# COMMENT ON "UNCERTAINTY, MARKET STRUCTURE AND PERFORMANCE: THE GALBRAITH-CAVES HYPOTHESIS REVISITED"

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The major contribution that Christofides and Tapon (CT) make in their comment is to clarify the applicability of Lintner's [1970] model of the firm to the Galbraith-Caves (GC) proposition that large firms with market power frequently take some of their potential profits in the form of risk avoidance. The authors' primary criticism of our treatment of the GC hypothesis is that while we "claim to make extensive use of Lintner's model, [we] use in fact only half of [his] model, namely Lintner's specification of managerial preferences" [CT, 1979, p. 720]. Further, they accuse us of working "... with a different specification of market opportunities available to firms" ... and "contend that there exist *two* models, the Edwards-Heggestad (EH) model and the Lintner model, that they differ in fundamental respects and that there are some internal difficulties with the EH model ..." [CT, 1979, p. 720].

Let us at the outset, therefore, immediately plead guilty to the charge of not adhering strictly to Lintner's model, or of using "only half of [it]." The reason we did not stick to his model, of course, is because Lintner's model is incapable of illustrating everything we wished to show; in particular, it cannot be used to show that a monopolist confronts a different set of market opportunities. What CT do in their Comment is to provide an elegant demonstration of this point. They show that within the strict context of Lintner's model monopolists will face the same market opportunities as competitive firms, although some risk-return combinations available to monopolists will not be available to competitors. It is important to note that the reason for this result is that Lintner's model of the firm is a single-product model in which the firm has only one decision variable: the price of its final product.

What CT do not demonstrate is the general proposition that monopolists and competitors will necessarily operate on the same market opportunity frontier. Had they done so, their Comment would have made a significant contribution to the Galbraith-Caves debate. As it is, they merely clarify the applicability of the Lintner model to this debate, but provide no further illumination of the GC proposition.

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To better assess the nature of CT's contribution, it is useful to compare their conclusions with the goals of our 1973 paper. Our primary goal was to provide a framework within which to view Caves's two explanations of why large firms with market power are observed to display greater uncertainty-avoidance behavior, and then to use this framework to test the Galbraith-Caves hypothesis. These explanations are that the managerial personnel of monopoly firms have different utility functions—they are more risk-averse—and that firms with market power operate on a different opportunity frontier that allows them greater freedom to trade off profits for less risk [EH, 1973, p. 457]. We demonstrate that these explanations, although conceptually distinct, are difficult to distinguish empirically [EH, 1973, p. 457].

That CT do not contribute meaningfully to these goals is clear from their major conclusions. They say that

> by arguing that market power may "shift" *both* preferences and opportunities, EH may confound these two effects. We suggest one particular means of closing the model and establish that, consistent with EH's claims, an increase in managerial risk aversion leads to lower risk-taking as measured by EH. If high risk aversion coincides with high market power, then the model's predictions would accord with the Galbraith-Caves hypothesis. Our argument ignores (but does not rule out) any impact of market power on the efficiency set and hence avoids confounding preferences and opportunities. [CT, 1979, p. 726.]

It hardly seems like a contribution to "avoid confounding preferences and opportunities" by *ignoring* one of them. Would the authors argue that in order to avoid confounding income and price effects in the theory of demand we should close our eyes to one of these effects?

The real gap in our understanding of the GC proposition is that we do not have a general theoretical model that spells out the circumstances under which monopolists will *either* be more risk averse or have superior market opportunities. Our paper simply assumes these to be possibilities and proceeds to derive empirical tests that can capture both phenomena. (CT also appear to accept our empirical procedures as valid tests of the GC hypothesis.) It would require a different paper to develop a general theory capable of showing whether either of these phenomena is likely to occur and, if so, under what circumstances. To our knowledge, such a paper has not yet been written. It goes without saying that if one or both of Caves's explanations for the GC phenomenon could be ruled out by a general theory, our empirical findings would have a more precise interpretation. CT's Comment falls far short of this goal.

Before concluding, we wish to point out an important aspect of

our earlier work that perhaps we did not emphasize sufficiently. In our view the Galbraith-Caves proposition is critically dependent upon the notion or assumption of managerial discretion. If managers were not highly independent of stockholders and their interests, we know of no theoretical structure that can support the alleged GC monopoly-risk behavioral relationship. Thus, any attempt to develop a general theory of the firm to explain the monopoly-risk relationship must successfully blend managerial discretion theory of the firm with modern finance theory—a task not unlike that of mixing oil and vinegar. It will take a lot of careful stirring.

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