

Reply to Referee Report #2

First, I would like to thank to the reviewer for his/her efforts and valuable comments. It is an honor to be aware that someone struggled to understand something that I tried to develop. Below, I dispose my replies (in blue).

Main issue:

The motivation of the paper is not clear. This raises concerns regarding the contribution of the paper with respect to the literature, as well as its economic sense. The author mentions the example of night clubs but does not explicitly explain why this is a good example for the research questions tackled here, nor why this analysis or the results are of interest. It also remains unclear what are the novel results with respect to the literature that are driven by the unconstrained location choice.

The author does not explain why he chooses to extend the two papers cited above, and what is the relationship between both research questions. The author should work on finding a precise motivation and research question and try to tackle this issue, instead of investigating (apparently) unrelated problems. If the author's main interest lies in the penetration pricing strategy with network effects and the first part of the paper simply is a way to obtain the equilibrium before entry, then this should be put forward.

My goal is to expose a research note where I model network effects in a duopoly à la Zacharias and Serfes (2012) in an unrestricted location regime.

Network effects constitute an intrinsic characteristic of nightclubs: the utility of a certain agent attending to a nightclub increases with the number of other agents that 'get on board'. Moreover, most clubs are located outside the residential areas. Therefore, the product space occupied by men and women (residential area where they live) clearly differs from the product space occupied by nightclubs (which is larger when the clubs are located outside the residential areas). I humbly think that this issue is clear in the manuscript.

I try to expose the argument that the vast majority of markets embracing network effects incorporate this feature (unrestricted location regime). However, the literature on network effects fails to adopt such an environment. The vast majority of articles dealing with network effects assume that the product space occupied by firms and consumers is always the same.

However, once the referee checks the literature on location theory the same does not occur,

i.e in markets without network effects there are authors studying unrestricted location regimes (e.g., Lambertini (1997); Bárcena-Ruiz and Cazado-Izaga (2005), among others thereafter).

Therefore, it seems appropriate to extend the study of an unrestricted location regime to markets embracing network effects.

Moreover, I provide real world examples (where (i) the product space occupied by firms differs from the one occupied by consumers and (ii) network effects constitute an intrinsic characteristic of such market) to facilitate the comprehension and not to confuse the audience and this constitutes the motivation of my research.

Above, I exposed the argument in the case of nightclubs. The same applies to the other mentioned examples in the manuscript. My availability is total to change the number of provided examples and, for instance, to focus only in one convincing example or to explain in detail the mechanism behind all the mentioned cases. At this stage, I think that is clear that the benchmark model constitutes one important advance in the literature of network effects.

Moreover, I use the first part of the paper to obtain an equilibrium before entry but already introducing a notion of sequentiality in the location stage. This is important because (i) one result is to understand what happens emerging from a sequential location choice with two incumbents; (ii) another result is to understand what happens emerging from sequential entry (of a new intermediary).

Then, as the referee mentions, the paper's goal is to use point (i) as a benchmark and, then, to study point (ii). I totally agree that this should be put forward. I am glad that the referee has clearly understood this point.

Moreover, the research highlights results with an important economic meaning.

Regarding the benchmark (follower incumbent in location):

(i) I show the need to embrace models with unrestricted locations when the topic under scrutiny is network effects (this constitutes an extension relatively to prior works such as Lambertini (1997), Bárcena-Ruiz and Casado-Izaga (2005), among others since these

models of location theory do not capture the role of the network effects);

(ii) In an unrestricted location regime, the leader in location behaves as more aggressively competitor;

(iii) The follower in the location choice should avoid such an unrestricted location regime;

(iv) According to points (ii) and (iii), tipping equilibrium is more likely with an unrestricted regime (due to the presence of network effects).

Regarding entry (third intermediary trying to access the market):

(v) In an unrestricted location regime, a new entrant is indifferent between fighting for the niche or for the larger market;

(vi) However, the entry deterrence is more likely when the pre-entry condition is tipping (the deterrence of entry is independent of any marginal change in level of the network effect) while the deterrence of entry is more likely with an increment of the network externality when the pre-entry condition is maximum differentiation or asymmetric locations (the deterrence of entry depends on the marginal change of the level of the network effect)

From (iv) and (vi) note that:

1) A follower incumbent in the location choice should avoid an unrestricted regime because tipping (thus, inactivity) is more likely;

2) A follower entrant should avoid an unrestrictive regime with tipping because the distance L above which entry deterrence occurs is the lowest (relatively to the other two types of equilibrium) and such a distance does not depend on the level of the network effect.

As a result and regarding tipping in an environment of unrestricted locations in a market embracing network effects, I think that 1) and 2) clarify an interesting difference between being a follower incumbent in the location choice and being a follower entrant trying to get access to a particular market: in 1) the avoidance of an unrestricted regime with tipping depends on the likelihood of the network effects' domain, i.e. depends on the level of the network effects while in 2) the avoidance of an unrestricted regime with tipping does not

depend on the level of the network effects. Thus, the goal of studying both topics (sequential location with two incumbents and sequential entry) is not simply because of an ad-hoc reason.

Indeed, it is clear that a sequential location choice is different from a sequential entry. However, the manuscript goes further and explains the role of network effects in such difference.

The main messages of the paper are summarized as it follows.

In an unconstrained location regime:

- (i) The leader in location has more room to be a more aggressive competitor in the short run due to the presence of network effects (this is not captured by Lambertini (1997) and Bárcena-Ruiz and Cazado-Izaga (2005) since these models do not embrace network effects);
- (ii) Tipping equilibrium is more likely relatively to a restricted location regime (follows from a direct comparison with Serfes and Zacharias (2012));
- (iii) Entry deterrence is more likely with the presence of network effects (constitutes an extension of Gabszewicz and Wauthy (2012) where network effects are not considered).

Minor points

The paper is too often unclear and needs some editing in order to match academic standards. First, the writing is sometimes unclear. An example is that of Lemma 1 where the author states that firms' profit is U-shaped, but not in which variable. Other examples can be found in the text ("the fact that accommodates" page 1, "the ex-ante product space (...) is clearly lower" page 3, etc).

Second, the author should number propositions and lemmas accordingly to their category, and not across categories. Proposition 2 (respectively, 4) should be Proposition 1 (resp., 2) and Lemma 3 should be Lemma 2.

Finally, the literature should be updated and working paper versions of Serfes and Zacharias (2012) do not need to be cited here — the author mentions them when referring to a proof which is apparently also available in the published version.

I am very grateful for the assistance w.r.t. the mentioned minor points. A comprehensive English proof-reading will be undertaken to meet scientific standards in the revised version of the manuscript. My sincere apologies for this fact.

References

Bárcena-Ruiz, J.C. and F.J. Casado-Izaga (2005). Should shareholders delegate location decisions?. *Research in Economics* 59(3): 209-222.

Gabszewicz, J. J. and X.Y. Wauthy (2012). Nesting horizontal and vertical differentiation. *Regional Science and Urban Economics* 42(6): 998-1002.

Lambertini, L. (1997). Unicity of the equilibrium in the unconstrained Hotelling model. *Regional Science and Urban Economics* 27(6): 785-798.

Serfes, K. and E. Zacharias (2012). Location Decisions of Competing Networks. *Journal of Economics & Management Strategy* 21(4): 989-1005.