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## Letter to the Editor re: Prostate Cancer in Pilots

**Dear Editor:** 

The second meta-analysis on prostate cancer among pilots by Raslau et al. was published in June last year;<sup>16</sup> the first metaanalysis by the same authors was published in February 2015.<sup>17</sup> The first study<sup>17</sup> was criticized by Connolly the same year<sup>6</sup> for including inappropriate data from two studies on U.S. Air Force servicemen when it was intended to study pilots, and for including duplicate data as the joint analysis on Nordic pilots by Pukkala et al.<sup>13</sup> also included cohorts from four national studies.<sup>8,9,12,14</sup> Raslau and coworkers were praised in an ethical paper<sup>3</sup> for retracting their first paper.<sup>15,17</sup> In the second attempt these errors were corrected;<sup>16</sup> however, new mistakes were made and some former mistakes were repeated.

In the Methods section it says that literature was searched until August 2015,<sup>16</sup> but the authors failed to include two recent mortality studies published in 2014.10,18 The study of Hammer and coworkers is a large mortality study of nine European cohorts of pilots,<sup>10</sup> but earlier analysis of nearly the same group of researches<sup>4</sup> were strangely not included in the first metaanalysis of Raslau et al.<sup>17</sup> These joint analyses of the ESCAPE studies<sup>4,10</sup> include the pilots of the Italian<sup>1</sup> and the German<sup>11</sup> cohorts, and the later ESCAPE study with longer follow-up<sup>10</sup> would have been appropriate in the meta-analysis. The first study of the Canadian pilots<sup>2</sup> is missing in both analyses of Raslau and coworkers (however, included in the reference list of both meta-analyses<sup>16,17</sup>), as well as a large study of U.S. airline pilots.<sup>5</sup> It is of interest to know if these studies were considered and, if so, why they were omitted from the metaanalyses.16,17

Raslau and coworkers do not mention what keywords or combinations they used when they or their librarians were searching the literature.<sup>16,17</sup> However, it can be seen from the flowchart of the selection process in the later article<sup>16</sup> that the studies obtained had been primarily divided into incidence and mortality studies. The description of the procedure about the study strategy have nearly exactly the same wording in the Methods of the two publications.<sup>16,17</sup> The only noteworthy difference is the date of the end of the literature search.<sup>16,17</sup> In the flowchart it is stated that the studies involved met inclusion criteria, however, these criteria are never explained.<sup>16</sup>

Table II shows the quality assessment in the later publication of Raslau et al.<sup>16</sup> and here there is some lack of precision. There is confusion between Hammar and Hammer, the study of Hammer et al.<sup>11</sup> is included in the meta-analysis, but not the study of Hammar et al.,<sup>12</sup> and there is an exaggeration in the footnote of the table.<sup>16</sup>

It is a pity that the selection of the studies for the meta-analysis is still all in a mess,<sup>16</sup> and that the description of the selection process is so deficient that it is not possible point out what exactly went wrong.

Finally, one may wonder whether it is a good idea to combine pilots from military and commercial/civilian services in a meta-analysis or whether the different services are better evaluated separately. In one of the studies of pilots which dealt with both types of pilots, it was discussed whether such a combination was appropriate because of differences between military and commercial airline pilots regarding types of aircrafts, and duration and altitude of the flights.<sup>12</sup> Furthermore, the military and the airline pilots were assumed to have different lifestyles.<sup>12</sup>

This letter is written because of a desire for a correctly conducted and robust meta-analysis of prostate cancer among airline pilots. The 5-year survival rate for cancer of the prostate during the last 15 years in developed countries has been 80 to 90%,<sup>7</sup> and recognition of that fact would be welcome in the discussion of the meta-analysis.

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## In Response:

We appreciate the interest of Dr. Rafnsson in our systematic review and meta-analysis.<sup>1</sup> We also appreciate the dedication and continued effort by the Aerospace Medicine community (readers of this journal) to keep the evidence base about pilots' health as precise and rigorous as possible.

The topic of prostate cancer in pilots is important and the relevant literature is dynamic and evolving, with new studies being published as more groups examine this area. The Letter to the Editor points to some additional studies about the topic. These studies were not included in our analysis due to either the search date of our review or due to our interest in excluding any study that was not done exclusively in pilots (which was the challenge we had in the first meta-analysis<sup>2</sup>). Therefore, we excluded studies that described a part of their cohort as Air Force Servicemen, cabin crew, etc.) and sought to only include those that explicitly and unambiguously described their cohort as pilots. We recognized that this restrictive criterion would lead to excluding some pilots from these cohorts; however, in our second meta-analysis we were erring on the side of exclusion to be more precise when providing inferences about pilots' health. In reviewing the additional suggested studies, we note that the overall conclusion from the current evidence base remains the same. That is, pilots may have a very small relative increase in the incidence of prostate cancer (with unknown clinical significance), but they do not have an increase in prostate cancer mortality.

In future evidence-based reviews we expect potential associations and relationships to be better elucidated as the number of studies and number of studied aircrew expands.

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