for smoothing per simulation (32 points for each survey year and 40 survey years). The average of the MAC/MAP Combination across the 100 simulations when only 32 estimation points from Year 0 are used is shown below in Figure 13. When there is no disruption in linear mortality decline, the method consistently underestimates at reference points approximately one to five years prior to the survey, and overestimates prior to this point. The MAC/MAP combination closely estimates the direct estimate at the survey year, however (Table 19). Although not shown here, the pattern above was consistent across the five survey years of 0, 1, 5, 10, and 20. Across the years examined, when the direct estimate of the reference year was compared to the Combined MAC/MAP estimate, the difference was greater than 10% of the direct estimate only in three of the 100 simulations in survey year 20.

Figure 13: Average estimates across 100 simulations comparing direct and MAC/MAP Combination method using only data from Year 0 – baseline scenario

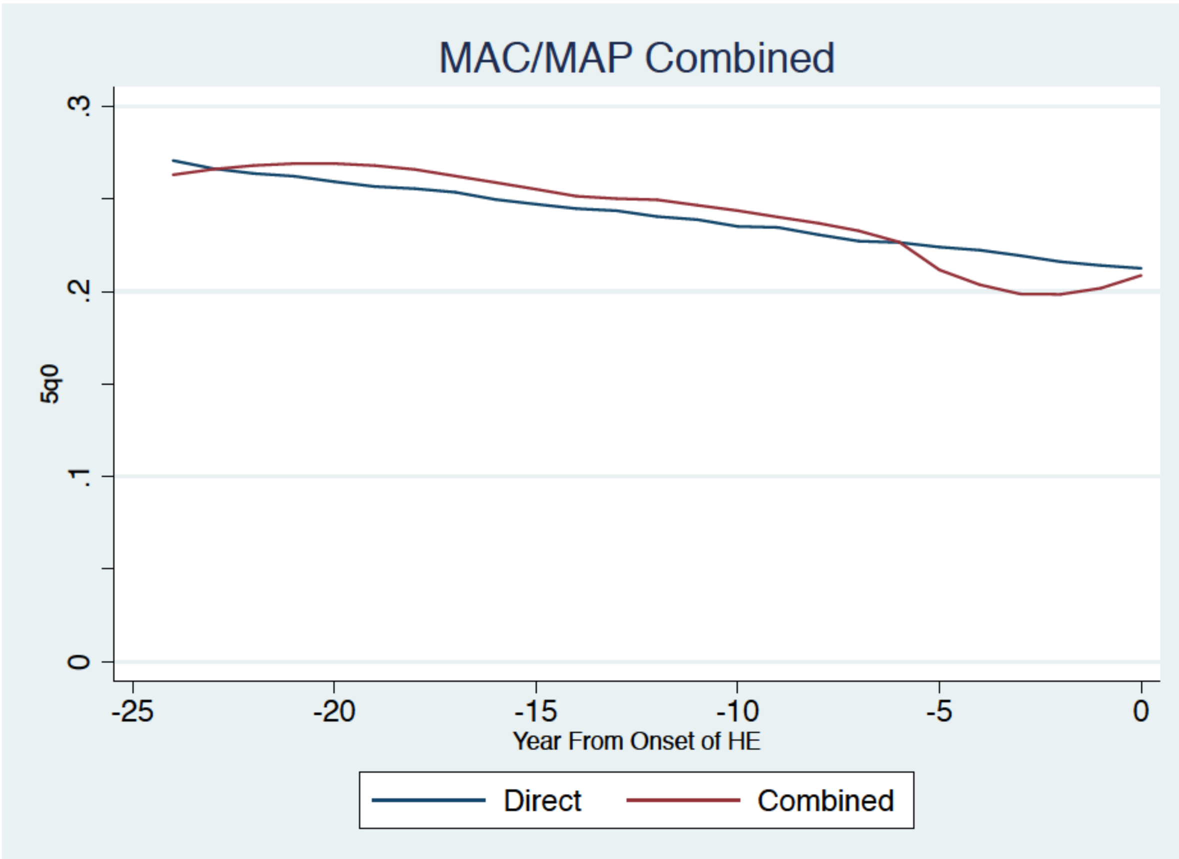


Table 19 below shows the average difference of the direct estimate from the yearly estimates generated from both versions of the combined methods (using all four methods and only the two maternal age methods) using data from Year 0. In almost every year, the combination of all methods differs from the average direct estimate by over 10 deaths per 1,000 live births, while the MAC/MAP method differs by more than 10 deaths in 6 out of 25 years (shaded numbers in Table 19, below). Importantly, for the most recent reference period, .5 years, before the survey, the MAC/MAP combined estimate differs by only 4 deaths per 1,000 live births from the direct estimate.

Table 19: Mean difference (per 1,000 live births) of direct estimates from All Methods Combined and MAC/MAP combined for reference years using data from Year 0 - baseline scenario

Year	*All methods Combined*	*MAC/MAP Combined*
-24	-26	-8
-23	-4	0
-22	16	4
-21	35	7
-20	53	10
-19	69	11
-18	81	10
-17	87	9
-16	84	9
-15	56	8
-14	27	7
-13	26	7
-12	36	9
-11	55	8
-10	53	9
-9	47	6
-8	40	6
-7	30	6
-6	35	0
-5	34	-12
-4	25	-19
-3	22	-21
-2	20	-18
-1	16	-12
0	14	-4

Table 20 compares the average estimates derived for Year 0 by each method, the direct, Brass, the unsmoothed MAC, TFBC, MAP, TFBP, and the smoothed MAC, TFBC, MAP, TFBP and the two smoothed, combined methods. It also includes the average reference period between Year 0 and the year in which the survey was conducted; for example, the estimate of child mortality for Year 0 would be derived from a survey conducted approximately six years later. The unsmoothed MAC and

TFBC methods do not generate one point estimate for Year 0 so the age group of 25-29, which has a comparable reference period to the Brass estimate (5.8 versus 6.2), and the time since first birth cohort group of 15-19 (reference period of 6.0) are shown below.

The unsmoothed MAP method, on average, gives the closest estimate to the direct method, differing by less than .001, and derives the estimate for the same year as the survey. The Brass method overestimates the direct estimate by, on average, 3 deaths per 1,000 live births, using data from Year 6 to estimate mortality in Year 0. Finally, the combination of the MAC/MAP method underestimates the direct method by an average of 4 deaths per 1,000 live births using data from Year 0. The smoothed cohort methods show consistent underestimation when using the MAC method and overestimation using the TFBC method, and both use data derived from surveys after Year 0. In the case of the MAC method, Year 0 estimates can first be derived using data 3.5 years after Year 0, while in the case of the TFBC method, Year 0 estimates can be derived using data from 1.5 years after Year 0. The method that combined all four methods performs better than either of the individual cohort methods or the TFBP method, does not perform as well as the combination of only the MAC and MAP methods.

Table 20: Comparison of averages across 100 simulations of estimates derived from all methods using data from Year 0 - baseline scenario

			Unsmoothed				Smoothed				Combined	
	Direct	Brass	MAC	TFBC	MAP	TFBP	MAC	TFBC	MAP	TFBP	All	MAC/ MAP
Estimate	.212	.215	.192	.314	.213	.188	.166	.291	.209	.191	.227	.209
Reference Period	-	6.2	5.8	6.0	0	0	3.5	1.5	0	0	0	0
Mean difference Indirect-Direct	-	.003	-.020	.102	<.001	-.025	-.054	.079	-.004	-.022	.014	-.004
Mean absolute difference Indirect-Direct	-	.007	.020	.102	.001	.025	.054	.079	.006	.022	.015	.007

Based on the above table, only the estimates derived from the MAC, the unsmoothed MAP estimates, the smoothed MAC/MAP methods, and the Brass estimates will be further explored in the context of humanitarian emergencies. However, because the MAC method consistently underestimates mortality in years recent to the survey and provides reliable estimates only after approximately 15 years, it will not be shown in Table 21 below, which summarizes the estimates that are derived using data from survey years Year 0, Year 1, Year 5, Year 10, and Year 20. The table compares the direct estimate for the survey year and the estimates for child mortality that are derived using data from that year and the corresponding reference period.

For example, in Year 0, the direct estimate is .212. Using data from Year 0, the Brass method estimates 5q0 of .230 for a reference period 6.27 years prior to the survey. In that year, the direct estimate was estimated to be .226. The Brass method then overestimated the corresponding direct estimate by 4 deaths per 1,000 live births and the average of the absolute differences was .007 or 7 deaths per 1,000 live births. When there are no disruptions in mortality, each of the methods performs well, with a maximum difference from the direct estimate of 6 deaths per 1,000 live births (unsmoothed MAP estimate). Both the MAP estimate and the MAC/MAP estimate are able to predict mortality within .5 years of the survey year while the Brass estimate is able to accurately predict mortality, but only for time periods 5-6 years prior to the survey.

For each of the five years, Table 21 also summarizes the number of simulations in which the Brass estimate and the direct estimate differ by 10% and 20% of the direct estimate for the reference year. In the case of linearly declining mortality, differences were greater than 10% a maximum of 5 times. In no simulation across the five time periods, did the estimates differ by more than 20%.

Table 21: Comparison of direct and selected indirect methods for five time points (n=100) - baseline scenario

	Time Point and Estimate														
	Year 0 Direct Estimate: .212			**Year 1 Direct Estimate: .210**			**Year 5 Direct Estimate: .201**			**Year 10 Direct Estimate: .190**			**20 Direct Estimate: .167**		
Measure	Brass	MAP – Ref .5	MAC/ MAP	Brass	MAP – Ref .5	MAC/ MAP	Brass	MAP – Ref .5	MAC/ MAP	Brass	MAP – Ref .5	MAC/ MAP	Brass	MAP – Ref .5	MAC/ MAP
Reference Year	-6	0	0	-5	1	1	-1	5	5	0	6	6	4	10	10
Direct 5q0 – Reference Year	.226	.212	.212	.224	.210	.210	.214	.201	.201	.204	.190	.290	.182	.167	.167
Indirect 5q0 – Reference Year	.230	.212	.209	.228	.211	.207	.218	.202	.199	.205	.192	.189	.186	.173	.170
Average of differences Indirect-Direct	.004	.000	-.004	.004	.001	-.003	.004	.002	-.001	.001	.002	-.001	.004	.006	.004
Average of absolute differences	.007	.006	.006	.008	.005	.006	.008	.005	.006	.007	.005	.005	.007	.007	.006
% where differences greater than 10%	3	0	0	4	0	0	1	0	0	1	0	0	5	0	3
% where differences greater than 20%	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0

Humanitarian Emergencies 1-4

There are three aspects to determining how well a method performs in humanitarian emergency that will be explored here; 1) how quickly an increase mortality can be detected by the method, and similarly how long after an emergen begins must the researcher wait to obtain estimates for the crisis period; 2) hc well the method actually predicts the level of mortality in an emergency and final 3) how long after a crisis period ends will estimates from a method be biased previous increases in mortality.

Four mortality patterns were simulated for the humanitarian emergencies: a hi but short spike in mortality characterizes Humanitarian Emergency 1 (HE 1), a m increase in mortality over a five-year period is simulated in HE 2, high probabiliti over a period of 3 years are applied in HE 3, and HE 4 is modeled by inducing o year spikes in mortality, followed by five years of decline, over a period of 18 year

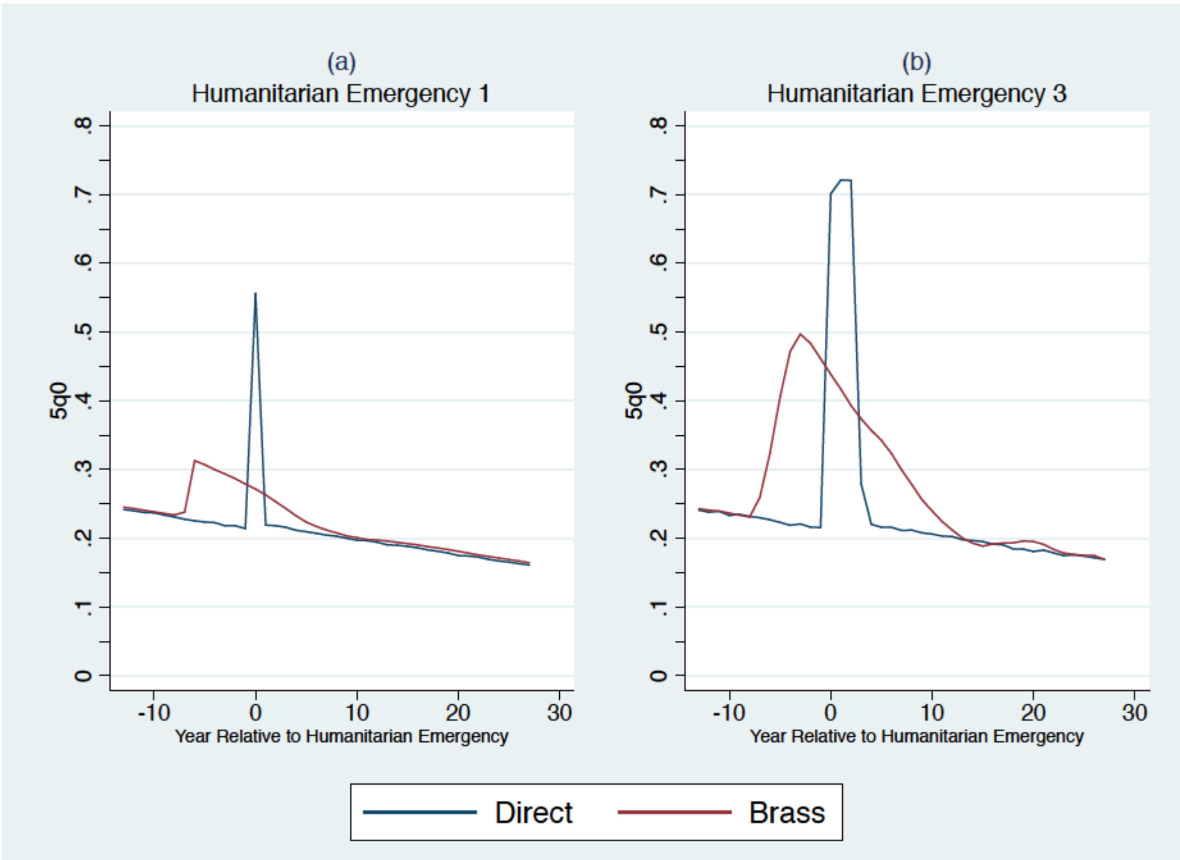
Based on the comparisons of methods in the baseline scenario, particularly t distribution of the differences between the direct and the method specific estimat I have chosen to show here only the comparisons of Brass, MAC, MAP, and t combined MAC/MAP methods. Although the MAC method did not perform as w as either the Brass or MAP method in the baseline scenario, because it is used in t combined MAC/MAP estimator, the preferred method of analysis according IHME, I will show how well it performs in the Humanitarian Emergency scenarios.

