Comments on "Strategic complementarity and asymmetric price setting among firms"

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Summary of the paper

Koga et al (2019) is an interesting, well written and timely paper based on data from quarterly *Tankan* surveys covering around 10,000 firms in Japan between 2004 and 2017. Although the paper contains a number of secondary findings, there are five key findings. First, firms' pricing decisions exhibit strategic complementarity, in that they are affected by the pricing decisions of competitor firms. Second, this complementarity is asymmetrically stronger when prices decline, a finding the authors attribute to the existence of kinked demand. Third, higher inflation expectations raise the likelihood of any given firm raising its own price. Fourth, firms with a larger market share exhibit much less sensitivity to competitors' price changes. And finally, firms that report a higher degree of uncertainty delay price changes, an evidence of "wait and see" behaviour. The overall contribution here is one of more details on the state-dependent nature of price changes.

In their well written paper with a thorough literature review, the authors work hard to link the empirical estimations to the theoretical model in Dotsey and King (2005), in which a firm's price relative to the overall price level depends on a number of characteristics of the market environment. Koga et al (2019) further log-linearise the pricing equation of Dotsey and King (2005). Although this is not a structural equation in the sense of mapping prices to some fundamental drivers (instead, the prices depend on costs, labour costs, local demand, and the prices of competitors), Koga et al (2019) use it to derive their empirical model to bring to the data. Because the prices are not observed in the *Tankan* survey, the authors use a limited dependent model (ordered probit).

Comments

In my discussion of the paper, I elaborated on a number of comments. I think the key comment is that the evidence of the asymmetry uncovered in the empirical results of Koga et al (2019) does not need be driven by a kinked demand curve. The authors may want to think more broadly, and in any case beyond the model of Dotsey and King (2005), when trying to interpret their results. But given that the equation is not a structural demand equation, it may just as well be that the "kink" to which the authors attribute the asymmetry is on the supply rather than the demand side. However, the point I want to make here is even broader. The economic literature on the economics of price adjustment is indeed very large, and there will be a number

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of models that could possibly support their empirical findings. For example, Diamond (1971) presents a theoretical framework of price adjustment to a long-run, non-competitive equilibrium. In the model, firms exhibit different market shares, and their pricing decisions depend on both short-term profits and longer-term market share considerations. Each firm *j* maximises

$$\alpha_t^j p_t^j X_t(p_t^j) + V_t^j(\alpha_{t+1}^j)$$

where α_t^j is the market share of firm *j* at time *t*, $X_t(p_t^j)$ is the demand for firm *j*'s product, and $V_t^j(\alpha_{t+1}^j)$ is the value of future profits, dependent on the future expected market shares of firm *j*. Even with such a simple introduction, it seems possible that the Diamond (1971) model may bear out a number of the empirical regularities discussed in Koga et al (2019). First, if competitors' prices change, Diamond's firm will worry about losing its market share and follow suit. Because this threat is present when prices decline rather than increase, the co-movement should be asymmetrically stronger for the cases of price declines. Second, it is easy to think of a reason why firms worry about market shares less when they are larger.[2] Thus, while I am not formally evaluating the fit of Diamond's 1971 model to Koga et al's data, it seems that the model could capture their first, second and fourth results, without invoking a kinked demand scenario. I think it is likely there are many other models the authors could allude to with similar or better "success" in explaining their findings. It would be a welcome contribution if they discussed a wider family of possible theoretical interpretations of their empirical findings.

Additionally, the paper would benefit from clarifying a number of issues. It would help to understand why a survey of 220,000 firms is distilled to a mere 10,000 observations. Although the *Tankan* survey is reasonably well known, it would still be worth spelling out that it only contains surveys of pricing intentions, rather than surveys of actual prices. Furthermore, it would help to clarify whether the observations in the survey refer to changes from "*t*" to "*t*+1", or "*t*–1" to "*t*". This is not clear from the quotation included from the survey definitions. Furthermore, it is not clear whether the survey records the data for "current" and "forecasted" variables as a single (difference) variable, or as two separate variables.

The study concerns the behaviour of a firm and its competitors. It would therefore seem key to have a well identified measure of competitors. The paper identifies competitors for firm *j* as all other firms in a given industry. While this may be a good description of reality for some industries that are highly traded, possibly in durables, there are many other economic sectors where competition is highly localised. It would be useful to include the location information (eg distance) in the analysis, if available. Furthermore, given that the price adjustment relative to "competitors" is in fact a measure of price adjustment relative to the industrial average, a worry I have is that industry-specific shocks are causing within-industry price dispersion, and this is being misinterpreted as a strategic complementarity.

By the virtue of only containing data on qualitative margins, the study is predisposed to find support for time- rather than state-dependent pricing models.

[2] For example, the capacity of competitors to "absorb" switching customers can be limited by the competitors' size, which in many industrial structures is likely to be inversely related to the size of the firm we study. Or, the fraction of customers who are actively engaged in searching for cheaper alternatives is limited, and such a fixed number is of a lesser consequence for a larger firm than for a smaller one. A number of other reasons likely exist.

Thus, it is reassuring that the results are very significant. However, the authors may wish to discuss in more detail the temporal frequency of price adjustment. Given that 74% of all quarterly observations are "no change", a more careless reader may interpret this as "once a year" price adjustment, quite common in time-dependent pricing literature. It would be worth dispelling that concern.

I think it would also be worthwhile to spend more time discussing the size of the asymmetry: the authors find that the complementarity is four times larger when prices are being cut. Furthermore, the asymmetry is only significant when also controlling for market power, although this masks the finding that firms with a higher market power respond less to competitors than those with a smaller market power. I think these findings should be moved out of the extensions, and into the main results. Furthermore, a firm's *own* market share lowers the price change probability – this is a new finding in the literature, and should also make it to the main results, I think.

Finally, I would like to ask the authors to think about linking their findings on "demand uncertainty" to a literature on business cycle turning points. Business cycle turning points are clearly periods of heightened uncertainty of demand, and given that the sample spans the period around the Great Financial Crisis, the data contain a good period to check the robustness of the "wait and see" hypothesis. Presumably, the "wait and see" results should be stronger then than immediately prior to the onset of the crisis.

Let me just conclude by saying that I have learned a lot of interesting and stimulating new facts that seem to give further support to the state-dependent pricing hypothesis in the world's third largest economy. I hope that my comments will provide the authors with some ideas to further improve and strengthen their paper.

I am also grateful to the organisers of the conference for inviting me to their stimulating event.

References

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