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The internal consistency of perfect competition

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Abstract: This article surveys some arguments brought forward in defense of the theory of perfect competition. While some critics propose that the theory of perfect competition, and thus also the theory of the firm, are logically flawed, (mainstream) economists defend their most popular textbook model by a series of apparently different arguments. Here it is examined whether these arguments are comparable, consistent and convincing from the point of view of philosophy of science.

Keywords: perfect competition, mainstream economics, theory of the firm, conflicting assumptions

Introduction

In introductory economics textbooks and lectures - those key lectures which are not only directed to economics students, but also those of business administration, sociology and other related subjects and therefore contribute much to the standard economic rhetoric in societal discourse - are mostly dominated by simple (micro)economic models about individuals and firms on partial markets. One of the most important cornerstones of the knowledge conveyed in those lectures is the *standard theory of the firm*. This theory is mostly applied in conjunction with the traditional framework of perfect competition, where all firms behave rationally in order to maximize their profits. Due to the harsh competition and the high number of firms in the market one standard assumption is that in equilibrium price is equal to marginal revenue, which in turn is equal to marginal costs. This provides an efficient and pareto-optimal allocation of goods (and services) and therefore leads to a maximum of social welfare. In this model firms are understood as mere "price-takers"; they accept the market price as exogenously given and are unable to alter it. The findings of this model are not only central to the economic education students all over the world receive - for a survey related to the global homogeneity of economics textbooks see Lee and Keen (2004, 171-172) - but may also have some influence on economic policy itself.

Since the publication of *Debunking Economics* (Keen 2000), which offered an offensive critique of the standard theory of the firm, extended in later publications (Keen 2003, Keen 2004a, Keen 2004b, Standish and Keen 2004, Keen 2006, Keen and Standish 2006), the mathematical foundations of microeconomic theory are controversially discussed. [1] This article tries to summarize the debate on Keen's objections and the reactions to his argument. The aim is (a) to identify which of those objections are effectively valid and (b) to evaluate if the problem articulated by Keen requires further consideration. In order to provide a well-arranged discussion the main contradiction, as exposed by Stigler (1957) and Keen (2000) is presented at the beginning of the paper. Subsequently it is asked whether this actually is a logical contradiction. The examination continues with the question whether logical contradictions can be resolved by an instrumentalist view (following Friedman 1953), which reinterprets the relevant conditions as "approximately correct" assumptions. Finally, the explicit focus is directed on some of these arguments, which suggest interpreting the relevant conditions not literally but as productive assumptions. Some conclusive thoughts are offered at the end of the paper.

A basic contradiction

The core argument in this debate is based on two decisive assumptions regarding the neoclassical model of perfect competition. The first one is that prices are exogenously given for the individual firm (firms as price-takers), meaning that individual firms do not have any impact on the overall price level:

$$\frac{\partial P}{\partial q_i} = 0 \qquad (1)$$

Second it is important to consider that the demand curve [2] has a negative slope respectively is falling, or expressed mathematically:

$$\frac{\partial P}{\partial Q} < 0 \qquad (2)$$

In this setup P is the market price for some product, q_i is the amount of production of a single firm i and Q is the aggregate industry production of all firms:

$$Q = \sum_{i=1}^{n} q_i \qquad (3)$$

According to the standard conception of the firm in the framework of perfect competition it is also assumed that firms are rational profit maximizers (4), have the same technology and size (5) and act independently (6). Together with equations (1)-(3) we have now identified six basic assumptions. These represent the properties of the firms' role in the standard perfect competition model.
In this setup it is intuitively plausible to argue that *if there are a lot of small (atomistic) firms, none of them can influence the overall price level.* [3] But checking these properties for internal consistency leads to the following confusing result:

$$\frac{\partial P}{\partial q_i} = \frac{\partial P}{\partial Q}\frac{\partial Q}{\partial q_i} = \frac{\partial P}{\partial Q} \qquad (4)$$

