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Global Infant Mortality: Initial results from a cross-country infant mortality comparison project

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Abstract: The United Nations Millennium Development Goals have highlighted the usefulness of the infant mortality rate as a measure of progress in improving neonatal health care services, and more broadly as an indicator of basic health care overall. However, prior research has shown that infant mortality rates can be underestimated dramatically, depending on the live birth criterion, vital registration system, and reporting practices in a particular country. These problems are especially great for perinatal mortality. This study seeks to assess infant mortality undercounting for a global dataset using an approach popularized in economics some three decades ago, when researchers sought to create internationally comparable, purchasing power parity-adjusted per capita income measures. Using a one-sided error, frontier estimation technique, it is possible to recalculate rates based on estimated parameters to obtain a standardized infant mortality rate for all countries, and at the same time to derive a measure of likely undercount for each nation.

1. INTRODUCTION

Comparative measures of economic development or social welfare are difficult to devise. The proxies traditionally used suffer from severe imperfections, and in consequence, new measures have emerged. In particular, the economists' use of (deflated) GDP per capita has met with severe and justified criticism, even when the exchange rates used to convert various currencies to a common unit are adjusted for differences in purchasing power. "Augmented" GDP measures have been devised that account for natural resources, subtracting social bads such as pollution costs, and treating certain expenditures (for example, on maintaining social order) as an intermediate rather than a final product that should not be included. Yet, these measures still miss the fundamental point that human welfare has many components, of which many are not economic. This awareness in turn has given rise to a set of eight "millennium development goals" (MDGs), promoted under the aegis of the United Nations, that are intended to capture the multi-dimensional aspect of economic and social development (see http://www.un.org/millenniumgoals).

The fourth millennium development goal is to reduce child mortality. These broad goals are in turn broken down into pieces and ancillary but broadly related objectives. For example, for Botswana, the child mortality MDG contains further objectives of reducing infant mortality from 48 per thousand live births in 1991 to 27 in 2011, to reduce the under five years mortality rate (U5MR) by two-thirds over this same period, to reduce child protein energy malnutrition (PEM) from 18% in 1998 to 8% in 2011, and to immunize 80% of all one-year olds by 2009 (Republic of Botswana, 2004). In all, Botswana had 22 specific goals, some of which seem ideological (such as #21, "develop further an environment conducive for beneficial trade and foreign direct investment"), but which for the main part reflect aspects of social welfare far more clearly than GDP measures.

This broader concept of development also would appear to have the advantage of being easier to calculate, especially for components such as infant and child mortality. Economists who specialize in poor and middle-income countries especially tend to value this feature, since economic indicators are often fraught with a range of measurement errors. Since many of the social components of the MDGs are almost certain to be highly correlated with economic prosperity, tracking them is useful for assessing overall economic policy success as well, and measurement errors are likely to be less.

Or so it has long been assumed by development experts disinterested in data sources and quality. In this paper, we argue that infant mortality rates tend to be wildly and systematically inaccurate, but that it is possible to bring some order to comparative assessments by making systematic, consistent corrections across countries. It is important to emphasize the systematic nature of the corrections. At present, the researcher either must use inconsistent data reported by national statistical services (and generally available on the WHO website at http://www.who.int/healthinfo/morttables/en/index.html), or must accept corrections made by United Nations' demographers.

The underlying problem with vital statistics data is that they do not provide universal coverage. Furthermore, the errors are not random: they tend to be much larger in poor and less urbanized nations. The problem is particularly acute in terms of measuring deaths during the first day of life, and, to a lesser extent, days 2-6. Differences in what is regarded as a live birth further weaken cross-country comparability, while varying quality of national statistical offices' (NSO) efforts can make time series comparisons problematic as well. Most critically, the errors are essentially one-sided: the ratio of unreported infant deaths to live births is almost certainly high. In response, we seek to derive estimates that are reasonably comparable, and that reflect systematic rather than somewhat idiosyncratic corrections to official NSO data.

We begin the narrative by discussing and documenting the problem. Section 3 then addresses estimation strategy, while the following section provides a first pass at estimating a "true" relationship between infant mortality and socioeconomic variables, using UN data. Section 5 then uses these results to derive an initial correction of WHO data. We emphasize that these results are preliminary and incomplete: Section 6 summarizes additional corrective steps needed.

2: THE DATA: UNDER-REPORTING CORRECTIONS AND INFANT MORTALITY PATTERNS

Broadly speaking, there are three sources of data on infant mortality across countries. First, the World Health Organization (http://www.who.int/whosis/mort/en) collects data from NSOs throughout the world, and reports them without correction, though terse assessments of quality are offered. The United Nations Statistics Division (http://unstats.un.org/unsd/demographic/products/vitstats) also collects data and assesses quality; efforts as well are made to correct for under-reporting. Finally, bodies such as the EU's Eurostat (http://epp.eurostat.ec.europa.eu/portal/page?_pageid=1090,30070682,1090_33076576&_dad=portal&_schema=PORTAL), WHO's regional Pan-American Health Organization (http://www.paho.org/english/dd/ais/coredata.htm), or CIS Stat (http://www.cisstat.com/rus) offer separate and in some cases independent assessments of mortality in particular regions. An excellent way to get a sense of credibility of a particular mortality value is to compare it, if possible, with estimates from DHS surveys (http://www.measuredhs.com).

No data are flawless. Mortality rate estimates can be understated if deaths are more likely to be reported than an undercounted base population. Error in age-specific mortality is likely to arise as well if there are systematic errors in reporting age of death. In the case of infant mortality, unreported deaths relative to reported deaths are likely to exceed unreported births relative to all births, at least in developing countries, leading to a systematic downward bias in infant mortality statistics. Indeed, given the difficulty in consistently counting live births in developing countries, Kramer *et al.* (2002) recommend that countries with weak monitoring systems report a combined measurement of stillbirths and neonatal mortality. One could also follow a strategy implied in Wegman (1996), subtracting first hour deaths when comparing infant mortality across nations. More conventionally, demographers such as Kingkade and Sawyer (2001) and Aleshina and Redmond (2005) employ data fitting techniques to correct for underreporting in the first months of life.

