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Letter to the Editor

Author response: Short-Communication: Revisiting conclusions of the report titled "The impact of psychological factors on self-reported sleep disturbance among people living in the vicinity of wind turbines by Jalali et al.", published in Environmental Research, Volume 148, July 2016, 401–410

We thank Mr. Palmer for his Letter to the Editor highlighting some issues in the interpretation of data from our study (Jalali et al., 2016c). Unfortunately, we disagree with his statement that the findings do not support the conclusions of the paper. Our response below responds to each of the points he raised and provides further evidence to support our conclusion that psychological factors are associated with self-reported sleep quality.

Mr. Palmer first asks "*Was the reported lack of significant difference in average A-weighted and Z-weighted nosie, measured before and after turbine operation, indicative of unchanged acoustic environment conditions?*" He is concerned that data provided by the original paper showed an increase in low frequency noise when wind turbines (WTs) were in operation. He has based his computations and assumptions on Table 4 in the paper (Jalali et al., 2016c), which contains summary data for only three participants, as the table was added to provide an example of the contents of the larger dataset. To draw such a conclusion, would first require a full analysis of the data from all participants and wind speeds at the height of 95 m for both Time 1 (T1) and Time 2 (T2), as a significant source of low frequency noise is the wind itself. We analyzed the full data set (for 16 locations and 32 nights) with Lzeq and Laeq adjusted for wind speed and no significant differences from T1 to T2 in Lzeq-Laeq (Lzeq minus Laeq) values were found. In fact as Fig. 1 shows, there were more cases in which the Lzeq-Laeq values were lower at T2.

Moreover, the wind speeds reported in the paper were the average of the wind speeds at the height of 10 m. For example, the data for receptor 3 (R3) in Table 4, which shows a wind speed average of 2.36 m/s and 1.86 m/s at a height of 10 m in T2, has an average wind speed of 5.35 m/s and 3.74 m/s at the hub, which is a criterion for the operation of WTs. Therefore, the fact that Lzeq-Laeq for R3 at T2 is 15 dB lower than T1 cannot be linked to a non-operating WTs at T2.

Mr. Palmer also states that data provided for the average of all 32 nights of monitoring showed an 8.5 dB increase in the mean of (Lzeq+SD)-(Laeq+SD) from T1 to T2. His conclusion is based on a reported Standard Deviation (SD) for Lzeq in the text of our Noise and Health paper (Page 6, Line 21–23) (Jalali et al., 2016b), where a typographical error was identified and reported to the editor and is in the process of being corrected. When all the data and noise graphs presented in both papers are considered, it is obvious that the Lzeq could not have such a large SD. Table 1 clarifies this fact and provides the mean and SD of 32 nights of noise measurement for period of Time in Bed (TIB) and one hour (1 h) at T1 and T2. As can be seen, (Lzeq+SD)-(Laeq+SD) is decreased at T2 for both TIB and 1 h. Therefore, the full dataset does not support Mr. Palmer's claim regarding an increase in low frequency noise at T2. In fact there was a decrease at T2 based on the mean+SD values.

To further support our study findings, we have conducted more statistical analyses. Fig. 2 shows the relationship between Lzeq-Laeq and distance to WTs for both times, and no significant association was found ($p=0.390$ in T1 and $p=0.335$ in T2).

In another analysis we examined the association between Lzeq-Laeq and wind speed, and the association was significant (P at T1=0.01 and p at T2=0.001) (Fig. 3). This finding shows that wind speed made a significant contribution to low frequency noise at both T1 and T2 and indicates that wind itself was the main source of low frequency noise and an important contributor even when the WTs were not operating in T1.

These additional analyses of the full dataset and scrutiny of the noise data from varying perspectives provide ample evidence that Mr. Palmer's concerns are not valid.

Mr. Palmer's second point raises concerns about the larger number of "*predominantly calm nights*" in March 2015 (compared to March 2014), which should have resulted in less annoyance after the WTs started operating. Even though we distributed questionnaires in March, we received the filled-out surveys over the following eight weeks. Mr. Palmers's statement only can be applied to the objective sleep study and so comparing the reports of windy weather in March 2014 and 2015 for a survey study is invalid.

Mr. Palmer also states that "*A careful review of the report's data shows the change in sleep disorder occurs coincident with a change in acoustic environment*". The surveys for T2 were distributed in March, whereas noise data were collected about six months earlier. It is not possible to conclude that health concerns that manifested themselves in the spring were related to the noise of two nights measured in fall. Our purpose in noise measuring in this study was to report the average inside noise before and after WT operations (adjusting for wind speed), and at no time were noise and subjective health effects measured simultaneously. Moreover, based on the data and analyses of noise provided in the previous section, his assumption that a change in acoustic environment occurred is invalid. As additional evidence, our previous extensive objective sleep data did not find any association between objective sleep disturbance and simultaneous noise measurement (Jalali et al., 2016b).

The author also states that this study "*does not prove that a change in visual cues produces a change in the sleep indices*". We clarify that being able to see WTs from their own property had an indirect effect on participants reporting of sleep disturbance, not the mere fact of WTs being present in the community. Moreover, based on the literature, rotational movement of blades in the daytime (in a fairly still environment) and flashing lights at night combined with their possible noise, can attract the eye easily to a WT, and could also increase awareness of the sound and hence also the possibly of noise annoyance and consequence health effects (Pedersen and Persson Waye, 2007). Participants in Pedersen and Waye's (2008) study described the movement of rotor blades as a constant rotation that always attracted the eye. Additionally, in the community under study, after WT

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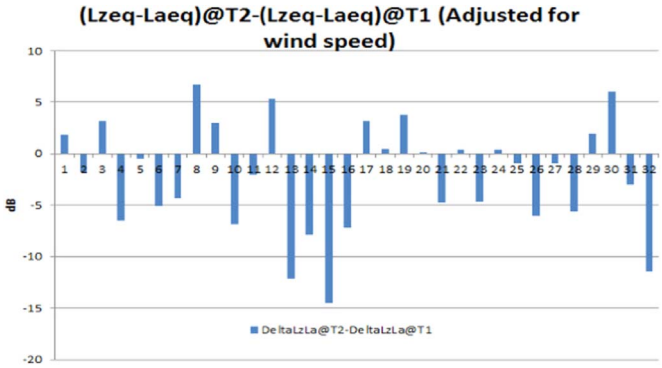

Fig. 1. (Lzeq-Laeq) at T2 minus (Lzeq-Laeq) at T1 adjusted for wind speed.

Table 1
Mean and SD of 32 nights of noise measurement for Time in Bed (TIB) and 1 h.

Parameter	T1-TIB	T2-TIB	T1-1H	T2-1H
Lzeq Mean (SD)	63.78(5.07)	61.93(6.00)	59.93(5.22)	57.44(5.33)
Laeq Mean (SD)	36.55(4.18)	36.50(4.20)	31.52(5.16)	31.23(4.91)
Lzeq(Mean+SD)	68.85	67.93	65.15	62.77
Laeq(Mean+SD)	40.73	40.7	36.68	36.14
Lzeq(Mean+SD) minus Laeq(Mean+SD)	28.12	27.23	28.47	26.63

installation, anti-WT organizations and concerned residents continued fighting to stop operation of the WTs as they believed that the project violated the setback limits. Although the visual appearance existed during T1, people did not consider the WTs as permanent structures, as they were hoping to stop the project and have them removed from the area.

Mr. Palmer was also concerned that we did not explain that WTs had been erected during T1. However, we clearly stated in the "Participant Selection" section of paper that "*The first round of data collection was conducted post turbine erection but pre operation, to avoid construction noise effects on sleep quality*". He also mentioned that *"there is a more significant change effect from turbines in T1, when they were new, than in T2, when they had already been present for a period of time for habituation*". The presence of WTs before operation may have caused some stress and concerns in residents and so we specifically took this possibility into consideration and collected plenty of concern and attitude-related data in T1. Table 2 shows some of these examples. Those residents who had health concerns and negative attitude to WTs in T1, significantly reported lower score in mental health in T2 (Jalali et al., 2016a) (the Mental component score (MCS) has been derived from the SF-12 survey, and scores range from 0 to 100 (Ware et al., 1996)).

Putting all these different results together, we would reiterate that our data have been clearly and concisely interpreted, and that reported health effects observed in this study can be related to a range of social and psychological factors. These factors may increase worry about windfarm communities and consequently the likelihood of affected individuals reporting symptoms in connection to them. We do agree that larger wind farms tend to generate more noise than smaller ones. It is also common for old WTs to generate more noise. Moreover, several other factors impact measurement and exposure to WT noise and further studies with a larger sample size are required.

