Title: Attitudes and Behavior related to Performance-Enhancing Substance Use among Elite Saudi Football Players

Authors:
Mohammed Al Ghobain (alanezi@hotmail.com)

Version: 2 Date: 22 Jul 2019

Author’s response to reviews:

Technical Comments:

Editor Comments:
Please reduce text overlap in the methods, ( done )
Title page + abstract to be included in main manuscript file, ( done )
Indicate whether consent was written or verbal ( written ,, added in the method section )

BMC Sports Science, Medicine and Rehabilitation operates a policy of open peer review, which means that you will be able to see the names of the reviewers who provided the reports via the online peer review system. We encourage you to also view the reports there, via the action links on the left-hand side of the page, to see the names of the reviewers.

Reviewer reports:
Andrzej Pokrywka, Ph.D. (Reviewer 3): 1) In the parts discussion and results, there is the phrase "our study". In the manuscript I did not find any information about other co-authors or co-workers than primary author. It is a single author study. The phrase ( our study ) changed to ( this study )

2) Table 2 - In reply to reviewers Author wrote: "The test results for column 3 are the same as in column 2 ( from 1 to 7 ), so I did not repeat it in column 3." I have been misunderstood.

In Line "Doping susceptibility" - In column 3 the response scale (range) is given only (from 1 to 7). There are no answers from the respondents, just like in the following lines. (e.g. 5 participants answered 7 = 1.2%; etc.)
Added to the column : Doping susceptibility = 17.1%
74 participants answered yes to items 4 to 7

Antonio Ignacio Cuesta-Vargas, PhD (Reviewer 4): Please to include in the abstract the magnitude
value of correlations and not only th significance statistic ,, the estimates were added to the abstract

Katharina Diehl (Reviewer 5): Dear author,
Dear editors,
I would like to thank you for the opportunity to review the above-named manuscript. My review is based on the revised manuscript and two letters addressing a previous version of this manuscript. The manuscript is very interesting and focuses on results from a population, which is understudied up to now. However, I have some major comments.

* As previous reviewers also pointed out, there is still missing a clear definition of PES and its delineation towards ,,doping''.
This paragraph added to the beginning of the introduction as definition of PES ( Two of the following three criteria must be met for a substance to consider as PES; (1) The substance increases or has the potential to increase performance; (2) the substance represents an actual or potential health risk to the athlete; and (3) the substance violates the spirit of sport

* What remains unclear after reading the abstract and the methods section is, whether the participants had to be Saudi Arabian Citizens or whether they had to play for a Saudi Arabian team/ sports club. The study was limited to Saudi nationality players
We used the statement : elite Saudi football player throughout the manuscript including the title
We delete the statement : elite football players in Saudi Arabia

* In addition, a clearer definition of „elite football player" is needed.
Player who has a contract as professional player at national or international level ( not amateur player )
The definition was added to the beginning of method section

* The random sample selection technique remains unclear. If 408 elite athletes out of 16,779 elite football players participated, the response rate cannot be 68%.
We selected participants by random selection technique till we reached our target sample size ( we selected 600 participants to reach 408 participants which make the response rate is 68 % )

* In the methods section the author uses the term „banned PES". Again a clear definition is needed.
The word banned deleted throughout

* In the results section it is unclear whether a three or a four factor solution was chosen. Please report also the factor loadings of the individual items.

* I agree with former reviewer 1 that the discussion mainly consists of „results". A real scientific discussion is still missing.
Discussion revised ,, some extra comments added

From my point of view, the paper needs more additional work. However, it is an important study in an understudied population. Therefore, I would like to encourage the author to revise the manuscript.