This result is obtained because it is assumed that the firms are acting independently from each other ($(\partial Q / \partial q_i) = 1$), respectively ($\partial Q_R / \partial q_i) = 0$, where Q_R is the quantity produced by the rest of the industry) and has generally been known since George Stigler's article *Perfect competition historically contemplated* (1957). Equation (4) may also have some severe implications for economic theory, since the two main assumptions combined here (equation 1 and 2) cannot exist together in a single logical universe, if the assumptions (3)-(6) should hold too. Hence, price-taking behavior and a falling demand curve are logically incompatible, meaning that such a model is simply an "impossible" one. Taking into account the deductive nature of economic theory, this paradox does indeed pose a challenging problem: Accepting equation (4) would imply the formal necessity to model single firms as able to influence price as long as there is a falling demand curve. Thus the standard formulation of marginal revenue ($mr_i = P$) would have to be changed accordingly to:

$$mc_i = mr_i = \frac{\partial(P(q_i)\cdot q_i)}{\partial q_i} = P + \frac{\partial P}{\partial q_i} q_i \qquad (5)$$

In the following sections we analyze the validity and practical relevance of equation (4).

Is this really a logical problem?

Some economists object to the result obtained in equation (4) asserting that there is a mathematical or logical error contained in the derivation. The most prominent of these general objections are discussed in the following three subsections.

The continuum argument

This argument is known in two different variations. The less complex version goes as follows: If n is infinite the quantity produced by any firm (q_i) is zero. In this case the term ($\partial Q/\partial q_i$) would also be zero (not one, as in the above calculation). Considered from a technical perspective we make the assumption that at least one firm changes the quantity produced, which leads to the following notation:

$$\frac{\partial P}{\partial q_i} = \frac{\partial P}{\partial Q}\frac{\partial Q}{\partial q_i} = \frac{\partial P}{\partial Q}\left(\sum_{j=1}^{n}\frac{\partial q_j}{\partial q_i}\right) = \frac{\partial P}{\partial Q}\left(\frac{\partial q_i}{\partial q_i} + \frac{\partial Q_R}{\partial q_i}\right) = \frac{\partial P}{\partial Q}(1+0) = \frac{\partial P}{\partial Q} \qquad (6)$$

Equation (6) clearly shows that the less complex version of the continuum argument stems from a simple mathematical error. The more complex variant on the contrary states that it is necessary to assume a *continuum* of traders. Here the term *continuum* refers to an infinite amount of sizeless entities, like the number of points on a line in geometry. While such an approach does not lead to mathematical fallacies, it still implies a completely different way of formalizing the problem discussed here (see Aumann (1964) for a proof related to a pure exchange market and Auld (2002) for a defense of the standard perfect competition model related to Aumann). Consequently such an attempt would lead to a kind of economic model completely different from the conventional theoretical structure. Moreover, *only* systems with infinite traders can be consistently described using this technique, which radically restricts its area of applicability. In fact such a model would also exhibit another troubling implication: it would assert that the total quantity of products traded on the market (Q) is zero, since infinitely summing up zeros (what is implied by equation (3) when accepting $q_i = 0$ and $n = \infty$, even more clearly if we rewrite the equation as $Q = (n \cdot q_i)$) gives us just another zero, which is a rather curious result.

This implies that the standard formulation of the theory of firm is simply incompatible with the continuum-approach. To make this line of research applicable for extensive economic modeling would require quite an effort without guaranteed success, while it adds a huge degree of complexity to economic modeling. Furthermore, Aumann (1964, 39) asserts that even "though writers on economic equilibrium have traditionally assumed perfect competition, they have, paradoxically, adopted a mathematical model that does not fit this assumption" thereby confirming, not contradicting, Keen's central criticism.

Lastly the continuum-approach makes use of "highly formal methods not familiar to the profession" (Telser 1996, 85) indicating that this type of modeling is far from being established in the discipline's theoretical discourse. [4] To fully explore the potentials associated with this type of modeling would go beyond the scope of this paper but it seems rather obvious that quite a lot of work is still necessary to develop an adequate model structure according to the continuum-approach. Moreover, this most likely wouldn't lead to a solution to the logical problem, as exposed by equation (4), within the standard model.

The elasticity argument

One approach to resolve the alleged contradiction is to analyze the price-taking behavior of firms in the framework of perfect competition by comparing the difference between the elasticity of quantities for a single firm ($e_i = (P/q_i) \cdot (\partial q_i/\partial P)$) and the whole industry ($E = (P/Q) \cdot (\partial Q/\partial P)$), where $e > E$. Therefore the reactivity of quantity to price has to be much greater for a single firm.

In this case it is overlooked that the difference between e_i and E is simply a mathematical artifact, which stems from the ratio of Q and q_i ($e_i/E = Q/q_i$) [5]

and that the elasticity has nothing to do with the construction of marginal revenue, which can be easily seen when looking at equation (5). This argument has no connection to the slopes of the demand-curve, which is in focus here. The Elasticity Evasion is, thus, simply irrelevant (see also Keen 2004a).

