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ARTICLE

Unemployment and health: A meta-analysis

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Abstract

This paper reports a meta-analysis of the relationship between unemployment and health. Our meta-dataset consisted of 327 study results taken from 65 articles published in peer-reviewed journals between 1990 and 2021. We found that publication bias is important, but only for those study results obtained by means of difference-in-differences or instrumental variables estimators. On average, the effect of unemployment on health is negative, but quite small in terms of partial correlation coefficients. We investigated whether the findings were heterogeneous across several research dimensions. We found that unemployment has the strongest impact on the psychological domains of health and long-term unemployment spells are more detrimental than short-term ones. Furthermore, women are less affected, studies dealing with endogeneity issues find smaller effects and the health penalty is increasing with unemployment rate.

KEYWORDS
health, meta-analysis, meta-regression, publication bias, unemployment

JEL CLASSIFICATION
C52, I10, I12, J64

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1 INTRODUCTION

The literature on unemployment and its consequences continues to flourish. The effects of unemployment, in terms of both labor market outcomes and health, are of primary interest for research in various fields (Arulampalam, 2001; Fergusson et al., 2014; Gathergood, 2013; Jacobson et al., 1993; Kalousova & Burgard, 2014; Reine et al., 2013). The recent outbreak of the COVID-19 pandemic has given even more importance to this topic (Donnelly & 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 on which to rely, individuals lack the financial means which are necessary for their livelihood. This lack is likely to turn into lower consumption possibilities, which may affect either their diet or their routine habits, leading to a potential worsening of their health (Pieters & Rawlings, 2020). Furthermore, the occurrence of unemployment may lower reservation wages and depreciate human capital (Arulampalam, 2001; Chan & Stevens, 2001). Jahoda (1982) emphasized that unemployment is a threat not only because it can reduce financial resources, but also because it may erase a series of noneconomic elements, such as contributing to build social identity or providing opportunities to create social connections, which are deemed important for people's health. These elements may be referred to as latent benefits of employment. Janlert and Hammarström (2009) stated that economic deprivation and lost latent benefits are the two most appropriate and reliable notions with which to comprehend unemployment's consequences on health.

Fully understanding the impact of unemployment events is crucial from a policy perspective. When policy-makers set their objectives and calibrate their interventions, they may seek to minimize the trade-off between providing insurance against unemployment while maintaining incentives to avoid unemployment and the consequent depreciation of health and human capital of the unemployed (Hyslop et al., 2021; Spinnewijn, 2020). 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) obtained unclear 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 self-reported health and mental health declining after job loss. Otterbach and Sousa-Poza (2016) confirmed such results and extended these negative findings 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 was reduced by controlling for social and psychological mediators, such as self-esteem or social status. Marcus (2013) showed that unemployment generates negative spillovers on the laid-off worker's partner, who is impaired almost as much as s/he is. In addition, Nikolova and Ayhan (2019) found that the negative effect of unemployment on life satisfaction is mostly due to noneconomic costs. Böckerman and Ilmakunnas (2009) and Salm (2009) failed to find negative health effects of unemployment, either in the mental or in the physical domains of health. Bubonya et al. (2017) showed that such unemployment effects are nil also for the partners of unemployed persons, contradicting the findings in Marcus (2013). Finally, Johansson et al. (2020) pointed out marked discrepancies between results from self-reported health and those from more objective health measures, with the former being much more sensitive to the effects of unemployment than the latter.

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One of the main challenges in this strand of the literature is identifying 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 econometric technique employed or the type of sample used. Barnay (2016) pointed out that measurement 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 factors. For example, the interviewee's cultural heritage or the way and time in which the questionnaire is administered may play a crucial role in his/her reply to those questions on which the subjective or self-reported measures are constructed. Furthermore, individuals' responses often suffer from the so-called "justification bias," that is, the tendency of interviewees to adjust their responses according to the reference category to which they belong or to the social expectations relative to their status (McGarry, 2004; Schmitz, 2011). Moreover, under-reporting due to social stigma is a further crucial contributor to the measurement error problem (Bharadwaj et al., 2017). Finally, another problem consists in the structural difficulty of assessing in which direction causality runs, that is, reverse causality, because health deterioration may affect the probability of job loss. Indeed, Haan and Myck (2009) found a bidirectional causal effect.

In this paper, we report 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 great help in summarizing bodies of research literature, which have grown so 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 can, avoiding the risk of narrative reviews of under-(over-)reporting certain results in favor (at the expense) of others (Stanley et al., 2013).

Our meta-analysis is not the first to summarize the empirical relation between unemployment and measures of health. Paul and Moser (2009) collected results on 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 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 5 years after the job loss.

These meta-analyses have the feature in common of being either too narrowly focused on a single health dimension or not checking or weakly testing for publication bias and effect heterogeneity. Moreover, they consider unemployment from a broad perspective by either studying it as a cumulative event or by focusing on past spells. The main contribution of our paper is, therefore, that it provides 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; and (iii) analyses possible sources of heterogeneity among several characteristics of the study results. Moreover, in order to avoid criticisms of arbitrary choices in the study selection criteria and in the modeling techniques to aggregate study results, we have followed the

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guidelines of the Meta-Analysis of Economics Research Network (MAER-Net) (Havránek et al., 2020; Stanley et al., 2013). These guidelines are aimed at creating a shared agreement in approaching meta-analyses in economics and thereby at improving transparency, replicability and quality of the reported meta-analytic results. Hence, the ultimate goal of our meta-analysis is to provide a comprehensive picture of 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, our effect size, and its characteristics (e.g., type of health outcome, identification strategy, reason for unemployment, etc.) for the analysis of the effect heterogeneity. Section 3 addresses 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 rather heterogeneous in the definition of unemployment. We decided to focus on studies which defined unemployment as the current status/situation of the individual, that is, *current unemployment*. Hence, we removed those studies in which the treatment was cumulative unemployment occurrences or unemployment events in the past, independently of the status of the individual at the time of the interview (see, e.g., Kalousova & Burgard, 2014). The purpose of this selection was to have in our sample results which were as homogeneous as possible because they dealt with the effect of a single unemployment episode on an individual's health, instead of getting mixed with the consequences of a repetition of unemployment events, for which the number of occurrences may be relevant, or with long-term impacts, for which the duration between the treatment and the measurement of the outcome variable may be key for obtaining different findings. Indeed, when the sample includes studies that are too heterogeneous in the definition of the treatment, but also in other study characteristics, the "apples and oranges problem" (Rosenthal & DiMatteo, 2001; Sharpe, 1997) may arise. This is the problem of the interpretability of the meta-analytic findings when studies, which are too diverse, are pooled (Eysenck, 1978) without proper consideration of their heterogeneity. Since we could not find precise information with which to code the heterogeneity in terms of number of unemployment spells or a time-horizon for long-term effects, we preferred not to include them in our sample.

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 by sticking to a rigid definition of unemployment, we chose not to discard those studies not applying the ILO definition of unemployment.

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

Finally, we only considered studies which employed microdata and sought to find evidence at individual level.