Unreported deaths are especially likely when the infant lives only a very short period, so that no registration has occurred. Indeed, midwives may announce to the mother and family that a stillbirth occurred, rather than a live birth followed shortly by death, regarding their report as an act of mercy to a grieving family. It seems plausible that unreported death will be more likely for births outside of hospitals; both because risks are higher and reporting systems are weaker. Non-hospital births are more common in poorer countries and rural areas, and there is evidence of dramatic rural under-reporting in some countries (Anderson and Silver, 1986; Becker *et al.*, 1998). In former Soviet republics, live births were recorded as such only if gestation and weight conditions were met (Anderson and Silver, 1997; Kramer *et al.*, 2002; for a discussion of global practices, see Wegman, 1996). While most countries have officially changed this policy to conform to WHO practice, in practice the old conditions are often used, again especially in rural areas. Several former Soviet republics also serve as examples of large recorded improvements in infant mortality that almost certainly reflect deteriorating data collection rather than genuine health improvements (Anderson and Silver, 1997; Becker *et al.*, 1998).

These points have long been recognized, and several are discussed at greater length in Hill and Choi (2006). They use DHS surveys to assess neonatal mortality, focusing on death heaping (at day 7) and underreported early neonatal mortality rates (ENMRs, defined as day 0-6 mortality) relative to late neonatal mortality (LNMRs, day 7-27 mortality). They adjust data to correct for heaping, and then compare adjusted ENMR/LNMR ratios for developing countries relative to historic rates for England and Wales, controlling for total infant mortality rate. They find little evidence of systematic bias in the ENMR/LNMR ratios over time, though the ratio does vary considerably across region. Thus, once day-7 death heaping has been corrected, there is little reason to believe in systematic relative undercounting from DHS data. However, the issue is not fully resolved, since DHS surveys are neither universal nor annual, and since it is not obvious that the historic comparison employed is appropriate. Most importantly, there are several reasons to suspect that even DHS infant mortality rate data suffer from some under-counting, even if ENMR/LNMR ratios do not (Hill and Choi, 2006:443-444; note in particular the comparison with a detailed site analysis from Maharashtra discussed in Bang *et al.*, 2002).

The consequences of these various sources of under-reporting can be large. Wuhib *et al.* (2003) find that switching from Soviet to WHP live birth definitions raised the 1996 infant mortality rate in Kazakhstan's Zhambyl oblast (province) from 32 deaths per thousand live births to 58.7 deaths. The extent of underreporting in official data for transition nations is detailed in Aleshina and Redmond (2005), who contrast (still possibly underreported) DHS estimates with official tallies. The largest discrepancy occurred in Azerbaijan, where the official 2001 IMR, 17, contrasts with the survey estimate of 74. In a majority of cases, the survey IMR estimate was more than double the official estimate. Aleshina and Redmond (2005) also estimate that adjusting the live birth definition to WHO standards would raise recorded IMRs from 5% to 40%, depending on the country and year. Thus, while definition matters, it hardly explains the entire discrepancy. Kingkade and Sawyer (2001) force transition nations' mortality patterns in the first three months of life relative to month 4-10 infant mortality to replicate US and German data from periods of similar overall mortality. Doing this raises 1987-2000 IMRs from a low of 0.3% in Slovakia (to 11.6 deaths/thousand) to highs of 167% in Azerbaijan (to 60.5) and 111% in Albania (59.8). Aleshina and Redmond (2005) use Trussel's (1975) version of the Brass method and use model life tables to convert survey survivorship data for older ages into infant mortality rates for Kazakhstan, Tajikistan, and Azerbaijan. While a wide range of possible IMRs result, they tend to be well above official estimates, especially for Tajikistan and Azerbaijan.

Comparison of official statistics and survey data also generate very different regional patterns. DHS and similar surveys almost always find considerably higher rural than urban infant mortality. For example, in their analysis of a fairly typical survey, Sullivan and Tureeva (2004) report rural IMR 74% greater than urban IMR in Uzbekistan. This pattern is confirmed for India as well (National Neonatology Foundation, 2004: 20). However, because of greater under-reporting, official data commonly find higher urban IMR, at least in transition nations (Becker *et al.*, 1998)

Aggregate infant mortality data are shown in Appendix Tables A1 (for countries with 85% or better coverage of vital events) and A2 (for the rest). Obviously, estimates from the four sources – UN Statistical Division, UNICEF, WHO aggregate estimates, and the summation of total infant morality by four sub-periods and by specific causes of death from the WHO mortality database – are not always equal. In particular, the summed values tend to bed lower than other estimates, even for countries with very high levels of coverage, though there are cases where the summed values are greater than other estimates. Furthermore, the detailed breakdown is not available for most very low-income countries, while it is generally present for high income countries.

It also can be seen that even for countries with very high rates of vital statistics coverage that huge differences in reported values may occur. In countries such as Thailand, Belize, or Mexico, the large range may reflect weaker reporting at the disaggregated (cause and sub-period of infant death) level. But countries such as Albania, Egypt, and Mongolia have very different data reported by different sources. Somewhat ironically, the level of conformity among IMR estimates is often greatest among some of the poorest (and likely worst enumerated countries), presumably because all sources report imputed values based on population structure and fertility estimates. Thus, for example, the UN and WHO figures are virtually identical for Niger, Myanmar, or Côte d'Ivoire, while they differ substantially for Turkey or South Africa. Paraguay appears to be in a category of its own in terms of having an astonishing level of disagreement.

So, what is the researcher to do? Economists tend to grab whichever data set is handiest without concern for the possibility that the IMR numbers reported may differ markedly from other reported values. To repeat our earlier point, we are most troubled by the apparent inconsistency in generating specific values, and by systematic biases that are likely to emerge. At present, the data sets use estimates from vital statistics (perhaps with a few, country-specific corrections in many cases) when these are of high quality and with good coverage. Where data are poorer, the estimates may be generated by retrospective surveys (for a discussion of problems in doing so, see Sullivan and Tureeva, 2004). Otherwise, the international bodies fall back on estimated imputed via a modified Brass method from population size and structure, and fertility estimates. However, as all demographers know, these imputation techniques make strong assumptions on population and mortality stability, and on low population movements (Aleshina and Redmond, 2005). These assumptions were reasonable for the Africa of the 1960s that Brass and Trussel had in mind as they developed imputation techniques. They are much less well suited for the more turbulent and mobile world of today.

The primary alternative to date has been to use data from DHS and similar surveys to find patterns for low and middle-income countries. In a detailed presentation on neonatal mortality rates, Hall (2002) surveys what is known, presents detailed data, and discusses limitations to the surveys. The growing number of regular surveys makes this a valuable exercise. This is particularly the case now that several countries, and most importantly India, generate consistent regional surveys with reasonable frequency (for a detailed study of India, see National Neonatology Forum, 2004). Nonetheless, these advances do not address the need to generate a consistent set of estimates for all countries.

3: ESTIMATION STRATEGY

We approach the under-reporting problem differently, seeking to use reported mortality data rather than making standard Brass-Trussel corrections. Our rationale for doing so is driven in part by a desire to generate a consistent, comprehensive cross-country panel data set for an extended time series; it also reflects concern that the underlying Brass-Trussel model assumptions are less appropriate today than in the 1960s and 1970s, when the framework was first developed.

