# The negative mental health effect of unemployment: Meta-analyses of cross-sectional and longitudinal data

Inaugural-Dissertation

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To my parents,

Artur and Hannelore Paul,

in love and gratitude.

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# 1. Introduction

## 1.1. Research questions

"We entered Marienthal as scientists. We left it with the wish that our time will soon be relieved of the tragic opportunity for such an experiment" (Jahoda, Lazarsfeld & Zeisel, 1933/1975, p. 107, translation into English by the author). This is the last sentence of the renowned Marienthal study, without doubt one of the most influential studies in the field of psychological unemployment research. It brought deep insights into the devastating effects of unemployment and is still regarded as exemplary in the field (Fryer, 2001; Wacker, 2001). However, the wish that the authors expressed in the early thirties was not fulfilled, as unemployment rates remained depressingly high during the 30ties in most parts of the western world and later, after a phase of low unemployment rates following World War II, mass unemployment came back during the 70ties and is still a problem in many countries. Thus, researchers far too often had the "tragic possibility" to study the psychological effects of unemployment during the 20th century. As a result, psychologists, sociologists, and researchers from other disciplines have accumulated a vast amount of knowledge concerning the psychological aspects of unemployment, especially its effects upon mental health. However, this knowledge is spread over thousands of published and unpublished research reports, making it difficult even for experts to review all the existing information.

Other scholars have already done a good job in meta-analysing interesting aspects of this knowledge. However, two of the three existing meta-analyses included only a very small number of primary studies (Foster, 1991; Murphy & Athanasou, 1999), and the third's focus lay upon "psychological (…) well-being during unemployement" (Mc-Kee-Ryan, Song, Wanberg, & Kinicki, 2005, p.53), i.e. this study was primarily concerned with correlates of distress *within* the group of unemployed persons. In contrast to this, the present analysis will focus upon the difference between unemployed and employed persons with regard to mental health, the moderators of this difference, the causal mechanisms behind it, and its stability when publication bias and related biaees are tested. Therefore, I try to answer the following questions in the present meta-analysis: How strong is the difference between unemployed and employed persons with regard to mental health? Does unemployment primarily lead to depression and low self esteem or does it also cause other forms of distress such as anxiety or

psychosomatic symptoms? Do important demographic variables such as age, gender, social status, and marital status act as moderators of the negative mental health effect of unemployment? Are there differences between countries and what characteristics distinguish countries where unemployment is more deleterious from countries where it is less deleterious? Are unemployed persons' elevated distress levels really caused by unemployment or are other explanations more relevant, for example selection effects (= persons with impaired mental health lose their jobs earlier and need more time to find new jobs than more healthy persons)? How valid are the meta-analytic findings? Is it possible to identify biases, particularly a publication bias, that might have distorted the results?

## 1.2. Structure of the thesis

The structure of the thesis is as follows: The first chapter after the introduction is concerned with the historical development of the concept of unemployment and its contemporary definition. Furthermore, some unemployment figures are reviewed in order to inform the reader about the economic size of the problem. The second chapter is concerned with psychological unemployment research. After a quick overview of the historical development of this research field, a short "review of reviews" follows, where the results of older reviews and meta-analyses are summarized. Then an introduction to the three most important theories in psychological unemployment research follows. After that, two paragraphs deal with more specific, yet important questions. The first of these questions concerns a possible unemployment syndrome, i.e. a specific cluster of symptoms that might be typical for unemployed people. The second is the question of causality, i.e. whether unemployment is only correlated with distress or really causes it. Next, short theoretical introductions to several moderator variables follow that will be tested in the present meta-analysis. The theoretical part of the thesis ends with an introduction to the problem of publication bias and related problems.

In the methods section, first the literature search and the inclusion criteria for the meta-analysis are described. Then the operationalizations of employment, unemployment, and mental health are described that were used in the present meta-study. Following that come chapters concerning the statistical methods used here. The statistical methods for longitudinal data are described separately, as they are important and have some special characteristics that distinguish them from the methods for cross-sectional data. To ease the reading of the text, a

more detailed description of the methods employed in the meta-analysis, especially of the coding procedures, was separated from the main text and can be found in the technical appendix.

The results section begins with a short description of the sample of studies meta-analysed here. Then, a short chapter on the intercorrelations of the six mental health variables used in the present study follows, in which I try to answer the question whether they really measure the same underlying construct, i.e. mental health. Then the results for cross-sectional comparisons are presented, followed by the description of the results for longitudinal comparisons. A presentation of the results of the sensitivity analysis, checking the validity of the other results, finishes this section.

The discussion starts with a summary of the findings. Then the following topics are discussed: the validity of the meta-analytic findings, the degree of agreement with results of other meta-analyses, some important findings, and research gaps. Finally, the thesis ends with a discussion of political and societal implications.

# 2. Unemployment: The history of the concept, definitions, unemployment figures

This chapter introduces the reader to the historical development of the concept of unemployment and to its contemporary definition. Some unemployment figures are also briefly reviewed.

## 2.1. The concept of unemployment

People whom we would today consider "unemployed" probably lived during all ages. In ancient Athens, for example, Pericles undertook "vast projects of buildings and designs of work" with the goal "that the undisciplined and mechanic multitude … should not go without their share of public salaries, and yet should not have them given for sitting still and doing nothing" (Plutarch, cited after Garraty, 1978, p. 13). The bible also knows persons who have problems finding work despite their ability and motivation to work: "For the kingdom of heaven is like unto a man that is an householder, which went out early in the morning to hire labourers into his vineyard. And when he had agreed with the labourers for a penny a day, he sent them into his vineyard. (…) And about the eleventh hour he went out, and found others standing idle, and saith unto them, Why stand ye here all the day idle? They say unto him, Because no man hath hired us." (*Matthew*, 20: 1-7).

However, in times of old unemployed people were not perceived as a distinct social group, they were simply an unrecognized subgroup of the large group of paupers. The abstract concept of "unemployment" did not emerge before the industrial revolution (Garraty, 1978; Niess, 1979). This was the time when the modern, "free" employer-employee-relationship was established, an important prerequisite of mass unemployment. The business cycle fluctuations that are typical for the modern economic system started and forced European and northern American societies to accept that it is not true that there is always enough work to do for everyone who is willing and able to work, an attitude that had dominated the thinking of earlier centuries. This process of understanding was a slow process, and therefore the modern English word "unemployment" and the German equivalent "Arbeitslosigkeit" did not come into general use until 1890. At the same time the institutions were created that are used today in order to deal with the unemployment phenomenon and that are so typical for contemporary

societies, e.g. regular unemployment statistics and unemployment insurance systems (Garraty, 1978; Niess, 1979).

## 2.2. The definition of unemployment

There is still no complete agreement concerning the definition of unemployment between different countries, and even within a country different institutions sometimes adhere to different definitions of unemployment (Hollederer, 2002). Within the field of psychological unemployment research, diverging definitions also prevail. Feather (1990) for example used the following definition that is rather close to the concept of the International Labour Office (ILO) (see below): Unemployed people are persons „who are of working age and who are active job seekers. (...) we exclude discouraged unemployed people who have given up looking for a job and others who, for whatever reason (e.g., poor health, family responsibilities), are unemployed but not actively seeking a job“ (p. 3). Jahoda (1986), on the other hand, used a much broader definition: “all those can be considered as unemployed, who have no job but would like to have one, or have to rely on financial support in order to survive as long as they don’t have a job” (p. 32, translation into English by the author). However, most contemporary national definitions of unemployment are oriented towards the concept of the International Labour Office and include the three core elements of this definition that represents an international compromise:

”The ’unemployed‘ comprise all persons above a specific age who during the reference period were:

(a) ’without work‘, i.e. were not in paid employment or self-employment (...);

(b) ’currently available for work’, i.e. were available for paid employment or self-employment during the reference period; and

(c) ’seeking work‘, i.e. had taken specific steps in a specified reference period to seek paid employment or self-employment“ (ILO, 2000a, p. 429).

This definition shows that unemployment is a complex, multidimensional construct, involving not only situational aspects (non-employment), but also motivational aspects (“seeking work”) and medical and legal aspects (being “available for work”).

## 2.3. The size of the problem – unemployment figures

With the exception of the war year 1944 the number of unemployed persons in the USA - the world's most economically powerful nation during the 20th century - has always been more than a million since 1929. In some years (1932-1935, 1938, 1982-1983) even more than ten million Americans were involuntarily out of work (United States Department of Labor, 1990ab). In times of peace, full employment, i.e. an unemployment rate below three percent, has been rare during the 20th century. The same is true for other western countries. In France, for example, World War I was preceded and World War II was followed by periods of very low unemployment rates, while the rest of the 20th century was characterized by much less favourably labour market conditions (Dormois, 2004). A similar picture emerges for Germany, where the only phase of full employment that lasted for several years in peace times was during the late 1950ies and the 1960ies (Niess, 1979). Thus, mass unemployment was a rather frequent phenomenon in industrialized and post-industrialized economies during the last hundred years.

The unemployment rate for the whole world was 6.3% in 2003, meaning that several hundreds of millions of people were unemployed somewhere in the world. The world regions with the highest unemployment rates were the Middle East and North Africa with 12.2%, and Sub-Saharan Africa with 10.9% unemployment in 2003 (International Labour Office, 2005). Thus, unemployment today is not only a problem of western/industrialized economies, but of the whole world.

Although already rather worrying, official statistics such as the ones reported above may underestimate the size of the problem, as some groups of people who are in a situation very similar to unemployment are not counted as unemployed here. Discouraged workers, for example, i.e. persons who are not currently working and who do not actively look for work because they feel that no jobs are available, are not included in the official statistics of unemployment (Jensen & Slack, 2003).

We can conclude that high levels of unemployment appear to be a problem that repeatedly bothered western economies during the last century and that currently affects a large number of countries. However, is this economic problem also a problem for public health? As Jahoda and many other researchers in her succession surmised that unemployment negatively affects mental health, a large-scale meta-analysis of empirical studies on this question is justified.

# 3. Psychological unemployment research

The intention of this third chapter is to inform the reader about important aspects of the field of psychological unemployment research. This includes the historical development of this research field, important findings as they are summarized in reviews and meta-analyses, and important theories that were applied to or developed with regard to unemployment. The question of an unemployment syndrome is also discussed, as well as the important topic of causation. Finally, theoretical considerations about moderator variables conclude the chapter.

## 3.1. Historical development

Scientists started shortly after the concept of unemployment was established during the end of the 19th century to do research on the psychological aspects of this phenomenon. As early as 1911, Rowntree and Lasker published a study that included the observation that the effects of unemployment were "demoralising" and "deleterious (...) on a man's character" (p. 309). Subsequently, research interest slowly increased until there was a sharp peak during the 1930ies, when unemployment rates were incredibly high everywhere in the industrialized world. This first phase of research was summarized in the seminal review of Eisenberg and Lazarsfeld (1938). Later on, interest in the psychological aspects of unemployment was renewed in the 1970ies when unemployment rates were rising again in the Western World. Nowadays, research interest continues to be high.

Psychologists were interested in a large number of different research questions concerning unemployment during the first century of research. However, in my judgement two topics always received more intense attention than other topics. One is the question psychological aspects of the job search process. What are important psychological antecedents of job search behavior? Which methods of job searching are more successful than others? Important results from this branch of research were recently meta-analysed by Kanfer, Wanberg, and Kantrowitz (2001). The second topic that received even more attention, in my impression, is the question of the effect of unemployment on health, particularly on mental health. Is unemployment associated with distress? How strong is this effect? Is unemployment really the cause of distress or merely a correlate? Empirical findings concerning these and other questions regarding the mental health effects of unemployment will be meta-analysed here.

## 3.2. Existing reviews and meta-analyses in psychological unemployment research

Several reviews have been published that summarise and integrate the results of nearly a century of scientific efforts concerning the psychological effects of unemployment (e.g., Catalano, 1991; DeFrank, & Ivancevich, 1986; Dooley, Fielding & Levi, 1996; Eisenberg & Lazarsfeld, 1938; Ezzy, 1993; Frese & Mohr, 1978; Fryer & Payne, 1986; Hammarström, 1994; Kasl, Rodriguez & Lasch, 1998; Lennon, 1999; McKee-Ryan & Kinicki, 2002; Morris, & Cook, 1991; Olafsson & Svensson, 1986; Price, Friedland, & Vinokur, 1998; Schultz-Gambard & Balz, 1988; Wanberg, Kammeyer-Mueller, & Shi, 2001; Warr, Jackson, & Banks, 1988; Winefield, 1995). These reviews have provided valuable insights into the psychological effects of unemployment: For example, they agree that unemployment is associated with an increase in distress symptoms and with diminished well-being as measured on a large range of scales for depression, hopelessness, anxiety, psychosomatic symptoms, life satisfaction, self esteem, etc. However, most of these reviews have been written in the traditional narrative form, using varying standards of problem formulation, literature search, data extraction and evaluation, data analysis, as well as interpretation and presentation of results (Cooper & Hedges, 1994). Most importantly, they usually did not provide information on the size of the effects under consideration, which makes it difficult to gauge the strength and practical importance of the findings.

Several researchers (e.g., Catalano, 1991; Dooley & Catalano, 1980; Frese & Mohr, 1978; Hammarström, 1994; O'Brien, 1986; Platt, 1985; Warr, 1985; Warr, Jackson, & Banks, 1988; Warr & Perry, 1982) attempted to introduce a more systematic approach to the review of research in the field of psychological unemployment research. They presented important design characteristics and/or the main results of the primary studies they reviewed in tabular form. These authors, however, abstained from integrating the findings from primary studies by computing an overall mean effect size. Fryer and Payne (1986) took the next step and gave an estimation of the strength of the negative effects of unemployment on mental health by computing the median of a small sample of results from selected primary studies. Their estimation of the correlation between unemployment and distress was $r = .34$ (median of five correlations).

Foster (1991) was the first researcher who used modern meta-analytic techniques in summarising and integrating research results concerning the mental health effects of

unemployment. His very interesting work, which he called an "exploratory meta-analysis" with the goal to "take a quick and dirty look at what a cross-study effect size might be" (p. 159), is not well known in the field, presumably because the author hid it in the appendix of his dissertation thesis. Foster integrated 22 effect sizes from 10 primary studies that had been recently published at the time he conducted his meta-analysis and computed an average (unweighted) effect size of $d = 0.19$. This is surprisingly small and seems to contradict the conclusions of earlier narrative reviewers, who concluded that being unemployed has a considerable negative effect on mental health. The small average correlation possibly is the result of Foster`s questionable operationalizations of unemployment and mental health (see discussion).

Murphy and Athanasou's (1999) review of the effects of unemployment on mental health was especially concerned with problems of causality. They wanted to know whether unemployment is merely correlated with distress, or whether it directly causes psychopathological symptoms and diminished well-being. The review included 16 longitudinal studies, nine of which could be used in the meta-analysis. The conclusion is in favor of direct causation of mental health problems by unemployment: "job loss provokes psychological distress, rather than the reverse" (p. 89). The authors reported effect sizes of $d = 0.36$ for status-changes from employment to unemployment and of $d = 0.54$ for changes in the opposite direction. This means that job loss was longitudinally associated with an increase in distress symptoms, whereas finding a new job was associated with a strong reduction of distress. The effects were of medium size. These longitudinal results provide support for the hypothesis that unemployment is not only correlated with distress, but actually causes distress.

In addition to computing overall effect sizes, Murphy and Athanasou (1999) conducted several moderator analyses: For nationality (Anglo-Saxon versus European), for age (young versus adult), for gender (male versus mixed gender), and for type of measurement procedure (General Health Questionnaire versus other instruments). No significant moderator effects were found. This, however, may be the result of low test power, caused by the rather small number of primary studies incorporated in the Murphy and Athanasou (1999) meta-analysis.

Recently, another meta-analysis concerning the mental health effects of unemployment was published that included 104 empirical studies that had been published in peer-reviewed journals (McKee-Ryan, Song, Wanberg, & Kinicki, 2005). This meta-analysis found a difference between employed and unemployed persons with regard to mental health of

$d = 0.52$ ($k = 60$) with unemployed persons reporting lower well-being than employed persons. With regard to life satisfaction the difference was $d = 0.44$ ($k = 7$) and with regard to marital/family satisfaction the difference was $d = 0.20$ ($k = 4$). Thus, with the exception of marital satisfaction, which is based on a rather small sample of studies, the effects were of medium size. The authors also meta-analysed longitudinal studies and reported the longitudinal detrimental effect of job loss on mental health to be $d = 0.35$ ($k = 10$) and the longitudinal positive effect of reemployment on metal health to be $d = 0.82$ ($k = 19$), while the longitudinal positive effect of reemployment on life satisfaction was surprisingly large with $d = 2.79$ ($k = 2$). A test for selection effects, comparing unemployed individuals who found reemployment later on with unemployed individuals who remained unemployed did not reveal a significant difference. With $d = 0.09$ ($k = 9$), slightly favoring the persons who became reemployed later on, the effect size was rather small.

As the focus of this meta-analysis lies on "well-being during unemployment" (p. 53) it includes a range of meta-analytic correlations - restricted to unemployed samples - of well-being with other variables of interest. This list of correlates of well-being among unemployed persons includes coping resources and coping strategies, and variables representing modes of cognitive appraisal, (e.g. internal attributions, and demographics). Moderator tests for the difference between unemployed and employed persons with regard to mental health were done for four variables: National or regional unemployment rate, level of unemployment protection, duration of unemployment, and whether the study was conducted with school leavers or adults. Unemployment rate and the level of unemployment protection did not moderate the relationship between employment status and mental health. For long term unemployed persons (>= 6 months) the association with negative well-being was significantly stronger than for short-term unemployed persons (< 6 months). Furthermore, the association was found to be significantly stronger among school leaver samples than among adult samples.[1]

[1] Note that the meta-analysis of Temple, Fillmore, Hartka, Johnstone, Leino, and Motoyoshi, (1991) is not summarized here because the operationalization of unemployment seems to be problematic in this study (no explanation of the operationalization of unemployment; synonymous use of the term "unemployment" and the terms "not employed" and "out of the labor force", questioning the homogeneity of the unemployed samples). Furthermore, this study is inclined with a very specific measure of mental health (quantity of alcohol consumption on a typical day when drinking occurs), based on only nine studies, and uses an unusual outcome measure (regression coefficients). Therefore, its relevance to the present study was judged to be low.

Thus, it can be concluded that much has been done in synthesizing the results of psychological unemployment research. However several important questions still remain. For example, only the four moderator tests in the McKee-Ryan et al. (2005) meta-analysis were done with sufficient power. Thus, important moderator such as gender, age, marital status, minority/majority status, socioeconomic status, level of economic development, etc. have not yet been adequately tested. The already existing quantitative research syntheses usually aimed at the global construct of "mental health", while only two more specific facets of psychological well-being, life satisfaction and marital satisfaction, have been meta-analyzed up to now (McKee-Ryan et al., 2005). Thus, we do not know yet how strong the effects of unemployment on other facets of metal health are, such as depression, anxiety, psychosomatic symptoms, or self esteem. Furthermore, the longitudinal analyses are incomplete up to now: Mental health changes that accompany status changes from school to unemployment or from school to employment have not been meta-analyzed. We also do not know what the baseline of mental health changes over time is, i.e. what happens among persons who are continuously employed or continuously in school. Such information might be helpful in interpreting the longitudinal results for employed persons who become unemployed or for unemployed persons who find new jobs. Meta-analyzing longitudinal data for persons who are continuously unemployed would be helpful in order to validate the findings from the cross-sectional moderator test for unemployment duration. Furthermore, two important questions concerning selection effects in the labor market still remain unanswered: Does mental health predict job loss among employed persons? Does mental health predict post-school employment status among pupils? Finally, publication bias and related threats to the validity of a meta-analysis have not yet been tested in the field of psychological unemployment research up to now. All these open questions will be treated in the present study.

## 3.3. Theories in psychological unemployment research

Several general psychological theories have been applied to the problem of unemployment, e.g. helplessness theory (Baum, Fleming, & Reddy, 1986; Frese & Mohr, 1978; Kirchler, & Kirchler, 1989; MacKay Lynd-Stevenson, 1996; Ostell, & Divers, 1987; Rodriguez, 1997; Rothwell, & Williams, 1983; Tiggemann, Winefield, Winefield, & Goldney, 1991a, 1991b; Tiggemann, Winefield, Goldney, & Winefield, 1992; Winefield, Tiggemann, & Smith, 1987), Weiner's (1985) attributional theory (Winefield, Tiggemann, Winefield, & Goldney, 1993),

expectancy-value theory (Feather, 1990; Feather & Barber, 1983; Feather & Davenport, 1981; Winefield et al., 1993), self-determination theory (Vansteenkiste, Lens, De Witte, De Witte, & Deci, 2004); Erikson's (1959) developmental theory (Gurney, 1980; Meeus, Dekovic & Iedema, 1997; Winefield, Winefield, Tiggemann, & Goldney, 1991; Winefield, et al., 1993), stress and coping models (Latack, Kinicki, & Prussia, 1995; Kinicki, Prussia, McKee-Ryan, 2000; Lena & Feldman, 1988; Leana & Feldman, 1988, 1998; Price, Van Ryn, & Vinokur, 1992), social comparison theory (Sheeran, Abrams, & Orbell, 1995), symbolic interactionism (Sheeran, & Abraham, 1994), self-discrepancy theory (Sheeran, & McCarthy, 1992), self-consistency theory (Shamir, 1986a), cognitive adaptation theory (Wanberg, 1997), stigma theory (McFayden, 1995), alienation theory (Kieselbach, 1998), object relations theory (Raber, 1996), and psychoanalysis (Rupp, 1988). However, the most influential ones were three more specific theories that were originally developed within the field of psychological unemployment research: Jahoda's latent-deprivation model, Warr's vitamin model and Fryer's agency-restriction approach. They will be shortly reviewed below.

According to Jahoda's (1981, 1982, 1997) latent deprivation model, employment has not only a manifest function (earning a living), but also latent i.e. unintended functions that are also important for mental health. She specified five latent functions of employment: "time structure, an enlarged social network, participation in collective efforts, definition of social identity, and required regular activity" (Jahoda, 1997, p. 318). Jahoda (1983) stated that these latent functions correspond to "deep seated needs" (p. 298). Therefore, the amount of access to the latent functions should have a direct impact upon a person's mental health in her model. In modern societies, employment is the only institution that can provide the latent functions in a sufficient amount. Other institutions, such as organized religion or voluntary associations, cannot serve as substitutes for employment to a satisfying degree (Jahoda, 1988). Therefore, according to this model, employment usually is necessary in order to be psychologically healthy, while unemployed persons are at risk of experiencing distress symptoms and a loss of well-being.

Warr's (1987) vitamin model is similar to Jahoda's (1981, 1982, 1997) deprivation theory in assuming the environment to be the main determinant of a person's mental health. Both authors hypothesize that certain characteristics of the environment predict well-being, although the environmental features specified by Warr (1987) are not identical with those specified by Jahoda (1981, 1982, 1997): Opportunity for control, opportunity for skill use, externally generated goals, variety, environmental clarity, availability of money, physical security, opportunity for interpersonal contact, and valued social position. Warr (1987)

hypothesized that the environment influences mental health "in a manner analogous to the effect of vitamins on physical health" (Warr, 1987, p. 9). Thus, low levels of the nine environmental features are assumed to have negative effects on mental health, while increasing levels are assumed to have positive effects on mental health. However, an increase beyond a certain limit should not result in further improvements. For some of the environmental features (e.g. physical security), very high levels are hypothesized to have no additional impact at all. Other environmental features are even thought to be harmful in very high doses (e.g. variety). Unemployed persons are typically confronted with an environment that contains only limited amounts of each of Warr's (1987) "vitamins". According to this model this group of persons should thus be characterized by mental health problems.

Fryer (1986) criticized Jahoda's (1981, 1982, 1997) deprivation model for pragmatic, methodological, and empirical reasons, as well as for the view of human nature that is implied in the model. According to Fryer, situation-centred theories such as Jahoda's or Warr's are based on a view of the person as a passive, reactive, dependent, and mainly extrinsically motivated being. In contrast to this, Fryer (1986, 1997a) assumes humans to be "agents actively striving for purposeful self-determination, attempting to make sense of, initiate, influence, and cope with events in line with personal values, goals, and expectations of the future" (1997a, p. 12). However, unemployment severely restricts and frustrates agency as well as undermines planning and purposeful action because it is usually associated with poverty, future insecurity, and low social power. In other words: "agency theory tries to focus upon what people bring with them to a situation which is unfamiliar and problematical rather then upon what is taken away from them [by the loss of employment]" (1986, p. 16). We can conclude that in Fyer's (1986) agency restriction theory human beings are assumed to feel a "desire for self-directedness" (p. 16), which is frustrated by unemployment and the poverty that is often associated with unemployment, resulting in distress and low well-being.

We can conclude that most general theories that have been applied to the problem of unemployment or have been developed originally within the field of psychological unemployment research agree that unemployment is associated with distress and diminished well-being. These theories also predict that unemployment not only correlates with, but actually causes distress (see chapter 3.6). Therefore, in accordance with previous results and with theoretical considerations, I proposed the following hypothesis:

*Hypothesis 1:* The difference between unemployed and employed individuals with regard to mental health is significantly different from zero and of medium size.

## 3.4. Types of distress that are particularly closely related to unemployment

A large range of symptoms of distress and clusters of such symptoms have been proposed as consequences of unemployment, e.g. depression, hopelessness, apathy, anxiety, psychosomatic symptoms, low self esteem, low life satisfaction, negative mood, alcoholism, and (para)suicide. Yet, unemployment might not affect all aspects of mental health in a similar way. Its negative consequences might be restricted to a typical group of symptoms. Depression, for example, has been assumed to be particularly prominent as a sign of unemployment distress (e.g. Frese & Mohr, 1978). Low self-esteem is also often discussed as very prevalent within this group. As an explanation for the preponderance of these symptom types, the learned helplessness theory that was developed by Seligman (1975) and Abramson, Seligman and Teasdale (1978), has been cited. According to this line of thinking, repeatedly frustrated attempts to find a new job, together with powerlessness encountered due to economic deprivation are experienced as a loss of control by the unemployed and create a state of learned helplessness. This state of learned helplessness typically is associated with feelings of depression and low self-esteem (Frese & Mohr, 1978; Rodriguez, 1997). With regard to some other forms of distress, researchers have expressed doubts whether they are important correlates of unemployment, e.g. psychosomatic symptoms (Frese & Mohr, 1978) or alcoholism (Winefield, 1995). In sum, depression and low self esteem might represent the most prominent signs of distress among unemployed persons, while other types of distress symptoms can be expected to be less important.

*Hypothesis 2:* Depression and self esteem show larger effect sizes than the other indicators of mental health.

## 3.5. Moderator variables

The search for moderators of the negative mental health effects of unemployment is important for two reasons: Significant moderator effects help to identify the most distressed groups of persons who are in need for special help. Significant moderator effects also help to identify groups of persons who do not suffer when unemployed or suffer less than others do. Studying the living conditions and coping mechanisms of such resilient people could help to develop successful interventions against unemployment distress.

Several moderator variables have been examined in psychological unemployment research, e.g. gender (e.g. Artazcoz, Benach, Borrell, & Cortes, 2004; Fielden & Davidson, 2001; Harris, Heller, & Braddock, 1988; Muller, Hicks, & Winocur, 1993; Snyder & Nowak, 1984, Taylor, 1988), age (Hepworth, 1980; Jackson & Warr, 1984), socio-economic status (e.g. Hepworth, 1980; Turner, 1995), cultural or ethnic minority/majority status, unemployment duration (e.g. Frese, 1987; Hepworth, 1980; Jackson & Warr, 1984; Warr; Jackson, & Banks, 1982), reasons for job loss (Hepworth, 1980; Miller, & Hoppe, 1994; Winefield, Tiggemann, & Winefield, 1992a), social support (e.g. Atkinson, Liem, & Liem, 1986; Gore, 1978; Kessler, Turner, & House, 1988; Mallinckrodt & Bennett, 1992; Sarboe & Iversen, 1989, Schwarzer, Hahn, & Jerusalem, 1993; Shams, 1993), employment commitment (e.g. Jackson, Stafford, Banks, & Warr, 1983; Stafford, Jackson, & Banks, 1980), stability of career line (Kokko, & Pulkkinen, 1998); coping (e.g. Kessler, Turner, & House, 1988; Waters, & Moore, 2001); personality factors such as self esteem (e.g. Jex, Cvetanovski, & Allen, 1994; Kessler, Turner, & House, 1988; Shamir, 1986a), and optimism (Lai & Wong, 1998), time use and level of activity (Haworth & Evans, 1987; Hepworth, 1980, Lennings, 1993; Shamir, 1986b; Winefield, Tiggemann, & Winefield, 1992b), time structure (Martella, & Maass, 2000), religiosity (e.g. Shams & Jackson, 1993), local unemployment rate (e.g. Jackson & Warr, 1987; Turner, 1995), rural vs. urban community (Harding & Sewel, 1992; Leeflang, Klein-Hesselink, & Spruit, 1992), and collectivistic vs. individualistic culture (Martella, & Maass, 2000).

Within the present meta-analysis, the focus will be laid upon demographic variables as moderators and on economic and cultural variables on country level. The reason for this focus is the fact that meta-analytic moderator tests are restricted to the analysis of variation on the between-study level. This is a strength as well as a weakness because some variables are hard to test within a single primary study (e.g. country differences), but are easy to test meta-analytically, while for other variables usable information on study-level typically is not available, precluding meta-analytic tests (e.g. personality characteristics). The demographic variables were selected with regard to their importance and the favorability of the data situation (i.e. whether there exist enough studies that report necessary information). Therefore, the following demographic variables will be examined as moderators in the present study: Gender, age, marital status, socio-economic status, and majority/minority status. In addition, unemployment duration and year of publication will be analyzed, the first because long-term unemployment has been hypothesized to have particular deleterious effect on mental health and the second because I was interested in the question of whether there exists

something like a secular trend with regard to the psychological reactions to unemployment and a process of cultural adaptation (Schaufeli, 1997).

On country level, the following variables will be examined as moderators: Level of economic development, level of income inequality, level of unemployment protection, labour market opportunities, and collectivistic vs. individualistic culture. The situation on the labour market might induce hope or despair in persons who look for a job and therefore influence unemployed persons' mental health. A country's economic development and income inequality are important variables because they are likely to influence every unemployed individual's financial situation, which is probably an important mediator of unemployment distress. The same is true for the level of unemployment protection that is provided in a country. Collectivism/ individualism will be analysed because it is the only variable related to culture that has yet been hypothesized to moderate the mental health effects of unemployment. More detailed descriptions of the hypothesized moderator effects will follow in the next sections.

In order to test the validity of the meta-analytic findings, a third group of variables will also be tested here, i.e. design characteristics that might have influenced the outcomes of the primary studies. An introduction to this topic will conclude the present chapter.

### 3.5.1. Gender

Traditionally, psychological unemployment research was primarily concerned with men, as men were assumed to suffer more from unemployment than women. An unemployed man should experience stronger distress according to this point of view "because in his own eyes he fails to fulfil what is the central duty of his life, the very touchstone of his manhood – the role of family provider" (Komarovsky, 1940, p. 74). In other words: Masculine identity is intricately linked to having a job in Western societies and is severely threatened by unemployment (McFayden, 1995). For women, on the other hand, work is seen as only one of several possible roles. The roles of wife and mother are assumed to be at least as important as work in women's lives. According to Shamir (1985), this opinion can be broken down into two main arguments: First, men are assumed to have a higher commitment to the work role than women, resulting in stronger distress when deprived of this role. Second, women are assumed to have an alternative role that can serve as a substitute to employment. Another argument is that unemployed married or cohabiting women can expect more financial support from their husbands than unemployed men can expect from their wives or partners, as men

still earn more money than women do (Leana & Feldman, 1991; Shamir, 1985). Thus, the distressing effects of unemployment-related financial deprivation should be stronger for males than for females. This argument can be extended to other aspects of the job: On average, men still have more attractive jobs than women, e.g. with regard to hierarchical position, decision latitude, social exchange etc., resulting in a more severe loss when the job is lost (Mohr, 1993). Another argument is that stigmatization might be stronger against unemployed men than against unemployed women (Kulik, 2000; Shamir, 1985). Furthermore, as employed wives usually invest more time in domestic work and parenting behavior than employed husbands do, unemployment might liberate them from the stress of multiple role engagement, lessening the negative effects of unemployment (Mohr, 1993; Muller, Hicks, & Winocur, 1993; Warr & Parry, 1982).

However, the assumption that men suffer more from unemployment than women is not undisputed. Perrucci, Perrucci and Targ (1997), for example, state that females often experience discrimination in the labor market, reducing their chances of reemployment and increasing unemployment duration (see also Fielden & Davidson, 2001). Women are also likely to become reemployed in less attractive jobs than men. It seems reasonable to assume that such gloomy prospects will reduce psychological well-being among unemployed females in comparison to unemployed males. Furthermore, recent decades have seen far-reaching changes in women's work patterns and work orientations, which may affect their reactions to unemployment. Kulik (2000) refers to "the massive entry of women into the labor market" in this context. This author also recognizes "changes in women's general life and career orientation which have brought them closer to men" (p. 488). Thus, there may be only weak differences between women and men today with regard to their commitment to the work role and their reaction to unemployment (Ensminger & Celentano, 1990). In addition, the status of the role of housewife and mother might be lower than it was in the past.

Yet, although there have without doubt been changes with regard to the social roles of women, traditional value sytems are still rather strong in many western societies and men still have more attractive jobs, probably increasing the psychological distance to unemployment. Thus, although possibly weakened, there still should be a moderator effect of gender. I proposed the following:

*Hypothesis 3a:* Gender moderates the negative effect of unemployment on mental health. The effect will be stronger for males than for females.

### 3.5.2. Socioeconomic status

„The higher the climb the harder the fall“ (Bakke, 1940, p. 232). This sentence is cited very often in psychological unemployment research when the moderating effects of socioeconomic status are discussed. According to this point of view, higher-status individuals can be expected to suffer more from unemployment than lower-status individuals because they usually lose more attractive jobs with regard to income, reputation, physical conditions, promotion opportunities, task content, intrinsic motivational potential, etc. than lower status individuals do (Eisenberg & Lazarsfeld, 1938; Kaufman, 1982; Payne, Warr, & Hartley, 1984). For high-status individuals the job may also be a very important means for identity stabilization: “work is generally much more central to the identity of professionals than it is to those in other occupations. Consequently, job loss is probably a greater blow to the ego and self-esteem of professionals than of other workers, and the former would be expected to experience more severe psychological stress” (Kaufman, 1982, p. 25). Furthermore, it has been suggested that persons with higher socioeconomic status experience greater stigmatization than persons with lower status, because white-collar or professional unemployment is rare and harder to justify than blue-collar unemployment (McFayden, 1995).

On the other hand, it has been argued that high-status persons usually have access to better financial and social resources and may possess better coping strategies than low status persons (Kulik, 2000; Little, 1976; Payne, Warr, & Hartley, 1984; Schaufeli & van Yperen, 1992). Furthermore, the loss of status and identity should be less severe among well-educated persons because the educational attainment itself may provide a certain amount of status and identity, buffering the negative effects of unemployment. In addition, unemployment rates are usually higher among low status groups. This means that members of this group know that it will be difficult to find a new job, a knowledge that may have negative effects upon their mood. Furthermore, long-term effects of former unemployment spells may weaken resistance capacities.

Financial deprivation has repeatedly been shown to be an important mediator of the negative mental health effect of unemployment. (Feather, 1997). Indeed, high-status unemployed persons may get access to some of the latent functions of employment with the help of their better financial resources. Social contact for example is likely to be easier to find when one can afford to invite other people to a drink. Thus, I think that the financial resources-argument

is of particular importance here and that it is unlikely that high status-persons will experience more distress than low status-persons. I hypothesized the following:

*Hypothesis 3b*: Socioeconomic status moderates the negative effect of unemployment on mental health. The effect will be stronger for persons with lower socioeconomic status than for persons with higher socioeconomic status.

### 3.5.3. Majority / minority status

The question whether the effects of unemployment differ for members of ethnic or cultural minority groups as opposed to majority groups has not received much attention in psychological unemployment research as of yet. However, high unemployment rates among minorities imply that minority members often experience several periods of joblessness throughout their lives. This may result in a weakening of a person's resistance capacity against unemployment. Furthermore, minority groups often are economically disadvantaged and suffer discrimination, which might result in an accumulation of stress factors among them (Jahoda, 1982; Shams & Jackson, 1994). And when jobs are rare, as is typically the case among disadvantaged minority groups, the low probability of getting new employment should result in more distress than when reemployment is easy to obtain (Broman, Hamilton, Hoffman, & Mavaddat, 1995).

However, minority members who experience several periods of unemployment throughout life may develop a tolerance for it (Dressler, 1986). High subgroup unemployment rates may also reduce the sense of stigma that is associated with unemployment (Jones-Webb & Snowden, 1993). One reason for this effect may be that members of minority groups often prefer to compare themselves with other minority members, a comparison that will often lead to the conclusion that it is quite normal to be unemployed, as so many other members of the same group are also without a job. It has also been suggested that job loss may have comparatively weak negative effects among minority members because a strong 'blame the system' ideology is prevalent among them and helps them to cope with this live event and to maintain a positive self-concept despite failure (Neighbors, Jackson, Bowman, & Gurin, 1983).

The situation is further complicated by the fact that the (sub)culture of minority groups often is quite different to the culture of the majority group. This implies that the amount and distribution of stressors, resources, and coping mechanisms may differ considerably between both groups. Religiosity, for example, has been shown to buffer the impact of unemployment

and is likely to be more prevalent among some (but not all) minority groups than among members of the majority group in Western countries (Dressler, 1986; Shams, & Jackson, 1993). In sum, there are good arguments in favor of both assumptions, that unemployment has particularly strong as well as particularly weak effects on the mental health of minority members in comparison to majority members. I therefore abstain from proposing a specific hypothesis concerning the moderating effect of minority/majority status.

### 3.5.4. Marital / relationship status

Marital status has not received much attention as a moderator of the unemployment-distress relationship. As this variable indirectly indicates several aspects of social structure and social relations, it is not easy to estimate its relevance as a moderator variable (Lahelma, 1989). Furthermore, family relations and marital adjustment itself are influenced by unemployment, complicating the role of marital status with regard to the mental health effects of unemployment (Larson, 1984; Liem & Liem, 1988).

One argument is that married persons often are financially responsible for other family members, especially children, possibly enhancing the negative psychological effects of unemployment (Fielden & Davidson, 2001). On the other hand, incomes of spouses may alleviate the impact of this responsibility (Lahelma, 1989). Furthermore, social support was repeatedly shown to buffer the negative effects of unemployment (e.g. Atkinson, Liem, & Liem, 1986; Bolton, & Oatley, 1987; Gore, 1978). Thus, insofar as being married is associated with social support, it could be hypothesized that married individuals are less distressed by unemployment than single persons are (Leana & Feldman, 1991). However, processes of social undermining, i.e. negative social support in intimate relationships, have also been reported (Vinokur & van Ryn, 1993) and may minimize or even eliminate the positive effects of social support. As no theoretical argument is truly convincing with regard to marital/relationship status, no explicit hypotheses were formulated for the respective moderator effect.

### 3.5.5 Age

It is often assumed that persons of middle age suffer most from unemployment, while younger and older persons are less severely affected (e.g. Broomhall & Winefield, 1990; Eisenberg & Lazarsfeld, 1938; Fryer, 1997b; Hepworth, 1980; Winefield, 1995). Middle-aged

persons often have family responsibilities, increasing the financial importance of a job, while younger and older persons usually have to cope with fewer financial pressures (Jackson & Warr, 1984). Furthermore, persons of middle age are likely to display a strong career commitment, possibly making employment more important for their well-being than it is for older workers who are close to the end of their careers, and for younger persons who are not yet completely integrated in the world of work and employment (Lahelma, 1989). Furthermore, while persons of middle age have little access to alternative identity constructions beyond the work role, young persons may be able to find alternative identities in subcultures and older unemployed persons can categorize themselves not as "unemployed", but as "early retired", a process that is likely to buffer the negative mental health effects of unemployment (McFayden, 1995).

However, there are also arguments against the hypothesis that younger and older unemployed workers suffer less than middle-aged unemployed workers. The physical, social and emotional problems that are part of the maturation process may compound with the stress of unemployment among youths, making the experience of unemployment more detrimental for them than for adults (Gurney, 1980). Another argument is that "adolescence is an important time for establishing independence and consolidating a sense of personal identity, and one way of achieving this is through employment" (Tiggemann & Winefield, 1984, p. 34). The first job has been called "the capitalist equivalent of initiation rites in primitive society" (Windschuttle, 1979, p. 65, as cited in Tiggemann & Winefield, 1984, p. 34). Thus, unemployment may act as an obstacle to a healthy personality development among youths and possibly has especially negative effects in this age group. Furthermore, older unemployed workers usually have greater difficulties in obtaining employment than middle-aged workers (Rife & Belcher, 1994). Unemployment rates for young workers were also comparatively high in many countries during the last decades (Fryer, 1997b). The knowledge of being a member of a disadvantaged group with regard to labor market opportunities may increase the negative mental health effects of unemployment.

However, the assumption that unemployment has particularly negative effects among middle-aged persons is well established in the research field and the arguments referring to high career commitment and strong financial responsibilities among middle-aged persons are indeed convincing. I therefore hypothesized the following:

*Hypothesis 3c*: Age moderates the negative effect of unemployment on mental health. This moderating effect is characterized by a curvilinear association with large effect

sizes for persons of middle age and small effect sizes for persons of younger or older age.

### 3.5.6. Duration of unemployment

The cumulative stress model of unemployment distress argues for a strictly linear deterioration of mental health with increasing duration of unemployment. According to this line of thinking, unemployed workers may become more discouraged over time by continuing failures in job seeking. Furthermore, financial pressures may become greater as time passes, savings are used up, and personal or household items wear out or break down and require repair or replacement (Jackson & Warr, 1984; Warr, Jackson, & Banks, 1982). However, a range of more complex stage models was also formulated to describe the psychological deterioration process that is associated with unemployment. All of these models assume that job loss is followed by a specific sequence of reactions that should be identical for all unemployed persons. Eisenberg and Lazarsfeld (1938), for example, proposed a three-phasic model: "First there is shock, which is followed by an active hunt for a job, during which the individual is still optimistic and unresigned; he still maintains an unbroken attitude. Second, when all efforts fail, the individual becomes pessimistic, anxious, and suffers active distress; this is the most crucial state of all. And third, the individual becomes fatalistic and adapts himself to his new state but with a narrower scope. He now has a broken attitude" (p. 378). Other stage models have been proposed that differ from Eisenberg and Lazarsfeld's model in number and description of the phases (e.g. Amundson & Borgen, 1982; Dreiss, 1983; Harrison, 1976; Kaufman, 1982). Some stage models are formulated in analogy to the process of grief (Hill, 1978; Raber, 1996; see also Archer & Rhodes, 1987, 1993, for a explicitly "non-stage" grief model of job loss), and one stage model was even formulated in analogy to the process of dying (Winegardner, Simonetti, & Nykodym, 1984).

Although the proposed stage models differ from each other, most of them share the assumption that in the first weeks after job loss the mental health of unemployed persons is comparatively good, followed by a severe deterioration in the months that follow. After this crisis (and possibly other intermediate phases), a final stage of adaptation and stabilization at a low level of mental health is usually proposed. Warr and Jackson (1987) assume that this adaptation is the result of either the development of interests and activities outside the labor market that act as substitutes for paid employment and are associated with increased levels of aspirations, autonomy, and sense of competence (constructive adaptation), or is the result of a

simple reduction in employment commitment that is not associated with aspirations, autonomy, and sense of competence (resigned adaptation). However, in both cases adaptation takes place. In sum, according to the stage model approach, a curvilinear association between length of unemployment and mental health exists with good mental health in the beginning, followed by severe deterioration when unemployment lasts longer than a few months, and finally some recovering to low but stable levels of mental health among very long term unemployed people. Therefore, I hypothesized the following:

*Hypothesis 3d:* Duration of unemployment moderates the negative effect of unemployment on mental health. Stronger effects are expected for long term unemployed people in comparison to short-term unemployed persons. In addition, a curvilinear association is expected with stabilizing or slightly decreasing levels of distress among very long term unemployed persons.

### 3.5.7. Year of data collection

The first two decades after the Second World War were characterized by comparatively low unemployment rates in most Western countries. During this time an expectation of permanent full employment was prevalent, and many scientists and politicians, as well as ordinary people assumed that unemployment was a problem of the past (Garraty, 1978; Jahoda, 1982). When unemployment came back during the 70ties, it met unprepared societies and optimistic individuals who hoped to be employed throughout their lives. When these hopes suddenly were frustrated, the distressing effects of unemployment possibly were particularly strong (Jahoda, 1982). After this period, when unemployment rates remained elevated during the 80ties, a slight cultural change has been assumed to have taken place, characterized by what has been called a "normalization" of unemployment, meaning that being unemployed became more socially acceptable and less stigmatizing (Schaufeli & van Yperen, 1992; Sheeran, Abrams, & Orbell, 1995), possibly resulting in weaker differences between unemployed and employed persons in more recent compared to older studies. Thus, I hypothesized the following:

*Hypothesis 3e:* Year of data collection moderates the negative effect of unemployment on mental health. Effect sizes from older studies are expected to be larger than effect sizes from more recent studies.

### 3.5.8. Economic development

The availability of money plays an important role in Jahoda's and Warr's theoretical explanations of unemployment distress and is the central determinant of distress in Fryer's agency restriction model (Fryer, 1986, 1997a; Jahoda, 1981, 1982, 1997; Warr, 1987). The Marienthal-study draws a very clear picture of the devastating psychological consequences of unemployment that is associated with severe economic deprivation (Jahoda, Lazarsfeld, & Zeisel, 1933/1975). This study, conducted at a time when unemployed people received much less financial support than is usual today in Western countries, suggests that there might exist a level of absolute poverty, characterized by starvation and a lack of clothing, that does inevitably lead to despair, apathy and other signs of severe distress (the "broken attitude" in the terminology of Jahoda et al., 1933/1975, p. 96). It seems reasonable to assume that the unemployed are a group of people who typically are more in danger of absolute poverty than other groups in most societies. And as the level of a country's economic development is negatively associated with the percentage of persons who are doomed to live under conditions of absolute poverty (United Nations Development Programme UNDP, 2003), it could be expected that unemployment in less developed countries is more harmful than unemployment in more developed countries. Based on these considerations I proposed the following:

*Hypothesis 3f:* Level of economic development moderates the negative effect of unemployment on mental health. Effect sizes from less developed countries are expected to be stronger than effect sizes from developed countries.

### 3.5.9. Income inequality

The population in countries with high levels of income inequality has been shown to be characterized by low levels of health and a shortened average life expectancy in comparison to the population of more egalitarian countries. Reductions in social cohesion have been hypothesized to mediate this effect, as social cohesion is larger in more egalitarian societies than in societies characterized by inequality (Wilkinson, 1996). As a reduction of social contacts is one of the most important mediators of unemployment distress on the individual level (Jahoda, 1982; Warr, 1987), unemployment may have more deleterious effects in less egalitarian societies, because unemployed persons may be more in danger of "drop out" and of losing basic emotional bonds to society. Furthermore, as unemployed people usually can be

expected to be at the lower end of a country's income distribution, their income loss is likely to be stronger in less egalitarian societies, were the differences between poor and rich are greater. Therefore, not only the level of a country's economic development per se might predict unemployment distress, but also the country's income distribution. Based on these considerations, I hypothesized the following:

*Hypothesis 3g:* Income inequality moderates the negative effect of unemployment on mental health. Effect sizes from countries with strong inequalities with regard to the income distribution are expected to be larger than effect sizes from countries with a more egalitarian income distribution.

### 3.5.10. Unemployment protection

Unemployment protection systems differ widely between countries with regard to the wage replacement ratio, to coverage duration, and to the percentage of unemployed persons who receive unemployment benefits (International Labour Office, 2000b). Therefore, the economic pressure unemployed people experience on average is also likely to vary between countries. As was already outlined above (section 3.3.), important theorists assume financial deprivation to be one of the most important mediators of unemployment distress (Fryer, 1986, 1997a; Jahoda, 1981, 1982, 1997; Warr, 1987). Therefore, the effect sizes for the negative mental health effects of unemployment are likely to be larger in countries with comparatively low levels of unemployment protection. Based on these considerations I proposed the following:

*Hypothesis 3h:* Level of unemployment protection moderates the negative effect of unemployment on mental health. Effect sizes from countries with medium or low levels of unemployment protection are expected to be stronger than effect sizes from countries with high levels of unemployment protection.

### 3.5.11. Labor market opportunities

Labor market opportunities, especially the local or national unemployment rate, have been hypothesized to moderate the unemployment-distress relationship. One argument is that a bad economic climate, expressing itself in a high unemployment rate, should be particularly threatening to unemployed persons because they perceive lowered opportunities for their job search and a lowered likelihood for reemployment, resulting in a depressed mood (Dooley,

Catalano, & Rook, 1988, McKee-Ryan et al., 2005). Furthermore, high national unemployment rates can be expected to be associated with a lengthening of the average unemployment spell. When long-term unemployment is more detrimental than short-term unemployment (see section 3.5.6) this effect should also contribute to increased distress levels among unemployed persons in countries with high unemployment rates. However, high unemployment rates may also have a negative impact upon the mental health of employed persons, as such high rates may induce fears of losing their own jobs and may increase average levels of job stress due to the loss in bargaining power of employed individuals and unions (Dooley, Catalano, & Rook, 1988). This would lead to lowered effect sizes in countries with high unemployment rates. Furthermore, when unemployment rates are persistently high, repeated experiences of unemployment will become widespread in a country. This will possibly lead to the development of a tolerance against the negative effects of unemployment among those who experience unemployment more than once in their lives. Furthermore, high general unemployment rates render unemployment more "normal", possibly reducing social stigmatization and reducing self-blaming attributions among the unemployed (Jackson & Warr, 1987). However, values concerning the importance of employment in one's life have been shown to be rather stable (Paul & Moser, in press). Therefore, it will take some time before something like a "normalization" of unemployment takes place. In sum, the arguments in favor of stronger negative mental health effects in more depressed labor markets are more convincing in my opinion. I proposed the following:

*Hypothesis 3i:* Labor market opportunities moderate the negative effect of unemployment on mental health. Effect sizes from countries with less favorable labor market opportunities are expected to be stronger than effect sizes from countries with more favorable labor market opportunities.

### 3.5.12. Collectivism / Individualism

Individualistic societies are societies "in which the ties between individuals are loose: Everyone is expected to look after him/herself and his/her immediate family only" (Hofstede, 2001, p. 225). In contrast to this, collectivistic societies are societies "in which people from birth onwards are integrated into strong, cohesive in-groups, which throughout people's lifetime continue to protect them in exchange for unquestioning loyalty" (Hofstede, 2001, p. 225). People in individualistic societies are more satisfied with their lives than people in collectivistic societies are (Diener, Diener, & Diener, 1995). However, it has been

hypothesized that this pattern may be reversed in moments of distress (Diener et al., 1995). Persons in individualistic societies usually feel greater personal responsibility for their successes as well as for their failures. Therefore, when they lose their job they may be more distressed than people in collectivistic societies who feel less personal responsibility and can rely on strong social networks to help them cope with unemployment (Martella & Maas, 2000). I therefore expect that collectivism acts as a buffer against the negative mental health effects of unemployment. The hypothesis was:

*Hypothesis 3j:* Individualism/collectivism moderates the negative effect of unemployment on mental health. Effect sizes from more individualistic countries are expected to be larger than effect sizes from more collectivistic countries.

## 3.6. The problem of causality

Although the elaborated psychological unemployment theories assume that unemployment causes distress (see chapter 3.3), reverse causation has been hypothesized, too. The reverse causation or selection hypothesis assumes that the association between unemployment and reduced mental health arises because persons with pre-existing mental health problems have a higher probability of losing their jobs and – when unemployed – need more time to find new employment (Frese & Mohr, 1978; Mastekaasa, 1996; Toppen, 1971; Winefield, 1995). In this line of thinking, unemployment is thus conceptualised as a *consequence* of mental health problems. How could such differential selection into and out of unemployment be possible? Mastekaasa (1996) proposed a theoretical model of labor market mechanisms that could explain such an effect. She focused on four factors: The productivity of the employee, the quality of the employer's information concerning this productivity, other forces that influence lay-off or hiring decisions, e.g. unions or the law, and job search efficiency. Mastekaasa (1996) assumes that a rational, profit-maximizing employer generally will try to hire the most productive and fire the least productive employees. To the degree that mental health influences a person's productivity level, the employer's decision may thus be influenced by the employee's psychological well-being. Mental health problems may lead to lower productivity either directly through unsatisfactory job performance or indirectly through increased number or prolonged duration of absence times. However, the employer's decision, especially with regard to lay-offs, is not always free and will be influenced by other forces, e.g. unions or legal regulations. Seniority, for example, is an alternative criterion that will

influence lay-off decisions in many countries in addition to the productivity criterion. To the degree that seniority is less influenced by mental health than productivity, selection effects of mental health will thus be limited in such situations. In contrast to lay-off decisions, in hiring decisions only two actors are usually involved, the employer and the applicant. Thus, a rational employer should select the most productive applicant in this situation without being influenced by other forces, such as unions (although in some countries unions also have the right to involve themselves in this decision, but this may be a comparatively rare situation). According to Mastekaasa (1996) one could therefore assume that health selection should be stronger for the transition from unemployment to employment than for the transition from employment to unemployment. However, the employer has an information problem with regard to hiring decisions, as he or she lacks information concerning the productivity of the unknown job applicant. This is a problem that is less likely to occur with regard to firing decisions, i.e. with well-known employees. Many employers use personnel selection techniques to reduce this information problem, but still they are very likely to be less well informed about applicants than about employees. According to Mastekaasa (1996) this information problem possibly reduces health selection for the transition from unemployment to employment. Furthermore, due to the employer's lack of information concerning the applicant's productivity, hiring decisions are likely to be influenced by the applicant's impression management, a variable that may be influenced by mental health (see also Warr, Jackson & Banks, 1982). Mental health may also directly influence hiring decisions, when the employer is able to recognize an applicant's mental health state and uses this knowledge to steer his or her decision. Psychological problems may also be expected to reduce general job search efficiency to some extent, resulting in health related selection for the transition from unemployment to employment. For example, Self-esteem, a variable closely related to mental health, has been shown to be significantly correlated with the intensity of job search behavior ($r = .25$) in the meta-analysis of Kanfer et al. (2001).

However, there is no reason to think that causation and selection are mutually exclusive processes, and it is possible that both contribute to the accumulation of mental health symptoms among the unemployed. With longitudinal studies, it is possible to shed some light on the problem of causation/selection: Support for causation of distress by unemployment is provided when status changes, e.g. transitions from employment to unemployment or from school to unemployment, are associated with changes in mental health. Support for selection effects is provided when distress is related to subsequent lay-offs among employed persons, when distress is related to subsequent reemployment among unemployed persons, or when

distress is related to job-search success among school leavers. Therefore, the following hypotheses were formulated:

*Hypothesis 4:* Changes in a person's employment status are associated with changes in mental health that are consistent with the assumption that unemployment causes distress.

*Hypothesis 4a:* Status changes into unemployment (from employment or from school) are associated with significant deteriorations in mental health.

*Hypothesis 4b:* Status changes into employment (from unemployment or from school) are associated with significant improvements in mental health.

*Hypothesis 5:* Before a status change occurs there are differences in mental health between those persons who will experience the change event and those who will not experience the change event that are consistent with the assumption that selection effects are one of the reasons for the mental health differences between employed and unemployed persons.

*Hypothesis 5a*: Employed persons who will lose their jobs in the near future show more distress than employed persons who will stay employed.

*Hypothesis 5b:* Unemployed persons who will find new jobs in the near future show less distress than unemployed persons who will stay unemployed.

*Hypothesis 5c:* Pupils (and students) who will find a job after school (or university) show less distress while in school than pupils who will become unemployed after leaving school.

## 3.7. Publication bias

Psychological unemployment research can be described as socially engaged science motivated by a wish to help. This, for example, is reflected in Feather's (1990) question "Does the research (...) help us to devise ways of helping unemployed people?" (p. 251) and in Kelvin and Jarrett's (1985) statement: "In practice, the motivation and purpose of psychological research on unemployment seems to have been to help the unemployed" (p. 3). Furthermore, there is a strong correlation between research interest and the perception of unemployment as a social problem: Peaks of published articles and books on the

psychological effects of unemployment can be observed in the 1930s, when unemployment rates in the Western World where incredibly high, and then again in the 1970s and 1980s, when, after a period of low unemployment following World War II, the rates were rising again in the Western World. This argues for the interpretation of psychological unemployment research as – at least partly – driven by altruistic motives. However, such an altruistic motivation, albeit admirable from an ethical point of view, may have introduced a problem of selective publication into the research field. In order to help, researchers (and editors) first of all have to show that the group of people they want to help really needs help. Thus, findings demonstrating that unemployed people suffer from unemployment and do not enjoy their situation possibly had a higher probability for publication than non-significant or negative results. As a consequence, the field of psychological unemployment research may be characterized by a publication bias favoring "positive" results that demonstrate that unemployed people suffer. An argument against such a publication bias might be seen in the existence of the very early and well-known review published by Eisenberg and Lazarsfeld (1938). This review possibly had a discouraging effect on the publication of results that "only" replicated what had already been outlined in that review, i.e. that unemployment has a negative effect on psychological health. The large number of more recent reviews possibly ameliorated this discouraging effect. However, most review authors called for further research concerning the mental health effects of unemployment. Eisenberg and Lazarsfeld (1938), for example, finished their paper with a statement about "our very deficient knowledge of the psychological effects of unemployment" (p.385). The reviews might thus have stimulated, not discouraged, the conceptualization and publication of new studies. In sum, it must be stated that publication bias has been shown to be common in several other fields of research and might be found in the present meta-analytical data-set, too (e.g. Begg & Berlin, 1988; Begg, 1994; Spence & Blanchard, 2001; Sutton, Duval, Tweedie, Abrams, & Jones, 2000). If this is the case, the validity of the meta-analytic findings is threatened and all further conclusions must be drawn with considerable caution.

# 4. Methods

In this chapter, after outlining the main strategies used in the literature search, a description of the criteria that were used in order to decide whether a primary study was included in the meta-analysis or not will be provided. Following this is a description of the operationalizations of unemployment, employment, and mental health. Then the statistical methods are described that were used to estimate the effect sizes and to conduct the meta-analytic computations. The coding process and data retrieval, respectively, for the potential moderator variables is outlined. After this, the methods employed in the sensitivity analysis are outlined. The statistical methods for the analysis of longitudinal data are described separately as they differ from the methods for cross-sectional data with regard to several important aspects.

In the technical appendix more detailed descriptions of the methods employed here are available.

## 4.1. Literature search

Three main search strategies for locating relevant primary studies were used in the present meta-analysis:

First, I scanned the reference list of several narrative reviews for relevant studies (Frese & Mohr, 1978; Fryer & Payne, 1986; Catalano, 1991, Hammarström, 1994; Winefield, 1995; Kasl, Rodriguez, & Lasch, 1998; Hanisch, 1999; McKee-Ryan & Kinicki, 2002). Second, I made use of several computerized literature-databases. This search included PsycINFO (psychological literature, mainly in English), MedLine (medical literature, mainly in English), Sociological Abstracts (sociological literature, mainly in English), ERIC (educational literature, mainly in English), Dissertation Abstracts International (dissertations from a wide range of disciplines, mainly in English), Psyndex (psychological literature, mainly in German), WISO (sociological and economic literature, mainly in German) and Diss-CD (dissertations in German from a wide range of disciplines). According to Reed and Baxter (1994), a search in the controlled vocabulary ("Key terms") does not always lead to satisfying results. Therefore I decided to do a free-text search, using the complete information a data base contains, including headlines, abstract texts etc. (The exact search term can be found in

the technical appendix.) Third, the ancestry method was used, i.e. the reference lists of the relevant primary studies located by the first two search strategies were manually searched to yield additional studies. As an amplification to the three main search strategies, I screened the library catalogues of four German Universities (Gießen, Marburg, Frankfurt/Main, and Erlangen-Nürnberg) for relevant degree dissertations, master theses, and other unpublished material.

## 4.2. Inclusion criteria

A study was included in the meta-analysis only when it matched all of the following six inclusion criteria.

1. The publication year of the primary study is 1950 or later. This criterion was formulated as I intended to meta-analyze only the research results of the second half of the 20$^{th}$ century in order to guarantee a minimum relevance for the present time (an excellent review of the results of the first half of the 20$^{th}$ century can be found in Eisenberg & Lazarsfeld, 1938).
2. Unemployed and employed persons were compared with regard to at least one of the following indicators of mental health: Mixed symptoms of distress (e.g. GHQ), depression, anxiety, psychosomatic symptoms, well-being/life satisfaction, self esteem (see 4.3.3., "measurement of mental health").
3. The measurement of mental health was done via a standardized and objective quantitative procedure, usually a questionnaire or a structured interview. Studies using qualitative techniques (e.g. Conti, 1988), or partly qualitatively techniques such as unstructured interviews with subsequent content analysis (e.g. Ulich et al., 1985; Viney & Tych, 1984) were excluded.
4. The data presentation allowed the computation of an effect size estimation (see 4.4.1., "computation of effect sizes").
5. The sample was not drawn from a population of patients of medical institutions. This criterion was formulated (a) to avoid possible confounding effects of physical illness and (b) to ensure comparability with the general, non-institutionalized population.

6. The unemployed group was sufficiently homogenous (see 4.3.1., “measurement of unemployment”).

## 4.3. Operationalizations

### 4.3.1. Measurement of unemployment

In the psychological literature, unemployed persons are sometimes mixed with other non-employed persons who do not seek work or who are not available for work, for example homemakers, students, and retired persons. (For a discussion of the insufficient differentiation between unemployed women and homemakers, see Warr & Parry, 1982.) As a consequence, a study was only included in the meta-analysis when it matched at least one of the following categories for which I expected the number of persons fitting the standard definition of unemployment (see 2.2 “the definition of unemployment”) to be sufficiently high enough to ensure homogeneity and to justify the label “unemployed”:

(a) *Officially registered as unemployed.* It was explicitly stated in the studies that the participants are officially registered as unemployed persons at an employment office, a union, a self-help organization, or a similar institution.

(b) *Involuntarity of unemployment explicitly stated.* In studies belonging to this category it was explicitly stated in the text that the participants were involuntarily unemployed, e.g. by formulations like “involuntarily terminated from their job” (Carnes, 1985, p. 88) or “seeking employment” (e.g. Feather, & Bond, 1983, p. 244). Studies with more indirect but still unequivocal formulations like "workers who still belonged to the work force (employed or unemployed)" (Liira, & Leino-Arjas, 1999, p. 43) were also included in this category.

(c) *Sufficient differentiation from other groups of non-employed persons.* The sample of unemployed persons was clearly differentiated from other groups of non-employed persons (students, homemakers, retirees), either by completely excluding such persons from the study or by reporting the results for these groups separately.

(d) *Lost job within last three years and still out of work (plant closing studies).* It was explicitly stated in the text that the unemployed persons lost a job within the last three years before examination and were currently out of work. This category was designed mainly to include the so-called factory- or plant-closing studies, where it was often only stated that the examined persons lost theirs jobs due to a mass layoff and are still without

a job some time later. This may include a few cases of early retirement, resumption of education, or switching to the role of a housewife, but I assume that most persons in such a situation are involuntarily out of work. Therefore, these studies were also included in the meta-analysis.

For a more detailed description of the categories and for a description of some special cases with regard to the measurement of unemployment, see the technical appendix.

### 4.3.2. Measurement of employment

Generally, I accepted all studies that used a comparison group labelled as "employed" or "working" or "with a job" or used a similar term. Studies using apprentices or re-employed, i.e. formerly unemployed persons, as a comparison group were also included. When data for more than one employed group were reported and a choice between different alternatives with regard to the comparison group was possible, I used the group that was most prototypical for the institution of employment in industrialized and post- industrialized societies, i.e. full-time dependent non-temporary employment.

### 4.3.3. Measurement of mental health

Mental health is a very broad concept and difficult to define (Jahoda, 1988). In addition, concerning the operationalization of the dependent variable, a meta-analyst always depends upon the decisions of the primary authors whose studies he or she reviews. Therefore, I decided to use a data-driven, bottom-up approach. I first acquainted myself with the research field by reading several reviews and important primary studies. Then I selected six variables as indicator variables for mental health that were used very consistently within the research field and that seemed unequivocal to me with regard to their relevance to mental health. These six indicator variables were: Mixed symptoms of distress, depression, anxiety, psychosomatic symptoms, subjective well-being, and self-esteem. Only studies that measured one of these variables were used in the meta-analysis. In order to test whether these variables really are indicators of a common higher-order factor of mental health, the intercorrelations of the indicators were coded and meta-analysed (see results section).

