Mortality before and after the 2003 invasion of Iraq: cluster sample survey

Les Roberts, Riyadh Lafta, Richard Garfield, Jamal Khudhairi, Gilbert Burnham

Published online October 29, 2004 http://image.thelancet.com/extras/04art10342web.pdf

Center for International Emergency Disaster and Refugee Studies, Johns Hopkins Bloomberg School of Public Health, Baltimore, MD, USA (L Roberts PhD, G Burnham MD); **Department of Community Medicine, College of Medicine, Al-Mustansiriya University, Baghdad, Iraq** (R Lafta MD, J Khudhairi MD); **and School of Nursing, Columbia University, New York, NY, USA** (Prof R Garfield DrPH)

Correspondence to: Dr Les Roberts **les@a-znet.com**

Summary

Background In March, 2003, military forces, mainly from the USA and the UK, invaded Iraq. We did a survey to compare mortality during the period of 14·6 months before the invasion with the 17·8 months after it.

Methods A cluster sample survey was undertaken throughout Iraq during September, 2004. 33 clusters of 30 households each were interviewed about household composition, births, and deaths since January, 2002. In those households reporting deaths, the date, cause, and circumstances of violent deaths were recorded. We assessed the relative risk of death associated with the 2003 invasion and occupation by comparing mortality in the 17·8 months after the invasion with the 14·6-month period preceding it.

Findings The risk of death was estimated to be 2·5-fold (95% CI 1·6–4·2) higher after the invasion when compared with the preinvasion period. Two-thirds of all violent deaths were reported in one cluster in the city of Falluja. If we exclude the Falluja data, the risk of death is 1·5-fold (1·1–2·3) higher after the invasion. We estimate that 98 000 more deaths than expected (8000–194 000) happened after the invasion outside of Falluja and far more if the outlier Falluja cluster is included. The major causes of death before the invasion were myocardial infarction, cerebrovascular accidents, and other chronic disorders whereas after the invasion violence was the primary cause of death. Violent deaths were widespread, reported in 15 of 33 clusters, and were mainly attributed to coalition forces. Most individuals reportedly killed by coalition forces were women and children. The risk of death from violence in the period after the invasion was 58 times higher (95% CI 8·1–419) than in the period before the war.

Interpretation Making conservative assumptions, we think that about 100 000 excess deaths, or more have happened since the 2003 invasion of Iraq. Violence accounted for most of the excess deaths and air strikes from coalition forces accounted for most violent deaths. We have shown that collection of public-health information is possible even during periods of extreme violence. Our results need further verification and should lead to changes to reduce non-combatant deaths from air strikes.

Introduction

The number of Iraqis dying because of conflict or sanctions since the 1991 Gulf war is uncertain.[1,2] Claims ranging from a denial of increased mortality[3–7] to millions of excess deaths[8] have been made. The Coalition Provisional Authority and the Iraqi Ministry of Health have identified the halving of infant mortality as a major objective.[9] In the absence of any surveys, however, they have relied on Ministry of Health records. These data have indicated a decline in young child mortality since February, 2001, but because only a third of all deaths happen in hospitals, these data might not accurately represent trends.[10] No surveys or census-based estimates of crude mortality have been undertaken in Iraq in more than a decade, and the last estimate of under-five mortality was from a UNICEF-sponsored demographic survey from 1999.[11,12]

Morgue-based surveillance data indicate the post-invasion homicide rate is many times higher than the preinvasion rate. In Baghdad, a city of 5 million people, 3000 gunshot-related deaths happened in the first 8 months of 2004.[13] One project has kept a running estimate of press accounts of the number of Iraqi citizens killed by coalition forces: at present, the estimated range is 13 000–15 000 (http://www.iraqbodycount.net). Aside from the likelihood that press accounts are incomplete, this source does not record deaths that are the indirect result of the armed conflict. Other sources place the death toll much higher.[14] In a recent BBC article decrying the lack of a reliable civilian death count from the war in Iraq, Ken Roth of Human Rights Watch purports that it will not be possible “to come up with anything better than a good guess at the final civilian cost”.[14]

In the present setting of insecurity and limited availability of health information, we undertook a nationwide survey to estimate mortality during the 14·6 months before the invasion (Jan 1, 2002, to March 18, 2003) and to compare it with the period from March 19, 2003, to the date of the interview, between Sept 8 and 20, 2004.

Methods

We designed the cross-sectional survey as a cohort study, with every cluster of households essentially matched to itself before and after the invasion of March, 2003. Assuming a crude mortality rate of 10 per 1000 people per year, 95% confidence, and 80% power to detect a 65% increase in mortality, we derived a target sample size of 4300 individuals. We assumed that every household had seven individuals, and a sample of 30 clusters of 30 households each (n=6300) was chosen to provide a safety margin. We selected 33 clusters in anticipation that 10% of selected clusters would be too insecure to visit.

