We would like to thank the anonymous reader for the time he/she devoted to read the paper extensively and make generous comments and really helpful suggestions. Our responses are provided in detail below.

1. The paper lacks a compelling motivation. There are now many meta-analyses of the minimum wage literature. TABLE 1 from the paper reports six studies. While these are discussed in the literature review, it is not made clear why another meta-analysis of the subject is called for. Or to state it differently, Why did the authors do this meta-analysis given that there were already several meta-analyses published in the literature? This is a really important point and the authors need to address this.

This issue is also mentioned in the first comment (See Referee Report 1) where we explain in detail the motivation of our study. Our paper tries to answer two basic questions. The first aim of the paper is to investigate if there is a publication selection bias in the minimum wage research. The second aim is to investigate the impact of minimum wage on employment. Our paper is based on the methodology developed by Doucouliagos and Stanley (2009). There are differences between our meta-analysis and the other meta-analyses. The differences are found in the time period in which studies are published, in the number of studies included in the sample, in the data used by the studies and in the methodology. We could say that the contribution of our paper to the empirical literature is based on three points: a) the first is to study again the question of the existence of a publication selection bias as the number of studies published during the last five years is growing, b) the second is the use of studies based on time series and panel data over the world in order to study the impact of the minimum wage on employment and c) the third is to test the validity of the results found by Doucouliagos and Stanley (2009) using a greater variety of countries and a different time period. Due to the importance of the point we present again some points developed in that response:

a) Not large but extremely growing recent literature. The impact of minimum wage on employment is a controversial issue in the literature. The searching of the literature provided more than 100 studies in only the five-year period 2010-2014 indicating that the study by Doucouliagos and Stanley (2009) gave a rise towards the direction of re-

investigating the employment effect of minimum wages. Our meta-sample consists of 45 studies published in a journal and we have rejected additional 13 for reasons described in appendix (see table A.2 in the discussion paper). Moreover we found 43 unpublished studies which report at least one estimate of the impact of minimum wage on employment as presented in the appendix of the that response, indicating that in only five years 101 empirical studies on the relationship between minimum wage and employment have been published as journal articles or have been made working papers. This probably implies that Doucouliagos and Stanley's study had an impact on the minimum wage research and created its own controversy.

b) Time period. Minimum wage is one of the more debatable tools of government policy for influencing employment and this issue is currently being discussed in the USA in the prospect of increasing the federal minimum wage. Furthermore, the OECD Employment Outlook (2015) includes a Special section on the role of minimum wages since the onset of the financial and economic crisis in 2008 stating that they added new momentum to minimum wage debates. Therefore, in this frame of strong interest in minimum wages after the crisis, it has a significant interest to see the genuine effect of minimum wages on employment using a meta-sample of the most recent published studies in the post-crisis period.

c) Results. Another aspect that has to be taken into account is that we find evidence of publication selection bias like previous meta-analysis find, but with respect to the genuine effect once this bias is corrected this true effect depends on the effect sizes used. More specifically, in the elasticities meta-sample, consisted of 1.048 estimates, the effect is negative and ranges from -1.7% to -3%, but in the coefficients' meta-sample, combining 484 estimates, the effect alters sign and is slightly positive.

d) The value of replication. One of the purposes of our study is to establish the correctness of a previous meta-analysis on the employment effect of minimum wages using the most recent published in an academic journal studies. This field of research has gained growing attention due to its significance and importance for the entire world. Judging by the extremely growing related literature, the study by Doucouliagos and Stanley seems to have made an impact on minimum wage research. The value of replication as enriched by The

Replication Network (TRN) is to enable researchers to (i) determine the fragility or robustness of previous research findings, and (ii) identify why studies reach different conclusions on the same subject and we hope that our study has something to offer towards this direction.

2. The authors divide estimates into two groups, elasticities and coefficients. However, for both elasticities and coefficients, it is very important that the respective estimates measure the same thing. What is/are the employment variables used in the original studies? Is it ln(total employment)? Is it employment rates? Is it relative employment rates? More elaboration of the underlying dependent variables needs to be provided. If the case cannot be made that the original studies are measuring the same thing, then the author needs to use partial correlation coefficients.

We welcome this comment and are grateful for these suggestions. Indeed, the coefficients don't measure the same thing and in the coefficients meta-sample there is a great diversification in the employment measure used (e.g. employment rate, relative employment, employment to population ratio, hours worked) or in the minimum wage measurement (e.g. minimum to average wage, level of real minimum wage, dummy, minimum wage coverage). Therefore we should (and we will) address this issue by calculating the partial correlation coefficients as you stress in your comment in order to ensure the robustness of our analysis. We will present these in the revised form of this paper.

3. In FIGURE 2, the authors show that the range of coefficient values range from below -20 to above +20. What is the interpretation of these coefficients? What are they measuring?

The extreme value of these coefficients is an issue of measurement. In the first case the estimated coefficient is 22.320. The dependent value is the "change in full-time workers" and the minimum wage measurement is a dummy (if minimum wage exists = 1, otherwise = 0). In the second case the value is -22.458. The dependent value is the "change in hours worked" and the minimum wage measurement is "minimum wage cost gap". We believe that all estimates in a study should be included in the meta-sample so as not to be

bias ourselves. However, the point you raise is important and we will address it by using the partial correlations and we will make robustness checks for the outliers which exist in the literature.

