Using media reports about suicide as data: Response to Etchells et al. (2021)

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We appreciate Etchells et al.’s (2021) response to our paper ‘PUBG-related suicides during the COVID-19 pandemic: Three cases from Pakistan’ (Mamun et al., 2020a). The authors presented a number of issues related to our paper under the headings of (1) errors and poor practice in the data presented, (2) ethical concerns in relation to the depiction of suicide, and (3) a lack of scientific rigor. Here we respond to all of their concerns which are simply unfounded and/or misleading.

Before responding to each of the claims, it is worth mentioning that we used a well-established method of retrieving suicide data from the press media. Much of the criticism of our paper concerns the method we used to collect the data. This is a method that has been widely used by researchers globally as well as in South Asia, particularly among countries with no (or inactive) suicide surveillance system for collecting suicide information. Previous studies using this method include those in Bangladesh (e.g., Arafat et al., 2018, 2020; Arafat & Hossain, 2018; Islam et al., 2021; Shah et al., 2017; Soron & Islam, 2020), India (e.g., Armstrong et al., 2019; Patel, 2019; Pathare et al., 2020; Sripad et al., 2021; Chahal et al., 2021; Shoib et al., 2020; Balaji & Patel, 2020; Kar et al., 2020), Ghana (e.g., Abdulai, 2020; Adinkrah, 2020; Quashie et al., 2015), Nigeria (e.g., Olibamoyo et al., 2021; Oyetunji et al., 2021), Pakistan (e.g., Khan et al., 2009; Naz, 2016), Sri Lanka (Sørensen et al., 2021), Uganda (Kaggwa et al., 2021), and the United States (Kronenberg, 2021) as well as cross-national studies examining suicides from media reports in India, Bangladesh, Saudi Arabia, UK, Germany and Italy (e.g., Kar et al., 2021; Thakur & Jain, 2020). In sum, there are dozens of papers that have used exactly the same method as us (and none of the aforementioned references involved us as authors or co-authors), but as far as we are aware, Etchells et al. have not written a commentary on these or any other published paper using this method.

1. ERRORS AND POOR PRACTICE IN THE DATA PRESENTED

First Claim: The case presentation in all three cases comprises the recycling of text from news reports reporting the deaths, with minimal rephrasing attempts (see Table 1 for an example below).

Author Response: We attempted to rephrase information from newspaper reports where possible. We wanted to remain as faithful to the original reports as possible without changing the original meanings, and we meticulously cited all our sources. The way we reported the data is no different from many other similar reports published elsewhere using the same method.
Second Claim: No further information beyond that found in the news reports is offered, nor does it appear to be the case that any of the authors had direct contact with either the victims, those close to them, or clinicians or law enforcement representatives involved in the cases.

Author’s Response: As mentioned already, the method we used is well established in the psychological literature, and given that the only data sources are the media reports, it is very obvious that we (like the many other researchers who have used this exact same method) have not had any direct contact with anyone concerned with the cases. We have never claimed that the method does not have limitations. The question arises as to why our particular paper was the target of criticism rather than the many others that have been published previously.

Third Claim (first part): Furthermore, some elements of the news reports have been misinterpreted or incorrectly represented. For example, in Case 2, the authors report that “the teenager used to play game [sic] for many hours on a daily basis, and was said by those who knew him to be addicted to the game.” However, the original source notes the following: “It was purely a case of addiction as the boy used to play PUBG game [sic] for many hours a day,” the SP said. This quote is in fact attributed to a police superintendent, and not someone close to the victim.

Author Response: We strongly disagree with this claim that this alleged statement was not taken from someone close to the victim. The police rarely give out information unless they are convinced of its veracity. They would not say such things unless they had evidence to support their assertions. Here, the statement of the policeman was confirmed by the victim's father. However, the actual news report said: “It was purely a case of addiction as the boy used to play PUBG game for many hours a day. The boy’s father also confirmed about his obsession with the online game on which he spent many hours a day, the police officer said (sic)” (The Hindustan Times, 2020). It was the father who claimed his son was addicted to PUBG and this was reported in over a dozen number newspapers in both Urdu and English (e.g., Gulf News, 2020; New Indian Express, 2020a; 2020b; Sports Keeda, 2020; Tribune, 2020).