Brass

Figure 14 and Figure 15 (below) depict the patterns of mortality in each emergency scenario. Due to the differences in scale between HE 1/HE 3 and HE 2/HE 4, I show the patterns separately initially, allowing for a better understanding of the patterns seen when mortality increases are less extreme. In Figure 14, HE 1 and HE 3 are shown on a scale of 0 to .8. This is equivalent to an 80% probability of a child dying by their fifth birthday. This is an extremely high level of mortality and cannot really exist for extended periods of time, however these scenarios are shown here to demonstrate the ability of each method to estimate extreme shifts in mortality. Although HE 1 and HE 3 had similar mortality parameters programmed initially, the mortality increase in HE 1 appears lower that in HE 3. This is a result of the increased probability for death being applied for only a two-month period in HE 1 but 5q0 being estimated over an annual period, masking the extreme level of mortality that might be expected.

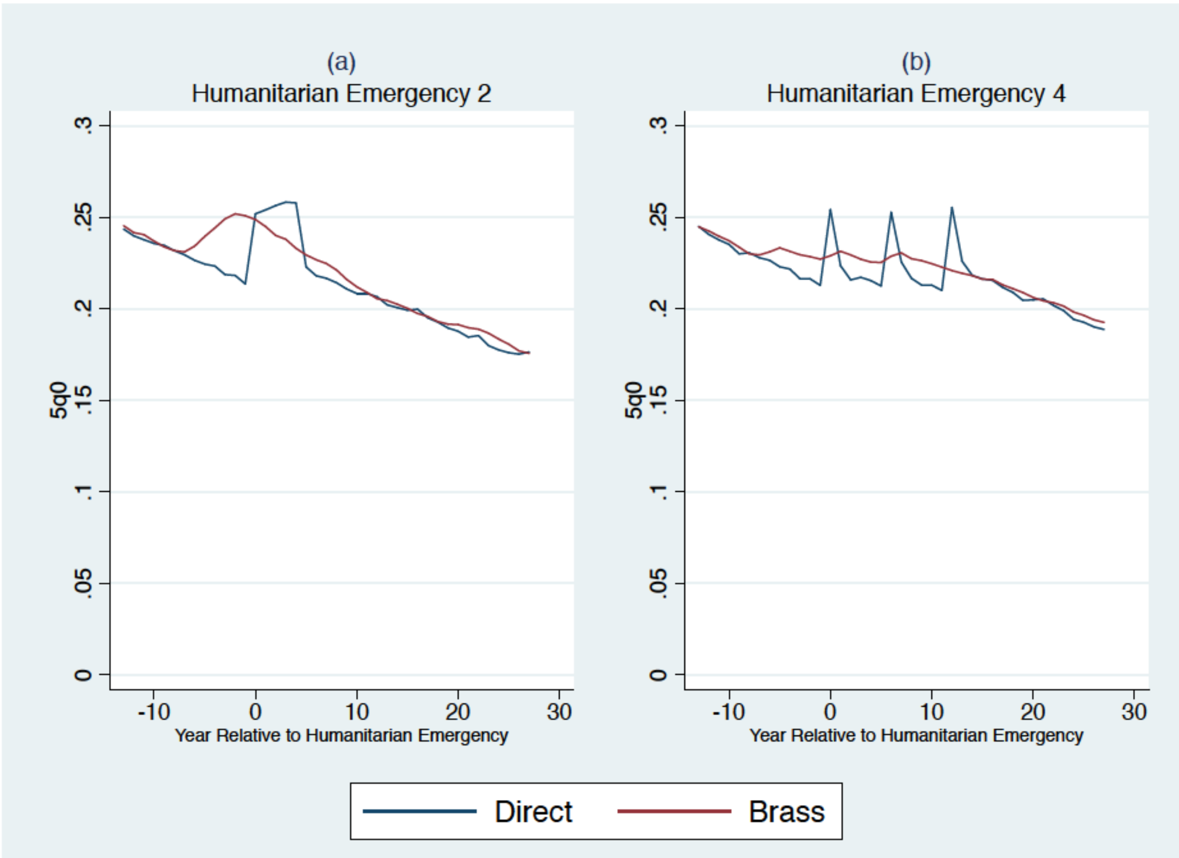
In all of the scenarios, when Brass estimates are adjusted to the reference year, the increase in mortality appears to have occurred between five and ten years before the actual increase in mortality and the estimates during the crisis are considerably less than the actual estimate. While the abrupt increase in mortality in HE 1 and HE 3 are partially captured by the Brass method, the equally abrupt return to lower mortality is not. Rather, the Brass method shows a slow decline in child mortality over a ten-year period in the case of HE 1 and a twenty-year period in HE 3.

Figure 14: Average across 100 simulations of 5q0 estimates derived from Brass and direct methods in Humanitarian Emergency situations 1 and 3



In HE 2, which has elevated mortality for five years, the Brass estimator shows a more gradual increase and decrease in under-5 mortality than actually occurs (Figure 15). Finally, in HE 4, the method is not able to differentiate between fluctuations over time. Rather, the Brass method predicts an almost constant mortality level across the fifteen years of crisis, remaining slightly elevated prior to the crisis and very close to the direct estimates after the crisis.

Figure 15: Average across 100 simulations for 5q0 estimates derived from Brass and direct methods in Humanitarian Emergencies 2 and 4



If the data are not centered to the reference year, the increase in mortality is generally closer in time to the true crisis period (Figure 16). For example, in HE 1 above, when the data was appropriately centered to the reference year, the increase in mortality estimates started approximately seven years prior to the crisis period. When the data is not centered, the increase in mortality begins during the crisis period. However, when this is done, the method consistently overestimates mortality in the subsequent non-crisis period.

Figure 16: Average across 100 simulations for 5q0 estimates derived by direct and Brass methods for four humanitarian emergency scenarios – not adjusted to reference year

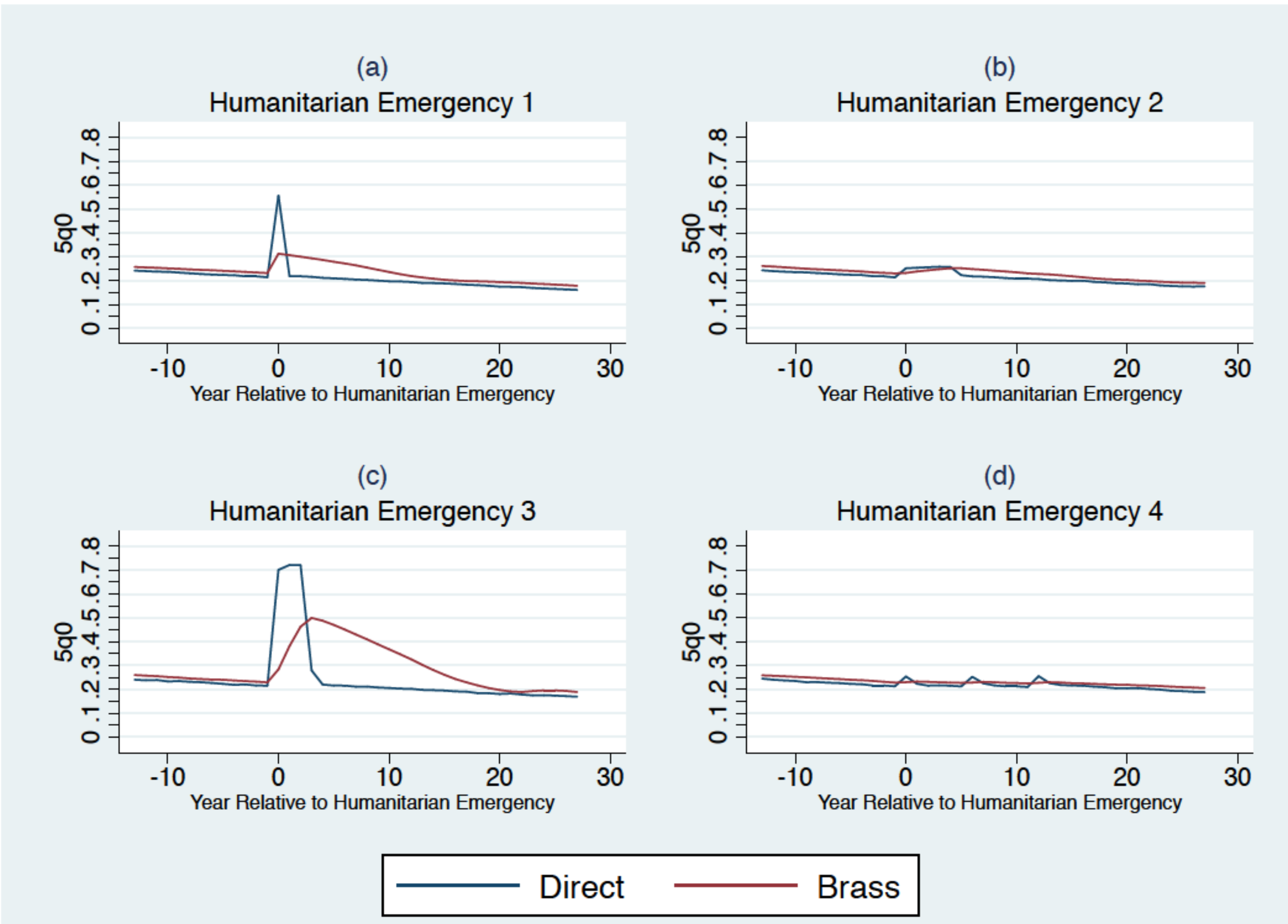


Table 22 below summarizes the comparison of the Brass estimates to the direct estimate for the four humanitarian emergency scenarios using data from Year 0 and the estimates that are derived for Year 0, using data from Year 6. When data are used from the first year of the crisis period, the Brass method overestimates mortality in the reference period. This overestimate is strongest when mortality spiked and drops in a short time period (HE 1). When mortality is increased for a longer duration, the increase in mortality estimated by Brass is more gradual (as seen in Figure 14, above and Table 22, below). When estimating mortality that took place in approximately Year 0, using data from Year 6, the methods all underestimate the true amount, except in the case of Humanitarian Emergency 2. In this case, only 9 simulations differed by more than 10% of the direct estimate.

Table 22: Average of 100 simulations comparing direct and Brass estimate for Year 0 and Year 6 in four humanitarian emergency scenarios

	Year of 'Survey' and HE model							
	Year 0 Data				**Year 6 Data**			
Measure	**HE 1**	**HE 2**	**HE 3**	**HE 4**	**HE 1**	**HE 2**	**HE 3**	**HE 4**
Reference Period	**6.29**	**6.48**	**6.52**	**6.48**	**6.21**	**6.21**	**6.48**	**6.32**
Direct 5q0 – Reference Year	.226	.224	.225	.227	.556	.251	.701	.254
Indirect 5q0 – Reference Year	.314	.233	.284	.232	.272	.250	.455	.229
Average of differences Indirect-Direct	.088	.009	.061	.005	-.284	-.001	-.245	-.024
Absolute value of differences	.081	.008	.003	.006	.276	.01	.125	.02
% where differences greater than 10%	100	17	100	20	100	9	100	48
% where differences greater than 20%	100	5	57	0	100	0	100	3

Finally to see how long the Brass method is affected by disruptions in mortality, Table 23 (below) summarizes the survey year in each humanitarian emergency in which 90% of the average Brass estimates differed by less than 10% from the direct estimate for the corresponding reference year and the corresponding reference period for that time.

Table 23: Survey year and reference period in which 90% of simulations differ by less than 10% from direct estimate

	HE 1	*HE 2*	*HE 3*	*HE 4*
Survey Year	13	16	15	18
Reference Period	6.42	6.51	6.24	6.41

MAC, MAP, and Combined

Figure 17 and Figure 18 show estimates derived from the MAC methods over the 40-year time interval for each scenario. Figure 17 summarizes the MAC results for HE 1 and HE 2, and Figure 18 does the same for HE 2 and HE 4, showing the average point estimates for each age group across the 100 simulations for each year.

Before smoothing, the MAC method captures an abrupt uptick in mortality in HE 1 and HE 3 but centers the increase prior to the emergency, similar to the Brass method. The sharp increase in mortality is captured best by the age groups 15-19 and 20-24, which center the increase approximately one and three years prior to the emergencies, respectively. The levels estimated from these two age groups largely underestimate the direct estimate, however. In HE 2, the youngest age groups capture some increase in mortality, but the fluctuations are largely lost in the older age groups. When the MAC methods are used in HE 4, there is very little fluctuation in the estimates, missing the sharp increases over time.

Figure 17: Average across 100 simulations of MAC 5q0 estimates and direct estimates in two high mortality emergency scenarios

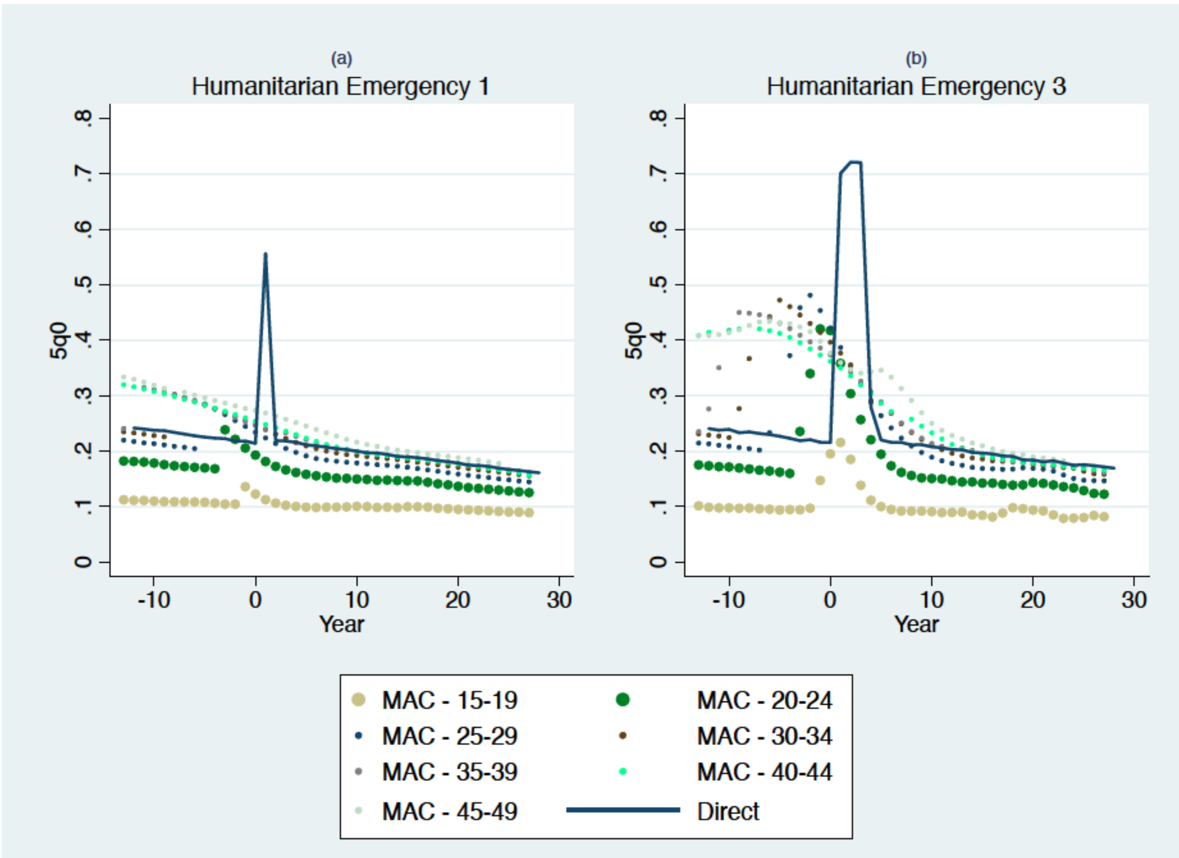


Figure 18: Average across 100 simulations of MAC 5q0 estimates and direct estimates in two low mortality humanitarian emergencies