From this point of view it seems as if the above contradiction as such can not be resolved. But philosophers of science argue that eventually all theoretical problems can be resolved by ad-hoc modifications within the theoretical framework (Quine 1951). We can often observe that a theory is defended against conflicting empirical results by ad-hoc modifications in the axiomatic structure, e.g. by assuming additional auxiliary hypotheses or by curtailing the field of application via an extension of the ceteris paribus clause etc. On the contrary it is quite uncommon to come across such defenses in a theoretical debate when trying to solve logical problems. However, we encountered two noteworthy variants of ad-hoc variations to preserve the consistency of the model of perfect competition, which we present in the following section.

Ad-hoc variations

First the contradiction asserted in equation (4) could be resolved by assuming that firms are not price-takers, because they optimize their behavior in order to maximize their profits, but simply act as if they were price-takers. [6] This would imply to give up the assumption of rigorous profit maximization (assumption number 4) and replace it with a fixed behavioral rule. Since profit maximization is a prominent concept in economics, this axiomatic variation seems somehow radical and is factually attacking another very basic tenet of neoclassical microeconomic theory. A more precise version of this argument is developed at the end of the paper and leads basically to the same result as here: To save the consistency of the model it seems necessary to withdraw or modify another basic assumption, namely that of perfectly rational actors, which is constitutive for the homo oeconomicus-concept of neoclassical economics.

Second some authors (like Anglin 2008) present a more sophisticated possibility to resolve the contradiction by varying the axiomatic structure, which rests on changing assumption (5). By arguing that each firm may have "a slightly different cost function" firms may also differ in size implying that "$Q = (n \cdot q_i)$ for all i is unlikely to be true of an equilibrium" (Anglin 2008, 278). This axiomatic variation is more tractable since it starts by modifying a less prominent and more implicit assumption traditionally incorporated in the model. It is also more interesting since it has two important implications for the contradiction discussed. Depending on the precise formulation of how the different marginal cost curves look like one could argue that this variation also affects assumption (6) - that firms act independently - and thereby directly challenges the validity of equation (4). Consequently one would have to analyze different reaction strategies for different firms, which is due to the (necessarily) simultaneous actions of all firms very difficult to achieve because "it is hard to build an internally consistent model which asks a person to react to a choice made by somebody else *before* that person has made their choice" (Anglin 2008, 279; emphasis in original). In fact it is not only "hard" to build such a model, but eventually impossible as illustrated by the Newcomb-paradox. [7] These consequences obviously make this line of argument much less attractive, especially as long as the successively growing literature on strategic market games cannot offer any framework with strategic actors definitely *converging* to a Walrasian equilibrium (Koutsougeras 2009).

Another implication of varying assumption (5) as discussed here is that it challenges our conclusions from one of the preceding sections, especially the argument that an infinite number of firms would produce a total output Q equal to zero is no longer true. The reason for this is that $Q = 0$ if $n = \infty$ rests on the proposition that all firms have exactly the same size (assumption 5). Thus, if assumption (5) is changed, one could alternatively assume that the output of single firms follows a geometric series leading to a market output which is neither 0 nor infinite. A simple example for such a process would be:

$$Q = q_1 + q_2 + \ldots + q_n = \frac{1}{2} + \frac{1}{4} + \frac{1}{8} + \ldots + \frac{1}{2^n} = 1 - \frac{1}{2^n} \qquad (7)$$

The example sketched in equation (7) prohibits the undesirable result $Q = 0$, but it is hardly plausible that such a setup would represent "perfect competition" since firm 1 has half of the market share. An example more close to the intuitive picture of a competitive market would look like:

$$Q = q_1 + q_2 + \ldots + q_n = \frac{1}{1.01} + \frac{1}{1.01^2} + \frac{1}{1.01^3} + \ldots + \frac{1}{1.01^n} = \frac{100^{1+n} \cdot (1.01^n - 1)}{101^n} \qquad (8)$$

