Response to reviewers: MS: 4349069741258385
Knowledge and use of emergency contraception in South Africa: A cross-sectional study (Myer et al.)

Reviewer #1: Caroline Moreau

Reviewer’s report:

General
This paper addresses an important and still scarcely documented research question on knowledge and use of emergency contraception in the critical Southern African context; contend with both high rates of unintended pregnancies and high rates of HIV transmission. In this context, it is hypothesized that EC used as a back-up contraceptive method where condom failure occurs, could have a significant role in reducing unintended pregnancies.

Using data from a cross sectional multi-center survey of 931 women, recruited at 30 public sector clinics in the province of Cape Province, South Africa, the authors found that only 30% of women had heard of EC. Awareness was significantly lower in the most rural area and among single women and less educated women.

The study design and statistical analysis are appropriate and well described. The results are clearly reported and the discussion thoroughly written.

No changes required.

A few suggestions would improve the understanding of some of the results.
1. When describing the women’s contraceptive use at last intercourse, it is unclear to me to which population these results refer to. Is it all women or women who are at risk of unintended pregnancy (excluding those who are pregnant or intended to get pregnant)? Analysis should be restricted to the later (women at risk). It would also be useful to know when the last sexual intercourse occurred (in the last month? 3 months? year?).
2. While overall knowledge of EC in the general population is interesting, it seems however more relevant to examine EC awareness among potential users. Therefore, I would suggest restricting the analysis of EC knowledge to non sterilised women. This remark includes tables 1, 2, 3
3. It would be helpful to add the p value for the comparison of rural vs urban for spontaneously citing EC.

In response to these issues, we have:

1. Clarified that these analyses are restricted to women at risk of unintended pregnancy; unfortunately data on when the last sexual contact occurred are not available.
2. Following the reviewer’s comment, we have added to our presentation of these results to focus on women who may use EC (ie, those who have not been sterilized). It is important to note that these data do not alter the results substantially (eg, 31% of unsterilized women have heard of EC, compared to 30% of all women), largely due to the relatively low number of sterilized women in this sample. In this light, to avoid unnecessary confusion, and to ensure these results are comparable to the findings from other studies (which do not typically stratify results based on sterilization status), we have kept the overall estimates as the principle results. But again, the results restricted to non-sterilized women have been added in the text.
3. We have added the p-value for the rural/urban comparison of spontaneously citing EC.
Discretionary Revisions (which the author can choose to ignore)
Few other specific remarks follow
Abstract:
Introduction: In the first sentence, authors make a direct link between availability of ECPs and the remaining high rates of teenage and unintended pregnancies. In fact, no studies up to date have shown that increased access to ECPs results in lower rates of unintended pregnancies or abortions. Unintended pregnancy rates are driven by multiple dimensions (including sexual activity, contraceptive coverage and effectiveness). ECPs are unlikely to have an impact alone. I would suggest breaking this first sentence into 2: Emergency contraception is available free of charge at all public sector clinics in South Africa since xxxx. In the same time, rates of teenage and unintended pregnancies are high."
Discussion:
It is unclear why EC would increase contraceptive coverage.
Line 4 of the discussion/ remove the word “it”: for it’s use, before the need of ECP arises.

We have made the requested changes to the abstract.

Introduction
Line 7: “has the potential to reduce the incidence of unintended pregnancy”; maybe add, “if used when the need arises”.
Paragraph 2: same remark as in the Introduction paragraph of the abstract.

We have made the requested changes to the introduction.

Also, consider changing to “75% of pregnancies in South Africa”
Paragraph 3: are ECPs available over or behind the counter?

Findings:
Page 6, first sentence: remove “meaning they were not protected from pregnancy or STI at last intercourse”. We have no information on the woman’s and her partner’s HIV or STI status. If both partners have no infection and have shared that information, they are not at risk
Page 6, line 7. It would be helpful to add the p value for the comparison of rural vs urban in spontaneously citing EC.
Page 7, line 7. “associated with the type…”
Page 9, line 14. “context of high levels”
Page 9, 3rd paragraph 1st sentence: seeking EC at the pharmacy rather than at public clinics may also reflect a selection “bias”; women who are aware and use ECPs are more educated (and possibly wealthier?)
Page 9, line 21. change “at weekends” by “on weekends”
Page 10, last line: remove the word “it”: for it’s use, before the need of ECP arises.

Table 1
Change to
“did not know if ECPs were available at public clinics
“thought ECPs were not available at public clinics

We have implemented the requested changes to the results section and table 1.
Reviewer #2: Douglas Ball

Reviewer's report:

General
Overview: A useful addition to the literature on awareness of hormonal emergency contraception (EC). However, it needs to be written for an international audience and placed in the context of previous data from the region. There are serious concerns about the adequacy of the statistical analysis used and its interpretation. These will need to be addressed for it to be suitable for publication.

Title
Minor essential
- Change to reflect that it is a study just in Western Cape province of South Africa.

Discretionary
- Cross-sectional study

Abstract
- The Introduction suggests "few data in South Africa but there is more than in many/most developing countries."
- Methods is too thin on details for reader to understand how sample selected.
- More results could be provided.
- Discussion should be more focused to the results of the study than general.

