Sex ratio effects on reproductive strategies in humans

Ryan Schacht, Monique Borgerhoff Mulder

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Note: Reports are unedited and appear as submitted by the referee. The review history appears in chronological order.

Review History
RSOS-140085.R0(Original submission)

Review form: Reviewer 1(François-Xavier Dechaume-Moncharmont)

Is the manuscript scientifically sound in its present form?
Yes

Are the interpretations and conclusions justified by the results?
No

Is the language acceptable?
Yes

Is it clear how to access all supporting data?
Data not yet accessible

Do you have any ethical concerns with this paper?
No

Have you any concerns about statistical analyses in this paper?
Yes

Recommendation?
Major revision is needed (please make suggestions in comments)
Comments to the Author(s)
This paper is a study about mating strategy in human population. It addresses an important question about the origin of sex roles in human. It is a rather neat and cleverly designed study which deserves publication in a high impact journal. The introduction and discussion section are based on an abundant, carefully chosen and suitable evolutionary literature (which is, sadly, not so common in many anthropological studies).

Major comments:

(1) My first comments are related to the statistical analysis. I totally agree with the authors about their choice of using AIC approaches to compare the ability of the two hypotheses to explain the observed mating efforts. This approach is perfectly legitimate in such a case. However, the authors did not use the full power of such analysis, while citing the reference book about it (Burnham and Anderson 2002). I would recommend them reading the following introductory paper for clear examples of analysis and result presentation: Symonds and Moussalli (2011). A brief guide to model selection, multimodel inference and model averaging in behavioural ecology using Akaikes’s information criterion. Behavioral Ecology and Sociobiology, 65(1):13-21

   More specifically, the authors clearly overstate some of their results when they conclude: “FOR WOMEN there is no effect of ASR on SOI” (lines 154-155) or “the behavior OF MEN varies in predictable ways with the ASR” (lines 170-171). Nothing in their analysis (as presented in the current version of the MS) allows concluding to a non–significant effect of the ASR on the women SOI. The fact that the interaction between ASR and sex appear to be statistically supported (Tab. 2), is not sufficient to conclude that the men behavior is affected by the ASR while the women behavior is not.

   In addition, I regret that the authors focus on only four models:
   Model 1: SOI ~ sex
   Model 2: SOI ~ sex*ASR
   Model 1+: SOI ~ sex + relationship.status
   Model 2+: SOI ~ sex*ASR + relationship.status

   Following Burnham and Anderson (2002) or Symonds and Moussalli (2011), I would have been very interested in interpreting the full set of biologically meaningful models (for example those including only additive terms,without interactions) and not only a small fraction of it. Several R packages such as MuMin allow quick and efficient comparisons of competing models.

(2) My second major comment is related to a possible confounding effect link to the age structure. It is possible that the reported difference in mating behavior was not linked to the adult sex-ratio (ASR) but to a difference in age structure between the communities. It is possible that older individuals (with previous reproductive experience) have different SOI. I thus recommend the authors to either include in their analysis (see point 1, above) an effect of age or, at least, to test for differences in age structure between communities.

(3) My last question concerns the stability across time of the ASR. I understand that men or women temporarily leave their village to seek job opportunity outside their community. As the ASR is the main hypothesis investigated to explain the mating behavior is this study, I would like to know how variable this ASR is across season. It is not sufficient to consider the ASR as an invariable parameter within a given community. As the survey was carried over a 16 month period, one should easily find some data on the variation of ASR and report it in Tab. 1, at least.

Minor comments:

Line 35: what does PI means?
Reference list formatting: there is too many capital letters in article titles.

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Review form: Reviewer 2 (Tamas Szekely)

Is the manuscript scientifically sound in its present form?  
Yes

Are the interpretations and conclusions justified by the results?  
Yes

Is the language acceptable?  
Yes

Is it clear how to access all supporting data?  
Not clear

Do you have any ethical concerns with this paper?  
No

Have you any concerns about statistical analyses in this paper?  
Yes

Recommendation?  
Accept with minor revision (please list in comments)

Comments to the Author(s)

This work joins a growing number of studies that investigate the influence of social environment on behaviour in humans and non-human animals. It reports that male reproductive strategies are related to adult sex ratios (ASRs). This is an important result suggesting that a somehow overlooked aspect of sexual selection, ie ASR, does modulate sexual strategies.

