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Reply

Reply to "Remarks on Bank Competition and Convergence Dynamics"

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Abstract: In this reply, we provide detailed answers to the remarks made by Tsionas on the use of stochastic frontier-based measures of market power in a part of our empirical study, which examines the fragmentation and convergence dynamics of market power, concentration and credit risk in the euro area banking sector during 2005–2017. Our answers clarify all the issues raised by Tsionas and show that the only challenging, in our opinion, point of the criticism has been based on a hypothesis that does not hold in the case of our study.

Keywords: Lerner index; stochastic frontiers; convergence analysis

1. Introduction

Recently, Tsionas (2020, MT) made some remarks regarding the use of stochastic frontier-based measures of market power in a part of our empirical study (Karadima and Louri 2020, KL), which examines the evolution, as well as the fragmentation and convergence dynamics, of market power, concentration and credit risk in the euro area banking sector during 2005–2017. To facilitate the comparison of the arguments of both sides, our answers are provided below in the same order as MT's remarks.

The present reply is organized as follows. In Section 2, we describe the context in which the term "profit maximization" is used in our research. In Section 3, we show that the calculation of the Lerner index, following the Kumbhakar et al. (2012) stochastic frontier methodology, does not involve the direct estimation of a translog cost function per se, but only the estimation of its partial derivative with respect to output. In Section 4, we explain why our basic equation cannot be considered as equivalent to the equation proposed by MT. In response to a general comment on output price, we take the opportunity in Section 5 to underline, using our own equations, one of the biggest innovations from Kumbhakar et al. (2012). As some auxiliary calculations had not been included for brevity in our research, we illustrate in Section 6 how we arrived at our Equation (5), starting from our Equation (4). In Section 7, we provide our answers to the criticism on the use of a single-output cost function. Finally, we show in Section 8 that the MT's criticism on our convergence analysis has been based on the incorrect hypothesis of the use of individual-bank data. Since our analysis has actually been based on country-level aggregated data, we consider that the criticism at this point is not relevant.

2. Calculation of a Lerner Index following a Stochastic Frontier Methodology

2.1. Point of Criticism

"KL state incorrectly that . . . this assumption is, of course, fairly weak". (Page 1 of MT's remarks)

2.2. Answer

Our reference to a profit maximization behavioral assumption is exclusively related to the calculation of the Lerner index per se. This should not be confused with the cost minimization behavioral assumption, which is behind the estimation of a cost function.

As it is well known in the literature, the calculation of the Lerner index is based on the assumption of static profit maximization, hence the mark-up should always have to be non-negative. However, as Kumbhakar et al. (2012, p. 113) note, when the marginal cost (*MC*) is calculated through the estimation of a cost function there is no guarantee that it would take a non-negative value for each observation. In contrast, the Kumbhakar et al. (2012) stochastic frontier methodology, which we follow in our research, rectifies this problem by always producing non-negative values of the Lerner index.

3. The Reasons for Not Estimating the Translog Cost Function

3.1. Point of Criticism

> *"Clearly, KL did not estimate a cost function as claimed . . . half-normally distributed [. . .] (page 9)". (Page 2 of MT's remarks)*

3.2. Answer

The above comment is apparently based on our phrase "so we derive its value from the estimation of the following translog cost function" (see Page 7 of our research), which should have rather been expressed in a more general manner (e.g., "so the most common approach is to derive its value from the estimation of a translog cost function"), since our only aim there was to show the steps that should have been followed if we had adopted the traditional approach.

In this context, we first state that the basis of our Lerner index estimations is our Equation (7) (see Figure 1).

Taking the partial derivative in (3), we get:

$$\frac{\partial lnTC_{it}}{\partial lnQ_{it}} = \alpha_Q + \alpha_{QQ} lnQ_{it} + \sum_{k=1}^{2} \alpha_{Qk} \ln\left(\frac{W_{k,it}}{W_{3,it}}\right) + \alpha_{EQ} lnE_{it} + \alpha_{TQ} T \quad (6)$$

Substituting (6) into (5), we get:

$$\frac{TR_{it}}{TC_{it}} = \alpha_Q + \alpha_{QQ} lnQ_{it} + \sum_{k=1}^{2} \alpha_{Qk} \ln\left(\frac{W_{k,it}}{W_{3,it}}\right) + \alpha_{EQ} lnE_{it} + \alpha_{TQ} T + v_{it} + u_{it} \quad (7)$$

Figure 1. A part of the Lerner index calculation process.