We obtained January, 2003, population estimates for each of Iraq's 18 Governorates from the Ministry of Health. No attempt was made to adjust these numbers for recent displacement or immigration. We assigned 33 clusters to Governorates via systematic equal-step sampling from a randomly selected start. By this design, every cluster represents about 1/33 of the country, or 739 000 people, and is exchangeable with the others for analysis. Most communities visited consisted of fewer than 739 000 people. Thus, when referring to a specific cluster by name, this group of 30 households is representing 1/33 or 3% of the country, which may extend beyond the confines of that village or city.

During September, 2004, many roads were not under the control of the Government of Iraq or coalition forces. Local police checkpoints were perceived by team members as target identification screens for rebel groups. To lessen risks to investigators, we sought to minimise travel distances and the number of Governorates to visit, while still sampling from all regions of the country. We did this by clumping pairs of Governorates. Pairs were adjacent Governorates that the Iraqi study team members believed to have had similar levels of violence and economic status during the preceding 3 years. The paired Governorates were: Basrah and Missan, Dhi Qar and Qadisiyah, Najaf and Karbala, Salah ad Din and Tamin, Arbil and Sulaymaniya, and Dehuk and Ninawa.

All clusters were assigned to Governorates without regard to any security considerations. Then, for the six sets of paired Governorates, a second phase of cluster assignment took place. The populations of the two Governorates were added together, and a random number between 0 and the combined population was drawn. If the number chosen was between 0 and the population of the first Governorate, all clusters previously assigned to both clusters went to the first. Likewise, if the random number was higher than the first Governorate population estimate, the clusters for both were assigned to the second. Because the probability that clusters would be assigned to any given Governorate was proportional to the population size in both phases of the assignment, the sample remained a random national sample. This clumping of clusters was likely to increase the sum of the variance between mortality estimates of clusters and thus reduce the precision of the national mortality estimate. We deemed this acceptable since it reduced travel by a third. Table 1 presents cluster groupings and figure 1 shows the location of Governorates.

We assigned clusters to individual communities within the Governorates by creating cumulative population lists for the Governorate and picking a random number between one and the Governorate population. Once a town, village, or urban neighbourhood was selected, the team drove to the edges of the area and stored the site coordinates in a global positioning system (GPS) unit. We assumed the population was living within a rectangle, with the dimensions corresponding to the distances spanned between the site coordinates stored in the GPS unit. The area was drawn as a map subdivided by increments of 100 m. A pair of random numbers was selected between zero and the number of 100 m increments on each axis, corresponding to some point in the village. The GPS unit was used to guide interviewers to the selected point. Once at that point, the nearest 30 households were visited.

	Estimated populations (millions)	Clusters initially assigned at random	Clusters visited after grouping process
Baghdad	5·139	7	7
Ninawa (1)	2·349	3	4
Dehuk (1)	0·650	1	0
Sulaymaniya (2)	1·100	2	3
Arbil (2)	1·100	1	0
Tamin (3)	0·703	1	0
Salah ad Din (3)	1·099	2	3
Diala	1·436	2	2
Anbar	1.261	1	1
Babil	1·828	3	3
Karbala (4)	1·047	1	3
Najaf (4)	1·021	2	0
Wasit	0·815	1	1
Qadisiyah (5)	0·779	1	0
Dhi Qar (5)	1·537	2	3
Muthanna	0·514	0	0
Basrah (6)	1·330	2	0
Missan (6)	0·685	1	3

Numbers in parentheses denote pairings of Governorates.

Table 1: **Estimated populations of Governorates (January, 2003) and assignment of clusters**

The study teams included at least a team leader and one male and one female interviewer. Five of the six Iraqi interviewers were medical doctors. All six were fluent in English and Arabic. All interviewers participated in the revisions and two rounds of field-testing of the questionnaire. Data were recorded in English.

Households were informed about the purpose of the survey, were assured that their name would not be recorded, and told that there would be no benefits or penalties for refusing or agreeing to participate. We defined households as a group of people living together and sleeping under the same roof(s). If multiple families were living in the same building, they were regarded as one household unless they had separate entrances onto the street. If the household agreed to be interviewed, the interviewees were asked for the age and sex of every current household member. Respondents were also asked to describe the composition of their household on

Jan 1, 2002, and asked about any births, deaths, or visitors who stayed in the household for more than 2 months. Periods of visitation, and individual periods of residence since a birth or before a death, were recorded to the nearest month. Interviewers asked about any discrepancies between the 2002 and 2004 household compositions not accounted for by reported births and deaths. When deaths occurred, the date, cause, and circumstances of violent deaths were recorded. When violent deaths were attributed to a faction in the conflict or to criminal forces, no further investigation into the death was made to respect the privacy of the family and for the safety of the interviewers. The deceased had to be living in the household at the time of death and for more than 2 months before to be considered a household death.

