Ph.D. Thesis

Essays on Subjective Well-Being and Human Capital

Thesis submitted for the Degree of Doctor of Philosophy

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Declaration of work

I declare that this thesis was composed by myself, and the work contained herein is my own, except explicitly stated otherwise. Parts of the second chapter are based on research that was conducted in collaboration with Alexander M. Danzer, who contributed in particular to sections 3.6 and 3.7.

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Abstract

This thesis in applied microeconomics consists of three chapters that are devoted to specific questions in the fields of subjective well-being (life and job satisfaction) and human capital formation. All three chapters aim at deepening our understanding of the determinants of human well-being by investigating causal medium and long-term effects of unexpected and exogenous political, institutional or environmental changes.

The first chapter analyses the question whether different levels of job satisfaction between public and private sector workers represent public sector rents or merely a spurious correlation due to workers' self-selection across sectors. The novel identification strategy exploits the massive post-Soviet privatization process in Ukraine as a quasi-experiment based on which each individual can be assigned an ex ante, exogenous privatization probability. The results point to a causal public sector satisfaction premium which even slightly increases after correcting for self-selection.

Chapter 2 is devoted to the assessment of the long-run effects of the Chernobyl catastrophe in 1986 on subjective well-being in post-Soviet Ukraine. The analysis is based on a nationally representative Ukrainian data set and reveals that even 20 years after the accident subjective well-being is negatively associated with self-reported assessments of being affected by the catastrophe. The causal long-term effect of the disaster on life satisfaction is being tested and supported by exploiting official radiation measures which are linked to survey respondents through information on their place of living in 1986.

The final chapter turns to the question whether long-term cognitive child outcomes are affected by the duration of parental leave, i.e. by the time mothers spend at home after giving birth and before returning to work. Employing a difference-in-difference approach, this chapter evaluates an unexpected reform in Austria which extended the maximum duration of paid and job protected parental leave from 12 to 24 months for children born on July 1, 1990 or after. While the results based on the pooled sample suggest no significant overall impact of extended maternal leave taking on standardized PISA test scores of children aged 15, the subgroup of boys of highly educated mothers seem to have benefited.

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1 Introduction

This thesis in applied microeconomics consists of three chapters that are devoted to the empirical analysis of specific questions in the fields of subjective well-being (life and job satisfaction) and human capital formation. All three chapters aim at deepening our understanding of the determinants of human well-being in the broadest sense by investigating causal medium and long-term effects of unexpected and exogenous *institutional* (Chapter 2), *environmental* (Chapter 3) or *policy* changes (Chapter 4). In particular, the objective of this thesis is to answer the following research questions:

(1) Do observed job satisfaction differences between public and private sector workers represent *rents* – i.e., genuine differences in objective job and workplace characteristics – or are they an artificial result caused by non-random self-selection of workers based on unobservable personality traits?

(2) Does a large nuclear catastrophe like the one of Chernobyl in 1986 affect subjective well-being and mental health in the long run (i.e. more than 17 years after the disaster)? If so, how big is the corresponding utility loss in monetary terms (compensating differential)?

(3) Does prolonged paid and job protected parental leave (in combination with reduced maternal employment) up to a child's second birthday affect its cognitive skills in the medium run?

All chapters of this thesis take advantage of various 'natural' experiments in order to identify causal effects. Empirical research which cannot directly randomize the assignment into treatment ex ante faces the challenge of potential self-selection of individuals into treatment and control group. Not accounting for this endogenous allocation will bias the estimates. One solution to this empirical challenge is to exploit unexpected institutional, environmental or policy changes which induce an assignment into treatment and control group that is 'as good as random'. The virtue of using quasi-experimental research designs is that they produce empirical results which are characterized by a high degree of internal validity. However, this benefit comes at the costs of a usually limited external validity: due to the uniqueness and the particularity of such quasi-experiments (e.g., affecting only a subgroup of the population at a specific point in time and place) it is by and large problematic to generalise the findings to other institutional or geographic environments or other time periods. Yet, it should be kept in mind that even the 'gold standard' of empirical research—randomised experiments—is always bound to time and place.

The empirical analysis in Chapter 2 addresses a long-standing question in labour economics, namely whether public sector workers receive rents that give them a net advantage in the form of higher levels of job satisfaction compared to their colleagues in the private sector. Answering this question has potentially profound implications for the incentive structure and design of the public sector labour market (e.g., wage setting policies). However, the empirical analysis is generally non-trivial since the observed allocation of individuals across sectors is typically a result of choices and thus non-random. This self-selection of certain types of workers (e.g. inherently happy or unhappy persons) into particular jobs could lead to a spurious correlation of sector affiliation and job satisfaction. The main contribution of the job satisfaction analysis in Chapter 2 is to solve the problem of self-selection into public and private sector jobs by using a natural experiment which induced a random allocation of workers across sectors. More specifically, the identification strategy rests on an instrumental variable approach which exploits the unique large-scale privatization process that emerged after the break-down of the Soviet Union and created a private sector. The empirical results suggest a significantly positive public sector satisfaction premium in Post-Soviet Ukraine which holds even after correcting for worker self-selection. This finding implies that public sector workers indeed seem to enjoy rents (i.e. comparatively better jobs). Furthermore, the public-private job satisfaction gap can be partly explained by sector differences in job and workplace characteristics, and in particular by more generous fringe benefits in the public sector.

Chapter 3 is devoted to the assessment of the long-term subjective well-being and mental health toll of a major nuclear accident – the Chernobyl disaster of April 26, 1986. While the previous research interest was mainly directed towards possible adverse implications for somatic health outcomes, primarily cancer, psychologists started to explore possible effects on mental health. Health and predominantly mental diseases are major determinants of life satisfaction – even more important than income (Layard 2005). Hence, in order to grasp the full dimension of the long-term effects of the

Chernobyl catastrophe it is crucial to add to the existing evidence an evaluation of the disaster's causal implications for subjective well-being and mental health. The main contributions of Chapter 3 are to provide such an empirical assessment based on two large and nationally representative Ukrainian datasets from the years 2003 to 2007 and to compute the induced loss in life satisfaction in monetary terms.

In order to identify the causal effect of the 1986 nuclear catastrophe, the empirical analysis takes advantage of the fact that the unique survey data from Ukraine allow to link 1986 official radiation doses to survey participants according to their places of residence in the year of the disaster. The results based on these regional radiation doses as well as on individual self-reported measures of affectedness indicate that the Chernobyl disaster has a significantly negative effect on subjective well-being and adverse implications for mental health more than 17 years after the event. There is furthermore suggestive evidence that individuals exposed to higher levels of radiation seem to engage in some precautionary health behaviour and appear to be more dependent on state transfers. Expressed in monetary terms, the estimated amount of income required to compensate for the experienced utility loss amounts to a tremendous annual cost of about ten percent of Ukraine's GDP.

Chapter 4 sheds light on potential second order effects of parental leave policies on child outcomes. In many countries parental leave entitlements (including job protection and/or income compensation) belong to the main policy instruments aimed at helping young families reconcile working life with family life. However, up to now, the possible advantages and disadvantages of such policies for female labour market as well as family and child outcomes are still disputed. The main contribution of Chapter 4 is to add a further piece of evidence to the few causal empirical assessments of the effect of prolonged parental leave on medium-term child outcomes. To this end, the chapter presents estimations of the net reduced form or intention-to-treat effect of a twelve months expansion of paid and job-protected parental leave which came into effect in Austria on July 1, 1990. The analysed child outcomes are cognitive skills of 15 year old children – measured by standardized PISA test scores (Programme for International Student Assessment). The main empirical strategy is based on a Difference-in-Differences (DID) design which exploits the variation in the duration of parental leave created by the specific cut-off date of the reform. The estimated results of this assessment suggest that the overall effect of the parental leave extension on test scores is close to zero and not statistically significant. However, separate regressions by subgroups (educational level of mother; gender of child) reveal potentially important heterogeneity in the reform impact: the main DID estimates indicate a strong and statistically significant positive reform effect for children (especially for boys) of highly educated mothers. This finding appears to be robust to most – but not to all – sensitivity checks and thus has to be treated cautiously.

2 Job Satisfaction and Self-Selection into the Public or Private Sector: Evidence from a Natural Experiment

2.1 Introduction

This chapter revisits the established research question on public-private wage differentials from a subjective well-being perspective. In particular, it focuses on differences in *job satisfaction* between state and private sector employees in Ukraine and thereby follows the comparatively recently emerging economics literature which has assessed public-private sector differentials in terms of subjective well-being outcomes in several Western European countries (see Clark and Senik 2006 for France and Great Britain; Demoussis and Giannakopoulos 2007 for Greece; Ghinetti 2007 for Italy; Heywood, Siebert and Wei 2002 for the UK; Luechinger, Meier and Stutzer 2008 for 25 European and 17 Latin-American countries; Luechinger, Stutzer and Winkelmann 2006, 2010 for 19 European countries and a separate analysis for Germany). Strikingly, the majority of these studies find that public sector workers are on average more satisfied with their jobs than their private sector counterparts (this is true, for instance, for France, Germany, Greece, Italy and the United Kingdom).

Taking this finding as a starting point, the main aim of this chapter is to investigate whether observed average public-private satisfaction gaps represent *rents* – i.e., genuine differences in objective job and workplace characteristics – or whether they are an artificial result caused by non-random self-selection of workers based on unobservable personality traits. In this context, the problem of self-selection would imply that workers with intrinsically higher satisfaction levels tend to sort into one particular sector, thus creating a spurious correlation between average satisfaction levels and sector affiliation. The empirical analysis in this study complements and extends the existing literature by applying a new empirical strategy to correct for this potentially endogenous selection of individuals into sectors.

This research objective is not only motivated by the intention to shed more light on the functioning of the labour market in general. There is a long-standing research interest among labour economists in public sector labour markets due to the public sector's sizeable share in the total labour force in most countries (at least 15 percent in developed countries) and its distinct particularities, like the collective wage setting mechanisms and the distinct objectives that guide decision making, e.g. affirmative action (Altonji and Blank 1999; Blank 1985; Ehrenberg and Schwarz 1987; Gregory and Borland 1999). For instance, according to OECD data, in 1995 the share of public sector employment in total employment was slightly above 30 percent in Denmark, Norway and Sweden, was around 25 percent in Finland and France, around 20 percent Australia, Austria, Belgium, Canada and Iceland and around 15 percent in Germany, Ireland, Italy, Spain, Switzerland, the United Kingdom and the United States (Gregory and Borland 1999). Public sector employment made up only about 10 percent of total employment in Luxembourg and the Netherlands and about 6 percent in Japan (Gregory and Borland 1999). Furthermore, the question of whether public sector compensation is too high is currently highly debated in the United Kingdom and other Western economies (see, for instance, Danzer and Dolton (2011) for the UK public debate). This political debate is not only fuelled by the fact that public sector wages represent costs that have to be ultimately borne by tax payers, but also because of their implications for private sector employers in terms of hiring and labour costs.

To this date, the extensive economics literature – dating back as far as Adam Smith – has mainly focused on the phenomenon of substantial and long-term *wage* differentials between industries and sectors, and in particular between public and private sector workers. From a theoretical point of view, there are several explanations for why wage differentials between industries or sectors might exist and why it might be optimal for certain firms (or sectors) to pay higher wages than others. One the one hand, these wage differences could reflect *compensating differentials* (monetary compensation for unpleasant job aspects) implying market clearing wages (by equalizing a worker's utility across jobs or sectors).[1] On the other hand, these differences could represent systematic 'rents' (non-compensating differentials) which could be driven, for instance, by efficiency wages or rent-sharing motives, union bargaining power or political economy considerations related to elections (Katz and Autor 1999).

[1] According to the theory of compensating or equalizing wage differentials dating back to Adam Smith, such differences might simply reflect a higher remuneration for 'unpleasant' job aspects (that employers have to offer in order to attract workers to these jobs e.g., hazardous jobs, night shifts) (Rosen 1987). The underlying idea is that when choosing employers employees tradeoff benefits (pecuniary and non-pecuniary job aspects) against costs of particular jobs as to maximize their utility. For the marginal worker who chooses between two jobs, it is thus the net advantage from benefits and costs (effort) that tend to equalize.

However, the empirical analysis of inter-industry or inter-sector wage differentials faces at least two major challenges which the present analysis strives to overcome: first, the concept and measurement of wages and secondly, unobserved heterogeneity among workers (i.e. omitted variables like ability, productivity or personality traits). As regards the first challenge, a thorough investigation requires detailed information not only on worker, job and workplace characteristics, but also a good measure for *total compensation*, comprising not only wages but also other pecuniary and non-pecuniary components and fringe benefits, for instance, pension schemes, paid vacation, sick leave, and flexibility of working times. However, a comprehensive measurement and monetary expression of these fringe benefits and non-pecuniary job aspects poses a serious challenge to the empirical research.[2] The second challenge in analysing the wage gap is to account for possible self-selection into industries and sectors that is driven by unobservables.[3] If certain sectors attract workers with higher productivity or ability (unobservable by the researcher) and these workers receive higher wages, then simple least squares estimates of the wage premium in this sector will be upward-biased (Gibbons and Katz 1992). Hence, the correction of the potential endogeneity of the sector variable is crucial in order to identify true wage differences.

In the following empirical analysis, the first challenge of appropriate measures of total compensation and job disamenities is addressed by focusing on *job satisfaction*. The virtue of job satisfaction as a single measure is that it presumably represents a comprehensive assessment of all relevant job aspects, amenities and disamenities, and thus overcomes the problem of assessing total compensation and non-pecuniary job aspects. According to Hamermesh (2001, p. 2): "Only one measure, the satisfaction that workers derive from their jobs, might be viewed as reflecting how they react to the entire panoply of job characteristics. Indeed, a potentially useful view is that job satisfaction is the resultant of the worker's weighting in his/her own mind of all the job's aspects." Hence, to the extent that differences in job satisfaction reflect an unequal distribution of relative net advantages across sectors and jobs, the analysis of inter-sector satisfaction differentials can be informative about the existence of sector specific

[2] See Hamermesh (1999) for an early account of the role of nonpecuniary aspects in the development of overall earnings inequality in the US. The most comprehensive attempt to measure total compensation in the public and private sectors is performed by Danzer and Dolton (2011) who combine various data sources to account for differences in e.g., pension schemes, fringe benefits, unemployment risks, in the UK.

[3] In fact, the problem of self-selection is omnipresent whenever outcomes of groups are being compared and the group 'membership' is a choice variable.

rents and thus extends and complements previous studies focusing on differences in measurable, objective earnings measures. However, the merit of job satisfaction as a single, comprehensive metric comes at the cost of its complexity, which is the result of relative assessments, prior expectations, adaptation/habituation and personality traits (for a detailed review see Clark, Frijters and Shields (2008)).

As regards the empirical problem of self-selection, in an ideal experimental setting this question could be answered by comparing the average job satisfaction of individuals who were randomly assigned either to the public or the private sector (the average treatment effect).[4] This would provide information on the counterfactual scenario, of how much more or less an average person would be satisfied by working in the either the private or the public sector. Since carrying out such a randomized experiment is unfeasible, the identification strategy in this study rests on a unique natural experiment which generated an exogenous variation in the share of public sector employment in total employment: a fraction of the workforce was de facto randomly re-allocated from the public into the private sector.[5] More specifically, the empirical strategy exploits the tremendous changes in enterprise ownership structure (from a situation of exclusive state ownership of all firms to a mixture of state and private ownership across firms) that were part of the transition process in Eastern European countries following the break-up of the Soviet Union. The data comes from a unique and nationally representative survey of Post-Soviet Ukraine (2003-2007) that contains almost complete retrospective individual work histories since Soviet times. This way, the data provides crucial information on pre-determined job and workplace characteristics (Soviet jobs) as well as on the privatization process during the 1990s. To correct for possible self-selection bias in the Post-Soviet public sector satisfaction premium, an instrumental variable approach is applied that assigns a certain 'privatization probability' to each individual based on his/her Soviet pre-transition job characteristics.

This research contributes to the previous literature in several ways. First, in contrast to earlier studies on public-private sector satisfaction differentials, the empirical strategy of this research paper is to use for the first time an instrumental variable

[4] It has to be noted, that the results will always reflect the distribution of individual preferences for either of the two sectors. Imagine a situation where 70 percent of the population would be better off working in the private sector, but the share of private sector employment would only be 50 percent of total employment. A random assignment to the sectors would generate a situation in which 70 percent of the private, but only 30 percent of the public sector employees would be satisfied with their job.

[5] In Section 3 it will be argued that this allocation is random conditional on observables.

approach to correct for worker self-selection between the public and the private sector and to thereby identify the causal effect of public sector employment on job satisfaction. The empirical approach exploits the exogenous changes in ownership structure during the transition process from planned to market economy. To the best of my knowledge this is the first study that strives to identify the pure sector satisfaction differential based on such an exogenous variation in the sector allocation of workers.

Second, it adds further empirical evidence to the literature on inter-industry and inter-sector wage differentials by providing further indirect evidence for non-equalizing differences in total compensation as measured by significant public-private job satisfaction gaps.

Third, detailed survey information on fringe benefits and payment schemes allows an assessment of whether the public sector satisfaction premium is driven by different wage levels and/or non-pecuniary job amenities like social benefits. This research thus contributes to the literature on the importance of job amenities and payment schemes for job satisfaction.[6]

Fourth, to the best of my knowledge the present study is also one of the first to analyse *job* satisfaction in a country of the former Soviet Union based on a nationally representative dataset. It thus complements and extends the strand of literature that has analysed overall *life* satisfaction in Russia (e.g., Eggers, Gaddy, and Graham 2006; Frijters, Geishecker, Haisken-DeNew and Shields 2006; Graham and Pettinato 2002; Guriev and Zhuravskaya 2009; Senik 2004). However, by shifting the focus of the attention to job satisfaction, the present work highlights one of the domains that has been particularly affected by the transition process, namely the labour market. Against this background the results of the present study might yield important insights for any policies or reforms aimed at further fostering and establishing a market-based economic system which is closely connected with the development of a well-functioning private sector.

The remainder of the chapter is organized as follows: the next section explains how job satisfaction is interpreted in the present analysis and provides a brief discussion of empirical approaches and findings of previous studies on job satisfaction differentials between public and private sector employees. Section 2.3 explains the empirical

[6] A recent study which determines the value of job amenities explicitly in the context of compensating differentials is Helliwell and Huang (2010) for Canada.

problem of self-selection and describes the identification strategy in greater detail. These considerations are complemented by further background information on the emergence of the private sector following the collapse of the Soviet Union (Section 2.4). A description of the data set as well as the construction of the instrumental variables is provided in Section 2.5. Section 2.6 presents the main regression results (without and with correcting for self-selection) and discusses several robustness and sensitivity checks. The role of sector differences in the provision of social and financial benefits is assessed in Section 2.7. Section 2.8 concludes.

2.2 Job satisfaction and previous findings on public-private sector satisfaction gaps

2.2.1 The concept of job satisfaction

The underlying interpretation of reported job satisfaction in the present analysis is derived from the theory of 'Evolutionary Efficiency and Happiness' by Rayo and Becker (2007) and the theoretical considerations presented by Hamermesh (2001), Lévy-Garboua and Montmarquette (2004) and Clark, Frijters and Shields (2008).[7]

The model by Rayo and Becker (2007) centres on the key idea that happiness and satisfaction functions (utility in general) involve relative comparisons and that the optimal happiness function is a *purely relative measure of success*. In this way their model can also be related to the growing evidence of the importance of relative comparisons in subjective well-being assessments (intertemporal and interpersonal comparisons). "In any given period, the agent's happiness depends exclusively on the difference between his output y and an endogenous reference point $\hat{y}$ which serves as a performance benchmark (the difference $y - \hat{y}$ is the carrier of happiness). This reference point $\hat{y}$ is positioned according to the current opportunities faced by the agent and is updated over time in tandem with changes in these opportunities." (Rayo and Becker 2007, p. 306)

This basic setup can be adapted to the context of job satisfaction in the following way: the 'output y' is being replaced by a measure $\varphi_i(k_j)$, which reflects an individual

[7] Rayo and Becker (2007) present a theoretical model of happiness, in which the degree of happiness is a device, by which agents can successfully discriminate between different alternatives and thereby increase the efficiency of their choices. However, in my opinion, the main arguments can be applied to the analysis and interpretation of job satisfaction as well.

specific 'net advantage' (rent or net utility) from a job with a given vector of job characteristics k_j (job amenities and disamenities). The benchmark $\hat{\varphi}_i(k_j*)$ will in this case reflect a reference point or benchmark level of 'net advantage' that the same individual could achieve in another job with an individually optimal vector of job characteristics k_j* (among all combinations of realizations of the elements of k_j which are available on the labour market, k_j* is the vector of job characteristics that maximizes $\varphi_i(k_j)$). The distance $\varphi_i(k_j) - \hat{\varphi}_i(k_j*)$ will affect the level of job satisfaction: the larger the distance, the less satisfied the individual. The function $\varphi_i(\bullet)$ that translates the vector of job characteristics into a measure of 'net advantages' is individual specific (i.e. dependent on ability, health and family situation).

If the benchmark is endogenously determined and updated each period, what are the factors shaping $\hat{\varphi}_i(k_j*)$ or the optimal vector of job characteristics k_j*? According to the large psychology and economics literature investigating the effect of absolute and relative income on life satisfaction and happiness, this reference point can be influenced by outcomes of others (social comparison) as well as by one's own past (habituation) (Clark, Frijters and Shields 2008). Hamermesh (2001) emphasizes the important role of prior expectations for overall job satisfaction. In this sense, the size of $\hat{\varphi}_i(k_j*)$ could also be interpreted as a prior expectation which is formed based on one's own experience and the achievement of others. Any shortfall in realized 'net advantages' from this prior expectation will then result in lower satisfaction. In the model by Lévy-Garboua and Montmarquette (2004) a worker's reported job satisfaction reflects the *experienced preferences* for his/her job. In their framework job satisfaction "indicates how one's own experienced sequence of jobs compares with mentally experienced alternatives" and thus reflects whether the individual would "wish to repeat his past career if he now had to choose again" (Lévy-Garboua and Montmarquette 2004, p. 136). This notion can be incorporated into the above framework by interpreting $\varphi_i(k_j)$ and $\hat{\varphi}_i(k_j*)$ as mirroring the complete sequences of realized 'net advantages' versus potential 'net advantages' since the beginning of the working life.

To sum up, in the following analysis job satisfaction will be interpreted as an indicator of the distance of an individual's current job (vector of job aspects) from his benchmark job (generating a shortfall from the potential 'net advantage'). The implication for labour market outcomes is that unsatisfied workers will aim at attaining a higher job satisfaction by switching jobs (if the benefit of higher job satisfaction outweighs the switching costs). In this sense, job satisfaction can be seen as an economic variable that is informative about the processes and situations preceding decisions and choices in the labour market. For instance, Freeman (1978) demonstrates that lower job satisfaction is associated with a higher quit probability (see also Clark, Frijters and Shields (2008) for examples of panel data studies demonstrating the predictive power of job satisfaction for future behaviour).

2.2.2 Previous literature on public-private satisfaction differentials

Recent empirical studies have shown that many countries exhibit substantial differences in job satisfaction levels between the public and the private sector (Table 2-1 provides a list of all recent economic studies with an explicit focus on public-private sector job satisfaction differentials). Although the size and the sign of the public-private satisfaction gap are likely to depend on the country specific institutional framework which typically varies a lot across nations, surprisingly, the majority of these studies find a public sector satisfaction *premium* (e.g., in Germany, Greece, Italy and the United Kingdom).

Table 2-1: Empirical studies on public and private sector job satisfaction differentials

Study	Country and sample	Empirical Method	Findings
Heywood, Siebert and Wei (2002)	UK; British Household Panel Study (BHPS) 1991-94; employed workers with complete information in the 4 waves (men and women)	OLS (with pooled panel data), Panel fixed effects and first difference estimates of job switchers [Dependent Variable (DV): job satisfaction]	OLS and First difference: significant public sector satisfaction premium Panel FE: insignificant effect
Clark and Senik (2006)	France and Great Britain; French part of the European Community Household Panel (ECHP) – 1994 to 2001; British Household Panel Survey (BHPS) – 1991 to 2001	OLS and fixed effects panel estimation [DV: job satisfaction and wages]	France: significant public sector satisfaction premium in OLS and Panel FE UK: OLS: insignificant effect; Panel FE: significant satisfaction premium

Table 2-1 continued

Demoussis and Giannakopoulos (2007)	Greece; Greek component of the European Community Household Panel (ECHP) from 1995 to 2001	Random effects ordered probit model [DV: total job satisfaction; satisfaction with earnings, job security, working hours, working time]	Significant public sector satisfaction premium in overall job satisfaction as well as in each other job aspect
Ghinetti (2007)	Italy; Household Survey from the Bank of Italy 1995	Ordered probit model (and ordered probit switching model) [DV: satisfaction with six different types of non-pecuniary job amenities]	Ordered probit model: significant public sector premium on satisfaction with constructed indicator for overall non-pecuniary aspects
Luechinger, Stutzer and Winkelmann (2006)	19 European countries (no Central and Eastern European transition countries); European Social Survey (ESS) 2002 and 2004 (waves 1 and 2); GSOEP 2004	Ordered probit models and endogenous ordered probit switching models [DV: ESS: life satisfaction; GSOEP: job satisfaction]	Pooled sample, simple ordered probit: public sector satisfaction premium; authors find significant aggregate gains from sorting as opposed to a random allocation of workers
Luechinger, Meier and Stutzer (2008)	25 European countries and 17 Latin-American countries; ESS 2002 and 2004; Latinobarometer 1997, 2000, 2001, 2003; Eurobarometer (14 waves, 1989-1994)	Estimation of country specific satisfaction differentials that are then included as DV in a cross-country macro estimation [DV: life satisfaction]	Cross country variation in sign and size of public sector satisfaction premium. The majority of countries show a positive public sector premium.
Luechinger, Stutzer and Winkelmann (2010)	Germany; German Socio-Economic Panel (GSOEP) 2004; male workers aged 25-60 in West Germany	Ordered probit with endogenous binary variable and endogenous ordered probit switching models [DV: job satisfaction]	Without controlling for self-selection: small public sector premium; Correcting for self-selection, the gap increases; Evidence for negative selection into public sector

For example, expressed in terms of foregone wages, an average Greek public sector employee would be willing to accept a wage cut by 4.5 percent in order to avoid a job in the private sector – holding initial job satisfaction levels constant (Demoussis and Giannakopoulos 2007). In Italy, public sector workers are on average 3.5 percent more likely to be very satisfied with their jobs than their private sector colleagues (Ghinetti

2007). The papers by Clark and Senik (2006), Luechinger, Meier and Stutzer (2008) and Luechinger, Stutzer and Winkelmann (2010) interpret and assess these satisfaction gaps with an explicit reference to the existence of rents in the labour market. Potential underlying factors behind these satisfaction premiums in the public sector could involve higher wages, monetary and nonmonetary fringe benefits as well as bribes (Luechinger, Meier and Stutzer 2008).

Furthermore, sector differences in job and income security may also play an important role. Job security as a job amenity might be especially valuable during economic recessions or in generally more volatile economic circumstances in developing countries: using German panel data and European cross-sectional data, Luechinger, Meier and Stutzer (2010) demonstrate that *life satisfaction* of public sector workers is less affected by economic downturns and increasing rates of unemployment rates than life satisfaction of private sector workers. The authors interpret their findings as evidence for the fact that public sector employees are better shielded from economic shocks and that this higher economic security is valued by public sector workers. Further evidence on higher levels of job security of public sector workers is provided by Clark and Postel-Vinay (2009). Using data on satisfaction with job security for 12 European countries from the European Community Household Panel the authors show that perceived job security is also related to the extent of employment protection as well as to the generosity of unemployment insurance benefits. In addition, Rodrik (2000) argues that especially in developing countries which are vulnerable and exposed to external risks, higher levels of comparatively secure public sector jobs might reflect partial insurance of households against income risks (private sector workers can benefit indirectly from these secure jobs through economic spill-over effects).

However, the empirical research on public-private sector satisfaction gaps has to deal with and overcome the same empirical challenges caused by the sorting of workers as the research on wage differentials. If unobserved heterogeneity of workers leads to systematic self-selection into the public or private sector then simple OLS estimates of the satisfaction differential will be biased. Much in line with the literature on wage differentials, the empirical studies on job satisfaction have accounted for the problem of self-selection by either panel methods, controlling for individual fixed effects (Clark and Senik 2006; Heywood, Siebert and Wei 2002) or by estimating endogenous switching regression models (Luechinger, Stutzer and Winkelmann 2010). In the case of panel estimations either individual fixed effects were included or first difference (within) estimators were applied to the subsample of sector switchers. In both cases estimation of the public sector coefficient relies on individuals who switch between sectors. Although this approach improves the simple cross-sectional estimates by controlling for unobserved heterogeneity between individuals, results might still be biased as long as the decision to switch is still endogenously determined and thus the subsample of switchers represents a non-random sample of the population.[8] Furthermore, if levels of job satisfaction prior to a voluntary leave are particularly low (and thus foster the actual switching) then the immediate gain in satisfaction after switching jobs might overstate the true effect (similar to the Ashenfelter dip in the evaluation literature).[9] Hence, panel data methods are ultimately limited in shedding light on random counterfactual outcomes to evaluate causal effects. This can instead be achieved with credible and valid instrumental variables which cause an exogenously determined allocation of workers across sectors.

The second approach that has been used in empirical studies on the public-private job satisfaction differential, endogenous switching regressions, strives to account for the selection by explicitly modelling and estimating the selection equation based on observables into the two sectors and by correcting the sector specific coefficients for the self-selection bias (Ghinetti 2007; Luechinger, Stutzer and Winkelmann 2006, 2010). This approach has been widely used in the analysis of public-private wage differentials. Its focus is on the question whether individual and job characteristics generate different satisfaction returns in the two sectors. Although this empirical strategy loosens the restrictions on the coefficients by allowing for sector specific variation, the identification (correction for self-selection) rests on particular functional form assumptions (as well as valid instruments to act as exclusion restrictions in the selection equation).

[8] Luechinger, Stutzer and Winkelmann (2006) stress this point in their discussion. This problem of fixed effects panel estimations has also been addressed in the literature on wage differentials (e.g. Gibbons and Katz 1992).

[9] This would correspond to the problem of the Ashenfelter dip or pre-programme dip in the evaluation literature (referring to the seminal article by Ashenfelter 1978). In fact, a study by Boswell, Boudreau and Tichy (2005) furthermore shows that levels of job satisfaction do not only tend to be lower prior to voluntary job changes but also ‘too’ high immediately after the job change and declining afterwards (the authors call this effect the honeymoon-hangover effect). However, a panel data study by Clark, Diener, Georgellis and Lucas (2008) demonstrates that there are substantial anticipatory and adaptation effects of certain life events on *life satisfaction*, e.g., unemployment, marriage, and that both ‘lead’ and ‘lag’ effects can go in the same direction. If this was also true for job satisfaction and voluntary job changes fixed effects models focusing on job switchers would underestimate the true job satisfaction differential.

In the previous job satisfaction literature, the main debate in terms of self-selection thus far focused on the question whether low levels of job satisfaction of *union workers* reflect endogenous sorting or not (see, e.g. Bender and Sloane 1998; Borjas 1979; Bryson, Cappellari, and Lucifora 2004; Heywood, Siebert and Wei 2002). Apart from panel data methods these studies have attempted to solve the problem of self-selection into union membership by using instrumental variable techniques, although credible instruments for union membership are difficult to find (for instance, these studies used attitudes towards unions, occupational or industry sector dummy variables as instrumental variables).

2.3 Identification strategy in the context of self-selection

This section explains the problem of self-selection into the public and the private sector in the context of job satisfaction in greater detail. It also presents the identification strategy of the empirical analysis and critically discusses the identifying assumptions.

2.3.1 Possible bias through self-selection into the public and private sector

To assess the causal effect of genuine differences in job characteristics between private and public sector jobs, one would ideally observe and compare each person's level of job satisfaction in similar jobs across the two sectors. Since such counterfactuals are generally not observable, one could simply compare the average levels of job satisfaction between sectors (controlling for observable characteristics) if the distribution of individuals across these two sectors was random with respect to intrinsic satisfaction levels (or at least random conditional on observable, exogenous characteristics).

However, in free, market based economies the observed allocation of workers in the labour market is generally the outcome of a selection and sorting process through which individuals (and firms) search and find their best matching jobs (and employees) according to their characteristics, preferences and information set. For instance, several empirical studies show that employees report higher levels of job satisfaction the better their job matches their skills (Belfield and Harris 2002; Vieira 2005). As a result, there should be utility gains from sorting in the labour market (Luechinger, Stutzer and

Winkelmann 2006).[10] In other words, individuals choose their jobs in order to maximize their personal 'net advantage' from working (i.e., minimize the distance to their individual benchmark). However, a priori, there are no reasons why this sorting process should lead to systematic higher job satisfaction levels in the state than in the private sector (in a well-functioning labour market). And thus the question remains as to how the observed higher levels of job satisfaction among public sector workers can be explained.

One possible explanation is that the public-private job satisfaction gap is caused by the fact that the public sector offers on average better remuneration packages (including non-pecuniary job amenities) and working conditions than the private sector. Such a situation would imply queues for public sector jobs (as suggested by Krueger (1988) for the US[11]). This should lead to a lower average satisfaction level among private sector employees, since they feel they would be better off in the public sector on average (i.e., they could achieve a higher level of 'net advantage' in the public sector). If this is the case, the public sector satisfaction premium would indeed reflect 'rents', i.e. non-compensating differentials in total compensation.[12] Furthermore, if public sector jobs are indeed more attractive and there is queuing for these jobs, then a random reallocation of workers should not cancel out the observed satisfaction gap (i.e. the positive job satisfaction public-private satisfaction gap should remain after correcting for self-selection).

The alternative explanation for the observed positive public-private satisfaction gap is that those worker characteristics which drive the sorting across sectors are *correlated* with unobserved intrinsic levels of happiness (in the absence of queuing for public sector jobs, i.e. public sector jobs are on average as attractive as private sector jobs). This would imply a non-random allocation of workers in terms of their inherent

[10] In fact, this 'matching' process into the private and public sector should lead to a higher aggregate job satisfaction level as explained and shown by Luechinger, Stutzer and Winkelmann (2006) for Germany. Their analysis demonstrates that the share of 'very satisfied' workers (among *all* workers) is significantly higher in the actual allocation of workers as compared to a purely random allocation of workers across sectors. However, it is not clear to what extent this re-allocation should affect the public-private job satisfaction gap, since this will depend on the distribution of preferences in the population as well as the size of the public vis-à-vis the private sector.

[11] For an overview and a discussion of the distinct feature of the public sector labour market as well as differences in employment composition between public and private sectors see, e.g. Gregory and Borland (1999) and Krueger (1988).

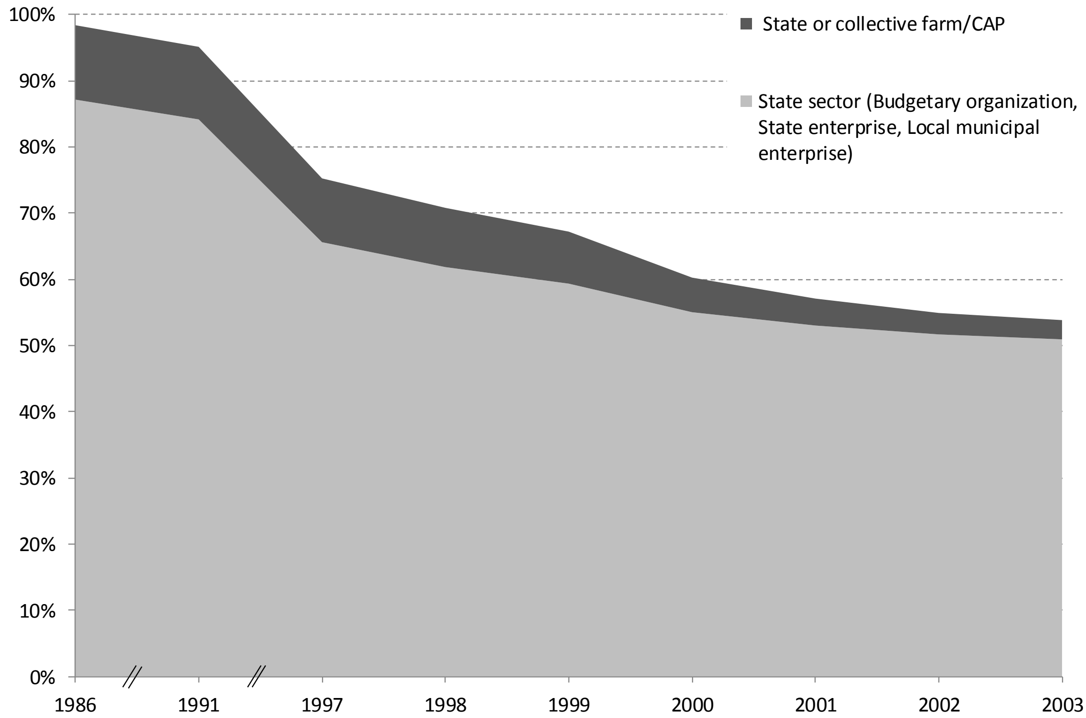
[12] The size of 'bureaucratic rents' is likely to depend on country specific institutional settings that determine the 'remuneration packages' of public sector employees (wages, fringe benefits, pension schemes, job and income security, etc.) as well as weak institutions or market imperfections, including corruption (Clark and Senik 2006; Luechinger, Meier and Stutzer 2008). Furthermore, the flexibility of labour contracts and the ease of mobility of workers (switching costs) will affect the size of the rents.

satisfaction levels. In this scenario, the positive public-private satisfaction gap would be a *spurious* result and not indicative of sector differences in job and workplace characteristics. This spurious satisfaction gap would be caused by the fact that individuals from the upper part of an underlying satisfaction distribution (those who are inherently more happy or more easily satisfied) sort into the public sector (Heywood, Siebert and Wei 2002). Correcting for this type of selection by randomly re-allocating workers across sectors should eliminate the spurious satisfaction premium.

