

A Reply to Katzner and Case

EMMANUELLE BENICOURT

*Université de Valenciennes et du Hainaut-Cambrésis, Paris, France;
e-mail: ebenicourt@free.fr*

BERNARD GUERRIEN

*Université Paris 1 (Panthéon-Sorbonne), Paris, France;
e-mail: bguerrien@sfr.fr*

In their respective comments on our paper, Katzner and Case adopt two different angles of attack. Katzner proposes some modified models of his own that he believes are immune to our objections. Case prefers to submerge us with details and bring up some commonplace concepts such as “circular flows,” households that “weigh opportunity costs,” and firms that “expand” or “contract” if their profits are higher or lower than the “normal” level, as if these were unique to microeconomics.

Both authors feel the need to preach to us a beginners’ course on methodology: on the necessarily abstract or “analytical” nature of all theory (as if we had never noticed). They seem to hope that the reader will overlook the fact that they have eluded our precise objections to microeconomics and, more particularly, have not made explicit the meaning of certain words they use. This is particularly true of the word “market,” which Katzner uses twenty-four times and Case fifteen times. As if they had not noticed that one of the main points we make in our article is that when microeconomists use the word “market” (as in “perfectly competitive markets,” “market demand,” “market supply”) they implicitly assume a centralized system, although they always suggest the contrary (i.e., a decentralized system).

At first sight it might seem that Katzner is addressing our objection when he proposes a new “market-level mechanism” in which the single auctioneer is replaced by an “announcing agent” who “adds up quantities demanded and supplied” by other agents and modifies the prices announced until he or she finds the (equilibrium) price at which he or she is “able to buy and sell what he wants of each good.” Looking at Katzner’s innovation more closely, it turns out that the essential flaw of the “mechanism” remains: the model is still centralized since it requires that everybody’s supplies and demands are “added up” by someone. Furthermore, Katzner retains the same Walrassian rule according to which “purchases and sales cannot proceed until equilibrium is reached.” Nobody can seriously believe that this fairy-tale market is a serious theoretical representation (an abstract model) of “real-world markets” as Katzner suggests.

Concerning partial equilibrium, Case limits himself to vague statements such as “demand curves exist” (Where? In never-never land?) and they are “somehow (sic!) driven by the tastes and preferences of households or individuals.” On this point, Katzner adopts

This is a slightly earlier version of a paper that appeared in:
Review of Radical Political Economics, Volume 40, No. 3, Summer 2008, 336-340

a slightly more serious position: he admits that “market” demand and supply curves “cannot be observed in reality” and speaks of the need of a “theoretical structure” that will help understand the real world. We can only agree with him on this point, but not on the precise “theoretical structure” he chooses, a structure in which there are imaginary “market” supplies and “market” demands (that “cannot be observed” but nevertheless convey information to agents). As we underlined in our article, *people trade with each other, and not with “the market.”*¹

Economic relations are always between two agents and never between an agent and “the market.” Let us consider a “theoretical structure” different from Katzner’s—one that takes this obvious fact into account, while maintaining the neoclassical approach. Let us consider, for example, a firm that wants to sell the good it produces. As there is no auctioneer to help it, the firm will have to set the price all by itself. This price will depend on the demand that the firm *expects* from potential buyers of the good. This expected demand depends particularly on prices that may be proposed by other sellers of the good, or of its substitutes. One does not need a PhD in economics to understand this. Any firm, big or small, then tries to estimate its expected demand at different prices and determines the price that, for example, maximizes its expected profit. The situation is immediately, *from the beginning*, of the same kind as those described by microeconomic models of monopoly or of monopolistic competition. We here insist that such models can be applied to numerous situations, from the grocery store “around the corner” to the Boeing Company. Both have to estimate the specific demand for the goods they sell.

The grocery store has to take into account the nearby shops and the prices they propose; Boeing cannot ignore Airbus. Firms’ decisions depend on the information they have concerning the tastes and incomes of their potential buyers and their beliefs. Firms’ decisions also depend on their conjectures about the other firms’ reactions and on their expectations about their current and future choices. Finally, these decisions also depend on the rules of the game, that is, the regulations, and the social, historical, and economic environment they are confronted with.