We cross-checked many newspapers reports for each case (in both English and Urdu). Because the presented information for each of the suicide cases was collected from multiple newspaper sources (e.g., Gulf News, 2020; New Indian Express, 2020a; 2020b; Sports Keeda, 2020; Tribune, 2020), we did not cite every single one of these each time but simply used one newspaper report rather than cite every single newspaper that had reported the same thing. In short, we took the decision to save journal space by just citing one rather than many sources. The newspaper article we cited for this case (Dawn News), was chosen because it was Pakistani and a well-reputed newspaper published in English. Given that most readers of our paper would not be fluent in Urdu, we cited English sources so that readers could check if they so wished. We also had other Indian (English language) newspaper sources, but we preferred a Pakistani source as the case was from Pakistan. As far as we are aware, no information was misinterpreted or incorrectly represented in our paper.

Third Claim (second part): Finally, given the brevity and journalistic nature of the original news stories themselves, it is unlikely that such materials, when not analyzed critically in conjunction with other data sources or materials pertinent to the cases in question, create an image that would be considered neutral or reliable by clinical or scientific, rather than media, standards.

Author Response: We meticulously checked all the available news stories related to the suicide case, and faithfully reported the facts. In our other papers (e.g., Mamun & Griffiths, 2020a), we
have outlined our preference for psychological autopsies in reporting suicide cases, but that was not possible for this type of methodology. We did not invent or popularize this method of data collection. We simply used a much-used previous method. We followed what the standard practice had been utilized in this area of research (i.e., media reporting suicide data). Etchells et al. may not like the method, but we are unclear as to why we have been singled out and why Etchells et al. have not taken issue with all the many previous papers published using this method.

Third Claim (third part): As such, it is unclear how the paper adds any intellectual contribution to our understanding of suicide.

Author Response: The issue of whether a paper adds intellectually to an area is ultimately subjective. The paper underwent peer review, and it was deemed worthy of publication based on its merits. We believe the paper makes a novel contribution to the literature given the rarity and alleged reasons for such types of suicide.

2. ETHICAL CONCERNS OVER THE DEPICTION OF SUICIDE

First Claim: A further concern with Mamun et al. relates to the ethical implications of publishing information about suicide, which contradicts best practice guidelines. For example, the World Health Organization provides guidance on reporting suicide and makes reference to these practices explicitly.

Author Response: The World Health Organization’s (WHO) guidelines that Etchells et al. refer to are guidelines for the media not guidelines for academic research (WHO, 2017; ‘Preventing Suicide: A resource for media professionals – Update 2017). If the media guidelines were adhered to in academic papers, there would be almost no academic papers on suicide published at all.

First Claim: The first of these concerns the reporting of suicide methods: “Avoid explicit description of the method used in a completed or attempted suicide.” (p3) In Case 1 of Mamun et al., the method of suicide is reported, which would violate acceptable standards of reporting suicide.

Author Response: We have not violated anything at all. As we have already noted, the guidelines are for reporting suicide in the media, not publishing such information in academic papers (WHO, 2017). Almost every study ever published in the suicide literature mentions the method of suicide. We can provide literally hundreds of examples where the suicide method is the primary study outcome in academic paper reporting. In our paper, the method of suicide was not the primary outcome, it was just part of the case description. However, even if it was the primary outcome measure, we have not violated any guidelines. Etchells et al. are completely mistaken and are using guidelines for the media and wrongly applying them to academic dissemination.

Second Claim: The World Health Organization guidelines also remind authors that suicide should not be oversimplified in the way causes are attributed: “The factors that lead an individual to suicide are usually multiple and complex, and should not be reported in a simplistic way. Suicide is never the result of a single factor or event.” (p7). Suicide is a complex and emotive issue, and rarely is it the case that it can be attributed to a single factor alone. However, Mamun et al.’s claims are not in line with what is considered good practice in suicide research.
Author Response: Again, Etchells et al. are using guidelines for media reporting and wrongly applying them to academic dissemination. We are well aware that suicide is a complex issue with multiple causative factors, as reported in our previous studies (e.g., Arafat & Mamun, 2019; Griffiths & Mamun, 2020; Mamun et al., 2020b; 2020c; Mamun & Griffiths, 2020b; 2020c; Sakib et al., 2021). However, Etchells et al. appear to think we reported three cases’ suicide as being based on a single causative factor. This is simply untrue. In Abstract, we said: “All three were young males aged 16–20 years and their suicides appear to be related to PUBG addiction. It is likely that all three individuals experienced other predisposing factors relating to psychological instability.” The second sentence appears to have been ignored. In the Discussion, we specifically said: “The three cases highlighted here appeared to be addicted to the PUBG game (based on corroborative reports), and their failure to stop playing the game and/or failures within the game appears to have been the precipitating factor that led them to suicide.” We deliberately used the words ‘appear’ and ‘precipitating factor’ but quite clearly did not say it was the only factor.

