Collusion in Industrial Economics—A Rejoinder

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Abstract. This rejoinder responds to some issues raised in the comments to my survey on collusion. The focus is on different assumptions on the punishment phase and on some aspects of competition policy.

1. Introduction

The five comments complement the survey of collusion in various important directions. Cabral and Kühn (2005) draw attention to some recent developments in collusion theory and point out unresolved issues for further research. Porter (2005) focusses on asymmetries among firms and the arising difficulties for reaching an agreement, and points out the possibility of asymmetric punishments. Mehta (2005) and Okada (2005) discuss the policy implications of collusion theory and compare them with aspects of the practice of competition policy. In the rejoinder I focus on the discussion of different concepts of punishment and on some enforcement issues.

2. Discussion of punishment strategies

Both Cabral and Kühn are critical of the ubiquitous use of grim strategies in the theory of collusion. In particular, Kühn calls for results that are based on the full set of equilibria in the repeated game considered, and not only on those relying on Nash-reversion as punishments.

  He describes results of still unpublished work showing that it indeed makes a difference whether grim strategies or optimal punishments are used and that it is possible to perform comparative statics on the full set of equilibria analysing for instance the effects of product differentiation on the ability of firms to collude (Kühn and Rimler 2005).

  The theory of optimal punishments has made a lot of progress recently, and it turns out that in a number of situations they are tractable and may even have a simple structure (albeit the reasoning to derive these results remains difficult). Nevertheless, I am somewhat puzzled by Kühn's claim that optimal punishments are "very simple" (footnote 4). Kühn only discusses price-setting firms (footnote 6), but we know that

optimal punishments in Cournot settings are highly complex and that many economists do consider the analysis of quantity setting oligopolies to be relevant.

Moreover, Abreu et al. (1990) and the work building on them do not characterize the *set of equilibria* in the first place, but the set of *equilibrium values*, which is often sufficient for the questions considered (e.g., Athey and Bagwell 2001). Actually, this is the crucial idea which makes the analysis tractable. But firms do not play "*equilibrium values*." They need to know what the corresponding equilibrium strategy is, what price to set and what quantity to produce in every period of the punishment phase. Even if we have algorithms to find the corresponding equilibria, this does not imply that they have a simple structure. Consider for instance the case of a symmetric Bertrand game described by Kühn. One of the results is that "the analysis of of *symmetric* punishments fully captures the qualitative impact on the full equilibrium value set," and this result greatly facilitates the derivation of comparative static results. But if the idea is that the results should be based on the full set of equilibria, firms would ultimately have to realize the optimal *asymmetric* punishment. Do we also know that these strategies have a simple structure?

The work on optimal punishments cited by Kühn refers to price-setting oligopolies with homogenous and differentiated products. But to analyse the factors that facilitate or hinder collusion based on the full set of equilibria, we would have to have comparative static results for oligopolies with varying asymmetric capacities, for cases of semi-collusion, for various types of fluctuating demand etc. In a some these situations, applying the concept of optimal punishments still seems to be untractable.

However, problems of analytical tractability are not the only reason why many authors hesitate to use optimal punishments in applications analysing factors that facilitate or hinder collusion and in deriving policy recommendations. Since in general, optimal punishments are very complex, the question of their relevance for explaining real-world behavior of industries has also been raised. Davidson and Deneckere (1990, footnote 16) make this point in a comment on optimal punishments: "Thus it is highly unlikely that such strategies would ever be implemented in the real world. On the other hand, standard trigger strategies require only simple calculations and are easily understood by industry participants. It is more readily imagined that firms will use these simple punishments to support tacit agreement." Similarly, in her empirical analysis of Vancouver's gasoline-price wars Slade (1992) finds that station behavior is captured by rather simple strategies and she writes: "In particular, it is important that players know in advance what constitutes a punishment and when they are being punished." In a situation of tacit collusion, this requirement would not be consistent with complex rules.

Admittedly, these quotations refer to tacit collusion, whereas Kühn clearly has explicit collusion in mind, where firms meet and can explicitly agree on their strategies including punishments. But in reality, there are not only the two extremes of completely tacit collusion or of being able to exactly agree on strategies however complicated they are.

In his comment, Porter draws attention to the relevance of firm asymmetries and the associated difficulties in reaching an agreement on the prices and quantities of the collusive outcome. Of course, these considerations apply all the more if in addition an agreement on complex punishment strategies is needed.

