

We would like to thank all three reviewers as well as the editor for their comments. In the following we respond to the comments of all reviewers. In particular the serious concerns of Reviewer A on data validity have led to some significant changes. We would like to thank Reviewers B and C for their in general more positive assessment of our work. The comments of both reviewers have further helped us to clarify and improve the manuscript in various sections throughout the paper as indicated below.

Reviewer A:

| Comments | Response/remarks |
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| <p>1. It seems to me that the information about ownership of mobile phones must be erroneous. The statistics from Eurostat says 76 subscriptions per 100 for UK in 2001. In this data set 44 % of the respondents in London own a mobile phone. The discussion about this low rate on page 11 does not convince me. I think this is a very serious inaccuracy of the data, which means that the results will not be trustworthy.</p> | <p>We admit that there are differences between the EUROSTAT data and the LATS 2001 data. We believe that a main reason for this is due to the sampling method: EUROSTAT data are subscription or sales based data whereas the LATS data are individual based survey data.</p> <p>We considered this issue and whether there are serious biases in our sample. Below are these additional explanations, which are also found in Section 3.2 of the revised paper.</p> |
| <p>2. Even if this had been the situation there is also missing information about the use of the mobile phone, which a think is of importance if they are going to relate it to travelling. Ownership is just an indicator of use, and I'm not sure how good it is.</p> | <p>We agree that there is lack of information especially on the number of hours of mobile phone use, which is considered as one of the limitations of our study. However, as written in the conclusions, "we believe our findings that distinguishing those with and without mobile phone possession reveals differences in travel behaviour are interesting. It might show that the perceived freedom gained by mobile phones reflects itself in travel patterns, independent of the amount of time the mobile phone was actual used."</p> |
| <p>3. Use of PC for work at home is used as an indication of telecommuting in a way I think is rather problematic. People can use their pc at home for work purposes, some for many hours a week, and still</p> | <p>We acknowledge the reviewer's comment and added the definition of what we now call "informal telecommuting" in this paper.</p> <p>"In our study, we define every respondent who uses his PC from home as a telecommuter.</p> |

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| <p>have a daily work trip. Rather than classifying teleworking in three – the variable could be used as continuous in the regression analysis.</p> | <p>Therefore an “informal telecommuter” as defined here might not necessarily replace his work trips, but, for example, he finishes remaining works in the evening from home.”</p> <p>Given that there is a large group of informal telecommuters we believe looking at the travel behaviour of this group is important.</p> <p>We considered using a continuous variable but as explained in Section 3.3 we find a distinction into three groups more meaningful. It distinguishes those just briefly working from home from those doing more significant amount of work from home. See also our argument based on Figure 3 on differences in trips made by no, some much informal telecommuters.</p> |
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1. Data sources

EUROSTAT 2001:

Source: Mobile phone subscriptions or sales (whole UK).

OFTEL 2001:

Source: OFTEL residential survey (whole UK).

“The survey was conducted amongst a representative sample of UK adults.”

ONS-UK 2000-01:

Source: Family Expenditure Survey, Office for National Statistics (whole UK).

“The Family Expenditure Survey (FES) is a voluntary survey of a random sample of private households in the United Kingdom carried out by the Office for National Statistics (ONS).”

LATS 2001:

Source: London Area Travel Survey by Transport for London (TfL). The survey collected information on the regular weekday travels of a representative sample of people living in Greater London. All interviews were conducted on a personal basis, and respondents were asked to fill in a 1-day travel survey.

- **LATS 2001 ALL:** All respondents with age 16 and above are considered including those unemployed with a sample size of 53,020.
- **LATS 2001 SAMPLE:** The sample is filtered to include only the employed respondents aged 16+ reducing the sample size to 27,634.