Within clusters, an attempt was made to confirm at least two reported non-infant deaths by asking to see the death certificate. Interviewers were initially reluctant to ask to see death certificates because this might have implied they did not believe the respondents, perhaps triggering violence. Thus, a compromise was reached for which interviewers would attempt to confirm at least two deaths per cluster. Confirmation was sought to ensure that a large fraction of the reported deaths were not fabrications. Death certificates usually did not exist for infant deaths and asking for such certificates would probably inflate the fraction of respondents who could not confirm reported deaths. The death certificates were requested at the end of the interview so that respondents did not know that confirmation would be sought as they reported deaths. We defined infant deaths as deaths happening in the first 365 days after birth. Violent deaths were defined as those brought about by the intentional acts of others.

For most clusters, the latitude and longitude was recorded. At the end of interviewing every 30 household cluster, one or two households were asked if in the area of the cluster there were any entire families that had died or most of a family had died and survivors were now living elsewhere. We did this to explore the likelihood that families with many deaths were now unlikely to be found and interviewed, creating a survivor bias among those interviewed. Houses with no one home were skipped and not revisited, with the interviewers continuing in every cluster until they had interviewed 30 households. Survey team leaders were asked to record the number of households that were not home at the time of the visit to every cluster.

We tabulated data and calculated the number of births, deaths, and person-months associated with every cluster. For every period of analysis, crude mortality, expressed as deaths per 1000 people per year, was defined as: (number of deaths recorded/number of person-months lived in the interviewed households) ×12×1000. We estimated the infant mortality rate as the ratio of infant deaths to livebirths in each study period

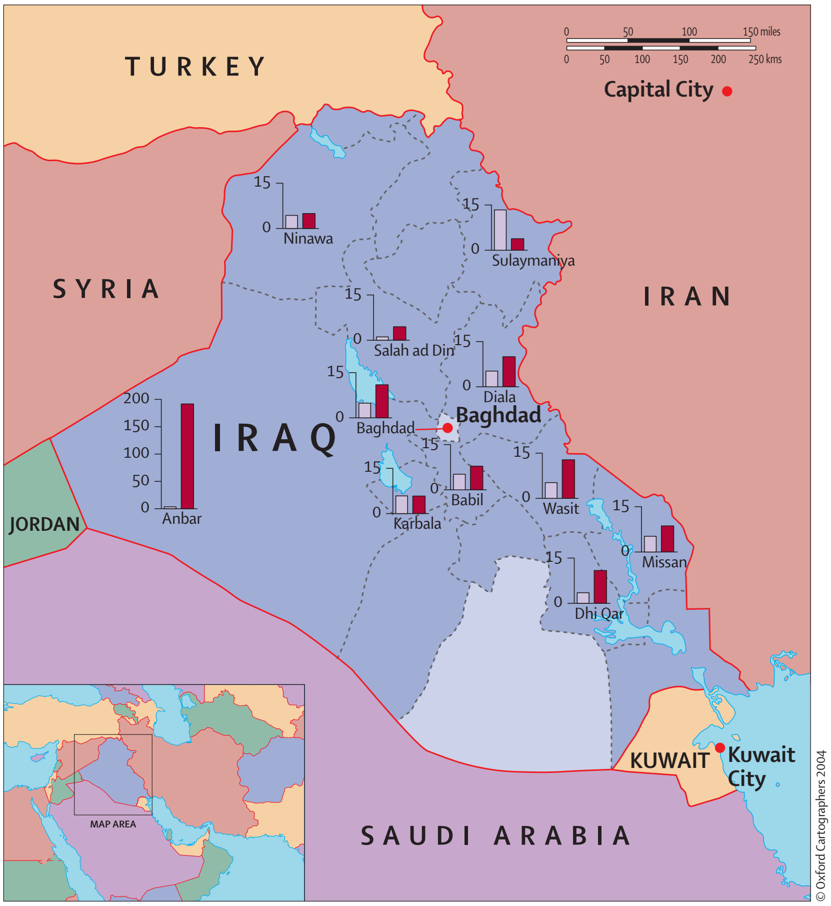

Figure 1: **Crude mortality per 1000 people per year, by Governorate, before and after the invasion**
Bar graphs represent number of deaths per 1000 person-years. Governorate rates are on a scale of 15 deaths per 1000 person-years, except for Anbar governorate, where deaths were more than ten times higher.

and presented this rate as deaths per 1000 livebirths. Mortality rates from survey data were analysed by software designed for Save the Children by Mark Myatt (Institute of Ophthalmology, UCL, London, UK), which takes into account the design effect associated with cluster surveys, and reconfirmed with EpiInfo 6.0. We estimated relative and attributable rates with generalised linear models in STATA (release 8.0). To estimate the relative risk, we assumed a log-linear regression in which every cluster was allowed to have a separate baseline rate of mortality that was increased by a cluster-specific relative risk after the war.[15] We estimated the average relative rate with a conditional maximum likelihood method that conditions on the total number of events over the pre-war and post-war periods, the sufficient statistic for the baseline rate.[16] We accounted for the variation in relative rates by allowing for over-dispersion in the regression.[15] As a check, we also used bootstrapping to obtain a non-parametric confidence interval under the assumption that the clusters were exchangeable.[17] The confidence intervals reported are those obtained by bootstrapping. The numbers of excess