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2.1 | Search strategy and selection criteria

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

Between October 2021 and December 2021, we searched for primary studies published from 1990 until 2021 using four scientific databases: Web of Science (WoS), Scopus, Science Direct, and IDEAS/RePEc. We started our search with WoS using a combination of the following keywords in the papers' titles: ("unemployed" or "unemployment" or "parental unemployment" or "partner unemployment" or "spouse unemployment") and ("well-being" or "health"). We obtained 746 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, therefore excluding working papers, book chapters, reports, and theses.[1]
2. We restricted the sample to subject categories relevant to the topic analyzed (*Economics*, *Health Policy Services*, *Social Sciences Interdisciplinary*, *Psychology Social*, *Psychology Multidisciplinary*, *Management*, and *Industrial Relations Labor*).[2]
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 65 papers. We then moved to the next stage, that is, the "full text screening" (FTS).

We repeated the same PTS in Scopus. After removing duplicates, that is, articles already obtained using WoS, we were able to add 40 new papers to the 65 obtained using WoS. Thereafter, we used the same PTS in Science Direct and IDEAS/RePEc. After removing duplicates, we added seven more articles from Science Direct and six from IDEAS/RePEc, for a total of 118 studies admitted to the FTS. Finally, we followed Stanley and Doucouliagos (2012), and after carefully reading the related literature reviews, we included 11 additional studies not detected by the previous searches.[3] At this point we had 129 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. In regard to the PTS, the referring boxes are highlighted in the upper-left part of Figure 1. The first column of boxes presents the total number of studies obtained from each scientific database only using the keywords. The second column presents the number of articles left after the PTS and removing duplicates.

The pointed grid in the upper-right part of Figure 1 visually explains the steps of the FTS. We began with the exclusion of those studies, which we judged problematic 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 modeling (see, e.g., Fors Connolly & Gärling, 2022; Houssemand & Meyers, 2011; Lai et al., 1997; Schwarzer et al., 1994).

We excluded also studies in which the model specification presented interactive terms on the coefficient(s) of interest (Stanley & 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) (Dolan & Powdthavee, 2012).

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 (Stauder, 2019; Theodossiou,

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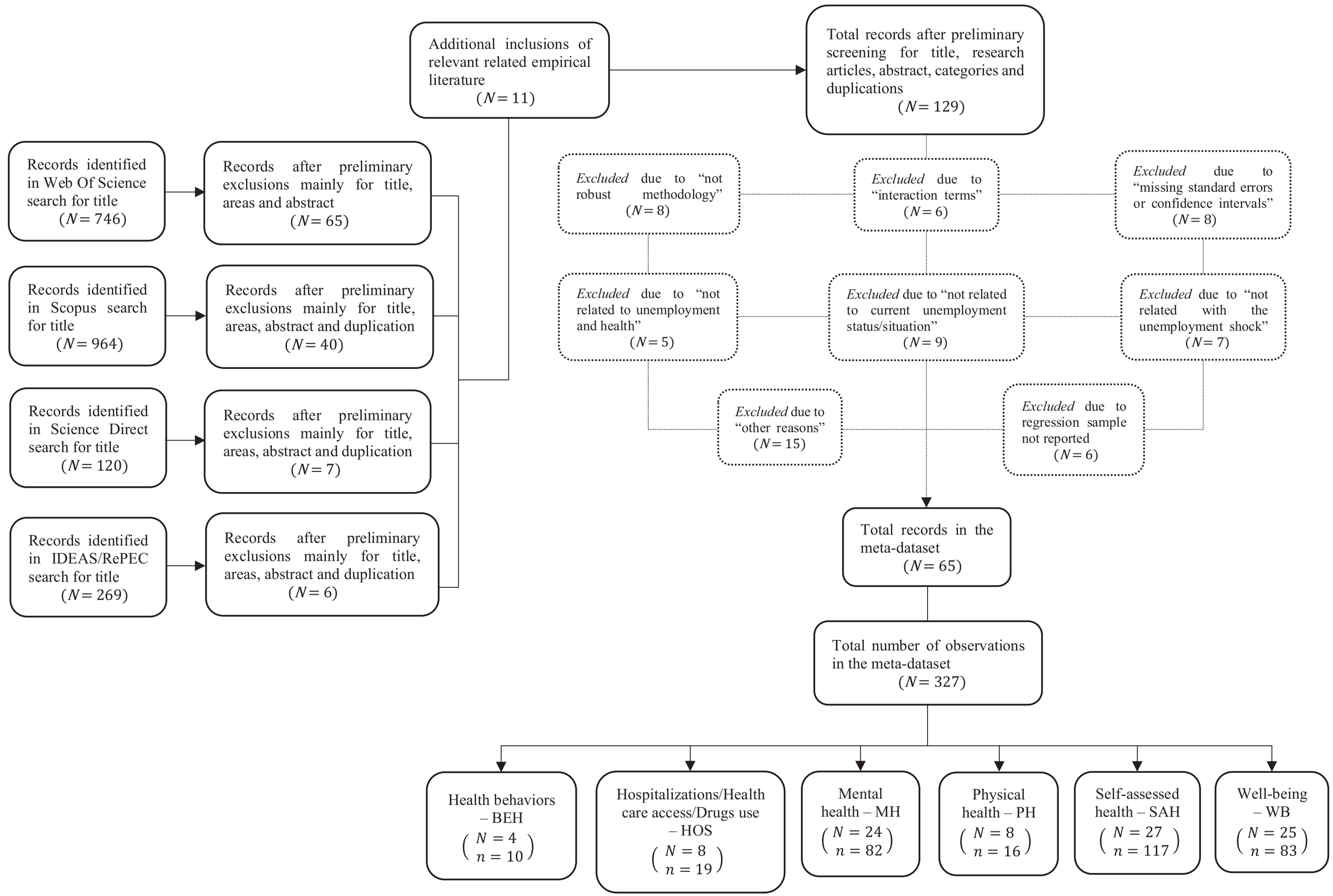

FIGURE 1 PRISMA flow chart.

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.

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1998; Taht et al., 2020). Moreover, we excluded those papers in which the treatment was not *current unemployment* (de Goede & Spruijt, 1996; Kalousova & Burgard, 2014; Lam & 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 (Axelsson & Ejlertsson, 2002; Hamilton et al., 1997; Hald Andersen, 2009; Hamilton et al., 2015; Lee et al., 2021).

We removed 15 more articles for "other reasons," that is, because the empirical analysis was not at the micro-level (Monsef & Shahmohammadi Mehrjardi, 2018), the effect size of unemployment was not computable for reasons different from those mentioned above (Crost, 2016; Kozieł et al., 2010; Sousa-Ribeiro et al., 2014; Sage, 2015), or the analysis was conducted on a sample of only unemployed individuals (Korpi, 1997; Strandh et al., 2013; Takahashi et al., 2015).

Finally, we dropped six studies because they did not contain information on the sample size, which is fundamental for computing the effect size of each study result (Beland et al., 2002; Buffel et al., 2017; Colman & Dave, 2018; Cooper et al., 2008; Milner et al., 2016; Sleskova et al., 2006). In fact, we opted for the partial correlation coefficient (r) as a measure of the effect size, and the sample size is needed for its computation.

In the end, we had a final sample of 65 primary studies for a total of 327 results. Table A in appendix reports all the articles with the associated relevant information. The number of study results is more than five times larger than the number of selected articles because one study might contain several results for different reasons. For example, one study might 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).

