
In this paper the author considers a general symmetric quantity-setting oligopoly and models the intensity of competition by assuming that firms care about their profits, plus a weighted average of the profits of the other firms. This is a way to parametrize the intensity of competition in the industry. This methodology can be related to the "coefficient of cooperation" defined by Cyert and DeGroot (1973). However, a better explanation provided by the author of such comparison can be found in his paper published in 2015 on The B.E. Journal of Theoretical Economics, which is correctly quoted in the text. The analysis is simple and relatively easy to replicate. The main message of the paper is that the so-called merger paradox, widely known in the literature since the seminal paper by Salant, Switzer and Reynolds (1983), heavily depends on the assumption of Cournot competition. When allowing for more competitive market structures, horizontal mergers are more likely to subsequently occur. In particular, the author demonstrates that the incentive to merge is increasing in the intensity of competition. Using a linear demand function, he further shows that, the higher the intensity of competition, the lower the output expanding response by non-merging firms. It follows that the minimum proportion of participants required to activate the merger decreases with the intensity of competition.

The paper touches upon very interesting policy issues, and the model is properly solved, as I mentioned above. However, I have some criticisms. The first and probably most important point relates to the main assumption adopted by the author to start his theoretical model. In particular, I am used to thinking about the presence of explicit collaborative links to justify the fact that firms may care about their profits plus a weighted average of the profits of other firms. Here, collaborative links are absent, and I do not exactly understand how in the literature that you quote, a payoff function based on such relative performance can be justified. Could you please provide additional explanation and empirical evidence to support your claim?

My second concern is related to the fact that, in order to understand the contribution
of the present paper, I found it extremely important to read the complementary paper that the author published in The B.E. Journal of Theoretical Economics, which I mentioned above. I believe that a more elaborated paper would have helped the reader to fully grasp the importance of your result. The present version resembles more a work in progress than a well-defined piece of work.

Finally, it is well known in the literature that mergers usually foster collusion. However, in your paper, mergers are considered as an alternative to collusion in situations in which the antitrust authority is profoundly committed to fighting cartel formation. Specifically, on page 3, you write: "Intuitively, in industries where firms are already co-operating, mergers add less to the high profits that firms are otherwise achieving". How could you reconcile these two interpretations? Moreover, coming back to my first criticism, a careful antitrust authority would be extremely suspicious about the fact that firms take into account the weighted average of the profits of the rivals in their payoff function. Again, it is not clear to me how this may work.

Minor remarks:

• On page 3, please explain the sentence: "Under this interpretation, a fall in the importance given to rivals' profits might correspond to a lower critical discount factor in an infinitely repeated game." Here it seems that a lower weight assigned to rivals' profits increases the interval region in which implicit collaboration can be supported. Hence, cartel activity is more likely to take place, and therefore there is less need to merge. Is that correct?

• Footnote 5 between pages 4 and 5 is not clear. Please explain.