	Children (<15 years)		Men (15–59 years)		Women (15–59 years)		Elderly people (≥60 years)		Total	
	Preinvasion*	Post-invasion†	Preinvasion*	Post-invasion†	Preinvasion*	Post-invasion†	Preinvasion*	Post-invasion†	Preinvasion*	Post-invasion†
Living individuals	2726 (36·7%)	3084 (39·2%)	2187 (29·3%)	2220 (28·2%)	2205 (29·6%)	2233 (28·4%)	320 (4·3%)	331 (4·2%)	7438	7868
Number of deaths	12	53	4	51	9	13	21	25	46	142
Accident-related deaths	1	2	2	7	1	4	0	0	4 (9%)	13 (9%)
Heart attack or stroke	0	2	0	5	3	1	8	10	11 (24%)	18 (13%)
Other chronic disorder	1	2	1	0	2	1	7	8	11 (24%)	11 (8%)
Infectious diseases	0	4	0	0	1	1	0	0	1 (2%)	5 (4%)
Neonatal and unexplained infant	6	10	..	..	..	..	..	..	6 (13%)	10 (7%)
Other‡	4	5	0	1	2	1	6	5	12 (26%)	12 (8%)
Violence	0	28	1	38	0	5	0	2	1 (2%)	73 (51%)
Violent deaths in all clusters except Falluja	..	4	..	13	..	2	..	2	..	21 (15%)
Violent deaths in Falluja	..	24	..	25	..	3	..	0	..	52 (37%)

Note that the preinvasion and post-invasion periods were different lengths. *Living individuals in January, 2002, and number of deaths between Jan 1, 2002, and March 18, 2003. †Living individuals in September, 2004, and number of deaths between March 19, 2003, and Sept 16, 2004. ‡Included different illnesses for the preinvasion and post-invasion periods.

Table 2: **Living household residents and types of deaths reported in 988 households before and after the Iraq invasion, by age**

deaths (attributable rates) were estimated by the same method, using linear rather than log-linear regression. Because the numbers of deaths from the specific causes of death were generally very small, EpiInfo (version 3.2.2, April 14, 2004) was used to estimate the increased risk of cause-specific mortality without regard to the design effect associated with the cluster data.

We estimated the death toll associated with the conflict by subtracting preinvasion mortality from post-invasion mortality, and multiplying that rate by the estimated population of Iraq (assumed 24·4 million at the onset of the conflict) and by 17·8 months, the average period between the invasion and the survey.

This study was approved by the Johns Hopkins Bloomberg School of Public Health Committee on Human Research.

Role of the funding source

The sponsors had no role in the design of the study beyond requiring that the crude mortality be measured and that the portion attributable to violence be documented, and they had no role in data collection, data analysis, data interpretation, or writing of the report. The corresponding author had full access to all the data in the study and had final responsibility for the decision to submit for publication.

Results

All 33 randomly selected locations were visited and 988 households were chosen between Sept 8 and 20, 2004. These households contained 7868 residents on the date of interview. Of these residents, 237 (3%) were younger than 1 year, 1004 (13%) were younger than 5 years, and 3084 (39%) were younger than 15 years. Of the 4453 (57%) residents age 15–59 years, 2220 were men. Of the 331 (4%) residents age 60 years or older, 152 were men.

Five (0·5%) of the 988 households refused to be interviewed. In the 27 clusters with proper absentee records, we visited 872 households and 64 were absent (7%). No households were identified in which all the household members were dead or gone away, except in Falluja, where there were 23. Confirmation of deaths was attempted at 78 households and death certificates were provided in 63 of them.

During the period before the invasion, from Jan 1, 2002, to March 18, 2003, the interviewed households had 275 births and 46 deaths. The crude mortality rate was 5·0 per 1000 people per year (95% CI 3·7–6·3; design effect of cluster survey=0·81). Of the deaths, eight were infant deaths (29 deaths per 1000 livebirths [95% CI 0–64]). After the invasion, from March 19, 2003, to mid-September, 2004, in the interviewed households there were 366 births and 142 deaths—21 deaths were children younger than 1 year. The crude mortality rate during the period of war and occupation was 12·3 per 1000 people per year (95% CI 1·4–23·2; design effect=29·3) and the estimated infant mortality was 57 deaths per 1000 livebirths (95% CI 30–85). More than a third of reported post-attack deaths (n=53), and two-thirds of violent deaths (n=52) happened in the Falluja cluster. This extreme statistical outlier has created a very