The work is carried out carefully, and the MS well written except in regards to the following issues.

1. Terminology. SOI is not really a user-friendly acronym. I wonder whether labelling this variable simply as "mating strategy" or something along this line would make the study more accessible for a wider audience.

Similarly, "low" and "high" sex ratios are rarely used outside the primate and social science literature, and I wonder whether it would make more sense adopting "male-biased" vs "female-biased" usage - as done in pretty much all walks of evolutionary biology. Humans are afterall just one, somehow weird, species.

2. Data collection and statistics. I found the method description and statistical justification not very helpful. Given that SOI is the major response, it needs to be explained how it was collected and possibly verified. What were the specific interview questions? how was the overall score calculated?
I'm a bit confused what was the unit of statistical analyses: individual males and females that were interviewed? Or subset of villages somehow selected from the ones that were interviewed?

I presume, mating strategies might relate to the age of males and females, the size of their household, and the number of their children. Were these potentially confounding effects included in the models?

What was the justification for using 4 models, and not, let say, 2 or 6?

Fig 2 is quite fuzzy in my version, and not clear how they were picked given that 4 statistical models were presented.


Tamas Szekely

Decision letter (RSOS-140402)

09-Sep-2014

Dear Dr Schacht,

Manuscript ID RSOS-140085 entitled "Sex-Differentiated Behavior in Humans: Sex Ratio Effects on Reproductive Strategies" which you submitted to Royal Society Open Science, has been reviewed. The comments from reviewers are included at the bottom of this letter.

In view of the criticisms of the reviewers, the manuscript has been rejected in its current form. However, a new manuscript may be submitted which takes into consideration these comments.

Please note that resubmitting your manuscript does not guarantee eventual acceptance, and that your resubmission will be subject to peer review before a decision is made.

You will be unable to make your revisions on the originally submitted version of your manuscript. Instead, revise your manuscript and upload the files via your author centre.

Once you have revised your manuscript, go to http://mc.manuscriptcentral.com/rsos and login to your Author Center. Click on "Manuscripts with Decisions," and then click on "Create a Resubmission" located next to the manuscript number. Then, follow the steps for resubmitting your manuscript.

Your resubmitted manuscript should be submitted within 6 months.

I look forward to a resubmission.

Sincerely,

Charlotte Wray
Senior Publishing Editor, Royal Society Open Science
openscience@royalsociety.org
Author's Response to Decision Letter

See Appendix A.

RSOS-140402.R1(Revision)

Review form: Reviewer 1(François-Xavier Dechaume-Moncharmont)

Is the manuscript scientifically sound in its present form?
Yes

Are the interpretations and conclusions justified by the results?
Yes

Is the language acceptable?
Yes

Is it clear how to access all supporting data?
The original data does not seem to be freely available at this stage.

Do you have any ethical concerns with this paper?
No

Have you any concerns about statistical analyses in this paper?
No

Recommendation?
Accept as is

Comments to the Author(s)
The authors have convincingly responded to all my previous comments. They have made considerable effort to modify the MS in order to cope with my concerns. I am now satisfied with the MS in its present form, and I still consider that this is an important study which deserves publication in a major journal.

My only last concern is related to the availability of the dataset. Unless I am mistaken, the original data does not seem to be freely available at this stage. I still consider giving the opportunity to colleagues to access original data will be rewarding for future studies (including meta-analysis), and I therefore recommend the authors to follow the journal recommendation about disclosure of supporting data.

FX Dechaume-Moncharmont

Decision letter (RSOS-140402.R1)

18-Nov-2014

Dear Dr Schacht

On behalf of the Editor, I am pleased to inform you that your Manuscript RSOS-140402 entitled "Sex Ratio Effects on Reproductive Strategies in Humans" has been accepted for publication in
Royal Society Open Science subject to minor revision in accordance with the referee suggestions. Please find the referees' comments at the end of this email.

The reviewers and Subject Editor have recommended publication, but also suggest some minor revisions to your manuscript. Therefore, I invite you to respond to the comments and revise your manuscript.

