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Kamila Cygan-Rehm
Friedrich-Alexander University Erlangen-Nürnberg

Daniel Kuehnle
Friedrich-Alexander University Erlangen-Nürnberg and IZA

Michael Oberfichtner
Friedrich-Alexander University Erlangen-Nürnberg and IAB

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IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Bounding the Causal Effect of Unemployment on Mental Health: Nonparametric Evidence from Four Countries

An important, yet unsettled, question in public health policy is the extent to which unemployment causally impacts mental health. The recent literature yields varying findings, which are likely due to differences in data, methods, samples, and institutional settings. Taking a more general approach, we provide comparable evidence for four countries with different institutional settings – Australia, Germany, the UK, and the US – using a nonparametric bounds analysis. Relying on fairly weak and partially testable assumptions, our paper shows that unemployment has a significant negative effect on mental health in all countries. Our results rule out effects larger than a quarter of a standard deviation for Germany and half a standard deviation for the Anglo-Saxon countries. The effect is significant for both men and women and materialises already for short periods of unemployment. Public policy should hence focus on early prevention of mental health problems among the unemployed.

JEL Classification: I12, J64
Keywords: mental health, unemployment, bounds

Corresponding author:
Daniel Kühnle
Department of Economics
Friedrich-Alexander University Erlangen-Nürnberg
Lange Gasse 20
90403 Nürnberg
Germany
E-mail: daniel.kuehnle@fau.de

1 Introduction

An important mechanism of economic growth is the destruction of jobs through the creation of new jobs in more innovative or technologically-advanced firms (OECD, 1988; Aghion et al., 2016). While this Schumpeterian-process of 'creative destruction' benefits society at large, an extensive literature documents that individuals who become unemployed suffer from their displacement. Unemployment might affect individual's psychological well-being, in particular mental health, through losses of social relationships (e.g., Jahoda, 1981), income reductions (Couch and Placzek, 2010), and in some countries reduced access to medical services (Gruber, 2000).

Yet, identifying the causal effect of unemployment on mental health is challenging. Although numerous studies show descriptively that unemployed have worse mental health than employed workers, this negative correlation might be driven by selection bias and reverse causality. The most recent studies that address the endogeneity of unemployment generally agree that unemployment harms mental health (e.g., Schaller and Stevens (2015) for the US, Marcus (2013) for Germany, Kuhn et al. (2009) for Austria, Browning and Heinesen (2012) for Denmark, Eliason and Storrie (2009a) for Sweden), though there are some exceptions (see Section 2).

One major impediment to drawing general, policy-relevant conclusions from the literature is the limited comparability of studies. The estimates rely on different identifying assumptions and examine various countries, with substantially different institutions and labour markets at different points in time. Furthermore, the estimation samples are hardly comparable given that they consider subpopulations with different socio-demographic characteristics. Moreover, the various data sources collect different measures of mental health ranging from self-reported measures, such as life satisfaction, to more objective but crude measures such as specific causes of deaths.[1] Another central issue for policy development, especially for the timing of potential

[1] Some studies use self-rated mental health conditions or questions about physician-diagnosed mental health conditions (e.g., Björklund, 1985; Böckerman and Ilmakunnas, 2009). Several studies combine various self-reported components into a single scale (e.g., Schmitz, 2011; Drydakis, 2015; Salm, 2009). Other authors use a respondent's life satisfaction to proxy for mental health (Clark et al., 2001; Green, 2011). Studies using adminis-

interventions, relates to the role of unemployment duration. While several studies discuss the importance of unemployment duration (e.g., Paul and Moser, 2009; Classen and Dunn, 2012), the literature lacks evidence from rigorous empirical designs.

Against this backdrop, we contribute new, comparable evidence on the causal effect of unemployment on mental health from four large OECD countries. Specifically, we explore representative household survey data from Australia (HILDA), Germany (SOEP), the UK (BHPS and Understanding Society), and the US (PSID). We thereby study countries that differ in the level of social protection, the provision of health services and labour market conditions.[2]

Absent a common source of exogenous variation that applies equally to all countries, we identify the causal effect of unemployment on mental health in a nonparametric bounds analysis (Manski and Pepper, 2000).[3] This method has two main advantages. First, we can identify the treatment effect without exogenous variation in unemployment relying on fairly weak and partially testable assumptions. Second, we identify the same parameter for all countries, namely the average treatment effect (ATE). In contrast, previous studies estimate either an average treatment effect on the treated (ATT), i.e., only for the group of unemployed individuals, or a local average treatment effect (LATE) for a specific group of compliers. Using bounds hence avoids the common caveat in cross-country comparisons that differences in methods, affected groups, or sources of exogenous variation might drive differences in results. However, these advantages come at some cost: We cannot obtain a point estimate, but rather we identify an interval that contains the ATE. Broadly speaking, we view these nonparametric bounds as a complementary method, rather than a substitute for alternative methods, as the bounds provide a useful "sniff test" (Hamermesh, 2000) for the plausible magnitude of point estimates.

Our analysis demonstrates that unemployment has a significant negative causal effect on mental health irrespective of the institutional setting. Notwithstanding, the institutional setting seems to matter for effect size. While our bounds rule out effects of more than a quarter of

trative records use prescriptions of psychotropic drugs or hospitalisations and deaths due to mental health disorders (e.g., Kuhn et al., 2009; Browning and Heinesen, 2012; Eliason and Storrie, 2009a, 2010).

[2]Appendix A provides details on the relevant labour market and health institutions.

[3]For applications of this method, see, e.g., Pepper (2000), Gerfin and Schellhorn (2006), Gundersen and Kreider (2009), De Haan (2011), and De Haan (2015).

a standard deviation for Germany, which has the most pronounced welfare regime among the four countries, the bounds allow for effects of up to half a standard deviation for Australia, the UK, and the US. These effect sizes are of similar magnitude as the association between marital separation and mental health. Furthermore, we show that the negative effect already emerges with short unemployment spells, i.e. less than three months, and our subgroup analysis reveals that the negative effect is significant for both men and women. The results are robust to different measures of mental health and identifying assumptions.

The paper proceeds as follows. Section 2 summarises the findings on the effect of unemployment on mental health. Section 3 presents the econometric method and Section 4 describes our data. Section 5 presents our results and discusses their robustness. Section 6 concludes.

2 Literature review

An extensive literature documents that unemployed individuals have worse mental health than employed individuals (e.g., Björklund and Eriksson, 1998; Murphy and Athanasou, 1999). Most of the early studies rely on cross-sectional variation and generally assume the exogeneity of unemployment. The economic literature commonly argues that the negative relationship results from reductions in income and the loss of employer-provided health insurance, particularly for the US, implying both worse health care coverage and lower investments in health-enhancing goods (e.g., Grossman, 1972; Ruhm, 1991). The research within related disciplines emphasises the role of adverse social and psychological consequences of unemployment such as the loss of work relationships, valued social position, a collective purpose, sense of control, meaning in life, and time structure (e.g., Warr, 1987; Ezzy, 1993; Goldsmith et al., 1996; Cavanagh et al., 2003). Generally, all these mechanism lead to the conjecture that unemployment has a negative effect on mental health.

However, identifying the causal effect is not straightforward due to the well-established issues of reverse causality and unobserved individual heterogeneity. Consequently, cross-sectional studies are highly likely to yield biased results. The more recent literature has generally adopted two strategies to address the endogeneity of unemployment: the first one relies on longitudinal data to estimate fixed-effects models that account for time-invariant heterogeneity (e.g., Björklund, 1985; Clark et al., 2001; Green, 2011). The second strategy explores (arguably) exogenous variation in employment from plant closures, mass lay-offs, and other firm-level employment reductions (e.g., Kuhn et al., 2009; Eliason and Storrie, 2009a, 2010; Browning and Heinesen, 2012; Marcus, 2013). Most studies within this framework use matching techniques to make displaced and non-displaced workers comparable. Combining the two approaches, some studies include an interaction term of unemployment status with a plant closure within a fixed-effects design (e.g., Schmitz, 2011; Drydakis, 2015).

At first glance, the results seem inconclusive. For example, while several fixed-effects analyses confirm that unemployment is associated with deteriorating mental health (e.g., Clark et al. (2001) for Germany, Green (2011) for Australia, Drydakis (2015) for Greece, and Schaller and Stevens (2015) for the US), other authors document statistically insignificant coefficients from fixed-effects regressions (e.g., Schmitz (2011) for Germany). Similarly, among studies that explore various employment reductions as sources of exogenous variation, the estimates range from clear negative effects (e.g., Browning and Heinesen (2012) for Denmark, Marcus (2013) for Germany) to statistically insignificant results (e.g., Salm (2009) for the US).

However, most of the null results seem to come from a lack of power rather than the absence of an effect. Already the early fixed-effect analysis by Björklund (1985) recognises that small treatment groups often lead to large standard errors when estimating the effect of unemployment on mental health. In contrast, based on a fairly small number of individuals affected by a business closure, Salm (2009) concludes that there is no causal effect of job loss on mental health in the US. However, the estimates are imprecise and the study further lacks generalisability because it uses the Health and Retirement Survey that covers only older workers. Exploring more representative and richer data from the Medical Expenditure Panel Survey, Schaller and Stevens (2015) document that involuntary job loss significantly impairs mental health. The interpretation problem related to imprecise zero estimates arises also in the study by Schmitz (2011). He argues that the significant correlation between unemployment and mental health disappears after accounting for the endogeneity of unemployment. However, his study is also limited by a small number of job losses from plant closures, which fails to generate sufficient power within a fixed-effects design. In comparison, using the same data from the German Socio-Economic Panel, Marcus (2013) applies non-parametric matching techniques based on entropy balancing, which burden the data less than fixed-effect estimations. Despite small sample sizes, he finds significant decreases in mental health not only for individuals directly affected by plant closures but also for their spouses.

Recent studies using large administrative data generally reach the consensus that job loss has various adverse consequences for mental health. For example, by using Austrian health insurance data, Kuhn et al. (2009) show that unemployment significantly increases expenditures for hospitalizations due to mental health problems and prescriptions of psychotropic drugs for men. They argue there are no severe consequences for women who might be less economically and emotionally distressed by job loss due to their alternative roles within the family. For Sweden, Eliason and Storrie (2009a,b, 2010) find increased short-run risk of suicides, alcohol-related mortality, and hospitalizations, and several gender-specific effects: increased deaths from traffic accidents and self-harm among men and inpatient psychiatric hospital admission among women. However, since many of the outcomes are extremely rare events, the gender-specific confidence intervals largely overlap. On a larger sample of Danish men, Browning and Heinesen (2012) confirm the short-run effects on suicides. They also find large effects on deaths and hospitalizations due to alcohol-related diseases as well as hospitalisation for mental disorders and deaths from circulatory diseases.[4] Most of these do not vanish up to 20 years after displacement. While not explicitly focusing on mental health, Sullivan and Von Wachter (2009) also find highly persistent increases in overall mortality among mature male workers in Pennsylvania. By using U.S. state-level panel data, Classen and Dunn (2012) confirm that local mass-lay-offs are a significant risk factor for the number of suicides for both men and women.[5] However, in contrast to immediate effects in the Scandinavian studies, Classen and

[4]However, the literature consistently establishes no effects on the risk of hospitalizations due to circulatory diseases (Browning et al., 2006; Kuhn et al., 2009; Eliason and Storrie, 2009b; Browning and Heinesen, 2012).