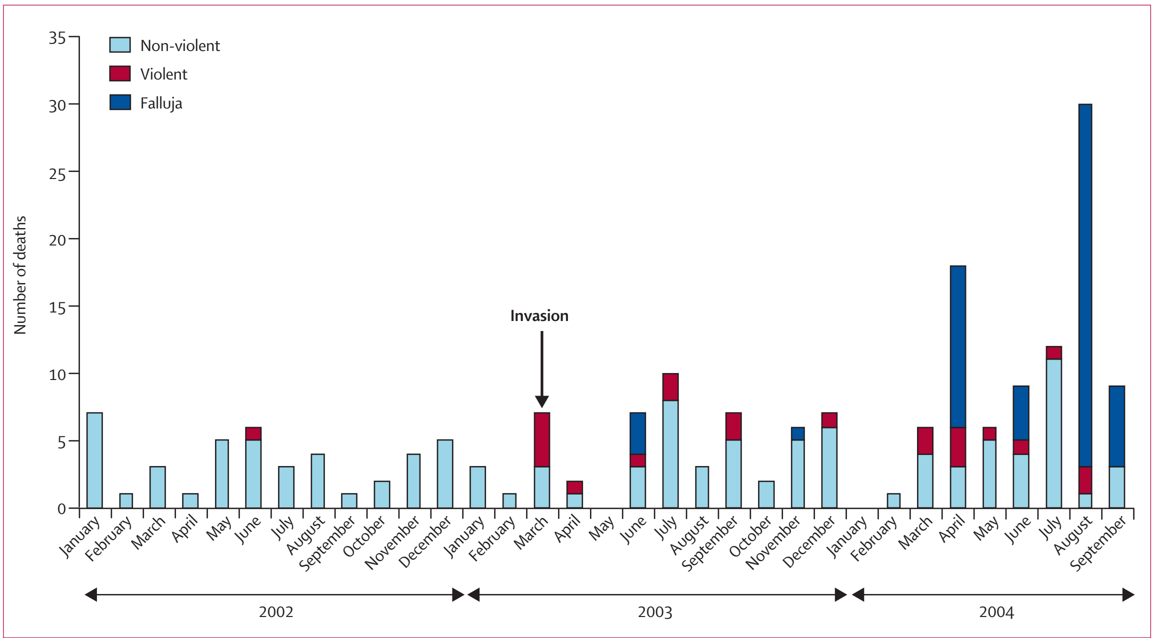

Figure 2: **Number of deaths reported between January, 2002, and September, 2004**

broad confidence estimate around the mortality measure and is cause for concern about the precision of the overall finding. If the Falluja cluster is excluded, the post-attack mortality is 7·9 per 1000 people per year (95% CI 5·6–10·2; design effect=2·0).

After the invasion, 142 deaths were reported in 138 439 person-months of residency. Before the invasion, respondent households reported 46 deaths during 110 538 person-months of residency. As mentioned above, the Falluja cluster is an obvious outlier and might not belong with the others. When included, we estimate that the rate of death increased 2·5-fold after the invasion (relative risk 2·5 [95% CI 1·6–4·2]) compared with before the war. When Falluja was excluded, we estimated the relative risk of death for the rest of the country was 1·5 (95% CI 1·1–2·3).

The main causes of death reported for the 14·6 months before the invasion were myocardial infarction, cerebrovascular accidents, and consequences of other chronic disorders, accounting for 22 (48%) reported deaths (table 2). After the war began, violence was the most commonly reported cause of death, either including (73/142 [51%]) or excluding (21/89 [24%]) the Falluja data, followed by myocardial infarction and cerebrovascular accidents (n=18) and accidents (n=13; table 2).

Figure 2 shows the number of deaths reported during the study period, disaggregated as non-violent deaths, violence in Falluja, and violence in all other clusters. An increase of violent death was noted during the occupation, and violence was geographically widespread, with violent deaths reported in 15 of 33 clusters (45%). Violence-specific mortality rate went up 58-fold (95% CI 8·1–419) during the period after the invasion. Table 2 includes 12 violent deaths not attributed to coalition forces, including 11 men and one woman. Of these, two were attributed to anti-coalition forces, two were of unknown origin, seven were criminal murders, and one was from the previous regime during the invasion. Of the 28 children killed by coalition forces (median age 8 years), ten were girls, 16 were boys, and two were infants (sex was not recorded). Aside from a 14-year-old boy, all these deaths were children 12 years or younger.

Evidence suggests that the mortality rate was higher across Iraq after the war than before, even excluding Falluja. We estimate that there were 98 000 extra deaths (95% CI 8000–194 000) during the post-war period in the 97% of Iraq represented by all the clusters except Falluja. In our Falluja sample, we recorded 53 deaths when only 1·4 were expected under the national pre-war rate. This indicates a point estimate of about 200 000 excess deaths in the 3% of Iraq represented by this cluster. However, the uncertainty in this value is substantial and implies additional deaths above those measured in the rest of the country.