- Ethics statement
As discussed previously please provide details of IRB and local approval in this section (you should state that the UC Davis IRB, the University of Guyana and the Ministry of Amerindian Affairs, Guyana granted approval)

- Data accessibility
As discussed previously, it is a condition of publication that all supporting data are made publicly and permanently available either as supplementary information or preferably in a suitable permanent repository. The data accessibility section should state where the article’s supporting data can be accessed. This section should also include details, where possible of where to access other relevant research materials such as statistical tools, protocols, software etc can be accessed. If the data has been deposited in an external repository this section should list the database, accession number and link to the DOI for all data from the article that has been made publicly available. Data sets that have been deposited in an external repository and have a DOI should also be appropriately cited in the manuscript and included in the reference list.

- Competing interests
Please declare any financial or non-financial competing interests, or state that you have no competing interests.

- Authors’ contributions
All submissions, other than those with a single author, must include an Authors’ Contributions section which individually lists the specific contribution of each author. The list of Authors should meet all of the following criteria; 1) substantial contributions to conception and design, or acquisition of data, or analysis and interpretation of data; 2) drafting the article or revising it critically for important intellectual content; and 3) final approval of the version to be published.

All contributors who do not meet all of these criteria should be included in the acknowledgements.

We suggest the following format:
AB carried out the molecular lab work, participated in data analysis, carried out sequence alignments, participated in the design of the study and drafted the manuscript; CD carried out the statistical analyses; EF collected field data; GH conceived of the study, designed the study, coordinated the study and helped draft the manuscript. All authors gave final approval for publication.

- Acknowledgements
Please acknowledge anyone who contributed to the study but did not meet the authorship criteria.

- Funding statement
Please list the source of funding for each author.

Because the schedule for publication is very tight, it is a condition of publication that you submit the revised version of your manuscript within 7 days (i.e. by the 27-Nov-2014). If you do not think you will be able to meet this date please let me know immediately.

To revise your manuscript, log into https://mc.manuscriptcentral.com/rsos and enter your Author Centre, where you will find your manuscript title listed under "Manuscripts with
Decisions”. Under “Actions,” click on “Create a Revision.” You will be unable to make your revisions on the originally submitted version of the manuscript. Instead, revise your manuscript and upload a new version through your Author Centre.

When submitting your revised manuscript, you will be able to respond to the comments made by the referees and upload a file "Response to Referees” in "Section 6 - File Upload”. You can use this to document any changes you make to the original manuscript. In order to expedite the processing of the revised manuscript, please be as specific as possible in your response to the referees.

When uploading your revised files please make sure that you have:

1) A text file of the manuscript (tex, txt, rtf, docx or doc), references, tables (including captions) and figure captions. Do not upload a PDF as your "Main Document”.
2) A separate electronic file of each figure (EPS or print-quality PDF preferred (either format should be produced directly from original creation package), or original software format
3) Included a 100 word media summary of your paper when requested at submission. Please ensure you have entered correct contact details (email, institution and telephone) in your user account.
4) Included the raw data to support the claims made in your paper. You can either include your data as electronic supplementary material or upload to a repository and include the relevant doi within your manuscript
5) Included your supplementary files in a format you are happy with (no line numbers, vancouver referencing, track changes removed etc) as these files will NOT be edited in production

Once again, thank you for submitting your manuscript to Royal Society Open Science and I look forward to receiving your revision. If you have any questions at all, please do not hesitate to get in touch.

Best wishes
Emilie Aime
Senior Publishing Editor
openscience@royalsociety.org
Dear Charlotte Wray,

We are very thankful for the careful reading given our paper, and your evaluation of its potential for *Royal Society Open Science*. In our response we place the reviewers’ and the Subject Editor’s comments in blue, and insert our responses in black.

**Subject Editor comments:**

The first reviewer comments partly on statistical design and partly on over-interpretation of results.

I agree with the first reviewer that the introduction is an excellent and rare review of the field. 1) However the authors use the term "sexual selection" throughout when they mean "mate choice." Sexual selection requires dependence on sexually dimorphic secondary features, as Darwin (who invented the concept) pointed out. 2) Their caricature of his ‘ardent male, coy female’ hypothesis substantially underappreciates the complexity of his thinking, and they may wish to read *The Descent of Man* for a more nuanced view.