[5]To the best of our knowledge, Classen and Dunn (2012) is the only study that uses aggregate longitudinal

Dunn (2012) argue that the negative effects do not emerge immediately after job loss. Instead, they emphasise the destructive role of prolonged unemployment spells.

3 Methodology and identifying assumptions

Our empirical methodology departs from the potential outcomes framework (Rubin, 1974). Consistent with the standard terminology, let D_i denote our binary treatment variable for unemployment of person i, where $D_i = 1$ means unemployment and $D_i = 0$ means employment; let Y denote our mental health outcome variable, where higher values reflect better mental health; and let Y_i^t denote person i's potential outcome with treatment t, where t takes on the values 0 and 1 as defined for D_i.

To estimate the ATE, $\Delta_{ATE} = E[Y^1] - E[Y^0]$, we need some identifying assumptions about the counterfactual outcomes $E[Y^0|D = 1]$ and $E[Y^1|D = 0]$. Given a bounded outcome variable, Manski (1989) proposed using the extrema of the outcome variable as counterfactual outcomes to bound the effect of interest without any further assumptions. Focusing on the potential outcome under unemployment, this method yields the following bounds:

$$E[Y^1]_{LB} = E[Y^1|D = 1] \cdot P(D = 1) + Y_{min} \cdot P(D = 0)$$
$$\leq E[Y^1] = E[Y^1|D = 1] \cdot P(D = 1) + E[Y^1|D = 0] \cdot P(D = 0) \leq$$
$$E[Y^1|D = 1] \cdot P(D = 1) + Y_{max} \cdot P(D = 0) = E[Y^1]_{UB},$$

where Y_{min} (Y_{max}) indicates the smallest (largest) possible value of the mental health measure. To calculate the lower bound $E[Y^1]_{LB}$, we thus assume that those observed in employment would have the worst possible mental health when unemployed. To compute the upper bound $E[Y^1]_{UB}$, we conversely assume that they would have the best possible mental health. Extending the same logic to $E[Y^0]$ and taking the difference between the lower and upper bounds, we

instead of individual data. Although aggregating data largely solves the reverse causality concern, it may lead to aggregation bias.

obtain the following bounds for the ATE:

$$E[Y^1]_{LB} - E[Y^0]_{UB} \leq \Delta_{ATE} \leq E[Y^1]_{UB} - E[Y^0]_{LB}. \tag{1}$$

Although these bounds contain the effect of interest, they are too wide to be informative. In particular, they always include zero. Hence, we need some further, albeit weak, assumptions to tighten these bounds (Manski, 1997; Manski and Pepper, 2000).

First, we impose the *monotone treatment selection* (MTS) assumption:

$$E[Y^t|D=1] \leq E[Y^t|D=0], \qquad t=0,1 \tag{MTS}$$

which states that the unemployed have worse (or equal) average potential mental health than the employed; this assumption needs to hold irrespective of the realised employment status. Thus, the MTS assumption intuitively says that unemployed individuals are negatively selected. As illustrated in Figure 1, we can hence replace the unobservable minimum potential outcome under employment with the observed average outcome of the unemployed. The MTS assumption thus lifts the lower bound on the mean potential outcome in unemployment and yields the following bounds for potential mental health in unemployment:

$$\begin{gathered} E[Y^1]_{LB-MTS} = E[Y^1|D=1] \cdot P(D=1) + E[Y^1|D=1] \cdot P(D=0) = E[Y^1|D=1] \\ \leq E[Y^1] \leq \\ E[Y^1|D=1] \cdot P(D=1) + Y_{max} \cdot P(D=0) = E[Y^1]_{UB-MTS}. \end{gathered}$$

Further, the MTS assumption implies that individuals observed in unemployment would not have better mental health than the individuals observed in employment if both were employed. Thereby, the MTS assumption reduces the upper bound on the mean potential outcome in case of employment $E[Y^0]$. The MTS assumption, however, does not affect the upper bound on the mean potential outcome in unemployment nor the lower bound on potential outcomes in employment. Compared with the worst-case bounds, the MTS assumption thus lifts the lower bound on the ATE, that is the largest negative effect of unemployment on mental health, to the observed mean difference in mental health between employed and unemployed persons.

To reduce the upper bound on the ATE, which is unaffected by the MTS assumption, we impose the *monotone treatment response* (MTR) assumption

$$Y_i^1 \leq Y_i^0, \qquad \forall i \tag{MTR}$$

which states that potential outcomes are non-increasing for each individual in the treatment, i.e., becoming unemployed does not improve mental health. While this assumption may seem more controversial than the MTS assumption, it is consistent with theoretical views of the deteriorative effects of unemployment on mental health. Moreover, the existing empirical evidence essentially rules out any systematic positive effects of unemployment on mental health (see Section 2), thereby making the MTR assumption plausible from an empirical perspective. To further eliminate potential positive short-term effects, we exclude individuals who are unemployed or out-of-the labour force voluntarily, as this assumption may be violated for such individuals. Combining the MTS and MTR assumptions yields the following bounds for potential mental health in unemployment:

$$E[Y^1]_{LB-MTS-MTR} = E[Y^1|D=1] \cdot P(D=1) + E[Y^1|D=1] \cdot P(D=0) = E[Y^1|D=1]$$
$$\leq E[Y^1] \leq$$
$$E[Y^1|D=1] \cdot P(D=1) + E[Y^0|D=0] \cdot P(D=0) = E[Y^1]_{UB-MTS-MTR}.$$

When computing the bounds for the ATE according to equation (1), the MTR assumption reduces the upper bound to zero. Hence, the MTS-MTR bounds range from the observed mean difference in outcomes to zero and they will only include (weakly) negative treatment effects. Hence, the lower bound is the strongest effect and the upper bound is the weakest effect.

As implied by De Haan (2011), the MTS and MTR assumptions combined require unemployed persons to have worse average mental health than employed persons. We use this necessary condition to empirically test the MTS-MTR assumptions. If unemployed persons have, on average, better mental health than employed persons, we should reject the MTS-MTR assumptions.

To further tighten the bounds on the ATE, we rely on the *monotone instrumental variable* (MIV) assumption (Manski and Pepper, 2000) which allows for a weak monotonic (here, increasing) relation between the instrument and the outcome. Formally, we assume that the MIV v satisfies:

$$\begin{aligned} & m_1 \leq m \leq m_2 \Rightarrow \\ & E[Y^t|v = m_1] \leq E[Y^t|v = m] \leq E[Y^t|v = m_2], \qquad t = 0, 1. \end{aligned} \tag{MIV}$$

Intuitively, a feasible MIV resembles a covariate for which we have a strong prior about its sign. In our empirical analysis, we therefore use maternal education as an MIV. Unlike in standard instrumental variable (IV) estimation, our identifying assumption does not require mean independence between maternal education and an individual's potential mental health. Rather, the MIV assumption requires that maternal education and an individual's potential mental health are not negatively related, thus allowing for a positive correlation between maternal education and her children's mental health. Importantly, the MIV assumption neither requires causality nor a strictly positive association between education and mental health. Thus, this assumption is considerably weaker than standard IV assumptions.

In our setting, the MIV assumption is not only relatively weak but also plausible. Vast evidence documents positive relationships between education and (mental) health (Cutler and Lleras-Muney, 2006), and between parental education and children's outcomes (including health) (Currie, 2009), as well as documenting the intergenerational transmission of education (Holmlund et al., 2011). Thus, parental education may directly impact children's mental health (through various parental behaviours) and may indirectly affect mental health through other channels, such as higher family resources or even children's own education. Taken together, the evidence supports our MIV assumption of a non-negative relationship between maternal education and her children's mental health.[6]

To tighten the bounds on expected average outcomes using the MIV assumption, we proceed in two steps, which we again illustrate for the potential outcome under unemployment. First, we compute the MTS-MTR bounds for mental health at each value of v.[7] Second, we tighten the bounds at each value of v using the MIV assumption, which states that potential mental health outcomes do not decrease in maternal education. To this end, we compare the MTS-MTR lower bound on mental health in unemployment at a given level of maternal education ($E[y^1|v = m]_{LB-MTS-MTR}$) with the MTS-MTR lower bounds at a lower level of maternal education ($E[y^1|v = m_1]_{LB-MTS-MTR}$). The MIV assumption allows us to use the highest of these two lower bounds as the lower bound at the given level $E[y^1|v = m]_{LB-MTS-MTR-MIV}$. Symmetrically, we compare the MTS-MTR upper bound at that level ($E[y^1|v = m]_{UB-MTS-MTR}$) with the upper bound at a higher level of maternal education ($E[y^1|v = m_2]_{UB-MTS-MTR}$) and use the smallest of these two values as the upper bound at the given level ($E[y^1|v = m]_{UB-MTS-MTR-MIV}$). Repeating this procedure for all possible combinations of v, we obtain the MTS-MTR-MIV bounds on mental health in case of unemployment by taking the weighted averages over the conditional bounds:

$$E[y^1]_{LB-MTS-MTR-MIV} = \sum_{m \in M} P(v = m) \cdot [\underset{m_1 \leq m}{max} E[Y^1|v = m_1]_{LB-MTS-MTR}]$$

$$\leq E[Y^1] \leq$$

$$\sum_{m \in M} P(v = m) \cdot [\underset{m_2 \geq m}{min} E[Y^1|v = m_2]_{UB-MTS-MTR}] = E[y^1]_{UB-MTS-MTR-MIV}.$$

Having obtained the bounds for mental health under unemployment and employment this way, we bound the ATE as in equation (1). While the MTR and MTS assumptions mechanically tighten the bounds on the ATE, the MIV assumption does not necessarily narrow the bounds. Whether the MIV assumption helps to tighten the bounds depends on the observed outcomes at

[6] To further alleviate concerns about our identification, we show in Section 5.2 that the results are robust to the use of other plausible MIVs.