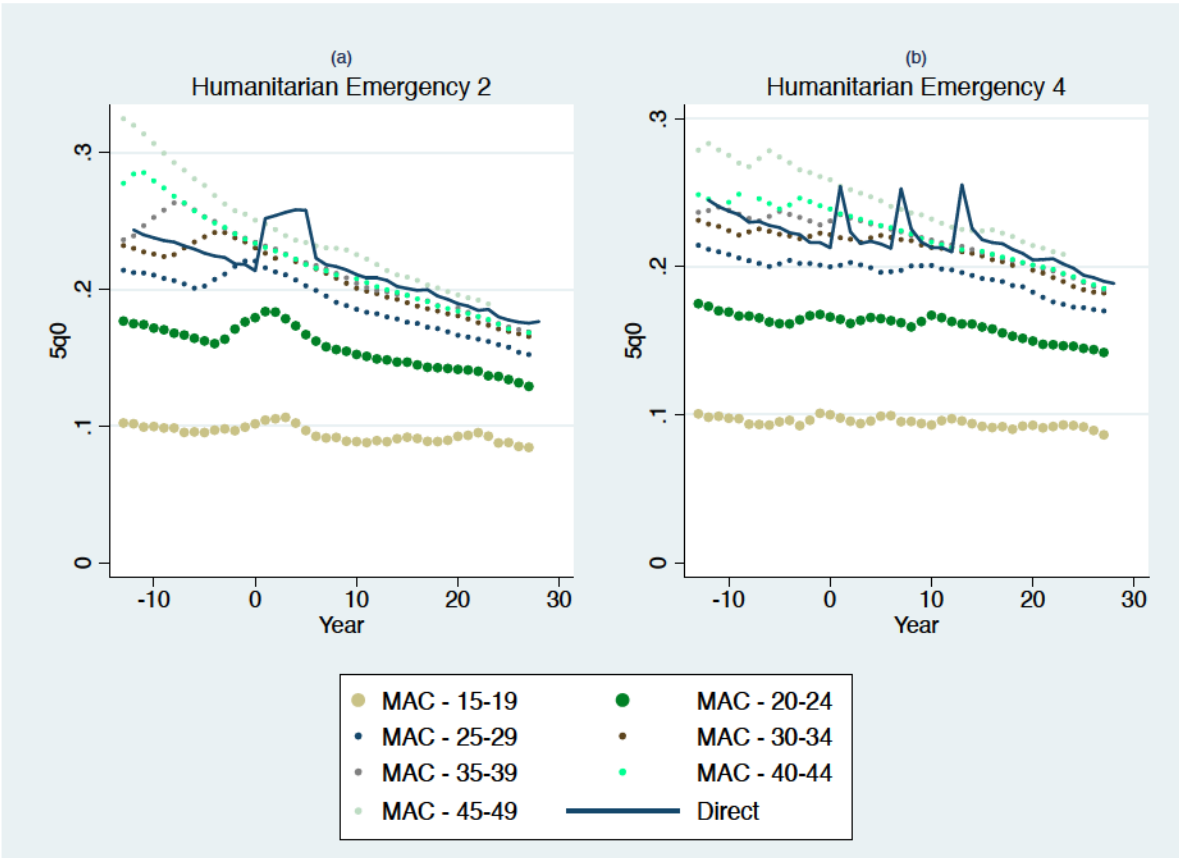


Figure 19 and Figure 20 (below) show the corresponding MAP estimates derived from each scenario. The MAP methods, particularly for the reference periods of .5 and 1.5 years prior to the survey, capture the increases in mortality across each of the humanitarian emergencies, but the estimates in HE 1 and HE 3 are significantly below the direct estimates during the crisis period. In HE 1, the MAP method returns to baseline levels by year 10, but in the HE 3 scenario, continues to overestimate the direct estimates through the end of the evaluation period. For HE 2 and HE 4, the MAP method overestimates the mortality rate at the end of the crisis period and continues to slightly overestimate 5q0 through the end of the simulation period, although the values are not as variable as the other scenarios (Table 26 - Table 29, below).

Figure 19: Average across 100 simulations of estimates for three reference period derived using MAP methodology and average direct estimates in two high mortality humanitarian emergency scenarios

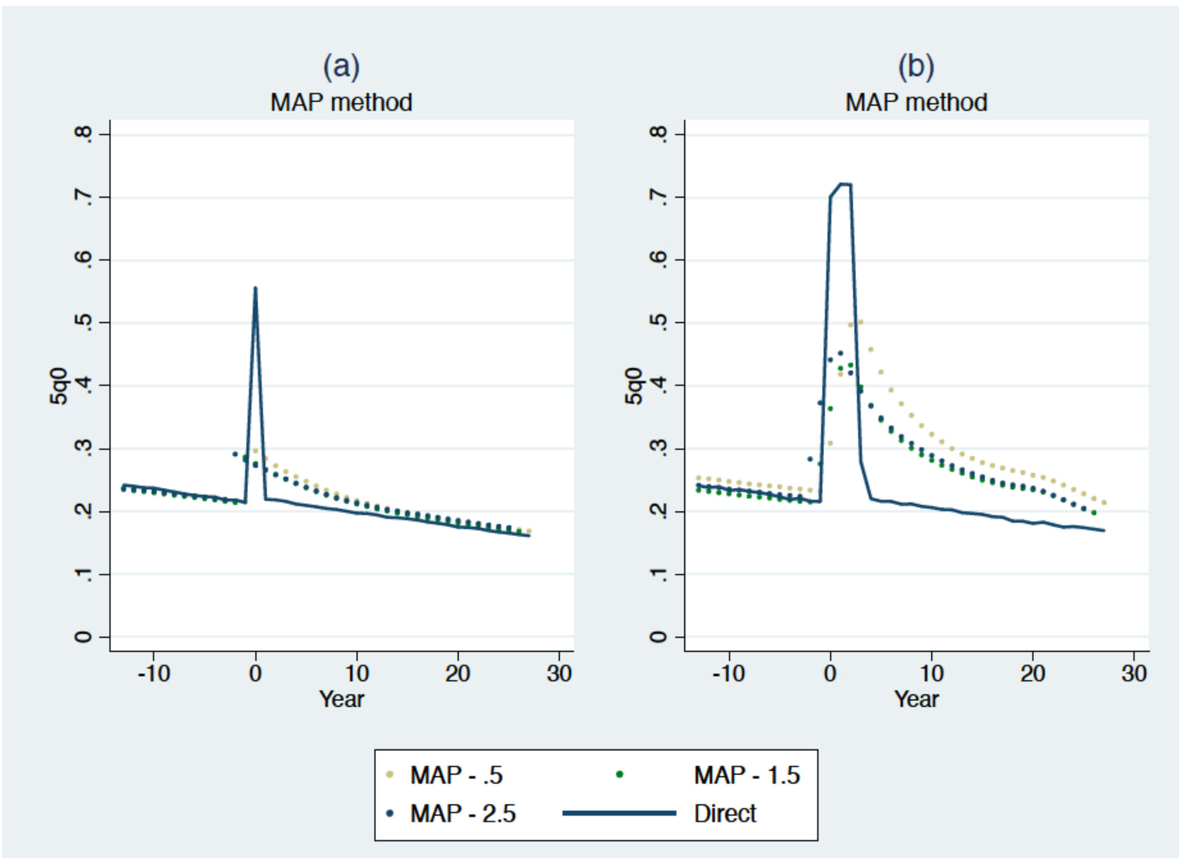


Figure 20: Average across 100 simulations of estimates for three reference periods derived using MAP methodology and average direct estimates in two low mortality humanitarian emergency scenarios

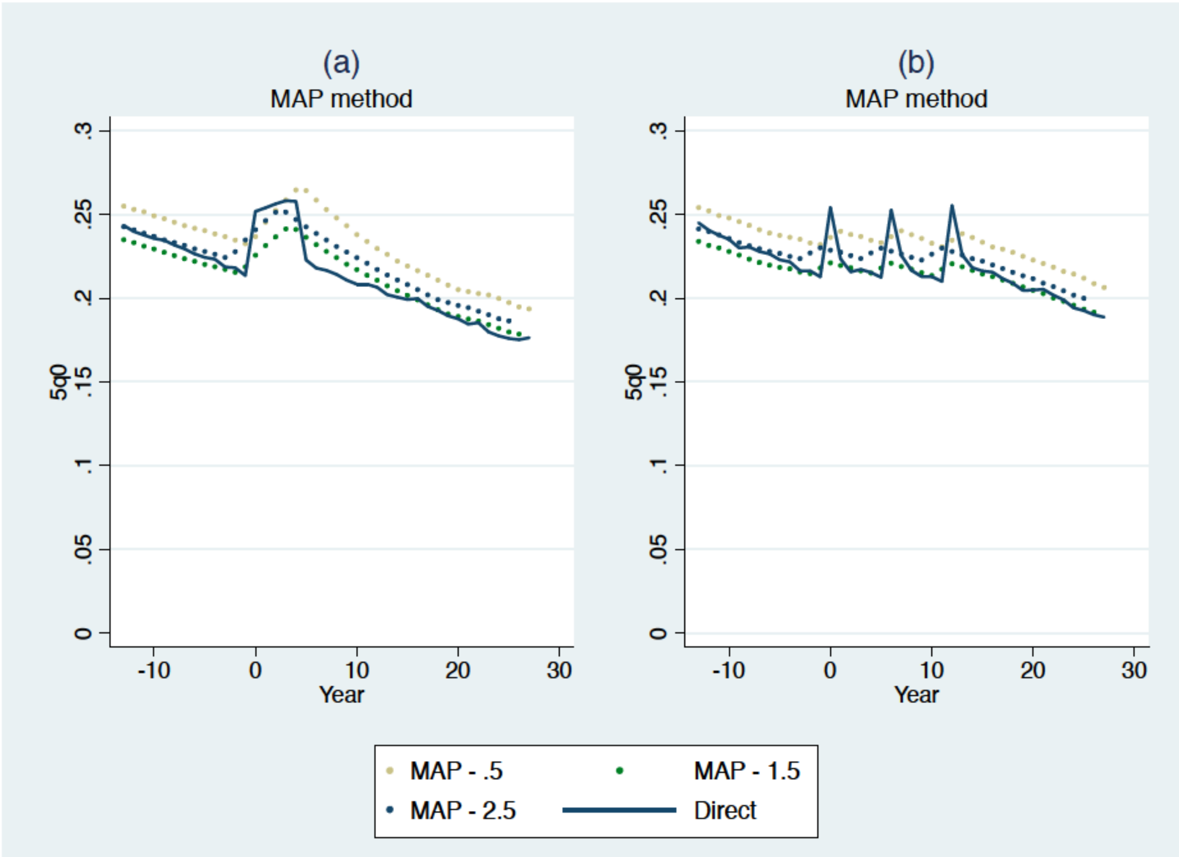


Table 24 below shows the point estimates for a reference period of .5 years using data from Year 0, the crisis period, and Year -1, the year prior. In HE 1, when the increase in mortality is sudden and for a short duration, in the year prior to the emergency, the mean MAP estimate differs from the mean direct estimate by less than 1 death per 1,000 live births. The difference is greater for HE 3 (.017), but is still less than the differences for HE 2 and HE 4. When mortality increases suddenly in Year 0, the MAP estimate does capture some of the increase in HE 1 and HE 3, increasing by .081 and .076, respectively, but it does not capture the true extent of the increase (.342 and .485, respectively). When the mortality increase is of a longer duration, such as in HE 2 and HE 4, the MAP estimate overestimates mortality in the year prior to the crisis period, by 19 deaths per 1,000 live births and 20 deaths per 1,000 live births respectively, and does not show an appreciable increase in mortality in the following year, increasing by only 4 deaths per 1,000 live births in both scenarios.

Table 24: Average of 100 simulations comparing direct and MAP estimates (reference year .5) for Year -1 and Year 0 across four humanitarian emergency scenarios

	Year -1				*Year 0*			
	HE 1	**HE 2**	**HE 3**	**HE 4**	**HE 1**	**HE 2**	**HE 3**	**HE 4**
Direct 5q0	.214	.214	.216	.213	.556	.251	.701	.254
Indirect 5q0	.215	.233	.232	.232	.296	.237	.308	.236
Average of differences Indirect-Direct	<.001	.019	.017	.020	-.260	-.015	-.392	-.019
Absolute value of differences	<.001	.019	.017	.021	.261	.017	.389	.013
% where differences greater than 10%	0	45	37	45	100	23	100	28
% where differences greater than 20%	0	4	0	8	100	0	100	0

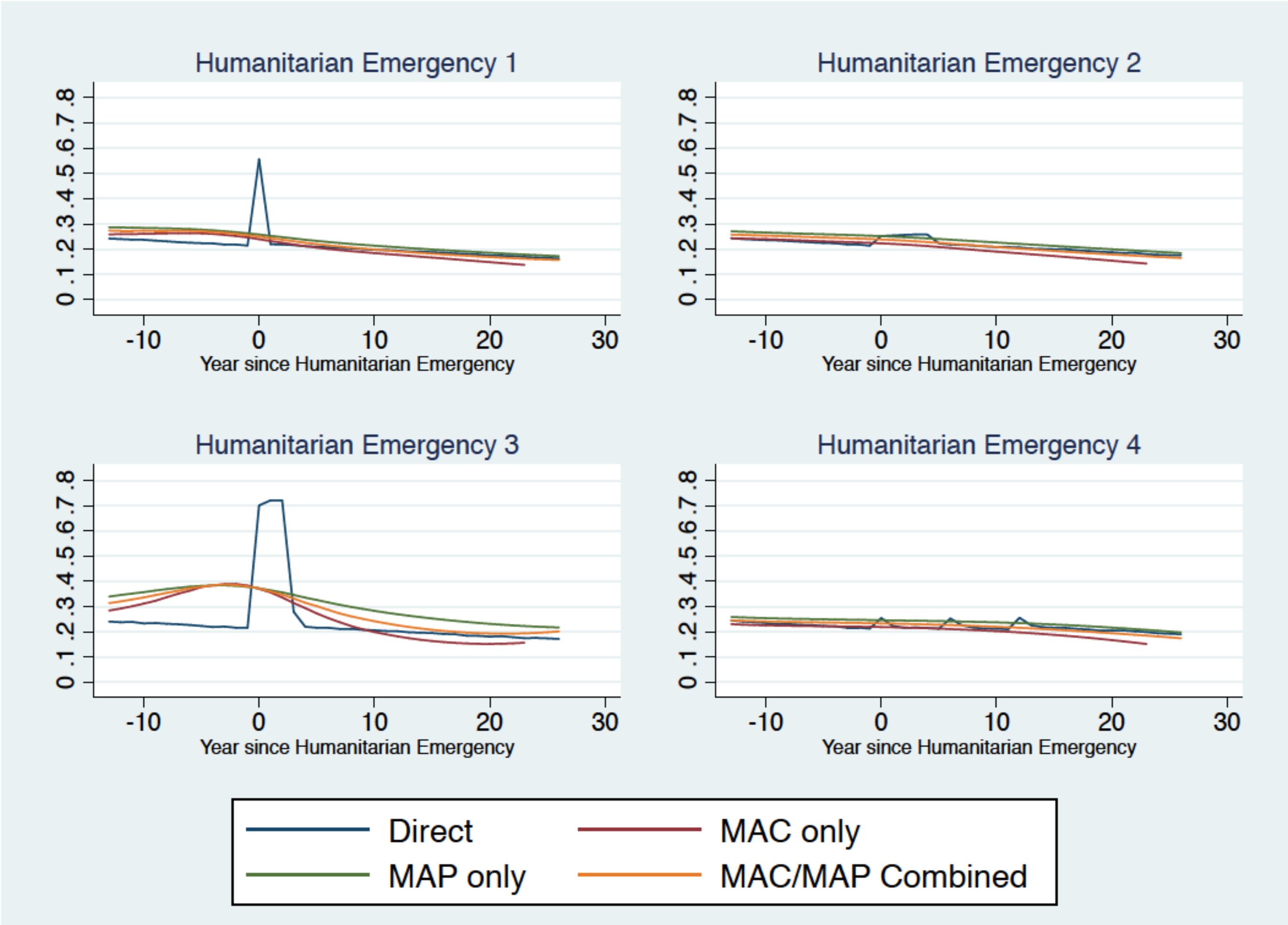
As with the baseline scenario, the estimates from the MAC and MAP methods were smoothed using the Loess smoother to show the trend predicted by the IHME method, rather than the specific point estimates. In Figure 21 below the smoothed MAC and MAP estimates and the smoothed combined estimates are shown along with the direct estimate[3].