In order to decide whether a scale used in a primary study represented an appropriate measure of one of the six indicator variables of mental health, I used the following information: (a) the name of the scale (e.g. "Beck's Depression Inventory"), (b) the content description given by

the authors, (c) sample items reported in the primary study, and, if necessary, (d) the original scale items. Usually the decision whether a measure was an adequate operationalization of one of the six indicator variables of mental health was straightforward. In a few cases, however, the names of self-constructed measures were misleading (e.g. Kopasci, 1990). Such problems usually could be solved by inspection of the scale items (for more details see technical appendix). Table 1 gives a summary of the most frequent measures for each of the six indicators of mental health. It can be seen that in two cases a single scale dominates the measurement of an indicator of mental health: The General Health Questionnaire (GHQ, Goldberg & Hillier, 1979) was used in two-thirds of all cases as measure of global distress. When self-esteem was measured, in nearly half of the cases this was done with Rosenberg's scale (Rosenberg, 1965). To measure subjective well-being, in 40% of all cases a single-item measure was used. Usually, this item asked for global life satisfaction or happiness. No dominance of a single measurement method was evident for depression, anxiety, and psychosomatic symptoms. Overall, 7 – 25% of the measures were ad-hoc scales constructed by the authors of the primary studies.

Table 1: **Frequency of mental-health measures**

| *Mental health indicator* | *Measurement instrument* | *Frequency* |
|---|---|---|
| Mixed symptoms of distress | GHQ or derivates (Goldberg & Hillier, 1979) | 66% |
| | SCL-90 or derivates (Derogatis, 1977) | 6% |
| | CIS or derivates (Goldberg et al., 1970) | 3% |
| | Other established measures | 17% |
| | Scale constructed by authors of primary study | 8% |
| | Single item | 1% |
| Depression | BDI or derivates (Beck et al., 1961) | 18% |
| | CES-D-scale or derivates (Radloff, 1977) | 16% |
| | SCL-90 or derivates (Derogatis, 1977) | 10% |
| | Zung-scale or derivates (Zung, 1965) | 8% |
| | Zerssen-scale or derivates (Zerssen, 1976) | 6% |
| | GHQ-subscale | 6% |
| | FPI or derivates (Fahrenberg, & Selg, 1970) | 5% |
| | Other established measures | 21% |
| | Scale constructed by authors of primary study | 8% |
| | Single item | 1% |
| Anxiety | SCL-90 or derivates (Derogatis, 1977) | 21% |
| | GHQ-subscale | 15% |

| *Mental health indicator* | *Measurement instrument* | *Frequency* |
|---|---|---|
| | Zung-scale or derivates (Zung, 1971) | 13% |
| | STAI (Spielberger, 1983) | 8% |
| | Other established measures | 27% |
| | Scale constructed by authors of primary study | 15% |
| | Single item | 2% |
| Psychosomatic symptoms | SCL-90 or derivates (Derogatis, 1977) | 22% |
| | FPI or derivates (Fahrenberg, & Selg, 1970) | 20% |
| | GHQ-subscale | 2% |
| | Other established measures | 29% |
| | Scale self-constructed by study-authors | 27% |
| Subjective well-being | Diener life satisfaction-scale (Diener et al., 1985) | 10% |
| | Bradburn affect-balance scale (Bradburn, 1969) | 6% |
| | Warr life satisfaction-scale (Warr, Cook, & Wall, 1979) | 4% |
| | Other established measures | 17% |
| | Scale constructed by authors of primary study | 25% |
| | Single item | 38% |
| Self-esteem | Rosenberg-scale or derivates (Rosenberg, 1965) | 48% |
| | GHQ-subscale | 3% |
| | Osgood semantic differential (Osgood, Suci, & Tannenbaum, 1957) | 5% |
| | Tennesse-scale or derivates (Fitts, 1965) | 2% |
| | Offer-scale or derivates (Offer, Ostrov, & Howard, 1982) | 2% |
| | Other established measures | 15% |
| | Scale constructed by authors of primary study | 21% |
| | Single item | 5% |

## 4.4. Statistical computations for cross-sectional data

### 4.4.1. Computation of effect sizes

I used the standardized mean difference *d* as measure of effect size. This is the difference between two groups, in the present case employed and unemployed persons, divided by an estimate of the within-group standard deviation. When means and standard deviations were not available, *d* was estimated from correlations, *t*-values, *F*-values, sum of squares, proportions, odds ratios, exact p-values, level-*p*-values, verbal statements about significance, and, in one case, from a statement about the percentage of explained variance. In three cases, results were directly reported in standard score units. Transformation formulas were taken

from Glass, McGaw and Smith (1981), Hedges and Olkin (1985), Morris and DeShon (1997), Rosenthal (1994), and Sanchez-Meca, Marin-Martinez and Chacon-Moscoso (2003). (For a detailed description of the estimation procedures see technical appendix.) Effect sizes were usually computed on study-level. The level of subgroups was chosen when data were reported separately for such subgroups that were relevant for the intended moderator analyses (e.g. separately for males and females).

### 4.4.2. Computation of overall effect sizes

To get a single overall effect size for each sample, it was necessary to combine the effect sizes of two or more indicators of mental health whenever researchers had examined more than one of these indicators, e.g. depression in addition to mixed symptoms. A formula provided by Marin-Martinez and Sanchez-Meca (1999, p. 34), based on a method reported in Rosenthal and Rubin (1986) was used to compute these combined, or "composite" effect sizes. This method requires the knowledge of the mean intercorrelation of the variables included in the composite. To estimate this mean intercorrelation, I coded all intercorrelations of two indicators of mental health that were reported in the primary studies used here (see appendix C, table C-2). Then I ran a series of small meta-analyses on these intercorrelations, one for each possible combination of two indicators of mental health. The resulting average intercorrelations ranged from $r = 0.36$ to $r = 0.67$ with a median of $r = 0.48$ (see chapter 5.2.). This value, $r = 0.48$, was then used to compute the composite effect size. (For more details concerning the computation of the composite effect sizes see technical appendix).

Finally, a correction for unreliability (artifact-distribution method) was applied with the methods described by Hunter and Schmidt (1990), using the alpha-coefficients for internal consistency that were reported in the primary studies.

### 4.4.3. Meta-analytic model

I used a random effects model in order to combine the effect sizes meta-analytically. This was done because I was interested in results that could be generalized to other studies that were not included in the meta-analytic data set. In other words, I was interested in results that could be generalized to the whole field of psychological unemployment research (Hedges & Vevea, 1998). As random effects models have lower power than fixed effects models (Cohn &

Becker, 2003) I will interpret not only significant results but also findings that are only marginally significant ($p < .10$).

The meta-analytic computations were done with SPSS, using syntaxes provided in Lipsey and Wilson (2001). To estimate the sampling variances of the effect sizes, formulas provided in Hedges and Oklin (1985) and Sánchez-Meca, Marín-Martínez and Chacón-Moscoso (2003) were used.

### 4.4.4. Moderator analyses

To test the effect of the demographic moderator variables on the difference between unemployed and employed persons with regard to mental health, a series of weighted regression analyses, adapted for meta-analytic purposes by Lipsey and Wilson (2001), were conducted. To check the stability of the weighted regression results, I repeated the analysis after exclusion of the three studies with outlying values that were revealed in the sensitivity analysis. In this step I also controlled the influence of two design variables that were shown to affect the magnitude of the effect sizes during the sensitivity analysis. These variables were language (English vs. German) and questioning format (oral vs. written examination). Furthermore, I also controlled whether the composite effect sizes included a measure of psychosomatic symptoms in this second step, as psychosomatic symptoms had a considerably weaker effect size than the other indicators of mental health, possibly confounding the results (see chapter 5.3.1.). To test the possible curvilinear relationships of unemployment distress with age and unemployment duration, I ran separate analyses including power polynomials in the regression equations (Cohen, Cohen, West, & Aiken, 2003).

On country level, five moderators were tested: economic development, inequality, level of unemployment protection, labor market favorability, and individualism/collectivism. With one exception, each of these moderators was measured by two indicator variables (a description can be found in chapter 4.4.6.).

To conduct the moderator tests, the countries were dichotomized corresponding to their values on each of these indicator variables (median split). The dichotomization was done as follows: First, I assigned the appropriate indicator values to each sample. Some of the variables were constant over time, e.g. the indicators for individualism/collectivism, while other variables changed within a country over time, e.g. the unemployment rate. When the variable changed over time I always coded the value that was closest to the time of data collection. Then I computed the mean value of each indicator variable within each country,

e.g. the mean of all unemployment percentages for the United Kingdom. Next, the median of all country means for each indicator was computed and used to conduct a median split, producing two groups of countries for each variable, one with countries high and one with countries low on the respective variable (e.g. countries that had high vs. low unemployment rates during the time the primary studies were conducted). For unemployment protection the procedure was more simple as the categorization of the International Labour Office (2000b) could be used directly, resulting in one group of countries with high-level unemployment protection and one group of countries with medium- or low-level unemployment protection. (A list of all groupings of countries can be found in appendix C, list C-1.) Finally, I compared the mean effect sizes for the two groups of countries per variable with a subgroup moderator test, using the appropriate SPSS-syntaxes provided in Lipsey and Wilson (2001).

### 4.4.5. Meta-analytic methods for case rates

As effect size statistics such as *d* are rather abstract values, I also meta-analysed the proportions of "cases" of mental illness, i.e. the proportions of persons with a test value above a certain threshold that is regarded to be indicative of psychological problems of clinicaly significant intensity. Only established clinical screening tests for mixed symptoms, depression or anxiety, were used in this analysis. The logit transformation was applied in these meta-analyses of proportions. This method is recommended when the observed proportions are less than 0.2, which was often the case in the present data set (Lipsey & Wilson, 2001). The proportions were meta-analytically integrated and a weighted average proportion was computed with a random effects model using the SPSS-syntaxes provided in Lipsey and Wilson (2001).

### 4.4.6. Coding and measurement of moderator variables

Gender was coded as percentage of female persons in a sample. Age was coded as the average age of the sample in years. Minority / majority status was coded as the percentage of members of ethnic or cultural minority groups in a sample. Marital status was coded as percentage of persons with an intimate relationship in a sample (married persons and cohabiting persons). Socioecenomnic status was measured with two variables, i.e. occupational status and level of education. Occupational status was coded as the percentage of blue-collar workers in a sample. Level of education was coded as years spend in formal education. Unemployment

duration was coded in months. Year of data collection was directly coded, if available. Otherwise it was estimated as publication year minus four years, four years being the average difference between the year of data collection and the publication year among those studies that reported both values. Type of publication was coded with four categories: (1) peer reviewed scientific journal, (2) book or book-chapter, (3) dissertation thesis, (4) other (e.g. master thesis, unpublished research report). Language was coded as either English or German. I also coded whether unemployment was the main topic of a publication or not and whether the employed comparison group consisted of reemployed or continuously employed subjects. Part-time employment was coded as the percentage of part-time employed persons among the employed comparison group. (See the section concerning publication bias for more information about the latter variables.)

As indices of a country's economic development I used (1) the Gross Domestic Product per capita (GDP), and (2) the Human Development Index (HDI), an index that measures a country's level of development through the use of three factors: longevity (mean life expectancy in the nation), knowledge (rate of literacy and school population), and purchasing power (GDP per person) (Sotelo & Gimeno, 2003). To measure inequality, two indexes of income distribution were selected: (1) A nation's percentage of persons with income under relative, i.e. country-specific, poverty lines (usually 50% of the median income), and (2) the Gini Index, an index that measures the extent to which the distribution of income among individuals or households within a country deviates from a perfectly equal distribution (UNDP, 2003). The data concerning economic development and inequality were retrieved from the Human Development Report 2003 (UNDP, 2003), and the World Labour Report 2000 (ILO, 2000). The number of available years for which data were provided in these sources ranged from only one year for the percentage of persons under the poverty line (located in the 1980ies or the 1990ties, depending on the country of reference) to six years for the HDI index, covering the period from 1975 to 2001. In the meta-analysis I always used the measurement year that was closest to the year of data collection in the respective primary study. As indicators of labor market favorability, the Labour Market Security Index (LMSI) and the national unemployment rate were used. The LMSI is a complex composite of several single indicators of the "level of access to reasonable income-earning activities" in a country (International Labour Office, 2004, p. 131). National unemployment rates were retrived from the Organisation for Economic Co-operation and Development (OECD; 2005, 1995, 1985). As measure of the level of unemployment protection, a list provided by the International Labour Office (2000b) categorizing countries according to the generosity of their

unemployment protection system was used (high level vs medium and low level). Finally, index values adopted from Hofstede (2001) and Spector, Cooper, Sanchez, O'Driscoll, Sparks, Bernin, et al. (2001) were used as indicators of individualistic/collectivistic culture.

### 4.4.7. Checking publication bias

In order to detect the possibility of publication bias in a research field (i.e. the results of the studies influence the probability of publication), it is recommended to correlate the observed effect sizes with important features of study design that are risk factors for such a bias (Begg, 1994). The most important design feature, with regard to publication bias, is the sample size, because studies with statistically significant results usually are more likely to be published than studies with non-significant results and the probability of significant findings is associated with sample size, with large sample sizes having a higher probability of significance for a given effect size than small sample sizes. Therefore, the sample size represents a design feature that should be checked for possible problems with regard to publication bias (Begg, 1994). Indeed, sample size is the design feature that is regarded as most important in this context and some authors have even argued for completely excluding underpowered studies from meta-analyses in order to avoid bias (Kraemer, Gardner, Brooks, & Yesavage, 1998). As I took an inclusionist standpoint and tried to use all available information in the present meta-analysis, I did not exclude underpowered studies. However, I was aware of the potential threat that publication bias can pose to the validity of meta-analytic results, and therefore I intended to carefully check for this kind of bias. This was done by three methods: (1) visual inspection of the funnel plot, i.e. of a figure plotting the effect size of the primary studies against their standard errors (Wang & Bushmann, 1998), (2) the "Trim and Fill" method (Duval & Tweedie, 2000a, 2000b), a rank-based data augmentation technique, formalizing the use of funnel plots to estimate the number and adjust for the outcomes of suppressed or otherwise missing studies, (3) Begg and Mazumdar's (1994) rank correlation test, a method to detect publication bias that is based on the correlation between the (standardized) effect sizes and their variances.

Furthermore, the publication status was also checked, as unpublished studies such as master's theses or doctoral dissertations can be expected to report smaller effect sizes than published studies if a publication bias exists (Wang & Bushman, 1998). In addition, research that was conducted in non-English-speaking countries has been shown to more likely be published in the English language if the results are "good", i.e. large and in the expected direction, whereas

negative results are more likely to be published in the national language (Egger, Zellweger-Zähner, Schneider, Junker, Lengeler, & Antes, 1997). Therefore, in order to test the existence of such a language bias, the results from German-speaking countries (Austria, Germany, Switzerland) that have been published in English were compared to the results from the same countries that have been published in German. Furthermore, employment status is a variable that is often measured and reported although it is not the main theme of the respective research project. In such cases, when unemployment is only a minor topic, publication bias might be less likely than in cases where unemployment is the main topic of research. Therefore, effect sizes from both types of studies were compared. Furthermore, in order to estimate the tolerance of the present results for unpublished null or negative results, I used the fail-safe N statistic (Orwin, 1983; Rosenthal, 1995) that gives an estimation of the number of unpublished studies with null or negative results that are needed to reduce the average meta-analytic effect size to a certain limit, for example, to a null effect.[2]

Although not directly relevant to the question of publication bias, I also had a look at two other design features that may have affected the results of studies on the psychological effects of unemployment and may be important with regard to the stability and generalizability of such effects. Both of these features are related to the composition of the employed comparison group. The first feature pertains to the past labor market experiences of the comparison group: Many studies in the field use comparison groups consisting of re-employed persons with a history of former unemployment experiences. Such comparison groups are used for reasons of sampling convenience, for example in longitudinal studies, making it unnecessary to search for employed participants, but may underestimate the negative effects of unemployment whenever the re-employed persons did not fully recover from the negative mental health effects of their former unemployment experience (see Lucas, Clark, Georgellis, & Diener, 2004). Therefore, I compared studies using re-employed comparison groups with studies that used continuously employed groups to check whether there is a moderating effect of this design feature. I also checked whether the percentage of part-time employed persons in the comparison group correlates with a study's effect size. Part-time employees have been reported to differ from full-time employees with regard to

[2] Rosenthal's (1979) original fail-safe N method has been critizised for its assumption that the mean effect size of the unpublished studies is zero (Gilbody, Song, Eastwood, & Sutton, 2000; Hsu, 2002). However, Orwin's (1983) formula, which was used here, allows for computation of a fail-safe N under other assumptions, for example, when the mean effect size of the unpublished studies are negative.

their well-being (Armstrong-Stassen, Al-Ma'aitah, Cameron, & Horsburg, 1998). Therefore, there may exist a moderating effect for full- vs. part-time employed comparison groups.

### 4.4.8. Outlier analysis

I used standardized residuals to locate potential outlier studies, as recommended by Hedges and Olkin (1985, p. 253ff.). The SPSS-syntax provided in Rustenbach (2003) was used to compute the standardized residuals. Then, following a suggestion by Huffcutt and Arthur (1995) I ranked the absolute values of the residuals from the highest to the lowest, plotted them, and visually inspected them in the same manner as is done for the purpose of a scree test in exploratory factor analysis.

## 4.5. Meta-analysis of longitudinal data

Longitudinal studies were used to test the causation hypothesis, as changes in mental health that accompany changes in employment status are strong hints in favor of a causal influence of unemployment upon mental health. Therefore, weighted average repeated measures effect sizes (Gibbons, Hedecker, & Davis, 1993; Morris & DeShon, 2002) were computed for the following groups: (1) persons who changed from unemployment to employment; (2) persons who changed from employment to unemployment; (3) persons who changed from education to unemployment; (4) persons who changed from education to employment. To test whether mental health deteriorates during unemployment I also meta-analysed change effects for (5) persons who were unemployed at both measurement times of a longitudinal study. To get an impression of what the baseline of such mental health development among employed people and young persons in formal education might be, I also meta-analysed change values for (6) persons who were unemployed at both measurement times and for (7) persons who were in formal education at both measurement times.

Effect sizes were computed from means and standard deviations of change scores, repeated measures t-values, p-values for change tests (repeated measures t-test, Wilcoxon test), and verbal statements concerning the significance of change. When authors of primary studies reported only means and standard deviations of "raw scores" for both measurement times, it was possible to compute a repeated measures effect size only when the correlation between the values at T1 and T2 was available or could be estimated (Morris & DeShon, 2002,

p. 109). As this correlation usually was not reported, a series of small-scale meta-analyses was run to estimate the average T1-T2-correlation for the six groups of persons mentioned above (for the results of these meta-analyses see results section). These meta-analytically derived estimates of the T1-T2-corrrelations then were used to estimate the repeated measures effect sizes when only raw-score means and standard deviations were reported for T1 and T2. The sampling variances of the repeated measures effect sizes were estimated with a formula provided in Morris and DeShon (2002). Again, a random effects model was used to integrate the effect sizes meta-analytically (Hedges & Vevea, 1998; Lipsey & Wilson, 2001). (A detailed description of the methods used for the meta-analyses of longitudinal data can be found in the technical appendix.)

With longitudinal data it is also possible to test whether selection-effects exist. For this purpose, the following cross-sectional comparisons were conducted with regard to mental health at the first measurement point of longitudinal studies: (1) continuously employed persons compared to persons who lost their jobs between T1 and T2; (2) continuously unemployed persons compared to unemployed persons who found new employment between T1 and T2; (3) school leavers who found jobs after school compared to school leavers who did not succeed in their job hunt and were unemployed at T2. If these groups differ, it is reasonable to conclude that a person's level of mental health influences the probability of subsequent job loss and (re)employment. The effect sizes were computed and meta-analytically integrated with the usual methods for cross-sectional data (see above).

# 5. Results

The results section consists of five chapters. In the first chapter, descriptive data concerning the sample of studies included in the meta-analysis are presented. In the second sub-chapter the meta-analytically derived intercorrelations of the six indicator variables for mental health are reported. Then the results of the meta-analysis of cross-sectional data concerning the mental health effects of unemployment follow. After this, the results of the analysis of longitudinal data are presented, first the results pertaining to the question of social causation, then the results pertaining to the question of selection effects on the labor market. Finally, the chapter ends with the presentation of the results of the sensitivity analysis including the following topics: Outlier analysis, design characteristics as moderators of the mental health effect of unemployment, tests for publication bias.

## 5.1. Sample description

I identified 237 cross-sectional studies containing 323 independent samples that compared unemployed and employed persons with regard to mental health. Altogether, nearly half a million participants were examined in these studies ($N$ = 458,820). The median sample size was $Mdn$ = 219 with a range from $Min$ = 17 to $Max$ = 248,393. The studies originated from 26 predominantly Western countries, especially the USA, the UK, Germany, Australia, Finland, Canada and the Netherlands, with each contributing ten or more samples to the data set. The studies were published between 1963 and 2004. Of the total samples, 72% were part of journal publications, 15% were reported in books or book chapters, 8% in dissertation theses, and 4% came from other sources such as master theses or unpublished research reports.

## 5.2. The intercorrelations of the mental health indicators

First, to check whether the six indicator variables of mental health really can be seen as measures of the same underlying construct, i.e. mental health, I meta-analysed the intercorrelations of the six constructs as they were reported within the primary studies (for the list of intercorrelation see appendix C, table C-2). The results show that all six indicators were

substantially intercorrelated with each other, ranging from an average intercorrelation of $r$ = 0.36 for the association between mixed symptoms and psychosomatic symptoms to $r$ = 0.67 for the association between depression and anxiety (see table 2). The median of the average intercorrelations was $r$ = 0.48. An exploratory principal component analysis with varimax rotation of the intercorrelation matrix resulted in a very clear one-factor solution, with the first factor explaining 89.91% of the variance of the intercorrelations (see appendix B, figure B-1, table B-1). All six variables loaded with .90 or higher on the first unrotated component. Therefore, it can be concluded that the six indicator variables assembled here indeed measure a common underlying construct that could be called "mental health".

Table 2: **Meta-analyses of the intercorrelations of six indicators of mental health**

| *Mental health variables* | *k* | *n* | *r* | *SEr* | *95% CI* | *p* | *Q* | *H* |
|---|---|---|---|---|---|---|---|---|
| ms * depression | 5 | 1341 | 0.54 | 0.081 | 0.42 - 0.64 | 0.0000 | 25.05*** | 2.50 |
| ms * anxiety | 4 | 965 | 0.37 | 0.180 | 0.03 - 0.63 | 0.0348 | 49.59*** | 4.07 |
| ms * psysom | 1 | 59 | 0.36 | --- | --- | --- | --- | --- |
| ms * swb | 9 | 1436 | 0.47 | 0.051 | 0.39 - 0.55 | 0.0000 | 20.04* | 1.48 |
| ms * se | 10 | 1359 | 0.43 | 0,065 | 0.32 - 0.52 | 0.0000 | 42.41*** | 2.17 |
| depression * anxiety | 17 | 5524 | 0.67 | 0.065 | 0.60 - 0.74 | 0.0000 | 341.67*** | 4.62 |
| depression * psysom | 9 | 4989 | 0.57 | 0.036 | 0.52 - 0.62 | 0.0000 | 31.99*** | 2.00 |
| depression * swb | 6 | 1562 | 0.54 | 0.132 | 0.33 - 0.70 | 0.0000 | 113.24*** | 4.76 |
| depression * se | 17 | 3865 | 0.51 | 0.030 | 0.46 - 0.55 | 0.0000 | 46.59*** | 1.71 |
| anxiety * psysom | 7 | 2405 | 0.64 | 0.050 | 0.57 - 0.69 | 0.0000 | 27.83*** | 2.15 |
| anxiety * swb | 5 | 1369 | 0.51 | 0.075 | 0.39 - 0.61 | 0.0000 | 21.28*** | 2.31 |
| anxiety * se | 8 | 7249 | 0.42 | 0.040 | 0.35 - 0.48 | 0.0000 | 26.62*** | 1.95 |
| psysom * swb | 2 | 234 | 0.41 | 0.148 | 0.14 - 0.62 | 0.0035 | 3.81, n.s. | 1.95 |
| psysom * se | 1 | 125 | 0.38 | --- | --- | --- | --- | --- |
| swb * se | 14 | 4445 | 0.44 | 0.056 | 0.35 - 0.53 | 0.0000 | 109.67*** | 2.90 |

*Note.* $k$ = number of correlations; $n$ = total sample size; $r$ = random effects average correlation; *SEr* = standard error of $r$; *CI* = 95% confidence interval for $r$; $p$ = significance level of $r$; $Q$ = test statistic for heterogeneity; $H$ = descriptive heterogeneity statistic; *** $p < 0.001$; * $p < 0.05$; ms = mixed symptoms of distress; psysom = psychosomatic symptoms; swb = subjective well-being (high values indicate negative well-being); se = self-esteem (high values indicate low self-esteem); all computations were done with the SPSS-syntax provided in Lipsey and Wilson (2001), using the method of moments; correlations were Fisher-z-transformed before meta-analysing them, weights used as recommended by Lipsey and Wilson (2001, p. 64).

## 5.3. Results from meta-analyses of cross-sectional data

In this chapter, first the cross sectional effect sizes for the comparison of employed and unemployed persons with regard to six indicators of mental health are presented. Then the results of the moderator analyses follow, first for demographic variables, then for potential moderator variables on country level. Finally, the robustness of the negative mental health effect of unemployment is tested: Does the effect still exist when a person's life situation is favorable with regard to several moderator variables?

### 5.3.1. Mean effect sizes for six indicators of mental health

The meta-analysis of cross-sectional data revealed a clear association between unemployment and mental health: Unemployed persons showed significantly more symptoms of distress and impaired well-being than employed persons did. With an average weighted effect size of $d = 0.54$ ($k$ = 323, $n$ = 458,820) the overall effect was of medium size with a narrow confidence interval from 0.50 to 0.57, clearly excluding effects of small size (see table 3). Thus, hypothesis one was endorsed by the meta-analytic results.[3]

With $d = 0.55$ ($k$ = 163, $n$ = 375,163) the average weighted effect for mixed symptoms was significant and nearly identical to the effect size for the overall comparison using the composite effect sizes. The mean effect sizes for depression ($d = 0.50$; $k$ = 130; $n$ = 59,816), anxiety ($d = 0.40$; $k$ = 49; $n$ = 28,233), subjective well-being ($d = 0.51$; $k$ = 68, $n$ = 40,985) and self-esteem ($d = 0.45$, $k$ = 87; $n$ = 28,680) were all also significant and of medium size. With $d = 0.11$ ($k$ = 41, $n$ = 13,857) the average weighted effect size for psychosomatic symptoms was rather small, albeit significantly different from zero. Thus, the effect of unemployment on psychosomatic symptoms is obviously much weaker than the effect of unemployment on the other indicator variables of distress examined here. The effects of unemployment on these other indicators are rather similar to each other, although the confidence intervals of anxiety and self esteem (after exclusion of the outliers) did not overlap with the confidence interval for mixed symptoms.

[3] For a graph of the distribution of the unweighted effect sizes and a list of the statistical parameters see appendix B, figure B-2 and table B-2.

We can conclude that there is no specific type of distress that is particularly strongly related to unemployment. Although depression and reduced self-esteem were revealed to be important characteristics of unemployment distress, they did not dominate over other types of distress such as reduced subjective well-being and anxiety. Thus, hypothesis two had to be rejected. However, it is interesting to learn that psychosomatic symptoms, the only variable that is not only an indicator for mental health, but also taps aspects of physical health, differed from the other variables that focus solely on mental health symptoms. For psychosomatic symptoms a much smaller effect size was found.

The correction for unreliability slightly enhanced the average effect sizes. They ranged from $d = 0.12$ for psychosomatic symptoms to $d = 0.59$ for mixed symptoms of distress after the correction. The corrected overall effect size was $d = 0.60$. However, the pattern of results (which variables had a comparatively large and which had a small effect size) did not change due to the correction because the average reliabilities were very similar for all six indicator variables of mental health.

An outlier analysis identified three studies that possibly could distort the validity of the meta-analytic results (see chapter 5.5.1). The exclusion of these outlying studies slightly reduced the overall-effect (from $d = 0.54$ to $d = 0.51$) and the effects for mixed symptoms (from $d = 0.55$ to $d = 0.52$) and self-esteem (from $d = 0.45$ to $d = 0.38$). However, the general pattern of results did not change as a result of these exclusions.

While the heterogeneity of effect sizes was reduced by the exclusion of outlying studies, it remained large ($H = 1.91 – 2.70$) and highly significant for all indicators of mental health, suggesting searching for moderating variables. After the exclusion, the largest heterogeneity ($H = 2.70$) was found for subjective well-being. This may be a consequence of the composition of this variable that included measures from two distinct, albeit closely related domains, i.e. life satisfaction and mood/positive affectivity.[4]

The meta-analysis of case rates of psychological disorders estimated by clinical screening tests such as the GHQ (Goldberg & Hillier, 1979) or the BDI (Beck, Ward, Mendelson, Mock, & Erbaugh, 1961) revealed the following results: The average proportion of “cases” in

[4] As the Q-statistic depends on the number of samples involved in an analysis, I also report H here. This is a descriptive measure of heterogeneity that holds constant the number of studies, easing the comparison of different meta-analyses. Values exceeding H = 1.5 can be interpreted as indicating “notable heterogeneity” according to Higgins and Thompson (2002, p. 1553).

the unemployed samples was $p = 0.34$ ($CI = 0.31 – 0.38$, $k = 76$, $n = 13388$, $Q = 1058.15$***). The average proportion of "cases" in the employed samples was $p = 0.16$ ($CI = 0.14 – 0.18$, $k = 74$, $n = 74473$, $Q = 3528.85$***). Thus, the proportion of unemployed persons who must be seen as considerably distressed, possibly in need of psychological or medical treatment, more than doubles the proportion of considerably distressed employed persons. This result shows that a medium effect size such as $d = 0.51$ can have strong practical significance, especially in light of unemployment statistics nowadays.

Table 3: **Meta-analyses of cross-sectional data: Comparison of unemployed and employed persons with regard to six indicator variables of mental health**

| *Mental health variable* | *k* | *n* | *d* | $d_{corr}$ | *SEd* | *95% CI* | *P* | *Q* | *H* |
|---|---|---|---|---|---|---|---|---|---|
| overall | 323 | 458820 | 0.54 | 0.60 | 0.0184 | 0.50 – 0.57 | 0.0000 | 2261.20*** | 2.65 |
| overall (outliers excluded) | 315 | 209379 | 0.51 | 0.56 | 0.0179 | 0.47 – 0.54 | 0.0000 | 1762.86*** | 2.37 |
| mixed symptoms | 163 | 375163 | 0.55 | 0.59 | 0.0233 | 0.50 – 0.59 | 0.0000 | 1066.01*** | 2.57 |
| mixed symptoms (outliers ex.) | 157 | 126122 | 0.52 | 0.56 | 0.0235 | 0.48 - 0.57 | 0.0000 | 838.39*** | 2.32 |
| Depression | 130 | 59816 | 0.50 | 0.55 | 0.0260 | 0.45- 0.55 | 0.0000 | 475.09*** | 1.92 |
| Anxiety | 49 | 28233 | 0.40 | 0.45 | 0.0379 | 0.32 - 0.47 | 0.0000 | 175.92*** | 1.91 |
| psychosomatic symptoms | 41 | 13857 | 0.11 | 0.12 | 0.0467 | 0.02 - 0.20 | 0.0000 | 152.46*** | 1.95 |
| subjective well-being | 68 | 40985 | 0.51 | 0.56 | 0.0449 | 0.43 – 0.60 | 0.0000 | 488.72*** | 2.70 |
| Self-esteem | 87 | 28680 | 0.45 | 0.51 | 0.0462 | 0.36 – 0.54 | 0.0000 | 666.97*** | 2.78 |
| Self-esteem (outliers ex.) | 85 | 28280 | 0.38 | 0.43 | 0.0359 | 0.31 – 0.45 | 0.0000 | 367.02*** | 2.09 |

*Note.* $k$ = number of effect sizes; $n$ = total sample size; $d$ = random effects average effect size; $d_{corr}$ = random effects average effect size corrected for unreliability; *SEd* = standard error of $d$; $CI$ = 95% confidence interval for $d$; $p$ = significance level of $d$; $Q$ = heterogeneity test statistic; $H$ = descriptive heterogeneity statistic; *** $p < 0.001$; "outliers ex." = all outlying studies were exluded; all computations were done with the SPSS-syntax provided in Lipsey and Wilson (2001).

## 5.3.2. Moderator analyses

In this chapter I first describe the results of the moderator analysis of several demographic variables. For two of these variables (age and unemployment duration) curvilinear moderator tests are also described. The tests for interaction effects follow. The section on demographic variables ends with the results of a multivariate moderator test examining the effects of several potential moderators simultaneously. In the last section, country differences are examined and potential moderator variables on country level are tested.

### *5.3.2.1. Moderator tests*

Gender, measured by the percentage of female participants in a sample, was found to be a significant moderator of the mental health effects of unemployment (see table 4). For samples with a large proportion of female participants, the effect sizes were weaker than for samples with a small proportion of female participants. This moderator effect was highly significant and comparatively strong (beta = -.22, $p$ = .0000). It remained highly significant and was only slightly weakened when the three outlying studies were removed from the data set and the design characteristics were controlled (beta = -.20, $p$ = .0001). Thus, hypothesis three (a) was endorsed by the meta-analytic results.

For occupational status, i.e. the percentage of blue-collar workers in a sample, a weak trend emerged (beta = .10, $p$ = .1210) with blue-collar samples showing stronger effect sizes than white-collar samples. This effect became significant when the outlying studies were removed from the analysis and the design features were controlled (beta = .19, $p$ = .0102). Thus, occupational status was revealed as a moderator variable of the negative mental health effects of unemployment in the present meta-analysis. As expected, the level of education measured by the average number of years persons spent in formal education was negatively associated with the magnitude of the effect sizes, meaning that the negative mental health effects of unemployment were larger among persons with lower education than among persons with higher education. This effect was not significant, however (beta = -.13, $p$ = .2665). The beta increased slightly in the second step, when the outlying studies were removed from the analysis and the design features were controlled, but remained insignificant (beta = -.17, $p$ = .1910). Note that the number of studies that were available for these analyses was comparatively small ($k$ = 55 and $k$ = 49 respectively). In sum, the empirical results partly supported hypothesis three (b). All effects were in the expected direction with persons with

lower socioeconomic status suffering more from unemployment than persons with higher status. In one instance, this effect was significant.

Table 4: **Moderator analyses of the cross-sectional association between unemployment and mental health**

| *Moderator* | *N* | *Const.* | *b* | *SE* | *p* | *beta* | *Q-Model* | *df-Mdl* | *Q-Residual* | *df-Res* |
|---|---|---|---|---|---|---|---|---|---|---|
| prct. female | 302 | 0.636 | -0.003 | 0.0005 | 0.0000 | -0.22 | 26.16*** | 1 | 527.63*** | 300 |
| prct. female (with controls) | 271 | 0.564 | -0.002 | 0.0005 | 0.0001 | -0.20 | 31.60*** | 4 | 348.35*** | 266 |
| prct. blue-coll. | 148 | 0.49 | 0.001 | 0.0009 | 0.1210 | 0.10 | 2.40 | 1 | 254.65*** | 146 |
| prct. blue-coll. (with controls) | 131 | 0.474 | 0.002 | 0.0008 | 0.0102 | 0.19 | 14.40** | 4 | 167.15** | 126 |
| education | 55 | 0.725 | -0.021 | 0.0192 | 0.2665 | -0.13 | 1.23 | 1 | 77.59* | 53 |
| education (with controls | 49 | 0.365 | -0.029 | 0.0220 | 0.1910 | -0.17 | 5.23 | 4 | 65.84* | 44 |
| prct. minority | 87 | 0.559 | 0.001 | 0.0008 | 0.1358 | 0.12 | 2.22 | 1 | 152.92*** | 85 |
| prct. minority (with controls) | 80 | 0.120 | 0.002 | 0.0009 | 0.0989 | 0.17 | 6.63 | 4 | 102.60* | 75 |
| prct. married | 154 | 0.526 | 0.000 | 0.0009 | 0.7634 | -0.02 | 0.09 | 1 | 206.08** | 152 |
| prct. married (with controls) | 142 | 0.426 | 0.000 | 0.0010 | 0.9215 | -0.01 | 3.56 | 4 | 154.75 | 137 |
| mean age | 307 | 0.620 | -0.002 | 0.0019 | 0.2553 | -0.05 | 1.29 | 1 | 544.39*** | 305 |
| mean age (with controls) | 276 | 0.433 | 0.000 | 0.0019 | 0.9917 | 0.00 | 19.94*** | 4 | 360.95*** | 271 |
| unemp. duration | 165 | 0.438 | 0.007 | 0.0029 | 0.0237 | 0.13 | 5.11* | 1 | 276.01*** | 163 |
| unemp. duration (with controls) | 144 | 0.535 | 0.006 | 0.0029 | 0.0565 | 0.14 | 23.10*** | 4 | 171.43* | 139 |
| year of data collection | 323 | 4.166 | -0.002 | 0.0025 | 0.4589 | -0.03 | 0.54 | 1 | 557.80*** | 321 |
| year of data collection (with controls) | 290 | 2.056 | -0.001 | 0.0024 | 0.7447 | -0.02 | 18.68*** | 4 | 372.62*** | 285 |

*Note. N* = number of samples; Const. = regression constant; *b* = unstandardized regression weight; *SE* = standard error of b; *p* = significance level for b; *beta* = standardized regression weight; *Q-Model* = heterogeneity explained by regression model; *df-Mdl* = degrees of freedom for Q-Model; *Q-Residual* = unexplained heterogeneity; *df-Res*. = degrees of freedom for *df-Res*.; "with controls" = computations done after exclusion of outlier studies and with important design characteristics held constant; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$; all computations were done with the SPSS-syntax provided in Lipsey and Wilson (2001), using a weighted regression model with the method of moments.

Minority status as a moderator revealed a very weak trend with samples with a large percentage of minority members having stronger effect sizes than sample with a small percentage of minority members. This weak trend was not significant in the first step of the analysis (beta = .12, $p$ = .1358). In the second step, when outlier studies were excluded and design features controlled, the trend became stronger and marginally significant (beta = .17, $p$ = .0989). Thus, there was some evidence supporting the assumption that the negative mental health effects of unemployment may be stronger among minority groups than among majority groups. However, this evidence must be seen as preliminary at the moment and in need of further replication. Marital status did not moderate the negative mental health effects of unemployment in the sample of studies meta-analysed here, neither in the first step nor in the second step when the outlying studies were excluded and the controls were applied.

There was no linear association between age and the effect sizes. Thus, according to this test age did not moderate the negative mental health effects of unemployment in the sample of studies meta-analyzed here. The moderator effect remained insignificant when outlying studies were excluded from the data set and when the design characteristics were controlled.[5,6]

Average unemployment duration clearly revealed itself as a significant moderator variable (see table 4): The longer people were unemployed, the more pronounced were the negative mental health effects of unemployment (beta = .13, $p$ = .0237). This effect did not change much when the outlying studies were excluded and design characteristics were controlled (beta = .14, $p$ = .0565). We can conclude that hypothesis three (d) was endorsed by the meta-analytic results, as unemployment distress increased with an increasing length of the present unemployment spell.

The moderator test for the year of data collection revealed an insignificant result. This means that there is no "secular trend", i.e. no general tendency for the negative mental health effects of unemployment to become stronger or weaker during the four decades covered by the data

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[5] McKee-Ryan et al. (2005) found a significant effect for the comparison of school-leaver studies and adult studies, with larger effect sizes for the school-leaver studies. In order to be as close to this analysis as possible, I dichotomized the age distribution at 21 years and then compared young and adult samples. Again, no moderator effect of age was found ($Q_b$ = 1.36, p = 0.2439), although the average effect sizes for the younger samples were slightly larger (d = 0.59) than for the older samples (d = 0.54).

[6] This is one of the few instances where the more conservative random effects model that was employed here did not replicate the findings of an earlier fixed effects analysis with a subsample of the studies meta-analysed here (Moser & Paul, 2001).

set used here (beta = -.03, $p$ = .4589). This result was not changed when the outlier studies where excluded and the design variables controlled (beta = -.02, $p$ = .7447). Thus, hypothesis three (e) was not endorsed by the data.

In sum, only gender, occupational status and unemployment duration were found to be moderators of the unemployment – distress relation, whereas for minority status a very weak trend emerged. The analysis for formal education was hampered by a small sample size with regard to the number of studies. For age and marital status, no moderator effects emerged, although the power was caparatively large for these analyses.

*5.3.2.2. Curvilinear moderator tests*

Theoretical considerations suggest curvilinear associations between unemployment distress and two moderator variables: age and duration of unemployment (see chapter 3.5). For age, the argumentation is that persons, particularly men, in middle age should suffer most from unemployment because during middle age, career commitment and financial pressures due to family responsibilities are at a maximum. The results did not indicate the existence of such a curvilinear relationship between age and the negative mental health effects of unemployment, as the squared age was not significant in the respective meta-regression (beta = 0.02, $p$ = 0.7240; see table 5). When the outlying studies were removed and design variables controlled, a weak trend emerged that was marginally significant (beta = 0.09, $p$ = 0.0724). However, the sign of the beta unexpectedly was positive, indicating a u-shaped association. This means that middle-aged persons suffered *less* from unemployment than younger or older persons, which is a rather surprising result. The analysis was repeated for those samples including predominantly (> 50%) men. No significant curvilinear effect emerged (beta = -0.42, $p$ = 0.3136). This result remained stable when the outliers were removed and the design variables were controlled (beta = 0.04, $p$ = 0.6315). Thus, hypothesis three (c) was not endorsed by the data. Unexpectedly, the linear association between age and unemployment distress was marginally significant in this analysis with older males suffering more from unemployment than younger males (beta = 0.18, $p$ = 0.0550).

With regard to unemployment duration, it is possible that adaptation processes occur after a phase of increasing distress in the first months of unemployment (Jackson & Warr, 1984), leading to a stabilization of mental health levels among long-term unemployed persons. The polynomial meta-regression revealed a marginally significant effect for the quadratic term (beta = -0.16, $p$ = 0.0711), along with the linear effect that has already been

Table 5: **Tests for curvilinear moderation effects**

| *Equation* | *Predictor* | *N* | *Const.* | *b* | *SE* | *beta* | *p* | *Q-Model* | *df-Mdl* | *Q-Residual* | *df-Res* |
|---|---|---|---|---|---|---|---|---|---|---|---|
| both sexes | | 307 | 0.5408 | | | | | 1.41 | 2 | 541.15*** | 304 |
| | age | | | -0.0021 | 0.0020 | -0.05 | 0.2958 | | | | |
| | squared age | | | 0.0001 | 0.0002 | 0.02 | 0.7240 | | | | |
| both sexes, controlled | | 276 | 0.4079 | | | | | 23.07*** | 5 | 356.26*** | 270 |
| | age | | | 0.0005 | 0.0019 | 0.01 | 0.7815 | | | | |
| | squared age | | | 0.0003 | 0.0002 | 0.09 | 0.0724 | | | | |
| only males | | 108 | 0.1778 | | | | | 1.47 | 2 | 178.81*** | 105 |
| | age | | | 0.027 | 0.0245 | 0.47 | 0.2662 | | | | |
| | squared age | | | -0.0004 | 0.0004 | -0.42 | 0.3136 | | | | |
| only males, controlled | | 93 | 0.5112 | | | | | 10.02$^{+}$ | 5 | 114.69* | 87 |
| | age | | | 0.0063 | 0.0033 | 0.18 | 0.0550 | | | | |
| | squared age | | | 0.0001 | 0.0003 | 0.04 | 0.6315 | | | | |
| All samples | | 165 | 0.5377 | | | | | 8.35* | 2 | 271.54*** | 162 |
| | unemp. duration | | | 0.0121 | 0.0042 | 0.25 | 0.0042 | | | | |
| | unemp. duration squared | | | -0.0002 | 0.0001 | -0.16 | 0.0711 | | | | |
| All samples, controlled | | 144 | 0.5819 | | | | | 26.28*** | 5 | 166.86* | 138 |
| | unemp. duration | | | 0.0119 | 0.0045 | 0.30 | 0.0084 | | | | |
| | unemp. duration squared | | | -0.0002 | 0.0001 | -0.21 | 0.0657 | | | | |
| All samples, controlled, duration outlier excluded | | 143 | 0.5036 | | | | | 26.18*** | 5 | 166.07* | 137 |
| | unemp. duration | | | 0.0130 | 0.0050 | 0.25 | 0.0092 | | | | |
| | unemp. duration squared | | | -0.0003 | 0.0003 | -0.11 | 0.2342 | | | | |

*Note. N* = number of samples; *Const.* = regression constant; *b* = unstandardized regression weight; *SE* = standard error of b; *p* = significance level for b; *beta* = standardized regression weight; *Q-Model* = heterogeneity explained by regression model; *df-Mdl* = degrees of freedom for *Q-Model*; *Q-Residual* = unexplained heterogeneity; *df-Res*. = degrees of freedom for *df-Res*.; controlled = outlier samples excluded and three design variables controlled, results for control variables not reported here. * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$; all computations were done with the SPSS-syntax provided in Lipsey and Wilson (2001), using a polynomial weighted regression model with the method of moments; Age and unemployment duration were centered before the regressions were conducted.

reported. This effect increased slightly in its size and remained significant when outlier studies were removed and design features were controlled (beta = -0.21, $p$ = 0.0657). The sign of the quadratic term was negative in both regressions, indicating an inverted u-shaped association, as expected. Inspection of the scatterplot, however, revealed that this trend was possibly due to a single outlying study (Brown & Gary, 1985) that combined a very long duration of unemployment (74 months) with an effect size only slightly above the mean (d = 0.64). Exclusion of this study reduced the curvilinear trend to insignificance (beta = -.11, $p$ = 0.2342). Thus the curvilinear part of hypothesis three (d) was only weakly supported by the meta-analytic results.

In sum, I found no clear proofs for curvilinear relationships between unemployment distress and either age or unemployment duration.

#### *5.3.2.3. Supplementary analysis: Tests for interactions of two moderator variables*

The female gender role has changed substantively during the last few decades, resulting in an increased participation in the labor force and – possibly – in an increased commitment to employment (see chapter 3.5.1.). Therefore I expected that the difference between males and females with regard to the negative effects of unemployment should be smaller in more recent studies than in older studies. To test this, a meta-regression with three predictor variables was conducted: Gender, year of data collection, and an interaction term of gender and year of data collection (see table 6). The results did not reveal any signs of an interaction effect between gender and the year of data collection with regard to the distressing effects of unemployment (beta = 0.00, $p$ = 0.9773). Excluding the outlying studies and controlling for possibly confounding design features did not change this result (beta = 0.02, $p$ = 0.6301).

The latter analysis showed that the changes in the female gender role that occurred during the last decades obviously were not yet strong enough to substantially influence the difference between males and females with regard to the distressing effects of unemployment. This result suggests testing another possible interaction effect, i.e. an interaction between gender and marital status. Marriage may increase a male's employment commitment in order to fulfill his traditional duties as a family provider while it may reduce a female's commitment to the work role due to increased family obligations. However, the results did not support this reasoning, as no interaction effect could be found (beta = -0.01, $p$ = 0.8984). Excluding the outlying studies and controlling for possibly confounding design features did not change this result (beta = -0.05, $p$ = 0.5360).

Table 6: **Tests for interaction effects**

| *Interaction* | *Predictor* | *N* | *Const.* | *b* | *SE* | *beta* | *p* | *Q-Model* | *df-Mdl* | *Q-Residual* | *df-Res* |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Gender * year of data collection | | 302 | 0.5362 | | | | | 26.29*** | 3 | 529.40*** | 298 |
| | Percentage females | | | -0.0026 | 0.0005 | -0.22 | 0.0000 | | | | |
| | Year data collection | | | -0.0005 | 0.0025 | -0.01 | 0.8521 | | | | |
| | Interaction term | | | 0.0000 | 0.0001 | 0.00 | 0.9773 | | | | |
| Gender * year of data collection (controlled) | | 271 | 0.4876 | | | | | 31.82*** | 6 | 348.02*** | 264 |
| | Percentage females | | | -0.0019 | 0.0005 | -0.20 | 0.0002 | | | | |
| | Year data collection | | | -0.0002 | 0.0025 | 0.00 | 0.9506 | | | | |
| | Interaction term | | | 0.0000 | 0.0001 | 0.02 | 0.6301 | | | | |
| Gender * marital status | | 149 | 0.5122 | | | | | 9.95* | 3 | 195.22** | 145 |
| | Percentage females | | | -0.0020 | 0.0006 | -0.22 | 0.0021 | | | | |
| | Percentage married | | | -0.0007 | 0.0009 | -0.05 | 0.4419 | | | | |
| | Interaction term | | | 0.0000 | 0.0000 | -0.01 | 0.8984 | | | | |
| Gender * marital status (controlled) | | 137 | 0.3349 | | | | | 14.05* | 6 | 149.86 | 130 |
| | Percentage females | | | -0.0023 | 0.0007 | -0.26 | 0.0020 | | | | |
| | Percentage married | | | -0.0005 | 0.0010 | -0.04 | 0.6015 | | | | |
| | Interaction term | | | 0.0000 | 0.0000 | -0.05 | 0.5360 | | | | |

*Note. N* = number of samples; *Const.* = regression constant; *b* = unstandardized regression weight; *SE* = standard error of b; *p* = significance level for b; *beta* = standardized regression weight; *Q-Model* = heterogeneity explained by regression model; *df-Mdl* = degrees of freedom for *Q-Model*; *Q-Residual* = unexplained heterogeneity; *df-Res*. = degrees of freedom for *Q-Residual*; controlled = outlier samples excluded and three design features controlled, results for control variables not reported here. * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$; all computations were done with the SPSS-syntax provided in Lipsey and Wilson (2001), using a weighted regression model with the method of moments; variables were centered before the interaction terms were computed.

*5.3.2.4. Multivariate moderator test*

In order to test whether the moderator effects that emerged in the bivariate analysis remained stable when the influence of the other demographic variables was held constant, I intended to conduct a weighted multiple regression using all potential moderator variables simultaneously. However, several of the variables had very large numbers of missing values. Therefore, I dropped all variables that caused severe reductions in the sample size of the multiple regression. These were: percentage of minority members, percentage of blue-collar workers, percentage of married participants, and unemployment duration. Therefore, only the year of data collection, age, and gender could be used for the multivariate moderator analysis (see table 7).

Table 7: **Moderators of the unemployment – distress relationship (weighted multiple regression)**

| | *Model 1* | | | *Model 2 (outliers excluded + design controlled)* | | |
|---|---|---|---|---|---|---|
| *Variables* | *b* | *SE* | *beta* | *b* | *SE* | *beta* |
| Year of data collection | -.001 | 0.0026 | -0.01 | -0.001 | 0.0026 | -0.02 |
| Percentage females | -0.003*** | 0.0005 | -0.21 | -0.002*** | 0.0005 | -0.20 |
| Mean age | -0.003 | 0.0020 | -0.06 | 0.000 | 0.0019 | 0.01 |
| constant | 2.333 | 5.1300 | | 2.071 | 5.0947 | |
| *k* | 290 | | | 261 | | |
| $R^2$ | 0.05 | | | 0.09 | | |
| *Q-Model* | 26.33*** | | | 31.88*** | | |
| *df-Model* | 3 | | | 6 | | |
| *Q-Residual* | 516.26*** | | | 341.42*** | | |
| *df-Residual* | 286 | | | 254 | | |

*Note.* *k* = number of samples; *b* = unstandardized regression weight; *SE* = standard error of *b*; *p* = significance level for *b*; *beta* = standardized regression weight; $R^2$ = explained variance; *Q-Model* = heterogeneity explained by regression model; *df-Mdl* = degrees of freedom for *Q-Model*; *Q-Residual* = unexplained heterogeneity; *df-Res*. = degrees of freedom for *Q-Residual*.; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$; all computations were done with the SPSS-syntax provided in Lipsey and Wilson (2001), using a weighted regression model with the method of moments; outlier samples were excluded in the second analysis and three design features were controlled, results for control variables not reported here.

The meta-regression revealed a very clear moderating effect for gender (beta = -.21***): The more females were included in a sample, the lower the difference between unemployed and

employed persons with regard to mental health. This highly significant effect remained stable when the three studies with outlying values were excluded and when the influence of the three design features was controlled (beta = -.20***). No other significant moderator effect emerged. Thus, the results of this – restricted – multivariate moderator test replicated the results of the bivariate analyses described above.

*5.3.2.5. Differences between countries*

The dataset included effect sizes from 26 countries (see table 8). A moderator test for differences between the countries was highly significant ($p < 0.001$). After exclusion of the outlier studies, the moderator effect was reduced to a weak trend that was no longer marginally significant ($p = 0.1114$). This is probably the result of the very small cell frequencies that dominated this analysis.

With one exception, mean effect sizes for all countries were positive, meaning that unemployed persons showed more symptoms of distress than employed persons in all countries but one. The exception was Japan, with an insignificant effect size of $d = -0.53$ in the opposite direction. The only available Japanese study (Imai, 2001) examined only female nurses, questioning its representation of the Japanese labor force.[7]

The confidence intervals for 17 countries excluded zero and demonstrated that unemployment had a negative effect on mental health in these countries (USA, UK, Germany, Australia, Finland, Canada, Netherlands, Ireland, Austria, Sweden, Italy, New Zealand, Denmark, Norway, Israel, France, and Mexico). In the case of nine other countries with comparatively small databases (either $k = 1$ or $n < 1{,}000$ in each case), the effect has not definitely been proven yet, as the confidence intervals included zero in each of these cases (China /Hong Kong,

[7] The restriction to only one occupation may be an explanation for the unexpected direction of the effect in Imai's (2001) study. The author cites other studies that reported high levels of distress and burnout among nurses. Thus, nursing may be such an overdemanding and stressful job that unemployment indeed has positive effects here (Fryer & Payne, 1984). It seems premature, though, to draw such a conclusion from the results of only one study and further evidence clearly is needed with regard to this question. Another possible reason for the unexpected direction of effect in this study may be seen in the use of an American self-esteem scale (Rosenberg, 1965) in East-Asia. Self-evaluations have been shown to differ between Eastern and Western samples (Heidemeier, 2005) and the validity of Rosenberg's scale in Japan may yet be questionable (no information concerning the validity of this scale in Japan is given in the report).

Table 8: **Mean effect sizes for 26 countries**

| *Country* | *k* | *n* | *d* | *SEd* | *95% CI* | *p* | *Q* | *H* |
|---|---|---|---|---|---|---|---|---|
| USA | 87 | 301,861 | 0.51 | 0.0345 | 0.44 – 0.58 | 0.0000 | 113.44* | 1.15 |
| USA (out. ex.) | 86 | 53,468 | 0.51 | 0.339 | 0.44 – 0.58 | 0.0000 | 118.18$^{+}$ | 1.18 |
| UK | 74 | 61,227 | 0.65 | 0.0383 | 0.57 – 0.72 | 0.0000 | 148.83*** | 1.43 |
| UK (out. ex.) | 69 | 60,579 | 0.59 | 0.0381 | 0.52 – 0.67 | 0.0000 | 64.57 | 0.97 |
| Germany | 40 | 18,846 | 0.47 | 0.0508 | 0.37 – 0.57 | 0.0000 | 75.91*** | 1.40 |
| Australia | 27 | 13,265 | 0.52 | 0.0635 | 0.39 – 0.64 | 0.0000 | 30.81 | 1.09 |
| Finland | 17 | 6,176 | 0.40 | 0.0780 | 0.25 - 0.56 | 0.0000 | 17.61 | 1.05 |
| Canada | 12 | 18,207 | 0.40 | 0.0897 | 0.22- 0.57 | 0.0000 | 8.31 | 0.87 |
| Netherlands | 10 | 10,202 | 0.40 | 0.0981 | 0.21 - 0.60 | 0.0000 | 15.31$^{+}$ | 1.30 |
| Ireland | 7 | 5,653 | 0.78 | 0.1320 | 0.52 - 1.04 | 0.0000 | 3.18 | 0.73 |
| Austria | 7 | 725 | 0.52 | 0.1338 | 0.26 – 0.79 | 0.0001 | 11.72$^{+}$ | 1.40 |
| Sweden | 6 | 2,594 | 0.56 | 0.1331 | 0.30 – 0.82 | 0.0000 | 1.18 | 0.49 |
| Italy | 4 | 1,147 | 0.51 | 0.1554 | 0.20 – 0.81 | 0.0011 | 6.34$^{+}$ | 1.45 |
| New Zealand | 4 | 564 | 0.68 | 0.1946 | 0.30 – 1.06 | 0.0005 | 7.35$^{+}$ | 1.57 |
| Denmark | 3 | 2,553 | 0.55 | 0.1643 | 0.22 – 0.87 | 0.0009 | 1.60 | 0.89 |
| India | 3 | 1,928 | 2.04 | 0.1898 | 1.66 – 2.41 | 0.0000 | 31.59*** | 3.97 |
| India (out. Ex.) | 1 | 1,528 | 0.57 | 0.3147 | -0.05 – 1.19 | 0.0694 | --- | --- |
| Norway | 3 | 1,354 | 0.41 | 0.1696 | 0.08 – 0.74 | 0.0162 | 0.08 | 0.20 |
| Israel | 3 | 657 | 0.44 | 0.1734 | 0.10 – 0.78 | 0.0108 | 2.21 | 1.05 |
| France | 3 | 517 | 0.49 | 0.2142 | 0.07 – 0.91 | 0.0234 | 0.51 | 0.50 |
| Hong Kong | 3 | 393 | 0.35 | 0.1903 | -0.03 – 0.72 | 0.0697 | 0.23 | 0.34 |
| Mexico | 2 | 1,550 | 0.72 | 0.2091 | 0.31 – 1.13 | 0.0006 | 4.64* | 2.15 |
| Belgium | 2 | 598 | 0.37 | 0.2143 | -0.05 – 0.79 | 0.0810 | 0.13 | 0.36 |
| Turkey | 1 | 3,517 | 0.20 | 0.2711 | -0.33 – 0.74 | 0.4517 | --- | --- |
| Chile | 1 | 1,943 | 0.55 | 0.2912 | -0.02 – 1.13 | 0.0567 | --- | --- |
| Spain | 1 | 1,427 | 0.32 | 0.2826 | -0.23 – 0.87 | 0.2584 | --- | --- |
| Japan | 1 | 904 | -0.53 | 0.2905 | -1.10 - 0.04 | 0.0687 | --- | --- |
| Greece | 1 | 860 | 0.55 | 0.3184 | -0.07 – 1.18 | 0.0822 | --- | --- |
| Switzerland | 1 | 152 | 0.41 | 0.3408 | -0.26 – 1.08 | 0.2268 | --- | --- |

| | | |
|---|---|---|
| Q-between countries | 103.49*** | df = 25 |
| Q-within countries | 480.98*** | df = 297 |
| Q-total | 584.47*** | df = 322 |
| Q-between countries (out. ex.) | 33.84 | df = 25 |
| Q-within countries (out. ex.) | 378.14*** | df = 289 |
| Q-total (out. ex.) | 411.98*** | df = 314 |

*Note.* $k$ = number of samples; $n$ = total sample size; $d$ = random effects average effect size; $SE_d$ = standard error of $d$; CI = 95% confidence interval for $d$; $p$ = significance level of $d$; $Q$ = heterogeneity test statistic; $H$ = descriptive heterogeneity statistic; $+ p < 0.10$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$; out. ex. = outliers excluded; all computations were done with the SPSS-syntax provided in Lipsey and Wilson (2001), countries were ordered according to the number of samples / participants;

Belgium, Turkey, Chile, Spain, Japan, Greece, Switzerland, and India). Ignoring Japan, the average effect sizes varied between $d$ = 0.20 for Turkey and $d$ = 0.78 for Ireland. The heterogeneity within the countries was rather low in most cases, with a clearly significant $Q$ only for Germany after exclusion of the outlier studies.

On country level, five moderators were tested: Economic development, inequality, unemployment protection, labor market favorability, and individualistic vs. collectivistic culture. The results of these analyses clearly revealed that a country's level of economic development moderates the negative mental health effects of unemployment (see table 9). The difference between more and less developed countries was highly significant for the HDI-index ($Q_b$ = 17.98; $p$ = 0.0000) as well as for the GDP per capita ($Q_b$ = 11.84; $p$ = 0.0006). In countries with a low HDI-score the negative effects of unemployment were stronger ($d$ = 0.63) than in countries with a high HDI-score ($d$ = 0.47). The same was true for countries with a small GDP per capita ($d$ = 0.62) as opposed to countries with a large GDP ($d$ = 0.49). To test the stability of these results, I also repeated the analysis after application of some measures to control for the influence of possibly biasing factors.[8] Although the moderator effect of economic development was slightly weakened by these exclusions, it remained clearly significant with regard to both indicator variables of development (HDI: $Q_b$ = 7.15; $p$ = 0.0075; GDP: $Q_b$ = 7.36; $p$ = 0.0067). Thus, hypothesis three (f) was clearly endorsed by the meta-analytic results, since in countries with a high level of economic development, the negative mental health effects of unemployment were alleviated compared to countries with a low level of economic development.

Inequality also moderated the negative mental health effects of unemployment: Countries with high Gini-scores or large proportions of poor citizens exhibited significantly larger effect sizes than countries with low Gini-scores or small percentages of poor citizens (Gini: $d$ = 0.57 vs. $d$ = 0,48, $Q_b$ = 4.97; $p$ = 0.0258; percentage paupers: $d$ = 0.58 vs. $d$ = 0.45, $Q_b$ = 10.71; $p$ = 0.0011). With regard to the percentage of poor citizens, this moderator effect was weakened to non-significance after the exclusion of possibly biasing studies ($Q_b$ = 2.25; $p$ = 0.1336). The effect remained stable with regard to the Gini-index, however ($Q_b$ = 5.29; $p$ = 0.0214). Thus, hypothesis three (g) was endorsed by the meta-analytic results. In countries

[8] This was done by excluding the outlying studies and all studies with possibly biasing design characteristics, i.e. studies published in German, studies that used a measure of somatization, and studies that examined the participants via an oral interview.

with high levels of income inequality, the negative mental health effects of unemployment were stronger than in more egalitarian countries.

Level of unemployment protection also moderated the difference between employed and unemployed persons with regard to mental health, endorsing hypothesis three (h). In countries with a high level of unemployment protection, the effect sizes were significantly weaker than in countries with a medium or low level of unemployment protection (unemployment protection high ($d$ = 0.46 vs. $d$ = 0,58, $Q_b$ = 8.61; $p$ = 0.0033). After the exclusion of possibly biasing studies, the difference between the average effect sizes was slightly reduced and not significant any longer ($d$ = 0.41 vs. $d$ = 0.49, $Q_b$ = 2.03; $p$ = 0.1539). Thus, high levels of unemployment protection are able to reduce the detrimental health impact of unemployment, but this result was not stable, possibly due to the reduced power in the controlled analysis.

There was a marginally significant moderating effect for labor market favorability as measured by the national unemployment rate ($Q_b$ = 3.04; $p$ = 0.0814). For countries with high unemployment rates, I found significantly stronger effect sizes than for countries with low unemployment rates ($d$ = 0.55 vs. $d$ = 0.49). Thus, high unemployment rates may be associated with increased distress among the unemployed. Yet, this moderator effect diminished when outlying studies were excluded and important design features controlled ($Q_b$ = 1.76; $p$ = 0.1844). Furthermore, for the labor market security index (LMSI), no significant result emerged, neither in the uncontrolled ($Q_b$ = 0.20; $p$ = 0.6545), nor in the controlled analysis ($Q_b$ = 0.98; $p$ = 0.3215). Thus, hypothesis three (i) was only weakly endorsed by the results, as high unemployment rates were associated with increased negative mental health effects of unemployment in the present meta-analytic data-set, but this effect was not stable and could not be replicated with an alternative measure of labor market favorability. Therefore, caution is advisable with regard to conclusions concerning the existence of a moderating effect of labour market favorability.

