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ABSTRACT

Unemployment and Health: A Meta-Analysis*

This paper is a meta-analysis on the relationship between unemployment and health. Our meta-dataset is made up of 327 study results coming from 65 articles published in peer-reviewed journals between 1990 and 2021. We find that publication bias is important, but only for those study results obtained through difference-in-differences or instrumental variables estimators. The average effect of unemployment on health is negative, but small in terms of partial correlation coefficient. We investigate if findings are heterogeneous among several research dimensions. We find that unemployment is mostly effective on the psychological domains of health and that short- and long-term unemployment spells equally affect health. Dealing with endogeneity issues is important and, when this is done, the unemployment effects on health are closer to be nil.

JEL Classification: C52, I10, I12, J64

Keywords: unemployment, health, meta-analysis, meta-regression, publication bias

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1 Introduction

The literature on unemployment and its consequences has always be flourishing. The unemployment effects, both in terms of labor market and health, represent a primary interest of the scientific production from various fields (Jacobson et al., 1993; Arulampalam, 2001; Gathergood, 2013; Reine et al., 2013; Fergusson et al., 2014; Kalousova and Burgard, 2014). The recent outbreak of COVID-19 pandemic gave even more relevance to this topic (Donnelly and Farina, 2021; Griffiths et al., 2021; Posel et al., 2021).

Unemployment may impair health. Often, the primary theoretical point put forward is that, without a job which to rely on, individuals lack financial means which are necessary for their living. This lack is likely to turn in lower consumption possibilities, which may affect either their diet or routine habits, leading to a potential worsening of their health (Pieters and Rawlings, 2020). Further, the occurrence of unemployment may lower reservation wages and depreciate the human capital (Arulampalam, 2001; Chan and Stevens, 2001). Jahoda (1982) emphasized that unemployment is a threat not only because it can reduce the financial resources, but also because it may erase a series of non-economic elements which are deemed to be relevant to health. Janlert and Hammarström (2009) stated that the economic deprivation and the lost latent benefits are the two most appropriate and reliable arguments to comprehend the unemployment consequences on health.

Understanding the full picture played by unemployment events is crucial from a policy perspective. When a policy maker sets its objectives and calibrate the aids, it may seek to minimize the trade-off between the moral hazard of the individuals and the depreciation of their health and human capital (Hyslop et al., 2021). Knowing the terms of the trade-off is therefore important, but the pieces of evidence provided by the scientific literature do not always lead to clear-cut conclusions.

In one of the first studies on the health effects of unemployment, Björklund (1985) found blurred results: unemployment was found to impair health in the cross-sectional design, but this effect disappeared in a longitudinal analysis. After controlling for selection bias, Burgard et al. (2007) found a negative effect, with both the self-reported health and the mental health dropping after the job loss. Otterbach and Sousa-Poza (2016) confirmed such results and extended these negative findings also to the physical domain. Álvaro et al. (2019) and Neubert et al. (2019) showed that unemployment lowers mental health scores and increases depression, but part of these impacts were smoothed by controlling for social and psychological mediators, as the self-esteem or the social status.

Marcus (2013) showed that unemployment generates negative spillovers on the partners, who are almost equally impaired as the laid-off worker. In addition, Nikolova and Ayhan (2019) found that the negative effect of unemployment on life satisfaction is mostly due to non-economic costs. Böckerman and Ilmakunnas (2009) and Salm (2009) failed to find negative health effects of unemployment, neither in the mental nor in the physical dimension of health. Bubonya et al. (2017) showed that such unemployment effects are nil also for the partners of the unemployed, contradicting the findings in Marcus (2013). Finally, Johansson et al. (2020) pointed out relevant discrepancies between results from self-reported health and more objective health measures, with the former being much more sensitive to the effects of unemployment compared to the latter.

One of the main challenge in this strand of the literature is to identify the causal effect of unemployment on health. Avendano and Berkman (2014) presented an extensive discussion on how the results may change across different studies because of the employed econometric technique or the type of the sample. Barnay (2016) pointed out that measurements errors are also very likely in this framework. Since health is a complex and multifaceted phenomenon, its definition and its analysis require high quality data which are often unavailable. This has led to the use of subjective and self-reported measures, which are influenced by a series of unobserved factor. For example, the cultural heritage or the way and time in which the questionnaire is administered might play a crucial role for the reply of the interviewee to those questions on which the subjective or self-reported measured are constructed. Furthermore, often individuals' responses suffer from the so called 'justification bias', i.e. the tendency of the individuals to adjust their responses according to the reference category they belong to or to the social expectations relative to the position they are in. In addition, under-reporting due to social stigma is a further crucial contributor to the measurement error problem (Bharadwaj et al., 2017). Finally, another problem is embodied in the structural difficulty to assess in which direction causality runs, i.e. reverse causality, as health deterioration may affect the probability of job loss. Indeed, (Haan and Myck, 2009) found a bidirectional causal effect.

In this paper, we conduct a meta-analysis on the health effects of unemployment. The number of studies on this topic is large and increasing. Meta-analytic tools may be of big help in summarizing bodies of research literature which have grown much. According to Havránek et al. (2020), meta-analysis is "*the systemic review and quantitative synthesis of empirical economic evidence on a given hypothesis, phenomenon, or effect*". It can provide a more objective and rigorous picture than narrative reviews, avoiding the risk of narrative reviews of under-(over-)reporting certain results in favor (at expense) of others (Stanley et al., 2013).

Our meta-analysis is not the first one summarizing the empirical relation between unemployment and measures of health. Paul and Moser (2009) collected results about the relation between unemployment and mental health using studies published between 1963 and 2004. They found a significant negative effect. The size of the effect corresponded to a Cohen's $d = 0.51$, which is a medium size effect (Cohen, 1988). Murphy and Athanasou (1999) computed a smaller effect using the same outcome variable ($d = 0.36$). More recently, Kim and von dem Knesebeck (2016) conducted a meta-analysis on the relation between unemployment and job insecurity and depression. They selected 15 studies published between 2005 and 2014 and with a longitudinal design only. The average effect was negative, with both unemployment and job insecurity increasing the likelihood of developing/exacerbating depressive phenomena. Milner et al. (2013) studied the relation between the long-term unemployment and suicide with a sample of 16 studies. They found that longer unemployment spells are associated with higher odds of suicide, especially within five years after the job loss.

These meta-analyses have the common trait to be either too narrowly focused on a single health dimension or not to check or to weakly check for publication bias and effect heterogeneity. Moreover, they considered unemployment under a broad perspective, by either studying it as a cumulative event or focusing on past spells. The main contribution of our paper is therefore to provide an up-to-date meta-analysis which: i) covers a comprehensive set of health outcomes with a more homogeneous definition of unemployment; ii) checks and corrects for publication bias; iii) analyses eventual sources of heterogeneity among several characteristics of the study results. In addition, in order to avoid criticisms of arbitrary choices in the study selection criteria and in the modelling techniques to aggregate study results, we follow the guidelines of the Meta-Analysis of Economics Research Network (MAER-Net) (Stanley et al., 2013; Havránek et al., 2020). These guidelines are indeed aimed at creating a shared subjectivity in performing meta-analyses in economics and improving therefore transparency, replicability and quality of the reported meta-analytic results. Hence, the ultimate goal of our meta-analysis is to provide a comprehensive picture on the relation between unemployment and health, so as to be the reference for policy-makers and future scholars.

This article is organized as follows. Section 2 describes how we built the meta-dataset, the effect size, and the result characteristics for the analysis of the effect heterogeneity.

Section 3 faces the problem of publication bias. Section 4 explores effect heterogeneity and reports the main findings. Section 5 concludes.

2 Meta-dataset

The literature on unemployment effects is quite heterogeneous in the definition of unemployment. We tried to make the analysis as homogeneous as possible by focusing on those studies which defined unemployment as the current status/situation of the individual, i.e. *current unemployment*. Hence, we removed those studies in which the treatment is cumulative unemployment occurrences or unemployment events in the past, independently on the status of the individual at the moment of the interview (see e.g. Fergusson et al., 2014; Kalousova and Burgard, 2014; Strandh et al., 2014).

We further realized that the definition of *current unemployment* was not always the same among studies. In some cases, the definition of unemployment according to the International Labour Organization (ILO) was not followed. In order to avoid losing observations because of sticking to a rigid definition of unemployment, we chose not to discard those studies not in line with the ILO definition of unemployment.

Unemployment is a disruptive event which may impair the health not only of the laid-off worker (Green, 2011; Schmitz, 2011; Gathergood, 2013), but also of individuals who are close to the one who suffered the unemployment event. Hence, we also included in our meta-dataset those studies which investigated the intra-household spillover effects of unemployment on health (Marcus, 2013; Powdthavee and Vernoit, 2013; Pieters and Rawlings, 2020). Including them may enlarge the view on the effects that unemployment exerts on individuals' life and the life of their relatives and may shed more light on the socio-economic costs of unemployment.

Finally, we only considered studies which employed microdata and were aimed at finding evidence at individual level.

2.1 Search strategy and selection criteria

Our meta-analysis follows the MAER-Net guidelines (Stanley et al., 2013; Havránek et al., 2020). These guidelines set a benchmark to reduce the subjectivity during the 'picking' and 'analysis' phases.

Between October 2021 and December 2021, we searched for studies published from 1990 until 2021 using one scientific research engine and four scientific databases: Google Scholar, Web Of Science (WoS), Scopus, Science Direct and IDEAS/RePEc. We started our search with Google Scholar with a combination of the following keywords: ('unemployed' or 'unemployment' or 'parental unemployment' or 'partner unemployment' or 'spouse unemployment') and ('well-being' or 'health'). We obtained 142 results. Then, we filtered them according to the following steps:

1. we retained only research articles written in English and published in peer-reviewed journals, excluding therefore working papers, book chapters, reports, and thesis;
2. we removed articles not having the words 'unemployed' or 'unemployment' in the title;
3. we applied an 'abstract screening' to retain only studies on the impact of *current unemployment* on health.

After this 'preliminary text screening' (PTS), we were left with 53 papers, which moved to the next stage, i.e. the 'full text screening' (FTS).

We repeated the same PTS in WoS with the only difference that we searched only in journals belonging to the following subject categories: *Economics*, *Health Policy Services*, *Social Sciences Interdisciplinary*, *Psychology Social*, *Psychology Multidisciplinary*, *Management* and *Industrial Relations Labor*. After removing duplicates, i.e. articles already obtained using Google Scholar, we were able to add 45 new papers to the 54 obtained using Google Scholar.

We moved on using the same PTS in Scopus, Science Direct, and IDEAS.[1] After removing duplicates, we added further 30 articles from Scopus, 7 from Science Direct and 6 from IDEAS, for a total of 141 studies admitted to the FTS.

Figure 1 presents the PRISMA Flow Diagram (Moher et al., 2009) describing our search strategy. The PTS and FTS stages are depicted in the top part of the diagram. About the PTS, the rolling procedure is highlighted in the north-west part of Figure 1. The first column of boxes presents the gross number of studies that we obtained from each source. The second column presents instead the number of articles left after the PTS and removing duplicates. The dashed lines show how the duplication check worked. Each

[1] In both Scopus and Science Direct, the subject categories are defined differently. We searched papers in the following four subject categories: *Social Sciences*, *Psychology*, *Economic Econometrics and Finance* and *Business Management and Accounting*.

search of studies obtained using the 4 scientific databases was compared with the previous ones in order to remove duplicates.

The pointed grid on the north-east part of Figure 1 visually explains the steps of the FTS. We began with the exclusion of those studies which we judged as flawed from the methodological point of view. For example, papers which based their conclusions on the comparison of simple unconditional means for the treated and the untreated individuals or on path modelling (see e.g. Schwarzer et al., 1994; Lai et al., 1997; Taris, 2002; Houssemand and Meyers, 2011; Fors Connolly and Gärling, 2022).