2. Tables

| Table A1: Mobile phone penetration rate | | | | |
|--|------------|-----------------|--|-------------------------------------|
| EUROSTAT 2001 | OFTEL 2001 | ONS –UK 2000-01 | LATS 2001 SAMPLE (Sample size: 27,634) | LATS 2001 ALL (Sample size: 53,020) |
| 76 | 67* | 47 | 44 | 35 |

Note: * = own or use, 15% uses mobile phone as the main mode of telephony.

| Table A2: Mobile phone penetration rate by Age | | | | |
|---|------------|------------------|-----------------|--|
| Age group | OFTEL 2001 | LATS 2001 SAMPLE | LATS 2001 ALL | <i>Difference between OFTEL and LATS 2001 SAMPLE</i> |
| 15-24 | 83 | 48 [#] | 40 [#] | 35 |
| 25-34 | 84 | 48 | 44 | 36 |
| 35-44 | 78 | 45 | 42 | 33 |
| 45-54 | 70 | 41 | 37 | 29 |
| 55-64 | 59 | 36 | 29 | 23 |
| 65-74 | 41 | 29 | 16 | 12 |
| 75 and over | 13 | 21 | 8 | -8 |

Note: [#] = age 16-24

| Table A3: Mobile phone penetration rate by Income | | |
|--|----------------|------------------|
| Income bracket | ONS-UK 2000-01 | LATS 2001 SAMPLE |
| Top fifth | 66 | 52 |
| Next fifth | 60 | 49 |
| Middle fifth | 52 | 43 |
| Next fifth | 34 | 40 |
| Bottom fifth | 23 | 36 |

Table A4: Penetration rate by Employment type (LATS 2001 Sample)

| Employment Type (Sample size) | Penetration rate |
|-------------------------------|------------------|
| White collar (4503) | 49.25 |
| Admin (2971) | 40.98 |
| Health (3071) | 43.65 |
| Blue collar (4464) | 39.81 |
| Transport related (494) | 44.62 |
| Self employed (41) | 32.79 |

3. Observations

Table A1 presents the mobile phone penetration rate from agencies that conducted the survey including mobile phone. Whereas EUROSTAT data are based on subscriptions or sales data, OFTEL, ONS and LATS are based on individual surveys. ONS and LATS mobile penetration rates are fairly similar, whereas the rates given in EUROSTAT and OFTEL appear significantly different. OFTEL data are, however, only partly compatible as these are data on “possesses or uses” a mobile phone. Note also that the percentage of those using their mobile phone as main mode of telephony is significantly lower (15%). Both LATS and ONS rates are based on surveys interviewing individuals. We therefore suspect that the difference in statistics is partly due to differences between sales based and individual based statistics of mobile phone possession. Sales based data might overestimate possession of actively used mobiles due to multiple ownership of phones whereas individual based data might underestimate possession of mobile phones due to omitting to report the possession of mobiles that are seldom used.

Tables A2 to A4 discuss some sociodemographic characteristics of mobile phone users in our LATS sample. Firstly we note that the extracted working population sample has a slightly higher penetration than the total LATS sample. This is however expected due to income effects on mobile ownership as shown in Table A3. The difference compared to all LATS as well as OFTEL data is fairly constant among younger age groups though decreasing for those near retirement. One might speculate that this is because middle aged and older persons less frequently omit the reporting of their mobile. Especially for the 75+ our sample size is further (as expected) very small though (with 61 out of 27634 aged 75+).

Table A3 further illustrates that the difference in penetration rate between ONS-UK data and the LATS 2001 sample differs between income groups. Whereas LATS data report lower ownership rates for high income groups, ownership in lower income groups is higher. The reasons for this are not fully understood. One might argue that this is partly a London effect where, among those being employed, income might not be as strong a determinant for mobile ownership as in other parts of the UK with on average lower incomes. Table A4 groups ownership by those employment types also subsequently distinguished in this paper. Those with blue collar jobs have lower ownership rates as one would expect according to their income. Our sample of self-employed is too low to conclude that the difference is significant.

Finally, note that in general we would expect to see higher mobile phone ownership rates in our sample compared to the other, whole UK based, data sources used in this section. As discussed, ownership is related to employment and income which is higher in London than in other part of the UK. Further factors likely to favor higher ownership rates in London are network availability, more dispersed travel patterns and family structures.

