The Estimation of Unwanted Fertility: Development of A New Method

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15 March 2006

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Paper for presentation at the annual meeting of the Population Association of America, Los Angeles, 30 March - 1 April 2006. This research is supported by grant R03 HD051697-01 from the National Institute of Child Health and Development. The paper has benefited from comments at seminars at the Ohio State University and the City University of New York.

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ABSTRACT

The estimation of unwanted fertility is a major objective of demographic surveys, including the Demographic and Health Surveys [DHS]. Levels and trends in unwanted fertility are important input to the formulation of population policy, and they are used in the evaluation of family planning programs. Yet the existing methods, by common agreement, possess important deficiencies. We have developed a new method for the estimation of unwanted fertility that in most settings will suffer from less downward bias than existing methods (Casterline & el-Zeini 2005). The method is simple to apply and makes minimal data demands, in particular relying on the most valid and reliable attitudinal item (prospective fertility preferences). Illustrative results are presented from application of the new estimator to six recent DHS surveys. In all six countries, the new estimator yields substantially higher estimates of unwanted fertility than existing methods. This paper develops the new estimator further and considers several potential sources of error: a strategy for calculating an unwanted TFR is proposed; robustness of the estimator to trends in fertility preferences is assessed; and sampling errors are calculated.

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I. INTRODUCTION

The estimation of unwanted fertility is a major objective of demographic surveys, including the NSFG in the U.S. and the Demographic and Health Surveys [DHS] conducted in Asia, Africa, and Latin America. Estimates of unwanted fertility serve multiple purposes. First, they are input to forecasts of future trends in fertility, indicating the scope for fertility to fall via more perfect fertility control. Secondly, and following closely from the first purpose, estimates of unwanted fertility provide a rationale for public and private investment in the provision of family planning services, and they may be used in the targeting of such services (by geography, socioeconomic status, or demographic characteristics). Third, unwanted births (and, possibly, their families and communities) are posited to be disadvantaged with respect to a large variety of outcomes, including health/mortality, cognitive development, mental health, schooling and other human capital investments, and economic well-being.

Estimates of unwanted fertility are extremely influential. In the U.S., the IOM volume by Brown and Eisenberg (1995) was a notable contribution to public discussion of unwanted fertility rates and their societal and individual consequences. Periodically estimates of unintended fertility make an impression in the media and policy arenas, a recent example being Henshaw's (1998) estimate, derived by combining survey data with estimates on induced abortion obtained from other sources, that roughly one-half of U.S. pregnancies – 3 million per annum – are unintended. In low-income societies, levels and trends in unwanted fertility are important input to the formulation of population policy at the national level, and they are routinely adduced as evidence for the success (or failure) of family planning programs. In the periodic release of findings from the most recent DHS survey, estimates of unwanted fertility often attract considerable attention in national forums.

A large literature examines the consequences of unwantedness, and this literature will not be reviewed here. We do acknowledge, however, that the nature and magnitude of the effects of unwanted fertility is a subject of considerable dispute. One reason for this dispute is doubt about the measurement of unwanted fertility, i.e. the validity of the classification of pregnancies/births as unwanted (or unintended, including mistimed conceptions) (e.g. Rosenzweig and Wolpin 1993, Trussell *et al.* 1999, Joyce *et al.* 2002,).

There has also been much discussion of the relative merits of a set of closely-related subjective measures of fertility (and their corresponding survey items): ideals, desires (or preferences), intentions, expectations. Each of these can be framed with reference to lifetime births/children or with reference to having another birth/child. Thoughtful critical discussions of how these concepts can be measured, what they represent, and their utility for various analytical purposes include Ryder and Westoff (1971), McClelland (1983), Adler (1992) Miller (1994), Miller and Pasta (1995), Petersen and Moss (1997), Fischer *et al.* (1999), Stanford *et al.* (2000), and Santelli *et al.* (2003). These pieces focus primarily on the U.S. There has been far less wrestling with these conceptual distinctions in research on fertility in low-income countries, although see McClelland (1983), Stycos (1984), Lightbourne (1985), and Bongaarts (1990). There is undoubtedly scope for improvement in the measurement of the subjective dimensions of fertility in surveys in developing countries; in particular, very much in order would be measurement strategies that allow for *continuums* rather than a few discrete categories, that capture qualities such as *intensity* and *certainty*, and that permit fertility intentions to have *multiple dimensions* (Thomson and Brandreth 1995, Bachrach and Newcomer 1999, Stanford *et al.* 2000, and Santelli *et al.* 2003).

Such advances in measurement are not our immediate concern in this research. Rather, we take as given decisions made three decades ago, from which has accumulated – and continues to accumulate -- an enormous body of empirical data on fertility preferences and desired family size: 40 WFS surveys, several dozen Contraceptive Prevalence Surveys [CPS], and (to date) over 100 DHS surveys. Our goal is to develop methods for making optimal use of this data. In tackling the problem of estimating unwanted fertility, we assume this will continue to be a high-priority objective of demographic surveys, especially surveys (such as DHS) conducted in low-income settings and where fertility is above replacement level.

II. EXISTING METHODS OF ESTIMATING UNWANTED FERTILITY

Existing methods for estimating unwanted fertility have important defects that are well-recognized. There are three methods commonly applied to national survey data from developing countries (such as the DHS)[1]:

Retrospective Direct Question [Method #1].

In most DHS surveys, women have been asked birth-by-birth whether births in a recent reference period leading up to the survey (typically three-to-five years) were wanted at the time of conception. The item is: "At the time you became pregnant with <name>, did you want to

[1] We do not include Bongaarts (1990), who introduces a method for calculating wanted and unwanted total fertility rates, from which an estimate of the proportion of births unwanted can be derived via calculations that are not explained. Nor do we include Collumbien *et al*. (2004), who employ macro-simulation to calculate expected proportions of births unwanted during a twelve-month prospective period, making use of measured patterns of fertility preferences and contraception and assumed schedules of fecundability, method failure, and induced abortion.

become pregnant then, did you want to wait until later, or did not want (more) children at all?" On the face of it, this method for detecting unwanted births/pregnancies has much to recommend it: it has the considerable virtue of attempting to measure fertility preferences at the time of conception, the phenomenon of interest if the ultimate goal is to assess the potential impact of more perfect fertility control; both unwanted and mistimed births can be detected; because each reported birth in the reference period is classified as wanted or unwanted, this method gives the analyst maximum flexibility in investigating the covariates of unwanted births/pregnancies (causes or consequences).

Unfortunately, this method suffers from an enormous shortcoming, namely the tendency of women to engage in *ex post* revision of the declared wantedness of a birth/pregnancy. This problem has been recognized for many decades. In low-income and non-Western settings, a reluctance to report a past birth as unwanted appears to be the more common source of bias (Bongaarts 1990, Adetunji 1998). Usually these past births are living children at the time of the interview. Two types of evidence are indicative of this aversion to declaring births unwanted: first, in most applications to survey data, this method yields a lower fraction of births unwanted than Method #2 below; second, in longitudinal studies where wantedness can be assigned both prospectively (stated preference for another birth in an interview preceding the conception) and retrospectively, there is a marked tendency for women to report births as wanted retrospectively even though prospectively the woman indicated a desire to terminate childbearing (Williams and Abma 2000, Bankole and Westoff 1998, Casterline *et al.* 2001, Collumbien *et al.* 2004, Koenig *et al.* 2005). Tellingly, the downward bias in this method has been recognized and accepted by the DHS. While retrospective wantedness data are collected and presented in basic tabulations in the DHS country reports, these data are not the basis for DHS estimation of unwanted fertility rates. Instead the DHS uses Method #2.