Eventually equation (8) would imply to replace the assumption that all firms in the market are identical "atoms", which is traditionally used to establish the model, with a very narrow concept of "slightly different firms", where the differences between firms have to be small enough to justify the label of perfect competition and have to exhibit a very specific structure (e.g. that of a geometric series) to deliver the desired result. On the one hand this argument is able to resolve the contradiction by an axiomatic ad-hoc modification and to describe a market framework, where we could in a logically plausible way try to apply the continuum perspective (thereby not resolving the other problems associated with this approach). [8] On the other hand this seems to be a far-reaching ad-hoc assumption about market structures, which reduces the plausibility of the model dramatically. It is - as already indicated - a matter of judgment, whether one may accept this solution (or the other ad-hoc-solution discussed above) or one may reject it as a classical example of immunization against critique. In any case this way of solving the problem requires a very specific assumption, which is - from our point of view - only acceptable if it is embodied in most applications of the model of perfect competition including related research, teaching and public reasoning. We are absolutely not convinced that this is the case, since the argument only appears in a very special discourse and seems to have been (re)developed exactly to encounter the critics on this front. [9] Hence, this argument seems hardly plausible, at least as an ultimate solution.

Such a debate on assumptions can of course be endlessly prolonged, but most economists a priori decide to withhold their point of view: The reasoning in this case is methodologically, follows Milton Friedman's instrumentalist view (1953) and implies that the asserted contradiction is not worth debating. Friedman argued that, while the assumptions in economics are often not very realistic, they are "productive", i.e. they are alleged to deliver adequate approximations of economic behavior and therefore create plausible and practically useful conclusions. This argument is, with a certain focus on the problem at hand, discussed in the following section.

Logical contradiction vs. reasonable approximation

Strictly speaking Friedman's argument is not about contradicting assumptions within a certain model, but is concerned with the question, whether the usual assumptions are justifiable despite their obviously virtual character. For the sake of the argument we will extend Friedman's argument to contradicting assumptions and ask for possible implications of this point of view.

From a perspective of philosophy of science internal contradictions within a certain model appear to be more problematic than the fictitious character of assumptions. This is due to the simple reason, that in the latter case one may dispute the adequacy of a certain abstraction according to any given problem. This somehow mirrors usual and well-known problems appearing in most sciences, like "are the factors we interpret as causally relevant really the causal ones?" or "which additional (auxiliary) hypotheses do we need in order to apply our model in practice?" In the former case there are no such familiar questions. Moreover from a logical point of view no further discussion is possible: One may accept, reject or maybe neglect internal contradictions but it seems logically implausible to have a dispute about the appropriateness of such an abstraction. Since "consistency" is understood as "the most important [requirement]" of any "axiomatic-deductive system" it is unsurprising that a lack of consistency leads to a "logical catastrophe" (Albert 1964, 55-56). The discussion of such deductive systems is still coined by the traditional critical-rationalist view, strictly opposing the suggestion to ignore the proposed contradiction, because it leads to a reasonable approximation.

An example from physics may further illuminate this argument: When Isaac Newton first tried to apply his conception of physical motion to a planetary system he designed a model consisting of a fixed point-like sun and a single point-like planet. This strategy of model-building actually is very similar to what economists do, namely constructing models on the grounds of simplifying assumptions. But Newton already knew that this approach is principally not sufficient for the matter at hand. The reason for this was the axiomatic structure of the proposed model, which consisted of four main axioms (Newton's three laws of motion and the law of gravitation) and (at least) two auxiliary hypotheses (AH) [10], namely:

> AH1: The sun obtains a fixed position.
>
> AH2: All celestial bodies are point-like.

The problem we experience with regard to the model described above is that AH1, the fixed sun, is obviously in conflict with Newton's third law of motion, often shortly expressed as *actio = reactio*, since there is no possible impact of the planet onto the sun. In this spirit Lakatos (1970, 135) argues that "therefore the model had to be replaced by one in which both sun and planet revolved round their common centre of gravity." Eventually Newton could not accept this kind of model, not even as an approximation, since it would have been an oxymoronic conception. This and other problems related to logical consistency lead Newton to reformulate the proposed model in various ways. The technical difficulties associated with designing a model, which is not self-contradictory, delayed the publication of Newton's "Principia" by more than a decade.

Economists on the contrary seem to accept contradicting assumptions arguing generally that the model proposed by using these is still an adequate approximation of *reality*. But in the concrete case economists argue that the model of perfect competition is - for high *n* - an adequate approximation of the Cournot model, which again is an adequate approximation of reality (or just of another model?). This kind of argument suffers from three basic problems: First such a *chain of approximation* seems implausible as long as we have no rules and no discourse on how to anchor such a construction in reality. The problem is about how long it is allowed to relate to an "approximation of an approximation of an approximation…"? Can this procedure be applied till infinity or is there a necessary empirical borderline to stop an *approximation-chain*? This problem is not discussed – neither by the economists who put it forward, nor by the specialist texts in philosophy of science – indicating a critical rupture regarding the approximation-argument even if we ignore the logical considerations discussed above.