2.2 | Effect size

For each of the 327 study results in our meta-sample, we computed the corresponding t-statistic either by 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 & 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 two cases (0.6%) was the effect positive.

Panel (a) of Figure A.1 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 A.1 clearly presents extreme values. This raises concerns, because outlying observations may generate systematic distortions in a regression analysis (Zaman et al., 2001) like the one reported in this paper. One way to prevent distorted results due to deviant observations is winsorization, that is, 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 A.1 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 about the magnitude of the effect. We decided, therefore, to move on from it and make a more suitable choice, that is, the partial correlation coefficient (r) (Rosenthal & DiMatteo, 2001). Contrary to the t-statistic, the partial correlation coefficient

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conveys information about 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 with the partial correlation coefficient and not simply by using the estimated coefficients as reported in the selected studies. Our study results were heterogeneous for several reasons. The estimated effects came from models with different specifications. Some models were linear in the estimated parameters, some others were nonlinear. The set of covariates included in the equations to be estimated varied among studies. Although we selected only articles with a similar interpretation and definition of unemployment, there were discrepancies in the definition of the treatment variable. Last but not least, the measure of health was very dissimilar among studies, both because the scale might be different given the same kind of health measure, and because the health measures varied 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 entailed that almost each study result had its own interpretation, which was not directly comparable to the others. Because the partial correlation coefficient (r) is a unit-free measure retaining information on the magnitude of the effect, it restores comparability among study results (Rosenthal, 1991) and it is now widely employed by meta-analyses in the economic literature (see, e.g., Doucouliagos, 1995; Doucouliagos & Laroche, 2003, 2009; Filomena & Picchio, 2023; Picchio, 2023; Xue et al., 2020, 2021).

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 although 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), who attempted to refine the rule of thumb in Cohen (1988) to assess if a correlation is large, looked at the empirical distribution of thousands of partial 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 called "large," "medium," and "small," respectively.[4]

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 could be extracted without problems, because the estimated parameters and their standard errors were almost always reported. Nevertheless, this was sometimes not the case, for example because the author 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 cases, 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 in the manner explained in Picchio (2023, §3.3.3).[5]

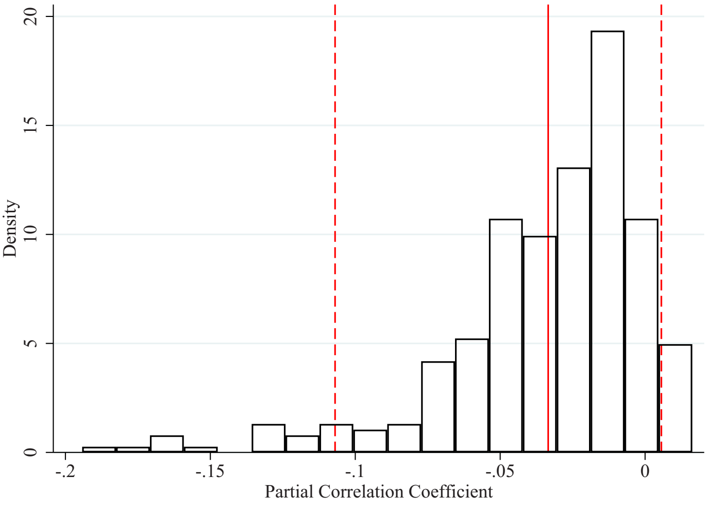

FIGURE 2 Density plot of the partial correlation coefficient (r).
[Colour figure can be viewed at wileyonlinelibrary.com]
Notes: The red dashed lines are the 5th and 95th percentiles, respectively. The red solid line is the average value (−0.033).

The other ingredient for the computation of the partial correlation coefficient, the degrees of freedom, may be difficult to retrieve in many cases. Whereas the size of the sample used to obtain 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 not possible to retrieve it even 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 so, we approximated it with the sample size minus 2.[6] In our sample, the minimum value of df was 76, while the maximum was about 18 million. Similarly to the winsorization based on the distribution of the t-statistic, we performed a winsorization on the degrees of freedom df. Figure A.2 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, that is, the partial correlation coefficient r, was computed using the winsorized versions of both the t-statistics and the degrees of freedom.[7] 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 be interpreted as unemployment negatively (positively) affecting health. Figure 2 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.

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.

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 for 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,

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TABLE 1 Summary statistics of the partial correlation coefficient (r) by health outcome.

	Mean	Median	Std. Dev.	95% Confidence interval	Min.	Max.	Obs.
Overall	−0.0334	−0.0253	0.0346	[−0.0372; −0.0297]	−0.1942	0.0163	327
By type of health outcome							
Health behaviors (BEH)	−0.0199	−0.0181	0.0310	[−0.0420; 0.0023]	−0.0742	0.0160	10
Health care utilization (HOS)	−0.0294	−0.0329	0.0260	[−0.0419; −0.0168]	−0.0795	0.0152	19
Mental health (MH)	−0.0475	−0.0351	0.0428	[−0.0569; −0.0381]	−0.1942	0.0079	82
Physical health (PH)	−0.0193	−0.0101	0.0441	[−0.0428; 0.0042]	−0.1734	0.0143	16
Self-assessed health (SAH)	−0.0238	−0.0181	0.0230	[−0.0281; −0.0196]	−0.1113	0.0163	117
Well-being (WB)	−0.0384	−0.0352	0.0342	[−0.0458; −0.0309]	−0.1672	0.0079	83

Note: Column “Mean” displays the unweighted average of the partial correlation coefficient.

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how much they were satisfied about their life. Headey et al. (1993) assessed that although life satisfaction is not strictly conceivable as health, it displays 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, ranging 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 (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. In 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, for instance dietary 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 varies somewhat 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 instead shows the lowest value. Therefore, the psychological domains of health seem to be the most exposed to unemployment, whilst the physical side seems the least affected.

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

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

As anticipated in the previous subsection, one of the dimensions across which we distinguished the results was the type of health outcome. The next dimension was the identification strategy used to estimate the health effects of unemployment. Different identification assumptions and different estimation methodologies may play a significant role in the estimation of the causal effect of unemployment on health (Avendano & 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 respectively accounted for 31.5 and 34.9% of our sample. The next two strategies often employed are difference-in-differences and duration models, which amounted to 11.0 and 6.4% of the sample, respectively.

Because unemployment may be more harmful in areas with a weaker welfare system, we included controls for the geographical area to which the study referred. About 65% of the study results referred to European countries. Because an older worker's job separation is often a

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 not 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 (≤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

(Continues)

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TABLE 2 (Continued)

	Variables	Observations	Mean	Std. dev.
(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

one-way street into unemployment, we controlled for the average sample age, which was 40 years on average.[8] In addition, since single-breadwinner models based on gender still persist 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 came from samples made up of both men and women, and 15% of the study results came from female samples.

We coded the presence of controls for income or previous health in the regression analysis. Their inclusion in modeling 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 came 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 unemployment spell. Since the longer the unemployment spell, the higher the depreciation of human capital and the tighter the liquidity constraints, we expected the negative health effects of unemployment to increase according to its duration (Becker, 1962; Grossman, 1972, 2000). We coded the duration of unemployment into three categories: short and long, following the ILO definition,[9] and a third residual category for those study results which did not provide information about the duration of the unemployment event.