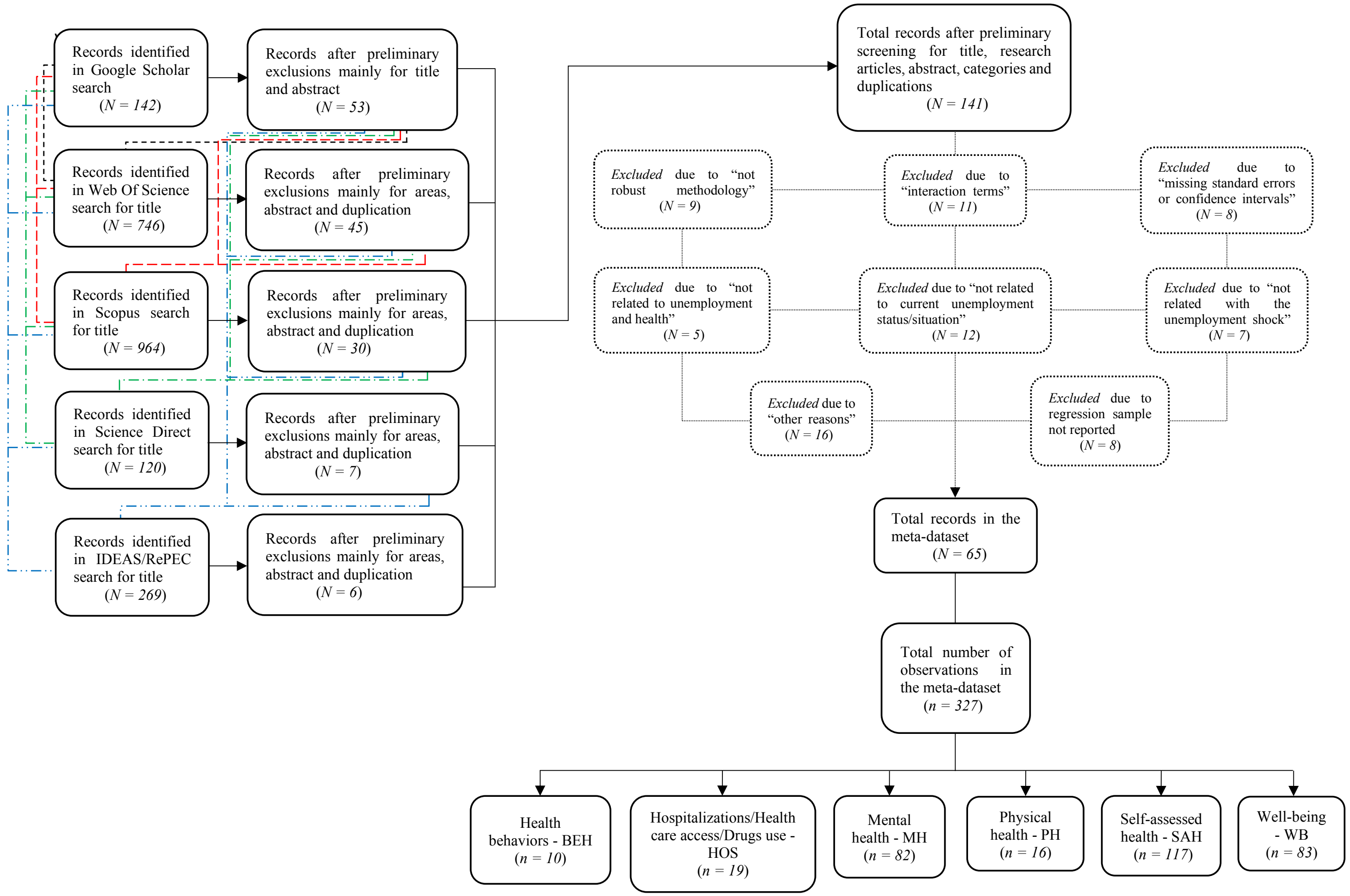
We excluded also studies in which the model specification presented interactive terms on the coefficient(s) of interest (Stanley and Doucouliagos, 2012), because this is problematic for recovering the effect size for the whole population and for the group(s) identified by the interactive term(s) (Clark, 2003; Knabe and Rätzel, 2011; Dolan and Powdthavee, 2012; Álvaro et al., 2019; Howley and Knight, 2021).

We dropped those studies for which the computation of the t-statistic (or the z-statistic) was not possible due to unreported standard errors and confidence intervals (Theodossiou, 1998; Stauder, 2019; Taht et al., 2020). Moreover, we excluded those papers in which the treatment was not the *current unemployment* (de Goede and Spruijt, 1996; Fergusson et al., 2014; Kalousova and Burgard, 2014; Strandh et al., 2014; Backhans et al., 2016; Lam and Ambrey, 2019), did not have health outcomes as dependent variables (see e.g. Lindström, 2009; Plessz et al., 2020), or the treatment was not unemployment (Hamilton et al., 1997; Axelsson and Ejlertsson, 2002; Hald Andersen, 2009; Hamilton et al., 2015; Lee et al., 2021).

We removed further 16 articles for 'other reasons', i.e. because the empirical analysis was not at the micro-level (Monsef and Shahmohammadi Mehrjardi, 2018), the effect size of unemployment was not computable for reasons different from those mentioned above (Kozieł et al., 2010; Sousa-Ribeiro et al., 2014; Sage, 2015; Crost, 2016), or the analysis was conducted over a sample of only unemployed individuals (Korpi, 1997; Strandh et al., 2013; Takahashi et al., 2015).

Finally, we dropped 8 studies because they did not contain information on the sample size, which is fundamental to compute the effect size of each study result (Beland et al., 2002; Breslin and Mustard, 2003; Sleskova et al., 2006; Cooper et al., 2008; Stavrova et al., 2011; Milner et al., 2016; Buffel, Missinne and Bracke, 2017; Colman and Dave, 2018). We indeed opted for the partial correlation coefficient (r) as a measure of the effect size and, for its computation, the sample size is needed.

Figure 1: PRISMA flow chart



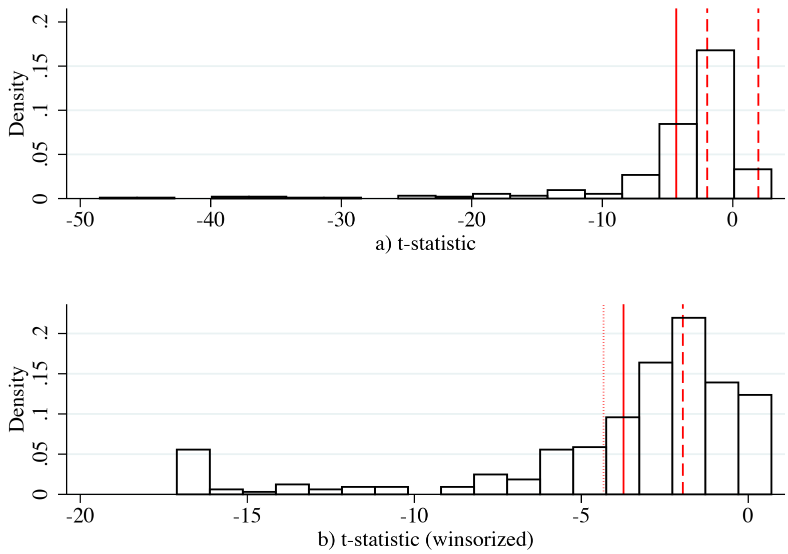
Notes: N indicates the number of articles. n is the number of study results. The left-hand side of the diagram focuses on the PTS. The right-hand side explains the selection criteria and the sample reduction during the FTS.

In the end, we were left with a final sample of 65 studies for a total of 327 results. Table A.1 in appendix reports all the articles with the associated relevant information. The number of study results is more than 5 times larger than the number of selected articles because one study may contain several results for different reasons. For example, one study may estimate the impact of unemployment on multiple health outcomes or on a given health outcome for different subpopulations (e.g. by gender or by country in multi-country studies).

2.2 Effect size

For each of the 327 study results in our meta-sample, we computed the corresponding t-statistic either as taking the ratio between the β coefficient and its standard error or by applying a suitable transformation of the odds (or hazard) ratios whenever the estimated effects came from nonlinear models (Altman and Bland, 2011). Overall, 189 results (57.8% of the total) pointed to a statistically significant negative effect of unemployment on health; for 136 observations (41.6%) the effect was nil; in only 2 cases (0.6%) the effect was positive.

Figure 2: Density plot of t-statistics of study results



Notes: In panel a) the red dashed lines refer to the standard thresholds for the 5% significance level (i.e. ± 1.96), whilst the red solid line refers to the average value. In panel b), the red dotted line indicates the average value in panel a). The averages for the winsorized and not winsorized t-statistics are -3.733 and -4.332, respectively.

Panel a) of Figure 2 displays the density distribution of the t-statistics. We set the t-statistic to be negative (positive) whenever unemployment was found to have a negative (positive) effect on health. The distribution of the t-statistics in panel a) of Figure 2 clearly presents extreme values. This raises concerns, because outlying observations might generate systematic distortions in a regression analysis (Zaman et al., 2001), like the one undertaken in this paper. A way to prevent distorted results due to deviant observations is winsorization, i.e. the correction of the extreme outliers with a chosen value selected from a specific threshold of the cumulative distribution function of the variable of interest (Xue et al., 2021). We therefore applied the winsorization of the t-statistics at the 5th and 95th percentiles of their distribution. Panel b) of Figure 2 reports the winsorized density plot of the t-statistics.

The average t-statistic, equal to -3.733 in its winsorized form, suggests that the conclusions of studies on the effect of unemployment on health are significantly negative on average. However, the t-statistic does not convey information on the magnitude of the effect. We decided therefore to move on from it in favor of a more suitable choice, the partial correlation coefficient (r) (Rosenthal and DiMatteo, 2001). Contrary to the t-statistic, the partial correlation coefficient conveys information on the size of the effect of interest. Since r is a correlation coefficient, it is bounded between -1 and 1.

There is another relevant reason why we decided to measure the effect size by the partial correlation coefficient and not simply using the estimated coefficients as reported in the selected studies. Our study results are heterogeneous for several reasons. The estimated effects come from models with different specifications. Some models are linear in the estimated parameters, some others are nonlinear. The set of covariates included in the equations to be estimated varies among studies. Although we selected only articles with a similar interpretation and definition of unemployment, there are discrepancies in the definition of the treatment variable. Last but not least, the measure of health is very dissimilar among studies, because both the scale may be different given the same kind of health measure and the health measures vary from mental health measures to physical health measures, from objective measures of healthcare utilization, like number of doctor visits, to subjective scales of self-perceived general health. These differences imply that almost each study result has its own interpretation, which is not directly comparable to the others. The partial correlation coefficient (r), being a unit-free measure retaining information on the magnitude of the effect, restores comparability among study results (Rosenthal, 1991) and it is now largely employed in meta-analyses in the economic literature (see e.g. Doucouliagos, 1995; Doucouliagos and Laroche, 2003, 2009; Xue et al., 2020, 2021; Filomena and Picchio, 2022; Picchio, 2022).

The partial correlation coefficient is computed according to the following formula:

$$r_i = \frac{t_i}{\sqrt{t_i^2 + df_i}}, \tag{1}$$

where t_i is the t-statistic for study result i and df_i denotes the degrees of freedom of the model from which result i was retrieved. The standard error of the partial correlation coefficient is

$$SE(r_i) = \sqrt{\frac{1 - r_i^2}{df_i}}. \tag{2}$$

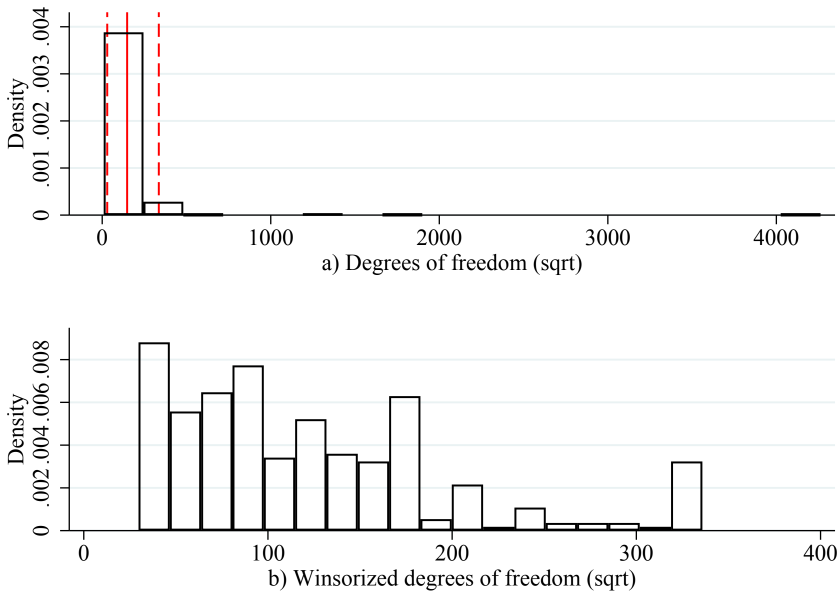
Although the use of the partial correlation coefficient allows comparability of the study results and it is informative about the strength of a correlation, it has nonetheless a limit. Its size is not able to quantify real economic phenomena. Doucouliagos (2011) tried to shed light on when a partial correlation is large by looking at the empirical distribution of thousands of correlations from different kinds of economic studies. He suggested that partial correlations above 0.33, between 0.33 and 0.17, and between 0.17 and 0.07 may be named as ‘large’, ‘medium’ and ‘small’, respectively.

Equation (1) clarifies that for computing the partial correlation coefficient only two ingredients are needed: the t-statistic and the degrees of freedom. In most cases, the t-statistic can be extracted without problems, as the estimated parameters and their standard errors are almost always reported. Nevertheless, sometimes this is not the case, for example because the authors reported the estimated parameter along with its 95% confidence interval, the p-value or the p-value being smaller than a certain value. In some other case, the author may have reported the risk/odds/hazard ratio between the treated and the untreated units as the estimated effects. We dealt with these special cases as explained in Picchio (2022, § 3.3.3).