Second, we would like to emphasize that the Kumbhakar et al. (2012) stochastic frontier methodology does not require the estimation of a translog cost function per se, but only the estimation of its partial derivative with respect to output. As shown in Figure 1, we arrived at our Equation (7) after having taken the partial derivative of the translog cost function and "fitting" it into our Equation (5). This explains the reason why we have not included in our research any description at all, either in an equation or in a descriptive text form, of the calculation of marginal cost (*MC*).

4. Differences between Our Equation (7) and the Equation (7) in MT's Remarks

4.1. Point of Criticism

> *"It is not clear why one would want . . . is equivalent (up to statistical noise) to our Equation (7)". (Page 2 of MT's remarks)*

4.2. Answer

First, regarding the question why the Lerner index calculation is based on our Equation (7) (see Figure 2), instead of having been based on the cost function (our Equation (3)), the answer is that the direct estimation of our Equation (3) could not provide the residual term u (see our Equation (7)), which captures the mark-up. We have clearly stated that the methodology we have followed is that of Kumbhakar et al. (2012). The calculation of the residual term u is a very essential task in the implementation of their stochastic frontier methodology.

$$\frac{TR_{it}}{TC_{it}} = \alpha_Q + \alpha_{QQ} lnQ_{it} + \sum_{k=1}^{2} \alpha_{Qk} \ln\left(\frac{W_{k,it}}{W_{3,it}}\right) + \alpha_{EQ} lnE_{it} + \alpha_{TQ} T + v_{it} + u_{it} \quad (7)$$

Figure 2. KL's basic equation in the Lerner index calculation process.

Second, regarding the equation $\Theta = u/E_{CQ}$ (mentioned in the above MT's remark), we confirm that we have indeed used the "return-to-dollar" specification in our Equation (7) in order to calculate the term u, through which we first calculated Θ and then L (please note that the equation $\Theta = u/E_{CQ}$, mentioned in the above remark, is exactly the same as our Equation 8, although using different notation).

To summarize, we followed the steps below:

a. We defined the translog cost function (our Equation (3)).
b. We "fitted" its first derivative (our Equation (6)) into our Equation (5) in order to finally arrive at our Equation (7).
c. We estimated our Equation (7) to derive the value of the residual u, which captures the mark-up.
d. We used the value of u to calculate Θ and L.

All the above steps have been performed following the Kumbhakar et al. (2012) stochastic frontier methodology.

The alternative procedure proposed in the criticism (as we understand it) follows the steps below:

a. It estimates the translog cost function in order to calculate E_{cq}, where $E_{cq} = (\partial lnTC)/(\partial lnQ)$.
b. Based on the estimated value of Ecq, derived in step (a), it estimates either Equation (6) (in the criticism) in order to derive directly the value of L, or, alternatively, it estimates Equation (7) (in the criticism) in order to first derive Θ and then L.

First, it is clear that the alternative approach proposed by MT is the usual approach followed in the empirical literature.

Second, in response to the MT's comment regarding the statistical noise, we clarify that in the methodology of Kumbhakar et al. (2012) the term u is uniquely related to the mark-up. For further details about the term u, the reader can refer to Kumbhakar et al. (2012, p. 115). Regarding the statistical noise, it is captured by v, which is a symmetric two-sided noise term (Kumbhakar et al. (2012, p. 114)). We also take the opportunity here to explain that although the term u is calculated in a way that resembles the estimation of cost inefficiency in a cost frontier model, in Kumbhakar et al. (2012) the term u is uniquely related to the mark-up, leading Kumbhakar et al. (2012) to consider their approach as a non-standard application of stochastic frontier models. More precisely, their approach is not a cost frontier model, but a revenue share to total cost (TR/TC) frontier model. The bigger the distance between the observed TR/TC value from the minimum level it can reach (the frontier), the bigger the mark-up (and the market power).