4. References

EUROSTAT 2001

<http://epp.eurostat.ec.europa.eu/tgm/table.do?tab=table&init=1&plugin=1&language=en&pcode=tin00060>

OFTEL 2001

<http://www.statistics.gov.uk/STATBASE/Product.asp?vlnk=10989&More=Y>

ONS-UK 2001

<http://www.statistics.gov.uk/StatBase/ssdataset.asp?vlnk=5199&More=Y>

Reviewer B:

| Important Comments | Response/remarks |
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| <p>1. Differences in trip frequency between people that have or do not have a mobile phone (figure 3) are small in a numerical sense. The same goes for differences as a consequence of telecommuting (table 2). Although differences may (often) be statistically significant, I have the feeling that the author(s) should make more explicit throughout the text (and maybe also in the conclusions) that differences are relatively small. Moreover, in the case of large samples (as to a certain extent is the case here) small differences often get statistically significant just because of the size of the sample (a sort of ‘illusive’ significance). Do the author(s) think this might also be the case in this article and, if so, what are the implications?</p> | <p>Thank you for this comment. Firstly, we added the significance value for work trips and stated that shopping trips did not exhibit significant difference between those heavy and occasional mobile phone user.</p> <p>Further, in various places we have toned down our description on significant differences if these are not large. We think illusive significance might be an issue for the t-tests on two independent samples but less likely for our regression results. We have added following sentence after our first t-test results:</p> <p>“The unpaired <i>t</i>-test analysis confirms that this difference is statistically significant ($N = 27634$, $t = 4.58$, $p < 0.001$); however, one should possibly be slightly cautious with this and the following t-test results, as our large sample size of two independent samples will easily lead to significant <i>t</i>-values.”</p> |
| <p>2. The author(s) indicate(s) that the data are already quite outdated. It would be nice if the author(s) could elaborate a bit more on the possible implications of improved (mobile) ICT possibilities in recent years on (work) trips made. This could be done in the conclusion section</p> | <p>The reviewer is right to point this out, however, the sentence below is already written in the conclusion part of the paper.</p> <p>“Though one might argue that 2001 data are already slightly outdated, the effects of mobile phone will be more difficult to disentangle in the analysis of surveys carried out nowadays, when mobile phone possession has become almost standard. Some of the effects described in this paper might be general trends in societies where</p> |

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| | <p>communication is increasingly based on mobile phones. Our results might further be of interest for some developing countries where the level of mobile phone possession is nowadays similar to the one in London in 2001.”</p> |
| <p>3. Unfortunately, and as reported by the author(s), the data set does not have any information on how much a person is using the mobile phone (for instance, frequency and duration). The analysis focuses on mobile phone possession. I would like to see some reflection on this limitation in the conclusion section. Overall, it would be advisable to use the conclusion section to reflect more also on other possible limitations of the study.</p> | <p>We acknowledge and include more discussion on the limitations of our study, particularly on the information of mobile phone. We further added following sentences:</p> <p>“A limitation of our study clearly is missing information on how much a person uses his/her mobile. However, our findings show that by distinguishing those with and without mobile phone reveals differences in travel behavior. This suggests that the perceived freedom gained by mobile phones reflects itself in travel patterns, independent of the amount of time the mobile was actually used.”</p> |
| <p>4. In my opinion, section 2.1 is rather unstructured. The section would really improve if the author(s) would make clearer what the focus is and how the different studies mentioned ‘relate’ to each other. Also a short conclusion of the main direction of findings at the end of the section would largely increase the readability and would offer a stronger link between section 2.1 and 2.2 where the hypotheses are presented. For example, what is the main effect of ICT/mobile phone use on different trips made (e.g., substitution, complementarity,..)? What is the general image that can be derived from studies regarding the effect of telecommuting on (the frequency of) different trips? Do we see a large consistency in literature or are there large differences? Etc.</p> | <p>In our revised version we added some introductory remarks to better explain the structure of our review. We further added some comments to link 2.1 and 2.2 to emphasize that we consider 2.2. as the conclusions of 2.1.</p> |
| <p>5. I think the author(s) should explain more clearly/extensively in section 4.1 why count data analysis is not suitable (e.g., Poisson regression)? What is ‘wrong’ with the distribution?</p> | <p>We tested count data but found worse model fit than ordered probit model. We, we further argue that the link with the utility theory for ordered probit models is an argument in favour of our preferred</p> |