Second it is fairly unclear what kind of approximation is postulated in this context. Well-established approximations in physics, like the model of an ideal gas, are normally associated with a specific framework allowing for the evaluation and interpretation of differences between predicted and observed values (e.g. the *Virial*-equations in case of an ideal gas). In the case of perfect competition and the Cournot-model there is of course a specific relationship between the predictions of these two models, which can be formally depicted, but in sharp contrast to physics "observed values" only rarely enter the respective debate. More specifically the argument is that the results of the Cournot model approximate the results of the model of perfect competition for a passable *n*. But we definitely lack a framework to compare the results from our models with the proceedings in real markets, and, thus, are unable to specify under which conditions our models will deliver a plausible approximation to real market behavior.

> "Along with formulating the ideal conditions, scientists have developed additional postulates which connect the ideal conditions with actual conditions so that deviations between the laws holding under these ideal conditions and actual cases can be taken into consideration and accounted for." (Barr 1974, 57)

But in economics there is no such tool, which "accounts" for possible "deviations" or gives a "framework" for evaluating "differences between predicted and observed values". Thus there is a decisive distinction between an idealization in the natural sciences and an *approximation* in economics.

> "First of all they [idealizations in economics] are intuitive rather than theoretical idealizations in the sense that the corresponding principles are not deduced, as special cases, from a broader theory which covers also the nonrational and noneconomic factors affecting human conduct. No suitable more general theory is available at present, and thus there is no theoretical basis for an appraisal of the idealization involved in applying the economic constructs to concrete situations." (Hempel 1965, 170)

In this context the alleged difference between an idealization in physics and an approximation in economics is determined by the fact, that the former is precisely relatable to *reality*, i.e. observed values, while the latter relates just to another, maybe less imperfect, *theoretical model*. Thus the superficial similarity between these two types of idealizations does not hold if analyzed thoroughly.

A third problem related to the approximation-argument is that there are a lot of models of oligopolistic behavior in economics. While some of them lead to similar results as the perfect competition case if n goes to infinity, others do not. So the choice of the Cournot model as "the" relevant reference model for approximations of firm behavior is at least questionable, if not arbitrary.

Is this really a (practical) problem?

Since there exist disagreements and uncertainties on all fields discussed so far we see no alternative to go on by discussing the quality of the asserted approximations. The arguments discussed in the following sections are normally put forward to illustrate the approximate adequacy of the perfect competition model.

The infinity argument

This argument is quite similar to the simple version of the continuum argument and states the following: If n goes to infinity then the quantity produced by any firm (q_i) goes to zero. In this case the term $(\partial P/\partial q_i) \cdot q_i$ in equation (5) goes to zero, which shows that $mr_i = P$ is a solid approximation to a situation with many, but finite traders. This solution is even perfectly correct in the *limit of an infinite number of firms*, but bears the same technical problem as discussed before (leading to $Q = 0$, which is definitely a bad approximation). If the number of firms is very large, but smaller than infinite, the deviation caused by the additional term $((\partial P/\partial q_i) \cdot q_i)$ seems negligible since q_i is very small (namely Q/n). As the market demand curve is equated to aggregate marginal revenue in the standard formulation it is a *necessary procedure* to multiply the individual marginal revenue condition with n leading to $((Q \cdot n)/n) = Q$. Therefore the *seemingly negligible small term turns into a quite substantial fraction on the market level*, namely $(\partial P/\partial q_i) \cdot Q$. From this point of view it is not self-evident that $mr_i = P$ or even $MR = P$ is an adequate approximation.

One could object here that we are confusing the relevant level of analysis and argue (with Marshall) that each firm sets its individual marginal revenue equal to its individual marginal cost and that therefore our argument is merely misleading. We take the liberty of postponing the discussion of this objection to the section after next.

