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Forced migration and child health and mortality in Angola☆

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ABSTRACT

This study investigates the effects of forced migration on child survival and health in Angola. Using survey data collected in Luanda, Angola, in 2004, just two years after the end of that country's prolonged civil war, we compare three groups: migrants who moved primarily due to war, migrants whose moves were not directly related to war, and non-migrants. First, we examine the differences among the three groups in under-five mortality. Using an event-history approach, we find that hazards of child death in any given year were higher in families that experienced war-related migration in the same year or in the previous year, net of other factors. To assess longer-term effects of forced migration, we examine hazards of death of children who were born in Luanda, i.e., after migrants had reached their destinations. We again observe a disadvantage of forced migrants, but this disadvantage is explained by other characteristics. When looking at the place of delivery, number of antenatal consultations, and age-adequate immunization of children born in Luanda, we again detect a disadvantage of forced migrants relative to non-migrants, but now this disadvantage also extends to migrants who came to Luanda for reasons other than war. Finally, no differences across the three groups in child morbidity and related health care seeking behavior in the two weeks preceding the survey are found. We interpret these results within the context of the literature on short- and long-term effects of forced migration on child health.

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Introduction

Child health outcomes and childhood mortality, in particular, are widely accepted as indicators of the overall health and social welfare of a population. Under the framework of the Millennium Development Goals, sub-Saharan nations are enjoined to reduce childhood mortality rates by 2015 to one-third of the 1990 levels (MDG Report, 2006). However, progress towards these goals is not only undermined by poverty and economic crisis that prevails under peaceful conditions but also by armed conflicts that periodically flare up around the continent. Health and welfare of children of refugee and displaced populations have therefore been the main focus of humanitarian efforts in war-ridden African countries and have generated considerable attention in the literature (e.g., African-European Institute, 1990; Kinfu, 1999; Singh, Karunakara, Burnham, & Hill 2005a, 2005b). However, the complexities of the effects of military conflicts on health are poorly understood and rarely studied.

The literature on the consequences of forced migration for child health and mortality can be synthesized in two categories: studies focusing on immediate effects of forced migration and studies that examine longer term effects. In the immediate term, studies overwhelmingly demonstrate the mortality disadvantage of children of displaced populations compared to the host population. Thus in a study of published mortality rates from 37 conflict zones, Guha-Sapir and Gijsbert (2004) illustrated increased vulnerabilities among children under five. Similarly, O'Hare and Southhall (2007) and Hamill and Houston (2000) found a negative association between recent military conflict and under-five survival and other child and maternal health indicators in sub-Saharan Africa and 137 other countries around the world respectively. Studies of displaced populations in specific countries engaged in conflict such as the Democratic Republic of Congo, Mozambique, and Eastern Sudan, yielded similar findings (Ahoua, Tamrat, Duroch, Grais, & Brown, 2006; Doocy, Burnham, & Robinson, 2007; Macassa, Ghilagaber, Bernhardt, & Burstro, 2003; Van Herp, Parque, Rackley, & Ford, 2003).

In the longer term, however, findings on the effects of forced migration on child health and mortality are less conclusive, with some studies indicating that forced migration does not significantly increase under-five mortality relative to that of host populations. Thus Singh et al. (2005a) compared refugees and the host population in northwestern Uganda and Sudan and found no

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differences in under-five mortality between the two. In a follow-up study by the same authors, women who did not migrate in a situation of conflict and women who repatriated before the age of 15 experienced higher under-five mortality rates compared with women who were currently refugees and who repatriated after the age of 15 (Singh et al., 2005b). Hynes, Sheik, Wilson, and Spiegel (2002) also found better child health outcomes among refugees compared to the host population and those who had not migrated. Studies of infant and child mortality among Palestinian refugees documented a health advantage of refugees relative to their non-refugee counterparts (Khawaja, 2004; Madi, 2000). Yet other studies linked forced migration with enduring mortality disadvantages. Thus, in a study in rural South Africa, no differences in infant mortality were found between former Mozambican refugee households and households of southern African natives but differences were detected in childhood mortality four years after the arrival of refugees in host destination (Hargreaves, Collinson, Kahn, Clark & Tollman, 2004). And a study of former refugee and non-refugee households in Rwanda showed lower survival chances among children of refugee women (Verwimp & Van Bavel, 2005).