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| | <p>model. In 4.1 we write now:</p> <p>“The ordered probit regression is most suitable for modeling with a dependent variable that takes more than two values, where these values have a natural ordering. In contrast to a linear regression model, it does not assume cardinality. We further consider count data analysis (e.g., Jang, 2005) but find worth model fits and unexpected signs for our coefficients. Further, compared to count data analysis, ordered probit models are more in line with behavioural theory as argued by Roorda et al (2010).”</p> |
| <p>6. Section 4.3, p.4 (results regarding telecommuting). In my opinion the results on this page could be written down more clearly and structured (e.g. according to hypotheses, models, part-time versus full time).</p> | <p>We decided to structure the discussion in this order:</p> <ol style="list-style-type: none"> 1. the socio-demographic variables 2. geographical characteristics 3. ICT (mobile phone and telecommuting) <p>In our revised version we did, however, try to associate the results more clearly with the hypotheses made in Section 2.2.</p> |
| <p>7. Section 4.4, second paragraph: The statements made are rather firm, whereas the results in table 5 hardly differ. Also, there does not seem to be a clear pattern in the differences between having or not having a mobile phone. I suggest that the author(s) at least say something about this.</p> | <p>This comment is related to the first comment of the reviewer. Besides softening our talk on significance we modified the statements:</p> <p>“As shown in the cross table analysis in Table 9 (previously Table 5), those who mobile phone heavy users are likely to make more simple tours related to shopping and leisure activities ($N= 33,809$, $t=2.386$, $p < 0.001$)”</p> <p>The result of t-test is enclosed in the sentence to distinguish if it has significant difference.</p> |
| <p>8. Section 4.5, p.3, second paragraph: “The model</p> | <p>The definition of tour complexity is</p> |

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| <p>result also indicates that mobile phone exhibits no significance on tour complexity”. How is tour complexity defined in the analysis in table 7? It seems that the dependent variable consists of three categories (0, 1 and 2+ stops). Only the last interval seems to be representative for complex tours (according to section 4.4, p.5). Why did the author(s) not split up the 2+ category in more separate categories? This may influence the results also regarding the influence of mobile phone possession. According to the number of observations this further categorization seems to be possible. And, why did the author(s) not use count data analysis?</p> | <p>mentioned in Section 4.1 that tour complexity is measured by the number of stops per tour.</p> <p>Regarding number of cut points in the tour complexity models, we actually tested models with more cut points. However, our model results remain largely unchanged (in particular regarding mobile phone). Further, our sample size for 3+stop tours is rapidly decreasing (8.4%).</p> <p>Regarding count data analysis. See our response to comment 5.</p> |
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| <p>More detailed and minor comments</p> | <p>Response/remarks</p> |
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| <p>1. Section 2.1, p.5, line 11: “...particularly <i>on</i> mobile phone...”. <i>On</i> should be <i>of</i>.</p> | <p>According to the reviewer’s comment, the sentence is modified.</p> |
| <p>2. Section 2.1, p.5, line 12: I do not understand the use of ‘however’ at this place.</p> | <p>The previous literature says mobile phone has complementary effect. And the next literature says mobile phone says it has substitution effect and in order to make a contrast the word however is used.</p> |
| <p>3. Section 2.1, p.6, line 14: ‘constraint’ should be ‘constrained’</p> | <p>According to reviewers comment, the word is modified by changing it from constraint to constrained.</p> |
| <p>4. Section 2.1, p.6, last line. “... substitution between travel and ICT is <i>also</i> supported...”. The use of ‘also’ seems to be rather strange compared to the studies described before that seem to point at a complementarity effect.</p> | <p>According to the reviewer’s comment, the sentence is modified by deleting the word also.</p> |
| <p>5. Section 3.1, p.10, line 10: ‘respondent’ should be ‘respondents’</p> | <p>According to the reviewer’s comment, the sentence is modified by replacing respondent with respondents.</p> |
| <p>6. Section 3.1, p.10, line 16: ‘activity’ should be ‘activities’</p> | <p>According to the reviewer’s comment, the sentence is modified by replacing activity with activities.</p> |
| <p>7. Section 3.2, p.11, first line: I think the 44% mentioned in table 1 should correspond with the</p> | <p>We acknowledge this observation and we replaced the Figure 2 in the first draft to</p> |