[3] Point of clarification. For each individual survey year, the IHME methods will predict a trend after smoothing by the Loess. Thus the Loess smoother when applied to all data effectively combines all of

When the MAC and MAP methods are smoothed using the Loess smoother, the fluctuations in mortality that are captured by the non-smoothed point estimates are almost entirely eliminated. Particularly problematic is the scenario in HE 4, where none of the methods show fluctuating mortality, instead showing constant or slowly declining mortality over time.

the smoothed data trends over time. However, the direct estimate generates only one point estimate for child mortality. While it may appear that the direct method is smoothed, it is actually the connection of the individual annual averages over time.

Figure 21: Comparison of direct, ,smoothed MAC, smoothed MAP, and smoothed MAC/MAP combined across four humanitarian emergencies – data from all years



Error! Reference source not found. below summarizes how well the smoothed IHME estimates predict the mortality levels and the timeliness of the estimates in the four emergencies. All of the methods are able to predict a peak in mortality within five years of the crisis period for HE 3, although all methods underestimate the peak in mortality by almost 50%. Only the smoothed MAC method is able to detect any increase in mortality over the simulation period, predicting an increase in mortality at Year -7. The smoothed MAP and smoothed MAC/MAP method do not show any increase in mortality over the time period for HE 1. In HE 2 and HE 4, the increase in mortality is completely obscured by the smoothing for all methods, with none of the variants predicting an increase in mortality over the simulation period.

Table 25: Comparison of peak mortality year and level derived from direct method, smoothed MAC, smooth MAP, and MAC/MAP combined

	Peak Mortality Year	Peak Mortality Estimate
Direct Method		
HE 1	0	.556
HE 2	4	.556
HE 3	1	.721
HE 4	12	.255
MAC Smoothed		
HE 1	-7	.263
HE 2	-13	.242
HE 3	-2	.390
HE 4	-13	.230
MAP Smoothed		
HE 1	-13	.287
HE 2	-13	.271
HE 3	-3	.384
HE 4	-13	.259
MAC/MAP Smoothed		
HE 1	-13	.273
HE 2	-13	.258
HE 3	-3	.387
HE 4	-13	.245

Figure 21 show the patterns that arise when data from every year are included in the Loess smoother. However, it is more likely that data from only one or two survey rounds would be available to a researcher. Thus, Figure 22 - Figure 25 show how well the smoothed methods predict mortality when only one year of data is used. The data that re used in the figures below are from Year 0, Year 1, Year 5 and Year 10 and demonstrate both how well smoothed estimates predict the point estimate for the given year and the trend over time as mortality declines from crisis levels.

Beginning with HE 1 (Figure 22) when the analysis is restricted to data from only one year, the MAC, MAP, and MAC/MAP Combined overestimate the under-5 mortality rate trend. At both Year 0 and Year 1, the estimates for the survey year and the predicted trend prior to the crisis period are elevated, however, they do not reflect the sharp increase of the emergency itself. By Year 5, the estimates for the survey year derived from the MAP and the MAC/MAP combined methods slightly overestimate the direct estimate and by Year 10 are comparable to the direct estimate. The trend of child mortality in the past, however, is consistently overestimated. After the crisis period, the MAC method consistently underestimates 5q0 up to five years prior to the survey and overestimates 5q0 in the more distant past.

In HE 2 (Figure 23), the MAC estimate consistently underestimates mortality for the survey year and approximately five years previously. In Year 0, when mortality has not been raised for a long period of time, the trend that is predicted by the MAC method differs from the direct estimate on average by less than 10% for all survey years prior to Year -5. However, after Year 0, when the increase in mortality remains sustained, the predicted MAC trend is elevated above the direct estimates. This pattern is repeated in the MAP and MAC/MAP. While the estimate for the survey year predicted by the MAC methods is always an underestimate, in Year 5, the estimates predicted by both the MAP and the MAC/MAP combined method (.251 and .252, respectively) are an overestimate of the direct estimate (.210). In Year 10, 10 years after the crisis period, both methods continue to predict estimates above the direct estimate of .208, both by approximately 18 deaths per 1,000 live births.

The large and sustained increase in mortality seen in HE 3 leads to significant overestimates in the trends predicted by all of the methods when data from only one year is used, except during the crisis period itself (Figure 24). The smoothing of the estimates masks most of the increase in mortality during the crisis period itself and leads to consistent overestimation over time. As with the other methods, the MAC method, when smoothed, is unreliable close to the survey period. While the MAP and MAC/MAP Combined method return to baseline levels by Year 10 in the HE 1 scenario, the levels remain too high even 10 years after the crisis in HE 3. By Year 10, when the direct method estimates mortality at .206, the smoothed MAP and MAC/MAP estimates are .302.

The increase in mortality in HE 2 and HE 4 in Year 0 is the same, and this is reflected in the similarity of patterns of the MAC, MAP, and MAC/MAP Combined method derived from Year 0 in both scenarios (Figure 23 and Figure 25). The levels estimated using the smoothed MAP in Year 0 for HE 2 and HE 4 are .227 and .226, respectively, while the levels estimated using MAC/MAP Combined is .226 for both scenarios. When mortality remains elevated, such as in HE 2, the methods continue to underestimate mortality. In Year 1, 32% of the MAC/MAP estimates differ by more than 10% from the direct estimate. However, if mortality decreases, such as in HE 4, the smoothed MAP estimate differs by only 6 deaths per 1,000 live births and only 12% of the MAC/MAP estimates differ by 10% or more. However, the smoothed methods are not able to capture fluctuation. When mortality increases again in Year 6, 50% of the smoothed MAP method and 52% of the smoothed combined estimates differ by more than 10% from the direct estimate.

Figure 22: Comparison of direct, smoothed MAC, smoothed MAP, and MAC/MAP combined using data from specified survey year only – Humanitarian Emergency 1

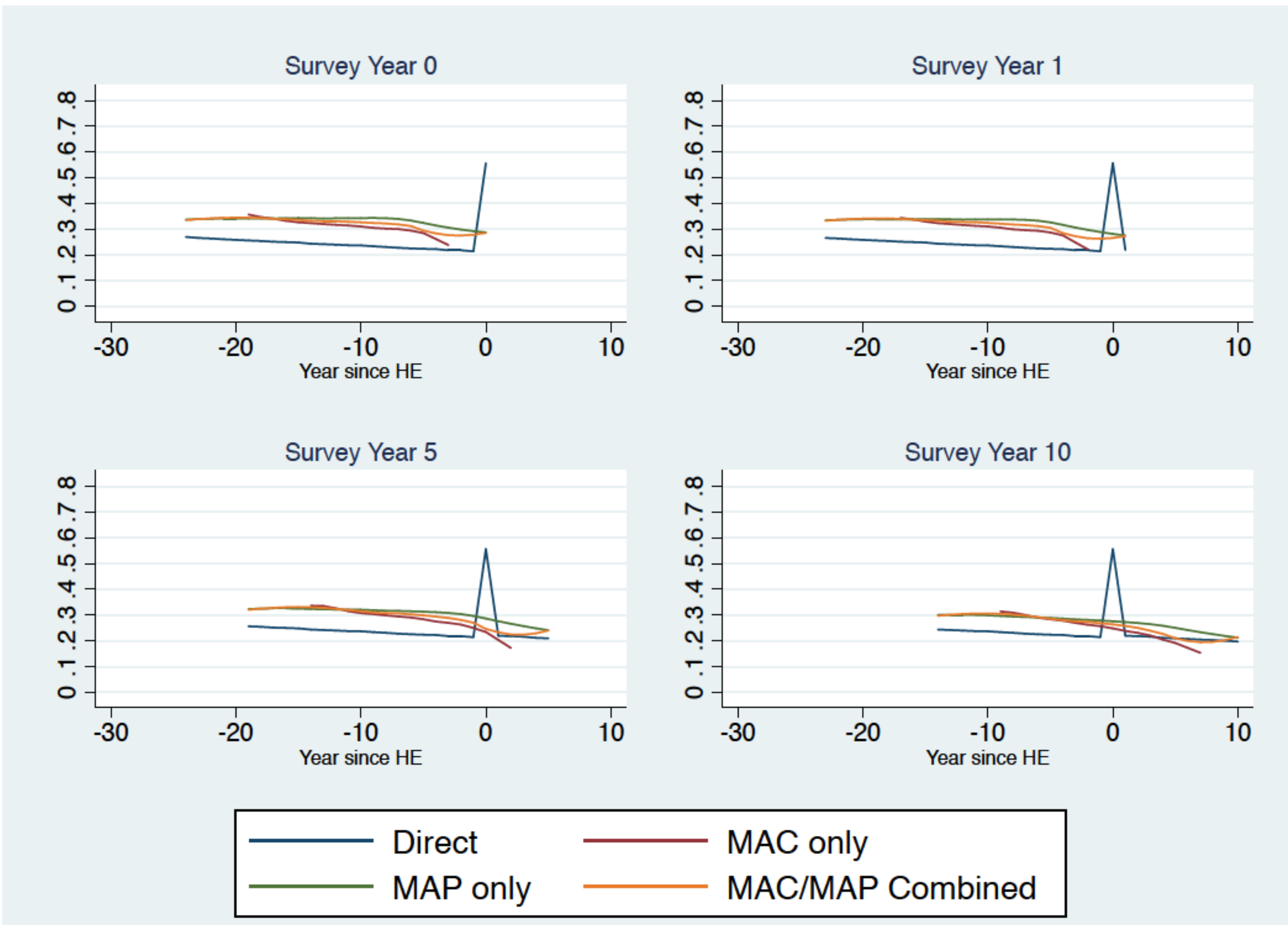


Figure 23: Comparison of direct, smoothed MAC, smoothed MAP, and MAC/MAP combined using data from specified survey year only – Humanitarian Emergency 2

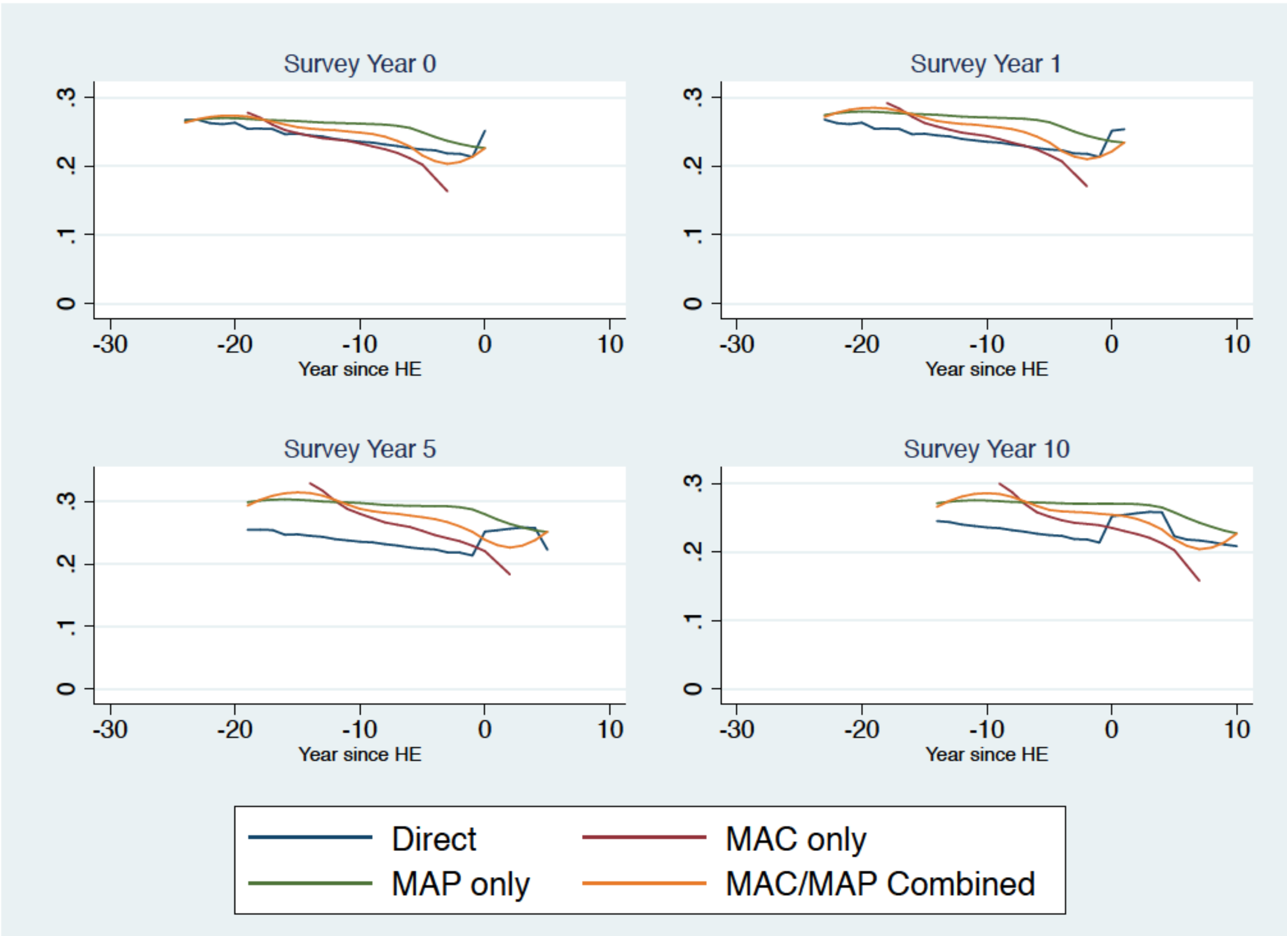


Figure 24: Comparison of direct, smoothed MAC, smoothed MAP, and MAC/MAP combined using data from specified survey year only – Humanitarian Emergency 3