The first step is to develop a model of the determinants of infant mortality, restricting our sample to countries for which there are reasonably good data. What constitutes "reasonably good" is of course a vague notion, and requires several alternate measures and substantial discussion below. Our point of departure is to use (a) UN data for countries with 85% or better coverage, (b) UN data for all countries, and (c) WHO data for countries with 85% or better coverage. Our goal is to regress IMR measures for these samples against plausible determinants. The literature suggests that IMR will decline with:

- The level of economic and social development
- Effort devoted to public health
- Access to medical care
- Quality of individual health practices.

We use a simple measure of economic development; namely, gross domestic product per capita in US dollars, converted (if possible) using purchasing-power parity (PPP) adjustments to correct exchange rates, ***gdp***. A plausible adjustment would be to correct for windfalls from minerals rents that accrue to government, but that may have only a modest impact on living standards of the general population. In that case, we could add a variable that captures the percentage of GDP from oil, gas, and minerals, ***oilshr***. Public health measures are somewhat problematic due to endogeneity issues: our interest is in identifying good effort conditional on health levels. The easiest measure, and one that also picks up some of the access and individual practice effect, is a related outcome measure – the maternal mortality rate, ***matmort***. Access to medical care is also picked up in part by the overall level of urbanization, ***urb***, while quality of individual practices will be related in aggregate to the adult literacy rate, ***adlit***. In practice, these variables turn out to be highly collinear, and the regressions reported below emphasize GDP, maternal mortality, and urbanization.

The next issue concerns estimation. As long as we are dealing with aggregate IMR estimates, simultaneity problems seem minor. However, the nature of errors is that underestimates are almost certainly more likely than overestimates. The latter will occur to the extent that deaths are reported accurately while births are underreported; the former will be common if deaths are underreported relative to births. Underreporting of deaths is universally more common, possibly excluding tiny errors in a few highly developed countries. Therefore, we argue that errors will be one-sided, making standard OLS "average" infant mortality regressions inappropriate, since they assume that errors have zero mean, and in effect result from random reporting error.

This problem was first addressed in production and cost analysis, with the aim of identifying firm inefficiency. Production and cost functions were recognized as being envelopes, and hence the frontier approach both enabled estimation of the envelope, and measurement of the extent of inefficiency of particular firms (for example, see Huang, 1984; for a detailed econometric presentation, see Kumbhakar and Lovell, 2000). While the **stochastic frontier function** technique became standard in productivity and cost analysis, its application to other questions appears to have been quite limited. To our knowledge, the only example of its use in a demographic-economic setting is Morrison (1993), who created measures of regional efficiency and productivity to analyze inter-regional migration in Peru.

Its application in mortality analysis seems natural. If situations as those depicted in **Figure 1** prevail, in which the true relationship (the solid line) is obscured by under-reporting in many if not all cases, then an OLS estimate (dashed line) will produce biased coefficients. If the errors are negatively correlated with level of economic development, literacy, urbanization, and recorded maternal mortality – all of which seems likely – then these coefficient estimates will be biased upward. That is, the true negative relationship will be understated. Furthermore, the predicted IMR and number of infant deaths in poor countries will be systematically understated.

Once these equations are estimated, "true" frontier value estimates can be calculated for each country as a function of its characteristics, and the level of error (corresponding to the estimate of firm inefficiency) can be determined. Forces underlying these estimated errors in turn can be explored as well, with the error terms regressed on plausible explanatory variables – including ***gdp***, the estimated degree of vital statistics coverage, and restrictive IMR definition practices common to the former Soviet Union, suggesting a ***fsu*** dummy.

More generally, it is possible to express the error term as having two components.[1] These consist of one error term V_i that is symmetrically distributed i.i.d. as N (0, σ_v^2), capturing the effects of random measurement error and random shocks to the observations. The other error component U_i is a one-sided term that is distributed i.i.d. as N (θ, σ_u^2), capturing the effects of non-random measurement error – that is, systematic underreporting. Then the observed mortality rate MR_i can be expressed as a function of non-stochastic determinants $\boldsymbol{X}$ and an error term $\boldsymbol{\varepsilon}_i$ as

$$MR_i = \boldsymbol{\beta}'\boldsymbol{X}_i + \boldsymbol{\varepsilon}_i \text{ and } \boldsymbol{\varepsilon}_i = V_i - U_i. \qquad \textbf{(1)}$$

An EM (expected-maximization) algorithm is then used to estimate the parameter vector $\boldsymbol{\Theta}' = (\boldsymbol{\beta}, \sigma_v^2, \sigma_u^2)$. Letting ZMR_i represent the true mortality rate – more conventionally, the latent frontier – we can write:

$$VMR_i = \boldsymbol{\beta}'\boldsymbol{X}_i + V_i\,. \qquad \textbf{(2)}$$

Hence,

$$MR_i = VMR_i - U_i. \qquad \textbf{(3)}$$

The algorithm involves an iterative procedure that includes an expectation step that estimates sufficient statistics of VMR given the observed MR. A maximization procedure then estimates a new $\boldsymbol{\Theta}'$ using a maximum likelihood procedure on (2) and (3). These new $\boldsymbol{\Theta}'$ estimates then generate new sufficient statistics, and the procedure repeats until, if all goes well, the algorithm converges. The algorithm is available as an option in STATA 8, which we use.

As noted, the frontier estimation process also has the attractive feature that it is possible to separately estimate determinants of the level of inefficiency, or, in our case, underreporting. A disadvantage is that the estimates of underreporting are sensitive to functional form. Demographic and economic theory offer good insights into which variables should affect mortality rates, but there is little *a priori* restriction on functional form. The results reported below use log-log forms throughout for consistency, with quadratic terms included when fits are improved. However, we emphasize that statistically significant estimates of the one-sided error do not obtain in every specification. On the other hand, since we do not know the appropriate specification, a reasonable approach is to try alternates and hunt for the best fits (so far, log-log), and then examine whether underreporting exists in those cases. We should note as well that poor specification of functional form can be

[1] This presentation follows Huang (1984) and Morrison (1993).

expected to increased the V_i term relative both to the one-sided error and the non-stochastic component, so that we are more likely to miss than overestimate the extent of underreporting.