1) ‘Sexual selection’ appears 7 times in the first 3 pages of the manuscript. Most usages are followed by ‘theory’ which seems appropriate, given that mate preferences constitute an important component of sexual selection and that the theory tested in this paper derives directly from new advances in sexual selection theory (e.g., Kokko, et al. 2006). However in one place we use the term in a way that may be of concern to the reviewer:


We appreciate that sexual selection only occurs when fitness variance is associated with sexually dimorphic secondary traits. This has repeatedly been stressed from, for example, Grafen’s cautioning chapter in Clutton Brock’s reproductive success book (Grafen 1988) to, for example, the recent debates about the value of the opportunity for selection (e.g., Klug, et al. 2010; Krakauer, et al. 2011). But we do not see how our sentence quoted above violates the claim the “sexual selection on men and women can be highly variable”. Furthermore we believe that adding a series of clauses, e.g. “sexual selection can be quite variable as a result of both differences in fitness variability and the covariation with sexually dimorphic traits”, while accurate, would be a distraction from our more simple point. Accordingly we make no change to the MS. However if we have misunderstood the reviewer’s meaning we are happy to consider an appropriate edit.

2) We agree that Darwin’s view was highly nuanced. He spent considerable time collecting and describing diversity in morphology and behavior across taxa. We want to emphasize, however, that for many evolutionary social scientists and evolutionary psychologists the key take home message from the “Descent of Man” was that males benefit from eagerness and females from exercising restraint in reproduction. A portion of a representative statement is quoted in the text of our MS, here it is in full:
“From an evolutionary perspective, sex differences in sociosexuality should be consistently observed across human cultures, in part because of fundamental differences in the evolved reproductive strategies of men and women.” p. 248 (Schmitt 2005)

So while Darwin’s views were indeed more nuanced, what was subsequently embraced by researchers was the rule of thumb that males are generally ardent and females coy. The effect this has had on the field is enormous (Brown, et al. 2009; Dewsbury 2005; Hrdy 1986) and is what we are challenging in this paper. To avert the possible misunderstanding that we are claiming that Darwin’s arguments were simplistic (which we suspect is the reviewer’s concern, we amend the first sentence in the abstract:

“Characterizations of coy females and ardent males are rooted in models of sexual selection that are increasingly outdated.” Lines 10-11

We have also modified our text in the second paragraph accordingly: “While Darwin was careful to nuance this viewpoint…” Lines 33-34

Lastly, to avoid misunderstanding, we revise our definitely misleading original statement, our final mention of Darwin, from:

“In this respect, contemporary evolutionary anthropology is advanced well beyond the view expressed by Darwin and interpretations based on Bateman.”

To:

“In this respect, contemporary evolutionary anthropology is advanced well beyond overly simplistic interpretations of the work of Darwin and Bateman (see Dewsbury 2005).” (Lines 190-191)

In short, we did not mean to imply that we were challenging Darwin, but instead the oversimplified views that stem from his work and result in sexual stereotypes.

Apart from this it is difficult for them to draw the conclusions they do, in my view, because in only one of their populations were females more numerous than males, and it was a much larger one than the others. If more such populations showed that females in greater numbers do the same things than when males are in greater numbers, then there is no sex difference in these behaviors. I am not convinced that their sample is adequate to present more than a one-sided approach.

In this paper we follow standard scientific procedure by examining how behavioral traits are exhibited in populations with varying sex ratios (e.g., Liker, et al. 2013; Liker, et al. 2014; Schacht, et al. 2014), providing what reviewer 1 calls a “neat and cleverly-designed” study of the impact of sex ratios. If we observe variation in sociosexuality scores in response to an incremental change in ASR in the predicted direction we have support, albeit not conclusive evidence insofar as the data (like most behavioral ecological studies) are correlational, for the specific prediction derived from the theoretical model. The reviewer is correct, that a larger sample of populations (>8) would strengthen statistical inference, but the sampling strategy and
research design themselves are neither flawed nor unconventional. We therefore see no need to revise the MS, with the exception of adding a clause to the effect that 8 populations is a relatively small sample size.

“While a larger sample of populations would of course add to the strength of our inference…”
Lines 178-179

Reviewer: 1
This paper is a study about mating strategy in human population. It addresses an important question about the origin of sex roles in human. It is a rather neat and cleverly designed study which deserves publication in a high impact journal. The introduction and discussion section are based on an abundant, carefully chosen and suitable evolutionary literature (which is, sadly, not so common in many anthropological studies).