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

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

There is a debate on whether the health effects of unemployment are exacerbated or mitigated by the macroeconomic conditions. On the one hand, the occurrence of unemployment may be less harmful in economic downturns because individuals may feel less stigmatized when unemployment becomes the prevailing social norm (Clark, 2003; Clark et al., 2010; Chadi, 2014). On the other hand, losing one's job during an economic downturn may impair mental health to a greater extent because the laid-off worker may fear that his/her job loss is a one-way street into unemployment given the bad economic situation. Thus, in order to control for the business cycle and 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.[10]

The scientific quality of a study may be related to the effect size. Studies that more strictly follow epistemologically consistent rules of research conduct may be less subject to questionable research practices, like HARKing (Kerr, 1998) and the selective reporting of results, which affect the published study results. In order to proxy for the quality of the studies, we used the SCImago Journal Ranking (SJR) index,[11] 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 yet available; (iii) the article was 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 it was indexed in SCImago for the first time.

Finally, as suggested by the MAER-Net guidelines (Havránek et al., 2020), we controlled for publication year. Publication year may be related to the effect size, because new or refined methodologies with which to identify the causal effect may have been developed over time. Moreover, the publication year is a proxy for the year of the data used and may, therefore, be related to the effect size if, for example, institutions have changed. The average publication year is 2013, due to the fact that the number of studies on unemployment and health have grown considerably in the past twenty years.

3 | DETECTING PUBLICATION BIAS

3.1 | Visual inspection

One of the most common threats to the validity of a meta-analysis is publication bias, which occurs when certain results are more likely to be published than others. Typically, but not necessarily, these results are those that achieve statistical significance, which may be found more interesting by journals and editors (Brodeur et al., 2016; Franco et al., 2014).[12] Publication bias may also arise as a researcher's response to the difficulty of publishing insignificant results. For example, a researcher may run many regressions, detect some significant outcomes, look for ad hoc theoretical reasons to explain them, and not report the insignificant findings (Picchio, 2023). The meta-sample is, thus, affected by sample selectivity, which undermines the conclusions of the meta-analysis. Publication bias affects the majority of social and medical sciences, including economic areas of research. See, among others, Stanley & 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 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

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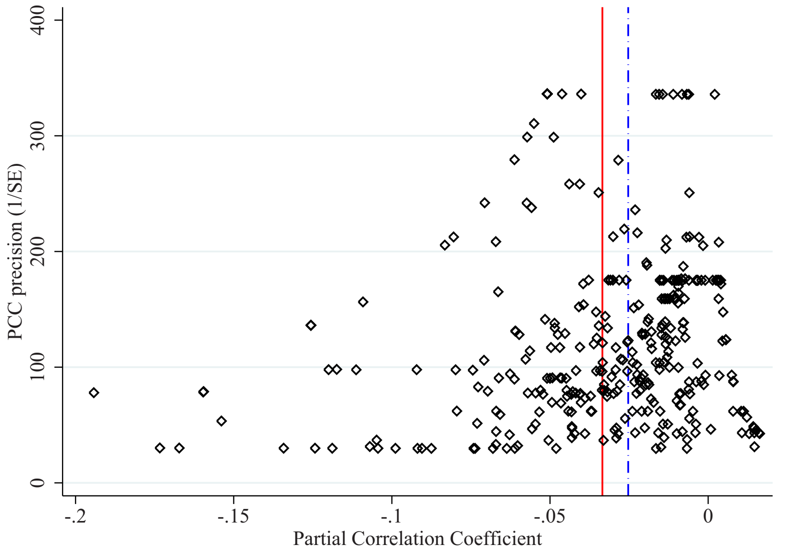

FIGURE 3 Funnel plot.
[Colour figure can be viewed at wileyonlinelibrary.com]
Notes: The red solid line indicates the unweighted average of the partial correlation coefficient. The blue dashed line shows the median.

the shape of an inverted 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 3 is our funnel plot. The vertical (red) solid and (blue) dashed lines indicate the average and the median of the partial correlation coefficients, respectively. The strong asymmetry with the pronounced left-skewness may suggest that publication bias is present and substantial in our meta-sample. Although unemployment may theoretically affect health both positively and negatively, common sense typically associates unemployment with a worsening of the situation.[13] The shape of the asymmetry is then consistent with the selective publication of positive results, that is, negative unemployment effects on health are preferentially reported and positive unemployment effects are under-represented. This may be due, for example, to the use by researchers of the criterion of a negative and significant unemployment effect as a guide for their empirical specification (Card & Krueger, 1995). This selectivity is more likely to be carried out if the precision is low. Indeed, averaging 10 percent of the most precise estimates, because publication bias should be negligible at the top of the funnel plot (Stanley & Doucouliagos, 2010), returns a partial correlation coefficient equal to −0.0369, whilst it is almost twice as big in absolute value (−0.0660) for the worst 10% estimates.[14]

However, the funnel plot is not a formal test. Moreover, it is based on the hypothesis that there is a homogeneous "true" effect common to all results. If results are heterogeneous, for example because they are derived from studies using different populations or methodologies (Stanley & Doucouliagos, 2010), the funnel's skewness may arise as a statistical artifact. Hence, any conclusion drawn from this visual inspection should be taken with caution. In the next subsection, we use an extensive set of bias-adjusted methods to test whether this asymmetry is formally present. Later, in Section 4, we move away from univariate publication bias tests to take explicit account of potential systematic heterogeneity in a multivariate meta-regression framework.

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TABLE 3 Publication bias testing and correction.

	FAT-PET		PEESE	WAAP
	WLS-FE	WLS-FE[a]	WLS-FE	WLS-FE
	(1)	(2)	(3)	(4)
Precision effect (δ_0)	−0.0287***	−0.0273***	−0.0288***	−0.0293***
	(0.0067)	(0.0068)	(0.0048)	(0.0047)
Publication bias (δ_1)	−0.1946	−0.4713		
	(0.6750)	(0.7143)		
Variance (δ_1)			−22.0522	
			(19.5321)	
Sample size	327	327	327	180
R^2	0.0008	0.0040	0.0056	0.0000

Note: Clustered standard errors robust to heteroskedasticity and within-study correlation are reported in parentheses.
a$SE(r_i)$ is replaced with the inverse of the square root of the sample size.
***Significant at 1%.

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 by 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, that is, 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 should be nil. If, after the estimation of Equation (3), the null hypothesis of $\delta_1 = 0$ is not rejected, there is evidence of no publication bias, translating into the symmetry of the funnel plot. In the 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 the evidence of the significance of the unemployment effect on health on average, 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. 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 was 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 replaced $SE(r_i)$ with its square

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(variance). 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 nonzero effect arising from FAT-PET (Stanley & Doucouliagos, 2012, 2014).