A complication emerges when we turn to disaggregated components of IMR; namely, birth day mortality, day 1-6 mortality (week 1 less day 0), day 7 – 27 mortality (weeks 2-4 mortality), and day 28 – 364 or **post-neonatal** mortality. Underreporting incidence declines with infant age, suggesting that more accurate measures of aggregate IMR can be obtained if we divide the overall rate into its components. Because of the measurement error problems in the first three **neonatal mortality** components associated with birth heaping, for simplicity we focus on separate determinants of neonatal (NNMR) and post-neonatal (PNNMR) infant mortality, but also present the more disaggregated results.

The WHO database enables us to distinguish NNMR and PNNMR rates. Once again, we want to explore how these measures vary with socio-economic determinants of infant mortality. However, we cannot estimate the two equations independently, unless the explanatory variables are identical, since the error terms will be correlated. The standard procedure for dealing with this problem is to use **seemingly unrelated regressions** (SUR). Unfortunately, this technique is not at present integrated with frontier techniques, forcing us to choose between them. Our approach in this paper is to run independent frontier regressions using identical explanatory variables, in which case SUR collapses to ordinary regression. A subsequent paper will investigate simultaneity, with NNMR and shorter period mortality augmented with the estimated error from the first frontier regressions. These regressions then will be jointly estimated with PNNMR in an SUR framework. These sub-period regressions can then be used to calculate estimated components. The greater of estimated and reported values will be taken at all times; one option is to scale up the subperiod estimates to yield the frontier IMR estimate. The advantage of the simultaneous equations approach is that we can permit PNNMR to depend on general health variables, while NNMR alone will depend on maternal mortality.

An alternative correction that has the potential to remove biases is to generate a panel data set, and then use a fixed effects model to capture country-specific biases. This approach is intended to be the topic of a companion paper, but we note here that it is not without problems. In particular, data quality systematically varies over time in many countries: it improves with overall economic development, and deteriorates with crises. This complicates time series analysis, and for simplicity we stick with a simple cross-country analysis here – while noting that refined estimates will need to introduce data from multiple periods.

4: SOCIO-ECONOMIC DETERMINANTS OF INFANT MORTALITY

The core regression results appear in Tables 1-4. **Table 1** presents results using UN data, both for all countries for which estimates are available, and for the smaller sample that generate detailed vital statistics that are used by WHO. This smaller group is anything but random, as it excludes poorer countries with weaker statistical gathering capacity. However, the excluded group also includes many formerly socialist countries whose data practices are not fully reconciled with WHO. As these data are readily available (see http://demoscope.ru/weekly/pril.php or http://www.mednet.ru/statistics/), in future versions we hope to extend this analysis to include merged data.

For the global dataset, there is no indication of systematic underreporting. The σ_u^2 term is not significant, and the frontier regression collapses to OLS. Alternate specifications with different variables and functional forms do not affect this conclusion. Examining the coefficients, the overall infant mortality rate either declines linearly with GDP, or does so at an ultimately accelerating rate shortly after reaching a maximum at about USD 30. Not surprisingly, *IMR* rises with maternal mortality, though the significance level is modest.

In contrast, when observations over UN data are limited to those countries with good vital statistics coverage, there does appear to be some undercounting, as the σ_u^2 term is significant. An obvious conclusion to draw is that the UN imputations made for countries with poor databases do not suffer from systematic underestimation. The *GDP* effects are weaker in significance in the smaller, better vital statistics data set, but this is no surprise: these countries exclude almost all very poor nations, and therefore there is much less variance in incomes. Note as well that the coefficient on the significant quadratic maternal mortality term is nearly three times greater, both in OLS and frontier regressions.

Table 2 presents similar regressions using WHO data. The coefficients in regressions that included both linear and quadratic terms for income and maternal mortality were all insignificant; as the underreporting estimates based on σ_u^2 were essentially unchanged, regressions (5) – (10) only report the linear specifications. There are no surprising signs. Indeed, we encountered no surprising signs in virtually any regressions: all mortality, regardless of occurrence, declines with measures of economic development, and, when included, with urbanization. Infant mortality and its components also secularly rise with estimated maternal mortality.

Comparison of results for first month (neonatal, *NNMR*) and month 2-12 (post-neonatal, *PNMR*) mortality confirm the hypothesis above that underreporting is concentrated in early infancy. The *PNMR* frontier regressions show no signs of systematic underreporting, and therefore collapse to their OLS counterparts. This pattern holds as well for alternate functional forms and included variables. In contrast, there is very strong evidence of underreporting for neonatal mortality, and this finding is robust to alternative specifications.

One would expect underreporting to be greatest for countries that have the worst vital statistics coverage, and this result is indeed obtained. From **Table 3** it is apparent that the frontier regressions collapse to OLS regressions when the WHO dataset is restricted to those countries with 85% or better coverage. In unreported regressions, we regressed the σ_u^2 idiosyncratic error against coverage, but did not obtain a significant relationship. Since coverage clearly does matter, the obvious conclusion is that the effect of accuracy is nonlinear, and in effect we use a spline at 85%.

The next step is to ask whether one can gain additional information by further disaggregating neonatal mortality. **Table 4** presents results for first week mortality (*W1MR*), as well as for its components, first day mortality (*D1MR*) and day 2-6 mortality (*D2_6MR*). Clearly, the undercount is driven by first day error, and very strongly so. This is even true for the sub-group with 85% or better coverage, for which undercounting inefficiency is not caught when we aggregate to neonatal (first month) mortality. On the other hand, for those with a high level of coverage of vital statistics, there is no evidence of systematic undercounting for day 2-6 mortality. Thus, while heaping – and associated later first week mortality undercounts – may be a problem in specific countries, it is not a systematic problem.

Table 4 also provides an initial exploration into the causes of undercounting. The possibility of heaping, explored in regression (25), implies that first week underreporting will be made up later, in part in higher recorded post-neonatal mortality.[2] This does not appear to be the case. It is also possible that errors are greater at low levels of urbanization, and this does appear to be born out. The coefficients on the natural logarithm of urbanization and its square imply that undercounting will continue to decline until a country is 71% urban.

[2] An alternative specification would be to also include *W2_4MR* = *NNMR* – *W1MR*, in the anticipation that missed first week deaths would be recorded during weeks 2-4.

5: CORRECTED INFANT MORTALITY ESTIMATES

Do these various corrections in fact matter? If so, where are they most important? A definitive answer must await many more specifications and quite likely the use of a panel data set with merged transition economy data. However, a sense of the value of the approach appears in **Table 5**, which provides frontier mortality estimates as a percentage of recorded WHO and UN rates for 22 countries. The estimates are based on frontier regressions with log linear and quadratic *GDP* and maternal mortality, without specifying particular determinants of the undercount. The countries are chosen for illustrative purposes, as well as in an effort to include most of the low income countries that report detailed vital statistics data, and are not intended to be a representative sample.