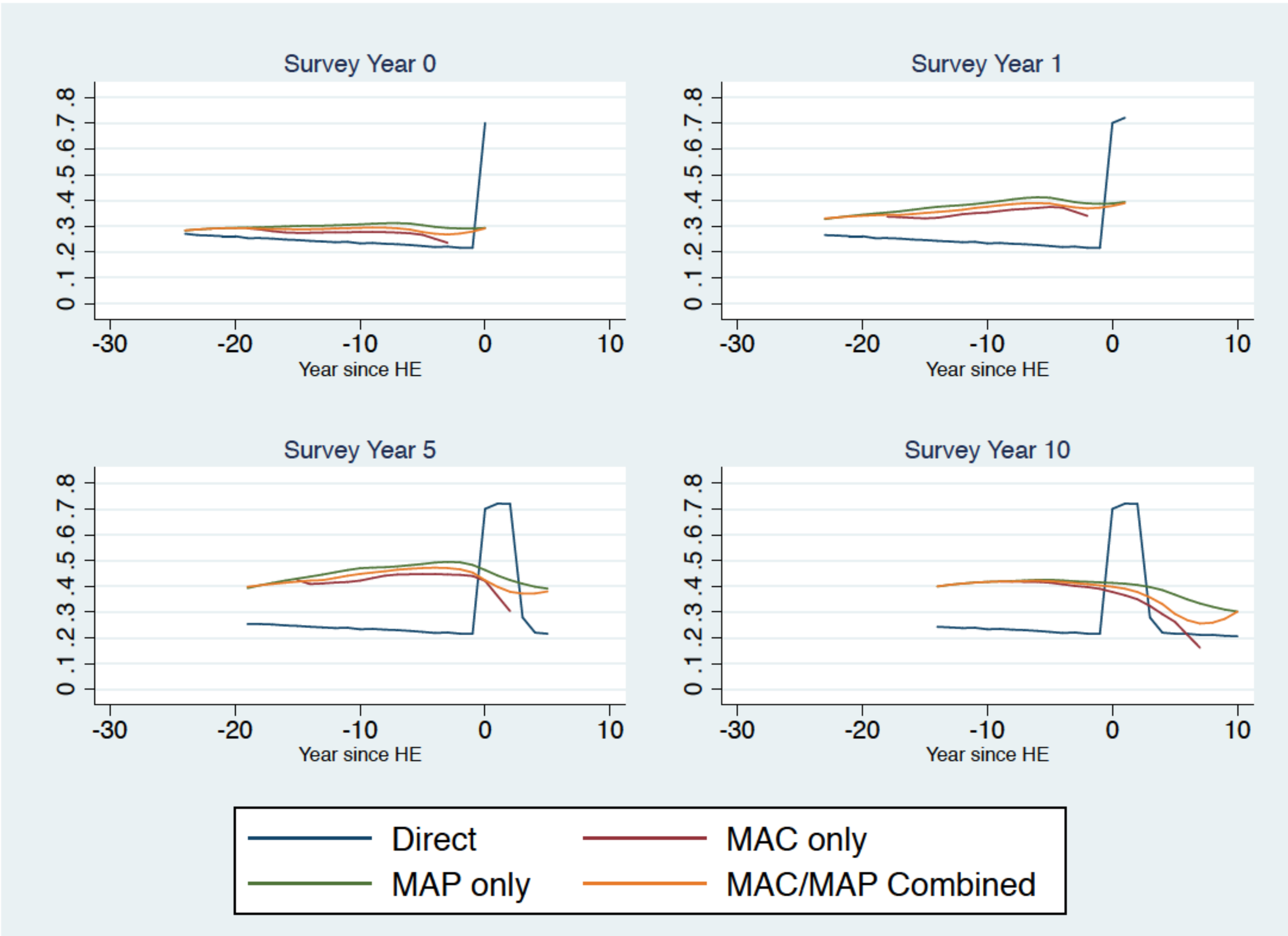
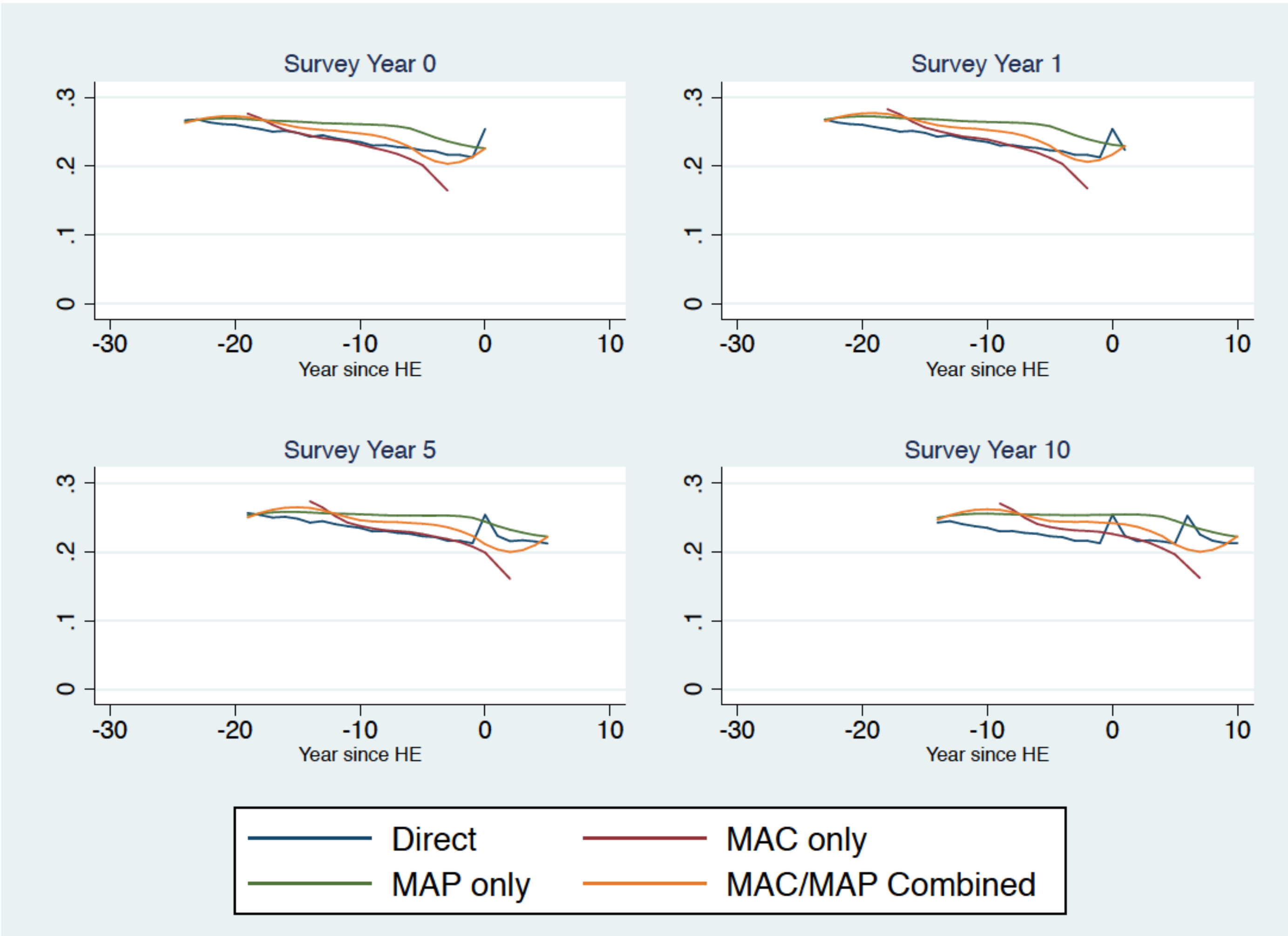


Figure 25: Comparison of direct, smoothed MAC, smoothed MAP, and MAC/MAP combined methods using data from specified survey year only – Humanitarian Emergency 4



After smoothing, the MAP and the MAC/MAP combined methods generate very similar estimates. However, when the MAP method is left unsmoothed it is better able to capture fluctuations in mortality, as we have already seen. To compare the better performing methods, the estimates from the Brass methodology, the unsmoothed MAP method with a reference period of .5 years, and the smoothed MAC/MAP combined methods are shown in Table 26 - Table 29 below. For each humanitarian emergency, estimates from six different years are derived using the Brass method, the unsmoothed MAP method, and the MAC/MAP combined method and compared. In each table, the direct estimate for the survey year is given, in addition to the indirect estimate derived from that survey year, the reference period, and the differences between the corresponding reference year and the estimate.

In addition to the years specified in the baseline scenario (Table 21), I have included Year 6. I have done this in order to illustrate the ability (or inability) of the Brass method to estimate the 5q0 of the crisis period after approximately six years (the average reference period for the Brass estimates) has passed. Although by Year 6, the mortality rate has returned to baseline in scenarios HE 1, HE 2, and HE 3, it is of interest to compare the estimates for the crisis period and the estimates derived from the Brass method for that time period.

In HE 1 (shown in Table 26 below), both the MAP and MAC/MAP Combined methods estimate 5q0 for the survey year and both predict numbers that are significantly lower than the actual mortality rate. One hundred percent of the simulations differ by more than 20% from the direct estimate. Although it would appear that the Brass estimate has a much lower relative error, this is because the Brass estimator generated is for a period of 6.3 years prior. In a year when child mortality approaches .556, the Brass method generates a point estimate of .314 for six years prior. When data from Year 6 is used, the Brass estimate is .272, less than half of the true mortality level in Year 0. Approximately ten years after the crisis period, the Brass method overestimates mortality, with 64% of the simulations differing by more than 10% from the direct estimate and all of these differences positive. On average, the simulations overestimate mortality by 25 deaths per 1,000 live births. By Year 20, the Brass method predicts a 5q0 of .194, an overestimate of approximately 4 deaths per 1,000 live births in the corresponding reference year and only 2 simulations differ by more than 10% from the direct estimate. Both the MAP and MAC/MAP continue to overestimate mortality to a greater extent than the Brass method as late as Year 20; in Year 20, the unsmoothed MAP method overestimates by approximately 10 deaths per 1,000 live births and the smoothed MAC/MAP overestimates by approximately 7 deaths, and 16% and 12% of simulations differ by more than 10% of the direct estimate in this year, respectively.

For the second humanitarian emergency scenario (Table 27), when the crisis period begins, the direct estimate is .251. The Brass method predicts a 5q0 of .233 for a period 6.48 years prior to the survey, in which the direct method estimates a mortality level of approximately .224. When data from Year 6 is used, the Brass estimate predicts mortality for Year 0 of .250, an underestimate of only 1 death per 1,000 live births. In Year 0, the unsmoothed MAP method predicts a 5q0 of .237 and in approximately one-quarter of the simulations (23%), the difference between the direct estimate and the MAP estimate is greater than 10%. The combined method predicts a 5q0 of .226 for the year and slightly more than half (51%) of the MAC/MAP combined estimates differ by more than 10% from the simulation specific direct estimate. The MAC/MAP combined method underestimates mortality by 25 deaths per 1,000 live births in the first year of the crisis, while the MAP method underestimates by approximately 15 deaths per 1,000 live births. In Year 5, when mortality has started to decline, the Brass method continues to overestimate mortality prior to the crisis period, with 90% of simulations overestimating mortality by more than 10% of the direct estimate. The mean of the absolute differences and the mean difference are both .039, indicating that all deviations are positive and that on average, the Brass method overestimate mortality by 39 deaths per 1,000 live births. By Year 20, all of the estimates generated by the indirect methods overestimate the direct estimate. While the Brass method on average overestimates the direct estimate by 2 deaths per 1,000 live births 16% of the simulations differ by more than 10% and 5% of the simulations differ by more than 20%. In the same survey year, 43% of the unsmoothed MAP estimates and 22% of the smoothed MAC/MAP estimates differ by more than 10% of the direct estimate.

The pattern seen in HE 3 is similar to, though more drastic than, the pattern seen in HE 1, and is shown in Table 28 below. The Brass method overestimates mortality in the time period prior to the crisis period, and underestimates during the crisis period by approximately 246 deaths per 1,000 live births. In every year, except Year 20, every Brass estimate differs by more than 10% from the direct estimate, always overestimating the direct estimate, except when estimating crisis mortality. Only in Year 20 is there a change in this pattern, when 25% of the estimates differ by more than 10%. In every year, all of the MAP estimates and the MAC/MAP estimates differ by more than 20% from the direct estimate, except for two out of 100 and four out of 100 simulations, respectively in Year 20. As with the Brass method, both the IHME methods underestimate mortality in the crisis period and overestimate mortality in the non-crisis years. Even 20 years after the crisis began, the MAP and MAC/MAP combined methods significantly overestimate mortality, by 77 deaths per 1,000 live births and 70 deaths per 1,000 live births, respectively.

In HE 4 (Table 29), in the years in which crisis parameters are applied, Year 1 and Year 6, the MAP method has the smallest mean differences relative to the direct method and the smallest absolute value of these differences for estimates for that year. Using data from Year 0, the Brass estimates have lower absolute differences then the MAP method, but this refers to the reference year and estimate of Year -6. When using data from Year 6, referring to Year 0, the differences between the Brass method and the direct method are larger than those generated from the MAP method. In years when mortality is declining, the absolute differences of the MAP and MAC/MAP methods are comparable, but the MAC/MAP Combined method has fewer simulations in which the estimates differ by more than 10% from the direct estimate. Although mortality fluctuates by approximately 40 deaths per 1,000 live births over the crisis period, the Brass estimates differ by no more than 5 deaths per 1,000 live births, the unsmoothed MAP estimates by no more than 7 deaths per 1,000 live births, and the smoothed MAC/MAP methods by no more than 7 deaths per 1,000 live births.