Findings of longer-term impact of forced migration are not limited to child mortality but are also documented for other child health outcomes. The importance of vaccination as a preventive measure for reducing child mortality has long been established in the general population (Brockerhoff & DeRose 1996; Kristensen, Aaby, & Jensen, 2000; Nyarko, Pence, & Debpuur, 2001), and several studies have examined child immunization among refugee and displaced populations. Thus Senessie, Gage, and von Elm (2007) in their study of child immunization in the context of the civil war in Sierra Leone found that children born during increased hostilities had inappropriate immunization for age. Another study by Nielsen, Benn, Bale, Martins, and Aaby (2005) found that vitamin A supplementation may have a particularly beneficial impact on child survival in conflict situations.

Conceptualization and hypotheses

Our study contributes to the literature on short- and long-term consequences of forced migration for childhood mortality and health by looking at childhood mortality and selected child health indicators. We go beyond the common assumption that physical threat is the main, if not only, motivation for migration during conflict (Davenport, Moore, & Poe, 2003; Moore & Shellman 2004). We argue that in settings of military conflicts, especially of those that are of a protracted and intermittent nature, not all migration is directly driven by fear of hostilities and therefore an exclusive focus on war-induced migration results in an incomplete picture of the complex relationship among armed conflict, migration, and health outcomes. We propose therefore, that war-driven migration should be distinguished from and analyzed in connection with migration that is not directly caused by hostilities.

The two types of migration are obviously different but may also bear similarities. The literature on migration and demographic, especially reproductive, behavior and outcomes can usefully illuminate these differences and similarities. This literature typically points to mechanisms of selection (migrants are self-selected on individual characteristics such as age, sex and socio-economic status), disruption (migrants are temporarily separated from their spouse, families or comfort zones) and adaptation (migrants eventually adapt to the social and cultural norms in their new place of residence) to explain fertility differences by migration status (e.g., Chattopadhyay, White, & Debpuur, 2006; Goldstein, White, & Goldstein, 1997). In this study, we adapt these perspectives to conceptualize the connections between family migration and child health outcomes. Thus, it is proposed that migratory moves that take place in the absence of direct threats to personal security might be better prepared than flights from all-out hostilities, which are usually unexpected and therefore are typically more disruptive. Similarly, non-war migrants may be selected based on distal factors such as the tendency to space births, avoidance of nutrient deficiency and health status, education and socio-economic characteristics. Lastly, adaptation mechanisms may eliminate any remaining differences in the three migration groups, thereby leveling off any short-term effects observed in child health outcomes in the process or immediate aftermath of migration.

Hence we compare child survival and health across three groups—migrants whose migration was triggered directly by hostilities (hereafter "war migrants"), migrants whose migration was motivated by reasons unrelated to war ("non-war migrants"), and long-term residents of a community that has served as the migration destination for the two migrant groups. To analyze the immediate effects of war and non-war migration experience on childhood mortality, we adopt a dynamic approach by comparing whether the probability of child death before the age of five in any given year is affected by experiencing war or non-war migration in that year relative to those not migrating. It is also plausible to expect that the short-term effects of migration experience may not exhibit within the year of migration, especially if migration occurs towards the end of the year, thus we also provide for the effects of migration to be lagged by one year. Within this approach, we hypothesize that, in the short term, war migration will be associated with higher probabilities of death relative to not migrating. At the same time, non-war migration will be associated with comparable or lower probabilities of death relative to not migrating. The rationale for this hypothesis is that war has a direct impact on childhood mortality both through malnourishment and through social and psychological distress that impacts on war migrants in the immediate aftermath of their migration. Although non-war migration may also be associated with considerable adversity, the effects of such adverse factors can be countervailed for by the positive selection of non-war migrants.

To examine the long-term effects of the type of migration on the probability of death, we limit our analysis to children born to the three groups under comparison in the place of migration destination. To provide a broader context for the analysis of the long-term effects of migration experience, we note that even in times of peace, poverty and economic conditions in sub-Saharan Africa compromise a country's ability to provide health services for children, especially those from marginalized socio-economic strata. This is exacerbated in times of war, when immunization programs and other interventions known to improve child health may either stagnate or wither (Samb, Aaby, Whittle, Seck, & Simondon, 1997). Within this context, we conceptualize that if adaptation mechanisms guide the long-term child mortality and health outcomes, we expect both war migrants and non-war migrants, ceteris paribus, to be similar to non-migrants in post-migration probabilities of child death. However, if the enduring effects of the selection mechanisms prevail, these mechanisms should work to reduce any disadvantage of non-war migrants relative to non-migrants. If the disruption mechanisms linger after migration, it would result in a continuing disadvantage of war migrants.