Forecasts from economically advanced countries with high levels of vital statistics coverage are very close to recorded values, and are not reported. Rather, we concentrate on low and middle income countries. Those middle income countries with excellent vital statistics coverage, such as Costa Rica, also yield estimates that are extremely close to recorded values. For both WHO and UN databases, estimated overall *IMR* for Costa Rica is within 1% of actual recorded values. Moreover, with the exception of week 2-4 mortality, all subperiod estimated mortality rates are within 7% of recorded rates.

For the UN population database, the estimated infant mortality rates are below reported values for many countries. This reflects our decision to base the forecasts on regressions restricted to countries with 85% or better recorded rates (which does not include many of the countries in Table 5). Unfortunately, this restriction imposes the risk of major error in making out-of-sample forecasts, and the relatively modest variance in some explanatory variables relative to structural differences may have resulted in understated coefficient estimates. Comparisons of forecast and actual values for specific countries also suggest omitted variables: the large under-forecasts are virtually all for countries that are either mineral exporters (Bahrain, Kuwait, Mexico, South Africa), or that have very high levels of inequality (Albania, Dominican Republic, and several of the mineral exporters).

Of course, these same problems hold for the WHO regressions, and also there is no doubt that inequality is high in countries such as the Philippines, where the ratio of 139% (UN) and 327% (WHO) suggests that considerable to massive mortality undercounting is likely. In such cases, the frontier regressions are still likely to underestimate actual mortality, but not by enough to compensate for undercounting. A comparison of UN and WHO estimates also attests to the improvements from the various UN imputations.

Looking first at the UN regressions, there grounds for some nervousness with respect to the infant mortality rates for very poor countries, though because of out-of-sample forecasting, this result needs to be verified with regressions based on a broader range of countries. The WHO forecasts suggest dramatically more error, mainly because these figures are not increased by various imputational procedures.

Of the countries in Table 5, the most striking are those with massive vital statistics undercounting (Dominican Republic, Kyrgyzstan, Peru, Philippines, and, above all, Haiti). These are all very poor to lower middle income countries, and it is not surprising that their vital statistics suffer from severe undercounting. On the other end are those such as Costa Rica, Kuwait, Mexico, and Romania, with apparently quite accurate reporting. The remainder consists of middle income countries and oil states, some of which appear to have only moderately inaccurate counts (Bahrain and South Africa), while the others have high to extremely high undercounting.

In breaking down mortality into smaller groups, we are struck by several patterns. First, for many countries (Bahrain, Haiti, Kuwait, Romania, and Thailand), the undercounts appear to be greatest during the first day of life. But for six others, led by Albania and South Africa, the greatest undercount occurs during weeks 2-4, while for another group, led by Kyrgyzstan and the Philippines, post-neonatal mortality undercounting appears to be the greatest problem. This runs counter to our hypothesis that first day mortality undercounting would be universally dominant. However, a great deal more testing remains before these patterns can be taken as established.

6: NEXT STEPS

Much work remains before it will be possible to generate a consistent panel data set with infant mortality estimates for nearly all countries on an annual basis. The first steps are obvious: it is necessary to examine alternative specifications and expand the number of observations to a multi-year panel in exploring determinants of sub-periods of infant mortality. The approach described above continues to be appropriate, though some complications are added by the time series.

There are also two related investigations that need to be conducted. One centers on the possibility that certain types of mortality are especially undercounted. We are not overly optimistic on this point, but the possibility should not be overlooked. More critically, the WHO data contain separate estimates of urban and rural mortality, and these clearly should be estimated separately, since rural infant mortality is likely to suffer from far greater undercounting than its urban counterpart. The second supplemental study is to econometrically investigate the determinants of idiosyncratic error. The approach is straightforward, but we have relatively little to guide us in terms of functional form and previous study. Table 4 contains two exploratory regressions, but more systematic analysis is needed.

These steps are conceptually simple. The most complex part of the study will be to simultaneously estimate sub-period (and likely sub-region) mortality rates in a panel data, frontier analysis setting. To our knowledge, the combination of the three tasks has not been undertaken, but there is no obvious reason that it cannot be done.

Once the estimates are in hand, the remaining work is straightforward. For each country, and each year, the regressions will yield an estimated *IMR*, as well as *D1MR, D2_6MR, W2_4MR, NNMR,* and *PNNMR*. The sub period estimates can then be summed to determine an alternate infant mortality estimate. The direct and indirect *IMR* estimates can then be compared to vital statistics data for the countries with high coverage rates, and possibly be replaced with directly counted numbers in a few cases. More commonly, the estimated equations will be used to "backcast" prior infant mortality rates, using previous estimates of GDP, urbanization, and maternal mortality – and, if we are successful, factors that are found to determine undercounting.

This work has not yet been completed, though Table 5 hints at likely findings. Undercounts are greatest in low and middle-income countries with substantial but inaccurate vital statistics reporting. These countries' infant mortality rates come mainly from counts rather than imputations, but undercounting is a major problem. Most of the world's poorest countries do not have comprehensive vital statistics, and so estimated infant mortality rates come from small to moderate surveys, or from imputational procedures. These approaches do not appear from our regressions to contain a systematic, or idiosyncratic, bias. However, these estimates are not constructed for the purpose of creating a consistent time series: rather, they tend to offer best guesses. The estimates generated from frontier panel regressions will provide the internal consistency needed. With luck, it will contribute to a better understanding of the actual picture of mortality at very young ages throughout the world.

