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Testing Metcalfe's law: Pitfalls and possibilities

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ABSTRACT

A small but burgeoning body of literature has tried to assess whether Metcalfe's law provides a realistic yardstick for the value of specific networks. In this paper, I uncover a number of flaws in the extant tests. First, a proper test of Metcalfe's law—or of any of the competing "laws"—requires correct identification of the type(s) of network effects involved and the relevant market(s). Second, a multi-market setting typically calls for scaled network sizes. Third, controlling for intertemporal changes in network quality may be imperative. Finally, indicators at the individual and aggregate levels should not be mixed. Armed with these insights, I re-examined Madureira et al. (2013)'s results. Unlike Madureira et al., I found that Metcalfe's law fits the data better than Briscoe's law.

1. Introduction: a world of networks

With the ever-increasing digitization of production and consumption processes, a growing number of goods and services in a growing number of industries have become, or are becoming, so-called network goods; that is, goods that derive at least part of their utility from their connection to a network of sorts. While most think of social networks in this regard, physical goods also are not immune to this trend. The online platform of Dutch startup 3D Hubs, for example, connects 3D printer owners with "makers" who would like to have something printed in 3D. In summer 2016, the 3D Hubs "community" comprised some 32,000 printers in over 150 countries[1].

Given their increasing prevalence, a deeper understanding of the economic value of "networks", "communities", or "platforms" has become imperative for researchers, practitioners, and policymakers alike. Abstracting from the risk of congestion, there is little doubt that the value of a network increases as it adds members; the question is by how much. A popular heuristic is Metcalfe's law, which states that the value of a telecommunications network is proportional to the square of the number of users. As Robert Metcalfe himself pointed out, however, until recently "nobody (including me) has ever made the case for or against Metcalfe's law with real data" Metcalfe (2013). A series of recent articles have examined Metcalfe's law since then, but unfortunately all the proposed tests can be criticized in one or more respects.

This paper uses the extant literature, and especially an article by Madureira et al. (2013), as a springboard to point out a number of pitfalls for researchers who would want to examine the value of networks. Specifically, I point out that a bona fide test of Metcalfe's law requires a correct identification of the type(s) of network effects involved as well as of the boundaries of the market. I also argue that a multi-market setting typically, but not always, calls for scaling of network sizes, and that controlling for inter-country differences or for intertemporal changes in the nature of the network may be required. Finally, I assert that it is vital to take the same point of view—either that of users or network owners—for all indicators involved.

The paper proceeds as follows: In Section 2, I introduce Metcalfe's law and its alternatives, and also summarize the existing approaches, in particular the efforts of Madureira et al. In Section 3, I raise five questions concerning the tests. In Section 4, I build on the answers to these questions to amend the approach of Madureira et al. and re-examine a selection of their results. In Section 5, I present my conclusions.

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[1] Source: 3D Hubs, 3D Printing Trends, July 2016 https://www.3dhubs.com/trends (last accessed 29.08.16).

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2. Existing tests of Metcalfe's law

As mentioned, the most popular rule of thumb for the value of a network is Metcalfe's "law", which focuses on the number of possible connections among members. Applying this logic, and assuming incompatibility with other networks, the individual utility of belonging to a network with n members would be proportional to $n-1$. The aggregate value of the network—that is, number of members *times* individual utility—would then be proportional to $n(n-1)$, or roughly to n^2.

Critics argue that Metcalfe's law is overoptimistic (Metcalfe, 2013). Especially Briscoe et al. (2006) take issue with Metcalfe's assumption that all connections are equally important[2], and consider Zipf's law, which assumes decreasing marginal utility, to be more realistic. Therefore, they propose that the value of a network of size n grows in proportion to $n\log(n)$, or $n\ln(n)$ in Madureira et al.'s (2013) notations. Madureira et al. call this "Briscoe's law", and view it as an extension that would hold for large networks, rather than as an alternative to Metcalfe's law. Metcalfe (2013) calls it "Odlyzko's law".

Briscoe et al. do not support their claim with data from real networks. In fact, Metcalfe himself and Madureira et al. developed the first empirical tests of Metcalfe's law only recently, and independently from one another. Metcalfe's article also triggered two follow-up papers.

Metcalfe's own test is very simple: he takes the annual revenues of Facebook as a surrogate for the value of its network, plots data for the period 2004–2013 in a graph, programs the Metcalfe's law function in Python (with a slider attached to the proportionality factor), and, after adjusting the slider, obtains "a pretty good visual fit" (2013, p. 30). However, Metcalfe has no point of comparison, as he does not attempt to fit Briscoe's law. Another problem is that, as Metcalfe acknowledges himself, "Facebook creates much more value than is captured and monetized by Facebook selling ads" (ibid.)[3].

Conversely, Madureira et al. (2013) do not attempt to directly measure the value of the Digital Information Networks (DINs) they study, but rather try to circumvent the problem by focusing on the use that is made of the networks. Specifically, they rely on the Holonic Framework (HF) developed by Madureira et al. (2011). This framework consists of 13 "capabilities" that enterprises or individuals can use to derive utility from accessing digital information. One such capability is "selectibility", defined as the "capability of a node/user in a network to scan or search for the unknown or [to] generate courses of action that improve on known alternatives" (Madureira et al., 2013, p. 248). Madureira et al. operationalize nine of these capabilities with Eurostat data on IT usage in 33 European countries. Selectibility, for example, is proxied by the fraction of enterprises using Internet search engines (Madureira et al., 2013, p. 250). Madureira et al. also posit the following causal chain: "DINs → capabilities → economic value" (2013, p. 249). While they operationalize only the first step, they assume that enterprises or individuals use capabilities because "they have direct returns on value from that use" (Madureira et al., 2013, p. 254). Hence, they argue, the selected usage indicators y_c (with c for capability) can be seen as proxies for the "real economic value in €s" (Ibid.).