The other ingredient for the computation of the partial correlation coefficient, the degrees of freedom, may be problematic to retrieve in many cases. Whereas the sample size to get the corresponding estimate of the treatment effect is almost always reported in published papers, the exact number of estimated parameters is often unclear. The number of estimated parameters is indeed rarely declared and, in some cases, it is also not possible to retrieve it indirectly, because the full set of estimation results is not displayed. We did our best to recover the degrees of freedom when they were undeclared. In those few cases in which we were not able to do it, we approximated it with the sample size minus 2.[2] In our sample the minimum value of df is 76, while the maximum is about 18 millions. Similarly to the winsorization based on the distribution of the t-statistic, we performed a winsorization on the degrees of freedom df. Figure 3 shows the plots for the squared root transformation of df for both the winsorized and not winsorized versions.

The effect size used in what follows, i.e. the partial correlation coefficient r, was computed using the winsorized versions of both the t-statistics and the degrees of freedom.[3] Since we set the t-statistic to be negative (positive) whenever unemployment was found to exert a negative (positive) effect on health and, as Equation (1) shows, the sign of the partial correlation coefficient is determined by the sign of the t-statistic, a negative (positive) value of r_i is to interpreted as unemployment negatively (positively) affecting health. Figure 4 plots the density of the partial correlation coefficient. According to the rule of thumb suggested by Doucouliagos (2011), our dependent variable mostly assumes a small effect size.

Figure 3: Density plot of the square root of the degrees of freedom

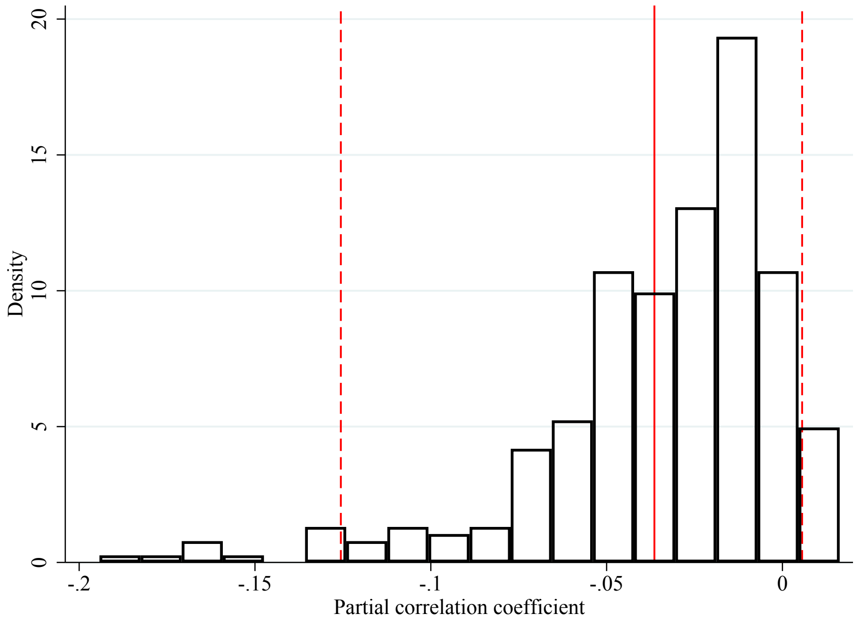


Notes: In panel a) the red solid line is the average and the two red dashed lines are the 5th and 95th percentiles, respectively.

[2]In microeconometric applications the sample size is typically much larger than the number of estimated parameters. Hence, the calculation of r_i is very robust in errors and approximations in deriving its df_i.

[3]Appendix C presents the results of a sensitivity analysis in which the winsorization is applied at the 1st and 99th percentiles of the distribution of the t-statistics and degrees of freedom as in Xue et al. (2021).

Figure 4: Density plot of the partial correlation coefficient (r)



Notes: The red dashed lines are the 5th and 95th percentiles, respectively. The red solid line is the average value (-0.033).

Table 1 reports summary statistics of the partial correlation coefficient distinguishing by different types of health outcomes. Since health is a complex and multifaceted phenomenon, in selecting study results for our meta-sample, we were as inclusive as possible in terms of health outcomes and grouped them in six broad categories. The last column of Table 1 shows the number of study results for each health category.

Table 1: Summary statistics of the partial correlation coefficient (r) by health outcome

	Mean	Median	Std. Dev.	Min.	Max.	Obs.
Overall	–0.0334	–0.0253	0.0346	–0.1942	0.0163	327
By type of health outcome						
Health behaviors (BEH)	–0.0199	–0.0181	0.0310	–0.0742	0.0160	10
Health care utilization (HOS)	–0.0294	–0.0329	0.0260	–0.0795	0.0152	19
Mental health (MH)	–0.0475	–0.0351	0.0428	–0.1942	0.0079	82
Physical health (PH)	–0.0193	–0.0101	0.0441	–0.1734	0.0143	16
Self-assessed health (SAH)	–0.0238	–0.0181	0.0230	–0.1113	0.0163	117
Well-being (WB)	–0.0384	–0.0352	0.0342	–0.1672	0.0079	83

The most numerous category is self-reported or self-assessed health (SAH), accounting for 35.8% of the observations. In this category, we grouped results based on health collected by asking individuals a general assessment on their own health, mostly using a 5-point Likert-scale, or if they were suffering from general self-reported chronic illnesses.

The second most numerous group (25.4%) is made up of results which assessed health using well-being scores (WB). Most of the time, individuals were asked to rank, using an 11-point scale, how much they were satisfied about their life. Headey et al. (1993) assessed that although life satisfaction is not strictly conceivable as health, it displayed a strong correlation with the mental health dimension without being collinear with it.

The next category contains results on mental health (MH, 25.1% of the observations). The way in which mental health is measured is heterogeneous, going from self reported scores on mental distress, anxiety or depression to more structured and composite indexes aggregating several variables.

The three remaining and least populated categories are healthcare utilization (HOS, 5.8%), physical health (PH, 4.9%), and health behaviors group (BEH, 3.1%). In the HOS category, we grouped study results measuring health using information on hospitalization, access to health care services, or drug prescriptions. Into PH group, we reported study results whose outcome variable was a measure of physical health, like for example the Body Mass Index (BMI), the levels of C-reactive protein or having suffered from a stroke. Finally, the BEH category contains results whose outcome variable is a health behavior, as for instance diet habits and alcohol or tobacco consumption.

The overall average of the partial correlation coefficient suggests that the unemployment effect on health is negative, but fairly small. The average effect size is somewhat different among different types of health measures. Mental health and well-being measures display the highest average, although the size of the average partial correlation coefficient still suggests that the relation is weak. Physical health shows instead the lowest value. Therefore, the psychological domains of health seem to be the most exposed to unemployment, whilst the physical side looks the least affected.

2.3 Descriptive statistics of the covariates used in meta-regression analysis

One of the main aims of our meta-analysis is to understand the sources of effect heterogeneity among different characteristics of studies/results. Technically, we do it using meta-regressions: the effect size is regressed on a set of study or result characteristics which are, based on theoretical arguments, likely to determine the sign and the magnitude of the effect size. Table 2 reports descriptive statistics for the covariates we employed to dig into this issue.

As already anticipated in the previous subsection, one of the dimensions across which we distinguished the results is the type of health outcomes. The next dimension is the identification strategy used for estimating the health effects of unemployment. Different identification assumptions and different estimation methodologies may play a relevant role in the estimation of the causal effect of unemployment on health (Avendano and Berkman, 2014), given the endogeneity of unemployment due to unobserved heterogeneity determining both unemployment and health, reverse causality, and measurement error. Different assumptions require different identification strategies, which employ different estimators, leading to different conclusions (Brodeur et al., 2020). In our sample most of the studies used the control function approach (CFA) or fixed effects (FE) strategy, which account for the 31.5% and 34.9% of our sample, respectively. The next two strategies often employed are difference-in-differences and duration models, which amount to 11.0% and 6.4% of the sample, respectively.

As unemployment may be more harmful in areas with a weaker welfare system, we included controls for the geographical area which the study refer to. About 65% of the study results refer to European countries. Because older workers' job separations are often a one-way street into unemployment, we controlled for the average sample age, which is 40 years on average.[4] In addition, since single-breadwinner models based on gender still persists in some societies and the event of unemployment may be differently perceived by men and women, we included a regressor for the gender of the sample. Overall, almost 70% of the observations come from samples made up of both men and women and 15% of the study results come from female samples.

We coded the presence of controls for income or previous health in the regression analysis. Their inclusion in modelling the relation between unemployment and health is important because they can net out spurious components induced by liquidity constraints or by state-dependence effects. In our sample, less than 35% of the observations come from studies which controlled for state-dependence health effects, while in less than 45% of the observations the analyst controlled for income.

We also decided to investigate the effect heterogeneity by the length of the unem-

[4] In some studies the average age of the sample is not declared. We deal with this missing information by coding at 0 the average age of the sample and by including in the meta-regression analysis also a dummy equal to 1 if the average age of the sample is missing (and 0 otherwise).

Table 2: Summary statistics of the covariates used in meta-regression analysis

	Variables	Observations	Mean	Std. Dev.
(1)	*Health measures*			
	Health Behaviors (BEH)	327	0.0306	0.1724
	Health care utilization (HOS)	327	0.0581	0.2343
	Mental Health (MH)	327	0.2508	0.4341
	Physical Health (PH)	327	0.0489	0.2160
	Self-Assessed Health (SAH)	327	0.3578	0.4801
	Well-Being (WB)	327	0.2538	0.4359
(2)	*Identification strategy*			
	Control Function Approach (CFA)	327	0.3150	0.4652
	Difference-in-Difference (DiD)	327	0.1101	0.3135
	Duration Models (DM)	327	0.0642	0.2455
	Fixed Effects (FE)	327	0.3486	0.4773
	Fixed Effects Instrumental Variables (FEIV)	327	0.0245	0.1547
	Instrumental Variables (IV)	327	0.0397	0.1957
	Mixed Models (MM)	327	0.0183	0.1344
	Random Effects (RE)	327	0.0428	0.2027
	Propensity Score Matching (PSM)	327	0.0367	0.1883
(3)	*Geographical area*			
	European area (EU)	327	0.6453	0.4792
	Non-European area (NON-EU)	327	0.2966	0.4575
	Multi-country (Multi)	327	0.0581	0.2343
(4)	*Sample age controls*			
	Sample average age if available	230	39.9297	8.4339
	Sample average age tot available	327	0.2966	0.4575
(5)	*Relevant controls in regression analysis*			
	Health controls	327	0.3486	0.4773
	Income controls	327	0.4434	0.4975
(6)	*Gender*			
	Men + Women	327	0.6942	0.4615
	Men	327	0.1529	0.3604
	Women	327	0.1529	0.3604
(7)	*Duration of unemployment*			
	Short-term unemployment ($\leq$ 12 months)	327	0.1529	0.3604
	Long-term unemployment ($>$ 12 months)	327	0.1407	0.3482
	Duration not specified	327	0.7064	0.4561
(8)	*Reason for unemployment*			
	Exogenous (e.g. plant closure)	327	0.0887	0.2847
	Endogenous (due to worker's behavior)	327	0.0214	0.1450
	Not specified	327	0.8899	0.3135
(9)	*Relation with the unemployed*			
	Herself/himself	327	0.9511	0.2160
	Other (i.e. parent/partner)	327	0.0489	0.2160
(10)	*Business cycle and labor market status*			
	Average GDP growth rate	327	0.0175	0.0232
	Average unemployment rate	327	0.0893	0.0384
(11)	*Study quality*			
	Yearly average Google Scholar citations	327	12.7104	12.6496
	SJR index	327	1.4158	1.0543
(12)	Year of publication	327	2013	6.2026

ployment spell. Since the longer the unemployment spell, the higher the depreciation of human capital and the tighter the liquidity constraints, we expect that the negative health effects of unemployment may be increasing in its duration (Becker, 1962; Grossman, 1972, 2000). We coded the duration of unemployment into three categories: short and long, following the ILO definition,[5] and a third residual category for those study results which did not provide information about the duration of the unemployment event.

Job separations may happen for different reasons. Only 11% of our observations come from studies in which the reason why a person became unemployed was exploited in the analysis. We grouped results based on the unemployment reason in 'exogenous', i.e. the reason is not related to the behavior of the laid-off worker (e.g. plant closure), and 'endogenous' for the remaining reasons of job loss.

Less than 5% of the study results estimated the spillover effects that unemployment generates on the health of another household member. We also coded this characteristic, because the spillover effect may have a different magnitude than the direct one.