Discussion

This survey indicates that the death toll associated with the invasion and occupation of Iraq is probably about 100 000 people, and may be much higher. We have shown that even in extremely difficult circumstances, the collection of valid data is possible, albeit with limited precision. In this case, the lack of precision does not hinder the clear identification of the major public-health problem in Iraq—violence.

Several limitations exist with this study. Most importantly, the quality of data about births, deaths, and household composition is dependent on the accuracy of the interviews. We attempted to confirm two non-infant deaths per cluster, but in four of the 33 clusters no non-infant deaths were reported, and in some clusters interviewers confirmed deaths in more than two households. In 63 of 78 (81%) households where confirmations were attempted, respondents were able to produce the death certificate for the decedent. When households could not produce the death certificate, interviewers felt in all cases that the explanation offered was reasonable—eg, the death had been very recent, the certificate was locked away and only the husband who was not home had the key. We think it is unlikely that deaths were falsely recorded. Interviewers also believed that in the Iraqi culture it was unlikely for respondents to fabricate deaths.

It is possible that deaths were not reported, because families might wish to conceal the death or because neonatal deaths might go without mention. In other settings, under-reporting of neonatal and infant deaths in similar surveys has been documented.[18,19] In particular, the further back in time the infant death occurred, the less likely it was to be reported. The recall period of this survey, 2·7 years, was longer than most surveys of crude mortality. Thus, infant deaths from earlier periods might be under-reported, and recent infant deaths might be more readily reported, producing an apparent but spurious increase in infant mortality. We do not think that this is a major factor in this survey for two reasons. First, the preconflict infant mortality rate (29 deaths per 1000 livebirths) we recorded is similar to estimates from neighbouring countries.[20] Second, the January, 2002, to March, 2003, rate applied to the 366 births recorded in the interview households post-invasion would project 10·4 infant deaths, whereas we noted 21 to have happened. Of these, three were attributed to coalition bombings and three to births at home when security concerns prevented travel to hospital for delivery. Thus, most of the increase in infant mortality is plausibly linked to the conflict, although we acknowledge the potential for recall bias to create an apparent increase in infant mortality.

We believe it unlikely that recall bias existed in the reporting of non-infant deaths, because of the certainty and precision with which these deaths were reported, and the importance of burial ceremonies in the Iraqi culture. The under-reporting of adult deaths recently or since the invasion to hide combatant deaths would lead us to underestimate the death toll associated with the invasion and occupation of Iraq.

Possibly, respondents did not accurately describe the composition of their households. Although certain individuals might wish to remain hidden, the study team thought that respondents would claim to have more household members than were actually present to justify more ration distributions. This would have the effect of lowering mortality estimates and thus lowering our estimate of the death toll associated with this conflict.

Finally, the sampling strategy somehow might not have captured the overall mortality experience in Iraq. This could occur through one of two mechanisms. First, the use of government population estimates and the selection of households might have under-represented groups such as the homeless, transients, and military personnel. The requirement that the deceased reside in the house for more than 2 months directly before the date of death probably excluded most military casualties. Second, as Spiegel and colleagues documented in Kosovo,[21] there can be a dramatic clustering of deaths in wars where many die from bombings. The cluster survey methodology we used may have, by chance, missed small areas where a disproportionate number of deaths occurred, or conversely, selected a neighbourhood that was so severely affected by the war that it represents virtually none of the population and thus has skewed the mortality estimate too high. The results from Falluja merit extra consideration in this regard.

Falluja was atypical, and perhaps a problematic cluster in three respects. First, it was probably the most violent city in Iraq at the time of the survey. Falluja was the only cluster where GPS units could not be used to find the random starting point. These devices have military uses and their possession resulted in the imprisonment and death of many Iraqis during the previous regime. Since interviewers were stopped and searched repeatedly getting into Falluja, the use of a GPS unit could have resulted in the killing of interviewers. Stopping a car in Falluja at a random point at the date of the visit (Sept 20) and walking away from it was also likely to result in the killing of interviewers. For Falluja, the team assumed an approximate size of the town. They picked a distance down a main road and a number of blocks to the side based on random number selection. Interviewers walked the final 700 m estimating the distance. This presents the potential of subconscious or other forces influencing the selection of the starting point.

Second, at all sites, only 64 households (<8%) were recorded as empty at the time of our visit, and none were abandoned after all or most of the residents had died. In Falluja, 23 households of 52 visited (44%) were either temporarily or permanently abandoned. Neighbours interviewed described widespread death in most of the abandoned houses but could not give adequate details for inclusion in the survey. This presents the possibility that far more deaths had occurred than were reported and the interviewees that remained were the relatively lucky ones (underestimating mortality), or large numbers of residents had fled elsewhere and were still alive. Thus, the deaths reported by the remaining families might represent a disproportionate number of deaths from the larger community that used to live in the area, leading the interview data to overestimate mortality.