Comparison of Ideal Number of Children with Number of Living Children [Method #2]

In DHS surveys, women are asked: "If you could go back to the time you did not have any children and could choose exactly the number of children to have in your whole life, how many would that be?" As developed by Lightbourne (1985) in analysis of WFS data, births during a reference period preceding the survey can be classified as wanted or unwanted by comparing the response to this question with the number of living children at the time of the conception. Generally this method classifies a larger number of births unwanted than Method #1 (as shown in Table 1, discussed below), evidence supportive of the greater validity of Method #2, assuming women are in fact reluctant to label children unwanted. Like Method #1, this method yields a classification of individual births, thus allowing for maximum flexibility in investigating the covariates of unwanted fertility.

The method has important defects, however. First of all, in some societies a non-negligible fraction of survey respondents do not provide a numeric response to the ideal number of children item (rather "not stated" or "up to God").[2] Second, the stated ideal number of children is known to be upwardly biased because women are reluctant to supply a number that is less than their current number of living children, a phenomenon often termed "rationalization". Third, numerous studies – in both high-income and low-income settings -- have demonstrated that the ideal number of children has relatively low test-retest reliability (e.g. MacDonald *et al.* 1978, O'Muircheartaigh and Marckwardt 1981, Stycos 1984, Curtis and

[2] Current DHS practice is to regard a non-numeric response as equivalent to a very large ideal number for the purposes of estimating unwanted fertility, and hence women giving non-numeric responses cannot have unwanted births.

Arnold 1994, Thomson and Brandreth 1995, Bankole and Westoff 1998). Fourth, a variety of factors can render the stated ideal number of children misleading for the purpose of ascertaining whether births are unwanted (Bongaarts 1990): (i) respondents may have preferences regarding sex of children that lead them to proceed beyond their ideal number; (ii) child deaths may lead respondents to have more live births than their stated ideal number; (iii) respondents may choose to curtail childbearing short of an "ideal", in deference to various real-world constraints. Fifth, in contrast to Method #1, Method #2 does not refer explicitly to preferences at the time of conception. It is likely that the largest sources of bias in this method are the first two – non-numeric responses and the rationalization of past birth history. Both lead to <u>under-estimation</u> of unwanted fertility in most contexts. We note again that Method #2 is used to calculate the unwanted fertility rates presented in DHS country reports, on the DHS website, and in DHS comparative analyses.

Prospective Assessment via Panel Survey [Method #3]

When panel data are available, births can be classified as wanted or unwanted according to the woman's prospective preferences. In DHS surveys women are asked: "Would you like to have (a/another) child, or would you prefer not to have any (more) children?" Further items ascertain how soon women would like to have the next birth. Based on these responses, births occurring in the period between interviews can be classified as unwanted, mistimed, or wanted and on time. This method has considerable appeal: for one thing, it does not suffer from the biases inherent in Methods #1 and #2, namely the reluctance of women to admit (explicitly or implicitly) that a past birth was unwanted; second, it relies on the fertility attitudinal item ("Do you want another child?") which the existing research literature generally regards as most valid and reliable[3]; third, and a corollary of the second, the fraction of respondents not providing a usable response to this item tends to be relatively low; fourth, like Method #1 but unlike Method #2, this method permits identification of births that are wanted but mistimed; finally, Method #3 shares with Methods #1 and #2 the advantage of classifying individual births/pregnancies, thereby facilitating analysis of the covariates of unwanted fertility.

This method might well be the method of choice were panel data widely available. But they are not, especially for national samples -- DHS surveys are virtually always cross-sectional surveys, with no follow-up. Where panel data are available, sample attrition is a chronic problem.

III. A NEW METHOD: THE AGGREGATE PROSPECTIVE ESTIMATOR

While panel survey data (Method #3) are rarely available for national samples in developing countries, many countries have conducted more than one DHS survey. The most common practice is to conduct DHS surveys at five-year intervals, but in some countries (e.g. Bangladesh and Egypt) they have been conducted at more frequent intervals. In each survey, women are asked about their desire to have another birth (i.e. their prospective preferences). While individual women are not re-interviewed, the design is prospective at the aggregate level: sub-groups of women interviewed at one date can be identified in survey data collected at a later date (e.g. parity sub-groups and birth cohorts). This provides the basis for the aggregate prospective estimator of unwanted fertility presented here. Furthermore, as shown

[3] Assessment of validity (via predictive validity) include Westoff and Ryder (1977), Hermalin *et al.* (1979), McClleland (1983), DeSilva (1991), Bankole and Westoff (1998), Schoen *et al.* (1999), Razzaque (2000), Morgan (2001), and DaVanzo *et al.* (2003). Reliability has been assessed via test-retest reliability, which shows prospective preferences to be far more reliable than the ideal number of children (or, for that matter, retrospective wantedness): e.g., Coombs (1977), Stycos (1984), Curtis and Arnold (1994), Thomson and Brandreth (1995), Bankole and Westoff (1998).

below, while the logic of this estimator presumes the existence of two successive cross-sectional measurements, it can just as easily be applied to data collected at one date, indeed there are a number of advantages to doing so.

To our knowledge, this estimator has not heretofore appeared in the literature.

The logic of the aggregate prospective method is straightforward. Assume that we have information on women at two dates, t_1 and t_2. Let ${}_1N^p$ denote the proportion of women of parity p at date t_1 who do not want another child. At the later date t_2, a survey offers information on prospective fertility preferences and contains a birth history as well. Using this birth history, women in the survey can be classified according to their parity p at t_1. For each parity cohort, the proportion who had a birth since t_1 – B^p -- can be computed. In addition, and the quantity on which the estimation hinges, we can compute for each parity cohort the value ${}_2v^p$, which is the proportion of women at time t_2 who do not desire to have an additional child and had no births during the period t_1 to t_2. Assuming constancy in preference between the two time points, ${}_2v^p$ can be interpreted as the subset of the proportion ${}_1N^p$ who successfully implemented their desires for no additional children. The difference between the two quantities, then, corresponds to the fraction who failed to implement their desires, i.e. have had unwanted births of order (p+1).

Given the values of ${}_1N^p$, ${}_2v^p$, and B^p, the fraction of births of order (p+1) born between t_1 and t_2 who were unwanted by their mothers, u^{p+1}, can be computed as,

$$u^{p+1} = ({}_1N^p - {}_2v^p) / B^p \qquad (1)$$

where

u^{p+1} is the proportion of births of parity (p+1) born during the period from t_1 to t_2 that are unwanted

$_1N^p$ is the proportion of women of parity p at time t_1 who do not desire additional children;

B^p is the proportion of women of parity p at time t_1 who had a child of order (p+1) before t_2 (whether wanted or not); and

$_2v^p$ is the proportion of women of parity p at time t_1 who desire no more children and did not have a birth within the interval between t_1 and t_2 -- that is, the proportion with successful implementation of their fertility preferences during the reference period. Assuming constant preferences, $_2v^p$ is obtained as the proportion of women of parity p at the t_2 survey who desire no further children and did not have a birth during the period since t_1.