[7] Thereby, we extend the MTS assumption to also hold conditional on v, as noted by Lafférs (2013).

the various combinations of the MIV and the treatment status.[8]

To estimate the MTS-MTR bounds, we rely only on expected values of average outcomes and the share of treated individuals, which we can estimate without bias in finite samples using the sample analogues. To estimate the MTS-MTR-MIV bounds, in contrast, we need minima and maxima over group averages. While the sample analogues estimate these parameters consistently, the analogues may suffer from finite sample bias and the resulting MTS-MTR-MIV bounds might hence be too narrow (for details, see further Manski and Pepper, 2000, 2009). To correct the MTS-MTR-MIV bounds for potential finite sample bias, we apply the bias-correction method of Kreider and Pepper (2007) and report both bias-corrected and non-corrected bounds.

The literature seems inconclusive regarding the most appropriate mode of inference. We hence report Imbens and Manski (2004) confidence intervals, Imbens and Manski (2004) bias-corrected confidence intervals, and bias-corrected percentile confidence intervals (Efron and Tibshirani, 1994) using 1,000 bootstrap repetitions.

4 Data

4.1 Data sets and sample selection

We use data from four comparable household surveys: the Household, Income and Labour Dynamics in Australia (HILDA), the British Household Panel Survey (BHPS), the US Panel Study of Income Dynamics (PSID), and the German Socio-Economic Panel (SOEP). All surveys provide nationally representative information on respondents' socio-demographic, employment, and mental health characteristics. We use all waves for which mental health data are available, i.e., 2001-2012 for Australia; 2002, 2004, 2006, 2008, 2010, and 2012 for Germany; 1991-2013 for the UK; and 2001, 2003, 2007, 2009, 2011, and 2013 for the US.[9]

[8]To tighten the bounds further, we could in principle use multi-dimensional instruments as in De Haan (2011); however, the weak monotonicity assumption necessary for identification becomes less convincing when we consider two MIVs and we therefore do not pursue this approach.

[9]Even though the data have a panel structure, our empirical analysis is essentially cross-sectional. Unfortunately, we cannot exploit the panel dimension within a fixed effects framework since the MIV is time-invariant. To ensure that treating our panel data like cross-sectional data does not drive our results, we redid our main analysis

We impose few restrictions on the samples. We focus on individuals aged 25–55, where the upper limit avoids retirement issues and the lower limit ensures that most individuals have completed their educations. We only consider individuals who are either employed or unemployed and looking for work. We therefore exclude individuals who are out of the labour force, e.g., discouraged workers or individuals on maternity leave, as well as the self-employed. We also exclude individuals with missing age, employment status, mental health score, or MIV information.[10]

4.2 Mental health measures

The data sets do not include the same measure of mental health for each country. Hence, we have to employ different screening tools for mental health, namely the Short-Form General Health Survey (SF-36 and SF-12), the General Health Questionnaire (GHQ-12), and the Kessler Psychological Distress scale (K6). These different measures do not pose difficulties in our analysis for three reasons. First, each has been shown to be an effective and psychometrically valid measure of mental health. Second, studies that compare these measures typically find that they produce similar, if not identical, results. Finally, we standardise each mental health measure (with mean 0 and standard deviation of 1) to make the different scales comparable, where higher values represent better mental health.[11]

For Australia, we use the SF-36 which assesses mental health using a five-item scale that captures both anxiety symptoms and mood disturbances. Numerous studies show that the SF-36 is an effective instrument for identifying mood disorders and major depression (e.g., Rumpf et al., 2001), as well as psychiatric disorders (Ware Jr. and Gandek, 1998). Moreover, Butterworth and Crosier (2004) examine its psychometric properties and show that the SF-36 meets

using only the first observation of each person. Table B.3 shows that we reach the same conclusions. This is unsurprising given that time-varying heterogeneity and reverse causality do not contradict our identifying assumptions.

[10]As we use paternal education in a robustness check, we only include observations for which we have information on both parents' level of education. As a further robustness check, Table B.4 shows that our results do not change when we include individuals for whom we have information on maternal education, our main MIV, but not on paternal education.

[11]Both the SF-36 and SF-12 range from 0 to 100, with higher scores indicating less psychological distress; the GHQ-12 ranges from 0 to 12, with higher scores indicating greater psychological distress; and the K6 ranges from 6 to 30, with higher scores indicating greater psychological distress.

validity criteria.

For Germany, we use the SF-12 which is a shortened version of the SF-36 (Ware Jr. et al., 1996). Similar to the SF-36, the SF-12 provides a generic measure of mental health, the Mental Health Component Summary, that captures different domains of psychological and psychosocial problems. Many studies show that the SF-12 is a reliable and valid measure of mental health (Gill et al., 2007; Salyers et al., 2000). Moreover, Gill et al. (2007) shows that the SF-12 performs very well compared to other measures of mental health, including the SF-36.

For the UK, we use the 12-item version of the GHQ. As one of the most widely used measures in mental health research (Gill et al., 2007), the GHQ-12 assesses depressive symptoms using 12 questions about the respondent's psychological distress symptoms over the past few weeks (Goldberg and Williams, 1988). Goldberg et al. (1997) provide an overview of studies that demonstrate the validity of the GHQ-12.

For the US, we use the 6-item Kessler Psychological Distress scale (K6, Kessler et al., 2002). The K6 was developed to identify non-specific psychological distress and has been shown to be an effective and psychometrically valid screening tool for psychological distress (Cairney et al., 2007; Furukawa et al., 2003). Furthermore, Gill et al. (2007) show that the K6 performs similarly to the SF-12 in diagnosing depression.

The Australian survey is the only one that provides two measures of mental health. In addition to the SF-36, the HILDA collects information on the K6 in 2008, 2010, and 2012. In Section 5.2, we show that the results for the SF-36 in our main analysis are robust to the use of the K6. We present the distribution of mental health measures in Figure B.1.

4.3 Summary statistics

Table 1 presents some descriptive statistics for our sample. Panel A shows average mental health scores for unemployed and employed individuals separately. As expected, in all countries, unemployed individuals have on average worse mental health compared with employed individuals. This is a necessary condition to hold for the validity of the MTS-MTR assumptions (De Haan, 2011). As we standardised the dependent variable throughout, we can compare the magnitudes across countries. In Germany, the mental health of unemployed persons is about a third of a standard deviation worse than the mental health of employed workers. By comparison, this difference amounts to more than half a standard deviation in Australia, the UK, and the US.

Panel B shows that the distributions of unemployment duration differ markedly across these countries. In particular, Germany exhibits the largest proportion of long-term unemployed workers. Finally, Panel C shows the distribution of our main MIV, maternal education, differs across countries. Importantly, such differences do not affect the validity of our analysis, though they might affect the width of the estimated bounds.[12]

5 Results

5.1 Effect of unemployment on mental health

We start our presentation of results with Figure 3 which displays estimated bounds – using different identifying assumptions – around the ATE of unemployment on mental health for the four analysed countries. We begin with the unconditional mean difference obtained under the strong exogenous treatment selection (ETS) assumption.[13] This mean difference overestimates the magnitude of the true effect if unemployment does not randomly affect individuals who differ in their mental health score. We then successively impose the MTS, MTR, and MIV assumptions, which do not require exogenous selection in unemployment, to arrive at the final MTS-MTR-MIV bounds. All displayed bounds use maternal education as the MIV. The figure shows how each assumption helps to tighten the worst case bounds considerably, and that the MIV assumption is required to tighten the MTS-MTR bounds below zero. For all countries, the MTS-MTR-MIV bounds exclude zero and the ETS point estimates.

Table 2 provides the analogous nonparametric estimates from the MTS-MTR-MIV bounds.

[12]Table B.1 shows how parental education is classified in the different data sets. Our analysis compares only education levels within countries and the coding therefore needs only to yield a correct ordering within each country, but levels do not have to be strictly equivalent across countries.

[13]If we assume that treatment assignment is unrelated to potential outcomes, the observed difference in means between the treated and the untreated yields the ATE. Therefore, we refer to the difference in means also as the estimate under *exogenous treatment selection* (ETS).

We estimate rather similar bounds for the Anglo-Saxon countries: for Australia, the UK, and the US, we find that unemployment reduces the mental health score by at most 0.408, 0.483, and 0.464 standard deviations, respectively. For Germany, our bounds imply that unemployment on mental health does not decrease mental health by more than 0.188 standard deviation. These effect sizes are of similar magnitude as the association between marital separation and mental health.[14] Columns 5 and 6 report the bias-corrected bounds obtained from the Kreider and Pepper (2007) method. However, a potential finite sample bias has negligible implications for our main findings because the bias-corrected bounds are similar to the non-corrected bounds. For the UK, the upper bound suggests a relatively small effect (although three out of four confidence intervals reject a zero effect).[15]

Given the relatively large size of the UK sample, it seems unintuitive that the bounds are relatively large for the UK. A closer inspection of the mental health and maternal education variables reveals that both measures vary least in the UK. In particular, mental health scores are very much concentrated around the mode, and maternal education is in the middle three categories for almost 93 per cent of the observations. Hence, the UK data convey less information than the other data, likely explaining the relative width of the bounds.

It is challenging to directly compare our nonparametric bounds to previous point estimates due to differences between the data sets, outcome measures, and estimation methods. Nevertheless, we compare our findings with those from studies using the same surveys and mental health measures. For Australia, Green (2011) interacts the unemployment status with self-perceived employability to show its moderating role. His fixed-effect estimations yield an (imprecise) zero effect for "hopers" and large mental health loses for "no-hopers" of about a third of the standard deviation. We calculate a 95% confidence interval for this estimate ranging between -0.450 and

[14] We estimate that compared to being married, being separated from a partner is associated with a reduction in mental health of 0.45 in Australia, 0.32 in Germany, 0.38 in the UK, and 0.30 in the US. All regressions include controls for age and its square, highest level of education, state of residence, marital status, number of children in the household, sex, ethnicity, maternal education, and interview month and year as in Table 7.

[15] To evaluate the propensity of the bias-corrected MTS-MTR-MIV bounds to hit the MTS and MTR constraints, we examine the bootstrap distribution for each estimated bound. For Australia, Germany, and the US, the lower bound hits the MTS constraint in less than 1% of repetitions, and the upper bound never hits the MTR constraint. For the UK, the lower (upper) bound hit the MTS (MTR) constraints in 19.3% (7.3%) of repetitions.