As expected, the effect sizes were larger in countries with an individualistic culture than in countries with a collectivistic culture. These differences were not significant, however. Thus, a country's level of individualism/collectivism did not moderate the distressing effects of unemployment, neither when measured with the Hofstede-scores ($Q_b$ = 1.57; $p$ = 0.2098), nor when measured with the scores provided by Spector et al. (2001) ($Q_b$ = 1.33; $p$ = 0.2482). When the outlying studies were excluded and the design features were controlled, these results remained stable (Hofstede: $Q_b$ = 1.48; $p$ = 0.2241; Spector et al.: $Q_b$ = 1.52; $p$ = 0.2176). Nevertheless, when only the outlying studies were excluded, but no further design

Table 9: **Country differences as moderators of the unemployment-distress relationship**

| *Moderator* | *Subgroup* | $Q_b$ | *p* | *k* | *n* | *d* | *95% CI* | $Q_w$ | *H* |
|---|---|---|---|---|---|---|---|---|---|
| economic development (HDI-Index) | | 17.98 | 0.0000 | | | | | | |
| | High dev. | | | 190 | 364,323 | 0.47 | 0.43– 0.52 | 245.44** | 1.14 |
| | Low dev. | | | 133 | 94,497 | 0.63 | 0.58– 0.69 | 291.00*** | 1.48 |
| economic development (HDI, controlled) | | 7.15 | 0.0075 | | | | | | |
| | High dev. | | | 74 | 35,709 | 0.41 | 0.35– 0.48 | 116.14** | 1.26 |
| | Low dev. | | | 83 | 53,055 | 0.53 | 0.47– 0.59 | 90.73 | 1.05 |
| economic development (GDP) | | 11.84 | 0.0006 | | | | | | |
| | High dev. | | | 203 | 374,713 | 0.49 | 0.44– 0.53 | 267.59** | 1.15 |
| | Low dev. | | | 120 | 84,107 | 0.62 | 0.56– 0.68 | 276.74*** | 1.52 |
| economic development (GDP; controlled) | | 7.36 | 0.0067 | | | | | | |
| | High dev. | | | 79 | 36,621 | 0.41 | 0.35– 0.48 | 137.11*** | 1.33 |
| | Low dev. | | | 78 | 52,143 | 0.53 | 0.47– 0.60 | 71.76 | 0.97 |
| Inequality (Gini-index) | | 4.97 | 0.0258 | | | | | | |
| | High inequal. | | | 215 | 392,789 | 0.57 | 0.52– 0.61 | 323.32*** | 1.23 |
| | Low inequal. | | | 108 | 66,031 | 0.48 | 0.42– 0.54 | 239.45*** | 1.50 |
| Inequality (Gini, controlled) | | 5.29 | 0.0214 | | | | | | |
| | High inequal. | | | 122 | 65,817 | 0.50 | 0.45– 0.55 | 161.78** | 1.16 |
| | Low inequal. | | | 35 | 22,947 | 0.38 | 0.29– 0.47 | 43.09 | 1.13 |
| Inequality (prct. Poverty) | | 10.71 | 0.0011 | | | | | | |
| | High inequal. | | | 203 | 383,972 | 0.58 | 0.54– 0.63 | 406.66*** | 1.42 |
| | Low inequal. | | | 95 | 62,080 | 0.45 | 0.39– 0.51 | 130.17** | 1.18 |
| Inequality (prct. Pov., controlled) | | 2.25 | 0.1336 | | | | | | |
| | High inequal. | | | 114 | 63,947 | 0.49 | 0.44– 0.54 | 163.05** | 1.20 |
| | Low inequal. | | | 31 | 21,526 | 0.41 | 0.32– 0.50 | 31.47 | 1.02 |
| Unemployment protection | | 8.61 | 0.0033 | | | | | | |
| | High | | | 97 | 46,291 | 0.46 | 0.39– 0.52 | 128.09* | 1.16 |
| | Low | | | 221 | 407,495 | 0.58 | 0.53 - 0.62 | 423.25*** | 1.39 |
| Unemployment protection (controlled) | | 2.03 | 0.1539 | | | | | | |
| | High | | | 31 | 11,484 | 0.41 | 0.31– 0.51 | 24.96 | 0.91 |
| | Low | | | 122 | 75,763 | 0.49 | 0.44– 0.54 | 174.55** | 1.20 |

| *Moderator* | *Subgroup* | $Q_b$ | *p* | *k* | *n* | *d* | *95% CI* | $Q_w$ | *H* |
|---|---|---|---|---|---|---|---|---|---|
| Labor market (LMSI) | | 0.20 | 0.6545 | | | | | | |
| | Favorable market | | | 170 | 123,517 | 0.55 | 0.50– 0.60 | 256.51*** | 1.23 |
| | Unfavorable market | | | 150 | 334,910 | 0.53 | 0.48– 0.58 | 287.04*** | 1.39 |
| Labor market (LMSI, controlled) | | 0.98 | 0.3215 | | | | | | |
| | Favorable market | | | 106 | 72,532 | 0.49 | 0.44– 0.55 | 127.17+ | 1.10 |
| | Unfavorable market | | | 49 | 16,004 | 0.44 | 0.36– 0.52 | 75.06** | 1.25 |
| Labor market (unemployment rate) | | 3.04 | 0.0814 | | | | | | |
| | Favorable market | | | 154 | 332,034 | 0.49 | 0.44– 0.54 | 231.73*** | 1.23 |
| | Unfavorable market | | | 160 | 120,972 | 0.55 | 0.50– 0.60 | 257.07*** | 1.27 |
| Labor market (unemp. rate, controlled) | | 1.76 | 0.1844 | | | | | | |
| | Favorable market | | | 51 | 18,782 | 0.43 | 0.36– 0.51 | 88.50*** | 1.33 |
| | Unfavorable market | | | 104 | 69,754 | 0.50 | 0.44– 0.55 | 113.22 | 1.05 |
| Individualism/collectivism (Hofstede) | | 1.57 | 0.2098 | | | | | | |
| | Individ. countries | | | 242 | 419,742 | 0.55 | 0.51– 0.59 | 345.68*** | 1.20 |
| | Collectiv. Countries | | | 81 | 39,078 | 0.50 | 0.43– 0.57 | 220.59*** | 1.66 |
| Individualism/collectivism (Hofstede, outliers exclud.) | | 4.88 | 0.0271 | | | | | | |
| | Individ. countries | | | 236 | 170,701 | 0.53 | 0.49– 0.57 | 273.97* | 1.08 |
| | Collectiv. Countries | | | 79 | 38,678 | 0.44 | 0.37 - 0.51 | 133.93*** | 1.31 |
| Individualism/collectivism (Hofstede, controlled) | | 1.48 | 0.2241 | | | | | | |
| | Individ. countries | | | 139 | 81,649 | 0.49 | 0.44– 0.53 | 179.10* | 1.14 |
| | Collectiv. Countries | | | 18 | 7,115 | 0.40 | 0.27– 0.53 | 25.81+ | 1.23 |
| Individualism/collectivism (Spector ) | | 1.33 | 0.2482 | | | | | | |
| | Individ. countries | | | 175 | 368,190 | 0.57 | 0.52– 0.62 | 272.21*** | 1.25 |
| | Collectiv. countries | | | 64 | 42,572 | 0.52 | 0.44– 0.60 | 180.62*** | 1.69 |
| Individualism/collectivism (Spektor, outliers exclud.) | | 4.92 | 0.0265 | | | | | | |
| | Individ. countries | | | 169 | 119,149 | 0.55 | 0.50– 0.59 | 205.32* | 1.11 |
| | Collectiv. Countries | | | 62 | 42,172 | 0.45 | 0.37– 0.52 | 96.82** | 1.26 |
| Individualism/collectivism (Spector, controlled) | | 1.52 | 0.2176 | | | | | | |
| | Individ. countries | | | 97 | 58,468 | 0.50 | 0.45– 0.56 | 136.13** | 1.19 |
| | Collectiv. countries | | | 14 | 12,939 | 0.41 | 0.28– 0.55 | 14.87 | 1.07 |

*Note.* $k$ = number of effect sizes; $n$ = sample size; $d$ = weighted average effect size; *CI* = 95% confidence interval for d, $Q_b$ = between-group heterogeneity estimate; $Q_w$ = within-group heterogeneity estimate; $H$ = descriptive heterogeneity statistic *** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$; $^{+}p < 0.10$;

controls were applied, significant moderator effects emerged for both kinds of scores (Hofstede: $Q_b = 4.88$; $p = 0.0271$; Spektor et al.: $Q_b = 4.92$; $p = 0.0265$). The distance between individualistic and collectivistic countries with regard to the average effect sizes were very similar for the analyses without outliers and the analyses without outliers plus design controls (always around 0.10). Thus, the inconsistent findings with regard to the significance level are possibly a consequence of a lack of power in the latter analysis due to the strongly reduced number of studies caused by the exclusion of studies with oral interviews, measures for somatization, and German language. In sum, it is possible to say that there was some evidence endorsing hypothesis three (j) concerning the moderating effect of culture on the distressing effect of unemployment, yet this evidence was not stable across different analyses.

Overall, the variables that emerged as clear moderators of the unemployment-distress relationship at country level were the level of economic development and income inequality. The distressing impact of unemployment was larger in economically less developed countries and in countries with high income inequality. Labor market favorability and individualism/collectivism could not be demonstrated as moderators of the mental health effects of unemployment.

### 5.3.4. Supplementary analysis: The robustness of the negative mental health effect of unemployment

The intention of this analysis was to check whether the negative mental health effect of unemployment is a robust phenomenon that generalizes across different life situations. If it really is a robust phenomenon it should be difficult to identify subgroups of persons who do not suffer from unemployment. Therefore, in order to find such subgroups of persons who are not affected by unemployment, I intended to combine several variables that were revealed as significant moderators in the moderator analysis. The assumption was that subgroups of persons with favorable values on several moderator variables might not be affected by the deleterious effects of unemployment. The following variables were selected: Economic development, income inequality, gender, occupational status, and unemployment duration. These variables were selected because of their comparatively strong and consistent moderator effects (see chapters 5.3.2.1. and 5.3.2.5.). It was necessary to confine the analysis to combinations of three moderator variables, as analyses with more variables resulted in extremely small *k*s.

As can be seen in table 10, all combinations of three moderator variables clearly reduced the size of the negative mental health effect of unemployment. The average effect sizes ranged from $d = 0.07$ to $d = 0.39$. However, most average effect sizes were significantly different from zero and above $d = 0.20$, i.e. above the value of a small effect according to Cohen (1977). Only combinations of two demographic variables in egalitarian countries resulted in insignificant average effects of small size. Yet, the latter analyses were based on rather small samples of studies and may lack stability. We can conclude that the distressing effect of unemployment is a rather robust phenomenon. Even when favorable values of three moderator variables are combined, the effect usually remains stable, though reduced in its size.

### 5.3.5. Summary of cross-sectional results

The cross sectional analyses demonstrated that unemployed persons are considerably more distressed than employed persons. This is true for all six indicator variables of mental health examined here. With the exception of psychosomatic symptoms, all effect sizes were of medium size. Thus, I found no specific unemployment syndrome, although mental health might be more affected by unemployment than physical health.

Among the demographic variables, gender, occupational status, and unemployment duration were found to be moderators of the unemployment-distress relationship, with males, workers from blue-collar jobs, and long-term unemployed persons showing larger effect sizes than females, workers from white-collar jobs, and short-term unemployed persons. For minority status, a weak trend was revealed with minority members showing stronger effect sizes than majority members. For education, age, marital status, and for the year of data collection, no moderating effects were found. No convincing evidence for curvilinear associations with age and unemployment duration was revealed.

For the majority of the 26 countries that contributed to the present meta-analysis, a significant negative mental health effect of unemployment has been demonstrated up to now. Economic development and income inequality were revealed as consistent moderators of the negative mental health effects of unemployment, with stronger effect sizes in less developed countries and countries with stronger inequality compared to more developed and more egalitarian countries. The level of unemployment protection also emerged as a significant moderator variable, although this effect was reduced to insignificance in the controlled analysis, possibly due to a lack of power. With regard to the favorability of the labor market, some evidence was

found for a moderator effect with stronger effect sizes in unfavorable labor markets comnpared to favorable labor markets. For individualism/collectivism, I also found significant moderator effects with larger effects in individualistic societies compared to collectivistic societies. However, the latter three moderator effects were not stable across different forms of analysis and are in need of further replication.

Table 10**: Anlysis of robustness: Combinations of moderator variables**

| *Selected countries* | *Moderator combinations* | *k* | *n* | *d* | *SEd* | *95% CI* | *P* | *Q* | *H* |
|---|---|---|---|---|---|---|---|---|---|
| all countries | majority female, majority white-collar, short-term unemployed | 7 | 3,242 | 0.39 | 0.1277 | 0.14 – 0.64 | 0.0025 | 49.71*** | 2.88 |
| only developed countries (HDI high) | majority female, short-term unemployed | 19 | 6,696 | 0.23 | 0.0706 | 0.09 – 0.37 | 0.0012 | 85.49*** | 2.18 |
| | majority female, majority white-collar | 14 | 4,760 | 0.30 | 0.1013 | 0.10 – 0.50 | 0.0028 | 74.81*** | 2.40 |
| | majority white-collar, short-term unemployed | 14 | 3,518 | 0.36 | 0.0813 | 0.20 – 0.52 | 0.0000 | 45.27*** | 1.87 |
| only egalitarian countries (Gini low) | majority female, short-term unemployed | 10 | 4,609 | 0.07 | 0.0809 | -0.09– 0.23 | 0.3692 | 31.45*** | 1.87 |
| | majority female, majority white-collar | 7 | 3,113 | 0.15 | 0.1614 | -0.17– 0.46 | 0.3683 | 37.47*** | 2.50 |
| | majority white-collar, short-term unemployed | 2 | 1,404 | 0.13 | 0.0785 | -0.02– 0.29 | 0.0871 | 0.99 | 0.99 |
| Only developed and egalitarian countries (HDI high + Gini low) | majority female | 38 | 20,927 | 0.32 | 0.0559 | 0.21 – 0.43 | 0.0000 | 237.56*** | 2.53 |
| | majority white-collar | 16 | 18,026 | 0.28 | 0.0815 | 0.12 – 0.44 | 0.0006 | 126.86*** | 2.91 |
| | short-term unemployed | 28 | 10,649 | 0.29 | 0.0604 | 0.18 – 0.41 | 0.0000 | 123.50*** | 2.14 |

*Note. k* = number of effect sizes; *n* = total sample size; *d* = random effects average effect size; *SEd* = standard error of d; *CI* = 95% confidence interval for d; *p* = significance level of d; *Q* = heterogeneity test statistic; *H* = descriptive heterogeneity statistic; *** $p < 0.001$; 'short term unemployed' mean less than 12 months of unemployment; all computations were done with the SPSS-syntax provided in Lipsey and Wilson (2001); outlier studies were excluded for this analysis

The distressing effect of unemployment was found to be a rather robust phenomenon, as it can be demonstrated even in groups where several moderator effects combine to lessen the negative impact of unemployment.

## 5.4. Results from meta-analyses of longitudinal data

### 5.4.1. Retest correlations

The meta-analyses of T1-T2-correlations revealed a moderate stability of mental health within the longitudinal studies assembled here (see table 11). The meta-analytic mean correlations for the seven status-track groups (employment to unemployment, unemployment to employment, etc.) ranged from 0.37 to 0.58. All average correlations were significantly different from zero ($p < 0.001$). There was a tendency for persons in stable employment situations (both times employed, both times unemployed, or both times in school) to show slightly larger T1-T2-correlations than persons experiencing a change event with regard to their employment status, for example job loss or finding a new job after a phase of unemployment. The former group of mean correlations ranged from $r = 0.46$ to $r = 0.58$, the latter ranged from $r = 0.37$ to $r = 0.48$. This might be interpreted as a sign of differential reactions to employment-related change events: Not all individuals react in the same way when they experience job loss, or re-employment, or the transition from school to the world of work. Some may experience considerable distress, while others may cope much better. Thus, such events tend to change the rank order with regard to mental health among the affected persons, reducing the respective retest-correlations. However, time intervals between measurements were longer for change groups than for non-change groups (employed-unemployed: 16 months; unemployed-employed: 14 months; school-employed: 21 months; school-unemployed: 27 months; employed-employed: 10 months; unemployed-unemployed: 10 months; school–school: 20 months). This finding may represent an alternative explanation for the comparatively weak, respectively strong, T1-T2-correlation among these groups. One would expect weaker retest-correlations for the groups experiencing change simply because of the fact that more time passed between time one and time two for this groups. Therefore, I suggest the finding of stronger stability estimates for mental health among persons in stable employment situations compared to unstable employment situations with some caution, as the time intervals were not comparable for both groups.

Table 11: **Meta-analyses of longitudinal studies: T1-T2-correlations of mental health scores for six groups of persons**

| | *k* | *n* | *r* | *SEr* | *95% CI* | *p* | *Q* | *H* |
|---|---|---|---|---|---|---|---|---|
| employment – unemployment | 3 | 502 | 0.37 | 0.0624 | 0.26 – 0.47 | 0.0000 | 2.85 | 1.19 |
| unemployment – employment | 18 | 2858 | 0.48 | 0.0488 | 0.40 – 0.55 | 0.0000 | 66.54*** | 1.98 |
| school – unemployment | 5 | 332 | 0.37 | 0.1067 | 0.18- 0.54 | 0.0003 | 10.41* | 1.61 |
| school – employment | 7 | 6949 | 0.40 | 0.0425 | 0.33 - 0.47 | 0.0000 | 44.98*** | 2.74 |
| unemployment – unemployment | 23 | 2267 | 0.58 | 0.0562 | 0.51 - 0.64 | 0.0000 | 73.69*** | 1.83 |
| employment – employment | 10 | 10006 | 0.46 | 0.0376 | 0.40 – 0.51 | 0.0000 | 64.71*** | 2.68 |
| school - school | 2 | 1568 | 0.48 | 0.0253 | 0.44 – 0.52 | 0.0000 | 0.19 | 0.44 |

*Note.* $k$ = number of correlations; $n$ = total sample size; $r$ = random effects average correlation; *SEr* = standard error of $r$; *CI* = 95% confidence interval for r; $p$ = significance level of r; $Q$ = heterogeneity test statistic; $H$ = descriptive heterogeneity statistic; *** $p < 0.001$; * $p < 0.05$; all computations were done with the SPSS-syntax provided in Lipsey and Wilson (2001), correlations were Fisher-z-transformed before meta-analysing them.

### 5.4.2. Social causation: Mean changes from T1 to T2

To examine changes in the average levels of mental health over time, repeated measures effect sizes for mean changes were computed and meta-analyzed for all available longitudinal studies (see chapter 4.5). These analyses revealed a significant increase of distress symptoms for persons who lost their jobs between the measurement times ($d$ = 0.19, $p$ = 0.0000; see table 12). I also found a significant reduction of distress for unemployed persons who found new jobs between T1 and T2 ($d$ = -0.35, $p$ = 0.0000). Both results support the assumption that unemployment not only correlates with, but also causes distress. The effect for re-employment was stronger ($d$ = -0.35) than the increase of distress associated with job-loss ($d$ = 0.19). A similar difference between both values was also reported by Murphy and Athanasou (1999) and McKee-Ryan et al. (2005). Thus, this difference in effect sizes for changes into versus changes out of unemployment was replicated in three independent meta-analyses. But despite the robustness of this finding, it is a surprising result, as one would intuitively expect that the improvement in well-being that is associated with reemployment is about the same size as the deterioration in well-being that is associated with job-loss. For school-leaver samples I found a similar, but even larger, difference in effect sizes. For young

job-finders, there was a significant reduction of distress symptoms with an effect size of $d$ = -0.30 ($p$ = 0.0001)[9]. For young persons becoming unemployed after school, a weak non-significant increase of distress symptoms with $d$ = 0.10 ($p$ = 0.2988) was found. Thus, it can be concluded that becoming employed is generally associated with larger absolute changes in mental health than becoming unemployed. How could this be explained? The results for continuously employed persons and young persons who stay within the educational system during the time of a longitudinal study may be able to shed some light on this question.

Continuously employed persons showed a small but significant reduction in distress between the first and the second measurement point ($d$ = -0.06, $p$ = 0.0155). Thus, when all participants were in an employment situation at T1 and at T2 that could be labeled as the "standard" situation for members of the labor force in Western societies, a slight improvement in mental health was observed despite the lack of external events that could explain this improvement. A similar effect was revealed for youths: When young people stayed within the educational system between T1 and T2, their distress levels decrease significantly ($d$ = -0.14, $p$ = 0.0013). As these results stem from comparatively large databases ($k$ = 26 $n$ = 24,679 for adults; $k$ = 14, $n$ = 5,564 for youths) and were found by a comparatively conservative statistical method (random effects meta-analysis), there are few doubts that the observed improvements in mental health scores are real. Therefore, we can conclude that when employment status does not change within a longitudinal study there is a general trend toward feeling better at the second measurement time. Testing effects may be an explanation for this phenomenon. Such unintended effects of repeated measurement have already been described by Wohlwill (1977) as a problem of longitudinal research designs. The General Health Questionnaire (Goldberg & Hillier, 1979) as the most frequently used instrument of measuring mental health in psychological unemployment research is particularly susceptible to such effects. For this instrument, artificial reductions of distress scores from one measurement point to the next have empirically been shown to be typical (Ormel, Koeter, & van den Brink, 1989).

The tendency of mental health scores to improve for persons who are employed or in school at both measurement points may help to explain the difference in the absolute size of the

[9] With H = 4.87 the heterogeneity was very large for this analysis. A closer inspection of the data revealed that one outlying study, Bachmann et al. (1978) combined the largest negative effect size (d = -0.88) with the second largest sample size (n = 1,205). Exclusion of this study reduced heterogeneity to H = 3.01. The average effect size was d = -0.24 after the exclusion.

mental health changes associated with losing and gaining employment. The increase in distress that is associated with becoming unemployed may partly be neutralized by the general tendency to feel better when people are tested repeatedly, while the improvement in mental health that is associated with becoming employed after unemployment or after school may be augmented by this effect. If we correct the effect sizes for the adult change groups for the tendency to feel better when tested repeatedly (by subtracting the employed-employed effect from the employed–unemployed and the unemployed-employed effect) both effect sizes become much more similar in their absolute size with $d = 0.25$ for job-losers and $d = -0.29$ for persons changing from unemployment to employment. For young people, such a correction would also lead to much more similar effect sizes, with $d = 0.24$ for persons who become unemployed after school and $d = -0.27$ for young people who are successful in their job hunt. This suggests that the differences in absolute size of the effects that emerged for changes into employment compared to changes into unemployment are caused by problems of repeated measurement.

Table 12: **Meta-analyses of longitudinal studies: mental health changes for six groups of persons**

| | *k* | *n* | *d* | *SEd* | *95% CI* | *p* | *Q* | *H* |
|---|---|---|---|---|---|---|---|---|
| Emp - Unemp | 19 | 1,933 | 0.19 | 0.0469 | 0.10 - 0.29 | 0.0000 | 55.06*** | 1.75 |
| Unemp - Emp | 45 | 4,513 | -0.35 | 0.0374 | -0.42 - -0.28 | 0.0000 | 185.30*** | 2.05 |
| School - Unemp | 15 | 957 | 0.10 | 0.0984 | -0.09 - 0.30 | 0.2988 | 91.70*** | 2.56 |
| School - Emp | 16 | 6,023 | -0.30 | 0.0782 | -0.45 - -0.14 | 0.0001 | 355.10*** | 4.87 |
| Emp - Emp | 28 | 24,679 | -0.06 | 0.0247 | -0.11 - -0.01 | 0.0155 | 198.54*** | 2.71 |
| Unemp. - Unemp. | 61 | 6,565 | -0.08 | 0.0322 | -0.14 - -0.01 | 0.0185 | 295.25*** | 2.22 |
| School - School | 14 | 5,564 | -0.15 | 0.0465 | -0.24 - -0.06 | 0.0013 | 107.91*** | 2.88 |

*Note.* $k$ = number of correlations; $n$ = total sample size; $d$ = average repeated measures effect size; *SEd* = standard error of $d$; *CI* = 95% confidence interval for d; $p$ = significance level of d; $Q$ = heterogeneity test statistic; $H$ = descriptive heterogeneity statistic; *** $p < 0.001$; ** $p < 0.01$; all meta-analytic computations were done with the SPSS-syntax provided in Lipsey and Wilson (2001) using a random effects model applying the method of moments; a positive effect size indicates an increase of distress symptoms between T1 and T2.

I found a significant moderating effect for unemployment duration in the meta-analysis of cross-sectional studies. Therefore, I expected that mental health would deteriorate with continuing unemployment in longitudinal studies. This was not the case. Instead, I found a small but significant reduction of distress symptoms between T1 and T2 for continuously unemployed persons ($d$ = -0.08, $p$ = 0.0185), meaning that mental health slightly improves when the unemployment period lengthens, which is a rather surprising result. A closer look at the database revealed that it includes several intervention studies that tested the effectiveness of psychological or other programs intended to help the unemployed. A moderator analysis comparing intervention and non-intervention samples resulted in a significant difference ($Q_b$ = 29.62; $p < 0.001$). Interventions resulted in significant reductions of distress symptoms ($d$ = -0.35, $p$ = 0.0000), while for longitudinal studies without interventions no significant changes in mental health emerged ($d$ = 0.03, $p$ = 0.4491) (see table 13). Thus, we can conclude that intervention programs for unemployed people are indeed effective as they are associated with an improvement in well-being that is not typical for permanently unemployed persons. However, even after the exclusion of intervention studies, the increase in distress among permanently unemployed persons still was surprisingly weak ($d$ = 0.03) and not significant. The aforementioned tendency to feel better when tested repeatedly may help to explain this unexpected result. This general trend of mental health ratings to improve through repeated measurement possibly overshadowed the deterioration that should be associated with permanent unemployment. Correcting the mean effect size of non-intervention samples for this general tendency to report improved mental health would lead to an effect size of $d$ = 0.09 which is more consistent with the results of the cross-sectional analysis.[10]

[10] Another explanation for the weak increase in distress among continuously unemployed persons could be selective panel attrition. When distressed persons have a higher probability of dropping out during the course of a longitudinal study than persons who are not distressed, the resulting estimate of the mental health deterioration between T1 and T2 would be artificially reduced. To check this assumption I screened all studies included in the longitudinal meta-analysis for permanently unemployed persons for hints to such a kind of attrition bias. Among the 51 independent studies involved in this meta-analysis, 17 reported tests comparing those persons who responded at both measurement points with those persons who were lost between T1 and T2 with regard to mental health. Only in one of these studies (Hamilton et al., 1993) was a significant difference found, with dropouts reporting slightly more distress at T1 than respondents who did not drop out. Thus, there is only very weak evidence that selective attrition due to different dropout probabilities among more and less distressed persons might have biased the results of the meta-analysis for continuously unemployed persons.

In sum the meta-analysis of mean change scores resulted in several interesting results: Finding a new job and finding one's first job were associated with significant improvements in mental health, clearly endorsing hypothesis four (b). Losing a job was associated with a significant deterioration in mental health. Becoming unemployed after school was associated with a deterioration in mental health, but this result was not significant. Thus, hypothesis four (a) was partly supported. In sum, these results clearly support the assumption that unemployment is causally related to mental health. A general tendency to report health improvement when repeatedly tested helps in understanding the unexpected differences in the absolute magnitude of the average effect sizes for changes into unemployment (weaker effects) compared to changes into employment (stronger effects). This tendency might have overshadowed deteriorations in mental health to a certain degree. It also helps in understanding the lack of significant deterioration of mental health among continuously unemployed persons. Again, the general tendency to report health improvement when repeatedly tested may have overshadowed deteriorations in mental health that are associated with prolonged unemployment.

Table 13: **Interventions as a moderator of mental health changes among continuously unemployed persons**

| *Group* | $Q_b$ | *k* | *n* | *d* | *SEd* | *95% CI* | *p* | $Q_w$ | *H* |
|---|---|---|---|---|---|---|---|---|---|
| | 29.62*** | | | | | | | | |
| intervention | | 16 | 1899 | -0.35 | 0.0595 | -0.47 – -0.23 | 0.0000 | 55.63*** | 1.93 |
| no intervention | | 45 | 4666 | 0.03 | 0.0361 | -0.04 – 0.10 | 0.4491 | 30.17 | 0.83 |

*Note.* *k* = number of correlations; *n* = total sample size; *d* = average repeated measures effect size; *SEd* = standard error of *d*; *CI* = 95% confidence interval for *d*; *p* = significance level of *d*; $Q_b$ = between-group homogeneity estimate; $Q_w$ = within-group homogeneity estimate; *** $p < 0.001$; *H* = measure of heterogeneity with *k* held constant; all meta-analytic computations were done with the SPSS-syntax provided in Lipsey and Wilson (2001) using a random effects model applying the method of moments; a positive effect size indicates an increase of distress symptoms between T1 and T2.

### 5.4.3. Selection effects: Cross-sectional comparisons at T1

In order to test the existence of selection effects, I also meta-analyzed cross-sectional comparisons from the first measurement point of longitudinal studies (see chapter 4.5.). These analyses revealed small but significant differences in mental health between persons who were more successful in the labor market and persons who were less successful in the labor

market, always favoring the former group (see table 14). Employed persons who lost their jobs during the course of a longitudinal study showed more signs of distress than continuously employed persons already at T1, when both groups were still employed. This effect, which is consistent with hypothesis five (a), was small, but highly significant ($d = 0.23$; $p = 0.0000$). Furthermore, at T1, when both groups were still unemployed, continuously unemployed persons showed more symptoms of distress than those unemployed persons who managed to find a new job in the near future. With $d = 0.15$ the effect again was small, but highly significant ($p = 0.0000$), endorsing hypothesis five (b). A similar result was revealed for school leavers: Those young persons who became unemployed after finishing school showed more symptoms of distress already at school than those young persons who managed to find a job after school. This effect was very small ($d = 0.08$), but significant ($p = .0033$) and endorsed hypothesis five (c).

Meta-analysing the T1-comparisons separately for the six indicator variables of mental health revealed two interesting findings (see appendix B, table B-4): The effect size for anxiety was always the weakest of the six, and in two of three cases it was even negative. The effect size for self-esteem was always the largest, or the second largest, and it was always larger than the overall effect size. Thus, self-esteem seems to be particularly important with regard to a persons's success in the labor market, while anxiety plays no prominent role here.

As the comparisons described above were made at T1, when both groups shared an identical employment status, they allow conclusions concerning the effects of mental health upon a person's labor market success. The results show that an impaired mental health precedes job loss among employed persons, while a good mental health precedes (re)employment among unemployed persons and students, indicating that there is a causal link from mental health to a person's employment status. However, all effect sizes were small or very small, showing that this causal link is likely to be of little practical importance.[11]

[11] If one interprets neuroticism as a personality dimension with close connections to mental health, the present findings with regard to selection effects of mental health are in good agreement with empirical results concerning personality: Longitudinal data show that neuroticism predicts employment status (de Fruyt & Mervielde, 1999) and a recent meta-analysis shows that neuroticism is a predictor of objective and subjective career success (Ng, Eby, Sorensen, & Feldman, 2005).

Table 14: **Meta-analyses of longitudinal studies, cross-sectional comparisons at T1: selection effects**

| *Status T1-T2* | *k* | *n* | *d* | *SEd* | *95% CI* | *p* | *Q* | *H* |
|---|---|---|---|---|---|---|---|---|
| Emp-Unemp vs. Emp-Emp | 21 | 18,477 | 0.23 | 0.0402 | 0.15 – 0.31 | 0.0000 | 36.50* | 1.35 |
| Unemp–Unemp vs. Unemp-Emp | 49 | 13,259 | 0.15 | 0.0382 | 0.08 – 0.23 | 0.0000 | 136.99*** | 1.69 |
| School-Unemp vs. School-Emp | 17 | 12,163 | 0.08 | 0.0281 | 0.03 - 0.14 | 0.0033 | 17.22 | 1.04 |

*Note.* $k$ = number of correlations; $n$ = total sample size; $d$ = average weighted effect size; $SEd$ = standard error of d; $CI$ = 95% confidence interval for $d$; $p$ = significance level of $d$; $Q$ = heterogeneity test statistic; $H$ = descriptive heterogeneity statistic; *** $p < 0.001$; * $p < 0.05$; all meta-analytic computations were done with the SPSS-syntax provided in Lipsey and Wilson (2001) using a random effects model applying the method of moments; a positive effect size means that (1) continuously unemployed persons showed more distress an T1 than unemployed persons who found new jobs until T2, (2) job losers showed more symptoms of distress at T1 than continuously employed persons, (3) school leavers who became unemployed showed more symptoms of distress at T1, while still in school, than school leavers who became employed later on.

### 5.4.4. Summary of longitudinal results

Summarizing the longitudinal analyses as a whole, it can be said that evidence was found for both causal mechanisms; selection as well as social causation. Thus, the relationship between unemployment and mental health can be described as a vicious circle: Persons with impaired mental health are more likely to lose their jobs. Unemployment then further impairs mental health, leading to lowered chances of finding a new job. However, the effect sizes supporting the social causation explanation were clearly larger than the effect sizes supporting the selection explanation. Thus, the former effect is likely to be of greater practical importance than the latter effect.

## 5.5. Sensitivity analysis

### 5.5.1. Outlier analysis

The outlier analysis revealed three samples that were clear outliers with considerably larger standardized residuals than the rest of the samples in the meta-analysis (see appendix B, figures B-3 and B-4). One outlier was among the samples of the study conducted by

Hepworth (1980) with a very large effect size of $d = 3.86$ ($n = 288$). The effect size estimates for the other samples in Hepworth's (1980) study were also comparatively large (all $d >= 1.08$). This phenomenon is possibly due to the fact that this study borrowed its employed comparison group from another study (referenced only as "Clegg and Wall, in preparation", p. 141). Thus, the paper actually uses data from two different studies from different research teams, one examining employed and the other unemployed persons, possibly calling into question the comparability of both datasets.

The two other outlying effect sizes ($d = 2.79$ and $d = 2.83$, $n = 200$ in both cases) belonged to one study examining the self-concepts of employed and long-term unemployed graduated young men (age 27-32) from urban middle-class families in India (Singh, Singh & Rani, 1996). However, setting aside the fact that a rather special group of persons from a non-Western country was examined here, I was not able to detect any unusual design characteristics from the report of this study.

Another problematic study (CDC, 1998) was not identified as an outlier by the analysis with standardized residuals because it had an effect size very close to the meta-analytic mean effect size. It was an extreme value, however, with regard to its sample size. With $n = 248,393$, this study included more than half of all participants taking part in the studies meta-analyzed here. Such large-sample outlier studies are problematic as they strongly influence the results of a meta-analysis, even a random effects meta-analysis as was conducted here, and may be the source of considerable bias (Osburn & Callender, 1992).

In sum, three samples were identified as outliers. In only one case, a clearly questionable design feature could be identified that might have distorted the results. As a consequence of the outlier analysis, the three outlier studies (including the large-sample outlier) were removed from the dataset, and all analyses were repeated without theses studies in order to check the stability of the results.[12] The results were usually stable and did not show large changes as a consequence of the exclusion (see chapter 5.3.).

[12] In the case of Hepworth's (1980) study, I removed the entire study, not only the sample that was identified as an outlier, since the other samples also had very large effect sizes and were also affected by the problems of the study design.

### 5.5.2. The influence of design characteristics on the results of primary studies

To check whether the design of the primary studies influenced the outcomes, a series of moderator tests with several variables measuring important design characteristics was conducted. With regard to the way a study was published, no significant result emerged ($Q_b = 3.78$; $p = 0.2863$) (see table 15). Effect sizes reported in dissertation theses were slightly smaller ($d = 0.46$) than the effect sizes reported in peer-reviewed journals ($d = 0.53$). Surprisingly, the effect sizes in the "other"-category, which included master theses, other degree dissertations, unpublished institute reports, and similar material, were the largest of all subgroups analyzed here ($d = 0.69$). As this effect size was based on a comparatively small number of samples, it is likely to be a spurious finding, however. Altogether, the results from this analysis did not indicate the existence of a publication bias.

Among studies that were conducted in primarily German-speaking countries (Germany, Austria, Switzerland) I found a strong difference between studies published in English ($d = 0.95$) and studies published in German ($d = 0.41$). The difference between the mean effect sizes was highly significant ($Q_b = 11.98$; $p = 0.0005$). Note that the mean effect size for the studies published in English is rather large, whereas the mean effect sizes for studies published in German was slightly below the meta-analytic overall mean effect size. Obviously, research results from German-speaking countries concerning the mental health effects of unemployment are likely to be published in English only when they are very "good", i.e. when effect sizes are strong and highly significant. Whether this is due to self-censoring of the authors, due to editorial policy, or due to other reasons is beyond the reach of the present study.

Questioning format, i.e. the comparison of studies presenting the questions to the participants in written vs. in oral form, also moderated the magnitude of effects ($Q_b = 13.73$; $p = 0.0002$). Studies presenting the items in written form had significantly lower effect sizes ($d = 0.48$) than studies presenting the items in oral form ($d = 0.63$). Personal interviews may be better suited toward reveal the distress that is felt by the unemployed than impersonal paper and pencil tests are. On the other hand, interviewer expectations and suggestive questioning may influence the answering behavior of the participants, leading to biased results. Thus, they differ from each other, but it is not yet clear whether results from interviews or from paper and pencil tests are more valid in the field of psychological unemployment research.

Whether unemployment was the main topic of a study or not had absolutely no influence on the strength of the unemployment-distress relationship ($Q_b$ = 0.03; $p$ = 0.8608). Studies that used “unemployment”, “employment”, “work” or a similar term in the headline ($d$ = 0.54) did not differ from studies that did not use these terms with regard to the size of the reported effect ($d$ = 0.54).

The four categories that I used to operationalize unemployment did not differ from each other with regard to the mean effect sizes. Studies examining persons affected by a factory closure or a mass-layoff had slightly smaller effect sizes ($d$ = 0.38) than the other three categories, which were very similar to each other ($d$ = 0.53, 0.54, 0.56). This difference was not significant, however ($Q_b$ = 6.03; $p$ = 0.1100).

With regard to the employed comparison group, it played no role whether the employees in this group had been formerly unemployed or not. While studies using formerly unemployed persons had slightly smaller effect sizes ($d$ = 0.49) than other studies ($d$ = 0.55), this difference was not significant ($Q_b$ = 1.65; $p$ = 0.1995). However, whether the comparison group included part-time employees did influence the outcomes of a study: Studies using only full-time employees as a comparison group ($d$ = 0.57), or including no more than 20% part-time employees in the comparison group ($d$ = 0.68), reported larger effect sizes than studies with more than 20% part-time employees in the comparison group ($d$ = 0.32). This moderator effect was significant ($Q_b$ = 9.16; $p$ = 0.0102).

Thus, it can be concluded that three of the seven design characteristics studied here significantly influenced the strength of the unemployment-distress relationship: The language of the publication, the questioning format, and the percentage of part-time employees among the employed comparison group. These results did not change when the outlying studies were excluded (see appendix B, table B-5). Therefore, I repeated all analyses with language and questioning format held constant. I also controlled the use of a somatization scale, as this

Table 15: **Study characteristics as moderators of the unemployment-distress relationship**

| *Moderator* | *Subgroup* | $Q_b$ | *p* | *k* | *n* | *d* | *SEd* | *95% CI* | $Q_w$ | *H* |
|---|---|---|---|---|---|---|---|---|---|---|
| Way of publication | | 3.78 | 0.2863 | | | | | | | |
| | Journal articles | | | 234 | 433,740 | 0.53 | 0.0215 | 0.49 - 0.58 | 457.80*** | 1.40 |
| | Books / book chapters | | | 50 | 16,987 | 0.55 | 0.0483 | 0.45 - 0.64 | 40.35 | 0.91 |
| | Dissertation theses | | | 25 | 5,324 | 0.46 | 0.0696 | 0.33 - 0.60 | 43.34** | 1.34 |
| | Other | | | 13 | 2,388 | 0.69 | 0.0946 | 0.50 - 0.87 | 14.02 | 1.08 |
| Language | | 11.98*** | 0.0005 | | | | | | | |
| | English | | | 8 | 5,178 | 0.95 | 0.1459 | 0.67 - 1.24 | 7.42 | 1.03 |
| | German | | | 40 | 14,545 | 0.41 | 0.0587 | 0.30 - 0.53 | 48.23 | 1.11 |
| Questioning format | | 13.73*** | 0.0002 | | | | | | | |
| | written | | | 207 | 102,788 | 0.48 | 0.0229 | 0.44 - 0.53 | 331.45*** | 1.27 |
| | oral | | | 91 | 340,742 | 0.63 | 0.0338 | 0.57 - 0.70 | 189.19*** | 1.45 |
| Focus on unemployment? | | 0.03 | 0.8608 | | | | | | | |
| | yes | | | 262 | 123,994 | 0.54 | 0.0210 | 0.49 - 0.58 | 503.23*** | 1.39 |
| | no | | | 61 | 334,825 | 0.54 | 0.0414 | 0.46 - 0.62 | 42.97 | 0.85 |
| Operationalization of unemployment | | 6.03 | 0.1100 | | | | | | | |
| | Officially registered | | | 137 | 45,962 | 0.56 | 0.0290 | 0.51 - 0.62 | 346.22*** | 1.60 |
| | Involuntarity explicitly mentioned | | | 52 | 39,848 | 0.53 | 0.0460 | 0.44 - 0.62 | 66.74$^{+}$ | 1.14 |
| | Involuntarity inferred | | | 107 | 368,764 | 0.54 | 0.0317 | 0.48 - 0.60 | 87.25 | 0.91 |
| | Factory closure | | | 27 | 4,246 | 0.38 | 0.0686 | 0.25 - 0.52 | 45.33* | 1.32 |
| Labour market history comparison group | | 1.65 | 0.1995 | | | | | | | |
| | Formerly unemployed | | | 69 | 16,958 | 0.49 | 0.0411 | 0.41 - 0.57 | 68.11 | 1.00 |
| | other | | | 254 | 441,861 | 0.55 | 0.0206 | 0.51 - 0.59 | 492.25*** | 1.39 |
| Prct. part-time in comparison group | | 9.16* | 0.0102 | | | | | | | |
| | 0% part time | | | 54 | 50,267 | 0.57 | 0.0358 | 0.50 - 0.64 | 63.93 | 1.10 |
| | 1-20% part-time | | | 9 | 2,330 | 0.68 | 0.0962 | 0.49 - 0.87 | 11.98 | 1.22 |
| | > 20% part-time | | | 9 | 8,293 | 0.32 | 0.0859 | 0.16 - 0.49 | 8.27 | 1.02 |

*Note.* $k$ = number of correlations; $n$ = total sample size; $d$ = average repeated measures effect size; *SEd* = standard error of $d$; *CI* = 95% confidence interval for $d$; $p$ = significance level of $d$; $Q_b$ = between-group homogeneity estimate; $Q_w$ = within-group homogeneity estimate; $^{+}p < 0.10$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$; $H$ = descriptive measure of heterogeneity; all meta-analytic computations were done with the SPSS-syntax provided in Lipsey and Wilson (2001) using a random effects model applying the method of moments.

measure of mental health produced much smaller effect sizes than the other measures.[13] This procedure, which was usually done together with the exclusion of outlying studies, allowed for checking of the stability of the results of the moderator analyses with regard to variations of the research methodology (see chapter5.3).

### 5.5.3. Publication bias

The shape of the funnel plot was fairly symmetrical (see figure 1), as one would expect when no publication bias exists. Although, to my subjective impression, the region left to the null value was less intensively dotted with data points than one would expect in the case of total symmetry; there was no clear "'bite' out of the lower left-hand corner of the plot, where sample sizes and effect-size estimates are small" as is regarded to be indicative for publication bias (Wang & Bushmann, 1998, p. 47). In sum, while there appeared to be no strong asymmetry in the shape of the funnel, less obvious signs of publication bias could indeed be detected.

Next, the "Trim and Fill" method (Duval & Tweedie, 2000a, 2000b) was applied to the data set in order to estimate the number of suppressed or otherwise missing studies. According to the *R0+* estimator, three samples have been suppressed. The *L0+* estimator equaled zero, indicating that no study has been suppressed. Usually, the next step of this method, the "filling" step, would imply the imputation of the missing values and a computation of a mean effect sizes that is adjusted for publication bias. Yet, due to the low number of missing studies, I abstained from applying this second "filling" step. In sum, according to the "Trim and Fill" method, publication bias appears to not be a problem for the present meta-analysis.

Begg and Mazumdar's (1994) rank correlation test represents another statistical method for detect publication bias. Applying this method to the present meta-analytic dataset resulted in a weak but highly significant correlation between the (standardized) effect sizes and their variances (Tau = 0.12, $p$ = 0.001). This result was stable after exclusion of the outlying studies (Tau = 0.11, $p$ = 0.003). Thus, according to this method, a publication bias exists, but appears to not be very strong in the present data set.

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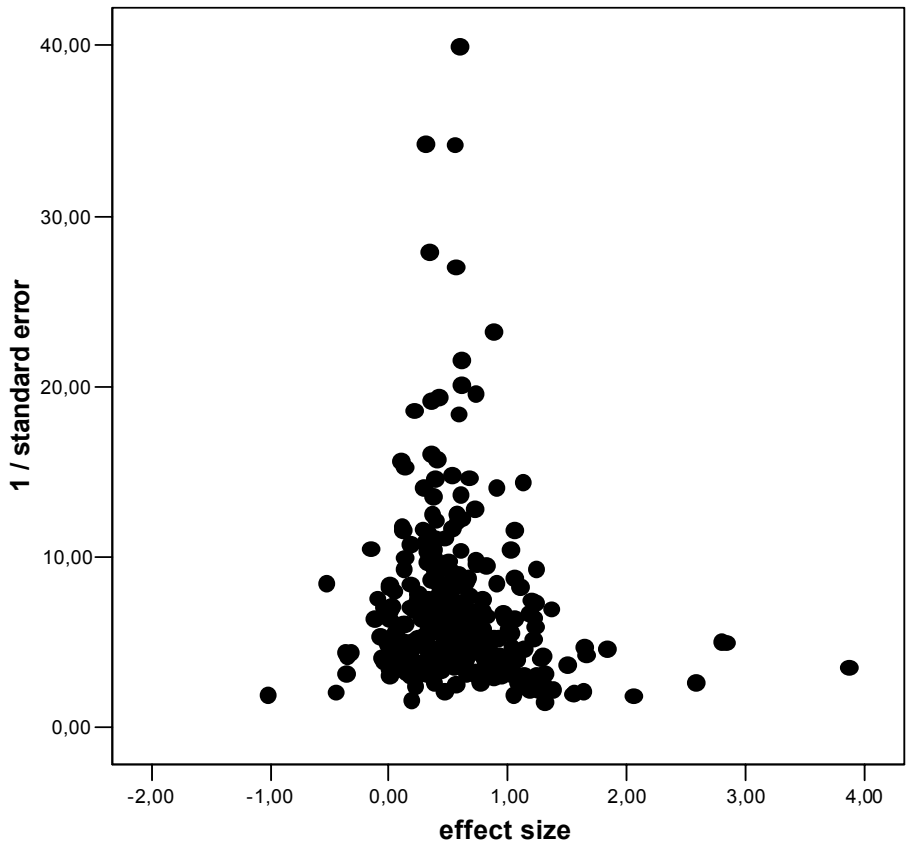
[13] The percentage of part-time employees was ignored, because data for this variable were too rare. Trying to control percentage of part-time employees would have meant severely reducing the number of samples and thus the power of the analyses.

Figure 1**: Funnel plot** (all samples)




*Note.* One value with 1/*se* = 72.56 was set to 1/*se* = 40.00 to ease visual inspection of the funnel plot

I also checked whether the size of the publication bias differed for different values of the moderator variables examined here. If this had been the case, such a differential publication bias could distort the results of the moderator tests. To do this, I treated each moderator variable in the following way: I computed the rank correlation test within each subgroup (e.g. separately for white collar samples and for blue collar samples). Then I tested the resulting rank correlations for the subgroups against each other with a method appropriate for the comparison of Tau-correlations (Bortz, Lienert, & Boehnke, 1990) to check whether publication bias differed on different levels of the moderator variable. The results did not reveal evidence supporting the assumption of differential publication bias: The Tau-correlation was slightly larger among younger studies that were conducted after 1984 (Tau = 0.18) than among older studies (Tau = 0.04). It was slightly larger among younger samples up to age 21 (Tau = 0.21) than among adult samples (Tau = 0.09). It was larger among persons with few educational attainments (Tau = 0.19) than among persons with high

educational attainments (Tau = -0.19), and it was slightly larger among white-collar samples (Tau = 0.26) than among blue-collar samples (Tau = 0.13) (see appendix B, table B-6). However, none of these differences were significant. Thus, it is unlikely that differential publication bias distorted the validity of the moderator analyses conducted here.

I also used the fail-safe N statistic (Orwin, 1983; Rosenthal, 1995) to estimate the tolerance of the present results for unpublished null or negative results. In other words, I estimated the number of studies with null or negative results that would be necessary to reduce the present findings to a certain limit. The limit I chose was $d$ = 0.20, the value of "small" effect sizes according to Cohen (1977). The resulting fail-safe-$N$ for null effects was 549, i.e. more than 500 studies with null-effects are needed to bring the mean effect size of $d$ = 0.54 for the mental health effects of unemployment down to a small effect of $d$ = 0.20. It appears to be quite unlikely that such a large number of unpublished studies with null effects exist. However, the assumption that unpublished studies usually have null effects might be too optimistic, as an inspection of the funnel plot suggests that possibly suppressed studies may have weak negative effects, not null effects (see above). Therefore, I repeated the fail-safe-$N$ analysis with the assumption that the suppressed studies have a mean effect size of $d$ = -0.51, half of $d$ = -1.02, which was the most negative value that was found in the present meta-analysis. Under this assumption, the tolerance value for suppressed studies was $N$ = 155. Although not impossible, it appears to be unlikely that so many studies with negative values have been suppressed. Please note that several of these negative effects with a mean of $d$ = -.51 would have been statistically significant, strongly reducing the likelihood of suppression, as a significant finding with the message that "unemployment makes you feel good" would have a certain sensational value and probably a high likelihood of publication.

In sum, these results are in excellent agreement with each other: The visual inspection of the funnel plot revealed only weak signs of publication bias. Although the rank correlation test was highly significant, the Tau-correlation itself was rather weak (please note that the power of this test was unusually large due to the large number of samples meta-analyzed here). According to the "Trim and Fill"-method, publication bias should also not be a considerable problem within the field of psychological unemployment research, as the estimated number of suppressed studies was rather small. The fail-safe-$N$ method shows that the tolerance of the present findings for unpublished null results, or unpublished negative effects, appears to be sufficiently high, as more than 150 suppressed studies would be needed to reduce the mean effect sizes to a small size.

### 5.5.3. Summary of results of the sensitivity analysis

The sensitivity analysis revealed three outlier studies. It also revealed that some design characteristics influenced the results of the primary studies on the mental health effects of unemployment, namely language of publication, questioning format, and the inclusion of part-time employees in the comparison group. For two important design characteristics that could be expected to correlate with the magnitude of the effect sizes when a publication bias exists – publication status and whether unemployment was the main topic of the study or not – no significant effects were found.

The visual inspection of the funnel plot and two more objective methods revealed evidence that a publication bias exists in the realm of psychological unemployment research, suppressing studies with small, insignificant effect sizes. However, this bias appears to be weak and should not distort the results of the present meta-analysis in an intolerable manner. Analyses with the fail-safe N statistic endorse this conclusion, as more than 150 suppressed studies with medium-size negative results would be necessary to reduce the distressing effect of unemployment to a small size.

# 6. Discussion

A summary of the results of the meta-analysis opens the discussion section.. In the next chapter, an in-depth consideration of several potential threats to the validity of meta-analyses is given. I discuss here whether and to what extent the results of the present meta-analysis on the mental health effects of unemployment might be distorted. Then, a comparison with other meta-analyses follows. The relevant questions here are: Is there agreement or disagreement between the meta-analyses, and if disagreement must be stated, what may be the reasons? A more detailed analysis concerning some specific findings is followed by a description of some important research gaps. Finally, political and societal implications of the present findings are discussed.

## 6.1. Summary of results

The meta-analysis revealed that the effect of unemployment on mental health is of medium size ($d$ = 0.51 after exclusion of outlying studies). The proportion of persons in danger of mental health problems among the unemployed (34%) is more than double the proportion among the employed (16%).[14] This finding shows that this medium effect size of $d$ = 0.51 has a strong practical significance. Five of the six indicator variables of mental health examined here (mixed symptoms of distress, depression, anxiety, subjective well-being, and self-esteem) were similar to each other with average effects of medium size ($d$ = 0.38 to $d$ = 0.52). Only the sixth indicator variable, psychosomatic symptoms, differed from the others with a small effect size ($d$ = 0.11). Thus, unemployment obviously has a rather global effect upon mental health.

Among the demographic variables, only gender and occupational status emerged as moderators of the unemployment-distress relationship. The effect sizes were larger in male compared to female groups and in blue-collar compared to white-collar groups. Furthermore, long-term unemployment had a significantly stronger effect on mental health than short-term unemployment. Thus, long-term unemployed male blue-collar workers represent a group of persons who are particularly threatened by the negative mental health effects of

[14] "*In danger*" because the screening tests used in most primary studies do not provide definite diagnoses of mental health but only provisional information needing further validation.

unemployment. Minority groups were found to be more strongly affected than majority groups, but this effect emerged only as a marginally significant trend after controlling possibly confounding design characteristics and the exclusion of outlying studies. The results for level of education were in the expected direction but not significant. For marital status, age, and year of data collection, no signs of a moderation effect were found.

A few theory-driven tests for interaction effects between gender and other variables that were conducted did not result in significant findings. Tests for curvilinear associations revealed a marginally significant trend for unemployment duration (inverted u-shaped). However, after exclusion of a single study that combined a very long average duration of unemployment with a medium effect size, this curvilinear association disappeared. For age, no signs of a curvilinear association with the effect sizes could be found.

For 17 out of the 26 examined countries, a negative mental health effect of unemployment was demonstrated, while for the remaining 9 countries the database was too small to enable such a demonstration. In economically developed countries the effect sizes were significantly lower than in less developed countries. In countries characterized by substantial income inequality the effect sizes were significantly larger than in more egalitarian countries. In countries with a high level of unemployment protection the effect sizes were smaller than in countries with a medium or low level of unemployment protection. With regard to the last variable, the effects were not stable after the application of measures to control some design characteristics, but this lack of stability might have been the results of low power due to the small number of samples involved in this analysis. For labor market favorability only one out of four analyses revealed a significant finding with stronger effect sizes in unfavorable labor markets. Culture (individualism/collectivism) also emerged as a significant moderator only in a minority of analyses (exclusion of outliers but no control of design characteristics). Thus, the result that unemployment is more distressing in unfavourable labor markets and in individualistic societies obviously needs further replication.

An analysis conducted in order to check whether the main result is robust showed that even combinations of positive values of several moderator variables usually did not reduce the negative mental health effect of unemployment to non-significance. Thus, the effect indeed is rather robust and generalizes across diverging life situations.

The longitudinal analysis revealed strong support for the hypothesis that unemployment causes distress: For employed persons who lost their jobs, a significant increase in distress was found ($d = 0.19$). For unemployed persons who found new jobs, a significant decrease in

distress was found ($d$ = -0.35). School leavers who found employment after school showed a significant improvement in mental health ($d$ = -0.41). And school leavers who became unemployed showed deterioration in mental health, although this result was not significant ($d$ = 0.10). Thus, changes in a person's employment status are accompanied with changes in mental health in the expected direction, a finding that clearly endorses the social causation hypothesis.

Despite the lack of status change, continuously employed persons showed a weak but significant improvement in mental health between T1 and T2 ($d$ = -0.06). The same result emerged for persons who were pupils or students at both measurement times ($d$ = -0.15). These results might be interpreted as a general trend toward feeling better in research designs that imply repeated testing. If this interpretation is correct it could help to explain the finding that the changes in distress that were associated with becoming employed were larger in their absolute magnitude than the distress changes that were associated with becoming unemployed. The general trend toward feel better may have enhanced the distress reduction associated with becoming employed and diminished the increase in distress that is associated with becoming unemployed. This general trend may also help to explain why continuously unemployed persons did not show the expected deterioration of mental health between T1 and T2 ($d$ = 0.04).

Cross sectional comparisons at the first measurement point of longitudinal studies resulted in findings that endorse the assumption of health related selection effects in the labor market: Continuously employed persons had better mental health than those employed persons who would soon lose their jobs ($d$ = 0.23). Unemployed persons who would soon find new employment had better mental health than unemployed persons who were prone to remain unemployed until T2 ($d$ = 0.15). And school leavers who would find a job after school had better mental health than school leavers who would find no job and become unemployed ($d$ = 0.08). Thus, the more mentally healthy individuals always had advantages in the labor market. Yet, although these effects were significant, they were of very small size, showing that selection effects in the labor market may be of little practical importance.

The sensitivity analysis revealed three outlier studies. Among the design characteristics only the questioning format and the amount of part-time employees in the comparison group had a significant influence on the magnitude of the effect sizes with larger effects for studies using oral interviews in comparison to written tests and studies with low proportions of part-time employees in the comparison group. A language bias was also found for studies from

German-speaking countries. Studies written in English included larger effect sizes than studies written in German when they came from Germany, Austria, or Switzerland. The way of publication, the thematic focus (unemployment main topic or minor topic), the operationalization of unemployment, and whether the comparison groups consisted of formerly unemployed persons or not did not influence the magnitude of the effect sizes. The results of these analyses were used to apply design controls for the moderator tests in order to check the stability of the results of these analyses.

The visual inspection of the funnel plot revealed signs of an existing yet not very strong publication bias. The "Trim and Fill"-method resulted in a rather small estimate of the number of suppressed studies. The rank-correlation test was highly significant, albeit the resulting Tau-correlation between the effect sizes and their variances was weak (Tau = 0.11). In sum, the sensitivity analysis showed that the present dataset is biased, but not substantially. The fail-safe-*N* is large enough to conclude that the true effect sizes for the association between unemployment and distress is very likely to be of medium size.

## 6.2. The validity of the meta-analytic results

Threats to the validity of a research synthesis arise from two important sources: (1) the way the primary studies were conducted, and (2) the way the research synthesis itself was conducted (Matt & Cook, 1994). Several of the common validity threads that often burden meta-analyses have been neutralized by the methods employed here. The problem of statistical dependencies among the effect sizes, for example, was solved by computing a composite effect size for each sample with the formula provided by Rosenthal and Rubin (1986). Other threats may have remained problematic, however. In the present section I will discuss some threats to the validity of the meta-analytic results that are particularly relevant to the present work.

### 7.2.1. Representativeness with regard to the population

One weakness of the studies meta-analysed here may be seen in the frequent use of convenience samples of unemployed or employed persons. The most common method of recruitment of unemployed participants was to directly approach them in an employment office or a similar institution. It could be argued that the use of such convenience samples is a

threat to the validity of the meta-analytic results as it is not clear how similar the persons examined in the primary studies were to the general population of persons who are in the labor force. Thus, the generalizability of the meta-analytic results may be questionable. However, in my opinion this problem is less severe than it might look at first glance for two reasons: (1) Variation of research designs (including sampling techniques) is a particular strength of meta-analysis, enhancing the robustness and generalizability of the findings (Rosenthal & DiMatteo, 2001). As a meta-analysis combines the results of several single studies, convenience sampling should only be a problem when a specific technique, which is possibly problematic, is used repeatedly by several researchers. The only method of convenience sampling that was repeatedly used to find unemployed participants was the one involving making direct contact in employment centers or similar institutions. Unemployed persons that can be found in such places may be more interested in employment than other persons who are registered as unemployed. As employment commitment is positively correlated with distress among the unemployed (Paul & Moser, in press), the persons in employment centers may be more distressed than persons who are registered as unemployed but can not be found in employment centers, leading to a slight overestimation of the true effect sizes in the present meta-analysis. However, an active search for a job is a defining characteristic of unemployment, rendering the persons who actively seek a job in such centers particularly prototypical of the group of persons studied here. Thus, the results found here may be of limited generalizability to unemployed persons who are not really interested in employment. However, it is questionable whether such persons should be called "unemployed" at all (see chapter 2.2.).[15] (2) Furthermore, although convenience sampling was frequent in the research field, it was predominantly used among the smaller studies with few participants. Large studies with several hundreds or thousands of participants were usually done with much more methodological rigor with regard to sampling techniques, ensuring generalizability to a larger population such as the population of a city or a whole country. As meta-analytic weights are negatively correlated with sample size, not only in fixed but also in random effects models such as the one used here, the large studies with representative

[15] For the sampling of employed persons, one method that was employed comparatively frequently but may be seen as problematic was the use of formerly unemployed persons who had found new jobs as an employed comparison group. Yet, as the mean effect size of such studies did not differ from the mean effect size of the other studies (see results section), this method of sampling is unlikely to threaten the validity of the meta-analytic results.

samples influenced the results of the meta-analysis much more than the small studies, neutralizing problems of convenience sampling to a considerable degree.

### 6.2.2. Restriction of range in the dependent variable

Related to the question of representative sampling is the problem of a possible restriction of range in the dependent variable. Such a range restriction may also threaten the validity of meta-analytic results (Hunter & Schmidt, 1990; Matt & Cook, 1994), and there are some hints that it may exist in the field of unemployment research. Several authors reported that, according to the impression of the interviewers, unemployed persons who showed signs of particularly strong distress were less willing to participate in the research study than other unemployed persons who appeared to feel better (e.g. Kaltseis, 1987; Kieselbach, 1987). Thus, those persons who suffer most from unemployment possibly were underrepresented among the samples meta-analysed here. This would lead to an artificial attenuation of the mean distress level of unemployed samples, reducing the difference between unemployed and employed persons. However, the standard deviation of the distress variable is also likely to be reduced when those who suffer most decline to participate. This would enlarge the effect size artificially, as the denominator of the effect size formula is attenuated (Matt & Cook, 1994). The second effect concerning the standard deviation is likely to be smaller than the first one concerning the mean difference. However, the statistical consequences of such a range restriction in the dependent variable are not really clear. Furthermore, it is not clear whether the phenomenon exists at all and how strong it is, i.e. whether the most distressed unemployed persons really are underrepresented in many studies and how large this under-representation is. At the moment, we are restricted to anecdotal evidence regarding this problem and no definite conclusion can be drawn.

### 6.2.3. Measurement of unemployment

Some samples of “unemployed” individuals that I encountered during the literature search were rather heterogeneous, including homemakers, persons in education, and other people who are usually not considered to be typical examples of unemployed persons. This problem is especially pressing among female samples where the states of unemployment and homemaking/being out of the labor force are often not easy to distinguish (Warr & Parry, 1982). In order to address this problem, four categories were formulated to ensure the

homogeneity of the unemployed sample, and each study included in the meta-analysis was required to match the conditions of at least one of these categories (see chapter 4.3.1.). The great majority of the studies fulfilled the requirements of more than one of the four categories. However, unemployment is a complex, multidimensional concept, and none of these categories is able to guarantee a perfect fit of each group member to the standard definition of unemployment. For example, even when all members of a group are officially registered as unemployed, some may not really be looking for work, throwing them out of the traditional conceptualization of unemployment (see Pernice, 1996, for a deeper empirical analysis of this problem). Therefore, none of the samples used in the present meta-analysis may have been a perfect operationalization of "pure" unemployment. As persons who are out of the labor force usually feel better than unemployed persons do (see Paul & Moser, in press for a summary of empirical results on this topic), such imperfect operationalizations of unemployment are likely to cause an underestimation of the true difference between employed and unemployed persons with regard to mental health. Thus, the true effect size may be even slightly larger than the $d = 0.51$ I found in the present meta-analysis.

### 6.2.4. Measurement of mental health

Mental health is a difficult and elusive concept (Jahoda, 1988). Therefore it is important to discuss the operationalization used in the meta-analysis to make clear for which kinds of "mental health" from the results presented validity can be claimed. While mental health traditionally was interpreted as the lack of symptoms of distress, more contemporary approaches emphasize the positive aspects of mental health, such as autonomy and integrated functioning (Warr, 1987). Most researchers in the field of unemployment research implicitly conceptualized mental health as a continuum, measured either by an overall scale of distress such as the GHQ or by one or several scales for sub-constructs, such as depression, anxiety, and psychosomatic symptoms. Some authors also added scales localized more at the positive pole of mental health, e.g. self-esteem and life satisfaction. In sum, the negative aspects of mental health predominate in the research meta-analysed here, although positive aspects were also represented. The meta-analysis of the intercorrelations proved that all six indicator variables selected for the meta-analysis are highly correlated with each other and tap a common underlying factor that can be called "mental health" in my opinion.

### 6.2.5. Confounding variables

There are limits to the extent to which non-experimental, correlational analyses can address complex issues of causation (Fergusson, Horwood, & Woodward, 2001). For example, the possibility exists that confounding factors may influence both employment status as well as mental health, creating a spurious correlation between unemployment and distress. One possible confounding factor - physical health - has been repeatedly discussed in the literature (e.g. Winefield, 1995) and may be of particular importance for the question of possible mental health effects of unemployment. Physical illness, especially prolonged illness, may lead to job loss and may at the same time impair the mood of the ill person. However, persons with severe illnesses are usually judged as not available to the labor market and, therefore, do not count as unemployed. As a consequence, in studies measuring both mental health as well as physical health, differences between employed and unemployed persons with regard to physical health are usually weaker than differences with regard to mental health (e.g. Grobe, Dörning & Schwartz, 1999). This is mirrored in my own findings: The mean effect size for psychosomatic symptoms, a variable with an obvious physical component, was much weaker than the mean effect sizes for the "pure" psychological indicators of mental health. Furthermore, the direct effect of physical illness upon mental health appears to be weaker than common sense assumptions might expect, even in the case of such a severe illness as cancer, where surprisingly good coping has been found (Beutel, 1988; de Haes & van Knippenberg, 1985). Therefore, the confounding effect of physical illness upon the association of unemployment and mental health is likely to be limited within the general population. Since I excluded studies drawing samples from populations of medical institutions, this confounding effect should be limited in the present dataset and should not pose a danger to the validity of the results. Furthermore, factory closure studies can be seen as applying a quasi-experimental design, where a large group of employees is made redundant at the same point in time regardless of individuals' health status. The finding that the mean effect size for this kind of study did not differ from the mean effect sizes for the other studies gives an additional argument against the assumption that physical health may act as a confounding variable, severely threatening the results of the meta-analysis.

Another problem of possibly confounding effects threatened the moderator analyses. Highly intercorrelated moderator variables could have influenced each other and possibly caused misleading conclusions. As the high number of missing values precluded a complete

multivariate analysis, I checked the intercorrelation matrix for possibly confounding effects. Among the demographic variables, the only large intercorrelations that might have been problematic related to minority/majority status. Samples with large proportions of minority members were also characterised by a longer average duration of unemployment ($r$ = 0.45) and a lower level of education ($r$ = -.45), two conditions associated with elevated effect sizes. Thus, it may be possible that the moderator effect of minority/majority status, which was only a weak trend anyway, was caused by confounding effects of socioeconomic status and unemployment duration. However, these are only speculations at the moment and we must wait until more studies are available to come to a conclusion here.