There is a debate on the health effects of unemployment being exacerbated or mitigated by the macroeconomic conditions. On the one hand, the occurrence of unemployment may be less harmful in economic downturns since individuals might feel less stigmatized when unemployment becomes the prevailing social norm (Clark, 2003; Clark et al., 2010; Chadi, 2014). On the other hand, losing a job during an economic downturn may impair mental health more importantly because the laid-off worker may fear that the job loss is a one-way into unemployment given the bad economic situation. Thus, to control for the business cycle and the labor market status, we included the GDP growth rate and the unemployment rate, averaged over the years covered by the sample of each study result.[6]

In order to control for the quality of the studies, we used the SCImago Journal Ranking (SJR) index,[7] and the yearly average of Google Scholar citations. In some cases, the SJR index of the year of the publication was not available for three reasons: i) in one case the article was published in a journal not indexed in SCImago, Pharr et al. (2012); ii) the article was published in 2021 and the SJR index was not available yet; iii) the article was

[5] According to ILO an unemployment spell is short if it is shorter than or equal to 12 months. It is defined as long otherwise.

[6] The results in Winkelmann and Winkelmann (1998) refer to the period from 1984 until 1989 in Germany. The unemployment rate was unavailable for that time span. We approximated the average unemployment rate in that period with the first available observation, i.e. 1991.

[7] The SJR index is provided by SCImagoLab (https://www.scimagojr.com/).

published in a journal that was not indexed in SCImago at the time of publication but it was indexed later. In the first case, we assigned 0 to the SJR index. In the second case, we assigned the 2020 value of the SJR index. In the third case, we assigned the SJR score obtained by the journal as soon as indexed in SCImago for the first time.

Finally, we controlled for publication year. The average publication year is 2013, due to the fact that the number of studies on unemployment and health have considerable grown in the past twenty years.

3 Detecting publication bias

3.1 Visual inspection

Publication bias occurs when certain results are more likely to be published typically, but not necessarily, those reaching statistical significance.[8] The meta-sample is then affected by sample selectivity undermining the conclusions of the meta-analysis. Publication bias may be due to the peer-review and editorial process, which may find significant results more interesting and more worthy of publication (Franco et al., 2014): "*journals like stars*" (Brodeur et al., 2016). It may also arise from researchers' malpractices in response to the difficulty of publishing insignificant results. For example, a researcher may run many regressions, detect some significant outcomes, look *ad hoc* theoretical reasons to explain them, and not report the insignificant findings (Picchio, 2022). Publication bias affects the majority of social and medical sciences, including economic areas of research. See, among others, Stanley and Doucouliagos (2012, § 4) for relevant references corroborating the existence and the relevance of publication bias in economics.

Egger et al. (1997) suggested a simple preliminary check to assess the presence of the publication bias. It is a visual inspection based on the 'funnel plot'. It is a scatter diagram which plots the effect size, the partial correlation coefficient (r) in our case, against the inverse of its standard error. The funnel plot provides rough but still useful preliminary insights relative to the presence of publication bias. In the absence of publication bias, the scatter plot should look like an inverted funnel, symmetric around its mean. Indeed, if a literature is not affected by selectivity issues, we expect a larger variability of the effect size for lower precisions (i.e. larger standard errors), giving the shape of an inverted

[8] See Chuard et al. (2019) for evidence of researchers who manipulate the tests to ensure nonsignificant results ('reverse p-hacking').

funnel. We also expect symmetry and randomness around the average effect, because an asymmetric profile signals a lower representation of low precision effect sizes, which are likely to result in insignificant findings.

Figure 5: Funnel plot

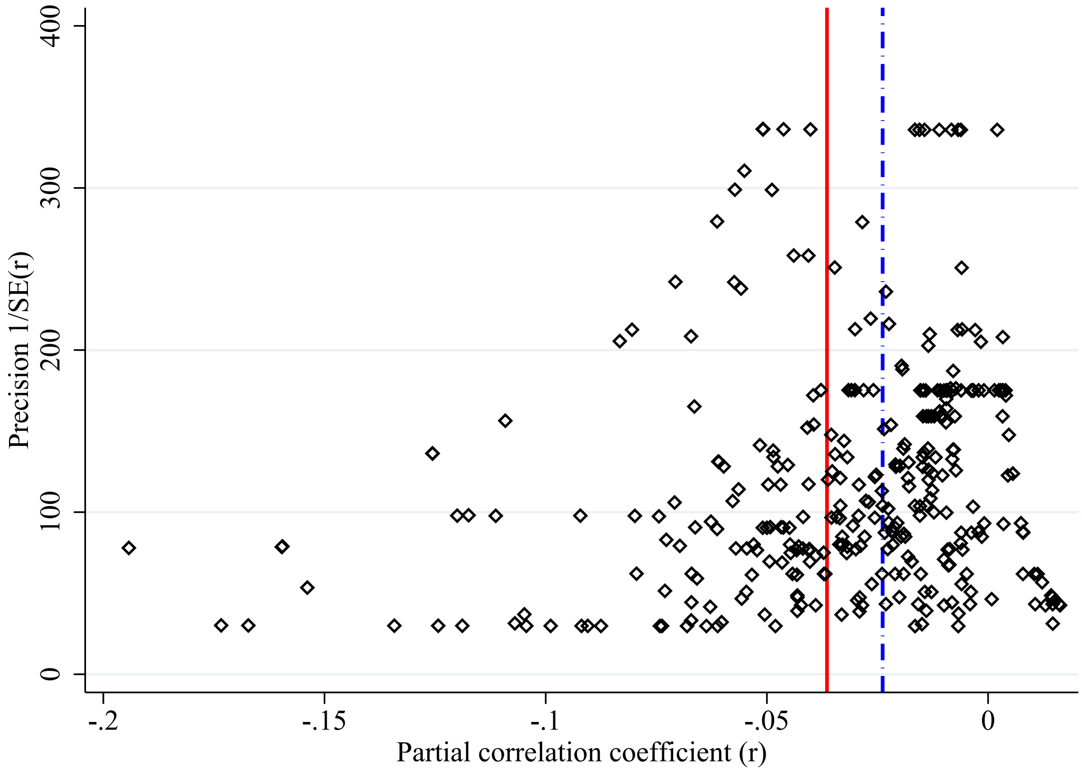


Figure 5 is our funnel plot. The vertical solid and dashed lines indicate the average and the median of the partial correlation coefficients, respectively. The strong asymmetry with the pronounced left-skewness suggests that publication bias is present and substantial. Nevertheless, conclusions drawn from the funnel plot should be taken with caution. Although the funnel plot is a quick method to detect evidence of publication bias, it is not a formal test. In the next subsection, we use a regression based formal test to detect publication bias.

3.2 Formal tests for publication bias

A regression based formal test to detect publication bias, which is based on the same idea of the funnel plot, is the 'Funnel Asymmetry Test – Precision Effect Test' (FAT-PET) (Stanley, 2005, 2008). It has two components: i) the 'Funnel Asymmetric Test' (FAT) and ii) the 'Precision Effect Test' (PET). It is computed regressing the effect size on a constant and its standard error:

$$r_i = \delta_0 + \delta_1 \times SE(r_i) + \varepsilon_i, \tag{3}$$

where ε_i is the error term and δ_1 captures the relation between the effect size and its standard error, i.e. the FAT component. If there is no publication bias, there should be no relation between the effect size and its standard error and δ_1 is expected to be nil. If, after the estimation of Equation (3), we do reject the null hypothesis of $\delta_1 = 0$, we have evidence of no publication bias, translating into the symmetry of the funnel plot. In case of rejection, the literature may suffer from some sort of manipulation in the published results. In Equation (3), δ_0 is the PET component. The rejection of the null hypothesis $H_0 : \delta_0 = 0$ is interpreted as a genuine average effect of unemployment on health corrected for publication bias.

The parameters of Equation (3) can be estimated by Ordinary Least Squares (OLS). However, the error term is heteroskedastic. The partial correlation coefficient has variance given by the square of the standard errors ($SE(r_i)^2$). The OLS estimator is not efficient in this circumstance. The knowledge of the variance of the error term in Equation (3) can be used to estimate the model by Weighted Least Squares (WLS) which, if $SE(r_i)^2$ is a consistent estimate of the variance of the effect size, will be consistent and asymptotically efficient. When Equation (3) is estimated by WLS, the estimate of δ_0 is the weighted average of the effect size with weights proportional to $1/SE_i^2(r)$, corrected for publication bias; results with lower variance will weigh more in the calculation.

The estimates of Equation (3) are displayed in column (1) of Table 3. Column (2) reports an alternative FAT-PET analysis, which served as a robustness check; the partial correlation coefficient is regressed on the inverse of the square root of the sample size, instead of the standard error. Finally, in column (3) we report the results if in Equation (3) we replace $SE(r_i)$ with its square to capture eventual non-linearities. This is the Precision Effect Estimate with Standard Error (PEESE) model, which is a meta-regression method to be preferred in correcting for publication bias when there is a genuine nonzero effect

(Stanley and Doucouliagos, 2012, 2014).

The most interesting finding in Table 3 is the absence of publication bias on average. In the three different models, the estimate of δ_1 is indeed not significantly different from zero. The estimate of δ_0 suggests that there is a significantly negative genuine effect of unemployment on health. However, the size of the effect, which is very stable across the three models, is fairly small (Doucouliagos, 2011).

Table 3: Meta-regression analysis and publication bias testing and correction

Variables	FAT-PET		PEESE
	WLS-FE (1)	WLS-FE† (2)	WLS-FE (3)
Precision effect (δ_0)	–0.0287***	–0.0273***	–0.0288***
	(0.0067)	(0.0068)	(0.0048)
Publication bias (δ_1)	–0.1946	–0.4713	–22.0522
	(0.6750)	(0.7143)	(19.5321)
R^2	0.0008	0.0040	0.0056

Notes: *** Significant at 1%. Standard errors robust to within-study correlation are in parenthesis.
† $SE(r_i)$ is replaced with the inverse of the square root of the sample size.

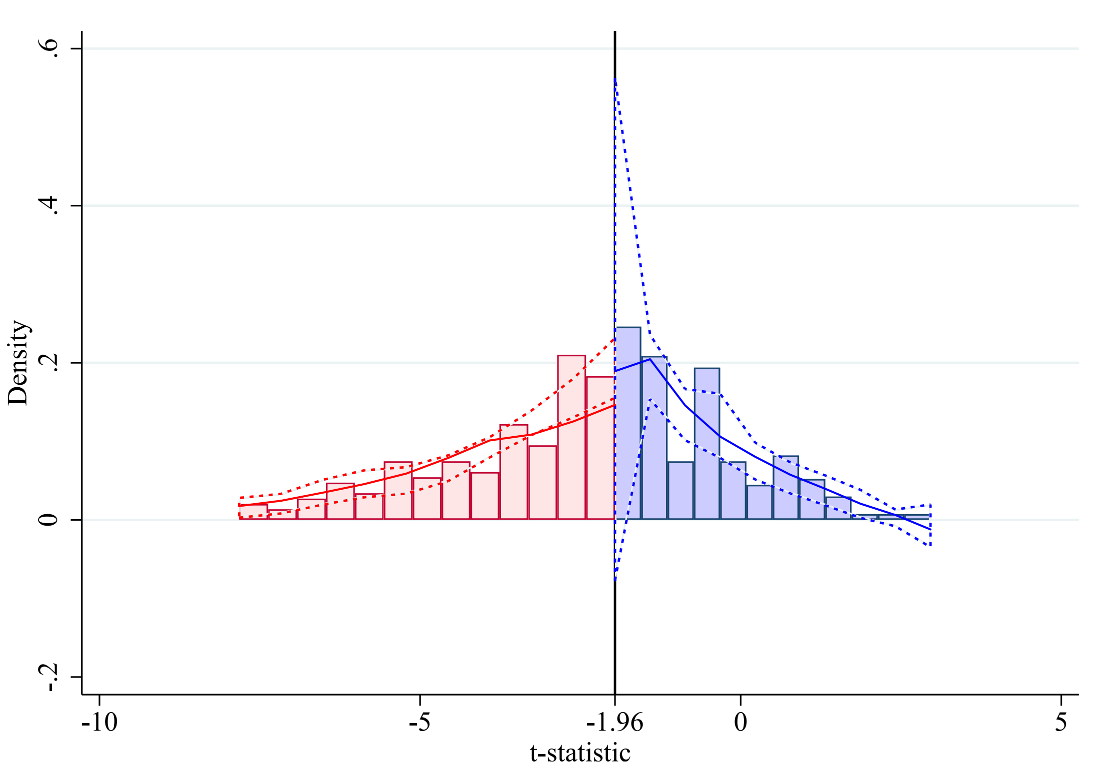
As an alternative to the PEESE, Bom and Rachinger (2019) proposed the Endogenous Kink (EK) meta-regression model to account for possible non-linearity between the effect size and its standard error. We estimated the EK meta-regression model, finding no evidence of publication bias on average.