The difference between $_1N^p$ and $_2v^p$ is the proportion of all women of parity p at the earlier date t_1 who had an unwanted birth in the interim interval. Division by B^p (proportion of women who had a birth in the interval) yields an estimate of the proportion of women with births of order (p+1) whose birth was unwanted (u^{p+1}). Note that strictly speaking u^{p+1} is the proportion of women of parity p at t_1 having unwanted births of order (p+1) in the historical period from t_1 to t_2. The aggregate prospective estimator uses this as an estimate of the proportion of births of order (p+1) that were unwanted. This equivalence holds if women have no more than one birth in the interval. Below we discuss the implications of the violation of this assumption (i.e. women having more than one birth in the period from t_1 to t_2).

The overall fraction of unwanted births, u, can then be obtained as the weighted average of u^{p+1} for all birth orders,

$$u = \sum_{p=0} g^{p+1} u^{p+1} \qquad (2)$$

where

g^{p+1} is the proportion of births of order (p+1) among all births occurring between t_1 and t_2.

The aggregate prospective estimator is very simple in its mathematical structure and has minimal data requirements (a birth history covering the period from t_1 to t_2, and current parity and prospective preferences at t_1 and t_2). Application to DHS (or equivalent) survey data requires attention to a number of further details that are discussed in Appendix B.

Sources for the Estimate of Prospective Preferences at the First Date

The parity-specific proportion of women who desire no more births at t_1 -- ${}_1N^p$ -- is a crucial quantity in the aggregate prospective estimator. How can one obtain a valid estimate of this quantity in the absence of a panel survey design? Two approaches are suggested here, a two-survey variant and a one-survey variant.

The two-survey variant of the aggregate prospective estimator can be used if the researcher has access to two comparable and closely-spaced cross-sectional surveys. If the two sample frames are essentially the same, then the two surveys can be regarded as representative of the same population of women, and hence the two surveys offer prospective observation at the aggregate level. To further enforce a prospective design, computations on women at the second survey should be restricted to women who were eligible for interview at the first survey. This entails restriction with respect to the woman's age at t_1 and, in those surveys that only interview ever-married women, whether or not the woman first married prior to t_1 (using the reported date of first marriage in the data collected at t_2). Finally, for reasons discussed in the Appendix, in most settings it is best if the two surveys are spaced only a few years apart, optimally no more than three years apart. When successive surveys are spaced farther apart (five years is a common interval for DHS surveys), t_1 can nevertheless be set closer to t_2 and ${}_1N^p$ obtained by interpolation between the two sets of estimates of parity-specific prospective preferences.

The aggregate prospective method does not require two surveys. When data from just one cross-sectional survey are available, the parity-specific preferences at t_2 can be used as valid estimates of fertility preferences at t_1. This requires an assumption of constancy in aggregate preferences between t_1 and t_2. But this assumption is implicit in equation (1) and also required by the two-survey variant. Drawing all required pieces of information from one survey has the further advantage of avoiding non-comparabilities between surveys in sample design and measurement procedures (questionnaire design, interviewer behavior, data processing). It also has the considerable appeal of substantially enlarging the applicability of the aggregate prospective estimator – in this variant, the new estimator can be applied wherever one cross-sectional demographic survey is available (provided the survey contains a birth history for the reference period, current parity, and the prospective preference item). Finally, as demonstrated below, when the assumption of stable aggregate-level fertility preferences is violated, the one-survey variant can be expected to yield a less biased estimate of unwanted fertility.

Equations (1) and (2) involve a number of simplifying assumptions, some of which are relaxed in the actual computations of the fraction of unwanted births u. The reader is referred to the Appendix A for a discussion of these assumptions and how likely violations are accommodated.

IV. ILLUSTRATIVE RESULTS

Estimates of unwanted fertility for the six countries, as generated by three methods -- retrospective direct question (Method #1), comparison of ideal number of children with number of living children at conception (Method #2), and the aggregate prospective estimator -- are presented in Table 1. The table shows estimates produced by the one-survey variant of the aggregate prospective estimator, for births in the 30 months preceding the survey.[4]

Before assessing the estimates from the new estimator, we can briefly compare the two conventional estimators. Three patterns emerge. In Bangladesh and Egypt, far fewer births are classified as unwanted in retrospective direct reports than appear to be unwanted when one compares ideal and actual numbers of children. The likely explanation for this differential is a strong aversion in Bangladesh and Egypt to admitting that children were unwanted at conception. In contrast, in Ghana the retrospective direct estimator yields a distinctly higher fraction of births unwanted. Inspection of order-specific estimates reveals that this discrepancy is due primarily to far higher proportions of lower-order births classified as unwanted in retrospective direct reports as against the ideal vs. actual comparison. This is a surprising phenomenon that merits further empirical investigation. Finally, in the remaining three countries (Kenya, Peru, and Philippines) the two conventional estimators produce roughly the same levels of unwanted fertility.

Of more central interest in this research, in Table 1 the aggregate prospective estimator consistently yields the highest estimate of the fraction of births unwanted: as compared to the ideal vs. actual method (currently used by DHS), the new estimates range from 26 percent higher in Bangladesh to 84 percent higher in Egypt. Discrepancies of this magnitude can radically alter the scientific understanding of fertility levels and trends, and have major implications for the design and evaluation of national population policies. Considering a broader universe of surveys, Figures 1 and 2 summarize analysis of 46 DHS surveys conducted from 1991 through 2005 (no more than one survey per country). Figure 1 shows that the

[4] Software (in Stata) for application of the aggregate prospective estimator on DHS standard recode files is available from the authors.

aggregate prospective estimator produces estimates that are, on average, about 12 percentage points higher than the conventional estimate in Asia and Latin America and about 7 percentage points higher in sub-Saharan Africa. Figure 2 presents these differences in proportionate terms: outside sub-Saharan Africa, the new estimator generates estimates of the percent of births unwanted that are almost 50 percent higher (ratio of 1.5) on average than the conventional estimator, whereas in sub-Saharan Africa the estimates are about 70 percent higher (ratio of 1.7) on average. These are large differences. The new estimator reveals that unwanted births are far more common in low-income societies than the most widely-distributed estimates indicate.[5]

But can these discrepancies be believed? Are the aggregate prospective estimates credible? Is it sensible to believe that the true level of unwanted fertility is so much higher than has been believed? Such doubts can be addressed via an examination of the of the survey data.

For this exercise we select Egypt, because the discrepancies between the conventional estimates and the new estimates are the largest among the six countries analyzed here. The further investigation must be conducted on a parity-specific (or, equivalently, birth-order-specific) basis. Consider the data for women of parity 2 and births of order 3, as reported in the 2003 survey:

					Percent Births Unwanted	
Parity	Percent wanting no more births	Percent having birth	Percent wanting no more births, & no births	Order	Ideal vs. Actual	Aggregate Prospective
2	58	37	41	3	24	49

[5] The percentages of conceptions that are unwanted will be even higher in settings where the incidence of induced abortion is high.