2.3.2 Identification strategy and identifying assumptions

To correct for any self-selection bias, the identification strategy in the following analysis takes advantage of the dramatic changes in the ownership structure of firms in Ukraine which began after the collapse of the Soviet Union. More specifically, the fundamental restructuring process in the Post-Soviet era provides the researcher with information on the allocation of workers in two scenarios: a) the Soviet scenario in which all jobs are state sector jobs and b) the Post-Soviet scenario which is characterized by distinct private and state sector labour markets.

Figure 2-1. Evolution of share of employment in state sector (budgetary organization, state enterprise, local municipal enterprise) and state or collective farms



Source: ULMS 2003, own calculations based on all observations with the relevant retrospective information (N= 7,058)

Within little more than 10 years the share of jobs in the Ukrainian private sector grew from virtually zero to over 50 percent (see Figure 2-1). This process of ownership restructuring creates a quasi-natural experiment in which workers in certain Soviet industries were reassigned from the state to the private sector.[13] Despite the massive privatization and restructuring processes during the 1990s, there were still many industries with a substantially higher share of state sector ownership in 2003 than is typical for advanced market economies. Any sorting on the labour market that took place *after* the collapse of the Soviet Union and as a response to the development of a market economy with a distinct private and state sector can be measured and corrected for, since pre-transition and hence pre-determined information of worker allocation into what would be state sector or private sector jobs is available in the data.

The empirical analysis strives to answer the question "How big would the public sector utility premium be in Post-Soviet Ukraine in the absence of deliberate and non-random sorting between the two sectors" (i.e., a random allocation across private and state sector conditional on observables). To this end, instrumental variable techniques will be implemented to correct for the possible non-random selection of certain personality types into the public sector and to identify the causal effect of potential differences in workplace characteristics. The constructed instrumental variable will assign to each worker a probability of working in the private sector in Post-Soviet Ukraine based on his/her pre-transition, Soviet job characteristics (industry sector and geographical location; the construction of the instrumental variable is described in more detail in Section 2.5.2 and in Appendix B at the end of this chapter). In this sense the instrumental variable represents an intention to treat (in the first stage), since not all individuals who were assigned to the treatment were actually treated. Some individuals in the treatment group might not have been 'treated' because either their firm was never privatized or because they switched industries during the 1990s.[14] However, this latter aspect will reduce the predictive power of the instrument (first stage results) only if it affects the allocation across treatment (private sector employment) and control group (state sector employment). Industry switchers *within* treatment and control group are not as problematic. The estimated effect represents a local average treatment effect (LATE)

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[13] This is similar to the approach and identification strategy by Fuchs-Schündeln and Schündeln (2005), who exploit the German re-unification 'experiment' to disentangle the estimation of precautionary savings from self-selection into low-risk occupations. In their setup the re-unification is interpreted as a 're-assignment' of income risks for certain occupational groups.

[14] In the sample used in the empirical analysis around 55 percent of the workers are still working in the same industry they used to work in during the Soviet Union.

since the identification of the effect is based on individuals who comply with their assigned treatment and switch sectors because of this assignment (Imbens and Angrist 1994).

In order to identify the causal effect of working in the public sector, the employed instrumental variables z_i have to satisfy several conditions: (i) z_i has to be sufficiently correlated with the endogenous state sector dummy variable $Cov(z_i, state) \neq 0$, (ii) z_i has to be uncorrelated with the error term $Cov(z_i, \varepsilon) = 0$, and (iii) z_i has to meet the monotonicity assumption (Imbens and Angrist 1994). While condition (i), the strength of the instrument, can be tested by inspecting the first stage regression results, conditions (ii) and (iii) are untestable and one has to specifically argue that the instruments meet these criteria. It is plausible that the monotonicity condition (iii) holds as follows: if a person would choose to work in the private sector anyways (even if the person was not assigned to the private sector) this person would also choose to work in the private sector if the instrument assigned the person to the private sector. In other words, having a higher likelihood of being assigned to the private sector should not reverse or reduce the original choice intentions of an individual.

Condition (ii) requires that during the Soviet Union there was no sorting (of happy types) into industries according to whether a job/industry was likely to become private or public sector later on. It is very plausible that this condition is satisfied simply because in the Soviet Union there was no private sector or any sector with features similar to those of the Western private sector (e.g. profit maximizing firms, competition, hard budget constraints, job uncertainty, and fringe benefits). Instead, full employment was guaranteed by the state (due to the constitutional right to work), wages, working hours and working conditions were set by the government (centralized regulations) and the wage distribution was relatively compressed (see Brown 1973; Gregory and Collier 1988). Furthermore, social security and social benefits as well as fringe benefits like enterprise kindergarten or hospitals were provided irrespectively of economic or industrial sector (Flemming and Micklewright 2000; Friebel and Guriev 2005).

The labour force participation of women was extremely high in comparison with Western standards at that time (Linz and Samykina 2008). In fact, since the Soviet Union was run and organized along political motives and principles, individual educational paths, fields of specialization as well as advancement in the professional career were more closely connected to political loyalty and political decisions rather than to individual ability or preferences (Titma and Roots 2006). For instance, it is estimated that about 60 to 70 percent of graduates starting their first job were allocated by the government to meet labour requirements in certain industries and regions (Haddad 1972). In addition, labour market choices and mobility of individuals were limited due to the internal passport system as well as to the administrative allocation of housing during the Soviet Union (Gregory and Kohlhase 1988). Choice options of individuals were also restricted by geographic availability of jobs and industries. The spatial segregation of production enforced by the planners limited the diversity of industries within certain regions (Friebel and Guriev 2005). In extreme cases, the entire population of an area was working in a single, large state-owned enterprise (one company towns). This evidence against a systematic self-selection of workers across sectors in the Soviet Union in terms of unobserved personality traits is similar to the one provided in the paper by Fuchs-Schündeln and Schündeln (2005). The authors use German re-unification as a natural experiment in order to analyse the importance of self-selection for precautionary savings. Although their empirical strategy is not based on an instrumental variable approach, their fundamental identification assumption has much in common to the one presented here: prior to the re-unification risk averse individuals living in the socialist German Democratic Republic (GDR) had no incentive to sort into particular sectors or occupations, since labour income risk was extremely low in all jobs and occupational choice was politically restricted.

Another important assumption for the instrument to be valid in the current analysis is that it affects Post-Soviet job satisfaction only through the assignment into the state or private sector (i.e. that this assignment is uncorrelated with unobservable characteristics that might be associated with higher levels of job satisfaction and that the assignment in itself does not have a direct effect on job satisfaction). This aspect is further addressed in Section 2.6.3.1 discussing and testing the robustness of the empirical results and the instrumental variables. Since the assignment is mainly based on Soviet industry affiliation, this requires that workers were randomly allocated across industries (or at least randomly across 'would be private' and 'would be state' industries). Given the description of worker allocation in the Soviet Union above, it seems plausible that this condition holds, even more so when one considers a random allocation conditional on other pre-determined observable personal characteristics.

Last but not least, from the point of view of a Soviet employee during the 1980s the collapse of the Soviet Union as well as the large scale privatization and restructuring process during the transition process were unexpected and unforeseeable.[15] Hence, one can rule out the possibility that Soviet worker allocation across industries was a result of anticipatory behaviour.

Given these particular features of worker allocation in the Soviet Union, the analysis in this study treats the distribution of workers across different industries (within geographical regions) during the Soviet Union as a valid counterfactual scenario against which the post-Soviet allocation of workers across state and privately owned firms can be assessed.

2.4 The evolution of the private sector in Post-Soviet Ukraine

The transition process from planned to market economy which followed the collapse of the Soviet Union led to an enormous change in the ownership structure of enterprises and firms. The evolution of the private sector was a result of at least two processes: the privatization process, through which formerly state owned entities were transformed into private ownership, and the creation of completely new firms (de novo firms) which had never been owned by the state.

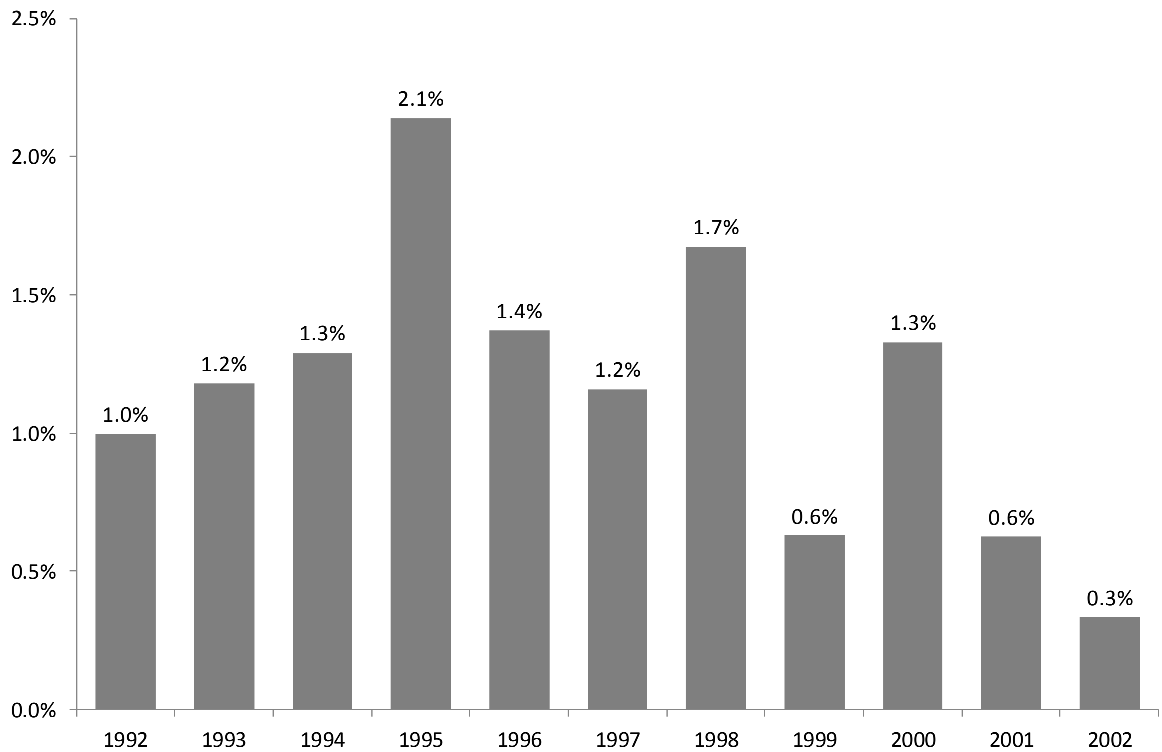
The privatization process in Ukraine started in 1992 and was slower in pace than in most other Central and Eastern European countries including Russia (Brown, Earle and Telegdy 2006).[16] Still, in the period from 1992 to 2004 more than 96,549 formerly state-owned entities were transformed into private ownership by means of privatization (overall there were 981,054 entities in the Unified State Register of Enterprises and Organizations of Ukraine in 2003; State Statistics Committee of Ukraine 2005, 2008).

Figure 2-2 reveals that privatization became relatively more frequent during the second half of the 1990s. In terms of labour market outcomes in the year 2003, 19.8 percent of the total workforce was employed in enterprises that had changed their type of ownership (this number excludes small businesses; State Statistics Committee of Ukraine 2010). Privatization took place in all industry sectors, but the extent of

[15] A large literature has assessed and evaluated the privatization process and its effects. See, for example, Bennett, Estrin, and Maw (2007), Brown, Earle, and Telegdy (2010), Brown, Earle and Vakhitov (2006), Estrin, Hanousek, Kocenda, and Svejnar (2009), and Megginson and Netter (2001).
[16] The Ukrainian Parliament approved the law on the First Privatization Program in July 1992 (Verkhovna Rada. The State Privatization Program for 1992; see Grygorenko and Lutz 2007).

privatization varied across industries.[17] The choice of privatization methods was influenced by political goals emphasizing the speed and social acceptability of the privatization process which led to large scale mass privatizations (involving the distribution of free privatization certificates to citizens or share transfers to employees), especially at the beginning of the privatization process (Pivovarsky 2001). As Brown, Earle and Telegdy (2006) point out, the privatization process that accompanied the transition process in Eastern Europe was universal, implying that firms were less selectively and carefully chosen than is typically true for Western countries.

Figure 2-2. Share of workers experiencing privatization of their workplace over time in Ukraine (affected workers among all employed in a given year)



Notes: These numbers are based on retrospective self-reported privatization experiences during job tenure of the participants of the ULMS (for more information on the survey and data set, see Section 2.5). Source: ULMS 2003, own calculations.

Generally, the privatization process in Ukraine can be divided into several phases. In the first years, between 1992 and 1994, the privatization process was very

[17] Table B 2-1 in the appendix shows the share of employees whose enterprise was privatized during the 1990s among 27 different industries. The figures refer to jobs/industries held by employees in December 1991 and were calculated based on the retrospective information in the Ukrainian Longitudinal Monitoring Survey 2003 (a more detailed description of the variable generation is provided in the appendix).

slow and resulted in privatizations of very few enterprises (on a case-by-case basis). However, the process gained momentum after the Ukrainian government launched a revised mass privatization program at the end of 1994 which had been prepared with the support of Western donors and advisors (USAID, World Bank, European Union and the EBRD) (USAID 1999). The goal of the programme was to privatize the universe of Ukraine's approximately 10,000 medium and large industrial enterprises by 1998 using the following objectives: (i) rapid and equitable distribution of shares to Ukrainian citizens (political objective), (ii) development of capital markets and the respective infrastructure, and (iii) fast creation of a critical mass of privately owned enterprises to trigger relevant modernization processes in the economy (USAID 1999). Interestingly, generation of state revenues was not the prior aim of the mass privatization programme. The objective to generate state revenues by means of privatization was given more priority in the later stages of the process after 1999 (Grygorenko and Lutz 2007).

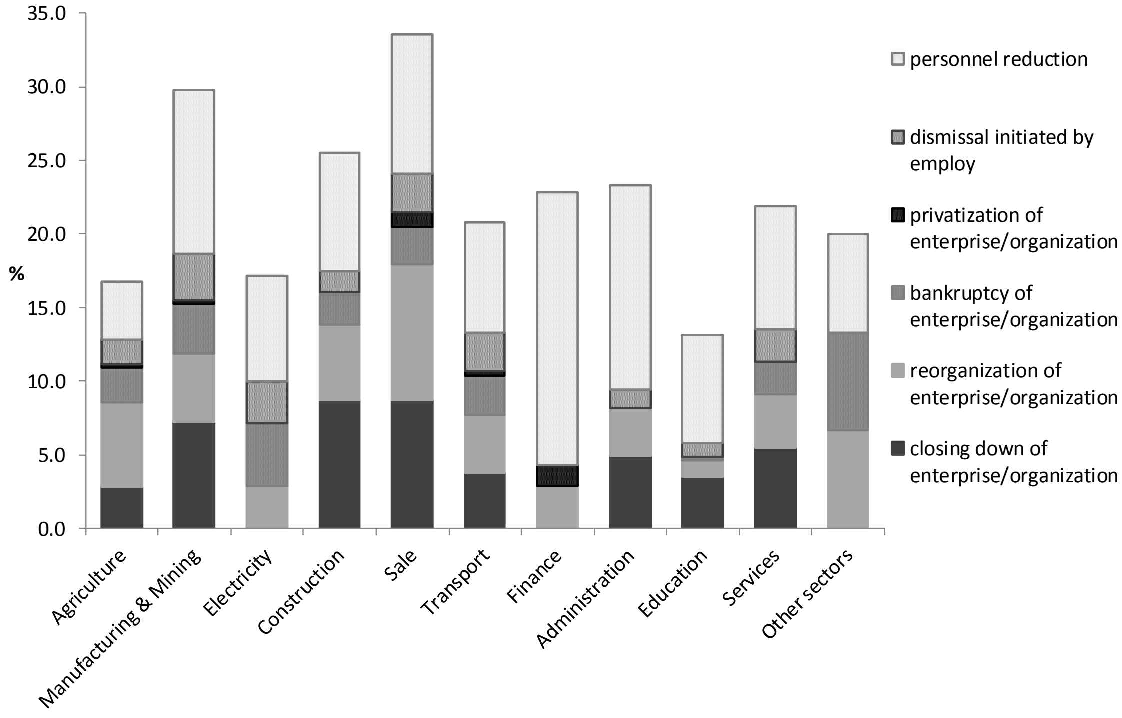
The method of mass privatization in Ukraine was carried out by distribution of privatization certificates or vouchers to all citizens. These vouchers could then be used to purchase shares of enterprises. Preferential purchase rights were given to employees and managers and remaining shares were sold to other persons holding privatization certificates (USAID 1999). In the largest enterprises, about 25 percent of shares were sold to employees and the public, the state initially kept between 25 to 51 percent of shares (especially in so-called 'strategic enterprises') and the remaining shares were sold through cash and/or investment tenders or local stock exchange sales (USAID 1999).[18] In addition to the mass privatization programme for medium and large scale enterprises, about 40,000 shops and retail establishments were privatized by means of buyout to employees or cash auctions. The design of Ukraine's mass privatization process fundamentally shaped the resulting ownership structure of enterprises: ownership became widely dispersed and dominated by insiders (managers and workers) (Grygorenko and Lutz 2007).

Next to the privatization procedure the destruction of old and inefficient (state-owned) enterprises and the creation of de novo private firms fostered further job flows and contributed to the growing share of employment in the private sector. Figure 2-3 gives an overview of the reasons why jobs that started during the Soviet period were

[18] In 1995 the Ukrainian Parliament issued a list of 5,200 'enterprises banned from privatization in view of their importance for the national economy', which were mainly enterprises in the energy sector (power networks and energy systems, hydro and nuclear power stations, combined heat and power stations) (USAID 2000). An additional list of strategic enterprises (mainly monopolies) was established in 1997 (Grygorenko and Lutz 2007).

terminated during the transition process in the 1990s (based on the ULMS 2003). Again, industries were heterogeneously affected by the extent of ‘closing down’, ‘bankruptcy’, ‘reorganizations’ and ‘mass lay-offs/personnel reductions’. Importantly, only very few job separations were caused by privatizations. Furthermore, when considering the evolution of these different reasons for job separations over time, ‘personnel reductions’, ‘closing down of enterprise’ and ‘reorganization of enterprise’ became the most frequent reasons job termination in the late 1990s and early 2000s (see Figure A 2-1 in the appendix).

Figure 2-3. Reasons for end of employment related to industrial restructuring, by industrial sector 1986 (share among all types of end of employment, incl. retiring, voluntary leaves, etc.)



Source: ULMS 2003, own calculations based on all persons who have worked during the Soviet time and for whom this retrospective information is non-missing (N= 4,650).

It is likely that the process of destruction and creation of firms (and jobs) not only affected the share of private employment across industry sectors, but even caused a shrinking of some industries and growth of others (e.g. shrinking industrial sector, growing service sector). Hence, probably some individuals were forced to leave their state sector employment in one industry and seek new opportunities in other industries.

Since the creation of new firms and jobs took mainly place in the private sector, it is likely that these structural changes de facto reallocated individuals into the private sector labour market.[19] However, as the analysis by Lehmann, Pignatti and Wadsworth (2006) for Ukraine demonstrates, only one third of workers who lost their jobs as a consequence of the restructuring process (due to mass lay-offs, plant closures, etc.) were able to find new employment immediately. For the majority of workers the job displacement resulted in long-term non-employment.

These two processes motivate the construction of two different industry specific instrumental variables to correct for the self-selection of workers across sectors: one is based on the privatization process only, while the other captures both, the extent of privatization and the creation of new firms across industries.

2.5 Data set, variables and sample description

The analysis is based on the Ukrainian Longitudinal Monitoring Survey (ULMS), a panel data set of a nationally representative sample of individuals aged 15 to 72 who were interviewed in three waves in the years 2003, 2004 and 2007.[20] The original raw sample size in 2003 consists of 8,537 individuals in 4,056 households, out of which 7,200 and 6,774 individuals were (re-)interviewed in 2004 and 2007 respectively (3,449 and 3,101 households). The survey provides detailed information on individual labour market activities, workplace and job characteristics plus all relevant and standard individual socio-demographic characteristics like education, marital status, children and health. Furthermore, information about the respondents' households, e.g., household size and composition, locality and quality of place of living, household income and household expenditure, is included.

There are two special features that make this data set unique in comparison to other surveys and hence, especially suitable to address the research question. Firstly, the data provides detailed retrospective information on the individuals' labour market

[19] Lehmann and Wadsworth (2000) analyse and compare the effect of tenure on the process of labour reallocation in Russia, Poland and the UK. For Russia, they find that turnover was lower in the state than in the private sector in the first decade of the transition process. If the same is true for Ukraine, then the instrumental variables could be 'more exact' for the state sector employees, if these remain in the same job and hence industries. As mentioned in footnote 14 about 55 percent of all individuals in the sample are still in the same industry as during the Soviet period.

[20] The survey was implemented by the Kiev Institute for Sociology (KIIS) following a multi-stage sampling procedure. Detailed information on the sampling procedure can be found in the technical reports provided by KIIS.

activities and job characteristics during the Soviet Union as well as their individual labour market histories during the transition process in the 1990s up to 2003. In particular, respondents were asked to provide labour market information for December 1986 and December 1991 as well as the complete job history starting from 1997 up to the respective interview date. The years 1986 and 1991 were chosen as two special dates in Ukrainian history (Chernobyl catastrophe in 1986; 1991 as year before independence) in order to serve as reference and memory anchors to reduce potential recall error (Ganguli and Terrell 2006).[21] This retrospective, pre-transition information on Soviet firm and job characteristics is the basis for the identification strategy.

In particular, this analysis makes use of the information on the one- and two-digit industry sector codes as well as on workplace location (five macro regions) in the Soviet era. Altogether there are 5,786 individuals in the cross-section 2003 for whom this Soviet labour market information is generally available. This implies that there is complete labour market information for almost all sampled individuals who were in working age at the time of the collapse of the Soviet Union (December 1991). Recall that there was no official unemployment in the Soviet Union. The retrospective information from these individuals is used to construct the two instrumental variables (see Section 2.5.2). Obviously, such Soviet information is not available for young persons who started their first job after December 1991, the independence of Ukraine. As regards the estimation sample, in the cross-section 2003 there are in total 3,583 individuals who have a paid job at the time of the interview. Out of these, 2,441 are old enough to have worked in the Soviet Union so that the relevant retrospective Soviet job information exists (a more detailed overview on the sample structure and available retrospective Soviet information is given in Table A 2-1 in the appendix). However, missing values in several of the variables included in the regressions furthermore reduce number of observations from the 2003 survey wave to 1,491. The final estimation sample is based on observations from all three survey years and includes all individuals who have a paid job in any of the three survey years 2003, 2004, 2007 and for whom the retrospective information is available yielding a pooled sample size of 4,191 observations (unbalanced panel).[22]

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[21] Ganguli and Terrell (2006) as well as Brown, Earle and Vakhitov (2006) discuss potential problems, advantages and quality of retrospective data collection in the ULMS. For instance, the fact that wage structures etc. were highly regulated and determined by a wage grid during the Soviet Union increases the likelihood for correct recalls of Soviet wages.

[22] In order to check for the sensitivity of the results to the sample size, the main regressions are re-estimated based on a larger sample by dropping a variable measuring risk aversion that is only asked in

Secondly, the survey collects detailed *ownership* information of the respondents' workplaces (13 different categories) and thus allows an exact classification of respondents into state or private employment.[23] Additionally, respondents were asked whether – and if so, when – their workplaces were privatized during their tenure at that specific firm or not.[24] This privatization information will be used to construct one of the instrumental variables (see section on instrumental variables further below as well as Appendix B).

In order to generate the second, alternative instrumental variable, another micro-data set is employed, the Ukrainian Household Budget Survey (UHBS). The UHBS is an annual cross-section of around 9,000 households and about 25,000 individuals and is conducted by the State Statistics Committee of Ukraine (UkrStat). The construction of the instrumental variable is based on four survey years (2003-2006) and makes use of the available labour market participation, ownership of workplace and place of living information in order to calculate industry-region specific shares of private sector employment in total employment (a detailed description on how the instrumental variable was calculated is again provided in the section on instrumental variables further below as well as in Appendix B).

2.5.1 Dependent and explanatory variables

The dependent variable job satisfaction is measured by the question "*Tell me, please, how satisfied are you with your current job?*" to which respondents can answer on a five-point Likert scale according to the following categories: 1–Fully dissatisfied, 2–Rather dissatisfied, 3–Neither satisfied nor dissatisfied, 4–Rather satisfied, 5–Fully satisfied.[25,26] Since the question does not refer to any particular job facet it can be

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one survey year and has many missing values (this increases the pooled sample size to 5,142 observations, while the 2003 sample increases from 1,491 to 2,059).

[23] This is an advantage in comparison with some other studies which do not have direct information on firm ownership and thus classify public sector workers based on industry codes.

[24] The reliability of self-reported ownership status in the ULMS is discussed by Brown, Earle and Vakhitov (2006), who use the ULMS to analyse wage and employment effects of privatization. They emphasize the careful wording and the fact that workers were directly involved in any privatization process which increases the likely accuracy of the answers. However, a measurement error in this variable (state sector employment) should lead to a downward bias in the estimated coefficients.

[25] This is the author's translation of the Russian and Ukrainian questionnaire. The English version of these answers provided by the survey institute KIIS is 1–Not satisfied at all/2–Less than satisfied/3–Rather satisfied/4–Satisfied/5–Fully satisfied, which is misleading.

[26] Detailed and critical discussions and assessments of the reliability and validity of such subjective, ordinal measures in empirical economic analyses can be found, for instance, in Bertrand and Mullainathan (2001), Kahneman and Krueger (2006) and Krueger and Schkade (2008).

interpreted as a comprehensive judgment of all relevant aspects. The main analysis will employ this categorical dependent variable.[27]

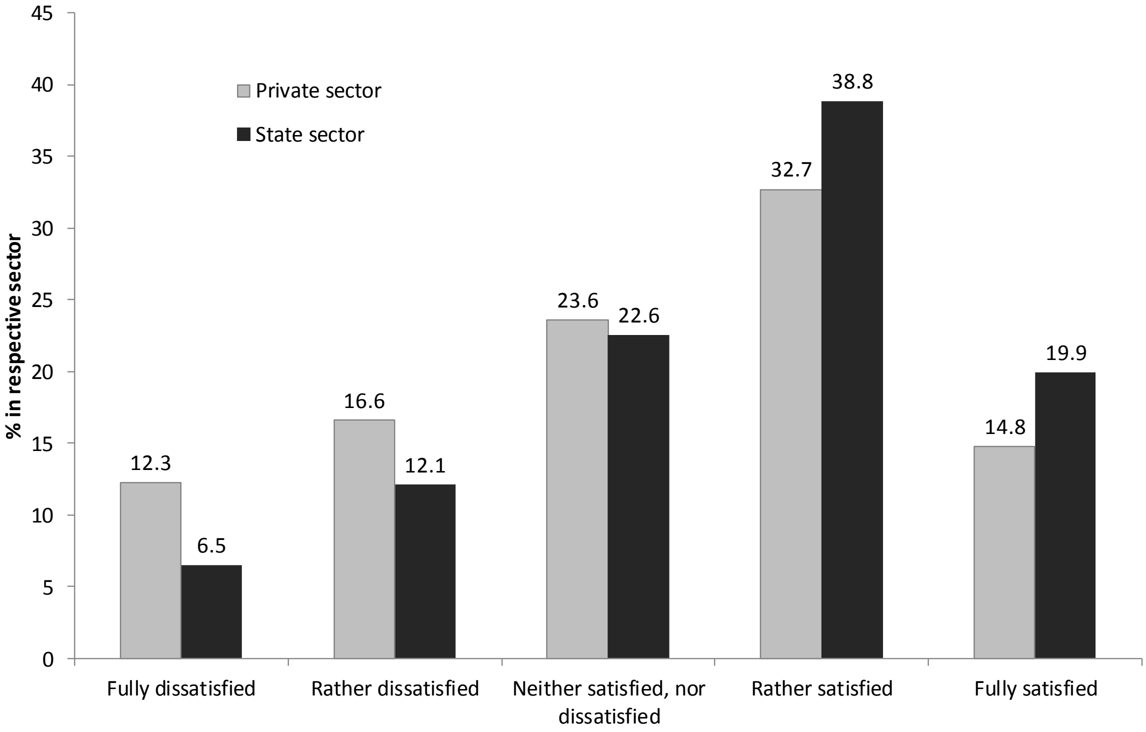
How satisfied are workers in the private and the state sector in Post-Soviet Ukraine? Figure 2-4 presents the raw and unconditional public-private job satisfaction gap for the pooled sample of workers (subsample used in the regressions; the respective graph based on the full ULMS sample including young individuals is provided in Figure A 2-5 in the appendix). Almost twice as many workers in the private sector are entirely dissatisfied with their job (12.3 versus 6.5 percent respectively). Looking at the other end of the spectrum of the satisfaction scale, the share of fully satisfied workers in the public sector surpasses the one in the private sector by 5 percentage points. These distributions imply that state sector workers are generally more satisfied with their jobs than their private sector counterparts. A similar pattern in raw differences between public and private sector employees has been found for other countries, like Germany and the UK (e.g., Luechinger, Meier and Stutzer 2008). When comparing the distribution of *life satisfaction* across sectors, the same picture emerges: state sector workers also tend to be more satisfied with their lives in general (see Figure A 2-2 in the appendix).

Interestingly, the left-skewness of the job satisfaction distribution stands in contrast to the *life satisfaction* distribution in Eastern European countries, which is comparatively right-skewed.[28] In other words, the relatively low levels of *life satisfaction* found in transition countries and also in Ukraine are not perfectly mirrored in the distribution of *job satisfaction* in Post-Soviet Ukraine as Figure 2-5 illustrates. These differences between job and life satisfaction are the same, when the sample is restricted to state sector workers only (see Figure A 2-3 in the appendix).

[27] There are two surveys that collect life and job satisfaction during the Soviet time. However, they do not cover any information on workplace details or job characteristics: the *World Values Survey* 1981 which was conducted in one Soviet region (Tambov), and the *Consolidation of Democracy in Central and Eastern Europe* survey of January 1991 that took place in the Ukrainian Soviet Socialist Republic. Unfortunately, these surveys cannot be used to learn more about the relationship between job characteristics and job satisfaction before the transition process due to lack of relevant variables.

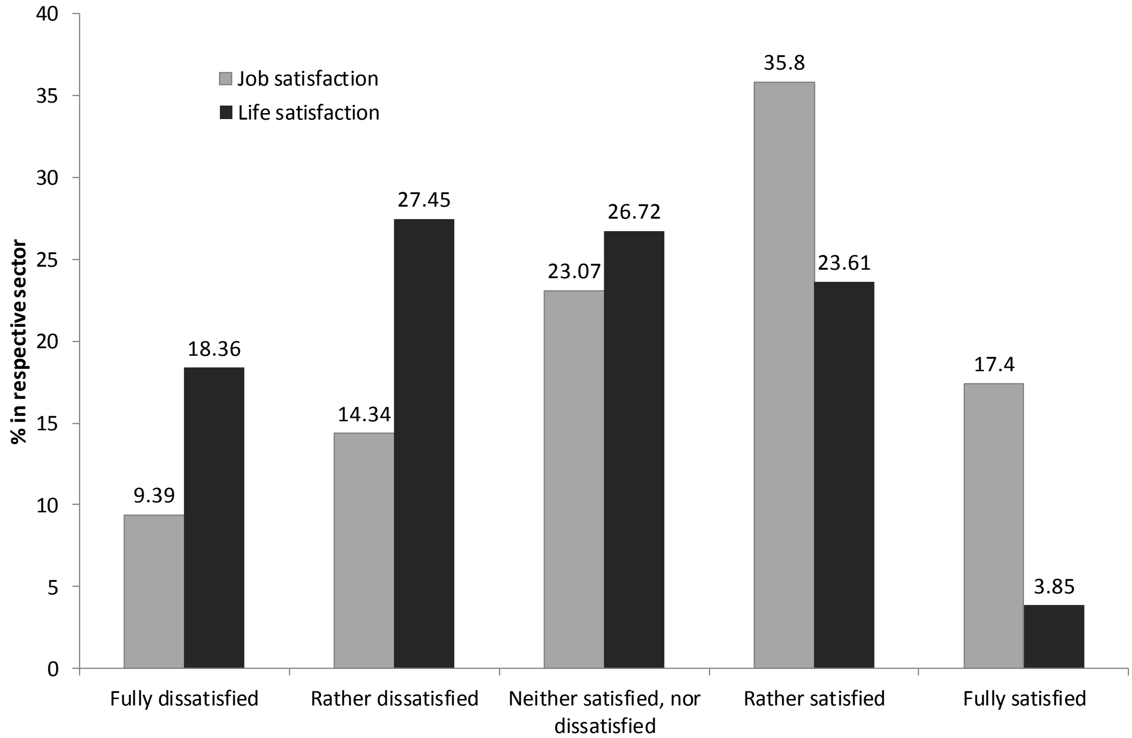
[28] For recent multi-country analyses of life satisfaction in transition countries, see e.g., Guriev and Zhuravskaya (2009), Sanfey and Teksoz (2007).

Figure 2-4. Job Satisfaction in the Private and Public Sector in Post-Soviet Ukraine



Source: ULMS 2003-2007, pooled sample of all individuals in working age with retrospective Soviet job information (extended regression sample; N=5,142); own calculations. A comparable diagram based on all observations in the original survey sample, i.e. including young workers, is given in the appendix.

Figure 2-5. Job Satisfaction and Life Satisfaction in Post-Soviet Ukraine



Source: ULMS 2003-2007, pooled sample of all individuals in working age with retrospective Soviet job information (extended regression sample; N=5,142); own calculations. A comparable diagram based on all observations in the original survey sample, i.e. including young workers, is given in the appendix.

The main explanatory variable is a binary state sector indicator, identifying individuals working in state owned as opposed to privately owned enterprises and organizations. The survey differentiates between 13 different types of ownership, out of which three can be unambiguously classified as state owned enterprises/organizations (i.e., budgetary organizations, state enterprises, and local municipal enterprises). The remaining categories are classified as private ownership (these are: newly established private enterprises, new private agricultural firms/farms, privatized enterprises, freelance work/self-employment, international organizations, public/religious/self-financing organizations, collective or state farms, collective enterprises, cooperatives, other.[29] Further control variables which are successively added to the regressions are (a) standard socio-demographic individual characteristics (including job related pre-transition background information), (b) job characteristics, and (c) workplace characteristics. Two additional sets of variables are included to investigate whether sector job satisfaction differences are driven by (d) personality traits or (e) differences in wages and pecuniary and non-pecuniary fringe benefits. As regards the personality traits, the analysis exploits two measures of "extroversion" and "neuroticism" based on interviewer assessments and one self-reported measure of risk aversion. A detailed description of all variables is provided in Appendix A (Table A 2-2). Table 2-2 presents separate summary statistics for all variables by sector of employment.

The descriptive statistics of the two sectors reveal differences in workplace characteristics as well as in the composition of the workforce that is much in line with the typical pattern for Western economies (see Gregory and Borland 1999): the share of women is higher in the state sector, state sector workers are on average more educated, slightly older, work fewer hours per week and are more concentrated in highly-skilled occupations (professional and technical occupations) as well as in larger establishments than their colleagues in the private sector. Interestingly, the two wage measures indicate that on average earnings tend to be lower in the state than in the private sector. Furthermore, state sector workers seem to be more open (extrovert) on average, but at the same time less risk loving. Finally, the mean comparisons show that present-day state and private sector workers also differ in their Soviet time characteristics, which might be partly driven by simple age and gender effects and partly by Soviet geographical and occupational (gender) segregation.