In this setup, Madureira et al. want to test whether and how usage of the capabilities—over time and across countries—correlates with the size of the relevant DIN. However, their Eurostat dataset does not provide absolute numbers of enterprises and households that have access to the Internet, only fractions. As a result, Madureira et al. cannot simply test Metcalfe's law as

$$y_c = k_{c,M} n^2, \tag{1}$$

with $k_{c,M}$ = the "coupling strength between the size of the network and the value generated by capability c" (2013, p. 247) and with the subscript M referring to Metcalfe's law. Luckily, or so Madureira et al. argue, replacing absolute by relative network size "only affects the value of the proportionality constant $k_{c,M}$" (2013, p. 248); therefore, they rewrite Eq. (1) as

$$y_c = k_{c,M} x^2, \tag{2}$$

with x = the *relative* size of the relevant network (the maximum value of which is 1).

For the left-hand side of Eq. (2), Madureira et al. succeed in matching 9 of the 13 HF capabilities with indicators in their Eurostat database. Importantly, not all of the data points that they collect in this way, for 33 European countries over the period 2002–2009, are of the country-year type. Madureira et al. also add observations at the level of economic sectors and regions within a country[4]. Depending on the capability, this gives them at least 191, and as many as 3635, observations (Madureira et al., 2013, p. 252).

Turning to Briscoe's law, Madureira et al. argue that, unlike for Metcalfe's law, they cannot replace n with x (2013, p. 248). They therefore compute n from

$$\begin{aligned} x &= n/I, \\ n &= xI, \end{aligned} \tag{3}$$

with I being "the potential maximum size of the DIN" (ibid.), and in this way obtain model (4):

$$y_c = k_{c,B} xI \ln(xI). \tag{4}$$

Madureira et al. find that, overall, both model (2) and model (4) fit the data "quite well" (2013, p. 254), with one exception. For selectibility, model (2) fails. Instead, usage behaves linearly, with a slope close to 1. However, Madureira et al. (2013, p. 253) argue that this is actually the upper part of a quadratic curve because for this capability there are no observations for small network sizes. More generally, Briscoe's law fits the strongly coupled capabilities, which include "adoptability" and "selectibility", better than Metcalfe's law does, but for the capabilities with a lower k the opposite is true. Madureira et al. refer to Briscoe et al. (2006) to conclude that "these results are in concordance with observations about the validity interval of Metcalfe's law" (2013, p. 254).

Finally, there are two new leaves on the branch of the literature started by Metcalfe (2013). Zhang et al. (2015) administer three improvements to Metcalfe's test: they examine two cases instead of just one (not only Facebook but also China's most popular social network, Tencent); they compute fit parameters and do not simply rely on a visual fit; and, most important, they compare the performance of four laws rather than limiting the analysis to just one. In particular, besides Metcalfe's and Odlyzko's laws, Zhang et al. also test Sarnoff's law, which holds that the value of a network increases linearly with the number of users, and even Reed's law, which asserts that network value scales exponentially. Zhang et al. find that Metcalfe's law fits the data best and that the fit is even better for Tencent than for Facebook. Van Hove (2016a), in turn, points out that the value of a social network may also be driven by increases in the variety and quality of the services offered; he therefore explicitly controls for such changes. Van Hove finds that Metcalfe's law now outperforms the other laws even more clearly.

[2] Briscoe et al. also proffer a theoretical argument, but this is refuted by Van Hove (2014).

[3] This would not be a problem if Facebook's "monetization ratio" were constant, but there is little doubt that Facebook has over time become more astute at capturing the value it creates.

[4] In Section 3, I argue against mixing observations in this way.

3. Pitfalls and possibilities

In this section, I evaluate the tests just discussed. I do so by raising five questions, ranked from general to more specific/technical. I scrutinize the methodology of Madureira et al. (2013) in more detail because their tests are the most comprehensive, and therefore the most open to criticism. Where relevant, I also exploit insight from studies that estimate network effects without actually testing any of the network value laws.

Question 1 – Does Metcalfe's law hold for any type of network?

The most fundamental question triggered by Madureira et al.'s efforts, in particular, is whether it is realistic to expect Metcalfe's law to hold *as is* for any type of network. Metcalfe devised his law with communications networks in mind (Metcalfe, 2013). Communications networks are two-way networks, and a message from node A to node B creates value for A in the same way as a message from B to A creates value for B.

Metcalfe (2013) and the two follow-up papers examine networks that are, at least at first sight, all about interaction, namely social networks. As for Madureira et al., several of their capabilities would effectively seem to be two-way, e.g., "coordinatibility" and "trustability." Capabilities such as "selectibility" and "adoptability", however, would appear to be predominantly one-way. "Adoptability", for example, is operationalized by the "fraction of individuals that have used [the] Internet for training and education" (Madureira et al., 2013, p. 250). To the extent that individuals are not just looking up peer-to-peer (P2P) content[5], there would appear to be two distinct sides to the mechanism—"trainees" and "trainers". Clearly, in this semi-formal[6] on-line learning context, the value creation process is not the same on both sides. More specifically, the network size that determines the utility of the mechanism is not the same.

This leads us to consider the distinction between direct and indirect network effects. Direct network effects arise when direct contact between users is of value, as in social networks or in the telecommunications industry. Indirect network effects arise when one user's adoption increases the utility of other users through an increase in the variety of complementary products or in the number of "complementors". In the 3D printing example used earlier, the hub is sufficiently useful for consumers only if there is a critical mass of participating printer owners in their city. Conversely, the attraction for printer owners depends on the number of consumers in the network.

Madureira et al. would appear to have modeled all nine capabilities in terms of direct network effects, whereas some capabilities, the relevant actors on the left- and right-hand side of their equations would seem to be different. For "adoptability", the relevant network size would not seem to be the number of households with access to the Internet (as measured by Madureira et al.'s n and x) but rather the number of websites with useful information, if such an indicator exists.

In practice, the situation can be even more complex, as some goods and services are subject to both direct *and* indirect network effects. Consider a mobile payments app that can be used for person-to-person payments as well as at the point of sale. The utility of such an app for individuals depends on both the number of other users and the number of merchants that accept it.

Such settings require a specific approach. Boudreau and Jeppesen (2015), for example, examine the case of on-line multi-player game platforms for which complementors develop "mods" (game modifications), and estimate a linear system that consists of *two* equations. In the first, the number of new mods generated for a given platform depends on both usage of the platform by players (i.e., cross-side network effects) and the size of the complementor side (same-side effects). The second equation captures the positive feedback whereby platform usage depends on the size of the complementor side (again cross-side but in the other direction). And then Boudreau and Jeppesen do not even look into any same-side interactions on the user side.

