Response to editor and reviewer comments

Thanks a lot for revising the paper, which has improved considerably (from its already high level). Apart from the paper's still-not-perfect motivation, there are just a couple of minor things that remain to be adapted. As for the motivation, I would (still) like to see a better argumentation for what the paper adds to our understanding of diffusion processes, which, in my eyes, can be best achieved by discussing some expectations of the relationship between diffusion mechanisms and the considered spatial scale. In light of the magnitude of the required changes, I recommend minor revisions. Please see my detailed responses to the author's responses below.

Thanks again for the constructive comments and suggestions. Really appreciated! Indeed, there is such a theoretical motivation and I agree that highlighting it increases the value of the work. Briefly, this is the Modifiable Areal Unit Problem.

The below only includes the comments raised at the last round. I grouped together the points linked to the theoretical motivation. For clarity, the first round comments are in teal and my responses in black fonts. Then, the second round comments are in red and my responses in blue fonts. I hope the below address all the reviewer comments.

3. Novelty: The study needs to be more explicit about its novel contributions to understanding spatial diffusion mechanisms. Currently, the discussion is too general.

The novelty of the paper lies in the fact that there are hardly any studies that analysed the role of these three spatial mechanisms in the diffusion of a new technology at such granular spatial scales. Importantly, the paper does this by adopting a long time series perspective which captures the commercial beginning of the Web until its maturity. The following paragraph in the introduction highlights this:

> This paper offers such a contribution: an economic geography study revealing the importance of spatial mechanisms for the diffusion of a digital technology that is the Web in the UK at a high level of spatial granularity from the very early days until its maturity (1996-2012). It specifically focuses on the diffusion of commercial websites. It does so by employing a novel source of big data and machine learning algorithms to model the *active* engagement with Web – that is creating and maintaining instead of just browsing a website – during that period and tests how different

> spatial and temporal mechanisms shaped its diffusion. This paper also exemplifies how the combination of data sources and state-of-the-art research methods which escape the traditional social science domain can offer new lenses to geographical research regarding the understanding of technological diffusion.

Reply: I understand the empirical novelty of the paper, which is unquestioned. However, I miss a deeper discussion of why it is important to understand diffusion at such a granular scale and why existing studies' results lack such data, are "insufficient" or require this investigation at a finer scale.

See response below.

4. Motivation: The motivation for the study is unclear. The text raises several issues, but it is unclear which are specific to digital technologies and which pertain to technological diffusion more broadly.

There are three paragraphs in the revised introduction that highlight the paper's motivation. The first paragraph highlights the importance of this work from a stakeholder perspective. The second emphasises the importance of technology and its adoption in economic geography and macroeconomic growth theories. The third one, motivates the paper from the standpoint of digital geographies and connects it to that literature.

> The motivation for this paper lies in the fact that there are various stakeholders who are interested in knowing how new digital technologies diffuse over space and time and use this knowledge to make predictions about the diffusion of related future technologies. As per Leibowicz, Krey, and Grubler (2016), historical studies agree that technologies diffuse differently in terms of times, rates, and geographies and can be driven by related policies (Victor 1993). Meade and Islam (2021) highlight that a variety of actors have a direct interest in gaining such knowledge including network equipment suppliers, network operators, regulatory and local authorities. These processes and their effects vary a lot across scales: although the diffusion of a new technology might not be optimal at a local level, it might be beneficial from a global perspective as it could lead to faster diffusion to less advantaged places (Leibowicz, Krey, and Grubler 2016). Despite the spatial heterogeneity of such diffusion mechanisms and the policy relevance, there is a scarcity of studies analysing the diffusion of new technologies at a detailed geographical level because of the lack of detailed enough data and adequate research methods.
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> This gap becomes even more prominent if we consider the importance of technology and its adoption in economic geography and macroeconomic growth theories. Simply put, differences in technological adoption and sophistication may lead to uneven development as new technologies can increase productivity and allow firms to introduce new products (Solow 1957; Aghion 1990; Kemeny 2011). Linked to the subject of this paper, the Web and, more specifically, commercial websites can be considered as the underpinning technology for the transformative process identified

by Capello, Lenzi, and Panzera (2024) as the *digital service economy*. Importantly, the focus here is not the Web in general, but instead commercial websites. After all, diffusion together with invention and innovation are considered the pillars of technological change (Das 2022). Findings from previous studies illustrated that early engagement with digital technologies such as the Web led to longstanding positive productivity gains for these regions that hosted early adopters (E. Tranos, Kitsos, and Ortega-Argilés 2021). Hence, obtaining a robust understanding of the spatial diffusion mechanisms is crucial in mapping future development trajectories.

