Author’s response to reviews

Title: Comparison of ankle-brachial pressure index and pulse wave velocity as markers of cognitive function in a community-dwelling population.

Authors:

Norio Sugawara (nsuga3@yahoo.co.jp)
Norio Yasui-Furukori (yasufuru@cc.hirosaki-u.ac.jp)
Takashi Umeda (tume@cc.hirosaki-u.ac.jp)
Ayako Kaneda (ayappe0924@yahoo.co.jp)
Yasushi Sato (hcgdb853@ybb.ne.jp)
Ippei Takashi (ippei_japan@yahoo.co.jp)
Masashi Matsuzaka (m-matt@cc.hirosaki-u.ac.jp)
Kazuma Danjo (kaz.danjo@nifty.com)
Shigeyuki Nakaji (nakaji@cc.hirosaki-u.ac.jp)
Sunao Kaneko (sk@cc.hirosaki-u.ac.jp)

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Author’s response to reviews: see over
12–Apr-2010
Sabina Alam, PhD
Senior Scientific Editor, BMC series journals

Dear Dr. Alam,

I have received your e-mail concerning our manuscript (MS: 4553979883464913) on Mar 30. We appreciate the additional comments and have made correction as follows. I hope the 2nd revised manuscript will be acceptable for publication in BMC psychiatry.

> First of all, it needs a radical editing operation in order to clear it of the mistakes and improper and unclear expressions. It also needs some clarifications about the procedure with which data were analyzed.

We divided our subjects in three group and re-analyzed data. The expression “the first report”, has been deleted because it is improper.

> Furthermore, it is not clearly said in the text,
> if there were any attempts to evaluate the possible increase in the discriminating power of the first index by the presence of the second index, while there is an expression where they say that the two indexes are significantly useful.

We found many previous studies about ABI and PWV. But, our data did not show a significant usefulness about ba-PWV. So, we did not state that two indexes are significantly useful in result.

As described in introduction and discussion, cf-PWV was used in various studies. But, a device which can simultaneously measure ABI and ba-PWV has become available. Therefore, we tried to compare ABI and ba-PWV as described in introduction.

> The other thing, which I underline that should have been done, is a comparison between balanced groups. It is not sound to compare two groups with a subject ratio close to 1 to 10 as they have done. The authors should extract the same amount of normal values people with a technique that would assure a case control balanced composition to the two groups and then perform their analysis.

We re-analyzed data according to associate editor’s suggestion and obtained almost the same result as table 1. There are some previous studies analyzing data with subject ratio similar to us. We would like to retain table 1.

> Another thing is not yet clear to me: do they say in the answering letter that MMSE values were not
> different in statistical terms but in the table they are still presented in this way? If I read
> correctly this, the table is false and in need to be amended.

In our answering letter, we could not find significant (p<0.05) difference of MMSE score between each
ABI quartile group. We observed significant difference of MMSE score between poor cognition and
control group. Table 1 is not false.

> Other less determinant things are: a)

   We rewrote the sentence concerning a cut-off point of MMSE.

> Other less determinant things are: b)

   As stated above, we divided our subjects in three group and re-analyzed data.