To sum up, a first lesson would be as follows. Metcalfe's law was devised for communications networks, where direct network effects reign supreme. Once indirect network effects enter into play—in some cases on top of the direct effects—ideally the two-sided nature of the market should be explicitly taken into account. McIntyre (2011), for example, fails to do this. He examines the market for application software in the U.S. and models only direct network effects. This approach is adequate if the sole purpose is to demonstrate the existence of network effects, but it does not yield insights into the inner workings of the cross-side mechanisms. This point matters because the strength and even the functional form of the network effects can be different on the software and the hardware side. For example, in examining videogame consoles Corts and Lederman (2009) allow for diminishing returns of additional games (i.e., their impact on console owners' utility decreases), but work with a linear effect of the user installed base on software availability[7]. Consumer heterogeneity complicates the picture even more. In a consumer study, Steiner et al. (2016) find that direct network effects (interaction with other users, and especially with friends and acquaintances) matter only for social gamers. Conversely, indirect network effects are markedly stronger for hardcore gamers than for the other segments. Hence, as Steiner et al. (216, p. 290) stress, "modelers who do not account for customer heterogeneity will estimate average effects".

Question 2 – What are the boundaries of the market?

As explained in Section 2, when assembling their dataset Madureira et al. do not only collect yearly data for 33 countries; they also collect data points "for various economic sectors and geographic regions" within these countries (2013, p. 249), and subsequently put all these observations together in one sample.

This approach raises two related concerns. First, is it appropriate to mix observations collected at different levels? Second, do Madureira et al. consider the relevant level(s)? In both cases, the key question is: what drives individual utility? Is it the number of Internet users within the sector, regionally, and/or nationally? Perhaps the network effects are (also) situated at the European or even the global level?[8] While the answer may well differ depending on the capability, the issue would seem to be of an "either/or" nature. Consider the capability of "cooperatibility", which is operationalized as the "fraction of enterprises that have ordered products or services via the Internet" (Madureira et al., 2013, p. 250). Suppose that EU companies frequently engage in online sourcing outside their own country and sector, but never outside the EU; if this is the case, then the EU level is the (only) relevant level, which Madureira et al. do not consider. Internet penetration at the country level, or lower, would not be a correct metric because there are foreign enterprises that add value.

[5] The Eurostat database that Madureira et al. use has a separate indicator for the "percentage of individuals using the Internet for consultation with the purpose of learning".

[6] The Eurostat ICT database also has a separate indicator for the percentage of "individuals who have used Internet, in the last 3 months, for doing an online course".

[7] Note the link with Question 1.

[8] For example, where "selectibility" is concerned, which is proxied by the fraction of enterprises using internet search engines, companies might benefit from finding information from all over the world, provided that they master the required languages. Conversely, in the 3D Hubs example the relevant market is distinctly local. It is no coincidence that the 3D Hubs website has maps with printer locations *per city*.

Therefore, my answer to Question 2 is as follows. First, observations relating to different levels should not be mixed, as this will result in an underestimation of the true strength of the network effect. Second, given that selecting the relevant level is essentially an empirical issue, it is vitally important to acquire an accurate understanding of the "boundaries" of the network/market, perhaps based on survey evidence[9]. In the absence of such direct indications, an attempt to identify the relevant installed base could be made by testing the level for which the fit of the regressions is strongest[10].

Question 3 – Should absolute numbers or relative network sizes be used?

Most authors are not faced with the question of "to scale or not to scale" because they examine a network that is supposedly global (Metcalfe, 2013; Boudreau and Jeppesen, 2015; Zhang et al., 2015, for the Facebook case; Garcia-Swartz and Garcia-Vicente, 2015) or because they use data for a single country (Ohashi, 2003[11]; Zhang et al., 2015, for Tencent).

In the setting of Madureira et al., it depends on the answer to Question 2 as stated above. If the relevant market is at the EU or global level (and if only observations at the corresponding level are used), there is obviously no problem. As long as only a single market is considered, absolute numbers and penetration rates will, by and large, dovetail[12]. But if markets are national and if multiple countries are considered, as Madureira et al. do, then the question does arise.

However, according to Madureira et al., the question is irrelevant in practice. As explained in Section 2, Madureira et al. claim that replacing absolute (n) by relative numbers (x) affects only the proportionality factor. In the working paper version of this article (Van Hove, 2016b), I show that this is correct within Madureira et al.'s setup and logic, but only because they assume that I, the potential maximum size of the DIN, is the same for all observations. In a multi-market setting, this obviously clashes with reality; hence, testing Metcalfe's law with absolute or relative data does make a difference. The intuition is straightforward: scaling with a constant, as Madureira et al. do, is not, in fact, scaling at all.

Given that the choice matters, at least in a multi-market setting, the next question is which is best: absolute or relative? On closer scrutiny, this may well depend on the type of network/capability. For communications and social networks, it would seem that users essentially value market penetration. The penetration rate of LinkedIn, for example, determines the probability that a new professional contact is also on LinkedIn, and thus determines whether or not the service is sufficiently efficient for managing one's contacts. Just as for e-mail, for example, this would seem to point towards the use of scaled numbers, provided that the borders of the business networks of the bulk of the users coincide with the borders of their country or sector; in other words, provided that the setting is multi-market rather than European or global. Another useful way of thinking about the issue is to think in terms of users' maximum utility or maximum willingness to pay, that is, their utility in a scenario where literally everyone in the country has e-mail or a LinkedIn account. Is this maximum utility, ceteris paribus, higher in larger countries than in smaller ones? If the answer is no, scaling is imperative.

Following this line of thinking, for other networks/capabilities, absolute numbers may well be more appropriate. Consider a wiki[13] and suppose that people value information only in their own language and/or written by people in their country or sector. Suppose, furthermore, that across countries/sectors, individuals are equally likely to contribute to the wiki and that their entries are of equal quality. Then, the collective knowledge produced by the citizens of a more populous country/sector will be larger, and thus more valuable. In such a setting—and provided that the above assumptions apply—scaling would be incongruous.