In addition to childhood mortality, we look at long-term effects of migration status on other indicators of child health. Again, if the disruptive effects caused by war migration endure after migration, we should expect children of war migrants to display disadvantages in health outcomes. Specifically, we hypothesize that compared to non-migrants and non-war migrants, children of war migrants would be less likely to be born outside a health facility, to have fewer prenatal consultations, to have a lower likelihood of being fully immunized, have higher levels of child morbidity, and lower use of health care. Yet if adaptation mechanisms prevail, these health indicators among the three groups should converge. Finally, the selection mechanisms could create an advantage for non-war migrants.

Setting

Our study is focused on Angola, a country of 13 million people in South-Western Africa. Angola was mired in prolonged periods of devastating civil war since it gained independence from Portugal in 1975. However, the hostilities effectively ended in 2002 and since that time the country has lived in relative peace. Despite the end of the civil war and a rapid macroeconomic growth fueled by high oil and commodity prices in years that followed, Angola's health indicators, and especially those pertaining to child health, remain among the worst in the world. At 132 infant deaths per 1000 live births, Angola ranks only below Afghanistan, Sierra Leone and Liberia in infant mortality (PRB, 2008). One in four Angolan children die before the age of five and children face other huge challenges such as malnutrition, landmines, and orphanhood due to HIV/AIDS. In the face of these numerous challenges, UNICEF recently labeled Angola one of the worst countries in the world to be a child (UNICEF, 2005).

A distinguishing feature of the civil war in Angola is that it did not affect the entire country equally: some parts of the country saw disproportionate hostilities and devastation, while in others the effects of war were largely indirect (Agadjanian & Prata, 2003). Thus, Luanda, Angola's capital city and by far the largest city, remained one of the safe havens that attracted displaced persons from the urban and rural parts of the country that were affected by intense fighting. Many migrants, however, came to Luanda for reasons not directly related to war—to join relatives and to look for better educational and economic opportunities. This combination of Luanda-bound war migration and non-war migration makes the city an ideal setting for this study.

Data and methods

Data

The data for this paper come from a 2004 survey conducted in two peri-urban municipalities of Greater Luanda—Samba and Viana. The two municipalities were chosen based on the estimated share of war migrants in their population. Thus, Samba, a more established municipality closer to the city core was known to have fewer war migrants than the more distant and less urbanized Viana. The sample was drawn separately in each municipality. Within each municipality, the sample for the survey was drawn separately in each *bairro* (administrative subdivision)—eight *bairros* in Viana and nine *bairros* in Samba—with each bairro's sample size being proportional to its estimated total population size. Households within each bairro were chosen based on a random walk algorithm. In each household, one resident, either a man aged 15–59 or a woman aged 15–49 was randomly selected for interview. The survey yielded 1081 observations, evenly divided between the two municipalities. Overall, 61 of the selected households had to substituted, of which 44 was because of unavailability of selected individuals and only 17 (less than 2%) due to refusal to participate in the study. Because of the relatively low non-response rate, no adjustments for non-response were made in the analysis.

The survey questionnaire was conducted in Portuguese, Angola's official language that is widely spoken in Luanda, by an interviewer of corresponding gender; most respondents had no difficulties answering the survey questions in that language (in the few cases when respondents had limited Portuguese proficiency, interviewers with relevant local language skills were used). The survey was approved by the Arizona State University Institutional Review Board. The Institute for Economic and Social Research (AIP), the Angolan contractor that implemented the data collection, obtained the necessary permissions and clearances in Angola.

In addition to standard socio-economic information, the survey gathered detailed data on birth histories (the month and year of birth and death if the child died), the place of delivery (institution vs. home or other) of the youngest living child, and the number of antenatal consultations before the birth of that child. The immunization information on the youngest child at the time of the survey was also collected. The respondents were asked whether any of their children had any of the following symptoms in the two weeks preceding the survey: fever, strong cough, respiration difficulties, headaches, stomach aches, or diarrhea. Finally, the respondents who reported those symptoms were asked whether they took the sick child to a public or a private health care facility or did not seek care from these facilities.

Like with any type of retrospective data, some deaths may have been underreported and dates of birth and death misreported. However, there are no reasons to expect that these problems vary systematically across migration categories and therefore they should not bias the analysis of differentials by migration experience.

The survey also collected detailed migration histories. Respondents who ever migrated were asked to give the localities of their previous residence, the timing of migration and the reasons why they migrated from the localities of their previous residence. These responses were classified by interviewers into six categories (war-related, economic, family, education, health or other). These responses are used to construct key predictors of the study—the migration experience and migration status (i.e., war migration, non-war migration, and non-migration).

Methods

Our analysis is limited to women who reported at least one live birth or men whose spouses have had at least one birth (66.5% of the sample; 380 women and 339 men). For the analysis of immediate impact of migration on child survival we use discrete-time logistic regression (Barber, Murphy, Axinn, & Maples, 2000). This method is appropriate for this analysis as it allows for a more efficient study of deaths and the handling of censored cases (in this case, children who survive the date of the survey) than continuous-time models such as Cox regression. Although deaths were recorded by month and year, migration information was collected only by year; therefore the unit of analysis is person-year. Each child is therefore at risk of death for each year of migration until the child dies or is censored (child survives to the day of interview). To account for clustering of observations due to the inclusion of several children born to the same respondent and to protect against deflated standard errors and biased hypotheses test, we use random intercept models. The model is fitted with the XTLOGIT procedure in STATA.

The outcome variable for this model is the probability of child death which is a dichotomous measure of whether a child died in a given year (1 if child died and 0 if child did not die). Migration experience in a given year is the predictor, operationalized as a set of dummy variables. In any given year, a respondent is classified as migrating because of war (1 if yes and 0 if no) if he or she migrated in that year and indicated that war was a reason for migrating (regardless of other reasons also stated). If migration took place in a year but war was not said to be a reason for migrating, then the respondent is classified as a non-war migrant in that year (1 if yes and 0 if no). The last migration experience category is made up of those who did not migrate in a given year (1 if did not migrate and 0 if migrated for any reason).

Consistent with previous literature on child mortality, the model controls for several child and parent characteristics (because we have detailed information on only one parent—the survey respondent—we use her/his characteristics as controls). Thus the model controls for childs and parent's age (in years) as time-varying covariates. It also controls for child's gender and parent's education (coded in three categories according to the level of education: up to four years of schooling; between five and eight years of schooling; and nine years or more of schooling) as time–invariant covariates. We also include parent's marital status as a time-varying dichotomous indicator of whether or not a child was living in a single-parent or a two-parent household. Lastly, because Angola's prolonged civil war varied in intensity over the years, the model also controls for the level of intensity of hostilities in a given year (coded 1 for years 1993, 1994, 1999, 2000, 2001, i.e., when hostilities were particularly intense, and 0 for other years, when the intensity of fighting was relatively low). Due to concerns about causal ordering, socio-economic characteristics reported for the time of the survey are not included as covariates.

The estimated model for under-five mortality can be specified in the following form:

$$\mathrm{In}(P_{jt}/1 - P_{jt}) = ß0_j + ß1X_{jt} + ß2z_j + ß3T_{jt}$$

where P_{jt} is the probability of dying for individual j in year t, β_{0j} is the intercept that varies randomly across children in the same families, $\beta_1, \beta_2, \beta_3$ are vectors of coefficients, X_{jt} is a vector of type of migration experience (war, non-war, none) in year t, z_j is a vector of covariates of child (child's age and child' sex) and respondent characteristics (respondents age, marital status and education) that impact on child health and T_{jt} is a specification for the baseline hazard of death.

The discrete-time models used in this study, like all discrete-time models, assume that each predictor included in the model has proportional and identical effects in each time period under consideration (Singer & Willett, 2003). This may or may not be valid for our analysis. However, the variation of hazards overtime is not a main theoretical focus of this paper nor can it be adequately captured with our data: although we have month and year of death, to study period effects, migration is only recorded by year. We thus acknowledge that this constitutes a potential limitation of our study.