Third, interviewers might, by chance, have gone to an atypical area for the Falluja cluster. We do not believe this to be the case. In the random selection process, other heavily damaged cities such as Ramadi, Najaf, and Tallafar were not selected. Moreover, the cluster in Thaura (Sadr City), the site of the most intense fighting in Baghdad, by random chance was in an unscathed neighbourhood with no reported deaths from the months of recent clashes. In Falluja, the team noted that vast areas of the city had been devastated to an equal or worse degree than the area they had randomly chosen to survey. We suspect that a random sample of 33 Iraqi locations is likely to encounter one or a couple of particularly devastated areas. Nonetheless, since 52 of 73 (71%) violent deaths and 53 of 142 (37%) deaths during the conflict occurred in one cluster, it is possible that by extraordinary chance, the survey mortality estimate has been skewed upward.

To account for the potential that the Falluja data are profoundly skewing the mortality estimate or the potential that there is a recall bias in the infant mortality data, a lowest plausible death toll has been calculated excluding the Falluja data and assuming that half the measured increase in infant mortality has been an artifact of selective recall. Removing half the increase in infant deaths and the Falluja data still produces a 37% increase in estimated mortality. The inclusion of this estimate does not mean that investigators believe that either bias has occurred. Instead, this estimation reflects the concern that investigators cannot fully discard the potential for bias from these two factors.

The increase in reported infant mortality among interviewed households is consistent with a well documented pattern seen in armed conflict.[22,23] Many mothers reported that security concerns led them to deliver their children at home since the invasion. It is surprising that beyond the elevation in infant mortality and the rate of violent death, mortality in Iraq seems otherwise to be similar to the period preceding the invasion. This similarity could be a reflection of the skill and function of the Iraqi health system or the capacity of the population to adapt to conditions of insecurity.

Passive surveillance systems often have low sensitivity, and the fact that the estimate of coalition casualties from http://www.iraqbodycount.net is a third to a tenth the estimate reported in this survey should be of little surprise. What is particularly revealing about the Iraqbodycount.net system is that, as a monitor of trends, it closely parallels the results found in this survey: most casualties arose after the end of major hostilities in May, 2003, and the rate of civilian deaths has been rising in recent months. This finding indicates that passive media-based monitoring should have a role in future conflicts where the collection of health data is not practical. However, it should be used as a monitor of trends rather than as a count estimator, as Iraqbodycount.net has been most commonly cited in the media.[14]

Despite widespread Iraqi casualties, household interview data do not show evidence of widespread wrongdoing on the part of individual soldiers on the ground. To the contrary, only three of 61 incidents (5%) involved coalition soldiers (all reported to be American by the respondents) killing Iraqis with small arms fire. In one of the three cases, the 56-year-old man killed might have been a combatant. In a second case, a 72-year-old man was shot at a checkpoint. In the third, an armed guard was mistaken for a combatant and shot during a skirmish. In the latter two cases, American soldiers apologised to the families of the decedents for the killings, indicating a clear understanding of the adverse consequences of their use of force. The remaining 58 killings (all attributed to US forces by interviewees) were caused by helicopter gunships, rockets, or other forms of aerial weaponry.

Many of the Iraqis reportedly killed by US forces could have been combatants. 28 of 61 killings (46%) attributed to US forces involved men age 15–60 years, 28 (46%) were children younger than 15 years, four (7%) were women, and one was an elderly man. It is not clear if the greater number of male deaths was attributable to legitimate targeting of combatants who may have been disproportionately male, or if this was because men are more often in public and more likely to be exposed to danger. For example, seven of 12 (58%) vehicle accident-related fatalities involved men between 15 and 60 years of age.

US General Tommy Franks is widely quoted as saying “we don’t do body counts”.[14] The Geneva Conventions have clear guidance about the responsibilities of occupying armies to the civilian population they control. The fact that more than half the deaths reportedly caused by the occupying forces were women and children is cause for concern. In particular, Convention IV, Article 27 states that protected persons “. . . shall be at all times humanely treated, and shall be protected especially against acts of violence . . .”. It seems difficult to understand how a military force could monitor the extent to which civilians are protected against violence without systematically doing body counts or at least looking at the kinds of casualties they induce. This survey shows that with modest funds, 4 weeks, and seven Iraqi team members willing to risk their lives, a useful measure of civilian deaths could be obtained. There seems to be little excuse for occupying forces to not be able to provide more precise tallies. In view of the political importance of this conflict, these results should be confirmed by an independent body such as the ICRC, Epicentre, or WHO. In the interim, civility and enlightened self-interest demand a re-evaluation of the consequences of weaponry now used by coalition forces in populated areas.