Major comments:

(1) My first comments are related to the statistical analysis. I totally agree with the authors about their choice of using AIC approaches to compare the ability of the two hypotheses to explain the observed mating efforts. This approach is perfectly legitimate in such a case. However, the authors did not use the full power of such analysis, while citing the reference book about it (Burnham and Anderson 2002). I would recommend them reading the following introductory paper for clear examples of analysis and result presentation: Symonds and Moussalli (2011). A brief guide to model selection, multimodel inference and model averaging in behavioral ecology using Akaike’s information criterion. Behavioral Ecology and Sociobiology, 65(1):13-21

More specifically, the authors clearly overstate some of their results when they conclude: “FOR WOMEN there is no effect of ASR on SOI” (lines 154-155) or “the behavior OF MEN varies in predictable ways with the ASR” (lines 170-171). Nothing in their analysis (as presented in the current version of the MS) allows concluding to a non-significant effect of the ASR on the women SOI. The fact that the interaction between ASR and sex appear to be statistically supported (Tab. 2), is not sufficient to conclude that the men behavior is affected by the ASR while the women behavior is not.

We completely agree! Indeed we are shocked we made this mistake! Nuestra culpa! Here we are asserting the null (i.e., that there is no effect). The statistical modeling does not allow us to do this. We will rephrase and say only what is appropriated (i.e., that we do not detect an effect)

Line 161 changed from “there is no effect” to “we do not detect an effect”

In addition, I regret that the authors focus on only four models:

Model 1: SOI ~ sex
Model 2: SOI ~ sex*ASR
Model 1+ : SOI ~ sex + relationship.status
Model 2+ : SOI ~ sex*ASR + relationship.status

Following Burnham and Anderson (2002) or Symonds and Moussalli (2011), I would have been very interested in interpreting the full set of biologically meaningful models (for example those
including only additive terms, without interactions) and not only a small fraction of it. Several R packages such as MuMin allow quick and efficient comparisons of competing models.

We agree with the reviewer that multiple approaches may be appropriate given a particular data set and we appreciate that in some cases model comparison can be used to simply sort among a larger array of potentially important variables. Our approach here however is somewhat different, given the intensive theoretical debates within the sexual selection literature, particularly with respect to parental investment and adult sex ratios (Kokko and Jennions 2008; McNamara, et al. 2000; Wade and Shuster 2002). Accordingly, in this context, we believe that the competing models should be drawn from theory. Furthermore we know of no theoretical model (nor see any reference to such a model in the reviewer’s proposal) in which these terms would be additive. Finally, to be somewhat of a curmudgeon, we would like to point out that we are also strictly following the advice of Burnham and Anderson (2002):

“We cannot overstate the importance of the scientific issues, the careful formulation of multiple working hypotheses, and the building of a small set of models to clearly and uniquely represent these hypotheses.” p.47

Finally, in our reading of Symonds and Mousali (Symonds and Moussalli 2011) we find that they are arguing that there are hazards associated with considering a lot of models, insofar as the interpretation of Δi can become problematic because models strongly competing with the best model (i.e. Δi<2) may differ very little structurally. As far as we understand it then, Symonds and Mousali are arguing for relatively simple models: indeed they warn investigators to eschew situations in which they are uncertain about whether variables are informative or not, offering the advice that every term in the candidate set included should be justified.

Accordingly we retain our focus on keeping the model set small and based on theory. However, we do revise the text to acknowledge that other approaches could be appropriate given the researcher’s interest:

“We while in some cases it is appropriate to explore a large set of models, following the advice of Burnham and Anderson (2002), we keep our model set small and based on theory.” Lines 333-335

(2) My second major comment is related to a possible confounding effect link to the age structure. It is possible that the reported difference in mating behavior was not linked to the adult sex-ratio (ASR) but to a difference in age structure between the communities. It is possible that older individuals (with previous reproductive experience) have different SOI. I thus recommend the authors to either include in their analysis (see point 1, above) an effect of age or, at least, to test for differences in age structure between communities.