Upon perusing papers that estimate network effects in a multi-market setting without testing any of the "network value laws", in particular papers concerning the telecom sector, I found that all but one effectively scale network sizes; see, amongst others, Liikanen et al. (2004), Suarez (2005) and Karaçuka et al. (2013)[14]. The exception is Fuentelsaz et al. (2015), who compute the value of the networks of 65 mobile operators in 20 European markets and do so based on a self-developed measure that, oddly enough, uses absolute subscriber numbers. If, as argued above, subscribers value market penetration, such an unscaled measure is biased against networks that are active in small markets (Van Hove, 2016c).

Focusing again on the Madureira et al. article, note that their answer to Question 3 is not consistent, in that they work with scaled data when testing Metcalfe's law, but use absolute numbers when testing Briscoe's law. This obviously ties in with their argument that the choice is irrelevant; however, as argued, the choice *does* matter. In my partial re-examination of Madureira et al.'s regressions in Section 4, I therefore, in the absence of clear indications either way, attempt to use both scaled and unscaled data.

This said, Madureira et al.'s use of a scaled *dependent* variable also is not neutral. Indeed, given that y_c is the *fraction* of enterprises or individuals who have a certain capability, the maximum value for y_c, which is a proxy for the value created, is the same in all countries. Hence, in view of my argument about the link between the size of a country and maximum utility, the use of a scaled variable would seem justifiable only if the size of the network—the variable on the right-hand side—is also scaled. In my re-examination, I therefore also experiment with a combination of unscaled dependent and independent variables, even though such

[9] Note that in two-sided markets these boundaries need not be the same on both sides. The utility of a PlayStation 4 console for an American gamer, for example, depends on the games that are available for the PS4 platform in the U.S. This needs not be the same selection of games as in Japan, for example. From this angle, studying the U.S. video games market in isolation, as Corts and Lederman (2009), Zhu and Iansiti (2012) and Cennamo (2016) do, can be justified. However, the actors on the software side may well have a more global view; that is, when selecting platforms certain game developers probably look at the installed base of users in multiple countries. This is not taken into account in the studies mentioned. In specific cases, the scope of the market can also evolve. With respect to video games, Corts and Lederman (2009) show that over time the relative importance of games that are released on more than one console has increased; consequently, cross-platform network effects have come to complement platform-specific effects.

[10] I owe this suggestion to an anonymous referee. Following this line of reasoning, it would be interesting to replicate the Facebook tests of Metcalfe, Zhang et al. and Van Hove with national data, in order to check whether Facebook is not in effect "multi-domestic" rather than global; that is, whether the Facebook networks of most users are not predominantly national, so that the network effects mainly work at a national level. There is anecdotal evidence of countries where the growth in the number of Facebook users has surpassed the overall growth rate (Source: "Facebook on course to reach 1bn users", *Financial Times*, July 22, 2010). Note that Tencent is a different case; because it is available only in Chinese, it is essentially a national phenomenon, even though it is of use to Chinese abroad.

[11] Ohashi (2003) studies the VCR market in the U.S.

[12] That is, abstracting from population growth.

[13] Wikipedia—itself a wiki—defines a wiki as "an application, typically a web application, which allows collaborative modification, extension, or deletion of its content and structure".

[14] Interestingly, in two of these papers there is a clear link with Question 2. Suarez (2005) analyzes the technology choices made by mobile operators in 47 countries in North, South, and Central America during the period 1992–2001. Suarez finds that "when choosing which [2G] technology to purchase, cellular operators tend to pay more attention to decisions made previously by other operators in a selected subset of countries with which they have strong ties than to the overall situation in the world" (2005, p. 716; my italics). In other words, in Suarez's setting the relevant market proves to be "supra-national", rather than either national or global. Conversely, Karaçuka et al. (2013), in a paper on Turkey, find that consumers' choice of mobile network is affected by operators' market shares at the *province* level rather than at the national level.

an unscaled dependent variable also has its drawbacks, as I explain in my answer to Question 5.

Question 4 – Are there disturbing factors that need to be controlled for?

An implicit assumption behind Metcalfe's law is that, aside from the number of users, the network itself does not change; that is, the technology remains the same and the operator continues to offer the same service(s). If the network being studied is, for example, a network of fax machines, this assumption can likely be ignored without creating major problems. Intertemporal improvements in quality, if any, are probably minor.

However, social networks, for example, are different. Compared to 10 years ago, Facebook now offers a wider variety of services, which are arguably of better quality; thus an increase in the value created by Facebook may not only be the result of the company's spectacular growth in terms of number of users. This is why in Van Hove (2016a) I explicitly incorporate a quality indicator into Zhang et al. (2015)'s tests for Facebook and Tencent. In particular, I compute a "cost per (active) user" or "cost per node" (CPN). The key assumption, which can be criticized, is that increases in this CPN are indicative of improvements in quality. I find that the inclusion of such a quality variable as an additional driver of network value does not invalidate Zhang et al.'s conclusions, on the contrary.

Mobile phone networks are another case in point. In a paper on the U.S. wireless industry, Weiergräber (2014) uses the average satisfaction level of a carrier's customers in the region to control for improvements in local coverage quality. In a study on Rwanda, Björkegren (2015) exploits direct information on the location of cell towers. Studies on the video games industry increasingly try to take into account not only the number of games that are available for a platform, but also their variety and quality (Cennamo, 2016). Quality is typically captured by counting the number of 'hits' (i.e., best-selling titles) and proves to be very important (Stremersch et al., 2007; Corts and Lederman, 2009; Lee, 2013; Cennamo, 2016).

Question 5 – Which point of view? Network owner or individual users?

Aside from possible methodological problems, this is in fact a non-issue. Although in the Conclusion I offer a number of reflections on which approach would seem to be more promising, in principle an attempt to explain either aggregate network value or individual utility could be made. Madureira et al., however, would seem to have a consistency issue, in that they do not take the same point of view on both sides of their equations. On the left-hand side, they measure, as a proxy for *individual* utility, certain ways that people/enterprises use the Internet. In other words, they take the point of view of the members of the network—the users. Logically, the right-hand side of their regressions should thus feature the size of the network that matters for users; that is, x_i rather than x_i^2. Indeed, while under Metcalfe's law aggregate network value grows quadratically, individual utility grows linearly; see Fig. 3a in Choi and Lee (2012).