The ideal vs. actual estimator (the conventional DHS methodology) classifies 24 percent as unwanted, whereas the aggregate prospective estimate is 49 percent unwanted. Which estimate is the more plausible? Virtually all of the births of order three in the 30 months prior to the 2003 survey were produced by women at parity two at the beginning of the reference period. According to the 2003 survey, 58 percent of such women want no more births. Now it is of course entirely possible that order-three births were produced disproportionately by the 42 percent of parity-two women who wanted to have further children, as suggested by the ideal vs. actual estimate of 24 percent unwanted. Given that 37 percent of parity-two women at the beginning of the reference period had a birth during the period, this implies that 9 percent of all parity-two women at the outset had an unwanted birth (0.24*37% = 9%), which would also represent 15 percent of the parity-two women who wanted no more children (9/58 = 15%). Thus far, all of this is believable, if one assumes relatively effective birth control on the part of parity-two women. But one also learns from the 2003 survey that 41 percent of these parity-two women had no births during the 30-month reference period and also want no more children in 2003. If these women are a subset of the 58 percent of parity-two women who wanted no more children – i.e., if stable preferences are assumed – this implies that 17 percent of parity-two women had an unwanted birth prior to the 2003 survey (58% - 41%), a figure far higher than the 9 percent implied by the ideal vs. actual estimator. These figures lead directly to the aggregate prospective estimate of 49 percent of order-three births unwanted. In short, considering the birth histories and prospective fertility preferences as reported in the 2003 survey, we submit that the aggregate prospective estimate of 49 percent unwanted is far more consistent with the empirical evidence than the conventional estimate of 24 percent unwanted.

The same exercise can be performed for births of order four and five, with the same conclusion: the higher estimates generated by the proposed new estimator are far more credible than the lower estimates currently accepted, provided that the birth history and prospective fertility preference are regarded as valid.[6] And, critically, one must also assume that fertility preferences are constant throughout the reference period. An alternative explanation for the discrepancies evident in Table 3 -- an explanation that cannot be readily dismissed -- is that a substantial fraction of the women who had births in the reference period changed their minds about their desire for another birth between the earlier survey and the time when they conceived. We note again that other existing methodologies for estimating unwanted fertility also assume constant fertility desires; this includes the estimator most commonly applied to DHS data (ideal vs. actual), which requires stability in the ideal number of children desired.

V. AN AGE-BASED VARIANT: CALCULATING TOTAL FERTILITY RATES

A common device for evaluating an estimate of unwanted fertility is to transform it into an unwanted total fertility rate (unwanted TFR). Not only are births per woman a readily-grasped metric, but this also reveals how much of the fertility in excess of replacement-level can be attributed to unwanted fertility. Calculating wanted and unwanted fertility rates is straightforward with conventional estimators, because individual births are classified as wanted or unwanted. To achieve the same end using the proposed aggregate prospective estimator, we develop an age-based variant. Once estimates of age-specific percents of births unwanted are obtained, the observed age-specific fertility rates can be broken into wanted and unwanted components.

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[6] Returning to the estimation of the percentage unwanted among order-three births in Egypt, for the conventional estimate to be correct, the percentage of parity-two women not wanting more children must be eight percentage points lower, or the percentage of parity-two women in 2003 not wanting more children and having no births in the preceding three years must be eight percentage points higher (or a combination of both errors that sums to eight percentage points). These would represent substantial errors in survey items – birth history, prospective preferences -- that are generally regarded as having high validity.

An age-based variant can be constructed along the lines of the order-based variant presented in section III. If we let *a* denote age-group at the beginning of the reference period, then

$$u^a = ({}_1N^a - {}_2v^a)/B^a \tag{3}$$

$$u = \sum_{a=1} h^a u^a \tag{4}$$

where

h^a is the proportion of births to women of age *a* at the beginning of the reference period among all births occurring between t_1 and t_2.

and other elements are as defined for equations (1) and (2).

Equations (3) and (4) apply the aggregate prospective estimator to age cohorts of women (i.e. birth cohorts) rather than parity cohorts, as in equations (1) and (2). The formal structure of the estimator remains the same, as do the various assumptions and technical issues discussed in Appendices A and B. Cohort-specific fertility rates can be multiplied by the u^a to obtain cohort-specific unwanted fertility rates, which in turn can be summed to obtain the unwanted TFR. Note that, rather than the more customary age-specific fertility rates, we calculate cohort-specific fertility rates, i.e. for women aged <20, 20-24, . . ., 45+ at the beginning of the reference period. These rates are, in fact, simpler to calculate than age-specific fertility rates (where both births and exposure are in terms of age during the reference period). In our analysis of DHS surveys to date, the TFRs calculated from cohort-specific fertility rates agree almost precisely, as they should, with the published TFRs derived from age-specific fertility rates.

But the wantedness of births is tied much more directly to women's parity than to their age. Because age is a weaker discriminator, application of equations (3) and (4) generally yields a lower estimate of the percent of births unwanted than equations (1) and (2). In an effort to prevent such attenuation, we can obtain the age-cohort-specific estimates (u^{a}) via stratification by parity (*p*) within age-cohort (*a*):

$$u^{a,p} = ({}_1N^{a,p} - {}_2\nu^{a,p}) / B^{a,p} \tag{5}$$

$$u^{a} = \sum_{p=0} d^{a,p} u^{a,p} \tag{6}$$

where

$d^{a,p}$ is the proportion of women of parity *p* among women of age *a* at the beginning of the reference period who have births between t_1 and t_2.

The results of applying the age-based estimator of equations (3) – (6) to the six DHS surveys are presented in Table 2. In the event, our effort to reconcile the order-based and age-based estimation via equations (5) and (6) is unsuccessful – in five of the six countries, the estimated percent of births unwanted differs between the two variants of the new estimator by three percentage points or greater. Bangladesh is the sole exception. In Peru and the Philippines, the order-based estimate exceeds the age-based estimate by more than six percentage points, an uncomfortably large difference. Generally the age-based estimate is lower than the order-based estimate; we regard this as evidence of downward bias in the algorithm for the age-based variant. In short, our work on this facet of the aggregate prospective is not yet complete. We will continue to work on the age-based variant, with the aim of developing an algorithm that yields age-based estimates that are consistent with the order-based estimates.[7]

Even with the apparent downward bias in the age-based estimates presented in Table 2, it is still the case that the aggregate prospective unwanted TFR exceeds the conventional estimate of the unwanted TFR in all six countries. The excess varies from 0.11 birth (Kenya) to 0.61 birth (Egypt), corresponding to ratios of 1.09 and 1.92, respectively. The ratio is more than 1.20 in four of the six countries, i.e. unwanted TFRs that are at least 20 percent greater than the conventional estimates. The overarching message is the same as in Table 1 and in Figures 1 and 2: the true rate of unwanted childbearing in Asia, Africa, and Latin America is higher than currently-accepted figures suggest.

VI. SAMPLING ERROR

Sampling error is an important form of error when population parameters are estimated from survey data. *A priori* one might posit that the aggregate prospective estimator would be subject to substantially larger sampling error than existing estimators because it relies on aggregate proportions that are themselves subject to sampling error.

In Table 3 we consider sampling error in the estimated proportions of births unwanted according to each of the three estimation methods of Table 1, again using the one-survey variant of the aggregate prospective method. For all three estimators, the sampling error is calculated via the bootstrap method (Efron and Tibshirani 1993, Horowitz 2001). Considering first the standard errors, these indicate that the aggregate prospective estimator produces the

[7] We have recently received a two-year award from NICHD to support this work.

least precise estimates. But this estimator also yields a distinctly higher percentage of births unwanted, and for this reason alone its standard error should be larger. The coefficient of variation (standard error divided by the mean) is a better basis for comparison across estimators. By this criterion the aggregate prospective estimator is not disadvantaged; indeed if anything, it appears to offer more precise estimates.

