Dear Editor and Reviewers,

Please find attached a report covering our detailed responses to the editor and the reviewer comments. Overall, we feel the paper has been substantially improved as a result of the constructive criticism and suggestions made in the review process, and we would like to sincerely thank both reviewers as well as the editor for that. We have structured this response by listing every comment and immediately following it with a blue box that includes our response.

Responses to Editor

1) The second paragraph in the literature is too long. Consider breaking it into two or three paragraphs based on reclassification of the literature, for example, by substitution and complementarity effects, or by urban and rural areas.

   Response: ET

2) For clarity, I would like to suggest you replace Zipf coefficient by Pareto exponent unless the coefficient equals 1, as you rightfully pointed it out in the paragraph below equation (1). This will help readers avoid some confusions.

   Response: YMI

3) Please clarify in equation (3) whether you want to include time trend delta*t or year fixed effect delta subscript t. You used delta*t in the equation but used delta subscript t in line 234. Tables 4 and 5 say year fixed effects.

   Response: ET

4) Using female labor force participation rate as IV for ICT usage is a bit exotic. Even females stay home as housewives, don't they still need to use phones and internet at home? You did not provide any empirical evidence that female labor force participation indeed increases ICT usage. It would be more convincing if you can find and cite such supportive references.

   Response: ET

5) Table 5: the impact on Zipf coefficient is very small: 0.001 or 0.002. How economically significant is this number in terms of city size distribution? Particularly, since a country uses both internet and fixed line phones and they have opposite, offsetting effects on Zipf coefficient, does this mean at the aggregate level there is no effect on the overall city size distribution? These are subtle questions and you may discuss a bit on this.

   Response: YMI

6) Line 504: you mentioned using absolute number of broadband tests in 2011 as an additional IV, isn't it a direct measure of ICT and similar to the main explanatory variable "broadband tests per capita 2011"?

> **Response: ET**

Responses to Reviewer 1

1) There are two kinds of spatial dispersion: within a city or across different cities. They are conceptually different; there could be decreased dispersion within a city but at the same time increased dispersion across cities. Both types of spatial dispersion can be affected by the advancement of information technology. This study examines the "Zipf coefficient" of an urban system or a city's rank in an urban system, thus it focuses on spatial dispersion across cities. Nothing is said about spatial dispersion within a city (e.g., whether ICT has resulted in more urban sprawl). The authors should make this clear upfront.

> **Response: YMI: maybe add a line or two in the intro where we talk about data?**

2) After equation (1), it'll be useful to add a brief discussion on why the Zipf coefficient measures spatial dispersion. Many readers should have seen the variance, Gini coefficient, or Herfindahl index used as a measure of dispersion. I am not sure how many of them has seen the Zipf coefficient, so it requires some explanation. I also wonder why those other measures are not used.

> **Response: YMI for the text, ET to check if we have estimates for alternative measures**

3) In Table 2, the Zipf coefficient ranges between -2 and -1, but has a mean of -1 and a standard deviation of 0. That seems odd. Also, what is the "St. error for Zipf est." in the second row?

> **Response: ET**

4) On several occasions (e.g., p. 9 and 15), the authors seem to suggest that a valid IV should not affect (or be correlated with) the outcome variable. This is not entirely accurate. A valid IV must be correlated with the endogenous variable. Thus, if the endogenous variable indeed has a causal effect on the outcome variable, the IV must be correlated with the outcome variable. It would be correct to say that an IV would not affect the outcome variable directly and would only affect it indirectly (i.e., through the endogenous variable). However, no direct effect is different from a zero correlation between the IV and the outcome variable. Thus showing a low correlation between the IV and the outcome variable (p. 12, 15, and 18) is not informative about the validity of the IV.

5) In the global study, the use of an IV has reversed the sign of an OLS estimate and made two statistically insignificant OLS estimates become significant. In both case studies, the use of an IV has produced much larger estimates than the OLS estimates. If the authors really trust the IV estimates, it will be helpful to discuss what have biased the OLS estimates in the particular direction. For example, in both case studies, one might suspect that omitted variables could bias the OLS estimates upward. However, surprisingly the IV estimates turned out to be even larger than the OLS estimates. How do we understand this?

6) When using an instrumental variable, it will be helpful to offer some intuition why the IV is correlated with the endogenous variable. For example, why does female participation in labor force affect internet and phone usage? Why does the absolute number of broadband tests in 2011 affect the endogenous variable (Internet speed)? These are not obvious, so some discussion is needed. Also, it might be helpful to show (or simply mention) some of the first-stage regression results to let readers know how the IV and the endogenous variable are correlated. For example, I would like to know whether and to what extent the absolute number of broadband tests in 2011 increased Internet speed. This information helps readers gauge whether the overall IV strategy makes sense.

7) In the two case studies, I am not sure that the normalization of the changed rank is helpful. I would prefer to use the unnormalized change in rank as the left-hand side variable for the OLS regression, because it makes the key coefficient easier to interpret. Normalization allows for the estimation of the quasibinomial GLM, but the GLM results are never discussed. It might make sense to use the unnormalized change in rank as the left-hand side variable, keep the OLS results only, and simply mention the GLM exercise in a footnote.

Responses to Reviewer 2

1) The authors used different instrumental variables for the multi-county, US, and UK analyses. Why is female labor participation not used as a consistent IV throughout the analyses? Is it because of different contexts or limited variation? The US and UK analyses also use similar yet different IV's. Perhaps the authors can further discuss their rationale in changing choices of IV's.

2) In interpreting the empirical results, the authors, to some extent, highlight the potential mechanism in explaining the positive relationship between internet adoption and urban structure. However, there is limited discussion on other interesting findings. For instance, why does fixed telephony have an opposite effect to internet and digital communication technologies?

Response: YMI, maybe an argument based on time/maturity of technology/link to the 2008 paper?

3) Visualizing the patterns in Table 1 in the format of maps may make the information more reader friendly.

Response: ET

4) There are minor formatting issues and typos throughout the paper. For examples, "Government expenditure (Trade (% of GDP)" in Table 4 and "cites" in line 343.

Response: ET