Similarly, our models impose the assumption of the absence of unobserved heterogeneity, implying that the population hazard function for each individual depends only on the predictor values included (Singer & Willett, 2003). Like in any analysis using retrospective data, we are unable to include all relevant predictors of the outcome of interest—childhood mortality. Hence, the model may be in violation of the assumption about the absence of unobserved heterogeneity. Fitting a model that accounts for unobserved heterogeneity ideally requires additional data on repeated events within individuals, which this study lacks. However, the use of random intercept models provides some solution to unobserved heterogeneity. Not only do random effects models test for dependence among observations but also capture the effects of omitted variables in the model (Rodriguez, 2005).

For the analysis of long-term effects of migration on childhood mortality, we use a slightly different predictor, which we define as "migration status." This definition anchors both war-related and war-unrelated migration in the history of Angolan civil conflict, using the year of 1992, when, after contested general elections, the country descended into a generalized (even if intermittent) civil war, as a milestone. Thus for this analysis, we fit an event-history model with the main effects of migration status on childhood mortality using three migration status groups: war migrants who came to Luanda after 1991 (i.e., after the outbreak of hostilities), non-war migrants who came to Luanda after 1991; and Luanda's longtime residents, i.e., those who were born in Luanda or moved there before 1992 regardless of the reasons for their move. Accordingly, this analysis is restricted to children born in Luanda after 1991. The model includes the same controls as the previous model (except for the intensity of hostilities) and also controls for municipality of residence at the time of the survey (our data suggest that mobility within the city was very low among the survey sample).

To study longer-term effects of migration status on child health we consider several outcomes. First, we look at the number of antenatal visits. Because this is a count variable with a non-normal distribution, we use negative binomial regression. This model is preferred over Poisson regression model data because it is better suited for overdispersed data. We also use logistic regression for binary outcomes to examine the probability of child delivery in a health facility and the probability of a child being fully immunized for age. The guidelines for child immunization in Angola at the time were such that all vaccinations should be completed by the age of eight months. To allow for possible inaccuracies in the reporting of child age and for delays in completing the immunization schedules, we limit the immunization-for-age analysis to children who were at least 12 months at the time of the survey. In all three tests we use the same migration status groups and limit our analysis to the youngest child born in Luanda after 1991. We employ similar covariates as used in the previous event-history model. The model predicting the youngest child's age-adequate immunization controls for child's age in months rather than years and also controls for the place of delivery of that child.

Results

Table 1 presents descriptive statistics of selected socio-demographic characteristics by migration status and gender. The sample was evenly distributed between the two municipalities; Viana (51.0%) and Samba (49.0%), by the study design, the share of migration status groups varied considerably between the two municipalities. More than two-thirds (73%) of war migrants in the sample were Viana residents, compared to about 38% of non-war migrants. The distribution of non-migrants was slightly more even, although more of them resided in Samba (55.5%) than in Viana (44.5%). Non-war migrants were the youngest (29.6 years) compared to war migrants and non-migrants who were both 32 years on average. Similarly, non-war migrants had the lowest mean number of children ever born (3.1) compared to war migrants (3.6) and non-migrants (3.4). However, they had the highest share of those with a current marital partner at the time of the survey (90.3%). Lastly, war migrants were by far the least educated with 42% of them having less than five years of schooling and only 10.7% in the highest-education category.

Table 2 presents the results of two sets of random-intercepts event-history models. The results presented in Panel A are from discrete-time logistic regression predicting the hazard of a child's death in any given year from migration experience in that year. The baseline model includes only migration experience and child's age (baseline hazard of death). As we can see, the fact of war-related migration significantly increases the probability of death relative to not migrating. The effect of migrating for non-war reasons is in a similar direction but is not statistically significant. When we added controls, the effect of war migration decreases slightly in magnitude and is now only marginally significant. Panel B presents the results from the same models but the effects of migration are now lagged by one year. The disadvantage of children experiencing war-driven migration is now even more conspicuous: in the baseline model, war migration is associated with a threefold increase in the odds of child's death relative to not migrating. When controls are added, the disadvantage of children who experienced war migration decreases in magnitude (OR = 2.3) but remains statistically significant.

Table 1
Socio-demographic profile of respondents by gender and migration status (percentages unless noted otherwise), 2004 Forced Migration Survey.