Another line of motivation has to do with the very nature of the Web as a digital technology and how it evolved over time. The digital geographies field has debunked premature claims about the Internet's placeless-ness by mapping various of its facets and technological layers (Emmanouil Tranos and Nijkamp 2013), but the lack of relevant long time-series data prevented researchers from observing digital phenomena overtime. Most of these studies focused on its hard infrastructure (e.g. Malecki 2002; Moss and Townsend 2000), on Internet subscribers (e.g. Blank, Graham, and Calvino 2018), or on social media users (e.g. Crampton et al. 2013). Exceptions include early work from Zook (2000), Zook (2000) and Moss and Townsend (1997), which utilised domain name registration data and use it to map and illustrate the spatial footprint of the web content. Pivotal as these contributions have been in understanding the economic geographies of the Internet, they were based on cross-sectional, often spatially detailed, mapping and on short temporal comparisons (e.g. 1993-1998) and did not observe the evolution and diffusion of the Web until its maturity. As we know from the relevant literature, different technologies diffuse at different rates and there is no theory that can explain these differences (Leibowicz, Krey, and Grubler 2016). Hence, mapping and modelling the diffusion of the Web can offer useful insights.

Reply: Ok, I understand these points and especially appreciate the praxis-oriented motivation. What is still a bit thin is why insights from previous studies (which are limited) are not enough and require an updated study at a finer granular level (this is the same as the previous point, so the two can be combined.

See response below.

2. Knowledge Gaps: The current review does not clearly identify the knowledge gaps that this study aims to address. Clarify whether the goal is to confirm known processes with new data and methods or to explore unique diffusion patterns related to websites.

The knowledge gaps have been explicitly highlighted in the restructured literature review section. For instance:

> Although the above processes have been studied in innovation studies, the distancing of economic geographers from research questions about the diffusion of new

technologies and innovations resulted to a knowledge gap regarding the explanatory power of these mechanisms at granular, local scales (Ding, Haynes, and Li 2010).

Although the early adoption and leapfrogging literature has not paid much attention on cities and regions (Yu and Gibbs 2018), the potential local economic benefits justify the need to investigate the dynamics of the web diffusion in different scales in the UK. Hence, the second empirical hypothesis states:

Reply: Please see my previous points concerning the motivation. While this is a valid argument, I still miss a strong claim as to why existing studies leave a knowledge gap and why filling this gap matters.

See response below.

2. Geographical Scale: While the effort to model diffusion curves at a fine geographical scale is commendable, the paper does not sufficiently explain the benefits or additional knowledge gained from this approach.

A new figure (Figure 3) and the following text have been added to address this point.

What escaped from previous studies was exploring whether such a pattern also describes cumulative adoption of new technologies at sub-national scales. To test this, the same logistic function is estimated for each of the 374 LAD. The detailed results are presented in Table **??** in the Appendix. The key finding is that for 342 LAD (91% of all LAD) the $R^2 > 0.9$ for the logistic function, which indicates that even at local scale the cumulative adoption of the Web is characterised by a pattern similar to the one observed for countries. Graphically, all these LAD curves are plotted in Figure **??**. The different colour lines illustrate whether a LAD reached t_0 faster or slower than the UK in total. This heterogeneity is further discussed later in this section.

This is the first time that such cumulative adoption curves have been estimated for areal units as small as the UK LAD. Understanding and modelling the dynamics of technological diffusion and, ultimately, being able to predict diffusion rates at such small local scales can directly support the deployment of infrastructural networks (e.g. 5G) and local technology policies. The above are in line with previous studies exploring the diffusion of web technologies (Papagiannidis et al. 2015), social media (Lengyel et al. 2020) and online shopping (Bakher Naseri and Elliott 2013).