Contributors

L Roberts was the lead investigator in the field and was principally responsible for the data analysis, interpretation, and preparation of this report. R Lafta was involved in study design, hired, trained, and oversaw the interview staff, led one of the two study teams, coordinated all logistical aspects of the study, and had a central role in data interpretation and preparation of this report. R Garfield advised on issues of study design, study execution, participated in the analysis and interpretation of data and preparation of this report, and initially organised the study team. J Khudhairi was involved in the study design, interviewer training, and oversaw one of the two survey teams in the field. G Burnham advised on issues of study design, study execution, participated in the analysis and interpretation of data and preparation of this report, and organised and facilitated the ethics review process at Johns Hopkins University.

Conflict of interest statement

We declare that we have no conflict of interest.

Acknowledgments

This survey was funded by the Center for International Emergency Disaster and Refugee Studies, Johns Hopkins Bloomberg School of Public Health and the Small Arms Survey in Geneva Switzerland, whose support is greatly appreciated. Special thanks to Walt Jones for swiftly facilitating this project. Reference support was provided by the Sidney Memorial Library in Sidney, NY, USA and assistance with figure 1 was provided by Marite Jones. This work could not have been completed without a host of brave Iraqis who endured danger, police interrogations, and the risk of being associated with foreign investigators. Many thanks to Elizabeth Johnson and Scott Zeger of the Johns Hopkins Bloomberg School of Public Health, Department of Biostatistics, for assistance with data analysis. Finally, thanks to Helen Wolfson for data cleaning and tabulation and Mary Grace Flaherty for editing this manuscript.

References

1 Global Policy Forum, Save the Children UK. Iraq sanctions: humanitarian implications and options for the future. http://www.globalpolicy.org/security/sanction/iraq1/2002/paper.htm (accessed Feb 7, 2003).

2 Campaign against sanctions on Iraq. http://www.cam.ac.uk/societies/casi/index.html (accessed Feb 7, 2003).

3 Clawson PA. Oil for food or the end of sanctions. *Policywatch* 1998; **303**: 1–4.

4 Cordesman AH. Sanctions and the Iraqi people: the WHO report and conflicting views. Washington, DC: Center for Strategic and International Studies, 1997.

5 Baram A. The effect of Iraqi sanctions: statistical pitfalls and responsibility. *Middle East J* 2000; **54:** 197–223.

6 Rubin M. Food fight. *The New Republic*; 2001 (June 18): 18.

7 Welch M. The politics of dead children: have sanctions against Iraq murdered millions? *Reason Magazine* 2002: 52–59. http://www.reason.com/0203/fe.mw.the.shtml (accessed Oct 26, 2004).

8 Reuters News Service. Iraq says sanctions have killed 1·4 million. http://CNN.com/WORLD/meast/9808/10/RB000433.reut.html (accessed Aug 11, 1998).

9 CPA. An historic review of CPA accomplishments. http://www.cpa-iraq.org (accessed Sept 26, 2004).

10 Garfield R. Morbidity and mortality among Iraqi children from 1990–1998: assessing the impact of economic sanctions. http://www.fourthfreedom.org/Applications/cms.php?page_id=7 (accessed Oct 26, 2004).

11 Ali MM, Shah IH. Sanctions and childhood mortality in Iraq. *Lancet* 2000; 355: 1851–57.

12 Ali M, Blacker J, Jones G. Annual mortality rates and excess deaths of children under five in Iraq, 1991-98. *Popul Stud* 2003, **57:** 217–26.

13 Berenson A. Killings surge in Iraq, and doctors see a procession of misery. *New York Times*, Sept 26, 2004.

14 Davis M. Counting the civilian cost in Iraq. http://news.bbc.co.uk/2/hi/middle_east/3672298.stm (accessed Sept 22, 2004).

15 McCullagh P, Nelder JA. Generalized linear models. London: Chapman and Hall, 1989.

16 Diggle PJ, Heagerty P, Liang KY, Zeger SL. Analysis of longitudinal data, 2nd edn. New York, NY: Oxford Science Publications, 2002.

17 Efron B. Bootstrap methods: another look at the jackknife. *Ann Stat* 1979; **7:** 1–26.

18 Becker SR, Thornton JN, Holder W. Infant and child mortality in two counties of Liberia: results of a survey in 1988 and trends since 1984. *Int J Epidemiol* 1993; **22** (suppl 1): S56–63.

19 Taylor WR, Chahnazarian A, Wienman J, et al. Mortality and use of health services surveys in rural Zaire. *Int J Epidemiol* 1993; **22** (suppl 1): S15–19.

20 WHO. Health action in crises. http://www.who.int/disasters/stats/baseline.cfm?countryID=62 (accessed Oct 16, 2004).

21 Spiegel PB, Salama P. War and mortality in Kosovo, 1998–99: an epidemiological testimony. *Lancet* 2000; **355:** 2204–09.

22 CDC. Famine-affected, refugee, and displaced populations: recommendations for public health issues. *MMWR Recomm Rep* 1992; **41**(RR-13): 1–76.

23 CDC. Elevated mortality associated with armed conflict: Democratic Republic of Congo, 2002. *MMWR Morb Mortal Wkly Rep* 2003; **52:** 469–71.