Stigler's original argument

In 1957 George Stigler not only developed equation (4) but also set out to discuss the problem further. He arrived at a solution very similar to the argument proposed in the preceding section. It is considered because it is a more prominent variant of the same idea, but technically a little more expulsory. In detail Stigler stated that if there are n firms, each producing q_i units, this fact could be interpreted by using $(\partial P/\partial q_i) = (\partial P/\partial Q)$: [11]

$$mr_i = \frac{\partial tr_i}{\partial q_i} = \frac{\partial (P(q_i) \cdot q_i)}{\partial q_i} = P + q_i \frac{\partial P}{\partial Q} = P + \frac{Q}{n}\frac{P}{P}\frac{\partial P}{\partial Q}$$

$$mr_i = P + \frac{P}{n \cdot E} \tag{9}$$

Stigler's argument is that if n goes to ∞ then $P/(n \cdot E)$ goes to 0. Therefore $mr_i = P$ is an adequate approximation of reality. This version basically relies on the same mechanism as discussed in the preceding section: The net effect of individual market power is - if n is tolerably large - negligible. But on the aggregated level the error pops up again (as observed above). Looking at the aggregated marginal revenue shows:

$$MR = \frac{\partial(P(Q)\cdot Q)}{\partial Q} = P + Q\frac{\partial P}{\partial Q} = P + P\frac{Q}{P}\frac{\partial P}{\partial Q} = P + \frac{P}{E} \qquad (10)$$

Even if the individual firms marginal revenue (mr_i) converges to the demand-curve as n goes to infinity the marginal revenue for the whole market stays the same ($MR = P + (P/E)$). It is, thus, quite the same debate about the relevant level of analysis.

The Cournot argument

The argument most often put forward to illustrate the adequacy of the postulated approximation compares the results of the model of perfect competition with those of the Cournot model. The main result of these comparisons is well-known and asserts that for a passable n the results of those two models converge (Auld 2002).

The line of conflict in this case is, once more, identical to that discussed above: While the Cournot model uses equation (5) for describing marginal revenue, it sets *individual marginal revenue equal to individual marginal cost*. Keen on the other hand claims to have established a proof by contradiction showing that the aggregated, not individual, marginal revenue is the economically relevant variable. In the end this is a question of *how rational firms would act*.

Keen's proof is based on the aggregation of individual marginal revenues and individual marginal costs and shows that *some firms would make a loss if all firms would equate* mr_i *with* mc_i. Thus, according to Keen's conclusion, these firms would not behave rationally. Based on these considerations he develops a modified relation for profit maximization looking like:

$$mr_i - mc_i = \frac{(n-1)}{n}(P - mc_i) \quad (11)$$

It seems that we should find out whose interpretation of the "correct level of analysis", aggregate or individual marginal revenue, logically suits the axiomatic structure under observation. Following the usual reasoning we assume all firms act exactly in the same moment and aggregate this procedure (the culmination process in the market). The result is just the sum of the profit-maximizing behavior of individual firms (where Π_i stands for the profit function of firm *i*):

$$\sum_{i=1}^{n} \frac{\partial \Pi_i}{\partial q_i} = 0 \qquad (12)$$

The ironic thing is that this is exactly the proposition Keen starts from when deriving his proof leading to equation (11). So if there is no mathematical error in Keen's proof (at least the authors could not find any; there are several variations of the proof in Keen 2003, Keen 2004b and Keen 2006) then Keen's criticism regarding the adequacy of the abstraction is fully plausible. Thus the claim that the basic contradiction is not problematic because no factual errors arise from it, only a negligible deviation is found, seems wrong. In fact if Keen's proof was accepted it would seem reasonable to assume that timeless existing perfectly rational entities, which are the subject in question, would prefer the logic of his profit maximization rule to the standard economic one.

Comparing the relation of major variables between the different model structures further strengthens this assertion. Such a comparison clearly shows that Keen's modification of maximization rules does make quite a difference and that asserting $mr_i = mc_i$ *is not an adequate approximation* of the "real" profit maximization problem [12]:

Table 1. Comparison between the results of the Keen- and the Cournot-model.

	Keen	Cournot
q_i	$\frac{a-c}{2nb+d}$	$\frac{a-c}{(n+1)b+d}$
Q	$n\frac{a-c}{2nb+d}$	$n\frac{a-c}{(n+1)b+d}$
P	$a-bn\frac{a-c}{2nb+d}$	$a-bn\frac{a-c}{(n+1)b+d}$

The crucial point behind this comparison is still Keen's starting point (equation (12)). But challenging the formal origin of his proof is fairly difficult. Firms must act *simultaneously* in order to deliver *any* market outcome since the market price P appears in their maximization problem. If they would optimize their behavior successively they would simply lack the relevant data. Another alternative would be to assume (a) *a one shot game* or *non-learning firms* with (b) *non-omniscient firms*, unaware of "Keen's solution". This would imply to "fix" their optimization rule at $mr_i = mc_i$ by assumption leading again to an obvious conflict with the assumption of rational firm behavior (assumption (4)).