[29] The categories collective enterprise, new private agricultural firm/farm, and public/religious/self-financing organization were added to survey in 2004.

Table 2-2: Summary statistics of estimation sample, by sector

	Private sector *Mean*	State sector *Mean*	t-statistic from mean comparison test	
Dependent variables				
Job Satisfaction (std. dev.: 1.18)	3.23	3.55	-9.00	***
Satisfaction with job security (std. dev.: 1.14)	3.40	3.86	-10.00	***
Instrumental variables				
IV1_privatized (min.: 0.00; max.: 0.48)	0.17	0.11	18.97	***
IV2_privateshare (min.: 0.00: max.: 0.96)	0.67	0.44	23.98	***
Socio-demographic characteristics				
Male	0.53	0.39	8.68	***
Age: 35 or less (omitted category)	0.11	0.10	1.80	*
Age: 36 up to 40	0.19	0.17	1.52	
Age: 41 up to 45	0.24	0.24	0.39	
Age: 46 up to 50	0.24	0.25	-0.85	
Age: 51 up to retirement age	0.22	0.25	-2.21	**
Primary education (omitted category)	0.09	0.05	4.82	***
General secondary education	0.46	0.39	4.71	***
Professional secondary education	0.29	0.31	-1.38	
Higher education	0.16	0.25	-7.24	***
Individual height in cm.	169.42	168.07	5.38	***
At least one parent has higher education	0.13	0.12	0.71	
Both parents have lower education	0.48	0.48	-0.46	
Single (omitted category)	0.03	0.03	0.87	
Married	0.81	0.80	0.37	
Divorced	0.12	0.12	0.69	
Widowed	0.03	0.05	-2.57	**
No children in household (omitted cat.)	0.49	0.50	-0.91	
One child in household	0.33	0.34	-0.66	
Two or more children in household	0.18	0.16	2.04	**
Chronic disease	0.51	0.55	-2.16	**
Urban settlement	0.57	0.55	1.06	
Job and workplace characteristics				
Professional occupation	0.17	0.23	-4.72	***
Technician occupation	0.09	0.21	-11.06	***
Skilled blue collar occupation	0.52	0.37	9.77	***
Unskilled occupation (omitted cat.)	0.22	0.19	2.52	**
Working hours: 30 or less	0.08	0.13	-5.11	***
Working hours: 31 up to 50 (omit. cat.)	0.74	0.84	-7.56	***
Working hours: 51 or more	0.17	0.03	15.80	***
Firm size 1-9 (omitted category)	0.29	0.09	17.72	***
Firm size 10-99	0.30	0.41	-7.91	***
Firm size 100 and more	0.41	0.50	-5.78	***

Table 2-2 continued

Log earnings	5.83	5.79	2.07	**
Log average industry wage	6.30	6.26	2.83	***
Weekend work	0.54	0.37	10.75	***
Shift work	0.13	0.15	-1.96	**
Personality traits, risk aversion				
Extrovert	0.13	0.17	-3.90	***
Neurotic	0.02	0.02	1.46	
Never smoked	0.46	0.58	-7.35	***
Risk loving	0.24	0.19	3.89	***
Retrospective information from Soviet period				
Place of birth: village	0.44	0.46	-1.64	
Place of birth: urban settlement	0.07	0.11	-4.47	***
Place of birth: town/city	0.39	0.34	3.50	***
Place of birth: abroad (omitted cat.)	0.11	0.10	1.29	
Place of work 1986: Kiev	0.05	0.06	-1.17	
Place of work 1986: Centre	0.25	0.24	0.50	
Place of work 1986: West	0.14	0.20	-5.26	***
Place of work 1986: East	0.23	0.25	-1.36	
Place of work 1986: South	0.27	0.21	5.01	***
Place of work 1986: Russia (omitted cat.)	0.03	0.02	1.84	*
Place of work 1986: other (omitted cat.)	0.02	0.01	1.49	
Marital status in Dec. 1991: married	0.85	0.89	-3.55	***
Number of own children in Dec. 1991	1.43	1.50	-2.27	**
Log of wage in December 1986	-5.62	-5.86	1.88	*
Log of wage in December 1991	-5.45	-5.56	0.84	
Wage December 1986 missing	0.36	0.34	1.49	
Wage December 1991 missing	0.37	0.36	0.58	
Worked in Dec 1986	0.78	0.82	-3.34	***
Worked in Dec 1991	0.96	0.96	0.55	
Soviet job 1986/91: white collar occupation [A)]	0.30	0.46	-10.53	***
Soviet job 1986/91: has at least one subordinate	0.14	0.18	-2.77	***
Soviet job 1986/91: info subordinate - missing	0.20	0.13	6.25	***
Number of observations	*2,035*	*2,156*		

Notes: Pooled sample of all individuals in working age during time of interview with full information on all variables, who were 16 or older in December 1991 and for whom information on their job during the Soviet time (December 1986 or December 1991) exist. Reported t-statistics refer to mean comparison tests between state and private sector; *** p<0.01, ** p<0.05, * p<0.1. The regressions include furthermore: 2 survey year dummy variables, 25 oblast dummy variables, 8 interview month dummy variables. [A)] Professionals and technicians. Source: ULMS 2003-2007; own calculations.

2.5.2 Instrumental variables

Two instrumental variables were constructed to overcome the potential endogeneity of the state sector variable. In both cases the instrumental variable assigns to each individual based on his or her Soviet job characteristics (two digit industry sector and regional location in December 1986 or 1991) a probability of 'whether the enterprise where he/she used to work during Soviet times would eventually be privatized and become a private sector workplace'. These probabilities are cell probabilities within two-digit industry sector codes and regional location (only one of the instruments takes regional location into account).

The cell probabilities for the two instruments were calculated based on two different data sources in the following way: The first instrument (IV_privatized) was constructed using the detailed retrospective labour market information on individual privatization experiences taken from the ULMS dataset. In the first step a dummy variable was created identifying those jobs/firms among all jobs that started in the Soviet era, i.e. in December 1991 or earlier, and that were privatized. Reported episodes of privatization were only counted as privatized, if the majority of shares of the enterprise/organization were transferred into private ownership.[30] In a second step, the share of these privatizations within each of the 27 separate economic sector cells was computed. This categorization of industries into 27 economic sectors is more disaggregated than in the case of the second instrumental variable. However, the downside of this refined industry categorization is that regional variation cannot be taken into account since cell sizes would become too small.

In the case of the second instrument (IV_privateshare), the contemporary share of jobs in the private sector (i.e. all jobs other than jobs in the national/local government or state-owned enterprises) in each cell was calculated based on the pooled data from the UHBS. In this case, the cells are defined by 16 industrial sectors and five macro regions of Ukraine (i.e. 16x5 different cells).[31] Unfortunately, the UHBS data does not contain more disaggregated measures of industry affiliation. In contrast to the first instrumental variable which reflects the share of workplaces that were affected by privatization within each cell the second instrumental variable captures the share of

[30] This information is provided by the respondents in the survey.
[31] A few respondents in the ULMS survey were not living or working on Ukrainian territory in December 1986 or 1991. These respondents were assigned the 'private sector shares' calculated from the UHBS based on industry affiliation only (instead of industry-region affiliation).

private sector jobs within each cell. The correlation between the two instrumental variables is 0.75. A more detailed description of the construction and the calculated cell probabilities is provided in the appendix.

2.6 Empirical results: public sector satisfaction premium and self-selection of workers

At the core of the empirical analysis lies the estimation of a job satisfaction function of the following type: job satisfaction is explained by a binary variable indicating employment in the state sector, a set of individual present-day and Soviet time socio-demographic characteristics (X_i), job and workplace characteristics (J_i and Pl_i), and a normally distributed error term[32]:

$$JS_i = \beta_0 + \beta_1 state + X_i'\gamma + J_i'\theta + Pl_i'\delta + \varepsilon_i$$

Although the dependent variable job satisfaction is categorical, standard linear estimation techniques – assuming cardinality – will be applied and reported to simplify the interpretation of the coefficients (in particular, Random Effects Generalized Least Squares (GLS) and Generalized Two-Stage-Least Squares (G2SLS) for the pooled panel data sample; OLS and 2SLS for the sensitivity checks using the cross-section 2003 only).[33] Even if these estimates are less efficient than models taking into account the ordinal nature of the dependent variable, the GLS estimates are consistent (under classical assumptions).[34] Furthermore, whether estimating cardinal (e.g., OLS, GLS) or ordinal (e.g., ordered probit or logit) models generally does not affect the results substantially (Ferrer-i-Carbonell and Frijters 2004).

To determine whether state sector employees enjoy a significant satisfaction premium, the empirical analysis will proceed in the following steps: at first, the sector

[32] Since the main empirical analysis will employ panel data with repeated observations for participating individuals, the assumption of an independent and identically distributed error term is likely to be violated, since the individual error terms are correlated over time. In fact, in the panel data model the error term becomes $\alpha_i + \varepsilon_{it}$ (a time-invariant component and a time-varying component). In this case, the instrument has to be uncorrelated with this composite error term in order for the instrumental variable random effects model to generate consistent estimates of the regression parameters (Cameron and Trivedi 2005, chapter 22).

[33] The GLS estimation accounts for the fact that the panel data has repeated observations on each individual and, hence, standard errors are likely to be correlated within units of observation (applying Ordinary Least Squares might be inefficient in this case). Fixed effects estimations cannot be conducted, since the relevant instrumental variables are time-invariant.

[34] The qualitative GLS results hold when the equation is estimated by a more efficient Random Effect Ordered Probit model.

affiliation of a worker will be taken as exogenously determined. In order to help correct any bias stemming from composition effects, individual, pre-determined control variables will be successively added to the regression equation. Since the private and public sectors might also differ in their distributions of job and workplace characteristics which may be correlated with job satisfaction, further controls for occupation, working hours and firm size will be added. However, these variables could already capture 'rents' (e.g., when a particular amount of working hours is seen as job amenity) and could be endogenous if workers self-selected into specific occupations or firms. In a next step, average industry wages (average wages at the one-digit industry classification level; data are from the State Statistics Committee of Ukraine) as well as individual earnings will be added to the regression. Again, differential wage levels might be part of the 'satisfaction premium' and hence any change in the state sector coefficient accompanying the inclusion of wage information could be interpreted accordingly. However, individual income itself might also be endogenously determined (e.g., depending on positive or happy personality traits). Finally, further controls for potential job *disamenities* (shift work and weekend work) as well as several proxies aimed at capturing personality traits and risk aversion are added to the regression. Nevertheless, all specifications including these possibly endogenous variables should be interpreted cautiously.

In the second part of the analysis the state sector variable is no longer assumed to be exogenously determined, since workers may self-select into sectors based on unobservables which would lead to a correlation of the error term with sector affiliation. The existence of a binary endogenous regressor in addition to a limited dependent variable theoretically complicates the econometric approach (due to non-linearities). However, following the arguments by Angrist (2001), the estimation will rely on simple two-stage least squares techniques for panel data models using the constructed instruments, since the main aim is the estimation of the causal treatment effect. Furthermore, although the estimation of linear probability models (instead of probit models) in the first stage is less efficient, the estimated coefficients are consistent (Angrist 2001).

2.6.1 Basic GLS results

Table 2-3 reports the GLS-RE estimates of job satisfaction in Ukraine based on the pooled sample (2003-2007) for different specifications (the table shows only the most relevant coefficients; the full set of estimated regressors can be found in Table A 2-3 in the appendix). The estimated coefficients of the state sector dummy variable are reported in the first row. The first column reveals that the raw difference between satisfaction levels in both sectors is 0.340 (significant at the 1 percent significance level), indicating that public sector employees are significantly more satisfied with their jobs than their private sector counterparts. As expected, the inclusion of covariates to control for composition effects gradually reduces the size of the coefficient and leads to an improvement in the overall model fit (increasing R^2). More specifically, the inclusion of general socio-demographic background characteristics as well as pre-determined Soviet period variables, reduces the coefficient to 0.259 (Table 2-3, column 3). Even in the regression controlling for the maximum number of covariates, there remains a positive and highly statistically significant public sector satisfaction premium of 0.211 (Table 2-3, column 8). The standard errors remain essentially constant across specifications.

Moving from left to the right across the table, several interesting findings emerge. First, job satisfaction increases with educational attainment: individuals with higher education are significantly more satisfied with their jobs than persons with primary education only (base category). However, these significantly positive effects of more education vanish as soon as occupational information is added to the regression (column 4). This indicates that occupations are quite segregated in terms of educational achievement. Throughout all specifications the occupational dummy variables are highly significant. Furthermore, the higher the skill level of the occupation, the higher the return in terms of job satisfaction (professionals have a higher return than technicians or skilled-blue collar workers in comparison with the omitted category of basic, unskilled occupations). Job and workplace specific variables are also related to satisfaction levels (column 5): average job satisfaction is significantly lower among employees with less than 30 working hours in a typical week (i.e. part-time jobs) in comparison to employees with regular 40 hour jobs.

Table 2-3: Job satisfaction regressions with stepwise inclusion of covariates (GLS-RE estimates; selected coefficients; ULMS 2003-2007)

Dependent variable: Job satisfaction

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
State sector	0.340***	0.277***	0.259***	0.240***	0.217***	0.219***	0.212***	0.211***
	(0.043)	(0.042)	(0.043)	(0.042)	(0.043)	(0.041)	(0.041)	(0.041)
Male		-0.064	-0.038	-0.065	-0.073	-0.174***	-0.173***	-0.135*
		(0.062)	(0.066)	(0.066)	(0.065)	(0.063)	(0.063)	(0.076)
General secondary educ.		0.026	0.023	0.005	0.015	-0.005	-0.001	-0.004
		(0.076)	(0.076)	(0.077)	(0.078)	(0.075)	(0.075)	(0.075)
Professional sec. educ.		0.197**	0.121	0.025	0.033	-0.003	0.000	-0.001
		(0.081)	(0.083)	(0.085)	(0.086)	(0.084)	(0.083)	(0.084)
Higher education		0.413***	0.271***	0.041	0.050	0.018	0.019	0.012
		(0.088)	(0.094)	(0.103)	(0.103)	(0.101)	(0.101)	(0.101)
Chronic disease		-0.203***	-0.200***	-0.198***	-0.185***	-0.174***	-0.171***	-0.173***
		(0.037)	(0.037)	(0.037)	(0.037)	(0.036)	(0.036)	(0.036)
Urban settlement		0.135***	0.114**	0.086*	0.066	-0.003	-0.007	-0.005
		(0.052)	(0.052)	(0.052)	(0.052)	(0.051)	(0.051)	(0.051)
Professional occupation				0.583***	0.611***	0.423***	0.420***	0.412***
				(0.077)	(0.076)	(0.078)	(0.078)	(0.078)
Technical occupation				0.473***	0.459***	0.369***	0.364***	0.360***
				(0.075)	(0.075)	(0.073)	(0.073)	(0.074)
Skilled blue collar occ.				0.264***	0.250***	0.141***	0.138***	0.139***
				(0.054)	(0.054)	(0.053)	(0.053)	(0.053)
Less than 31 hours					-0.348***	-0.189***	-0.194***	-0.196***
					(0.065)	(0.065)	(0.065)	(0.065)
More than 50 hours					-0.086	-0.099	-0.077	-0.077
					(0.062)	(0.062)	(0.063)	(0.063)
Medium-sized firm					0.089	0.092	0.078	0.077
					(0.058)	(0.056)	(0.056)	(0.056)
Large-sized firm					0.131**	0.059	0.044	0.044
					(0.058)	(0.057)	(0.057)	(0.057)

Table 2-3 (continued);
Dependent variable: Job satisfaction

Log earnings						0.392***	0.396***	0.393***
						(0.047)	(0.048)	(0.048)
Log ave. industry wage						0.190***	0.175**	0.173**
						(0.073)	(0.074)	(0.074)
Weekend work							-0.081**	-0.080**
							(0.039)	(0.039)
Shift work							0.061	0.064
							(0.053)	(0.053)
Extrovert								0.078*
								(0.047)
Neurotic								-0.158
								(0.106)
Never smoked								0.053
								(0.057)
Risk loving								0.026
								(0.052)
Constant	3.029***	2.222***	2.498***	2.285***	2.338***	-1.586*	-1.484	-1.511
	(0.058)	(0.621)	(0.881)	(0.873)	(0.870)	(0.960)	(0.962)	(0.961)
Chi-squared	194.7	475.1	517.2	618.0	653.2	859.4	860.5	879.7
Region FE, age & birth place, marital status	-	✓	✓	✓	✓	✓	✓	✓
Soviet period controls	-	-	✓	✓	✓	✓	✓	✓
R-squared overall	0.0430	0.106	0.113	0.133	0.144	0.185	0.186	0.187

Notes: Dependent variable: Five-point Likert scale of job satisfaction with 1 (fully dissatisfied) to 5 (fully satisfied). Omitted categories: ‘primary education’, ‘single’, ‘occupation: unskilled’, ‘working hours: 31 to 50 hours’, ‘small-sized firm’, ‘village/rural settlement’. All regressions control for year and interview month fixed effects. Columns (2) – (8) furthermore control for individual height, parental education to account for family background characteristics; however, none of these variables has a significant effect on job satisfaction. A more comprehensive table reporting further coefficients is presented in the appendix to this chapter (Table A 2-3). Number of observations 4,191 (Number of id: 1,915). Standard errors are clustered on the individual level. Robust standard errors are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1; Source: ULMS 2003-2007; own calculations.

The inclusion of individual earnings and average industry wages (column 6) – both of which are positively associated with job satisfaction and are highly statistically significant – generates two surprising effects: the first is that the coefficient of the state sector dummy increases slightly. Thus, controlling for the wage levels increases the satisfaction gap. If sector satisfaction differentials simply reflected wage differentials one would expect the opposite effect. This also implies that public-private wage differences are not the main driving force behind the observed public sector job satisfaction premium. The second surprising effect is that gender differences become larger and significant only now, implying that the structurally higher wages for men hid a substantial gender gap in job satisfaction. In other words, the Ukrainian data reveal a gender gap in line with the 'contented female worker' phenomenon once differences in earnings are controlled for. Such a positive utility premium for women has been found in several Western countries and attracted a large literature (e.g., Clark 1997; Sousa-Poza and Sousa-Poza 2000; Bender, Donohue and Heywood 2005).

Furthermore, the average industry wage variable can also be interpreted as a crude measure for relative or reference group income which has often been found to be negatively correlated with job satisfaction in Western market economies (i.e., holding constant the individual income level, job satisfaction decreases with increasing levels of comparison income) (Clark and Oswald 1996; Clark, Frijters and Shields 2008). However, the positive coefficient of this variable in Table 2-3 is in line with evidence on a positive effect of comparison income on life satisfaction in Russia (Senik 2004). Senik (2004) explains these surprising findings as a 'tunnel effect': in economically difficult and uncertain times, people interpret wage levels of others as signals for their own future (information content).

Turning to the two measures of job disamenities, it seems that only weekend work has a significantly negative effect on job satisfaction. Last but not least, column 8 presents the results when including the four different proxies for personality traits and risk aversion. However, while all estimated coefficients have the expected sign, only the estimate on extroverted types is significantly positive. Job satisfaction is also significantly lower among chronically ill, singles and persons who live in households where children are present (these last results are not reported in Table 2-3, but can be found in the more extensive Table A 2-3 in the appendix).

To test whether these estimated patterns are robust or specific to the particular estimation sample or estimation technique, the same set of specifications was re-estimated based on the cross-section 2003 only (using OLS) as well as on an extended sample (using GLS-RE) in which the sample size was increased by dropping some variables with many missing values. The results of these additional regressions are very close to the estimates in Table 2-3 (results are reported in Table A 2-4 in the appendix).

2.6.2 Results from the instrumental variable regressions (G2SLS-RE regressions)

The results from the instrumental variable regressions in Table 2-4 help to better understand to what extent the previous findings are driven by a non-random self-selection of workers into particular sectors. For ease of comparison, the upper panel A of the table restates the corresponding GLS-RE results from the previous section (no correction for self-selection). In order for the simple GLS-RE model to produce consistent estimates, it is crucial that the composite error term $\alpha_i + \varepsilon_{it}$ (a time-invariant component and a time-varying component) and in particular α_i be uncorrelated with the regressors (see also footnotes 32, 33). In the presence of self-selection based on fixed individual personality traits and preferences, however, this assumption is likely to be violated (omitted variable bias). However, the instrumental variable approach aims at correcting for this omitted variable bias. The corresponding results of these generalized two-stage least squares random effects estimations (G2SLS-RE panel data models) are shown in panel B, while the two lower panel report the reduced form and the first stage regression estimates. Three specifications from Table 2-3 were re-estimated using each instrumental variable separately: the simple raw mean comparison without any controls, the specification controlling for all pre-determined and exogenous individual characteristics and the specification controlling for the full set of variables, including wages and job and firm specific variables. These specifications correspond to columns 1, 3 and 8 from Table 2-3 respectively. In all regressions standard errors are clustered on the individual level.

For both instruments, the first stage results at the bottom of Table 2-4 point to a strong association between the instrumental variables and the endogenous binary state sector variable. The z- and the Chi-squared-statistics (which correspond to the t- and F-statistics in simple 2SLS estimations) have high values indicating a substantial predictive power of the instrument.[35] The significantly negative effects confirm that a higher probability of privatization or having been employed in a Soviet industry that was less likely to remain dominated by state ownership reduces the likelihood of working in the state sector in Post-Soviet Ukraine. The estimated first stage coefficients (and the standard errors) using the first instrument (privatization) are almost twice as large as the estimates based on the second instrument (private sector share). This can be explained by the fact that the second instrument has a much larger variation (double in size): its values range from about 0.00 to 0.96 while the values of the first instrument (privatization) range only from about 0.00 to 0.48.

The estimates of the reduced form regressions of job satisfaction on the instrumental variables are also highly significant across all specifications and show the expected sign. Conditional on individual pre-determined characteristics as well as on job and workplace characteristics, workers who used to work in Soviet industries (industries and regions) in which more firms were transformed into private ownership due to privatization (instrument 1) or general restructuring (instrument 2) report lower levels of job satisfaction.

Turning to the upper part of Table 2-4, the results of the G2SLS-RE estimations confirm that the public sector satisfaction premium found in the simple GLS-RE regressions persists even after correcting for self-selection into sectors.[36] More precisely, the estimated coefficients of the state sector dummy variable are more than double in size than the GLS-RE estimates and significantly positive on the 1 percent significance level throughout almost all three specifications (this is true for both instrumental variables). However, standard errors are also much larger in the instrumental variable regressions than in the simple regressions (panel A.). Given these large standard errors, the G2SLS-RE are generally not significantly different from the GLS-RE estimates (the GLS-RE coefficients of columns 2, 3, 5 and 6 lie within the 95%-confidence interval of the G2SLS-RE estimates).

[35] See Stock, Wright and Yogo (2002) and Angrist and Pischke (2009) on the problem and test of weak instruments (as rough benchmark they mention 10 as a minimum critical F-value of the instrument in the first stage).

[36] These results also hold when re-estimating these instrumental variable regressions using the cross-section from 2003 only or the more extended sample (see Table A 2-5 in the appendix). In only one case (cross-section, instrument 1, specification in column 2) does the estimated coefficient become insignificant (the p-value is 0.156), but this is probably due to loss in precision because of the smaller sample size (in conjunction with generally larger 2SLS standard errors).

Table 2-4: Regression results (GLS-RE, G2SLS-RE, Reduced Form, First Stage)

	Instrumental variable 1: Privatization probability			Instrumental variable 2: Private sector share		
	(1)	(2)	(3)	(4)	(5)	(6)
A. GLS-RE	*DV: Job Satisfaction*			*DV: Job Satisfaction*		
State sector	0.340***	0.259***	0.211***	0.340***	0.259***	0.211***
	(0.043)	(0.043)	(0.041)	(0.043)	(0.043)	(0.041)
B. G2SLS-RE	*DV: Job Satisfaction*			*DV: Job Satisfaction*		
State sector	0.687***	0.391**	0.485***	0.940***	0.548***	0.515***
	(0.160)	(0.186)	(0.185)	(0.131)	(0.148)	(0.159)
C. GLS-RE Reduced form	*DV: Job Satisfaction*			*DV: Job Satisfaction*		
Instrumental variable	-0.827***	-0.414**	-0.501***	-0.475***	-0.249***	-0.207***
	(0.187)	(0.188)	(0.179)	(0.063)	(0.067)	(0.064)
D. GLS-RE First Stage	*DV: State sector (binary variable)*			*DV: State sector (binary variable)*		
Instrumental variable	-1.207***	-1.066***	-1.036***	-0.504***	-0.456***	-0.403***
	(0.070)	(0.074)	(0.071)	(0.023)	(0.024)	(0.023)
Z-statistic	-17.18	-14.42	-14.66	-22.08	-18.64	-17.21
Chi^2-statistic [A)]	161.1	100.5	110.8	290.9	183.0	170.8
Socio-demographic Post-Soviet & Soviet characteristics (incl. Soviet job)	-	✓	✓	-	✓	✓
Current job & firm characteristics (incl. log of earnings & average industry wages)	-	-	✓	-	-	✓
Personality traits, risk aversion	-	-	✓	-	-	✓
Observations	*4,191*	*4,191*	*4,191*	*4,191*	*4,191*	*4,191*

Notes: Dependent variable (DV) in A.-C.: Job satisfaction. DV in panel D.: State sector (binary variable). The table reports only the estimated coefficients of interest (state sector; instrumental variables) from the following regression specifications: columns (1) and (4) correspond to the specification in Table 2-3, column (1); columns (2) and (5) correspond to Table 2-3, column (3); columns (3) and (6) correspond to Table 2-3, column (8). [A)] The Chi-squared test statistic from the weak instrument test in the first stage regression are calculated from a separate first stage regression in which results differ slightly from the first stage results based on the Stata build-in xtivreg command (reported coefficient and z-statistic in the table above). However, the Chi-squared test statistic cannot be computed using the xtivreg command. Standard errors are clustered on the individual level; robust standard errors are shown in parentheses; *** p<0.01, ** p<0.05, * p<0.1; Source: ULMS 2003-2007; own calculations.

In the specifications that do not control for earnings or industry wages (columns 2 and 4), the estimated public-private satisfaction gap is about 0.39 for the privatization instrument and 0.55 for the private sector-share instrument which corresponds to roughly one third and half of a standard deviation of job satisfaction respectively (standard deviation 1.18). Using the coefficients based on the full set of covariates (columns 3 and 6), the estimated public sector satisfaction premium takes the size of about 42 – 44 percent of a standard deviation.

What can be inferred from these results about the process of self-selection? First of all, since the estimated coefficients increase after correcting for self-selection, it seems that the GLS-RE estimates were probably downward biased and thus underestimated the true public sector differential. This in turn implies that the positive satisfaction gap was not merely caused by a spurious correlation between state sector affiliation and individuals with inherently higher satisfaction levels. Secondly, the sorting and matching of worker across sectors has led to a reduction in the satisfaction gap, since the observed gap in job satisfaction between private and state workers (GLS-RE) is even larger under random assignment (G2SLS-RE). This sorting process was furthermore such, that workers from the lower part of the satisfaction distribution self-selected into the public sector and workers with higher intrinsic levels of job satisfaction sorted into the private sector (negative selection into the public sector). Interestingly, these results are in line with those found by Luechinger, Stutzer and Winkelmann (2010) for Germany. In the context of an emerging private sector in Ukraine it could be that more extroverted and optimistic individuals discover their entrepreneurial talents and sort into the private sector. This would be in line with the literature on public-private wage differentials in Western economies which suggests that high ability and very productive workers might find it beneficial to exit the public sector as the wage distribution is typically more compressed in the public than in the private sector (Gregory and Borland 1999). Accordingly, those who are unhappy in the private sector and cannot or are unwilling to cope with the new market structure and risks, self-select into the more stable (and possible more traditional) state sector. In both cases, the self-selection process increases the individual degree of job satisfaction if the new jobs generate a better match.

What can be learnt about the role of equalizing differentials in total compensation in Post-Soviet Ukraine? Following the theoretical considerations in Sections 2.2 and 2.3, the estimated satisfaction premium of state sector employees indicates that a larger fraction of workers in the private than in the public sector is unsatisfied with their jobs. Against the background of a continuously shrinking state sector this satisfaction gap potentially points to an increasing ‘shortage’ of public sector jobs, in the sense that a growing fraction of private sector workers would be better off in the public sector (‘queuing’). In this case the satisfaction premium would indeed represent a ‘rent’ that one and the same person received by working in the state rather than the private sector. If total remuneration in the public (private) sector was reduced (increased) this should lower the satisfaction differential.

2.6.3 Sensitivity and robustness checks

In order to test the strength and reliability of the main findings several sensitivity and robustness checks were carried out. The results of these additional regressions are reported in Table 2-5 (the table reports only the estimated state sector coefficients from the instrumental variable regressions).

2.6.3.1 Excluding recent privatizations

For each of the variables z_i to be valid instruments, two critical identifying assumptions have to hold: the instrument has to be sufficiently correlated with the endogenous variable $Cov(z_i, state) \neq 0$ (to prevent a weak instrument problem) while being uncorrelated with the error term $Cov(z_i, \varepsilon) = 0$ (see discussion in Section 2.3.2). This implies that the instrumental variable should affect the outcome of interest (job satisfaction) solely through its effect on the endogenous variable (state sector affiliation). In other words, having worked in a Soviet industry that was more likely to be affected by privatization or restructuring during the transition process should affect post-transition job satisfaction only through the fact that certain individuals have a higher probability of working in a privately owned enterprise, i.e. the privatization or restructuring process should have no additional, direct effect job satisfaction.

Although it is not possible to actually prove this claim, it is possible to provide suggestive evidence in support of the instruments. First, in contrary to the general public notion that privatization hurts workers, Brown, Earle and Vakhitov (2006) and Brown, Earle and Telegdy (2010) show for Ukraine and other transition countries that privatization did not lead to job losses and had either only very small or no effects on wages (in fact, in some cases privatization even caused an increase in firm employment and wages). Second, managerial turnover (on higher management levels) that could potentially trigger unpleasant organizational change or a general restructuring of firms and thus might lower job satisfaction was actually *less common* in de novo or privatized firms than in state-owned enterprises (see Warzynski 2003 using a sample of 300 Ukrainian firms in 1997). Warzynski (2003) explains this finding by the fact that the particular privatization process in Ukraine predominantly led to insider ownership (by workers and managers). Third, general changes in the economic and political system during the transition process, e.g., more competitive pressure from foreign firms, liberalization of consumer prices, affected all employees in Ukraine. Debardeleben (1999) analyses the attitudes of Russians towards the privatization process based on Russian survey data from 1993 to 1997. Surprisingly, individuals with personal privatization experiences are neither more nor less supportive of the privatization process and market liberalizations than persons without such experiences. The author concludes that disillusion with the political process and market transition as a whole led to a general negative assessment of single transition measures in the population. If similar mechanisms have taken place in Ukraine this would furthermore support the notion that privatization in itself did not affect satisfaction levels of individuals.

In addition to these arguments in support of the general validity of the instruments, it is possible to perform a simple test on whether the main estimation results are driven by recent – and possibly unpleasant – privatization experiences of some ULMS participants. Since the ULMS collects information on whether respondents ever experienced privatization of their workplace and if so, when, it is possible to identify individuals whose workplace has been privatized in the recent past. If privatization has a direct negative effect on job satisfaction, it seems reasonable to assume that this effect should be strongest immediately around or after the date of privatization and should gradually fade out over time. This is why the main G2SLS-RE regressions from Table 2-4 are repeated after *dropping* all observations that have experienced privatization within the last four years before the survey interview. As can be seen in Panel A in Table 2-5 the estimated state sector premium does not diminish after excluding these recent privatizations. In contrary, the estimates become even slightly larger thereby giving further support to the claim that the main regression results are not simply driven by possibly negative privatization experiences of workers.[37]

[37] These results hold also when excluding only individuals having experienced privatization within the last, the last two or the last three years (results not reported).

Table 2-5: Sensitivity and robustness checks: Instrumental variable estimates (G2SLS-RE)

	Instrumental variable 1: Privatization probability			**Instrumental variable 2: Private sector share**		
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	*Job satisfaction*			*Job satisfaction*		
Original G2SLS-RE regression results (Table 2-4; N=4,191)						
State sector	0.687***	0.391**	0.485***	0.940***	0.548***	0.515***
	(0.160)	(0.186)	(0.185)	(0.131)	(0.148)	(0.159)
Sensitivity and robustness checks						
A. Excluding privatizations in last 4 years (N=4,134)						
State sector	0.708***	0.430**	0.512***	0.947***	0.562***	0.523***
	(0.161)	(0.189)	(0.189)	(0.133)	(0.150)	(0.163)
B. Excluding 'Soviet social jobs' (N=3,465)						
State sector	0.384	0.456*	0.459**	0.864***	0.639***	0.478**
	(0.262)	(0.262)	(0.230)	(0.194)	(0.201)	(0.198)
C. Using only industry stayers (N=2,309)						
State sector	0.532***	0.360**	0.474***	0.806***	0.585***	0.595***
	(0.138)	(0.161)	(0.149)	(0.112)	(0.126)	(0.129)
D. More refined education measure (N=4,191) [A)]						
State sector	0.687***	0.383**	0.479***	0.940***	0.546***	0.513***
	(0.160)	(0.187)	(0.186)	(0.131)	(0.148)	(0.160)
E. Dependent variable: Satisfaction with job security (N=2,448) [B)]						
State sector	0.923***	0.719***	0.794***	1.029***	0.676***	0.616***
	(0.177)	(0.213)	(0.219)	(0.148)	(0.171)	(0.185)
Socio-demographic Post-Soviet and Soviet characteristics	-	✓	✓	-	✓	✓
Current job and firm characteristic (incl. earnings & average industry wages)	-	-	✓	-	-	✓
Personality traits, risk aversion	-	-	✓	-	-	✓

Notes: Dependent variable: Five-point Likert scale of job satisfaction with 1 (fully dissatisfied) to 5 (fully satisfied). The table reports only the estimated coefficients of interest (state sector; instrumental variables) from the following regression specifications: columns (1) and (4) correspond to the specification in Table 2-3, column (1); columns (2) and (5) correspond to Table 2-3, column (3); columns (3) and (6) correspond to Table 2-3, column (8). A) To better capture possible differences in skills and ability, a more refined measure of education is included in the regression: a set of 11 dummy variables for the highest educational attainment (from 'grades 1-6' to 'Candidate of Science/Post-doctoral lecture qualification'). B) The question on 'satisfaction with job in terms of job security' was only asked in the survey years 2004 and 2007, not in 2003, leading to a smaller sample size. The dependent variable 'satisfaction with job in terms of job security' in panel E. is a categorical variable based on a five-point Likert scale (as in job satisfaction). Standard errors are clustered on the individual level; robust standard errors are shown in parentheses; *** p<0.01, ** p<0.05, * p<0.1; Source: ULMS 2003-2007; own calculations.

2.6.3.2 Excluding 'social Soviet jobs'

Another possible threat to the validity of the instrumental variables would exist if Soviet workers actually *could* and *did* self-select into specific occupations and industries and if particular types of individuals (happy types, unobservable traits) were attracted to 'typical Western' public sector jobs even in the Soviet Union (i.e. public administration, education, health). However, in this context it is important to recall that during the Soviet time employees in the public administration, education or health care system (typically dominated by white-collar occupations) did not benefit from additional job amenities (like job security) which are typically attributed to public sector jobs in Western market economies vis-à-vis workers in the manufacturing or construction sector. Hence, any self-selection of workers into 'typical' public sector industries during the Soviet period on the basis of job characteristics which are commonly ascribed to Western public sector jobs (e.g. job amenities, job security) can be excluded. In fact, in the 1980s skilled manual workers received an official wage premium thereby earning *more* than managers and some professionals (Gerber and Hout 2004). Furthermore, apart from higher salaries, Soviet workers in blue-collar occupations generally benefited from better working conditions and incentives – much in line with Soviet ideology and its emphasis on manual labour (Zajda 1980). This ideologically motivated preferential treatment of blue collar workers (i.e. not 'typical public sector' jobs) seems to work against the idea that Soviet workers selected into 'typical public sector' industries due to beneficial job attributes.

However, to account for possible differences in skills and ability of blue- and white collar Soviet workers the main regressions in Table 2-4 already controlled for white or blue-collar Soviet occupations. As a further step, the sensitivity check in Panel B of Table 2-5, excluded all individuals who used to work in 'typical social' jobs during the Soviet time to find out whether the state sector satisfaction premium is merely caused by socially oriented personality types. The problem with this approach is that it is theoretically possible that people enjoy doing social or service work and that this type of job content might actually represent job amenities leading to higher job satisfaction. One would need extremely large sample sizes and a sufficient share of public and private sector jobs in each industry to learn more about within industry differences between public and private sector workers. Individuals are classified as 'Soviet social job' holders based on their Soviet 4-digit ISCO occupational codes whenever these are related to education, health and safety services, social and social security workers (overall 58 different occupations). Excluding these observations from the estimation tends to reduce the estimated public sector premium coefficient as well as the significance level slightly (especially in the regressions using the privatization instrument). However, the previous findings of a significantly positive job satisfaction premium of state sector workers are confirmed in these regressions, suggesting that the positive effect of working in the public sector is not driven by socially oriented worker types.