At country level, there was also a problem of variable overlap. Income (in)equality and level of unemployment protection were strongly interrelated. Ten of the twelve countries with high levels of unemployment protection belonged also to the group of countries with low income inequality (see appendix C, list C-1). The overlap between unemployment protection and poverty was also considerable. Thus, these concepts were hardly distinguishable within the sample of studies used here and it is not possible at the moment to specify which of the two is more relevant as a moderator of the unemployment-distress relationship. Level of unemployment protection may be seen as an aspect of a society's level of income inequality with particular relevance for unemployed people. It may act as a mediator variable between inequality and unemployment distress. However, this assumption could not be tested with the present dataset.

### 6.2.6. Reliabilty of meta-analytic codings

With regard to the quality of scientific data presentation, terms such as "shocking" and "appalling" have been used (Orwin, 1994, p. 140). This renders meta-analytic data coding a difficult and possibly error-prone undertaking that often has to rely on assumptions and guessing. Therefore, the reliability of the data used in the present meta-analysis might have been less than optimal. Since only one coder was responsible for coding all the data, it is not possible to estimate the reliability of the codings with a single coefficient such as Cohen's kappa. However, every problem that was encountered and every important decision that was made during the coding process was carefully documented. A very detailed description of the coding process, including all relevant decisions made during that process, is available in the technical appendix.

Meta-analysts usually tell the reader only what was coded and how reliable it was coded in terms of a certain coefficient of interrater-agreement such as kappa, but they do not describe how the coding was done, and why it was done this way and not that way. In the case of the present meta-analysis the reader is able to draw his or her own picture about the coding process and to come to a more informed and independent conclusion regarding its appropriateness, although he or she is not provided with a single coefficient of interrater-agreement. In my opinion, this is also an acceptable solution to the reliability problem. If there were inaccuracies during the coding process, they should have produced only unsystematic errors.

### 6.2.7. Effect size transformations

Some effect size transformations relying on incomplete data are likely to produce underestimations of the true effect size. For example, when only verbal statements such as "no significant effect" were available, I coded $d = 0.00$, although the effect possibly was larger. Effect size coefficients transformed from information about the level of significance (e.g. "$p < .05$") are also likely to underestimate the true effect size (Matt & Cook, 1994). However, in most primary studies, better information was available and therefore the underestimation of the true mean effect size caused by the usage of the aforementioned kinds of information should have introduced only a rather weak conservative bias into the meta-analysis.

### 6.2.8. Publication bias and representativeness with regard to the research field

The sensitivity analysis showed that a few design features correlated with the magnitude of the effect size coefficients. Yet, controlling these features as well as excluding a small number of outlying studies usually did not change the results of the meta-analysis, proving the robustness of the results. The comparison of published and unpublished material did not result in a significant difference with regard to the average effect size, nor did the comparison of studies with unemployment as the main topic and studies with unemployment as a minor topic result in a significant moderator effect. Both results do not favor the assumption that a publication bias exists in the field of psychological unemployment research. Duval and Tweedie's (2000a, 2000b) "Trim and Fill" method also did not favor this assumption. However, Begg and Mazumdar's (1994) rank correlation test was significant, indicating that

at least a small publication bias exists. Nevertheless, analyses with the fail-safe N statistic (Orwin, 1983) showed that it is unlikely that there are so many suppressed studies that their inclusion, if available, could reduce the mean effect size to a small effect of $d = 0.20$. Yet, some doubts remain concerning the exact size of the difference between unemployed and employed persons with regard to their mental health. The language bias that was identified here is more worrying in my opinion because it means that the results from German–speaking nations that have been published in English, exaggerate the true amount of distress that accompanies unemployment in these countries. Possibly, unemployment researchers from Austria, Germany, and Switzerland prefer to use their "good" results, i.e. those with large effect sizes, for international publications, while they publish the "bad" results in German. An alternative explanation could be that results from German-speaking countries generally have a small chance of acceptance in English journals, and are only accepted when they include particularly "good" results. While I can only speculate about the reasons for this language-bias phenomenon, it is clear that a monolingual English-speaking meta-analyst would have no chance of retrieving a dataset that adequately represents the research results from these countries. As we do not know whether such a bias also exists in other cultures, this finding casts some doubts on all the mean effect sizes from non-English speaking countries presented here. It also shows the usefulness of cross-cultural meta-analyses, where a meta-analyst is able to retrieve, integrate and compare the results of different research cultures on the same topic. English-written scientific literature may be seen as a rather selective sub-sample of all existing research. With regard to some research topics, e.g. the effects of unemployment on mental health, it might be important to retrieve and use research that is not published in English, too.

### 6.2.9. Summary concerning threats to validity

Convenience sampling might lead to a slight overestimation of the results if the persons who can be met in employment centers are uncharacteristically strongly committed to employment. Whether a range-restriction in the dependent variable would lead to an under- or to an overestimation of the true effect size is not yet clear, although a slight underestimation is more probable similar to the effect of range restriction in correlational analyses. Lacking homogeneity of the unemployed sample would probably cause underestimation, as persons who are out of the labor force tend to feel better than unemployed people do. Some effect size transformations lead to a slight underestimation of the true effect size. Publication bias leads

to an overestimation of the true effect size. The other validity threats discussed here do not pertain directly to the problem of the true effect size, but to the question of causality (confounding variable) or conceptual appropriateness (measurement of mental health). In sum, forces toward an overestimation and toward an underestimation might cancel each other out, leaving only a weak trend in one of the two directions. Taken together, it is fair to say that $d = 0.51$ is close to the true effect size for the comparison of employed and unemployed persons with regard to mental health.

## 6.3. Comparison with results from other meta-analyses

With $d = 0.51$ ($k = 315$, $n = 209{,}379$), the mean meta-analytic effect size for the cross-sectional association of unemployment and mental health that was found in the present study is nearly identical to the result of d = 0.52 that was reported by MyKee-Ryan et al. (2005) based on a smaller number of samples ($k = 60$, $n = 21{,}735$). It should be noted here that MyKee-Ryan et al.'s (2005) meta-analysis was published when the statistical computations for the present meta-analysis were nearly finished. Thus, both of these meta-analyses can be seen as independent replications of each other. The fact that the average effect sizes are so close together is encouraging, especially as both meta-analyses used different meta-analytic methods, the Hunter and Schmidt-method in the case of MyKee-Ryan et al. (2005), and the Hedges and Olkin-method in the present meta-analysis.

Both newer meta-analyses, i.e. the present one and the one published by McKee-Ryan et al. (2005) found average effect sizes that were considerably larger than Foster's (1991) effect size. He reported an (unweighted) average mean difference of $d = 0.19$. However, Foster's (1991) effect size estimation may have been artificially reduced by his rather inclusionist meta-analytic approach (Kraemer, Gardner, Brooks, & Yesavage, 1998): While the usual design in unemployment research consists of comparisons between groups of employed and unemployed persons, Foster (1991) also included studies in his meta-analysis that correlated job insecurity among *employed* persons with the distress levels reported by the same persons. He also included studies that compared *spouses* of employed and unemployed persons with regard to their levels of mental health. Thus, more indirect consequences of unemployment among groups of persons who were not unemployed themselves were mixed with the direct effects of unemployment. Furthermore, Foster (1991) incorporated infrequently used measures of mental health, such as the number of "bed days" (p. 160). These methods of

operationalizing unemployment and mental health may have caused an underestimation of the true effect size in his meta-analysis.

In addition to their effect size for global mental health, MyKee-Ryan et al. (2005) also reported an average effect size for a more specific variable, life satisfaction, that is similar to subjective well-being as examined in the present study. The average effect sizes were $d = 0.44$ ($k = 7$, $n = 1{,}249$) and $d = 0.51$ ($k = 68$, $n = 40{,}985$) in the MyKee-Ryan et al.'s (2005) meta-analysis and in the present meta-analysis, respectively. Again, the results are in good agreement with each other.

Other results that could possibly be compared were the effect sizes for psychosomatic symptoms from the present meta-analysis ($d = 0.11$, $k = 41$, $n = 13{,}857$) and the effect sizes for "subjective physical health" ($d = 0.41$, $k = 3$, $n = 1{,}136$) and "objective physical health" ($d = 0.89$, $k = 3$, $n = 484$) as reported by MyKee-Ryan et al. (2005). These results are not in strong agreement, with MyKee-Ryan et al.'s (2005) effect sizes being much larger than the effect sizes found here. However, the database for the results concerning physical health was rather small in MyKee-Ryan et al.'s (2005) meta-analysis and the effect size estimates might not be stable yet.

Weak agreement also exists for the longitudinal results. While Murphy and Athanasou (1999) reported effect sizes of $d = 0.36$ for the change from employment to unemployment ($k = 5$), MyKee-Ryan et al. (2005) reported an average effect size of d = 0.35 (k = 10, n = 660) for this kind of change. With $d = 0.19$ ($k = 19$, $n = 1{,}933$), the result of the present meta-analysis was lower. For the change from unemployment to employment Murphy and Athanasou (1999) reported and effect sizes of $d = -0.54$ ($k = 10$) and MyKee-Ryan et al. (2005) reported an average effect size of $d = -0.82$ ($k = 19$, $n = 1{,}911$).[16] The result of the present meta-analysis was $d = -0.35$ ($k = 45$, $n = 4{,}513$). Again, the result of the present meta-analysis was clearly smaller than that of the other two meta-analyses. Thus, the question arises as to why the present meta-analysis reported smaller longitudinal effects than the older meta-analyses. One explanation for the diverging findings might be the fact that the present meta-analysis included dissertation theses and other unpublished material while the other meta-analyses did not. Possibly, the research quality in such unpublished material is weaker, resulting in attenuated effect size estimations. However, I did not find differences between published and

[16] Note that this effect size estimate for the longitudinal effect was considerably larger in magnitude than McKee-Ryans et al.'s (2005) estimate for the cross-sectional mean difference of $d = 0.52$!

unpublished studies in the cross-sectional analysis. Furthermore, the other results of the MyKee-Ryan et al. (2005)-study are in strong agreement with results of the present meta-analysis, although MyKee-Ryan et al. (2005) restricted themselves to published material in all analyses. Therefore, I believe that the reason for the lack of agreement with regard to the longitudinal results is likely to have other reasons.

One alternative explanation for the differences between the meta-analyses with regard to the longitudinal effects might be seen in the operationalization of mental health. Murphy and Athanasou's (1999) meta-analysis was clearly dominated by the GHQ, a measure that can be assumed to be rather sensitive to change because of its atypical answering format. The answer "no more than usual" receives a low scoring, while "rather more than usual" and "much more than usual" both receive a high scoring in the GHQ. Thus, chronic conditions may be under-weighted by the GHQ. In the MyKee-Ryan et al. (2005)-meta-analysis, the GHQ also played an important role together with typical measures for depression and anxiety. In contrast, the influence of the GHQ on the results of the present meta-analysis was less strong, as the conceptualization of mental health was broader and more alternative measures of mental health were included. Some of them, particularly self-esteem scales and measures of psychosomatic symptoms, might need a comparatively long time in order to change in response to changes in a person's employment status, lowering the magnitude of the average effect sizes for longitudinal data. However, these assumptions are only partly in accordance with empirical data, as an inspection of the change effects computed separately for each mental health indicator shows (appendix B, table B-3). For changes from employment to unemployment, the mean effect size for mixed symptoms, i.e. the variable that includes the GHQ, was $d = 0.38$, very similar to the effect sizes reported in the other meta-analyses. Yet with $d = -0.40$, the mean effect size for mixed symptoms with regard to reemployment still was clearly smaller than the mean effect sizes reported in the other analyses. Thus, the broad conceptualization of mental health that was used here could only partly explain the differences in the mean change effects between the three meta-analyses on the mental health effects of unemployment.

Another alternative explanation could be the meta-analytic method used. Meta-analysis of longitudinal data is more complicated than meta-analysis of cross-sectional data. In particular, one must decide whether to use the raw-score effect size or the repeated-measures effect size (Morris & DeShon, 2002). In the present meta-analysis, repeated measures effect sizes were used. Furthermore, the T1-T2-correlation is often not reported in the primary studies but necessary for the computation of repeated measures effect sizes as well as for the computation

of the sampling variances of the effect sizes (Morris & DeShon, 2002). In the present study I dealt with this problem by meta-analysing the retest-correlations first and using the average retest-correlation as an estimate for further computations. As these average retest-correlations were lower than $r = 0.50$, the resulting repeated-measures effect sizes are lower than the respective raw-score effect sizes would have been (Morris & DeShon, 2002), albeit the resulting difference in magnitude is supposed to be rather small. However, neither Murphy and Athanasou (1999) nor MyKee-Ryan et al. (2005) described the methods they used with regard to these problems. Therefore, it is not yet possible to decide how appropriate the methods applied in the three meta-analyses were and which results might be the best estimates of the true longitudinal effects.

Furthermore, with regard to the question of selection effects, there was another disagreement between the studies. While McKee-Ryan et al. (2005) found a non-significant effect size of $d = 0.09$ ($k = 9$, $n = 5{,}135$) for the effect of distress upon the probability of reemployment among unemployed persons, the effect size that was found in the present meta-analysis was $d = 0.15$ and was significant ($k = 49$, $n = 13{,}259$). Thus, with regard to its magnitude both average effect sizes were similar; both were rather small, but one was significant while the other was not. It is likely that this difference in findings was the result of the apparent differences in power between both meta-analyses.

With regard to the results of moderator tests there is agreement as well as disagreement between the meta-analyses. In contrast to Murphy and Athanasou's (1999) results, the moderator effect for gender was highly significant in the present analysis with the magnitude of the effect sizes being negatively correlated with the percentage of females in the sample. Probably the lack of a significant finding in the Murphy and Athanasou (1999) study is due to the very low power of the moderator tests conducted by these authors (only nine studies involved). Country differences were not found by Murphy and Athanasou (1999) but were found in the present analysis. However, Murphy and Athanasou (1999) grouped the countries by another principle (Anglo-saxon vs. European) than I did, rendering the analyses non-comparable. McKee-Ryan et al.'s (2005) moderator tests for the unemployment rate was not significant. The present meta-analysis was also not successful in demonstrating such a moderator effect. McKee-Ryan et al.'s (2005) moderator tests for length of unemployment (< 6 months vs. >= 6 months) was successful, as was mine. Thus, both meta-analyses agree that longer unemployment is associated with greater distress than shorter unemployment, while the unemployment rate has no moderating influence. McKee-Ryan et al.'s (2005) test for a

moderating influence of the level of unemployment protection was not significant. In contrast to that, the same moderator test was clearly significant in the present meta-analysis, together with moderator tests for income inequality, a variable highly correlated with the level of unemployment protection. Again, differences in power might be an explanation for the disagreement between the meta-analyses.

McKee-Ryan et al. (2005) found a significant moderator effect for study type (school-leavers vs. adults) with larger effect sizes among the school-leaver samples than among adult samples. As the former group of persons can be expected to be considerably younger than the latter group of persons, this result could possibly be interpreted as indirect evidence for a moderating effect of age. However, the direction of the result is surprising, as younger groups of persons are usually not expected to suffer more from unemployment than adults. Furthermore, neither Murphy and Athanasou (1999) nor the present meta-analysis were able to demonstrate a direct significant moderator effect for age. Thus, McKee-Ryan et al.'s (2005) finding is slightly puzzling but might have to do with special design characteristics of the small sample of school-leaver studies in their meta-analysis ($k = 12$).

In sum, with regard to the cross-sectional analysis there is usually strong agreement between the results reported in the three more recent meta-analyses. Some instances of disagreement may be the result of differences in statistical power. With regard to the longitudinal effects there is less agreement, as the average effect sizes differ considerably between the studies. However, all three meta-analyses that analysed longitudinal data came to the conclusion that changes in employment status are associated with significant changes in mental health that clearly endorse the assumption that unemployment has a causal effect on mental health (social causation).

Several of the findings in the present meta-analysis were completely new and can thus not be compared with other results. The most important were: The mean cross-sectional effect sizes for depression, anxiety, and self-esteem; the case rates for psychological disorders among unemployed and employed persons; several moderator analyses, e.g. the analyses for marital status, socioeconomic status, minority/majority status, economic development, income inequality, and individualism/collectivism; the curvilinear moderator tests; the tests for interactions of moderator variables; the computation of an average effect size for each country; the longitudinal effects sizes for school leavers who became employed or who became unemployed after school, the longitudinal effects sizes for continuously employed and continuously unemployed persons and for persons who stay in the educational system; the

tests for selection effects of mental health on job loss among employed persons and the tests for selection effects on the post-school employment status of pupils; and, finally, the tests for publication bias and related threats to the validity of the meta-analytic results. For all these new results we have to await meta-analytic replications in order to learn more about the stability of our findings.

## 6.4. Discussion of some specific findings

While five of the six indicator variables of mental health had comparable effect sizes, the sixth indicator variable, psychosomatic symptoms, had a considerably smaller average effect size than the others. One explanation for this difference in findings might be the close relation of psychosomatic symptoms to physical health. Indeed, scales measuring psychosomatic symptoms consist mainly of widespread, unspecific bodily symptoms such as headaches and back pain. As a consequence of these results, it could be speculated that unemployment affects mental health to a larger degree than it affects physical health. This conclusion would be in sound agreement with the results of a large-scale analysis of German health insurance data, where unemployment strongly increased the frequency of mental health diagnosis but elevated the frequency of diagnoses of physical illnesses only to a limited degree (Grobe, Dörning, & Schwartz, 1999). However, before such a far-reaching conclusion is drawn, alternative explanations should be examined and - if possible - refuted.

One possible explanation for the small average effect size might be found in the quality of the measurement of this variable. With 27%, a comparatively large proportion of studies used ad-hoc measures that were constructed by the study-authors themselves. Thus, the measurement of psychosomatic symptoms might not have been as reliable and valid as the measurement of the other variables. However, the measurement of subjective well-being appears to be much more problematic, with more than half of the measures being either self-constructed ad-hoc scales or single item measures. Yet, despite this fact the average effect size for subjective well-being was much closer to the overall average effect sizes than the average effect size for psychosomatic symptoms. Furthermore, with a mean internal consistency of alpha = 0.82 the reliability of the measures for psychosomatic symptoms was very similar to the mean reliabilities of the other indicators of mental health that ranged from 0.78 for self-esteem to

0.87 for mixed symptoms of distress. Therefore, it is unlikely that measurement problems explain the unusually weak effect size for psychosomatic symptoms.

An inspection of the database shows that one study with a large sample size (four subsamples, overall $n$ = 2,517) in combination with small effect sizes ($d$ = -0.38 to 0.02) had a strong influence on the result of the analysis for psychosomatic symptoms (Brinkmann & Potthoff, 1983). The small effect sizes might be a consequence of the very short mean unemployment duration that characterizes this study (5.5 weeks). Possibly, physical symptoms due to unemployment need more than six weeks' time to evolve than six weeks. However, when this study was excluded from the dataset, the resulting average effect size for psychosomatic symptoms still was small ($d$ = 0.15). Thus, the unusually short duration of unemployment in the Brinkmann and Pothoff (1983) study is also not a sufficient explanation for the small average effect size for psychosomatic symptoms. In sum, no convincing alternative explanation for this small average effect sizes was found, allowing for the retention of the assumption that unemployment affects mental health and physical health differently.

Another result that deserves a detailed discussion is the finding of small but significant selection effects for the way into and out of unemployment in longitudinal studies. Persons with good mental health were more successful in the labor market than persons with impaired mental health: Unemployed people who found jobs later on already had better mental health at T1 than those unemployed persons who remained unemployed. Pupils and students who found a job after leaving school already had better mental health at T1 than those pupils who became unemployed after school. Furthermore, employed persons who stayed in their jobs had better mental health than those employed persons who lost their jobs subsequently. Thus, being in good mental health is followed by more positive events with regard to employment than being in poor health. It is reasonable to interpret this as a sign of selection effects. However, another interpretation is also possible: Human beings make plans and anticipate future events. With regard to the labor market this means that employed persons may perceive that their jobs are threatened, a perception that probably affects mental health in a negative direction. Indeed, such negative effects of the anticipation of subsequent job loss have been demonstrated already (Pelzmann, 1985; Schnall et al., 1992). A similar effect may exist among the unemployed: They evaluate their chances in the labor market, and may possibly even be good at this task. However, probabilities of reemployment that individuals correctly estimate to be small or negligible are likely to cause resignation and impaired well-being. Thus, those who will become long-term unemployed may already experience elevated distress levels in earlier phases of unemployment due to the anticipation of the problems they will

encounter during their job-hunt. As it is not clear how strong such anticipation effects might be, it appears to be reasonable to interpret the T1-differences that were found in the present meta-analysis as *upper limits* of possible selection effects on the labor market.

## 6.5. Research gaps

While little doubt remains that unemployment is not only associated with distress but is a direct cause of distress, the mechanisms that mediate that association are not well known as of yet. Jahoda's (1981, 1982, 1997) latent deprivation model, Warr's (1987) vitamin model, and Fryer's (1986, 1997a) agency approach were frequently cited, but empirical evidence in favor of these models is not as abundant as one might expect for such renowned theories. Jahoda's model has received some empirical support recently with several authors having been able to show that employed persons have more access to Jahoda's latent functions and that the amount of access to theses functions correlates with mental health (e.g. Brief, Konovsky, Goodwin, & Link, 1995; Creed, Muller, & Machin, 2001; Creed & Reynolds, 2001; Creed & Watson, 2003; Isaksson, 1989; Waters & Moore, 2002a; Evans, 1986). Warr (1987) cited rich empirical evidence from older studies when he formulated his model, but in most cases this evidence did not involve unemployed persons. After its formulation, the vitamin model has rarely been tested directly, and when it has, it was usually tested solely with employed persons (De Jonge & Schaufeli, 1998; Jeurissen & Nyklicek, 2001; for a test with unemployed persons see Jackson, 1999). Empirical evidence in favor of the agency restriction approach is mainly limited to some qualitative studies at the moment (Fryer, 1997a; Fryer & Payne, 1984; see also Fryer & McKenna, 1984 for a quantitative study). Thus, while there are hundreds of studies that show that unemployment is correlated with distress, and dozens of longitudinal studies that show that this distress is not only correlated with, but actually caused by unemployment, we have less understanding of the mediating mechanisms of this effect. For example, the question of whether poverty is a stronger or a weaker mediator of unemployment distress than the lack of the latent functions of employment as specified by Jahoda (1981, 1982, 1997) is still not answered in an unequivocal way. Therefore, further research on the mediating mechanism of unemployment distress clearly is needed. To develop and test new theories might also be a promising agenda for future research aspirations. Paul and Moser (in press), for example, recently proposed the incongruence hypothesis of unemployment distress that is not based on the fact that unemployed people do not work, as

Jahoda's (1981, 1982, 1997) and Warr's (1987) theories are, but on the desire for work that is prototypical for unemployed persons (referring to the common definition of unemployment and its "search for work"-criterion). This hypothesis states that unemployed people are a group of persons characterized by a strong commitment to employment, similar to employed persons. However, in contrast to employed persons, they find themselves in a situation of incongruence between their values (high commitment for work) and the reality (no work). This incongruence between values and life goals on the one hand and the reality on the other hand is hypothesized to be one of the reasons for the distress unemployed persons experience. Paul and Moser (in press) also meta-analysed empirical evidence endorsing this hypothesis.

Furthermore, comparisons of unemployed persons with persons who are out of the labor force, for example homemakers and students, are also promising research themes. The reason is that unemployment is not simply non-employment, as unemployed persons look for work, while other persons who are out of employment such as students or homemakers usually do not. Yet, persons who are out of the labor force typically feel better than unemployed persons do (Paul & Moser, in press). Therefore, it might be interesting to complement the research field by comparing unemployed persons with persons who are out of the labor force, in order to learn more about the specifically distressing features of unemployment that are distinct from being in or out of paid work.

Some important and interesting groups in the labor market have also not yet received the attention they deserve. Additional primary studies that examine these special groups could be very valuable. I initially intended to conduct a moderator analysis for parental status, for example. But I later abandoned this plan. One of the reasons was that I learned that only a minority of studies reported the necessary information for such an analysis. And rather few studies were especially concerned with the problems of unemployed parents, though they may be serious, particularly for impoverished single mothers and for males who stick to their traditional role as a family provider. The "family provider"-argument is often cited in the research field, yet empirical data on this topic are not abundant. Studies with older unemployed workers are also rare, although most industrialized societies experience demographic changes that will lead to a higher proportion of elder persons in the labor market in the near future. Furthermore, I was interested in a moderator analysis comparing unemployment in urban versus rural environments, but I found only very few studies with rural samples, rendering a moderator analysis pointless. Nevertheless, this might be an interesting research theme in my eyes, as the countryside represents something like a "different world" within industrialized societies and it might be helpful to study

unemployment there in order to get a better understanding of unemployment in urban surroundings. There is also a lack of studies from non-Western countries in the English-written literature, limiting the generalizability of meta-analyses to three continents: North America, Europe, and Australia/ New Zealand. However, studies from non-Western countries, especially from countries where values concerning work and employment differ from Western ones, could be very helpful for the understanding of negative mental health effects of unemployment. Another promising topic could be the examination of unemployed persons from particularly stressing jobs, aiming at better knowledge about the robustness of the negative mental health effect of unemployment in extreme groups. Does unemployment lead to distress even when the job you lost was really bad with regard to working conditions, income, and other important characteristics? Furthermore, studies that are explicitly concerned with minority groups are rare, too, although they are very promising as they enable researchers to do cross-cultural research within one society. In addition, minority groups are often disadvantaged in the labor market and are therefore a group that deserves special attention by unemployment researchers as they carry a larger part of the burden of high unemployment figures than do other groups. In sum: We already have many studies about "the unemployed", but unemployment among several special yet important groups is not well understood up to now.

Furthermore, the moderator effects that were analyzed in the present meta-analysis are also in need of further scrutiny. Some of them emerged only as weak trends (e.g. educational level), leaving some doubts whether they really exist. To resolve these doubts, more primary studies are needed. Other moderating effects undoubtedly exist, but the question of their exact structure and meaning is not completely answered yet. Gender, for example, emerged as a highly significant moderator of negative mental health effects of unemployment. Yet this only means that the difference between unemployed and employed persons with regard to mental health is larger among males than among females. We do not know from this result whether this difference in effect sizes arises because unemployed males suffer more than unemployed females, or whether it arises because males feel better when they have a job than females feel when they have a job. Both possibilities have rather different implications. In the first case, unemployed males would be identified as a group of persons in need of special assistance. In the second case, the moderator effect could simply be seen a result of the fact that men on average still have better jobs in most Western societies than females have, improving male average mental health in comparison to female average mental health. To get a more complete picture of the moderator effect, meta-analytic comparisons of unemployed males and

unemployed females on the one hand and comparisons of employed males and employed females on the other hand are required. McKee-Ryan et al. (2005) already took the first step here and meta-analyzed correlations of mental health with several demographic variables within the group of unemployed persons. According to their results, unemployed males feel significantly better than unemployed females do, although the effect is weak ($r = 0.09$, $k = 14$, $n = 6{,}763$). This correlation supports the second aforementioned explanation for the differences in the effect sizes between males and females that I found here. Thus, the moderator effect for gender appears to result primarily from male mental health advantages in employment, while there are only weak (and unexpected) differences between male and female mental health in unemployment. In other words: Unemployed men experience a deeper fall than unemployed women because they stood higher when both were employed. More such detailed analyses would be very helpful for the other moderators, too. [17]

Recently, the concept of underemployment was proposed with the intention of substituting the traditional dichotomous measure of employment status (employment vs. unemployment) with a more continuous measure of the psychological adequacy of employment (Dooley & Catalano, 2003; Dooley, 2003; Jensen & Slack, 2003). Within this concept, discouraged workers, involuntary part time workers, low-wage workers, and persons characterized by an occupation-education-mismatch are categorized as underemployed, together with the unemployed. In my opinion the concept of underemployment is valuable as it reflects contemporary changes in the labor markets of the Western World where traditional full-time jobs are replaced more and more by part-time work, temporary work, and other forms of "atypical" employment. Because such forms of "atypical" employment might impair mental health, it is without doubt useful to study their psychological effects. However, unemployment is likely to be a stronger threat to mental health than "atypical" employment is (see, for example, Prause & Dooley, 2001, for empirical data endorsing this assumption). Therefore, research interest in underemployment should not substitute, but complement research interest in unemployment.

[17] McKee-Ryan et al.'s (2005) correlations of mental health with race, occupational status, and education among samples of unemployed persons are based on very small numbers of studies (all *k*s <= 10) and do not appear to be very stable yet. Theorefore, they will not be interpreted here.

## 6.6. Political and societal implications

The negative effect of unemployment upon mental health has a size around $d = 0.51$, meaning that the health level of unemployed persons is half a standard deviation below the health level of employed persons. This is an effect with considerable practical importance, as it is equivalent to an increase in the rates of persons with psychological problems with potential clinical severity from 16% to 34%. Abundant longitudinal evidence demonstrates that unemployment is not only associated with, but is highly likely to cause mental health problems. As unemployment is a widespread problem that affects several percent of the labor force in most countries at each moment of time, one can conclude that unemployment is an economic phenomenon with a considerable negative impact on public health. As a consequence, it can be expected that a country's health care system, particularly its mental health-branch, will be more strained when unemployment is high than when unemployment rates are low.

Therapists and other members of the health care system may be inspired by the results presented here to reconsider their therapeutic strategies for persons without jobs. As unemployment itself is likely to cause distress, the employment situation should not be underestimated in comparison to other potentially pathogenic factors (e.g. problematic intimate relationships), and it should be taken into account in the treatment of persons with psychological problems. Sometimes it may even be good advice to incorporate elements of outplacement counseling into the treatment of clients who are unemployed.

A result from the present meta-analysis that gives some hope is the finding of a complete recovery after reemployment. Thus, the suffering that is caused by mass unemployment would disappear if unemployment could be eradicated. Regrettably, the present meta-analysis is of little help with regard to the question of how this goal could be achieved, as this is an economic question that cannot be answered by psychologists. Nevertheless, one result of the meta-analysis is that the psychological "costs" of unemployment are immense, possibly an interesting finding for economic theorists.

Another hopeful result is the finding that psychological interventions for unemployed people emerged as rather effective. Some interventions achieved improvement in mental health near $d = 1.00$, which is impressingly large (e.g. Creed, Machin, & Hicks, 1999; Harry & Tiggeman, 1992; Proudfoot, Gray, Carson, Guest, & Dunn, 1999; Saam, Wodtke, & Hains,

1995). Due to their costs, such interventions will always be the privilege of a minority of unemployed persons, though (see Fryer, 1999, for a discussion of the problems of psychological interventions for unemployed people).

One important question that arises in view of the results of the present meta-analysis is whether employment is *always* better than unemployment and whether political measures may be justified that try to combat unemployment by the creation of low-quality jobs such as temporary jobs or low-wage jobs. This question has already been investigated empirically. O'Brien & Feather (1990) for example operationalized "quality of employment" with a scale measuring "skill-utilization" (p. 156). Persons in poor employment did not differ from unemployed persons with regard to mental health. Furthermore, several authors report that former unemployed persons who were unsatisfactorily reemployed (measured by asking directly for satisfaction with the new job) felt as bad as unemployed persons (e.g. Leana & Feldman, 1995; Wanberg, 1995). Although this research is not free of methodological problems, the available evidence suggests that low-quality jobs are no better than unemployment with regard to mental health and trying to solve the unemployment problem by the creation of such jobs would not be a great improvement for public health.

With regard to the demographic moderator effects found here, one consequence could be to steer the allocation of public resources according to these results, for example, by giving special support to those people who were identified to suffer most from unemployment in the moderator analyses: male, minority, blue-collar workers suffering from long-term unemployment. However, as the picture is not yet complete with regard to most of these moderator variables (see chapter 5.3.2.), I am wary at the moment of concrete suggestions concerning such resource allocation decisions.

Unemployment has less malignant effects in economically more developed countries in comparison to less developed countries. It also has less malignant effects in more egalitarian societies (where the level of unemployment protection usually is higher) than in less egalitarian societies. Yet, the political implications of these findings may be less unequivocal than they might seem at first glance as both goals - economic development and income equality – may compete with each other. Furthermore, high levels of unemployment protection have been accused of elevating the unemployment rate (see International Labour Office, 2000b, for a discussion of these arguments). Nevertheless, the fact remains that unemployment is a weaker threat to public health in countries like Canada or Sweden that managed to achieve both goals simultaneously, development as well as income equality.

In sum, a large number of unemployment researchers sacrificed immense amounts of strength, passion, diligence, and lifetime in order to demonstrate the devastating effects of unemployment. The result is a clear and unequivocal warning that unemployment is a severe risk for public mental health that must be fought by all possible means. Thus, the implementation of article 23 of the United Nation's Universal Declaration of Human Rights should have high political priority: "Everyone has the right to work, to free choice of employment, to just and favourable conditions of work and to protection against unemployment" (Office of the High Commissioner for Human Rights, 2005).

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# 8. Appendix

# Appendix: Contents

# Appendix A: Technical report

This technical report describes the meta-analytic methods applied here in more detail than would have been appropriate in the methods section of the main text.

## A 1. Literature search – exact search terms in computerized literature data bases

In English-language databases, all combinations of terms in the following two groups of terms were used during the search: (a) unemploy*, job loss* (b) mental heal*, mental illnes*, mental disord*, distres*, depres*, anxi*, psychosoma*, somatizatio*, well bein*, life satisfactio*, self estee*[1]. In German-language databases, all combinations of terms in the following two groups were used: (a) arbeitslos*, erwerbslos* (b) psychisch* krank*, psychisch* gesund*, psychisch* Störu*, Depress*, Angst*, psychosoma*, Wohlbefind*, Lebenszufried*, Selbstvertrau*, Selbstwert*.

## A 2. Inclusion criteria

According to the fifth inclusion criterion, samples that were drawn from a population of patients of medical institutions were not included in the meta-analysis. In some instances a study examined both samples from patient populations and samples from healthy populations. Only the latter were used in such cases (e.g. Jackson, Iezzi, & Lefreniere, 1996). In two cases an exception from the “no patient populations”-rule was made: In one study (Linn, Sandifer, & Stein, 1985) the participants were partly recruited in "outpatient ambulatory clinics" (p. 503). But only persons who were diagnosed to be “free from major illnesses” (p. 503) were included in the study. Therefore, the study was not excluded from the meta-analysis. In another study (Saurel-Cubizolles, Romito, Ancel, & Lelong, 2000) all participants are mothers, recruited in three hospital “maternity units” (p. 186), and examined 12 months after childbirth. This sample was not excluded, because - on the one hand - pregnancy cannot be regarded as a disease and - on the other hand - the measurement was 12 months after birth when recovery from possible health effects of birth (e.g. postpartum depression) should be complete.

[1] * represents the appropriate joker sign in the respective data base.

# A 3. Operationalization of unemployment

## A 3.1. General issues

The following paragraphs give a more detailed description of the application of three of the four operationalization categories for unemployed samples. For the fourth category ("*Lost job within last three years and still out of work")* no further descriptions are necessary, as all relevant information is already given in the main text. In the second section of this chapter some special cases are described.

(a) *Officially registered as unemployed.* It is explicitly stated in the text that unemployed persons are officially registered as unemployed at an employment office, a union, a self-help organization, or a similar institution.

Usually, the authors of studies in this category explicitly stated that their participants were officially registered as unemployed. However, sometimes the registration status of the participants was not explicitly mentioned, but the sample was recruited in or near or with the help of one of the aforementioned institutions, for example by approaching "person(s) waiting in line to apply for unemployment insurance or to speak with a job counselor" in a state employment agency (Ginexi, Howe, & Caplan, 2000, p. 325; also: Henwood & Miles, 1987; Heubeck, Tausch, & Mayer, 1995). It was inferred from such descriptions that the participants very likely were officially registered as unemployed.

Two studies included in this category only reported unsuccessful trials with unemployment offices for the purpose of recruiting unemployed participants. Although the participants were recruited by other measures in the end, these unsuccessful trials show that the authors were interested in persons matching the official definitions of unemployment (Baumann, Becker, Gerstemaier, Schickle, & Tippelt, 1979; Döring, 1982). Therefore, both studies were included here, too.

(b) *Involuntarity of unemployment explicitly stated.* In studies belonging to this category it was explicitly stated in the text that the participants were involuntarily unemployed, e.g. by formulations like "involuntarily terminated from their job" (Carnes, 1985, p.88) or "seeking employment" (e.g. Feather, & Bond, 1983, p.244). Studies with more indirect but still unequivocal formulations like "workers who still belonged to the work force

(employed or unemployed)" (Liira, & Leino-Arjas, 1999, p. 43) were also included in this category.

A few studies were included in this category that did not explicitly state that all unemployed participants were involuntarily out of work, but used variables like "job seeker behaviour" or "perceived chances of getting a job" (Cullen, Ryan, Cullen, Ronayne, & Wynne, 1987, p. 136). The use of such variables is typical for studies using highly homogenous samples of unemployed people and indicates that the authors were aware of the fact that unemployment is not identical with non-employment and that the authors aimed to study persons involuntarily out of work. Therefore, such studies were included here, too.

(c) *Sufficient differentiation from other groups of non-employed persons.* The unemployed sample is clearly differentiated from other groups of non-employed persons (students, homemakers, retirees), either by excluding such persons from the study or by reporting the results for these groups separately.

For men of all age groups and for women under 21, no differentiation from homemakers was regarded necessary (e.g. Hammer, 1993). For adolescents and young adults, no differentiation from retirees was regarded necessary (e.g. Hammer, 1993; Doherty & Davies, 1984), and for older age groups, no differentiation from students was regarded necessary. This was done because of the very small number of cases like male homemakers, juvenile retirees, and old-age students in the general population.

In some cases, the unemployed group was distinguished from students, homemakers and retirees simultaneously, but without explicitly mentioning them, e.g. by addressing persons who are "not in the active work force" (Graetz, 1991, p. 134)

## A 3.2. Special cases concerning the operationalization of unemployment

Some studies explicitly reported that their "unemployed"-group included persons other than those fitting the standard definition of unemployment. When the percentage of such non-fitting persons was small, they were tolerated and the study was included in the meta-analysis. For example, Patton and Noller's (1984) unemployed group included two participants (9.5%) who were part-time employed and were looking for full-time employment. This study was not excluded.

In some cases the study fitted into one of the operationalization categories for unemployed people, but the unemployed group was divided into subgroups of different prototypicality for unemployment. If data necessary for an effect size estimation were reported for all subgroups separately, I always used only the subgroup that was most prototypical for unemployment and ignored the other subgroups in such cases. For example, in some studies, (Pernice, Trlin, Henderson, & North, 2000; Pernice & Long, 1996; Rodgers, 1991; Rodriguez, Allen, Frongillo, & Chandra, 1999; Warr, 1978; Warr & Payne, 1982) the unemployed group was split into subgroups with different levels of job search activity or different levels of interest in finding employment: (e.g. “actively looking for a job” versus “not really looking at the time of interview” Warr, 1978, p. 119). Only the subgroups labeled as “actively looking” (Warr, 1978, p. 119) or “wanting employment” (Pernice & Long, 1996, p. 317) were included in the meta-analysis in these cases. In the study of Creed and Reynolds (2001) subgroups were formed according to the amount of “access to any paid work" (p. 171) the participants had. All members of all groups were officially registered as unemployed. However, I decided to use only the “no access to any paid work"-group and to ignore the other groups, e.g. the group of unemployed persons with “access to some intermittent part-time or casual work” (Creed & Reynolds, 2001, p. 171).

## A 4. Operationalization of employment

The following paragraphs give a more detailed description of the operationalization of the employed comparison groups than the main text includes.

Generally, I accepted all studies that used a comparison group labeled as “employed” or “working” or “with a job” or used a similar term.[2] Studies using apprentices as a comparison group were also included (e.g. McPherson & Hall, 1983). The same is true for studies using re-employed subjects, i.e. persons who had found new jobs after a period of unemployment (e.g. McKenna & Payne, 1989). Mixing part time employees together with full time

[2] Minor deviations from the rule that all controls should be employed were tolerated. Westman, Etzion and Horovitz (2004), for example, state that four percent of the persons in their comparison group were not employed at all waves of their longitudinal study. Nevertheless, the study was included in the meta-analysis.

employees in one comparison group was tolerated (e.g. Lynd-Stevenson, 1996). In one case, the mixing of persons in "government's Youth Opportunities Scheme (..) receiving work experience or (...) relatively short training" together with persons in regular employment was tolerated and the study was included (Warr & Jackson, 1983, p. 358). In one case, the only available comparison group consisted of "workers who are in time-limited, government subsidized employment" (Verhaegen, Deykin, & Sand, 1994, p. 125). This study also was included in the meta-analysis.

Sometimes, data for more than one employed group was reported and a choice between different alternatives with regard to the comparison group was possible. I used the group that was most prototypical for the institution of employment in industrialized societies in these cases, i.e. full-time non-temporary paid employment. This rule resulted in the exclusion of self employed persons (e.g. Stansfield, Gallacher, Sharp, & Yarnell, 1991; Bradburn & Caplovitz, 1965), part-time employed persons (e.g. Harrison, Barrow, Gask, & Creed, 1999; Bradburn & Caplovitz, 1965), and re-employed (= formerly unemployed) persons (e.g. Ensminger & Celentano, 1988) whenever a choice between one of these groups and persons in full-time non-temporary paid employment was possible. Furthermore, young people in Youth Opportunity Schemes were excluded when a comparison group of regularly employed persons was available (e.g. Banks & Jackson, 1982). The same is true for groups of "Kurzarbeiter", i.e. persons working on reduced time schedules due to work shortage (e.g. Vagt & Stavemann, 1980, p. 516). Subgroups of working persons receiving welfare (Rodriguez, Allen, Frongillo, & Chandra, 1999) and employed persons who will lose their jobs in the near future (Winkelmann & Winkelmann, 1998) were also excluded. When groups of "secure" and "insecure" employed persons were available (e.g. Gallie & Vogler 1990, p. 95; Halvorsen, 1998; Koskela, Viinamäki, Niskanen, & Kontula 1994), I only used the secure group. I also only used the "adequately employed" group in the study of Dooley, Prause, and Ham-Rowbottom, (2000, p. 425), excluding the group labeled "inadequately employed".

Whenever the comparison group was divided into subgroups that did not differ with regard to their prototypicality for employment and there was no other persuasive argument to prefer one of the subgroups, the data were collapsed. For example, whenever "satisfied" and "dissatisfied" employed (e.g. Wanberg, 1995, p. 48) or "satisfactorily" and "unsatisfactorily"

employed persons (Fineman, 1983, p. 145) were differentiated, I pooled the results for both groups, as it is unclear whether "satisfaction" is prototypical of employment.[3, 4]

A very small number of studies only reported correlations with an employment status variable that implied a weighting process of different forms of employment. For example, Montada and Dieter (1999) used a variable of measuring employment status that was coded as (1) "unemployed", (2) "job creation measure or paid training program", and (3) "employed in the first labor market" (p. 21, translations into English by the author). Prussia, Kinicki and Bracker (1993) coded "working full-time (3), working part-time (2), or not working (1)" (p. 385). Such implicit weightings were tolerated, as the middle group (the one with questionable typicality for employment) usually was small and should not have influenced the results to a considerable degree (see also Gowan, Riordan, & Gatewood, 1999).

As a consequence of the decision rules described above, potential comparison groups that were excluded because a more appropriate group was available (e.g. apprentices in the study of Behle, 2001), were used as a comparison group in other cases, when they represented the only available comparison group (e.g. apprentices in the study of McPherson & Hall, 1983). This can be criticized, but it seemed to me to be the best compromise between two important meta-analytic goals, i.e. (1) to draw a large sample of studies with maximum possible representativeness for the research field, and (2) to use comparison groups that are as prototypical of the institution of employment as possible.

[3] Another example for questionable prototypicality: In one case, the employed group was split into subgroups according to the kind of occupation the participants were employed in , e.g. "blue collar/farmers", "executives and white collar", etc. (Ohayon, Priest, Guilleminault, & Caulet, 1999, p. 303). I pooled the data in this case, too, as neither subgroup is more prototypical in my eyes than the other.

[4] Sometimes, in special situations, the decision whether to select or to collapse was not grounded on prototypicality, but on other reasons. For example in the case of Baumann et al. (1979) depression data were reported only for one of the two employed groups. Therefore, I selected the group with the better data situation in this case.

## A 5. Selection and classification of measures of mental health

To decide whether a scale used in a primary study represented an appropriate measure of one of the six indicator variables of mental health meta-analyzed here, I used (a) the name of the scale (e.g. "Becks Depression Inventory"), (b) the content description given by the authors of the primary study, and (c) sample items reported in the primary study. Usually the decision whether a measure could be used or not was straightforward. In a few cases, the names of self-constructed measures were misleading, (e.g. Kopasci, 1990). However, such problems always could be solved by inspection of the items.

Sometimes, the same construct was measured with more than one measurement instrument. When one of the instruments was clearly preferable for methodological or other reasons, only this instrument was selected and used in the meta-analysis. For example, Sheeran and McCarthy (1992) measured „private self-esteem" and „public self-esteem". Private self esteem was operationalized as discrepancy between ideal and actual self-concept. Public self esteem was operationalized as other people's estimated opinion concerning the own person. As the operationalization of private self-esteem is much more typical for the field of unemployment research, only this measure was selected and used in the meta-analysis. However, in some cases no measurement instrument was clearly preferable. For example, Carroll (1985) used the PERI-Demoralization-Scale (Dohrenwend, Levar, & Shrout, 1980) as well as the 90-Item-Version of the Symptom Check List (SCL-90, Derogatis, 1977). Both are reliable and valid measures of distress and seemed to be equally appropriate for use in the present meta-analysis. Thus, the mean of both effect sizes was used in further computations[5]. When a measure of mental health was broken down into sub-constructs (e.g. negative and positive self esteem in case of Haworth and Evans, 1987), I computed an effect size for every sub-construct and used the mean of these effect sizes for the meta-analysis.

## A 6. Computation of effect sizes

I used the standardized mean difference *d* as measure of effect size. This is the difference between two groups, in the present case employed and unemployed persons, divided by an

[5] When a mental health indicator was measured with two or more scales, usually no information concerning the intercorrelation of the scales was given. Therefore, the construction of a composite effect size would have been restricted to a minority of cases. Because of this, as a general rule, I always computed the arithmetic mean in such cases, an acceptable solution to this problem according to Hunter and Schmidt (1990).

estimate of the within-group standard deviation. More specifically, I first computed the coefficient *g* as defined by Hedges and Olkin (1985. p. 78-79). In a second step, to get an unbiased estimate of *d,* I corrected for the positive bias for small sample sizes that is inherent in *g* using the correction factor *c(m)* (Hedges, 1981).

When means and standard deviations were reported or could be estimated with the available data, I used the formulas presented in Hedges and Olkin (1985, p. 78-79) to compute *g*.[6, 7, 8] When no means and standard deviations were available, but correlations, t-values, or *F*-values from one-factor-ANOVAs with $df = 1$ were reported, I used the formulas provided in Rosenthal (1994) to compute effect sizes.[9] When *F*-values and means from single-factor ANOVAs with $df > 1$ were reported, I first computed the mean square within-value as the quotient of the mean square between and the *F*-value. Then I used the square root of the mean square within as standard deviation-estimate to compute an effect size (Glass, McGaw, & Smith, 1981, p. 128).

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[6] In a few cases, the mean had to be retrieved from a diagram by use of a ruler (e.g. Mohr, 1993/1997; Palt, 1986). In one case (Fagin & Little, 1984), means and standard deviations had to be computed from the raw data list.

[7] If standard errors or confidence intervals were reported instead of a standard deviation, I used standard formulas to re-construct the standard deviation (Koskela, Viinamäki, Niskanen, & Kontula, 1994; Vega, Kolody, Hough, & Fugueroa, 1987). In one case (Dew, Bromet, & Penkower, 1992) the standard deviations had been logarithmically transformed while the means were reported in the original metric. I retransformed the standard deviations before using them for the effect size estimation.

[8] In some cases, a standard deviation was reported for only one of the two groups of employed and unemployed people but not for the other group. In other cases, a standard deviation was only reported for a larger sample, including persons outside the labor market such as students or homemakers, in addition to unemployed and employed persons (e.g. Behle, 2001; Heady & Smyth, 1989; Radloff, 1975; Vagt & Stavemann, 1980; Warr, 1978; Warr & Jackson, 1985). In such cases the only possible way to proceed was to use these standard deviations as crude estimations of the pooled within group standard deviation of the employed and unemployed groups.

[9] In a few cases, correlations were based on an employment status variable with more than two values (e.g. “unemployed”, “employed in the second labour market” and “employed in the first labor market”, Montada & Dieter, 1999, p. 31, translation into English by the author). As Rosenthal’s (1994) transformation formula requires the knowledge of exactly two sample sizes - for employed and unemployed persons - I collapsed groups where appropriate to compute an effect size estimate (e.g. by adding the “second labor market”-group to the unemployed in the case of Montada & Dieter, 1999).

Data from ANOVA-designs with more than one factor were transformed to effect sizes in two different ways: When only *F*-values but no sums of squares were reported (e.g. Aneshensel, Frerichs, & Clark, 1981; Feather, 1982), I used the procedure described in Morris and DeShon (1996) to estimate a corrected *F*-value. Then, depending on the ANOVAs *df*-value, I preceded either with the method reported in Rosenthal (1994) or with the method reported in Glass et al. (1981). In cases of $df = 1$, I followed Rosenthal (1994), using the square root of the *F*-value as an estimate of the *t*-value. In cases of $df > 1$, I proceeded with the method reported in Glass et al. (1981) using the mean square within as a variance estimate (see above). When sums of squares from two- or more-factor ANOVAs were reported directly (e.g. Palt, 1986), I used the formulas described in Glass, McGaw, and Smith (1981, p. 119) to estimate the pooled standard deviation.

When only *p*-values were reported, I followed recommendations of Rosenthal (1994) and Glass, McGaw, and Smith (1981) and used specific computer software (Faul & Erdfelder, 1992) to retrieve the appropriate *t*-values.[10] In the case of some not very specific verbal statements, assumptions had to be made: I assumed $p < .05$ in the case of a statement like "the difference was significant" (e.g. Tiffany, Cowan, & Tiffany, 1970, p. 92) and $p < .01$ in the case of a statement like "was *highly* significant" (e.g. Miles, 1983, p .54). If the results were reported to be "not significant" or a statement like "there was no difference" was given (Ehrhardt, 1993, p. 149, translation into English by the author), I coded $d = 0.00$. In results tables with significance signs like "*", the missing of such signs was coded as $d = 0.00$ (e.g. Daniels, 1986).

When only proportions or the absolute number of "cases" and "non-cases" were reported, I first computed an odds ratio and then used the method proposed by Cox (1970), which showed very good performance in a recent Monte-Carlo study (Sánchez-Meca, Marín-

[10] Following a recommendation of Glass, McGaw, and Smith (1981), this includes *p*-values from Mann-Whitney-*U*-Tests (e.g. Dieth, 1995). *P*-values from the Wilcoxons rank sum test for independent samples (e.g. Dobberstein, 1979) were treated the same way, as this test is mathematically equivalent to the *U*-test (Diehl & Arbinger, 1990). When only *p*-values were reported for a comparison of two groups without any specification concerning the kind of test that was conducted (e.g. Harding & Sewel, 1992; Henwood & Miles, 1987), I assumed the usage of a t-test and proceeded as described above. When only *p*-values for comparisons with more than two groups were reported without any specification of the statistical test that was conducted (e.g. Buss & Redburn, 1983), I assumed the usage of ANOVA, used computer software to estimate the appropriate *F*-value, and proceeded as described above.

Martínez, & Chacón-Moscoso, 2003) to estimate the standardized mean difference.[11, 12] When only odds ratios were reported (e.g. Catalano, Aldrete, Vega, Kolody, & Aguilar 2000), the Cox-formula was also used.

When significance levels were reported in addition to proportions, a decision between two suboptimal kinds of data was necessary, as both kinds of information do not allow an exact estimation of an effect size (e.g. Lahelma, 1989). I used the proportions in such cases, as the resulting effect size may be less biased than the effect size computed with a level-*p*, especially when samples sizes are large.

In three cases, results were directly reported as standard scores (Campbell, Converse, & Rodgers, 1976[13]; Kasl, 1979[14]; Kessler, Turner, & House, 1987[15]).

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[11] For some studies (Berger & Mohr, 1986; Bradburn, 1969; Brenna, Marinoni, Renieri, & Torre, 1987; Büchtemann & van Rosenbladt, 1981; Hesketh, Shouksmith, & Kang, 1987; Little, 1973), proportions for more than two categories were reported (e.g. "Very Happy", "Pretty Happy" and "Not Too Happy" in the study of Bradburn & Caplovitz, 1969, p. 184). In these cases, a dichotomous data situation was created by collapsing categories, then odds ratios were computed.

[12] In one study, the proportion of "cases" was zero among one group (employed males), preventing the computation of an odds ratio for male participants (Cullen, Ryan, Cullen, Ronayne, & Wynne, 1987). To enable the computation of an effect size, I change the data situation by assuming that at least one person was a "case" in this group and by using the resulting proportion for further computations. This procedure may be justified, as the bias it causes leads to a slight underestimation of the true effect size, i.e. it is a conservative bias.

[13] While Campbell, Converse, and Rodgers (1976) provide easily interpretable data for their male subsample ("average scores a full standard deviation lower than those of men with full-time jobs" p. 313), their statement for the female sample is difficult to interpret: "average scores less than a third of a standard deviation below those of women with full-time jobs" (p. 313). I assumed that the real value was close to a third and coded $d = 0.33$.

[14] Reported data are "standard scores (*Mean* = 0, *SD* = 1) with the data on stably employed controls (...) used as a basis for standardization" (p. 189). This means that the standard deviation of the employed group, not the pooled standard deviation, was used as denominator. As no other useful information was given, the reported standard scores have been used as effect size estimates.

[15] "All outcomes scores were standardized to a mean of 0 and variance of 1 before computations, so the means can be interpreted in standard deviation units. For example, the score of 0.38 in the top left corner of the table indicates that currently unemployed respondents reported an average level of anxiety that is 38% of a standard deviation above the average for the entire sample" (p. 953). The "entire sample" consists of more groups than unemployed and employed. Thus the standard scores are based on a standard deviation not identical to the pooled standard deviation of the employed and the unemployed group. As no other useful information was

In one case, the only available information concerning the size of the effect was the following verbal statement: "The difference in employment status between the two groups only explained (...) about 2 per cent of the variance in depression" (Lynd-Stevenson, 1996, p. 128). I used the square root of the explained variance as a correlation-estimate and then proceeded with the formula provided in Rosenthal (1994).

In line with common meta-analytic practice, I used only information from bivariate analyses and ignored data resulting from multivariate techniques that control for the influence of third variables, such as multiple regression or ANCOVA. Studies reporting only *F*- or *p*-values from repeated-measures-ANOVAs (e.g. Stokes & Cochrane, 1984) also could not be used, as Morris and DeShon's (1997) correction formula is not appropriate for such designs.[16] (All effect sizes are documented in appendix C, table C-8.)

With the exception of effects sizes estimated from proportions and odds ratios, all effect size-variances were computed with the formula given in Hedges and Oklin (1985, p. 86). The variances for effect sizes estimated from odds ratios and proportions were computed with the formula reported in Sánchez-Meca et al. (2003, p. 452) to be appropriate for $d_{Cox}$.[17]

## A 7. Computation of composite effect sizes

As described in the main text, a formula provided by Marin-Martinez and Sanchez-Meca (1999, p. 34), based on a method reported in Rosenthal and Rubin (1986), was used to

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available, these standard scores had to be used. For effect size-estimation I subtracted the standardized mean of the "stably employed" group (e.g. -0.238) from the standardized mean of the unemployed group (e.g. 0.380).

[16] Weighting procedures that were applied to guarantee the representativity of a sample were tolerated (e.g. Margraf & Poldrack, 2000).

[17] This formula requires the knowledge of the cell frequencies in the 2*2-table the $d_{Cox}$-coefficient is based on. In a few cases, these frequencies were not reported. Catalano, Aldrete, Vega, Kolody, and Aguilar-Gaxiola (2000), for example, report only an odds ratio, which allows the computation of $d_{Cox}$ but do not report the cell frequencies, which hinders the computation of a sampling variance with a formula appropriate for dichotomous data. I used the Hedges and Olkin (1985)-formula for continuous data that does not require the knowledge of cell frequencies in such cases as a rough estimate of the sampling variance.

compute the combined, or "composite" effect sizes. A minor problem sometimes emerged when the sample sizes for different indicators of mental health differed within one study. Creed (1999), for example, reports a sample size of $n = 89$ for mixed symptoms and $n = 87$ for self-esteem. In such cases, I usually coded the smallest available sample size as the sample size of the composite. This is a conservative method to deal with this problem, as it reduces the meta-analytic weight of the respective study. This method was always used when the deviations with regard to the samples sizes were small. However, in a few cases (e.g. Büchtemann & van Rosenbladt, 1981) the sample sizes reported for two or more indicators of mental health differed strongly from each other. I abstained from the computation of an overall effect size in these cases and used only the effect size from the largest sample instead. This was done in order to avoid a too severe underweighting of the respective study, as would have been the consequence of the other method.

When the measure of an indicator of mental health was a subscale of another scale that was used to construct the composite, the subscale was ignored and not included in the composite. Graetz (1991), for example, used subscales of the GHQ to measure anxiety and depression. Thus, identical questions were part of the measures of mixed symptoms (i.e. the GHQ) on the one hand and depression/anxiety on the other hand. Therefore, I ignored depression and anxiety when computing the composite in this case. (All composite effect sizes are documented in appendix C, table C-8.)

*Sampling variances* for composite effect sizes usually were computed with the standard formula for continuous data (Hedges & Olkin, 1985, p. 86). The only exceptions to this rule were studies that reported only proportions of "cases" for each indicator of mental health. In such cases I first computed the average proportion of cases for unemployed and for employed persons and then used these average proportions to estimate the sampling variance by using the formula appropriate for dichotomous data (Sánchez-Meca et al., 2003, p. 452). This method leads to more conservative results, i.e. larger variance estimates, than the use of the formula for continuous data, as reported in Hedges and Olkin (1985, p. 86).

## A 8. Artifact correction

As no other data concerning biasing artifacts were available, I corrected the mean effect sizes only for unreliability, using the formulas provided in Hunter and Schmidt (1990). Altogether, 166 reliability estimates were coded. These estimates ranged from alpha = .51 to alpha = .97 with a mean of alpha = .82. The mean for measures of mixed symptoms of distress was alpha = .87. The mean for depression was alpha = .82, for anxiety alpha = .79, for psychosomatic symptoms alpha = .82, for subjective well-being alpha = .82, and for self-esteem alpha = .79. The attenuation factors were computed as square roots of the alphas. To correct the overall effect size I used the simple mean of all available attenuation factors.

## A 9. Use of longitudinal studies for cross-sectional comparisons

For many longitudinal studies (e.g. Banks & Jackson, 1982; Beiser, Johnson, & Turner, 1993; Caplan, Vinokur, Price, & van Ryn, 1989; Isaksson, 1990; Kirchler, 1985; Lahelma, 1989; Winkelmann & Winkelmann, 1998) it is possible to compute several cross-sectional effect sizes comparing unemployed and employed persons, one for each time of measurement. However, as the same persons are examined several times in such longitudinal studies, computing one effect size for each time would lead to stochastically dependent effect sizes. Therefore, I selected the measurement time with the highest number of unemployed persons and used only this single effect size for the cross-sectional meta-analysis.

In a few cases no clear decision by this sample size-rule was possible, as sample sizes were identical for all measurement times (e.g. Bolton & Oatley, 1987; Patton & Noller, 1990). Individual decisions were necessary in such cases. In the case of Bolton and Oatley (1987), for example, T2 was chosen, because for the second measurement time the data situation was better than for T1 (exact *t*-value reported at T2 instead of “no sig. differences”-statement at T1).

## A 10. Coding of intercorrelations of measures of mental health

Usually the coding of the intercorrelations of the mental health indicators was straightforward, as the relevant data were directly reported as correlations. However, sometimes a specific

measure of mental health was a subscale of another more global measure. In these cases the intercorrelation of both scales was ignored. The anxiety measure in the Graetz (1991)-study, for example, was constructed with items from the GHQ. Thus, its intercorrelation with the GHQ as a measure of general distress was ignored, as it represents a part-whole-correlation and therefore is likely to overestimate the true correlation of the two indicators of mental health. When only a mean intercorrelation for several measures of mental health was reported in a study (Kasl, 1979) I coded this mean value for all possible pairings of two mental health measures. (For a list of the intercorrelations between the six indicators of mental health, see appendix C, table C-2.)

# A 11. Coding of moderator variables

## A 11.1 General issues

### *A 11.1.1. Level of analysis*

Effect sizes were usually computed on study-level. However, the level of subgroups was always chosen when data were reported separately for such subgroups that were relevant for the intended moderator analyses (e.g. separately for males and females). In some very rare cases a decision between two or more moderator variables was necessary because it was not possible to use data for more than one moderator. For example, Clark and Oswald (1994) reported data separately for males and females on the one hand, and for three age groups on the other. Theoretically, this study could have been used in both moderator analyses, for gender as well as for age. However, to do so would have caused complex problems with the handling of the data set. Therefore, I decided to use such studies for only one moderator analysis. In the case of the Clark and Oswald (1994) study, this was age, as the authors reported interesting and rare data for older unemployed persons. An Austrian study posing the same problem was also used for the moderator analysis of age (Studnicka, Studnicka-Benke, Wögerbauer, Rastetter, Wenda, Gathmann, & Ringel, 1991).

### *A 11.1.2. Group-specificity of moderator information*

Whenever possible, I coded only information referring specifically to employed and unemployed persons for the moderator analyses. This information was intended to represent

the whole group of economically active persons examined in a primary study. However, in several studies information about moderating variables was not reported for the whole group of economically active persons, but only for a subgroup, for example only for unemployed persons, but not for employed persons (e.g. Brown & Gary, 1985; Haworth & Evans, 1987; Melville, Hope, Bennison, & Barraclough, 1985). In other studies, information concerning relevant moderator variables was only reported for a larger group including other persons together with the employed and unemployed persons. Economically active persons, for example, were often studied together with persons who are out of the labor force, such as students or retirees, and information concerning potential moderators was often only reported for the whole sample in such studies (e.g. Aneshensel, Frerichs, & Clark, 1981; Araya, Rojas, Fritsch, Acuna, & Lewis, 2001; Bachman, O'Malley, & Johnston, 1978; Banks & Jackson, 1982; Beiser, Johnson, & Tuerner, 1993; Eaton & Kessler, 1981; Jenkins, et al., 1997; Michelson, Bolund, Nilsson, & Brandberg, 2000). Whenever this was the case, namely, when the only available information with regard to moderator variables referred to a subgroup or to an "extended" group, this information had to be used as a rough estimate for the group of economically active persons in the study.

#### *A 11.1.3. Source of moderator information*

In two instances of insufficient data reporting, information concerning moderator variables had to be retrieved from other publications on the same study (in the case of Brenner, Petterson, Arnetz, & Levi, 1989 from Levi, 1984; in case of Kasl, 1979 from Cobb & Kasl, 1977).

## A 11.2. Specific issues

The following paragraph describes the coding process for all moderator variables where coding from the primary studies was necessary. (The results of the coding process are documented in appendix C, table C-4.)

### *A 11.2.1. Gender*

The percentage of female participants was coded for each sample. Usually, the information concerning gender was unambiguous and no coding problems occurred. In a few cases the gender of the participants had to be guessed from context information. For example, when "steelworkers" were studied and "his wife" was mentioned with regard to the typical participant, it was clear that the sample consisted of men, although this was not explicitly stated in the text (Buss & Redburn, 1983, p. 70).

### *A 11.2.2. Age*

The average age (in years) of the participants was coded for each sample included in the meta-analysis. Whenever possible, the arithmetic mean was used for this purpose. In some cases I had to use the median, as this was the only available measure of the central tendency of the age distribution (e.g. Bachman, O'Malley, & Johnston, 1978; McCarthy & Ronayne, 1984). However, often neither the arithmetic mean nor the median were reported directly. Therefore it was necessary to estimated the mean age from other data in such cases:

Several authors of primary studies divided the age distribution into distinct intervals (e.g. "<30", "30-44", "45-64", Roberts, Stevenson, & Breslow, 1981, p. 776) and reported the absolute number of persons or the proportion of persons falling in each of these categories. I computed the sample size-weighted mean of all category means in these cases and used this value as estimate of the mean age of the whole sample. Sometimes, the lowest and/or the highest category was open, e.g. "under 35" and "55 and older" (Beiser, Johnson, & Turner, 1993, p. 734). Here I estimated the minimum age as 18 and the maximum age as 65.[18.] When a category included only persons above 65 years, the category was ignored (e.g. Eaton & Kessler, 1981; Harrison, Barrow, Gask, & Creed, 1999).