Andrews and Kasy (2019) proposed a further way to detect publication bias. If it is absent, the density distribution of the t-(z-)statistic or the p-value should not bounce or present discontinuities around sensitive values like ± 1.96 for the t-(z-)statistics or 0.05 for the p-values, where manipulations in the results are more likely to occur (Brodeur et al., 2016). We followed Cattaneo et al. (2018, 2020) and used a nonparametric local polynomial density estimator to check whether the density of the t-statistics presents a significant discontinuity at -1.96. Figure 6 reports the estimated local quadratic polynomial density of the t-statistics.[9] The plot shows a jump and the statistical test for its significance returned a p-value equal to 0.0687. The jump is peculiar: in the presence of publication bias which favors significant results, we would expect a larger mass to the left of -1.96 and not to the right.[10]

[9] We performed the same test but with a linear local polynomial density estimator. Results are similar and available from the authors upon request.

[10] We replicated the same test but by setting the cutoff at at -1.64. We found no evidence of discontinuity in the density of the t-statistics at -1.64. These results are displayed in Appendix B.

Figure 6: Discontinuity test of the density of the t-statistic at -1.96



Notes: The thick solid lines are the estimated local polynomial density of the t-statistics (Cattaneo et al., 2018, 2020). The order of local polynomial is 2 (quadratic). The thin dashed lines indicate the bias-corrected confidence intervals at 95%. The two-sided tale test for the significance of the discontinuity at -1.96 returns a p-value equal to 0.0687.

Table 4: Two-sided binomial tests of equal masses in equal size windows around -1.96

Interval	Window length	Observations below -1.96	Observations above -1.96	p-value
(–2.015, –1.905)	0.110	9	11	0.8238
(–2.070, –1.850)	0.220	12	16	0.5716
(–2.125, –1.795)	0.330	20	26	0.4614
(–2.180, –1.740)	0.440	27	35	0.3742
(–2.235, –1.685)	0.550	31	47	0.0888

Notes: Interval bounds are computed as $-1.96 \pm \frac{\textit{window length}}{2}$.

Table 4 reports the two-sided binomial test for the null hypothesis of equal mass in equal size windows around -1.96.[11] Consistent with the previous test, there are no sizeable differences between the number of observations below and above -1.96, neither at broader nor at tighter window lengths.

All the results of the previous formal tests suggest that publication bias should not be a concern under the implicit assumption of a common true effect. However, publication bias may be linked to study characteristics. Brodeur et al. (2020) showed that, over a sample of more than 21,000 hypothesis tests published in 25 top economic journals, tests based on the DiD or IV approaches are more likely to suffer from publication bias. We therefore considered the possibility that publication bias may correlate with the econometric methodology to identify the health effects of unemployment. We enriched our FAT-PET model in Equation (3) as follows:

$$r_i = \boldsymbol{\delta}_0 \mathbf{Z}_i + \boldsymbol{\delta}_1 \mathbf{Z}_i \times SE(r_i) + \varepsilon_i, \tag{4}$$

where $\mathbf{Z}_i$ is the set of variables across which the tendency for publication bias is suspected to be heterogeneous. $\boldsymbol{\delta}_1$ and $\boldsymbol{\delta}_0$ are two parameter vectors, corresponding to the FAT and PET components, respectively. We divided the study results in three categories according to the methodology used for the identification of the effect. In the first category, 'Observables', we pooled together those results which faced selectivity issues based on observables. In a second category, we collected observations that either came from the difference-in-differences or the instrumental variables methods. In the third category, we grouped all the remaining study results which tackled selectivity based on unobservables. Table 5 presents the results. Consistently with the findings in Brodeur et al. (2020), we detect publication bias in study results using DiD or IV approaches. The PEESE estimates of the precision effect in Model (2), which are to be preferred to the FAT-PET one in correcting for publication bias when there is a genuine nonzero effect (Stanley and Doucouliagos, 2012, 2014), suggest that the strongest negative effect comes from study results with an identification strategy based on observables. When endogeneity concerns are tackled more seriously, the average effect size moves towards zeros, although still statistically different from 0.

In summary, from the conducted tests, publication bias is not of much concern, except

[11] We report in Appendix B the corresponding table with the results of the two-sided binomial test for the null hypothesis of equal mass in equal size windows around -1.64.

for study results based on DiD or IV. The average effect size of unemployment on health, once corrected for publication bias, is fairly small, especially when it comes from DiD or IV estimates.

Table 5: Publication bias test and correction by identification strategy

Variables	FAT-PET WLS-FE (1) Coeff.	Std. Err.	PEESE WLS-FE (2) Coeff.	Std. Err.
Precision effect for Observables (δ_{10})	–0.0525***	0.0034	–0.0493***	0.0034
Precision effect for DiD-IV (δ_{20})	0.0020	0.0041	–0.0098***	0.0023
Precision effect for Fixed Effects (δ_{30})	–0.0185*	0.0097	–0.0200***	0.0056
Publication bias for Observables (δ_{11})	0.6933	0.4811	19.8401	19.4871
Publication bias for DiD-IV (δ_{21})	–2.2796***	0.5285	–67.1271***	13.0440
Publication bias for Fixed Effects (δ_{31})	–0.4810	1.3256	–31.0920	49.9201
R^2	0.6988		0.6980	

Notes: * Significant at 10%, *** significant at 1%. The category 'Observables' includes CFA, DM, PSM and RE estimates (150 observations). The category 'Fixed Effects' contains FE, FEIV and MM estimates (128 observations). The category 'DiD-IV' includes DiD and IV estimates (49 observations).

4 Meta-regression analysis for effect heterogeneity

4.1 A multivariate analysis for uncertainty

In the previous section we detected heterogeneous publication bias among the identification strategies. The PEESE correction for publication bias is therefore the stepping stone for building the model to investigate sources of heterogeneity in the health effects of unemployment. We modified Equation (4) by including a linear index in the covariates presented in Table 2:

$$r_i = \boldsymbol{\delta}_0\mathbf{Z}_i + \boldsymbol{\delta}_1\mathbf{Z}_i \times SE(r_i)^2 + \boldsymbol{\beta}\mathbf{X}_i + \varepsilon_i \tag{5}$$

where $\mathbf{X}_i$ is the $k \times 1$ vector of relevant additional covariates potentially explaining finding heterogeneity. The WLS-FE estimation of Equation (5) is equivalent to the OLS estimation of the following transformed model:

$$\frac{r_i}{SE(r_i)} = \frac{\boldsymbol{\delta}_0\mathbf{Z}_i}{SE(r_i)} + \boldsymbol{\delta}_1\mathbf{Z}_i \times SE(r_i) + \frac{\boldsymbol{\beta}\mathbf{X}_i}{SE(r_i)} + \frac{\varepsilon_i}{SE(r_i)}. \tag{6}$$

There is always uncertainty about which regressor to include in Equation (6), especially when the number of observations is not very large; some of the covariates may contain similar information, generating multicollinearity and therefore difficulty with the reliability of the estimates of the model parameters. In order to avoid arbitrary exclusions of covariates, we relied on data driven algorithms as recommended by Havránek et al. (2020).

First, we estimated Equation (6) using Bayesian Model Averaging (BMA), which deals with uncertainty by estimating all the possible models from a set of k covariates, each time applying different subsets of regressors. It begins with the null model and then moves towards all the possible combinations. Then, it computes the weighted averages of the estimated coefficients. The weights are defined as the 'posterior model probabilities' (PMP) and correspond to the goodness-of-fit of each estimated model. Their sum generates the 'posterior inclusion probability' (PIP) which roughly indicates, for each covariate, the probability of being part of the true model. We followed Magnus et al. (2010), who split the covariates in two groups. The first group (k_1) includes the 'focus' regressors. This set of covariates are always included in the model specification, because they are considered of crucial importance. The other group (k_2) includes the 'auxiliary' regressors, which are considered of potential interest but not fundamental, thus their inclusion in the model specification is iteratively tested. An auxiliary variable is considered to belong to the true model if its PIP is equal or greater than 0.5 (Xue et al., 2021). In our case, the focus regressors are the covariates we used previously for the publication bias analysis by econometric methodology for the identification of the causal effect. The auxiliary regressors are instead all the other variables presented Table 2. The main drawback of this framework is the computational burden which grows exponentially with the number of covariates (Magnus et al., 2010). Furthermore, BMA outcomes are sensitive to the assumptions on the priors for the model parameters (De Luca and Magnus, 2011; Steel, 2020). Typically, after the BMA estimation, a frequentist check is conducted by estimating the model without the covariates with PIP below 0.5 (see, e.g., Havranek et al., 2015; Xue et al., 2021).

Second, we estimated Equation (6) by Weighted-Average Least Squares (WALS) (Magnus et al., 2010; De Luca and Magnus, 2011). The WALS estimator is a hybrid between the Bayesian and the frequentist approaches. It differs from the BMA in two aspects. The first one is practical; the WALS relies on a preliminary orthogonalization of the k_2 auxiliary regressors and associated parameters, which largely reduces the computational burden. Second, it uses a Laplace or a Subbotin prior for the k_2 auxiliary regressors rather than a multivariate Gaussian, ruling out the possibility of unboundedness of the estimator (Magnus et al., 2010).

Table 6 reports the estimated coefficients. Model (1) displays the BMA estimates, whereas Models (2a) and (2b) show the WALS estimates for the Laplace and Subbotin priors, respectively.[12] Finally, Model (3) presents the frequentist check; we estimated by OLS the parameters in Equation (6) after removing the auxiliary regressors with a PIP smaller than 0.5.

Panel (a) of Table 6 shows the estimated coefficients of the focus regressors, whilst panel (b) refers to the auxiliary covariates. All estimates suggest that publication bias stems from study results using DiD or IV even after controlling for a large set of study/result characteristics. The OLS estimates on the subset of auxiliary covariates which are relevant according to the BMA return a large value of the R^2; the covariates in the frequentist check explain 82% of the variance of the partial correlation coefficient of the study results.

For the auxiliary regressors, the first block shows that the unemployment effect differs across the health measures used. The strongest negative effects are found for well-being and mental health. From the WALS estimates, health behaviors are negatively affected as are mental health and well-being.

No sizable effects emerge for the role of the geographical area. The coefficients for the sample average age suggests that older cohorts suffer less unemployment compared to younger ones. From the theoretical point of view, the unemployment effects for older workers may be either stronger or weaker. On the one hand, older individuals may have poorer health and face increasing difficulties to relocate themselves in the labor market in case of job loss. On the other hand, younger individuals may face more binding budget constraints, for example because they are more likely to have dependent children and mortgages to repay, suffering therefore the most health consequences after job loss. Our findings suggest that the last effect dominates.[13]

The fourth block focuses on the heterogeneity of the effect according to the use in the regression analysis of key control variables, aimed at netting out spurious components from the relationship between unemployment and health. The coefficient for the use of 'income controls' presents a positive sign and is strongly significant in all the models.

[12] We used the Stata commands `bma` and `wals` developed by De Luca and Magnus (2011).

[13] We estimated an alternative specification with a quadratic term for age in order to detect eventual non-linearity, but we did not find it.

Table 6: Model averaging for uncertainty in the relation between unemployment and health