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Figure 1

Average OLS vs. Frontier function estimates of infant mortality rates

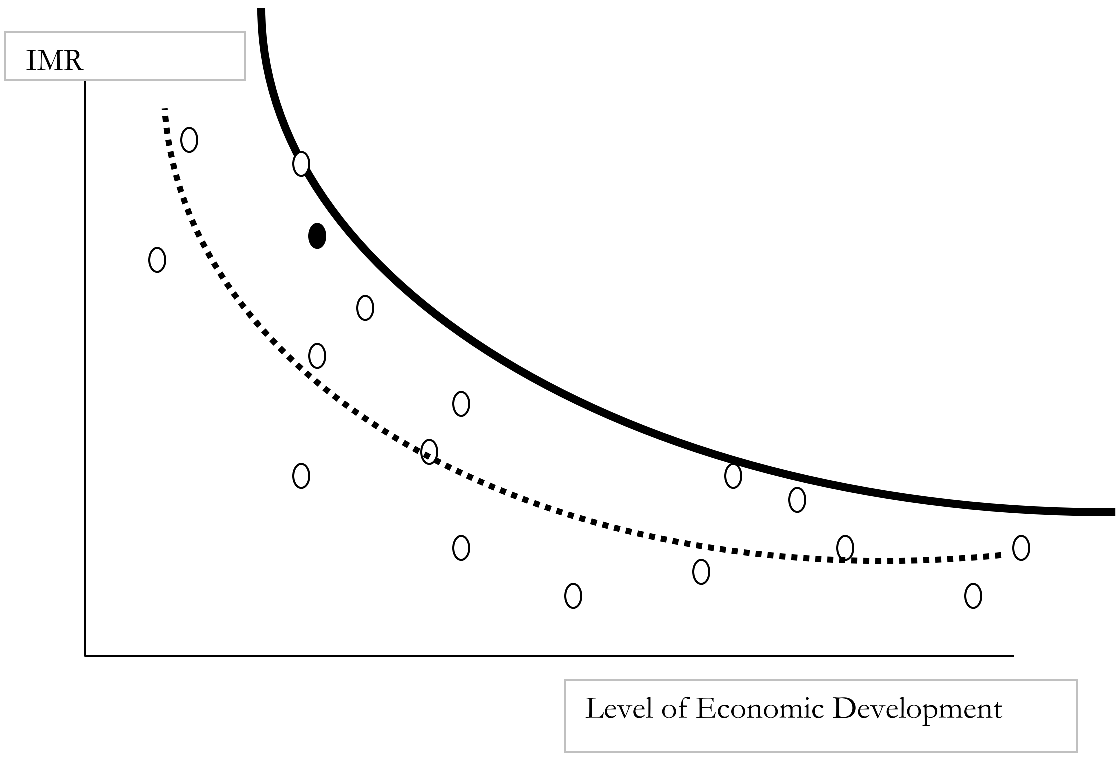

Table 1
Infant Mortality Parameter Estimates from Frontier Function and OLS Regressions, UN data

Regression	*(1)*	*(2)*	*(5)*	*(4)*
Specification	Log-log	Log-log	Log-log	Log-log
Data source	UN	UN	UN	UN
Regression type	Frontier, normal/half normal	OLS	Frontier, normal/half normal	OLS
Dependent variable	IMR	IMR	IMR	IMR
Regressors:				
Constant	2.744[a]	2.742[a]	7.043[a]	6.574[b]
GDP	0.235	0.235	-0.717[c]	-0.636
GDP^2	-0.037[a]	-0.037[a]	0.201	0.015
Matmort	0.097	0.097	-0.146	-0.180
$(Matmort)^2$	0.024[c]	0.024[b]	0.064[b]	0.067[b]
$\ln \sigma_u^2$	-11.64		-2.66[a]	
$\ln \sigma_v^2$	-2.28[a]		-3.41[a]	
Likelihood ratio test of $\sigma_u^2=0$: $\bar{\chi}^2(01)$	0.00		1.25[d]	
Pr $\sigma_u^2 \leq \bar{\chi}^2(01)$ / *F*	0.00	638.17	0.87	95.24
R^2				
N	159	159	61	61

Notes: *Standard errors in parentheses* *N = Number of observations* [a] *Significant at the .01 level* [b] *Significant at the .05 level* [c] *Significant at the .10 level* [d] *Significant at the .15 level*
IMR: *Infant mortality rate*
GDP: *per capita gross national product, US dollars*
Matmort: *Maternal mortality rate (deaths per hundred thousand births)*

Table 2
Infant Mortality Parameter Estimates from Frontier Function and OLS Regressions

Regression	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*	*(10)*
Specification	Log-log	Log-log	Log-log	Log-log	Log-log	Log-log
Data source	WHO	WHO	WHO	WHO	WHO	WHO
Regression type	Frontier, normal/half normal	Frontier, normal/half normal	Frontier, normal/half normal	OLS	OLS	OLS
Dependent variable	NNMR	PNNMR	IMR	NNMR	PNNMR	IMR
Regressors:						
Constant	3.298[a]	3.345[a]	4.125[a]	2.987[a]	3.339[a]	3.910[a]
GDP	-0.196[a]	-0.332[a]	-0.258[a]	-0.197[a]	-0.332[a]	-0.261[a]
GDP^2						
Matmort	0.133[b]	0.221[a]	0.162[a]	0.101[c]	0.221[a]	0.151[a]
% urban						
$(\% \text{ urban})^2$						
$\ln \sigma_u^2$	-1.218[a]	-9.916	-2.112[b]			
$\ln \sigma_v^2$	-3.210[a]	-1.876[a]	-2.657[a]			
Likelihood ratio test of $\sigma_u^2=0 : \bar{\chi}^2(01)$	4.90[b]	0.00	0.40			
Pr $\sigma_u^2 \leq \bar{\chi}^2(01) / F$	.99	.00	.74	19.56	84.37	67.95
R^2				.52	.76	.71
N	66	66	66	66	66	66

Notes: *Standard errors in parentheses* *N = Number of observations* [a] *Significant at the .01 level* [b] *Significant at the .05 level* [c] *Significant at the .10 level*

NNMR: *Neonatal mortality rate (deaths per thousand live births)*
PNNMR: *Post-neonatal mortality rate*
IMR: *Infant mortality rate*
GDP: *per capita gross national product, US dollars*
Matmort: *Maternal mortality rate (deaths per hundred thousand births)*

Table 3

Infant Mortality Parameter Estimates from Countries with 85% or Better Coverage

Regression	*(11)*	*(12)*	*(13)*	*(14)*	*(15)*	*(16)*
Specification	Log-log	Log-log	Log-log	Log-log	Log-log	Log-log
Data source	WHO	WHO	WHO	WHO	WHO	WHO
Regression type	Frontier, normal/half normal	Frontier, normal/half normal	Frontier, normal/half normal	OLS	OLS	OLS
Dependent variable	NNMR	PNNMR	IMR	NNMR	PNNMR	IMR
Regressors:						
Constant	1.666	2.587[b]	9.175	-8.642	24.561	8.889
GDP	-0.254[a]	-0.462[a]	-1.362[b]	0.034	-2.584[a]	-1.370[a]
GDP^2			0.055[c]	-0.016	0.115[a]	0.055[b]
Matmort	0.179[a]	0.285[a]	0.204[a]	0.191[a]	0.241[a]	0.203[a]
% urban	0.402	0.440[c]	-0.290	4.725	-5.482	-0.184
$(\% \text{ urban})^2$			0.078	-0.520	0.718	0.066
$\ln \sigma_u^2$	-8.302	-9.427	-4.133			
$\ln \sigma_v^2$	-2.376[a]	0.209[a]	-2.753[b]			
Likelihood ratio test of $\sigma_u^2=0 : \bar{\chi}^2(01)$	0.00	0.00	0.004			
Pr $\sigma_u^2 \leq \bar{\chi}^2(01) / F$	0.00	0.00	0.53	32.03	61.78	70.05
R^2				.64	.85	.81
N	48	48	48	48	48	48