It is also very clear from the confidence intervals in Table 3 that the new estimates of the proportion of births unwanted are significantly different from estimates generated by existing methods.

VII. TRENDS IN PROSPECTIVE PREFERENCES

Equation (1) assumes that women of parity p at t_1 who want no further children are the sole source of unwanted births in the period between t_1 and t_2. This in turn requires an assumption of unchanging parity-specific preferences. Let us relax this assumption and evaluate the consequences for estimation of the proportion of births unwanted.

To determine the nature of the bias introduced by trends in preferences, we make the simplifying assumption that the timing of this change in a given population of women is distributed uniformly within the reference period t_1 to t_2. If so, simple algebra leads to a pair of expressions for correcting equation (1) for bias due to trends in prospective preferences. Both expressions assume that ${}_2v^p$ -- the proportion of women of parity p at t_1 who desire no more children and did not have a birth within the interval between t_1 and t_2 – is obtained at t_2. The first expression applies when ${}_1N^p$ -- the proportion of women of parity p at t_1 who do not desire additional children -- is obtained from a survey at t_1 (two-survey variant of the estimator), and the second expression applies when ${}_1N^p$ is obtained from a survey at t_2 (one-survey variant).

$$u^{p+1} = \{({}_1N^p + \frac{c^p}{2}) - ({}_2\nu^p - \frac{c^p(1-B^p)}{2})\}/B^p \qquad (7a)$$

$$u^{p+1} = \{({}_1N^p - \frac{c^p}{2}) - ({}_2\nu^p - \frac{c^p(1-B^p)}{2})\}/B^p \qquad (7b)$$

where,

c^p is the net proportion of women of parity p who shifted from wanting another child at t_1 to wanting no more children at t_2; and

all other quantities are as defined previously.

Equations (7a) and (7b) contain two adjustments for trends in prospective preferences. (i) The estimated proportion of women during the reference period who desire no further children is inflated/deflated by one-half of the proportion that switched to this status between t_1 and t_2. (ii) The estimated proportion of women with successful implementation during the reference period is deflated to take account of an over-estimation of the proportion of women who successfully implement a desire to have no further children throughout the entire reference period (over-estimated because some women were not in this status the entire period, rather switched to desiring no more between t_1 and t_2.)

In considering what these equations imply about the bias created by trends in preferences, we will assume that c^p is positive, reflecting a secular decline in desired fertility.[8] However, c^p is not constrained to be positive, particularly if the data sources at t_1 and t_2 are not comparable (in sample design, in measurement of preferences) but also simply due to sampling error. Indeed, an examination of DHS data reveals numerous instances of negative c^p.

[8] Even if preferences are unchanging, improved birth control will cause trends in parity-specific proportions of women wanting no further births. At the lower parities, this proportion will increase as successful birth control results in a cumulation of women desiring to stop. At higher parities, the effect of improved birth control on parity-specific preferences is less certain, but the more likely trend is a decrease in the proportion desiring no more children as higher parities become more selective of women with desires for a larger number of children.

The direction and magnitude of bias depends on the source of ${}_1N^{p}$. In the two-survey variant of the estimator, in which ${}_1N^{p}$ is obtained from a first survey at t_1, expression (7a) applies and the net adjustment to u^{p+1} calculated via equation (1) is $[\{(c^{p}/2)*(2 - B^{p})\}/ B^{p}]$. Because B^{p} is a proportion, this adjustment must be positive, implying an under-estimation of the proportion of births unwanted if trends in preferences are not taken into account. In the one-survey variant, in which ${}_1N^{p}$ is measured at t_2, expression (7b) applies and the net adjustment to u^{p+1} calculated via equation (1) is $[- c^{p}/2]$. That is, the proportion of births unwanted is over-estimated if trends in preferences are not taken into account. From this it is apparent that the bias in the presence of trends in preferences is opposite in direction for the two variants of the estimator -- when the weighted sum of the c^{p} is positive (weighted by the distribution of births by order (p+1)), the two-survey variant under-estimates unwanted fertility whereas the one-survey variant over-estimates it. A final point is that because B^{p} is a proportion, the absolute value of the bias in the two-survey variant ordinarily will exceed the absolute value of the bias in the one-survey variant: $|[\{(c^{p}/2)*(2 - B^{p})\}/ B^{p}]| > |[- c^{p}/2]|$ This differential diminishes as B^{p} increases and would disappear if B^{p} were to equal 1.0 (i.e. all women having at least one birth in the reference period).

In Table 4, equation (7b) is applied to the DHS survey data from the six countries, from this an amount of error due to trends in preferences is calculated. An estimate of c^{p}, the change in the parity-specific proportion wanting no further children, is obtained by comparing the two most recent surveys in each country. The message in Table 4 is unmistakable:

estimates of the percentage of births unwanted are essentially unaffected by observed trends in fertility preferences.[9]

From this exploration of the bias induced by trends in prospective preferences, we conclude that such bias is likely to pose little threat if the reference period is relatively brief (three years or less) and/or if the one-survey variant of the new estimator is employed. While the more widely-applicable one-survey variant over-estimates the proportion of births unwanted in the presence of anti-natalist trends in preferences, the absolute amount of bias is likely to be negligible in most contemporary settings.[10] A final point is that when the ideal number of children is declining, the current estimator of choice for DHS data (Method #2 above) is subject to bias in the same direction as the one-survey variant of the aggregate prospective estimator, i.e. an over-estimation of unwanted fertility.

VIII. CONCLUDING COMMENTS

The most important strength of the aggregate prospective method is that it relies on the attitudinal item that is generally thought to be most valid and reliable, namely the prospective preference item. Its other data requirements are minimal: the dates of recent births; and, where birth histories and fertility preferences are only obtained from ever-married women, the date of first marriage.

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[9] Elsewhere (Casterline and el-Zeini 2005) we show that, as expected, typically trends in preferences result in the two-survey variant under-estimating the percent of births unwanted. Also as expected, the error in the two-survey estimates are opposite in sign and larger in magnitude than the error in the one-survey estimates. The larger magnitude of error in the two-survey estimates is also a function of a longer reference period: four of the six two-survey estimates draw on surveys spaced five years apart, whereas the reference period for the one-survey estimates is set at 30 months. Longer reference periods obviously permit more change in preferences to occur.

[10] We have examined trends in parity-specific preferences in the 37 countries that have conducted at least two DHS surveys since 1990. This reveals that the amount of change in prospective preferences observed in the six countries examined here is generally characteristic of the larger set of countries.

The aggregate prospective method is not without limitations. First of all, it produces aggregate-level estimates only. It does not deliver a classification of individual births as wanted or unwanted, and this makes the method less flexible for the analysis of covariates of unwanted fertility. But note that estimated differentials in unwanted fertility can be obtained by applying the aggregate prospective estimator to data stratified by population sub-group (as defined by type of place of residence, region, educational attainment, household wealth, and so forth).[11] Second, the incidence of mistimed births cannot be ascertained. Third, as already noted, the aggregate prospective estimator assumes stability of preferences over the reference period, though the probable bias when this assumption is violated appears to be slight in most applications (see Table 5).