4. In the third full paragraph of the Introduction, the authors state that one of the key motivations for doing this meta-analysis is to determine whether the minimum wage literature has been affected by Doucouliagos and Stanley's 2009 British Journal of Industrial Relations paper (henceforth D&S). If this is intended to be a serious research question, then it should get put to an hypothesis test. How would one do that? One possibility is to divide the literature of original studies into two samples: those that cite D&S and those that do not (and maybe another category for papers that cite Card and Krueger). And then test whether the associated dummy variable is significant. The authors do not do this. I suspect they don't do it because it would be a very large task to include so many papers. Fair enough. But then I don't think they should present the D&S study, and it is possible subsequent effects, as a key motivation for doing the paper because they don't put their paper in a position to directly address this question.

We welcome this comment. It would be very interesting to use a dummy for those studies which that cite Doucouliagos and Stanley and another for those which cite Card and Krueger to test whether the associated dummy variable is significant or not. We will address this issue in a revised version of the paper.

5. (Page 5) The authors state that they restrict their meta-sample to studies published in academic journals. They do not include unpublished papers or papers that appear in other publication outlets. I am not an meta-analysis expert, but I think it is standard practice to include unpublished research along with published research and then include a variable in the multiple MRA that tests whether these sets of studies are different. The problem with omitting unpublished research is that more recent studies will not have had enough time to make it through the publication process and appear as published studies. This is particularly problematic for a meta-analysis that is focused on studying the most recent literature.

The treatment of unpublished studies, especially of the most recent ones, is an important consideration when conducting meta-analysis. We chose to make an extremely extensive research in the literature in order to avoid omitting a study that should be included in the analysis. The reason for including only published in refereed, academic journals is that these studies have gone through the refereeing process and should be of greater quality than the unpublished ones. Additionally, the inclusion of 45 published studies is considered to be quite satisfactory in the related research, especially in a five-year period. It would be interesting though to test if the findings of our study change if it is replicated, especially when some of the 43 unpublished studies (which are presented in the appendix in our first reply) are published. Last but not least, if there is any published study that should be included in the meta-analysis, it is our mistake not to have it in the meta-sample and we will be happy to incorporate it into the meta-sample.

6. TABLE 4 reports four different estimation methodologies. This table is confusing to me. Notice that the first column reports OLS estimates. But the variables have all been weighted by SE. Thus, it is my understanding that this is WLS in the sense of Stanley and Doucouliagos (https://ideas.repec.org/p/dkn/econwp/eco_2013_1.html)? However, I am then confused about Columns 3 and 4. Are the REML and WLS estimators applied to the variables already weighted by SE? What then are the "weights" in the WLS model since the variables are already weighted by SE? It would be very helpful if the authors could explain these procedures in more detail, so the reader could have a better sense if they are appropriate.

This issue has been addressed by another reader as well. Therefore we have to be more specific and not to confuse the reader. In particular, column 1 shows the estimations when we use the benchmark OLS method, column 2 reports the econometric results when we use standard errors clustered at the study level, column 3 presents the results when we use Random Effects, and column 4 reports the results using the Weighted-Least-Squares estimation method. We naturally respect the reader's view on this matter and we will discuss on the estimation methods and what we are doing in more detail in the revised version.

7. Another issue has to do with different numbers of estimates per study. Across the 45 studies, some have as few as one or two estimates. One study has 258 estimates. The average number of estimates per study is 32. With such a wide diversity across studies, one has to be somewhat concerned that the results are driven by a relatively small number of studies with a disproportionately large number of estimates. Havranek and Irsova (2015) suggest in these cases that one weight by the inverse of the number of studies. The authors might give some thought as to whether this is a more appropriate way of handling unbalanced data such as theirs.

This is an issue raised in another comment as well (see Invited Reader Comment on 5th October). As we have already mentioned, we use inverse variance weights in our analysis in order to avoid having biasness in the standard errors. However, it is our purpose to improve the quality of the paper and to ensure the robustness of our analysis we will present additional robustness checks in the revised version possibly applying alternative weights using the number of estimates and/ or regressions including study dummies.

8. In TABLES 6 and 7, the authors interpret their 1/SE term as an estimate of the overall effect of minimum wages on employment. This is not correct. This is the estimated value of the effect of minimum wages on employment when all the explanatory variables take the value 0. For example, TABLE 7 is reproduced below. To recover the original equation with the estimated effect as the dependent variable, one needs to mutiply all the variables (including the constant term) by SE. Then (1/SE) "becomes" the constant term. However, the meaning of the constant term changes depending on which variables are included in the equation. Since the variables in Columns 1– 4 are different, the interpretation of the constant term is different, and thus cannot be a measure of overall effect.

The analysis proposed is correct. Indeed, we agree that the term 1/SE is the estimated value of the effect of minimum wages on employment under the condition that all the explanatory variables take the value 0. In the revised version of the paper we will give more value to the estimate of $_1$ in the PET test of the model $t_i = {}_0 + {}_1(1/SE_i) + v_i$ as the true/ genuine effect of the impact of minimum wage on employment.

Once again we would like to thank you for your comments and your useful suggestions. We are honored by the effort made to understand what we tried to develop.