In Models (1) and (2), the estimate of δ_1 is not significantly different from zero, which may suggest the absence of publication bias. FAT-PET estimates of δ_0 indicate that there is a significantly negative effect of unemployment on health. This is confirmed by PEESE's estimate. However, the size of the effect, which is very stable across the three models, is fairly small (Doucouliagos, 2011).[15]

An important question is whether the findings in Models (1) and (2) on publication bias are the consequence of low statistical power (Stanley et al., 2017). Low statistical power estimates pose a major challenge to empirical research. If a body of research literature consistently presents underpowered results, the credibility of its conclusions may be questionable (Ioannidis et al., 2017). We followed Ioannidis et al. (2017) and focused the analysis only on adequately powered estimates in the meta-sample. An estimate was considered adequately powered when its probability of committing a type-II error is less than or equal to 20% (see Ioannidis et al., 2017, for more details). This approach is referred to as weighted average of the adequately powered (WAAP) and it is computed using WLS on the most informative estimates in the meta-sample. It yields more reliable estimates of the true empirical effect because it is less exposed to selection bias (Ioannidis et al., 2017). Column (4) of Table 3 presents the WAAP results. Of the 327 original primary studies, only 180 results were adequately powered (approximately 55% of the meta-sample). The estimates of δ_0 do not substantially change.

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 took up this idea and tested if t-statistics show a significant discontinuity at -1.96. We did not detect evidence of publication bias. We describe in detail how we did it and what we obtained in Online appendix B.

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. Although performing multiple tests is of help in assessing the robustness of a result, no method comes without limitations (Bartoš et al., 2023; Carter et al., 2019). A comparative analysis of multiple methods for a single hypothesis requires that researchers know which are the ones best suited to use, which in turn requires knowledge about the data-generating process. However, researchers seldom possess such knowledge. Bartoš et al. (2023) proposed a novel approach, which does not require a priori all-or-none decisions: the robust bayesian meta-analysis (RoBMA). This involves using Bayesian model averaging (BMA) to simultaneously estimate several models under different assumptions and infer which one best predicts the data. RoBMA is therefore less likely to be affected by distortions due to model misspecification (Bartoš et al., 2023). Models can be grouped according to three categories, one for each component in a meta-analytic study and each reflecting a different hypothesis: (i) models assuming the presence of an effect; (ii) models assuming the presence of heterogeneity; and (iii) models assuming the presence of publication bias. In each category, each model is assigned the same individual prior probability. It is then estimated, and its posterior model probability (PMP) is updated according to the Bayes' rule. Models predicting the data well receive a boost, whilst the others lose relevance. The predictive performance of a (class of) model(s) can be assessed by the "Bayes factor" (BF). This is the ratio between the marginal likelihood of two (classes of) models and makes it possible to distinguish between "absence of evidence" and "evidence of absence." The following rule of thumb eases the interpretation: if the value of the BF is

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TABLE 4 Robust Bayesian meta-analysis.

			95% Confidence interval	
	Mean	**Median**	**Lower bound**	**Upper bound**
(a) Model-averaged estimates of effect size and heterogeneity parameters				
Effect size (δ_0)	−0.032	−0.032	−0.035	−0.028
Heterogeneity (τ)	0.029	0.029	0.027	0.032
	Models	Prior probability	Posterior probability	Bayes factor
(b) Model components				
Effect	10/20	0.500	1.000	9.411×10^{46}
Heterogeneity	10/20	0.500	1.000	∞
Publication bias	16/20	0.500	0.042	0.044

Note: The number of observations used was 327. The "Models" column indicates the total amount of models used in RoBMA. The numerator refers to the number of models that assumed the presence of a specific hypothesis. The "Prior probability" column refers to the *ex ante* probability assigned to each model in each category. The "Posterior probability" column shows the *ex post* probability with which each model satisfying the corresponding hypothesis is included in the relevant ensemble after the analysis.

above (below) 10 (0.1), there is "strong" evidence in favor of (against) a specific hypothesis. If it is between 3 and 10 (between 0.1 and 0.66), there is "moderate" evidence in favor of (against) a specific hypothesis, and if it is between 1 and 3 (between 0.66 and 1), there is "weak" evidence in favor of (against) a specific hypothesis. The final effect size is computed as a weighted average across all models, using the PMPs as weights. From simulations studies, RoBMA outperforms other conventional bias-corrected methods in terms of mean squared error and is robust to misspecification (Bartoš et al., 2023).

We performed RoBMA, using 20 different models.[16] Table 4 presents the model-averaged estimates of the effect size and the heterogeneity parameters (in panel a) and the results of model components (in panel b). The average value for the effect size is −0.032, which is in line with estimates from previous meta-regression models. Consistently with previous tests, RoBMA suggests that there is strong evidence against the presence of publication bias (BF is equal to 0.044). Furthermore, there is also strong evidence in favor of an effect (BF equal to 9.411×10^{46}). Finally, heterogeneity is extremely likely to be present.

The strong evidence in favor of the presence of heterogeneity sheds doubts on the visual inspection and the formal tests for publication bias conducted so far, because they are based on the absence of heterogeneity. To delve further into the issue of publication bias by accounting for heterogeneity, we hypothesized that publication bias may be linked to study characteristics. Brodeur et al. (2020) showed that, in 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. Therefore, we considered the possibility that publication bias may correlate with the methodology used to identify the health effects of unemployment. We generalized the 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 dummies for the method used to identify the health effect. $\boldsymbol{\delta}_0$ is the parameter vector corresponding to PET component. The parameter vector $\boldsymbol{\delta}_1$ associated with the interaction term $\mathbf{Z}_i \times SE(r_i)$ is instead the FAT component, and it captures the eventual presence of publication bias when using a specific methodology for identification of the causal effect. We

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		
Publication bias for DiD-IV (δ_{21})	−2.2796***	0.5285		
Publication bias for fixed effects (δ_{31})	−0.4810	1.3256		
Variance for observables (δ_{11})			19.8401	19.4871
Variance for DiD-IV (δ_{21})			−67.1271***	13.0440
Variance for fixed effects (δ_{31})			−31.0920	49.9201
Sample size	327		327	
R^2	0.6988		0.6980	

Note: Clustered standard errors robust to heteroskedasticity and within-study correlation are reported in parenthesis. The "Observables" category includes CFA, DM, PSM, and RE estimates (150 observations). The "Fixed effects" category contains FE, FEIV, and MM estimates (128 observations). The "DiD-IV" category includes DiD and IV estimates (49 observations).
** Significant at 10%, *** significant at 1%.

defined three categories for the methodology employed and sorted the study results accordingly. 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.[17] 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 nonzero effect (Stanley & Doucouliagos, 2012, 2014), suggest that the strongest negative effect arises from study results with an identification strategy based on observables. When endogeneity concerns are tackled more seriously, the average effect size moves towards zero, although it is still statistically different from zero.

To sum up, publication bias is not of much concern, except 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. In the next section, using the PEESE specification, we conduct a multivariate analysis to delve much more deeply into the determinants of the heterogeneity of the effect size.