Admittedly this is a serious set of limitations. But note that the second and third are shared by Method #2, the currently sanctioned approach for estimating unwanted fertility rates from DHS data. The aggregate prospective method improves on Method #2 by not relying on the ideal number of children, an item of questionable validity. This leaves the first limitation – aggregate-level estimates only. This is a regrettable feature of the method, but surely an acceptable price to pay for more valid estimates of unwanted fertility for the dozens of countries with DHS or similar demographic surveys. And, indeed, it is aggregate-level parameters – e.g. the proportion of births unwanted, and the unwanted TFR -- that feed directly into policy formulation and program evaluation.

In substantive terms, the important conclusion from application of the new estimator to recent DHS data is that existing estimates of the incidence of unwanted births are downwardly biased, and to a substantial degree in many countries. This is evident from the six-country

[11] Sub-group estimates will be subject to larger standard errors, which makes it all the more important to calculate sampling errors, as illustrated in Table 3.

analysis of Table 1 and from the 46-country analysis of Figures 1 and 2. This conclusion strengthens the case for policies and programs that facilitate effective practice of birth control, in order that unwanted births might be avoided. A parallel implication is that the potential impact on fertility levels of more effective practice of birth control is larger than currently-accepted figures suggest.

APPENDIX A

Aggregate Prospective Estimator: Important Underlying Assumptions

A1. Maximum of one birth per woman in the reference period t_1 to t_2.

Women can have more than one birth in the period between the two dates if the elapsed time is sufficiently long or there are multiple-birth outcomes. Equations (1) and (2) classify only the first birth that women have within the reference period. The overall proportion unwanted (u), in contrast, applies to all births in the reference period, because it is a weighted average of u^{p+1} that uses the distribution of all births as weights. In effect, u^{p+1} is applied to all births of order (p+1) whether or not the births were the first to women in the interval. The underlying assumption is that the likelihood of desiring a birth of order (p+1) is the same whether this birth was the first in the reference period or was preceded by another birth.

Clearly this assumption is more consequential as the reference period lengthens and a larger fraction of births are not the first to women in the period. Table 2 shows the distribution of women according to their number of births in the reference period t_1 to t_2, for periods varying in length from 24 months to 60 months. When the length is three years, less than twenty percent of women having a birth in the period have more than one birth, and in Bangladesh, Ghana and Peru this fraction is just above ten percent. When the interval is extended to five years, however, the fraction of women having more than one birth increases sharply, to above one-quarter in all six countries and to almost one-half in Kenya. Although the assignment procedure for births beyond the first is defensible, the data in Table 2 suggest that the aggregate prospective estimator is on firmer ground when the reference period is three years or less.

A2. Correct representation of t_1 women at date t_2.

There are two issues here: survival, and eligibility criteria.

> *Survival***.** Women who die between t_1 and t_2 may have given birth during this period. If the proportion of their births that are unwanted differs from the proportion among women who survive (as might be the case, for example, if mortality is disproportionately due to complications of induced abortion), the estimate of unwanted fertility is biased. Nevertheless, we introduce no correction for mortality of women between t_1 to t_2 because we have neither data on their relative fertility contribution nor a basis for assigning them a rate of unwanted fertility.

In any case, survival ratios throughout the reproductive ages are very high in most contemporary populations, and hence the implicit assumption that parity-specific fertility preferences do not differ by survival status cannot have any noticeable impact on the estimates of unwanted fertility.

*Eligibility***.** Not all women interviewed at t_2 were eligible for interview at t_1. In DHS surveys, the extended interviews with women of reproductive age have lower and upper age-bounds, typically 15 and 49. In a subset of DHS surveys, there is a further restriction to ever-married women. To cope with the age criterion, if the interval between t_1 and t_2 is d and the lower and upper age boundaries are m and n respectively, then ${}_1N^p$ should be calculated using women from age m through age (n-d), and ${}_2v^p$ and B^p should be calculated using women from age (m+d) through age n. In those countries where the sample is further restricted to ever-married women, ${}_1N^p$ can only be obtained from these women, and ${}_2v^p$ and B^p should be calculated using women who first married prior to t_1. As a consequence of this latter restriction, births to women who first married after t_1 do not contribute to the estimation of u^{p+1}, although their births are accounted for in the estimation of u (via the weights g^{p+1}). Most of these births will be first births, which are almost universally wanted in any case.

A3. Stability of preferences.

The proposed estimator assumes constancy of aggregate-level fertility preferences from t_1 to t_2. When the two dates are only a few years apart, this is a highly defensible assumption in most settings, judging from existing empirical evidence which shows that aggregate-level parity-specific fertility preferences hardly change over historical periods as brief as 2-3 years. Moreover, our estimator shares this assumption with Methods #2 and #3 above – stability of the ideal number of children in the case of Method #2 and stability in prospective preferences in the case of Method #3. Nevertheless, because this is clearly a fundamental assumption of the proposed new estimator, the implications of its violation are investigated more thoroughly in section VII of this paper.

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APPENDIX B

Implementation of the Aggregate Prospective Estimator: Some Technical Issues

Application of the aggregate prospective estimator using survey data (e.g. DHS) requires resolution of a number of technical issues. Three are singled out for discussion here.

B1. Missing Information for Prospective Preferences

Prospective preferences are not available for all women. Some women do not provide a usable response when asked, instead are recorded as "uncertain" or "not stated". Fortunately this fraction is very low in most surveys. For the most recent survey in the six countries, Table 1 shows the percentage of currently married women who did not provide an answer, by parity. The overall percentage is five percent or less in all six surveys. In contrast to missing data on ideal number of children, there is no strong patterning of the missing data on prospective preferences by parity. In the small fraction of cases with an "uncertain" or "not stated" response, we ascertain prospective preferences by comparing their stated ideal number of children with their number of living children at the interview.

Of far more consequence are women not even asked the prospective preferences item. In some surveys prospective preferences are only asked of women currently in union (Bangladesh and Egypt among the six countries analyzed here). In other surveys this question is asked of women irrespective of their union status, although in some of these surveys the question is not asked of never in-union women who have never had sex. Women not currently in union at time t_1 or at t_2 can contribute births during the reference period, hence a proxy for their prospective preferences is required (for use in the calculation of ${}_1N^p$ and ${}_2v^p$). As with women recorded as "uncertain" or "not stated", our solution is to determine prospective preferences by comparing the ideal number of children with the number of living children.

These solutions rely on the stated ideal number of children, an item commonly regarded as upwardly biased due to women's so-called rationalization of their actual experience (see critique above of Method #2). But note that this response error will not bias the measurement of prospective preferences to the extent that rationalization consists of simply matching the stated ideal with the number of living children.

B2. Women Currently Pregnant At Survey

Women pregnant at the survey present several problems. For these women, the prospective preference item explicitly refers to their reproductive career after the forthcoming birth: "After the child you are expecting, would you like to have another child or would you prefer not to have any more children?" These women are also asked about the wantedness of the current pregnancy: whether it is wanted at this time, sometime later, or not at all.

Consider first the treatment of women currently pregnant at the 1st survey in the two-survey variant of the aggregate prospective estimator. At issue is how to classify births eventuating from these pregnancies (which will be represented in the birth histories collected in the 2nd survey). The wantedness of these births could be ascertained from the declared wantedness of the pregnancies. But this would mean relying on what is effectively a retrospective wantedness item, with the attendant lower validity and reliability than the prospective preference item. To avoid this, we start the reference period not with the date of the 1st survey but rather some months later, allowing for the gestation of current pregnancies at the 1st survey and thereby confining the analysis to births to whom the prospective preference item applies. Our default is to set t_1 at seven months after the 1st survey, recognizing that very few women report pregnancies in the first few gestational months.