In several cases, the minimum and the maximum were the only available information regarding the age distribution, e.g. "15 – 24 year olds" (Morrell, Taylor, Quine, Kerr, & Western, 1994, p. 1555). I estimated the mean age of the sample as the midpoint of the age

[18] One exception of this rule was necessary: In the study of Saurel-Cubizolles, Romito, Ancel, and Lelong (2000) percentages for four age groups were reported, with the lowest and the highest category open. I estimated min as 18, as usual, and max as 45, as all participants were women one year after childbirth and an estimation of maximum age with 65 years seemed inappropriate for this special sample.

range.[19] When the sample was labeled only as "adolescents" (Doherty & Davies, 1984, p. 217), or as "Jugendliche" (Engel, 1982, p. 317; Krause, 1987, p. 116) the age mean was estimated to be 17. In one case, the mean age had to be computed from a raw data list (Fagin & Little, 1984).

Authors of longitudinal studies sometimes reported the mean of the age distribution only for the first time of measurement. When the mean age for the second time of measurement was required, I added the length of the interval to the mean value for T1 (e.g. Banks & Ullah, 1988; Beiser, Johnson, & Turner, 1993).

### *A 11.2.3. Minority/majority status*

The percentage of members of ethnic or cultural minority groups was coded for each sample included in the meta-analysis. As the great majority of studies were conducted in Europe, North America, and Australia/New Zealand, I used all terms referring to non-caucasian race as indicators of minority status for studies from these continents, for example "non-white" (Leana & Feldman, 1995, p. 1390), "ethnics" (Buss & Redburn, 1983, p. 52), "black" (e.g. Caplan, Vinokur, Price, & van Ryn, 1989, p. 761; Bachman, O'Malley, & Johnston, 1978, p. 13), "Negro subjects" (Lawlis, 1971, p. 216). Furthermore, all terms referring to a non-European geographic descent were interpreted as indicating minority status, for example "African American" (Ginexi, Howe, & Caplan, 2000, p. 325), "Afro-Caribbean" (Jenkins et al., 1997, p. 783), "Native Americans" (Garrett, 1988, p. 41), "Hispanic" (Dooley, Prause, & Ham-Rowbottom, 2000, p. 427), or "Asian/Oriental" (Jenkins et al., 1997, p. 783)[20, 21]. When it was used in clear opposition to words like "white" or "caucasian", the term "other" (e.g. Bachman, O'Malley, & Johnston, 1978, p. 13; Campbell, Converse, & Rodgers, 1976, p. 513)

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[19] Some authors reported age maxima larger than 65 years, as the study included retirees in addition to employed and unemployed persons (e.g. Aneshensel, Frerichs, & Clark, 1981). In such cases the age maximum was coded as 65.

[20] Other examples are: "Alaska Native/American Indian" (Shoemaker, 1993, p. 26), "Latinos" (Shoemaker, 1993, p. 26), "Latin American" (Lawlis, 1971, p. 216), "Mexican Americans" (Catalano, Aldrete, Vega, Kolody, & Aguilar-Gaxiola, 2000, p. 479), "southeast Asian refugees" (Beiser, Johnson, & Turner, 1993, p. 734); "Vietnamese-American" (Hinton et al., 1998, p. 677), "Pakistani and Bangladeshi males" (Shams & Jackson, 1994, p. 348).

[21] In the case of Westman, Etzion, and Horovitz. (2004), a study from Israel, birth "in the former soviet union " and "in other countries [outside Israel]" (p. 829) were used as indicators of minority status.

was also interpreted as indicating minority status. The term “immigrants” (Pernice, Trlin, Henderson, & North, 2000, p. 25) and the statement that a study was conducted in “a southern black community" (Dressler, 1986, p. 639) were interpreted as indicating minority status, too.

Sometimes authors reported the proportion of participants who belonged to the majority group of the respective country, for example “Deutsche Arbeiter” (Frese, 1979, p. 228), “Anglo-Saxon” (Lennings, 1993, p. 704), “white race” (Melville, Hope, Bennison, & Barraclough, 1985, p. 789), “White” (Wanberg, Griffiths, & Gavin, 1997). The proportion of minority members was estimated as 100% minus the percentage of majority members in such cases.

### *A 11.2.4. Marital status*

The percentage of married persons was coded for each sample. Usually, the respective information reported by primary study authors was unambiguous and easy to code. However, non-married persons in intimate relationships posed a coding problem in a few cases: Usually, the authors of primary studies did not mention this group at all and reported only the number or the proportion of “married” persons (e.g. Leana & Feldman, 1995, p. 1390) or “husbands” (Liem & Liem, 1988, p. 89). Non-married but cohabiting persons presumably were included in other groups, e.g. “singles”. In other studies, however, non-married couples obviously had been added to the “married”-category e.g. when percentages for a group labeled “married/cohabiting” (Jenkins et al., 1997, p. 782) or “living in a marriage or a marriage-like relationship” (Koskela, Viinamäki, Niskanen, & Kontula, 1994, p. 29) were reported. In both cases described above only one kind of information was available (either the proportion of married persons or the proportion of married persons mixed with “cohabiting” persons). Therefore, I had to use this information to estimate a sample’s proportion of persons with intimate relationships since it was the only information available. However, in other cases a decision was necessary: In some studies, percentages for married persons on the one hand and “cohabiting” persons (Araya, Rojas, Fritsch, Acuna, & Lewis, 2001, p. 231) or persons “living with partner” (Hodiamont, Peer, & Syben, 1987, p. 500) or persons in a “common law marriage” (Vega, Kolody, Hough, & Fugueroa, 1987, p. 1216) on the other hand were reported separately. Thus, I had to decide whether to include or to exclude the “cohabiting” persons when coding marital status. As I was primarily interested in the psychological situation of an intimate partnership, regardless of its legal status, I decided to add both figures, i.e. the percentage of “married” and of “cohabiting” persons, when they were reported

separately. Consistent with this, I also used Behle's (2001) study in the moderator analysis. In this study, only the percentage of persons with "partners" (p. 477) is given, while marriage is not mentioned. In one study (Iversen & Sarboe, 1987) only the proportion of "single" persons is reported. I estimated the percentage of married/cohabiting persons as 100% minus the percentage of "single" persons in this case. [22]

*A 11.2.5. Occupational Status*

The percentage of blue-collar workers in a sample (as opposed to white-collar workers and professionals) was coded as a measure of occupational status. Often, authors did not directly report the proportion of blue-collar workers but used other terms to describe a similar occupational status, for example "manual" as opposed to "non-manual" occupations (e.g. Araya, Rojas, Fritsch, Acuna, & Lewis, 2001, p. 229), "skilled", "semi-skilled" and "unskilled" occupations (Hepworth, 1980, p. 142), "industrial workers" (Leana & Feldman, 1995, p. 1381), "Laborer" (Jex, Cvetanovski, & Allen, 1994, p. 72), "wage-earning" as opposed to "salaried" (Iversen & Sarboe, 1987, p. 39), Social classes "V", "IV" and "III manual" (Jenkins, et al., 1997, p. 782), "Arbeiter" (Frese, 1979, p. 228), "gewerblich" (Döring, 1982, p. 122). Sometimes, more specific job descriptions were given, e.g. "Miners" (Friis, Carter, & Edling, 1998, p. 297), "steelworkers" (Carroll, 1985, p. 100), "blacksmith" (Fagin & Little, 1984, p. 218), or "fishers/fishplant workers" (Gien, 2000, p. 122). Usually, the categorization of the given job description as either blue-collar or white-collar did not pose a problem. However, in uncertain cases (e.g. "nurses", Imai, 2001, p. 13) the decision of how to code an occupation was achieved by the use of a publication of the United States Office of Personnel Management (1999), providing a comprehensive list of large numbers of blue- and white-collar occupations.

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[22] Other examples for studies mixing married with non-married but cohabiting persons: "married (...) or currently living with someone in a permanent committed relationship" (Ginexi, Howe, & Caplan, 2000, p. 325), "in a spousal or partner relationship" (Grant & Barling, 1994, p. 317), "mit Familie bzw. Mann/Partner" (Mohr, 1997, p. 191), "married or living in a de facto relationship" (McDonald, Vechi, Bowman, & Sanson-Fisher, 1996, p. 459) "married/cohabiting" (Viinamäki, Hintikka, Kontula, Niskanen, & Koskela, 2000, p. 179). Other examples for studies reporting separate categories for married and non-married but cohabiting persons are: "married"/"living with someone" (Garrett, 1988, p. 44), "married"/"cohabiting" (Platt, Martin, & Hunt, 1990, p. 128).

In sum, the coding process for occupational status required a large number of individual decisions concerning whether a term could be interpreted as sufficiently similar to "blue-collar" or not. Therefore, although my subjective certainty with regard to the coding decisions usually was high, the coding process for occupational status was possibly more error-prone than the coding-processes for other moderator variables. Thus, to enable the reader to form his or her own opinion, all coding decisions for this variable are documented in appendix C, table C-1.

### *A 11.2.6. Unemployment duration*

The average unemployment duration was coded in months.[23, 24] Sometimes the distribution of the individual values of unemployment duration was divided into distinct time intervals (e.g. 1-3 months, 4-6 months, 7-9 months etc.) and the proportion or the absolute number of individuals in each interval was reported. The average of the interval means, weighted by each interval's sample size, was computed as an estimate of the average duration of unemployment for such studies. In some cases (e.g. Claussen, Bjorndal, & Hjort, 1993; McLoyd, Jayaratne, Ceballo, & Borquez, 1994) the highest category was open (e.g. "> 9 months"). To be able to compute a category mean, I estimated the breadth of the highest category as identical to the breadth of the second highest category in such cases. The case of Claussen et al. (1993) may serve as an example: The range of the second highest category of unemployment duration was described as "53-80 weeks registered" (p. 15). The highest category was open with only the minimum, 81 weeks, reported. I estimated the range of the highest category to be 81-108 weeks, i.e. to have the same breadth as the second highest category.

When only minimum and maximum duration were reported, I estimated the average duration of unemployment to be the mean of these values. In a few cases (e.g. Dew, Bromet, & Penkower, 1992; Lawlis, 1971) only a maximum value of unemployment duration was reported. I estimated mean duration as half of the maximum in these cases.

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[23] Information given in weeks (e.g. Banks & Ullah, 1988) was transformed to months by dividing through 4.33.

[24] When only the median (e.g. Hartley, 1980; Hepworth, 1980) or the mode (Tiggeman & Winefield, 1984), but not the arithmetic mean, were reported, I used the median or the mode, respectively, to estimate average unemployment duration.

For some longitudinal studies, only the mean unemployment duration for the first measurement point was reported, but I needed a duration estimate for a later measurement point (e.g. Balz, Drewski, Schultz-Gambard, & Mowka, 1985; Fineman, 1983). In such cases, I usually added the time interval between the two measurement points to the reported mean unemployment duration for the first measurement point.[25]

For plant-closing studies, when all participants lost their job at the same time, the interval between the closing of the plant and the time of measurement was used to estimate the average unemployment duration when no other data were available (e.g. Leana & Feldman, 1995; Perfetti & Bingham, 1983).

In some rare cases, study authors did not report the average length of the current unemployment spell but the average length of unemployment within a certain time frame, e.g. “since leaving school" (Morch, 1986, p. 37). Lacking more exact information, these data were used for coding.

If data were reported separately for long term and short term unemployed persons within one study, I ignored the possibility for moderator analysis and collapsed the data of both unemployed groups to avoid statistical dependent data, as the same comparison group would have been used for two effect sizes, damaging their statistical independence (e.g. Carroll, 1985; Kokko & Pulkkinen, 1998; Liira & Leino-Arjas, 1999, Wacker & Kolobkova, 2000).

### *A 11.2.7. Year of data collection*

The year of data collection of each study was coded. When no year of data collection was reported, I subtracted four years from the publication year to estimate the year of data collection. Four years was the mean difference between the year of publication and the year of data collection for those studies that reported both figures.[26]

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[25] However, this method was not used when hints existed that suggested a large proportion of the unemployed group had not been continuously unemployed between T1 and T2 but instead had had intermittent jobs (e.g. Frese, 1979). In such cases, no average duration of unemployment was coded.

[26] When a study was published in more than one report and I had to use more than one report in the meta-analysis to retrieve all necessary information to compute an effect size (e.g. Kaufman, 1973 / 1982; Layton, 1986b / 1987), the year of the first publication was coded when no exact information concerning the year of data collection was available.

### *A 11.2.8. Type of publication*

Type of publication was coded with four categories: Whether a study is reported (1) in a peer reviewed scientific journal, (2) as a book or book-chapter, (3) as a dissertation thesis or (4) in another form (e.g. degree dissertation, institute report). Coding decisions were straightforward in all cases.

### *A 11.2.9. Language*

The language of the publication was coded as either English or German. Coding decisions were straightforward in all cases.

### *A 11.2.10. Unemployment as main topic*

Results for employment status often are reported as a by-product in papers that are not primarily concerned with the psychological effects of employment or unemployment. Therefore, I coded whether a study mentioned “unemployment” or “job loss” or “employment status” or a similar term in its title or not.

### *A 11.2.11. Reemployment status of the comparison group*

Several studies used comparison groups consisting of former unemployed persons who had subsequently found a job. Several other studies did not mention the question of employment continuity at all. Only a few studies stated explicitly that they did not use reemployed persons as a comparison group. Therefore, I employed a dichotomous coding scheme: I coded as “reemployed” when the authors of a primary study explicitly stated that the comparison groups consisted mainly (> 50%) of reemployed persons. I coded as “other” when the authors either explicitly stated that the comparison group consisted of continuously employed persons or when they did not mention the issue of employment continuity at all.

### *A 11.2.13. Percentage of par-time employed persons*

The percentage of persons in part-time jobs was coded for each employed comparison group. Coding decisions were straightforward in all cases.

## A 12. Coding of sample size

When the samples sizes for the different indicator variables of mental health differed within a study, a conservative procedure was chosen and the smallest sample size was coded as the sample size for the overall effect size (e.g. Kaufman, 1973/1982; Liira & Leino-Arjas, 1999).

Sometimes the sample size reported in the methods section differed from the sample size or degrees of freedom reported in the results section of a study (e.g. D'Arcy & Siddique, 1985). I used the sample size that seemed to be closest to the effect size in such situations, i.e. usually the sample size given in the results section. When all available information appeared to be equally close to the effect size, I used the more conservative, i.e. the smaller sample size in the meta-analysis (e.g. Vuori & Vesalainen, 1999). Sometimes only an overall sample size or an overall-*df,* not single sample sizes for employed and unemployed persons, was reported. Since the single sample sizes for both groups usually are needed for effect size estimation (computation of pooled standard deviation), I applied the proportions of employed and unemployed persons from the methods section (when available) to the overall-*df* from the results section to get an estimation of the sample sizes of the groups (e.g. D'Arcy & Siddique, 1985; Frost & Clayson, 1991; Jex, Cvetanovski, & Allen, 1994; Snyder & Nowak, 1984).[27]

Sometimes the sample sizes had to be estimated from other kinds of information. Morrell, Taylor, Quine, Kerr, and Western (1994), for example, reported only the absolute numbers of "cases" in addition to figures for "prevalence", but not absolute numbers of participants per se (p.1557). I reconstructed the sample sizes with the available data when possible. (All sample sizes are documented in appendix C, table C-8.)

[27] In one case (Proudfoot, Gray, Carson, Guest, & Dunn, 1999), exact sample sizes for both training groups (cognitive behavioral therapy vs. social support training) were not reported, but only the sample size for the overall-group. As the participants were "randomly assigned" (p. 41) to both groups, I estimated both sample sizes to be 50% of the total sample size

## A 13. Coding of reliability estimates

Cronbach's alpha was coded as the measure of reliability. Usually, coding was straightforward as alpha was either reported directly or not at all. However, in a few cases decisions were necessary. For example, when only a lower bound of reliability for several scales was reported (e.g. "Cronbach's alpha above 0.82 for all functions", Michelson, Bolund, Nilsson, & Brandberg, 2000, p. 479), the information was ignored and no reliability estimate was coded, as the lowest reliability of a group of scales would possibly mean underestimating the reliability of the specific scale of interest, resulting in an overcorrection of the effect size (for similar cases see also Burke, 1984; Kabbe, Setterlind, & Svensson, 1996). For a similar reason I ignored information concerning only subscales when the reliability of the complete scale was relevant. For example, Warr and Jackson (1983) reported alphas only for the "positive" and "negative" subscales of the Rosenberg self-esteem scale. As alpha is correlated with the length of a scale, to use the mean of the two rather short subscales would mean underestimat the reliability of the complete scale, resulting in an overcorrection of the effect size. When an interval of several reliabilities was reported, I used the mean of the interval (e.g. "the internal consistencies according to Cronbach's alpha were in the intervals .86 - .90", Brenner, Petterson, Arnetz, & Levi, 1989, p. 105). When only one reliability estimate was reported in a study with several subgroups relevant the meta-analysis (e.g. Brinkmann & Potthoff, 1983), the reliability estimate was coded for each subgroup. When reliabilities were reported separately for employed and unemployed persons, the mean of both reliability estimates was coded for the respective study (e.g. Waters & Moore, 2002). When one construct was measured with two scales and the mean of both effect sizes was used for the meta-analysis, I also used the mean of both reliabilities, if reported (e.g. Dalbert, 1993).

Formulations such as "internal consistency reliability" (Wanberg, 1995, p. 44), "internal reliability" (Creed & Reynolds, 2001, p. 172) or "reliability" (Kinicki, Prussia, & McKee-Ryan, 2000, p. 96) were interpreted as paraphrasing Cronbach's alpha and therefore the information was used. Data concerning the "split-half reliability" (Shamir, 1986; p. 64; see also Brown & Gary, 1985) or reliability estimates computed with the Kuder-Richardson formula (Hepworth, 1980) were ignored. (The results of the coding of reliability estimates are documented in appendix C, table C-3.)

## A 14. Treatment of printing errors

In some cases, obvious printing errors occurred in the primary studies. Whenever possible, I tried to find a reasonable way to correct them. The most relevant cases with regard to the outcomes of the meta-analysis are listed below.

Madianos, Vlachonikolis, Madianou, and Stefanis (1985) reported a value they call "*SD*" in table 1. However, this standard deviation is abnormally small (0.10 for the whole sample of 1,574 persons with a mean of 3.80). The effect size for the difference between employed and unemployed persons would be $d = 6.93$ when estimated with the "*SD*" reported in table 1, an unrealistically large figure. However, a closer look at table 1 reveals that the size of "*SD*" is correlated with the sample size: It is small for large subgroups such as married persons and large for small subgroups such as divorced persons. This pattern is consistent across different clusters of subgroups (e.g. subgroups for different levels of education or for different age categories). Therefore, I assumed that a printing error has occurred here and that the standard errors were reported in table 1 and not the standard deviations. Transforming the presumed standard errors into conventional standard deviations leads to an effect size of $d = 0.55$, which is much more plausible and thus was used in the meta-analysis.

Schaufeli and Van Yperen (1992) reported ".7" (p. 300) as standard deviation for their employed group's distress value at time two in the longitudinal analysis (table 3). The respective mean value is 6.3. The standard deviation of the employed comparison group is 6.4 with a mean of 8.2. Several other standard deviation values for employed groups from other measurement times for the same variable reported in table 2 range from 4.9 to 7.0. Thus, a printing error obviously occurred in table 3. As can be estimated from table 2, the standard deviation for "sample 2", where the error occurred, must have been around 6.6 at time two. Therefore, I estimated the standard deviation to be 6.7.

In the study of Frost and Clayson (1991) the degrees of freedom were reported to be $df = 629$ in the results section for the effect of unemployment on subjective well-being. However, the whole sample size of the study was only $n = 562$, as reported in the methods section. I used $df = 529$ instead of $df = 629$ to estimate the sample size.

Crepet, Piazzi, Vetrone, and Costa's (1993) study shows inconsistencies with regard to the sample sizes of males and females. The sums of the percentages of males and females are not identical in two different parts of table 1 and it is unclear which part is correct and which is not. As no reasonable way of correcting this error seemed available, I abstained from a

moderator analysis for sex in this case and computed only an effect size for the whole study, not for the subsamples of male and female persons.

Lahelma (1989) reports 83% cohabiting women, a number that appears to be quite large and does not add up to 100% with the other categories for marital status. As 8% does add up to 100% with the other marital status categories and appears to be more realistic, this figure was used.

# A 15. Longitudinal analyses

## A 15.1. Computation of repeated measure effect sizes

With longitudinal data it is possible to compute two different kinds of effect size: By the "raw score metric (...) the mean difference between conditions is compared with the variability of scores within each condition" (Morris & DeShon, 2002, p. 109). In contrast to that the "change score metric", also called "repeated measures effect size" (Morris & DeShon, 2002, p. 109), compares the mean change with the variability of change scores. The former statistic is appropriate when the research focus lies on group differences, while the latter statistic is appropriate for research focusing on change (Morris & DeShon, 2002). As I was interested in the amount of change in mental health that accompanies changes in a person's employment status, I used the change score metric in the present meta-analysis of longitudinal data.

Repeated measures effect sizes for change scores can easily be computed when a repeated measures *t*-value is available or when the mean and the standard deviation of the difference scores are available (Morris & DeShon, 2002). In such cases, effect sizes were computed with appropriate formulas provided in Morris and DeShon's (2002).[28] However, often only raw-score means and raw-score standard deviations for each measurement point were reported, but no change values.[29, 30] In such cases, it is possible to first compute a raw-score effect size and

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[28] In one case (Fagin & Little, 1984) means and standard deviations for change scores were not directly reported but could be estimated from a raw data list by use of statistical standard software.

[29] Sometimes only the standard deviation for one measurement point was reported (e.g. Waters & Moore, 2002b). In such cases, this standard deviation was used as an estimate of the missing standard deviation for the other measurement point. In other cases (e.g. Kanouse, et al. 1980) only standard deviations for larger groups, including other groups in addition to employed and unemployed persons, were available or only standard deviations for subgroups were available and had to be used.

then to transform this effect size into a change-score effect size. However, this requires an estimation of the correlation between T1- and T2- values, which usually was not reported in the primary studies. Therefore, I ran a series of small meta-analyses to estimate the average T1-T2-correlation for all relevant groups and used these estimates for the computation of the repeated measures effect size (for this series of meta-analyses see next chapter).

When only *p*-values from repeated measures *t*-tests were available (e.g. Bolton & Oatley, 1987; Lahelma, 1989; O'Brien & Feather, 1990), I reconstructed the *t*-value using appropriate software (Faul & Erdfelder, 1992) and proceeded as described above. When verbal statements speaking of "significant improvement" (Vinokur, Caplan, & Williams, 1987, p. 720) or "decreased significantly" (e.g. Verkleij, 1989, p. 87) were the only available information without any specification of the statistical test that was conducted, I assumed the use of a repeated measures t-test as well as conventional levels of significance and proceeded as described above in order to reconstruct the *t*-value. Verbal statement such as "did not show any change" (Vinokur, Caplan, & Williams, 1987, p. 721) or abbreviations such as "NS" (Jones, 1991, p. 13) were coded as $d = 0.00$. Missing asterisks in table of t-test-results were also coded as $d = 0.00$ (e.g. Claussen, 1999).

In two cases (Tiggemann & Winefield, 1980; Dobberstein, 1979) level-significances from Wilcoxon tests were the only available longitudinal information and had to be used to estimate the repeated measures effect size. This was done by treating this kind of information in the same way as level-significances from repeated measures *t*-tests.

When both, level-significances on the one hand and raw-score means and standard deviations on the other hand were available (e.g. Kieselbach, Klink, Scharf, & Schulz, 1998; Patton & Noller, 1984; Viinamäki; Koskela, & Niskanen, 1996; Wanberg, 1995) I used the latter to estimate the repeated measures effect size, although this required the use of meta-analytically derived estimates of the T1-T2-correlation. However, level-*p*s will almost always lead to an underestimation of the true effect size, while the use of the meta-analytically derived stability-estimates may be less systematically biased.

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[30] In one case (Harry & Tiggeman, 1992) *t*-values were reported but the numbers of their degrees of freedom were astonishingly large, much larger than the maximum sample sizes (e.g. for the GHQ-comparison the reported degrees of freedom were $df = 176$, although the number of participants was only $n = 118$). Therefore, I ignored the *t*-values and used the raw-score means and standard deviations for effect size computations here.

In one case (Cobb & Kasl, 1977) effect sizes for change in mental health were directly reported in standard scores and could be used for the meta-analysis without further treatment.

When proportions were the only available information on mental health change (e.g. Brinkmann, 1985; Büchtemann & Rosenbladt, 1981; Cohn, 1978; Heinemann, 1980; Huppert & Whittington, 1993), I abstained from using the data for the meta-analysis of longitudinal comparisons, as I do not know whether it is appropriate to compute an odds ratio for change data and to use this odds ratio to compute an effect size. None of the texts on meta-analytic methods I consulted seems to address this problem (e.g. Cooper & Hedges, 1994; Glass, McGaw, & Smitrh, 1981; Hedges & Olkin, 1985; Hunter & Schmidt, 1990; Lipsey & Wilson, 2001; Rustenbach, 2003; Schulze, Holling, & Böhning, 2003; Wang & Bushman, 1999). When only *F*-values from a repeated-measures ANOVA were available (e.g. Patton & Noller, 1990) I also abstained from the computation of an effect size for change. This was done because the correction method for F-values that is described in Morris and DeShon (1997) is explicitly said to be not appropriate for repeated measures designs by the authors.

Overall effect sizes combining the values for more than one indicator variable of mental health were computed analogous to the cross-sectional analysis, using Rosenthal and Rubin's (1986) method as described in Marin-Martinez and Sanchez-Meca (1999). To compute the appropriate weights for the repeated measures effect sizes, I used the appropriate formula for change score effect sizes provided by Morris and DeShon (2002).[31] (All repeated measures effect sizes used in the present meta-analsis are documented in appendix C, table C-6.)

## A 15.2. Estimation of mean T1-T2-correlations

Because the correlation between T1-values and T2-values was often not reported in the primary studies, a series of small meta-analyses was conducted to estimate the average correlation for the following groups of persons: (1) persons who are unemployed at both measurement times; (2) persons who change from unemployment to employment; (3) persons who are employed at both measurement times; (4) persons who change from employment to

[31] Two extremely small samples with only three participants (Fagin & Little, 1984) were excluded from the analysis, because it is not possible to compute an estimate for the sampling variance with such small sample sizes, resulting in a lack of meta-analytic weights.

unemployment; (5) persons who change from education to unemployment; (6) persons who change from education to employment. (7) persons who were in formal education at both measurement times.

All of these small meta-analyses used correlations that were either directly reported in the primary studies or could be reconstructed from other data.[32] (The correlations are documented in appendic C, table C-5.) A reconstruction was possible whenever, in addition to the raw-score standard deviations, the change score standard deviation was available. In such cases I used a formula provided by Morris and DeShon (2002) to reconstruct the T1-T2 correlation. When, in addition to the raw-score means for both measurement points, a repeated measure t-value was reported, I first reconstructed the standard deviation of the change scores with another formula provided by Morris and DeShon (2002) and then proceeded as described above [33, 34, 35]

The meta-analyses were done with the SPSS-Syntaxes provided by Lipsey and Wilson (2001) after the correlations had been Fisher-z-transformed. Weights were computed with the appropriate formula provided in Lipsey and Wilson (2001). In cases of multiple measurements within single studies (i.e. when mental health was measured with more than

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[32] In some cases the T1-T2-correlation was not reported for the relevant subgroups, but only for a larger group of persons, either combining two or more of the aforementioned groups e.g. continuously unemployed plus persons who changed from unemployment to employment (Wanberg, 1997), or including other persons who are not relevant for the present meta-analyses at all, for example students (e.g. Bachman, O'Malley, & Johnston, 1978). In such cases, the T1-T2-correlation reported for the larger group was assigned to each subgroup relevant to the present work and was used in the meta-analyses.

[33] In one case (Donovan, Oddy, Pardoe, & Ades, 1986) it was necessary to correct a printing error: The "standard error" for unemployed boys is reported to be 5.29 in this study, which results in the absurdly large standard deviation of 26.97. As a consequence, the formulas for the reconstruction of the T1-T2-correlation produced nonsensical results, i.e. a correlation of $r = 3.70$ (!). I assumed that the standard error was confused with the standard deviation here, because it is of a similar magnitude as the standard deviations for other groups reported in this study that were tested with the same scale. Therefore, I treated the reported value like a standard deviation, not like a standard error. This procedure lead to much more reasonable results.

[34] In one case (Westman, Etzion, & Horovitz, 2004) the reported and the reconstructed T1-T2-correaltions differed from each other. I preferred the correlation directly reported by the authors.

[35] Two samples from the Fagin and Little (1984)-study were not used because they were so small ($n = 3$ in both cases) that their meta-analytic weight equaled zero, making these samples useless for meta-analytic purposes.

one variable in a primary study), the mean of the Fisher-z-transformed correlations was used in the meta-analyses to avoid statistically dependent data.

## A 15.3. Selection of time intervals and groups of participants for the longitudinal analysis

When a longitudinal study has more than two measurement points (e.g. Jackson, Stafford, Banks & Warr, 1983; Vinokur, Caplan & Williams, 1987; Iversen & Sarboe, 1988; Warr & Jackson, 1983), it is theoretically possible to compute more than one effect size for longitudinal comparisons from this study, e.g. one effect size for the comparison of T1 and T2 and one effect size for the comparison of T2 and T3. However, this procedure would lead to statistically dependent data, as the same persons could be included in both effect sizes. For example, if a person is unemployed throughout the longitudinal study, he or she would influence two effect sizes on the mental health changes in continuous unemployment, causing dependent data. Therefore, I usually refrained from computing multiple effect sizes in such situations. Instead, I selected the time interval that included the largest number of change-events between different employment situations, e.g. changes from employment to unemployment. Selecting the time interval with the most change-events was done because such longitudinal data are more informative with regard to the social causation hypothesis than longitudinal data on persons who remain in the same employment situation throughout the study (e.g. continuously employed persons).[36, 37] When no changes between different

[36] Despite this rule in two studies [first study: Tiggeman & Winefield, (1984); Winefield & Tiggeman, (1990); second study: Dooley & Prause, (1995); Prause & Dooley, (2001); Dooley, Prause, & Ham-Rowbottom, (2000)] two effect sizes for change were computed for identical persons in a longitudinal study. This was done because at the first measurement point all persons were still in school. So the T1-T2-comparisons reflect only changes from school to the labor force, while the T2-T3-comparisons reflect only changes within the labor force, e.g. from employment to unemployment. Thus, there was no danger of using the same persons for the same kind of comparison more than once.

[37] In one longitudinal study with more than two measurement points (Winkelmann & Winkelmann, 1998), the data were reported only as sums of all identical change events from all available Tx - Tx+1 comparisons, combining several time intervals into one change value. In other words: All changes of a certain kind, for example, from unemployment to employment, were combined into one mean (and one standard error) value, regardless of whether they happened from T1 to T2 or from T2 to T3 etc. This procedure implies the danger of dependent data, as the *n* that is reported recurs to Tx - Tx+1 comparisons, not to persons. At least for the

employment status situations occurred in a longitudinal study (e.g. Zempel, Moser, & Jacometh, 2002), the interval with the largest sample size was selected and used for the meta-analysis.

Two longitudinal studies (Isaksson, 1990; Creed, 1999) reported longitudinal results for a group of persons who were unemployed at both measurement times, but had a spell of employment during the interval time. Both groups were not included in the meta-analysis of longitudinal results. Two other studies (Cobb & Kasl, 1977; Liira & Leino-Arjas, 1999) reported longitudinal results for a group of persons who were employed at both measurement times, but had a spell of unemployment during the interval time. These groups were also not included in the meta-analysis as the results of such comparisons are difficult to interpret.

In one case (Kanouse et al., 1980) I abstained from using longitudinal data in the meta-analysis of change scores, as the sample sizes rose considerably from T1 to T2 by augmentation with backup samples in this study. Therefore, I used this study only for the cross-sectional comparisons at T1 (testing selection effects), as this analysis is not affected by the augmentation procedure.

Sometimes a group of interest in a longitudinal study was divided into subgroups that were not relevant for the research question, e.g. employed persons looking for another job versus other employed persons (Hamilton et al., 1993). In such cases I collapsed the groups.

## A 15.4. Tests of selection effects with longitudinal data

Longitudinal studies were also used to test possible selection effects by comparing the well-being of persons who started from the same employment status at the first measurement point but differed in their employment status at the second measurement point. For example, I tested whether persons who were employed at T1 but lost their job by T2 had lower mental health scores than continuously employed persons already at T1, when both groups of persons

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continually employed persons, it is very likely that each person participated in more than one “employed-to-employed”-comparison. Some person may also have experienced the same change event, e.g. from employment to unemployment, more than once during the course of the study. However, longitudinal data sets are rare within unemployment research and no other data were available for this specific study. Therefore, I abstained from excluding the Winkelmann and Winkelmann (1998)-study and used it despite its dependent-data problem.

still were in employment. Effect sizes and sampling variances for such cross-sectional comparisons at T1 were computed with the same methods as in the other cross-sectional analyses (see chapter six).[38, 39] (All effect sizes for T1-comparisons are documented in appendix C, table C-7.)

[38] In one case (Verkleij, 1989) separate T1-comparisons for early and for late reemployed persons with continuously unemployed persons were conducted. To avoid dependent data (identical group of continuously unemployed persons used in both comparisons) I computed the sample-size weighted mean of both effect sizes.

[39] In one case (Dew, Bromet, & Penkower, 1992), the job losers were partly reemployed at T2. This was tolerated, as the comparison – continuously employed persons vs. persons who will lose their jobs in the near future - is still valid.

# Appendix B: Supplementary analyses

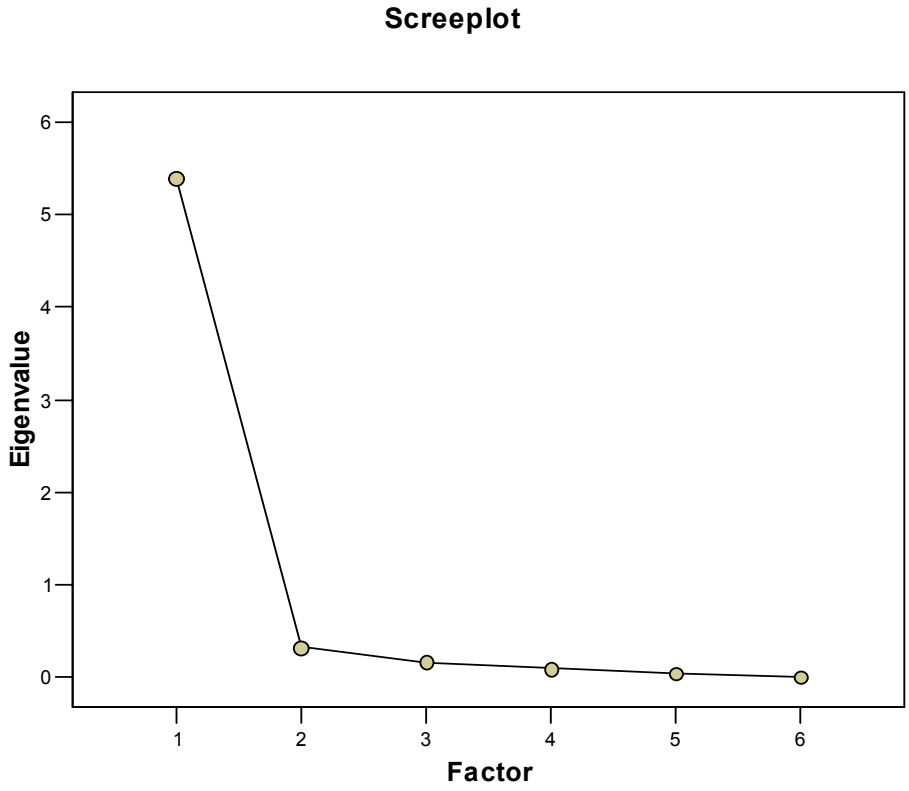



Figure B-1: **Principal component analysis for the intercorrelations of the six mental health indicators – scree-test**

Table B-1: **PCA for the intercorrelations of the six mental health indicators – Factor loadings**

| **Indicator variable** | **first unrotated component** |
|---|---|
| Mixed symptoms of distress | 0.90 |
| Depression | 0.98 |
| Anxiety | 0.96 |
| Psychosomatic symptoms | 0.93 |
| Subjective well-being | -0.97 |
| Self-esteem | -0.95 |
| Eigenvalue of the factor | 5.40 |
| Prct. explained variance | 89.91% |

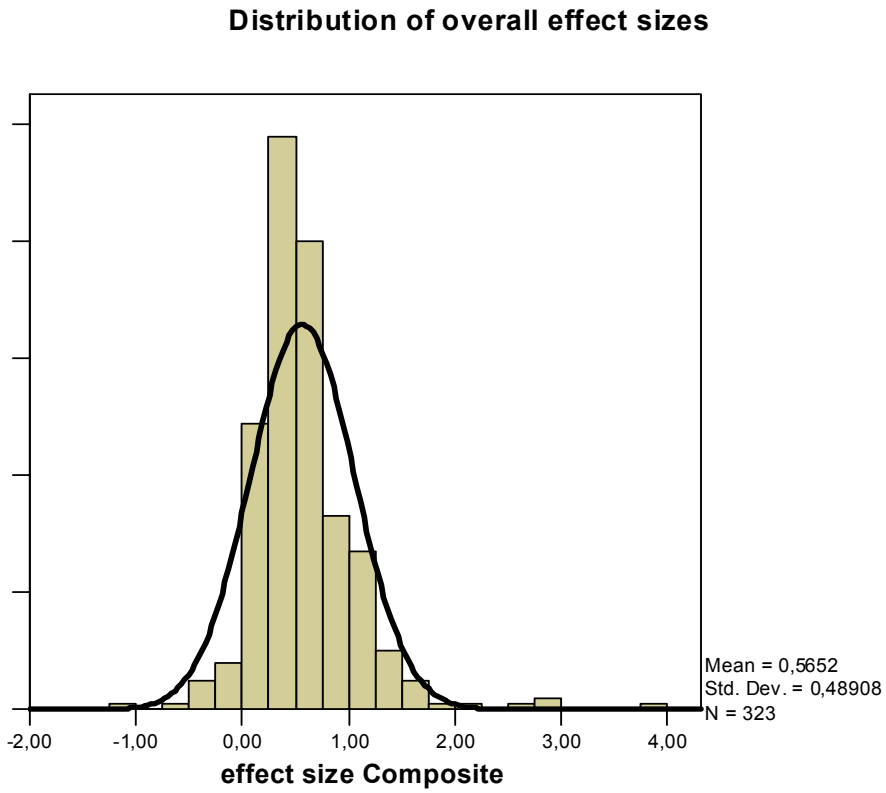



Figure B-2: **Distribution of effect size composites**

Table B-2: **Statistical parameters of unweighted effect size distribution**

| *parameter* | *value* |
|---|---|
| *Mean* | 0.57 |
| *Median* | 0.49 |
| *SD* | 0.489 |
| *Skewness* | 1.884 |
| *SE of Skewness* | 0.136 |
| *Kurtosis* | 9.208 |
| *SE of Kurtosis* | 0.271 |
| *Minimum* | -1.02 |
| *Maximum* | 3.86 |

Table B-3: **Meta-analyses of longitudinal studies: Mental health changes for six groups of persons, all indicators of mental health**

| *Status-change group* | *Variable* | *k* | *n* | *d* | *SEd* | *95% CI* | *P* | *Q* | *H* |
|---|---|---|---|---|---|---|---|---|---|
| Emp – Unemp | overall | 19 | 1933 | 0.19 | 0.0469 | 0.10 – 0.29 | 0.0000 | 55.06*** | 1.75 |
| | ms | 6 | 537 | 0.38 | 0.1344 | 0.11 – 0.64 | 0.0052 | 26.19*** | 2.29 |
| | dep. | 5 | 283 | 0.10 | 0.0956 | -0.09 – 0.28 | 0.3177 | 6.53 | 1.28 |
| | anx | 3 | 308 | 0.11 | 0.0578 | 0.002 – 0.23 | 0.0469 | 0.98 | 0.70 |
| | psysom | 5 | 723 | 0.07 | 0.0450 | -0.02 – 0.16 | 0.1101 | 5.35 | 1.16 |
| | swb | 3 | 339 | 0.34 | 0.0564 | 0.23 – 0.45 | 0.0000 | 0.60 | 0.55 |
| | se | 6 | 456 | 0.20 | 0.0480 | 0.11 – 0.29 | 0.0000 | 2.19 | 0.66 |
| Unemp – Emp | overall | 45 | 4513 | -0.35 | 0.0374 | -0.42 – -0.28 | 0.0000 | 185.30*** | 2.05 |
| | ms | 28 | 3361 | -0.40 | 0.0534 | -0.50 - -0.29 | 0.0000 | 172.25*** | 2.53 |
| | dep. | 14 | 1196 | -0.29 | 0.0472 | -0.38 – -0.20 | 0.0000 | 27.62* | 1.46 |
| | anx | 8 | 906 | -0.30 | 0.0779 | -0.45 - -0.15 | 0.0001 | 30.31*** | 2.08 |
| | psysom | 5 | 190 | 0.00 | 0.1131 | -0.22 – 0.22 | 0.9984 | 7.70 | 1.39 |
| | swb | 10 | 651 | -0.15 | 0.1026 | -0.35 – 0.05 | 0.1376 | 38.96*** | 2.08 |
| | se | 15 | 688 | -0.24 | 0.0703 | -0.37 - -0.10 | 0.0008 | 29.10* | 1.44 |
| School – Unemp | overall | 15 | 957 | 0.10 | 0.0984 | -0.09- 0.30 | 0.2988 | 91.70*** | 2.56 |
| | ms | 6 | 347 | 0.18 | 0.1236 | -0.06 – 0.42 | 0.1430 | 16.96** | 1.84 |
| | dep. | 6 | 303 | 0.22 | 0.1268 | -0.03 – 0.47 | 0.0786 | 15.69** | 1.77 |
| | anx | 1 | 135 | 0.20 | --- | --- | --- | --- | --- |
| | psysom | --- | --- | --- | --- | --- | --- | --- | --- |
| | swb | 4 | 282 | 0.25 | 0.0615 | 0.13 – 0.37 | 0.0000 | 1. 68 | 0.75 |
| | se | 11 | 857 | -0.05 | 0.1068 | -0.26 – 0.16 | 0.6590 | 71.41*** | 2.67 |
| School – Emp | overall | 16 | 6023 | -0.41 | 0.0782 | -0.45 - -0.14 | 0.0001 | 355.10*** | 4.87 |
| | ms | 7 | 1719 | -0.23 | 0.0428 | -0.31 - -0.15 | 0.0000 | 11.14+ | 1.36 |
| | dep. | 7 | 1243 | -0.10 | 0.0935 | -0.29 – 0.08 | 0.2693 | 46.24*** | 2.78 |
| | anx | 1 | 890 | -0.21 | --- | --- | --- | --- | --- |
| | psysom | --- | --- | --- | --- | --- | --- | --- | --- |

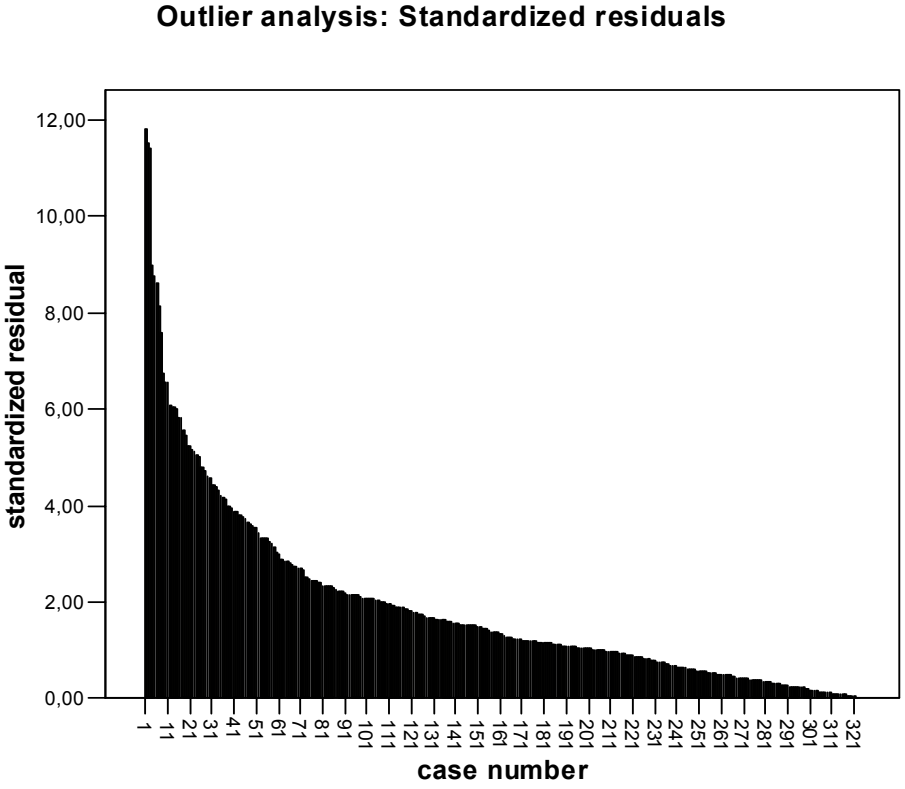
| *Status-change group* | *Variable* | *k* | *n* | *d* | *SEd* | *95% CI* | *P* | *Q* | *H* |
|---|---|---|---|---|---|---|---|---|---|
| | swb | 5 | 1219 | -0.06 | 0.0318 | -0.12 – 0.00 | 0.0641 | 4.73 | 1,09 |
| | se | 12 | 5742 | -0.26 | 0.0931 | -0.45 - -0.08 | 0.0045 | 365.39*** | 5.76 |
| Emp – Emp | overall | 28 | 24679 | -0.06 | 0.0247 | -0.11 - -0.01 | 0.0155 | 198.54*** | 2.71 |
| | ms | 11 | 4740 | -0.04 | 0.0288 | -0.10 – 0.02 | 0.1561 | 20.94* | 1.45 |
| | dep. | 9 | 5977 | -0.02 | 0.0447 | -0.11 – 0.07 | 0.6384 | 36.22*** | 2.13 |
| | anx | 4 | 391 | 0.00 | 0.0901 | -0.18 – 0.17 | 0.9716 | 8.20* | 1.65 |
| | psysom | 4 | 375 | 0.01 | 0.0563 | -0.10 – 0.12 | 0.9160 | 3.38 | 1.06 |
| | swb | 2 | 12711 | -0.02 | 0.0824 | -0.18 – 0.14 | 0.7873 | 4.60* | 2.14 |
| | se | 8 | 1915 | -0.14 | 0.0570 | -0.25 - -0.03 | 0.0162 | 32.94*** | 2.17 |
| Unemp. – unemp. | overall | 61 | 6565 | -0.08 | 0.0322 | -0.14 – -0.01 | 0.0185 | 295.25*** | 2.22 |
| | ms | 36 | 4683 | -0.09 | 0.0476 | -0.19 - 0.00 | 0.0489 | 273.13*** | 2.79 |
| | dep | 17 | 1498 | 0.00 | 0.0602 | -0.12 – 0.12 | 0.9977 | 59.17*** | 1.92 |
| | anx | 8 | 825 | -0.10 | 0.0787 | -0.25 – 0.05 | 0.2028 | 17.82* | 1.60 |
| | psysom | 7 | 254 | -0.01 | 0.1190 | -0.24 – 0.23 | 0.9487 | 13.57* | 1.50 |
| | swb | 9 | 879 | -0.07 | 0.1133 | -0.29 – 0.15 | 0.5367 | 62.49*** | 2.79 |
| | se | 23 | 1295 | -0.24 | 0.0651 | -0.36 - -0.11 | 0.0003 | 90.51*** | 2.02 |
| School - school | overall | 14 | 5564 | -0.15 | 0.0465 | -0.24 - -0.06 | 0.0013 | 107.91*** | 2.88 |
| | ms | 4 | 2463 | -0.07 | 0.0286 | -0.12 - -0.01 | 0.0206 | 4.45 | 1.22 |
| | dep | 7 | 2975 | -0.13 | 0.0631 | -0.26 - -0.01 | 0.0361 | 43.15 | 2.68 |
| | anx | --- | --- | --- | --- | --- | --- | --- | --- |
| | psysom | --- | --- | --- | --- | --- | --- | --- | --- |
| | swb | --- | --- | --- | --- | --- | --- | --- | --- |
| | se | 11 | 4637 | -0.16 | 0.0603 | -0.28 - -0.04 | 0.0079 | 109.67 | 3.31 |

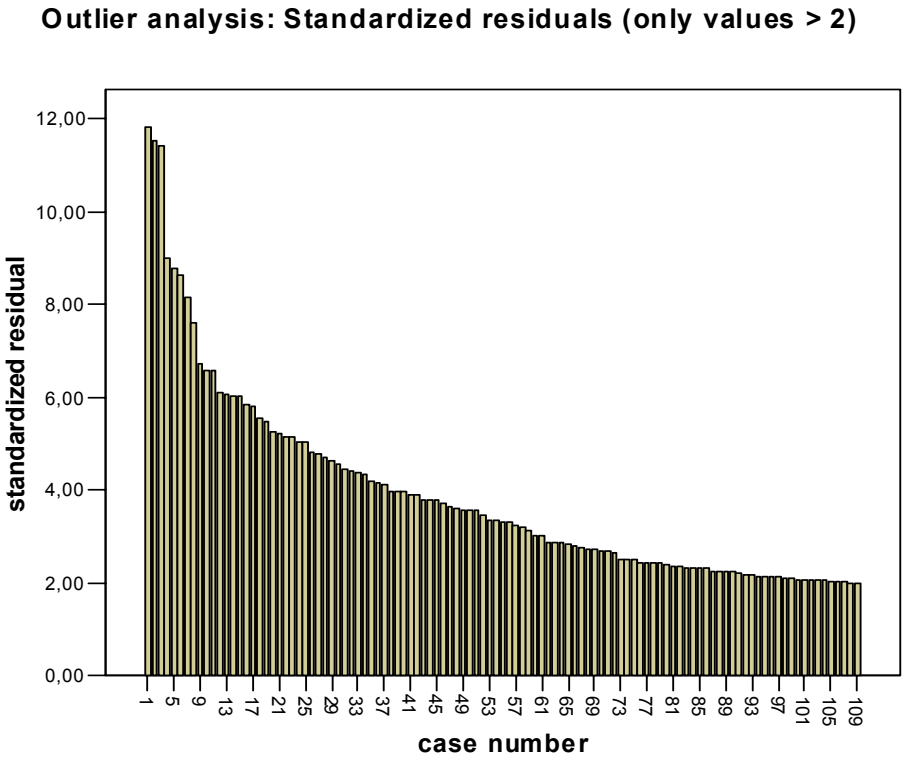
*Note.* $k$ = number of effect sizes; $n$ = total sample size; $d$ = random effects average repeated measurees effect size; *SEd* = standard error of $d$; *CI* = 95% confidence interval for $d$; $p$ = significance level of $d$; $Q$ = heterogeneity test statistic; *** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$; $H$ = descriptive measure of heterogeneity; ms = mixed symptoms of distress; dep = depression; anx = anxiety; psysom = psychosomatic symptoms; swb = subjective well-being; se = self-esteem; all computations were done with the SPSS-syntax provided in Lipsey and Wilson (2001) using a random-effects model applying the methods of moments; a positive effect size indicates an increase of distresss symptoms between T1 and T2.

Table B-4: **Meta-analyses of longitudinal studies, cross-sectional comparisons at T1: Selection effects (all indicators of mental health)**

| *Status T1-T2* | *Variable* | *k* | *n* | *d* | *SEd* | *95% CI* | *P* | *Q* | *H* |
|---|---|---|---|---|---|---|---|---|---|
| Un–Un vs. Un-Em | overall | 49 | 13259 | 0.15 | 0.0382 | 0.08 – 0.23 | 0.0000 | 136.99*** | 1.69 |
| | ms | 25 | 7261 | 0.14 | 0.0650 | 0.01 – 0.27 | 0.0286 | 114.85*** | 2.19 |
| | dep | 18 | 4147 | 0.15 | 0.0420 | 0.07 – 0.23 | 0.0003 | 21.31 | 1.12 |
| | anx | 4 | 1147 | -0.06 | 0.0629 | -0.18 – 0.07 | 0.3819 | 2.51 | 0.91 |
| | psysom | 6 | 1765 | 0.13 | 0.0775 | -0.02 – 0.28 | 0.0893 | 5.61 | 1.06 |
| | swb | 5 | 464 | 0.04 | 0.1017 | -0.16 – 0.24 | 0.6624 | 0.91 | 0.48 |
| | se | 16 | 2808 | 0.19 | 0.0778 | 0.03 – 0.34 | 0.0170 | 36.71** | 1.56 |
| Em-Un vs. Em-Em | overall | 21 | 18477 | 0.23 | 0.0402 | 0.15 – 0.31 | 0.0000 | 36.50* | 1.35 |
| | ms | 6 | 4635 | 0.22 | 0.1053 | 0.01 – 0.43 | 0.0360 | 11.31*** | 1.50 |
| | dep | 8 | 6803 | 0.20 | 0.0610 | 0.08 – 0.32 | 0.0009 | 9.61 | 1.17 |
| | anx | 3 | 382 | 0.11 | 0.1493 | -0.19 – 0.40 | 0.4774 | 2.61 | 1.14 |
| | psysom | 5 | 1998 | 0.20 | 0.0789 | 0.05 – 0.36 | 0.0109 | 5.22 | 1.14 |
| | swb | 2 | 3959 | 0.55 | 0.4143 | -0.26 – 1.37 | 0.1812 | 8.21** | 2.87 |
| | se | 6 | 2965 | 0.24 | 0.0938 | 0.05 – 0.42 | 0.0121 | 11.82* | 1.54 |
| Sc-Un vs. Sc-Em | overall | 17 | 12163 | 0.08 | 0.0281 | 0.03 - 0.14 | 0.0033 | 17.22 | 1.04 |
| | ms | 5 | 1406 | -0.03 | 0.0734 | -0.18 – 0.11 | 0.6470 | 1.19 | 0.55 |
| | dep | 6 | 973 | 0.08 | 0.0776 | -0.07 – 0.23 | 0.3029 | 2.18 | 0.66 |
| | anx | 1 | 1025 | -0.17 | --- | --- | --- | --- | --- |
| | psysom | --- | --- | --- | --- | --- | --- | --- | --- |
| | swb | 3 | 841 | 0.05 | 0.0865 | -0.12 – 0.22 | 0.5472 | 0.63 | 0.56 |
| | se | 13 | 11782 | 0.12 | 0.0375 | 0.04 – 0.19 | 0.0021 | 17.77 | 1.22 |

*Note.* $k$ = number of effect sizes; $n$ = total sample size; $d$ = average weighted effect size; *SEd* = standard error of $d$; *CI* = 95% confidence interval for $d$; $p$ = significance level of $d$; $Q$ = heterogeneity test statistic; *** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$; $H$ = descriptive measure of heterogeneity with $k$ held constant; ms = mixed symptoms of distress; dep = depression; anx = anxiety; psysom = psychosomatic symptoms; swb = subjective well-being; se = self-esteem; Un = unemployed; Em = employed; Sc = in school/studying; all meta-analytic computations were done with the SPSS-syntax provided in Lipsey and Wilson (2001) using a random effects model applying the method of moments; a positive effect size means that (1) continuously unemployed persons showed more distress an T1 than uenployed perons who found new jobs until T2, (2) job loosers showed more symptoms of distress at T1 than continuously employed persons, (3) school leavers who became unemployed showed more symptoms of diestress at T1, when still in school, than school leavers who became employed later on.




Figures B-3 and B-4: **Scree-test equivalents for standardized residuals**

Table B-5: **Study characteristics as moderators of the unemployment-distress relationship (outliers excluded)**

| *Moderator* | *Subgroup* | $Q_b$ | *p* | *k* | *n* | *d* | *Se* | *95% CI* | $Q_w$ | *H* |
|---|---|---|---|---|---|---|---|---|---|---|
| Way of publication | | 5.43 | 0.1429 | | | | | | | |
| | Journal articles | | | 226 | 184298 | 0.50 | 0.209 | 0.46 – 0.54 | 294.05** | 1.14 |
| | Books / book chapters | | | 50 | 16987 | 0.55 | 0.0465 | 0.46 – 0.64 | 43.15 | 0.94 |
| | Dissertation theses | | | 25 | 5324 | 0.46 | 0.0671 | 0.33 – 0.59 | 46.33** | 1.39 |
| | Other | | | 13 | 2388 | 0.69 | 0.0911 | 0.51 – 0.87 | 15.14 | 1.12 |
| Language | | 11.98*** | 0.0005 | | | | | | | |
| | English | | | 8 | 5178 | 0.95 | 0.1459 | 0.67 – 1.24 | 7.42 | 1.03 |
| | German | | | 40 | 14545 | 0.41 | 0.0587 | 0.30 – 0.53 | 48.23 | 1.11 |
| Questioning format | | 10.73** | 0.0011 | | | | | | | |
| | written | | | 205 | 102387 | 0.46 | 0.0217 | 0.42 – 0.50 | 257.74* | 1.12 |
| | oral | | | 85 | 91701 | 0.59 | 0.0325 | 0.52 – 0.65 | 114.35* | 1.17 |
| Focus on unemployment? | | 0.92 | 0.3366 | | | | | | | |
| | yes | | | 255 | 122948 | 0.50 | 0.0202 | 0.46 – 0.54 | 353.13*** | 1.18 |
| | no | | | 60 | 86432 | 0.54 | 0.0397 | 0.46 – 0.62 | 47.38 | 0.90 |
| Operationalization of unemployment | | 5.02 | 0.1703 | | | | | | | |
| | Officially registered | | | 130 | 44914 | 0.50 | 0.0282 | 0.44-0.55 | 181.32** | 1.19 |
| | Involuntarity explicitly mentioned | | | 52 | 39848 | 0.53 | 0.0439 | 0.44 – 0.62 | $72.84^{+}$ | 1.20 |
| | Involuntarity inferred | | | 106 | 120371 | 0.54 | 0.0304 | 0.48 – 0.60 | 93.95 | 0.95 |
| | Factory closure | | | 27 | 4246 | 0.38 | 0.0659 | 0.25 – 0.51 | 47.77** | 1.36 |
| Labor market history comparison group | | 0.29 | 0.5913 | | | | | | | |
| | Formerly unemployed | | | 69 | 16958 | 0.49 | 0.0397 | 0.41 – 0.57 | 72.56 | 1.03 |
| | other | | | 246 | 192421 | 0.51 | 0.0201 | 0.47 – 0.55 | 331.30*** | 1.16 |
| Prct. part-time in comparison group | | 9.16* | 0.0102 | | | | | | | |
| | 0% part time | | | 54 | 50267 | 0.57 | 0.0358 | 0.50 – 0.64 | 63.93 | 1.10 |
| | 1-20% part-time | | | 9 | 2330 | 0.68 | 0.0962 | 0.49 – 0.87 | 11.98 | 1.22 |
| | > 20% part-time | | | 9 | 8293 | 0.32 | 0.0859 | 0.16 – 0.49 | 8.27 | 1.02 |

*Note.* $k$ = number of correlations; $n$ = total sample size; $d$ = average repeated measures effect size; $SEd$ = standard error of $d$; $CI$ = 95% confidence interval for $d$; $p$ = significance level of $d$; $Q_b$ = between-group homogeneity estimate; $Q_w$ = within-group homogeneity estimate; $^{+}p < 0.10$; * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$; $H$ = descriptive heterogeneity statistic; all meta-analytic computations were done with the SPSS-syntax provided in Lipsey and Wilson (2001) using a random effects model applying the method of moments.

Table B-6: **Estimates of publication bias for different subgroups**

| *Moderator variable* | *subgroups* | *k* | *Tau-b* | *p* | *Standardized difference of taus* | *p* |
|---|---|---|---|---|---|---|
| level of development | HDI-index high | 190 | 0.07 | 0.132 | -0.63 | 0.5286 |
| | HDI-index low | 133 | 0.17 | 0.003 | | |
| | GDP high | 203 | 0.14 | 0.004 | 0.31 | 0.7566 |
| | GDP low | 120 | 0.09 | 0.127 | | |
| Inequality | Gini-index high | 215 | 0.08 | 0.098 | -0.61 | 0.5418 |
| | Gini-index low | 108 | 0.18 | 0.005 | | |
| | Poverty high | 203 | 0.10 | 0.031 | -0,29 | 0.7718 |
| | Poverty low | 95 | 0.15 | 0.031 | | |
| Labour market opporetunities | LMSI high | 170 | 0.13 | 0.011 | 0.13 | 0.8966 |
| | LMSI low | 150 | 0.11 | 0.049 | | |
| | Unemployment rate low | 168 | 0.09 | 0.084 | -0.38 | 0.7040 |
| | Unemployment rate high | 146 | 0.15 | 0.008 | | |
| Year of data collection | > 1984 (Median) | 156 | 0.04 | 0.429 | -0.90 | 0.3682 |
| | <= 1984 (Median) | 167 | 0.18 | 0.001 | | |
| minority status | most participants members of majority group | 65 | -0.02 | 0.847 | -0.32 | 0.7490 |
| | most participants members of minority group | 22 | 0.09 | 0.554 | | |
| age | youths (<= 21) | 57 | 0.21 | 0.019 | 0.59 | 0.5552 |
| | adults (>21) | 250 | 0.09 | 0.031 | | |
| gender | majority female | 114 | 0.16 | 0.011 | 0.24 | 0.8104 |
| | majority male female | 188 | 0.12 | 0.019 | | |
| Marital status | majority married | 124 | 0.01 | 0.893 | 0.00 | 1.0000 |
| | majority single | 30 | 0.01 | 0.929 | | |
| Education | up to 11 years of formal education | 28 | 0.19 | 0.155 | 1.01 | 0.3124 |
| | more than 11 years of formal education | 27 | -0.19 | 0.162 | | |
| occupational status | majority blue-collar | 88 | 0.13 | 0.073 | -0.56 | 0.5774 |
| | majority white-collar | 60 | 0.26 | 0.003 | | |
| Unemployment duration | short term unemployed (< 12 months) | 114 | 0.15 | 0.023 | -0.09 | 0.9282 |
| | long term unemployed (>= 12 months) | 51 | 0.17 | 0.084 | | |

Note: *k* = number of samples; *Tau-B* = rank correlation between szandardized effect sizes and sampling variances.

