Reviewer's report

Title: Personality patterns and smoking status: The role of gender and nicotine dependence in the results of a treatment for smoking cessation at 12-month follow-up

Version: 1 Date: 11 February 2013

Reviewer: Olaya O García-Rodríguez

Reviewer's report:

- Minor Essential Revisions - The author can be trusted to make these. For example, missing labels on figures, the wrong use of a term, spelling mistakes.
- In the abstract, the sentence “288 male and female smokers” is redundant. It would be better just to say “288 smokers”. As the study analyze gender differences, it is clear that the sample includes males and females.
- The Results section in the abstract may be improved. The first sentence is too long and it should be divided into two or three sentences that state each finding in this study.
- The authors state that the objective of the present study was to analyze the relation between personality traits and smoking status at the end of the treatment and at 12-months follow up. Surprisingly, the first result in the abstract does not refer either to the end of treatment nor the 12-months follow up but to differences in the intake. Differences in the intake as a function of gender do not appear to be an essential result for the abstract, taken into account the objectives of the study.
- The organization of the Background section could be improved. Last word in the first paragraph, should be relapse instead of smoking? If so, why those studies about relapse predictors are in the first paragraph and not in the second paragraph that is about relapse? Similarly, third paragraph present the idea of identifying personality patterns among smokers and non-smokers again (but from another model). It appears that the authors wanted to organize the background by theoretical models but all the paragraphs are presented depending of what part of the smoking process is studied. The definition of the Millon model would be better if presented before the empirical studies based on it. It is a very good literature review but it can be better arranged.
- In the Measures section, it is not correct to say that one of the variables registered with the Smoking Habit Questionnaire is the pre-treatment carbon monoxide level. I suppose this variable is registered with a cooximeter.
- Procedure section: despite participants were followed up at four different moments: 1, 3, 6 and 12 months after treatment, the authors only presented data for 12-months follow-up. Therefore, 1, 3 and 6 months follow-up should not be mentioned and last sentence in this section should start with “In the follow-up”. In the same way, some variables like marital status, educational level, age at onset
of regular smoking or pre-treatment carbon monoxide level are mentioned but do not appear in the results section so they are not relevant.

- Results section. My overall impression is that the results given in this study are much broader than the objectives and the background in which this study is based. The first part of the results (after presenting baseline characteristics of the smokers and quitting rates) analyze the relation between nicotine dependence, gender and age and smoking status (abstinent/smoker) at the end of the treatment and at 12 months follow-up. These may be interesting results but I don’t think that must be part of the results. In fact, the authors don’t mention anything about nicotine dependence, age or gender and their relation with smoking cessation in the background section so it is not clear why these results are given. What this paper adds to smoking research is the results about personality patterns and their relation with gender and nicotine dependence and, more important, with treatment outcomes, so the results section should be focused on that. The same comment is applicable to the discussion section.

- Results: Relationship between personality patterns and nicotine dependence. Despite some correlations between FTND and personality patterns were statistically significant, these correlation values are quite low and that is consistent with the low percentage of variance explained by the regression model. This should be acknowledge and discussed.

- In the discussion section, authors explained the women finding about squizoid pattern and less probability of quitting because higher levels of nicotine dependence but this variable was already controlled in the analyses so it does not appear to be a good explanation.

- Discretionary Revisions - These are recommendations for improvement which the author can choose to ignore. For example clarifications, data that would be useful but not essential.

- The title of the manuscript has too much information and it is a bit hard to read. Maybe the reference to nicotine dependence is not essential in the title. It is confusing to see in the title “the role of gender and nicotine dependence..” and then read that nicotine dependence will be use as independent and dependent variable. Maybe “Gender differences in personality patterns and smoking status after a smoking cessation treatment” could be an alternative title.

- “Psychological treatment” is too broad-spectrum. It would be better to say a “cognitive-behavioral treatment”.

- In the participants section, the inclusion criteria that refers to having complete pretreatment assessment does not appear to be no necessary.

- Data analyses: Stepwise multiple regression has a number of problems and there is some controversy concerning its use (Tabachnick & Fidell, 2007). For example, for stepwise regression, there should be a ratio of 40 cases for each independent variable and multicollinearity is a potential problem taking into account the kind of independent variables of this study (personality patterns). Authors should state if assumptions of multiple regression was or not accomplished.
- Several times, the authors use the phrase “giving up smoking”. Despite this is grammatically right, it is not usual to read that in the literature. “Stop/quit smoking” or “smoking cessation” would be a better choice.

- References: If the original version of a non-English book or manuscript is referenced, give the original title and, in brackets, the English translation

In the limitations section, authors acknowledge that clinical population of smokers differs from general population but any reference is given to support this idea. The generalizability of clinical samples is a relevant issue that has been widely discussed in recent years and that worth a deeper discussion and possible implications. Some useful references are below:


Le Strat Y., Rehm J., Le Foll B. How generalisable to community samples are clinical trial results for treatment of nicotine dependence: a comparison of common eligibility criteria with respondents of a large representative general population survey. Tob Control 2011; 20: 338-43

**Level of interest:** An article of outstanding merit and interest in its field

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests