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Bangladeshi medical students' suicide: A response to Arafat (2020)

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We read with interest the letter by Arafat (2020a) which made a number of claims in relation to our study 'Suicide of Bangladeshi medical students: Risk factor trends based on Bangladeshi press reports' (Mamun, Misti & Griffiths, 2020a). However, all of Arafat's claims are unfounded and/or simply incorrect.

Arafat's first claim [Part 1]: *"Firstly, the authors claimed that the number of cases of medical students' suicides was 13 which was potentially flawed as among them three cases (23%) were not medical students. They were medical technology student, medical institute student (which is supposed to be a technology student) and another one was a paramedical student. There are fundamental differences in merit, entrance criteria and curriculum between medical students and paramedic or medical technology students"* (p.1).

Our response: In our search for medical student suicide cases we excluded 45 of the 58 cases we identified because the degree they were studying for had absolutely nothing to do with study of medicine. The 13 remaining students were all on similar curricula and likely to be working in the same medical student environments. We were only too aware of the differences and we reported the details of these three students in the footnote of Table 1. It is only because of our total transparency relating to all 13 suicides that Arafat can even make an arguably ridiculous claim in the first place. The three students we included in our sample were more akin to the 10 medical students than the 45 non-medical students we excluded from the sample.

Arafat's first claim [Part 2]: *"Although the author* [sic] *mentioned them they did not exclude them from the analysis. Moreover, they included them as medical students, claimed their statement including them which is a fundamental misnomer"* (p.1).

Our response: Our study did not carry out any formal analysis so it is incorrect of Arafat to claim that these three students on medical curricula should have been excluded from the analysis. We think that what Arafat meant to say was that the three students should have been excluded from the sample we described (which is something totally different). However, we stand by our decision to include them based on our aforementioned reasoning and the fact that we were crystal clear in Table 1 as to what type of medical-related degree each student was studying on.

Arafat's second claim: *"Secondly, even including the contradictory three cases, the sample size is too small"* (p.1)

Our response: Firstly, the three cases are not *"contradictory"* by any dictionary definition that we are aware of (for instance, the *Cambridge Dictionary* (2020) says *"If two or more facts, pieces of advice etc. are contradictory, they are very different from each other"* [p.1]]). We included the three students because of their similarities with the other ten other medical students, not because

of their differences. More importantly, Arafat claims our sample size was too small. Too small for what exactly? The sample is too small to do any statistical analysis but we did not do any statistical analysis (unlike Arafat who said that *"23%"* of our sample were not medical students and has deliberately misused percentages in a situation that percentages clearly should not have been used). All of the data we collected were case studies. Would we criticize Arafat's studies that have low numbers of suicidality cases (i.e., Arafat, 2018 [one case]; Arafat & Hossein, 2018 [three cases]; Arafat & Mamun, 2019 [four cases]; Arafat et al., 2020a [three cases]) and say they were too small? No we would not.

Arafat's third claim: *"Thirdly, in the discussion section, authors claimed that "there is a greater incidence of medical student suicides in Bangladesh compared to suicide incidence rates reported elsewhere" which cannot be proved by data because there is no central suicide database in Bangladesh (Arafat et al., 2018). Moreover, their study also didn't assess the suicide rates among medical students in Bangladesh"* (p.1).

Our response: In our study, we reported all the previous studies that had reported the incidence of medical students' suicide globally. We then compared what we found in our study and concluded that the incidence rate was higher among Bangladeshi medical students than the other studies reported. The fact that Bangladesh does not have a central suicide base (something which we have noted on many previous occasions [e.g., Mamun & Griffiths, 2020a, b, c, d, e; Mamun et al., 2020b]) is one of the key reasons we had to rely on the method we chose. Also, Arafat appears to think our study was trying to *"assess the suicide rates of medical students in Bangladesh".* At no point in our study did we ever claim this and Arafat has invented a criticism for something that was not present in the first place. The only time we mentioned 'suicide rates' was in the limitations when we said *"the actual suicide rates of medical students (or university students more generally) among total suicides cannot be ascertained"* (p.2).

Arafat's fourth claim: *"Fourthly, the authors extracted the data from news reports which cannot be strict scientific data. Studies assessing the media reports suicide in Bangladesh found poor compliance with the media guidelines (Arafat et al., 2019; Arafat et al; 2020). The authors should have been mention such an important limitation."* (p.1).

Our response: Arafat has already made this claim against us before in another context (i.e., Arafat, 2020b) and we have responded to this by noting that we used the exact same methodology as Arafat has used in at least seven studies (the list of the seven studies are cited in Mamun & Griffiths [2020d]). To criticize us for using the same methodology as he himself uses appears misguided given his reliance on this method in many of his suicide studies. Also, the media guidelines that that Arafat refers to were published by the World Health Organization (2017) and were developed for journalists not academics (i.e., a list of 12 'dos and don'ts' for journalists when it comes to

reporting on suicide in the media). We noted that in our previous response to Arafat that the WHO reporting guidelines have so many criteria to adhere to, that almost every media report in (and outside of) Bangladesh would be rated as 'poor' based on these 12 criteria.

Arafat's conclusion: *"The paper demands cautious interpretation because of flaws in sample choosing, small sample size, unsupported claims, and the source of the data is not strictly scientific."*

Our response: The method we used for the present study has been used widely by researchers from countries that do not have an active national suicide database (such as that in Bangladesh). (e.g., Armstrong et al., 2019; Bhuiyan et al., 2020; Dsouza et al., 2020; Griffiths & Mamun, 2020; Mamun et al., 2020a, b; Mamun & Griffiths, 2020e; Mamun & Ullah, 2020; for a systematic review on global media reporting suicides, see Niederkrotenthaler et al., 2020). We are only too aware of the limitations of such studies. In response, (i) there is no evidence that there were flaws in the sample chosen (Arafat would get the same result if he repeated what we did), (ii) the sample size is not an issue as we were not carrying out statistical analysis and simply reporting case studies in the media (and we should all be thankful that the overall number of suicides is small), (iii) every claim we made in our study was supported by the data we collected and/or previously published empirical papers, and (iv) we followed the scientific method and used a method that Arafat himself has used many times before. We would also point out that Arafat should perhaps critique his own research more thoroughly before attacking others given in that the very week we wrote this response, not one, but two of his just published studies were heavily criticized for poor scientific methodology (see Sharma & Tikka, 2020).

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