We thank the reviewer for pointing out this reasonable confound. We have looked at age structure between communities and find little effect of including this in the model. For the MS we include age as fixed effect in both models (model 1++, 2++) and include summaries in Table 3. We find that adding age has little effect on the goodness of fit for either model. Accordingly we have revised the ms as follows:
“To counter the possibility that the ASR effect is confounded by relationship status or age, we added each (separately) as a main effect to both models (Model 1+ and Model 2+ include the term relationship status and Model 1++ and Model 2++ include age). As might be expected, single individuals have higher sociosexuality scores than those that are partnered; nevertheless the association of sociosexuality with community ASR and sex remains, and the relative evidence for Model 2+ is much stronger than for Model 1+ (lowered by an AIC difference of 13; Table 3). Adding age to the models, however, has little effect on the goodness of fit for either model (Table 3).”

(3) My last question concerns the stability across time of the ASR. I understand that men or women temporarily leave their village to seek job opportunity outside their community. As the ASR is the main hypothesis investigated to explain the mating behavior is this study, I would like to know how variable this ASR is across season. It is not sufficient to consider the ASR as an invariable parameter within a given community. As the survey was carried over a 16 month period, one should easily find some data on the variation of ASR and report it in Tab. 1, at least.

We believe the reviewer is concerned about inter and intra-annual variability in ASR. Regarding interannual variation, while we have no decisive data on this (we only censused each community once), we feel quite confident in the assumption that ASR reflects subsistence and employment conditions that have remained quite stable over the last 20 years, insofar as these are structured by ecological and geographic factors (mountains, forests, proximity to towns, etc). Regarding intra-annual variability, some employment opportunities are seasonal, but these are quite limited. So, yes there could be some fluctuations in ASR within and between years, but we do not have the longitudinal data to explore it – we chose instead to collect cross-sectional data in order to increase our sample of communities. However, it is possible that the reviewer is concerned about a time lag in our measure of ASR and our SOI survey. We made sure to avoid such a problem. Across the 16 month field period, we first conducted a census of a village immediately followed by administration of the questionnaire. In other words, we did not first spend 8 months on census work then 8 months on interviews. This approach was taken to ensure that we had the most accurate and current sex ratio measure when asking individuals about sociosexuality. Accordingly we revise the text:

“We used ethnographic and demographic methods to conduct a full census of the first village in order to determine the ASR (Table 1). We then administered the SOI to a randomly sampled minimum of 30 individuals (15 men and 15 women), aged between 18 and 45 years, from the village (see Table 1). This process was then continued across the subsequent 7 villages, for a total of 8.”

Additionally we added the following text to address potential concerns about the inter and intra annual variability in sex ratios:

“To test these models we capitalize on natural variation in ASR within a single ethnic group, the Makushi of Guyana living in the Rupununi Savannah (Fig. 1), where the relative numbers of
men and women in a village (Table 1) reflect subsistence and employment conditions that are quite stable over time.” LINES 107-110

Minor comments:

Line 35: what does PI means?

It is an abbreviation for the term it directly follows - parental investment.

Reference list formatting: there is too many capital letters in article titles.

We have formatted the references as specified by the journal on their web page.

Dr FX Dechaume-Moncharmont
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Reviewer: 2

This work joins a growing number of studies that investigate the influence of social environment on behaviour in humans and non-human animals. It reports that male reproductive strategies are related to adult sex ratios (ASRs). This is an important result suggesting that a somehow overlooked aspect of sexual selection, ie ASR, does modulate sexual strategies.

The work is carried out carefully, and the MS well written except in regards to the following issues.

1. Terminology. SOI is not really a user-friendly acronym. I wonder whether labeling this variable simply as "mating strategy" or something along this line would make the study more accessible for a wider audience.

We are very sympathetic to this comment, knowing that much social science and psychology is dense with acronyms, impeding accessibility. SOI however is a very widely-used scale (a Google scholar search returns just under 2,000 results) and we are rather nervous about replacing it with our own term which would probably be something like “reported tendencies towards promiscuity” – which is clumsy. Such a term may be more accessible to a biologist, but it is also perhaps a little disrespectful of our subjects. Promiscuity is frowned on among some social sets, whereas in other places or social circles there is no prejudice implied – accordingly it is dangerous to use such volatile terms. To try to avoid inaccessibility, however, we use the term ‘sociosexuality’ (which we now define early in the text; lines 121-122) in the place of most mentions of SOI, and SOI primarily when talking about the scale/index itself. We take care
never to use the acronym in the Discussion, so as to increase accessibility for a reader jumping to the significance of our findings.