4 | META-REGRESSION ANALYSIS FOR EFFECT HETEROGENEITY

4.1 | A multivariate analysis for uncertainty

In the previous section, we detected the strong presence of heterogeneity within our meta-sample and heterogeneous publication bias among the identification strategies. The PEESE correction for

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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. Equation (5) incorporates the PEESE correction for publication bias, which has to be preferred to FAT-PET if the precision effect is not nil, and "can address possible interaction of funnel plot asymmetry and moderator variables by simultaneously fitting a meta-regression and a publication bias model" (Stanley & Doucouliagos, 2014).[18] The WLS-FE estimation of Equation (5), recommended by Stanley and Doucouliagos (2017) and recently used by, for example, Doucouliagos et al. (2020), Xue et al. (2021), and Filomena and Picchio (2023), 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 problems 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 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" (PMPs) 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 is 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 but not fundamental interest, so that 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 to or greater than 0.5 (Xue et al., 2021). In our case, the focus regressors were the covariates that we had used previously for the publication bias analysis by econometric methodology for the identification of the causal effect. The auxiliary regressors were instead all the other variables presented in 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 & 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) (De Luca and Magnus, 2011; Magnus et al., 2010). The WALS estimator is a hybrid between the Bayesian and the frequentist approaches. It differs from the BMA in two respects. 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.[19] Finally, Model (3) presents the frequentist check; we estimated by means of 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.[20] 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 WLS 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 among 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 suggest that older cohorts suffer less from unemployment than younger ones do. 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 find it increasingly difficult to relocate themselves in the labor market in the case of job loss. On the other hand, younger individuals may be subject to more binding budget constraints, for example because they are more likely to have dependent children and mortgages to repay, and therefore suffer the most severe health consequences after job loss. Our findings suggest that the last effect dominates.[21]

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. Controlling for it, therefore, corrects for an omitted variable bias, which would instead bias the relationship downwards. 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 it crucial to be part of the active population because of societal expectations. For example, the society may regard them as the main financial providers of the household. A shift into unemployment deprives them of this role and triggers blame or shame. Furthermore, women may feel unemployment to a lesser extent because societal expectations may see their familial role as a valid substitute for 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. We find that long-term unemployment spells impair health more than short-term ones.[22]

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TABLE 6 Model averaging for uncertainty in the relation between unemployment and health.

Variables	BMA (1) Coefficient	PIP	WALS (2a) $q = 1$ Coefficient	$\|t\|$	(2b) $q = 0.5$ Coefficient	$\|t\|$	WLS frequentist check (3) Coefficient	95% Confidence interval	*p*-value
(a) Focus regressors									
Precision effect for observables (δ_{10})	−0.0485	1.00	0.0868	0.13	0.0943	0.13	−0.0632	[−0.0791; −0.0473]	.000
	(0.1595)		(0.6922)		(0.7099)		(0.0079)		
Precision effect for DiD-IV (δ_{20})	−0.0059	1.00	0.1292	0.19	0.1369	0.19	−0.0184	[−0.0344; −0.0024]	.025
	(0.1596)		(0.6930)		(0.7108)		(0.0080)		
Precision effect for fixed effects (δ_{30})	−0.0182	1.00	0.1160	0.17	0.1234	0.17	−0.0314	[−0.0476; −0.0152]	.000
	(0.1593)		(0.6918)		(0.7094)		(0.0081)		
Variance for observables (δ_{11})	11.3770	1.00	7.9570	0.50	6.9967	0.44	12.8742	[−32.5733; 58.3218]	.573
	(15.3754)		(15.8925)		(15.9961)		(22.7496)		
Variance for DiD-IV (δ_{21})	−95.7208	1.00	−89.9203	3.41	−90.9527	3.44	−96.4920	[−129.5158; −63.4682]	.000
	(26.1156)		(26.3703)		(26.4104)		(16.5307)		
Variance for fixed effects (δ_{31})	−39.3030	1.00	−39.5611	1.55	−40.0627	1.56	−37.8776	[−97.0208; 21.2656]	.205
	(26.1240)		(57.4832)		(25.6067)		(29.6052)		
(b) Auxiliary regressors									
(1) *Health measures (reference: self-assessed health —SAH)*									
Health behaviors (BEH)	−0.0054	0.30	**−0.0204**	**2.40**	**−0.0230**	**2.60**			
	(0.0095)		**(0.0085)**		**(0.0089)**				
Health care utilization (HOS)	−0.0001	0.05	0.0000	0.00	0.0002	0.03			
	(0.0017)		(0.0068)		(0.0070)				
Mental health (MH)	**−0.0258**	**1.00**	**−0.0225**	**6.16**	**−0.0236**	**6.32**	−0.0256	[−0.0347; −0.0164]	.000
	(0.0033)		**(0.0036)**		**(0.0037)**		(0.0046)		
Physical health (PH)	0.0000	0.04	−0.0017	0.29	−0.0023	0.37			
	(0.0013)		(0.0059)		(0.0061)				
Well-being (WB)	**−0.0287**	**1.00**	**−0.0261**	**7.38**	**−0.0279**	**7.70**	−0.0286	[−0.0385; −0.0187]	.000
	(0.0032)		**(0.0035)**		**(0.0036)**		(0.0050)		

(Continues)

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TABLE 6 (Continued)

	Variables	BMA (1)		WALS (2a) $q = 1$		(2b) $q = 0.5$		WLS frequentist check (3)		
		Coefficient	**PIP**	**Coefficient**	**$\|t\|$**	**Coefficient**	**$\|t\|$**	**Coefficient**	**95% Confidence interval**	***p*-value**
(2)	*Geographical area (reference: European countries)*									
	Non-European countries	0.0001	0.06	−0.0011	0.33	−0.0015	0.44			
		(0.0010)		(0.0033)		(0.0033)				
	Multicountry	−0.0011	0.17	**−0.0053**	**1.49**	**−0.0059**	**1.61**			
		(0.0030)		**(0.0036)**		**(0.0037)**				
(3)	*Sample age controls*									
	Sample average age if available	**0.0010**	**0.99**	**0.0009**	**2.90**	**0.0009**	**2.87**	0.0011	[0.0002; 0.0020]	.018
		(0.0003)		**(0.0003)**		**(0.0003)**		(0.0004)		
	Sample average age not available	**0.0087**	**0.89**	**0.0075**	**2.40**	**0.0077**	**2.36**	0.0098	[0.0015; 0.0180]	.021
		(0.0042)		**(0.0031)**		**(0.0033)**		(0.0041)		
(4)	*Relevant study controls in regression analysis*									
	Health controls	−0.0002	0.08	**−0.0045**	**1.56**	**−0.0051**	**1.67**			
		(0.0012)		**(0.0029)**		**(0.0030)**				
	Income controls	**0.0138**	**1.00**	**0.0135**	**4.06**	**0.0147**	**4.22**	0.0140	[0.0031; 0.0249]	.013
		(0.0032)		**(0.0033)**		**(0.0035)**		(0.0055)		
(5)	*Gender (reference: men)*									
	Men + Women	**0.0136**	**0.88**	**0.0122**	**2.74**	**0.0129**	**2.82**	0.0145	[0.0014; 0.0275]	.030
		(0.0066)		**(0.0044)**		**(0.0046)**		(0.0065)		
	Women	**0.0113**	**0.89**	**0.0102**	**3.00**	**0.0103**	**2.94**	0.0129	[0.0067; 0.0192]	.000
		(0.0053)		**(0.0034)**		**(0.0035)**		(0.0031)		
(6)	*Duration of unemployment (reference: duration not specified)*									
	Short term unemployment (≤12 months)	**0.0211**	**1.00**	**0.0165**	**4.95**	**0.0175**	**5.11**	0.0210	[0.0121; 0.0299]	.000
		(0.0038)		**(0.0033)**		**(0.0034)**		(0.0045)		
	Long term unemployment (>12 months)	**0.0139**	**0.98**	**0.0117**	**3.61**	**0.0126**	**3.79**	0.0141	[0.0020; 0.0261]	.023
		(0.0042)		**(0.0032)**		**(0.0033)**		(0.0060)		