A rather slight problem remains for women pregnant at the 1st survey. Because a fraction of these pregnancies will terminate in a fetal loss, some women who state a desire to terminate childbearing after the birth of the current pregnancy will instead want another birth, i.e. they are misclassified in ${}_1N^p$. This leads to upward bias in the estimate of the percentage of births unwanted. We correct for this misclassification of women pregnant with their last wanted child (defined as pregnant women at the 1st survey who state a desire for no further births and whose number of children after the birth from the current pregnancy equals their ideal number of children). A fetal loss rate of 5% is assumed[12], and the fraction of women projected to suffer a fetal loss is shifted from the "want no more" to the "want more" category. In the event, this adjustment is of little consequence – without this downward adjustment, the two-survey estimates of Table 3 are only 0.2% - 0.4% higher. This trivial impact is not surprising because the fraction of women affected is small (pregnant women who are pregnant with their last wanted child virtually always constitute less than ten percent of women in cross-sectional samples).

Turning to women pregnant at time t_2, these women complicate the estimation of ${}_2v^p$ **--** the fraction of women who want no more children and have had no birth since t_1. Because the prospective

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[12] We derive this percentage by using the life table probabilities of fetal loss from five studies deemed of high quality presented in Woods (1994: 257). These probabilities can be applied to the distribution of women by gestational month to obtain a weighted average proportion of expected fetal loss among women pregnant at the survey. Using the three most recent DHS surveys in Bangladesh and Egypt, we obtain average fetal loss rates that range from 3.0% to 6.5%.

preference item refers to their preferences after the birth of the current pregnancy, some of these women who state a desire to stop childbearing in fact wanted another child up to the survey interview at t_2 and hence should be assigned to the subset of women wanting another child at the survey. This is a minor problem from the standpoint of the fraction of sample experience affected – like the misclassification of women pregnant at the 1st survey discussed in the previous paragraph, this predicament applies only to women pregnant at the survey with their last wanted child. We determine this status – pregnant with last wanted child – via a double requirement: women must indicate that the current pregnancy is wanted and their ideal number of children must exceed their current number of living children by one child. Although the fraction of women affected is very small, it is unfortunate that we must rely on the stated wantedness of the current pregnancy, as this item is probably biased towards the pregnancy being wanted. Note that such bias underestimates ${}_2v^p$, resulting in a slight upward bias in the estimated fraction of births unwanted (see equation 1).

B3. Child Deaths within the Reference Period

Of potentially more consequence because of the fraction of women who might be affected in high-mortality settings are child deaths during the interval from t_1 to t_2. The prospective aggregate estimator is driven by the difference between ${}_1N^p$ (the proportion of women who wanted no births at t_1) and ${}_2v^p$ (the proportion of women who had no births in the interval between t_1 and t_2 and who wanted no further children at t_2). This difference is taken as an estimate of the proportion of women who had unwanted births in the interval. However, some of the difference may be attributable to women who did not want another child at t_1, but subsequently changed to wanting another child because one (or more) of their children died. Note these would be child deaths during the interval of children born prior to t_1.

To correct for this upward bias in the estimator, the difference between ${}_1N^p$ and ${}_2v^p$ is adjusted downward by the fraction of women who suffered a child death that dropped them below their desired number of children. The latter is ascertained using the stated ideal number of children. In the event, this correction has a relative minor impact on the estimated fraction of births unwanted in the six countries – without this correction, the two-survey estimates in Table 3 are 0.6% - 1.4% higher (Kenya is the largest discrepancy).

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Table 1. Estimates of unwanted fertility

Percentage of births unwanted according to conventional DHS estimators and aggregate prospective estimator

Country and Survey Date	Retrospective direct	Ideal versus actual	Aggregate Prospective	Number of births [a]
Bangladesh (2004)	13.7	27.7	35.0	3652
Egypt (2003)	10.9	17.3	31.8	3298
Ghana (2003)	19.0	13.9	23.0	1889
Kenya (2003)	20.7	20.4	28.7	3187
Peru (2000)	30.6	32.4	46.5	6163
Philippines (2003)	22.9	25.9	39.0	3523

[a] Births occurring in the 30 months preceding the survey interview.

Table 2. Estimates of unwanted fertility and total fertility rates

Percentage of births unwanted according to conventional DHS estimators and aggregate prospective estimator, total fertility rate, and unwanted total fertility rate

Country and Survey Date	Percent Births Unwanted [a]			Total Fertility Rates [b]		
	Conventional (Ideal vs. Actual)	Aggregate Prospective		Total Fertility Rate (TFR)	Unwanted TFR	
		Order Based	Age Based		Ideal vs. Actual	Aggregate Prospective
Bangladesh (2004)	27.7	35.0	33.6	2.82	1.07	1.22
Egypt (2003)	17.3	31.8	34.8	3.16	0.66	1.27
Ghana (2003)	13.9	23.0	19.2	4.40	0.76	1.00
Kenya (2003)	20.4	28.7	25.2	4.82	1.28	1.39
Peru (2000)	32.4	46.5	40.3	2.76	1.00	1.21
Philippines (2003)	25.9	39.0	32.6	3.39	0.97	1.17

[a] Births occurring in the 30 months preceding the survey interview.

[b] Reference period is the 30 months preceding the survey interview.

Table 3. Sampling error in the estimation of unwanted fertility

Bootstrap[a] sampling error, normal-based confidence interval, and coefficient of variation for percentage of births unwanted, by estimator

Estimator	Estimate	Bootstrap standard error	95% confidence interval		Coefficient of variation (%)
			Lower	Upper	
Bangladesh (2004)					
Retrospective direct	13.7	0.76	12.2	15.2	5.5
Ideal vs. actual	27.6	0.96	25.8	29.5	3.5
Aggregate prospective	35.0	1.21	32.7	37.4	3.5
Egypt (2003)					
Retrospective direct	10.9	0.60	9.7	12.1	5.6
Ideal vs. actual	17.3	0.80	15.7	18.9	4.6
Aggregate prospective	31.8	1.16	29.6	34.1	3.6
Ghana (2003)					
Retrospective direct	19.0	1.01	17.0	21.0	5.3
Ideal vs. actual	13.9	1.01	11.9	15.9	7.3
Aggregate prospective	23.0	1.47	20.2	25.9	6.4
Kenya (2003)					
Retrospective direct	20.6	0.92	18.8	22.5	4.5
Ideal vs. actual	20.4	0.95	18.5	22.2	4.6
Aggregate prospective	28.7	1.38	26.0	31.4	4.8
Peru (2000)					
Retrospective direct	30.6	0.81	29.0	32.2	2.7
Ideal vs. actual	32.4	0.85	30.7	34.1	2.6
Aggregate prospective	46.5	1.12	44.3	48.7	2.4
Philippines (2003)					
Retrospective direct	22.9	0.76	21.4	24.4	3.3
Ideal vs. actual	25.9	0.93	24.1	27.8	3.6
Aggregate prospective	39.0	1.17	36.7	41.3	3.0

[a] The number of bootstrap resamples is 500. Original stratified cluster designs are preserved in resampling. For sample size, refer to Table 1.

[b] Coefficient of variation = (standard error) / mean. The mean of the bootstrap replicates is used, not the estimate from the original sample.