Introduction

We have implemented most of these changes per the reviewer’s recommendations. However please note that we have left the abstract essentially unchanged, since the alterations recommended by the reviewer to the methods and results would significantly increase the abstract length, and the requested details are already in the main text. We have amended the discussion section of the abstract per the reviewer’s comments.

Introduction

Major
- It needs to be made clear that EC is referring only to hormonal emergency contraception.

- Para1: "the only form of contraceptive" – This is inappropriate wording. The use of IUD as effective EC should also be acknowledged in the text.

- Need to provide greater background in terms of previous studies of EC use in SA and why this study was necessary.

- State objective more precisely e.g. to investigate factors associated with EC awareness, ... and level of use. Any hypothesis or just looking? Why urban and rural? Why WC? How do these fit into the objectives of the study?

We have implemented these changes, and have attempted to phrase the objective of the study more clearly: to examine levels of awareness and uptake of hormonal EC among women in this setting. We feel that this is a suitably clear objective statement.
We have addressed these concerns in the revised manuscript. Note that the clinics were selected to allow follow-up of a previous survey, but that survey was sampled according to urban/rural location and clinic size, as described. We have revised the manuscript to reduce this confusion, and include details on the definition of urban and rural locations.

- One wonders whether low load clinics may be different to high use clinics and whether it should have been stratified rather than weighted. A clearer objective would help to decide this.

- How many clinics were actually sampled – it is not clear if it is 30 total, or 30 urban and 30 rural. If the former, need an indication of how many were rural and why sampling not stratified by location if it was intended to compare urban vs. rural.

- More information about the development, testing and validity of the interview instrument and training. Qualifications of interviewers.

- What variables were included in the regression model? A better description is required here and/or when results are presented.

- MAJOR ISSUE: The dependent variable (awareness of EC) is binary and logistic regression is more appropriate than linear regression models. This needs reanalysis and statistical review as the results are not robust.

The total sample was drawn from 26 clinics, and we have clarified this in the text. We have included a better description of the modeling process and interviewer qualifications. We apologise for any confusion in the reviewer’s reading of the text, but logistic regression was used throughout for binary outcomes (in fact, linear regression is never mentioned in the manuscript, and we are unsure of how the reviewer arrived at this impression). We have clarified this in the text.

Minor essential
- Give a brief overview of what the Western Cape province is and its population and health services in the South African context for international readers.

- 2004/2005 represents a potentially long study period. It should be defined more precisely.

- What gender were the interviewers? If males were used that could influence the responses.

- How private was the setting in which women were approached and asked to participate?

Discretionary
- Method more complicated than necessary and yet in spite of this there is no sample size calculation. Having clear objectives would enable this to be understood better.
We have included more detail on the population of the Western Cape province; clarified the duration of the study, the gender of the interviewers, and the study setting. Note that a sample size calculation is not appropriate in this context, as there was not a single quantitative hypothesis being tested in this study (i.e., we were not attempting to detect a prevalence or measure of association of a specific magnitude).

Results/Findings

Major
· Table 3 and its data is not referred to in the text.
· Since so much is made of the rural-urban divide and differences, a table comparing the sociodemographic data of the women from the two regions would be useful.

Minor essential
· Give IQR for age to go with the median
· Explain grade 10 level of education for international readers.
· Was any distinction made between hormonal and non-hormonal EC by the women or the interviewers? This could be made clear here and/or in describing the method.
· Don’t concentrate on Table 2 since some characteristics are likely to be interrelated and multivariate analysis is being used to control for covariance, interaction and confounding. Report the regression findings since it is the whole point of doing it.
· p6 last line - remove hyphen in “Nine-percent”

Discretionary
· p5 line1 - “meaning they were not protected from pregnancy or STI at last intercourse” – repetitive and obvious.
· p7 para4 - Remove reference to the ‘totally sexually active sample’ – this sounds like it is a subsample of the main sample of sexually active women. Just ‘total sample’ is adequate description.

We have made the requested changes, including adding a reference to table 3, included the requested IQR, explained grade 10, clarified that we are focusing only on hormonal EC. We have also added a table (now table 1) showing demographic differences by rural/urban region. Note that we have kept our discussion of table 2, as we feel that these crude associations are useful to interpreting the data (and the multivariate results in particular).
We disagree with the reviewer, and feel that the Ellertson review in *Contraception* (as cited) presents fairly clear data that knowledge of EC is higher in Europe and North America than was found in our study. This has also been borne out by subsequent research. We have made a slight change to this passage, to clarify the geographic regions we are referring to.

We agree with the reviewer that the mention of the previous study in the discussion is confusing, and have reworked the discussion to be clearer. This study was not designed to
replicate the previous research, but the indirect comparison of the two projects does allow some valuable insights.

We have altered the discussion of pharmacies, changing this to private sector facilities more generally.

We have included a p-value for the comparison of urban vs rural facilities in the text, per the first reviewer’s comments.

We have noted the other settings that are of interest.

We have addressed each of these points in the text of the revised manuscript. Note that we feel the crude associations are noteworthy, in terms of understanding the correlates that are readily apparent in clinical practice. We prefer to still report and discuss these, as well as the multivariate (adjusted) results. In the methods, we have added a discussion of the approach to model-building, which clarifies that all crude predictors (at P<0.10) were examined in the multivariate model; as a result, the factors that were not associated can be easily inferred from the comparison of the crude table and the multivariate model.