Principal author of PRIDE study responds to news story in The BMJ claiming that the study was based on “flawed” analysis

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On 19 March 2014, researchers from the PRIDE (Post-pandemic Review of anti-Influenza Drug Effectiveness) Consortium published the first outputs from a project investigating the effectiveness of neuraminidase inhibitors against outcomes of public health importance during the 2009 flu pandemic in the Lancet Respiratory Medicine.1,2 The headline results suggested that neuraminidase inhibitors were associated with statistically significant reductions in mortality: overall adjusted odds ratio 0.81 (95% CI 0.70 to 0.93; P=0.0024) versus no treatment and 0.50 (0.37 to 0.67; P<0.0001) if treatment was started within two days of symptom onset.

Within 48 hours, The BMJ published an article written by a staff journalist, which claimed that the new study “was based on flawed analysis.”3 Zosia Kmietowicz had contacted Mark Jones, University of Queensland, who is working with the Cochrane Collaboration on another project related to neuraminidase inhibitors. In turn, Jones had provided a detailed statistical critique of the PRIDE study, which formed the centrepiece of Kmietowicz’s article. The PRIDE Consortium was not forewarned about the article and, more importantly, not offered any a priori right of reply, as would normally be the case during post-publication correspondence. Faced with such a one-sided critique of its work, the PRIDE Consortium had no option but to post its initial rebuttal in The BMJ.4 There has since been a further critique from Jones and a further statistical rebuttal from the PRIDE Consortium.5,6 Thus, the correspondence and debate relating to a major publication in a Lancet Group paper has been played out in the pages of The BMJ, fronted by an entirely one sided article from a staff journalist on The BMJ. The major question here seems to be the propriety of The BMJ and Dr Jones in going beyond the reasonable response to a press release, by asking potential opponents for a detailed statistical critique without offering the authors of the study any right to reply alongside. A more conventional and considerably more ethical approach would have been to submit correspondence post-publication to the Lancet Respiratory Medicine, which could then have considered the response in the normal way, including offering the PRIDE Consortium a realistic period of time to consider the critique and write a rejoinder.

Competing interests: I am senior author of the paper that was critiqued.

1 University of Nottingham, Health Protection and Influenza Research. PRIDE Study: Post-pandemic Review of anti-Influenza Drug Effectiveness. www.nottingham.ac.uk/research/groups/healthprotection/projects/pride.aspx.


3 Kmietowicz Z. Study claiming Tamiflu saved lives was based on “flawed” analysis. BMJ 2014;348:g2228. (19 March.)

4 Myles PR, Leonardi-Bee J; PRIDE research consortium investigators. Re: Authors’ response to Dr Jones’s critique of the study by Muthuri et al (2014) [electronic response to Kmietowicz Z. Study claiming Tamiflu saved lives was based on “flawed” analysis]. BMJ 2014;348:g2228. (19 March.)

5 Jones M. Reply to Puja R Myles and Jo Leonardi-Bee [electronic response to Kmietowicz Z. Study claiming Tamiflu saved lives was based on “flawed” analysis]. BMJ 2014. www.bmj.com/content/348/bmj.g2228r/691879.

6 Myles PR, Leonardi-Bee J; PRIDE research consortium investigators. Further clarifications from authors of the Muthuri et al (2014) paper in response to Dr Jones’s second critique [electronic response to Kmietowicz Z. Study claiming Tamiflu saved lives was based on “flawed” analysis]. BMJ 2014. www.bmj.com/content/348/bmj.g2228r/692897.

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