2.6.3.3 Using only 'industry stayers'

By assigning the privatization probabilities according to Soviet industry or industry-region affiliation, the instrumental variables should be more powerful for those individuals who currently work in the same industry as they used to do during the Soviet period (workers might have changed jobs and firms, but remain in the same industry). Since this subsample of workers might generally differ from those workers who have switched industries, it is problematic to run the regression on this selected sample. Nevertheless, this exercise should help to test whether the instrument is 'functioning' in the right way, i.e. whether results become more precise even though the sample size is diminished. The results in Panel C of Table 2-5 show that this is indeed the case which is reassuring: while the point estimates change slightly in size (become slightly smaller/larger using instrument 1/2), the standard errors become smaller and the estimated coefficients are all highly significant.

2.6.3.4 More refined education measure

The original regressions distinguished only between four levels of education in order to limit the number of included variables (primary education, general secondary education, professional secondary education, and higher education). However, to better capture possible differences in skills and ability the main regressions were re-estimated using a more refined measure of education by including a set of dummy variables for 11 different possible educational attainments (from 'grades 1-6' to 'Candidate of Science/Post-doctoral lecture qualification'). As Panel D of Table 2-5 demonstrates, the main results remain virtually unchanged and are not sensitive to the type of education measure used in the regressions.

2.6.3.5 Satisfaction with job security

In order to account for the possibility that especially risk averse individuals are attracted to the public sector the main regressions included several proxies like subjective assessment of one's own willingness to take risks or an indicator for risky behaviour (i.e. smoking). This hypothesized self-selection of risk-averse individuals into the public sector implicitly assumes that state sector jobs are (at least perceived) more secure than private sector jobs. The ULMS waves 2004 and 2007 contain an additional job satisfaction question which provides a subjective measure of job security: Tell me, please, how satisfied are you with your current job in terms of job security? There are five answer possibilities to the question ranging from fully dissatisfied to fully satisfied. Panel E of Table 2-5 reports the effect of working in the state sector on this measure of satisfaction with job security. Across all specifications and instrumental variables the estimated effects are strikingly large (larger than in the job satisfaction regressions) and positively significant implying a substantial satisfaction premium in terms of job security in the state sector (even after correcting for self-selection into the public and private sector). The coefficients range between 0.54 and 0.90 of a standard deviation. These results seem to indicate that public sector jobs in the Ukraine are indeed perceived as more secure than private sector jobs and that this job facet might be a part of the overall public sector job satisfaction premium. This finding of higher satisfaction with job security among public sector workers is in line with the evidence shown by Clark and Postel-Vinay (2009) using data from the European Community Household Panel for 12 European countries. Another relevant finding of their analysis is that while perceived job security in the private sector seems to be influenced by the extent of granted Unemployment Insurance Benefits and the level of Employment Protection, there appear to be no such associations among public sector workers.

2.7 Empirical results: the role of monetary and non-pecuniary fringe benefits

There could be many reasons why average job satisfaction levels differ between the state and the private sector (e.g., the possibility that workers value job security has been discussed in the previous section). One potential driver might be utility rents generated from differences in job amenities. Since the ULMS asks respondents a long battery of questions on whether the employer provides social benefits and financial incentive schemes, it is possible to shed more light on this channel.

As regards the ULMS information on social benefits, there are 13 different possible answers (multiple answers are also allowed) to the question: *In this job do employees receive any of the following social benefits?* An advantage of this general question is that it is informative about the workplace and not about whether a particular person receives benefits during a particular period of time. This way, part of the individual level endogeneity problem (if there is individual heterogeneity in benefit receipt *within* firms) is alleviated, although it is still possible that workers sort into particular enterprises offering these types of job amenities. In order to reduce the dimensionality and complexity of the information, the 13 different social benefit types were grouped into three categories: social security (covering, e.g., regular paid vacation, paid sick leave), subsidies (among others free child care, discounted food, transportation or housing subsidies), and training (human capital enhancing measures like paid training or payment for trips to sanatoria).[38]

The information on financial benefits is based on the question whether respondents received any amount of money in addition to their regular salary in the past year. Since receipt of this additional money is measured on the individual level the problem of endogeneity is more pronounced again. Three different types of payments are distinguished: non-performance related benefits (i.e. 13th salary), performance related benefits (bonus payments or profit-sharing payment schemes) and risk compensation (compensation for non-normal work conditions).

A first descriptive analysis in Table 2-6 demonstrates that both, social benefits as well as financial benefits are much more common in the state than in the private sector. For instance, almost all state sector employees (97 percent) report that their workplace provides them with at least one of the following social security type of benefits: regular paid vacation, paid sick leave, paid maternity leave/child care leave, coverage of health related expenses (or treatment in enterprise polyclinic). The corresponding figure for the private sector is much lower (only 62 percent). The same pattern is true for the provision of training or subsidy type of benefits, like discounted food, transportation and housing subsidies, partial payment of child care, etc. It is important to note that these types of social benefits were generally available to *all* workers during the Soviet period – irrespective of their industry or occupation. Against this background the Post-

[38] A more comprehensive description of these variables is provided in the appendix.

Soviet discrepancy in the provision of these benefits between the state and the private sector is even more striking.

Table 2-6: Incidence and differences in social benefits and payment incentive schemes between private and state sector

Variable	Private sector (1) Mean	State sector (2) Mean	Difference (1)-(2)	t-statistic
Social benefits				
SB: Social security	0.62	0.97	-0.34***	-30.90
SB: Subsidies	0.17	0.33	-0.16***	-11.74
SB: Training	0.11	0.28	-0.17***	-14.12
Financial incentive pay scheme				
FB: Non-performance related	0.06	0.14	-0.08***	-9.03
FB: Performance related	0.18	0.27	-0.10***	-7.38
FB: Risk compensation	0.02	0.06	-0.03***	-5.55

Source: ULMS 2003-2007; own calculations. Number of observations: 2,035 (private sector), 2,156 (state sector). Pooled sample of all individuals in working age during time of interview with full information on all variables, who were 16 or older in December 1991 and for whom information on their job during the Soviet time (December 1986 or December 1991) exist. Reported t-statistics refer to mean comparison tests between state and private sector.

To analyse whether the estimated public job satisfaction premium can be explained by the variation in benefits across sectors, the GLS-RE and G2SLS-RE regressions from Section 2.6 are further extended by including sets of dummy variables for different types of benefits successively (only social benefits, only financial benefits, both types of benefits). Several interesting findings emerge: firstly, adding controls for the provision of fringe benefits reduces the size of the estimated GLS-RE coefficients of the state sector dummy variable (Table 2-7, columns 1 to 3). Recall that the GLS-RE state sector coefficient in the most elaborate regression specification was 0.211 and it drops to 0.141 once all types of benefits are included in the regression. In the GLS-RE regressions, the coefficients on social benefits as well as pay schemes show the expected positive sign and are significantly different from zero. However, despite the inclusion of these variables, there remains an unexplained positive satisfaction gap. Secondly, re-running the regression by G2SLS-RE, reveals the same pattern: the estimated coefficients drop in size, but stay significant. Interestingly, the positive effect of social security type benefits on job satisfaction becomes insignificant in the G2SLS-RE regression. This might be driven by the almost universal coverage of these types of benefits in state sector jobs.

To sum up, the public-private job satisfaction differential can be partly related to the different levels of provision of social and financial fringe benefits in the two sectors. However, the explanatory power of these non-wage components of total compensation is limited, since there remains a significant public sector premium even after controlling for these types of benefits. Apparently, there are further aspects related to state sector employment that make employees in state owned enterprises/organizations better off than their private sector counterparts. For instance, such aspects could be related to sector differences in job and time flexibility (including formal as well as informal rules, e.g. specific firm culture and attitudes), risk of job loss (see Section 2.6.3.5) or differences in wage compression across sectors, which would matter if individuals cared about wage inequality and relative wages within firms.

Table 2-7: The role of fringe benefits; GLS-RE and G2SLS-RE estimates; Dependent variable: Job Satisfaction

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		GLS-RE			**G2SLS-RE, IV 1: Privatization probability**			**G2SLS-RE, IV 2: Private sector share**	
State sector	0.154***	0.187***	0.141***	0.453**	0.471**	0.452**	0.480***	0.496***	0.472***
	(0.044)	(0.042)	(0.044)	(0.199)	(0.188)	(0.200)	(0.170)	(0.163)	(0.172)
Social benefits									
Social security	0.159***		0.144**	0.054		0.037	0.045		0.030
	(0.060)		(0.060)	(0.090)		(0.089)	(0.082)		(0.082)
Subsidies	0.073*		0.054	0.058		0.042	0.057		0.041
	(0.044)		(0.044)	(0.045)		(0.045)	(0.045)		(0.045)
Training	0.140***		0.118**	0.112**		0.092*	0.110**		0.091*
	(0.046)		(0.047)	(0.050)		(0.050)	(0.050)		(0.049)
Financial benefits									
Non-performance rel.		0.151***	0.124**		0.112*	0.094		0.109*	0.093
		(0.058)	(0.058)		(0.065)	(0.063)		(0.063)	(0.062)
Performance related		0.156***	0.136***		0.145***	0.133***		0.144***	0.133***
		(0.042)	(0.042)		(0.043)	(0.043)		(0.043)	(0.043)
Risk compensation		0.155*	0.133		0.119	0.100		0.116	0.099
		(0.082)	(0.082)		(0.089)	(0.089)		(0.088)	(0.088)
Observations	4,191	4,191	4,191	4,191	4,191	4,191	4,191	4,191	4,191
Full set of controls	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: Dependent variable: Five-point Likert scale of job satisfaction with 1 (fully dissatisfied) to 5 (fully satisfied). Full set of controls include all control variables from Table 2-3, column (8). Standard errors are clustered on the individual level. Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1; Source: ULMS 2003-2007; own calculations.

2.8 Conclusions

This chapter analysed differences in job satisfaction between public and private sector employees in Post-Soviet Ukraine. The main research question asked whether the raw public-private sector satisfaction gap observed in Ukraine reflects genuine differences in job characteristics (i.e. rents in the public sector) or whether it is driven by self-selection of workers according to unobserved heterogeneity (i.e. personality types). The present study is the first to address this particular research question using instrumental variable techniques. The identification strategy rests on a quasi-natural experiment generated by the fundamental changes in ownership of enterprises during the transition process, during which workers were randomly reallocated from state into private sector jobs.

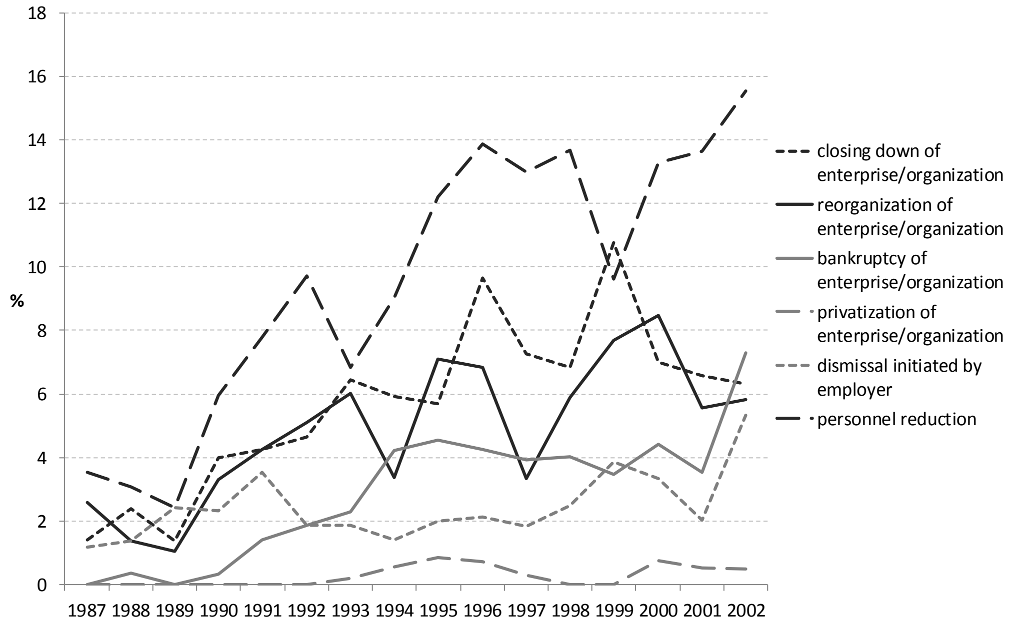
The empirical results reveal that the significantly positive public sector satisfaction premium in the naïve GLS-RE regressions holds even after correcting for self-selection. In fact, accounting and correcting for the non-random sorting into state and private sector in the G2SLS-RE regressions leads to an increase in the size of the gap. These findings indicate a possible downward bias in the GLS-RE estimates, suggesting a negative selection of workers into the public sector. The same pattern of negative selection has been found for Germany using structural switching regression models (Luechinger, Stutzer and Winkelmann 2010). However, the results are in contrast with the conclusions from Heywood et al. (2002), who find a positive selection into public sector employment in the UK based on panel fixed effects estimations. The fact that the public-private satisfaction gap is even larger under random allocation (G2SLS-RE) suggests that sorting of workers led to an equalization of job satisfaction levels between the public and the private sector in Ukraine.

Against the background of the theory of equalizing differentials, the regression results point to a significant satisfaction premium or rents of public sector employees in Ukraine which in turn could be related to queues for state sector employment. Assessing the potential drivers of the satisfaction differential reveals that a certain fraction of the state sector premium can be explained by different levels of fringe benefits in the two sectors. However, as the significant public sector satisfaction premium remains even after controlling for fringe benefits and self-selection, the open question is as to which additional factors might explain the gap. For example, there could be sector differences in job flexibility which in turn could affect the ability to combine working with private life (family obligations). Another explanation could be differences in perceived job and income uncertainty in the two sectors. In absence of functioning financial markets public sector employment could act as insurance mechanism (Rodrik 2000). Furthermore, public sector rents could be related to unofficial payments or bribes to public sector employees (Luechinger, Meier and Stutzer 2008). Such a hypothesis would find empirical support from the study by Gorodnichenko and Sabirianova Peter (2007) which demonstrates significant levels of unofficial payments to public sector employees in Ukraine. Nevertheless, having established the causality of the public sector job satisfaction premium and having investigated one potential source of satisfaction difference, i.e. fringe benefits, the study of other relevant job attributes and differences creating the public sector satisfaction premium remains for future research.

2.9 Appendix to Chapter 2

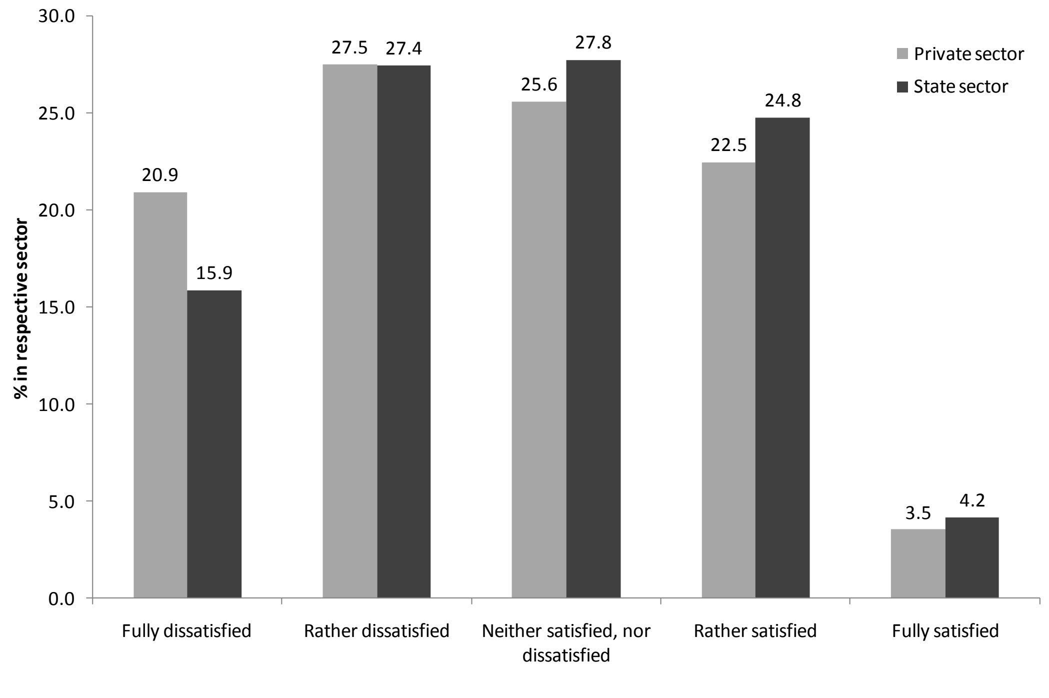
Appendix A

Figure A 2-1: Reason for end of job held in December 1986 (or 1991)



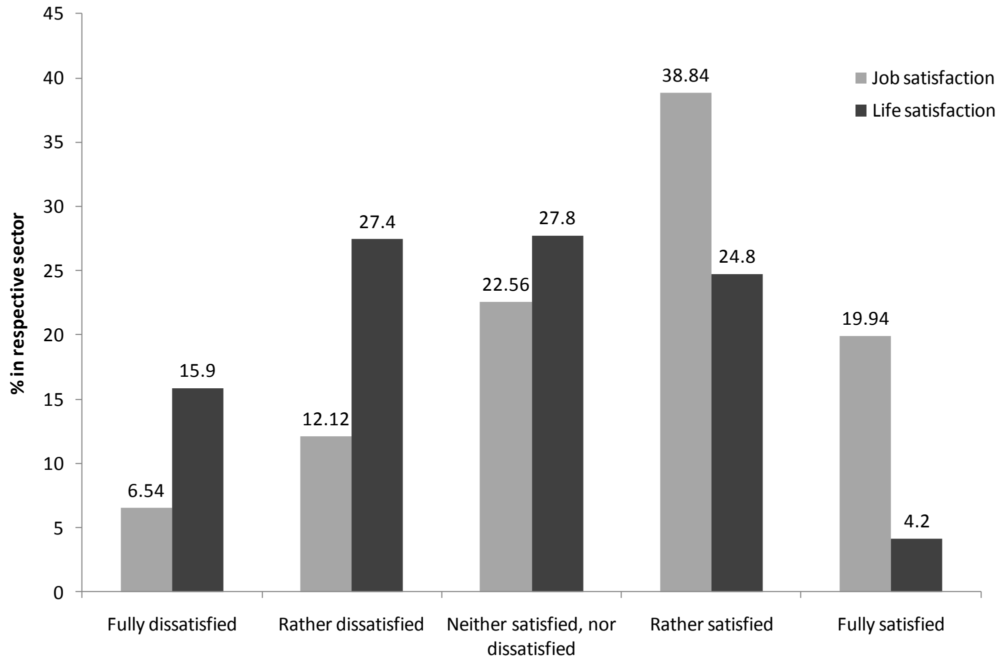
Source: ULMS 2003, own calculations based on full sample with retrospective information.

Figure A 2-2: Life Satisfaction in the Private and Public Sector in Ukraine based on regression sample (ULMS 2003-2007)



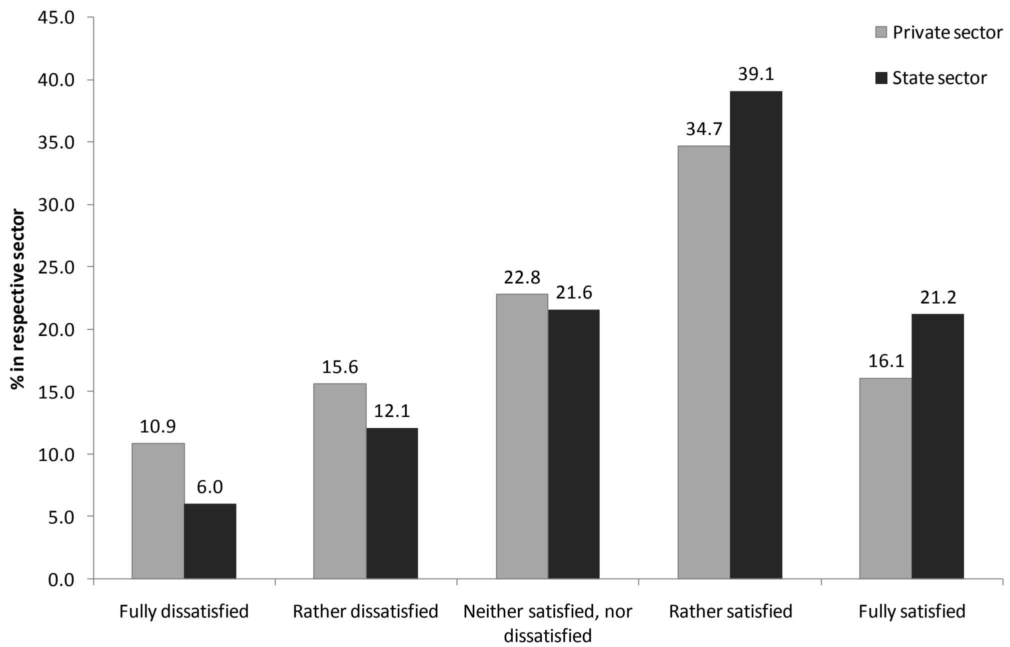
Source: ULMS 2003-2007, regression sample with non-missing life satisfaction (N=5,083); own calculations.

Figure A 2-3: Job and Life Satisfaction State Sector in Ukraine based on regression sample (ULMS 2003-2007)



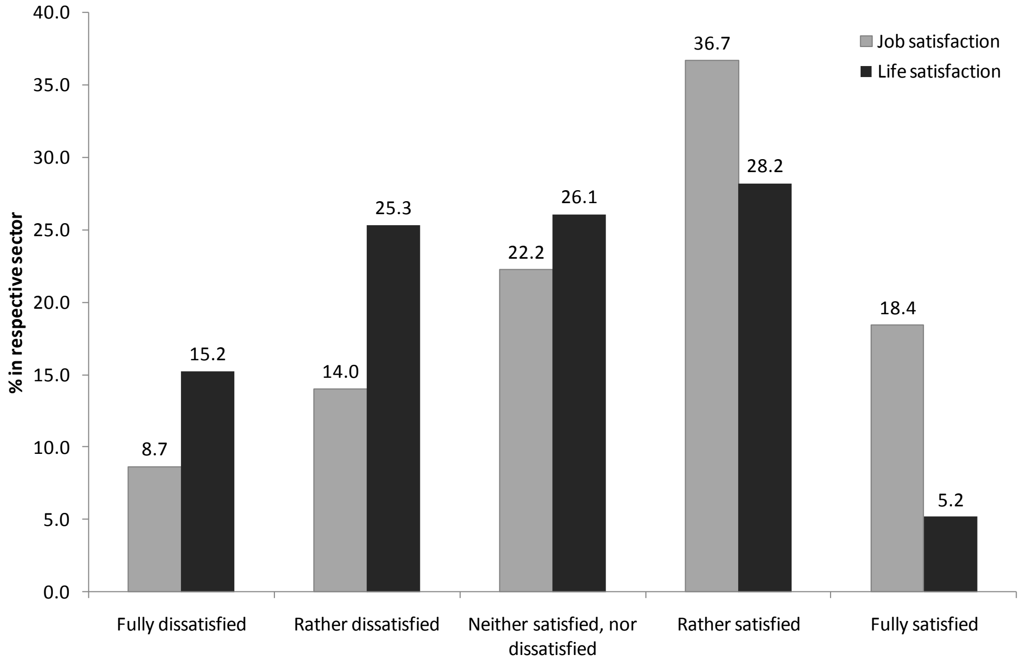
Source: ULMS 2003-2007, regression sample (life satisfaction: N=5,083; job satisfaction: N=5,142), own calculations.

Figure A 2-4: Job Satisfaction in the Private and Public Sector in Ukraine based on old and young workers (all available observations from the original survey; ULMS 2003-2007)



Source: ULMS 2003-2007, pooled sample of all individuals in working age (N=9,510); own calculations.

Figure A 2-5: Job and Life Satisfaction in Ukraine based on old and young workers (all available observations from the original survey; ULMS 2003-2007)



Source: ULMS 2003-2007, pooled sample of all individuals in working age (N=9,510); own calculations.

Table A 2-1: Overview on final sample size and labour market composition of original sample

	Cross section 2003	
	Absolute numbers	***Percentage***
Information on final sample size		
Total sample	8,537	100.0%
Working [A)]	3,583	42.0%
out of which		
Working [A)] and with Soviet Job Information	2,441	
Working [A)] and with Soviet Job Information and non-missing information in included variables [B)]	**1,491**	
Unemployed, ILO	783	9.2%
Out of labour force/student, working age	1,990	23.3%
Out of labour force, pension age	1,827	21.4%
Out of labour force, other	354	4.1%

Source: ULMS 2003; own calculations. [A)] In official working age (younger than official pension age). [B)] Excluding some specific variables with fewer observations, the regression sample size becomes 2,059 observations.

Table A 2-2: Variable definitions

Variable name	Variable definition	Comments
Satisfaction measures		
Job satisfaction	Job Satisfaction based on the question: *Tell me, please, how satisfied are you with your current job? Answer option: 1 Fully dissatisfied/ 2 Rather dissatisfied/ 3 Neither satisfied, nor dissatisfied/ 4 Rather satisfied/ 5 Fully satisfied*	
Life satisfaction	Life Satisfaction: *To what extent are you satisfied with your life in general at the present time?* (Five answer possibilities as in job satisfaction.)	
Satisfaction with job security	Based on the question: *Tell me, please, how satisfied are you with your current job in terms of job security?* (Five answer possibilities as in job satisfaction.)	This question was only asked in the survey years 2004 and 2007.

Sociodemographic and household characteristics		
Age_35 (omitted cat.) Age_3640 Age_4145 Age_4650 Age_51ret	Age group dummy variables according to age calculated using birth year, month, day and interview year, month, day (Last age group: 51 to official retirement age: 55/60 for women/men)	'Corrected age information'; Birth year, month and day have corrected based on cross-year consistency checks
Male	= 1, if male; =0 otherwise	
Marital status		
Single	= 1, if single	
Married	= 1, if married (lives in registered or unregistered marriage)	
Divorced	= 1, if separated or divorced	
Widowed	= 1, if widowed	
Education		
Primary education	= 1, if person has primary or unfinished secondary education	Coded according to Kupets (2006)
General secondary education	= 1, if person has diploma of high-school or PTU with secondary education (vocational secondary education)	Coded according to Kupets (2006)
Professional secondary education	= 1, if person has diploma from college (technical, medical, music, etc.) or incomplete professional education (at least 3 years in institute, university, etc.)	Coded according to Kupets (2006)
Higher education	= 1, if person has diploma from institute/university (bachelor, diploma, Master, Doctor of science)	Coded according to Kupets (2006)
Parents have higher education	= 1, if at least one parent has diploma from institute/university (bachelor, diploma, Master, Doctor of science)	
Parents have primary education	= 1, if both parents have primary or unfinished secondary education	
Chronic disease	= 1, if person has at least one of seven chronic diseases (self-reported): heart disease, illness of the lungs, liver disease, kidney disease, gastrointestinal disease, spinal problems, other chronic illnesses.	
Height	Individual height in cm	Corrected based on consistency checks across survey years.
One child in HH	= 1, if there is one child younger than 18 in the HH	Calculated from household file
Two or more children in HH	= 1, if there are two or more children younger than 18 in the HH	Calculated from household file
Place of birth: village	Indicating that birth place was village	

Place of birth: urban settlement	Indicating that birth place was urban settlement	
Place of birth: town/city	Indicating that birth place was town/city	
Place of birth: abroad (omitted cat.)	Indicating that birth place was abroad (not on Ukrainian territory)	
Never smoked	= 1, if person has never smoked	
Extrovert	Personality trait indicator generated on the basis of interviewer assessment at the end of the interview. Answer '3' to question: *Assess the sincerity and openness of the respondent. The respondent was: 1 – very introverted, insincere; 2 – as sincere and open as most respondents; 3 – more sincere and open than most respondents.*	
Neurotic	Personality trait indicator generated on the basis of interviewer assessment at the end of the interview. Answer '1' to question: *Assess the respondent's behaviour during the interview. The respondent: 1 – was nervous; 2 – was occasionally nervous; 3 – felt comfortable*.	
Risk loving	Indicator variable for all persons who answered 6 or higher on the question: *Are you generally a person who is fully willing to take risks or do you try to avoid taking risks? Please give me a number from 0 to 10, where the value 0 means: "Completely unwilling to take risks" and the value 10 means: "Completely willing to take risks"*.	This question was asked only in one wave of the survey (in 2007). Hence, the individual's answer to the 2007 question was assigned to the other survey years.
Job characteristics		
Occupation		
Professional	*= 1, i*f the worker's occupation is in the one-digit ISCO categories of legislators, senior officials, and managers (1) or professionals (2).	Categories based on one-digit ISCO codes as in Brown, Earle and Vakhitov (2006)
Technician	*= 1,* if the worker's occupation is in the one-digit ISCO category of technicians and associate professionals (3).	Categories based on one-digit ISCO codes as in Brown, Earle and Vakhitov (2006)
Skilled blue-collar	*= 1,* if the worker's occupation is in the one-digit ISCO categories of clerk (4), service workers and shop and market sales workers (5), skilled agricultural and fishery workers (6), craft and related	Categories based on one-digit ISCO codes as in Brown, Earle and Vakhitov (2006)

	workers (7), or plant and machine operators and assemblers (8).	
Unskilled (omitted category)	=1, if the worker's occupation is in the one-digit ISCO category of elementary occupations (9).	Categories based on one-digit ISCO codes as in Brown et al. (2006)
Less than 31 hours	Indicating respondents working 30 hours or less in a typical week	
31 to 50 hours (omitted category)	Indicating respondents working between 31 to 50 hours in a typical week	
More than 50 hours	Indicating respondents working 51 hours or more in a typical week	
Log wage	Monthly wage (contractual wage) or earnings from primary job (after tax), deflated to Dec 2003 Ukrainian Hryvna	Missing wage information was imputed based on a standard Mincer equation controlling for gender, age, schooling, marital status, weekly working hours, occupational group, settlement type and year dummy variables.
Log average monthly industry wage	Average monthly wages for one digit industry groups; deflated to Dec 2003 Ukrainian Hryvna	Source: Monthly industry wages from Ukrainian State Statistics Office
Weekend work	Indicator variable for person having worked at least once on Saturday and/or Sunday in the past four weeks.	
Shift work	Indicator variable for persons having worked more than one shift in the past four weeks.	
Firm characteristics		
State sector firm	Dummy variable with value 1 if ownership type is any of the following: Budgetary organization, State enterprise, Local municipal enterprise. Takes value 0 for 'Privatized', 'De-novo, private (incl. new agric)', 'Freelance, Self-employed', 'Collectives, Cooperatives', 'Intl' organization; other organization', missings.	The variable has been extensively cleaned based on consistency checks across waves and information on enterprise names.
 Small size firm (om. cat.) Medium sized firm Large sized firm	Indicating firms with - less than 10 employees - 10 to 99 employees - 100 and more employees	
Fringe benefits		
Social benefits	Three dummy variables based on the survey answers on the following question: *In this job do employees receive any of the*	Social benefit answer categories: 1 Regular paid vacation 2 Paid sick leave

	following social benefits?	3 Paid maternity leave, child care leave
SB: Social security	=1, if answered ‘yes’ to at least one of following security type benefits (cat: 1, 2, 3, 4)	4 Free treatment in an enterprise polyclinic, full or partial payment for treatment in other medical institutions
SB: Subsidy	=1, if answered ‘yes’ to at least one of following subsidy type benefits (cat: 6, 7, 8, 10, 11, 12, 13)	5 Payment for trips to sanatoria, rest homes, tourist bases, or children camps
SB: Training	=1, if answered ‘yes’ to at least one of following quality enhancement type benefits (cat: 9, 5)	6 Free child care in an enterprise kindergarten, or full or partial payment for child care in another kindergarten 7 Free or discounted food/subsidies 8 Transportation subsidies 9 Training paid for by the organization 10 Loans and credit 11Possibilities to rent/purchase garden and land plot at below market prices 12 Equipment for additional earnings and private needs 13 Housing subsidies
Financial benefits	Based on the survey answers on whether respondent received ‘any amount of money in addition to your regular salary’ in the past year:	
FB: non-performance related	=1, if received 13th salary	
FB: Performance related	=1, if received performance-based bonus or payment scheme involved profit-sharing	
FB: Risk compensation	=1, if received compensation for non-normal work conditions	
Control variables from the Soviet period (December 1986 or 1991)		
Place of work 1986: Kiev, Center, West, East, South, Russia, Other country	Set of 6 dummy variables indicating place of work in December 1986.	Where this information is not available or the respondent started his working life after December 1986, the respective information from December 1991 is used.
Log of wage in December 1986	Expressed in real terms in July 2004 Hryvnia	
Log of wage in December 1991	Expressed in real terms in July 2004 Hryvnia	
Wage December 1986 missing	Indicator variable for missing wage information	
Wage December 1991 missing	Indicator variable for missing wage information	

Worked in Dec 1986	Indicator variable for working status in December 1986
Worked in Dec 1991	Indicator variable for working status in December 1991

Other controls	
Oblast	A set of dummy variables for each of the 26 oblasts of Ukraine
Urban	= 1, if settlement has status of small town or more, = 0, if village
Year 2004, year 2007	Year fixed effects for survey years (omitted category: year 2003)

Table A 2-3: Job satisfaction regressions with stepwise inclusion of covariates (a more complete representation of Table 2) (GLS)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable	*Job satisfaction*							
State sector	0.340***	0.277***	0.259***	0.240***	0.217***	0.219***	0.212***	0.211***
	(0.043)	(0.042)	(0.043)	(0.042)	(0.043)	(0.041)	(0.041)	(0.041)
Male		-0.064	-0.038	-0.065	-0.073	-0.174***	-0.173***	-0.135*
		(0.062)	(0.066)	(0.066)	(0.065)	(0.063)	(0.063)	(0.076)
General secondary educ.		0.026	0.023	0.005	0.015	-0.005	-0.001	-0.004
		(0.076)	(0.076)	(0.077)	(0.078)	(0.075)	(0.075)	(0.075)
Professional sec. educ.		0.197**	0.121	0.025	0.033	-0.003	0.000	-0.001
		(0.081)	(0.083)	(0.085)	(0.086)	(0.084)	(0.083)	(0.084)
Higher education		0.413***	0.271***	0.041	0.050	0.018	0.019	0.012
		(0.088)	(0.094)	(0.103)	(0.103)	(0.101)	(0.101)	(0.101)
Individual height		0.001	0.001	0.002	0.001	-0.000	-0.000	-0.000
		(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
Parents: primary educ.		0.110	0.100	0.083	0.089	0.099	0.101	0.101
		(0.068)	(0.068)	(0.067)	(0.067)	(0.065)	(0.065)	(0.064)
Parents: higher educ.		-0.009	-0.004	-0.001	-0.004	0.000	0.003	0.003
		(0.049)	(0.048)	(0.047)	(0.047)	(0.045)	(0.045)	(0.045)
Married		0.367***	0.452***	0.410***	0.427***	0.416***	0.421***	0.425***
		(0.114)	(0.123)	(0.123)	(0.123)	(0.116)	(0.116)	(0.117)
Divorced		0.305**	0.378***	0.347***	0.354***	0.365***	0.371***	0.380***
		(0.121)	(0.128)	(0.129)	(0.128)	(0.121)	(0.122)	(0.123)
Widowed		0.413***	0.509***	0.476***	0.492***	0.477***	0.482***	0.487***
		(0.147)	(0.154)	(0.154)	(0.154)	(0.148)	(0.148)	(0.149)
1 child in HH		-0.084*	-0.078*	-0.091**	-0.080*	-0.069	-0.068	-0.071*
		(0.045)	(0.045)	(0.045)	(0.044)	(0.043)	(0.043)	(0.043)
2+ children in HH		-0.155***	-0.128**	-0.125**	-0.111*	-0.090	-0.089	-0.094*
		(0.057)	(0.058)	(0.058)	(0.058)	(0.057)	(0.057)	(0.057)

Chronic disease	-0.203***	-0.200***	-0.198***	-0.185***	-0.174***	-0.171***	-0.173***
	(0.037)	(0.037)	(0.037)	(0.037)	(0.036)	(0.036)	(0.036)
Urban settlement	0.135***	0.114**	0.086*	0.066	-0.003	-0.007	-0.005
	(0.052)	(0.052)	(0.052)	(0.052)	(0.051)	(0.051)	(0.051)
Professional occupation			0.583***	0.611***	0.423***	0.420***	0.412***
			(0.077)	(0.076)	(0.078)	(0.078)	(0.078)
Technical occupation			0.473***	0.459***	0.369***	0.364***	0.360***
			(0.075)	(0.075)	(0.073)	(0.073)	(0.074)
Skilled blue collar occ.			0.264***	0.250***	0.141***	0.138***	0.139***
			(0.054)	(0.054)	(0.053)	(0.053)	(0.053)
Less than 31 hours				-0.348***	-0.189***	-0.194***	-0.196***
				(0.065)	(0.065)	(0.065)	(0.065)
More than 50 hours				-0.086	-0.099	-0.077	-0.077
				(0.062)	(0.062)	(0.063)	(0.063)
Medium-sized firm				0.089	0.092	0.078	0.077
				(0.058)	(0.056)	(0.056)	(0.056)
Large-sized firm				0.131**	0.059	0.044	0.044
				(0.058)	(0.057)	(0.057)	(0.057)
Log earnings					0.392***	0.396***	0.393***
					(0.047)	(0.048)	(0.048)
Log average industry wage					0.190***	0.175**	0.173**
					(0.073)	(0.074)	(0.074)
Weekend work						-0.081**	-0.080**
						(0.039)	(0.039)
Shift work						0.061	0.064
						(0.053)	(0.053)

Extrovert								0.078*
								(0.047)
Neurotic								-0.158
								(0.106)
Never smoked								0.053
								(0.057)
Risk loving								0.026
								(0.052)
Constant	3.029***	2.222***	2.498***	2.285***	2.338***	-1.586*	-1.484	-1.511
	(0.058)	(0.621)	(0.881)	(0.873)	(0.870)	(0.960)	(0.962)	(0.961)
Chi-square test	194.7	475.1	517.2	618.0	653.2	859.4	860.5	879.7
Prob > chi2	0	0	0	0	0	0	0	0
Region FE	-	✓	✓	✓	✓	✓	✓	✓
Age & birth place	-	✓	✓	✓	✓	✓	✓	✓
Soviet period controls	-	-	✓	✓	✓	✓	✓	✓
R-squared overall	0.0430	0.106	0.113	0.133	0.144	0.185	0.186	0.187

Notes: Dependent variable: Five-point Likert scale of Job satisfaction with 1 (fully dissatisfied) to 5 (fully satisfied). Omitted categories: ‘primary education’, ‘single’, ‘occupation : unskilled’, ‘works 31 to 50 hours in typical working week’ ‘small-sized firm’, ‘village or rural settlement’. All regressions control for year fixed effects and interview month fixed effects. Number of observations 4,191 (Number of id: 1,915). Standard errors are clustered on the individual level. Robust standard errors are shown in parentheses; *** p<0.01, ** p<0.05, * p<0.1; Source: ULMS 2003-2007; own calculations.