-0.170 standard deviations, which is of similar magnitude compared to our bias-corrected 95% confidence intervals for Australia (-0.528 to -0.052). For Germany, the non-parametric matching results by Marcus (2013) imply a negative effect of about 0.268 standard deviations which closely corresponds to our bias-corrected lower bound (-0.269). Comparing his 95% confidence interval (-0.412 to -0.124) with ours (-0.269 to -0.046) shows that our bounds are slightly lower and slightly more precisely estimated. We thus exclude effects larger than 0.269 standard deviations, while the lower bound of his confidence interval reaches 41%. For UK, using FE estimation, Binder and Coad (2015) find that individuals becoming unemployed reduces mental health by 0.33 standard deviations. This point estimate lies in the middle of our 95% confidence interval. However, the study also controls for a number of objective and subjective health measures, which makes it difficult to interpret the results. Despite the differences in methods and data, this discussion shows that our nonparametric bounds are consistent with other studies, and that bounds may even yield more precise 95% confidence intervals than conventional approaches.

Next, we examine whether the duration of unemployment matters for mental health and whether this effect changes over time. Several psychological studies emphasise that mental health deteriorates with increasing unemployment duration because the effect accumulates over time, although this relationship is not necessarily linear. In contrast, the adaptation hypothesis (Warr and Jackson, 1987) states that long-term unemployed individuals adapt to lower, but stable, levels of mental health after long spells of unemployment. This adaptation hypothesis potentially contradicts the *monotone treatment response* assumption when directly comparing mental health by unemployment duration, as it suggests that some individuals partly recover from initial mental health problems. To circumvent this issue, we estimate the effects of unemployment for different durations using employment as the reference category. Here, the MTR assumption implies that mental health does not improve by being unemployed for any duration compared to being employed. However, we do not impose any assumptions on how mental health evolves over the course of an unemployment spell.

Figure 4 displays the bounds calculated for three different durations of unemployment lasting less than three months, three to 12 months, and longer than one year.[16] The figure illustrates that even short spells of unemployment have substantial negative causal effects on mental health. For all countries, the upper bounds for durations of less than three months are negative. For Germany, the effect is fairly precisely estimated (ranging from -0.144 to -0.065), whereas the bounds for the other countries range from approximately 0.5 standard deviations to just under zero. Longer unemployment spells seem to have more negative effects on mental health in Germany, whereas in the UK, shorter spells seem to have more negative consequences. This finding is intriguing given that long-term unemployment in the UK is considerably lower than in Germany, which exhibits the highest proportion of long-term unemployed persons. Nevertheless, the figures for all countries consistently show that unemployment negatively impacts mental health, regardless of duration.

Finally, we investigate the effects of unemployment on mental health separately for men and women, thereby testing the common conjecture that unemployment has less severe consequences for women than for men (e.g., Paul and Moser, 2009).[17] Table 3 shows that in the Australian and German samples, the bounds are very similar for men and women, supporting the conclusion that unemployment has serious adverse mental health consequences regardless of gender. The upper bounds for women in the UK and the US are not significantly different from zero. However, in a complementary analysis using paternal education as an alternative MIV, we found that three out of four confidence intervals for women in UK and US exclude zero, thereby supporting significantly negative effects.

5.2 Robustness tests

To test the robustness of our main results, we first focus on the dependent variable and test the extent to which the use of a specific measure of mental health might drive our results. As HILDA is the only data set that provides two measures, we investigate an alternative outcome

[16]For this analysis, we collapse the MIV to ensure that we observe individuals with different unemployment durations for each level of maternal education. We hence combine the upper and lower two education categories. Reassuringly, Table B.2 shows that our main results are robust to this recoding.

[17]The psychological literature proposes several arguments for gender-specific effects, e.g., stronger social pressure on men to hold a job and easier access to alternative roles for women (e.g., Paul and Moser, 2009).

for Australia. In addition to the SF-36, the HILDA collects information on the K6 in 2008, 2010, and 2012. We therefore repeat our calculations using a sample restricted to these three waves and replacing the outcome variable. Table 4 shows the results. For comparison, Panel A reports the baseline findings for Australia from Table 2.

The nonparametric bounds and their confidence intervals based on the K6 in Panel B are very similar to our baseline results and confirm that unemployment has a significant negative effect on mental health in Australia. Panel C demonstrates that the alternative results are not driven by restricting the sample to three survey years. This sensitivity analysis supports the argument that different mental health measure do not change our main conclusions.[18]

We next turn to the MIV. Our main analysis draws on maternal education as an MIV because parental education is typically available in household surveys and allows us to apply a comparable research design across countries. Our MIV is valid if across the educational levels, the children of better-educated mothers do not have worse mean potential outcomes than the children of less-educated mothers. Obviously, as mean potential mental health is not observed in the data, this assumption is not testable. It is therefore crucial that the results remain robust to the use of plausible alternative MIVs.

We start with the most natural and easily available alternative – paternal education. Table 5 shows that only the bounds for the US are somehow sensitive to this change in the MIV. The most striking difference is that in contrast to Table 2, the confidence intervals for the US now do not exclude a zero effect, which suggests that the identifying power of paternal education is weaker. Nevertheless, the bounds remain negative and, thus, qualitatively in line with the main results. For the UK, the upper bound becomes slightly more negative, i.e. the weakest possible effect is stronger when using paternal education as MIV. For the remaining countries, the bounds and surrounding confidence intervals remain remarkably stable. Overall, the results in Table 5 support our main conclusion that unemployment has adverse consequences on mental health across these countries.

[18]However, the K6 bounds are considerably wider than the SF-36 bounds presumably due to less variation in the K6. Thus, the analysis suggests that the ATE for the US could be estimated more precisely if SF-36 data were available.

We also investigate individuals' own education as it is closely related to parental education through the intergenerational transmission of human capital. We find that using own education (either grouped into categories for the highest degree or as years of schooling) yields wider bounds, but does not qualitatively change our findings. Figure 5 illustrates the results for Australia. We focus on the Australian sample because another important advantage of the HILDA is that it includes Socio-Economic Indexes for Area (SEIFA). The SEIFA are summary measures that rank geographic areas in terms of their socio-economic characteristics.[19] Local measures of socio-economic status are suitable MIVs because the risk of mental disorders is higher in socio-economically disadvantaged areas (WHO and Calouste Gulbenkian Foundation, 2014), which is consistent with the MIV assumption. Figure 5 demonstrates that our results are highly robust to using different MIVs. Unfortunately, we cannot perform this robustness check for the other countries as the data lack comparable regional indices.

Finally, for all countries, we tested whether the results are robust to a change in the period of analysis and to conditioning on broad age groups. First, we split each sample into periods before and after the 2008 economic crisis. Table 6 shows that both the pre- and post-crisis bounds are significantly negative and largely overlap. Only for UK can we not exclude a zero effect after 2008.

Second, we conduct separate estimations for individuals above or below age 40 to allow for different patterns of the onset of mental health issues over the life-course.[20] Table B.5 shows that our general conclusions do not change as unemployment continues to have a significant causal effect on mental health for both age groups (except in the US under-40 group). For some groups, particularly in Australia and Germany, conditioning on age helps to tighten the bounds. However, the patterns are mixed and we do not find systematic differences across age groups. Yet, our basic conclusion remains unaffected.

[19] Details are given on the HILDA's website: http://melbourneinstitute.com/hilda/ [Last accessed: 01.04.2016].

[20] Note that our time-invariant MIVs do not permit modelling such processes dynamically; neither is this modelling required to estimate the bounds consistently under the maintained assumptions.

5.3 Comparison with other empirical approaches

In contrast to our analysis, the previous literature imposes stronger assumptions to address the bias of the ETS estimates. The most common approaches include conditioning on various socio-demographic characteristics or assuming that the bias is entirely due to individual time-invariant heterogeneity. For comparison, we apply these conventional approaches to our data. Table 7 provides the point estimates from OLS and fixed-effects (FE) regressions that condition on the individual's age (and its square), level of education, state of residence, marital status, number of children in the household, and interview month and year.[21]

A brief comparison of Tables 2 and 7 reveals only slight differences between the unconditional ETS and conditional OLS point estimates. Differences in observable characteristics between the unemployed and employed are therefore not a major explanation for the observed mental health disadvantages of the unemployed. Adding the individual fixed-effects considerably weakens the relationship between unemployment and mental health, but the relationship remains qualitatively large and statistically significant. The estimates are generally consistent with those reported in previous studies using fixed-effects and the same surveys for Australia (Green, 2011), Germany (Clark et al., 2001), and the UK (Binder and Coad, 2015).

Comparing our nonparametric bounds around the causal effect from Table 2 to the conventional estimates from Table 7 leads to two main conclusions. First, in most cases, the nonparametric bounds exclude the OLS point estimates, thereby confirming that the OLS regressions generally overestimate the magnitude of the causal effect of unemployment on mental health. Second, the FE estimates lie within the bias corrected bounds for the ATE suggesting that the role of time-varying heterogeneity (if any) is rather limited and that the FE approach might still yield informative estimates of the causal effect.

[21]The included control variables are common for numerous previous studies on the issue. We deliberately do not condition on an individual's health insurance status (public and/or private) since it is a potential transmission mechanism and endogenous to the employment status.

6 Conclusion

An extensive literature documents that the risk of experiencing mental illness is substantially higher among unemployed individuals than among employed individuals. Identifying the causal effect of unemployment on mental health is however challenging due to the potential bias from unobserved heterogeneity and reverse causality. The previous literature applies different empirical strategies to address the endogeneity of unemployment. While the findings differ across studies, most of the carefully conducted analyses find a negative effect. However, the results from these studies are not directly comparable due to differences between the data sets, samples, institutions settings, and identifying assumptions.

We take a more general approach and contribute comparable evidence on the effect of unemployment on mental health from four large OECD countries: Australia, Germany, the UK, and the US. These countries differ in various aspects related to labour market institutions and health care systems. To investigate the causal effect absent a common source of exogenous variation, we analyse the effect nonparametrically and compute bounds for the ATE (Manski and Pepper, 2000). The main advantages of this method are that we do not require exogenous variation in unemployment, that we rely on fairly weak and partially testable assumptions, and that we study the same parameter across countries.

For all four countries, we demonstrate that unemployment impairs mental health. This effect is similar for men and women. Moreover, we show that the negative impact on mental health materialises even with short spells of unemployment. Our results are robust to different identifying assumptions and different measures of mental health.

As unemployment impairs mental health in all four countries with their different welfare regimes, a generous welfare system seemingly does not nullify the effect of unemployment on mental health. Our bounds rule out effects of more than a quarter of a standard deviation for Germany, whereas the bounds allow for effects of up to half a standard deviation for Australia, the UK, and the US. Therefore, our results suggest that a more pronounced welfare regime–like the German one–might dampen the effect. To further scrutinise the role of the welfare regime for the effect of unemployment on mental health, generating comparable evidence from countries with even higher levels of social protection, such as the Nordic countries, is a promising avenue for future research.