In addition to this point, we have also discussed other gaming-related suicide cases and quite clearly noted multiple factors. For instance, we noted: “an 18-year-old student committed suicide with issues related to gaming although there were other multiple suicide factors including (i) not getting the highest marks in an exam (which may have been because of excessive gaming); (ii) being very stressed about an upcoming exam; (iii) not being happy with his physical appearance (i.e., weight and skin color); (iv) suffering from some mental health problems including depression (probably because of his poor exam performance and dislike of his physical appearances); and (v) being addicted to playing videogames.” The idea that we think there are single causes of suicide is frankly unfounded. The paper we cited was also one we co-authored related to gaming-related suicide.

In the last paragraph of the Discussion, we stated: “For some, such negative consequences may start with pain and hopelessness, and then physiological, and/or psychological states which have the capacity to facilitate suicidal ideation if such disruptions persist continuously.” Here, we briefly discussed the possible pathway of suicide, where multicausality of suicide is also clearly stated. The suicide pathway is explained by the ‘three step theory of suicide’ (3ST) (Klonsky et al., 2016; Klonsky, & May, 2015). 3ST is the most recent established suicide theory, which tried to overcome the limitations of the prior other theories such as interpersonal theory (Joiner 2005) and integrated motivational-volitional theory (O’Connor 2011). In short, we did not report suicide causality as a single factor related to PUBG.

Third Claim: As a group of researchers with expertise in video game effects, technology effects and science communication, we are deeply concerned that a paper of this nature has been published, and the potential impact it can have on public discourse regarding the impact of video game play. There is scant evidence to date that individual games can be considered as having a direct causal effect in suicide or suicidal ideation. Moreover, given the ubiquity of video game playing as a pastime, it is highly likely that many of those who die by suicide in younger age groups play them, making any claims of causality extremely tentative. There is, however, a history of grand claims regarding video game effects that are then subsequently found to be inaccurate, sometimes to the point where a retraction is deemed necessary.

Author Response: Firstly, we never made any claim that gaming was the only contributory factor to suicide in our paper (see our earlier response). Secondly, one of the co-authors has published many papers on the many benefits of gaming (e.g., de Freitas & Griffiths, 2007; Griffiths, 2002, 2019; Griffiths et al., 2017; Nuyens et al., 2019). We firmly believe that video gaming can be beneficial to health. We even started our paper by saying: “Most research demonstrates that videogame
playing has positive influences on players' psychological health, and can have beneficial social, educational, therapeutic, and cognitive benefits." We also referenced the ‘debate’ surrounding gaming-related harm in the final section of our paper. However, PUBG-related suicide is such a rare occurrence which is why we wanted to write about the issue.

**Fourth Claim:** More broadly, there is a wealth of research that documents the harmful effects that improper news reporting can have on suicide rates. Such stories often originate from published academic work, and as such, researchers—and journals—have a duty to ensure that the work in question is rigorous, conforms to ethical standards, and isn't sensationalist or speculative, else there is the risk that coverage and promotion of the work can have extremely negative consequences. This is particularly the case for younger populations who may be at greater risk of suicide contagion.

**Author Response:** We agree that improper news reporting can impact suicide rates. This is why the WHO introduced their guidelines on how the media should report it. However, we have done nothing wrong in the reporting of the cases. We also agree that suicide can be ‘contagious’ due to improper media reporting, especially among younger populations. However, very few younger individuals routinely read papers in academic journals.

### 3. LACK OF SCIENTIFIC RIGOR

**First Claim:** Mamun et al. present no objective or robust research data to demonstrate that individual games have a causal impact on suicide, yet their discussion makes strong causal claims about the relationship between these variables. These claims are supported in a limited fashion by references to papers written by Mamun and Griffiths, which follow a similar format: presenting information gleaned from newspaper articles as objective case reports without additional scientific data, and which involve extensive self-citation.

**Author Response:** First, we refer you to our previous responses concerning the multicausality of suicide. We never claimed PUBG gaming to be a single cause of suicide. Second, Etchells et al. assert our claims are supported in a limited fashion by our own papers following a similar method (i.e., data from media-reported suicides). Yes, some of our previous publications use this method (but the majority of our research using this method does not). “Extensive self-citation” is a ‘fuzzy quantifier,’ and we only cited our own papers if relevant. The number of self-cited papers was an extremely small percentage of the papers we have published on the topics of gaming and/or suicide. In short, there was no extensive self-citation.