Unlike Madureira et al., Metcalfe (2013) and Fuentelsaz et al. (2015) take the point of view of network owners[15], Facebook and mobile operators, respectively. It is therefore no coincidence that they have on the right-hand side of their regressions *aggregate* network value (respectively, n^2 and $n\log(n)$). But for Madureira et al.'s individual members, aggregate network value is *not* what matters. A quadratic specification, as in Madureira et al.'s Eq. (2), also dramatically alters the scales among countries[16]. All this explains why, in my re-examination below, my preferred specifications are models with individual utility on the right-hand side.

4. Madureira et al.'s results: a re-appraisal

In this section, I apply as many of the amendments suggested above as possible to a selection of Madureira et al.'s data and compare the outcome with their original results.

4.1. Data

As mentioned, a peculiarity of Madureira et al.'s set-up is that they do not collect data points only at the national level, but also for "various economic sectors and geographic regions" (2013, p. 249). Their article does not provide details, but email exchanges with lead author Antonio Madureira made clear that "regions" does not refer to the observations at the supranational level (EU28, etc.) that appear in the publicly available Eurostat data, but rather to various (and varying) breakdowns of the national figures[17].

Aside from the problem of identifying exactly which breakdowns were used for which capability, I decided not to use sub-national figures and to limit the analysis to the data points at the national level mainly for two reasons. First, it seemed unwise to mix different-level observations; see the discussion of Question 2. Second, if a choice needs to be made, the national level would seem to make the most sense. Indeed, given that Madureira et al. examine how individuals and companies use the Internet, it is difficult to argue that markets would have sub-national delineations[18]. Conversely, it would have been interesting to alternate country- and European-level observations, but for the European aggregate there are simply not enough observations[19].

Given this preference for national-level data, I decided to limit the analysis to the two capabilities that relate to individuals, namely biddability (operationalized as the fraction of individuals who have used the Internet for selling goods) and adoptability (proxied by the fraction of individuals who have used the Internet for training and education). The rationale behind this decision is that for the capabilities that relate to enterprises, Madureira et al. include not only regional observations but also data points at the level of sectors within a country. As a result, the differences between their dataset and a dataset comprised solely of national-level data would be too big.

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[15] The same is true for the follow-up papers on Metcalfe.

[16] This can be illustrated as follows. According to the Eurostat data used by Madureira et al., in 2009 Internet access among households happened to be identical in Italy and Cyprus, with $x_i = 53\%$. If consumers value market penetration and if the relevant market is essentially national in nature, then in 2009, access to the Internet was equally valuable for citizens of both countries. Using aggregate network value—individual utility times the number of members—would give a completely different picture. To see this, we need data on n_i; that is, on the number of households in Italy and Cyprus. Such numbers are not readily available on a cross-country basis, but estimates can be obtained by dividing the total population by the average number of persons per household (Source: Eurostat, Population and Social Conditions, http://ec.europa.eu/eurostat/web/main/home, last accessed 06.07.2016). Based on such estimates, roughly 13.8 million Italian families had access to the Internet in 2009 vs. 183,000 in much smaller Cyprus. Hence, the aggregate value of the internet would have been proportional to $13{,}836{,}472 * 0.53 = 7{,}333{,}330$ in Italy and $183{,}640 * 0.53 = 97{,}329$ in Cyprus. Clearly, the huge difference—a factor of roughly 75—reflects only the difference in market size. Aggregate network value is an adequate measure of the value to be reaped by Internet Service Providers, for example, but it is not as if access to the Internet was 75 times more valuable for the average Italian than it was for the average Cypriot.

[17] A recent article (Madureira et al., 2016) more clearly talks about "regional and sectoral breakdowns".

[18] For example: one of the breakdowns in the Eurostat database used by Madureira et al. splits up households depending on whether they live in densely populated, intermediate urbanized, or sparsely populated areas. It is improbable that people living in a sparsely populated area in, for example, Italy, use the Internet to sell goods *only* to people living in other sparsely populated areas in Italy.

[19] Madureira et al.'s dataset spans just 8 years, and due to missing values I would, in practice, have three observations for adoptability and a maximum of seven for biddability. This said, the two capabilities that I examine relate to households (as discussed later in the main text) rather than to enterprises. For households, it is less evident that the "right" level is the European level; see Section 4.2.

4.2. Results

Tables 1a and b summarize the results of my partial re-examination of Madureira et al.'s estimates. For each capability and law, I first restate Madureira et al.'s results; for example, concerning adoptability and Metcalfe's law, see row (M.a) in Table 1a. (M stands for Metcalfe and B stands for Briscoe.) In the second row, I have re-estimated the models for my more limited samples (which contain only country-level observations). Concerning biddability, this straightforward replication is reassuring: both the coupling strengths and the R^2s are similar or very similar (and the relative standard deviations (RSDs) are actually lower). For adoptability, the differences are larger—especially concerning Briscoe's law—but then the difference in the number of observations is substantially larger as well. The third row presents more estimations of Madureira et al.'s models, but for an even smaller sample, in order to have the best possible point of comparison for the tests that follow.

From the fourth row onwards, I progressively apply the amendments suggested in Section 3. Before discussing the results, I recapitulate the amendments and explain whether and how I have applied them.

In my answer to Question 1, I argue that for some of the capabilities the network effect seems to be of the indirect type, which would call for a more specific measure of the network size on the right-hand side (rather than just the total number of households/enterprises that have access to the Internet); however, I could not conceive of a way to implement this for the two capabilities that I examine. For "adoptability", a more specific indicator does not exist, as far as I know. For "biddability", there may well be two sides to the market—buyers and sellers—but the problem is that many of the individuals who sell via the Internet probably also are buyers.

Question 2 relates to the geographical dimension. Concerning biddability, although cross-border online shopping is on the increase in the EU, according to the European Commission "the Internet is used to make purchases mainly from sellers or providers based in the respondent's own country"[20]. In 2012, only 15% of EU consumers had purchased once or more times from an online seller/provider in another EU country in the previous 12 months[21]. Gomez-Herrera et al. (2014) use data from an online consumer survey in the EU27 to investigate whether geographical distance still matters for e-commerce of physical goods. They find that distance-related trade costs are greatly reduced, but that socio-cultural variables such as language increase in importance. Gomez-Herrera et al. conclude that, on balance, "there are no indications that home bias is less significant online than offline" (2014, p. 84). For adoptability, the home bias could be smaller because we are considering pure information products, but here too the linguistic and cultural fragmentation of the EU market may play a role.