As we bound the ATE whereas most of the recent studies estimate an ATT or LATE, any comparison of the results has to be somewhat tentative. In particular, one could always insist that we focus on a different parameter and that this explains any diverging findings. Nevertheless, our findings stand in contrast to previous studies that do not find significant effects of unemployment on mental health (e.g., Salm, 2009; Schmitz, 2011). We argue that the null results in the literature come from a lack of power rather than from the absence of an effect.

Our results are important for public policy measures aimed at reducing mental health problems, which have high direct and indirect costs (Collins et al., 2011). As even short periods of unemployment impair mental health, measures to prevent such problems must be implemented early. To design targeted and cost-effective policies, future research needs to further explore heterogeneities in the causal effect and to identify policies that mitigate the adverse consequences of unemployment.

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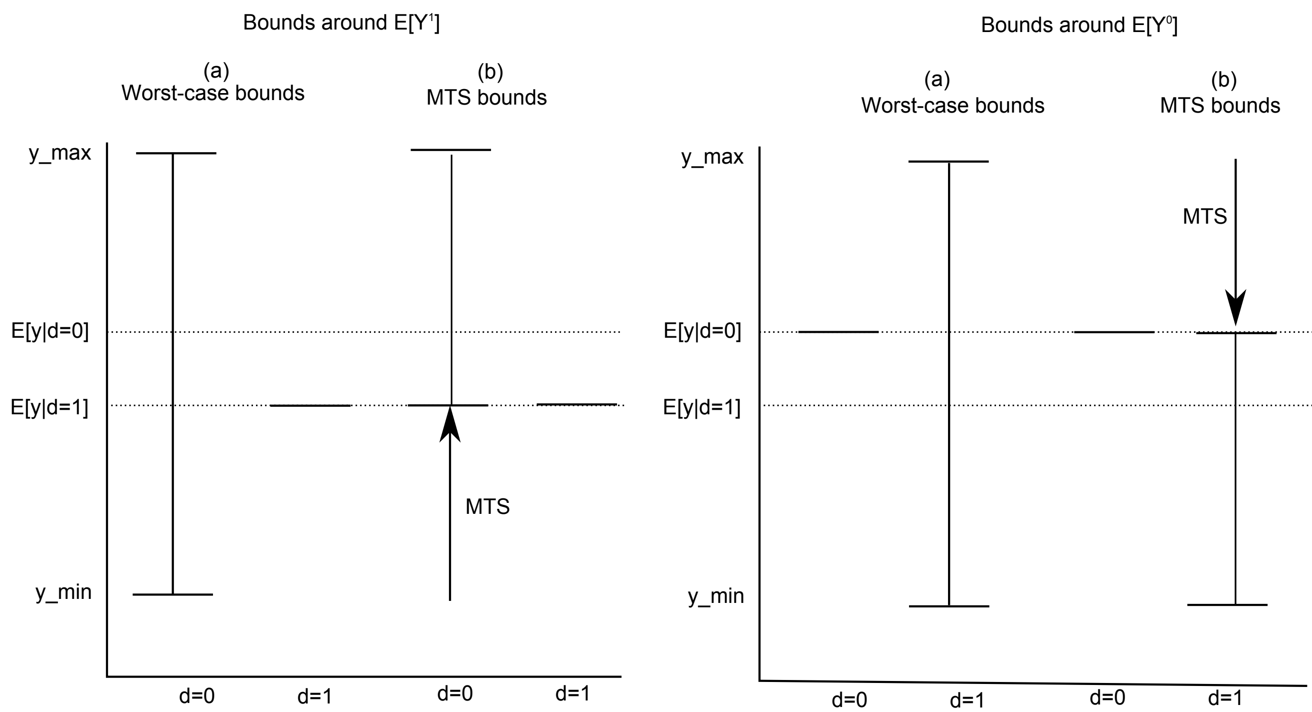
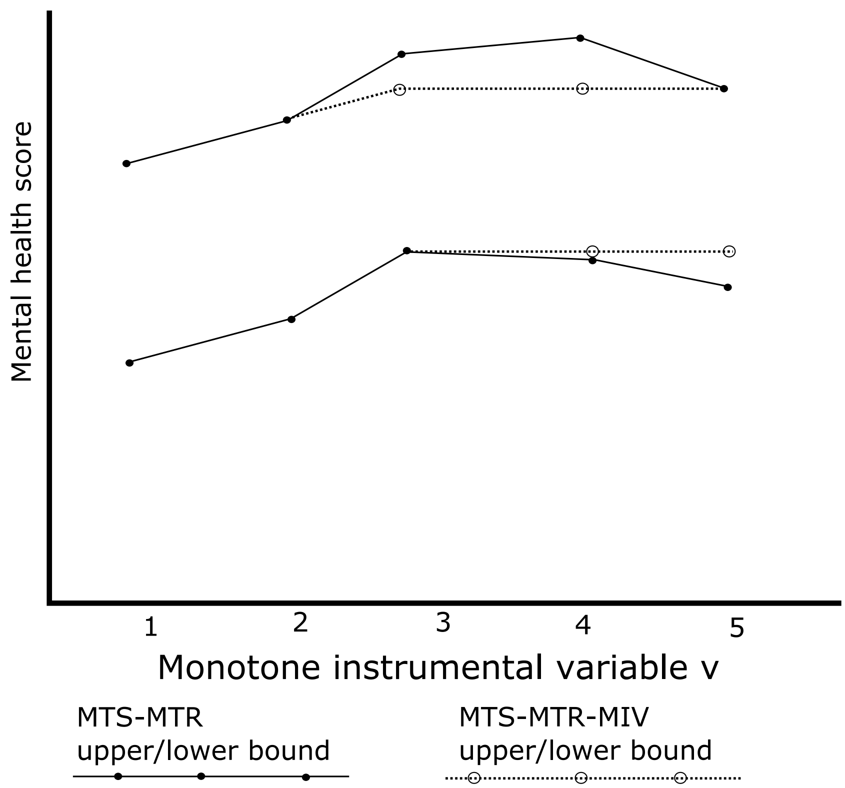
Figure 1: MTS Bounds



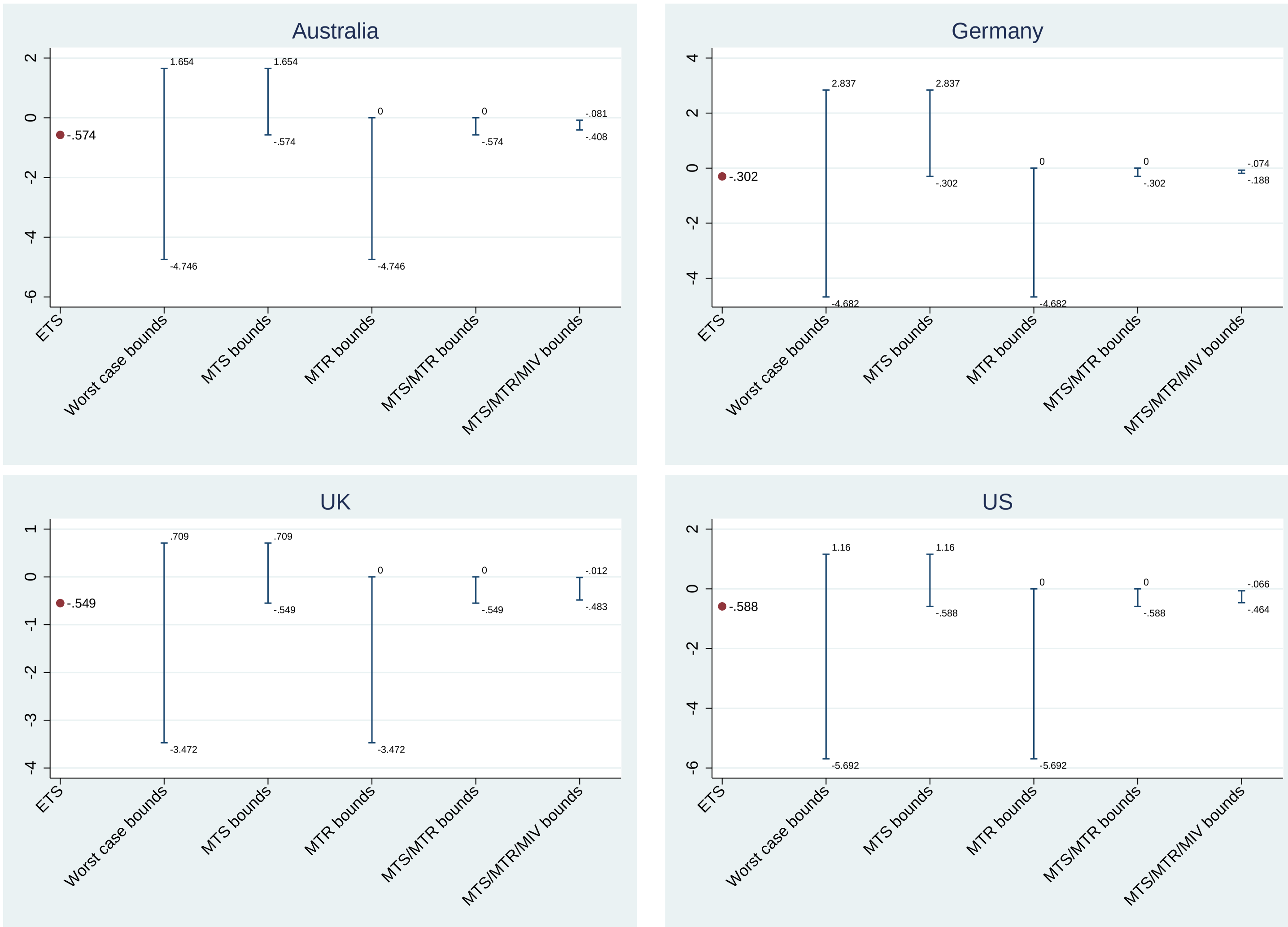
Note: Own illustration based on De Haan (2011).

Figure 2: MTS-MTR-MIV Bounds



Note: Own illustration based on De Haan (2011).

Figure 3: Bounds on Average Treatment Effect.