(Continues)

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TABLE 6 (Continued)

	Variables	BMA (1) Coefficient	PIP	WALS (2a) $q = 1$ Coefficient	$\lvert t \rvert$	(2b) $q = 0.5$ Coefficient	$\lvert t \rvert$	WLS frequentist check (3) Coefficient	95% Confidence interval	p-value
(7)	*Reason for unemployment (reference: nonexogenous)*									
	Exogenous (e.g., plant closures)	**0.0227**	**1.00**	**0.0164**	**3.36**	**0.0168**	**3.22**	0.0228	[0.0118; 0.0337]	.000
		(0.0048)		**(0.0049)**		**(0.0052)**		(0.0055)		
(8)	*Relation with the unemployed (reference: herself/himself)*									
	Other (i.e., parent/partner)	**0.0291**	**1.00**	**0.0240**	**4.26**	**0.0244**	**4.31**	0.0307	[0.0158; 0.0455]	.000
		(0.0065)		**(0.0056)**		**(0.0057)**		(0.0074)		
(9)	*Business cycle and labor market status*									
	Average GDP growth rate in time interval	0.0001	0.07	0.0006	0.29	0.0005	0.26			
		(0.0007)		(0.0020)		(0.0020)				
	Average unemployment rate in time interval	**−0.0073**	**1.00**	**−0.0058**	**3.54**	**−0.0061**	**3.68**	−0.0075	[−0.0147; −0.0004]	.039
		(0.0017)		**(0.0016)**		**(0.0017)**		(0.0036)		
(10)	*Study quality*									
	SJR index	**−0.0022**	**0.63**	**−0.0030**	**2.44**	**−0.0032**	**2.54**	−0.0031	[−0.0078; 0.0017]	.202
		(0.0020)		**(0.0012)**		**(0.0013)**		(0.0024)		
	Average Google Scholar citations per year	−0.0000	0.06	0.0001	0.51	−0.0000	0.36			
		(0.0000)		(0.0001)		(0.0001)				
(11)	Year of publication	−0.0000	0.05	−0.0001	0.20	−0.0001	0.21			
		(0.0001)		(0.0003)		(0.0003)				

Note: The number of observations (studies) is 327 (65). In the WLS frequentist check, clustered standard errors robust to heteroskedasticity and within-study correlation are reported in parentheses. 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 WLS frequentist check.

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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 to other household members than it is to the individual who has experienced the unemployment event.

Study results do not vary with the GDP growth rate at the time when the sample was used. The unemployment rate instead exhibits a significant negative influence on the health effects of unemployment. We interpret this finding as showing that displaced individuals are more negatively affected by loss of their job 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 Google Scholar citations, which is a rough measure of the quality of the study. The other proxy for the quality of the study, that is, the SJR index, is negatively related to the effect size, but this relation is statistically insignificant in the frequentist check.

Table 6 is informative about the heterogeneity of the study results, but only with respect to the reference categories; at a first sight, it does not provide information about the average effect of unemployment on health for particular combinations of study/result characteristics. To shed light on this, we (i) identified the 10 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 was absent; (iv) predicted the expected effect size for each combination using the estimates from the WLS frequentist check in column (3) of Table 6; (v) displayed the expected effect sizes in Table 7. The 10 most frequent combinations account for a total of 153 observations, which is 46.80% of the entire sample.

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 Section 3.2, we discussed the differences in the precision effects among different identification strategies, which are 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, when 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 closure, that is, "exogenous." We conclude that, independently of the other result/study characteristics, whenever selectivity

TABLE 7 Expected effect size for the 10 most frequent combinations of categorical regressors.

Covariate combinations		Effect size	95% Confidence interval	*p*-value	Absolute frequency	Relative frequency
(1)	"Fixed effects" + "Men + Women"	−0.0169***	[−0.0261; −0.0078]	.0005	32	9.79%
(2)	"DiD-IV" + "Men + Women"	−0.0039	[−0.0097; 0.0018]	.1770	27	8.26%
(3)	"Observables" + "Income controls" + "Men + Women"	−0.0347***	[−0.0451; −0.0243]	.0000	18	5.50%
(4)	"Fixed effects" + "Well-Being" + "Men + Women" + "Short-term unemployment"	−0.0245***	[−0.0375; −0.0115]	.0004	17	5.20%
(5)	"Observables + "Men + Women"	−0.0488***	[−0.0584; −0.0391]	.0000	15	4.59%
(6)	"Fixed effects" + "Well-Being" + "Men + Women" + "Long-term unemployment"	−0.0315***	[−0.0474; −0.0155]	.0002	10	3.06%
(7)	"Fixed effects" + "Well-Being" + "Income controls" + "Men + Women"	−0.0330***	[−0.0470; −0.0190]	.0000	9	2.75%
(8)	"Observables" + "Well-Being" + "Income controls" + "Men + Women"	−0.0633***	[−0.0737; −0.0529]	.0000	9	2.75%
(9)	"Fixed effects' + "Well-Being" + "Income controls"	−0.0460***	[−0.0616; −0.0303]	.0000	8	2.45%
(10)	"Observables" + "Mental health" + "Income controls" + "Men + Women"	−0.0603***	[−0.0708; −0.0497]	.0000	8	2.45%
	Total observations				153	46.79%

Note: Each continuous variable is set at its median value. Categorical variables not mentioned in the 10 combinations are set to the reference category. Absence of publication bias is assumed ($\delta_1 = 0$).

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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	95% Confidence interval	*p*-value	Absolute frequency	Relative frequency
(a)	*Difference-in-differences/instrumental variables (DiD-IV)*					
	"DiD-IV" + "Men + Women"	−0.0039	[−0.0097; 0.0018]	.1770	27	8.26%
	"DiD-IV" + "Men + Women" + "Exogenous"	0.0188***	[0.0063; 0.0313]	.0038	6	1.83%
	"DiD-IV" + "Mental Health" + "Men + Women" + "Exogenous"	−0.0067	[−0.0186; 0.0052]	.2623	3	0.12%
	"DiD-IV" + "Mental Health" + "Income controls" + "Men + Women" + "Exogenous"	0.0073	[−0.0084; 0.0230]	.3579	3	0.12%
	"DiD-IV" + "Mental Health" + "Income controls" + "Men + Women" + "Other" + "Exogenous"	0.0380***	[−0.0055; 0.0360]	.0019	3	0.12%
	Total observations				42	12.84%
(b)	*Fixed effects*					
	"Fixed effects" + "Men + Women"	−0.0169***	[−0.0261; −0.0078]	.0005	32	9.79%
	"Fixed effects" + "Well-Being" + "Men + Women" + "Short-term unemployment"	−0.0245***	[−0.0375; −0.0115]	.0004	17	5.20%
	"Fixed effects" + "Well-Being" + "Men + Women" + "Long-term unemployment"	−0.0315***	[−0.0474; −0.0155]	.0002	10	3.06%
	"Fixed effects" + "Well-Being" + "Income controls" + "Women"	−0.0330***	[−0.0470; −0.0190]	.0000	9	2.75%
	"Fixed effects" + "Well-Being" + "Income controls"	−0.0460***	[−0.0616; −0.0303]	.0000	8	2.45%
	Total observations				76	23.24%

Note: 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 ($\delta_1 = 0$).