Turning to Question 3, in order to be able to scale I needed data on the number of households per country, which are not readily available; however, estimates can be obtained by dividing total population by the average number of persons per household[22]. Unfortunately, for the latter statistic there are several missing values, resulting in a substantial loss of observations. As can be seen in Table 1b, for biddability the number drops from 178 in row (b) to 124 in row (d). In order to make sure that differences in the results are not caused by differences in sample size, I perform all regressions with overlapping sets; see row (c). In specification (d) I first scale the network size variable on the right-hand side in a different way compared to Madureira et al. Indeed, in Van Hove (2016b) I demonstrate that Madureira et al. make a mistake when formulating Metcalfe's and Briscoe's laws in relative terms. In specification (f), I use an unscaled network size variable, but I combine this with an unscaled usage indicator on the left-hand side.

Given the wide range of countries covered and the many service facets involved, I did not see a straightforward way to implement the suggestion proffered in Question 4 to control for improvements in quality over time, even though there is little doubt that between 2002 and 2009, for example, average Internet connection speed increased substantially in many countries.

Finally, in line with my answer to Question 5, in specifications (g) to (j) I have replaced aggregate network value by individual utility. As announced, I try both scaled and unscaled dependent and independent variables; but my preferred specifications—indicated by boxes in column 3—are those in which both are either scaled or unscaled.

Turning to the results, a first observation points precisely to the remark just made: models with inconsistent scaling—specifications (e), (h), and (i)—almost invariably give poor results, which is reassuring. In 10 of the 12 cases, the R^2s lie in the range 0.16–0.43, and the only real exception is the 0.86 for specification (B.i). I discard all these specifications and do not discuss them any further.

Concerning the choice between aggregate network value and individual utility – as discussed in Question 5, the results seem to favor the latter. To draw this conclusion, specifications (d) and (g), and (f) and (j) need to be compared. In five of the eight cases, the specification with individual utility on the right-hand side yields substantially better results; in one case—(M.f) vs. (M.j) in Table 1b—the results are virtually equal (with R^2s of, respectively, 0.94 and 0.91), and in two cases the specification with aggregate network value performs best, because specification (j) performs badly for Briscoe's law.

This brings us to the key question: which "law" performs best? In the tables, I have visualized the results of the pairwise (M.z)-(B.z) comparisons by putting the highest R^2 in bold each time. At first glance, the answer would seem to depend on the capability that is examined: for adoptability (in Table 1a) Briscoe's law has the highest number of bold R^2s; for biddability (in Table 1b) it is the opposite. However, if the inconsistent specifications (e), (h), and (i) are discarded, as well as Madureira et al.'s specifications (a), (b), and (c), which I believe to be misspecified, and, in particular, if we concentrate on my preferred specifications (g) and (j), the picture changes completely. Indeed, looking at the boxed R^2s, it becomes apparent that Metcalfe's law consistently outperforms Briscoe's law, and to a substantial degree in three out of four cases.

Also, even though my samples are smaller, my preferred specifications for Metcalfe's law tend to have lower RSDs and/or higher R^2s than Madureira et al.'s specification (a): for adoptability I have an R^2 of 0.91–0.92 vs. 0.85 for Madureira et al.; for biddability the corresponding values are 0.76–0.91 and 0.86. This could be seen as providing indirect support for my criticisms. Finally, if we continue to concentrate on Metcalfe's law, concerning biddability, my unscaled specification (M.j) outperforms the scaled variant (M.g)[23]. For adoptability there is hardly any difference.

[20] Source: European Commission, "Consumer attitudes towards cross-border trade and consumer protection", *Flash Eurobarometer*, Nr. 358, June 2013, p. 16.

[21] There are, however, countries where more consumers have purchased from a site located in another EU country than from a site in their own. This indicates that the relevant market can actually differ from one country to the next.

[22] Note that, to the best of my knowledge, these data are available only at the national level. This constitutes a third, pragmatic reason to use only national-level data. Note also that data on the number of active enterprises (not used in my regressions, as explained above) can be found in the Eurostat Structural Business Statistics. See http://ec.europa.eu/eurostat/web/structural-business-statistics for data on 2008-2012. Data for earlier years can be found at http://appsso.eurostat.ec.europa.eu/nui/show.do?dataset=bd_9a_l_form (last accessed 22.10.15).

[23] This suggests that for individuals selling goods on the internet, the absolute number of potential buyers matters.

Table 1a
Estimates of coupling strengths: comparison of tests, adoptability.