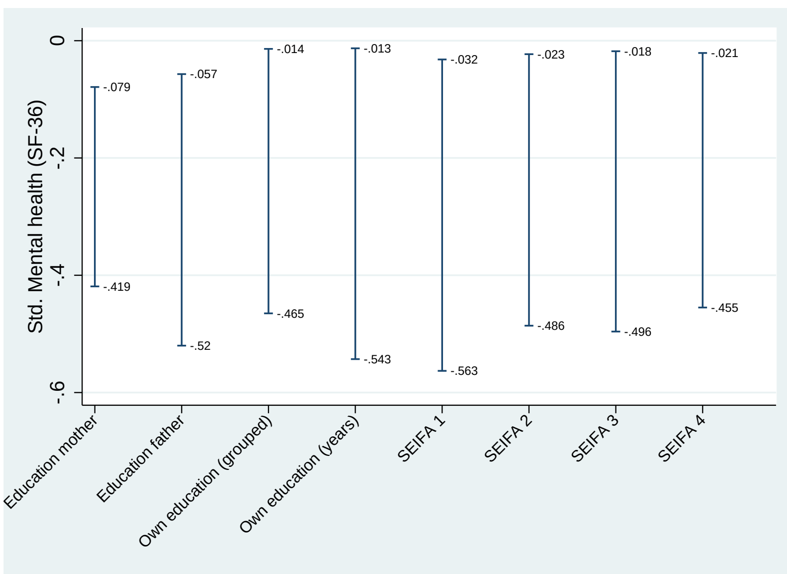


Note: MTS-MTR-MIV bounds use maternal education as MIV.

Figure 4: MTS-MTR-MIV bounds for the effect of unemployment duration versus employment

Notes: Bias-corrected MTS-MTR-MIV bounds use maternal education as MIV.

Figure 5: MTS-MTR-MIV bounds using different MIVs and HILDA



Notes: Bias-corrected MTS-MTR-MIV bounds. Bounds calculated for observations (N=55,672) with non-missing information on all MIVs.

Table 1: Descriptive statistics

	Australia	Germany	UK	US
Panel A: Average mental health scores				
Employed	.016	.021	.029	.040
(% employed)	97.1	93.0	94.6	93.2
Unemployed	-.558	-.281	-.520	-.548
(% unemployed)	2.9	7.0	5.4	6.8
N	*55,703*	*48,116*	*96,230*	*19,938*
Panel B: Employment and duration of unemployment				
Employed (%)	97.4	93.9	96.8	93.2
Unemployed <3 month (%)	1.4	1.3	2.5	3.3
Unemployed 3-12 months (%)	0.9	1.8	0.5	2.2
Unemployed 12+months (%)	0.3	3.1	0.2	1.3
N	*55,530*	*47,649*	*94,062*	*19,921*
Panel C: maternal education (MIV, monotone instrumental variable)				
None/primary (%)	12.2	5.0	1.7	7.1
Some secondary (%)	43.6	29.4	43.4	11.6
Secondary (%)	16.0	37.0	31.8	47.1
Secondary plus training (%)	19.8	18.5	17.4	16.4
University (%)	8.3	10.0	5.7	17.8
N	*55,703*	*48,116*	*96,230*	*19,938*

Note: Panel B has fewer observations because unemployment duration data are not available for all unemployed individuals. We use the following standardised measures of mental health: SF-36 for Australia, SF-12 for Germany, GHQ-12 for the UK, and the K6 for the US (see Section 4.2). See Table B.1 for exact definitions of the education categories for each country.

Table 2: MTS-MTR-MIV bounds on effect of unemployment on mental health

	ETS	Lower Bound	Upper Bound	Bias-corrected Lower Bound	Bias-corrected Upper Bound
Panel A: Australia	-.574	-.408	-.081	-.422	-.079
N=55,703		(-.519	-.059)	(-.533	-.056)
		[-.515	-.057]	[-.528	-.052]
Panel B: Germany	-.302	-.188	-.074	-.205	-.073
N=48,116		(-.273	-.048)	(-.289	-.047)
		[-.254	-.046]	[-.269	-.046]
Panel C: UK	-.549	-.483	-.012	-.506	-.009
N=96,230		(-.549	-.001)	(-.549	0)
		[-.530	-.005]	[-.543	-.001]
Panel D: US	-.588	-.464	-.066	-.473	-.065
N=19,938		(-.536	-.041)	(-.545	-.040)
		[-.541	-.036]	[-.559	-.033]

Note: The dependent variable is a standardised measure of mental health (see Section 4.2 for details). Estimates based on the exogenous treatment selection (ETS) assumption are calculated as the raw mean difference in mental health between unemployed and employed individuals. We use maternal education as the monotone instrumental variable (MIV). Imbens and Manski (2004) 95% confidence intervals reported in parentheses. Percentile 95% confidence intervals reported in brackets. Bias-corrected bounds and confidence intervals calculated using 1000 bootstrap repetitions.

Table 3: MTS-MTR-MIV bounds on effect of unemployment on mental health by gender

				Bias-corrected	
	ETS	Lower Bound	Upper Bound	Lower Bound	Upper Bound
Panel A: Australia					
Men	-.594	-.255	-.071	-.258	-.069
N=28,263		(-.440	-.040)	(-.444	-.038)
		[-.468	-.037]	[-.479	-.034]
Women	-.549	-.432	-.091	-.456	-.090
N=27,440		(-.529	-.058)	(-.549	-.056)
		[-.517	-.055]	[-.549	-.051]
Panel B: Germany					
Men	-.270	-.180	-.082	-.198	-.079
N=24,950		(-.246	-.047)	(-.264	-.044)
		[-.238	-.045]	[-.267	-.041]
Women	-.320	-.096	-.061	-.107	-.053
N=23,166		(-.268	-.024)	(-.273	-.016)
		[-.251	-.034]	[-.261	-.017]
Panel C: UK					
Men	-.554	-.498	-.032	-.537	-.025
N=44,968		(-.554	-.011)	(-.554	-.004)
		[-.528	-.014]	[-.554	-.008]
Women	-.581	-.456	-.001	-.490	0
N=51,262		(-.581	0)	(-.581	0)
		[-.569	0]	[-.587	0]
Panel D: US					
Men	-.472	-.254	-.147	-.282	-.146
N=7,911		(-.373	-.105)	(-.401	-.103)
		[-.352	-.103]	[-.398	-.102]
Women	-.670	-.589	-.018	-.606	-.007
N=12,026		(-.670	0)	(-.670	0)
		[-.670	-.001]	[-.670	0]

Note: The dependent variable is a standardised measure of mental health (see Section 4.2 for details). Estimates based on the exogenous treatment selection (ETS) assumption are calculated as the raw mean difference in mental health between unemployed and employed individuals. We use maternal education as the monotone instrumental variable (MIV). Imbens and Manski (2004) 95% confidence intervals reported in parentheses. Percentile 95% confidence intervals reported in brackets. Bias-corrected bounds and confidence intervals calculated using 1000 bootstrap repetitions.

Table 4: MTS-MTR-MIV bounds using different mental health measures.

			Bias-corrected	
	Lower Bound	Upper Bound	Lower Bound	Upper Bound
Panel A: SF-36 (years 2001-2012)				
N=55,703	-.408	-.081	-.422	-.079
	(-.519	-.059)	(-.533	-.056)
	[-.515	-.057]	[-.528	-.052]
Panel B: K6 (years 2008, 2010, 2012)				
N=14,600	-.311	-.082	-.326	-.079
	(-.491	-.045)	(-.506	-.041)
	[-.507	-.044]	[-.521	-.039]
Panel C: SF-36 (years 2008, 2010, 2012)				
N=14,600	-.249	-.109	-.268	-.104
	(-.439	-.065)	(-.457	-.060)
	[-.453	-.066]	[-.483	-.057]

Note: The dependent variable is a standardised measure of mental health (see Section 4.2 for details). Results are based on the Australian data only. We use maternal education as the monotone instrumental variable (MIV). Imbens and Manski (2004) 95% confidence intervals reported in parentheses. Percentile 95% confidence intervals reported in brackets. Bias-corrected bounds and confidence intervals calculated using 1000 bootstrap repetitions.

Table 5: MTS-MTR-MIV bounds on effect of unemployment on mental health

				Bias-corrected	
	ETS	Lower Bound	Upper Bound	Lower Bound	Upper Bound
Panel A: Australia	-.574	-.482	-.060	-.496	-.059
N=55,703		(-.556	-.043)	(-.570	-.042)
		[-.550	-.042]	[-.569	-.041]
Panel B: Germany	-.302	-.176	-.072	-.191	-.068
N=48,116		(-.268	-.032)	(-.284	-.028)
		[-.243	-.034]	[-.252	-.030]
Panel C: UK	-.549	-.490	-.032	-.516	-.028
N=96,230		(-.549	-.001)	(-.549	0)
		[-.526	-.012]	[-.549	-.007]
Panel D: US	-.588	-.550	-.019	-.565	-.018
N=19,938		(-.588	0)	(-.588	0)
		[-.588	0]	[-.588	0]

Note: The dependent variable is a standardised measure of mental health (see Section 4.2 for details). Estimates based on the exogenous treatment selection (ETS) assumption are calculated as the raw mean difference in mental health between unemployed and employed individuals. We use paternal education as the monotone instrumental variable (MIV). Imbens and Manski (2004) 95% confidence intervals reported in parentheses. Percentile 95% confidence intervals reported in brackets. Bias-corrected bounds and confidence intervals calculated using 1000 bootstrap repetitions.

Table 6: MTS-MTR-MIV bounds on effect of unemployment on mental health, before and after the 2008 crisis

				Bias-corrected	
	ETS	Lower Bound	Upper Bound	Lower Bound	Upper Bound
Panel A: Australia					
Pre-crisis	-.582	-.402	-.106	-.436	-.105
N=30,693		(-.529	-.072)	(-.563	-.071)
		[-.501	-.069]	[-.548	-.069]
Post-crisis	-.564	-.401	-.065	-.433	-.064
N=25,010		(-.556	-.035)	(-.564	-.034)
		[-.538	-.031]	[-.564	-.030]
Panel B: Germany					
Pre-crisis	-.279	-.118	-.08	-.131	-.071
N=33,431		(-.233	-.040)	(-.242	-.033)
		[-.218	-.048]	[-.227	-.033]
Post-crisis	-.364	-.213	-.073	-.234	-.064
N=14,685		(-.330	-.034)	(-.351	-.025)
		[-.323	-.040]	[-.353	-.022]
Panel C: UK					
Pre-crisis	-.484	-.300	-.059	-.331	-.057
N=59,087		(-.484	-.032)	(-.484	-.029)
		[-.423	-.034]	[-.428	-.032]
Post-crisis	-.618	-.548	0	-.571	0
N=37,143		(-.618	0)	(-.618	0)
		[-.615	0]	[-.618	0]
Panel D: US					
Pre-crisis	-.622	-.426	-.081	-.435	-.075
N=9,630		(-.543	-.044)	(-.551	-.038)
		[-.558	-.046]	[-.580	-.032]
Post-crisis	-.569	-.471	-.050	-.525	-.046
N=10,308		(-.569	-.015)	(-.569	-.011)
		[-.561	-.018]	[-.569	-.013]

Note: We define survey years including 2008 as pre-crisis, later years as post-crisis. The dependent variable is a standardised measure of mental health (see Section 4.2 for details). Estimates based on the exogenous treatment selection (ETS) assumption are calculated as the raw mean difference in mental health between unemployed and employed individuals. We use maternal education as the monotone instrumental variable (MIV). Imbens and Manski (2004) 95% confidence intervals reported in parentheses. Percentile 95% confidence intervals reported in brackets. Bias-corrected bounds and confidence intervals calculated using 1000 bootstrap repetitions.