# Appendix C: Data documentation

Table C-1: **Coding decisions for occupational status**

| *Study* | *Coded as blue-collar* | *Not coded as blue-collar* |
|---|---|---|
| Araya, et al. (2001) | "low- status and unstable occupation – involving manual non-specialised working freelance", "low-status but stable occupation – involving manual non-specialised employees" | "middle-status occupation – involving non-manual workers, with no professional qualifications", "high-status occupation – involving non-manual professional or business people with prestigious posts" (p. 229) |
| Aubry, et al. (1990) | "skilled and semiskilled blue-collar workers´"(p. 101) | --- |
| Avery, et al. (1998) | "social class bands (…) IIIM - V" | "social class bands (…) I - IIINM" (p. 170) |
| Bachman, et al. (1978) | "nonfarm laboreres", "operatives and service workers", "craftsmen" | "professional and technical", "manager and proprietor", "clerical and sales", "farmers" (p. 16) |
| Bradburn (1969) | "blue-collar" | "white-collar" (p. 193) |
| Brenna, et al. (1987) | "blue collar workers of industrial firms" (p. 131) | --- |
| Brenner, et al. (1989) / Brenner, et al. (1988) | "blue-collar" (see study title) | --- |
| Broman, et al. (1994) | explicitly called "blue-collar workers" (p. 88) | --- |
| Brown, & Gary (1985) | "skilled jobs, service, semi-skilled and labourer occupations" | "white collar" (p. 741) |
| Büchtemann & van Rosenbladt (1981) | "Arbeiter" | „Angestellte" (p. 28) |
| Buss, & Redburn (1983) | "steelworker" (p. 72) | --- |
| Caplan, et al. (1989) | "blue-collar" | "professional and managerial", "service and clerical" (p. 761) |
| Carlson (1982) | "factory employees" (p. 11) with "low income" (p. 13), and "little or no formal job training or specific skills" (p. 64) | --- |
| Carnes (1985) | "blue collar workers" (p. 20) | --- |
| Carroll (1985) | "steelworkers" (p. 100) who also are "hourly workers" (p. 112) | --- |
| Clark, & Clissold (1982) | workers recruited "in working class suburbs", "with minimal educational attainment and low job skills" (p. 888) | --- |
| Claußen et al. (1993) | "unskilled workers" and "skilled workers" | "salaried employees", "self employed and liberal professions", "agriculture and fishers" and "others" (p. 15) |
| Cohn (1978) | "blue-collar" | "white-collar" (p. 85) |
| Cullen, et al. (1987) | "blue collar" | "white-collar" (p. 139) |
| D'Arcy, & Siddique (1985) | "blue-collar", | "white-collar", "professional" (p. 617) |
| Dalbert (1993) | "Arbeiterinnen" | „Lehrerinnen" (p. 297) |
| Dasch (1989) | --- | „white collar" (p. 68) |
| Dew et al. (1992). | "blue-collar women" (p. 751) | --- |
| Dieth (1995) | --- | "Akademiker und Führungskräfte" (p. 39) |

| *Study* | *Coded as blue-collar* | *Not coded as blue-collar* |
|---|---|---|
| Döring (1982) | “gewerblich" | "kaufmännisch" (p. 122) |
| Engel (1982) | "junge Arbeiter" (p. 116) | --- |
| Ensminger & Cementano (1988) | "blue-collar workers" | “service jobs”, “clerical and sales”, “technical, managerial and professional occupations” (p. 242) |
| Estes & Wilensky (1978) | --- | "professionals" (p. 280) |
| Evans (1986) | "skilled occupations", "partly skilled occupations", "unskilled occupations". | "professional", "intermediate", "skilled occupations (non-manual)", "others (e.g. armed forces", "unclassified” (p. 99c) |
| Fagin & Little (1984) | “blacksmith”, “HGV driver”, “assembly line operator”, “scaffolder”, “bus driver”, “miner”, “machinist”, “chef”, “sheet metal worker”, “labourer”, “service operator”, “die-caster”, “general handyman” | “TV-manager”, “safety officer”, “design engineer”, “mechanical engineer”, “engineer” (p. 19-23) |
| Feather & Bond (1983) | --- | “university graduates” (p. 241) |
| Fineman (1983) | --- | "white collar unemployment" (see title), "managers and professionals" (p. 25) |
| Frese (1979) | "Deutsche Arbeiter" (p. 228) | --- |
| Friis et al. (1998) | "miners" (p. 297) | --- |
| Frost & Clayson (1991) | "blue-collar workers" (p. 1406) | --- |
| Gallie & Vogler (1990) | "skilled manual workers", "semi- & unskilled manual workers" | "lower technicians/supervisors", "self - employed", " "routine non-manual", "lower service class", "higher service class" (p.86) |
| Garrett (1988) | "warehouse maintenance enginer", "other warehouse personnel" "truck driver" ,"meat wrapper" | "meat department manager", "meat cutter journeyman", "utility clerk", "variety clerk", "courtesy clerk", "division office person", ”pharmacist", "store manager", "food clerk" (p. 48) |
| Gien (2000) | "fishers/fishplant workers" (p. 122) | --- |
| Ginexi et al. (2000) | "unskilled laborers", "machine operators/semiskilled workers", "skilled craftsmen/clerical sales" (50% of the last group were counted as blue-collar, 50% as white-collar) | "skilled craftsmen/clerical sales" (50%), "medium business owner/minor professional or technical worker", "major business owner or professional" (p. 325) |
| Glenn (1988) | "industrial workers" (p .8) “production" and "maintenance workers” (p. 27) | --- |
| Hartley (1980) | --- | "managers" (p .149) |
| Heady & Smyth (1989) | "skilled occupations - manual", "partly skilled occupations", “unskilled occupations" | "professional occupations", "intermediate and managerial occupations", "skilled occupations - non-manual" (p. 6) |
| Heinemann et al. (1980) | "Fach- und Vorarbeiterin" and "einfache Arbeiterin" | "Selbständige", "Mithelfende im Familienbetr.", "Beamtin", "einfache Angestellte", mittlere Angestellte", "leitende Angestellte", "Sonstiges" (p. 26, Bd. 2) |
| Hepworth (1980) | "skilled III", "semi-skilled IV", "unskilled V" | "managerial II", "white-collar III” (p. 142) |
| Hodiamont et al. (1987) | "unskilled manual", "skilled manual" | "self-employed", skilled non-manual", "managerial", "professional and executive" (p. 501) |
| Huppert & Garcia (1991) | ”manual workers" " (p. 253) | ---- |
| Huppert & Whittington (1993) | "unskilled", "semi-skilled", "skilled manual" | "Professional" , "employers/managers" , "other non-manual", "unclassified/never employed", "armed forces” (p. 29) |

| *Study* | *Coded as blue-collar* | *Not coded as blue-collar* |
|---|---|---|
| Imai (2001) | --- | "nurses" (p. 13) |
| Iversen & Sarboe (1987) | "wage-earning employees" | "salaried" (p. 39) |
| Jenkins et al. (1997) | Social class "V", "IV" and "III manual" | Social class "I", "II" and "III non-manual" (p. 782) |
| Jex et al. (1994) | "laborer" and "skilled trades" | "mgr-professional", "clerical", "service-Sales" (p. 72) |
| Jones-Webb & Snowden (1993) | "Class V = upper lower; Class VI = lower" | "Social Classes I and II = upper and upper middle class; Class III = middle; Class IV = lower middle" (p. 241) |
| Joseph (1999) | --- | "white-collar workers" (p.63), "managers and executives" (p. 189) |
| Kabbe et al. (1996) | --- | "top-managers" (p. 242) |
| Kaltseis (1987) | "Matallarbeiter und Elektriker", "Hotel-Gaststätten- und Küchenberufe", "Bauberufe", "Verkehrsberufe", "Holzverarbeiter und verwandte Berufe", "Nahrungs- und Genußmittelhersteller", „Steinarbeiter, Ziegelmacher, Glasarbeiter," "Hilfsberufe allg. Art", "Reinigungsberufe", "Friseure, Schönheitspfleger und verwandte Berufe", "Textilberufe" "Technische Berufe". | "Allgemeine Verwaltungs- und Büroberufe", "Handelsberufe", "Gesundheitsberufe, Fürsorger, Sozialarbeiter", "Lehr- Kultur- und Unterhaltungsberufe" (p. 86) |
| Kasl (1979) | "blue collar workers" (p. 188) | --- |
| Kaufman (1973/1982) | --- | "professionals" (p. 16) |
| Kessler (1995) | --- | „upper level executives" (p. 95) |
| Kopacsi (1990) | "factory workers" (…) "laid of from industrial jobs" (p. 2) | --- |
| Krause (1987) | "Arbeiter" (p. 108) | --- |
| Lahelma (1989) | "blue-collar" | "white-collar" (Tab. 3.4) |
| Lai et al. (1997) | "blue-collar" | "white-collar" (p. 501) |
| Lai & Wong (1998) | "blue collar" | "white-collar" (p. 461) |
| Larson (1984) | "blue collar jobs" (p. 505) | --- |
| Laubach et al. (1999) | "angelernte Arbeiter", "ungelernte Arbeiter", „Facharbeiter" | "ausführende Angestellte" ,"mittl. Angestellte", "leitend. Angestellte", "gehobene Beamte", "hohe Beamte" (p. 82) |
| Layton (1986b/1987) | "skilled and semi-skilled workers" (p. 53) | --- |
| Leana & Feldman (1995) | "industrial workers" (p. 1381) | --- |
| Liem & Liem (1988) | "blue (…) –collar workers" | "white-collar workers" (p. 89) |
| Liira & Leino-Arjas (1999) | "blue-collar employees" (p. 43) | --- |
| Little (1973) | --- | "technical-professionals" (see title) |
| Lynd-Stevenson (1996) | "manual work" | "clerical" and "professional and managerial" (p. 123) |
| Madianos et al. (1985) | "skilled workers", "unskilled workers" | "professionals/businessmen", "merchants/medium clerical" (p. 481) |
| Margolis & Farran (1984) | "skilled and semiskilled (...) manufacturing jobs" (p. 112) | --- |
| McCarthy & | "blue collar " | "white collar" (p. 17) |

| *Study* | *Coded as blue-collar* | *Not coded as blue-collar* |
|---|---|---|
| Ronayne (1984) | | |
| McKenna & Payne (1989) | "social class D (the unskilled and semi skilled 'working class' group)" | "social class A, B and C ('middle class')" (p. 4) |
| Miles (1983) | "skilled workers and foremen", "service, semi-skilled and agricultural workers", "unskilled" | "professional workers" , "employers and managers" , "other self-employed" , "non-manual workers", "armed forces" (p. 31b) |
| Miller & Hoppe (1994) | „blue-collar“ (p. 312) | --- |
| Mohr (1993/1997) | "Industriearbeiterinnen" (p. 187) | --- |
| Ockenfels (1995) | "unskilled worker / labourer", "technical", "skilled trade" | "professional" , "clerical/secretarial", "administration/management" , "sales" , "civil service" (p. 68) |
| Ohayon et al. (1999) | "blue collar / farmers" | "executives and white collar", "craftsmen/ tradesmen", "liberal professions" (p. 303) |
| Palt (1986) | "Stahlbauschlosser", "Dachdecker", "Mechaniker", "Fliesenleger", "Friseur", „Kellner“, "Maler", "Steinmetz", "Hafner", "Maurer", "Verkäufer", „Hausgehilfin", (p. 48) | --- |
| Pelzmann (1989) | "Industriearbeiter" (p. 23), "Arbeiter im Handwerk und Baugewerbe"(p. 27), "Saisonarbeiter des Fremdenverkehrs, Hotel- und Gastgewerbes"(p. 28), "Ganzjahresbeschäftigte des Hotel- und Gastgewerbes" (p. 28) | "Schulabgänger, Maturanten"(p. 25) "Universitätsabsolventen und Angestellte im öffentlichen Dienst"(p. 26) |
| Perfetti & Bingham (1983) | "all are skilled or semi-skilled workers" (p. 196) | --- |
| Perrucci et al. (1985) | “blue collar" (p. 240) | --- |
| Pieroni (1980) | "skilled", "unskilled" | "professional/managerial", "white collar" (p. 65) |
| Platt et al. (1990) | "manual social class" (p. 128) | “nonmanual” (p. 127) |
| Roberts et al. (1997) | "manual" | "non-manual" (p. 42) |
| Rodgers (1991) | “social class” “IIIM”, “IV”, “V” | “social class” “I”, “II”, “IIIN” (OPCS classification) (p. 108) |
| Rothwell & Williams (1983) | all participants lost job due to "closure of a large steelworks" (p. 139) | --- |
| Rowlands & Huws (1995) | “mineworkers” (p. 23) | --- |
| Rudin (1986) | --- | "professional / technical / executive / managerial" (p. 57) |
| Saurel-Cubizolles et al. (2000) | "manual and service workers" | “professional or intermediate occupations", "clerks" (p. 188) |
| Schaufeli & VanYperen (1992) | --- | "highly qualified (…) technical college"-graduates (p. 291) |
| Sethi (1974) | "labour", "skilled work " | "service", "business", "professional" (p. 244) |
| Shamir (1986) | --- | "highly educated" with "university degrees" (p. 61) |
| Shams & Jackson (1994) | "semi-skilled or unskilled manual job" (p. 348) | --- |
| Sheeran & McCarthy | "working-class job losers" "all subjects were skilled, semi-skilled or manual workers and | --- |

| *Study* | *Coded as blue-collar* | *Not coded as blue-collar* |
|---|---|---|
| (1990) | came from scales C-F of the Irish Social Class Scale" (p. 353) | |
| Singh et al. (1996) | --- | "graduates", "post-graduates" (p. 632) |
| Snyder & Nowak (1984) | "craftspersons or operators" (p. 94). | --- |
| Spruit. (1989) | “blue collar professions” | “white collar professions” (p. 46) |
| Stansfield et al. (1991) | “social class group” “V”, “IV”, “IIIM” | “I”, “II”, “IIINM” (p. 162) |
| Stevens (1991) | --- | "white collar workers" (p. 52) |
| Studnicka et al. (1991) | „blue-collar“ | „white-collar“ (p. 88) |
| Walk (1989) | “Facharbeiter" (p. 89) | --- |
| Wanberg (1997) | "processing", "machine trades", " bench work", "structural work" | "professional/technical/managerial", "clerical/sales", "service", "agricultural/fishery", "miscellaneous" (p. 734) |
| Warr & Jackson (1985) / Jackson & Warr (1984) | "unskilled and semi-skilled manual work" (p. 797) | --- |
| Warr (1978) | "unskilled", "semi-skilled", "skilled workmen" | "technical", "professional" “clerks” (p. 112) |
| Waters & Moore (2002a) | “blue-collar jobs” | “white-collar jobs” (p. 19) |
| Weiß (1988) | "laborer" | "professional", "clerical" (p. 77) |
| Westman et al. (2004) | --- | “Professionals” (p. 829) |
| Weyerer & Dilling (1987) | "the lower classes include partly skilled and unskilled occupations" | “upper and middle classes” (p. 250) |
| Wuggenig (1985) | "Arbeiter ohne abgeschlossene Berufsausbildung" (p. 209) | "Hochschulabsolventen" (p. 208) |

*Note.* --- = no participants were members of the respective occupational group.

## List C-1 **Groupings of countries after median splits for nine economic / cultural variables**

**HDI high:** Canada, China (Hong Kong), Denmark, Finland, France, Germany, Japan, Netherlands, New Zealand, Norway, Sweden, Switzerland, USA.

**HDI low:** Australia, Austria, Belgium, Chile, Greece, India, Ireland, Israel, Italy, Mexico, Spain, Turkey, UK.

**GDP high:** Australia, Austria, Belgium, Canada, China (Hong Kong), Denmark, Germany, Ireland, Japan, Netherlands, Norway, Switzerland, USA.

**GDP low:** Chile, Finland, France, Greece, India, Israel, Italy, Mexico, New Zealand, Spain, Sweden, Turkey, UK.

**Gini high:** Australia, Chile, China (Hong Kong), Greece, Ireland, Israel, Italy, Mexico, New Zealand, Switzerland, Turkey, USA, UK.

**Gini low:** Austria, Belgium, Canada, Denmark, Finland, France, Germany, India, Japan, Netherlands, Norway, Spain, Sweden.

**Poverty high:** Australia, Chile, France, India, Israel, Italy, Spain, USA, UK.

**Poverty low:** Belgium, Canada, Denmark, Finland, Germany, Mexico, Netherlands, Norway, Sweden.

**Unemployment protection high:** Austria, Belgium Denmark, Finland, France, Germany, Italy, Netherlands, Norway, Spain, Sweden, Switzerland.

**Unemployment protection medium or low:** Australia, Canada, Chile, China (Hong Kong), India, Ireland, Japan, Mexico, New Zealand, UK, USA.

**LMSI high:** Australia, Austria, Canada, Denmark, Finland, Ireland, Israel, Japan, Netherlands, Norway, Sweden, UK.

**LMSI low:** Belgium, Chile, France, Germany, Greece, India, Italy, Mexico, New Zealand, Spain, Switzerland, Turkey, USA.

**Unemployment rate high:** Australia, Belgium, Canada, Denmark, Finland, France, Ireland, Italy, Netherlands, Spain, UK.

**Unemployment rate low:** Austria, Israel, Germany, Greece, Japan, New Zealand, Norway, Sweden, Switzerland, Turkey, USA.

**Individualism high (Hofstede):** Australia, Belgium, Canada, Denmark, France, Ireland, Italy, Netherlands, New Zealand, Norway, Sweden, UK, USA.

**Individualism low (Hofstede):** Austria, Chile, China (Hong Kong), Finland, Greece, Germany, India, Israel, Japan, Mexico, Spain, Switzerland, Turkey.

**Individualism high (Spector et al.):** France, New Zealand, Spain, Sweden, UK, USA.

**Individualism low (Spector et al.):** Belgium, Canada, Chile, China (Hong Kong), Germany, India, Israel.

## Table C-2: **Intercorrelations between the six indicators of mental health**

| *Study* | *Intercorrelations* |
|---|---|
| Banks & Ullah (1988) | ms * dep: r=0.57; ms * anx: r=0.63; dep * anx: r=0.53 (n=730) |
| Brinkmann & Potthoff (1983); Brinkmann (1985) | dep * psysom: r=0.56 (n=2495) |
| Broman et al. (1994) | dep*anx: r=0.76; dep * psysom: r=0.63; anx * psysom: r=0.69 (n=1081) |
| Burke (1984) | psysom * swb: r=-0.295 (n=175) |
| Buss & Redburn (1983) | dep * anx: r=0.59; dep * psysom: r=0.47; anx * psysom: r=0.58 (n=266) |
| Caplan et al. (1989) | dep * anx: r=0.85 (n=712); dep * swb: r=-0.75 (n=710); dep * se: r=-0.59 (n=711); anx * se: r=-0.51 (n=711); anx * swb: r=-0.63 (n=710); swb * se: r=0.69 (n=710) |
| Carnes (1985) | dep * anx: r=0.47 (n=100) |
| Creed, et al. (1996) | ms * se: r= -0.25 (n=82) |
| Creed, et al. (1999) | ms * se: r= -0.65; ms * swb: r= -0.53; se * swb: r= 0.57 (n=65) |
| Creed (1999) | ms * se: r=-0.49 (n=166) |
| Crepet et al. (1993) | ms* dep: r=0.59; ms * se: r=-0.36; dep * se: r=-0.50 (n=348) |
| Dalbert (1993) | ms * swb : r= -.43 (unemp n=54); ms * swb r = -.41 (emp n=31); ms * se: r = -.57 (unemp n=54); ms * se: r = -.32 (emp n=31) ; swb * se: r = .57 (unemp n=54); swb * se: r=.04 (emp n=31) |
| Derenzo (1989) | dep * se: r=-0.59 (unemp n=120); dep * se: r=-0.57 (emp n=80); dep * swb: r = -.53 (unemp n=120); dep * swb: r = -0.35 (emp n=80) ; swb * se: r=0.49 (unemp n=121); swb * se: r=0.50 (emp n=80) |
| Dew et al. (1992) | dep * anx: r=0.77 (n=64) |
| Feather & Bond (1983) | dep * se: r=-0.69 (unemp n=43). dep * se: r=-0.54 (emp n=255) |
| Gowan et al. (1999) | dep * anx: r = 0.57 (n=210) |
| Graetz (1991) | anx * se: r=-0.43 (n=5638) |
| Harper (1987) | swb * se: r=0.36 (unemp n=146); swb * se: r=0.46 (emp n=2385) |
| Hepworth (1980) | ms * swb: r=-0.58 (n=648) |
| Jex et al. (1994) | dep * anx: r=0.58 (n=320); dep * swb: r=-0.36 (n=325); dep * se: r=-0.42 (n=320); anx * swb: r=-0.43 (n=325); anx * se: r=-0.39 (n=324); swb * se: r=0.50 (n=325) |
| Kasl (1979); Cobb & Kasl (1977) | "average intercorrelation" of the scales (dep, anx, psyom, se) : r=0.38 (n=125) |
| Kessler et al. (1987) | dep * anx: r=0.81; dep * psysom: r=0.63; anx * psysom: r=0.65 (n=330) |
| Kinicki et al. (2000) | swb * se: r=0.41 (n=100) |
| Kokko & Pulkkinen (1998) | ms * dep: r=0.50 (males); ms * dep: r=0.18 (females); ms * anx: r=0.41 (males). ms * anx r=0.02 (females); ms * se: r=-0.39 (males). ms * se: r=-0.20 (females); dep * anx: r=0.61 (males). dep * anx: r=0.57 (females); dep * se: r=-0.48 (males); dep * se: r=-0.36 (females); anx * se: r=-0.62 (males); anx * se: r=-0.39 (females) (males: n=88; females: n=88) |
| Kopacsi (1990) | ms * se: r=-0.25; ms * swb: r=-0.38; swb * se: r=0.26 (n=74) |
| Lai & Chan (2002) | ms * swb: r = -.40 (n=48) |
| Leana & Feldman (1995) | ms * anx: r=0.30; ms * psysom: r=0.36; ms * swb: r=-0.14; anx * psysom: r=0.69; anx * swb: r=-0.46; psysom * swb: r=-0.54 (n=59) |
| Little (1973) | dep * psysom: r= 0.52 (n=100) |
| McKenna & Payne (1989) | dep * anx: r=0.71 (n=185) |
| Miller & Hoppe (1994) | dep * anx: r = 0.81 (n=441) |
| Mohr (1993/1997) | dep * psysom: r=0.49 (n=48) |
| Morch (1986) | dep * se: r=-0.45 (n=225) |
| Patton & Noller (1984) | dep * se: r=-0.65 (n=45) |
| Patton & Noller (1990) | ms * dep: r=0.72; dep * swb: r=-0.69; dep * se: r=-0.56 (n=87) |
| Pieroni (1980) | swb * anx: r = -.39; se * swb: r = .25; se * anx: r = -.29 (n=35) |
| Prussia et al. (1993) | swb * se = 0.42 (n=79) |
| Schaufeli & VanYperen | dep * anx: r=0.79; dep * psysom: r=0.65; psysom * anx: r=0.68 (n=375); |

| *Study* | *Intercorrelations* |
|---|---|
| (1992) | dep * anx: r=0.73; dep * psysom: r=0.61; psysom * anx: r=0.69 (n=169) |
| Schmid (2004) | ms * swb: r = -0.52 (n=201) |
| Shamir (1986) | dep * anx: r=0.53; dep * swb: r=-0.42; dep * se: r= -0.42 ; anx * swb: r= -0.52; anx * se: r=-0.24; swb * se: r=0.27 (n=240) |
| Tiggemann & Winefield (1984) | dep * se: r=-0.59 (n=761) |
| Wanberg (1997) | ms * se: r=-0.60 (n=363) |
| Waters & Moore (2002a) | dep * se: r=-0.46 (unemp n=201); dep * se: r=-0.36 (emp n=128) |
| Wolf (2004) | ms * swb: r = -0.54 (n=256) |

*Note. n* = sample size; *r* = correlation; ms = mixed symptoms of distress; dep = depression; anx = anxiety; psysom = psychosomatic symptoms; swb = subjective well-being; se = self-esteem.

Table C-3: **Reliability estimates**

| *Study* | *Mental health variable* | *Reliability* |
|---|---|---|
| Bachman et al. (1978) | se | 0,81 |
| Banks & Ullah (1988) | anx | 0,74 |
| | anx | 0,74 |
| | anx | 0,74 |
| | anx | 0,74 |
| Banks & Jackson (1982) | ms | 0,85 |
| | ms | 0,83 |
| Behle (2001) | ms | 0,56 |
| | ms | 0,55 |
| Beiser et al. (1993) | dep | 0,88 |
| | dep | 0,88 |
| Bolton & Oatley (1987) | dep | 0,78 |
| Brenner et al. (1989) / Brenner & Starrin (1988) | ms | 0,88 |
| Hall & Johnson (1988) | dep | 0,89 |
| Brinkmann & Potthoff (1983), Brinkmann (1985) | dep | 0,72 |
| | psysom | 0,70 |
| | dep | 0,72 |
| | psysom | 0,70 |
| | dep | 0,72 |
| | psysom | 0,70 |
| | dep | 0,72 |
| | psysom | 0,70 |
| Broman et al. (1994) | dep | 0,87 |
| | anx | 0,86 |
| Buss & Redburn (1983) | dep | 0,61 |
| | anx | 0,59 |
| | psysom | 0,60 |
| Campbell et al. (1976) | swb | 0,89 |
| | swb | 0,89 |
| Caplan et al. (1989) | dep | 0,84 |
| | anx | 0,87 |
| | swb | 0,87 |
| | se | 0,72 |
| Creed & Reynolds (2001) | ms | 0,95 |
| Dalbert (1993) | ms | 0,91 |
| | swb | 0,82 |
| | se | 0,72 |
| Derenzo (1989) | dep | 0,86 |
| | swb | 0,86 |
| | se | 0,85 |
| Dressler (1986) | dep | 0,79 |
| Feather & O'Brien (1986) | ms | 0,89 |
| | dep | 0,78 |
| | swb | 0,86 |
| | se | 0,87 |
| Feather & Bond (1983) | dep | 0,84 |

| *Study* | *Mental health variable* | *Reliability* |
|---|---|---|
| | se | 0,90 |
| Frese (1979) | dep | 0,92 |
| Ginexi et al. (2000) | dep | 0,93 |
| Gowan et al. (1999) | dep | 0,90 |
| | anx | 0,75 |
| Halvorsen (1998) | ms | 0,92 |
| Hammer (1993) | ms | 0,86 |
| Harper (1987) | swb | 0,62 |
| | se | 0,74 |
| Heubeck et al. (1995) | dep | 0,85 |
| | dep | 0,85 |
| Jex et al. (1994) | dep | 0,86 |
| | anx | 0,90 |
| | swb | 0,86 |
| | se | 0,87 |
| Jones-Webb & Snowden (1993) | dep | 0,88 |
| | dep | 0,88 |
| Kessler et al. (1987) | dep | 0,90 |
| | anx | 0,80 |
| | psysom | 0,85 |
| Kinicki (1985) | anx | 0,86 |
| Kinicki et al. (2000) | swb | 0,82 |
| | se | 0,77 |
| Kokko & Pulkkinen (1998) | ms | 0,88 |
| | dep | 0,89 |
| | anx | 0,91 |
| | se | 0,79 |
| Kopacsi (1990) | ms | 0,84 |
| | swb | 0,84 |
| | se | 0,78 |
| Koskela et al. (1994) | ms | 0,96 |
| | dep | 0,89 |
| | psysom | 0,93 |
| | ms | 0,96 |
| | dep | 0,89 |
| | psysom | 0,93 |
| | ms | 0,96 |
| | dep | 0,89 |
| | psysom | 0,93 |
| | ms | 0,96 |
| | dep | 0,89 |
| | psysom | 0,93 |
| Lahelma (1989) | ms | 0,92 |
| | ms | 0,92 |
| Lai & Wong (1998) | ms | 0,87 |
| Lai & Chan (2002) | ms | 0,80 |
| | swb | 0,84 |
| Lai et al. (1997) | ms | 0,89 |
| | dep | 0,78 |
| | anx | 0,88 |

| *Study* | *Mental health variable* | *Reliability* |
|---|---|---|
| Leana & Feldman (1995) | ms | 0,97 |
| | anx | 0,84 |
| | psysom | 0,87 |
| | swb | 0,86 |
| Liira & Leino-Arjas (1999) | psysom | 0,91 |
| | psysom | 0,91 |
| Macky (1984) | ms | 0,87 |
| Martella & Maass (2000) | swb | 0,83 |
| McLoyd et al. (1994) | dep | 0,84 |
| Meeus et al. (1997) | ms | 0,86 |
| | dep | 0,83 |
| Miller & Hoppe (1994) | dep | 0,89 |
| | anx | 0,82 |
| Mohr (1993/1997) | dep | 0,85 |
| | psysom | 0,87 |
| Morch (1986) | dep | 0,76 |
| | se | 0,77 |
| O'Brien & Kabanoff (1979) | ms | 0,65 |
| | swb | 0,65 |
| Patton & Noller (1990) | ms | 0,69 |
| | swb | 0,69 |
| Paul et al. (2004) | dep | 0,75 |
| Pernice & Long (1996) | ms | 0,88 |
| | se | 0,81 |
| Pernice et al. (2000) | ms | 0,85 |
| Prause & Dooley (2001) / Dooley et al. (2000) | dep | 0,88 |
| | swb | 0,67 |
| | se | 0,80 |
| Roberts et al. (1981) | dep | 0,78 |
| | dep | 0,74 |
| Rudin (1986) | ms | 0,93 |
| Schaufeli & VanYperen (1992) | ms | 0,87 |
| | ms | 0,87 |
| | ms | 0,87 |
| | ms | 0,87 |
| Schmid (2004) | ms | 0,90 |
| | swb | 0,90 |
| Shamir (1986a) | anx | 0,67 |
| | se | 0,86 |
| | dep | 0,79 |
| | anx | 0,67 |
| | se | 0,86 |
| Shams & Jackson (1994) | ms | 0,89 |
| | dep | 0,68 |
| | anx | 0,75 |
| Sheeran & McCarthy (1990) | se | 0,76 |
| | se | 0,76 |
| Singh et al. (1996) | se | 0,83 |
| | se | 0,83 |
| Spruit (1989) | dep | 0,82 |

| *Study* | *Mental health variable* | *Reliability* |
|---|---|---|
| Stevens (1991) | se | 0,51 |
| Tiggeman & Winefield (1984) | dep | 0,58 |
| | se | 0,71 |
| | dep | 0,58 |
| | se | 0,71 |
| Vinokur et al. (1987) | ms | 0,83 |
| Vinokur et al. (2000) | dep | 0,91 |
| Vuori & Vesalainen (1999) | ms | 0,96 |
| Wacker & Kolobkova (2000) | se | 0,84 |
| Wanberg (1995) | ms | 0,92 |
| | swb | 0,90 |
| Wanberg (1997) | ms | 0,94 |
| Wanberg et al. (1997) | ms | 0,93 |
| Warr & Jackson (1985) / Jackson & Warr (1984) | ms | 0,96 |
| Waters & Moore (2002a) | dep | 0,94 |
| | se | 0,79 |
| Westman | anx | 0,94 |
| Whelan (1992) | ms | 0,82 |
| Wolf (2004) | ms | 0,91 |
| | swb | 0,92 |

*Note.* ms = mixed symptoms of distress; dep = depression; anx = anxiety; psysom = psychosomatic symptoms; swb = subjective well-being; se = self-esteem.

Table C-4: **Proportion of cases of psychological disorder according to clinical screening tests**

| *Study* | *mental health variable* | *n - unemp* | *n - emp* | *proportion cases unemp.* | *proportion cases emp.* |
|---|---|---|---|---|---|
| Araya et al. (2001) | ms | 121 | 1822 | 0.38 | 0.21 |
| | ms | 124 | 336 | 0.52 | 0.43 |
| | ms | 81 | 658 | 0.33 | 0.22 |
| Banks & Ullah (1988) | ms | 55 | 18 | 0.41 | 0.15 |
| | ms | 168 | 80 | 0.41 | 0.15 |
| | ms | 114 | 36 | 0.30 | 0.11 |
| | ms | 176 | 83 | 0.30 | 0.11 |
| Brenna et al. (1987) | ms | 302 | 241 | 0.07 | 0.28 |
| Brown & Gary (1985) | dep | 109 | 246 | 0.46 | 0.19 |
| Carnes (1985) | dep | 50 | 50 | 0.44 | 0.16 |
| Clark & Oswald (1994) | ms | 248 | 1582 | 0.50 | 0.31 |
| | ms | 168 | 2941 | 0.60 | 0.32 |
| | ms | 106 | 1104 | 0.47 | 0.23 |
| Claussen et al. (1993) | overall | 164 | 113 | 0.26 | 0.18 |
| . | ms | 164 | 113 | 0.34 | 0.28 |
| | dep | 164 | 113 | 0.25 | 0.13 |
| | anx | 164 | 113 | 0.20 | 0.12 |
| Cullen et al. (1987) | overall | 28 | 17 | 0.29 | 0.00 |
| | ms | 28 | 17 | 0.25 | 0.00 |
| | dep | 28 | 17 | 0.32 | 0.00 |
| Cullen et al. (1987) | overall | 21 | 37 | 0.29 | 0.21 |
| | ms | 21 | 37 | 0.29 | 0.31 |
| | dep | 21 | 37 | 0.29 | 0.11 |
| Dooley et al. (1994) | overall | 339 | 8059 | 0.05 | 0.04 |
| | dep | 342 | 8098 | 0.04 | 0.02 |
| | anx | 339 | 8059 | 0.07 | 0.06 |
| Eaton & Kessler (1981) | dep | 149 | 1608 | 0.28 | 0.13 |
| Hall & Johnson (1988) | dep | 96 | 51 | 0.41 | 0.13 |
| Halvorsen (1998) | ms | 220 | 116 | 0.19 | 0.07 |
| Hannan et al. (1997) | ms | 217 | 815 | 0.28 | 0.07 |
| Harrison et al. (1999) | ms | 2139 | 15924 | 0.40 | 0.21 |
| Hepworth (1980) | ms | 20 | 268 | 0.80 | 0.15 |
| | ms | 31 | 170 | 0.61 | 0.19 |
| | ms | 10 | 10 | 0.70 | 0.30 |
| | ms | 9 | 27 | 0.46 | 0.11 |
| | ms | 8 | 95 | 0.25 | 0.24 |
| Hinton et al. (1998) | dep | 283 | 595 | 0.19 | 0.04 |
| | dep | 221 | 745 | 0.14 | 0.05 |
| | dep | 109 | 852 | 0.15 | 0.07 |
| Hodiamont et al. (1987) | ms | 178 | 993 | 0.10 | 0.05 |
| | ms | 53 | 274 | 0.10 | 0.06 |
| Huppert & Garcia (1991) | ms | 95 | 525 | 0.45 | 0.24 |
| | ms | 67 | 472 | 0.51 | 0.18 |
| Huppert & Whittington (1993) | ms | 36 | 512 | 0.36 | 0.23 |
| | ms | 15 | 400 | 0.47 | 0.21 |
| Iversen & Sarboe (1987) | ms | 534 | 929 | 0.23 | 0.07 |
| Jenkins et al. (1997) | ms | 847 | 5034 | 0.26 | 0.12 |
| Jones-Webb & Snowden (1993) | dep | 62 | 164 | 0.25 | 0.16 |
| | dep | 20 | 140 | 0.34 | 0.12 |

| *Study* | *mental health variable* | *n - unemp* | *n - emp* | *proportion cases unemp.* | *proportion cases emp.* |
|---|---|---|---|---|---|
| Lahelma (1989) | ms | 268 | 94 | 0.54 | 0.18 |
| | ms | 251 | 90 | 0.37 | 0.17 |
| Lai et al. (1997) | ms | 86 | 79 | 0.54 | --- |
| Margraf & Poldrack (2000) | anx | 171 | 1185 | 0.11 | 0.05 |
| McCarthy & Ronayne (1984) | ms | 207 | 388 | 0.67 | 0.26 |
| McDonald et al. (1996) | ms | 35 | 45 | 0.54 | 0.16 |
| McPherson & Hall (1983) | ms | 139 | 161 | 0.48 | 0.27 |
| Melville et al. (1985) | overall | 98 | 98 | 0.51 | 0.18 |
| | ms | 98 | 98 | 0.83 | 0.3 |
| | dep | 98 | 98 | 0.18 | 0.06 |
| Miller & Hoppe (1994) | overall | 441 | 467 | --- | 0.10 |
| | dep | 441 | 467 | --- | 0.10 |
| | anx | 441 | 467 | --- | 0.10 |
| Morrell et al. (1994) | ms | 462 | 372 | 0.49 | 0.37 |
| | ms | 523 | 772 | 0.39 | 0.26 |
| Ohayon et al. (1999) | dep | 186 | 2775 | 0.12 | 0.04 |
| Platt et al. (1990) | ms | 26 | 219 | 0.58 | 0.32 |
| Prause & Dooley (2001) / Dooley et al. (2000) | dep | 521 | 5113 | 0.33 | 0.16 |
| Radloff (1975) | dep | 77 | 381 | 0.30 | --- |
| | dep | 24 | 644 | 0.30 | --- |
| Roberts et al. (1997) | ms | 689 | 6298 | 0.36 | 0.22 |
| Rodgers (1991) | ms | 75 | 1525 | 0.12 | 0.03 |
| | ms | 55 | 454 | 0.13 | 0.08 |
| Rowlands & Huws (1995) | ms | 93 | 91 | 0.79 | 0.43 |
| Saurel-Cubizolles et al. (2000) | ms | 47 | 93 | 0.45 | 0.29 |
| | ms | 28 | 130 | 0.50 | 0.35 |
| | ms | 18 | 201 | 0.56 | 0.27 |
| Sethi et al. (1974) | ms | 191 | 1337 | 0.08 | 0.03 |
| Stansfield et al. (1991) | ms | 331 | 1038 | 0.29 | 0.16 |
| Toppen (1971) | ms | 50 | 50 | 0.30 | 0.08 |
| Verhaegen et al. (1994) | dep | 129 | 165 | 0.44 | 0.27 |
| | dep | 171 | 133 | 0.57 | 0.45 |
| Viinamäki et al. (2000) | ms | 115 | 442 | 0.28 | 0.13 |
| | ms | 131 | 100 | 0.24 | 0.18 |
| | ms | 72 | 387 | 0.19 | 0.15 |
| | ms | 86 | 395 | 0.35 | 0.19 |
| | ms | 109 | 511 | 0.37 | 0.25 |
| | ms | 84 | 426 | 0.33 | 0.24 |
| Westcott (1985) | ms | 22 | 46 | 0.20 | 0.15 |
| Weyerer & Dilling (1987) | ms | 9 | 296 | 0.78 | 0.21 |
| | ms | 16 | 434 | 0.56 | 0.13 |

*Note.* ms = mixed symptoms of distress; dep = depression; anx = anxiety; psysom = psychosomatic symptoms; swb = subjective well-being; se = self-esteem; overall = average of the proportions of the more specific measures of mental health; *n - unemp* = sample size of unemployed group; *n - emp* = sample size of employed group.

Table C-5: **T1-T1-correlations from longitudinal studies for six groups of persons with different employment tracks**

| *Study* | *Var-iable* | *r uu* | *n uu* | *r ue* | *n ue* | *r eu* | *n eu* | *r ee* | *n ee* | *r su* | *n su* | *r se* | *n se* | *r ss* | *n ss* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Bachman et al. (1978) | se | | | | | | | | | 0.30 | 111 | 0.30 | 1205 | 0.48 | 1488 |
| Behle (2001) | ms | 0.58 | 249 | 0.58 | 159 | | | | | | | | | | |
| | ms | 0.62 | 337 | 0.62 | 319 | | | | | | | | | | |
| Cobb & Kasl (1977) | overall | | | | | | | 0.62 | 43 | | | | | | |
| | dep | | | | | | | 0.62 | 43 | | | | | | |
| | anx | | | | | | | 0.62 | 43 | | | | | | |
| | psysom | | | | | | | 0.62 | 43 | | | | | | |
| | se | | | | | | | 0.62 | 43 | | | | | | |
| Creed et al. (1999) | overall | 0.61 | 38 | | | | | | | | | | | | |
| | ms | 0.39 | 38 | | | | | | | | | | | | |
| | swb | 0.69 | 38 | | | | | | | | | | | | |
| | se | 0.69 | 38 | | | | | | | | | | | | |
| | overall | 0.93 | 21 | | | | | | | | | | | | |
| | ms | 0.95 | 22 | | | | | | | | | | | | |
| | swb | 0.90 | 21 | | | | | | | | | | | | |
| Dew et al. (1992) | overall | | | | | | | 0.46 | 68 | | | | | | |
| | dep | | | | | | | 0.46 | 68 | | | | | | |
| | anx | | | | | | | 0.45 | 68 | | | | | | |
| Donovan et al. (1986) | ms | | | | | | | | | 0.58 | 26 | 0.51 | 29 | | |
| | ms | | | | | | | | | 0.02 | 14 | 0.17 | 16 | | |
| Dooley et al. (2000) | dep | | | | | 0.42 | 170 | 0.42 | 4437 | | | | | | |
| Fagin & Little (1984) | ms | 0.73 | 6 | | | | | | | | | | | | |
| Fineman (1983) | overall | 0.81 | 17 | 0.46 | 28 | | | | | | | | | | |
| | overall | | | 0.83 | 12 | | | | | | | | | | |
| | ms | 0.64 | 17 | 0.29 | 28 | | | | | | | | | | |
| | ms | | | 0.91 | 12 | | | | | | | | | | |
| | psysom | 0.91 | 17 | 0.62 | 28 | | | | | | | | | | |
| | psysom | | | 0.74 | 12 | | | | | | | | | | |
| | se | 0.80 | 17 | 0.44 | 28 | | | | | | | | | | |
| | se | | | 0.80 | 12 | | | | | | | | | | |
| Frese et al. (2002) | overall | 0.85 | 8 | | | | | | | | | | | | |
| | ms | 0.92 | 8 | | | | | | | | | | | | |
| | dep | 0.44 | 8 | | | | | | | | | | | | |
| | psysom | 0.93 | 8 | | | | | | | | | | | | |
| Graetz (1991/1993)[a] | ms | | | 0.28 | 521 | 0.35 | 323 | | | 0.23 | 152 | 0.41 | 954 | | |
| Hartley (1980) | se | 0.50 | 23 | | | | | 0.83 | 50 | | | | | | |

| *Study* | *Var-iable* | *r uu* | *n uu* | *r ue* | *n ue* | *r eu* | *n eu* | *r ee* | *n ee* | *r su* | *n su* | *r se* | *n se* | *r ss* | *n ss* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | se | 0.59 | 47 | | | | | | | | | | | | |
| Isaksson (1990) | ms | 0.32 | 10 | 0.45 | 23 | -0.19 | 9 | 0.79 | 18 | | | | | | |
| Joseph (1999)[b] | overall | 0.54 | 33 | 0.54 | 19 | | | | | | | | | | |
| | ms | 0.57 | 33 | 0.57 | 19 | | | | | | | | | | |
| | dep | 0.51 | 33 | 0.51 | 19 | | | | | | | | | | |
| Kanouse et al. (1980) | se | | | | | | | 0.33 | 2519 | | | 0.33 | 2519 | | |
| | se | | | | | | | 0.45 | 2149 | | | 0.45 | 2149 | | |
| Kinicki et al. (2000) | overall | 0.38 | 78 | 0.38 | 13 | | | | | | | | | | |
| | swb | 0.28 | 78 | 0.28 | 13 | | | | | | | | | | |
| | se | 0.47 | 78 | 0.47 | 13 | | | | | | | | | | |
| | overall | | | 0.38 | 9 | | | | | | | | | | |
| | swb | | | 0.28 | 9 | | | | | | | | | | |
| | se | | | 0.47 | 9 | | | | | | | | | | |
| Lai et al. (2002) | overall | 0.47 | 26 | 0.47 | 22 | | | | | | | | | | |
| | ms | 0.45 | 26 | 0.45 | 22 | | | | | | | | | | |
| | swb | 0.48 | 26 | 0.48 | 22 | | | | | | | | | | |
| Layton (1986b/1987 a) | overall | 0.47 | 39 | 0.54 | 62 | | | | | | | | | | |
| | ms | 0.45 | 39 | 0.58 | 62 | | | | | | | | | | |
| | anx | 0.50 | 39 | 0.49 | 62 | | | | | | | | | | |
| Layton (1986a) | ms | | | | | | | | | 0.65 | 29 | 0.63 | 77 | 0.44 | 80 |
| Mohr (1993/1997) | overall | 0.53 | 15 | 0.53 | 33 | | | | | | | | | | |
| | dep | 0.54 | 15 | 0.54 | 33 | | | | | | | | | | |
| | psysom | 0.53 | 15 | 0.53 | 33 | | | | | | | | | | |
| Morch (1986) | overall | 0.61 | 54 | 0.64 | 42 | | | | | | | | | | |
| | dep | 0.65 | 54 | 0.77 | 42 | | | | | | | | | | |
| | se | 0.56 | 54 | 0.46 | 42 | | | | | | | | | | |
| Prause & Dooley (2001) | dep | 0.57 | 117 | 0.50 | 208 | | | | | | | | | | |
| Vinokur et al. (2000) | dep | 0.36 | 343 | 0.36 | 1087 | | | | | | | | | | |
| Vuori & Vesalainen (1999) | ms | 0.48 | 311 | | | | | | | | | | | | |
| Wanberg (1995) | ms | | | 0.31 | 84 | | | | | | | | | | |
| Wanberg (1997) | ms | 0.47 | 186 | 0.47 | 177 | | | | | | | | | | |
| Wanberg et al. (1997) | ms | 0.70 | 163 | 0.37 | 40 | | | | | | | | | | |
| Warr & Jackson (1983) (group A)[b] | se | | | | | | | 0.40 | 274 | | | | | | |
| (group B)[b] | se | | | | | | | 0.39 | 335 | | | | | | |
| Westman et al. (2004) | anx | 0.55 | 113 | | | | | 0.43 | 113 | | | | | | |

| *Study* | *Variable* | *r uu* | *n uu* | *r ue* | *n ue* | *r eu* | *n eu* | *r ee* | *n ee* | *r su* | *n su* | *r se* | *n se* | *r ss* | *n ss* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Zempel et al. (2000) | overall | 0.77 | 33 | | | | | | | | | | | | |
| | dep | 0.63 | 33 | | | | | | | | | | | | |
| | psysom | 0.88 | 33 | | | | | | | | | | | | |
| | Se | 0.74 | 33 | | | | | | | | | | | | |

*Note. uu* = unemployed – unemployed; *ue* = unemployed – employed; *eu* = employed – unemployed; *ee* = employed – employed; *se* = school – employed; *su* = school – unemployed; *ss* = school – school; ms = mixed symptoms of distress; dep = depression; anx = anxiety; psysom = psychosomatic symptoms; swb = subjective well-being; se = self-esteem; [a] T1-T2-correlations only reported for mixed symptoms (GHQ) here, because other measures only subscales of GHQ; [b] mean of T1-T2-correlations for two subscales; correlations were Fisher-z-transformed before meta-analysing them; in case of studies with multiple measurement of mental health overall effect sizes were computed by averaging the Fisher-z-transformed correlations for single indicators of mental health, e.g. depression and anxiety.

Table C-6: **Repeated measures effect sizes for six groups of persons with different employment tracks**

| *Study* | *Int.* | *Time* | *MH-* | *Unun* | *Unun* | *Unem* | *Unem* | *Emun* | *Emun* | *Emem* | *Emem* | *Scun* | *Scun* | *Scem* | *Scem* | *Scsc* | *Scsc* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | *t1-t2* | *ind* | *n* | *es* | *n* | *es* | *n* | *es* | *n* | *es* | *n* | *es* | *n* | *es* | *n* | *es* |
| Bachman et al. (1978) | no | 90 | se | | | | | | | | | 111 | -0.58 | 1205 | -0.88 | 1488 | -0.26 |
| Banks & Ullah (1988) | no | 12 | ov | 513 | -0.04 | 217 | -0.28 | | | | | | | | | | |
| | | | ms | 513 | -0.09 | 217 | -0.23 | | | | | | | | | | |
| | | | dep | 513 | 0.00 | 217 | -0.23 | | | | | | | | | | |
| | | | anx | 513 | 0.00 | 217 | -0.23 | | | | | | | | | | |
| Behle (2001) | yes | 12 | ms | 249 | 0.03 | 159 | -0.29 | | | | | | | | | | |
| | yes | 12 | ms | 337 | 0.04 | 319 | 0.00 | | | | | | | | | | |
| Bolton & Oatley (1987) | no | 7 | dep | 20 | 0.57 | 15 | -0.45 | | | 45 | 0.00 | | | | | | |
| Brenner et al. (1989) | yes | 12 | ms | 98 | -0.24 | | | | | 100 | -0.08 | | | | | | |
| Claussen (1999) | no | 60 | ov | 57 | 0.08 | 76 | -0.07 | | | | | | | | | | |
| | | | ms | 57 | 0.00 | 76 | -0.23 | | | | | | | | | | |
| | | | dep | 57 | 0.00 | 76 | 0.00 | | | | | | | | | | |
| | | | anx | 57 | 0.00 | 76 | 0.00 | | | | | | | | | | |
| | | | ps | 57 | 0,27 | 76 | 0.00 | | | | | | | | | | |
| Cobb & Kasl (1977) | no | 5 | ov | 12 | -0.48 | 41 | -0.07 | 53 | 0.17 | 43 | 0.00 | | | | | | |
| | | | dep | 12 | -0.29 | 41 | -0.16 | 53 | 0.30 | 43 | 0.00 | | | | | | |
| | | | anx | 12 | -0.37 | 41 | -0.21 | 53 | 0.18 | 43 | -0.15 | | | | | | |
| | | | ps | 12 | -0.61 | 41 | 0.17 | 53 | -0.18 | 43 | 0.08 | | | | | | |
| | | | se | 12 | -0.22 | 41 | -0.02 | 53 | 0.22 | 43 | 0.08 | | | | | | |
| Creed (1999) | no | 4 | ov | 37 | 0.06 | 15 | -0.67 | | | | | | | | | | |
| | | | ms | 38 | 0.25 | 15 | -0.90 | | | | | | | | | | |
| | | | se | 37 | -0.15 | 15 | -0.25 | | | | | | | | | | |
| Creed et al. (1996) | yes | 3.7 | se | 30 | -0.90 | | | | | | | | | | | | |
| | no | 3.7 | se | 52 | -0.23 | | | | | | | | | | | | |
| Creed et al. (1999) | yes | 0.1 | ov | 38 | -1.09 | | | | | | | | | | | | |
| | | | ms | 38 | -1.02 | | | | | | | | | | | | |
| | | | swb | 38 | -0.79 | | | | | | | | | | | | |
| | | | se | 38 | -0.83 | | | | | | | | | | | | |
| Creed et al. (1999)n.i | no | 0.1 | ov | 21 | -0.20 | | | | | | | | | | | | |
| | | | ms | 22 | -0.22 | | | | | | | | | | | | |
| | | | swb | 21 | -0.27 | | | | | | | | | | | | |
| | | | se | 22 | 0.00 | | | | | | | | | | | | |
| Dew et al. (1992) | no | 12 | ov | | | | | | | 68 | -0.24 | | | | | | |
| | | | dep | | | | | | | 68 | -0.19 | | | | | | |
| | | | anx | | | | | | | 68 | -0.23 | | | | | | |
| Dobberstein (1979) | no | 3.5 | se | 20 | -0.54 | | | | | 30 | -0.44 | | | | | | |
| Donovan et al. (1986) | no | 11 | ms | | | | | | | | | 26 | 0.39 | 29 | -0.61 | | |
| | no | 11 | ms | | | | | | | | | 14 | 0.12 | 16 | -0.58 | | |
| Dooley & Prause (1995)/ Prause & Dooley (1997) | no | 84 | se | | | | | | | | | 253 | -0.37 | 2190 | -0.47 | 31 | -0.80 |
| Dooley et al. (2000) | no | 24 | dep | | | | | 170 | 0.00 | 4437 | -0.13 | | | | | | |
| Fagin & Little (1984) | no | 6 | ms | 6 | 0.46 | | | | | | | | | | | | |
| Fineman (1983) | yes | 6 | ov | 17 | 0.09 | 28 | -0.59 | | | | | | | | | | |

| *Study* | *Int.* | *Time* | *MH-* | *Unun* | | *Unun* | | *Unem* | | *Unem* | | *Emun* | | *Emun* | | *Emem* | | *Emem* | | *Scun* | | *Scun* | | *Scem* | | *Scem* | | *Scsc* | | *Scsc* | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | *t1-t2* | *ind* | *n* | *es* | *n* | *es* | *n* | *es* | *n* | *es* | *n* | *es* | *n* | *es* | *n* | *es* | *n* | *es* | *n* | *es* | *n* | *es* | *n* | *es* | *n* | *es* | *n* | *es* | *n* | *es* |
| | | | ms | 17 | 0.11 | 28 | -0.74 | | | | | | | | | | | | | | | | | | | | | | | | |
| | | | ps | 17 | -0.26 | 28 | -0.13 | | | | | | | | | | | | | | | | | | | | | | | | |
| | | | se | 17 | 0.37 | 28 | -0.57 | | | | | | | | | | | | | | | | | | | | | | | | |
| | yes | 6 | ov | | | 12 | 0.72 | | | | | | | | | | | | | | | | | | | | | | | | |
| | | | ms | | | 12 | 0.46 | | | | | | | | | | | | | | | | | | | | | | | | |
| | | | ps | | | 12 | 0.62 | | | | | | | | | | | | | | | | | | | | | | | | |
| | | | se | | | 12 | 0.66 | | | | | | | | | | | | | | | | | | | | | | | | |
| Frese & Mohr (1987) / Frese (1979) | no | 18 | dep | 26 | 0.60 | | | | | | | | | | | | | | | | | | | | | | | | | | |
| Frese et al. (2002) | yes | 0.1 | ov | 8 | -1.29 | | | | | | | | | | | | | | | | | | | | | | | | | | |
| | | | ms | 8 | -1.44 | | | | | | | | | | | | | | | | | | | | | | | | | | |
| | | | dep | 8 | -0.69 | | | | | | | | | | | | | | | | | | | | | | | | | | |
| | | | ps | 8 | -0.99 | | | | | | | | | | | | | | | | | | | | | | | | | | |
| Friis et al. (1998) | no | 12 | ms | 50 | 0.00 | | | | | | | | | | | | | | | | | | | | | | | | | | |
| Graetz (1991/1993)[a] | no | 12 | ov | 76 | 0.14 | 521 | -0.34 | 323 | 0.19 | 2647 | -0.04 | 152 | 0.23 | 954 | -0.26 | 459 | 0.01 | | | | | | | | | | | | | | |
| | | | ms | 76 | 0.14 | 521 | -0.34 | 323 | 0.19 | 2647 | -0.04 | 152 | 0.23 | 954 | -0.26 | 459 | 0.01 | | | | | | | | | | | | | | |
| | | | anx | | | 301 | -0.07 | 241 | 0.09 | | | 135 | 0.20 | 890 | -0.21 | | | | | | | | | | | | | | | | |
| | | | se | | | 301 | -0.45 | 241 | 0.24 | | | 135 | 0.18 | 890 | -0.24 | | | | | | | | | | | | | | | | |
| Gurney (1980) | no | 4 | se | | | | | | | | | 32 | 0.00 | 132 | 0.00 | 84 | 0.00 | | | | | | | | | | | | | | |
| | no | 4 | se | | | | | | | | | 23 | 0.00 | 82 | -0.34 | 59 | 0.00 | | | | | | | | | | | | | | |
| Hamilton et al. (1993) / Broman et al. (1994) | no | 12 | dep | 89 | -0.07 | 95 | -0.30 | 16 | -0.34 | 134 | -0.04 | | | | | | | | | | | | | | | | | | | | |
| Harry & Tiggemann (1992) | yes | 0.76 | ov | 80 | -1.22 | | | | | | | | | | | | | | | | | | | | | | | | | | |
| | | | ms | 80 | -1.50 | | | | | | | | | | | | | | | | | | | | | | | | | | |
| | | | dep | 80 | -0.77 | | | | | | | | | | | | | | | | | | | | | | | | | | |
| | | | se | 80 | -0.69 | | | | | | | | | | | | | | | | | | | | | | | | | | |
| Hartley (1980) | yes | 1.85 | se | 47 | -0.32 | | | | | | | | | | | | | | | | | | | | | | | | | | |
| | no | 1.85 | se | 23 | -0.11 | | | | | 50 | -0.20 | | | | | | | | | | | | | | | | | | | | |
| Heady & Smyth (1989) | no | 12 | ms | 830 | -0.02 | 686 | -0.60 | | | | | | | | | | | | | | | | | | | | | | | | |
| Isaksson (1990) | no | 12 | ms | 10 | 0.39 | 23 | -0.50 | 9 | 0.66 | 18 | -0.31 | | | | | | | | | | | | | | | | | | | | |
| Iversen & Sarboe (1988) | no | 12 | ms | 185 | -0.03 | 268 | -0.45 | 36 | -0.19 | 786 | -0.15 | | | | | | | | | | | | | | | | | | | | |
| Iwasaki & Smale (1998) | no | 84 | swb | | | 29 | -0.44 | 13 | 0.27 | | | | | | | | | | | | | | | | | | | | | | |
| Jackson et al. (1983) | no | 15 | ms | 21 | 0.05 | 19 | -0.56 | 58 | 0.75 | 273 | 0.02 | | | | | | | | | | | | | | | | | | | | |
| | no | 16 | ms | 31 | -0.04 | 19 | -0.97 | 60 | 0.59 | 335 | -0.09 | | | | | | | | | | | | | | | | | | | | |
| Jones (1991) | no | 3.5 | dep | 62 | 0.00 | 73 | -0.40 | | | | | | | | | | | | | | | | | | | | | | | | |
| Kieselbach et al. (1998) | yes | | ms | | | 15 | -0.66 | | | | | | | | | | | | | | | | | | | | | | | | |
| Kinicki et al. (2000) | no | 4 | ov | | | 9 | 0.23 | | | | | | | | | | | | | | | | | | | | | | | | |
| | | | swb | | | 9 | 0.65 | | | | | | | | | | | | | | | | | | | | | | | | |
| | | | se | | | 9 | -0.26 | | | | | | | | | | | | | | | | | | | | | | | | |
| | no | 4 | ov | 78 | 0.13 | 13 | -0.66 | | | | | | | | | | | | | | | | | | | | | | | | |
| | | | swb | 78 | 0.26 | 13 | -0.39 | | | | | | | | | | | | | | | | | | | | | | | | |
| | | | se | 78 | -0.03 | 13 | -0.74 | | | | | | | | | | | | | | | | | | | | | | | | |
| Kristensen (1991) | yes | 6 | se | 311 | 0.00 | | | | | 553 | -0.04 | | | | | | | | | | | | | | | | | | | | |
| Lahelma (1989) | no | 14 | ms | 121 | 0.00 | 130 | -0.23 | | | 90 | 0.00 | | | | | | | | | | | | | | | | | | | | |
| | no | 14 | ms | 155 | 0.00 | 113 | -0.32 | | | 94 | 0.00 | | | | | | | | | | | | | | | | | | | | |
| Lai & Chan (2002) | no | 8 | ov | 26 | 0.84 | 22 | 0.62 | | | | | | | | | | | | | | | | | | | | | | | | |
| | | | ms | 26 | 0.72 | 22 | 0.50 | | | | | | | | | | | | | | | | | | | | | | | | |
| | | | swb | 26 | 0.73 | 22 | 0.56 | | | | | | | | | | | | | | | | | | | | | | | | |
| Layton (1986a) | no | 10.5 | ms | | | | | | | | | 29 | 0.18 | 77 | -0.22 | 80 | -0.12 | | | | | | | | | | | | | | |
| Layton (1986b/1987) | no | 6 | ov | 39 | 0,07 | 62 | -0.55 | | | | | | | | | | | | | | | | | | | | | | | | |

| *Study* | *Int.* | *Time* | *MH-* | *Unun* | *Unun* | *Unem* | *Unem* | *Emun* | *Emun* | *Emem* | *Emem* | *Scun* | *Scun* | *Scem* | *Scem* | *Scsc* | *Scsc* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | *t1-t2* | *ind* | *n* | *es* | *n* | *es* | *n* | *es* | *n* | *es* | *n* | *es* | *n* | *es* | *n* | *es* |
| | | | ms | 39 | 0,35 | 62 | -0.30 | | | | | | | | | | |
| | | | anx | 39 | -0,23 | 62 | -0.65 | | | | | | | | | | |
| Liira & Leino-Arjas (1999) | no | 60 | ps | | | | | 191 | 0.18 | | | | | | | | |
| | no | 60 | ps | | | | | 236 | 0.07 | 71 | 0.08 | | | | | | |
| | no | 60 | ps | | | | | 66 | 0.06 | | | | | | | | |
| | no | 60 | ps | | | | | 177 | 0.06 | 132 | -0.13 | | | | | | |
| Maysent & Spera (1995) | yes | | ms | 76 | -0.21 | | | | | | | | | | | | |
| | no | | ms | 37 | 0.32 | | | | | | | | | | | | |
| Mean Patterson (1997) | no | 11 | ov | 48 | -0.09 | 32 | -0.43 | | | | | | | | | | |
| | | | ms | 48 | -0.05 | 32 | -0.66 | | | | | | | | | | |
| | | | se | 48 | -0.10 | 32 | -0.07 | | | | | | | | | | |
| Mohr (1993/1997) | no | 84 | ov | 15 | 0.09 | 33 | -0.57 | | | | | | | | | | |
| | | | dep | 15 | 0.26 | 33 | -0.65 | | | | | | | | | | |
| | | | ps | 15 | -0.11 | 33 | -0.33 | | | | | | | | | | |
| Morch (1986) | no | 3 | ov | 54 | -0.03 | 42 | -0.07 | | | | | | | | | | |
| | | | dep | 54 | 0.20 | 42 | -0.04 | | | | | | | | | | |
| | | | se | 54 | -0.24 | 42 | -0.08 | | | | | | | | | | |
| O'Brien & Feather (1990) | no | 24 | ov | | | | | | | | | | | 271 | -0.09 | | |
| | | | ms | | | | | | | | | | | 271 | -0.16 | | |
| | | | dep | | | | | | | | | | | 271 | 0.00 | | |
| | | | swb | | | | | | | | | | | 271 | -0.12 | | |
| | | | se | | | | | | | | | | | 271 | 0.00 | | |
| | no | 24 | ov | | | | | | | | | 112 | 0.18 | 277 | 0.02 | 1536 | 0.04 |
| | | | ms | | | | | | | | | 112 | -0.19 | 277 | -0.12 | 1536 | -0.07 |
| | | | dep | | | | | | | | | 112 | 0.25 | 277 | 0.18 | 1536 | 0.07 |
| | | | swb | | | | | | | | | 112 | 0.25 | 277 | 0.00 | 1536 | 0.07 |
| | | | se | | | | | | | | | 112 | 0.25 | 277 | 0.00 | 1536 | 0.07 |
| Patton & Noller (1984) | no | 5 | ov | | | | | | | | | 12 | 0.48 | 11 | -0.74 | 33 | -0.38 |
| | | | dep | | | | | | | | | 12 | 0.65 | 11 | -0.55 | 33 | -0.34 |
| | | | se | | | | | | | | | 12 | 0.16 | 11 | -0.72 | 33 | -0.32 |
| | no | 5 | ov | | | | | | | | | 9 | 1.84 | 13 | -0.04 | 35 | -0.86 |
| | | | dep | | | | | | | | | 9 | 1.91 | 13 | -0.22 | 35 | -0.90 |
| | | | se | | | | | | | | | 9 | 1.26 | 13 | 0.15 | 35 | -0.58 |
| Payne & Jones (1987) | no | 12 | ov | | | 90 | -0.34 | | | | | | | | | | |
| | | | ms | | | 90 | -0.36 | | | | | | | | | | |
| | | | dep | | | 90 | -0.36 | | | | | | | | | | |
| | | | anx | | | 90 | -0.36 | | | | | | | | | | |
| | | | swb | | | 90 | 0.00 | | | | | | | | | | |
| | no | 12 | ov | | | 54 | -0.61 | | | | | | | | | | |
| | | | ms | | | 54 | -0.47 | | | | | | | | | | |
| | | | dep | | | 54 | -0.47 | | | | | | | | | | |
| | | | anx | | | 54 | -0.47 | | | | | | | | | | |
| | | | swb | | | 54 | -0.47 | | | | | | | | | | |
| Pernice & Long (1996) | no | 12 | ov | 12 | 0.00 | 22 | -0.68 | | | | | | | | | | |
| | | | ms | 12 | 0.00 | 22 | -1.16 | | | | | | | | | | |
| | | | se | 12 | 0.00 | 22 | 0.00 | | | | | | | | | | |
| Prause & Dooley (2001) | no | 24 | dep | 117 | 0.00 | 208 | -0.26 | | | | | | | | | | |
| Proudfoot et al. (1999) | yes | 1.62 | ov | 122 | -0.91 | | | | | | | | | | | | |

| *Study* | *Int.* | *Time* | *MH-* | *Unun* | | *Unun* | | *Unem* | | *Unem* | | *Emun* | | *Emun* | | *Emem* | | *Emem* | | *Scun* | | *Scun* | | *Scem* | | *Scem* | | *Scsc* | | *Scsc* | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | *t1-t2* | *ind* | *n* | *es* | *n* | *es* | *n* | *es* | *n* | *es* | *n* | *es* | *n* | *es* | *n* | *es* | *n* | *es* | *n* | *es* | *n* | *es* | *n* | *es* | *n* | *es* | *n* | *es* | *n* | *es* |
| | | | ms | 122 | -1.00 | | | | | | | | | | | | | | | | | | | | | | | | | | |
| | | | swb | 122 | -0.48 | | | | | | | | | | | | | | | | | | | | | | | | | | |
| | | | se | 122 | -0.71 | | | | | | | | | | | | | | | | | | | | | | | | | | |
| | yes | 1.62 | ov | 122 | -0.47 | | | | | | | | | | | | | | | | | | | | | | | | | | |
| | | | ms | 122 | -0.60 | | | | | | | | | | | | | | | | | | | | | | | | | | |
| | | | swb | 122 | -0.23 | | | | | | | | | | | | | | | | | | | | | | | | | | |
| | | | se | 122 | -0.30 | | | | | | | | | | | | | | | | | | | | | | | | | | |
| Saam et al. (1995) | yes | 1,39 | anx | 20 | -1.19 | | | | | | | | | | | | | | | | | | | | | | | | | | |
| | no | 1,39 | anx | 22 | -0.15 | | | | | | | | | | | | | | | | | | | | | | | | | | |
| Schaufeli & Van Yperen (1992) | no | 12 | ms | 84 | -0.31 | 82 | -0.12 | | | | | | | | | | | | | | | | | | | | | | | | |
| Schaufeli (1997) | no | 12 | ms | | | | | | | | | | | | | | | | | 14 | 0.74 | 95 | -0.32 | 388 | -0.13 | | | | |
| Shamir (1986a) | no | 6,5 | ov | 49 | 0.08 | 65 | -0.60 | 14 | 0,22 | 167 | 0.01 | | | | | | | | | | | | | | | | | | | | |
| | | | dep | 49 | 0.14 | 65 | -0.71 | 14 | 0,34 | 167 | 0.05 | | | | | | | | | | | | | | | | | | | | |
| | | | anx | 49 | 0.14 | 65 | -0.57 | 14 | 0,35 | 167 | 0.12 | | | | | | | | | | | | | | | | | | | | |
| | | | swb | 49 | -0.06 | 65 | -0.51 | 14 | 0,13 | 167 | -0.12 | | | | | | | | | | | | | | | | | | | | |
| | | | se | 49 | 0.02 | 65 | -0.07 | 14 | -0,15 | 167 | 0.00 | | | | | | | | | | | | | | | | | | | | |
| Tiggemann & Winefield (1980) | no | 7 | ov | | | | | | | | | | | | | | | | | 26 | 0.35 | 54 | 0.00 | | | | | | | | |
| | | | dep | | | | | | | | | | | | | | | | | 26 | 0.33 | 54 | 0.00 | | | | | | | | |
| | | | swb | | | | | | | | | | | | | | | | | 26 | 0.33 | 54 | 0.00 | | | | | | | | |
| | | | se | | | | | | | | | | | | | | | | | 26 | 0.17 | 54 | 0.00 | | | | | | | | |
| Tiggemann & Winefield (1984)/ Winefield et al. (1993) | no | 12 | ov | | | | | | | | | | | | | | | | | 86 | -0.01 | 307 | -0.41 | 526 | -0.13 | | | | |
| | | | dep | | | | | | | | | | | | | | | | | 86 | -0.12 | 307 | -0.36 | 526 | -0.10 | | | | |
| | | | swb | | | | | | | | | | | | | | | | | 86 | 0.33 | 307 | -0.13 | | | | | | | | |
| | | | se | | | | | | | | | | | | | | | | | 86 | -0.24 | 307 | -0.50 | 526 | -0.12 | | | | |
| Tiggemann & Winefield (1984)/ Winefield et al.(1993) | no | 12 | ov | | | | | | | | | | | | | | | | | | | | | 122 | -0.29 | | | | |
| | | | dep | | | | | | | | | | | | | | | | | | | | | 122 | -0.20 | | | | |
| | | | se | | | | | | | | | | | | | | | | | | | | | 122 | -030 | | | | |
| Tiggemann & Winefield (1984)/ Winefield et al. (1993) | no | 12 | ov | | | | | | | | | | | | | | | | | 58 | 0.01 | 310 | -0.14 | 594 | -0.12 | | | | |
| | | | dep | | | | | | | | | | | | | | | | | 58 | 0.12 | 310 | -0.15 | 594 | -0.09 | | | | |
| | | | swb | | | | | | | | | | | | | | | | | 58 | 0.12 | 310 | 0.00 | | | | | | | | |
| | | | se | | | | | | | | | | | | | | | | | 58 | -0.21 | 310 | -0.19 | 594 | -0.11 | | | | |
| Tiggemann & Winefield (1984)/ Winefield et al. (1993) | no | 12 | ov | | | | | | | | | | | | | | | | | | | | | 129 | 0.05 | | | | |
| | | | dep | | | | | | | | | | | | | | | | | | | | | 129 | 0.00 | | | | |
| | | | se | | | | | | | | | | | | | | | | | | | | | 129 | 0.08 | | | | |
| Verkleij (1989) | no | 24 | dep | 214 | 0.13 | 147 | -0.16 | | | 489 | 0.09 | | | | | | | | | | | | | | | | | | | | |
| Viinamäki et al. (1996) | no | 12 | ov | 112 | 0.17 | | | | | 129 | 0.18 | | | | | | | | | | | | | | | | | | | | |
| | | | ms | 118 | 0.18 | | | | | 138 | 0.20 | | | | | | | | | | | | | | | | | | | | |
| | | | dep | 114 | 0.00 | | | | | 131 | 0.15 | | | | | | | | | | | | | | | | | | | | |
| | | | ps | 112 | 0.23 | | | | | 129 | 0.07 | | | | | | | | | | | | | | | | | | | | |
| Vinokur et al. (1987) | no | 8 | ms | 88 | 0.00 | 81 | -0.28 | 51 | 0.43 | 222 | 0.00 | | | | | | | | | | | | | | | | | | | | |
| Vuori & Vesalainen (1999) | yes | 12 | ms | 311 | 0.01 | | | | | | | | | | | | | | | | | | | | | | | | | | |
| Wanberg (1995) | no | 9 | ov | 28 | 0.06 | 11 | 0.05 | | | | | | | | | | | | | | | | | | | | | | | | |

| *Study* | *Int.* | *Time t1-t2* | *MH-ind* | *Unun n* | *Unun es* | *Unem n* | *Unem es* | *Emun n* | *Emun es* | *Emem n* | *Emem es* | *Scun n* | *Scun es* | *Scem n* | *Scem es* | *Scsc n* | *Scsc es* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | ms | 28 | -0.23 | 11 | 0.00 | | | | | | | | | | |
| | | | swb | 28 | 0.33 | 11 | 0.08 | | | | | | | | | | |
| | no | 9 | ov | | | 84 | -0.42 | | | | | | | | | | |
| | | | ms | | | 84 | -0.79 | | | | | | | | | | |
| | | | swb | | | 84 | 0.06 | | | | | | | | | | |
| Wanberg et al. (1997) | no | 3 | ms | 163 | 0.11 | 40 | -0.54 | | | 37 | -0.18 | | | | | | |
| Warr & Jackson (1983) | no | 16 | se | 21 | -0.29 | 19 | -0.55 | 58 | 0.13 | 274 | -0.41 | | | | | | |
| | no | 15 | se | 32 | 0.10 | 19 | -0.42 | 60 | 0.17 | 335 | -0.07 | | | | | | |
| Warr & Jackson (1985) | no | 9 | ms | 467 | 0.09 | 161 | -0.86 | | | | | | | | | | |
| Waters & Moore (2002b) | no | 6 | se | | | 30 | -0.34 | | | | | | | | | | |
| Westman et al. (2004) | no | 2 | anx | 113 | -0.02 | | | | | 113 | 0.14 | | | | | | |
| Winefield & Tiggemann (1990) | no | 12 | ov | 35 | -0.09 | 40 | -0.30 | 30 | 0.19 | 463 | -0.13 | | | | | | |
| | | | dep | 35 | -0.12 | 40 | -0.24 | 30 | 0.15 | 463 | -0.09 | | | | | | |
| | | | se | 35 | -0.03 | 40 | -0.28 | 30 | 0.18 | 463 | -0.12 | | | | | | |
| Winkelmann & Winkelmann (1998) | no | 12 | swb | 395 | 0.02 | 274 | -0.34 | 312 | 0.35 | 12544 | 0.04 | | | | | | |
| Zempel et al. (2000) | yes | 2,77 | ov | 33 | -0.06 | | | | | | | | | | | | |
| | | | dep | 33 | -0.10 | | | | | | | | | | | | |
| | | | ps | 33 | 0.06 | | | | | | | | | | | | |
| | | | se | 33 | -0.09 | | | | | | | | | | | | |

*Note.* Int. = intervention study? (yes – no); Time t1-t2 = duration of T1-T2- interval in months; MH-ind = mental health indicator variable; Unun-es = repeated measures effect size for continuously unemployed persons; Unem-es = repeated measures effect size for persons changing from unemployment to employment; Emun-es = repeated measures effect size for persons changing from employment to unemployment; Unun-es = repeated measures effect size for continuously employed persons; Scun-es = repeated measures effect size for persons changing from school to unemployment; Scem-es = repeated measures effect size for persons changing from school to employment; Scsc-es = repeated measures effect size for persons remaining in the educational system; Unun-n, Unem-n, Emun-n, Emem-n, Scun-n, Scem-n, Scsc-n = sample sizes for corresponding effect size; ov = overall; ms = mixed symptoms; dep = depression, anx = anxiety; ps = psychosomatic symptoms; swb = subjective well-being; se = self-esteem; [a] ov = ms for this study as all other measures are only subscales of the ms-measure.