Table 26: Comparison of direct and selected indirect estimates (Brass, unsmoothed MAP (reference period .5), MAC/MAP smoothed combined) for six time points - Humanitarian Emergency 1

	Survey Year, Estimate, and Method																	
	Survey Year 0 Direct Estimate: .556			**Survey Year 1 Direct Estimate: .219**			**Survey Year 5 Direct Estimate: .210**			**Survey Year 6 Direct Estimate: .207**			**Survey Year 10 Direct Estimate: .197**			**Survey Year 20 Direct Estimate: .175**		
Measure	Brass	MAP – Ref .5	MAC/ MAP	Brass	MAP – Ref .5	MAC/ MAP	Brass	MAP – Ref .5	MAC/ MAP	Brass	MAP – Ref .5	MAC/ MAP	Brass	MAP – Ref .5	MAC/ MAP	Brass	MAP – Ref .5	MAC/ MAP
Reference Year	-6	0	0	-5	1	1	-1	5	5	0	6	6	4	10	10	14	20	20
Direct 5q0 in Reference Year	.226	.556	.556	.224	.219	.219	.214	.210	.210	.556	.207	.207	.204	.197	.197	.190	.175	.175
Mean 5q0 – Reference Year	.314	. 296	.286	.307	.284	.273	.279	.248	.240	.272	.240	.234	.236	.217	.215	.194	.185	.182
Average of Differences Indirect-Direct	.088	-.260	-.270	.083	.065	.054	.065	.038	.030	-.284	.033	.027	.025	.020	.018	.004	.010	.007
Average of absolute differences	.088	.261	.270	.083	.065	.054	.065	.038	.030	.284	.033	.027	.025	.020	.018	.006	.010	.008
Differences greater than 10%	100	100	100	100	100	100	100	97	88	100	96	79	64	50	39	2	16	12
Differences greater than 20%	100	100	100	100	100	85	96	32	8	100	19	4	3	1	1	0	0	0

Table 27: Comparison of direct and selected indirect estimates (Brass, unsmoothed MAP (reference period .5), MAC/MAP smoothed combined) for six time points - Humanitarian Emergency 2

	Survey Year, Estimate, and Method																	
	Survey Year 0 Direct Estimate: .251			**Survey Year 1 Direct Estimate: .254**			**Survey Year 5 Direct Estimate: .222**			**Survey Year 6 Direct Estimate: .218**			**Survey Year 10 Direct Estimate: .208**			**Survey Year 20 Direct Estimate: .188**		
Measure	Brass	MAP – Ref .5	MAC/ MAP	Brass	MAP – Ref .5	MAC/ MAP	Brass	MAP – Ref .5	MAC/ MAP	Brass	MAP – Ref .5	MAC/ MAP	Brass	MAP – Ref .5	MAC/ MAP	Brass	MAP – Ref .5	MAC/ MAP
Reference Year	-6	0	0	-5	1	1	-1	5	5	0	6	6	4	10	10	14	20	20
Direct 5q0 in Reference Year	.224	.251	.251	.224	.254	.254	.214	.222	.222	.251	.218	.218	.236	.208	.208	.204	.188	.188
Mean 5q0 – Reference Year	.232	. 237	.226	.239	. 245	.235	.252	.264	.252	.249	. 259	.247	.235	. 237	.226	.203	. 205	.197
Average of Differences Indirect-Direct	.009	-.015	-.025	.015	-.009	-.019	.039	.041	.029	-.001	.041	.029	-.021	.030	.018	.002	.017	.009
Average of absolute differences	.013	.017	.026	.019	.014	.021	.039	.041	.029	.012	.041	.029	.023	.030	.019	.011	.018	.010
Differences greater than 10%	17	23	51	25	32	32	90	93	70	9	91	69	46	73	45	16	43	22
Differences greater than 20%	5	0	1	8	0	0	44	42	14	0	41	15	0	25	6	5	12	4

Table 28: Comparison of direct and selected indirect estimates (Brass, unsmoothed MAP (reference period .5), MAC/MAP smoothed combined) for six time points - Humanitarian Emergency 3

	Survey Year, Estimate, and Method																	
	Survey Year 0 Direct Estimate: .701			**Survey Year 1 Direct Estimate: .721**			**Survey Year 5 Direct Estimate: .216**			**Survey Year 6 Direct Estimate: .216**			**Survey Year 10 Direct Estimate: .206**			**Survey Year 20 Direct Estimate: .181**		
Measure	Brass	MAP – Ref .5	MAC/ MAP	Brass	MAP – Ref .5	MAC/ MAP	Brass	MAP – Ref .5	MAC/ MAP	Brass	MAP – Ref .5	MAC/ MAP	Brass	MAP – Ref .5	MAC/ MAP	Brass	MAP – Ref .5	MAC/ MAP
Reference Year	-6	0	0	-5	1	1	-1	5	5	0	6	6	4	10	10	14	20	20
Direct 5q0 in Reference Year	.225	.701	.701	.222	.721	.721	.215	.201	.216	.701	.216	.216	.218	.206	.206	.195	.181	.181
Mean 5q0 – Reference Year	.284	.308	.292	.381	.418	.390	.472	.422	.380	.455	.393	.354	.372	.323	.302	.200	.257	.251
Average of Differences Indirect-Direct	.061	-.392	-.409	.163	-.303	-.332	.256	.206	.164	-.245	.178	.138	.153	.117	.096	.004	.077	.070
Average of absolute differences	.061	.392	.409	.163	.303	.332	.256	.206	.164	.245	.178	.138	.153	.117	.096	.015	.077	.070
% Differences greater than 10%	100	100	100	100	100	100	100	100	100	100	100	100	100	100	100	25	100	100
% Differences greater than 20%	57	100	100	100	100	100	100	100	100	100	100	100	100	100	100	0	98	96

Table 29: Comparison of direct and selected indirect estimates (Brass, unsmoothed MAP (reference period .5), MAC/MAP smoothed combined) for six time points - Humanitarian Emergency 4

	Survey Year, Estimate, and Method																	
	Survey Year 0 Direct Estimate: .254			**Survey Year 1 Direct Estimate: .223**			**Survey Year 5 Direct Estimate: .212**			**Survey Year 6 Direct Estimate: .253**			**Survey Year 10 Direct Estimate: .213**			**Survey Year 20 Direct Estimate: .205**		
Measure	Brass	MAP – Ref .5	MAC/ MAP	Brass	MAP – Ref .5	MAC/ MAP	Brass	MAP – Ref .5	MAC/ MAP	Brass	MAP – Ref .5	MAC/ MAP	Brass	MAP – Ref .5	MAC/ MAP	Brass	MAP – Ref .5	MAC/ MAP
Reference Year	-6	0	0	-5	1	1	-1	5	5	0	6	6	4	10	10	14	20	20
Direct 5q0 in Reference Year	.227	.254	. 254	.221	.223	. 223	.213	.212	. 212	.254	.253	.253	.216	.213	. 213	.218	.205	. 205
Mean 5q0 – Reference Year	.232	. 236	.226	.234	. 240	.229	.228	. 233	.222	.229	. 237	.226	.228	. 233	.223	.220	.223	.213
Average of Differences Indirect-Direct	.005	-.019	-.028	.014	-.017	.006	.015	.021	.010	-.024	-.016	-.027	.012	.020	.010	.002	.019	.008
Average of absolute differences	.013	.020	.029	.017	.018	.012	.016	.021	.013	.025	.018	.027	.015	.021	.014	.014	.019	.013
% Differences greater than 10%	20	28	58	34	39	12	32	53	21	48	17	52	28	50	21	19	44	23
% Differences greater than 20%	0	0	2	2	5	2	1	9	2	3	0	1	1	8	5	2	8	2

Chapter 5: Discussion

This dissertation explores the ability of indirect estimation techniques to accurately measure under-5 mortality in countries that have been affected by a humanitarian emergency. While the findings confirm some of what is already known regarding the Brass and IHME methodologies, this is the first paper to comprehensively compare how these two methods perform in situations with fluctuating mortality, looking not only on how well indirect estimation techniques measure mortality in an emergency, but also how long after an emergency indirect estimates may be affected by disruptions in mortality.

Table 30 below summarizes qualitatively how well each method performs in both the baseline and overall humanitarian emergency scenarios.

Table 30: Qualitative summary of findings by method

Method	***Summary***
Brass	**Baseline** – Slight overestimation of mortality (average of 3 deaths per 1,000 live births) with an average reference period of 6.12 years **HE** – Detects abrupt changes in mortality but estimates increases in mortality prior to the survey period, extreme levels are not captured, and declines are smoothed over time leading to overestimation in post-conflict periods
MAC	**Baseline** - Underestimation of mortality for youngest age groups and recent time periods. Generally accurate for age groups 25 and above and corresponding reference periods **HE** – Age group 20-24 detects increases in mortality and centers the increases two to three years prior to the crisis. Levels are underestimated. Smoothing leads to overall increases and of mortality and decline in estimates for the crisis period.
TFBC	**Baseline** – Overestimation of mortality at all levels. **HE** –Not shown
MAP	**Baseline** – Smoothed MAP estimates overestimate mortality between 5-10 years previous to survey but point estimates for .5 and 1.5 years are generally accurate **HE** – Reference periods of .5 and 1.5 years detect abrupt change in mortality but the level estimated for extreme changes is too low. Point estimates are elevated after emergency generating overestimates of mortality. When smoothed, estimates are too high as a result of the crisis period and fluctuations are masked
TFBP	**Baseline** – Underestimates mortality for most recent time periods and overestimates mortality five years and more previous to survey. **HE** – Not shown
All-methods combined	**Baseline** – Overestimates due to inclusion of time since first birth estimates **HE** – Not shown
MAC/MAP combined	**Baseline** – Estimates for survey year are accurate, but mortality is underestimated one to six years previous to survey. **HE** – Smoothed estimates mask fluctuations and are elevated in non-crisis periods as a result of mortality increases in crisis period.

Beginning with the baseline simulations, the findings are discussed in more detail below.

Baseline

Referring to the Brass method, my findings confirm those of Silva (2012) and others; the Brass method performs comparably to direct estimation techniques in situations in which there are gradual mortality and fertility declines. Using simulated data with no reporting bias, that is, knowing exactly the age of the mother and the number of children born and dead, the Brass method was able to estimate child mortality within 10% of the direct method in 97.2% of the simulated annual estimates. The average reference period of 6.12 years throughout the simulation period, however, underscores a primary limitation of the Brass method even in a situation with ideal data and demographic conditions. Having estimates of mortality five to seven years previous to the survey year is not an efficient way to measure program or policy impact and makes it difficult to keep track of progress in improving child health. This limitation effectively excludes the Brass method as a method of estimation in an emergency if the purpose is to measure rapid changes in mortality associated with the emergency.

The IHME methodology, which includes four separate methods to measure under-5 mortality and two combined methods, had mixed success. The Maternal Age Cohort Method (MAC) predicts seven separate 5q0s and their corresponding reference periods from each round of survey data, one for each 5-year age group. On average, the 5q0s that are predicted for the two youngest age groups, 15-19 and 20-24, were significantly lower than the direct estimate for the corresponding reference year (50% and 24% lower, respectively). As these age groups also have the most recent reference years, and are thus the best groups to estimate recent changes in mortality, this is a significant limitation to using the MAC method alone if attempting to estimate recent changes in mortality. While in this dissertation, the estimates from the youngest age groups were consistent underestimates, IHME notes that the MAC method was more likely to overestimate than underestimate mortality in their original development of the method (26). The coefficients were developed from a multitude of datasets, including more recent datasets from countries in which childbearing and mortality have decreased (26). It may be that the coefficients that were developed, based largely from countries that have undergone a fertility decline, are not appropriate to use in scenarios such as these simulations, with persistent high fertility and mortality. The MAC method however, is consistent with direct estimates when restricting the estimates to the older age groups. The average differences over the simulation period were -10%, -3%, and -.1% of the direct estimate for age groups 25-29, 30-34, and 35-39, however the reference periods (5.7, 8.4, and 11.8) are even longer than for the Brass methodology, so these are not an improvement over the Brass method.

The Time Since First Birth Cohort method (TFBC) was marginally better than the MAC method at estimating recent under-5 mortality, but 5q0 was still overestimated by more than 10% on average. When estimating under-5 mortality amongst women who gave birth ten or more years before the survey, the TFBC method consistently and significantly overestimates child mortality. As with the MAC method, this may be a limitation of the method in situations of high and early childbearing during the simulation period. When applied to a population where fertility remains high, this method should not be applied.

Both the Maternal Age Period (MAP) method and the Time Since First Birth Period (TFBP) method performed better than their respective cohort-derived methods. Each of the period based measures generates 25 different estimates and 25 different reference periods, from .5 to 24.5 years before the survey. Of particular interest are the estimates for the reference periods of .5 and 1.5 years. If either of these methods is able to accurately predict under-5 mortality for a recent period of time, this is preferable for use relative to the Brass method. Across the 40-year simulation period, the mean MAP estimate for the reference period of .5 years was .199. On average, the mean MAP estimate thus overestimated the direct method by 2 deaths per 1,000 live births or 1% of the average direct estimate, comparable to the Brass overestimate of 3 deaths per 1,000 live births. Similarly the average estimates derived for a reference period of 1.5 years was .195, a total difference of less than 1% below the 5 direct estimate.

The TFBP, while it performed better than the TFBC method, was not as accurate as the MAP method. The TFBP underestimated the direct estimate by an average of 11% and 3% for the reference periods of .5 and 1.5 years, respectively, but when combined with the estimates from the more distant past, led to a systematic overestimation of mortality. The overestimation of both of the time since first birth methods indicates that the coefficients developed by IHME for estimating child mortality based on time since first birth need further refinement. This may be because the simulated populations seen here have high and early childbearing, which may be different than the datasets used to develop the time since first birth coefficients. An additional consideration is that many datasets that collect summary birth histories do not have information on time since first birth. IHME acknowledges that there were many fewer datasets available when the coefficients were derived for this method and the lack of data possibly led to estimates with less accuracy. An additional explanation for some of the biases present in the period-derived methods is the reliance on using country specific and regional patterns of childbearing to predict the coefficients for estimating 5q0. Both of the period-derived methods use patterns of CD/CEB that are derived from hundreds of country specific datasets (26) and the predicted log 5q0s that are derived depend on the regional and country specific CD/CEB that were derived in the original creation of the method. In these simulations, when the data do not originate from any one country or region, the application of these methods may not be appropriate. This underscores a limitation of the IHME methods relative to the Brass methodology; the Brass method can be applied to any dataset regardless of region once the decision is made regarding the underlying age pattern of mortality and the corresponding life table is chosen. The IHME methods, however, depend on region- and country-specific estimates. The IHME period-derived methods can potentially better predict under-5 mortality than the Brass methodology if the correct country and region is chosen, but when the data do not conform to expected region and country specific patterns, the IHME methods are probably less accurate.

One of the strengths of the IHME methods, as argued by the authors, is the ability to combine any of the four methods using a weighted Loess smoother (26). If each method performs relatively well in relation to the direct estimate, this is a potential advantage over the Brass methodology as there are more data points that can be used to derive changes in under-5 mortality over time. When the methods do not perform well however, as we see with both of the time since first birth methods, the combined estimate is also biased. In this dissertation, when the true underlying under-5 mortality is known, it was easy to make the decision to eliminate the time since first birth methods from the smoothing procedure as they obviously inflated the estimates. In reality, however researchers will not be able to make this decision with as much confidence. If the maternal age and time since first birth estimates are very different, then the use of the maternal age methods is preferable.

Based on the systematic overestimation of child mortality from both time since first birth methods in the baseline scenario, only the results of the MAC and the MAP methods and the combined MAC/MAP estimate for the humanitarian emergency scenarios were presented. In general, the MAC/MAP combined method underestimated under-5 mortality in the five years previous to the survey. This is due to the fact that the weighted Loess is generated by inverse weighting each data point by the number of points generated by the method. That is, when the methods are combined, each MAP method is weighted to $1/25^{th}$ of the estimate and each MAC is weighted to $1/7^{th}$ of the estimate. Thus, the systematic underestimation of the 20-

24 year age group pulls the estimates for recent years down. However, because the most recent estimates, those within three years of the survey year, are based only on the MAP estimates, the most recent estimates are not as biased downward.

Since the MAP method is the primary driver of the MAC/MAP combined method for reference periods less than three years, the two methods generate very similar estimates in the baseline simulation. For example, using data from only Year 0 in which the mean direct estimate is .212, the unsmoothed estimate of the MAP method for a reference period .5 years before the survey is .212 and both the smoothed estimate of the MAP method and the MAC/MAP combined method are .209. Thus, if a researcher has only one year of survey information, s/he should be hesitant about drawing conclusions regarding recent changes in under-5 mortality using only the combined method. A thorough examination of the point estimates for both the MAC and MAP estimates and a comparison of the smoothed estimates for MAC and MAP separately should be explored before deciding if the methods should be combined.