Table 7: OLS and FE estimates for the effect of unemployment on mental health

	OLS		FE	
Panel A: Australia	-0.501	***	-0.158	***
	(0.038)		(0.029)	
N	*55,703*		*55,703*	
Panel B: Germany	-0.267	***	-0.163	***
	(0.026)		(0.028)	
N	*48,116*		*48,116*	
Panel C: UK	-0.562	***	-0.494	***
	(0.023)		(0.028)	
N	*96,230*		*96,230*	
Panel D: US	-0.480	***	-0.277	***
	(0.042)		(0.038)	
N	*19,938*		*19,938*	

*Notes: Each cell represents a separate linear regression. The dependent variable is a standardised measure of mental health (see Section 4.2 for details). Each regression includes controls for the respondent's age and its square, highest level of education, state of residence, marital status, number of children in the household, sex, ethnicity, maternal education, and interview month and year. Cluster-robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.*

Appendix A: Institutions

In a worldwide comparison, Australia, Germany, the UK, and the US rank among the highest income countries with relatively high labour force participation rates.[22] However, these countries differ considerably in several dimensions that might alter the effect of unemployment on mental health. Table A.1 illustrates the magnitudes of these differences by providing a few recent statistical highlights from the relevant labour market institutions and health care systems.

In 2013, the unemployment rates in Australia and Germany (approximately 5.5%) were two percentage points lower than the levels in the UK and US. Figure A.1 provides a closer look at the development of the unemployment rates in the analysed countries since 2000. Germany improved from the country with the highest unemployment rate in 2000 to the lowest unemployment rate in 2013. Interestingly, the unemployment rate in Germany began to decline after the introduction of substantial labour market reforms in the mid-2000s (Hartz reforms). The other countries had relatively low and even declining unemployment rates until the beginning of the economic crisis in September 2008. Since then, unemployment in Australia, the UK, and the US increased considerably and has not yet returned to pre-crisis levels.

Unemployment patterns and durations also differ across countries. Table A.1 shows that Germany still has substantially more long-term unemployed workers than the remaining countries. At the same time, 88%, that is, the vast majority of German unemployed persons receive some unemployment benefits compared to, e.g., only 26.5% in the US. These numbers are likely to reflect different social security systems.

We now briefly illustrate some differences in the relevant institutions.[23] Australia does not provide an UI system[24], while in Germany and the UK, employees must contribute to UI schemes (OECD, 2015a). Generally, a claimant must have contributed to the system for at least 12 months in the last 2 years to be eligible for UI benefits. However, specific eligibility criteria, strictness, benefit duration, and generosity differ between the two countries. While the German *Arbeitslosengeld I* replaces 60% of the previous net earnings and might last for two years[25], the British *Jobseeker's Allowance* (JSA) is payable as a fixed amount (relative to the average worker wage)[26] for up to 182 days i.e., less than 6 months in one job-seeking period. In Australia, unemployed individuals younger than 22 are eligible for a *Youth Allowance* and those older than 22 for a *Newstart Allowance*. The rates depend on an individual's living arrangements (single/partnered) and on the presence of dependent children (OECD, 2015a). In the US, the legislation for UI benefits varies by state. Typically, regular state UI benefits last up to 20 weeks and are conditional on a minimum number of weeks worked or wages earned in the base period (Venn, 2012; OECD, 2015a).

In contrast to Australia, Germany, and the UK, there are no unemployment assistance (UA)

[22] During the 2000s, the GDP per capita values of these countries have continuously exceeded 30,000 USD (in 2005 USD) and their labour force participation rates exceeded 70% (of the population aged 15–64) (World Bank, 2015).

[23] We follow the OECD definitions of unemployment insurance (UI) and assistance (e.g., see OECD, 2015b).

[24] However, in Australia, individuals still receive unemployment benefits from the social income support system that is funded through general taxation.

[25] The duration of benefits generally ranges from 6 to 12 months and depends on the contribution period and employment record. However, special rules apply to individuals aged 50 and above whose eligibility period increases gradually to 24 months (OECD, 2015a).

[26] In 2013, the weekly amount for a single person was 56.80 GBP if aged 16–24 and 71.70 GBP if 25 or over (OECD, 2015a).

schemes in the US.[27] UA benefits are usually available for working-age individuals who are able to work and whose families are in severe financial need, e.g., because UI benefits have been exhausted. In all three countries, UA benefits are subject to means-testing on family income and payable at a fixed amount as long as the jobseeker meets specific eligibility conditions (see, e.g., OECD, 2015a). Table A.1 shows a simple statistic that attempts to compare the generosity of these different UA schemes in 2010; the maximum benefit amount was 18% of the average wage in Australia and approximately 10% in Germany and the UK.

The four countries also exhibit several striking differences in terms of their health care systems and access to mental health services. Save for the US, their entire populations are covered by a social health protection systems. While the US has the highest average out-of-pocket expenditures per capita (858 USD), its government accounts for the lowest share of the total health expenditure (47%). With regard to mental health, all four countries have officially approved mental health policies (WHO, 2015c) but the availability of mental health services varies across countries. For example, Germany's facilities provide the largest number of beds for mental health treatment. Regarding health professionals, the US display the lowest provision of psychiatrists and nurses, which might, to some extent, be offset by relatively high numbers of psychologists and social workers in the mental health sector.

Given that care delivery might be demand driven (and vice versa), Table A.1 summarises comparable numbers on treatment for mental health reasons. The number of patients treated in mental health outpatient facilities is highest in the UK, followed by Australia and the US. While treatment in outpatient facilities is relatively uncommon in Germany, it has the highest number of patients admitted to mental hospitals, which is rather rare in Australia. A brief inspection of suicide rates suggests that the UK population exhibits by far the lowest incidence rates. Nevertheless, we observe a common trend in all four countries: the risk of suicide for men is over three times higher than for women (WHO, 2015b).

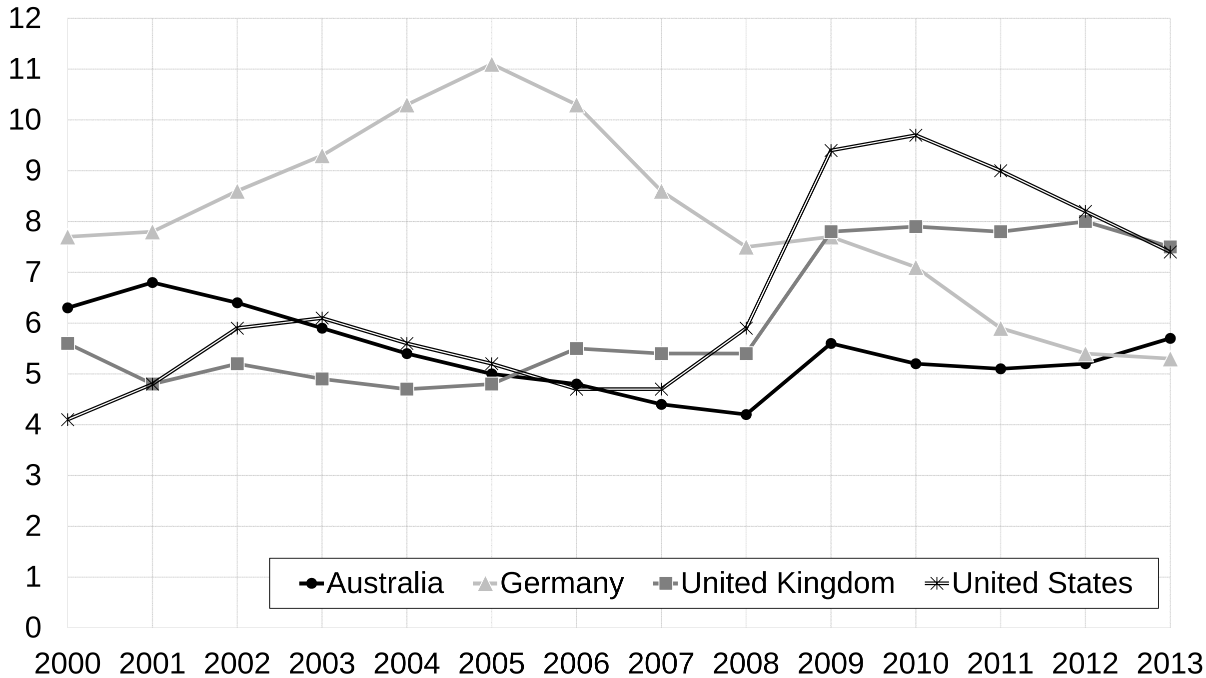
[27] However, other programs assist individuals who have low incomes, e.g., the Supplemental Nutrition Assistance Program (SNAP) or Temporary Assistance for Needy Families (TANF).

Table A.1: Selected indicators on labour market institutions and health care systems

	Australia	Germany	UK	USA
Unemployment rate (% of labour force)[a]	5.7	5.3	7.5	7.4
Long-term unemployment (% of unemployment)[b]	20.3	45.2	34.7	29.3
Effective coverage by benefits (% of unemployment)[c]	52.7	88.0	62.6	26.5
Unemployment insurance (UI) benefits[d]	not available	max. 12 months, replace 60% of net earnings	max. 6 months, fixed amount (9.9% of AW)	state-specific
Unemployment assistance (UA) benefits[d]	18% of AW	10.2% of AW	9.9% of AW	not available
Social health protection coverage (% of population)[e]	100	100	100	84
Government expenditure on health (% of total health expenditure)[f]	66.4	76.8	83.5	47.1
Out-of-pocket expenditure per capita (in 2005 USD)[f]	606	548	323	858
Beds for mental health treatment (per 100,000 population)[g]	49.1	88.7	60.9	56.1
Psychiatrists in mental health sector (per 100,000 population)[h]	12.8	15.2	17.7	7.8
Nurses in mental health sector (per 100,000 population)[h]	69.5	56.1	83.2	3.1
Psychologists in mental health sector (per 100,000 population)[h]	16.7	no information	12.8	29.6
Social workers in mental health sector (per 100,000 population)[h]	8.6	no information	2.0	59.8
Persons treated in mental health outpatient facilities (per 100,000 population)[h]	1,534.0	559.7	2,340.2	931.9
Admissions to mental hospitals (per 100,000 population)[h]	56.7	641.5	232.3	256.9
Age-standardised suicide rates (per 100,000 population)[i]	10.6	9.2	6.2	12.1

Note: [a]In 2013. Source: World Bank (2015). [b]In 2012; number of people with continuous periods of unemployment extending for a year or longer. Source: World Bank (2015). [c]In 2012; % of unemployment. Source: ILO (2015). [d]In 2010; AW denotes average worker wage. Source: OECD (2015b). [e]In 2010. Source: ILO (2015). [f]In 2013. Source: WHO (2015a). [g]In 2011; sum of beds in general hospitals, community residential facilities, and mental hospitals; own calculations. Source: WHO (2015c). [h]In 2011. Source: WHO (2015b). [i]In 2012. Source: WHO (2015b).