Here Cabral's comment comes in that we need to understand better what the real world constraints and the inner working of cartels and price fixing agreements are. In addition to the issues discussed in his comment, one of the real world constraints might be that strategies must be "sufficiently simple." Of course, this is not a well-defined concept of a feasible set of equilibria, and no natural definition exists. Using grim trigger strategies in models where the optimal punishment is very complex (or still unknown) can be one way out of this dilemma, as Nash reversion is singled out by the feature that it is the only punishment equilibrium without any short-run incentive to deviate and thus the only one that does not have in turn to be enforced by threats. Alternatively, some authors focus on symmetric punishments in symmetric games or—in asymmetric situations—to punishments with constant market shares (e.g., Athey et al., 2004; Compte et al., 2002). From a theoretical point of view, these restrictions seem rather arbitrary, too.

Another real world constraint could arise from the fact that optimal punishments may involve very aggressive behavior at the beginning with prices well below marginal costs. For various reasons (e.g., bankruptcy, ban of dumping) this may not be feasible in real life.

The discussion on optimal, grim and other sets of punishment strategies is qualified by the question whether requiring subgame-perfectness is a sufficient equilibrium concept used in this context. As pointed out by Cabral and Kühn, the possibility of renegotiation after a punishment has started is another very important aspect with respect to which we should understand much better the inner working of cartels. I fully agree on the need of investigating how possible renegotiation affects the ability to collude and how firms cope with this challenge. In this context, the question of complexity of strategies arises again. But as for optimal punishments, renegotiation-proof equilibria may have a simple structure in some cases (Abreu et al., 1993).

Finally, I would like to draw attention to two further questions on possible punishments raised in the comments. Cabral points out the possible relevance of reputation of firms within a cartel, for instance the reputation to retaliate hard. Similarly, Mehta mentions the issue of "credibility" of a retaliation mechanism. Closely related is the question discussed in Cabral's and Kühn's comments, what role learning plays in implicit or explicit collusion. These concepts are widely used in other economic contexts where repetition plays a role but have not yet found much attention in the theory of collusion. Understanding them would certainly broaden our knowledge on the working of implicit and explicit collusion.

3.   Enforcement issues

Mehta and Okada give their comments from the perspective of enforcement practitioners, and their viewpoints are valuable supplements to the survey that focusses on collusion theory. For me it was striking that a number of issues were mentioned in both comments—and without disparaging the other aspects discussed by them—I would like to draw attention to these points.

According to the experience of competition policy, there seems to be more scope for identifying industries suspicious of collusion by market monitoring than suggested by Phlips' (1996) argument that collusive and non-collusive outcomes cannot be distinguished by using observed prices and quantities because the competition authorities lack the necessary information on costs. Mehta points out that cartels often do not take the form of simple price fixing, but may for instance involve information exchange or allocation of customers, projects or geographical areas to individual firms, which might be possible to detect. Okada discusses some U.S. antitrust cases where circumstantial evidence was regarded as sufficient to prove a conspiracy and other cases, where it was not. However, the standards of evidentiary requirements at court has risen over time. The increased requirement to gain hard evidence may be one reason why leniency programs were introduced.

Both Mehta and Okada underline the importance of leniency programs in fighting collusion, and in this context, they refer to the limited resources of antitrust authorities. Thus leniency programs cannot only help in detecting conspiracies but play an significant role in gaining hard evidence after an investigation was started. Okada provides impressing evidence that in Japan, the increase in investigators starting in 1990 has lead to a surge in recommendations in antitrust cases. The current introduction of a Japanese leniency program should save resources of the antitrust authorities, thereby making it possible to investigate more cases.

4.   Conclusion

Investigating collusion by applying the theory of repeated games has helped a lot in understanding tacit collusion and cartels, which is a precondition for rational antitrust policy. The comments to the survey draw attention to questions where further research is needed. For instance, conceptual work on how to model feasible punishments considering the real world constraints of cartels and incorporating aspects like renegotiation after retaliation has started is highly desirable. Moreover, studying the interaction between cartel behavior and competition policy, as e.g., leniency programs, remains a relevant topic. The analysis of collusion among oligopolistic firms will continue to be an exciting area of industrial economics.

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