	Test	Source	Explanation (changes compared to Madureira et al.)	n	k_c	RSD_{k_c} (%)	R^2
Metcalfe's law							
(M.a)	$y_{c,i} = k_{c,M} x_i^2$	Madureira et al. (2013)	Eq. (2)	220	0.68 ± 0.05	7	0.85
(M.b)		This paper		97	0.79 ± 0.03	4	0.86
(M.c)		This paper		87	0.79 ± 0.04	5	0.84
(M.d)	$y_{c,i} = k_{c,M} n_i x_i$	This paper	Q3 – RHS variable scaled differently	87	3.3E-14 ± 0.5E-14	16	0.31
(M.e)	$y_{c,i} = k_{c,M} n_i^2$	This paper	Q3 – RHS variable unscaled; Eq. (1)	87	7.1E-6 ± 1.8E-6	25	0.16
(M.f)	$yabs_{c,i} = k_{c,M} n_i^2$	This paper	Q3 – RHS variable unscaled + LHS variable unscaled	87	4.9E-8 ± 0.4E-8	7	0.69
(M.g)	$y_{c,i} = k_{c,M} x_i$	$\boxed{\text{This paper}}$	Q5 – individual utility on RHS	87	0.55 ± 0.02	3	$\boxed{\mathbf{0.91}}$
(M.h)	$yabs_{c,i} = k_{c,M} x_i$	This paper	Q5 + Q3 – individual utility on RHS + LHS variable unscaled	87	1.1E+5 ± 0.2E+5	15	**0.35**
(M.i)	$y_{c,i} = k_{c,M} n_i$	This paper	Q5 + Q3 – individual utility on RHS + RHS variable unscaled	87	2.5E-6 ± 0.4E-6	15	0.35
(M.j)	$yabs_{c,i} = k_{c,M} n_i$	$\boxed{\text{This paper}}$	Q5 + Q3 – individual utility on RHS + RHS variable unscaled + LHS variable unscaled	87	1.31 ± 0.04	3	$\boxed{\mathbf{0.92}}$
Briscoe's law							
(B.a)	$y_{c,i} = k_{c,B} x_i I ln(x_i I)$	Madureira et al. (2013)		220	19.7E−12 ± 0.2E−12	1	**0.99**
(B.b)		This paper		97	9.9E−12 ± 0.3E−12	3	**0.91**
(B.c)		This paper		87	9.7E−12 ± 0.3E−12	3	**0.91**
(B.d)	$y_{c,i} = k_{c,B} n_i \frac{ln(n_i)}{ln(l_i)}$	This paper	Q3 – RHS variable scaled differently	87	2.5E−6 ± 0.3E−6	15	**0.35**
(B.e)	$y_{c,i} = k_{c,B} n_i ln(n_i)$	This paper	Q3 – RHS variable unscaled	87	1.4E−7 ± 0.2E−7	16	**0.33**
(B.f)	$yabs_{c,i} = k_{c,B} n_i ln(n_i)$	This paper	Q3 – RHS variable unscaled + LHS variable unscaled	87	0.08 ± 0.00	3	**0.91**
(B.g)	$y_{c,i} = k_{c,B} \frac{ln(n_i)}{ln(l_i)}$	$\boxed{\text{This paper}}$	Q5 – individual utility on RHS	87	32.1 ± 1.4	4	$\boxed{0.86}$
(B.h)	$yabs_{c,i} = k_{c,B} \frac{\ln(n_i)}{\ln(l_i)}$	This paper	Q5 + Q3 – individual utility on RHS + LHS variable unscaled	87	6.6E+5 ± 1.0 + 5	4	0.33
(B.i)	$y_{c,i} = k_{c,B} ln(n_i)$	This paper	Q5 + Q3 – individual utility on RHS + RHS variable unscaled	87	2.13 ± 0.09	4	**0.86**
(B.j)	$yabs_{c,i} = k_{c,B} ln(n_i)$	$\boxed{\text{This paper}}$	Q5 + Q3 – individual utility on RHS + RHS variable unscaled + LHS variable unscaled	87	4.8E+5 ± 0.6E+5	13	$\boxed{0.40}$

Notes: n = number of observations; RSD = relative standard deviation. Q3 and Q5 refer, respectively, to Question 3 and 5 in Section 3.

Table 1b
Estimates of coupling strengths: comparison of tests, biddability.

	Test	Source	Explanation (changes compared to Madureira et al.)	n	k_c	RSD_{kc} (%)	R^2
Metcalfe's law							
(M.a)	$y_{c,i} = k_{c,M}x_i^2$	Madureira et al. (2013)	Eq. (2)	191	0.19 ± 0.03	16	**0.86**
(M.b)		This paper		178	0.21 ± 0.01	3	**0.84**
(M.c)		This paper		124	0.23 ± 0.01	4	**0.86**
(M.d)	$y_{c,i} = k_{c,M}n_i x_i$	This paper	Q3 – RHS variable scaled differently	124	$1.2E{-}6 \pm 0.1E{-}6$	10	**0.46**
(M.e)	$y_{c,i} = k_{c,M}n_i^2$	This paper	Q3 – RHS variable unscaled; Eq. (1)	124	$2.8E{-}14 \pm 0.4E{-}14$	14	0.29
(M.f)	$yabs_{c,i} = k_{c,M}n_i^2$	This paper	Q3 – RHS variable unscaled + LHS variable unscaled	124	$2.1E{-}8 \pm 0.0E{-}8$	2	**0.94**
(M.g)	$y_{c,i} = k_{c,M}x_i$	$\boxed{\text{This paper}}$	Q5 – individual utility on RHS	124	0.14 ± 0.01	5	$\boxed{\mathbf{0.76}}$
(M.h)	$yabs_{c,i} = k_{c,M}x_i$	This paper	Q5 + Q3 – individual utility on RHS + LHS variable unscaled	124	$3.3E{+}4 \pm 0.5E{+}4$	15	**0.27**
(M.i)	$y_{c,i} = k_{c,M}n_i$	This paper	Q5 + Q3 – individual utility on RHS + RHS variable unscaled	124	$7.9E{-}7 \pm 0.8E{-}7$	10	0.43
(M.j)	$yabs_{c,i} = k_{c,M}n_i$	$\boxed{\text{This paper}}$	Q5 + Q3 – individual utility on RHS + RHS variable unscaled + LHS variable unscaled	124	0.46 ± 0.01	3	$\boxed{\mathbf{0.91}}$
Briscoe's law							
(B.a)	$y_{c,i} = k_{c,B}x_i Iln(x_i I)$	Madureira et al. (2013)		191	$2.6E{-}12 \pm 0.2E{-}12$	12	0.83
(B.b)		This paper		178	$2.4E{-}12 \pm 0.1E{-}12$	4	0.76
(B.c)		This paper		124	$2.5E{-}12 \pm 0.1E{-}12$	5	0.76
(B.d)	$y_{c,i} = k_{c,B}n_i \frac{ln(n_i)}{ln(l_i)}$	This paper	Q3 – RHS variable scaled differently	124	$8.1E{-}7 \pm 0.8E{-}7$	10	0.43
(B.e)	$y_{c,i} = k_{c,B}n_i ln(n_i)$	This paper	Q3 – RHS variable unscaled	124	$4.6E{-}8 \pm 0.4E{-}8$	11	**0.42**
(B.f)	$yabs_{c,i} = k_{c,B}n_i ln(n_i)$	This paper	Q3 – RHS variable unscaled + LHS variable unscaled	124	0.03 ± 0.00	3	0.93
(B.g)	$y_{c,i} = k_{c,B}\frac{ln(n_i)}{ln(l_i)}$	$\boxed{\text{This paper}}$	Q5 – individual utility on RHS	124	7.0 ± 0.6	8	$\boxed{0.54}$
(B.h)	$yabs_{c,i} = k_{c,B}\frac{ln(n_i)}{ln(l_i)}$	This paper	Q5 + Q3 – individual utility on RHS + LHS variable unscaled	124	$1.7E{+}6 \pm 0.3E{+}6$	19	0.19
(B.i)	$y_{c,i} = k_{c,B}ln(n_i)$	This paper	Q5 + Q3 – individual utility on RHS + RHS variable unscaled	124	0.36 ± 0.04	12	**0.63**
(B.j)	$yabs_{c,i} = k_{c,B}ln(n_i)$	$\boxed{\text{This paper}}$	Q5 + Q3 – individual utility on RHS + RHS variable unscaled + LHS variable unscaled	124	$1.2E{+}5 \pm 0.2E{+}5$	16	$\boxed{0.24}$