Variables	BMA (1)		WALS (2a) $q=1$		WALS (2b) $q=0.5$		OLS frequentist check (3)	
	Coefficient	PIP	Coefficient	$\|t\|$	Coefficient	$\|t\|$	Coefficient	p-value
(a) Focus regressors								
Precision Effect for Observables (δ_{10})	–0.04852	1.00	0.08678	0.13	0.09434	0.13	–0.06322	0.000
	(0.15954)		(0.69222)		(0.70991)		(0.00795)	
Precision Effect for DiD-IV (δ_{20})	–0.00588	1.00	0.12920	0.19	0.13691	0.19	–0.01840	0.025
	(0.15965)		(0.69305)		(0.71077)		(0.00803)	
Precision Effect for Fixed Effects (δ_{30})	–0.01823	1.00	0.11598	0.17	0.12345	0.17	–0.03142	0.000
	(0.15935)		(0.69177)		(0.70942)		(0.00810)	
Publication Bias for Observables (δ_{11})	11.37697	1.00	7.95696	0.50	6.99675	0.44	12.87424	0.573
	(15.37545)		(15.89255)		(15.99606)		(22.74961)	
Publication Bias for DiD-IV (δ_{21})	–95.72085	1.00	–89.92030	3.41	–90.95271	3.44	–96.49203	0.000
	(26.11560)		(26.37034)		(26.41040)		(16.53067)	
Publication Bias for Fixed Effects (δ_{31})	–39.30303	1.00	–39.56114	1.55	–40.06271	1.56	–37.87759	0.205
	(26.12398)		(57.48319)		(25.60672)		(29.60520)	
(b) Auxiliary regressors								
(1) *Health measures (reference: self-assessed health – SAH)*								
Health Behaviors (BEH)	–0.00544	0.30	**–0.02039**	**2.40**	**–0.02299**	**2.60**		
	(0.00947)		**(0.00851)**		**(0.00883)**			
Health Care Utilization (HOS)	–0.00010	0.05	0.00001	0.00	0.00020	0.03		
	(0.00174)		(0.00677)		(0.00699)			
Mental Health (MH)	**–0.02585**	**1.00**	**–0.02251**	**6.16**	**–0.02362**	**6.32**	–0.02556	0.000
	(0.00329)		**(0.00365)**		**(0.00374)**		(0.00456)	
Physical Health (PH)	0.000003	0.04	–0.00169	0.29	–0.00228	0.37		
	(0.00129)		(0.00587)		(0.00614)			
Well-Being (WB)	**–0.02869**	**1.00**	**–0.02607**	**7.38**	**–0.02788**	**7.70**	–0.02858	0.000
	(0.00319)		**(0.00353)**		**(0.00362)**		(0.00496)	
(2) *Geographical area (reference: European countries)*								
Non-European countries	0.00009	0.06	–0.00107	0.33	–0.00146	0.44		
	(0.00101)		(0.00326)		(0.00333)			
Multi-country	–0.00114	0.17	**–0.00533**	**1.49**	**–0.00590**	**1.61**		
	(0.00301)		**(0.00358)**		**(0.00367)**			
(3) *Sample age controls*								
Sample average age if available	**0.00105**	**0.99**	**0.00087**	**2.90**	**0.00091**	**2.87**	0.00108	0.018
	(0.00028)		**(0.00030)**		**(0.00032)**		(0.00045)	
Sample average age tot available	**0.00866**	**0.89**	**0.00751**	**2.40**	**0.00768**	**2.36**	0.00978	0.021
	(0.00417)		**(0.00313)**		**(0.00326)**		(0.00414)	
(4) *Relevant study controls in regression analysis*								
Health controls	–0.00023	0.08	**–0.00453**	**1.56**	**–0.00509**	**1.67**		
	(0.00119)		**(0.00290)**		**(0.00304)**			
Income controls	**0.01379**	**1.00**	**0.01347**	**4.06**	**0.01466**	**4.22**	0.01403	0.013
	(0.00319)		**(0.00332)**		**(0.00348)**		(0.00546)	
(5) *Gender (reference: men)*								
Men+Women	**0.01363**	**0.88**	**0.01221**	**2.74**	**0.01288**	**2.82**	0.01447	0.030
	(0.00657)		**(0.00445)**		**(0.00456)**		(0.00652)	
Women	**0.01126**	**0.89**	**0.01021**	**3.00**	**0.01033**	**2.94**	0.01293	0.000
	(0.00531)		**(0.00340)**		**(0.00352)**		(0.00312)	
(6) *Duration of unemployment (reference: duration not specified)*								
Short term unemployment ($\leq$ 12 months)	**0.02113**	**1.00**	**0.01654**	**4.95**	**0.01748**	**5.11**	0.02099	0.000
	(0.00384)		**(0.00334)**		**(0.00342)**		(0.00447)	
Long term unemployment (> 12 months)	**0.01389**	**0.98**	**0.01175**	**3.61**	**0.01264**	**3.79**	0.01407	0.023
	(0.00419)		**(0.00325)**		**(0.00333)**		(0.00603)	
(7) *Reason for unemployment (reference: non-exogenous)*								
Exogenous (e.g. plant closures)	**0.02272**	**1.00**	**0.01638**	**3.36**	**0.01676**	**3.22**	0.02277	0.000
	(0.00483)		**(0.00487)**		**(0.00521)**		(0.00549)	
(8) *Relation with the unemployed (reference: herself/himself)*								
Other (i.e. parent/partner)	**0.02910**	**1.00**	**0.02396**	**4.26**	**0.02444**	**4.31**	0.03067	0.000
	(0.00655)		**(0.00563)**		**(0.00566)**		(0.00744)	
(9) *Business cycle and labor market status*								
Average GDP growth rate in time interval	0.00014	0.07	0.00058	0.29	0.00053	0.26		
	(0.00074)		(0.00201)		(0.00205)			
Average unemployment rate in time interval	**–0.00735**	**1.00**	**–0.00582**	**3.54**	**–0.00615**	**3.68**	–0.00755	0.039
	(0.00167)		**(0.00164)**		**(0.00167)**		(0.00357)	
(10) *Study quality*								
SJR index	**–0.00216**	**0.63**	**–0.00303**	**2.44**	**–0.00324**	**2.54**	–0.00308	0.202
	(0.00197)		**(0.00124)**		**(0.00128)**		(0.00239)	
Average Scholar citations per year	–0.00001	0.06	0.00006	0.51	–0.00005	0.36		
	(0.00004)		(0.00012)		(0.00013)			
(11) Year of publication	–0.000006	0.05	–0.00007	0.20	–0.00007	0.21		
	(0.00008)		(0.00034)		(0.00035)			

Notes: A value in bold indicates that the corresponding auxiliary variable should be included in the final model (i.e. PIP > 0.5 for the BMA and $|t| \geq 1$ for the WALS). In the WALS estimates, $q = 1$ and $q = 0.5$ indicate the use of the Laplace and Subbotin priors, respectively (De Luca and Magnus, 2011). $R^2 = 0.8191$ in the OLS frequentist check. The p-values in the OLS frequentist check are robust to within-study correlation. Standard errors are in parenthesis.

Controlling for it corrects therefore for an omitted variable bias which would instead bias downwards the relationship. This is the case when the correlation between income and health is positive and the correlation between income and unemployment is negative.

The results in the fifth block about gender are clear-cut. The unemployment effects are more severe for men. The male breadwinner model finds support. Men may consider crucial being part of the active population because of societal expectations. For example, the society may refer to them as the main financial providers of the household. An eventual shift into unemployment deprives them of this role and triggers blame or shame. Furthermore, women might feel unemployment to a lesser extent because the societal expectations may see in their familiar role a valid substitute to their current unemployment.

Block (6) focuses on whether the duration of unemployment matters. Those studies which did not report specific information about the duration of unemployment are taken as the reference category. Long and short unemployment durations similarly affect health; it is more the occurrence of unemployment that matters, rather than its duration.

Block (7) investigates whether the study results are different if the reason for the unemployment event is exogenous. The estimates suggest that when the treatment endogeneity is duly treated, for example using plant closures as an exogenous shock, the severity of the unemployment effect eases.

The eighth block allows us to understand if unemployment generates spillover health effects on other members of the household, like a parent or the partner. We find that unemployment is less detrimental on other household members than is on the individual who experienced herself/himself the unemployment event.

Study results do not vary with the GDP growth rate at the time in which the sample was used. The unemployment rate exhibits instead a significant negative influence on the health effects of unemployment. We interpret this finding as displaced individuals being more negatively affected by the job loss when it is more difficult to find a new one because of the already high unemployment rate and low tightness of the labor market.

Finally, effect size does not vary with the year of publication or with rough measures of the quality of the study, like the SJR index of the journal where the article was published or the yearly average number of Google Scholar citations.

Table 6 is informative about the heterogeneity of the study results, but only with respect to the reference categories; it does not provide information at a first sight about the genuine effect of unemployment on health for particular combinations of study/result characteristics. To shed light on this, we: i) identified the ten most frequent combinations of our categorical regressors; ii) fixed the continuous regressors at their median value; iii) set δ_1 to zero, therefore pretending that publication bias is absent; iv) predicted the expected effect size for each combination using the estimates from the OLS frequentist check in columns (3) of Table 6; v) displayed the expected effect sizes in Table 7. The ten most frequent combinations account for a total of 153 observations, which is 46.80% of the entire sample.

Table 7: Expected effect size for the ten most frequent combinations of categorical regressors

	Covariate combinations	Effect size	F-stat.	p-value	Absolute frequency	Relative frequency
(1)	'Fixed effects' + 'Men+Women'	-0.0169^{***}	13.62	0.0005	32	9.79%
(2)	'DiD-IV' + 'Men+Women'	-0.0039	1.86	0.1770	27	8.26%
(3)	'Observables' + 'Income controls' + 'Men+Women'	-0.0347^{***}	44.53	0.0000	18	5.50%
(4)	'Fixed effects' + 'Well-Being' + 'Men+Women' + 'Short term unemployment'	-0.0245^{***}	14.24	0.0004	17	5.20%
(5)	'Observables + 'Men+Women'	-0.0488^{***}	101.88	0.0000	15	4.59%
(6)	'Fixed effects' + 'Well-Being' + 'Men+Women' + 'Long term unemployment'	-0.0315^{***}	15.59	0.0002	10	3.06%
(7)	'Fixed effects' + 'Well-Being' + 'Income controls' + 'Men+Women'	-0.0330^{***}	22.16	0.0000	9	2.75%
(8)	'Observables' + 'Well-Being' + 'Income controls' + 'Men+Women'	-0.0633^{***}	147.39	0.0000	9	2.75%
(9)	'Fixed effects' + 'Well-Being' + 'Income controls'	-0.0460^{***}	34.34	0.0000	8	2.45%
(10)	'Observables' + 'Mental health' + 'Income controls' + 'Men+Women'	-0.0603^{***}	130.41	0.0000	8	2.45%
	Total observations				153	46.79%

Notes: Each continuous variable is set at its median value. Categorical variables not mentioned in the ten combinations are set to the reference category. Absence of publication bias is assumed ($\delta_1 = 0$).

The expected partial correlation coefficient varies from -0.0633 to -0.0039. Combination (2) displays the smallest health penalty of unemployment, while combinations (8) and (10) show the strongest ones (-0.0633 and -0.0603, respectively). The most important penalties emerge when the outcome variable was mental health or well-being, the sample was composed of both men and women, the identification strategy was based on selection on observables including controls for income. Nevertheless, the size of the relation between unemployment and health is fairly small.

At the end of Subsection 3.2 we discussed the differences in the precision effects across different identification strategies, which is confirmed in panel (a) of Table 6; the strongest negative effect comes from study results with an identification strategy based on observables, whereas when endogeneity is tackled more seriously, the average effect size shrinks towards zero. This feature is also visible in Table 7, comparing the expected partial correlation coefficients of combinations (3), (5), (8) and (10), based on selection on observables, with the remaining ones, based on selection on unobservables.

Table 8 zooms in the expected effect sizes when selectivity is dealt with unobservables. More in detail, panel (a) displays the five most frequent combinations based on DiD-IV (42 observations) and panel (b) shows the five most frequent combinations based on fixed-effects (76 observations). In both cases, the predicted partial correlation coefficients are very small, and they become even positive when the unemployment event is the result of a plant dismissal, i.e. 'exogenous'. We conclude that, independently of the other result/study characteristics, whenever selectivity into unemployment is more seriously tackled, the negative effect of unemployment on health becomes negligible and, in some cases, it disappears.

Table 8: Expected effect size for the five most frequent combinations of the categorical variables for results based on DiD.IV and fixed effects

	Covariate combinations	Effect size	F-stat.	p-value	Absolute frequency	Relative frequency
(a)	*Difference-in-differences/instrumental variables (DiD-IV)*					
	'DiD-IV' + 'Men+Women'	–0.0039	1.86	0.1770	27	8.26%
	'DiD-IV' + 'Men+Women' + 'Exogenous'	0.0188***	9.04	0.0038	6	1.83%
	'DiD-IV' + 'Mental Health' + 'Men+Women' + 'Exogenous'	–0.0067	1.28	0.2623	3	0.12%
	'DiD-IV' + 'Mental Health' + 'Income controls' + 'Men+Women' + 'Exogenous'	0.0073	0.86	0.3579	3	0.12%
	'DiD-IV' + 'Mental Health' + 'Income controls' + 'Men+Women' + 'Other' + 'Exogenous'	0.0380***	10.45	0.0019	3	0.12%
	Total observations				42	12.84%
(b)	*Fixed effects*					
	'Fixed effects' + 'Men+Women'	–0.0169***	13.62	0.0005	32	9.79%
	'Fixed effects' + 'Well-Being' + 'Men+Women' + 'Short term unemployment'	–0.0245***	14.24	0.0004	17	5.20%
	'Fixed effects' + 'Well-Being' + 'Men+Women' + 'Long term unemployment'	–0.0315***	15.59	0.0002	10	3.06%
	'Fixed effects' + 'Well-Being' + 'Income controls' + 'Women'	–0.0330***	22.16	0.0000	9	2.75%
	'Fixed effects' + 'Well-Being' + 'Income controls'	–0.0460***	34.34	0.0000	8	2.45%
	Total observations				76	23.24%

Notes: Each continuous variable is set at its median value. Categorical variables not mentioned in the combinations are set to the reference category. Absence of publication bias is assumed ($\delta_1 = 0$).

5 Conclusions

This paper quantitatively surveys the literature on the relation between unemployment and health using meta-analytic techniques. To the best of our knowledge, this is the first meta-analysis to use a comprehensive set of health outcomes and questioning the effect size heterogeneity among them. We followed the MAER-Net guidelines to minimize the arbitrariness in the selection criteria for including studies or results in our meta-analytic sample (Stanley et al., 2013; Havránek et al., 2020). We collected 327 observations from 65 articles published in English in peer-reviewed journals from 1990 until 2021. We checked the presence of publication bias. When we detected publication bias in results adopting particular identification strategies, we corrected it. We used a large set of controls to exploit eventual sources of effect size heterogeneity.