Table A 2-4: Robustness checks I.: A. OLS estimates of job satisfaction based on cross-section 2003 only; B. GLS Random effects estimates based on larger sample (ULMS 2003-2007)

	A. OLS estimates based on cross-section ULMS 2003			B. GLS Random effects estimates based on larger sample (ULMS 2003-2007)		
	(1)	(2)	(3)	(4)	(5)	(6)
State sector	0.308***	0.221***	0.192***	0.321***	0.250***	0.208***
	(0.064)	(0.066)	(0.066)	(0.038)	(0.039)	(0.038)
Male		0.013	-0.038		-0.012	-0.154**
		(0.100)	(0.119)		(0.060)	(0.067)
General secondary educ.		0.199*	0.133		0.050	0.015
		(0.116)	(0.114)		(0.066)	(0.064)
Professional sec. educ.		0.184	0.028		0.145**	0.018
		(0.127)	(0.126)		(0.073)	(0.072)
Higher education		0.209	-0.032		0.271***	-0.014
		(0.145)	(0.155)		(0.084)	(0.088)
Individual height		-0.008	-0.009*		0.001	0.001
		(0.006)	(0.006)		(0.003)	(0.003)
Parents: primary educ.		0.123	0.139		0.102	0.091
		(0.112)	(0.108)		(0.063)	(0.059)
Parents: higher educ.		-0.015	0.001		0.005	0.010
		(0.075)	(0.072)		(0.044)	(0.041)
Married		0.402**	0.377**		0.367***	0.297***
		(0.200)	(0.187)		(0.109)	(0.105)
Divorced		0.351	0.371*		0.303***	0.261**
		(0.221)	(0.208)		(0.115)	(0.111)
Widowed		0.580**	0.550**		0.346**	0.280**
		(0.243)	(0.234)		(0.140)	(0.135)
1 child in HH		0.022	0.018		-0.102**	-0.098**
		(0.077)	(0.075)		(0.041)	(0.039)
2+ children in HH		-0.034	-0.019		-0.136**	-0.106**
		(0.099)	(0.095)		(0.053)	(0.052)
Chronic disease		-0.145**	-0.120*		-0.209***	-0.177***
		(0.066)	(0.064)		(0.034)	(0.033)
Urban settlement		0.141*	-0.026		0.167***	0.027
		(0.083)	(0.081)		(0.047)	(0.046)
Professional occ.			0.246*			0.467***
			(0.136)			(0.071)
Technical occ.			0.307**			0.383***
			(0.120)			(0.067)
Skilled blue coll.			0.063			0.180***
			(0.089)			(0.049)
Less than 31 hours			-0.194*			-0.168***
			(0.113)			(0.060)
More than 50 hours			-0.048			-0.093
			(0.119)			(0.057)
Medium-sized firm			0.054			0.048
			(0.097)			(0.051)

	(1)	(2)	(3)	(4)	(5)	(6)
Large-sized firm			-0.024			0.053
			(0.096)			(0.052)
Log earnings			0.582***			0.410***
			(0.066)			(0.044)
Log ave. industry wage			0.210**			0.181***
			(0.102)			(0.065)
Weekend work			-0.105			-0.072**
			(0.067)			(0.035)
Shift work			0.065			0.046
			(0.104)			(0.048)
Extroverted			-0.009			0.091**
			(0.098)			(0.043)
Neurotic			-0.295			-0.236**
			(0.234)			(0.100)
Never smoked			0.151*			0.007
			(0.092)			(0.050)
Risk loving			0.049			--
			(0.082)			
Constant	3.065***	3.590**	-2.530*	3.072***	2.622***	-1.757**
	(0.067)	(1.433)	(1.534)	(0.050)	(0.799)	(0.868)
Socio-demographic Post-Soviet & Soviet characteristics (incl. Soviet job)	-	✓	✓	-	✓	✓
Current job & firm characteristics (incl. log of earnings & average industry wages)	-	-	✓	-	-	✓
Personality traits, risk aversion	-	-	✓	-	-	✓
Observations	1,491	1,491	1,491	5,142	5,142	5,142
R-squared [A)]	0.0157	0.0986	0.185	0.0381	0.103	0.182

Notes: Dependent variable: Five-point Likert scale of job satisfaction with 1 (fully dissatisfied) to 5 (fully satisfied). The table reports selected estimated from the following regression specifications: columns (1) and (4) correspond to the specification in Table 2-3, column (1); columns (2) and (5) correspond to Table 2-3, column (3); columns (3) and (6) correspond to Table 2-3, column (8). Omitted categories: ‘primary education’, ‘single’, ‘occupation: unskilled’, ‘works 31 to 50 hours in typical working week’, ‘small-sized firm’, ‘village or rural settlement’. [A)] Standard R-squared in columns (1)-(3); R-squared overall in columns (4)-(6). Number of id in columns (4)-(6): 2,566. All regressions control for year fixed effects and interview month fixed effects. Standard errors are clustered on the individual level. Robust standard errors are shown in parentheses; *** p<0.01, ** p<0.05, * p<0.1. Source: columns (1)-(3) ULMS 2003, columns (4)-(6) ULMS 2003-2007; own calculations.

Table A 2-5: Robustness checks II: Instrumental variable regressions based on cross-section 2003 (A) and (B) pooled sample with more observations (ULMS 2003-2007)

	Instrumental variable 1: Privatization probability			**Instrumental variable 2: Private sector share**		
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	*Job Satisfaction*			*Job Satisfaction*		
A. OLS estimates based on cross-section ULMS 2003						
OLS regressions						
State sector	0.308***	0.221***	0.192***	0.308***	0.221***	0.192***
	(0.064)	(0.066)	(0.066)	(0.064)	(0.066)	(0.066)
2SLS regressions						
State sector	0.635***	0.325	0.516**	0.858***	0.529***	0.567***
	(0.224)	(0.229)	(0.252)	(0.183)	(0.177)	(0.184)
First stage t-statistic	*-8.87*	*-9.40*	*-9.83*	*-12.55*	*-11.60*	*-9.49*
First stage F-statistic	*78.60*	*88.43*	*96.61*	*157.44*	*134.63*	*90.06*
Observations	*1,491*	*1,491*	*1,491*	*1,491*	*1,491*	*1,491*
B. GLS Random effects estimates based on larger sample (ULMS 2003-2007)						
GLS regressions						
State sector	0.321***	0.250***	0.208***	0.321***	0.250***	0.208***
	(0.038)	(0.039)	(0.038)	(0.038)	(0.039)	(0.038)
G2SLS regressions						
State sector	0.553***	0.300*	0.413**	0.812***	0.456***	0.444***
	(0.148)	(0.174)	(0.173)	(0.115)	(0.132)	(0.143)
First stage z-statistic	*-18.48*	*-15.59*	*-15.76*	*-24.81*	*-21.09*	*-19.28*
First stage chi-2-statistic	*161.1*	*100.5*	*110.9*	*290.9*	*183.0*	*170.6*
Observations	*5,142*	*5,142*	*5,142*	*5,142*	*5,142*	*5,142*
Socio-demographic Post-Soviet and Soviet characteristics (incl. Soviet job)	-	✓	✓	-	✓	✓
Current job and firm characteristic (incl. log of earnings & average industry wages)	-	-	✓	-	-	✓
Personality traits, risk aversion	-	-	✓	-	-	✓

Notes: Dependent variable: Five-point Likert scale of job satisfaction with 1 (fully dissatisfied) to 5 (fully satisfied). The table reports only the estimated coefficients of interest (state sector; instrumental variables) from the following regression specifications: columns (1) and (4) correspond to the specification in Table 2-3, column (1); columns (2) and (5) correspond to Table 2-3, column (3); columns (3) and (6) correspond to Table 2-3, column (8). In panel (A) the estimation procedure allows to cluster standard errors on industry-macro region level; in panel (B) standard errors are clustered on the individual level. In the top panel A, the p-value for the state sector coefficient from the 2SLS regression in column (2) is 0.156. Robust standard errors are shown in parentheses; *** p<0.01, ** p<0.05, * p<0.1; Source: ULMS 2003-2007; own calculations.

Appendix B. Construction of instrumental variables

First Instrumental Variable: Share of Employees experiencing privatization in 27 industries

The first instrumental variable reflects the probability of privatization for a worker who started her working life during the Soviet Union in a specific industry sector. The calculation of the instrumental variable exploits the retrospective information of the ULMS conducted in 2003 and is based on the sample of individuals who started their job during the Soviet period, i.e. before December 1991, and who were working in this job at least until January 1992 ('*transition jobs*'). (This drops all individuals who used to be in the labour force during the Soviet Union, but retired or left the workforce before December 1992 and thus could not possibly experience privatization.) The calculation of industry specific privatization probabilities involved two steps: (1) In the first step a binary indicator was generated for each individual that took the value '1', if the work place was privatized during the tenure of the job that started during Soviet time and lasted at least until January 1993 *and* if most of the shares of the enterprise/organization were owned by private entities after the privatization, and '0' otherwise. (2) The share of these privatization incidences within a certain industry sector was then calculated for 27 different industry sectors (see Table B 2-1). These industry specific privatization probabilities were then assigned to all workers with non-missing Soviet work history information based on the industry affiliation of their work place held in December 1986. For the subsample of workers who did not work in December 1986, but started to work before December 1991, the industry specific privatization probability is assigned based on the December 1991 work place industry sector. It should be noted, that there are two aspects that potentially reduce the exactness of this instrumental variable in reflecting the true industry specific privatization probability. First, the ULMS survey was originally designed to be representative of the Ukrainian population aged 15 to 72 in 2003 and not in 1986/1991. This implies that the true privatization probabilities might differ to the extent that the distribution of workers across industries based on the retrospective information of the sample 2003 differs from the actual distribution (if, for instance, mortality rates or migration vary across industries). Second, the construction of the instrumental variable is based only on those jobs that started during the Soviet period and thus neglects all possible privatization experiences after a job change in the Post-Soviet period. However, this will only bias the estimates if the workers propensity to leave a 'still state-owned' enterprise before its privatization differs systematically across sectors. Table B 2-1 presents the calculated privatization probabilities and the respective cell sizes for the 27 industry sectors (for the sample used to construct the probabilities, i.e. all '*transition jobs'*).

Table B 2-1: Overview of industry categories and industry specific share of privatization episodes in the ULMS

Industry sector categories I (ULMS)		Privatization probability (IV_privatized)	Number of observations per cell
1	Agriculture, forestry, fishing	14.76%	725
2	Mining	6.27%	257
3	Manufacture of foodstuffs, beverages and tobacco	24.60%	189
4	Manufacture of textiles and leather products	34.62%	132
5	Manufacture of cellulose and paper industry; wood; printing	35.94%	64
6	Manufacture of coke, refined petroleum products and nuclear fuel	28.57%	14
7	Manufacture of chemicals and chemical products	23.91%	46
8	Manufacture of rubber and plastic products	48.39%	31
9	Manufacture of other non-metallic mineral products	29.17%	24
10	Metallurgy and production of finished metal products	32.27%	223
11	Manufacture of machinery and equipment	24.79%	123
12	Manufacture of electrical, electronic and optical equipment	21.28%	189
13	Manufacture of transport vehicles and equipment	29.66%	119
14	Manufacture other	26.32%	19
15	Production and distribution of electricity, gas and water	16.46%	79
16	Construction	12.66%	242
17	Trade; repair of motor vehicles, household appliances, personal demand items	15.77%	283
18	Activity of hotels and restaurants	13.51%	37
19	Transport	11.32%	318
20	Post and Telecom	6.67%	60
21	Financial intermediaries	8.82%	68
22	Public administration and defence	1.01%	198
23	Education	0.24%	419
24	Health care and provision of social aid	1.48%	341
25	Provision of communal and individual services	8.19%	172
26	Communal and individual services: culture, sport, leisure, entertainment	5.26%	57
27	Other activities	0.00%	22
	Total	13.99%	4,451

Second Instrumental Variable: Share of Private Sector Employment in 16 industries in 5 macro regions in Post-Soviet Ukraine (2003-2006)

The construction of the second instrumental variable is based on a pooled sample of the Ukrainian Budget Household Survey for the years 2003, 2004, 2005, 2006, restricted to individuals who work at the time of the interview (excluding unpaid family helpers) and who have non-missing information on industry category and ownership type of work place (N=34,344). The information on ownership distinguishes 7 categories: (1) Public/State enterprise, organization, office/institute; (2) Collective enterprise, cooperative; (3) Joint stock enterprise; (4) Leased enterprise; (5) Joint venture or foreign enterprise; (6) Private enterprise or private individual/person; (7) Other. 17 industry sectors are distinguished in the questionnaire (16 categories are used for constructing cell shares as two categories were merged due to few observations per cell). The candidate instrumental variable reflects the share of individuals who answered '(1) Public/State enterprise, organization' in a specific industry sector and in a specific macro-region of Ukraine. This allows for possible regional variation in state or private sector employment. The result are 80 industry sector-macro region cells (16 industries x 5 macro regions). All cell shares are calculated using individual sampling weights.

Table B 2-2: Overview of industry-region categories and industry-region specific shares of private sector employment in the UHBS

Region	Industry sector categories II (UHBS)	Private sector share (IV2_private-share)	Number of observations per cell
Kiev	Agriculture, hunting and forestry	30.94%	15
Kiev	Fishery	75.65%	5
Kiev	Mining	44.48%	17
Kiev	Manufacturing	55.85%	225
Kiev	Production and distribution of electricity, gas and water	34.39%	33
Kiev	Construction	72.66%	207
Kiev	Wholesale and retail trade; sale of transportation means; repair services	88.61%	339
Kiev	Hotels and restaurants	89.82%	44
Kiev	Transport and communications	52.64%	222
Kiev	Financial intermediation	61.71%	87
Kiev	Real estate transactions, lease and services to legal persons	35.55%	86
Kiev	Public administration	1.27%	166
Kiev	Education	6.62%	218
Kiev	Health care and social aid	14.50%	144
Kiev	Collective, social and individual services	64.50%	264

Kiev	Extra-territorial organizations and bodies	51.30%	12
Centre	Agriculture, hunting and forestry	83.25%	1587
Centre	Fishery	84.55%	15
Centre	Mining	70.72%	132
Centre	Manufacturing	87.32%	1249
Centre	Production and distribution of electricity, gas and water	44.90%	276
Centre	Construction	80.92%	466
Centre	Wholesale and retail trade; sale of transportation means; repair services	95.21%	917
Centre	Hotels and restaurants	85.53%	90
Centre	Transport and communications	42.68%	674
Centre	Financial intermediation	55.06%	130
Centre	Real estate transactions, lease and services to legal persons	58.49%	62
Centre	Public administration	0.53%	624
Centre	Education	0.41%	1010
Centre	Health care and social aid	5.34%	793
Centre	Collective, social and individual services	59.55%	271
Centre	Extra-territorial organizations and bodies	51.30%	12
West	Agriculture, hunting and forestry	77.86%	839
West	Fishery	91.72%	5
West	Mining	47.45%	202
West	Manufacturing	82.37%	1122
West	Production and distribution of electricity, gas and water	43.37%	334
West	Construction	81.20%	714
West	Wholesale and retail trade; sale of transportation means; repair services	93.88%	1046
West	Hotels and restaurants	90.14%	160
West	Transport and communications	38.69%	681
West	Financial intermediation	42.68%	117
West	Real estate transactions, lease and services to legal persons	55.36%	49
West	Public administration	0.66%	793
West	Education	0.60%	1215
West	Health care and social aid	7.10%	863
West	Collective, social and individual services	55.36%	339
West	Extra-territorial organizations and bodies	51.30%	12
East	Agriculture, hunting and forestry	92.14%	423
East	Fishery	92.48%	9
East	Mining	20.93%	565
East	Manufacturing	84.37%	1368
East	Production and distribution of electricity, gas and water	33.82%	301
East	Construction	86.23%	358
East	Wholesale and retail trade; sale of transportation means; repair services	95.74%	891
East	Hotels and restaurants	91.76%	64
East	Transport and communications	46.50%	505
East	Financial intermediation	49.50%	93
East	Real estate transactions, lease and services to legal persons	42.81%	97
East	Public administration	0.52%	399
East	Education	1.80%	640
East	Health care and social aid	10.14%	515
East	Collective, social and individual services	58.07%	170
East	Extra-territorial organizations and bodies	51.30%	12

South	Agriculture, hunting and forestry	82.46%	1146
South	Fishery	88.99%	41
South	Mining	73.95%	212
South	Manufacturing	79.56%	1492
South	Production and distribution of electricity, gas and water	33.47%	290
South	Construction	87.98%	622
South	Wholesale and retail trade; sale of transportation means; repair services	96.11%	1254
South	Hotels and restaurants	91.88%	144
South	Transport and communications	46.84%	800
South	Financial intermediation	55.19%	155
South	Real estate transactions, lease and services to legal persons	62.32%	76
South	Public administration	0.17%	579
South	Education	2.25%	865
South	Health care and social aid	8.38%	734
South	Collective, social and individual services	65.69%	332
South	Extra-territorial organizations and bodies	51.30%	12
Total		52.77%	33,994

3 The long-term effects of Chernobyl on Subjective Well-Being and Mental Health

3.1 Introduction

Natural and man-made disasters produce fear and desperation. Victims of such events suffer from anxiety, posttraumatic stress and depression (Goenjian, Steinberg, Najarian, Fairbanks, Tashjian and Pynoos 2000) – not only in the immediate aftermath of the event but also in the medium- to long-run (Havenaar, Rumyantzeva, van den Brink, Poelijoe, van den Bout, van Engeland and Koeter 1997).[39] This chapter evaluates the long-term effects of the Chernobyl catastrophe, the largest civic nuclear accident to date which took place on April 26, 1986, in terms of subjective well-being and mental health.[40]

Similar to such rare aggregate shocks like natural catastrophes (floods, hurricanes, tsunamis and earthquakes), terrorist attacks and other man-made accidents (like the chemical catastrophe of Bhopal, India, in 1984), the Chernobyl disaster represents a non-insurable 'public bad'. This implies that it is ultimately the state which has to bear the costs by paying for disaster relief and recovery work. Against this background, it seems to be highly relevant to provide an assessment of the aggregate utility loss caused by such an event (Luechinger and Raschky 2009).

Up to now, no large scale analysis based on a nationally representative sample has evaluated the long-term effect of the Chernobyl disaster on subjective well-being and the mental health of the Ukrainian population. The empirical analysis of this chapter aims at filling this gap. The scientific research on Chernobyl so far

[39] "The concept of stress is invoked to explain the widespread damage to general health and well-being. Stress can be defined as the process by which adverse mental experiences have negative effects on bodily functions. The mechanism is physiological, mediated through the autonomic nervous system and the endocrinological system." (Lee 1996, p. 283)

[40] Despite the fact that nuclear accidents seem relatively rare, several events during the past 60 years were categorized as accidents according to the International Nuclear Event Scale (INES scale 4-7): Chalk River 1952 (USA), Kyshtym 1957 (USSR), Sellafield 1957 (UK), Los Alamos 1958 (USA), Simi Valley 1959 (USA), Idaho Falls 1961 (USA), Charlestown 1964 (USA), Monroe 1966 (USA), Lucens 1969 (Switzerland), Rocky Flats 1969 (USA), Sellafield 1973 (UK), Leningrad 1974 (USSR), Belojarsk 1977 (USSR), Bohunice 1977 (CSSR), Three Mile Islands 1979 (USA), Saint-Laurent 1980 (France), Chernobyl 1982 (USSR), Buenos Aires 1983 (Argentinia), Wladiwostok 1985 (USSR), Chernobyl 1986 (USSR), Goiânia 1987 (Brazil), Sewersk 1993 (Russia), Tokaimura 1999 (Japan), Fleurus 2006 (Belgium), and Fukushima 2011 (Japan).

has mainly focused on health effects and the relationship between radiation and cancer (see the summary of the findings of the medical literature in, for instance, two United Nations reports from 2001 and 2002 (United Nations 2001, 2002), two UNSCEAR reports from 2000 and 2008 (UNSCEAR 2000, 2008) as well as a national report from Ukraine (Baloga, Kholosha and Evdin 2006)). In general, the evidence on cancer and the total health toll of the disaster seem inconclusive albeit moderate—with the important exception of a significant rise of the incidence of thyroid cancer in children.

In economics, three recent papers have used the catastrophe of Chernobyl to investigate various effects. The first two use an identification strategy that exploits regional variation in radiation levels—a method which will also be employed in the current chapter: Lehmann and Wadsworth (2011) focus not only on health outcomes but also on long-term labour market consequences of the 1986 nuclear accident using the Ukrainian Longitudinal Monitoring Survey (ULMS). While they find substantially worse health perceptions among the affected population in Ukraine, the effect on somatic health and risky behaviour (drinking, smoking) seems weak and is mostly not significantly different from zero. Yet, their empirical evidence seems to suggest that Chernobyl victims have a lower attachment to the labour market which, however, does not translate into income losses. Almond, Edlund and Palme (2009) evaluate the effect of low-dose pre-natal radiation exposure caused by the Chernobyl disaster on cognitive and health child outcomes in Sweden. While they do not find any causal effect on health outcomes at birth or incidence of hospitalisation during childhood, there seem to be significant adverse consequences on cognitive ability measured by schooling outcomes in secondary school (around age 16). Finally, Berger (2009) analyses the impact of the Chernobyl catastrophe on life satisfaction and on being concerned about the protection of the natural environment in Germany by taking advantage of the fact that the 1986 wave of the German Socio-Economic Panel was collected between March and August. Her empirical approach is to compare average levels of life satisfaction of respondents who were randomly interviewed shortly before with those interviewed shortly after the nuclear accident. She finds a significant increase in the likelihood of being very concerned about the environment immediately after the catastrophe (by 20 percent), but life satisfaction remained unaffected. This empirical strategy – to exploit variation over time in combination with randomly assigned interview dates – is similar to a US study using weekly data and finding a negative short term effect of hurricane Katrina on life satisfaction in 2005 (Kimball, Levy, Ohtake and Tsutsui 2006). The results of this latter study reveal that the negative effect on average US life satisfaction lasts slightly longer in regions close to the affected area. A refinement of this identification strategy is implemented by Metcalfe, Powdthavee and Dolan (2011) who assess the impact of the September 11 attacks on mental distress in the UK in 2001. It is possible that average subjective well-being levels move systematically over the year, i.e. that there are underlying trends which are not related to a particular event. This is why simple before-and-after comparisons might be biased. Metcalfe, Powdthavee and Dolan (2011) apply a difference-in-difference method which accounts for such possible seasonality effects by including control years in which there was no attack. The difference-in-differences estimation is based on interviews randomly split by the attacks into two samples (treatment and control group). Their findings show a significantly lower subjective well-being immediately after September 11.

While these latter studies have focused on short-term changes in life satisfaction, there is increasing evidence that certain shocks can also lead to long run changes in subjective well-being and thus a shift in the personal baseline level of happiness (Clark, Frijters and Shields 2008; Diener, Lucas and Scollon 2006; Heady 2008; Oswald and Powdthavee 2008).

The empirical analysis of this study will use a self-reported measure of 'being affected by the Chernobyl catastrophe' – which will be referred to as 'self-reported affectedness' in the remainder of this chapter – as well as objective radiation doses from 1986 to establish the causal link between the Chernobyl disaster and life satisfaction as well as mental health. The results indicate that having been exposed to Chernobyl has a significantly negative effect on subjective well-being in the long run and adversely affects mental health. The results prove robust to several sensitivity checks. Furthermore, there is suggestive evidence that responses vary by gender (women seem to be more adversely affected). On average, individuals exposed to higher levels of radiation seem to engage in some precautionary health behaviour and appear to be more dependent on state transfers. Following the recent literature using subjective well-being regressions to evaluate the monetary costs associated with specific life events (life satisfaction approach) the amount of income required to compensate for their experienced utility loss is calculated (for other papers calculating such compensatory payments, see, for instance, Clark and Oswald 2002, Luechinger and Raschky 2009, van Praag and Baarsma 2005).

This empirical study contributes to the literature on life satisfaction as well as the literature assessing the impacts of the Chernobyl disaster in several ways: First, it estimates the causal Chernobyl effect on long-term life satisfaction and mental health outcomes using large and nationally representative surveys. Second, it investigates the potential endogeneity of self-reported affectedness measures using objective radiation measures and instrumental variable techniques. Thus it contributes to the important question whether individuals overstate their true affectedness level (with implications for benefit claims). Third, the study computes the value of the utility loss caused by the Chernobyl catastrophe which corresponds to about ten percent of annual Ukrainian GDP – a tremendous amount considering the fact that the estimates refer to a period of 20 years after the accident. This implies that the psychological costs of this nuclear disaster are enormous.

The remainder of the chapter is structured as follows: Section 3.2 provides relevant background information on the nuclear accident of Chernobyl and outlines potential transmission channels through which the catastrophe might have affected life satisfaction and mental health. Section 3.3 describes the two data sets employed for the empirical analysis of the paper, the Ukrainian Longitudinal Monitoring Survey (ULMS) and the Ukrainian Household Budget Survey (UHBS). The methodological approach is described in Section 3.4. This is followed by the main empirical results as well as several robustness checks (Section 3.5 and 3.6). Section 3.7 presents the estimates of the required income compensation. The subsequent sections 3.8 and 3.9 present two extensions to the main empirical analysis – investigating gender differences as well as behavioural consequences of the Chernobyl effect. The final Section 3.10 concludes.

3.2 Background on the Chernobyl catastrophe and its consequences

The following sections provide detailed information on the nuclear accident and the size of the affected population in Ukraine. Furthermore, potential channels through which the Chernobyl catastrophe might affect subjective well-being in the long-run are outlined with reference to the previous literature.

3.2.1 The accident of Chernobyl[41]

On April 26, 1986 one block of the nuclear power plant in Chernobyl, nowadays in Ukraine and close to the Belorussian border, exploded leading to the biggest civil nuclear accident ever recorded (UNSCEAR 2000). After the initial explosion, a nuclear cloud formed and contaminated substantial areas of Belarus, Ukraine and western Russia with radioactive fallout. Later more western and northern parts of Europe were also affected due to strong eastern winds. Inside the power plant the fight against the fire lasted for a fortnight and the Soviet government reacted on a broader basis to the accident only after the global measurement of the parts-per-trillion concentration of radioactive isotopes in the atmosphere prevented the incidence from being kept secret. In early May 1986, several ten thousand inhabitants from the immediate vicinity to the reactor were evacuated and in the following month approximately 170,000 residents were resettled from inside a 30 kilometre zone of alienation. Medical treatment with iodine drugs which could prevent the absorption of radioactive iodine started only days after the catastrophe and control of milk and foodstuff remained insufficient. Taken together, several hundred thousand people in Ukraine were exposed to significant levels of radioiodine (iodine-131) and radiocaesium-137 (caesium-137) either as clean-up workers (fire fighters, liquidators, construction workers of the concrete shield) or nearby inhabitants (see footnote 41).

[41] This section is based on diverse national and international reports on the nuclear accident of Chernobyl. More detailed accounts of the timeline of the events as well as technical details can be found, for instance, in the following publications: the 1998 European Commission Atlas of caesium deposition on Europe after the Chernobyl accident (European Commission 1998), two United Nations reports from 2001 and 2002 (United Nations 2001, 2002) and two UNSCEAR reports from 2000 and 2008 (UNSCEAR 2000, 2008) as well as a national report from Ukraine (Baloga, Kholosha and Evdin 2006).

Given contradictory statements about the humanitarian and environmental damage caused by the disaster in the scientific literature, the losses and costs are still hard to quantify: In the early period, liquidators and close inhabitants were most strongly affected by radiation exposure. However, only 31 deaths were officially registered by the Soviet government as a direct consequence of the catastrophe. On a long term basis, many more people suffered from internal radiation through inhalation or the consumption of contaminated food. According to the United Nations more than eight million people were and are affected by this catastrophe in the three most affected countries Belarus, Russia and Ukraine (United Nations 2001). The number of immediate casualties is highly debated and varies substantially between nuclear power proponents and critiques. However, the impact of the accident on strong increases in the incidence of thyroid cancer among children in Belarus, Russia and Ukraine is less disputed (Demidchik, Mrochek, Demidchik, Vorontsova, Cherstvoy, Kenigsberg, Rebeko and Sugenoya 1999; UNSCEAR 2000). (Demidchik, Mrochek, Demidchik, Vorontsova, Cherstvoy, Kenigsberg, Rebeko and Sugenoya 1999; UNSCEAR 2000). Vast areas of natural resources became unusable for agricultural production.[42] Government spending to alleviate the consequences in Ukraine alone are estimated at 148 billion USD from 1986-2015 (currently, five to seven percent of Ukraine's annual budget are spent on the alleviation of long term consequences; Oughton, Bay-Larsen and Voigt 2009).[43] The social costs related to stigma, anxiety and the perception of risk have only started to be understood. The disaster has affected almost every dimension of human life: For instance, women

[42] The total area removed was 784,000 ha of agricultural land and 694,000 ha of forest in Belarus, Ukraine and Russia together (United Nations 2002). This is equivalent to the size of Kuwait and larger than the state of Connecticut.

[43] Unfortunately, the source from which the information of the 5-7 percent of GDP expenditure on Chernobyl-related benefits and programmes does not contain a more detailed breakdown of the cost. The overall economic costs of the Chernobyl catastrophe which have to be covered by the national budget comprise a complex and extensive benefit programme (containing more than 50 different types of privileges and benefits in the late 1990s), expenditure on resettling families from contaminated zones, expenditure on physical infrastructure and housing (new buildings for resettled and evacuated families), expenditure on clean-up work and sealing off the reactor, radiation monitoring and research (Chernobyl Forum 2006). According to another source, a UNDP report from 2002 (UNDP 2002), the share of Chernobyl-related budget expenditures in Ukraine devoted to resettlement and improvement of living conditions has gradually decreased during the 1990s, while the proportion spend on "social protection" made up to 87 percent in 2000. In the years 2002 to 2004 about 77 percent of central government spending related to Chernobyl were allocated to "social assistance" (incl., e.g., food subsidies and free food for children of Chernobyl sufferers, pension payments, transport and communication services subsidies), between 9 to 13 percent were devoted to "medical assistance", between 2 to 5 percent were given to "resettlement related construction outlays" and between 5 and 13 percent were allocated towards "nuclear safety measures" (Osiatynski 2004).

who were resettled from the most affected regions hide their origin as they would be facing difficulties in finding a partner due to widespread fears of congenital anomalies (Oughton, Bay-Larsen and Voigt 2009). Also, resettlement itself had devastating psycho-social consequences so that some people who stayed behind in the most affected areas are in better psychological conditions than those resettled from the same areas (United Nations 2002).

3.2.2 Quantifying the amount of affected persons in Ukraine

Determining who has been affected by the nuclear disaster of Chernobyl is not an easy task. It is not clear whether to only define someone as a victim whose health has already deteriorated (and even so, by how much?). What about those who were exposed to a radioactive dose without having – physically – suffered so far? The problem of radiation lies in the fact that people were supposedly 'treated' with a specific dosage a long time ago, but that the effect of this treatment might or might not have manifested itself in potentially adverse somatic health outcomes and that there is uncertainty as to whether one eventually will suffer from long-term effects. Furthermore, there might be psychological effects leading individuals to actually feel negatively affected in their daily life and in their health (apart from officially diagnosed mental disease this form of affectedness might be difficult to capture using standard somatic health outcomes). Therefore, this study applies and compares two measures of affectedness: first, a self-assessment of respondents about whether their or any of their family members' health was affected by the disaster and second, official effective radiation doses in humans according to their place of living in 1986. Before discussing these two measures in more detail further below (see Section 3.3.2), this section will provide an overview of the scale of the disaster based on official numbers published by the State Statistics Committee of Ukraine (UkrStat) and the United Nations Scientific Committee on the Effects of Atomic Radiation (UNSCEAR).

According to UkrStat, the number of Ukrainian adults with the status of having been seriously affected by the Chernobyl catastrophe (recognized by the state as Chernobyl victim) was around 2 million adults on January 1, 2004 (this number excludes children, see Table 3-1), which corresponds to about 4.2 percent of the Ukrainian population of that year (overall 47.6 million inhabitants at the beginning of

2004). This number is very close to self-reported affectedness in the survey, which will be used in the following empirical analysis (UHBS data, wave from December 2002): in the sample, around 1,850,000 adults claim to have been seriously affected by Chernobyl.[44] Including children defined as Chernobyl disaster victims the number of officially registered Chernobyl victims rises to over 2.7 million persons (5.8 percent of the total population, see Table 3-1). The figures also show that the number of liquidators were about 320,000 in these years (about 0.68 percent of the total population). Although this is a large number, the liquidators make up only for a small fraction in the total number of registered victims.

Table 3-1: Persons registered as victims of the Chernobyl nuclear power station disaster, by type (absolute numbers and population shares)

	January 1, 2004		January 1, 2005	
	absolute number	*% of total population*	*absolute number*	*% of total population*
Total victims, persons	2,772,060	5.82%	2,646,106	5.60%
out of which				
Chernobyl disaster liquidators	324,332	0.68%	318,016	0.67%
Chernobyl disaster victims	1,689,941	3.55%	1,682,280	3.56%
Children defined as Chernobyl disaster victims	754,934	1.59%	643,030	1.36%
Other persons eligible for benefits	2,853	0.01%	2,780	0.01%
Families receiving benefits due to loss of breadwinner (whose death is related to Chernobyl disaster)	15,801		17,448	

Source: Information on absolute numbers from State Statistics Committee of Ukraine (2004, 2005); figures on the population shares are based on own calculations using population numbers for the present population at the beginning of each year (from the same source).

An UNSCEAR publication from the year 2000 has provided a somewhat lower estimate of the number of persons affected by Chernobyl (Table 3-2). However, this can partly be explained by the fact that evacuation and resettlements after 1986 are not accounted for and that the number of recovery operations workers was underestimated. Specifically, the complexity and difficulty in counting the number of affected persons can be seen from a more recent UNSCEAR report (UNSCEAR 2008) which adjusted the previous number of recovery operation

[44] The questionnaire does not specify what ‘seriously’ means.

workers (liquidators) upwards by 40 percent. Overall it is important to note that the comparability of numbers across studies and years is difficult due to different definitions of affected and base populations (numerator and denominator).