Humanitarian Emergencies

In a situation with ideal data, that is, no age-misreporting, no interviewer bias, no missing data and constantly declining mortality and fertility, the Brass method and the IHME MAC and MAP methods performed well. However, once mortality was no longer linearly declining, the methods became less reliable. The tendency for each of the methods to smooth mortality patterns over time led to consistent overestimation in the time period before mortality increased, underestimation during the crisis period, and overestimation after mortality declined.

In situations in which mortality increased very rapidly and reached high levels (HE 1 and HE 3), the abrupt shift is captured by the Brass method, although it is both misplaced in time (on average, between six and seven years prior to the crisis) and lower than the actual level. In Year 0 for HE 1 and HE 3, the level of under-5 mortality estimated using the direct method is .556 and .701. When estimates for Year 0 are derived with the Brass method, using data from Year 6, the estimates are .272 (48.9% of the direct estimate) and .455, (64.9% of the direct estimate) a difference of 284 deaths and 246 deaths per 1,000 live births, respectively. These are substantially different numbers that would greatly mislead researchers on the extent of mortality experienced by children under 5 in a humanitarian emergency. The Brass method then overestimates mortality (more than 10% of the simulations differ by more than 10% of the direct estimate) until Year 13 and Year 15, in scenarios HE 1 and HE 3, respectively. Keeping in mind the average reference period of approximately 6 years using the Brass estimator, this means that it could take almost 20 years after an emergency with an extreme spike in mortality before the Brass method of estimation could be used to reliably estimate child mortality.

Even when the mortality shifts are not as drastic, such as in HE 2 and HE 4, the Brass method returned inflated estimates of mortality for the years prior to the survey. In

HE 2, five years prior to Year 0, the average Brass estimate overestimated mortality by 15 deaths per 1,000 live births (6.7% higher than the direct estimate of 224 deaths per 1000 live births) and, in the year prior to the Year 0, by 37 deaths per 1,000 live births (17.4% higher than the direct estimate of 214 deaths per 1000 live births). At the end of the crisis period, the Brass method underestimated mortality by 25 deaths per 1,000 live births (9.6% lower than the direct estimate of 258 deaths per 1000 live births). Although this difference is less than 10% of the direct estimate, it is still over twice that of the 10 deaths per 1,000 live births that Alkema and colleagues (2012) flagged as a problematic difference when comparing UN IGME and IHME estimates of child mortality. Even in these situations of lower mortality increase, it is not until Year 16 and Year 18 for HE 2 and HE 4, respectively, that 90% of the Brass estimates are within 10% of the direct estimate. In these cases, this is approximately ten years after the crisis period ends and four years after the crisis period ends. Thus, researchers who are attempting to estimate child mortality in places affected by even moderate increases in child mortality may generate biased estimates up to ten years after mortality has returned to baseline levels.

Perhaps the most difficult scenario to interpret is HE 4, where moderately increased mortality levels fluctuate over time. In this case, the Brass method smoothed out almost all variation across the crisis period, showing stagnant under-5 mortality rates. At each spike in mortality, the average estimates across the simulations underestimated mortality by between 24 and 34 deaths per 1,000 live births (between 9.5% and 13.5% lower than the direct estimate), while overestimating mortality during periods of lower mortality by approximately 10.7 deaths per 1,000 live births (an average overestimate of 5.0% of the direct estimate). While these levels are not extreme, approximately half (48%) of the simulations generated Brass estimates that differed by more than 10% from the direct estimate during the peak periods of mortality. Once the crisis period was over, the Brass method was better able to estimate mortality, however, its inability to show fluctuations during the crisis period underscores its limitations as a method for under-5 estimation during times when mortality is not stable. If the purpose of the research is to estimate excess mortality during periods of crisis or estimate changes in mortality patterns as a result of a humanitarian emergency, the Brass method should not be used.

The results of the Brass method when applied to situations of mortality shifts thus lead to three conclusions;

First, the Brass method is not suitable for research done during or immediately after the crisis period of an emergency, because estimates of crisis mortality will be both underestimated and removed in time, overestimating the true mortality of the past years, underestimating the mortality of the crisis period, and obscuring the extent of mortality shifts. While the Brass method was not able to correctly estimate the level of mortality increase when extreme shifts in mortality occurred, it was able to detect that the shifts did occur and generate estimates that reflected an abrupt increase in mortality between two years, rather than a gradual increase over several years. When the estimates generated from years in a crisis period are not centered to the reference year, they are thus able to detect that an abrupt shift in mortality does occur in the crisis period. This is particularly true if the shift in mortality is very abrupt and short-lived, such as the Rwandan genocide. When this is done however, estimates before and after the crisis period are biased, which limits the utility of not-adjusting the Brass estimates during a period of crisis.

Secondly, researchers conducting surveys in places where there have been extreme increases in mortality in the recent past should be aware that even after mortality has stabilized, estimates may be subject to bias for an extended period of time. This is particularly an issue if using the Brass methodology, since researchers must also address the reference period. If increases in mortality continue to affect the Brass method for up to ten years post-crisis, it is only data gathered fifteen years or more post-crisis that may be free of this bias. Finally, in situations where there are continuous fluctuations in mortality across time, the Brass method will largely smooth out these fluctuations over time. If the purpose of the research is to gather the average mortality of an extended crisis period, the Brass method may be suitable, but if the purpose is to examine the pattern of mortality fluctuations in crises or estimate excess deaths during specific time periods, the Brass method is unsuitable.

Based on the consistent overestimation of the time since first birth methods in the baseline simulation, only maternal age derived methods were considered. Although these methods perform well in the baseline simulations, they were not able to successfully capture fluctuations in mortality. The MAC method, when not smoothed, was able to pick up the abrupt increase in mortality simulated in HE 1 and HE 3, particularly in the younger age groups, but the baseline levels were underestimated to such an extent that the abrupt increase in under-5 mortality estimates was still a significant underestimate. Additionally, the estimates were still removed in time from the onset of the crisis. For example, in HE 1, the MAC method for age group 15-19 increased from .104 two years before the crisis to .136 the year before the crisis. The abrupt increase in mortality was a result of the spike in mortality generated by the crisis but was estimated both a year prior to the crisis and underestimated 5q0 by 420 deaths per 1,000 live births from the true crisis mortality (an underestimation of 75.6%). For age group 20-24, the spike in mortality was estimated approximately three years prior to the survey, when mortality increased from .168 to .239. The difference in the mortality levels estimated by the direct method between the pre-crisis and crisis year was approximately 342 deaths per 1,000 live births; the difference estimated by the MAC method is only 71 deaths per 1,000 live births, 21% of the difference estimated by the direct method. Unfortunately, none of the other age groups showed an appreciable effect of the crisis on estimated mortality levels. In situations where child mortality abruptly and significantly increases, the MAC methods seemed unable to accurately capture the fluctuation, either in the scope of the mortality change or at the appropriate moment in time.

With an increase in mortality that is more moderate, the MAC method is still unsatisfactory. Although data from the youngest age groups do pick up a slight increase in mortality, the overall levels are too low and while the older age groups are better able to estimate non-crisis mortality, they show very little fluctuation during the crisis period. When mortality fluctuates over short periods of time, the MAC method shows almost no variation in any the age groups, and would lead one to conclude that mortality has remained stable across the time period. In all, the MAC method alone does not provide satisfactory estimates of mortality in situations when mortality is not declining linearly.

The MAP method, whose primary advantage is that it provides timely estimates of under-5 mortality, also does not capture extreme fluctuations in mortality well. In HE 1 and HE 3, in Year 0, the MAP estimates for a reference period of .5 years do demonstrate an abrupt increase in mortality. In HE 1, in the year before the crisis, under-5 mortality is estimated at .215 and during the crisis year increases to .296 (53.2% of the direct estimate of .556 for Year 0). While this is an increase of 81 deaths per 1,000 live births, it is an underestimate of the true change in mortality of approximately 76.3%. In HE 3, the MAP method underestimates the true change in mortality by 84.5% and underestimates the direct estimate for Year 0 by 56.1%. With increasing time in an emergency, the MAP method improves; in HE 3, after two years, the difference between the direct and the MAP method estimate is 223 deaths per 1,000 live births, an underestimate of approximately 31.0% of the direct estimate of 720 deaths per 1,000 live births. Once the emergency period is over, the

MAP method overestimates mortality; again in HE 3, almost 30 years post-emergency, mortality estimates are overestimated by 45 deaths per 1,000 live births, 26.5% higher than the direct estimate of .169 in the final year of the simulation. Even a moderate increases in mortality continues to affect the MAP method for a short reference period of .5 years, as shown in scenario HE 2. Twenty-five years after the crisis period ends, the MAP estimate continues to overestimate mortality by 17 deaths per 1,000 live births, overestimating the direct estimate by approximately 9.8%. Relative to both the Brass and MAC methods, the MAP method better captures the shorter, sharp fluctuations in mortality seen in HE 4, however, again it continues to overestimate mortality once it has started to decline. During the peak mortality spikes, when mortality reaches .254, .253, and .255 according to the direct method, the MAP method estimates .236, .236, and .235. In comparison, the Brass method generated estimates of .229, .229, and .221 for the same years. The MAP method overestimates mortality in the years between the mortality increases; the years prior to the second and the third mortality spikes, Year 5 and Year 11, the MAP method overestimates the direct estimate of .212 by 9.7% and the direct estimate of .210 by 9.8%, respectively. This is equivalent to approximately 21 deaths per 1,000 live births in each case. Estimates for the reference period of .5 continue to remain elevated after the crisis period is over; approximately 15 years after the final spike, mortality is overestimated by 18 deaths per 1,000 live births, or 9.3%.

Here we see both the strengths and limitations of the MAP method. If increases in mortality are relatively moderate and persist for an extended period of time, the MAP method may be an accurate estimator during the crisis period. The method loses its strength, however, when mortality begins to decline and it consistently overestimates mortality for years after. This is of concern for researchers who may be conducting under-5 mortality work after a crisis has ended. Even if mortality has consistently declined or been stable for several years, the MAP estimator may overestimate child mortality, at least in the very recent past, thus mitigating its greatest attraction of being able to estimate mortality change within a short reference period from the survey.

While the point estimates that are derived for reference periods less than a year are able to capture sharp fluctuations in mortality, the IHME methods are actually designed to show trends in under-5 mortality over time using the weighted Loess. Unfortunately, when the methods are smoothed according to IHME recommendations, the smoothed MAC, smoothed MAP, and smoothed MAC/MAP combination perform poorly. Even more than the Brass method, the IHME methods both mask fluctuations in mortality and bias estimates either upwards or downwards, depending on the point of the humanitarian emergency. When data from every year is incorporated into the smoothed estimates, the IHME methods show an almost constant decline in mortality over time. Even in the most extreme mortality scenario, when the 5q0 reaches .721 the maximum smoothed IHME estimate is .387, 53.7% of the direct estimate, and is centered three years before the crisis period. With less extreme increases, the smoothed IHME estimates show almost no fluctuation over time. This is also the case when data from only one year are used to estimate mortality. In these instances, when using data generated during or immediately after the crisis period, the IHME smoothed estimates show constant and elevated mortality previous to the crisis period. Although using the smoothed estimates does seem to improve the accuracy of the IHME methods in the post-survey period, if there have been any fluctuations in mortality, they will not be captured and overall mortality will likely be overestimated. In an emergency or post-emergency setting, these methods will not improve accuracy over the Brass methodology.

In sum, neither of the indirect methods, or their variants, performed sufficiently well to recommend their use to measure under-5 mortality in an emergency setting. All methods lead to an overestimate of mortality in the period immediately prior to the onset of a crisis and underestimate mortality during the crisis period, masking the true extent of crises on child mortality. Additionally, indirect methods are likely to overestimate child mortality once mortality rates have declined. When mortality fluctuates over the crisis, none of the methods provide reliable estimates during the crisis period.

These results come from the use of simulation data. In scenarios where data are less than ideal, which this dissertation did not test, it is likely that the indirect estimation methods will have greater variability and be even less reliable. Therefore, if the purpose of a research study is to estimate under-5 mortality in a crisis or to estimate excess mortality due to a crisis, indirect methods cannot be used reliably. To truly understand the impact of a crisis on child mortality, the only method that can produce even somewhat reliable estimates is the direct method.

Limitations and Strengths

With all studies there are limitations. Perhaps the greatest limitation of this dissertation is also its greatest strength, that is, the data are not real. These are simulated populations, which allow for the elimination of all other potential sources of interviewer, sampling, and respondent bias, but the lessons learned here are not based on real data and the particular challenges that arise from data collection in the field. Thus, while it is possible to examine in more detail the result of disruptions in mortality without needing to disaggregate the effect of other violations in assumptions, limitations arise regarding the generalizability of these findings. Along these lines, the simulations themselves, while based in part on historical precedence, do not model any one emergency and at least one simulation, HE 3, with extremely high mortality lasting for an extended period of time, is (hopefully) unlikely to occur. While these exact emergencies are unlikely to occur, however, the patterns themselves are have historical precedent, which lends credence to the chosen parameters. It may be beneficial to model additional patterns, with more gradual mortality increases and decreases, to see if indirect methods are more appropriate when mortality changes are not so abrupt.

Unfortunately, there is a limit to the number of permutations that can be tested by any one researcher, but this does justify further research on the subject.

Additionally, in these simulations, fertility, marriage, and migration were held constant across scenarios to isolate the effect of mortality fluctuations. In reality, these are important demographic events that are impacted by humanitarian emergencies. Changes in fertility that may occur as a result of these emergencies will impact the ability of the indirect estimates to accurately predict child mortality. Migration, though it may or may not increase the risk of mortality, will certainly lead to underestimates of excess mortality if emigrating populations are not properly sampled or accounted for. These are important limitations when considering how to properly measure mortality in emergency settings. However, I would argue that, in fact, the effect of humanitarian emergencies on these demographic processes further justifies the need to use complete birth histories if under-5 mortality is truly of interest. Given the inability of indirect methods to accurately capture mortality changes when these are held constant, the additional biases that may arise as a result of changing fertility further justifies using the complete birth history.

From a more technical perspective, one limitation, already briefly discussed, is the country and region specific patterns used by IHME. I used the region-specific estimates for sub-Saharan Africa, as it remains, in general, a high-fertility, high-mortality setting similar to the simulations programmed here. However, the IHME coefficients were generated for specific countries and in the absence of using data specifically from the countries used in the original development of the multipliers, these estimates may be biased. Future research estimating the impact of these coefficients on the mortality estimates could be undertaken. This, however, is a limitation of the method itself as much as a limitation of this dissertation. While one limitation of the Brass method is the reliance on choosing an underlying life table to derive probabilities and coefficients, so too is the IHME methodology limited by the decision to use country specific estimates. If the underlying mortality pattern differs from either the chosen life table or the chosen country, biases will be introduced.

Despite these limitations, there are several strengths to this dissertation. It employs an innovative use of simulations to model mortality patterns that are difficult to estimate in reality. Situations in which these morality patterns are likely to be seen are generally associated with poor quality data and data are often either discarded or changed based on expert opinion (4,7). As mentioned above, the use of simulations allows for the creation of ideal data, limiting the potential of other biases to distort the effect of interest. Additionally, simulation allows for the creation of hundreds of datasets with random fluctuations so that conclusions can be drawn without concern that they are drawn from data that may be random outliers. While it was not possible to realistically model a humanitarian emergency, the patterns of mortality seen here were based on historical precedence, using ratios of emergency to pre-emergency mortality to inform the creation of different scenarios.