Microeconomists have tried, for a long time, to illustrate this kind of situation in models to which they have associated such names as Cournot, Bertrand, Edgeworth, Stackelberg, Bowley, Kreps, and Scheinkman. Indeed, different kinds of conjectures and of players’ strategies imply important differences in the form of the equilibria or the models’ “results.”

Case says that he prefers monopolistic competition models to duopoly ones. He alludes to his textbook where a “simple model . . . includes price setting firms, product differentiation, reasonably free entry and a zero-profit equilibrium” (is this really “simple”?). Yet, Case certainly knows that, since Hotelling in 1929, neoclassical theoreticians have tried to give a precise form to models much more simple than his. They rapidly realized that this raises numerous difficulties and logical problems, at least if one wants to deal with the issue in a serious manner. For Case, this is not what we should teach undergraduates, this “is not what Econ 101 or 201 is all about.” He seems to think that anecdotes and superficial analyses are

1. We have just published a book titled *La Théorie Néoclassique* where we present the main microeconomic models, without ever using the word “market,” apart from expressions such as the “complete market” assumption (which, indeed, represents a very high centralized system).

enough for students taking these courses. Only the elite—who will pursue higher economic studies—will have the right to know that in reality the theory of monopolistic competition is complex, and that the results of its numerous models are sensitive to the (arbitrary) values given to their parameters. One just has to glance at the *Handbook of Industrial Organization* (Schmalensee and Willig 1989) or to the textbooks of Kreps (1990) and Mas-Colell, Whinston, and Green (1995) to confirm this basic observation.

We do not think that these models are devoid of interest but let us try to place ourselves in the role of a microeconomist who seriously wants to present his or her theory using words that have been given a clear meaning. Then, the microeconomist does not need concepts such as “market” supply, “market” demand, “market” equilibrium. He or she only has to consider the demand or supply that firms or households expect or, at least, the demands and supplies expected by price-making agents. But if a microeconomist adopts this natural (or “scientific”) approach, the message concerning the “efficiency of competitive markets” disappears, at least if he or she excludes the centralized version of the theory.

In spite of all this, it is this “simple” message that students in Econ 101 and 201 are supposed to swallow. Contrary to all good sense, microeconomists assume that all agents are price-takers, without saying *who* makes and changes these prices (unless it is “the market”). Not only do microeconomists dedicate a great proportion of their courses to this absurd model, on top of it all they fall into a certain number of logical errors, which we highlighted in our text. This attitude can only be explained by the importance of a priori beliefs and by microeconomists’ profound conviction that “markets” are, by nature, efficient, even if they can have some “deficiencies” or “imperfections.” We observe, once again, how difficult it is to escape from the dominant ideology.

Without this (ideological) explanation, it is impossible to understand why Katzner would write that “*not all* real-world markets operate under the perfectly competitive conditions,” when he knows perfectly well that *none of them* do, not even approximately (the “conditions” are those defined by Arrow and Debreu: all prices—present and future—are given, supplies and demands are centralized by someone, no trading takes place before equilibrium price is reached, exchanges are made by a “clearing house,” without cost, etc.). Of course, if by the expression “perfect competition” he means the more popular understanding of these words (that there are “many” buyers and “many” sellers), then it is obvious that the issue of the process of exchange is largely inefficient, as costs of bargaining and search can be very important (remember Kreps’s quotation in our paper).

Curiously, both Case and Katzner talk of “zero profit equilibrium,” which we did not even mention in our text for lack of space. It is, however, a question which led neoclassical economists to heavily debate among themselves. It all started with John Bates Clark’s delirium in his 1898 book *The Distribution of Wealth*. Clark ([1898] 1908) believed that he had demonstrated that if all “factors of production” were paid according to their marginal productivity, then capitalism was both an efficient and a just system. He did not see, however, that his theory implied constant returns to scale and therefore indeterminateness of supply at any given price. His “zero profit condition” (or, as he says, the lack of a “residual”) was ridiculed by Edgeworth who argued that “no amount of authority and explanation can make it other than a strange use of language to describe a man who is making a large income, and striving to make it larger, as ‘making neither gain, nor loss.’”