Figure A.1: Unemployment rates 2000–2013



Note: Unemployment rate (% of labour force).
Source: World Bank (2015).

Appendix B: Additional tables

Table B.1: Definition of parental education in the data.

	HILDA	BHPS	PSID	SOEP
1	None/primary	None	Less than high school (0-8 grades)	None/primary
2	Some secondary, no further	Left school, no qualifications	Did not finish high school (9-11 grades)	Low secondary (Hauptschule)
3	Secondary plus training	Left school, some qualifications	High school (12 grades)	Low secondary (Hauptschule) plus training
4	Year 12 (plus training)	Further educational qualifications	High school plus further training	Medium secondary (Realschule) plus training
5	University	University/higher degree	University/higher degree	High secondary schooling/ University

Table B.2: MTS-MTR-MIV bounds on effect of unemployment on mental health

	ETS	Lower Bound	Upper Bound	Bias-corrected Lower Bound	Bias-corrected Upper Bound
Panel A:	-.574	-.541	-.025	-.547	-.024
N=55,703		(-.574	-.010)	(-.574	-.009)
		[-.574	-.008]	[-.574	-.007]
Panel B: Germany	-.302	-.256	-.046	-.265	-.044
N=48,116		(-.297	-.029)	(-.302	-.027)
		[-.297	-.027]	[-.302	-.024]
Panel C: UK	-.532	-.494	-.012	-.495	-.011
N=96,230		(-.527	-.003)	(-.528	-.003)
		[-.532	-.003]	[-.532	-.003]
Panel D: US	-.588	-.475	-.045	-.476	-.045
N=19,938		(-.546	-.024)	(-.547	-.024)
		[-.556	-.021]	[-.557	-.021]

Note: The dependent variable is a standardised measure of mental health (see Section 4.2 for details). Estimates based on the exogenous treatment selection (ETS) assumption are calculated as the raw mean difference in mental health between unemployed and employed individuals. We use a recoded variable of maternal education as the monotone instrumental variable (MIV), where we combine the upper and lower two categories shown in Table B.1, respectively. Imbens and Manski (2004) 95% confidence intervals reported in parentheses. Percentile 95% confidence intervals reported in brackets. Bias-corrected bounds and confidence intervals calculated using 1000 bootstrap repetitions.

Table B.3: MTS-MTR-MIV bounds on effect of unemployment on mental health

				Bias-corrected	
	ETS	Lower Bound	Upper Bound	Lower Bound	Upper Bound
Panel A: Australia	-.480	-.211	-.080	-.227	-.074
N=9,417		(-.402	-.029)	(-.417	-.023)
		[-.396	-.031]	[-.426	-.023]
Panel B: Germany	-.271	-.180	-.067	-.230	-.049
N=8,422		(-.271	0)	(-.271	0)
		[-.267	-.016]	[-.271	-.002]
Panel C: UK	-.600	-.573	-.005	-.654	0
N=16,437		(-.600	0)	(-.600	0)
		[-.600	0]	[-.600	0]
Panel D: US	-.593	-.467	-.043	-.491	-.038
N=5,965		(-.593	-.003)	(-.593	0)
		[-.593	-.006]	[-.593	-.001]

Note: The dependent variable is a standardised measure of mental health (see Section 4.2 for details). Estimates are based on the first observation of each individual. Estimates based on the exogenous treatment selection (ETS) assumption are calculated as the raw mean difference in mental health between unemployed and employed individuals. We use maternal education as the monotone instrumental variable (MIV). Imbens and Manski (2004) 95% confidence intervals reported in parentheses. Percentile 95% confidence intervals reported in brackets. Bias-corrected bounds and confidence intervals calculated using 1000 bootstrap repetitions.

Table B.4: MTS-MTR-MIV bounds on effect of unemployment on mental health

	ETS	Lower Bound	Upper Bound	Bias-corrected Lower Bound	Bias-corrected Upper Bound
Panel A: Australia	-.567	-.359	-.087	-.366	-.085
N=57,645		(-.475	-.064)	(-.482	-.062)
		[-.481	-.062]	[-.485	-.059]
Panel B: Germany	-.295	-.201	-.073	-.224	-.076
N=49,280		(-.292	-.016)	(-.295	-.019)
		[-.256	-.04]	[-.275	-.036]
Panel C: UK	-.535	-.479	-.016	-.535	0
N=102,170		(-.535	0)	(-.535	0)
		[-.513	-.007]	[-.535	0]
Panel D: US	-.595	-.485	-.063	-.518	-.076
N=21,694		(-.595	-.032)	(-.595	-.045)
		[-.554	-.016]	[-.511	-.014]

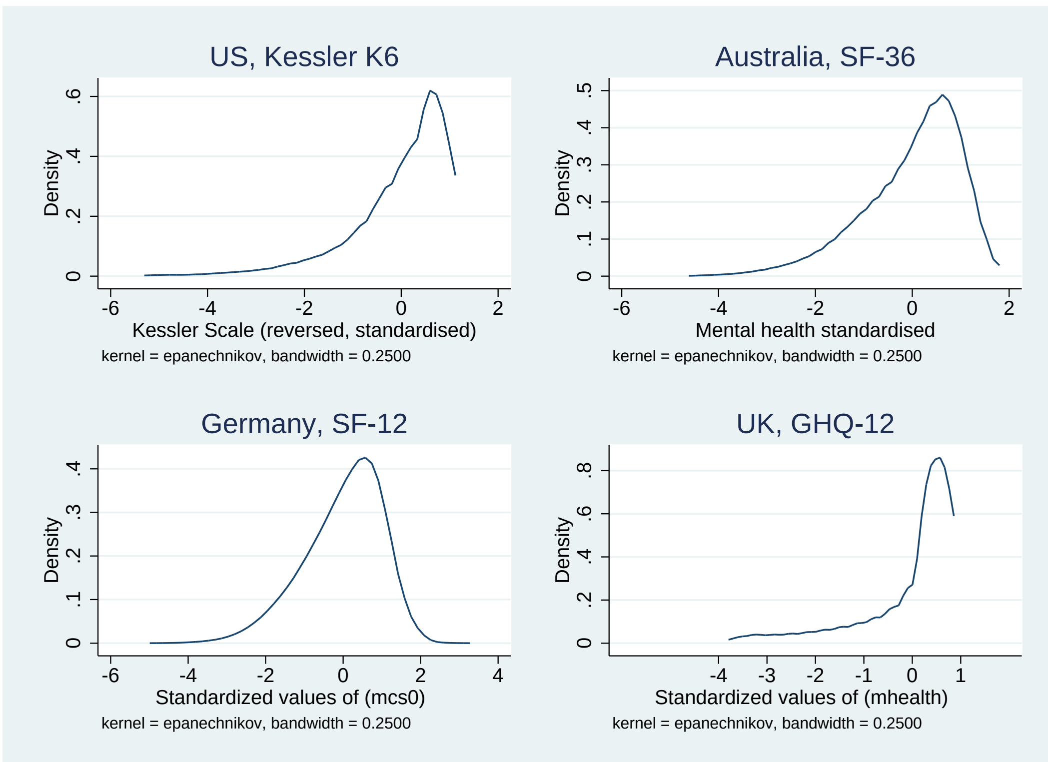
Note: The dependent variable is a standardised measure of mental health (see Section 4.2 for details). Estimates based on the exogenous treatment selection (ETS) assumption are calculated as the raw mean difference in mental health between unemployed and employed individuals. We use maternal education as the monotone instrumental variable (MIV). Imbens and Manski (2004) 95% confidence intervals reported in parentheses. Percentile 95% confidence intervals reported in brackets. Bias-corrected bounds and confidence intervals calculated using 1000 bootstrap repetitions.

Table B.5: MTS-MTR-MIV bounds on effect of unemployment on mental health by age groups

	ETS	Lower Bound	Upper Bound	Bias-corrected Lower Bound	Bias-corrected Upper Bound
Panel A: Australia					
Under 40	-.601	-.421	-.053	-.524	-.009
N=28,829		(-.601	0)	(-.601	0)
		[-.538	-.026]	[-.549	-.005]
Over 40	-.509	-.254	-.139	-.263	-.138
N=28,816		(-.379	-.091)	(-.389	-.090)
		[-.397	-.086]	[-.411	-.081]
Panel B: Germany					
Under 40	-.215	-.149	-.073	-.197	-.07
N=22,320		(-.215	-.040)	(-.215	-.037)
		[-.194	-.040]	[-.215	-.037]
Over 40	-.362	-.252	-.056	-.275	-.051
N=26,960		(-.357	-.015)	(-.362	-.010)
		[-.333	-.013]	[-.347	-.006]
Panel C: UK					
Under 40	-.517	-.459	-.022	-.482	-.019
N=53,255		(-.517	-.003)	(-.517	-.001)
		[-.498	-.006]	[-.517	-.005]
Over 40	-.555	-.444	-.035	-.465	-.033
N=48,915		(-.555	-.014)	(-.555	-.012)
		[-.530	-.012]	[-.538	-.010]
Panel D: US					
Under 40	-.592	-.523	-.005	-.592	0
N=12,524		(-.592	0)	(-.592	0)
		[-.590	0]	[-.592	0]
Over 40	-.561	-.422	-.079	-.442	-.068
N=9,170		(-.540	-.040)	(-.559	-.030)
		[-.542	-.047]	[-.561	-.026]

Note: The dependent variable is a standardised measure of mental health (see Section 4.2 for details). Estimates based on the exogenous treatment selection (ETS) assumption are calculated as the raw mean difference in mental health between unemployed and employed individuals. We use maternal education as the monotone instrumental variable (MIV). Imbens and Manski (2004) 95% confidence intervals reported in parentheses. Percentile 95% confidence intervals reported in brackets. Bias-corrected bounds and confidence intervals calculated using 1000 bootstrap repetitions.

Figure B.1: Distribution of mental health measures



Note: Calculated for estimation samples.