Similarly, "low" and "high" sex ratios are rarely used outside the primate and social science literature, and I wonder whether it would make more sense adopting "male-biased" vs "female-biased" usage - as done in pretty much all walks of evolutionary biology. Humans are afterall just one, somehow weird, species.

Yes, we agree! We will throughout update low and high to male- and female-biased, for example line 91: “Polygyny is associated with low-female-biased sex ratios”

2. Data collection and statistics. I found the method description and statistical justification not very helpful.

We were somewhat confused by the reviewer’s observation that the” methods and statistical justification were not very helpful”. Without specific points, we cannot see where/how to edit the text which currently covers, for the methods, the key point that the census was conducted prior to the SOI study, the sample sizes, the age range of subjects, the number of villages studied, the potential biases and our attempts to counters such biases; and for the statistical approach, our modeling assumption (Gaussian), our use of log transformed scores on both axes, our hierarchical modeling structure, and our use of AIC. Perhaps the problem lies in the fact that some of this information is repeated, or in the case of this journal’s format, presaged in the Results section. We strongly adhere to the philosophy that when Methods come at the end of a paper it is best to provide some methodological guidance in the results section, to help the reader who simply skims the results in evaluating a study. However, if our reviewer has more specific concerns about why our section is not helpful, we are happy to work further on it.

Given that SOI is the major response, it needs to be explained how it was collected and possibly verified. What were the specific interview questions? how was the overall score calculated?

We now include the SOI questionnaire as supplemental material, with a slightly expanded summary of its strengths and weaknesses:

“we used the Sociosexual Orientation Inventory (SOI; see the electronic supplemental materials for additional information, the list of questions and score calculation).” LINES 307-308

I'm a bit confused what was the unit of statistical analyses: individual males and females that were interviewed? Or subset of villages somehow selected from the ones that were interviewed?

Our unit of analysis is person nested in village. We amend the first sentence of the results section to make this clear:
“Our sample is composed of 300 individuals from 8 rural communities” LINE 133

I presume, mating strategies might relate to the age of males and females, the size of their household, and the number of their children. Were these potentially confounding effects included in the models?

Indeed there is a huge literature on how human mating strategies are affected, in women, for example, by menstrual cycle (Alvergne and Lummaa 2010) or education (Goodman, et al. 2012), and in men, for example, by wealth (Borgerhoff Mulder 1990) or number of kin (Hewlett 1988), and in both genders by age and parity (Winking, et al. 2007). In designing a study to test the influence of ASR we emphasize the merit in conducting an analysis WITHIN a single ethnic group, and indeed within a geographical zone where there is relatively little socioeconomic heterogeneity, to try to rule out confounding economic and cultural factors (see lines 225-236 in current discussion). We appreciate age may have an effect, hence the control in models 2++. More generally our response is as above; our study was designed to test models drawn from two competing theories rather than to identify the full set of variables affecting SOI in this population.

What was the justification for using 4 models, and not, let say, 2 or 6?

Please see our response to reviewer 1 and the quote from Burnham and Anderson. We do not see approaches that look to more models as wrong, however here we focus on a small set of theoretically informed models because of the guiding theoretical question of our research.

Fig 2 is quite fuzzy in my version, and not clear how they were picked given that 4 statistical models were presented.

We have provided a sharper version of fig 2, and we have clarified in the figure legend the precise model used to generate the predicted points.


We were not aware of the recent J. Evol. Biol. paper, which is an absolutely terrific review and would have helped us enormously (it looks like it was published after we had submitted this though). We have now cited this to make the following point:

“Fourth, it is important to recognize that responses to the ASR may occur over evolutionary or ecological time scales (reviewed in Székely, et al. 2014).” Lines 243-244
We are also thankful to be pointed to the argument made in Szekely et al 2000 regarding the importance of population-level factors affecting mating and parenting decisions, that we now cite:

Reproductive strategies are clearly not an invariant, species-specific characteristic, but rather facultative responses to individual- and population-level social and ecological circumstances (e.g., Nettle 2009; Owens and Thompson 1994; Szekely, et al. 2000)… LINE 40-42

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Wade, M. J., and S. M. Shuster

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