Notes: *Standard errors in parentheses* *N = Number of observations* [a] *Significant at the .01 level* [b] *Significant at the .05 level* [c] *Significant at the .10 level*

NNMR: *Neonatal mortality rate (deaths per thousand live births)*
PNNMR: *Post-neonatal mortality rate*
IMR: *Infant mortality rate*
GDP: *per capita gross national product, US dollars*
Matmort: *Maternal mortality rate (deaths per hundred thousand births)*

Table 4

Perinatal Mortality Parameter Estimates from Frontier Function and OLS Regressions

Regression	*(17)*	*(18)*	*(19)*	*(20)*	*(21)*	*(22)*	*(23)*	*(24)*
Specification	Log-log	Log-log	Log-log	Log-log	Log-log	Log-log	Log-log	Log-log
Data source	WHO	WHO	WHO	WHO	WHO	WHO	WHO	WHO
Regression type	Frontier, normal/half normal	Frontier, normal/half normal	OLS	OLS	Frontier, normal/half normal	Frontier, normal/half normal	OLS	OLS
Dependent variable	D1MR	D1MR	D1MR	D1MR	D2_6MR	D2_6MR	D2_6MR	D2_6MR
Regressors:	*Did not converge*							
Constant		-7.583	-35.468	-30.325	17.373[d]	-0.175	4.927	-0.172
GDP		0.403	1.414	0.139	0.964[c]	-0.033	0.787	-0.034
GDP^2		-0.030	-0.097	-0.022	-0.065[b]	-0.011	-0.058	-0.011
Matmort		0.056	-0.114	0.011	0.218[a]	0.311[a]	0.169[b]	0.311[a]
% urban		2.861	14.538	14.196[d]	-9,571[c]	0.408	-3.423	0.404
$(\% \text{ urban})^2$		-0.268	-1.669	-1.601[d]	1.129[c]	-0.039	0.405	-0.039
$\ln \sigma_u^2$		-0.802[b]			-1.074[b]	-9.846		
$\ln \sigma_v^2$		-3.468[a]			-2.889[a]	-2.014[a]		
Likelihood ratio test of $\sigma_u^2=0$: $\bar{\chi}^2(01)$		4.21			2.38	0.00		
Pr $\sigma_u^2 \leq \bar{\chi}^2(01)/F$		.98	4.39	6.48	.94	.00	19.13	36.97
R^2			.19	.29			.61	.68
N	66	48	66	48	66	48	66	48

Notes: *Standard errors in parentheses* *N = Number of observations* *[a] Significant at the .01 level* *[b] Significant at the .05 level* *[c] Significant at the .10 level*

D1MR: *First day of life mortality rate (deaths per thousand live births)*

D2_6MR: *Day 2 through 6 mortality rate*

IMR: *Infant mortality rate*

GDP: *per capita gross national product, US dollars*

Matmort: *Maternal mortality rate (deaths per hundred thousand births)*

Table 4, continued

Perinatal Mortality Parameter Estimates from Frontier Function and OLS Regressions

Regression		(25)	(26)	(27)	(28)
Specification		Log-log	Log-log	Log-log	Log-log
Data source		WHO	WHO	WHO	WHO
Regression type		Frontier, normal/half normal	Frontier, normal/half normal	OLS	OLS
Dependent variable		W1MR	W1MR	W1MR	W1MR
Regressors:					
Constant		-0.175	3.013[a]	-1.147	2.656[a]
GDP		0.375	-0.197[a]	0.583	-0.188[a]
GDP^2		-0.030		-0.044	
Matmort		0.486[c]	0.137[b]	0.382	0.082
$(Matmort)^2$		-0.047		-0.040	
$\ln \sigma_v^2$		-3.410[a]	-3.142[a]		
$\ln \sigma_u^2$	Constant	-0.745[c]	72.532[c]		
	PNMR	-0.120			
	ln % urban		-34.715[c]		
	$(\ln \%\ urban)^2$		4.076[c]		
Pr $\sigma_u^2 \leq \bar{\chi}^2(01)/F$				13.23	13.23
R^2				.41	.41
N		66	66	66	66

Notes: Standard errors in parentheses N = Number of observations [a] Significant at the .01 level [b] Significant at the .05 level [c] Significant at the .10 level
W2_4MR: Week two through four mortality rate (deaths per thousand live births) W1MR: Week 1 mortality rate
GDP: per capita gross national product, US dollars
Matmort: Maternal mortality rate (deaths per hundred thousand births

Table 5

Frontier Estimated as a Percent of Recorded Infant Mortality for selected countries

	WHO mortality database							UN Pop database
	D1MR	**D2_6MR**	**W2_4MR**	**NNMR**	**PNMR**	**IMR**		**IMR**
Albania	213	159	375	231	94	145		73
Bahrain	379	163	126	191	75	123		60
Brazil	96	128	171	146	196	170		106
Colombia	97	136	173	143	220	175		97
Costa Rica	97	94	139	107	106	101		99
Dominican Republic	152	157	290	202	320	252		75
Guyana	72	69	152	97	205	146		80
Haiti	8179	904	175	554	1000	735		212
Kenya	n/a	n/a	n/a	n/a	n/a	n/a		224
Kuwait	119	78	77	90	64	74		63
Kyrgyz Republic	111	92	194	132	481	270		96
Malawi	n/a	n/a	n/a	n/a	n/a	n/a		368
Mexico	117	79	124	106	82	92		64
Moldova	138	102	260	155	274	216		105
Mongolia	119	104	120	125	114	116		74
Peru	155	328	226	284	355	335		123
Philippines	150	199	347	242	407	327		139
Romania	294	71	123	124	72	96		90
South Africa	121	149	201	172	89	126		59
Tanzania	n/a	n/a	n/a	n/a	n/a	n/a		232
Thailand	990	175	319	326	121	195		79
Zimbabwe	n/a	n/a	n/a	n/a	n/a	n/a		254

Note: *Regressions are taken from a data set covering countries with 85% or better vital statistics coverage. Specification is log-log; Frontier with normal/half-normal errors. Explanatory variables include a constant, GDP, GDP^2, maternal mortality, and $(\text{maternal mortality})^2$.*

Table A.1

Background Data: Infant Mortality (per thousand live births) for Countries with 85% or Better Coverage