	War migrants			Non-war migrants			Non-migrants			
	Women	Men	All	Women	Men	All	Women	Men	All	All
Municipality										
Samba (more urbanized)	15.9	37.8	27.3	67.3	56.3	62.1	53.0	58.5	55.5	49.0
Viana (less urbanized)	84.2	62.2	72.7	32.7	43.8	37.9	47.0	41.5	44.5	51.0
Age (Mean)	29.7	34.3	32.1	27.9	31.7	29.6	29.4	35.3	32.1	31.7
Children ever born (mean)	3.9	3.2	3.6	2.9	3.2	3.1	3.1	3.7	3.4	3.4
Currently married/living with a partner	75.6	84.4	80.2	92.7	87.5	90.3	76.1	86.5	80.9	82.1
Education										
4 years or less	68.4	18.9	42.0	35.2	13.0	25.0	35.0	13.5	25.1	29.4
5–8 years	26.6	65.6	47.3	48.2	65.2	56.0	44.0	49.0	46.3	47.9
9 years or more	5.1	15.6	10.7	16.7	21.7	19.0	20.9	37.5	28.6	22.8
Number of cases	82	90	172	55	48	103	234	200	434	719[a]
Percentage in sample	11.4	12.52	23.92	7.65	6.68	14.33	32.55	27.82	60.36	100[a]

[a] 10 missing cases excluded.

Table 3 presents the results of event-history models that focus on the effect of parent's migration status on the probability of death of children born in Luanda in twelve years preceding the survey (since the outbreak of hostilities following the 1992 elections, a major milestone in Angola's transition to a generalized, even if intermittent, civil war). The results from the baseline model point to a higher likelihood of death among children of both war and non-war migrants relative to children of non-migrants. However, although both corresponding coefficients are similar in magnitude, only the coefficient for war migrants is significantly different from zero. When we add controls, the effect of being a child of a war migrant decreases more than that of being a child of a non-war migrant, but neither effect is now statistically significant.

To further examine possible longer term effects of war migration and non-war migration we now turn to selected indicators of potential child health risks. To remind, all the remaining tests are also restricted to children who were born in Luanda, i.e., after both war and non-war migrants had reached their last migration destination. First, we look at the number of antenatal consultations before the birth of the youngest child. Panel A of Table 4 displays the results of a negative binomial regression predicting the number of antenatal consultations before the birth of the youngest child. Again, in the baseline model, war migrants display a strong and statistically significant disadvantage compared to non-migrants and long-term residents. Non-war migrants also tended to have fewer antenatal consultations than their non-migrant counterparts, but the difference is much more modest than in the case of war migrants. When controls are added to the model, the gap between war migrants and non-migrants narrows but remains statistically

Table 2
Odds ratios from random-intercept discrete-time hazard models of the effects of migration experience in same or preceding year on the probability of child death, 2004 Forced Migration Survey.

	A. Migration in same year		B. Migration in preceding year	
	Model 1	Model 2	Model 1	Model 2
Migration				
War migration	2.250*	2.112+	3.035**	2.334*
Non-war migration	1.703	1.434	0.667	0.589
[No migration]				
Child's age (years)	0.613**	0.633**	0.612**	0.631**
Child's sex				
Male		0.762+		0.762+
[Female]				
Respondent's age (years)		0.964**		0.964*
Respondent's marital status				
Married		0.505**		0.762**
[Not married]				
Number of children ever born		1.040		0.762
Respondent's education				
[Four years or less]				
Five to eight years		0.862		0.867
Nine years or more		0.406**		00.3.043099*
Intensity of hostilities in given year				
High Intensity		1.339+		1.298
[Low intensity]				
Intercept	0.035**	0.242**	0.035**	0.248**
Variance(rho)	0.328	0.306	0.328	0.307
Log-Likelihood	−991.77	−942.683	−989.976	−942.1
Person-years	10282	10282	10282	10282

Notes: Reference categories in brackets; significance level: $^{**}p < 0.01$; $^{*}p < 0.05$, $^{+}p < 0.1$.

Table 3
Odds ratios from random-intercept discrete-time hazard models of the effects of parent's migration status on the probability of death of children born in Luanda after 1991, 2004 Forced Migration Survey.