Implications

Implications for Policy

Generating accurate estimates of the impact of humanitarian emergencies on populations is important. Decisions regarding humanitarian assistance, allotment of resources, and funding for post-conflict reconstruction efforts are often – although by no means always – made with an understanding of mortality and morbidity patterns. When estimates of mortality and morbidity are released, there may be significant political backlash, from all sides. A UN report estimating excess mortality in Internally Displaced Persons (IDP) camps in Northern Uganda generated intense debate over the reliability and validity of the estimates (6,75). Humanitarian organizations were accused of inflating numbers to receive additional funding, while government officials called for a re-estimation of the number in an attempt to limit political backlash (6). Reports that maternal and child health have significantly improved in Afghanistan since the involvement of the US-led coalition and collapse of the Taliban received considerable media attention, while simultaneously generating debate among scholars regarding the plausibility of the improvements (49,53,57,76,77). Such debate is not rare and whether mortality data are used to advocate for or against aid, engagement, or assistance, the accuracy of the estimates is important. The veracity of mortality data depends on what methods are used for data collection and analysis and it is imperative that researchers use the appropriate methods to estimate the impact of humanitarian emergencies on populations.

Implications for Research

While this dissertation is able to demonstrate the shortcomings of two extant methods to estimate child mortality in humanitarian emergencies and recommend the use of complete birth histories in periods of crisis, this is not an ideal solution. The complete birth history is not without its limitations, as previously described. The additional training of interviewers, time in the field, and significant possibility for respondent biases are real concerns. A question raised by this dissertation is whether the Brass methodology can be used to estimate fluctuations in mortality if the estimates are not shifted to the reference year, but instead are used as estimates for the survey year. While the estimates themselves are too low, it merits further investigation whether a re-estimation of the coefficients may yield higher estimates. If summary birth histories can be used with an adaptation to the Brass methodology, this could prove beneficial for fieldwork.

Finally, this research could be expanded to include research on indirect adult mortality estimation. Much of adult mortality is generated through sibling histories, which rely on similar assumptions of fertility and mortality patterns. The same questions explored here, regarding how fluctuations in mortality affect indirect estimates, can be explored with the same simulation program to gain a better understanding of potential biases that may arise in these methods.

Ethical Implications

There continues to be a debate in the research community on the ethical implications of conducting research in humanitarian emergencies. How does one balance the safety and wellbeing of the survey team against the need to understand the impact of the crisis on the affected community? One argument that is made is to limit the scope and questions that are asked, both to limit the strain on respondents and to decrease time spent in a potentially dangerous situation for the surveyor (78,79). This concern may encourage researchers to use the summary birth history in preference over the complete birth history as it limits the time spent in the field for the research team. I would argue, based on what I have shown in this dissertation, that until sufficient analytic methods exist, the use of the summary birth history is no more ethical to use than the complete birth history. While the use of the summary birth history may be faster, if the properties of the analytic methods lead to systematic over and underestimation, it makes the estimates highly questionable and, thus, the risk taken by the data collector will be of little substantive benefit. If the estimation of child mortality is truly of concern to the research team, the complete birth history should be prioritized. Extensive training for interviewers in order limit respondent and interviewer bias should be undertaken and security considerations must be balanced with the need to gain a comprehensive understanding of the impact of the humanitarian emergency.

Conclusions

To the advanced demographer, this dissertation may confirm what was already known regarding indirect estimation of child mortality, but beyond the field of formal demography, this lesson does not seem to be widely promulgated. The literature on demographic measurement in complex emergencies tends to focus either on generating estimates of excess mortality in specific emergencies (43,80,81), on guidance to improve sampling techniques and survey implementation (6,82,83), or on the ethical implications of conducting research in humanitarian emergencies (79). There is little guidance on which questions to include in a survey or the appropriate analytic methods to use in order to generate accurate estimates. This dissertation was an attempt to fill that void; to identify which methods if any, will successfully estimate child mortality in an emergency, and if a summary birth history must be done, what indirect method may serve the researcher best.

It is no small challenge to conduct surveys in an emergency or post-emergency setting. There are a host of challenges that can threaten not just the quality and integrity of the research process, but the very safety and lives of the research team. It is still imperative, however, to quantify the human costs of war and the challenge to improve estimation methods falls on the demographic community. We should no longer accept that "between 12 and 32 million people" may die in an emergency; rather, we must strive as a research community to develop accurate estimates of the demographic consequences of war, with the purpose of improving assistance to populations in need.

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Appendix I – Socsim Fertility Parameters

Age-specific fertility rates are derived using Schmertmann's graphic parameters (70). Inputs necessary to derive the age-specific rates are the total TFR, the minimum age at marriage (alpha), age at peak child bearing, and the age between peak child bearing and age 50 when fertility reaches half of peak fertility (H).

Table 31: Parameters to derive age-specific fertility rates from Schmertmann's graphically intuitive parameters model (70)

Parameters			
TFR	alpha	Peak	H
7.06	12	23	39
6.85	12	23	39
6.60	12	23	38
6.12	12	23	38
5.94	12	23	36
5.99	13	23	36
5.84	13	24	36
5.59	13	24	35
5.42	13	24	35
5.34	13	24	35
5.25	13	24	35

Appendix II – Socsim Nuptiality Parameters

Parameters necessary for input into Socsim nuptiality file. All other parameters are default parameters in Socsim.

Table 32: Input parameters for Socsim nuptiality simulations

Parameters	***Value***
Ideal age difference (groom-bride) (months)	60
Maximum age difference (groom-bride) (months)	240
Maximum age difference (bride-groom) (months)	180
Marriage rates (female)	75% married between age 12-25 25% married between age 26-45
Marriage rates (male)	75% married between age 18-25 25% married between age 26-45
Marriage slope ratio	7

5/21/14

CURRICULUM VITAE

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PERSONAL DATA

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EDUCATION AND TRAINING

PhD/2014 Johns Hopkins Bloomberg School of Public Health, Department of Population Family and Reproductive Health

MPH/2008 Emory University, Rollins School of Public Health, Global Health Department

BA/2004 Whitman College, Politics

PROFESSIONAL EXPERIENCE

Johns Hopkins University — **Baltimore, Maryland**
Research Assistant – Gates Institute — **Sept 2012-Present**

- Performance Monitoring and Accountability 2020 (PMA 2020)
  Project Director, Scott Radloff
  - Active role in design, implementation, and monitoring of multi-country study using mobile phone technology to monitor FP progress at the national level
  - Household and service delivery point questionnaire design, including programming in XML
  - Development of interviewer and supervisor manuals and protocols for implementation of family planning survey
  - On-site training in mobile technology and survey protocols for household and service delivery point data collection
  - Sustainable knowledge transfer for training of trainers on mobile technology and family planning
  - Monitoring and evaluation of real-time data collection and preliminary analyses

- Family Health and Wealth Study
  Primary Investigator: Amy Tsui
  - Data management and quality control of multi-country longitudinal study with focus on family planning and family size effects
  - First author of manuscript examining effect of family size on educational attainment of children in four countries (ongoing)

Research Assistant: Dr. Courtland Robinson — **Sep 2009 – Present**

- Analyze UNHCR HIS dataset for UNHCR funded paper on RH status of refugees in 82 camps worldwide including family planning service statistics
- Data analysis using STATA and Excel estimating Somali migration and famine associated mortality for FEWS-NET and UNHCR. Results published in report:
- Development of quantitative and qualitative research question, protocol, instrument design, and training for three-country study of sexual and reproductive health needs of very young adolescent (VYA) refugees with Women's Refugee Commission
- Lead technical assistance for Ethiopia implementation including training in qualitative methods, survey development, qualitative and quantitative analysis

Research Assistant: Dr. Vladimir Canudas-Romo **June 2011-Dec 2012**
- Data compilation and manuscript development for paper examining potential gains in life expectancy through elimination of maternal mortality
- Data cleaning and analysis on multi-country, Family Health and Wealth Study, using STATA and Excel
- Analysis and manuscript preparation of educational attainment and parity analysis
- Data management and preliminary analysis for World Bank brief "Age- and Cause-Decomposition of the Difference in Life Expectancies Between EU-15 and Eastern EU Countries"

Student Investigator: Dr. Linda Bartlett **Jan 2011-April 2012**
- In-country field work developing the qualitative research component of national RAMOS study evaluating maternal mortality in Afghanistan including questionnaire development, hiring, training, and management of data collectors, quality control, and analysis and write-up (PI: Dr. Linda Bartlett)
- Quantitative data management, quality control, and analysis
- Writing and revision of final report and manuscript development

Research Assistant: Dr. Gilbert Burnham **Jan 2012-July 2012**
- Demographic analyses of complex household survey dataset to estimate excess mortality due to Gulf and Iraq wars. Used multiple demographic methods to estimate mortality and Arc-GIS software to examine geographic distribution.

Research Assistant: Dr. Stan Becker **Sep 2010-May 2012**
- Analyzing data and preparing manuscript for publication on results of longitudinal study of an NIH funded microcredit study in Bangladesh

USAID/ Public Health Institute Washington, DC
Global Health Fellows **June 2010-Aug 2010**
Family Planning/ HIV Integration Intern
- Researched and wrote series of briefs summarizing USAID family planning and HIV programs, focusing on current FP and HIV integrated programs, identifying best practices, and identifying integration opportunities

Emory University Kigali, Rwanda
Rwanda Zambia HIV Research Group **Aug 2008 – Aug 2009**
CVCT/ Data Intern
- Implement family planning and HIV integration program in CVCT site including organizing trainings, advertisements, and quality assurance procedures

- Provide monthly reports to CDC, IAVI, and Rwandan MOH regarding uptake of services and surveillance data
- Create, edit, and perform quality control and assurance of source documents and Case Report Forms

CARE Atlanta, Georgia
CARE – CDC Health Initiative Program Associate **Sep 2007 – Aug 2008**

- Research and write best-practices document of HIV&AIDS referral networks among CARE sponsored projects in Sub-Saharan Africa, Asia, and Latin America
- Coordinate program while replacement director was hired
- Assemble, edit, and submit portfolio proposals to CDC for multi-million dollar grant

United National Population Fund (UNFPA) Gulu, Uganda
Primary Researcher **May 2007 – Aug 2007**

- Awarded funding to research quality of family planning programs available to Internally Displaced Persons in IDP camps in Northern Uganda culminating in award winning thesis and presentation at PAA conference
- Conducted key informant interviews and focus group discussions with IDP community members, health care workers, and district and national level health care officials
- Analyzed qualitative data and presented findings to district and national level implementing partners of UNFPA
- Participated in regional planning meetings attended by UN affiliates and implementing partners

PROFESSIONAL ACTIVITIES

Society Membership

- Population Association of America (PAA)
- International Union for the Scientific Study of Population (IUSSP)
- Delta Omega Honor Society

Consultatations
Stanton-Hill Research **Sept 2012 – Dec 2013**

- Data analysis and manuscript development comparing adolescent and under-5 mortality in a complete sample of available DHS surveys
- Statistical analysis for Lancet Commisioned report "Global Health 2035: a world converging within a generation", December 3, 2013

EDITORIAL ACTIVITIES

Peer Review Activities

- Reviewer for special issue of International Journal of Gynecology and Obstetrics "Family Planning: Selected Research Papers form the Second International Conference on Family Planning, Dakar, Senegal, November 2011".

- Reviewer for Global Public Health

HONORS AND AWARDS

- **Caroline Cochran Scholarship Fund in Population and Reproductive Health** for outstanding student in population studies (2011 and 2012)
- **Edward J Dehne Award in Population Studies** for outstanding student in population studies (2011 and 2013)
- **Best Thesis** in Global Heath Department, Rollins School of Public Health – May 2008

PUBLICATIONS

Peer Review

- Canudas-Romo, V., Liu, L, **Zimmerman, L.,** Ahmed, S., Tsui, A. (2014). "Potential Gains in Reproductive-Aged Life Expectancy by Eliminating Maternal Mortality: A Demographic Bonus of Achieving MDG 5" PLoS ONE 9(2): e86694. doi:10.1371/journal.pone.0086694
- Hill, K., **Zimmerman, L.**, Jamison, D. (2014) "Mortality Among Older Children and Younger Adolescents in Low- and Middle-Income Countries" (Submitted, In Review)

Reports

- Robinson, C., **Zimmerman, L.**, Checchi, F. (2013) "Internal and External Displacement among Populations of Southern and Central Somalia Affected by Severe Food Insecurity and Famine during 2010-2012" FEWS-NET, Washington DC. (Accepted, awaiting dissemination)
- Bartlett, L., LeFevre, A., Becker, S., Koblinsky, M., Rosen, H., Winch, P., **Zimmerman, L.,** et al. (2013) "Reproductive Age Mortality Survey II: Maternal Mortality in Afghanistan". USAID, Washington DC. (Accepted, awaiting dissemination)

PRACTICE ACTIVITIES

Media and Communication

Johns Hopkins Health Newsfeed "Mobile Health" – December 19, 2013

CURRICULUM VITAE
Linnea A. Zimmerman, MPH
Part II

TEACHING

Guest Lecturer

- Dr. Canudas-Romo Life Tables course. "Introduction to R Statistical Package". January 31, 2013
- Johns Hopkins Global Health Strategies for Stability Course. "Burden of Disease in LMIC; demographic and epidemiological patterns". August 13, 2012

Teaching Assistant

Johns Hopkins University **Oct 2011-Present**

- Led discussion groups, graded assignments and advised students for graduate level courses. Led labs on using large data sets from DHS, UN, WHO and Census. *Population Dynamics and Public Health, Applications of Population Data for Policy & Practice, Maternal & Neonatal Mortality in Low-and Middle-Income Countries, Family Planning Programs and Policies,* and *Clinical Aspects of Reproductive Health* courses

Emory University **Jan 2008 – May 2008**

- Teach 2 classes per week of 10-15 graduate students each for International Strategies and Proposal Development classes each
- Prepare lab content on the development of grant proposals and on data programming and complex survey analysis, logframes, and budget

ACADEMIC SERVICE

- Departmental Admissions Committee - Spring 2013

PRESENTATIONS

Scientific Meetings

- **Zimmerman, L.,** Koffi, A., Ahmed, S. "Family Size and Educational Attainment in Three sub-Saharan African Countries". Session presentation at International Conference on Family Planning November 13, 2013
- **Zimmerman, L.**, Canudas-Romo, V., Tsui, A., "Age-Specific Maternal Mortality Ratios". Session presentation at Population Association of America 2012 Annual Meeting May 3, 2012

Posters

- Amin, R., **Zimmerman, L.** "Microcredit Drift in Depth of Outreach to the Poor in Rural Bangladesh". Poster Presentation at Population Association of America 2013 Annual Meeting April 11, 2012. Presenter
- **Zimmerman, L,** Robinson, W.C., Packer, C. "Reproductive Health in Post-Emergency Refugee Camps". Poster Presentation at Population Association of America 2011 Annual Meeting April 1, 2011. Presenter
- **Zimmerman, L.,** Haussamen, L., Stephenson, R. "Quality of Reproductive Health Care in War-Affected Northern Uganda". Poster Presentation at Population Association of America 2008 Annual Meeting, April 17, 2008. Presenter

ADDITIONAL INFORMATION

Keywords
Family planning, reproductive health, maternal mortality, demography, refugee and emergency health, indirect estimation, under-5 mortality, mobile technology, population dynamics, monitoring and evaluation