Our results suggested that unemployment exerts on average a small negative effect on health. The effect size heterogeneity analysis showed that the effect of unemployment on health depends on how health is measured, with the psychological domain of health being more negatively impacted. Moreover, part of the negative effect of unemployment on health seems to be spurious; when the identification strategy relied on selection on unobservables, on exogenous unemployment shocks – like plant closure – and on controlling for income, the effect size becomes negligible. We also found that experiencing long and short unemployment spells similarly affected health, suggesting that it is the occurrence of unemployment that matters, rather than its duration. The spillover effects of unemployment towards other family members are less important than the unemployment effect on the displaced worker's health. We found that the negative consequences of unemployment on health decreases with age and they are more important for men. Finally, the status of the labor market is an additional source of effect heterogeneity, with the health effects becoming more negative when the labor market conditions are worse.

From a policy perspective, two results are important: i) the psychological domains of health are those most sensitive to unemployment; ii) it is rather the occurrence of unemployment that matters, rather than its duration. The policy maker and members of the health care system may consider the need for therapeutic strategies for the unemployed promptly after the job loss, as even short-term unemployment impairs mental health (Cygan-Rehm et al., 2017).

Finally, our meta-analysis did not investigate the effect heterogeneity along the mental and physical distress in the prior occupation, for example approximated by the distinction of workers between blue and white collars. In fact, only a limited number of articles studied the effect heterogeneity of unemployment on health by the type of occupation. Because the negative health effects of unemployment may be more pronounced for less physically and mentally demanding jobs, future research may take this further dimension of heterogeneity into account.

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Appendix

A List of articles used in the meta-analysis

Table A.1: Studies included into the meta-sample

Author(/s) and year	Country[⋆]	Time Interval	Citations[†]	Outcome(/s)	Selection on	Type(/s) of unemployment	Subject(/s) affected	Effect(/s)	Heterogeneity
Aydiner-Avsar and Piovani (2021)	United States of America	2013-2014	3	MH	OBSE	Status	DIR	-	INC
Hoang and Knabe (2021)	United Kingdom	2014	13	WB	OBSE	Status	DIR	-	INC
Johansson et al. (2020)	Finland	2000;2011	21	PH, SAH	OBSE	Status	DIR	-, N	HEA
Chen and Hou (2019)	Multi-country[⋆]	2009-2014	21	WB	OBSE	Status	DIR	-	HEA, INC
Pieters and Rawlings (2020)	China	1997;2000;2004	21	HOS, PH, SAH	UNOB	Status	OT	-, N, +	INC
Ronchetti and Terriau (2019)	France	2013-2016	8	SAH	BTH	Status	DIR	N	HEA
Sulemana et al. (2019)	Ghana	2012	2	SAH	UNOB	Status	DIR	-, N	INC
Farré et al. (2018)	Spain	2006;2011	92	HOS, MH, PH, SAH, WB	UNOB	Status	DIR	-, N	No
Heggebø and Elstad (2018)	Multi-country[⋆]	2010-2013	21	SAH	UNOB; BTH	Status	DIR	-, N	HEA
Inanc (2018)	United Kingdom	1991-2008	42	MH, WB	UNOB	Status	DIR, OT	-, N	INC
Krug and Eberl (2018)	Germany	2009-2014	38	SAH	UNOB	Status	DIR	-	HEA, INC
Mousteri et al. (2018)	Multi-country	2006	58	MH, SAH, WB	OBSE	Status	DIR	-	HEA, INC
Roex and Rözer (2018)	Multi-country	2002-2008	9	WB	OBSE	Duration	DIR	-	INC
Voßemer et al. (2018)	Multi-country	2002-2012	100	SAH, WB	OBSE	Status	DIR	-	No
Zuelke et al. (2018)	Germany	2011-2014	49	MH	OBSE	Status	DIR	-, N	INC
Buffel, Beckfield and Bracke (2017)	Multi-country	2005-2010	17	HOS	OBSE	Status	DIR	-	HEA
Cygan-Rehm et al. (2017)	Multi-country[⋆]	2001-2013	57	MH	UNOB	Status	DIR	-	No
Leopold et al. (2017)	Germany	1990-2014	19	WB	UNOB	Duration	DIR	-	No
Thern et al. (2017)	Sweden	1983-1986; 1991-1994	66	HOS	OBSE	Duration	DIR	-	HEA
Von Scheve et al. (2017)	Germany	2007-2014	55	MH, WB	UNOB	Duration	DIR	-, N	No
Heggebø (2016)	Multi-country[⋆]	2007-2010	14	SAH	UNOB	Status	DIR	N	INC
Tøge (2016)	Multi-country	2008-2011	13	SAH	UNOB	Status	DIR	-	HEA
Aguilar-Palacio et al. (2015)	Spain	2006-2012	62	BEH, MH, PH, SAH	OBSE	Status	DIR	N	No
Barlow et al. (2015)	Greece	2008-2011	16	SAH	OBSE	Status	DIR	-, N	No
Binder and Coad (2015)	United Kingdom	1996-2008	10	MH, WB	OBSE	Duration	DIR	-	HEA, INC
Buffel et al. (2015)	Multi-country	2006-2012	63	MH	OBSE	Status	DIR	-	INC
Cooper et al. (2015)	United Kingdom	1991-2005	20	PH, SAH	OBSE	Status	DIR	-	HEA, INC
Drydakis (2015)	Greece	2008-2013	264	MH, SAH	UNOB	Status	DIR	-	No
Tøge and Blekesaune (2015)	Multi-country	2008-2011	80	SAH	UNOB	Status	DIR	-	HEA, INC
Urbanos-Garrido and Lopez-Valcarcel (2015)	Spain	2006;2011	214	MH, SAH	OBSE, UNOB	Duration	DIR	-	HEA
Helliwell and Huang (2014)	United States of America	2005-2010	184	MH, WB	OBSE	Status	DIR	-	INC
Mörk et al. (2014)	Sweden	1992-2007	54	HOS	UNOB	Status	OT	-	HEA, INC
Van der Meer (2014)	Multi-country	2004	133	WB	OBSE	Status	DIR	-	No
Gathergood (2013)	United Kingdom	1991-2009	66	MH	UNOB	Status	DIR	-, N	HEA, INC
Marcus (2013)	Germany	2002-2010	298	MH	BTH	Status	DIR, OT	-, N	HEA, INC
Oesch and Lipps (2013)	Multi-country[⋆]	1984-2010	141	WB	BTH	Duration, Status	DIR	-, N	INC
Reine et al. (2013)	Sweden	1981;1983;1986;1995	57	BEH, SAH	OBSE	Status	DIR	-	HEA
Powdthavee and Vernoit (2013)	United Kingdom	1994-2008	80	WB	UNOB	Status	OT	N	No

(continued on next page)

Table A.1: *Continued from previous page*

Author(/s) and year	Country⋆	Time Interval	Citations†	Outcome(/s)	Selection on	Type(/s) of unemployment	Subject(/s) affected	Effect(/s)	Heterogeneity
Van Hoorn and Maseland (2013)	Multi-country	1981-1984; 1990-2009	119	WB	OBSE	Status	DIR	-	HEA, INC
Pharr et al. (2012)	United States of America	2009	114	BEH, HOS, PH, SAH	OBSE	Duration	DIR	-, N	INC
Green (2011)	Australia	2007-2011	327	MH, WB	UNOB	Status	DIR	-	INC
Schmitz (2011)	Germany	1991-2008	326	HOS, MH, SAH	UNOB	Status	DIR	-, N, +	No
Wulfgramm (2011)	Germany	2006-2007	82	WB	UNOB	Duration	DIR	-	HEA, INC
Clark et al. (2010)	Germany	1984-2006	388	WB	UNOB	Status	DIR	-	INC
Ervasti and Venetoklis (2010)	Multi-country	2002	146	WB	OBSE	Status	DIR	-	HEA, INC
Giatti et al. (2010)	Brazil	2002	89	SAH	OBSE	Status	DIR	-	HEA
Schunck and Rogge (2010)	Germany	2003	60	BEH, PH	OBSE	Status	DIR	-	HEA, INC
Böckerman and Ilmakunnas (2009)	Finland	1996-2001	403	SAH	UNOB, OBSE	Status	DIR	N	No
Kassenboehmer and Haisken-DeNew (2009)	Germany	1991-2006	346	WB	UNOB	Status	DIR	-	HEA, INC
Winkelmann (2009)	Germany	1984-2004	384	WB	OBSE	Status	DIR	-	No
Carroll (2007)	Australia	2001-2003	182	WB	OBSE, UNOB	Status	DIR	-	HEA, INC
Åhs and Westerling (2006)	Sweden	2001	131	HOS	OBSE	Status	DIR	N	INC
Cooper et al. (2006)	Multi-country⋆	1994-2001	52	SAH	OBSE	Status	DIR	-, N	INC
Kennedy and McDonald (2006)	Australia	1994;1995;1997	84	MH	OBSE	Duration	DIR	-, N	INC
Gordo (2006)	Germany	1984-2001	103	SAH	OBSE	Duration	DIR	-, N	HEA, INC
Artazcoz et al. (2004)	Spain	1994	816	MH	OBSE	Status	DIR	-	No
Clark et al. (2001)	Germany	1984-1994	895	WB	OBSE	Status	DIR	-	INC
Korpi (2001)	Sweden	1981-1991	168	SAH	UNOB	Duration, Status	DIR	N	HEA
Flatau et al. (2000)	Australia	1995;1997	121	MH	OBSE	Duration, Status	DIR	-, N	HEA, INC
Novo et al. (2000)	Sweden	1986;1994	48	MH, SAH	OBSE	Status	DIR	-, N	No
Winkelmann and Winkelmann (1998)	Germany	1984-1989	2239	WB	UNOB	Status	DIR	-	HEA, INC
Rodriguez et al. (1997)	United States of America	1987-1988	70	MH	OBSE	Status	DIR	-, N	INC
Turner (1995)	United States of America	1986	402	MH, PH	OBSE	Duration	DIR	-	No
Chen et al. (1994)	United States of America	1986	21	SAH, WB	OBSE	Status	DIR	-, N	No
Clark and Oswald (1994)	United Kingdom	1991	3414	MH	OBSE	Status	DIR	-	HEA

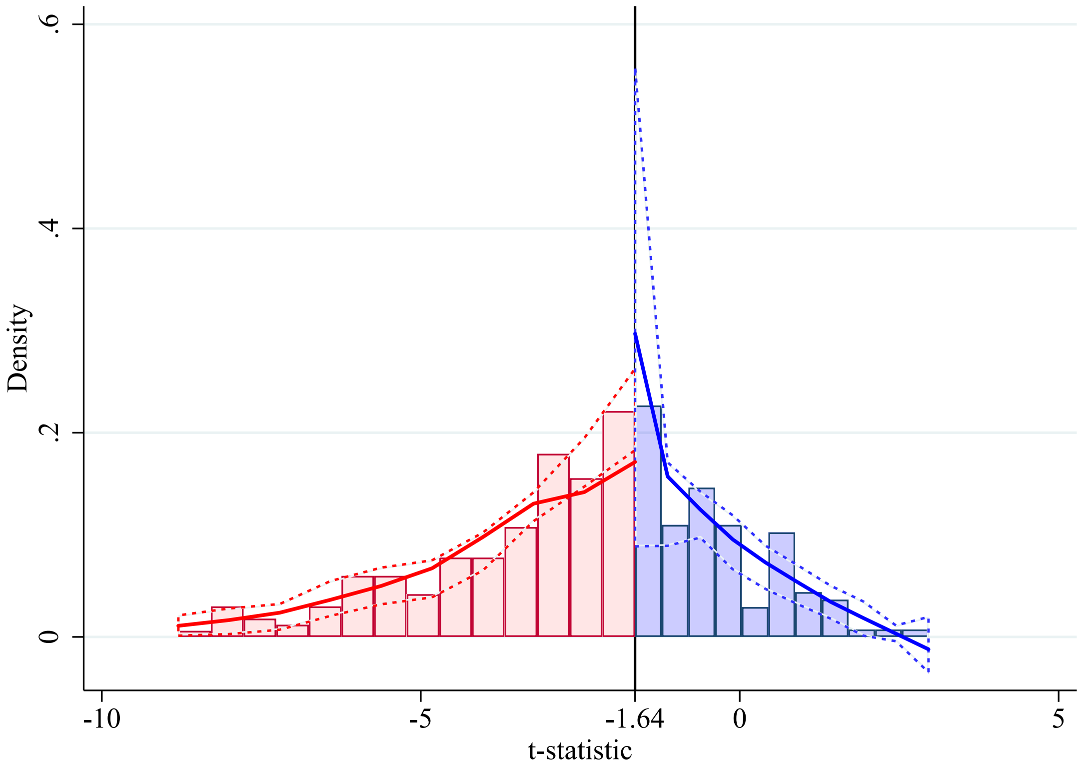
⋆ If countries are disaggregated into the meta-dataset.

† Overall citations from Google Scholar, last check in 29/12/2021.