Wicksell, then Walras and Hicks, tried to solve the dilemma (i.e., why would anyone produce if he is going to make zero profit, after having paid the factors of production?) and

proposed the condition of minimum unit cost. But, as Samuelson (1947: 83) highlighted in his *Foundations of Economic Analysis*, the objective of firms is to maximize their profits, not to minimize their costs. Samuelson even suggested that we should stop talking altogether about “production factors” (84)—to avoid ideological drifts à la Clark (211)—and proposed the idea that profit would “in the long run” be null because of “free entry.” Yet, here, Samuelson also falls into a logical error. In effect, the presence of fixed costs—needed for the U-shaped cost curve—implies that the entry of a new firm makes the aggregate supply curve “shift upwards.” Thus there is no reason for the number of firms producing a given good to be such that the demand curve passes *exactly* through the point where their unitary cost curve is minimum. This is what theoreticians—at least those who have thought about the question—call “the integer problem” (Mas-Colell et al. 1995: 339; Tirole 1988: 278).² For more detail on this debate see: www.bernardguerrien.com/competition-and-zero-profit.pdf.

The student in Econ 101 and 201 is probably not entitled to know all of this. Yet, if he or she were told, the student would no longer have to worry about the eighteen curves presented in chapter 9 of Case and Fair’s (2007) *Principles of Microeconomics*. Indeed, he would immediately understand that these curves assume a “long term” equilibrium that does not exist because of the integer problem but also because nobody can seriously admit that firms are so stupid that they “enter” while knowing that they will make, after paying their factors of production, zero profit! Katzner must have noticed that the tale of “free entry” and of the “zero profit in the long run” is inconsistent, since he prefers going back to constant returns to scale and uses Walras’s and Hick’s idea of minimizing costs. This enables him to find Clark’s result: “the zero-profit condition implies that all revenues accruing to firms are divided among the factors of production.” But to avoid Samuelson’s objections, Katzner introduces a mysterious “controlling factor.” We can but admire his imagination and his mathematical virtuosity, but we wonder what this has to do with reality. It is difficult not to see in this device an additional diversion to avoid responding to our elementary objections. But perhaps it is impossible to reply?

References

- Case, K. E., and R. C. Fair. 2007. *Principles of economics*. 8th ed. Old Tappan, NJ: Pearson Education.
- Clark, J. B. [1898] 1908. *The distribution of wealth: A theory of wages, interests and profits*. New York: Macmillan.
- Hotelling, H. 1929. Stability in competition. *Economic Journal* 39 (153): 41–57.
- Kreps, D. M. 1990. *A course in microeconomic theory*. Princeton, NJ: Princeton University Press.
- Mas-Colell, A., M. D. Whinston, and J. R. Green. 1995. *Microeconomic theory*. New York: Oxford University Press.
- Samuelson, P. 1947. *Foundations of economic analysis*. Cambridge, MA: Harvard University Press.
- Schmalensee, R., and R. Willig, eds. 1989. *Handbook of Industrial Organization*. New York: Elsevier North-Holland.
- Tirole, J. 1988. *The theory of industrial organization*. Cambridge, MA: MIT Press.

2. The Arrow-Debreu general equilibrium model assumes that profit can be strictly positive at (“competitive”) equilibrium. Household i receives the part θ_{ij} of the profit of firm j . The coefficients θ_{ij} , the number of firms, and the sets of production are “given,” exogenous variables (so that there is no “free entry”). Not all firms have constant returns to scale so that there is no “exhaustion” of production by factors’ “contributions.”

Emmanuelle Benicourt is a lecturer in economics at the Université de Valenciennes (France). In her PhD thesis in socioeconomics of development she developed a critique of Amartya Sen's capability theory in relationship to development and poverty issues ("Amartya Sen's Place in UNDP and World Bank Analysis of Poverty and Development," EHESS, Paris, 2005). She also questions the relevance of standard microeconomics and the way this approach is taught (see, for example, "Five Pieces of Advice for Students Studying Microeconomics" in Edward Fullbrook, ed., A Guide to What's Wrong with Economics, England: Anthem Press, pp. 84–94; and Benicourt and Guerrien, La Théorie Néoclassique, Paris: La Découverte, 2008).

Bernard Guerrien has two PhDs, one in mathematics and one in economic theory (general equilibrium). He has published books on mathematics for economists, on game theory, on microeconomic theory, and a Dictionnaire d'analyse économique. He is a member of the SAMOS (Statistique appliqué et modélisation stochastique) of the Université de Paris I (Panthéon-Sorbonne).