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into unemployment is more seriously addressed, the negative effect of unemployment on health becomes negligible; and, in some cases, it disappears.

5 CONCLUSIONS

This paper has quantitatively surveyed 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 investigate 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 (Havránek et al., 2020; Stanley et al., 2013). We collected 327 observations from 65 articles published in English in peer-reviewed journals from 1990 until 2021. We checked for the presence of publication bias. When we detected publication bias in results by adopting particular identification strategies, we corrected it. We used a large set of controls to exploit possible 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 became smaller. We also found that long unemployment spells impair health more than short ones. The spillover effects of unemployment on 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 decrease with age, and that 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. First, the psychological domains of health are those most negatively impacted by unemployment. Indeed, the estimates reported in Table 6 showed that, when the outcome variable is well-being or mental health, the negative unemployment effect is stronger. Second, the results in Table 6 proved that long-term unemployment spells impair health more than short-term events. Hence, not only longer unemployment events generate more negative effects on labor market outcomes, such as future earnings and re-employment probability (see, e.g., Cockx & Picchio, 2013; Gregory & Jukes, 2001; Kroft et al., 2013), but they also increasingly reduce health.[23] Considering this and taking into account that health and labor market instability may present a bidirectional causal effect (Haan & Myck, 2009), policy-makers and members of the health care system may consider the need for therapeutic strategies for the unemployed promptly after the job loss in order to prevent short-term impairments evolving in long-term scars (Cygan-Rehm et al., 2017).

Finally, our meta-analysis did not investigate the effect heterogeneity along mental and physical distress in the prior occupation, as approximated, for example, by the distinction of workers between blue and white collars. In fact, only a limited number of articles have 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 might take this further dimension of heterogeneity into account.

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DATA AVAILABILITY STATEMENT

The authors confirm that the data supporting the findings of this study are available within its supplementary materials.

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ENDNOTES

[1] On the one hand, the removal of these types of studies may have exacerbated publication bias in our analysis. Indeed, some of them may have failed to get published in peer-reviewed journals because their results were less appealing to the reviewers and the editors, for example, due to insignificant results (Brodeur et al., 2016). On the other hand, by removing them, we eliminated papers, which had not yet undergone the peer review process or which had failed to successfully pass through (several) review processes. Hence, by excluding them, we reduced the risk of including in the sample results, which might have been flawed from the scientific point of view. In what follows, we will pay attention to correcting our findings for publication bias and therefore limit the disadvantage due to including in our meta-analytic sample only articles published in peer-reviewed journals.

[2] In Scopus and Science Direct, the subject categories are defined differently. We retained studies in the following four subject categories: *Social Sciences*, *Psychology*, *Economic Econometrics and Finance,* and *Business Management and Accounting*. In IDEAS/RePEc, we did not restrict over potentially meaningful categories, as this search option is not available.

[3] Nine of the 11 additional studies did not satisfy the "keyword search" in their titles, presenting "unemployment" or "unemployed" but not having "health" or "well-being." However, all of them dealt with the main topic of the meta-analysis. The other two studies were included because, although they satisfied the "keyword search" criteria, they were not returned by any of the previous searches. The 11 additional inclusions are: Clark and Oswald (1994), Winkelmann and Winkelmann (1998), Clark et al. (2001), Kassenboehmer and Haisken-DeNew (2009), Wulfgramm (2011), Pharr et al. (2012), Oesch and Lipps (2013), Thern et al. (2017), Von Scheve et al. (2017), Zuelke et al. (2018), and Chen and Hou (2019).

[4] In Cohen (1988), the thresholds are 0.5, 0.3, and 0.1, respectively.

[5] If the study reported only the p-value, we recovered the t-statistic by computing the inverse of the t-distribution. If the authors reported only the 95% confidence interval, we computed the standard error of $\widehat{\beta}_i$ with the formula $SE_i = \frac{UB_i - LB_i}{2\cdot 1.96}$, where UB_i and LB_i are the upper and lower bounds of the 95% confidence interval, respectively. If the study reported a risk ratio, an odds ratio or a hazard ratio and the corresponding standard error, we retrieved the t-statistic for testing if the natural logarithm of the ratio is 0 (i.e., no difference between the treated and the untreated units in terms of risk, odd, or hazard rates) using the formula $t_i = \ln(\widehat{\beta}_i)\cdot\widehat{\beta}_i/SE_i$, after applying the delta method to calculate the standard error of the log ratio. Finally, if the authors reported a risk ratio, an odds ratio or a hazard ratio and the corresponding 95% confidence interval, we computed the t-statistic as $t_i = \frac{\ln(\widehat{\beta}_i)}{[\ln(UB_i) - \ln(LB_i)]/(2\cdot 1.96)}$ (Altman & Bland, 2011).

[6] 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.

[7] Online appendix D presents the results of a sensitivity analysis in which the winsorization was applied at the 1st and 99th percentiles of the distribution of the t-statistics and degrees of freedom as in Xue et al. (2021).

[8] In some studies, the average age of the sample was not declared. We dealt 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 was missing (and 0 otherwise).

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[9] According to ILO, an unemployment spell is short if it is shorter than or equal to 12 months. Otherwise, it is defined as long.

[10] 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, that is, 1991.

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

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

[13] Positive effects of unemployment on health may originate, for example, from the unemployed spending more time in enjoyable activities than the employed (Hoang & Knabe, 2021). Further, although unemployment is typically associated with a higher degree of dissatisfaction, having a job does not necessarily imply higher levels of well-being. Using German data, Knabe et al. (2010) showed that workers who perceive their job as highly insecure are not better off than the unemployed.

[14] Among the worst 10% of the results in terms of precision, 70% have an effect size smaller than the sample average. Among the top 10%, this fraction diminishes to 59%.

[15] As an alternative to the PEESE, Bom and Rachinger (2019) proposed the Endogenous Kink (EK) meta-regression model to account for possible nonlinearity between the effect size and its standard error. We estimated the EK meta-regression model, finding no evidence of publication bias on average. These results are available in Online appendix E.

[16] We used the command `RoBMA()` from the R package `RoBMA` developed by Bartos and Maier (2020).

[17] We report the corresponding funnel plots in Online appendix C.

[18] Costa-Font et al. (2011), Doucouliagos et al. (2012), Vooren et al. (2019), and Filomena and Picchio (2023) are examples of studies which estimated meta-regression models both incorporating the PEESE correction for publication bias and accommodating systematic heterogeneity.

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

[20] In a sensitivity check, we estimated the BMA model with all the regressors considered as auxiliary. The results are very similar to those in Table 6. They are available from the authors upon request.

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

[22] The Wald test for the equality of the coefficients of these two covariates returned a p-value equal to .032.

[23] Although the predicted partial correlations reported in Table 7 are quite small if compared to the guidelines in Doucouliagos (2011) or Cohen (1988), there is no consensus on what the economic implications of a partial correlation may be in a given topic, especially when dealing with health shocks, which may relevantly impair individuals' quality of life and have important economic implications.

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