‡ *Notes*: Column 'Outcomes' presents the various health outcomes, namely: *BEH* (i.e. health behavior), *HOS* (i.e. hospitalization/diagnosis/health care utilization), *MH* (i.e. mental health), *PH* (i.e. physical health), *SAH* (i.e. self-assessed health) and *WB* (i.e. well-being/life satisfaction). The column 'Selection on' presents three categories: *OBSE*, *UNOBS* and *BTH*. *OBSE* groups the following identification strategies: Control Function Approach (CFA), Duration Models (DM), Propensity Score Matching (PSM) and Random Effects (RE). *UNOB* groups the following: Fixed Effects (FE), FE/IV, Difference-in-Difference (DiD) and Instrumental Variables (IV). Finally, *BTH* groups mixed strategies as Mixed Models (MM) or combination of DiD-PSM. In column 'Subject affected' *DIR* means 'Directly affected' whereas *OT* is a residual group for 'Spouses', 'Partner' and 'Parents'. In column 'Effect(/s)' the - sign refers to a significant negative effect found, whilst the + sign refers to a significant positive one ; instead, the *N* refers to a null effect found. Finally, column 'Heterogeneity' presents separately the following main moderator(/s) included in the regression: *INC* stands for 'income control(/s) present', *HEA* stands for 'health control(/s) present', *No* for none relevant control present.

B Discontinuity tests at a different cutoff

Figure B.1: Discontinuity test at –1.64 of the density of the t-statistic



Notes: The thick solid lines indicate the estimated local polynomial density of the t-statistics (Cattaneo et al., 2018, 2020). The order of local polynomial is 2 (quadratic). The thin dashed lines indicate the bias-corrected 95% confidence interval. The two-sided tale test for the significance of the discontinuity at -1.64 returns a p-value equal to 0.8495.

Table B.1: Two-sided binomial test of equal mass in equal size windows around -1.64

Observed interval	Window length	Observations below a	Observations above a	p-value
[–1.695; –1.585]	0.110	10	10	1.0000
[–1.750; –1.530]	0.220	15	21	0.4050
[–1.805; –1.475]	0.330	25	26	1.0000
[–1.860; –1.420]	0.440	34	33	1.0000
[–1.915; –1.365]	0.550	38	38	1.0000

Notes: Observed interval bounds are computed as $a \pm \frac{window\ length}{2}$.

C Alternative winsorization at 1st and 99th percentiles

In this Appendix we present the robustness checks for the results showed in Sections 3 and 4. We changed the winsorization thresholds from the 5th and 95th to the 1st and 99th percentiles of the CDF for the t and df variables. As in Section 3, we present the funnel plot for the new thresholds.

Figure C.1: Funnel plot with alternative winsorization

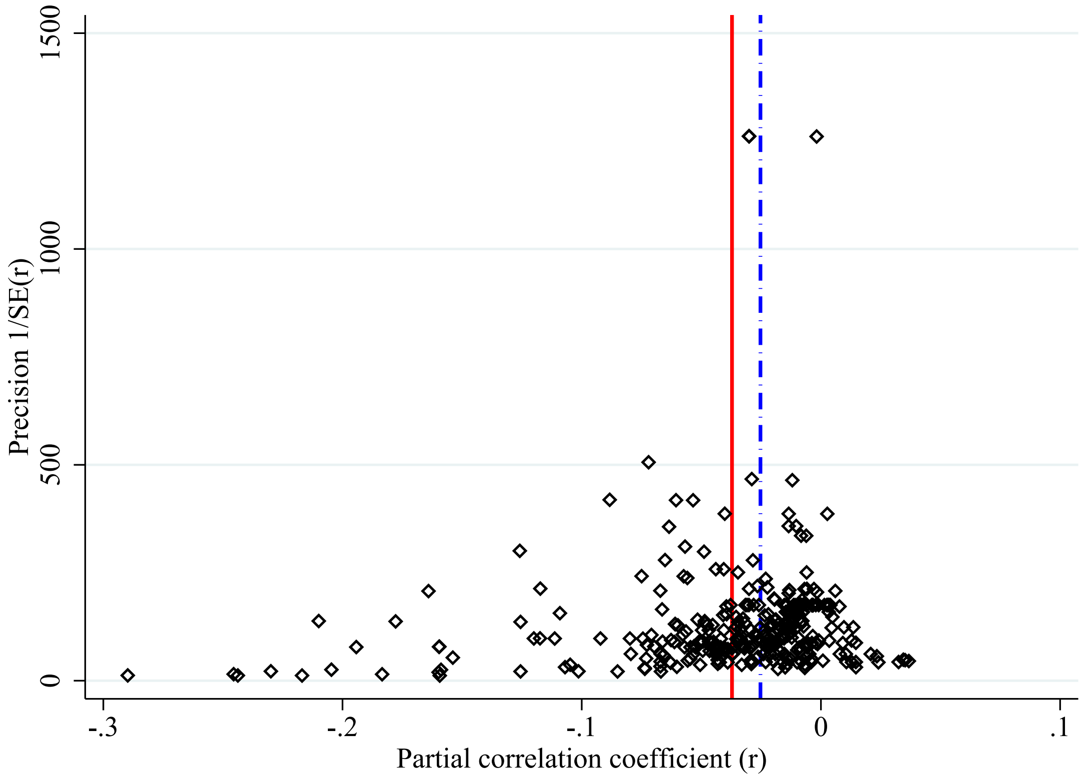


Table C.1: Meta-regression analysis and publication bias and correction with alternative winsorization

Variables	FAT-PET WLS-FE (1)	FAT-PET WLS-FE† (2)	PEESE WLS-FE (3)
Precision Effect (δ_0)	–0.0262***	–0.0256***	–0.0278***
	(0.0060)	(0.0060)	(0.0047)
Publication Bias (δ_1)	–0.6962	–0.9310	–28.9110*
	(0.8106)	(0.8446)	(15.5572)
R^2	0.0091	0.0146	0.0087

Notes: * and *** indicates a p-value < 0.1 and < 0.01, respectively. Standard errors robust to within-study correlation are reported in parenthesis.
† $SE(r_i)$ is replaced with the inverse of the square root of the sample size.

Table C.2: Publication bias and correction test by identification strategy with alternative winsorization

Variables	FAT-PET WLS-FE (1) $\hat{\delta}$	FAT-PET WLS-FE (1) $\hat{\sigma}$	PEESE WLS-FE (2) $\hat{\delta}$	PEESE WLS-FE (2) $\hat{\sigma}$
Precision Effect for Observables (δ_{10})	–0.0348***	0.0038	–0.0379***	0.0044
Precision Effect for DiD-IV (δ_{20})	0.0023	0.0037	–0.0114***	0.0021
Precision Effect for Fixed Effects (δ_{30})	–0.0120	0.0108	–0.0163**	0.0081
Publication Bias for Observables (δ_{11})	–1.4317*	0.7690	–29.6930	21.5887
Publication Bias for DiD-IV (δ_{21})	–2.3046***	0.4792	–35.7770***	1.4575
Publication Bias for Fixed Effects (δ_{31})	–1.5124	1.4566	–56.5873	71.8781
R^2	0.6310		0.6189	

Notes: * Significant at 10%, ** significant at 5%, *** significant at 1%. Given the small sample size, some categories were pooled as follows: 'Observables' contains CFA, DM, PSM and RE estimates (150 observations); 'Fixed Effect' contains FE, FEIV and MM estimates (128 observations); 'DiD-IV' contains DiD and IV estimates (49 observations).

Table C.3: Model averaging for uncertainty in the relationship between unemployment and health with alternative winsorization

	Variables	Bayesian Model Averaging (1)			Weighted-Average Least Square (2)			Ordinary Least Square (3)		
		$\hat{\theta}$	$\hat{\sigma}$	PIP	$\hat{\theta}$	$\hat{\sigma}$	$\lvert t\rvert$	$\hat{\theta}$	$\hat{\sigma}$	p-value
(a) Focus regressors										
	Precision Effect for Observables (δ_{10})	–0.0927	0.2513	1.00	–0.3919	0.8907	0.44	–0.0758	0.0139	0.000
	Precision Effect for DiD-IV (δ_{20})	–0.0431	0.2513	1.00	–0.3453	0.8913	0.39	–0.0253	0.0141	0.077
	Precision Effect for Fixed Effects (δ_{30})	–0.0521	0.2511	1.00	–0.3586	0.8903	0.40	–0.0327	0.0131	0.015
	Publication Bias for Observables (δ_{11})	–6.9156	19.4206	1.00	–1.4419	19.1943	0.08	–5.6597	29.7260	0.850
	Publication Bias for DiD-IV (δ_{21})	–45.4135	18.2974	1.00	–45.2195	18.0037	2.51	–45.4024	6.6071	0.000
	Publication Bias for Fixed Effects (δ_{31})	–40.5485	34.9558	1.00	–40.8029	34.2808	1.19	–44.0664	33.9451	0.199
(b) Auxiliary regressors										
(1)	*Health measures (reference: self-assessed health – SAH)*									
	Health Behaviors (BEH)	**–0.0223**	**0.0189**	**0.66**	**–0.0392**	**0.0105**	**3.75**	–0.0315	0.0143	0.031
	Health Care Utilization (HOS)	0.0003	0.0034	0.07	–0.0011	0.0095	0.11			
	Mental Health (MH)	**–0.0349**	**0.0049**	**1.00**	**–0.0376**	**0.0046**	**8.07**	–0.0348	0.0063	0.000
	Physical Health (PH)	–0.0011	0.0043	0.10	**–0.0146**	**0.0083**	**1.75**			
	Well-Being (WB)	**–0.0394**	**0.0046**	**1.00**	**–0.0415**	**0.0045**	**9.19**	–0.0396	0.0077	0.000
(2)	*Geographical area (reference: European countries)*									
	Non-European countries	**0.0219**	**0.0045**	**1.00**	**0.0148**	**0.0036**	**4.05**	0.0224	0.0073	0.003
	Multi-country	–0.0009	0.0030	0.12	–0.0043	0.0044	0.99			
(3)	*Sample age controls*									
	Sample average age if available	**0.0011**	**0.0004**	**0.98**	**0.0014**	**0.0004**	**3.43**	0.0010	0.0004	0.020
	Sample average age tot available	–0.0002	0.0016	0.08	**0.0049**	**0.0036**	**1.36**	–0.0026	0.0052	0.612
(4)	*Relevant study controls in regression analysis*									
	Health controls	–0.0015	0.0031	0.23	**–0.0064**	**0.0033**	**1.97**			
	Income controls	**0.0353**	**0.0048**	**1.00**	**0.0324**	**0.0040**	**8.01**	0.0364	0.0097	0.000
(5)	*Gender (reference: men)*									
	Men+Women	**0.0183**	**0.0079**	**0.93**	**0.0216**	**0.0051**	**4.23**	0.0190	0.0112	0.096
	Women	**0.0108**	**0.0082**	**0.71**	**0.0139**	**0.0049**	**2.85**	0.0154	0.0044	0.001
(6)	*Duration of unemployment (reference: duration not specified)*									
	Short term unemployment ($\leq$ 12 months)	**0.0203**	**0.0053**	**0.99**	**0.0185**	**0.0043**	**4.28**	0.0195	0.0094	0.042
	Long term unemployment (> 12 months)	**0.0157**	**0.0053**	**0.96**	**0.0165**	**0.0043**	**3.86**	0.0150	0.0076	0.052
(7)	*Reason for unemployment (reference: non-exogenous)*									
	Exogenous (e.g. plant closures)	**0.0223**	**0.0083**	**0.95**	**0.0199**	**0.0061**	**3.27**	0.0238	0.0049	0.000
(8)	*Relation with the unemployed (reference: herself/himself)*									
	Other (i.e. parent/partner)	0.0090	0.0117	0.44	**0.0273**	**0.0086**	**3.16**			
(9)	*Business cycle and labor market status*									
	Average GDP growth rate in time interval	–0.0006	0.0019	0.14	**–0.0052**	**0.0027**	**1.95**			
	Average unemployment rate in time interval	**–0.0060**	**0.0037**	**0.81**	**–0.0083**	**0.0023**	**3.53**	–0.0068	0.0043	0.119
(10)	*Study quality*									
	Average Scholar citations per year	0.0001	0.0001	0.22	**0.0002**	**0.0002**	**1.55**			
	SJR index	**–0.0043**	**0.0026**	**0.83**	**–0.0046**	**0.0015**	**3.03**	–0.0047	0.0040	0.244
(11)	Year of publication	0.00001	0.0001	0.06	0.0002	0.0004	0.36			

Notes: A value in bold indicates that the variable reached the cutoff and it is considered significant for the true model (*ie.* PIP > 0.5, for the BMA, $|t| \geq 1$, for the WALS). The WALS estimates are computed using the Laplace prior ($q = 1$). Results did not changed for the Subbotin prior ($q = 0.5$). They are not reported but are available upon request to the authors. For the OLS estimates, $R^2 = 0.8012$.