	IMR WHO 2004	IMR UN Pop 2005	IMR UN Unicef 2003	All 1 Year WHO Mortality database
Northern & Western Europe				
Austria	5	5	4	3.9
Belgium	4	4	4	8.9
Denmark	4	5	3	5.7
Finland	3	4	4	2.9
France	4	5	4	4.3
Germany	4	5	4	4.3
Greece	4	7	4	5.2
Iceland	2	3	3	2.5
Ireland	5	6	6	5.0
Italy	4	5	4	4.7
Netherlands	4	5	5	4.9
Norway	3	4	3	4.1
Portugal	4	6	4	5.0
Spain	4	5	4	3.5
Sweden	3	3	3	3.4
Switzerland	4	4	4	4.9
United Kingdom	5	5	5	4.9
Middle East & North Africa				
Egypt	26	37	33	.
Israel	5	5	5	6.3
Kuwait	10	10	8	8.5
Syrian Arab Republic	15	18	16	.
Asia				
Japan	3	3	3	3.0
Republic of Korea	5	4	5	.
Thailand	18	20	23	8.1
Latin America & Caribbean				
Argentina	16	15	17	16.0
Belize	32	31	33	19.5
Chile	8	8	8	8.4
Cuba	6	6	6	6.9
Guatemala	33	39	35	30.2
Mexico	23	21	23	14.6
Panama	19	21	18	14.6
Trinidad and Tobago	18	14	17	.
Uruguay	12	13	12	13.6
Venezuela	16	18	18	15.7
Africa				
Mauritius	12	15	16	15.4

North America & Oceania				
Australia	5	5	6	4.8
Canada	5	5	5	5.4
New Zealand	5	5	5	6.2
USA	6	7	7	6.8
Formerly Socialist Countries				
Albania	16	25	18	12.7
Belarus	8	15	13	.
Bosnia and Herzegovina	13	14	14	.
Bulgaria	12	13	14	12.7
Croatia	6	7	6	7.1
Czech Republic	4	6	4	3.8
Estonia	6	10	8	5.5
Hungary	7	8	7	7.6
Latvia	9	10	10	9.5
Lithuania	8	9	8	7.7
Macedonia	13	16	10	.
Mongolia	41	58	56	36.9
Poland	7	9	6	7.8
Romania	17	18	18	16.9
Russian Federation	13	17	16	.
Serbia and Montenegro	13	13	12	.
Slovakia	7	8	7	8.8
Slovenia	4	6	4	3.7
Ukraine	14	16	15	.

Table A.2

Background Data: Infant Mortality (per thousand live births) for Countries with Less Than 85% Coverage

Name	IMR WHO 2004	IMR UN Pop 2005	IMR UN Unicef 2003	All 1 Year WHO Mortality DB
Middle East & North Africa				
Algeria	35	37	35	.
Afghanistan	165	149	165	.
United Arab Emirates	7	9	7	.
Bahrain	9	14	12	6.8
Cyprus	4	6	4	.
Iran	32	34	33	.
Iraq	102	94	102	.
Jordan	23	23	23	.
Lebanon	27	23	27	.
Libya	18	19	13	.
Morocco	38	38	36	.
Oman	10	16	10	.
Pakistan	80	79	81	.
Qatar	10	12	11	.
Saudi Arabia	22	23	22	.
Tunisia	21	22	19	.
Turkey	28	42	33	.
Yemen	82	69	82	.
Asia				
Bangladesh	56	59	46	.
Brunei Darussalam	8	6	5	.
Bhutan	67	56	70	.
Indonesia	30	43	31	.
India	62	68	63	.
Sri Lanka	12	17	13	.
Myanmar	75	75	76	.
Malaysia	10	10	7	.
Nepal	59	64	61	.
Philippines	26	28	27	11.9
Papua New Guinea	67	71	69	.
Singapore	2	3	3	2.0
East Timor	64	94	87	.
Latin America & Caribbean				
Bolivia	54	56	53	.
Brazil	32	27	33	16.9
Chile	8	8	8	8.4
Côte d'Ivoire	118	118	117	.
Colombia	18	26	18	14.4
Costa Rica	11	11	8	10.8

Dominican Republic	27	35	29	10.5
Ecuador	23	25	24	15.0
Guyana	47	49	52	26.8
Honduras	31	32	32	.
Haiti	74	62	76	17.9
Jamaica	17	15	17	7.2
Nicaragua	31	30	30	11.2
Peru	24	33	26	12.1
Paraguay	21	37	25	12.9
El Salvador	24	26	32	9.9
Suriname	30	26	30	12.0
Africa				
Angola	154	139	154	.
Benin	90	105	91	.
Botswana	75	51	82	.
Burkina Faso	97	121	107	.
Burundi	114	106	114	.
Cameroon	87	94	95	.
Central African Republic	115	98	115	.
Chad	117	116	117	.
Congo	79	72	81	.
Congo, Dem Rep	129	119	129	.
Djibouti	100	93	97	.
Equatorial Guinea	123	102	97	.
Eritrea	52	65	45	.
Ethiopia	110	100	112	.
Gabon	59	58	60	.
Gambia	89	77	90	.
Ghana	68	62	59	.
Guinea	101	106	104	.
Guinea-Bissau	126	120	126	.
Kenya	78	68	79	.
Lesotho	55	67	63	.
Liberia	157	142	157	.
Madagascar	76	79	78	.
Malawi	109	111	112	.
Mali	121	134	122	.
Mauritania	78	97	120	.
Mozambique	102	101	109	.
Namibia	42	44	48	.
Niger	152	153	154	.
Nigeria	103	114	98	.
Rwanda	118	116	118	.
Senegal	78	84	78	.
Sierra Leone	165	165	166	.
Somalia	133	126	133	.
South Africa	54	43	53	20.2
Sudan	62	72	63	.
Swaziland	102	73	105	.

Tanzania	78	104	104	.
Togo	79	93	78	.
Uganda	81	81	81	.
Zambia	104	95	102	.
Zimbabwe	78	62	78	.
North America & Oceania				
Fiji	16	22	16	.
Former or Current Socialist Nations				
Armenia	29	30	30	.
Azerbaijan	75	76	75	.
China	26	35	30	.
Cuba	6	6	6	6.9
Czech Republic	4	6	4	3.8
Georgia	41	41	41	.
Croatia	6	7	6	7.1
Kazakhstan	63	61	63	.
Kyrgyzstan	58	55	59	19.6
Cambodia	97	95	97	.
Lao PDR	65	88	82	.
Moldova	23	26	26	12.6
Korea (North)	42	46	42	.
Tajikistan	91	89	92	.
Turkmenistan	80	78	79	.
Uzbekistan	57	58	57	.
Viet Nam	17	30	19	.