Moreover by using simulations of iterative games it is possible to show that a market with rational actors will converge around a "Keen equilibrium", further confirming the results obtained so far (Standish and Keen 2004, Keen and Standish 2006).

Conclusion

In surveying the different arguments in defense of the perfect competition model we found that the plausible arguments are related to a common root. This common root is what we referred to as the "question on the relevant level of analysis", i.e. whether individual or aggregate marginal revenue is the decisive variable. But even anchoring the defense strategy in this point does not lead to a logically consistent framework of the perfect competition model.

Thus it seems reasonable to ask why this well known heuristic of supply and demand is still intensely perpetuated in economic teaching and research. Since economics is a discipline dominated by a major paradigm - neoclassical economics - and perfect competition is a central cornerstone within this paradigm, we presume that what we can observe here is best described by the Nietzscheian motif of the "twilight of idols" (Nietzsche 1990). The idea is that all people, including scientists, fear to lose powerful symbols, devices or concepts - an idea perfectly corresponding to the Kuhnian description of a paradigm facing crisis and, thus, insisting on its axiomatic foundations and closing the ranks of the discipline (Kuhn 1970).

Moreover, there is an important amplifying mechanism supporting this paradigmatic stability. The deterministic structure of neoclassical theory leads to strange dependencies on certain core concepts, like utilitarianism, equilibrium or atomism. If these central tenets of the theoretical edifice are under attack the whole neoclassical building starts shaking. The anticipation of this problem often seems to be the main reason why economists a priori eschew to question their theoretical foundations - significantly frequently they close the respective debate by the usage of arguments *ad nauseam* or *ad hominem*.

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Endnotes

[1] In fact Keen's attempt was inspired by an article written by George Stigler (1957).

[2] One could object here that the falling demand curve is a *theorem* (deduced from the consumers' utility function), not an *axiom*. Anyway since the model of perfect competition is discussed and the terms *theorem* and *axiom* are always interpreted relative to a certain theoretical system (meaning some model or theory; in this case the model of perfect competition) the terminology is correct so far (cf. Albert 1964, 54).

[3] But one question remains unanswered from an intuitive point of view: If all firms are simple price-takers, who or what determines the price? At least we know that prices are not set by some kind of higher authority (thereby neglecting that the famous terms of the "invisible hand" and the "auctioneer" may suggest the existence of such an authority).

[4] Similarly the approach of Makowski and Ostroy (2001), who are also focusing on the deficiencies of the standard model due to the price-taker assumption, would require a complete reformulation of the standard model.

[5] Note that $(\partial q_i/\partial P) = (\partial Q/\partial P)$, because $(\Delta q_i/q_i) = (\Delta Q/q_i)$ since $(\partial Q_R/\partial q_i) = 0$ and therefore e_i can be formulated as $e_i = (P/q_i \cdot \partial Q/\partial P)$.

[6] Gabszewicz and Vial (1972, 400) follow this suggestions by stating, that "for the competitive equilibrium concept, one has not to worry about this specification [i.e. that firms might influence prices to maximize their profits] since, by assumption, the firms do not exert any influence on the direction of prices".

[7] There is a fictitious entity in Newcomb's paradox (cf. Nozick 1969), the so-called *predictor*, who can a priori react to not-yet-performed actions and is either a perfect prophet (omniscience about future events) or a time traveling entity. Both modes seem rather impossible.

[8] Moreover, the basic variant of the continuum-approach as presented in Aumann (1964) is incompatible with the idea of slightly different firms, since all firms are regarded as sizeless entities within the continuum approach.

[9] Peck (2001) represents a notable exception suggesting to use the continuum approach in undergraduate classes.

[10] From the viewpoint of deductive logic there is no difference between law-like axioms and auxiliary hypotheses in the explanans. Any contradiction between premises renders a deductive argument invalid.