5. The Innovation of Kumbhakar, Baardsen and Lien with Respect to the Output Price

5.1. Point of Criticism

"Moreover, KL use as a proxy for Q . . . is often not available". (Page 2 of MT's remarks)

5.2. Answer

Although Kumbhakar et al. (2012, p. 110) describe first the problem that the marginal cost (*MC*) is usually not observable, pointing out additional weaknesses of the traditional approaches in later pages, one of their innovations is indeed related to output price. As it can easily be seen in Figure 2, the estimation of our Equation (7) does not require the availability of separate information on output price, since information on total revenue is sufficient (Kumbhakar et al. 2012, p. 116).

6. Description of Some Auxiliary Equations

6.1. Point of Criticism

"Moreover, KL write that . . . in view of our (6) and (7)". (Page 2 of MT's remarks)

6.2. Answer

The fact that both P and $r = TR/TC$ are on the left side of our Equations (4) and (5), respectively, in no way means that $P = r$. In the Figure 3 below, we illustrate how we arrived at our Equation (5), starting from our Equation (4). In our paper, we have not included for brevity ("after doing some mathematics") the auxiliary calculations presented in Figure 3 (in which, for simplicity, we have not used subscripts).

$$P \geq MC \equiv \frac{\partial TC}{\partial Q} \qquad (4)$$

If we multiply both terms of (4) by Q/TC, we get:

$$P\,\frac{Q}{TC} \geq \frac{\partial TC}{\partial Q}\,\frac{Q}{TC} \qquad (4a)$$

from which we get

$$\frac{PQ}{TC} \geq \frac{\partial lnTC}{\partial lnQ} \qquad (4b)$$

From (4b) we get:

$$\frac{TR}{TC} = \frac{\partial lnTC}{\partial lnQ} + u + v \qquad (5)$$

where TR indicates total revenues.

Figure 3. Description of the intermediate steps between Equations (4) and (5).

7. Comments on the Use of a Single-Output Cost Function

7.1. Point of Criticism

"It must also be pointed out that . . . KL's (7) is better than (9)". (Pages 2–3 of MT's remarks)

7.2. Answer

The use of a single output (total assets) in our research is due to constrains on data availability with respect to the sub-period 2005–2010. For this reason, we preferred the use of total assets as our single aggregate output factor, following the most common strand in the empirical banking literature (e.g., Amidu and Wolf 2013; Angelini and Cetorelli 2003; Anginer et al. 2014; Berger et al. 2009; Carbó et al. 2009; Cruz-García et al. 2017; De-Ramon et al. 2018; Fernández and Garza-García 2015; Fernández de Guevara et al. 2007; Fu et al. 2014; Fungacova and Weill 2013; Leroy and Lucotte 2017; Liu and Wilson 2013; Sanya and Gaertner 2012; Turk-Ariss 2010; Weill 2013). In the opposite case, we would have preferred the use of an aggregated weighted-average Lerner index (e.g., Kick and Prieto 2015), as this could fit better to the goals of our study.

Regarding the remark on the use of the term u, see our answer in Section 4.

8. Convergence Analysis with Respect to Market Power

8.1. Point of Criticism

"However, there is an additional mistake . . . as they are all negative!". (Pages 3–4 of MT's remarks)

8.2. Answer

It is clear that the criticism has been based on the incorrect hypothesis that the underlying dataset of our Equation (14) contains raw data (i.e., a Lerner index for each bank).

Actually, the underlying dataset of our Equation (14) contains weighted data (in terms of total assets), which have been aggregated at the country level. The index ***i*** in Figure 4 explicitly refers to countries, not to individual banks.

In the case of competition, the β-convergence test is performed through the estimation of Equation (14).

$$\ln\left(\frac{C_{it}}{C_{i,t-1}}\right) = \alpha + \beta \ln C_{i,t-1} + \sum Country_i + \varepsilon_{it} \quad (14)$$

where C_{it} is the level of competition, as expressed by the (inverse of) the Lerner index in country i in year t, α and β are parameters to be estimated, $Country_i$ are dummy variables to control for possible

Figure 4. The β-convergence test with respect to competition.

Besides, it is clear that Section 7 in our research investigates the existence of β-convergence (with respect to competition) of countries belonging to three euro area country groups (all 19 countries, core countries, and periphery countries, respectively). There is nowhere any reference to convergence of individual banks with respect to competition.

Based on the above, we consider that the criticism on our convergence analysis is not relevant.

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