Notes: n = number of observations; RSD = relative standard deviation. Q3 and Q5 refer, respectively, to Question 3 and 5 in Section 3.

5. Concluding remarks

This paper has scrutinized the empirical literature on Metcalfe's law and its alternatives, with the goal of laying bare the key choices that need to be made when testing these "laws". I present the lessons learned in the form of answers to five questions, and apply some of the suggested amendments to Madureira et al. (2013)'s validation efforts.

After making these modifications, I find markedly different results. Madureira et al. conclude that, overall, their results are "qualitatively the same irrespectively of using Metcalfe's law or Briscoe's adaptation of it" (2013, p. 55). For adoptability, Briscoe's law even shows a better fit. Conversely, for the two capabilities that I consider—one of which is adoptability—I find that Metcalfe's law performs better. This finding comes with four caveats. First, I have been able to run tests for only two of Madureira et al.'s nine capabilities. Second, it remains uncertain at what level the tests should be conducted—regional, national, or supra-national. Third, my tests inherit Madureira et al.'s implicit (and unproven) assumption that, for a given capability, the proportionality factor between network value and network size is identical across countries[24]. Finally, I take Madureira et al.'s (2011, 2016) Holonic Framework as a given.

More generally, this paper shows, if anything, that examining network value is far more complex than a simple expression such as Metcalfe's law conveys, especially in a multi-country and/or multi-sector setting. Also, I concur with Karaçuka et al. when they write that "[i]n the analysis of network effects on consumer choice both industry-level and firm-level studies utilize what has been called 'macro empiricism' [...], inferring individuals' preferences from the observation of aggregate market behaviour" (2013, p. 335–336)[25]. The main takeaway thus appears to be that future efforts would benefit greatly from additional "preparatory research"; that is, research that would help toward making well-founded choices concerning, for example, the geographical boundaries of the market. Surveys of whether users value market penetration or absolute numbers would also be welcome, as would research that examines people's willingness to pay – why not by means of choice experiments, as in Sobolewski and Czajkowski (2012) or Steiner et al. (2016).

The quote from Karaçuka et al. is also an excellent stepping stone to the highest-level question that the present paper will probably trigger among readers: Which of the two branches of the literature that have been identified is the most promising? Should one try to proxy and explain aggregate network value, as in Metcalfe (2013), Zhang et al. (2015), and Van Hove (2016a), or should one instead try to test Metcalfe's law at the root and focus on individual consumer utility, as in Madureira et al. (2013)? In fact, both types of indicators have drawbacks.

Using operator revenues to proxy aggregate network value is particularly problematical when the monetization is indirect and relies, for example, on advertising (as in the case of Facebook). Also, as argued in Van Hove (2016a), if the company behind the network offers a variety of services, it may prove necessary to filter out revenues that are not driven by network externalities, and thus do not obey Metcalfe's law. Tencent, for example, derives substantial revenues from e-commerce transactions (ibid.). The available data may not always allow for filtering out such revenues sufficiently accurately.

Data on usage would seem intrinsically better suited, but can also have downsides. By simply tallying the *number* of users, as Madureira et al. do, one does not capture their full willingness to pay; one measures the number of economic agents whose willingness to pay exceeds the cost involved[26]. There may, however, be better options. One possibility would be to exploit on-line product ratings, as Liu et al. (2015, p. 680) do in their paper on-line video games: "The literature shows that these ratings are a useful measure of consumption experience and satisfaction, which directly relate to consumption utility that underlies network externalities [...]. As various Internet mediums make online product ratings widely available, they provide an exciting opportunity for research on network externalities". However, given that not all users post ratings, their representativeness could be called into question. Also, as Liu et al. themselves point out, "a larger installed base might affect user ratings through a social proof effect (i.e., there are a lot of people playing, so it must be good)" (2015, p. 689).

The use of (individual-level) big data of another nature may be more informative. Björkegren (2015) has comprehensive transaction data from Rwanda's dominant mobile phone operator covering a period of 4.5 years and no fewer than 5.3 billion transaction records in total. Crucially, 99% of the accounts are prepaid, so that "the person placing a call pays for it on the margin, by the second" (Björkegren, p. 3). In other words, the calling decision reveals that the caller is willing to pay *at least* for the cost of the call, allowing Björkegren to estimate the utility of adopting a phone based on its eventual usage[27]. This would seem to be an ideal setting in which to test Metcalfe's law, particularly since, as pointed out in Section 4, Björkegren's data also allows him to control for coverage.

But, all in all, other lines of research deserve encouragement too. In a world of networks, there is value in examining network value from every possible angle.

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[24] In their case, depending on the capability, across countries *as well as* across (sectors and) regions within a country.

[25] Stremersch et al. (2007, p. 61) acknowledge this explicitly: "We do not have data on consumer utility, because such data can be obtained only through experiments, surveys, or panels". Also: "We were also unable to test the underlying theoretical mechanisms of our model, such as consumers' utility considerations and software providers' profitability considerations' (2007, p. 69).

[26] Revenue-based indicators will typically not capture consumers' full WTP either, unless, that is, under a pay-as-you-go revenue model (or first-degree price discrimination).

[27] Björkegren's approach is similar to that of Ryan and Tucker (2012), who study the adoption of a desktop-based, internal video-calling technology in a large multinational investment bank.

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