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| <p>LATS 2001 data presented in figure 2. However, in figure 2 mobile phone possession for hardly any age group exceeds 40 percent. Please explain this difference.</p> | <p>Table 3 in the revised version. Detailed explanations on this significant difference are found on Section 3.2.</p> |
| <p>8. Section 3.2, p.11, line 2-3: “By comparing this to the statistics of the Office of Telecommunications, UK (OFTEL), it reveals a significant difference. Please clarify in the text what the difference is.</p> | <p>We acknowledge this observation and we make some explanations on this significant difference on Section 3.2.</p> |
| <p>9. Section 3.2, p.11, line 3-10: the given explanation is rather unclear. I do not get why people would have answered ‘no’. Did they have to write down both telephone numbers if they wrote down the landline number first and then answered the question about mobile phone possession with yes?</p> | <p>Again, we acknowledge this observation. This is also in connection to the previous comment, this has been revised, and the explanations are found in Section 3.2.</p> |
| <p>10. Section 3.2, p.12: The author(s) may consider to use (other kinds of) gray shadings in figure 2 and especially in figure 3. In black and white the different categories are rather unclear.</p> | <p>According to the reviewer’s comment, the sentence is modified by changing the graph into gray scale shading.</p> |
| <p>11. Section 3.2, p.13, first line: what is the reason for using category 20-30 yrs where figure 2 uses 25-34 yrs? And ‘significantly’ compared to what categories? All other age groups?</p> | <p>Thank you for detecting this typographical error. However, figure 2 has been changed to Table 3 and the discussion is changed a little bit.</p> |
| <p>12. Section 3.2, p.13, line 10-11: “Especially, for the relationship between work trips and mobile phone possession, though, the causal relationship between the two is not clear as argued above”. Please also shortly explain here or refer explicitly as to where ‘as argued above’ is.</p> | <p>According to the reviewer’s comment the sentence is modified and re-phrased: “Especially, for the relationship between work trips and mobile phone possession, though, the causal relationship between the two is not clear as argued in the preceding paragraph.”</p> |
| <p>13. Section 3.2, p.13, line 15: “mobile phone usage”. I think this should be “mobile phone possession”.</p> | <p>We opted to modify the term by changing mobile phone possession to mobile phone user.</p> |
| <p>14. Section 3.3, p.14, line 3: What is the reason for choosing the threshold of 4 hrs?</p> | <p>The reason of choosing the threshold of 4 hours for part time workers is to reflect the overall reduced working time.</p> |
| <p>15. Section 4.1: ordered probit regression is a quite standard technique. Equations and extensive description are not necessary. However, this is also</p> | <p>The reviewer is right to point out that ordered probit regression should be placed in the appendix yet the authors</p> |