Table 4. Error in estimation of unwanted fertility due to trends in preferences

Percentage point adjustment to estimates of percent of births unwanted to account for trends in prospective fertility preferences

Country and Survey Date	**Percentage of Births Unwanted [a]**	**Percentage Point Error due to Trends [b]**
Bangladesh (2004)	35.0	0.3
Egypt (2003)	31.8	0.0
Ghana (2003)	23.0	0.3
Kenya (2003)	28.7	-0.4
Peru (2000)	46.5	0.1
Philippines (2003)	39.0	0.3

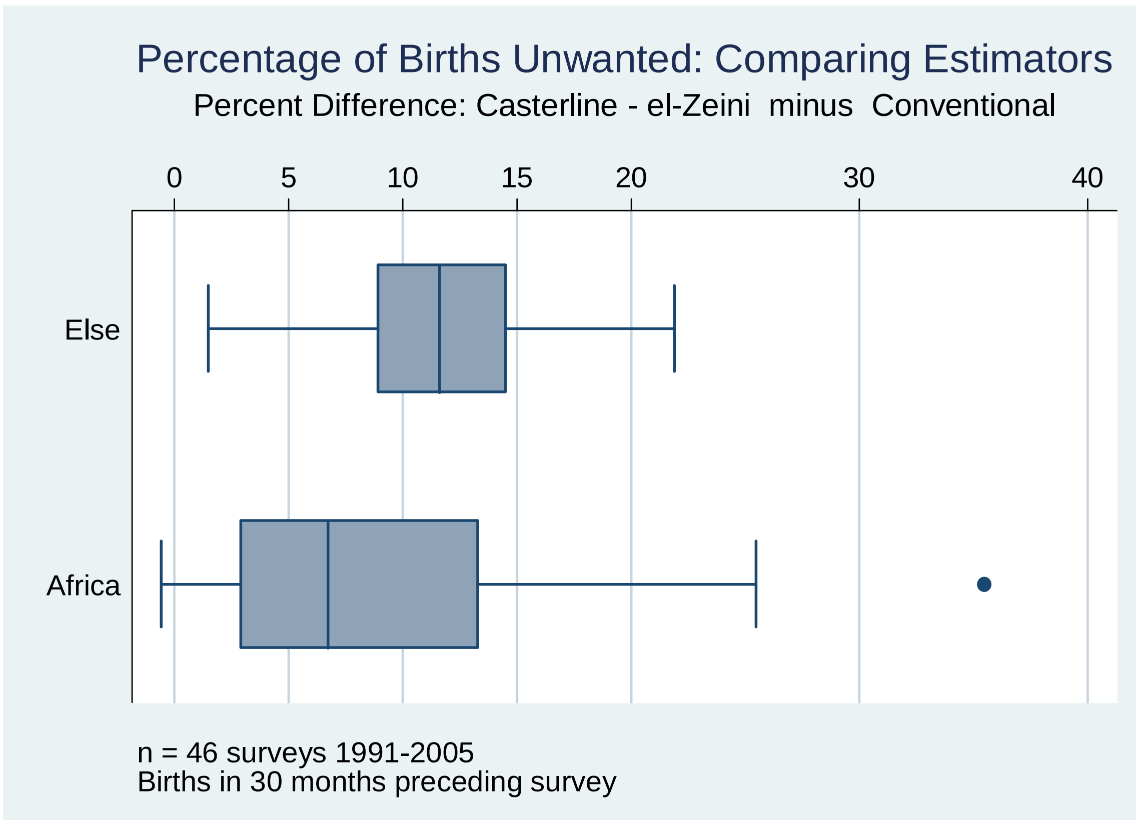
[a] Source: Table 1.

[b] For formula for calculation of error, see text.

[c] Reference period is 30 months preceding survey.

Figure 1. Comparing Estimators: Percentage Point Differences

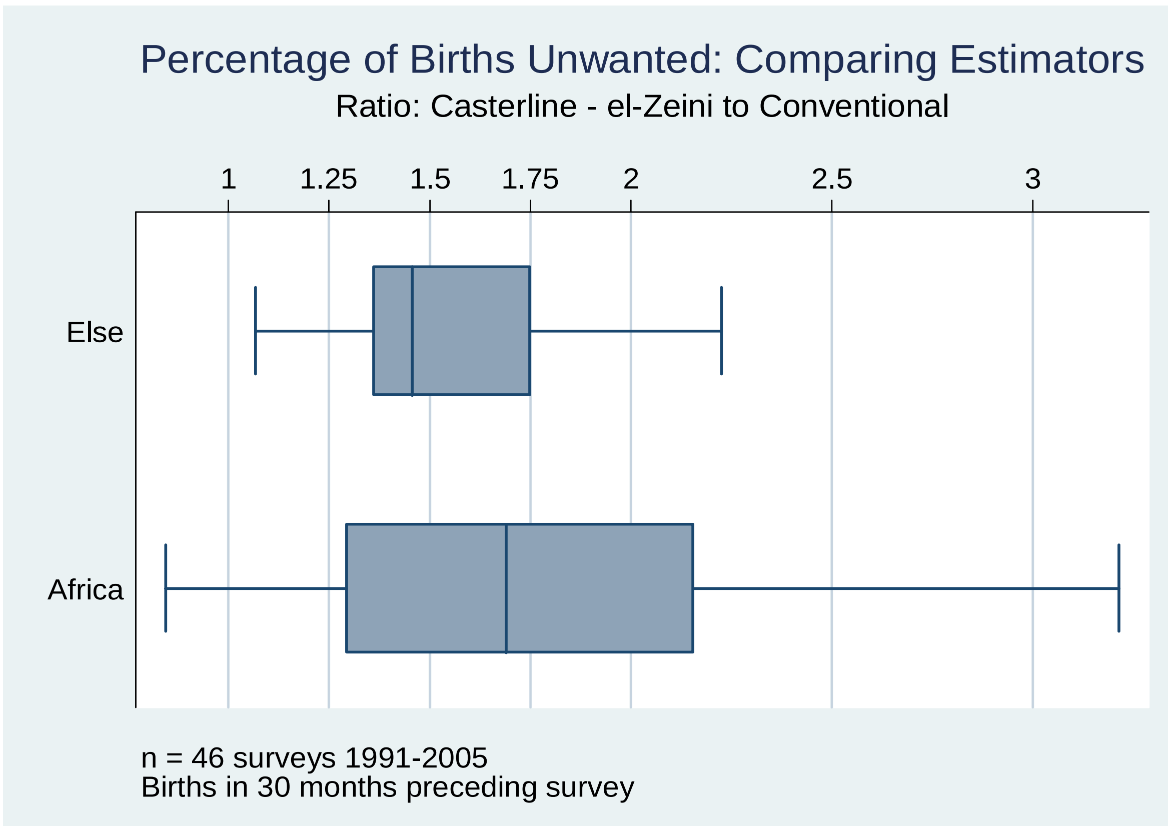
Percentage point difference in estimates of the percent of births unwanted, aggregate prospective estimator minus conventional [a]: 46 DHS surveys conducted from 1991 – 2005



[a] "Conventional" estimator is ideal family size vs. actual number of living children.

Figure 2. Comparing Estimators: Ratios

Ratio of estimates of the percent of births unwanted, aggregate prospective estimator and conventional [a]: 46 DHS surveys conducted from 1991 – 2005



[a] "Conventional" estimator is ideal family size vs. actual number of living children.