Table C-7: **Selection effects: Cross-sectional comparisons between groups with different labor market outcomes at the first measurement points of longitudinal studies**

| *Study* | *Int.* | *Time t1-t2* | *MH-ind* | *uu n* | *ue n* | *uu - ue es* | *eu n* | *ee n* | *eu - ee es* | *su n* | *se n* | *su - se es* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Bachman et al. (1978) | no | 90 | se | | | | | | | 111 | 1205 | 0.12 |
| Balz et al. (1985) | yes | 12 | ov | 40 | 41 | 0.00 | | | | | | |
| | | | dep | 42 | 41 | 0.00 | | | | | | |
| | | | ps | 42 | 41 | 0.00 | | | | | | |
| | | | se | 40 | 41 | 0.00 | | | | | | |
| Banks & Ullah (1988) | no | 12 | ov | 513 | 217 | 0.00 | | | | | | |
| | | | ms | 513 | 217 | 0.00 | | | | | | |
| | | | dep | 513 | 217 | 0.00 | | | | | | |
| | | | anx | 513 | 217 | 0.00 | | | | | | |
| Behle (2001) | yes | 12 | ms | 249 | 159 | 0.49 | | | | | | |
| | yes | 12 | ms | 337 | 319 | 0.54 | | | | | | |
| Beiser et al. (1993) | no | 24 | dep | 81 | 70 | 0.11 | 176 | 511 | 0.21 | | | |
| Bolton & Oatley (1987) | no | 7 | dep | 20 | 15 | 0.00 | | | | | | |
| Brinkmann (1985) | no | 18 | se | 360 | 378 | 0.19 | | | | | | |
| Büchtemann & Rosenbladt (1981) | no | 9,5 | ps | 404 | 366 | 0.30 | 50 | 531 | 0.27 | | | |
| Cohn, R.M. (1978) | no | 12 | se | | | | 537 | 543 | 0.46 | | | |
| Creed, P. (1999) | no | 4 | ov | 37 | 15 | 0.20 | | | | | | |
| | | | ms | 38 | 15 | -0.22 | | | | | | |
| | | | se | 37 | 15 | 0.55 | | | | | | |
| Dew et al. (1992) | no | 12 | ov | | | | 73 | 68 | -0.05 | | | |
| | | | dep | | | | 73 | 68 | -0.06 | | | |
| | | | anx | | | | 73 | 68 | -0.02 | | | |
| Donovan et al. (1986) | no | 11 | ms | | | | | | | 26 | 29 | 0.25 |
| | no | 11 | ms | | | | | | | 14 | 16 | -0.06 |
| Dooley, & Prause (1995) / Prause & Dooley (1997) | no | 84 | se | | | | | | | 253 | 2190 | 0.26 |
| Dooley et al. (2000) | no | 24 | dep | | | | 170 | 4437 | 0.32 | | | |
| Fagin & Little (1984) | no | 6 | ms | 6 | 3 | 0.99 | | | | | | |
| Fineman (1983) | yes | 6 | ov | 17 | 40 | 0.06 | | | | | | |
| | | | ms | 17 | 40 | 0.07 | | | | | | |
| | | | ps | 17 | 40 | -0.01 | | | | | | |
| | | | se | 17 | 40 | 0.08 | | | | | | |
| Frese (1994) | no | 12 | ov | | | | 67 | 417 | 0.00 | | | |
| | | | dep | | | | 67 | 417 | 0.00 | | | |
| | | | ps | | | | 67 | 417 | 0.00 | | | |
| | | | se | | | | 67 | 417 | 0.00 | | | |
| Frese & Mohr (1987) / Frese (1979) | no | 18 | dep | 26 | 15 | 0.14 | | | | | | |
| Gowan et al. (1999) | no | 6 | ov | 119 | 83 | -0.11 | | | | | | |
| | | | dep | 119 | 83 | 0.02 | | | | | | |
| | | | anx | 119 | 83 | -0.20 | | | | | | |
| Graetz, B. (1991/1993) [a] | no | 12 | ov | 76 | 521 | 0.02 | 323 | 2647 | 0.08 | 152 | 954 | -0.06 |

| *Study* | *Int.* | *Time t1-t2* | *MH-ind* | *uu n* | *ue n* | *uu - ue es* | *eu n* | *ee n* | *eu - ee es* | *su n* | *se n* | *su - se es* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | ms | 76 | 521 | 0.02 | 323 | 2647 | 0.08 | 152 | 954 | -0.06 |
| | | | anx | | | | | | | 135 | 890 | -0.17 |
| | | | se | | | | | | | 135 | 890 | 0.15 |
| Gurney, R.M. (1980) | no | 4 | se | | | | | | | 32 | 132 | 0.05 |
| | no | 4 | se | | | | | | | 23 | 82 | 0.31 |
| Halvorsen (1998) | yes | 18 | ms | 220 | 195 | 0.24 | | | | | | |
| Hamilton et al. (1993) / Broman et al. (1994) | no | 12 | dep | 89 | 95 | 0.21 | 16 | 134 | 0.48 | | | |
| Heady & Smyth (1989) | no | 12 | ms | 830 | 686 | 0.03 | | | | | | |
| Heinemann et al. (1980) | no | 5 | ov | 528 | 227 | 0.24 | | | | | | |
| | | | ps | 528 | 227 | 0.16 | | | | | | |
| | | | se | 528 | 227 | 0.25 | | | | | | |
| Huppert & Whittington (1993) | no | 84 | ms | | | | 19 | 381 | 0.77 | | | |
| | no | 84 | ms | | | | 20 | 492 | 0.36 | | | |
| Isaksson, K. (1990) | no | 12 | ms | 10 | 23 | -0.24 | 9 | 18 | -0.56 | | | |
| Jackson et al. (1983) | no | 15 | ms | 21 | 19 | -0.13 | 58 | 273 | 0.34 | | | |
| | no | 16 | ms | 31 | 19 | -0.14 | 60 | 335 | 0.21 | | | |
| Jones (1991) | no | 3.5 | dep | 62 | 73 | 0.36 | | | | | | |
| Joseph (1999) | yes | 2 | ov | 33 | 19 | 0.05 | | | | | | |
| | | | ms | 33 | 19 | 0.41 | | | | | | |
| | | | dep | 33 | 19 | 0.09 | | | | | | |
| | | | ps | 33 | 19 | -0.39 | | | | | | |
| Kanouse et al.(1980) | no | 6 | se | | | | | | | 659 | 2149 | 0.06 |
| | no | 6 | se | | | | | | | 429 | 2519 | 0.03 |
| Kieselbach et al. (1998) | yes | | ms | 9 | 28 | 0.00 | | | | | | |
| Kinicki et al. (2000) | no | 4 | ov | 78 | 22 | -0.22 | | | | | | |
| | | | swb | 78 | 22 | -0.15 | | | | | | |
| | | | se | 78 | 22 | -0.22 | | | | | | |
| Lahelma (1989) | no | 14 | ms | 121 | 130 | 0.16 | | | | | | |
| | no | 14 | ms | 155 | 113 | 0.12 | | | | | | |
| Lai & Chan (2002) | no | 8 | ov | 26 | 22 | 0.05 | | | | | | |
| | | | ms | 26 | 22 | -0.07 | | | | | | |
| | | | swb | 26 | 22 | 0.16 | | | | | | |
| Layton (1986a) | no | 10.5 | ms | | | | | | | 29 | 77 | -0.07 |
| Layton (1986b/1987) | no | 6 | ov | 39 | 62 | 0.11 | | | | | | |
| | | | ms | 39 | 62 | 0.13 | | | | | | |
| | | | anx | 39 | 62 | 0.07 | | | | | | |
| Leana & Feldman (1995) | no | 11 | dep | 25 | 34 | 0.29 | | | | | | |
| Liira & Leino-Arjas (1999) | no | 60 | ps | | | | 427 | 71 | 0.39 | | | |
| | no | 60 | ps | | | | 243 | 132 | 0.25 | | | |
| Linn, Sandifer & Stein (1985) | no | 6 | ov | | | | 30 | 30 | 0.00 | | | |
| | | | dep | | | | 30 | 30 | 0.00 | | | |
| | | | anx | | | | 30 | 30 | 0.00 | | | |
| | | | ps | | | | 30 | 30 | 0.00 | | | |
| Mallinckrodt (1990) | yes | 12 | ov | 8 | 16 | 0.00 | | | | | | |
| | | | dep | 8 | 16 | 0.00 | | | | | | |
| | | | se | 8 | 16 | 0.00 | | | | | | |
| Mean Patterson (1997) | no | 11 | ov | 48 | 32 | 1.85 | | | | | | |
| | | | ms | 48 | 32 | 1.88 | | | | | | |

| *Study* | *Int.* | *Time t1-t2* | *MH-ind* | *uu n* | *ue n* | *uu - ue es* | *eu n* | *ee n* | *eu - ee es* | *su n* | *se n* | *su - se es* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | se | 48 | 32 | 1.31 | | | | | | |
| Mohr (1993/1997) | no | 84 | ov | 15 | 33 | 0.34 | | | | | | |
| | | | dep | 15 | 33 | 0.47 | | | | | | |
| | | | ps | 15 | 33 | 0.11 | | | | | | |
| Morch (1986) | no | 3 | ov | 54 | 42 | 0.13 | | | | | | |
| | | | dep | 54 | 42 | 0.02 | | | | | | |
| | | | se | 54 | 42 | 0.21 | | | | | | |
| Patton & Noller (1984) | no | 5 | ov | | | | | | | 12 | 11 | -0.01 |
| | | | dep | | | | | | | 12 | 11 | 0.25 |
| | | | se | | | | | | | 12 | 11 | -0.27 |
| | no | 5 | ov | | | | | | | 9 | 13 | 0.36 |
| | | | dep | | | | | | | 9 | 13 | 0.00 |
| | | | se | | | | | | | 9 | 13 | 0.62 |
| Patton & Noller (1990) | no | 12 | ov | | | | | | | 40 | 47 | -0.06 |
| | | | dep | | | | | | | 40 | 47 | 0.00 |
| | | | se | | | | | | | 40 | 47 | -0.11 |
| Prause, J. & Dooley, D. (2001) | no | 24 | dep | 117 | 208 | 0.47 | | | | | | |
| Prussia et al. (1993) | no | 18 | ov | 51 | 28 | 0.25 | | | | | | |
| | | | swb | 51 | 28 | 0.04 | | | | | | |
| | | | se | 51 | 28 | 0.40 | | | | | | |
| Schaufeli & Van Yperen (1992) | no | 12 | ms | 84 | 82 | 0.37 | | | | | | |
| Schaufeli (1997) | no | 12 | ms | | | | | | | 14 | 95 | 0.00 |
| Shamir (1986a) | no | 6,5 | ov | 49 | 65 | -0.02 | 14 | 167 | 0.80 | | | |
| | | | dep | 49 | 65 | 0.04 | 14 | 167 | 0.38 | | | |
| | | | anx | 49 | 65 | -0.20 | 14 | 167 | 0.49 | | | |
| | | | swb | 49 | 65 | 0.10 | 14 | 167 | 1.02 | | | |
| | | | se | 49 | 65 | 0.00 | 14 | 167 | 0.60 | | | |
| Tiggemann & Winefield (1980) | no | 7 | ov | | | | | | | 26 | 54 | 0.00 |
| | | | dep | | | | | | | 26 | 54 | 0.00 |
| | | | swb | | | | | | | 26 | 54 | 0.00 |
| | | | se | | | | | | | 26 | 54 | 0.00 |
| Tiggemann & Winefield (1984) / Winefield et al. (1993) | no | 12 | ov | | | | | | | 86 | 307 | 0.00 |
| | | | dep | | | | | | | 86 | 307 | 0.00 |
| | | | swb | | | | | | | 86 | 307 | 0.00 |
| | | | se | | | | | | | 86 | 307 | 0.00 |
| | no | 12 | ov | | | | | | | 58 | 310 | 0.33 |
| | | | dep | | | | | | | 58 | 310 | 0.24 |
| | | | swb | | | | | | | 58 | 310 | 0.14 |
| | | | se | | | | | | | 58 | 310 | 0.41 |
| Verkleij (1989) | no | 24 | dep | 214 | 149 | 0.12 | | | | | | |
| Vinokur et al. (2000) | yes | 25 | dep | 343 | 1087 | 0.09 | | | | | | |
| Vuori & Vesalainen (1999) | yes | 12 | ms | 290 | 86 | 0.00 | | | | | | |
| Wanberg (1995) | no | 9 | ov | 28 | 95 | -0.03 | | | | | | |
| | | | ms | 28 | 95 | -0.11 | | | | | | |
| | | | swb | 28 | 95 | 0.06 | | | | | | |
| Wanberg (1997) | no | 3 | ov | 186 | 177 | -0.12 | | | | | | |
| | | | ms | 186 | 177 | -0.14 | | | | | | |
| | | | se | 186 | 177 | -0.06 | | | | | | |

| *Study* | *Int.* | *Time t1-t2* | *MH-ind* | *uu n* | *ue n* | *uu - ue es* | *eu n* | *ee n* | *eu - ee es* | *su n* | *se n* | *su - se es* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Wanberg et al. (1997) | no | 3 | ms | 163 | 40 | -0.26 | | | | | | |
| Warr & Jackson (1983) | no | 16 | se | 21 | 19 | 0.13 | 58 | 274 | 0.06 | | | |
| | no | 15 | se | 32 | 19 | -0.37 | 60 | 335 | 0.19 | | | |
| Warr & Jackson (1985) | no | 9 | ms | 467 | 162 | -0.02 | | | | | | |
| Waters & Moore (2002b) | no | 6 | se | 73 | 30 | 0.26 | | | | | | |
| Winefield & Tiggemann (1990) | no | 12 | ov | 35 | 40 | 0.55 | 30 | 463 | 0.34 | | | |
| | | | dep | 35 | 40 | 0.62 | 30 | 463 | 0.34 | | | |
| | | | se | 35 | 40 | 0.32 | 30 | 463 | 0.24 | | | |
| Winkelmann & Winkelmann (1998) | no | 12 | swb | | | | 248 | 3530 | 0.18 | | | |

*Note.* Int. = intervention study? (yes – no); Time t1-t2 = duration of t1-t2- interval; MH-ind = mental health indicator variable; uu – ue es = effect size for the comparison of continuously unemployed persons and unemployed pesons who managed to find a new job by t2; eu – ee es = effect size for the comparison of continuously employed persons and employed persons who lost their job by t2; su – se es = effect size for the comparison of school leavers who became unempoloyed after school and school leavers who became employed; uu-n, ue-n, ee-n, eu-n, su-n, se-n = sample sizes for corresponding groups; ov = overall; ms = mixed symptoms; dep = depression, anx = anixiety; ps = psychosomatic symptoms; swb = subjective well-being; se = self-esteem; [a] ov = ms for this study as all other measures are only subscales of the ms-measure.

Table C-8: **Effect sizes for cross-sectional comparisons between unemployed and employed persons/moderator values**

| *Study* | *Lang-uage* | *Year dat. coll.* | *Coun-try* | *Way of pub.* | *UE main topic?* | *UE-opera.* | *Re-emp?* | *Prct. part-time?* | *Prct. fem.* | *Prct. minor.* | *Age* | *Prct. mar.* | *Years educ.* | *Prct. bl. col.* | *Mean UE-dur.* | *Ques. form* | *MH-var.* | *N-ue* | *N-emp* | *d* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Aneshensel et al. (1981) | E | 1979 | USA | Jour. | no | diff other. | other | 0 | 50 | --- | 42 | 50 | --- | --- | --- | oral | dep | 69 | 581 | 0.67* |
| Araya et al. (2001) | E | 1996 | CHL | Jour. | no | diff other. | other | --- | 40 | --- | 37 | 57 | --- | 38 | --- | oral | ms | 121 | 1822 | 0.55* |
| Aubry et al. (1990) | E | --- | CAN | Jour. | yes | regist. | other | 0 | 100 | --- | 33 | 100 | --- | 100 | --- | writ. | ms | 32 | 31 | 0.89* |
| Avery et al. (1998) | E | 1994 | GBR | Jour. | yes | plant clos. | other | --- | 0 | --- | 38 | --- | --- | 91 | 2 | writ. | ms | 124 | 336 | 0.24* |
| | E | 1994 | GBR | Jour. | yes | diff other. | other | --- | 0 | --- | 35 | --- | --- | 52 | --- | writ. | ms | 81 | 658 | 0.34* |
| Bachman et al. (1978) | E | 1974 | USA | Book | no | diff other. | other | --- | 0 | 10 | 23 | 51 | --- | 60 | --- | writ. | se | 111 | 1205 | 0.37* |
| Balz et al. (1985)a | G | 1984 | GER | Book | yes | regist. | reemp | --- | 0 | --- | 38 | --- | --- | --- | 24 | writ. | comp | 42 | 41 | 0.18* |
| | | | | | | | | | | | | | | | | | dep | 42 | 41 | 0.45 |
| | | | | | | | | | | | | | | | | | psysom | 42 | 41 | 0.00 |
| | | | | | | | | | | | | | | | | | se | 42 | 41 | 0.00 |
| Banks & Ullah (1988) | E | 1983 | GBR | Book | yes | regist. | reemp | --- | 100 | 100 | 17 | --- | --- | --- | 11 | oral | comp | 55 | 18 | 1.02* |
| | | | | | | | | | | | | | | | | | ms | 55 | 18 | 0.92 |
| | | | | | | | | | | | | | | | | | dep | 55 | 18 | 0.92 |
| | | | | | | | | | | | | | | | | | anxi | 55 | 18 | 0.64 |
| | E | 1983 | GBR | Book | yes | regist. | reemp | --- | 100 | 0 | 17 | --- | --- | --- | 12 | oral | comp | 168 | 80 | 0.56* |
| | | | | | | | | | | | | | | | | | ms | 168 | 80 | 0.45 |
| | | | | | | | | | | | | | | | | | dep | 168 | 80 | 0.45 |
| | | | | | | | | | | | | | | | | | anxi | 168 | 80 | 0.45 |
| | E | 1983 | GBR | Book | yes | regist. | reemp | --- | 0 | 100 | 17 | --- | --- | --- | 12 | oral | comp | 114 | 36 | 0.79* |
| | | | | | | | | | | | | | | | | | ms | 114 | 36 | 0.64 |
| | | | | | | | | | | | | | | | | | dep | 114 | 36 | 0.64 |
| | | | | | | | | | | | | | | | | | anxi | 114 | 36 | 0.64 |
| | E | 1983 | GBR | Book | yes | regist. | reemp | --- | 0 | 0 | 17 | --- | --- | --- | 11 | oral | comp | 176 | 83 | 0.55* |
| | | | | | | | | | | | | | | | | | ms | 176 | 83 | 0.44 |

| *Study* | *Lang-uage* | *Year dat. coll.* | *Coun-try* | *Way of pub.* | *UE main topic?* | *UE-opera.* | *Re-emp?* | *Prct. part-time?* | *Prct. fem.* | *Prct. minor.* | *Age* | *Prct. mar.* | *Years educ.* | *Prct. bl. col.* | *Mean UE-dur.* | *Ques. form* | *MH-var.* | *N-ue* | *N-emp* | *d* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | | | | | | | | | dep | 176 | 83 | 0.44 |
| | | | | | | | | | | | | | | | | | anxi | 176 | 83 | 0.44 |
| Banks & Jackson (1982) | E | 1980 | GBR | Jour. | yes | involun. | other | --- | 43 | 29 | 19 | --- | --- | --- | --- | --- | ms | 67 | 305 | 1.36* |
| | E | 1980 | GBR | Jour. | yes | involun. | other | --- | 48 | 19 | 18 | --- | --- | --- | --- | --- | ms | 77 | 329 | 1.20* |
| Baumann et al. (1979)a | G | 1977 | GER | Book | yes | involun. | other | --- | 44 | --- | 17 | --- | --- | --- | --- | oral | comp | 289 | 83 | 0.40* |
| | | | | | | | | | | | | | | | | | dep | 289 | 83 | 0.53 |
| | | | | | | | | | | | | | | | | | se | 289 | 83 | 0.16 |
| Behle (2001) | G | 2000 | GER | Jour. | yes | regist. | reemp | 0 | 0 | --- | 17 | 51 | --- | --- | --- | oral | ms | 337 | 319 | 0.57* |
| | G | 2000 | GER | Jour. | yes | regist. | reemp | 0 | 100 | --- | 17 | 65 | --- | --- | --- | oral | ms | 249 | 159 | 0.81* |
| Beiser et al. (1993) | E | 1983 | CAN | Jour. | yes | diff other. | other | --- | 44 | 100 | 34 | 53 | --- | --- | --- | oral | dep | 398 | 694 | 0.4* |
| | E | 1983 | CAN | Jour. | yes | diff other. | other | --- | 44 | --- | 34 | --- | --- | --- | --- | oral | dep | 58 | 178 | 0.37* |
| Berger & Mohr (1986) | G | 1978 | GER | Jour. | no | diff other. | other | --- | --- | --- | 42 | --- | --- | --- | --- | oral | swb | 26 | 125 | 0.23* |
| | G | 1984 | GER | Jour. | no | diff other. | other | --- | --- | --- | 42 | --- | --- | --- | --- | oral | swb | 44 | 129 | 1.27* |
| Bolton & Oatley (1987) | E | --- | GBR | Jour. | yes | regist. | other | --- | 0 | --- | 40 | --- | --- | --- | 7 | oral | dep | 20 | 45 | 0.82* |
| Bradburn (1969) | E | 1963 | USA | Book | no | diff other. | other | --- | 0 | 19 | 38 | 77 | --- | 57 | --- | oral | swb | 83 | 1101 | 1.18* |
| | E | 1963 | USA | Book | no | diff other. | other | --- | 100 | 19 | 38 | 77 | --- | 40 | --- | oral | swb | 27 | 219 | 0.95* |
| Bradburn & Caplovitz (1965) | E | 1962 | USA | Book | no | diff other. | other | 0 | 0 | --- | 42 | 90 | 9 | --- | --- | --- | swb | 73 | 520 | 0.78* |
| | E | 1962 | USA | Book | no | diff other. | other | 0 | 100 | --- | 42 | 77 | --- | --- | --- | --- | swb | 30 | 200 | 0.60* |
| Branthwaite & Garcia (1985) | E | 1982 | GBR | Jour. | yes | regist. | other | --- | 10 | --- | 18 | --- | --- | --- | 8 | writ. | dep | 6 | 14 | 1.04* |
| Brenna et al. (1987) | E | 1983 | ITA | Book | yes | regist. | other | --- | 0 | --- | 38 | --- | --- | 100 | 7 | oral | ms | 302 | 241 | 0.90* |
| Brenner et al. | E | 1984 | SWE | Jour. | yes | plant | other | --- | 89 | 36 | 41 | 80 | --- | 100 | 18 | writ. | ms | 98 | 100 | 0.70* |

| *Study* | *Lang-uage* | *Year dat. coll.* | *Coun-try* | *Way of pub.* | *UE main topic?* | *UE-opera.* | *Re-emp?* | *Prct. part-time?* | *Prct. fem.* | *Prct. minor.* | *Age* | *Prct. mar.* | *Years educ.* | *Prct. bl. col.* | *Mean UE-dur.* | *Ques. form* | *MH-var.* | *N-ue* | *N-emp* | *d* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| (1989) / Brenner & Starrin (1988) | | | | | | clos. | | | | | | | | | | | | | | |
| Brinkmann & Potthoff (1983) / Brinkmann (1985) | G | 1981 | GER | Jour. | yes | regist. | other | --- | 100 | --- | 55 | --- | --- | --- | 1 | writ. | comp | 205 | 149 | -0.10* |
| | | | | | | | | | | | | | | | | | dep | 220 | 142 | -0.05 |
| | | | | | | | | | | | | | | | | | psysom | 205 | 149 | -0.12 |
| | G | 1981 | GER | Jour. | yes | regist. | other | --- | 100 | --- | 35 | --- | --- | --- | 1 | writ. | comp | 496 | 244 | -0.16* |
| | | | | | | | | | | | | | | | | | dep | 519 | 236 | 0.1 |
| | | | | | | | | | | | | | | | | | psysom | 496 | 244 | -0.38 |
| | G | 1981 | GER | Jour. | yes | regist. | other | --- | 0 | --- | 55 | --- | --- | --- | 1 | writ. | comp | 277 | 168 | 0.18* |
| | | | | | | | | | | | | | | | | | dep | 277 | 186 | 0.29 |
| | | | | | | | | | | | | | | | | | psysom | 277 | 168 | 0.02 |
| | G | 1981 | GER | Jour. | yes | regist. | other | --- | 0 | --- | 35 | --- | --- | --- | 1 | writ. | comp | 689 | 289 | 0.29* |
| | | | | | | | | | | | | | | | | | dep | 742 | 282 | 0.56 |
| | | | | | | | | | | | | | | | | | psysom | 689 | 289 | -0.06 |
| | | 1982 | GER | Book | yes | regist. | reemp | --- | --- | --- | 46 | --- | --- | --- | 19 | writ. | se | 360 | 378 | 0.32[a] |
| Broman et al. (1994) | E | 1988 | USA | Jour. | yes | plant clos. | other | --- | 19 | 24 | 43 | --- | 12 | 100 | 12 | writ. | comp | 324 | 757 | 0.53* |
| | | | | | | | | | | | | | | | | | dep | 324 | 757 | 0.47 |
| | | | | | | | | | | | | | | | | | anxi | 324 | 757 | 0.44 |
| Brown & Gary (1985) | E | 1981 | USA | Jour. | yes | diff other. | other | --- | 66 | 100 | 40 | 25 | 10 | 85 | 74 | oral | dep | 109 | 246 | 0.64* |
| Büchtemann & van Rosenbladt (1981) | G | 1978 | GER | Jour. | yes | regist. | reemp | --- | 100 | --- | --- | 68 | --- | 58 | 11 | oral | anxi | 636 | 752 | 0.13* |
| | G | 1978 | GER | Jour. | yes | regist. | reemp | --- | 0 | --- | --- | --- | --- | 58 | 11 | oral | anxi | 510 | 1241 | 0.10* |
| | G | 1978 | GER | Jour. | yes | regist. | reemp | --- | --- | --- | --- | --- | --- | 58 | 11 | oral | psysom | 921 | 581 | 0.33[2] |
| Burke (1984) | E | 1983 | CAN | Jour. | yes | plant clos. | reemp | --- | 12 | --- | 41 | 74 | 10 | --- | 13 | writ. | comp | 102 | 73 | 0.43* |
| | | | | | | | | | | | | | | | | | psysom | 102 | 73 | 0.30 |
| | | | | | | | | | | | | | | | | | swb | 102 | 73 | 0.44 |

| *Study* | *Lang-uage* | *Year dat. coll.* | *Coun-try* | *Way of pub.* | *UE main topic?* | *UE-opera.* | *Re-emp?* | *Prct. part-time?* | *Prct. fem.* | *Prct. minor.* | *Age* | *Prct. mar.* | *Years educ.* | *Prct. bl. col.* | *Mean UE-dur.* | *Ques. form* | *MH-var.* | *N-ue* | *N-emp* | *d* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Buss & Redburn (1983) | E | 1978 | USA | Book | yes | plant clos. | other | --- | 0 | 31 | 42 | 79 | --- | 100 | 12 | writ. | comp | 60 | 128 | 0.34* |
| | | | | | | | | | | | | | | | | | dep | 60 | 128 | 0.14 |
| | | | | | | | | | | | | | | | | | anxi | 60 | 128 | 0.42 |
| | | | | | | | | | | | | | | | | | psysom | 60 | 128 | 0.25 |
| Campbell et al. (1976) | E | 1971 | USA | Book | no | diff other. | other | 0 | 0 | 13 | 39 | --- | --- | --- | --- | oral | swb | 36 | 679 | 1.00* |
| | E | 1971 | USA | Book | no | diff other. | other | 0 | 100 | 13 | 39 | --- | --- | --- | --- | oral | swb | 38 | 403 | 0.33* |
| Caplan et al. (1989) | E | 1986 | USA | Jour. | yes | regist. | reemp | --- | 54 | 15 | 36 | 47 | 13 | 30 | 8 | writ. | comp | 158 | 368 | 0.37* |
| | | | | | | | | | | | | | | | | | dep | 158 | 369 | 0.38 |
| | | | | | | | | | | | | | | | | | anxi | 158 | 369 | 0.33 |
| | | | | | | | | | | | | | | | | | swb | 158 | 368 | 0.35 |
| | | | | | | | | | | | | | | | | | se | 158 | 368 | 0.11 |
| Carlson (1982) | E | 1981 | USA | Diss. | yes | regist. | other | --- | 100 | --- | 25 | 33 | 11 | 100 | --- | writ. | se | 18 | 24 | 0.13* |
| Carnes (1985) | E | 1984 | USA | Diss. | yes | involun. | other | --- | 0 | 0 | 34 | 86 | 12 | 100 | 6 | --- | comp | 50 | 50 | 1.13* |
| | | | | | | | | | | | | | | | | | dep | 50 | 50 | 0.86 |
| | | | | | | | | | | | | | | | | | anxi | 50 | 50 | 1.09 |
| Carroll (1985) | E | --- | USA | Diss. | yes | plant clos. | other | --- | 0 | 12 | 35 | 100 | 12 | 100 | 14 | writ. | ms | 46 | 23 | 0.82* |
| | | | | | | | | | | | | | | | | | ms-a | 46 | 23 | 0.77 |
| | | | | | | | | | | | | | | | | | ms-b | 46 | 23 | 0.87 |
| | | | | | | | | | | | | | | | | | dep | 46 | 23 | 0.95 |
| | | | | | | | | | | | | | | | | | anxi | 46 | 23 | 0.73 |
| | | | | | | | | | | | | | | | | | psysom | 46 | 23 | 0.38 |
| | | | | | | | | | | | | | | | | | se | 46 | 23 | 1.11 |
| Catalano et al. (2000) | E | 1996 | USA | Jour. | yes | involun. | other | --- | 35 | 100 | 33 | --- | 10 | --- | 10 | oral | dep | 246 | 1378 | 0.90* |
| Center for Disease Control (1998) | E | 1993 | USA | Jour. | no | diff other. | other | --- | 58 | 18 | 40 | 57 | --- | --- | 12 | oral | ms | 16578 | 231815 | 0.59* |
| Clark & Oswald (1994) | E | 1991 | GBR | Jour. | yes | involun. | other | --- | 46 | --- | 24 | --- | --- | --- | --- | writ. | ms | 248 | 1582 | 0.39* |

| *Study* | *Lang-uage* | *Year dat. coll.* | *Coun-try* | *Way of pub.* | *UE main topic?* | *UE-opera.* | *Re-emp?* | *Prct. part-time?* | *Prct. fem.* | *Prct. minor.* | *Age* | *Prct. mar.* | *Years educ.* | *Prct. bl. col.* | *Mean UE-dur.* | *Ques. form* | *MH-var.* | *N-ue* | *N-emp* | *d* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | E | 1991 | GBR | Jour. | yes | involun. | other | --- | 46 | --- | 40 | --- | --- | --- | --- | writ. | ms | 168 | 2941 | 0.57* |
| | E | 1991 | GBR | Jour. | yes | involun. | other | --- | 46 | --- | 58 | --- | --- | --- | --- | writ. | ms | 106 | 1104 | 0.49* |
| Clark & Clissold (1982) | E | --- | AUS | Jour. | yes | involun. | other | --- | 0 | --- | 18 | 0 | --- | 100 | --- | writ. | se | 116 | 54 | 0.13* |
| Claussen et al. (1993) | E | 1990 | NOR | Jour. | yes | regist. | reemp | --- | 44 | --- | 33 | --- | 10 | 52 | 32 | --- | comp | 164 | 113 | 0.47* |
| | | | | | | | | | | | | | | | | | ms | 164 | 113 | 0.39 |
| | | | | | | | | | | | | | | | | | dep | 164 | 113 | 0.51 |
| | | | | | | | | | | | | | | | | | anxi | 164 | 113 | 0.39 |
| | | | | | | | | | | | | | | | | | psysom | 164 | 113 | 0.16 |
| Cohn (1978) | E | 1969 | USA | Jour. | yes | diff other. | other | --- | 15 | --- | --- | --- | --- | 59 | 6 | oral | se | 537 | 543 | 0.72* |
| Creed & Reynolds (2001) | E | --- | AUS | Jour. | yes | regist. | other | 0 | 50 | --- | 21 | --- | --- | --- | --- | --- | ms | 47 | 39 | 1.29* |
| Creed (1999) | E | --- | AUS | Jour. | yes | regist. | reemp | --- | 51 | --- | 19 | --- | 11 | --- | 18 | writ. | comp | 72 | 15 | 0.89* |
| | | | | | | | | | | | | | | | | | ms | 74 | 15 | 0.69 |
| | | | | | | | | | | | | | | | | | se | 72 | 15 | 0.84 |
| Crepet et al. (1993) | E | --- | ITA | Jour. | yes | diff other. | other | --- | 56 | --- | 24 | 0 | --- | --- | --- | writ. | comp | 167 | 181 | 0.12* |
| | | | | | | | | | | | | | | | | | ms | 167 | 181 | 0.04 |
| | | | | | | | | | | | | | | | | | dep | 167 | 181 | 0.13 |
| | | | | | | | | | | | | | | | | | se | 167 | 181 | 0.12 |
| Cullen et al. (1987) | E | --- | IRL | Jour. | yes | involun. | other | --- | 0 | --- | 20 | --- | --- | 50 | --- | writ. | comp | 28 | 17 | 1.30* |
| | | | | | | | | | | | | | | | | | ms | 28 | 17 | 1.01 |
| | | | | | | | | | | | | | | | | | dep | 28 | 17 | 1.23 |
| | E | --- | IRL | Jour. | yes | involun. | other | --- | 100 | --- | 20 | --- | --- | 14 | --- | writ. | comp | 21 | 37 | 0.38* |
| | | | | | | | | | | | | | | | | | ms | 21 | 37 | -0.06 |
| | | | | | | | | | | | | | | | | | dep | 21 | 37 | 0.70 |
| D'Arcy & Siddique (1985) | E | 1978 | CAN | Jour. | yes | involun. | other | --- | 38 | --- | 36 | 67 | --- | 38 | --- | writ. | comp | 1349 | 9358 | 0.31* |
| | | | | | | | | | | | | | | | | | ms | 1548 | 10737 | 0.21 |
| | | | | | | | | | | | | | | | | | swb | 1349 | 9358 | 0.32 |

| *Study* | *Lang-uage* | *Year dat. coll.* | *Coun-try* | *Way of pub.* | *UE main topic?* | *UE-opera.* | *Re-emp?* | *Prct. part-time?* | *Prct. fem.* | *Prct. minor.* | *Age* | *Prct. mar.* | *Years educ.* | *Prct. bl. col.* | *Mean UE-dur.* | *Ques. form* | *MH-var.* | *N-ue* | *N-emp* | *d* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Dalbert (1993) | G | 1991 | GER | Jour. | yes | regist. | other | --- | 100 | --- | 38 | --- | --- | 64 | --- | writ. | comp | 54 | 31 | -0.37* |
| | | | | | | | | | | | | | | | | | ms | 54 | 31 | -0.5 |
| | | | | | | | | | | | | | | | | | swb | 54 | 31 | -0.67 |
| | | | | | | | | | | | | | | | | | se | 54 | 31 | 0.27 |
| Daniels (1986) | E | 1982 | USA | Jour. | yes | diff other. | other | --- | 0 | 100 | 23 | 24 | 12 | --- | 10 | oral | ms | 26 | 58 | 0.00* |
| | | | | | | | | | | | | | | | | | dep | 26 | 58 | 0.00 |
| | | | | | | | | | | | | | | | | | anxi | 26 | 58 | 0.00 |
| | | | | | | | | | | | | | | | | | psysom | 26 | 58 | 0.00 |
| Dasch (1989) | E | --- | USA | Diss. | yes | regist. | other | --- | 53 | --- | 40 | 100 | --- | 0 | 6 | writ. | comp | 56 | 49 | 0.06* |
| | | | | | | | | | | | | | | | | | dep | 56 | 49 | -0.11 |
| | | | | | | | | | | | | | | | | | se | 56 | 49 | 0.21 |
| Derenzo (1989) | E | 1988 | USA | other | yes | regist. | reemp | 37 | 55 | 17 | 36 | --- | 12 | --- | 6 | writ. | comp | 121 | 80 | 0.20* |
| | | | | | | | | | | | | | | | | | dep | 122 | 80 | 0.16 |
| | | | | | | | | | | | | | | | | | swb | 121 | 80 | 0.15 |
| | | | | | | | | | | | | | | | | | se | 122 | 80 | 0.19 |
| Deuermeier & Sawalies (1978) | G | 1977 | GER | other | yes | diff other. | other | --- | 0 | --- | 18 | --- | --- | --- | 9 | writ. | comp | 24 | 21 | 1.23* |
| | | | | | | | | | | | | | | | | | dep | 24 | 21 | 0.87 |
| | | | | | | | | | | | | | | | | | psysom | 24 | 21 | 1.24 |
| Dew et al. (1992) | E | 1986 | USA | Jour. | yes | plant clos. | reemp | --- | 100 | 0 | 37 | 61 | --- | 100 | 6 | oral | comp | 20 | 44 | 0.18* |
| | | | | | | | | | | | | | | | | | dep | 20 | 44 | 0.19 |
| | | | | | | | | | | | | | | | | | anxi | 20 | 44 | 0.13 |
| Dieth (1995) | G | 1993 | CH | Diss. | yes | regist. | other | 4 | 32 | --- | 43 | --- | --- | 0 | 10 | writ. | comp | 125 | 27 | 0.41* |
| | | | | | | | | | | | | | | | | | psysom | 125 | 27 | 0.00 |
| | | | | | | | | | | | | | | | | | swb | 125 | 27 | 0.71 |
| Dobberstein (1979) | G | --- | GER | other | yes | regist. | other | --- | 73 | --- | 32 | 70 | --- | --- | 0 | writ. | comp | 20 | 20 | 0.36* |
| | | | | | | | | | | | | | | | | | dep | 20 | 20 | 0.64 |
| | | | | | | | | | | | | | | | | | psysom | 20 | 20 | 0.00 |
| | | | | | | | | | | | | | | | | | se | 20 | 20 | 0.25 |

| *Study* | *Lang-uage* | *Year dat. coll.* | *Coun-try* | *Way of pub.* | *UE main topic?* | *UE-opera.* | *Re-emp?* | *Prct. part-time?* | *Prct. fem.* | *Prct. minor.* | *Age* | *Prct. mar.* | *Years educ.* | *Prct. bl. col.* | *Mean UE-dur.* | *Ques. form* | *MH-var.* | *N-ue* | *N-emp* | *d* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Doherty & Davies (1984) | E | --- | GBR | Jour. | yes | diff other. | other | --- | 50 | --- | 17 | --- | --- | --- | --- | writ. | comp | 50 | 50 | 1.02* |
| | | | | | | | | | | | | | | | | | ms | 50 | 50 | 0.61 |
| | | | | | | | | | | | | | | | | | dep | 50 | 50 | 1.13 |
| | | | | | | | | | | | | | | | | | se | 50 | 50 | 0.74 |
| Donovan & Oddy (1982) | E | 1981 | GBR | Jour. | yes | regist. | other | --- | 100 | --- | 16 | --- | --- | --- | --- | writ. | comp | 12 | 12 | 1.38* |
| | | | | | | | | | | | | | | | | | ms | 12 | 12 | 1.34 |
| | | | | | | | | | | | | | | | | | dep | 12 | 12 | 0.60 |
| | | | | | | | | | | | | | | | | | anxi | 12 | 12 | 1.36 |
| | | | | | | | | | | | | | | | | | swb | 12 | 12 | 0.95 |
| | | | | | | | | | | | | | | | | | se | 12 | 12 | 1.00 |
| | E | 1981 | GBR | Jour. | yes | regist. | other | --- | 0 | --- | 16 | --- | --- | --- | --- | writ. | comp | 12 | 12 | 1.63* |
| | | | | | | | | | | | | | | | | | ms | 12 | 12 | 1.08 |
| | | | | | | | | | | | | | | | | | dep | 12 | 12 | 1.13 |
| | | | | | | | | | | | | | | | | | anxi | 12 | 12 | 1.19 |
| | | | | | | | | | | | | | | | | | swb | 12 | 12 | 1.84 |
| | | | | | | | | | | | | | | | | | se | 12 | 12 | 0.99 |
| Donovan et al. (1986) | E | 1983 | GBR | Jour. | yes | diff other. | other | 0 | 0 | --- | 17 | --- | --- | --- | 6 | writ. | ms | 26 | 29 | 0.24* |
| | E | 1983 | GBR | Jour. | yes | diff other. | other | 0 | 100 | --- | 17 | --- | --- | --- | 6 | writ. | ms | 14 | 16 | 0.76* |
| Dooley et al. (1994) | E | 1981 | USA | Jour. | yes | regist. | other | --- | --- | --- | --- | --- | --- | --- | 6 | oral | comp | 339 | 8059 | 0.27* |
| | | | | | | | | | | | | | | | | | dep | 342 | 8098 | 0.42 |
| | | | | | | | | | | | | | | | | | anxi | 339 | 8059 | 0.05 |
| Döring (1982) | G | 1981 | GER | other | yes | involun. | other | 0 | 0 | --- | 18 | --- | --- | 63 | 12 | writ. | ms | 12 | 12 | 1.17* |
| | | | | | | | | | | | | | | | | | dep | 12 | 12 | 1.23 |
| | | | | | | | | | | | | | | | | | anxi | 12 | 12 | 0.60 |
| Dressler (1986) | E | 1981 | USA | Jour. | yes | involun. | other | --- | 59 | 100 | 42 | 54 | --- | --- | --- | writ. | dep | 49 | 130 | 0.71* |
| Eaton & Kessler (1981) | E | 1975 | USA | Jour. | no | diff other. | other | --- | 56 | 9 | 44 | 79 | 11 | --- | --- | writ. | dep | 149 | 1608 | 0.58* |
| Ehrhardt (1993) | G | 1990 | GER | Book | yes | regist. | reemp | --- | 100 | --- | 36 | 75 | --- | --- | 6 | writ. | swb | 74 | 33 | 0.00* |

| *Study* | *Lang-uage* | *Year dat. coll.* | *Coun-try* | *Way of pub.* | *UE main topic?* | *UE-opera.* | *Re-emp?* | *Prct. part-time?* | *Prct. fem.* | *Prct. minor.* | *Age* | *Prct. mar.* | *Years educ.* | *Prct. bl. col.* | *Mean UE-dur.* | *Ques. form* | *MH-var.* | *N-ue* | *N-emp* | *d* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Engel (1982) | G | 1980 | GER | Jour. | yes | diff other. | other | --- | 53 | 0 | 17 | --- | --- | 100 | --- | writ. | se | 48 | 167 | 0.08* |
| Ensminger & Celentano (1988) | E | 1984 | USA | Jour. | yes | regist. | other | --- | 100 | 68 | --- | 39 | --- | 20 | --- | oral | ms | 64 | 43 | 0.59* |
| | E | 1984 | USA | Jour. | yes | regist. | other | --- | 0 | 68 | --- | 64 | --- | 72 | --- | oral | ms | 69 | 49 | 0.86* |
| Estes & Wilensky (1978) | E | 1971 | USA | Jour. | no | regist. | other | --- | 24 | --- | 42 | 78 | 18 | 0 | --- | writ. | ms | 157 | 228 | 0.44* |
| Evans (1986) | E | 1984 | GBR | Diss. | yes | regist. | other | --- | 42 | --- | 22 | 4 | --- | 47 | 12 | writ. | comp | 36 | 36 | 0.80* |
| | | | | | | | | | | | | | | | | | ms | 36 | 36 | 0.75 |
| | | | | | | | | | | | | | | | | | swb | 36 | 36 | 0.61 |
| | | | | | | | | | | | | | | | | | se | 36 | 36 | 0.59 |
| Fagin & Little (1984) | E | 1979 | GBR | Book | yes | regist. | reemp | --- | 0 | --- | 42 | 100 | --- | 70 | 6 | writ. | ms | 13 | 7 | 0.46* |
| Feather & O'Brien (1986) | E | 1981 | AUS | Jour. | yes | diff other. | other | --- | --- | --- | 18 | --- | 11 | --- | --- | writ. | comp | 116 | 497 | 0.44* |
| | | | | | | | | | | | | | | | | | ms | 116 | 497 | 0.24 |
| | | | | | | | | | | | | | | | | | dep | 126 | 535 | 0.28 |
| | | | | | | | | | | | | | | | | | swb | 122 | 519 | 0.39 |
| | | | | | | | | | | | | | | | | | se | 123 | 525 | 0.48 |
| Feather & Bond (1983) | E | 1981 | AUS | Jour. | yes | involun. | other | 0 | 43 | --- | 26 | --- | 11 | 0 | 7 | writ. | comp | 43 | 255 | 0.60* |
| | | | | | | | | | | | | | | | | | dep | 43 | 255 | 0.77 |
| | | | | | | | | | | | | | | | | | se | 43 | 255 | 0.26 |
| Feather (1982) | E | 1979 | USA | Jour. | yes | diff other. | other | --- | 52 | --- | 21 | --- | --- | --- | 7 | writ. | comp | 69 | 78 | 0.73* |
| | | | | | | | | | | | | | | | | | dep | 69 | 78 | 0.76 |
| | | | | | | | | | | | | | | | | | se | 69 | 78 | 0.49 |
| Fineman (1983) | E | 1976 | GBR | Book | yes | regist. | reemp | --- | 18 | --- | 42 | 65 | --- | 0 | 13 | writ. | comp | 17 | 40 | 0.34* |
| | | | | | | | | | | | | | | | | | ms | 17 | 40 | 0.48 |
| | | | | | | | | | | | | | | | | | psysom | 17 | 40 | -0.24 |
| | | | | | | | | | | | | | | | | | se | 17 | 40 | 0.59 |
| Frese (1979) | G | 1977 | GER | Book | yes | regist. | reemp | --- | 0 | 0 | 56 | --- | --- | 100 | --- | writ. | comp | 26 | 15 | 0.88* |

| *Study* | *Lang-uage* | *Year dat. coll.* | *Coun-try* | *Way of pub.* | *UE main topic?* | *UE-opera.* | *Re-emp?* | *Prct. part-time?* | *Prct. fem.* | *Prct. minor.* | *Age* | *Prct. mar.* | *Years educ.* | *Prct. bl. col.* | *Mean UE-dur.* | *Ques. form* | *MH-var.* | *N-ue* | *N-emp* | *d* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | | | | | | | | | dep | 26 | 15 | 1.00 |
| | | | | | | | | | | | | | | | | | psysom | 26 | 15 | 0.51 |
| Frese (1994) | G | 1992 | GER | Book | yes | diff other. | other | --- | --- | --- | 43 | --- | --- | --- | 8 | writ. | dep | 39 | 346 | 0.33* |
| Friis et al. (1998) | E | 1992 | SWE | Jour. | yes | plant clos. | other | --- | 2 | --- | 49 | --- | --- | 100 | 12 | writ. | ms | 50 | 90 | 0.36* |
| Frost & Clayson (1991) | E | --- | USA | Jour. | yes | regist. | other | --- | 10 | 6 | 38 | 87 | --- | 100 | 7 | writ. | se | 236 | 314 | 0.12* |
| | | | | | | | | | | | | | | | | | swb | 228 | 303 | 0.30 |
| Gallie & Vogler (1990) | E | 1987 | GBR | Jour. | yes | involun. | other | --- | 100 | --- | 41 | --- | --- | 42 | --- | writ. | ms | 89 | 380 | 0.42* |
| | E | 1987 | GBR | Jour. | yes | involun. | other | --- | 0 | --- | 41 | --- | --- | 43 | --- | writ. | ms | 109 | 435 | 0.50* |
| Garrett (1988) | E | 1988 | USA | other | yes | plant clos. | reemp | 37 | 36 | 16 | 37 | 61 | --- | 12 | 10 | writ. | dep | 32 | 59 | 0.69* |
| Gaskell & Smith (1981) | E | 1979 | GBR | Jour. | no | regist. | other | --- | 0 | 62 | 21 | --- | --- | --- | --- | oral | se | 99 | 93 | 0.00* |
| Gien (2000) | E | 1995 | CAN | Jour. | yes | involun. | other | --- | 46 | --- | 38 | --- | 11 | 41 | --- | writ. | comp | 291 | 152 | 0.13* |
| | | | | | | | | | | | | | | | | | ms | 291 | 152 | 0.01 |
| | | | | | | | | | | | | | | | | | dep | 291 | 152 | 0.12 |
| | | | | | | | | | | | | | | | | | anxi | 291 | 152 | -0.07 |
| | | | | | | | | | | | | | | | | | psysom | 291 | 152 | -0.09 |
| | | | | | | | | | | | | | | | | | swb | 291 | 152 | 0.39 |
| | | | | | | | | | | | | | | | | | se | 291 | 152 | 0.22 |
| Ginexi et al. (2000) | E | --- | USA | Jour. | yes | regist. | reemp | 34 | 39 | 44 | 36 | 100 | --- | 35 | 5 | writ. | dep | 104 | 119 | 0.49* |
| Glenn (1988) | E | 1987 | USA | Diss. | yes | involun. | other | --- | 16 | 33 | 43 | --- | --- | 100 | --- | writ. | comp | 125 | 35 | 1.64* |
| | | | | | | | | | | | | | | | | | dep | 125 | 35 | 1.42 |
| | | | | | | | | | | | | | | | | | anxi | 125 | 35 | 1.22 |
| | | | | | | | | | | | | | | | | | psysom | 125 | 35 | 1.02 |
| | | | | | | | | | | | | | | | | | se | 125 | 35 | 1.45 |
| Goodchilds & Smith (1963) | E | --- | USA | Jour. | yes | regist. | other | --- | 0 | --- | 27 | --- | --- | --- | 3 | writ. | se | 54 | 18 | 0.00* |
| Graetz (1991)[b] | E | 1985 | AUS | Jour. | no | diff other. | other | 0 | 50 | --- | 20 | --- | --- | --- | --- | oral | ms | 944 | 4694 | 0.33* |
| | | | | | | | | | | | | | | | | | anxi | 944 | 4694 | 0.13 |

| *Study* | *Lang-uage* | *Year dat. coll.* | *Coun-try* | *Way of pub.* | *UE main topic?* | *UE-opera.* | *Re-emp?* | *Prct. part-time?* | *Prct. fem.* | *Prct. minor.* | *Age* | *Prct. mar.* | *Years educ.* | *Prct. bl. col.* | *Mean UE-dur.* | *Ques. form* | *MH-var.* | *N-ue* | *N-emp* | *d* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | | | | | | | | | se | 944 | 4694 | 0.51 |
| Grant & Barling (1994) | E | --- | CAN | Book | yes | involun. | other | --- | 27 | --- | 40 | 100 | 12 | --- | 7 | writ. | dep | 85 | 85 | 0.55* |
| Grassi & Falzoni (1991) | E | --- | ITA | Jour. | no | involun. | other | --- | 0 | --- | 25 | --- | --- | --- | --- | writ. | ms | 40 | 87 | 0.09* |
| Gurney (1980) | E | 1979 | AUS | Jour. | yes | diff other. | other | --- | 0 | --- | 17 | --- | 11 | --- | 4 | --- | se | 32 | 132 | 0.19* |
| | E | 1979 | AUS | Jour. | yes | diff other. | other | --- | 100 | --- | 17 | --- | 11 | --- | 4 | --- | se | 32 | 132 | 0.60* |
| Hall & Johnson (1988)[c] | E | 1984 | SWE | Jour. | yes | regist. | other | --- | 100 | --- | --- | 84 | --- | 100 | 24 | writ. | dep | 96 | 51 | 0.58 |
| Halvorsen (1998) | E | 1992 | NOR | Jour. | yes | regist. | reemp | --- | 50 | --- | 34 | --- | 11 | --- | --- | oral | ms | 220 | 116 | 0.35* |
| Hammer (1993) | E | 1987 | NOR | Jour. | yes | diff other. | other | --- | 49 | --- | 21 | --- | --- | --- | 5 | writ. | comp | 103 | 638 | 0.41* |
| | | | | | | | | | | | | | | | | | ms | 103 | 638 | 0.45 |
| | | | | | | | | | | | | | | | | | se | 103 | 638 | 0.25 |
| Hannan et al. (1997) | E | 1987 | IRL | Jour. | yes | diff other. | other | --- | --- | --- | 22 | --- | --- | --- | --- | --- | ms | 217 | 815 | 0.72* |
| Harding & Sewel (1992) | E | 1984 | GBR | Book | yes | regist. | reemp | 0 | 0 | --- | 40 | --- | --- | --- | --- | writ. | comp | 49 | 142 | 0.59* |
| | | | | | | | | | | | | | | | | | ms | 49 | 142 | 0.69 |
| | | | | | | | | | | | | | | | | | swb | 49 | 142 | 0.33 |
| | E | 1984 | GBR | Book | yes | regist. | reemp | 0 | 100 | --- | 40 | --- | --- | --- | --- | writ. | comp | 31 | 153 | 0.11* |
| | | | | | | | | | | | | | | | | | ms | 31 | 153 | 0.20 |
| | | | | | | | | | | | | | | | | | swb | 31 | 153 | 0.00 |
| Harper (1987) | E | 1978 | USA | Diss. | yes | diff other. | other | --- | 41 | --- | 37 | 66 | --- | --- | --- | oral | comp | 146 | 2385 | 0.53* |
| | | | | | | | | | | | | | | | | | swb | 146 | 2385 | 0.63 |
| | | | | | | | | | | | | | | | | | se | 146 | 2385 | 0.27 |
| Harrison et al. (1999) | E | --- | GBR | Jour. | no | diff other. | other | 0 | 57 | 4 | 40 | --- | --- | --- | --- | writ. | ms | 2139 | 15924 | 0.55* |
| Hartley (1980) | E | --- | GBR | Jour. | yes | regist. | other | --- | --- | --- | 43 | --- | --- | 0 | 4 | writ. | se | 87 | 64 | 0.11* |
| Haworth et al. | E | --- | GBR | Jour. | yes | diff | other | --- | 0 | --- | 41 | --- | --- | --- | --- | writ. | ms | 12 | 12 | 1.26* |

| *Study* | *Lang-uage* | *Year dat. coll.* | *Coun-try* | *Way of pub.* | *UE main topic?* | *UE-opera.* | *Re-emp?* | *Prct. part-time?* | *Prct. fem.* | *Prct. minor.* | *Age* | *Prct. mar.* | *Years educ.* | *Prct. bl. col.* | *Mean UE-dur.* | *Ques. form* | *MH-var.* | *N-ue* | *N-emp* | *d* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| (1990) | | | | | | other. | | | | | | | | | | | | | | |
| Haworth & Evans (1987) | E | --- | GBR | Book | yes | regist. | other | --- | --- | --- | 22 | --- | --- | --- | --- | oral | comp | 36 | 36 | 1.06* |
| | | | | | | | | | | | | | | | | | ms | 36 | 36 | 0.74 |
| | | | | | | | | | | | | | | | | | swb | 36 | 36 | 1.25 |
| | | | | | | | | | | | | | | | | | se | 36 | 36 | 0.59 |
| Heady & Smyth (1989) | E | 1984 | GBR | Book | yes | regist. | reemp | 0 | 32 | --- | 27 | 0 | --- | 72 | 15 | writ. | ms | 42 | 36 | 0.89* |
| | E | 1984 | GBR | Book | yes | regist. | reemp | 0 | 0 | --- | 27 | 100 | --- | 72 | 15 | writ. | ms | 22 | 65 | 0.47* |
| | E | 1984 | GBR | Book | yes | regist. | reemp | 0 | 0 | --- | 29 | 100 | --- | 72 | 15 | writ. | ms | 252 | 204 | 0.60* |
| | E | 1984 | GBR | Book | yes | regist. | reemp | 0 | 32 | --- | 48 | 0 | --- | 72 | 15 | writ. | ms | 137 | 37 | 0.91* |
| | E | 1984 | GBR | Book | yes | regist. | reemp | 0 | 0 | --- | 52 | 100 | --- | 72 | 15 | writ. | ms | 184 | 145 | 0.37* |
| | E | 1984 | GBR | Book | yes | regist. | reemp | 0 | 0 | --- | 44 | 100 | --- | 72 | 15 | writ. | ms | 193 | 199 | 0.73* |
| Heinemann et al. (1980) | G | 1978 | GER | Book | yes | regist. | other | --- | 100 | --- | 35 | 70 | --- | 39 | 7 | oral | comp | 910 | 323 | 0.10* |
| | | | | | | | | | | | | | | | | | psysom | 910 | 323 | 0.10 |
| | | | | | | | | | | | | | | | | | se | 910 | 323 | 0.08 |
| Henwood & Miles (1987) | E | 1983 | GBR | Book | yes | regist. | other | 0 | 100 | --- | --- | --- | --- | --- | --- | writ. | swb | 20 | 28 | 0.00* |
| | E | 1983 | GBR | Book | yes | regist. | other | 0 | 0 | --- | --- | 75 | --- | --- | --- | writ. | swb | 35 | 56 | 0.42* |
| | E | --- | GBR | Jour. | yes | regist. | other | --- | 0 | --- | 35 | --- | --- | 100 | 9 | oral | ms | 20 | 268 | 3.86* |
| Hepworth (1980) | E | --- | GBR | Jour. | yes | regist. | other | --- | 0 | --- | 35 | --- | --- | 100 | 9 | oral | ms | 31 | 170 | 1.83* |
| | E | --- | GBR | Jour. | yes | regist. | other | --- | 0 | --- | 35 | --- | --- | 100 | 9 | oral | ms | 10 | 10 | 1.55* |
| | E | --- | GBR | Jour. | yes | regist. | other | --- | 0 | --- | 35 | --- | --- | 0 | 9 | oral | ms | 9 | 27 | 1.10* |
| | E | --- | GBR | Jour. | yes | regist. | other | --- | 0 | --- | 35 | --- | --- | 0 | 9 | oral | ms | 8 | 95 | 1.08* |
| Hesketh et al. (1987) | E | --- | NZ | Jour. | yes | regist. | reemp | --- | --- | --- | --- | --- | --- | --- | 1 | oral | swb | 27 | 24 | -0.45* |
| Heubeck et al. (1995) | E | 1987 | AUS | Jour. | yes | regist. | other | --- | 100 | --- | 18 | --- | --- | --- | 11 | oral | dep | 46 | 44 | 0.34* |
| | E | 1987 | AUS | Jour. | yes | regist. | other | --- | 0 | --- | 18 | --- | --- | --- | 11 | oral | dep | 48 | 43 | 0.51* |
| Hinton et al. (1998) | E | 1992 | USA | Jour. | no | diff other. | other | --- | 0 | 100 | 39 | --- | --- | --- | --- | oral | dep | 283 | 595 | 1.05* |
| | E | 1992 | USA | Jour. | no | diff other. | other | --- | 0 | 100 | 43 | --- | --- | --- | --- | oral | dep | 221 | 745 | 0.68* |