**Second Claim:** Typically, such case studies would draw on data or information from multiple sources, with the investigators engaging in in-depth data collection over an extended period of time via observations, interviews and other relevant quantitative or qualitative methods. Some researchers have gone further, and developed research checklists for case reports to ensure high standards of transparency and reliability. In not adhering to norms in the area, the present study therefore has extremely limited value in terms of informing the direction of future research, or in appropriately guiding clinical best practice.

**Author Response:** Again, we are being attacked for a method that we did not create or popularize. The method we used has been used many times, particularly in countries that do not have national databases or suicide surveillance systems.
4. CONCLUSION AND RECOMMENDATIONS

First Claim: In repackaging news articles as formal case reports, Mamun et al. appear to have breached guidelines regarding the ethical reporting of suicide, and risk confusing the public debate around video game effects. At face value, the article appears to be a clinical research report, and as such may lead some readers to believe that the conclusions are of evidential value. However, at no point were clinicians involved in the cases approached or represented as part of the research team. Given the subjective and anecdotal nature of the content and presentation, it is not clear what the article offers in terms of an evidence base that can be used to suggest implications for psychiatric care in the future. More worryingly, given that there is a clear literature showing the effects of poor reporting on population suicide rates, we are concerned that the present paper has the potential to cause harm by effectively encouraging sensationalist news stories as worthy of academic attention.

Author Response: We submitted our case studies as a letter. The journal published it under a different heading. We reported novel cases that we believe would be of interest to readers.

Second Claim: Following the WHO's inclusion of gaming disorder in ICD-11, the reputational stakes for video games effects research and researchers interested in the impact of digital play on health and wellbeing have drastically increased. As such, studies which make exceptional claims regarding the impact of games deserve close scrutiny; it is with this in mind that we closely read Mamun et al. and which inform our concerns about the paper. We find it noteworthy that the study passed peer review without any of the above concerns being addressed.

Author Response: The decision by the WHO to include ‘gaming disorder’ in the ICD-11 was made on the basis of a wealth of scientific research. Griffiths is a member of the WHO working party, a group of individuals who have been debating and discussing this issue over the past five years. Gaming-related suicides were not discussed at all in the decision to include gaming disorder in the ICD-11. Griffiths has arguably published more empirical studies on problematic gaming than any other academic in the world. Gaming-related suicides are an extremely rare occurrence. This is why we wrote our paper. It was not to sensationalize. It was to inform and disseminate.

Final Claims: Given the importance of the WHO's decision, and the gravity under which suicide should be considered, we recommend the following:

1. Researchers should refrain from drawing on newspaper reports as a sole source of information for such studies.
2. In the absence of direct contact with case subjects, or prolonged high quality data collection, researchers should avoid speculative causal statements, either directly or indirectly, about the potential triggers of suicide. This is especially the case when referring to single factors.
3. Policy implications should not be made on the basis of case report studies alone.
4. Particularly with regard to gaming effects, research should adhere to the best practice principles of open science.

It is only by demanding the highest of standards in terms of adherence to best-practice principles, that we can ensure that video games effects research is no longer dogged by unreliable or unhelpfully sensationalist work.
**Author Response:** Our paper utilized collecting information from media-reported suicides. We never claimed this is an optimal method of rigor. The limitations of the method are well-known. We used a method that is widely used in the field. We never claimed uncontrolled and addictive PUBG gaming as the sole suicide factor. We explicitly stated that gaming has many positive benefits. Our paper was not sensationalist. There are well over three billion gamers worldwide, and the number of gamer-related suicides comprises a handful of cases. We were reporting very rare behavior, which is what many academic case studies do (including many that use media reports as the basis for case studies). Video gaming has benefits to health, but for a small minority, gaming can have a detrimental negative impact. In short, we firmly believe the concerns raised by Etchells et al. (2021) concerning our paper are misleading and/or unfounded.

5. REFERENCES


Balaji, M., & Patel, V. (2020). Hunger, fear and isolation: An analysis of media reports of COVID-19 related suicides in India. *Research Square*. [https://doi.org/10.21203/rs.3.rs-99851/v1](https://doi.org/10.21203/rs.3.rs-99851/v1)