	Model 1	Model 2
Migration status		
War migrant	1.684*	1.356
Non-war migrant	1.623	1.591
[Native/long-term resident]		
Child's age (years)	0.586**	0.606**
Child's sex		
Male		0.684+
[Female]		
Respondent's age (years)		0.974
Respondent's marital status		
Married		0.315**
[Not married]		
Number of children ever born		1.059
Respondent's education		
[Four years or less]		
Five to eight years		0.995
Nine years or more		0.457*
Municipality of residence		
Samba		0.645+
[Viana]		
Intercept	0.040**	0.341+
Variance(rho)	0.246	0.228
Log-Likelihood	−590.563	−552.64
Person-years	5890	5890

Notes: Reference categories in brackets; significance level: $^{**}p < 0.01$; $^{*}p < 0.05$, $^{+}p < 0.1$.

Table 4
The effects of migration status on selected indicators of child health, youngest child born in Luanda. Negative binomial regression coefficients and standard errors (Panel A) and logistic regression odds ratios (Panel B and C), 2004 Forced Migration Survey.

	A. Number of antenatal consultations (Coefficients and SE)		B. Birth in health facility (Odds Ratios)		C. Age-adequate immunization (Odds Ratios)	
Migration status						
War migrant	0.847 (0.036)**	0.899 (0.035)**	0.452**	0.644*	0.458*	0.706
Non-war migration	0.928 (0.044)$^{+}$	0.923 (0.041)*	0.668^{+}	0.636^{+}	0.789	0.962
[Native/long-term resident]						
Child's age (years)		0.995 (0.004)		0.982		n/a
Child age in months	n/a	n/a		n/a	n/a	1.009^{+}
Child's sex						
Male		0.985 (0.026)		1.361^{+}		0.832
[Female]						
Respondent's age (years)		1.009 (0.003)**		1.046*		0.998
Respondent's marital status						
Married		1.095 (0.048)$^{+}$		1.121		2.313*
[Not Married]						
Number of children ever born		0.983 (0.008)**		0.789**		1.078
Respondent's education						
[Four years or less]						
Five to eight years		1.179 (0.033)**		2.493**		1.126
Nine years or more		1.218 (0.039)**		7.559**		2.448
Municipality of residence						
Samba (more urbanized)		1.104 (0.027)**		1.333		2.054*
[Viana (less urbanized)]						
Place of delivery						
Health facility	–	–	–	–		1.597
[Home]						
Intercept	6.816 (0.016)**	4.295 (0.083)**	1.614**	0.196	10.14**	1.424
Likelihood ratio chi-square	–	–	17.71**	130.503**	4.51	31.05**
Deviance	−0.032	−0.05	–	–	–	–
Number of cases	632	632	651	651	455	455

Notes: Reference categories in brackets; standard errors in parentheses; significance level: $^{**}p < 0.01$; $^{*}p < 0.05$, $^{+}p < 0.1$.

significant. The effect of non-war migration is now close in magnitude to that of war migration and is also statistically significant.

Next, we look at whether or not the youngest child was born in a health facility. Panel B of Table 4 shows the results of a baseline logistic regression model. As the results indicate, children of war migrants were significantly more likely to be born outside of a health facility than children of Luanda natives and long-term residents (OR = 0.45). In the case of children of non-war migrants the tendency was similar but the effect was much smaller and only marginally statistically significant. When we add controls, the difference between war migrants and non-migrants diminishes considerably but remains statistically significant. However, the effects of both types of migration status are now very similar in magnitude (even though the effect of non-war migration remains only marginally significant).

The last panel of Table 4 presents the results of a logistic regression model predicting whether the youngest child was fully immunized. As described in the Data and Methods section, this test includes only children who were 12 months or older at the time of the survey. The baseline model points to a disadvantage of children of war migrants: their odds of being fully immunized were only 0.46 of the odds of children of non-migrants and long-term residents. When controls are included, however, the disadvantage of children of war migrants diminishes greatly and is no longer statistically significant indicating that much of that disadvantage was due to other factors, especially parents' marital status and municipality of residence (but notably, not education). At the same time, the difference between children of non-war migrants and non-migrants/long-term Luandans, already small and not significant in the baseline model, completely disappears in the full model.