| *Study* | *Lang-uage* | *Year dat. coll.* | *Coun-try* | *Way of pub.* | *UE main topic?* | *UE-opera.* | *Re-emp?* | *Prct. part-time?* | *Prct. fem.* | *Prct. minor.* | *Age* | *Prct. mar.* | *Years educ.* | *Prct. bl. col.* | *Mean UE-dur.* | *Ques. form* | *MH-var.* | *N-ue* | *N-emp* | *d* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | E | 1992 | USA | Jour. | no | diff other. | other | --- | 0 | 100 | 41 | --- | --- | --- | --- | oral | dep | 109 | 852 | 0.50* |
| Hodiamont et al. (1987) | E | 1983 | NL | Jour. | no | diff other. | other | 0 | 0 | --- | 36 | 75 | --- | 40 | --- | oral | ms | 178 | 993 | 0.43* |
| | E | 1983 | NL | Jour. | no | diff other. | other | 0 | 100 | --- | 37 | 78 | --- | 32 | --- | oral | ms | 53 | 274 | 0.33* |
| Huppert & Garcia (1991) | E | --- | GBR | Jour. | no | diff other. | other | 0 | 0 | --- | 29 | --- | --- | 100 | --- | writ. | ms | 95 | 525 | 0.39* |
| | | | | | | | | | | | | | | | | | dep | 95 | 525 | 0.48 |
| | | | | | | | | | | | | | | | | | anxi | 95 | 525 | 0.21 |
| | | | | | | | | | | | | | | | | | se | 95 | 525 | 0.37 |
| | E | --- | GBR | Jour. | no | diff other. | other | 0 | 0 | --- | 52 | --- | --- | 100 | --- | writ. | ms | 67 | 472 | 0.78* |
| | | | | | | | | | | | | | | | | | dep | 67 | 472 | 0.80 |
| | | | | | | | | | | | | | | | | | anxi | 67 | 472 | 0.58 |
| | | | | | | | | | | | | | | | | | se | 67 | 472 | 0.49 |
| Huppert & Whittington (1993) | E | 1984 | GBR | Book | no | diff other. | other | 5 | 0 | --- | 29 | 59 | --- | 56 | --- | writ. | ms | 36 | 512 | 0.37* |
| | E | 1984 | GBR | Book | no | diff other. | other | 5 | 0 | --- | 48 | 88 | --- | 54 | --- | writ. | ms | 15 | 400 | 0.72* |
| Imai (2001) | E | --- | JPN | Jour. | yes | involun. | other | --- | 100 | --- | 33 | 58 | --- | 0 | --- | writ. | se | 352 | 552 | -0.53* |
| Iman (1995) | E | --- | USA | Diss. | yes | diff other. | other | --- | 0 | 100 | 38 | 35 | --- | --- | 15 | --- | ms | 40 | 36 | 0.47* |
| | E | --- | USA | Diss. | yes | diff other. | other | --- | 0 | 100 | 53 | 35 | --- | --- | 15 | --- | ms | 13 | 22 | 1.22* |
| Isaksson (1990) | E | 1985 | SWE | Jour. | yes | regist. | reemp | --- | 0 | --- | 28 | 0 | --- | --- | --- | writ. | ms | 45 | 27 | 0.63* |
| Iversen & Sarboe (1987) | E | 1983 | DNK | Book | yes | diff other. | other | --- | 0 | 0 | 40 | 78 | --- | 83 | --- | writ. | comp | 534 | 929 | 0.61* |
| | | | | | | | | | | | | | | | | | ms | 535 | 931 | 0.80 |
| | | | | | | | | | | | | | | | | | psysom | 534 | 929 | 0.25 |
| Jackson (1999) | E | --- | CAN | Jour. | yes | involun. | other | 0 | 66 | --- | 27 | --- | 15 | --- | 13 | writ. | ms | 41 | 44 | 0.62* |
| Jackson et al. | E | --- | CAN | Jour. | yes | involun. | other | --- | 51 | --- | 35 | 46 | 14 | --- | 16 | writ. | comp | 40 | 43 | 0.44* |

| *Study* | *Lang-uage* | *Year dat. coll.* | *Coun-try* | *Way of pub.* | *UE main topic?* | *UE-opera.* | *Re-emp?* | *Prct. part-time?* | *Prct. fem.* | *Prct. minor.* | *Age* | *Prct. mar.* | *Years educ.* | *Prct. bl. col.* | *Mean UE-dur.* | *Ques. form* | *MH-var.* | *N-ue* | *N-emp* | *d* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| (1996) | | | | | | | | | | | | | | | | | | | | |
| | | | | | | | | | | | | | | | | | ms | 40 | 43 | 0.52 |
| | | | | | | | | | | | | | | | | | psysom | 40 | 43 | 0.24 |
| Jehoel-Gijsbers & Groot (1989) | E | 1984 | NL | Jour. | yes | regist. | other | --- | --- | --- | 22 | --- | --- | --- | 18 | oral | swb | 897 | 265 | 1.23* |
| Jenkins et al. (1997) | E | --- | GBR | Jour. | no | diff other. | other | 0 | 50 | 5 | 41 | 67 | --- | 50 | --- | oral | ms | 847 | 5034 | 0.58* |
| Jex et al. (1994) | E | --- | USA | Jour. | yes | regist. | other | --- | 41 | --- | 34 | --- | --- | 33 | --- | writ. | comp | 187 | 133 | 0.52* |
| | | | | | | | | | | | | | | | | | dep | 187 | 133 | 0.43 |
| | | | | | | | | | | | | | | | | | anxi | 189 | 135 | 0.39 |
| | | | | | | | | | | | | | | | | | swb | 190 | 135 | 0.52 |
| | | | | | | | | | | | | | | | | | se | 190 | 135 | 0.29 |
| Jones-Webb & Snowden (1993) | E | 1984 | USA | Jour. | no | diff other. | other | --- | 73 | 100 | 41 | 23 | --- | 84 | --- | oral | dep | 62 | 164 | 0.34* |
| | E | 1984 | USA | Jour. | no | diff other. | other | --- | 70 | 0 | 44 | 48 | --- | 54 | --- | oral | dep | 20 | 140 | 0.81* |
| Jones (1991) | E | --- | USA | Jour. | yes | regist. | reemp | --- | 0 | 0 | 37 | 45 | --- | --- | 3 | oral | dep | 62 | 73 | 0.72* |
| Joseph (1999) | E | --- | USA | Diss. | yes | regist. | reemp | 0 | 40 | 17 | 47 | 71 | --- | 0 | 4 | writ. | comp | 33 | 19 | 0.07* |
| | | | | | | | | | | | | | | | | | dep | 33 | 19 | 0.22 |
| | | | | | | | | | | | | | | | | | psysom | 33 | 19 | -0.11 |
| Joshi et al. (1984) | E | --- | CAN | Jour. | yes | regist. | other | --- | 100 | --- | 28 | 44 | 13 | --- | 6 | writ. | se | 40 | 40 | -0.32* |
| Kabbe et al. (1996) | E | --- | SWE | Jour. | yes | involun. | other | --- | 18 | --- | 45 | --- | --- | 0 | 15 | writ. | comp | 32 | 367 | 0.69* |
| | | | | | | | | | | | | | | | | | dep | 32 | 367 | 0.98 |
| | | | | | | | | | | | | | | | | | anxi | 32 | 367 | 0.55 |
| | | | | | | | | | | | | | | | | | psysom | 32 | 367 | 0.2 |
| | | | | | | | | | | | | | | | | | swb | 32 | 367 | 0.78 |
| | | | | | | | | | | | | | | | | | se | 32 | 367 | 0.14 |
| Kaltseis (1987) | G | 1985 | AUT | Diss. | yes | regist. | other | --- | 27 | --- | 34 | 63 | --- | 77 | 6 | --- | dep | 70 | 66 | 0.32* |
| Kasl (1979) / Cobb & Kasl | E | --- | USA | Book | yes | plant clos. | other | --- | 0 | 8 | 48 | 100 | 10 | 100 | 4 | oral | comp | 51 | 74 | 0.31* |

| *Study* | *Lang-uage* | *Year dat. coll.* | *Coun-try* | *Way of pub.* | *UE main topic?* | *UE-opera.* | *Re-emp?* | *Prct. part-time?* | *Prct. fem.* | *Prct. minor.* | *Age* | *Prct. mar.* | *Years educ.* | *Prct. bl. col.* | *Mean UE-dur.* | *Ques. form* | *MH-var.* | *N-ue* | *N-emp* | *d* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| (1977) | | | | | | | | | | | | | | | | | | | | |
| | | | | | | | | | | | | | | | | | dep | 51 | 74 | 0.55 |
| | | | | | | | | | | | | | | | | | anxi | 51 | 74 | 0.17 |
| | | | | | | | | | | | | | | | | | psysom | 51 | 74 | -0.09 |
| | | | | | | | | | | | | | | | | | se | 51 | 74 | 0.34 |
| Kaufman (1973/1982) | E | 1971 | USA | other | yes | involun. | other | --- | 6 | --- | 29 | --- | --- | 0 | 6 | writ. | comp | 50 | 54 | 0.55* |
| | | | | | | | | | | | | | | | | | ms | 51 | 54 | 0.38 |
| | | | | | | | | | | | | | | | | | anxi | 51 | 54 | 0.27 |
| | | | | | | | | | | | | | | | | | swb | 51 | 54 | 0.55 |
| | | | | | | | | | | | | | | | | | se | 50 | 54 | 0.56 |
| Kessler (1995) | E | --- | USA | Diss. | yes | regist. | other | --- | 50 | --- | 50 | 100 | --- | 0 | 18 | writ. | dep | 100 | 100 | -0.06* |
| Kessler et al. (1987) | E | 1984 | USA | Jour. | yes | involun. | other | --- | 44 | 23 | 35 | 47 | 12 | --- | 26 | oral | comp | 146 | 184 | 0.66* |
| | | | | | | | | | | | | | | | | | dep | 146 | 184 | 0.63 |
| | | | | | | | | | | | | | | | | | anxi | 146 | 184 | 0.62 |
| | | | | | | | | | | | | | | | | | psysom | 146 | 184 | 0.36 |
| Kinicki (1985) | E | --- | USA | Jour. | yes | plant clos. | reemp | --- | 20 | --- | 35 | --- | --- | --- | 4 | writ. | anxi | 48 | 12 | 0.00* |
| Kinicki et al. (2000) | E | --- | USA | Jour. | yes | plant clos. | reemp | --- | 67 | --- | 35 | 52 | --- | --- | 4 | writ. | comp | 78 | 22 | 0.13* |
| | | | | | | | | | | | | | | | | | swb | 78 | 22 | 0.01 |
| | | | | | | | | | | | | | | | | | se | 78 | 22 | 0.21 |
| Kirchler (1985) | E | 1983 | AUS | Jour. | yes | involun. | reemp | --- | 46 | --- | 29 | --- | --- | --- | 2 | --- | swb | 23 | 3 | 0.19* |
| Kokko & Pulkkinen (1998) | E | 1995 | FIN | Jour. | yes | diff other. | other | --- | 49 | --- | 36 | 78 | --- | --- | 15 | writ. | comp | 58 | 177 | 0.49* |
| | | | | | | | | | | | | | | | | | ms | 58 | 177 | 0.22 |
| | | | | | | | | | | | | | | | | | dep | 58 | 177 | 0.48 |
| | | | | | | | | | | | | | | | | | anxi | 58 | 177 | 0.48 |
| | | | | | | | | | | | | | | | | | se | 58 | 177 | 0.35 |
| Kopacsi (1990) | E | 1987 | USA | Diss. | yes | plant clos. | reemp | --- | 100 | 25 | 38 | 54 | 12 | 100 | 21 | oral | comp | 21 | 53 | -0.05* |

| *Study* | *Lang-uage* | *Year dat. coll.* | *Coun-try* | *Way of pub.* | *UE main topic?* | *UE-opera.* | *Re-emp?* | *Prct. part-time?* | *Prct. fem.* | *Prct. minor.* | *Age* | *Prct. mar.* | *Years educ.* | *Prct. bl. col.* | *Mean UE-dur.* | *Ques. form* | *MH-var.* | *N-ue* | *N-emp* | *d* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | | | | | | | | | ms | 21 | 53 | 0.1 |
| | | | | | | | | | | | | | | | | | swb | 21 | 53 | -0.06 |
| | | | | | | | | | | | | | | | | | se | 21 | 53 | -0.16 |
| Koskela et al. (1994) | E | 1992 | FIN | Jour. | yes | plant clos. | other | --- | 100 | --- | 44 | --- | --- | --- | 5 | writ. | comp | 43 | 31 | -0.36* |
| | | | | | | | | | | | | | | | | | ms | 43 | 31 | 0.15 |
| | | | | | | | | | | | | | | | | | dep | 43 | 31 | -0.16 |
| | | | | | | | | | | | | | | | | | psysom | 43 | 31 | -0.85 |
| | E | 1992 | FIN | Jour. | yes | plant clos. | other | --- | 0 | --- | 43 | 100 | --- | --- | 5 | writ. | comp | 66 | 139 | 0.59* |
| | | | | | | | | | | | | | | | | | ms | 66 | 139 | 0.99 |
| | | | | | | | | | | | | | | | | | dep | 66 | 139 | 0.54 |
| | | | | | | | | | | | | | | | | | psysom | 66 | 139 | -0.1 |
| | E | 1992 | FIN | Jour. | yes | plant clos. | other | --- | 0 | --- | 43 | 0 | --- | --- | 5 | writ. | comp | 18 | 20 | 0.18* |
| | | | | | | | | | | | | | | | | | ms | 18 | 20 | 0.6 |
| | | | | | | | | | | | | | | | | | dep | 18 | 20 | 0.04 |
| | | | | | | | | | | | | | | | | | psysom | 18 | 20 | -0.21 |
| | E | 1992 | FIN | Jour. | yes | plant clos. | other | --- | 0 | --- | 43 | 0 | --- | --- | 5 | writ. | comp | 9 | 8 | -1.02* |
| | | | | | | | | | | | | | | | | | ms | 9 | 8 | -0.34 |
| | | | | | | | | | | | | | | | | | dep | 9 | 8 | -0.98 |
| | | | | | | | | | | | | | | | | | psysom | 9 | 8 | -1.17 |
| | E | 1992 | FIN | Jour. | yes | diff other. | other | --- | --- | --- | 42 | 77 | --- | --- | --- | oral | comp | 150 | 652 | 0.32* |
| | | | | | | | | | | | | | | | | | ms | 150 | 652 | 0.17 |
| | | | | | | | | | | | | | | | | | swb | 150 | 652 | 0.37 |
| Kristensen (1991) | E | 1985 | DNK | Jour. | yes | diff other. | other | --- | --- | --- | 36 | --- | --- | --- | 26 | writ. | se | 312 | 553 | 0.28* |
| Lahelma (1989) | E | 1983 | FIN | Jour. | yes | regist. | reemp | 9 | 0 | --- | 35 | 50 | --- | 83 | 11 | writ. | ms | 268 | 94 | 1.02* |
| | E | 1983 | FIN | Jour. | yes | regist. | reemp | 9 | 100 | --- | 36 | 73 | --- | 82 | 11 | writ. | ms | 251 | 90 | 0.64* |
| Lai & Wong (1998) | E | --- | HKG | Jour. | yes | regist. | other | --- | 100 | 0 | 40 | 76 | --- | 42 | 10 | writ. | ms | 102 | 78 | 0.42* |
| Lai & Chan | E | --- | HKG | Jour. | yes | regist. | reemp | 0 | 75 | --- | 41 | 81 | 10 | --- | 41 | writ. | comp | 26 | 22 | 0.19* |

| *Study* | *Lang-uage* | *Year dat. coll.* | *Coun-try* | *Way of pub.* | *UE main topic?* | *UE-opera.* | *Re-emp?* | *Prct. part-time?* | *Prct. fem.* | *Prct. minor.* | *Age* | *Prct. mar.* | *Years educ.* | *Prct. bl. col.* | *Mean UE-dur.* | *Ques. form* | *MH-var.* | *N-ue* | *N-emp* | *d* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| (2002) | | | | | | | | | | | | | | | | | | | | |
| | | | | | | | | | | | | | | | | | ms | 26 | 22 | 0.15 |
| | | | | | | | | | | | | | | | | | swb | 26 | 22 | 0.17 |
| Lai et al. (1997) | E | --- | HKG | Jour. | yes | regist. | other | --- | 100 | --- | 40 | 75 | --- | 51 | 18 | --- | ms | 86 | 79 | 0.36* |
| | | | | | | | | | | | | | | | | | dep | 86 | 79 | 0.23 |
| | | | | | | | | | | | | | | | | | anxi | 86 | 79 | 0.32 |
| Larson (1984) | E | --- | USA | Jour. | yes | regist. | other | --- | 0 | 0 | 35 | 100 | 12 | 100 | 6 | writ. | se | 41 | 40 | 0.07* |
| Laubach et al. (1999) | G | 1994 | GER | Book | yes | diff other. | other | --- | 58 | --- | 39 | --- | --- | 54 | 21 | --- | comp | 191 | 191 | 0.32* |
| | | | | | | | | | | | | | | | | | dep | 191 | 191 | 0.43 |
| | | | | | | | | | | | | | | | | | psysom | 191 | 191 | 0.00 |
| | | | | | | | | | | | | | | | | | swb | 191 | 191 | 0.35 |
| Lawlis (1971) | E | --- | USA | Jour. | yes | regist. | other | --- | 0 | 19 | --- | --- | --- | --- | 3 | writ. | se | 75 | 75 | 0.55* |
| Layton (1986b / 1987) | E | --- | GBR | Jour. | yes | plant clos. | reemp | --- | 0 | --- | 35 | --- | --- | 100 | 6 | writ. | comp | 39 | 62 | 0.58* |
| | | | | | | | | | | | | | | | | | ms | 39 | 62 | 0.68 |
| | | | | | | | | | | | | | | | | | anxi | 39 | 62 | 0.31 |
| Layton (1986a) | E | --- | GBR | Jour. | yes | diff other. | other | --- | 0 | --- | 17 | --- | --- | --- | 6 | writ. | ms | 29 | 77 | 0.31* |
| Leana & Feldman (1995) | E | --- | USA | Jour. | yes | plant clos. | reemp | --- | 2 | 8 | 41 | 78 | --- | 100 | 9 | writ. | comp | 25 | 34 | 0.47* |
| | | | | | | | | | | | | | | | | | ms | 25 | 34 | 0.48 |
| | | | | | | | | | | | | | | | | | anxi | 25 | 34 | 0.4 |
| | | | | | | | | | | | | | | | | | psysom | 25 | 34 | 0.33 |
| | | | | | | | | | | | | | | | | | swb | 25 | 34 | 0.28 |
| Lennings (1993) | E | 1982 | AUS | Jour. | yes | regist. | other | --- | 62 | 16 | 18 | --- | 10 | --- | --- | writ. | comp | 68 | 69 | 0.79* |
| | | | | | | | | | | | | | | | | | dep | 68 | 69 | 0.26 |
| | | | | | | | | | | | | | | | | | anxi | 68 | 69 | 0.28 |
| | | | | | | | | | | | | | | | | | swb | 70 | 70 | 1.37 |
| | | | | | | | | | | | | | | | | | se | 68 | 69 | 0.55 |
| Liem & Liem (1988) | E | 1982 | USA | Jour. | yes | regist. | other | --- | 0 | --- | 38 | 100 | --- | 50 | 2 | writ. | comp | 82 | 82 | 0.36* |

| *Study* | *Lang-uage* | *Year dat. coll.* | *Coun-try* | *Way of pub.* | *UE main topic?* | *UE-opera.* | *Re-emp?* | *Prct. part-time?* | *Prct. fem.* | *Prct. minor.* | *Age* | *Prct. mar.* | *Years educ.* | *Prct. bl. col.* | *Mean UE-dur.* | *Ques. form* | *MH-var.* | *N-ue* | *N-emp* | *d* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | | | | | | | | | ms | 82 | 82 | 0.31 |
| | | | | | | | | | | | | | | | | | dep | 82 | 82 | 0.31 |
| | | | | | | | | | | | | | | | | | anxi | 82 | 82 | 0.31 |
| | | | | | | | | | | | | | | | | | swb | 82 | 82 | 0.31 |
| Liira & Leino-Arjas (1999) | E | 1994 | FIN | Jour. | yes | involun. | other | --- | 0 | --- | 40 | --- | --- | 100 | --- | writ. | comp | 427 | 71 | 0.52* |
| | | | | | | | | | | | | | | | | | dep | 433 | 74 | 0.45 |
| | | | | | | | | | | | | | | | | | psysom | 427 | 71 | 0.44 |
| | E | 1994 | FIN | Jour. | yes | involun. | other | --- | 0 | --- | 40 | --- | --- | 100 | --- | writ. | comp | 237 | 132 | 0.58* |
| | | | | | | | | | | | | | | | | | dep | 248 | 134 | 0.56 |
| | | | | | | | | | | | | | | | | | psysom | 237 | 132 | 0.45 |
| Linn et al. (1985) | E | 1979 | USA | Jour. | yes | plant clos. | other | --- | 0 | 20 | 49 | 75 | 12 | --- | 4 | writ. | comp | 30 | 30 | 0.60* |
| | | | | | | | | | | | | | | | | | dep | 30 | 30 | 0.64 |
| | | | | | | | | | | | | | | | | | anxi | 30 | 30 | 0.57 |
| | | | | | | | | | | | | | | | | | psysom | 30 | 30 | 0.61 |
| | | | | | | | | | | | | | | | | | swb | 30 | 30 | 0.23 |
| | | | | | | | | | | | | | | | | | se | 30 | 30 | 0.23 |
| Little (1973) | E | 1972 | USA | Diss. | yes | regist. | other | --- | 0 | --- | 41 | 91 | --- | 0 | 5 | --- | comp | 40 | 40 | -0.37* |
| | | | | | | | | | | | | | | | | | dep | 40 | 40 | 0.35 |
| | | | | | | | | | | | | | | | | | psysom | 40 | 40 | -0.98 |
| Lynd-Stevenson (1996) | E | --- | AUS | Jour. | yes | regist. | reemp | 12 | 40 | --- | 21 | --- | --- | 64 | --- | writ. | dep | 43 | 100 | 0.31* |
| Macky (1984) | E | 1982 | NZ | Jour. | yes | involun. | other | --- | 55 | --- | 17 | --- | --- | --- | 5 | writ. | ms | 49 | 352 | 0.97* |
| Madianos et al. (1985) | E | 1980 | GRC | Jour. | no | diff other. | other | --- | 59 | --- | 41 | 72 | --- | 36 | --- | writ. | ms | 34 | 826 | 0.55* |
| Margolis & Farran (1984) | E | --- | USA | Jour. | yes | plant clos. | other | --- | 0 | --- | 32 | 100 | --- | 100 | 3 | --- | comp | 62 | 52 | 0.24* |
| | | | | | | | | | | | | | | | | | dep | 62 | 52 | 0.37 |
| | | | | | | | | | | | | | | | | | anxi | 62 | 52 | 0.37 |
| | | | | | | | | | | | | | | | | | psysom | 62 | 52 | 0.00 |
| | | | | | | | | | | | | | | | | | se | 62 | 52 | 0.00 |
| Margraf & | G | 1994 | GER | Jour. | no | diff | other | 0 | 53 | --- | 46 | 61 | --- | --- | --- | writ. | anxi | 171 | 1185 | 0.39* |

| *Study* | *Lang-uage* | *Year dat. coll.* | *Coun-try* | *Way of pub.* | *UE main topic?* | *UE-opera.* | *Re-emp?* | *Prct. part-time?* | *Prct. fem.* | *Prct. minor.* | *Age* | *Prct. mar.* | *Years educ.* | *Prct. bl. col.* | *Mean UE-dur.* | *Ques. form* | *MH-var.* | *N-ue* | *N-emp* | *d* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Poldrack (2000) | | | | | | other. | | | | | | | | | | | | | | |
| Marsh & Alvaro (1990) | E | 1985 | GBR | Jour. | no | diff other. | other | --- | --- | --- | 40 | 65 | --- | --- | --- | oral | swb | 265 | 2044 | 1.05* |
| | E | 1985 | ESP | Jour. | yes | diff other. | other | --- | --- | --- | 40 | 63 | --- | --- | --- | oral | swb | 221 | 1206 | 0.32* |
| Martella & Maass (2000) | E | --- | ITA | Jour. | yes | regist. | other | --- | 48 | --- | 24 | --- | --- | --- | 8 | writ. | comp | 78 | 51 | 0.88* |
| | | | | | | | | | | | | | | | | | swb | 78 | 51 | 0.97 |
| | | | | | | | | | | | | | | | | | se | 85 | 52 | 0.55 |
| McCarthy & Ronayne (1984) | E | 1982 | IRL | other | yes | regist. | other | --- | 53 | --- | 19 | 0 | --- | 48 | 4 | writ. | ms | 207 | 388 | 1.05* |
| McDonald et al. (1996) | E | 1992 | AUS | Jour. | no | diff other. | other | 0 | 54 | 100 | 37 | 64 | --- | --- | --- | oral | ms | 35 | 45 | 1.13* |
| McKenna & Payne (1989) | E | 1982 | GBR | Jour. | yes | regist. | reemp | --- | 0 | 0 | 33 | 100 | --- | 100 | 21 | writ. | ms | 71 | 24 | 0.62* |
| | | | | | | | | | | | | | | | | | dep | 71 | 24 | 0.62 |
| | | | | | | | | | | | | | | | | | anxi | 71 | 24 | 0.62 |
| | E | 1982 | GBR | Jour. | yes | regist. | reemp | --- | 0 | 0 | 33 | 100 | --- | 0 | 21 | writ. | ms | 43 | 47 | 0.55* |
| | | | | | | | | | | | | | | | | | dep | 43 | 47 | 0.55 |
| | | | | | | | | | | | | | | | | | anxi | 43 | 47 | 0.55 |
| McLoyd et al. (1994) | E | 1990 | USA | Jour. | yes | regist. | other | 22 | 100 | 100 | 37 | 0 | --- | --- | 20 | writ. | dep | 152 | 89 | 0.60* |
| McPherson & Hall (1983) | E | 1977 | AUS | Jour. | yes | regist. | other | --- | 0 | --- | 18 | --- | --- | --- | --- | writ. | ms | 139 | 161 | 0.55* |
| Mean Patterson (1997) | E | --- | GBR | Jour. | yes | regist. | other | --- | 46 | --- | 17 | --- | --- | --- | 13 | writ. | comp | 173 | 54 | 0.36* |
| | | | | | | | | | | | | | | | | | ms | 173 | 54 | 0.31 |
| | | | | | | | | | | | | | | | | | se | 173 | 54 | 0.31 |
| Meeus et al. (1997) | E | --- | NL | Jour. | yes | involun. | other | --- | 57 | --- | 22 | --- | --- | --- | --- | writ. | comp | 37 | 917 | 0.53* |
| | | | | | | | | | | | | | | | | | ms | 37 | 917 | 0.38 |
| | | | | | | | | | | | | | | | | | dep | 37 | 917 | 0.50 |

| *Study* | *Lang-uage* | *Year dat. coll.* | *Coun-try* | *Way of pub.* | *UE main topic?* | *UE-opera.* | *Re-emp?* | *Prct. part-time?* | *Prct. fem.* | *Prct. minor.* | *Age* | *Prct. mar.* | *Years educ.* | *Prct. bl. col.* | *Mean UE-dur.* | *Ques. form* | *MH-var.* | *N-ue* | *N-emp* | *d* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | | | | | | | | | swb | 37 | 917 | 0.41 |
| Melville et al. (1985) | E | 1982 | GBR | Jour. | yes | regist. | other | --- | 0 | 0 | 35 | 62 | --- | --- | 9 | writ. | comp | 98 | 98 | 1.22* |
| | | | | | | | | | | | | | | | | | ms | 98 | 98 | 1.36 |
| | | | | | | | | | | | | | | | | | dep | 98 | 98 | 0.73 |
| Michelson et al. (2000) | E | --- | SWE | Jour. | no | diff other. | other | --- | 53 | --- | 51 | 68 | --- | --- | --- | writ. | ms | 163 | 1582 | 0.56* |
| Miles (1983) | E | 1982 | GBR | other | yes | regist. | other | --- | 0 | --- | 43 | --- | --- | 65 | --- | writ. | comp | 150 | 100 | 0.39* |
| | | | | | | | | | | | | | | | | | ms | 150 | 100 | 0.33 |
| | | | | | | | | | | | | | | | | | swb | 150 | 100 | 0.33 |
| Miller & Hoppe (1994) | E | 1987 | USA | Jour. | yes | regist. | other | --- | 0 | 49 | 32 | 100 | 12 | 100 | --- | oral | comp | 441 | 467 | 0.67* |
| | | | | | | | | | | | | | | | | | dep | 441 | 467 | 0.62 |
| | | | | | | | | | | | | | | | | | anxi | 441 | 467 | 0.53 |
| Miner (1991) | E | --- | AUS | Jour. | no | diff other. | other | 0 | 40 | --- | 17 | --- | --- | --- | --- | writ. | se | 30 | 30 | 0.31* |
| | | | | | | | | | | | | | | | | | ms | 30 | 30 | 0.33 |
| Mohr (1993/1997) | G | 1987 | GER | Book | yes | diff other. | reemp | --- | 100 | --- | 41 | 75 | --- | 100 | --- | writ. | comp | 15 | 33 | 0.94* |
| | | | | | | | | | | | | | | | | | dep | 15 | 33 | 1.39 |
| | | | | | | | | | | | | | | | | | psysom | 15 | 33 | 0.23 |
| Montada & Dieter (1999) | G | --- | GER | Book | no | diff other. | other | --- | 59 | --- | 47 | --- | --- | --- | --- | writ. | ms | 185 | 165 | 0.39* |
| Morch (1986) | E | 1983 | DNK | Diss. | yes | regist. | other | 0 | 46 | --- | 22 | 39 | 10 | --- | 8 | oral | comp | 151 | 74 | 0.79* |
| | | | | | | | | | | | | | | | | | dep | 151 | 74 | 0.91 |
| | | | | | | | | | | | | | | | | | se | 151 | 74 | 0.45 |
| Morrell et al. (1994) | E | 1984 | AUS | Jour. | yes | involun. | other | --- | 100 | --- | 20 | --- | --- | --- | --- | writ. | ms | 462 | 372 | 0.28* |
| | E | 1984 | AUS | Jour. | yes | involun. | other | --- | 0 | --- | 20 | --- | --- | --- | --- | writ. | ms | 523 | 772 | 0.37* |
| O'Brien & Kabanoff (1979) | E | --- | AUS | Jour. | yes | involun. | other | --- | --- | --- | --- | --- | --- | --- | --- | writ. | comp | 74 | 1383 | 0.00* |
| | | | | | | | | | | | | | | | | | ms | 74 | 1383 | 0.00 |
| | | | | | | | | | | | | | | | | | swb | 74 | 1383 | 0.00 |
| Ockenfels | G | 1992 | USA | Diss. | no | involun. | other | 0 | 71 | --- | 37 | 43 | 15 | 35 | 13 | writ. | comp | 60 | 60 | 0.51* |

| *Study* | *Lang-uage* | *Year dat. coll.* | *Coun-try* | *Way of pub.* | *UE main topic?* | *UE-opera.* | *Re-emp?* | *Prct. part-time?* | *Prct. fem.* | *Prct. minor.* | *Age* | *Prct. mar.* | *Years educ.* | *Prct. bl. col.* | *Mean UE-dur.* | *Ques. form* | *MH-var.* | *N-ue* | *N-emp* | *d* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| (1995) | | | | | | | | | | | | | | | | | | | | |
| | | | | | | | | | | | | | | | | | ms | 60 | 60 | 0.64 |
| | | | | | | | | | | | | | | | | | anxi | 60 | 60 | 0.59 |
| | | | | | | | | | | | | | | | | | swb | 60 | 60 | 0.00 |
| Ohayon et al. (1999) | E | 1994 | GBR | Jour. | no | diff other. | other | --- | --- | --- | 38 | 51 | --- | 33 | --- | oral | dep | 186 | 2775 | 0.66* |
| Palt (1986) | G | 1985 | AUT | Diss. | yes | regist. | other | --- | 50 | --- | 17 | --- | --- | 100 | 6 | writ. | comp | 30 | 30 | 0.27* |
| | | | | | | | | | | | | | | | | | ms | 30 | 30 | 0.28 |
| | | | | | | | | | | | | | | | | | dep | 30 | 30 | 0.19 |
| | | | | | | | | | | | | | | | | | swb | 30 | 30 | 0.26 |
| | | | | | | | | | | | | | | | | | se | 30 | 30 | 0.58 |
| Patton & Noller (1984) | E | 1983 | AUS | Jour. | yes | diff other. | other | --- | 0 | --- | 15 | --- | --- | --- | 5 | writ. | comp | 9 | 13 | 2.05* |
| | | | | | | | | | | | | | | | | | dep | 9 | 13 | 2.00 |
| | | | | | | | | | | | | | | | | | se | 9 | 13 | 1.53 |
| | E | 1983 | AUS | Jour. | yes | diff other. | other | --- | 100 | --- | 15 | --- | --- | --- | 5 | writ. | comp | 12 | 11 | 1.25* |
| | | | | | | | | | | | | | | | | | dep | 12 | 11 | 1.75 |
| | | | | | | | | | | | | | | | | | se | 12 | 11 | 0.40 |
| Patton & Noller (1990) | E | 1987 | AUS | Jour. | yes | diff other. | other | --- | 67 | --- | 16 | --- | --- | --- | 6 | writ. | comp | 40 | 47 | 0.78* |
| | | | | | | | | | | | | | | | | | ms | 40 | 47 | 0.78 |
| | | | | | | | | | | | | | | | | | dep | 40 | 47 | 0.45 |
| | | | | | | | | | | | | | | | | | swb | 40 | 47 | 1.21 |
| | | | | | | | | | | | | | | | | | se | 40 | 47 | 0.89 |
| Paul et al. (2004) | E | 2003 | GER | other | yes | diff other. | other | --- | 43 | --- | 37 | 71 | --- | --- | --- | writ. | dep | 27 | 266 | 0.70* |
| Pelzmann (1989) | G | 1985 | AUT | Book | yes | involun. | other | --- | 32 | --- | 32 | --- | --- | 59 | 9 | --- | dep | 85 | 112 | 0.48* |
| Penta (1980) | E | 1978 | USA | Diss. | yes | regist. | other | 0 | 44 | 0 | 34 | --- | 14 | --- | 2 | writ. | dep | 124 | 84 | 0.17* |
| Perfetti & Bingham (1983) | E | 1977 | USA | Jour. | yes | plant clos. | other | --- | 0 | --- | 49 | --- | 9 | 100 | 10 | writ. | se | 20 | 34 | 2.58* |
| Pernice & Long (1996) | E | 1990 | NZ | Jour. | yes | regist. | reemp | --- | 44 | --- | 32 | --- | --- | --- | 41 | writ. | comp | 12 | 22 | 1.30* |

| *Study* | *Lang-uage* | *Year dat. coll.* | *Coun-try* | *Way of pub.* | *UE main topic?* | *UE-opera.* | *Re-emp?* | *Prct. part-time?* | *Prct. fem.* | *Prct. minor.* | *Age* | *Prct. mar.* | *Years educ.* | *Prct. bl. col.* | *Mean UE-dur.* | *Ques. form* | *MH-var.* | *N-ue* | *N-emp* | *d* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | | | | | | | | | ms | 12 | 22 | 1.27 |
| | | | | | | | | | | | | | | | | | se | 12 | 22 | 0.96 |
| Pernice et al. (2000) | E | 1998 | NZ | Jour. | yes | involun. | other | 0 | 24 | 100 | 37 | --- | --- | --- | --- | writ. | ms | 36 | 42 | 0.43* |
| Perrucci et al. (1985) | E | 1983 | USA | Jour. | yes | plant clos. | reemp | 41 | 55 | --- | 44 | --- | 11 | 100 | 14 | writ. | dep | 232 | 95 | 0.00* |
| Pieroni. (1980) | E | 1980 | CAN | Diss. | yes | involun. | other | --- | 0 | --- | 34 | 100 | --- | 75 | 27 | writ. | comp | 35 | 23 | 0.44* |
| | | | | | | | | | | | | | | | | | anxi | 35 | 23 | 0.44 |
| | | | | | | | | | | | | | | | | | swb | 35 | 23 | 0.46 |
| | | | | | | | | | | | | | | | | | se | 35 | 23 | 0.18 |
| Platt et al. (1990) | E | 1988 | GBR | Book | yes | diff other. | other | --- | 100 | --- | --- | 67 | --- | 70 | --- | writ. | ms | 26 | 219 | 0.65* |
| Prause & Dooley (2001); Dooley et al. (2000) | E | 1992 | USA | Jour. | yes | diff other. | other | --- | 45 | 17 | 31 | 60 | 14 | --- | --- | --- | dep | 521 | 5113 | 0.61* |
| Radloff (1975) | E | 1971 | USA | Jour. | yes | involun. | other | --- | 100 | 0 | 42 | 100 | --- | --- | --- | oral | dep | 77 | 381 | 0.04* |
| | E | 1971 | USA | Jour. | yes | involun. | other | --- | 0 | 0 | 42 | 100 | --- | --- | --- | oral | dep | 24 | 644 | 0.76* |
| Roberts et al. (1997) | E | 1994 | GBR | Jour. | yes | diff other. | other | 24 | 47 | --- | 39 | --- | --- | 41 | --- | writ. | ms | 689 | 6298 | 0.42* |
| Roberts et al. (1981) | E | 1974 | USA | Jour. | no | diff other. | other | --- | 57 | 100 | 40 | 53 | --- | --- | --- | writ. | dep | 33 | 165 | 0.88* |
| | E | 1974 | USA | Jour. | no | diff other. | other | --- | 55 | 0 | 39 | 70 | --- | --- | --- | writ. | dep | 67 | 1104 | 0.74* |
| Rodgers (1991) | E | 1982 | GBR | Jour. | yes | involun. | other | 0 | 0 | --- | 36 | --- | --- | 43 | 15 | oral | ms | 75 | 1525 | 0.92* |
| | E | 1982 | GBR | Jour. | yes | involun. | other | 0 | 100 | --- | 36 | 89 | --- | 32 | --- | oral | ms | 55 | 454 | 0.34* |
| Rodriguez et al. (1999) | E | 1992 | USA | Jour. | yes | involun. | other | 0 | 100 | 100 | 44 | --- | --- | --- | --- | oral | dep | 16 | 437 | 0.37* |
| | E | 1992 | USA | Jour. | yes | involun. | other | 0 | 100 | 0 | 44 | --- | --- | --- | --- | oral | dep | 45 | 1686 | 0.54* |
| | E | 1992 | USA | Jour. | yes | involun. | other | 0 | 0 | 0 | 44 | --- | --- | --- | --- | oral | dep | 47 | 1882 | 0.38* |
| Rothwell & Williams (1983) | E | --- | GBR | Jour. | no | plant clos. | other | --- | 0 | --- | 35 | --- | --- | 100 | 6 | writ. | comp | 20 | 20 | 0.25* |
| | | | | | | | | | | | | | | | | | dep | 20 | 20 | 0.27 |

| *Study* | *Lang-uage* | *Year dat. coll.* | *Coun-try* | *Way of pub.* | *UE main topic?* | *UE-opera.* | *Re-emp?* | *Prct. part-time?* | *Prct. fem.* | *Prct. minor.* | *Age* | *Prct. mar.* | *Years educ.* | *Prct. bl. col.* | *Mean UE-dur.* | *Ques. form* | *MH-var.* | *N-ue* | *N-emp* | *d* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | | | | | | | | | se | 20 | 20 | 0.17 |
| Rowlands & Huws (1995) | E | --- | GBR | Jour. | yes | plant clos. | other | --- | 0 | --- | 36 | 74 | --- | 100 | --- | writ. | ms | 93 | 91 | 0.73* |
| Rudin (1986) | E | 1985 | USA | Diss. | yes | involun. | reemp | 0 | 0 | 6 | 51 | 82 | --- | 0 | 6 | writ. | ms | 92 | 48 | 1.21* |
| Saurel-Cubizolles et al. (2000) | E | --- | FRA | Jour. | yes | involun. | other | --- | 100 | --- | 27 | 95 | --- | 100 | --- | writ. | ms | 47 | 93 | 0.41* |
| | E | --- | FRA | Jour. | yes | involun. | other | --- | 100 | --- | 28 | 96 | --- | 0 | --- | writ. | ms | 28 | 130 | 0.36* |
| | E | --- | FRA | Jour. | yes | involun. | other | --- | 100 | --- | 28 | 96 | --- | 0 | --- | writ. | ms | 18 | 201 | 0.73* |
| Schaufeli & VanYperen (1992) | E | 1986 | NL | Jour. | yes | diff other. | other | --- | 0 | --- | 24 | --- | --- | 0 | --- | writ. | ms | 91 | 113 | 0.02* |
| | E | 1986 | NL | Jour. | yes | diff other. | other | --- | 100 | --- | 24 | --- | --- | 0 | --- | writ. | ms | 101 | 70 | -0.12* |
| | E | 1988 | NL | Jour. | yes | regist. | reemp | --- | 0 | --- | 31 | --- | --- | 0 | --- | writ. | ms | 32 | 36 | -0.06* |
| | E | 1988 | NL | Jour. | yes | regist. | reemp | --- | 100 | --- | 31 | --- | --- | 0 | --- | writ. | ms | 53 | 48 | 0.51* |
| Schmid (2004) | G | 2003 | GER | other | yes | regist. | other | --- | 38 | 9 | 37 | 76 | 13 | --- | --- | writ. | comp | 100 | 102 | 0.95* |
| | | | | | | | | | | | | | | | | | ms | 100 | 102 | 0.79 |
| | | | | | | | | | | | | | | | | | swb | 100 | 102 | 0.85 |
| Schumacher (1988) | G | 1984 | GER | Book | yes | regist. | other | --- | 0 | --- | 23 | --- | --- | --- | 10 | writ. | comp | 40 | 40 | 0.04* |
| | | | | | | | | | | | | | | | | | dep | 40 | 40 | 0.26 |
| | | | | | | | | | | | | | | | | | psysom | 40 | 40 | -0.20 |
| Semmelmann & Fock (1989) | G | 1988 | GER | other | yes | regist. | other | --- | 50 | --- | 34 | 51 | --- | --- | 9 | writ. | se | 92 | 93 | 0.38* |
| Sethi et al. (1974) | E | 1969 | IND | Jour. | no | diff other. | other | --- | --- | --- | 31 | 65 | --- | 15 | --- | oral | ms | 191 | 1337 | 0.57* |
| Shamir (1986a) | E | 1982 | ISR | Jour. | yes | regist. | reemp | --- | 100 | --- | 34 | 68 | --- | 0 | 6 | writ. | comp | 92 | 148 | 0.24* |
| | | | | | | | | | | | | | | | | | dep | 92 | 148 | 0.26 |
| | | | | | | | | | | | | | | | | | anxi | 92 | 148 | 0.43 |
| | | | | | | | | | | | | | | | | | swb | 92 | 148 | 0.23 |
| | | | | | | | | | | | | | | | | | se | 92 | 148 | -0.17 |
| | E | 1982 | ISR | Jour. | yes | regist. | reemp | --- | 0 | --- | 34 | 68 | --- | 0 | 6 | writ. | comp | 78 | 113 | 0.82* |

| *Study* | *Lang-uage* | *Year dat. coll.* | *Coun-try* | *Way of pub.* | *UE main topic?* | *UE-opera.* | *Re-emp?* | *Prct. part-time?* | *Prct. fem.* | *Prct. minor.* | *Age* | *Prct. mar.* | *Years educ.* | *Prct. bl. col.* | *Mean UE-dur.* | *Ques. form* | *MH-var.* | *N-ue* | *N-emp* | *d* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | | | | | | | | | | dep | 78 | 113 | 0.71 |
| | | | | | | | | | | | | | | | | | anxi | 78 | 113 | 0.91 |
| | | | | | | | | | | | | | | | | | swb | 78 | 113 | 0.78 |
| | | | | | | | | | | | | | | | | | se | 78 | 113 | 0.15 |
| Shams & Jackson (1994) | E | 1987 | GBR | Jour. | yes | regist. | other | --- | 0 | 100 | 37 | 72 | --- | 100 | 42 | writ. | comp | 71 | 68 | 0.73* |
| | | | | | | | | | | | | | | | | | ms | 71 | 68 | 0.84 |
| | | | | | | | | | | | | | | | | | dep | 71 | 68 | 0.48 |
| | | | | | | | | | | | | | | | | | anxi | 71 | 68 | 0.44 |
| Sheeran & McCarthy (1990) | E | --- | IRL | Jour. | yes | regist. | other | 0 | 0 | --- | 25 | --- | --- | 100 | 4 | writ. | se | 21 | 24 | 0.44* |
| | E | --- | IRL | Jour. | yes | regist. | other | 0 | 100 | --- | 25 | --- | --- | 100 | 4 | writ. | se | 19 | 24 | 0.65* |
| Shoemaker (1993) | E | 1988 | USA | other | yes | diff other. | other | 0 | 100 | 23 | 39 | 58 | 14 | --- | --- | oral | dep | 33 | 69 | 0.32* |
| Singh et al. (1996) | E | 1992 | IND | Jour. | yes | regist. | other | --- | 0 | --- | 30 | --- | --- | 0 | --- | writ. | se | 100 | 100 | 2.79* |
| | E | 1992 | IND | Jour. | yes | regist. | other | --- | 0 | --- | 30 | --- | --- | 0 | --- | writ. | se | 100 | 100 | 2.83* |
| Sjöberg & Magneberg (1990) | E | --- | SWE | Jour. | no | regist. | other | 50 | 48 | --- | 30 | --- | --- | --- | --- | writ. | swb | 20 | 20 | 0.31* |
| Snyder & Nowak (1984) | E | 1982 | USA | Jour. | yes | plant clos. | reemp | 38 | 100 | --- | --- | 35 | --- | 100 | 18 | --- | ms | 63 | 52 | -0.08* |
| | E | 1982 | USA | Jour. | yes | plant clos. | reemp | 15 | 0 | --- | --- | --- | --- | 100 | 18 | --- | ms | 18 | 15 | 0.75* |
| Spruit (1989) | E | 1983 | NL | Book | yes | regist. | other | --- | 33 | --- | 40 | --- | --- | 33 | 19 | writ. | comp | 749 | 745 | 0.35* |
| | | | | | | | | | | | | | | | | | dep | 749 | 745 | 0.17 |
| | | | | | | | | | | | | | | | | | psysom | 749 | 745 | 0.08 |
| | | | | | | | | | | | | | | | | | swb | 749 | 745 | 0.61 |
| Stansfield et al. (1991) | E | --- | GBR | Jour. | no | diff other. | other | --- | 0 | --- | 56 | 88 | --- | 66 | --- | writ. | ms | 331 | 1038 | 0.46* |
| Stevens (1991) | E | --- | USA | Diss. | yes | involun. | reemp | --- | 46 | 3 | 36 | 77 | --- | 0 | 11 | writ. | se | 54 | 44 | -0.02* |
| Studnicka et al. (1991) | E | 1986 | AUT | Jour. | yes | regist. | reemp | --- | 18 | --- | 24 | --- | --- | 77 | 11 | oral | dep | 8 | 42 | 0.56* |

| *Study* | *Lang-uage* | *Year dat. coll.* | *Coun-try* | *Way of pub.* | *UE main topic?* | *UE-opera.* | *Re-emp?* | *Prct. part-time?* | *Prct. fem.* | *Prct. minor.* | *Age* | *Prct. mar.* | *Years educ.* | *Prct. bl. col.* | *Mean UE-dur.* | *Ques. form* | *MH-var.* | *N-ue* | *N-emp* | *d* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | E | 1986 | AUT | Jour. | yes | regist. | reemp | --- | 18 | --- | 38 | --- | --- | 77 | 11 | oral | dep | 16 | 34 | 1.02* |
| | E | 1986 | AUT | Jour. | yes | regist. | reemp | --- | 18 | --- | 56 | --- | --- | 77 | 11 | oral | dep | 42 | 30 | 1.49* |
| Tiffany et al. (1970) | E | --- | USA | Book | yes | regist. | reemp | --- | --- | --- | --- | --- | --- | --- | --- | writ. | se | 31 | 23 | 0.54* |
| Tiggeman & Winefield (1984) | E | 1981 | AUS | Jour. | yes | diff other. | other | --- | 100 | --- | 17 | --- | 11 | --- | 6 | writ. | comp | 86 | 307 | 0.43* |
| | | | | | | | | | | | | | | | | | dep | 86 | 307 | 0.24 |
| | | | | | | | | | | | | | | | | | swb | 86 | 307 | 0.55 |
| | | | | | | | | | | | | | | | | | se | 86 | 307 | 0.25 |
| | E | 1981 | AUS | Jour. | yes | diff other. | other | --- | 0 | --- | 17 | --- | 11 | --- | 6 | writ. | comp | 58 | 310 | 0.48* |
| | | | | | | | | | | | | | | | | | dep | 58 | 310 | 0.52 |
| | | | | | | | | | | | | | | | | | swb | 58 | 310 | 0.28 |
| | | | | | | | | | | | | | | | | | se | 58 | 310 | 0.36 |
| Tiggemann & Winefield (1980) | E | 1979 | AUS | Jour. | yes | regist. | other | 6 | 39 | --- | 16 | --- | 11 | --- | 4 | writ. | comp | 26 | 54 | 0.39* |
| | | | | | | | | | | | | | | | | | dep | 26 | 54 | 0.00 |
| | | | | | | | | | | | | | | | | | swb | 26 | 54 | 0.76 |
| | | | | | | | | | | | | | | | | | se | 26 | 54 | 0.20 |
| Toppen (1971) | E | --- | USA | Jour. | yes | regist. | other | --- | 0 | 39 | 41 | --- | 9 | --- | --- | oral | ms | 50 | 50 | 0.83* |
| Vagt & Stavemann (1980) | G | 1978 | GER | Jour. | yes | regist. | other | 0 | 48 | --- | 42 | --- | --- | --- | --- | writ. | comp | 509 | 60 | 0.52* |
| | | | | | | | | | | | | | | | | | dep | 509 | 60 | 0.74 |
| | | | | | | | | | | | | | | | | | psysom | 509 | 60 | 0.16 |
| Vega et al. (1987) | E | 1985 | MEX | Jour. | no | diff other. | other | 0 | 76 | --- | 36 | 68 | --- | --- | --- | oral | dep | 42 | 348 | 1.23* |
| Verhaegen et al. (1994) | E | 1986 | BEL | Jour. | yes | regist. | other | --- | 0 | --- | 19 | --- | --- | --- | --- | writ. | dep | 129 | 165 | 0.45* |
| | E | 1986 | BEL | Jour. | yes | regist. | other | --- | 100 | --- | 19 | --- | --- | --- | --- | writ. | dep | 171 | 133 | 0.30* |
| Viinamäki et al. (2000) | E | 1993 | FIN | Jour. | no | diff other. | other | --- | 0 | --- | 44 | 69 | --- | --- | --- | writ. | ms | 115 | 442 | 0.58* |
| | E | 1994 | FIN | Jour. | no | diff | other | --- | 0 | --- | 44 | 70 | --- | --- | --- | writ. | ms | 131 | 100 | 0.22* |

| *Study* | *Lang-uage* | *Year dat. coll.* | *Coun-try* | *Way of pub.* | *UE main topic?* | *UE-opera.* | *Re-emp?* | *Prct. part-time?* | *Prct. fem.* | *Prct. minor.* | *Age* | *Prct. mar.* | *Years educ.* | *Prct. bl. col.* | *Mean UE-dur.* | *Ques. form* | *MH-var.* | *N-ue* | *N-emp* | *d* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | E | 1995 | FIN | Jour. | no | other. diff | other | --- | 0 | --- | 44 | 67 | --- | --- | --- | writ. | ms | 72 | 387 | 0.17* |
| | E | 1993 | FIN | Jour. | no | other. diff | other | --- | 100 | --- | 44 | 67 | --- | --- | --- | writ. | ms | 86 | 395 | 0.50* |
| | E | 1994 | FIN | Jour. | no | other. diff | other | --- | 100 | --- | 44 | 66 | --- | --- | --- | writ. | ms | 109 | 511 | 0.34* |
| | E | 1995 | FIN | Jour. | no | other. diff | other | --- | 100 | --- | 44 | 68 | --- | --- | --- | writ. | ms | 84 | 426 | 0.27* |
| Vinokur et al. (1987) | E | 1982 | USA | Jour. | yes | other. regist. | other | --- | 0 | 13 | 31 | 68 | 13 | --- | 1 | oral | ms | 297 | 189 | 0.18* |
| Vinokur et al. (2000) | E | 1993 | USA | Jour. | yes | regist. | reemp | --- | 55 | 19 | 39 | 45 | --- | --- | 25 | writ. | dep | 343 | 1087 | 0.35* |
| Vuori & Vesalainen (1999) | E | 1994 | FIN | Jour. | yes | regist. | reemp | --- | 67 | --- | 37 | 69 | --- | --- | 19 | writ. | ms | 290 | 87 | 0.46* |
| Wacker & Kolobkova (2000) | G | 1997 | GER | Jour. | yes | regist. | other | --- | 45 | 41 | 35 | --- | --- | --- | 34 | oral | se | 48 | 49 | 1.66* |
| Walk (1989) | G | 1987 | AUT | Diss. | yes | regist. | other | --- | 0 | --- | 37 | --- | --- | 77 | --- | writ. | dep | 78 | 82 | 0.00* |
| Wanberg (1995) | E | --- | USA | Jour. | yes | regist. | reemp | --- | 40 | 8 | 36 | 50 | 13 | --- | 10 | writ. | comp | 28 | 95 | 0.44* |
| | | | | | | | | | | | | | | | | | ms | 28 | 95 | 0.43 |
| | | | | | | | | | | | | | | | | | swb | 28 | 95 | 0.33 |
| Wanberg (1997) | E | 1995 | USA | Jour. | yes | regist. | reemp | --- | 61 | 18 | 38 | 53 | 13 | 22 | 5 | writ. | ms | 186 | 177 | 0.41* |
| Wanberg et al. (1997) | E | 1993 | USA | Jour. | yes | regist. | reemp | --- | 34 | 11 | 41 | 71 | 13 | --- | 2 | writ. | ms | 202 | 37 | 0.05* |
| Warr & Jackson (1983) | E | 1981 | GBR | Jour. | yes | diff other. | other | --- | 100 | --- | 19 | --- | --- | --- | --- | writ. | se | 52 | 133 | 0.51* |
| | E | 1981 | GBR | Jour. | yes | diff other. | other | --- | 0 | --- | 19 | --- | --- | --- | --- | writ. | se | 36 | 183 | 0.74* |
| | E | 1981 | GBR | Jour. | yes | diff other. | other | --- | 100 | --- | 18 | --- | --- | --- | --- | writ. | se | 44 | 181 | 0.35* |
| | E | 1981 | GBR | Jour. | yes | diff | other | --- | 0 | --- | 18 | --- | --- | --- | --- | writ. | se | 51 | 190 | 0.48* |

| *Study* | *Lang-uage* | *Year dat. coll.* | *Coun-try* | *Way of pub.* | *UE main topic?* | *UE-opera.* | *Re-emp?* | *Prct. part-time?* | *Prct. fem.* | *Prct. minor.* | *Age* | *Prct. mar.* | *Years educ.* | *Prct. bl. col.* | *Mean UE-dur.* | *Ques. form* | *MH-var.* | *N-ue* | *N-emp* | *d* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Warr & Jackson (1985); Jackson & Warr (1984) | E | 1982 | GBR | Jour. | yes | other. regist. | reemp | --- | 0 | --- | 36 | 61 | --- | 100 | 15 | writ. | ms | 467 | 162 | 1.02* |
| Warr (1978) | E | --- | GBR | Jour. | no | involun. | reemp | --- | 3 | --- | 45 | --- | --- | 88 | 6 | oral | swb | 245 | 891 | 0.60* |
| Warr & Payne (1982) | E | --- | GBR | Jour. | no | diff other. | other | 0 | 0 | --- | 39 | --- | --- | --- | --- | --- | swb | 183 | 1162 | 0.36* |
| | E | --- | GBR | Jour. | no | diff other. | other | 0 | 100 | --- | 39 | --- | --- | --- | --- | --- | swb | 71 | 181 | 0.20* |
| Waters & Moore (2002a) | E | 1997 | AUS | Jour. | yes | regist. | other | 0 | 53 | --- | 33 | --- | --- | 60 | --- | writ. | comp | 201 | 128 | 1.10* |
| | | | | | | | | | | | | | | | | | dep | 201 | 128 | 0.86 |
| | | | | | | | | | | | | | | | | | se | 201 | 128 | 1.03 |
| Weiss (1988) | E | 1988 | USA | Diss. | yes | regist. | reemp | --- | 37 | 15 | 38 | 48 | --- | 25 | --- | writ. | ms | 29 | 90 | 0.76* |
| Westcott (1985) | E | --- | GBR | Book | yes | diff other. | other | 25 | 0 | --- | --- | --- | --- | --- | --- | oral | ms | 22 | 46 | 0.22* |
| Westman | E | 2001 | ISR | Jour. | yes | regist. | other | --- | 50 | 20 | 37 | 100 | --- | 0 | 0 | writ. | anxi | 113 | 113 | 0.29* |
| Weyerer & Dilling (1987) | E | --- | GER | Book | yes | diff other. | other | --- | 100 | --- | 38 | 56 | --- | 52 | --- | oral | ms | 9 | 296 | 1.55* |
| | E | --- | GER | Book | yes | diff other. | other | --- | 0 | --- | 38 | 79 | --- | 52 | --- | oral | ms | 16 | 434 | 1.30* |
| Whelan (1992) | E | 1987 | IRL | Jour. | yes | diff other. | other | --- | 51 | --- | 40 | 64 | --- | --- | --- | oral | ms | 697 | 3138 | 0.88* |
| WHO International Consortium | E | 1990 | CAN | Jour. | no | diff other. | other | --- | 51 | --- | 35 | 70 | 12 | --- | --- | oral | ms | 451 | 4564 | 0.60* |
| | E | 1995 | GER | Jour. | no | diff other. | other | --- | 51 | --- | 21 | 5 | 10 | --- | --- | oral | ms | 163 | 519 | 0.39* |
| | E | 1995 | MEX | Jour. | no | diff other. | other | --- | 57 | --- | 32 | 57 | 9 | --- | --- | oral | ms | 151 | 1009 | 0.32* |
| | E | 1996 | NL | Jour. | no | diff other. | other | --- | 49 | --- | 40 | 69 | 10 | --- | --- | oral | ms | 948 | 3602 | 0.56* |
| | E | --- | TUR | Jour. | no | diff | other | --- | 56 | --- | 34 | 81 | 7 | --- | --- | oral | ms | 390 | 3127 | 0.20* |

| *Study* | *Lang-uage* | *Year dat. coll.* | *Coun-try* | *Way of pub.* | *UE main topic?* | *UE-opera.* | *Re-emp?* | *Prct. part-time?* | *Prct. fem.* | *Prct. minor.* | *Age* | *Prct. mar.* | *Years educ.* | *Prct. bl. col.* | *Mean UE-dur.* | *Ques. form* | *MH-var.* | *N-ue* | *N-emp* | *d* |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | E | 1991 | USA | Jour. | no | other. diff other. | other | --- | 50 | --- | 35 | 59 | 13 | --- | --- | oral | ms | 431 | 4413 | 0.72* |
| Wiener et al. (1999) | E | --- | AUS | Jour. | yes | regist. | other | --- | 47 | --- | 34 | --- | --- | --- | 24 | writ. | ms | 110 | 120 | 0.32* |
| Wilhelm-Reiss (1980) | G | 1977 | GER | Diss. | yes | regist. | other | --- | 58 | --- | 16 | --- | --- | --- | 8 | writ. | comp | 152 | 100 | 0.41* |
| | | | | | | | | | | | | | | | | | dep | 152 | 100 | 0.34 |
| | | | | | | | | | | | | | | | | | psysom | 152 | 100 | 0.36 |
| Winkelmann & Winkelmann (1998) | E | 1985 | GER | Jour. | yes | regist. | other | --- | --- | --- | 42 | --- | --- | --- | --- | --- | swb | 233 | 3043 | 1.12* |
| Winter (1982) | G | 1977 | GER | Book | yes | regist. | other | --- | 50 | --- | 17 | --- | --- | --- | 2 | writ. | se | 28 | 58 | 0.59* |
| Wolf (2004) | G | 2003 | GER | other | yes | regist. | other | 12 | 48 | 2 | 36 | 76 | --- | 25 | --- | writ. | comp | 121 | 135 | 1.23* |
| | | | | | | | | | | | | | | | | | ms | 121 | 135 | 0.69 |
| | | | | | | | | | | | | | | | | | swb | 121 | 135 | 1.43 |
| Wuggenig (1985) | G | 1982 | GER | Book | yes | diff other. | other | --- | --- | --- | 22 | --- | --- | 100 | 6 | writ. | comp | 47 | 85 | 0.44* |
| | | | | | | | | | | | | | | | | | dep | 47 | 85 | 0.35 |
| | | | | | | | | | | | | | | | | | anxi | 47 | 85 | 0.10 |
| | | | | | | | | | | | | | | | | | swb | 47 | 85 | 0.47 |
| | | | | | | | | | | | | | | | | | se | 47 | 85 | 0.45 |
| | G | 1982 | GER | Book | yes | diff other. | other | --- | --- | --- | 30 | --- | --- | 0 | 6 | writ. | comp | 27 | 144 | 0.33* |
| | | | | | | | | | | | | | | | | | dep | 27 | 144 | 0.24 |
| | | | | | | | | | | | | | | | | | anxi | 27 | 144 | -0.01 |
| | | | | | | | | | | | | | | | | | swb | 27 | 144 | 0.23 |
| | | | | | | | | | | | | | | | | | se | 27 | 144 | 0.56 |

*Note.* Language = language of publication (German or English); Year dat. coll. = year of data collection; Country = country were study was conducted; way of pub. = way of publication (Journal, book, dissertation or other); UE main topic? = unemployment main topic of the publication? (yes or no); UE opera. = Operationalization of unemployment; (officially registered as unemployed, involuntarity of unemployment explicitly stated, sufficient differentiation from other groups of non-employed persons, lost job due to plant closing or mass lay-off); re-emp? = comparison group consisting of formerly unemployed persons? (yes or other); Prct. part-time = percentage of part-time employed persons in the comparison group; Prct. fem. = percentage of females in the sample; Prct. minor. = percentage of minority members in the sample; Age = mean age in years; Prct. mar. = percentage of married (or cohabiting) persons in the sample; Years educ. = average years of formal education; Prct. bl. col. = percentage of blue-collar workers in the sample; Mean UE-dur. = mean unemployment duration in months; Ques. form = Questioning format (written or oral); MH-var. = Mental health variable (mixed symptoms, depression, anxiety, psychosomatic symptoms, subjective well-being, self-esteem, or composite of the other measures of mental health); N-ue =

sample size of unemployed group; N-emp = sample size of employed comparison group; d = effect size statistic; Effect sizes with an asterisk were used in the overall-analysis; [a] effect size excluded from overall-analysis due to considerable reduction in sample size in comparison to other mental health indicators; [b] only ms used for overall analysis all other measures were only subscales of the ms-measure; [c] same sample as in Brenner et al. (1989), therefore not used in overall analysis.

# Karsten Ingmar Paul: Lebenslauf

| | |
|---|---|
| Geburtsdatum/-ort: | 05.03.1970, Kaufbeuren |
| Staatsangehörigkeit: | deutsch |
| Familienstand: | ledig |

**Berufliche Tätigkeit**

| | |
|---|---|
| 2001 - 2005 | Wissenschaftlicher Mitarbeiter am Lehrstuhl für Psychologie, Insb. Wirtschafts- und Sozialpsychologie Prof. Dr. Moser, Friedrich-Alexander-Universität Erlangen-Nürnberg |

**Ausbildung**

| | |
|---|---|
| 2001 - 2005 | Promotion zum Dr. rer. pol. an der Wirtschafts- und Sozialwissenschaftlichen Fakultät der Friedrich-Alexander-Universität Erlangen-Nürnberg |
| 1997 - 2000 | Ausbildung in klientenzentrierter Gesprächsführung bei der Gesellschaft für wissenschaftliche Gesprächspsychotherapie (GwG) |
| 1992 - 2000 | Studium der Psychologie an der Justus-Liebig-Universität Gießen; Nebenfach: Medizin |
| 1989 - 1991 | Studium der Germanistik an der Ludwig-Maximilian-Universität München; Nebenfächer: Theaterwissenschaft, Psycholinguistik |
| 1980 - 1989 | Besuch des Allgäu-Gymnasiums Kempten |
| 1976 - 1980 | Besuch der Grundschule Wiggensbach |