[11] Regarding the following calculation it is important to consider that $(Q/n) \cdot (P/P) \cdot (\partial P/\partial Q) = (P/n) \cdot (Q/P) \cdot (\partial P/\partial Q) = (P/n) \cdot (1/E)$

[12] In calculating these values we used a standard demand function (like $P = \mathrm{a} - \mathrm{b}Q$) and a standard cost function ($tc_i = \mathrm{C} + \mathrm{c}q_i + (\mathrm{d}/2) \cdot q_i^2$ resp. $mc_i = \mathrm{c} + \mathrm{d}q_i$).

References

Albert, H. (1964), 'Probleme der Theoriebildung. Entwicklung, Struktur und Anwendung sozialwissenschaftlicher Theorien', in Albert, H. (ed), *Theorie und Realität*, Tübingen: Mohr, 3-71

Anglin, (2008), 'On the proper behavior of atoms: A comment on a critique', *Physica A*, 387, 277–280

Auld, M.C. (2002), 'Debunking debunking economics', Working Paper, University of Calgary, http://jerry.ss.ucalgary.ca/debunk.pdf, 14 November 2009

Aumann, R. J. (1964), 'Markets with a continuum of traders', *Econometrica*, 32 (1/2), 39–50

Barr, W.F. (1974), 'A pragmatic analysis of idealizations in physics', *Philosophy of Science* 41 (1), 48-64

Friedman, M. (1953), *Essays in Positive Economics*, University of Chicago Press

Gabszewicz, J.J. and Vial, J.-P. (1972), 'Oligopoly "A la Cournot" in a general equilibrium analysis', *Journal of Economic Theory*, 4 (3), 381-400

Hempel, C.G. (1965), *Aspects of Scientific Explanation*, New York: The Free Press

Keen, S. (2000), *Debunking Economics - The Naked Emperor of the Social Sciences*, London: Zed

Keen, S. (2003), 'Standing on the toes of pygmies: why econophysics must be careful of the economic foundations on which it builds', *Physica A*, 324, 108-116

Keen, S. (2004a), 'Improbable, incorrect or impossible: the persuasive but flawed mathematics of microeconomics', in Fulbrook, E. (ed), *A Guide to What's Wrong in Economics*, London: Anthem, pp. 209-222

Keen, S. (2004b), 'Deregulator: Judgment Day for microeconomics', *Utilities Policy*, 12, 109-125

Keen, S. (2006), 'Why economics textbooks must stop teaching the standard theory of the firm', SSRN eLibrary

Keen, S. and Standish, R. (2006), 'Profit maximization, industry structure, and competition: a critique of neoclassical theory', *Physica A*, 370, 81–85

Koutsougeras, L.C. (2009), 'Convergence of strategic behavior to price taking', *Games and Economic Behavior*, 65, 234-241

Kuhn, T. (1970), *The Structure of Scientific Revolutions*, University of Chicago Press

Lakatos, I. (1970), 'Falsification and the methodology of scientific research programmes', in Lakatos, I. and Alan Musgrave (eds), *Criticism and the Growth of Knowledge*, Cambridge University Press, pp. 91-196

Lee, F. and Keen, S. (2004), 'The incoherent emperor: a heterodox critique of neoclassical microeconomic theory', *Review of Social Economy*, 62 (2), 169–199

Makowski L. and Ostroy, J.M. (2001), 'Perfect competition and the creativity of the market', *Journal of Economic Literature*, 39 (2), 479-535

Nietzsche, F. (1990), *Twilight of Idols*, Penguin classics

Nozick, R. (1969), 'Newcomb's problem and two principles of choice', in Rescher, N. (ed), *Essays in Honor of Carl G. Hempel*, Dordrecht: Reidel, pp. 114-146

Peck, R.M. (2001), 'Infinitesimal firms and increasing cost industries', *Journal of Economic Education*, 32, 41-52

Quine, W.V.O. (1951), 'Two dogmas of empiricism', *The Philosophical Review*, 60, 20-43

Standish, R.K. and Keen, S (2004), 'Emergent effective collusion in an economy of perfectly rational competitors', in Stonier et al. (eds), *Proceedings of the 7th Asia–Pacific Conference on Complex Systems*, Cairns, http://arxiv.org/abs/nlin/0411006v1, 14 November 2009

Stigler, G. (1957), 'Perfect competition, historically contemplated', *Journal of Political Economy*, 65(1), 1–17

Telser, L.G. (1996), 'Competition and the core', *Journal of Political Economy*, 104 (1), 85-107

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