The last two logistic regression models that we fit deal with child morbidity and use of health care. We first look at whether a respondent's child was sick in the two weeks preceding the survey. Although the baseline comparison again points to a disadvantage of children of war migrants, the corresponding statistical effect is not significant and all but disappears once controls are included. Likewise, when we test for differences across the three migration status groups in use of health care health care services to treat the sick child, the coefficient for war migrant points in the same direction but is not statistically significant even in the baseline models (we do not display the results of these tests, but they are available from the authors upon request).

Discussion and conclusions

By distinguishing between war migration and non-war migration and their corresponding influences on child health outcomes, our study sought to extend the debate on the direct impact of war to include its indirect consequences among the general population. Our hypotheses that in the immediate term, war migrants will be disadvantaged in child survival outcomes relative to non-war migrants and those not migrating were confirmed. It is noteworthy that the largest disadvantage of children of war migrants was detected in the year following war migration rather than in the year when that migration occurred. As stated in our conceptual framework, war migration may have a direct impact on childhood mortality through malnourishment and physical injuries but also an indirect effect, through psychological distress associated with a forced exit from one's usual environment and a slow integration into new life at the point of destination. This may increase the vulnerability of war migrants to childhood mortality compared to non-war migrants who might be better prepared to withstand the pressures of immediate disruption.

These results indirectly align with the earlier findings pointing to a considerable disadvantage in child health outcomes in the parts of Angola that were heavily affected by civil war relative to those that were largely spared hostilities (Agadjanian & Prata, 2003), as war migration was more likely to originate in the former while non-war migration in the latter. The results are also consistent with the literature on increased vulnerabilities of migrant children in the process of and immediately following forced migration (Doocy et al., 2007; Guha-Sapir & Gijsbert, 2004; O'Hare & Southhall, 2007).

However, our results also support the conclusions of some earlier studies that point to a reduction and eventual disappearance of war migrants' mortality disadvantages after the flight (Hynes et al., 2002; Khawaja, 2004; Singh et al., 2005a).

Furthermore our distinction between war-driven migration and migration that is not directly caused by hostilities provides a useful way of explaining the nature and trends in war migrants' disadvantages at least in some child health outcomes after they reach safe havens. In fact, in terms of child delivery at a medical facility and number of antenatal consultations the two migrant groups were rather similar, suggesting similar mechanisms that lead to the disadvantage of these groups relative to non-migrants and long-time city residents. However, neither migration group was significantly different from the non-migrants and longtime residents in age-adequate immunization and in most recent health outcomes (morbidity and health care seeking behavior), which supports the assumption of migrants' adaptation to the host environment regardless of their migration status. Adaptation, we argue, occurs when migrants—be their migration war or non-war related—are increasingly integrated into city life and are no longer marginalized.

Overall, our findings not only further our knowledge of the short- and longer-term health implications of war and non-war migration in Angola but also in general illuminate both the differences and similarities between the two types of migration and the mechanisms through which they might shape immediate and enduring child survival and health outcomes. As such these, findings could usefully inform post-war national health strategies and interventions in sub-Saharan countries by helping their policy-makers to calibrate these strategies and interventions to specific needs and vulnerabilities of different subgroups of their populations.

In closing, we want to discuss the limitations of our findings. First, like in most retrospective studies of health effects of war-induced migration, we do not have information on child survival and health outcomes of families that did not flee the origin communities or who fled but ended up in a different destination point. The reference group is the native and long-term residents of Luanda. While there may be considerable socio-economic and cultural differences between migrants and non-migrants, we believe that controlling for education and parity reduces these differences. Besides, it is important to keep in mind that the survey sample was drawn from two peripheral municipalities, where the levels of urbanization are relatively low. Second, the family migration, fertility, and mortality histories were obtained from key respondents half of whom were females (children's mothers) and the other half males (children's fathers). The statistical models use those respondents' characteristics such as age and education as parent-level controls. We acknowledge the non-conventionality of obtaining birth history data from fathers and using father's characteristics (age and marital status) as controls. However, we did not find evidence that these characteristics detracted from the key findings of this study. And third, as in most similar surveys, our data on family socio-economic background are limited to the characteristics recorded at the time of the survey which we cannot be used as covariates in models predicting past behavior. Their limitations notwithstanding these unique data from a post-war sub-Saharan setting allowed for sound analyses that shed important light on the connections between war-related migration and child mortality and health.

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