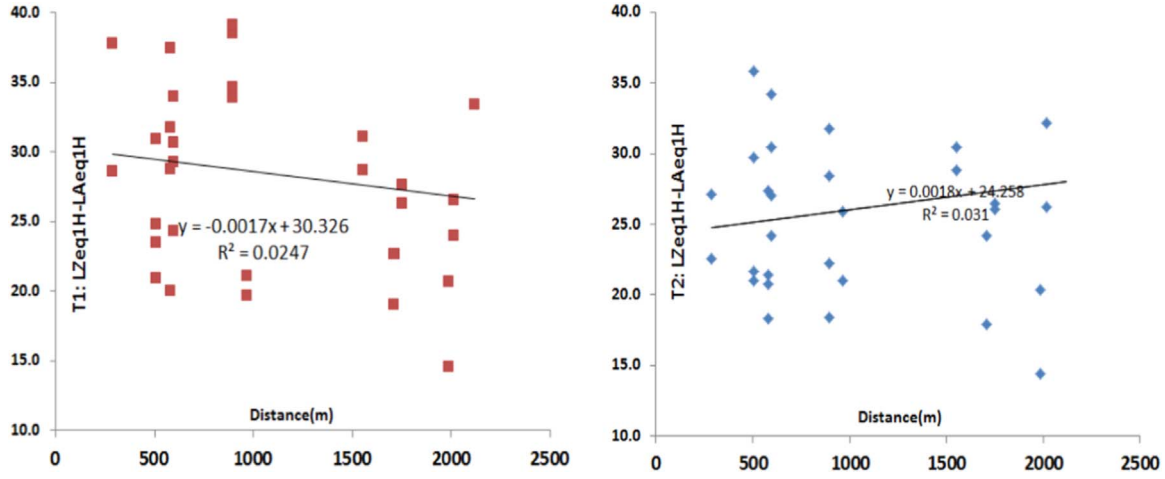

Fig. 2. Lzeq-Laeq versus distance to wind turbines for T1 and T2.

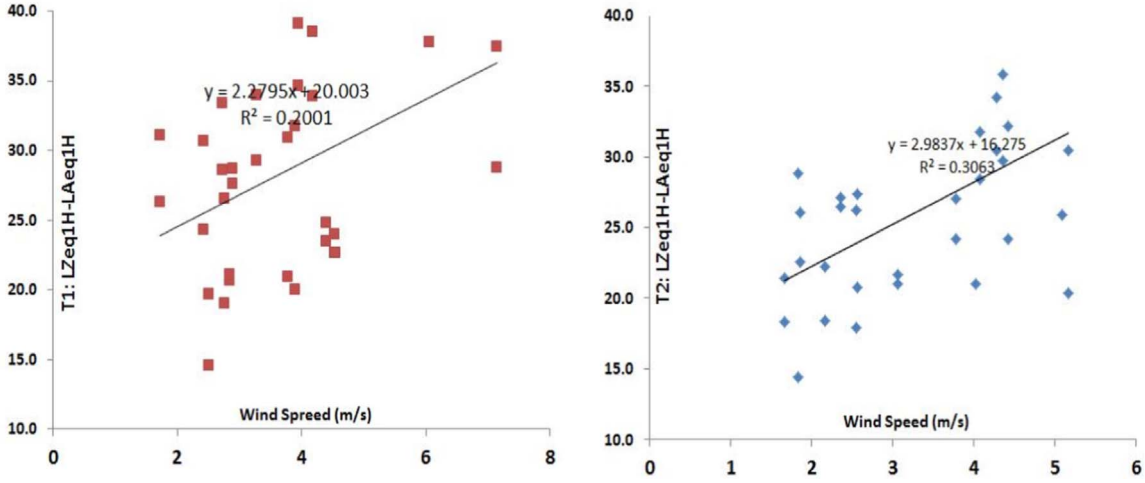

Fig. 3. Lzeq-Laeq versus wind speed for T1 and T2.

Table 2
Likelihood of reporting health effects and association with attitude and concern about wind turbines.

Attitude-related statement in Time 1	Mental component score (MCS) in Time 2	P
WTs Produce dirty energy	Those who agreed: 42.51 Disagreed:51.20	**0.050**
I am supportive of WTs in community	Agreed: 52.47 Disagreed:43.47	**0.015**
WT causes health effects	Agreed: 44.55 Disagreed: 54.88	**0.010**
My opinion was considered in the social and economic development of this area.	Agree:59.30 Disagree:49.04	**0.020**

Acknowledgments

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