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| <p>a matter of taste.</p> | <p>opted to show it in the body of the paper rather than placing it in the appendix for the reason of showing the different models with different dependent variable used and showing the appropriateness of the analysis used.</p> |
| <p>16. Section 4.1, p.18, line 2-5: reference to outline paragraph is incorrect and incomplete</p> <ul style="list-style-type: none"> ○ Indication of section 4.2 refers to actual section 4.3 ○ 4.3 should be 4.5 (regarding effects on tour complexity) ○ A reference to section 4.4 (effects on number of different tour types made) should be added | <p>According to the reviewer's comment, the sentence is modified. Subsection 4.2 is now replaced by 4.3 and 4.3 is replaced by 4.5.</p> <p>However, we think that subsection 4.4 will not be added since it does not use the ordered probit regression.</p> |
| <p>17. Section 4.2: please give some more brief explanations why certain control variables are included in the analysis. What is the expectation? (examples: regarding population density, ethnicity).</p> | <p>According to the reviewer's comment, we have added the following sentences:</p> <p>"The inclusion of these geographical attributes helps in understanding its interaction to trip making."</p> |
| <p>18. Section 4.2: 'Tble 3' should be 'Table 3'</p> | <p>According to the reviewer's comment, the sentence is modified this by changing Tble to Table.</p> |
| <p>19. Section 4.2, table 3, model 2: do I interpret the model correctly in the sense that people aged 16-24 make the most work trips? How could this be explained?</p> | <p>The age group 16-24 has a positive estimated parameter which would mean making more trips but t-value (0.627) says it is not significant. Hence, age group 16-24 does not give any meaning to the generation of work trips.</p> |
| <p>20. Section 4.3, third paragraph p.2 and p.3: explain at places more explicitly why certain relationships occur (e.g. for gender, household type, skin colour, PT, etc.). Only stating that the results are comparable to existing literatures is rather poor (also because no references are given).</p> | <p>In accordance to reviewer's suggestions, references are written with comparable result of socio-demographic characteristics to travel.</p> |
| <p>21. Section 4.3, p.3 (third paragraph): "People who live in Outer London tend to make more trips than those living in Inner London".... How does this relate to the statement "The number of trips increases for a respondent who has destinations</p> | <p>We have revised the sentence to...</p> <p>"Further, those living from Outer London tend to make more total trips than those</p> |

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| <p>within Central London....”?</p> | <p>living from Inner London...”</p> <p>To relate this to respondents who has destination within Central London...</p> <p>Those from Outer London tends to make more trips in Central London than those who are from Inner London.</p> |
| <p>22. Section 4.3, p.3, third paragraph: “For population density we find similar effects as for Inner and Outer London dummy variable. Those living in the most densely populated areas tend to make more leisure trips (Model 2) and more total trips if they leave their house during the day (Model 4)”. ...Did you check for correlations between population density and the variable ‘Inner London’?</p> | <p>We find some typographical error here. Actually, it is Model 3 (and not Model 2) that densely populated areas make most leisure trips.</p> <p>The correlation was checked, hence, we modified the sentence a bit ...</p> <p>“For population density, we find similar effects. Those living in the most densely populated areas, especially in Outer London, tend to make more leisure trips (Model 3) and more total trips if they leave their house during the day (Model 4).”</p> |
| <p>23. Section 4.4, p.2 first paragraph: “On the other hand, HYH tours.....”. What is the contradiction compared to the sentences before ‘on the other hand’?</p> | <p>The sentence is now re-phrased to: While, HYH tours....</p> |

Reviewer C:

| Comments | Response/remarks |
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| 1. This paper contributes some additional evidences regarding the complementary effects of ICT on travel. It enriches the literature by separating and contrasting the effects of different types of ICTs: ...More importantly, the dataset provides a comfortable balance of sub-samples with and without the possession of mobile phones, which is not possible to get today because most people have mobile phones. In this regard, the present study helps us understand what changes mobile phones have exerted on our travel behavior. | We would like to thank the reviewer for his positive comprehensive review, in particular also on the database – we have highlighted the advantage of this slightly outdated database in our review. |
| 2. I spot only one minor mistake: the number of observations for models 1 and 2 in Table 3 should be 27634, not 2763 | According to the reviewer’s comment, the sentence is modified and corrected the mistake by writing 27634 instead of 2763. |
| 2. Perhaps a suggestion: many places in the paper it says: less trips, it should be fewer trips. | According to the reviewer’s comment, the sentence is modified and replaced “less trips” to “fewer trips” . |