Table 3-2: Number of Chernobyl affected persons in Ukraine (UNSCEAR 2000)

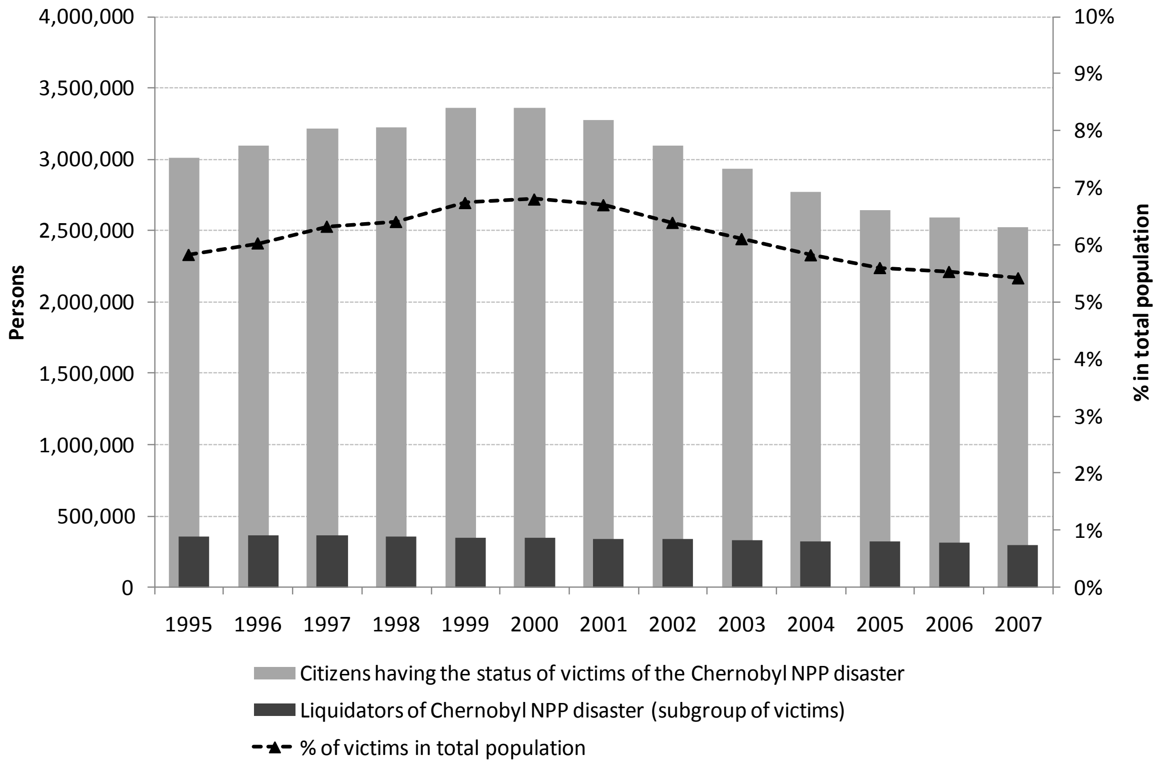
Recovery operation workers (liquidators)[a]	170,000
Evacuated in 1986[b]	91,406
Inhabitants of contaminated areas, >=37 kBq/m^2 (until 1995)[c]	1,295,600
Total	1,557,006

Source: UNSCEAR 2000; [a] See Table 18 of the report. The numbers refer to the years 1986-1989 in the Soviet Republic of Ukraine only. This number was raised in subsequent reports, however only giving aggregate numbers for the three affected countries (Belarus, Russia, Ukraine). [b] See Table 20 of the report. [c] See Table 26 of the report (distribution of inhabitants in 1995 of areas contaminated by the Chernobyl accident). The measurement unit kBq/m^2 is a measure of ground contamination with radioactive material. „Radioactivity from radionuclides (unstable atoms) is measured in becquerel (Bq) where 1 Bq = 1 disintegration per second and kBq/m2 = 1000 Bq of radionuclides over an area of 1 square metre." (WHO 2006). According to a publication by the European Commission (European Commission 1998), in Ukraine more than 11569 data points were used to construct a contour mapping of different levels of caesium-137 for the entire country (many of the data points for Ukraine represented aggregated values obtained from several thousands of original measurements). The measurements were based on soil sample analysis in the laboratory (soil depth of 200 mm) and airborne gamma spectrometry.

Over time, the number of registered victims initially rose but then has decreased steadily decreasing since the year 2000 (in absolute and relative terms). The numbers in Figure 3-1 reveal that the total number of registered victims went down to 2.5 million by 2007 and is likely to fall in the future due to the ageing of the most affected cohorts. Nevertheless, for the time being the number of official Chernobyl victims is still substantial and makes up a non-negligible part of the population.

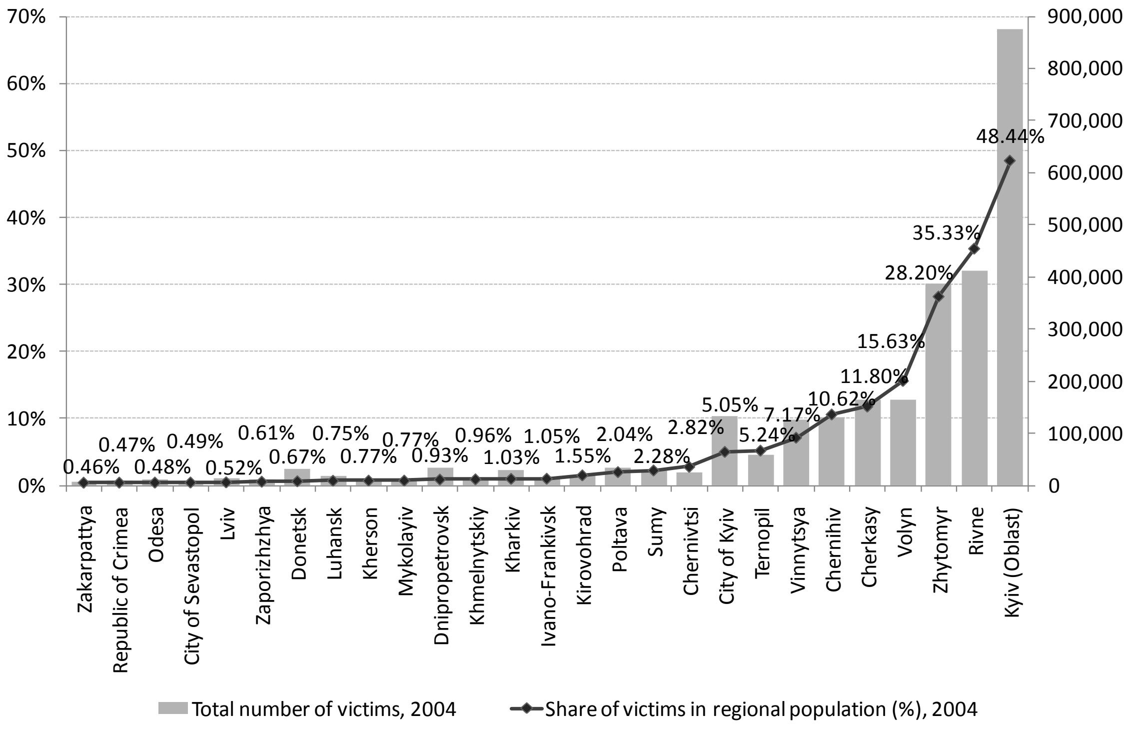
When summarizing these aggregate numbers it is also crucial to clarify that there was and is a substantial regional variation in the number of affected individuals – related to regionally heterogeneous radiation levels. The regions with by far the highest numbers of affected persons in absolute and relative terms are the Northern oblasts of Kiev (48.4 percent of the population are registered victims, i.e. almost 900,000 persons), Rivne (35.3 percent), Zhytomyr (28.2 percent), Volyn (15.6 percent) and Cherkasy (11.8 percent). In the most Western and Southern regions (Zakarpattya, Crimea, Odessa), the share of Chernobyl victims is less than half a percent.

Figure 3-1: Development of number of Chernobyl victims over time



Source: State Statistics Committee of Ukraine (2007)

Figure 3-2: Chernobyl victims by Oblast (region), 2004



Source: State Statistics Committee of Ukraine (2004)

3.2.3 Possible channels on subjective well-being and mental health

The aim of this study is to analyse whether there are long-lasting effects of the Chernobyl accident on subjective well-being and mental health 17 to 21 years after the event. Overall, there are at least three theoretical channels through which the 1986 nuclear catastrophe might have affected subjective life satisfaction and mental health. Since the catastrophe of Chernobyl was a truly exogenous and unanticipated event, the affected and unaffected populations should be on average comparable, as they were not systematically selected.[45] In other words, there should be no non-random selection into radiation exposure based on unobserved heterogeneity (see Section 3.4).

First, the exposure to radioactive fall-out (external exposure) and the intake of radionuclide through consumption of contaminated food or inhalation (internal exposure) might weaken the immune system of the body and lead to deteriorated physical health (*physical health channel*).[46] It is a highly debated issue whether the catastrophe of Chernobyl had long-term adverse somatic effects. In the health literature, higher cancer incidence rates, higher stillbirth incidence and higher mortality rates were recorded and controversially discussed (Ivanov, Gorski, Maksioutov, Tsyb and Souchkevitch 2001; Ivanov, Chekin, Parshin, Vlasov, Maksioutov, Tsyb, Andreev, Hoshi, Yamashita and Shibata 2005). Remennick (2002) shows in a study of immigrants from the former Soviet Union to Israel that health status of immigrants from contaminated regions was much lower than of immigrants from non-contaminated Soviet regions and that social adaptation was significantly poorer. The Chernobyl Forum, a group of eight United Nations organizations and the three most affected countries Belarus, Russia and Ukraine, has however reported that measurable health effects are of much lower scale than expected by common perception once one accounts for the intensified medical screening (United Nations 2002). Nevertheless, there seems to be consensus on a higher prevalence of thyroid cancer among children and adolescents in Belarus, Russia and Ukraine (UNSCEAR 2000). Lehmann and Wadsworth (2011) find a

[45] In the empirical analysis liquidators and evacuees will be excluded from the sample as they might have been a selected subsample of the population (e.g., military personnel). This will probably underestimate the size of the true effect.

[46] Radionuclides are atoms characterised by the instability of their nucleus. The instability implies radioactive decay through which gamma rays and subatomic particles are emitted.

negative association between Chernobyl exposure and subjective health status in Ukraine, while the association with objective health measures appears much weaker.

Second, the information policy of the Soviet government which deliberately concealed the scale and the danger of the accident in 1986 and thereby gave room to rumours about disastrous health consequences (Baloga, Kholosha and Evdin 2006; Gould 1990), the unresolved scientific debate on expected long-term health consequences as well as the inability to assess one's own type of affectedness have provoked deep rooted fears and uncertainty in the population (Abbott, Wallace and Beck 2006).[47] As a consequence, even physically healthy individuals might be afraid of falling ill. This worry and anxiety might manifest itself in lower subjective well-being, psychological distress or mental disease (*psychological channel*). Mental distress of people exposed to Chernobyl was found in numerous psychological studies, for both people still residing in affected areas (Havenaar, Rumyantzeva, van den Brink, Poelijoe, van den Bout, van Engeland and Koeter 1997) and those who emigrated abroad (Zilber and Lerner 1996; Cwikel, Abdelgani, Rozovski, Kordysh, Goldsmith and Quastel 2000). Symptoms related to the accident and the following events included, for instance headache, depression, sleep disturbance and emotional imbalance (UNSCEAR 2000). Significantly higher suicide rates among the Chernobyl affected population indicate the high mental toll associated to the catastrophe (Bromet and Havenaar 2007). However, psychological effects are also present when people care about others. Bridge (2004) finds that emotional stress of parents of disabled children in Ukraine is substantial and may cause second-order effects on their well-being and behaviour. Bromet, Goldgaber, Carlson, Panina, Golovakha, Gluzman, Gilbert, Gluzman, Lyubsky and Schwartz (2000) show that mothers of young children suffer from serious psychological trauma concerning the health status of their children. Self-abandonment, feeling of helplessness and lethargy have been described as mental reactions to uncertainty about one's own health status and the fear of suffering from cancer unknowledgeably (United Nations 2002). Overall, mental health stress has been less contradictory debated in the literature although studies have often used small samples.

[47] The study by Abbott, Wallace and Beck (2006) rests on qualitative case studies carried out in three different Chernobyl regions Belarus, Russia and Ukraine in 2003.

Third, there are potential second-order effects on economic success resulting from Chernobyl related impairment of physical or psychological health: for instance, worse labour market outcomes, lower income, deprivation or poverty. As Almond, Edlund and Palme (2009) show Swedish children exposed to the fallout have significantly lower educational outcomes – which in turn might lead to poorer labour market outcomes in the long-run. Thus, the catastrophe could also affect subjective well-being indirectly through these labour market and income channels (*indirect channel*). Perceived poverty is higher among those with lower mental well-being (Viinamäki, Kumpusalo, Myllykangas, Salomaa, Kumpusalo, Kolmakov, Ilchenko, Zhukowsky and Nissinen 1995). Loganovsky, Havenaar, Tintle, Guey, Kotov and Bromet (2008) find that affected clean-up workers are more often absent from their workplace due to mental stress.

3.3 Data and variables

The following subsections describe in more detail the two Ukrainian datasets as well as the variables used in this study. It also provides an overview of the basic descriptive statistics of the estimation sample.

3.3.1 Survey data

To analyse the long-term effects of a catastrophe like Chernobyl on subjective well-being requires data providing crucial retrospective information on place of living at the time of the accident (to identify the victims) as well as measures of radio-active irradiation and personal well-being. The Ukrainian Longitudinal Monitoring Survey (ULMS), a rich nationally representative panel data set, is a unique source fulfilling all these requirements. The survey was carried out in the summer months of 2003, 2004 and 2007 by the Kiev International Institute of Sociology (KIIS) comprising initially more than 8,500 adults aged 15 to 72 (see further information on the ULMS data in Section 2.4). The survey comprises an individual questionnaire covering information on socio-demographic characteristics, labour force participation and occupation, subjective well-being and health status as well as a household questionnaire focussing on household composition, incomes and expenditures. One of the main features of the ULMS which will be exploited in the analysis is a detailed coverage of the retrospective labour market history (and place of living history) of each individual starting in 1986 – the year of the Chernobyl catastrophe. The retrospective information is comparatively reliable in the Ukrainian context because employment details in the Soviet Union and later have been recorded in a worker specific labour booklet. Interviewers made use of these labour booklets whenever available.

The sample is restricted to individuals who were born before January 1987 – this includes all persons born before the catastrophe in April 1986 as well as those children *in utero* at that time (as Almond, Edlund and Palme 2009 demonstrate prenatal exposure was potentially harmful). Furthermore, persons who either acted as liquidators or who were evacuated from within the 30km exclusion zone or resettled as a consequence of the accident are not included in the regressions.[48] The latter two groups were exposed to the highest doses of external radiation—some of them with lethal doses of 6 Sv and more. The reasons for excluding these groups are that the aim is to shed light on the average population affected by low or moderate levels of radiation (and having a lower likelihood of suffering from somatic diseases) and that these particular persons received special treatment and attention (e.g., extra health checks and welfare supplements, see Lehmann and Wadsworth (2011)) so that they are likely to differ from the ordinary population.[49] Moreover, this approach circumvents the problems of selective assignment into clean-up work as well as selective survival of these strongly exposed individuals. The final sample amounts to 12,000 person-year observations. To be precise, the estimation results will be representative for the part of the current Ukrainian population which was not subject to evacuation or Chernobyl related liquidation work and will thus potentially underestimate the true costs of the catastrophe. It should also be noted that even within this part of the population it is possible that the most affected individuals will have had a lower survival probability until the year 2003 and hence a higher probability of being unavailable for the ULMS interviews (also because of potentially higher morbidity rates or being in hospitals or nursing homes; it could also be that affected individuals emigrated to other countries). Their absence from the sample should also generally weaken the results.

[48] Questions on whether individuals took part in the liquidation process (70 persons) or whether they were evacuated or resettled due to the Chernobyl catastrophe (52 persons) were only included in the ULMS survey in 2007.

[49] Evacuated and resettled persons also differ from other (inner) migrants who moved voluntarily, because they are likely to experience very different problems and chances in their new place of living.

A shortcoming of the ULMS is that it does not contain information of mental health status of the respondents. However, this information is available in another large Ukrainian micro-data set – the Ukrainian Household Budget Survey (UHBS) conducted by the Ukrainian Statistical Committee. This annual cross-sectional survey comprises household and individual level data and takes place each year in December. Each year around 24,000 individuals in about 9,500 households are interviewed. The survey contains an individual as well as a household questionnaire. Questions on mental health were included in the years 2004 to 2008, yielding a substantial sample size of more than 95,500 observations for the analysis. Importantly, in the UHBS survey, each individual is asked whether his/her health was not at all, somewhat or seriously affected by the Chernobyl catastrophe. The drawback of the UHBS data is that it lacks retrospective information on place of residence in 1986. To assure comparability between the two datasets, the UHBS sample is also restricted to respondents born before 1987 and not older than 72 years at the time of the interview.[50]

3.3.2 Self-reported and objective measures of being affected by the Chernobyl catastrophe

The ULMS 2003 wave contains the following question which is used to construct a binary measure of being affected by the catastrophe (self-reported affectedness): *"Was your health or the health of a family member affected by the catastrophe in Chernobyl?"* [51] The generated variable takes a value of unity for having been affected and zero otherwise. However, this self-reported measure of affectedness has to be treated with caution for at least two reasons: First, given the wording of the question, it is not clear whether the interviewed person was directly affected by the Chernobyl accident or not. Since the definition of *family* is rather

[50] A basic check of the similarity of the two samples was carried out by comparing specific variables: the results seem to suggest that the samples are rather similar. For instance, the share of smokers (see analysis of smoking behaviour in Section 3.9) in the UHBS (26.1 percent) is almost the same as in the ULMS (27.9 percent).

[51] Respondents could answer either yes or no. The question is located at the very end of the individual questionnaire (next to last question) in the subsection on ecology (containing four questions in total). Hence, since the question on life satisfaction (as well as on health and work) is covered earlier in the interview, it is highly unlikely that these answers are biased by having reminded respondents of the Chernobyl catastrophe (the ordering of the questions makes such emotional spill-overs impossible). Furthermore, since this question was only included in the 2003 ULMS wave, this 2003 answer is assigned to each individual in the other survey years as well (thus, emotional spill-overs due to the order of questions should be also highly unlikely for the life satisfaction answers in 2004 and 2007).

diffuse (the definition of family does not necessarily coincide with the definition of household in the survey) this self-reported variable provides a slightly blurred measure of affectedness (while the variable will measure *direct* individual affectedness with an error, it additionally captures possible *indirect* effects through affectedness of relatives and therefore provides a more comprehensive measure).[52]

Second, it is possible that the answers provided by interviewees are correlated with factors unobservable to the researcher (unobserved heterogeneity, e.g., omitted personality traits, household or family fixed effects). If these unobservable characteristics systematically and jointly determine the probabilities of reporting certain levels of life satisfaction as well as of reporting being affected by the Chernobyl catastrophe this will lead to biased estimates. Therefore, one of the main goals of this study is to analyse and test the validity and reliability of these self-reported measures of affectedness.

The corresponding Chernobyl question in the UHBS data is very similar: "*Has your health been affected by the Chernobyl catastrophe?"*[53] Respondents could answer either *"not at all", "somewhat affected" or "seriously affected"*. The advantage of this variable is that it is actually more refined than the ULMS question and more precisely targeted at the individual (rather than the family). However, it can still be influenced from omitted personality traits.

To this end, results based on the self-reported variable will be contrasted with estimations using official regional radiation and exposure data that can be matched to individuals based on their place of residence in the year 1986 (oblast level information).[54] This procedure follows the approach by Lehmann and Wadsworth (2011) who also use the retrospective location information to assign to each individual the corresponding measure of radioactive exposure. However, while Lehmann and Wadsworth (2011) employ settlement-level concentrations of caesium-137 deposition in their main analysis (surface contamination measured in kilobequerels per square metre, kBq/m^2), this study uses a measure of average

[52] If the variable of interest was direct *individual* affectedness, measurement error of the explanatory variable should lead to an attenuation bias (underestimation of the true effect).
[53] Translation by the author (the survey questionnaires are available in Ukrainian only.
[54] In the final estimation sample, there are about 500 person-year observations (4.5 percent of the sample) which did not used to life on Ukrainian territory in 1986. These individuals are assigned zero exposure doses.

effective *total* exposure doses (external and internal)[55] of iodine-131 and caesium-137 reflecting the energy absorbed by matter and measured in millisievert (mSv) (*variable name: radiation dose*).[56] While external exposure refers to irradiation from outside of the body, internal exposure denotes intake of radioactive material into the body through ingestion of food or inhalation. The relative importance of the external and internal exposure varied widely across regions in 1986: while the relative contribution of internal to total exposure was less than 30 percent in several settlements in Zhytomyrska oblast, it was almost 70 percent at the points of measurement in Vinnitska, Volynska and Cherkaska oblast (see Table 3.3.4 in the National Report by Baloga, Kholosha and Evdin 2006). Hence, the advantage of using a measure of the effective total dose (measured in millisievert, mSv) which combines all different types of radiation into a weighted dose taking into account the sensitivity of tissues and organs to get a measure reflecting the overall potential for causing harm to the human body (WHO 2011) is that it might capture the actual level of radioactive exposure and potential damage more comprehensively than a the measure of ground contamination by caesium-137 alone. This is especially true since household farming activities were widespread in the Soviet Union so that internal radiation was an important source of exposure. It should be noted, that Chernobyl-related radiation levels in our sample (as mentioned before we exclude liquidators and evacuees for several reasons) are modest and do mostly not exceed the amount of twice the natural background radiation. For the most affected children, the iodine-131 doses were stronger and equalled about 100 abdominal x-rays (for adults).

One caveat of radiation data in general is that it is only measured in certain location points (discrete sampling) and is then extrapolated to larger areas by the scientists.[57] In other words, individual level doses (based on individual medical examinations) are not available for the entire population and hence, the radiation variables reflect regional averages (data for the 26 Ukrainian oblasts). These

[55] Although iodine-131 was the most important source of exposure immediately after the accident while caesium-137 was relatively important, this relative importance changed over time due to the relatively short half-life of iodine-131 (about 8 days) and the comparatively long half-life of caesium-137 (half-life of 30.8 years).
[56] See also explanations in Almond, Edlund and Palme (2009) who use both types of measures in their analysis.
[57] The effective internal exposure was estimated based on almost 30,000 WBC measurements in 1986 (whole body counter; caesium-137 content in residents' organism; see Baloga, Kholosha and Evdin 2006). The average effective external exposure to caesium-137 takes into account measurements at all rural and urban settlement points.

averages hide intraregional variation in radioactive exposure (loss of precision in the measurement) which was caused by meteorological conditions (speed and direction of wind and rain) as well as natural borders and the roughness of the underlying surface (European Commission 1998). In an additional analysis, gender specific average thyroid doses absorbed by children and adolescents due to the fallout of radioiodine (especially iodine-131) will be used to investigate the long-run effect on individuals aged one to 18 years at the time of the accident.[58] Table 3-3 provides an overview of the radiation measures used in this study.

Table 3-3: Official exposure doses used in the empirical analysis

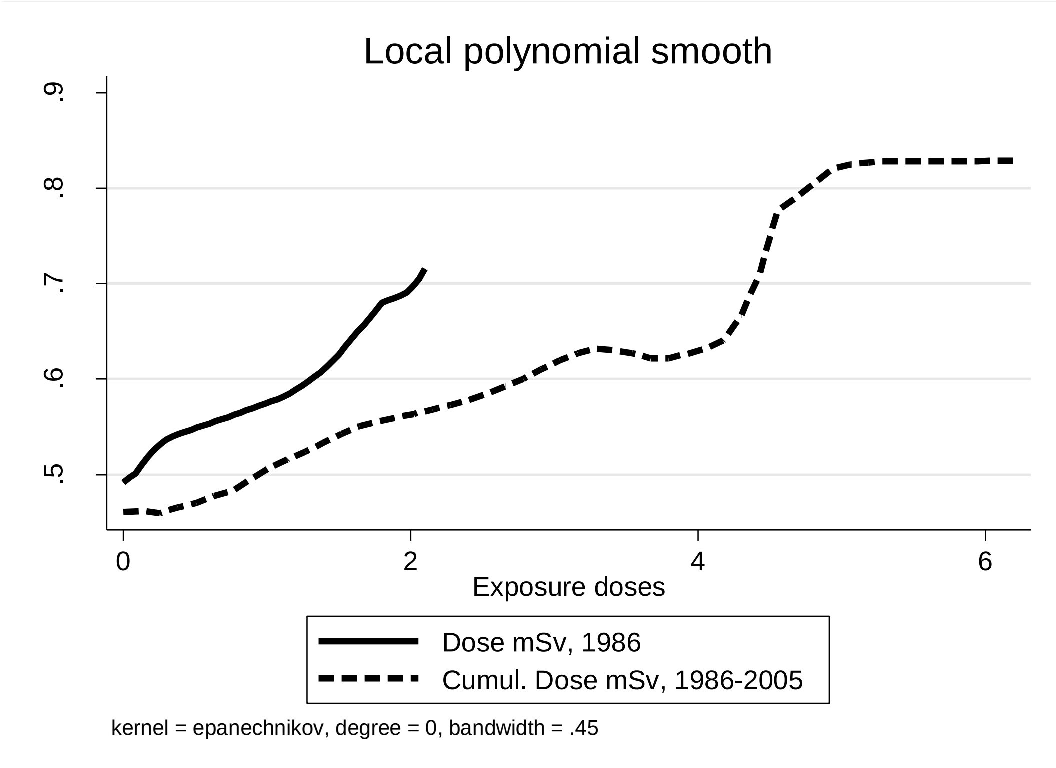
Measures of radiation
1. Average total (internal + external) exposure doses, accumulated in 1986, mSv [Variable name: Radiation dose]
2. Average total (internal + external) exposure doses, accumulated 1986-2005, mSv
3. Average thyroid doses (mGy) due to fallout of iodine-131, females aged 1-18 in 1986
4. Average thyroid doses (mGy) due to fallout of iodine-131, males aged 1-18 in 1986

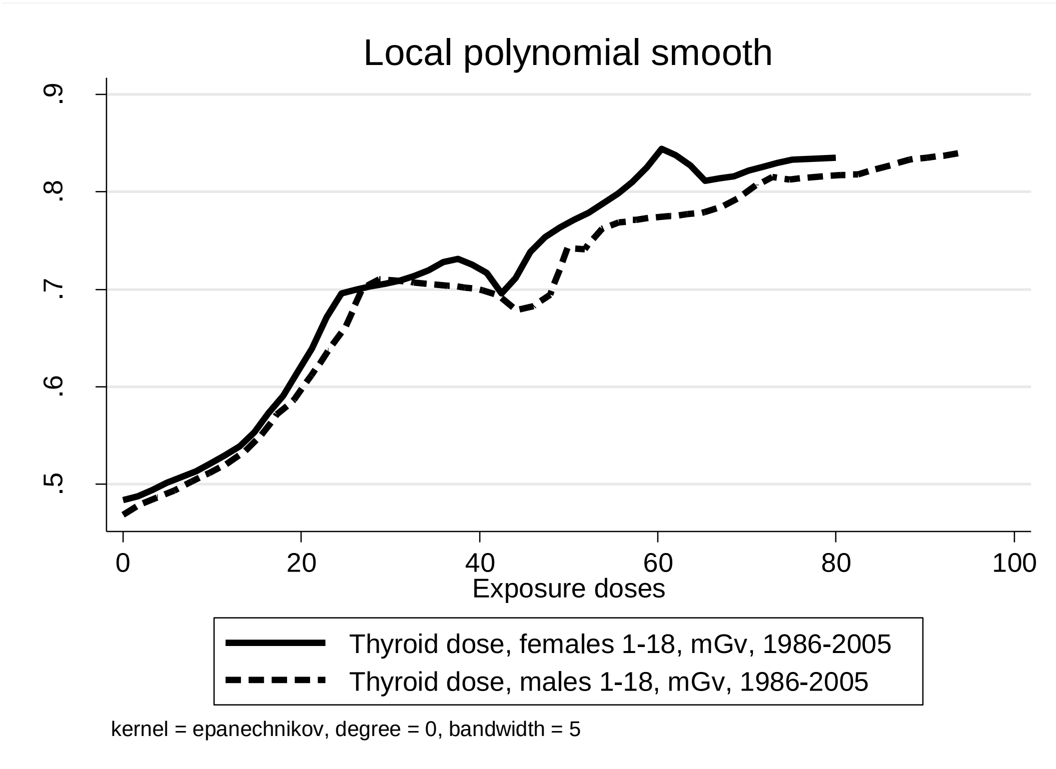
Notes: Data taken from the official report '20 years after Chornobyl Catastrophe. Future outlook: National Report of Ukraine', Tables 3.3.7 and 3.3.9 (Baloga, Kholosha and Evdin (2006), pages 45, 47, 48). While radiation doses of ground contamination measure the radioactivity of the material (in bequerel), the dose equivalent of ionising radiation measures the biological effects in the human organisms (in sievert; mSv – millisievert). The deposited energy is measured in gray (mGy - milligray).

The unconditional relationship between these official doses and the self-reported measure of affectedness is illustrated in the two graphs in Figure 3-3. These graphs plot estimates from smoothed kernel regressions of regional shares of self-reported affected individuals on objective average exposure doses. If self-reported measures were good representations of objective radiation one would expect a positive relationship. Indeed, both diagrams show a strong positive relationship between the two types of measures. These strong correlations will be the basis for the first stage in the instrumental variable approach, where the official radiation levels will be used to test for possible biases in the self-reported measure.

[58] These average regional absorbed thyroid doses are estimated based on 150,000 direct measurement of radioiodine activity in the thyroid gland of individuals living in the most contaminated regions (Baloga, Kholosha and Evdin 2006).

Figure 3-3: Relationship between official and self-reported measures of affectedness



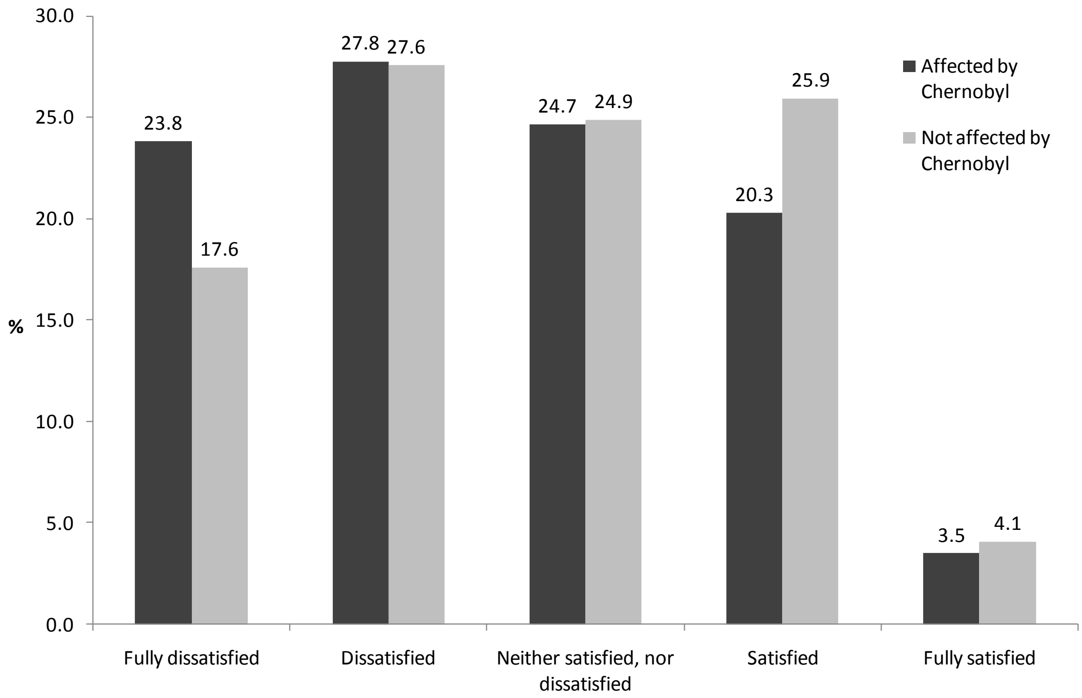


Notes: The deposited energy for the most affected children is equivalent to 100 abdominal X-ray scans for adults. Number of observations is 12,003. Source: ULMS 2003-2007, own calculations.

3.3.3 Outcome variables

Generally, subjective-wellbeing (utility) is not observable to the researcher. Therefore survey questions on the personal assessment of life satisfaction have been used as approximations in the literature. The justification for using these proxies rests on research during the past decades which has shown that life satisfaction is responsive to changes in external factors (Clark, Frijters and Shields 2008). The main dependent variable of individual subjective well-being used in the following analysis is measured on a five-point Likert scale from *fully dissatisfied* (1) to *fully satisfied* (5) and is based on the question: *"To what extent are you satisfied with your life in general at the present time?*"

Figure 3-4: Distribution of life satisfaction of affected and non-affected persons (self-reported measure; %)



Source: ULMS 2003-2007, estimation sample (number of observations: 11,065); own calculations.

The distribution of the responses to this question by self-reported affectedness is shown in Figure 3-4. The dark grey columns represent the answers of persons who say that they were affected by Chernobyl. Respondents in this group report being fully dissatisfied with their life in the period 2003 to 2007 more often than not affected persons (23.8 versus 17.6 percent). Conversely, individuals who say that they were not affected by Chernobyl have a higher likelihood to be satisfied or fully satisfied with their lives. Hence, this graph points to a negative relationship between self-reported affectedness and life satisfaction. A simple mean comparison test reveals that the difference between untreated persons (average life satisfaction: 2.68) and treated persons (average life satisfaction: 2.48) is highly significantly different from zero (difference: 0.20, std. error: 0.02; t-value: 10.74).

In addition to the five-point life satisfaction variable, a binary dependent variable (*unhappy*) was generated identifying all individuals who answered being fully dissatisfied with their life.

The assessment of the effect of the 1986 nuclear accident on subsequent mental health relies on two alternative outcome variables from the UHBS dataset: *depression* and *trauma*. These two variables are available in the UHBS surveys 2004 to 2008. *Depression* is a binary variable indicating persons reported to have 'chronic anxiety or depression' as a 6 months or longer illness or health problem. *Trauma* indicates respondents who have been diagnosed by a physician as suffering from a psychological trauma (post-traumatic stress).

In an extension to the main analysis, possible changes in subjective perceptions as well as behavioural consequences are investigated using the following additional dependent variables: (i) own survival probability to a specific age (ULMS 2007), (ii) currently smoking (binary variable indicating risky health behaviour; ULMS, UHBS) and (iii) dependency on social state transfers (UHBS). The latter variable represents the share of government transfers in personal income and is used to estimate the transfer dependency of a person affected by Chernobyl (these regressions control for employment status and the receipt of Chernobyl assistance).

3.3.4 Control variables

To control for possible differences in group composition as well as possible confounding effects (i.e., omitted variables), several sets of control variables relating to individual demographic and household characteristics, health status, work and wealth as well as personality traits will be included in the regressions. Furthermore, all regressions include a set of basic controls like survey year, interview month and region fixed effects. The standard socio-demographic controls are gender, age, marital status, education[59] and household size. In the literature age has been regularly found to exhibit a U-shaped impact on happiness (Blanchflower and Oswald 2008). Therefore a quadratic term is added to the regressions as well as a cubic in age which seems to further improve the fit of our regressions. Furthermore, log of per-capita household income and living space per capita (as a proxy for permanent income) are included to control for wealth status which has been shown to be positively correlated with subjective well-being in transition countries (Senik 2004; Blanchflower and Oswald 2004). The two health measures used in the analysis as explanatory variables are a dummy variable for all individuals having at least one out of seven different chronic diseases[60] (*chronic*) and a variable containing the individuals body mass index (*bmi*). As several of these controls might be endogenous in a life satisfaction regression, they are added in a stepwise fashion. Since the measure of Chernobyl affectedness is time-invariant, it is not possible to apply fixed effects estimation in order to control for time-invariant personality traits that are generally highly correlated with life satisfaction.[61] Nevertheless, following the suggestion by Ferrer-i-Carbonell and Frijters (2004) to account for this potential unobserved heterogeneity, the regressions will also include proxies for specific individual traits, in particular *extroversion* and *neuroticism*. Although the ULMS does not provide a full battery of questions to study psychological traits in detail, interviewers have to assess the respondent’s general behaviour and attitude at the end of each interview. Two of these questions are used to generate these two proxy variables (see Table A 2-2 in the Appendix to Chapter 2 for a more detailed description).[62]

[59] Education is recoded from highest educational degree into four educational categories.
[60] The seven categories are: heart disease, illness of the lungs, liver disease, kidney disease, gastrointestinal disease, spinal problems, or other chronic illnesses.
[61] Recall though that the individual level of affectedness of the nuclear accident should be orthogonal to the personality traits (as well as other unobserved heterogeneity) due to the randomness of the exogenous shock.
[62] A simple test on the stability of externally assessed traits over time shows substantial stability for extroversion, but more mixed evidence for neuroticism. It should be noted, that interviewers (who made the assessments) might differ over time.