Reply: Ok, that is interesting and relevant and can be partially used to address my comment about the motivation of the study. Yet, what is still not perfectly clear to me is whether the similarity of the findings at the finer spatial level is to be expected or if there are theoretical reasons to expect otherwise. In my eyes, using such a fine granular scale is the central (but not only) contribution of the paper. Motivating it better, i.e., why it matters from a theoretical

point and understanding of diffusion patterns, can elevate the paper's value. Put differently, are the diffusion mechanisms and drivers (spatially) scale invariant? Discussing this briefly would greatly contribute to the paper's message.

I think all the above points can be addressed together, hence I am responding to them here. Indeed, there is a deeper theoretical justification for testing how sensitive the diffusion mechanisms are to the geographical scale of observation and this is the Modifiable Unit Areal Problem. We know that various key theoretical concepts in economic geography are sensitive to the spatial scale at which they are observed, but given the lack of previous studies, we do not know whether the diffusion mechanisms of a new technology are also sensitive to the spatial scale as well. The following paragraph has been added in the introduction.

> The lack of previous granular studies about the diffusion of new technologies deprives us from a reliable prior regarding whether the diffusion mechanisms and drivers are sensitive or not to the spatial scale that the diffusion of a new technology is observed. As per other key concepts in economic geography (Briant, Combes, and Lafourcade 2010; Meliciani and Savona 2015; Andini and Andini 2019; Rosenthal and Strange 2001), diffusion mechanisms can indeed suffer from Modifiable Areal Unit Problem (MAUP): although the adoption of a new technology is at its core an individual process, it is hardly ever observed at this, or at a close enough, scale.

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3. Lack of Hypotheses: To clarify what the paper delivers and what knowledge gaps it fills, I strongly recommend developing a number of hypotheses that will be tested.

The following two hypotheses have been added and the whole paper is now structured around them:

> H_1: The three diffusion mechanisms namely distance, urban hierarchy and the S-shaped pattern of the cumulative level of adoption shaped the diffusion of the commercial Web in the UK even at granular geographical scales.
>
> H_2: The diffusion of the commercial Web is shaped by both early adoption and latecomer effects.

Reply: I do wonder a bit what the alternatives are. I mean, what factors other than the ones named in the hypothesis can be expected to share the diffusion? Please clarify in the text leading to the hypotheses, explaining why these factors can be expected to be dominant.

The below sentence has been added after the description of the three mechanisms:

> The diffusion of new technologies and innovations is driven by three different mechanisms (Grubler 1990; Morrill, Gaile, and Thrall 1988): (i) distance and proximity, (ii) urban hierarchies in the form of a centrifugal forces as new technologies diffuse from core to periphery, and (iii) over time the cumulative level of adoption follows an S-shaped pattern. Although some studies focused on other, case-specific drivers – for example Lengyel et al. (2020) on urban scaling and Bednarz and Broekel (2020) on regional demand – the literature is in agreement that these are the three fundamental diffusion mechanisms.

5. Retail Centers: The sudden shift to retail centers on page 15 is confusing, given that the data pertains to all commercial sites. Clarify the rationale for this focus.

The retail centres discussion has now been moved in the methods sections, so it appears much earlier in the paper. In addition, the following text has been modified in the relevant paragraph:

> To capture the hierarchical mechanism the model includes as predictors a one year lag of website density in London, the largest city in the UK, and a one year lag of the website density in the nearest city. Both variables clearly illustrate the urban hierarchy in the UK. Especially for the OA models, a third predictor is added to capture hierarchy at a much more granular scale: a one year lag of the website density in the the nearest retail centre [1]. As the literature illustrates, retail centres are key components of urban systems Jones (2021). Due to the small sizes of the retail centres, the latter is only relevant for and included in the OA models and not in the LAD models.

Reply: Ok. I would suggest refining the text a bit to make clear that this is an illustration of a specific empirical case, i.e., the consideration of an additional factor, as an illustration of the robustness of the results as well as potential adaptability of the empirical model to consider more factors in other circumstances.

The below sentence has been added in the above paragraph:

> By including this predictor, the model is better tuned towards the spatial characteristic of the OA scale. Hence, it increases the robustness of the model.

[1] The retail centres data have been obtained by the Consumer Data Research Centre, an ESRC data investment, under project ID CDRC 498-01, ES/L011840/1; ES/L011891/1: https://data.cdrc.ac.uk/dataset/retail-centre-boundaries-and-open-indicators.

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