Descriptive statistics of all these variables are provided in Table 3-4. The mean level of life satisfaction is 2.58 (with a standard deviation of 1.16). As established for other transition countries, average life satisfaction in Ukraine is lower than in industrialised Western economies (Selezneva 2011). About 22 percent of the sample report to be fully dissatisfied with their lives (*unhappy*) and 60 percent of the respondents answer that they were affected by the Chernobyl catastrophe. The majority of the sample lives in urban areas (town or city), is female (60 percent; this corresponds well to the gender gap documented in official national statistics, especially at older ages) and married (over 70 percent). The average age of respondents is about 46.5 years. About half of the sample is working in the reference week and about 20 percent of the observations are non-working pensioners in the official pension age. As regards the personality traits, only two percent of the observations are classified as ‘neurotic’, while 14 percent of the sample is classified as ‘extrovert’.

Table 3-4: Descriptive statistics (ULMS 2003-2007)

Variable	Mean	Min.	Max.	Number of observations
Dependent variables				
Life satisfaction (Std. deviation: 1.16)	2.58	1	5	12003
Unhappy	0.22	0	1	12003
Radiation measures				
Self-reported affectedness	0.60	0	1	12003
Radiation Dose (mSv)[63]	0.94	0	2.10	12003
Demographic and health controls				
Village (omitted category)	0.35	0	1	12003
Town	0.26	0	1	12003
City	0.39	0	1	12003
Male	0.40	0	1	12003
Age	46.56	17	75	12003
Age squared	2379.89	289	5625	12003
BMI	26.08	13.52	60.17	11270
Chronic disease	0.58	0	1	11789
Marital status and occupation				
Single (omitted category)	0.11	0	1	12003
Married	0.71	0	1	12003
Widowed	0.09	0	1	12003
Separated	0.09	0	1	12003
Working	0.54	0	1	12003
Unemployed	0.07	0	1	12003
Pensioner	0.24	0	1	12003
Inactive, working age	0.15	0	1	12003
Other occupation (omitted category)	0.05	0	1	12003
Household characteristics, wealth and education				
Household size	3.30	1	13	12003
Log household income	6.49	0	9.40	12003
Housing space per capita	23.21	0.67	152.00	12003
Primary education (omitted category)	0.18	0	1	12003
General secondary educ.	0.39	0	1	12003
Professional second. educ.	0.27	0	1	12003
Higher education	0.17	0	1	12003
Personality traits, Soviet job info				
Neurotic	0.02	0	1	12003
Extrovert	0.14	0	1	12003

Source: ULMS 2003-2007; own calculations.

[63] Average total (internal + external) exposure doses of caesium-137 and iodine-131, accumulated in 1986, mSv (source: National Report by Baloga, Kholosha and Evdin (eds.) 2006).

3.4 Methodology

In order to analyse the presence of long-term effects of the Chernobyl catastrophe on subjective well-being/mental health SWB_{it} the following model is estimated:

$$SWB_{it} = \beta_o + \beta_1 Affectedness_i + X\beta' + \varepsilon_{it}\,. \tag{3.1}$$

The coefficient of interest is β_1 which measures the impact of being affected by Chernobyl (according to the self-reported or official radiation measures) on subjective well-being. Should long-term psychological effects exist, the estimated $\hat{\beta}_1$ is expected to have a negative sign. Without adding further controls for potential transmission channels to the regressions, $\hat{\beta}_1$ should capture the overall or composite effect of the nuclear accident on today's life satisfaction and mental health. However, to shed light on possible transmission channels through which Chernobyl might have affected long-term well-being, different sets of control variables are included in X one after the other. Initially, pre-determined personal characteristics (gender and age) are added to the regressions to control for possible composition effects. This is followed by variables measuring the health status of individuals (*health channel*), marital status to capture possible effects of widowhood (*widowhood*), a set of dummy variables for current occupation (*labour force participation channel*), several education, income and wealth indicators (*human capital and income channel*) as well as proxies for extroversion and neuroticism (*personality traits*). If these sets of variables reflect different channels, their inclusion should gradually reduce to overall size of the $\hat{\beta}_1$ coefficient. All specifications control for type of settlement (village, town or city), region (26 oblasts) and year as well as month of interview fixed effects.[64]

As regards the estimation method, at first, pooled cross sectional regressions controlling for intrapersonal correlation of the error terms over time are estimated by ordinary least squares (pooled OLS with clustered standard errors). While OLS estimates are more easily and directly interpretable and are consistent under classical assumptions, they do not account for the categorical character of the dependent

[64] Clark and Oswald (2002) suggest the inclusion of day-of-the-week effects into well-being regressions. The inclusion of such controls changes the size of the coefficients of interest by less than one percent.

variable (and are therefore less efficient). To test the sensitivity of the results with respect to this estimation method, the same set of specifications will be re-estimated using ordered Probit methods (as argued by Ferrer-i-Carbonell and Frijters 2004 this should not change the results significantly). Furthermore, to account for the panel structure of the data (repeated individual observations over time) panel estimations will be performed as additional robustness checks. Due to the fact that *having been affected by the catastrophe* is a time invariant variable, it is not possible to perform fixed effects estimations (this is true for both, the self-reported and official radiation measures). Instead, random effects models will be estimated.

Another econometric issue which might threaten the validity and informational value of the estimated effects relates to the self-reported measure of affectedness. As already mentioned, the estimation will be biased if claiming to be affected by Chernobyl is endogenous (driven by omitted factors which simultaneously affect life satisfaction) or if it is plagued by measurement error (as the ULMS question alludes to family level rather than individual affectedness). The last aspect might be less problematic if the actual mechanism through which Chernobyl impacts subjective well-being involves family member’s health and (mental) well-being. Nevertheless, as regards the expected direction of these two potential biases, the second problem (measurement error) should lead to an attenuation bias, while the direction of the first potential bias is difficult to predict (there could be an upward bias (i.e. the effect could be overestimated) if, for instance, more risk averse individuals were more prone to report being affected as well as having lower levels of life satisfaction; another example would be, if persons with lower baseline levels of life satisfaction tend to claim to have been affected by the Chernobyl catastrophe in order to explain their lower happiness level).

To purge the estimates from these two potential biases individual self-reported affectedness will be instrumented using the official regional radiation doses. This approach is based on the implicit assumption that self-reported affectedness is related to radiation doses in the following way (first stage specification):

$$Affectedness_{it} = \beta_o + \beta_1 Radiation + X\beta' + \varepsilon_{it} \qquad (3.2)$$

Radiation is the objectively measured dosage that people living in particular regions have received according to their place of residence in 1986. X is a set of control variables. If the self-reported measures are biased, the instrumental variable approach should help to correct for both problems. The exclusion restriction of this instrumental variable approach assumes that there are no direct effects of radiation on life satisfaction – other than through perceived affectedness.

Another threat to the identification strategy would be if the 1986 location choice of individuals and families was endogenous, i.e. if, for instance, certain types were more likely to live in close proximity to potential sources of danger like nuclear power plants (Almond, Edlund and Palme 2009). However, this aspect does not seem to pose a threat to the current empirical analysis for at least three reasons: first, during the Soviet Union the geographical mobility of workers and families was highly regulated and monitored by the authorities (see also Section 2.3 in Chapter 2) so that location choice by individuals was rather limited; second, it is likely that the awareness of potential hazards by nuclear power plants was much lower before the Chernobyl disaster than afterwards (the fact that even the first days after the nuclear accident the event was concealed from the public and that Soviet mass media was prohibited to report about the recovery work[65] seems to support the idea that the public was not generally aware of potential dangers); third, the weather conditions caused substantial geographical variation in exposure doses so that the degree of radiation was not a simple monotonic function of distance to the nuclear power plant (Lehmann and Wadsworth 2011).

3.5 Is there a long-term effect of the Chernobyl catastrophe on subjective well-being?

3.5.1 Estimation results based on self-reported affectedness

The OLS estimation results based on the self-reported measure of affectedness are reported in Table 3-5. With only basic controls, the coefficient of interest is negative and highly significant suggesting a long-term negative impact of the Chernobyl catastrophe on subjective well-being (the estimate in column 1 corresponds to one sixth of a standard deviation).

[65] See Chapter 1 in the National Report by Baloga, Kholosha and Evdin (eds.) 2006.

Table 3-5: Self-reported affectedness and life satisfaction (Dependent variable: Life satisfaction; OLS estimations)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Self-reported affectedness	-0.190***	-0.115***	-0.101***	-0.106***	-0.099***	-0.098***	-0.098***
	(0.022)	(0.022)	(0.023)	(0.023)	(0.022)	(0.022)	(0.022)
Town	0.105***	0.097***	0.105***	0.114***	0.097***	0.042	0.041
	(0.028)	(0.027)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)
City	0.197***	0.178***	0.203***	0.221***	0.178***	0.072**	0.068**
	(0.027)	(0.026)	(0.027)	(0.027)	(0.027)	(0.028)	(0.028)
Male		0.082***	0.051**	0.022	-0.006	0.027	0.036
		(0.021)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)
Age		-0.118***	-0.119***	-0.162***	-0.199***	-0.188***	-0.186***
		(0.022)	(0.022)	(0.024)	(0.024)	(0.024)	(0.024)
Age squared		0.002***	0.002***	0.003***	0.004***	0.003***	0.003***
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age cubic[A]		-0.000***	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
BMI			0.004*	0.003	0.002	0.003	0.003
			(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Chronic disease			-0.290***	-0.284***	-0.258***	-0.248***	-0.249***
			(0.023)	(0.023)	(0.023)	(0.022)	(0.022)
Married				0.263***	0.241***	0.255***	0.249***
				(0.045)	(0.043)	(0.043)	(0.043)
Widowed				0.072	0.054	0.103*	0.100*
				(0.059)	(0.057)	(0.057)	(0.057)
Separated				0.003	-0.026	0.019	0.016
				(0.056)	(0.054)	(0.053)	(0.053)
Working					0.491**	0.402*	0.392*
					(0.229)	(0.218)	(0.220)
Unemployed					-0.239	-0.194	-0.200
					(0.231)	(0.221)	(0.222)
Pensioner					0.182	0.201	0.192

					(0.231)	(0.220)	(0.221)
Inactive					0.196	0.225	0.218
					(0.230)	(0.219)	(0.221)
Household size						-0.013	-0.012
						(0.010)	(0.010)
Log household income						0.173***	0.169***
						(0.016)	(0.016)
Housing space per capita						0.005***	0.005***
						(0.001)	(0.001)
General secondary educ.						0.030	0.028
						(0.031)	(0.031)
Professional second. educ.						0.147***	0.142***
						(0.033)	(0.033)
Higher education						0.378***	0.362***
						(0.037)	(0.038)
Neurotic							-0.210***
							(0.065)
Extroverted							0.122***
							(0.030)
Constant	3.099***	5.200***	5.122***	5.714***	5.921***	4.589***	4.552***
	(0.344)	(0.493)	(0.432)	(0.491)	(0.540)	(0.556)	(0.553)
R-squared	0.088	0.120	0.134	0.141	0.170	0.200	0.202

Notes: [A] The actual size of the estimated coefficient is -0.0000145 (column 2). All regressions control for region, year and interview month fixed effects. Number of observations is 12,003 (columns 1-2) and 11,065 (columns 3-7). Standard errors are clustered on the individual level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Source: ULMS 2003-2007, own calculations.

Adding basic demographic characteristics (gender and age, column 2) reduces the size of the estimated effect significantly (from -0.19 to -0.12). This seems to suggest that part of the estimated overall Chernobyl effect can be explained by gender and age differences (which in turn might be related to differential morbidity levels or psychological responses across age groups and gender). In general, men seem to be significantly more satisfied with their lives than women (however, this coefficient becomes smaller and insignificant once further controls are included in the estimation). Furthermore, the estimated age coefficients seem to support the notion that life satisfaction in Ukraine follows the U-shape pattern also found in other countries (Blanchflower and Oswald 2008).[66]

The inclusion of the two proxies for health status (column 3) reduces the Chernobyl coefficient only slightly, but leads to a drop in the estimated male coefficient (suggesting gender differences in health status). Perhaps not surprisingly, persons suffering from chronic illnesses have a lower life satisfaction than healthy persons. Starting from column 3, the estimated coefficient of being affected by the nuclear accident remains almost unchanged throughout all specifications (about -0.10 which corresponds to about one tenth of a standard deviation), suggesting only a minor direct role of these other possible channels. The separate reduced form estimates of having lived in regions that were affected by high radiation levels in the study by Lehmann and Wadsworth (2011) show that residents of these areas are slightly less attached to the labour market (lower probability of working and reduced working hours; but there seems to be no effect on monthly wages). Nevertheless, as regards the estimated effects of these other control variables on life satisfaction several important findings emerge: married persons and individuals in work are on average more satisfied with their lives, widowhood seems to be surprisingly positively related with life satisfaction. Furthermore, higher household income and wealth as well as higher levels of education are associated with higher levels of life satisfaction. The two indicators for personality traits seem to be significantly related to subjective well-being and show the expected sign: while neurotic persons are on average significantly less satisfied with their lives, extrovert individuals are more satisfied.

[66] Although the cubic coefficient is significantly negative, it is extremely small and excluding the cubic term from the regression does not affect the found U-shape in age, see Section 3.5.4.4.

These significantly negative findings of being affected by the Chernobyl catastrophe on subjective well-being based on the pooled OLS models also hold when estimating ordered Probit (pooled sample, clustering standard errors on the individual level) and random effects panel models (see Panel A in Table A 3-1 and Table A 3-2 in the appendix). The marginal effects for the five different satisfaction outcomes show that being affected by the nuclear accident significantly *increases* the probability of reporting lower levels of life satisfaction (being *fully dissatisfied* and *dissatisfied)* and *decreases* the probability of reporting higher levels of satisfaction with life. The size of the estimated coefficients based on the random effects panel models is almost identical to the pooled OLS regressions (however, the standard errors become slightly larger).

3.5.2 Testing causality using official radiation measures

To test whether the estimates based on the self-reported measure of affectedness suffer from endogeneity or measurement bias, the regressions are re-estimated using the official regional radiation doses to which individuals were exposed to during 1986 (according to their place of residence at that time). The coefficients of interest from these reduced form regressions are reported in Panel A in Table 3-6. Even though the estimates cannot be compared directly, since the self-reported measure is a binary variable while the radiation dose is a continuous variable, the qualitative findings remain the same. Throughout all specifications higher levels of radiation doses have a significantly negative impact on life satisfaction even 17 to 21 years after the nuclear accident. Having been exposed to a one millisievert higher radiation dose causes a drop in subjective well-being by 0.1 points on the five-point Likert scale. In contrast to the results using the self-reported measure, the estimates using radiation doses are very stable across specifications suggesting that the effect is largely unaffected by any of the controls. Still, the results from these reduced form regressions provide first evidence that the estimates based on the self-reported measures are not completely driven by biases (spurious correlations). The same is true when repeating the ordered Probit regressions using the official radiation doses: higher doses reduce the probability to have higher levels of life satisfaction and increase the likelihood of being fully dissatisfied with life (see Panel B of Table A 3-1).

Table 3-6: Causal effects of the Chernobyl catastrophe on life satisfaction (OLS and 2SLS estimations)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Reduced form (pooled OLS)							
Dependent variable				*Life satisfaction*			
Radiation dose (mSv)	-0.101**	-0.083*	-0.085*	-0.092**	-0.113**	-0.097**	-0.097**
	(0.046)	(0.046)	(0.047)	(0.047)	(0.046)	(0.045)	(0.045)
R-squared	0.082	0.118	0.133	0.140	0.169	0.198	0.200
B. First stage (2SLS)							
Dependent variable				*Self-reported affectedness*			
Radiation dose (mSv)	0.084***	0.075***	0.081***	0.080***	0.081***	0.078***	0.078***
	(0.019)	(0.018)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)
R-squared	0.1237	0.1555	0.1604	0.1612	0.1616	0.1621	0.1625
F-statistics	20.31	16.87	18.91	18.30	18.93	17.21	17.16
C. Second stage (2SLS)							
Dependent variable				*Life satisfaction*			
Instrumented self-reported	-1.196**	-1.106*	-1.041*	-1.147*	-1.387**	-1.248**	-1.248**
affectedness	(0.569)	(0.627)	(0.587)	(0.601)	(0.603)	(0.615)	(0.617)
Region & time FE	✓	✓	✓	✓	✓	✓	✓
Demographic controls	-	✓	✓	✓	✓	✓	✓
Health controls	-	-	✓	✓	✓	✓	✓
Marital status	-	-	-	✓	✓	✓	✓
Work status	-	-	-	-	✓	✓	✓
Income, wealth, HC	-	-	-	-	-	✓	✓
Traits	-	-	-	-	-	-	✓
Observations	12,003	12,003	11,065	11,065	11,065	11,065	11,065

Notes: Panel A contains the estimated coefficients of interest from pooled OLS regressions; Panel B and C report those from the first and second stage of the instrumental variable estimation (pooled 2SLS). Standard errors are clustered on the individual level. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Source: ULMS 2003-2007, own calculations.

However, a more powerful test of the OLS results based on the self-reported measures is to estimate instrumental variable regressions using the official radiation measures of 1986 as instruments for the self-reported affectedness. The corresponding first and second stage results are shown in Panel B and C (Table 3-6) respectively. The results of the first stage reveal that official radiation doses have a significantly positive effect on the likelihood of reporting to be affected by Chernobyl (the size of the effect is very similar across the different specifications). Furthermore, the instrument is powerful as suggested by the F-statistics of the instrument in the first stage regressions which are well above the critical value of 10 (Staiger and Stock 1997). Turning to the results of the second stage, the negative effect of Chernobyl on subjective well-being remains significant and becomes now even larger across all specifications (the significance levels decrease slightly due to loss of precision of the estimates). The size of the coefficients indicates that having been affected by Chernobyl reduces subjective well-being by about one standard deviation which is a substantial effect.[67] This finding seems to suggest that the naïve OLS regressions were downward biased (potentially due to an attenuation bias). However, in almost all specifications the IV estimates are not significantly different from the naïve OLS estimates and hence any interpretation in terms of an attenuation biases could be misleading (the standard errors of the 2SLS estimates are large). Overall though, these findings seem to suggest that the results based on the self-reported measures of affectedness have indeed a causal meaning and seem to represent a lower bound estimate of the effect of Chernobyl on subjective well-being.

3.5.3 Alternative dependent variable: being unhappy (binary variable)

The same set of regressions was also estimated using a simplified version of the dependent variable. To this end, the categorical life satisfaction variable was collapsed into a binary indicator taking the value ‘1’ for all persons reporting to be fully dissatisfied with their lives and ‘0’ for all other values. The results in Table 3-7 confirm the pattern and findings of the main specifications. Note that the sign of the estimated effect is reversed since the dependent variable is now ‘unhappy’.

[67] This pattern of the results also generally hold in the instrumental variable panel data estimations (G2SLS-RE; see the lower panels in Table A 3-2 in the appendix). However, with increasing standard errors the significance levels are slightly lower and some of the coefficients in the second stage become only borderline significant (with a p-value of less than 0.15).

Table 3-7: Causal effects of the Chernobyl catastrophe on ‘being unhappy’ (Probit and 2SLS regressions)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Naïve regressions (Probit)							
Dependent variable				*Unhappy (0/1)*			
Self-reported affectedness	0.054***	0.036***	0.034***	0.037***	0.035***	0.034***	0.034***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
B. Reduced form (Probit)							
Dependent variable				*Unhappy (0/1)*			
Radiation dose (mSv)	0.050***	0.042**	0.036**	0.038**	0.048***	0.044**	0.045**
	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)
C. First stage (2SLS)							
Dependent variable				*Self-reported affectedness*			
Radiation dose (mSv)	0.084***	0.075***	0.081***	0.080***	0.081***	0.078***	0.078***
	(0.019)	(0.018)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)
F-statistics	20.31	16.87	18.91	18.31	18.93	17.21	17.16
D. Second stage (2SLS)							
Dependent variable				*Unhappy (0/1)*			
Instrumented self-reported affectedness	0.561***	0.559**	0.486**	0.517**	0.593***	0.570**	0.582**
	(0.211)	(0.234)	(0.216)	(0.222)	(0.223)	(0.231)	(0.233)
Region & time FE	✓	✓	✓	✓	✓	✓	✓
Demographic controls	-	✓	✓	✓	✓	✓	✓
Health controls	-	-	✓	✓	✓	✓	✓
Marital status	-	-	-	✓	✓	✓	✓
Work status	-	-	-	-	✓	✓	✓
Income, wealth, HC	-	-	-	-	-	✓	✓
Traits	-	-	-	-	-	-	✓
Observations	12,003	12,003	11,065	11,065	11,065	11,065	11,065

Notes: Panel A and B report marginal effect from pooled Probit regressions for the binary variable ‘*unhappy*’; Panel C and D report the estimated coefficients from the 2SLS regressions (linear probability models). The variable ‘*unhappy*’ indicates individuals answering ‘*fully unsatisfied*’ on the life satisfaction question. Standard errors are clustered on the individual level. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Source: ULMS 2003-2007, own calculations.

The marginal effects from the naïve Probit regressions using the self-reported affectedness measure as well as the reduced form Probit regressions imply that having been affected by the Chernobyl catastrophe significantly increases the likelihood that individuals are unhappy with their life (by about 3.5 to 5 percentage points). Furthermore, using the official radiation doses as instruments for the self-reported measure of affectedness increases the estimated coefficient (and the standard errors) substantially. Hence, these new 2SLS results confirm the findings from the naïve estimates which rather tend to *underestimate* the full effect of the nuclear accident on subjective well-being (this is also true when estimating these specifications using random effects panel models; see Table A 3-3 in the appendix).

3.5.4 Further sensitivity analyses and robustness checks

Several additional analyses were performed in order to test the robustness of the main findings with respect to different potential threats. These tests and their results are summarized in the following subsections.

3.5.4.1 Personality traits and self-reported affectedness

Although the instrumental variable results already suggest that the estimated effects based on self-reported affectedness are not spuriously created through confounding omitted variables, it is possible to provide further support for this claim in the following way. In order to test whether individual personality traits influence the likelihood of answering being affected by the Chernobyl catastrophe (which they should not if the shock was truly random), simple regressions were estimated using the two proxies for extroversion and neuroticism as explanatory variables. The results of these regressions (pooled OLS) are reported in Table 3-8 (the table contains only the two coefficients of interest; the full set of results is provided in Table A 3-4 in the appendix). In contrast to the life satisfaction regressions in which both traits played a significant role, their effect on the propensity to report being affected by the nuclear accident is close to zero and insignificant (irrespective of the set of included control variables). It is reassuring that the results of these regressions reveal that the two traits do not explain any of the variation in self-reported affectedness. Generally, self-reported affectedness is only weakly correlated with demographic and household controls; the exception is the significantly negative male coefficient. Probably not surprisingly, health status (column 4) is positively associated to the propensity to report affectedness; it seems likely that there is reverse causation (affectedness affecting health) so that the health coefficients effects should not be interpreted in a causal way.

Table 3-8: Personality traits and self-reported affectedness (pooled OLS)

	(1)	(2)	(3)	(4)
Dependent variable	*Self-reported affectedness (0/1)*			
Neurotic	0.018	0.026	0.026	0.033
	(0.028)	(0.027)	(0.027)	(0.029)
Extrovert	0.009	-0.001	-0.002	-0.002
	(0.013)	(0.013)	(0.013)	(0.013)
Demographic, marital status	-	✓	✓	✓
Occupation and education	-	✓	✓	✓
Household income & wealth	-	-	✓	✓
Health variables	-	-	-	✓
R-squared	0.122	0.156	0.156	0.162

Notes: The table reports selected coefficients only. The full list of results is provided in Table A 3-4 in the appendix. All regressions control for year, month of interview and region fixed effects. Results remain unaffected when controls for official radiation doses are included. Number of observations is 12,003 (columns 1-3) and 11,065 (column 4). Standard errors are clustered on the individual level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Source: ULMS 2003-2007; own calculations.

3.5.4.2 Separate exclusion of most affected regions

The employed radiation doses refer to regional averages and the number of highly affected regions is rather limited so that the estimation results using these official radiation measures could potentially be driven by one specific region.

Therefore, six reduced form regressions of radiation doses on life satisfaction were estimated excluding each of the most affected regions one at a time (Table 3-9). The test confirms that the estimates are clearly robust to these omissions and not driven by any particular region with specific features (e.g., Kiev city being the capital of the country).

Table 3-9: Robustness check: separate omission of most affected regions

Dependent variable	*Life satisfaction*					
	(1) Without Kiev oblast	(2) Without Zhytomyr oblast	(3) Without Cherkasy oblast	(4) Without Rivne oblast	(5) Without Chernihiv oblast	(6) Without Kiev city
Radiation dose	-0.096**	-0.118**	-0.137***	-0.103**	-0.125***	-0.109**
	(0.047)	(0.048)	(0.047)	(0.046)	(0.046)	(0.047)
Demographic controls	✓	✓	✓	✓	✓	✓
Household controls	✓	✓	✓	✓	✓	✓
Health & traits	✓	✓	✓	✓	✓	✓
Observations	10751	10767	10743	10823	10870	10565
R-squared	0.199	0.199	0.204	0.195	0.205	0.201

Notes: Pooled OLS regressions. All regressions include full set of controls. Standard errors are clustered on the individual level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Source: ULMS 2003-2007; own calculations.

3.5.4.3 Falsification exercises

In order to further assess the credibility of the self-reported affectedness measures, two ‘falsification exercises’ are conducted using survey respondents who are less likely to have been immediately affected by the accident: the first group consists of young individuals who were born at least one year *after* the accident and the second group is made up of persons living in completely unaffected households according to the self-reported measure, i.e. households where all members answer that they have not been affected by the Chernobyl catastrophe.

The first robustness check asks to what extent self-reported affectedness reflects ‘personal’ affectedness of the survey respondent (instead of family affectedness). Those who were born after 1986 cannot have been personally affected by the most immediate impact of the catastrophe (especially through iodine-131 which has a half-life of about 8 days).[68] Interestingly, although the resulting coefficient has almost the same size as for those born before 1986 the coefficient is now insignificant (Table 3-10). This result indicates that individual subjective well-

[68] Altruistic feelings towards affected family members could theoretically still play a role. Hence this test also helps to understand to what extent this family spill-over matters. Generally though, children who were born after the accident and grew up in contaminated areas will have accumulated some radiation over time.

being in the post-disaster generation is on average not significantly related to the affectedness of other family members.[69]

The second question asks whether differences in official 1986 radiation measures assigned to each household member according to his/her place of residence in 1986, can generate significant differences in life satisfaction among households in which all members respond that no family member was affected by the catastrophe. If the self-reported measures are reliable, then the official measure should have no significant effect on life satisfaction in this particular subgroup. Indeed, the coefficient of the radiation dose is insignificant providing further credibility to the self-reported measure of affectedness. Overall, the results from these falsification exercises suggest that individuals and households respond rather accurately to the question on being affected by Chernobyl.

Table 3-10: Robustness checks with unaffected samples

Dependent variable	*Life satisfaction*	
Sample	(1) Born after 1986	(2) Completely unaffected households
Self-reported affectedness	-0.109	
	(0.104)	
Radiation dose		-0.081
		(0.075)
Demographic controls	✓	✓
Household controls	✓	✓
Health & traits	✓	✓
Observations	564	4,309
R-squared	0.210	0.216

Notes: All regressions control for full set of controls. Standard errors are clustered on the individual level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Source: ULMS 2003-2007; own calculations.

3.5.4.4 Different age specifications

Some authors have argued that it might be preferable to control for the natural logarithm of age in order to account for the subjective feeling that the years pass faster as individuals age (van Praag and Baarsma 2005). Therefore, the regression specification including the full set of controls was re-estimated using various age

[69] However, since the remaining sample is very small, the estimation of the coefficients might be imprecise (leading to larger standard errors rendering the effects insignificant). Thus, the power of this test might be limited.

specifications, i.e. including only age (i), age and age squared (ii) and age, age squared and age cubic (iii) in simple as well as in logarithmic form. The reduced form results of the radiation dose on life satisfaction are robust to these modifications (the estimated radiation effect changes only marginally; see Table A 3-5 in the appendix).

3.6 Long-term effects on mental health

As suggested by several psychological studies (see Baloga, Kholosha and Evdin 2006) as well as by the study by Lehmann and Wadsworth (2011) for Ukraine, the long-lasting toll of the Chernobyl catastrophe for the Ukrainian population as a whole works mainly through mental distress and subjective perceptions of poor health rather than through measurable somatic health effects. The empirical results of a negative long-term effect on subjective well-being shown in the previous section provide further evidence for this channel.

To shed more light on this psychological channel this section investigates the effect of the 1986 nuclear accident on two alternative subjective well-being measures which are more directly related to mental health. The UHBS questionnaires of the years 2004 to 2008 contained a health section, in which individuals were asked to provide information on their somatic and psychological diseases. From this list of questions, two binary indicator variables will be used as dependent variables in the analysis: suffering from (i) psychological trauma (diagnosed by a doctor) and/or (ii) depression (see detailed data description in Section 3.3.3). Given that the data is self-reported and partly subjective information it is possible that these variables are plagued by measurement error (through under- or over-reporting due to stigma, for instance). However, as long as the measurement error does only affect the dependent variable and not systematically related to any of the explanatory variables this only leads to less precise, but not biased estimates (since it increases the variance of the error term, see Wooldridge (2002, chapter 4)). Generally, the fraction of individuals suffering from depression is much lower in the UHBS sample than in many Western countries. This could potentially be explained by cultural norms: mental diseases tend to be more strongly related to stigma and less well diagnosed in Eastern European countries than in Western countries. Nevertheless, given the possible role of stigma associated with medically assessed psychological illnesses and doctor visits, it might turn out beneficial that the survey question on depression asks for subjective assessments of the individuals (and not about officially diagnosed illnesses).[70]

In order to identify individuals who have been affected by the Chernobyl catastrophe, the following analysis exploits data on self-reported affectedness. A binary variable is coded as '1' if respondents answered that they were either *somewhat* or *seriously affected* and '0' otherwise. The advantage of this measure is that it is actually more refined than the ULMS question and more precisely targeted at the individual (rather than the family). One shortcoming of the UHBS dataset is, however, that it does not contain any information on the place of living in 1986. Hence, the official radiation measures cannot be linked to a respondent's location at the time of the catastrophe in order to perform similar tests on the reliability of the self-reported affectedness measure as was done for the ULMS data. Nevertheless, the preceding ULMS analysis has demonstrated that the self-reported measures of affectedness appear to have a causal meaning and are not simply spurious results based on omitted personality traits. Thus, given the findings in the previous section in combination with the more refined affectedness measure in the new data set, there seems to be substantial supportive evidence for taking the results using self-reported affectedness measure as lower bound estimates for the causal effect of the Chernobyl catastrophe on mental health.[71]

Moreover, the analysis based on the self-reported affectedness will be complemented by an amended test making use of the official radiation measures. Instead of assigning the radiation doses to the respondent's place of living in 1986, the 1986 radiation doses are assigned according to the current residence (oblast). Since people in Post-Soviet Ukraine are in principle free to move to their preferred location and this location choice is likely to be endogenous, this test is likely to be less powerful.[72] However, the level of mobility from 1986 to 2003 was very low in Ukraine (especially among the older population). Lack of housing, liquidity constraints and other administrative barriers kept mobility very low even after the

[70] The ideal dataset would consist of a compulsory medical assessment of the entire population to circumvent the problem of self-selection into seeking medical examinations and treatment.
[71] Unfortunately, there are no variables on life satisfaction in the UHBS data to test whether the results based on the ULMS data can be replicated with this second dataset.
[72] The analysis by Lehmann and Wadsworth (2011) seems to suggest that mobility is slightly lower among Ukrainians living in contaminated areas rather than in other regions of the country.

collapse of the Soviet Union (as shown for Russia by Andrienko and Guriev 2004). In fact, the ULMS data reveal that only 7.4 percent of the sample in 2003 lived in a different region as compared to 1986.[73] Furthermore, less than one percent of those who moved to another region related the motivation behind their change of residence to the Chernobyl catastrophe (the ULMS questionnaire asks respondents to give the reasons for their moves).[74] Given this background information on comparatively limited mobility, the empirical problem due to potentially endogenous location choice should be less severe.

Table 3-11 presents the results from the mental health regressions based on five cross-sections of the UHBS. The first three columns refer to the regressions on trauma, while the other three columns denote the results related to depression – in each case controlling for the full set of covariates. The three reported estimates stem from (i) the naïve OLS regressions using the self-reported affectedness measure, (ii) the reduced form OLS regressions based on the regional radiation doses and (iii) the 2SLS estimations in which the self-reported affectedness is instrumented by the official radiation measures. The lower panel reports the first stage results from the 2SLS regressions.

The regression results based on the naïve OLS regression reveal that being affected by Chernobyl significantly increases the likelihood of suffering from psychological trauma or depression (see columns 1 and 4 respectively). This significantly negative long-term effect on mental health is also found when using the official radiation doses as a measure of affectedness (reduced form regressions, columns 2 and 4). The 2SLS estimates (with a highly significant first stage) provide further evidence that this long-term effect on mental health is indeed causally related to the nuclear accident from 1986. Hence, these results based on a second data set and using alternative measures of subjective well-being once more seem to confirm the long-lasting burden of large parts of the Ukrainian society due to the Chernobyl catastrophe which is manifested in lower subjective well-being and mental health.

[73] This refers to the estimation sample and includes persons who used to live outside the territory of Ukraine in 1986.
[74] Individuals were asked for their reasons of changing residence. The list of answers included 21 items. Most often, individuals changed residence because they moved out of their parents' home, married, purchased an apartment, were released from military service or started studying.

Table 3-11: The Chernobyl effect on mental health (UHBS 2004-2008)

	(1) OLS (naïve)	(2) OLS (reduced form)	(3) 2SLS (2nd stage)	(4) OLS (naïve)	(5) OLS (reduced form)	(6) 2SLS (2nd stage)
Dependent variable		*Trauma*			*Depression*	
Self-reported affectedness	0.003***		0.007***	0.003***		0.003*
	(0.001)		(0.002)	(0.001)		(0.002)
Radiation dose (Reduced form)		0.002***			0.001*	
		(0.001)			(0.001)	
First stage						
Dependent variable: Self-reported affectedness						
Radiation dose			0.338***			0.338***
			(0.003)			(0.003)
F-statistic			12,507			12,507
Age, age squared	✓	✓	✓	✓	✓	✓
Gender	✓	✓	✓	✓	✓	✓
Education	✓	✓	✓	✓	✓	✓
Employment status	✓	✓	✓	✓	✓	✓
Settlement FE	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓
Observations	95,452	95,452	95,452	95,452	95,452	95,452
R-squared	0.001	0.001	0.001	0.003	0.003	0.003

Notes: Sample for the years 2004-2008. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Source: UHBS 2004-2008; own calculations.

3.7 Compensation and state transfers

This section evaluates the aggregate toll of the Chernobyl disaster on subjective well-being in terms of required monetary compensating differentials (using the ULMS data). Additionally, it sheds more light on the relationship between affectedness and the existing state transfer system.

3.7.1 Estimation of the monetary value of the aggregate utility loss

As implied by the previous results on subjective well-being the disaster at the Chernobyl nuclear power plant exerted a large negative externality on the population. In most countries there are no comprehensive insurance schemes for nuclear plants and possible nuclear accidents. Furthermore, there are generally no available mechanisms through which individuals can insure themselves against such nuclear accidents. In other words, in case of emergency, it is most likely the state which has to bear the costs of the accident and to compensate individuals for the suffered damage. The nuclear accident of Fukushima (Japan 2011) showed that the state might have to bear a substantial part of the costs even when nuclear power plants are privately owned.

Using the estimates from the life satisfaction regressions in Section 3.5 the following calculations will provide an estimate of the monetary equivalent of the suffered loss in subjective well-being. In particular, it is possible to compute the amount of monetary compensation required to equalise the well-being of affected and non-affected groups of individuals (Clark and Oswald 2002; van Praag and Baarsma 2005; Winkelmann and Winkelmann 1998). This approach interprets equation (3.1) as a utility function where life satisfaction is assumed to proxy for direct experienced utility. Using the relative size of the affectedness coefficient to the income coefficient, it is possible to compute the compensating differential in monetary terms for the average individual. In other words, this method helps to assess the monetary equivalent required to raise the lower subjective well-being of affected individuals up to the level of unaffected persons.

Since the income measure enters the regression equation in log-form the statistical relationship between these two measures corresponds to a semi-log functional form in which the estimated income coefficient $\hat{\beta}_{inc}$ gives the change in the dependent variable (ΔSWB) due to a change in the log of income. In the meantime, the loss in subjective well-being due to being affected by the Chernobyl disaster is simply given by $\hat{\beta}_{affect}$.

$$SWB_i = \beta_o + \beta_{affect} Affectedness_i + \beta_{inc} \log(income_i) + X'\beta + \varepsilon_i \qquad (3.3)$$

The relative income change required for these two opposing effects to neutralize each other can be expressed by the following equation:

$$compensating\ income\ differential = \exp\left(\frac{-\hat{\beta}_{affect}}{\hat{\beta}_{inc}} \right) \qquad (3.4)$$

where the compensating income differential represents the ratio of the compensated income level over the uncompensated income level ($income_{compensated} / income_{uncompensated}$).

In order to express the monetary value of this required income change one has to multiply the compensating income differential with the uncompensated income level and calculate the difference between the compensated and uncompensated income levels. The results of these calculations are presented in Table 3-12.

Table 3-12: Compensating differentials and share of total compensation in GDP

	Set of controls	$\hat{\beta}_{affect}$	$\hat{\beta}_{inc}$	$\exp\left(\frac{-\hat{\beta}_{affect}}{\hat{\beta}_{inc}}\right)$	Compensating differential (in Hryvnia)	Share of GDP
I.	excluding health proxies	-0.114	0.178	1.90	833.4	10.8%
II.	including health proxies	-0.098	0.169	1.79	729.9	9.5%

Notes: Based on self-reported affectedness measure. Unless otherwise noted, the estimates stem from regressions including the full set of controls as in Table 3-5, column 7. All reported coefficients are significantly different from zero.

The income measure used in the regressions and the compensation calculations is total monthly household income (all values expressed in June 2004 values).[75] The estimates of $\hat{\beta}_{affect}$ and $\hat{\beta}_{inc}$ are taken from the main specification using the self-reported affectedness measure (i.e. using the lower bound estimate of $\hat{\beta}_{affect}$). With an average uncompensated real income of 928.8 UAH, the compensation amounts to substantial 833.4 UAH per affected household and month. This equals around 90 percent of average monthly household income. A back-of-the-envelope calculation of the fiscal costs of such a compensatory policy shows that the

[75] The household income measure includes all kinds of payments (including payments in the form of goods and services) and transfers that the household received in the last month (after tax). There are several advantages of using household instead of individual income: households tend to pool of resources and also have joint expenditures, the measure of household income provides a more complete assessment of non-wage income sources (some transfers are paid to households/families and not to individuals) and it is less dependent on an individual's labour market decision (which can be endogenous to health or Chernobyl affectedness).

Ukrainian government would have to additionally spend between 9.5 and 10.8 percent of annual GDP in order to pay for full compensation.[76] Given that the government already spends five to seven percent of annual GDP on Chernobyl related social programs, the overall long-term costs of the catastrophe including the loss in subjective well-being are enormous (Oughton, Bay-Larsen and Voigt 2009).

3.7.2 Assessment of the role of current Chernobyl assistance payments

The Ukrainian government runs a costly Chernobyl assistance program which offers an extremely complex mix of 50 different privileges and social benefits ranging from direct monetary compensation to subsidized health care, tax exemption, as well as travel and university grants (Oughton, Bay-Larsen and Voigt, 2009).[77] To what extent do these Chernobyl assistance payments help to mitigate the well-being loss of Chernobyl victims? To answer this question, the following analysis makes use of the ULMS data which contains information on whether individuals received Chernobyl assistance payments in the last 30 days (binary variable).[78] Furthermore, the previous model (3.1) is amended by introducing interaction terms between 1986 radiation doses (three categories: close to zero, medium and high levels) with the binary indicator variable for Chernobyl assistance payments:

$$W_{it} = \beta_o + \sum_{k=1}^{3} \beta_{1,k} Radiation + \beta_2 Assistance + \sum_{k=1}^{3} \beta_{3,k} Rad.*Assist. + X\beta' \qquad (3.5)$$

The estimated coefficients help to disentangle the effect of radiation exposure and state assistance of victims and to what extent these assistance payments help to mitigate the radiation effect. In particular, based on the estimated $\hat{\beta}_1, \hat{\beta}_2$ *and* $\hat{\beta}_3$ coefficients, it is possible to express the relative subjective well-being loss/gain of a person having been exposed to medium/high radiation doses and having/having not received Chernobyl assistance payments to the 'baseline' comparison group of

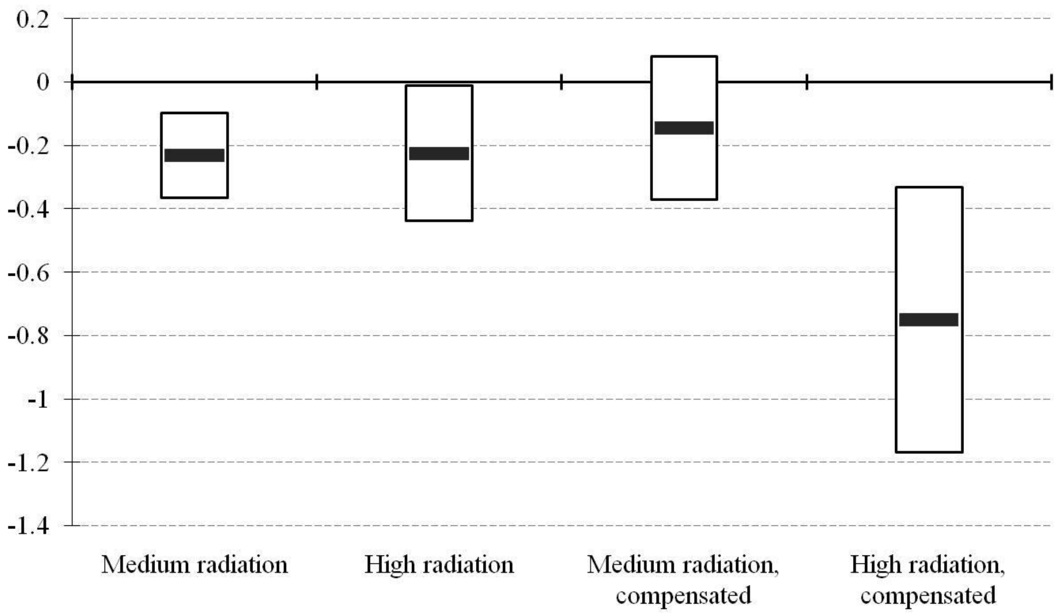
[76] A similar conclusion is reached when exploiting the reduced form coefficients of objective radiation doses with individual income data. Similar costs apply when compensating individuals for 2 mSv of additional radiation.

[77] The Ukrainian law «On the status and social protection of citizens who suffered from the ChNPP catastrophe» from February 29, 1991 – which was amended in the following years – is the legal basis for the social protection of the Chernobyl victims (see also Chapters 4 and 12 in the National Report from Baloga, Kholosha and Evdin (2006)).

[78] The corresponding question in the ULMS 2007 questionnaire asks respondents about whether they personally received any Chernobyl assistance in the last 30 days prior to the interview (monetary payments or payments in the form of goods or services). About 1.4 percent of the sample answer that they received such payments.

unaffected persons (zero radiation dose). For instance, in comparison to an unaffected person, the well-being loss/gain of someone with medium levels of radiation, but no assistance amounts to $\hat{\beta}_{1,2}$, while the loss/gain of a person having been exposed to the same radiation dose, but receiving compensatory assistance payments at the same time corresponds to the sum of the three coefficients $\hat{\beta}_{1,2} + \hat{\beta}_2 + \hat{\beta}_{3,2}$. These estimated well-being losses/gains have been calculated for the four different categories of persons and are represented graphically in Figure 3-5.

Figure 3-5: The effect of radiation levels and Chernobyl assistance on subjective well-being



Notes: Figure shows effects of radiation dose on subjective well-being (bar) with 90% confidence interval. Baseline category is ‘no additional radiation’. Effects based on regressions with full set of controls and individually clustered standard errors.

Several interesting findings emerge. Positive (non-zero) radiation levels have a significant negative impact on subjective well-being among those who do not receive any accident-related benefits (see the two left bars in Figure 3-5). The size of the coefficient is around -0.2 irrespectively of whether individuals received a medium or high dosage. Turning to the respondents receiving Chernobyl assistance payments, individuals with medium dosage still suffer from a well-being discount of more than -0.2, however, the increased standard errors render the effect insignificant.

In other words, medium affected individuals receiving assistance payments have no significantly lower well-being than non-affected individuals. In contrast are the results for those who suffered from high radiation doses and receive Chernobyl assistance. Their well-being toll amounts to almost -0.8 points despite the compensatory assistance payments. This surprising finding could be related to the fact that the official 1986 radiation measures used in the regressions refer to the regional level and represent average values and not personal radiation dosimetry measures (even in small areas there was non-negligible variation in radiation exposure across space). However, if benefits are targeted to the most affected individuals *within* regions this significantly negative effect could identify those individuals who were more severely exposed to the radiation so that the assistance payments are not sufficient to fully compensate them (in contrast to individuals with medium radiation exposure).[79]

3.7.3 Social state transfer dependency of Chernobyl victims

Psychologists have argued that traumatised individuals might suffer from psychological illnesses, depression, anxiety and lethargy leading to increased levels of state aid dependency (Osiatynski 2004; Udovyk 2007). To analyse whether this behavioural effect can be also found in the data used in this study the following analysis will take advantage of the fact that the UBHS data consistently collected relevant information on individual social state transfer receipt across years.[80] The dependent variable is the transfer share in total income which is constructed using the single income components reported by the individuals. A higher state aid dependency (higher transfer share) could indicate that affected persons indeed suffer from stronger feelings of powerlessness and are less able to help themselves.

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[79] On the other hand, the payment of compensation might work as a signal for the own (partially unobservable) radiation status and lead to lower well-being levels. The latter explanation, however, stands in contrast to the compensation effect found among those with medium radiation levels. There are more indications in favour of the first explanation: individuals in the group of high radiation levels receiving assistance are on average 60 percent more likely to suffer from one out of seven chronic diseases than those with similar radiation levels but no compensation. This higher incidence of poor health conditions also translates into substantially larger medical out-of-pocket expenditures (183 UAH per month compared to 65 UAH) and—conditional on working—more days of sickness absence during a period of the past three months (14.8 days compared to 6.7 days). These numbers indicate that compensated individuals in the high radiation group suffer indeed from a worse health status.

[80] Unfortunately, structural inconsistencies in the income sections of the ULMS over time prevent an analysis of the extent of transfer dependency using the panel data set.

Table 3-13 provides OLS and 2SLS results from this empirical assessment. Columns (1) to (4) exploit a wider definition of state transfers (including Chernobyl benefits) while columns (5) to (8) exclude all benefits related to the catastrophe. Results are provided for two levels of self-reported affectedness: The first dummy variable (*somewhat affected*) includes all individuals who report to be personally at least somewhat affected, while the second variable (*seriously affected*) identifies only those whose health was strongly affected.

The results in Table 3-13 reveal that there is a significant positive association between Chernobyl affectedness and transfer dependency: persons affected by the nuclear accident have a significantly higher transfer share in their total income – irrespective of whether explicit Chernobyl payments are included in the measure of state transfers or not (the coefficients decrease only marginally when Chernobyl payments are not accounted for). Furthermore, while the somewhat affected individuals have on average a two percentage point higher state transfer ratio, the effect rises to between four and nine percent for the seriously affected. The table also demonstrates that the average state dependency decreased over time as indicated by the time trend against the base year 2001. Moreover, women and older persons are on average more dependent upon state transfers (which probably relates retirement and old-age pensions; also note that the legal retirement age is five years lower for women (age 55) than for men (age 60)).

Table 3-13: The Chernobyl effect on the transfer share in total income

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
Dependent variable	*Transfer share in total income*				*Transfer share in total income (excluding all Chernobyl related benefits)*			
At least somewhat affected	0.017***	0.020***			0.016***	0.017***		
	(0.001)	(0.004)			(0.001)	(0.004)		
Seriously affected			0.042***	0.087***			0.038***	0.075***
			(0.003)	(0.019)			(0.003)	(0.019)
Age	0.005***	0.005***	0.005***	0.005***	0.005***	0.005***	0.005***	0.005***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Age squared	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Female	0.029***	0.029***	0.029***	0.029***	0.029***	0.029***	0.029***	0.029***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Year 2002	-0.012***	-0.014***	-0.004	-0.007**	-0.012***	-0.013***	-0.004	-0.006**
	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)
Year 2003	-0.030***	-0.032***	-0.022***	-0.024***	-0.030***	-0.031***	-0.023***	-0.024***
	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)
Year 2004	-0.024***	-0.026***	-0.016***	-0.018***	-0.025***	-0.025***	-0.017***	-0.018***
	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)
Year 2005	-0.024***	-0.026***	-0.016***	-0.018***	-0.024***	-0.025***	-0.017***	-0.018***
	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)
Year 2006	-0.031***	-0.033***	-0.023***	-0.025***	-0.031***	-0.032***	-0.023***	-0.025***
	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)
Year 2007	-0.033***	-0.034***	-0.024***	-0.025***	-0.033***	-0.033***	-0.025***	-0.025***
	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)
Year 2008	-0.035***	-0.037***	-0.027***	-0.028***	-0.035***	-0.036***	-0.027***	-0.028***
	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)
Constant	-0.206***	-0.205***	-0.207***	-0.205***	-0.204***	-0.204***	-0.205***	-0.204***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
R-squared	0.794	0.794	0.794	0.794	0.794	0.794	0.794	0.794

Notes: All regressions include controls for educational attainment, economic status as well as regions. The number of observations is 140,869 in all columns. The first stage F-statistics is 1937.4. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in parentheses. Source: UHBS 2001-2008; own calculations.

3.8 Extension: Separate results by subgroups

Recent research investigates the effect of gender on risk perception and crisis management. The literature review by Croson and Gneezy (2009) gives an overview of this huge strand of research which finds that women have on average stronger emotional experiences and are more fearful than men. For instance, while the prime emotional reaction following the September 11 attacks in the United States among men was anger, women became more fearful and pessimistic (Lerner, Gonzalez, Small and Fischhoff 2003). Furthermore, women are more risk averse than men (Borghans, Golsteyn, Heckman and Meijers 2009). To investigate whether men and women responded differently to the Chernobyl catastrophe the life satisfaction regressions are estimated separately by gender (Table 3-14): while both men and women show a similarly sized negative effect of self-reported affectedness on life satisfaction this is not true for the results using the official radiation measures (reduced form as well as 2SLS estimates). While the results for women reveal large and significantly negative effects of radiation doses on subjective well-being, the results for men – although having the expected sign – are close to zero and not significant.[81]

Table 3-14: Gender differences in subjective well-being (ULMS)

Dependent variable	*Life satisfaction*					
	(1)	(2)	(3)	(4)	(5)	(6)
	Naïve OLS		**Reduced form OLS**		**2SLS (2nd stage)**	
	Men	Women	Men	Women	Men	Women
Subjective affectedness	-0.089***	-0.111***			-0.013	-2.413**
	(0.033)	(0.029)			(0.933)	(1.024)
Radiation dose			-0.001	-0.184***		
			(0.069)	(0.060)		
Demographic controls	✓	✓	✓	✓	✓	✓
Household controls	✓	✓	✓	✓	✓	✓
Health & traits	✓	✓	✓	✓	✓	✓
Observations	4,491	6,574	4,491	6,574	4,491	6,574
R-squared	0.201	0.211	0.200	0.211	0.200	-0.556

Notes: All regression with full set of controls. Standard errors are clustered on the individual level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Source: ULMS 2003-2007; own calculations.

[81] The regressions from Table 3-14 allow all coefficients to vary by gender. When alternatively estimating a pooled model with a gender-Chernobyl interaction effect, the interaction term is only significant when only the basic controls are included in the regression. This would imply that there are no significant gender differences in responses to the Chernobyl catastrophe as suggested by the results using the self-reported affectedness measure in Table 3-14.

These potential gender differences in long-term subjective well-being would be in line with the above cited literature, where men are found to acquire a more positive attitude towards negative events. Actual radiation does not seem to negatively impact men's life satisfaction in the long run. The significant effect for males when using the self-reported affectedness measure (column 1) might reflect a compassion affect with affected female family members.

While there is generally mixed and inconclusive evidence regarding the effect of higher radiation exposure on the prevalence of leukaemia and most other somatic illnesses, there is consensus regarding the effect on increased incidence of thyroid cancer among children and adolescents (United Nations 2002; UNSCEAR 2008). Children and young individuals born prior to the accident appear to have been especially vulnerable to internal exposure of radioactive iodine (especially the isotope iodine-131) and have subsequently suffered more often from thyroid cancer. To test whether this is also related to subjective well-being among young individuals, separate reduced form regressions are estimated for the sample of children who were zero to 18 years old at the time of the catastrophe (Table 3-15). Since there exist gender specific information on absorbed doses of thyroid, these regressions are also repeated for girls and boys separately.

Table 3-15: Robustness check: Effect of absorbed thyroid doses on 1986-children and adolescents

	(1)	(2)	(3)	(4)
	Children aged 0-18 in 1986	Children aged 0-18 in 1986	Girls aged 0-18 in 1986	Boys aged 0-18 in 1986
Dependent variable	*Life satisfaction*			
Log thyroid dose females aged 1-18	-0.013* (0.007)		-0.020** (0.009)	
Log thyroid dose dose males aged 1-18		-0.013* (0.007)		0.003 (0.013)
Demographic controls	✓	✓	✓	✓
Household controls	✓	✓	✓	✓
Health & traits	✓	✓	✓	✓
Observations	3,532	3,532	2,052	1,480
R-squared	0.195	0.195	0.186	0.240

Notes: Samples comprise only children who were aged zero to 18 in the year 1986 (born before January 1987). Absorbed thyroid doses are log transformed. Standard errors are clustered on the individual level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Source: ULMS 2003-2007; own calculations.

The results in the first two columns of Table 3-15 reveal that exposure to higher iodine doses has a significantly negative effect on long-run life satisfaction. When splitting the sample by gender the picture is similar to the findings on the whole population: the reduced form estimates indicate a significant negative long-run effect for girls, while boys' life satisfaction seems to be unaffected.

3.9 Extension: Possible behavioural implications – fatalism and precaution

Fatalism is generally defined as an attitude or belief of having little power in determining one's life and being exposed to an inevitable fate. As presumably received radiation doses cannot be removed from human bodies, people might develop a fatalistic attitude towards their situation. It is possible to test whether people believe that their exposure to the Chernobyl disaster significantly reduces their remaining lifetime using the ULMS data. In the 2007 wave, individuals aged 45 and above were asked to name the probability that they would survive until a certain 'target age' in the future (typically around 10 years in the future).[82] This 'target age' was specified according to the current age of the respondent: For instance, all respondents aged 45 to 55 (56 to 60) were asked to assess their survival probability until age 65 (70) and so on (see notes below Table 3-16).

Table 3-16 shows the effects of self-reported and objective affectedness on these subjective survival probabilities (as well as the 2SLS results). If respondents identify themselves as being affected by the Chernobyl catastrophe (self-reported measure), the individual survival expectancy falls by 4.2 to 3.5 percentage points. The fact that there are significant effects in response to self-reported affectedness suggests that people think mostly about their personal affectedness (unless people believe that radiation in other household members can reduce their own life span[83]) when responding to the Chernobyl question. Using the official radiation doses the reduced form effects become even larger: A one mSv higher exposure dose reduces the expected survival probability by between 7.4 to 8.1 percentage points (a bit less than 30 percent of a standard deviation). These negative effects of the Chernobyl catastrophe on subjective survival

[82] The corresponding survey question reads: "What are the chances that you will live to be age [X] and older?". There are about 1,980 observations in the estimation sample for whom this variable is non-missing (the smaller sample size is due to the fact that the question was only asked in the 2007 wave). The mean of this variable is 53.9 percent (standard deviation of 27.0).

[83] This might be relevant if people perform stressful home care, for instance.

probabilities is also confirmed when using the instrumental variable approach (the estimated effects become extremely large – about minus 73.6 to 87.0 percentage points).

Table 3-16: Impact of affectedness on subjective survival probability

	(1)	(2)	(3)
Dependent variable	*Subjective probability of survival to target age (0% to 100%)*		
A. Naïve regressions (OLS)			
Self-reported affectedness	-4.178***	-3.935***	-3.640***
	(1.287)	(1.291)	(1.289)
B. Reduced form (OLS)			
Radiation dose (mSv)	-7.444***	-8.049***	-7.892***
	(1.644)	(1.585)	(1.573)
C. First stage (2SLS)			
Dependent variable	*Self-reported affectedness*		
Radiation dose (mSv)	0.101***	0.097***	0.091***
	(0.026)	(0.026)	(0.026)
F-statistic	15.53	13.98	12.27
D. Second stage (2SLS)			
Dependent variable	*Subjective survival probability*		
Instrumented self-reported affectedness	-73.628***	-83.313***	-86.962***
	(23.612)	(26.191)	(28.844)
Basic controls	✓	✓	✓
Individual and health controls	-	✓	✓
Traits and household controls	-	-	✓
Observations	1,981	1,881	1,881

Notes: The target age is 65 for those aged 45 to 55, 70 for those aged 56 to 60, 75 for those aged 61 to 65 and 80 for those aged 66 to 75. All regressions control for full set of age dummies. The questions on the survival probabilities were asked only in ULMS 2007 to individuals aged 45 and above—hence the limited sample sizes. In order to increase the degrees of freedom, regressions control for macro regions instead of single oblasts. Standard errors are clustered on the individual level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1 Source: ULMS 2007.

Once people become wary of their potential long-run effects suffered from the Chernobyl catastrophe or anticipate potentially shorter lifetimes, they might choose to do whatever is in their power to reduce the risk of falling ill, i.e. they might engage in precautionary behaviour. It is beyond the scope of this study to conduct a comprehensive analysis about potential behavioural implications of the Chernobyl catastrophe. Nevertheless, Table 3-17 provides suggestive evidence on potential behavioural effects in terms of smoking. Smoking is an immediate and individual-level indicator for risky behaviour (Cawley and Ruhm 2011).[84] Despite the fact that the awareness of specific health risks from smoking might be lower in middle- and low-

[84] Cawley and Ruhm (2011) provide a recent review article on the economics on risky behaviour.

income countries compared to industrialized nations (Steptoe, Wardle, Cui, Baban, Glass, Pelzer, Tsuda and Vinck, 2002) smoking is generally considered to be harmful. The binary smoking variable indicates persons who report to be currently smoking.

Table 3-17: Effect of Chernobyl affectedness on the propensity to smoke

	(1)	(2)	(3)
Dependent variables		***Smoking (0/1)***	
A. Naïve regression (Probit)			
Self-reported affectedness	-0.109***	-0.032**	-0.032**
	(0.014)	(0.014)	(0.014)
Pseudo R-squared	0.028	0.306	0.316
B. Reduced form (Probit)			
Radiation level	-0.079***	-0.048*	-0.060**
	(0.027)	(0.026)	(0.026)
Pseudo R-squared	0.019	0.306	0.316
C. First stage (2SLS)			
Radiation level	0.084***	0.081***	0.078***
	(0.030)	(0.029)	(0.030)
F-statistics	7.82	7.65	6.94
D. Second stage (2SLS)			
Instrumented self-reported	-0.955**	-0.544†	-0.679*
affectedness	(0.470)	(0.360)	(0.404)
Basic controls	✓	✓	✓
Individual and health controls	-	✓	✓
Traits and household controls	-	-	✓
Observations	12,003	11,065	11,065

Notes: Panel A and B report marginal effects. Standard errors are clustered on the individual level. Standard errors are clustered on the individual level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1, † p<0.15. Source: ULMS 2003-2007; own calculations.

As Panel A in Table 3-17 reveals, self-reported Chernobyl victims are almost 11 percentage points less like to smoke – an effect that falls to 3 percentage points but remains significant once individual and household controls (and health status as well as traits in specification (3)) are accounted for. A similar significant negative effect on smoking is found in the reduced form regressions using the official radiation doses (Panel B). Each additional millisievert of effective exposure dose reduces the probability to smoke by five to almost eight percentage points. As previously the 2SLS estimates are even larger than the simple Probit effects (marginal effects). However, due to the inflated standard errors, the effects become only marginally significant (the significance level varies across specifications).

Hence, these results seem to suggest that affected persons are – on average – less likely to smoke. This could be interpreted as a first indication for possible precautionary health behaviour induced by the Chernobyl catastrophe. However, surprisingly, these results are not in line with those by Lehmann and Wadsworth (2011) who do not find a significant effect of having lived in regions exposed to higher radiation levels in 1986 on contemporary smoking habits (reduced form regressions). Although their coefficients have the same negative sign like in the results presented in Table 3-17, they are marginally insignificant. These differences could be related to the different measures of affectedness and radiation exposure used in their study vis-à-vis those used in the present study (e.g., in their regression on smoking, the Chernobyl indicator is a dummy variable for having lived in the designated monitoring zone in 1986).

3.10 Conclusions

This chapter analysed long-term effects of the Chernobyl catastrophe on life satisfaction and mental health in Ukraine more than 17 years after the nuclear accident. To identify persons who were exposed to radiation the study uses self-reported affectedness measures as well as objective 1986 radiation doses which can be assigned to individuals according to their place of living in 1986. Since the Chernobyl disaster was unexpected and randomly affected certain parts of the Ukrainian population more than others (geographic variation in radiation doses) the empirical analysis can generate estimates of the causal effect of the nuclear accident on various outcomes. The results suggest that individuals who were affected by the catastrophe exhibit significantly lower levels of life satisfaction as well as higher probabilities of suffering from depression or psychological traumas (posttraumatic stress disorders). These results hold irrespective of the measure of affectedness used (self-reported or official measures), although the instrumental variable estimations which aim at correcting potential measurement as well as endogeneity problems of the self-reported measure seem to imply that the results based on the latter can be interpreted as lower bound estimates.

In order to evaluate the monetary costs of these subjective well-being losses (utility losses) and to assess the negative externality of the catastrophe on the general population and the economy as a whole, the chapter also provides estimates of the monetary value needed to compensate victims for their burden. The estimated compensating income differentials suggest a total annual cost around ten percent of

Ukrainian GDP. This is a remarkable sum considering the fact that the Chernobyl disaster took place such a long time ago.

In further extensions to the main analysis, the study also finds evidence on effects on subjective life expectancy (subjective survival probabilities during the next ten years) as well as precautionary health behaviour: individuals affected by the 1986 nuclear accident expect to have a lower life expectancy on average and are significantly less likely to smoke. However, it is beyond the scope of this study to draw any final conclusions about possible fatalistic or precautionary behavioural responses or attitudes due to the increased (health) uncertainty caused by the Chernobyl catastrophe. The task to explore and analyse these possible long-term effects in more depth is left for future research.

3.11 Appendix to Chapter 3

Table A 3-1: Ordered Probit regressions (marginal effects) using self-reported and official measures of affectedness

	(1)	(2)	(3)
Dependent variable		*Life satisfaction*	
A. Self-reported affectedness			
Self-reported affectedness *(β)*	-0.182***	-0.099***	-0.100***
	(0.025)	(0.026)	(0.025)
Pseudo R-squared	0.0304	0.0619	0.0751
Marginal effects			
Fully unsatisfied (outcome 1)	0.051***	0.026***	0.026***
	(0.007)	(0.007)	(0.007)
Unsatisfied (outcome 2)	0.021***	0.013***	0.014***
	(0.003)	(0.003)	(0.003)
Neither/nor (outcome 3)	-0.014***	-0.009***	-0.009***
	(0.002)	(0.002)	(0.002)
Satisfied (outcome 4)	-0.045***	-0.025***	-0.025***
	(0.006)	(0.007)	(0.006)
Fully satisfied (outcome 5)	-0.013***	-0.006***	-0.006***
	(0.002)	(0.002)	(0.001)
B. Official radiation measures			
Radiation dose *(β)*	-0.098*	-0.116**	-0.104**
	(0.052)	(0.052)	(0.051)
Pseudo R-squared	0.0284	0.0615	0.0747
Marginal effects			
Fully unsatisfied (outcome 1)	0.028*	0.031**	0.028**
	(0.015)	(0.014)	(0.014)
Unsatisfied (outcome 2)	0.011*	0.015**	0.014**
	(0.006)	(0.007)	(0.007)
Neither, nor (outcome 3)	-0.008*	-0.010**	-0.010**
	(0.004)	(0.005)	(0.005)
Satisfied (outcome 4)	-0.024*	-0.029**	-0.026**
	(0.013)	(0.013)	(0.013)
Fully satisfied (outcome 5)	-0.007*	-0.007**	-0.006**
	(0.004)	(0.003)	(0.003)
Region & time FE	✓	✓	✓
Demographics, health, work	-	✓	✓
Income, wealth, traits	-	-	✓
Observations	12,003	11,065	11,065

Notes: The included controls in columns (1), (2) and (3) correspond to columns (1), (5) and (7) in Table 3-5. Standard errors are clustered on the individual level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1, † p<0.15. Source: ULMS 2003-2007; own calculations.

Table A 3-2: Alternative estimation method: Generalized Least Squares Random Effects and Generalized Two-Stage Least Squares Random Effects (GLS-RE and G2SLS-RE)

	(1)	(2)	(3)
A. Naive GLS-RE			
Dependent variable		*Life satisfaction*	
Self-reported affectedness	-0.190***	-0.096***	-0.096***
	(0.027)	(0.026)	(0.025)
R-squared overall	0.0873	0.1691	0.2008
B. Reduced form GLS-RE			
Dependent variable		*Life satisfaction*	
Radiation dose	-0.088†	-0.102**	-0.088*
	(0.056)	(0.052)	(0.050)
R-squared overall	0.0819	0.1681	0.1997
C. First stage G2SLS-RE			
Dependent variable		*Self-reported affectedness*	
Radiation dose	0.083***	0.079***	0.077***
	(0.019)	(0.019)	(0.019)
z-value of instrument	4.40	4.11	3.99
D. Second stage G2SLS-RE			
Dependent variable		*Life satisfaction*	
Instrumented self-reported	-1.045†	-1.254*	-1.112†
affectedness	(0.713)	(0.768)	(0.737)
R-squared overall	0.0507	0.0845	0.1182
Region & time FE	✓	✓	✓
Demographics, health, work	-	✓	✓
Income, wealth, traits	-	-	✓
Observations	12,003	11,065	11,065

Notes: The included controls in columns (1), (2) and (3) correspond to columns (1), (5) and (7) in Table 3-5. Standard errors are clustered on the individual level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1, † p<0.15. Source: ULMS; own calculations.

Table A 3-3: Causal effects on the likelihood of being unhappy – alternative estimation method (GLS-RE and G2SLS-RE)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Naive GLS-RE							
Dependent variable				*Unhappy (0/1)*			
Self-reported affectedness	0.055***	0.036***	0.035***	0.037***	0.034***	0.034***	0.034***
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)
Reduced form GLS-RE							
Dependent variable				*Unhappy (0/1)*			
Radiation dose	0.045**	0.040**	0.038**	0.040**	0.047***	0.043**	0.044**
	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)	(0.017)	(0.017)
GLS-RE First stage							
Dependent variable				*Self-reported affectedness*			
Radiation dose	0.083***	0.073***	0.080***	0.078***	0.079***	0.077***	0.076***
	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)
Z-value of instrument	4.40	3.93	4.16	4.09	4.11	3.99	3.98
G2SLS-RE Second stage							
Dependent variable				*Unhappy (0/1)*			
Instrumented self-reported affectedness	0.519*	0.529*	0.468*	0.499*	0.579**	0.546*	0.558*
	(0.271)	(0.304)	(0.268)	(0.280)	(0.294)	(0.289)	(0.293)
Region & time FE	✓	✓	✓	✓	✓	✓	✓
Demographic controls	-	✓	✓	✓	✓	✓	✓
Health controls	-	-	✓	✓	✓	✓	✓
Marital status	-	-	-	✓	✓	✓	✓
Work status	-	-	-	-	✓	✓	✓
Income, wealth, HC	-	-	-	-	-	✓	✓
Traits	-	-	-	-	-	-	✓
Observations	12,003	12,003	11,065	11,065	11,065	11,065	11,065

Notes: Panel A and B report marginal effect from pooled Probit regressions for the binary variable '*unhappy*'; Panel C and D report the estimated coefficients from the 2SLS regressions (linear probability models). The variable '*unhappy*' indicates individuals answering '*fully unsatisfied*' on the life satisfaction question. Standard errors are clustered on the individual level. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Source: ULMS 2003-2007, own calculations.

Table A 3-4: Personality traits and self-reported affectedness (pooled OLS)

	(1)	(2)	(3)	(4)
Dependent variable	*Self-reported affectedness*			
Neurotic	0.018	0.026	0.026	0.033
	(0.028)	(0.027)	(0.027)	(0.029)
Extrovert	0.009	-0.001	-0.002	-0.002
	(0.013)	(0.013)	(0.013)	(0.013)
Male		-0.115***	-0.115***	-0.083***
		(0.009)	(0.009)	(0.011)
Married		0.015	0.015	0.029
		(0.019)	(0.019)	(0.020)
Widowed		0.013	0.016	0.028
		(0.024)	(0.024)	(0.025)
Separated		-0.031	-0.030	-0.017
		(0.023)	(0.023)	(0.024)
Age		0.007	0.008	0.009
		(0.010)	(0.010)	(0.010)
Age squared		0.000	0.000	-0.000
		(0.000)	(0.000)	(0.000)
Age cubic		-0.000	-0.000	-0.000
		(0.000)	(0.000)	(0.000)
General secondary educ.		0.005	0.004	-0.002
		(0.013)	(0.013)	(0.013)
Professional second. educ.		-0.018	-0.019	-0.026*
		(0.013)	(0.013)	(0.014)
Higher education		-0.006	-0.007	-0.018
		(0.016)	(0.016)	(0.016)
Working		-0.077	-0.079	-0.110
		(0.125)	(0.126)	(0.137)
Unemployed		-0.061	-0.059	-0.081
		(0.126)	(0.127)	(0.138)
Pensioner		-0.052	-0.052	-0.081
		(0.126)	(0.127)	(0.138)
Inactive		-0.067	-0.066	-0.102
		(0.126)	(0.126)	(0.137)
Household size			-0.006	-0.007**
			(0.003)	(0.004)
Log of household income			0.006	0.005
			(0.005)	(0.006)
Living space per capita			-0.000***	-0.000***
			(0.000)	(0.000)
BMI				0.002
				(0.001)
Chronic disease				0.077***
				(0.010)
Constant	0.225	0.051	0.053	0.068
	(0.226)	(0.274)	(0.261)	(0.260)
R-squared	0.122	0.156	0.156	0.162

Notes: Results remain unaffected when control for objective radiation doses are added. Regressions control for year, month of interview and region fixed effects. Number of observations is 12,003 (columns 1-3) and 11,065 (column 4). Standard errors are clustered on the individual level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Source: ULMS 2003-2007, own calculations.

Table A 3-5: OLS regressions of subjective well-being (reduced form), various age controls

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable			*Subjective well-being*			
Radiation dosage	-0.080*	-0.097**	-0.102**	-0.085*	-0.101**	-0.099**
	(0.046)	(0.045)	(0.045)	(0.045)	(0.045)	(0.045)
Age	-0.013***	-0.065***	-0.188***			
	(0.001)	(0.006)	(0.024)			
Age squared		0.001***	0.004***			
		(0.000)	(0.001)			
Age cubic			-0.000***			
			(0.000)			
Log(Age)				-0.595***	-7.157***	-34.845***
				(0.046)	(0.807)	(9.405)
Log(Age) squared					0.908***	8.658***
					(0.111)	(2.618)
Log(Age) cubic						-0.719***
						(0.242)
Full controls	✓	✓	✓	✓	✓	✓
Observations	11,065	11,065	11,065	11,065	11,065	11,065
R-squared	0.192	0.197	0.200	0.195	0.200	0.201

Notes: Full controls see Table 3-5. Standard errors are clustered on the individual level. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Source: ULMS 2003-2007; own calculations.

4 Labour Market Participation of New Mothers and Medium-Run Cognitive Child Outcomes

4.1 Introduction, Motivation and Research Question

This chapter investigates the question whether prolonged paid and job protected parental leave and thus reduced maternal employment in the first years of a child's life has effects on the offspring's cognitive development through the increase in the maternal time that can be devoted to child-rearing. In many industrialized countries, the provision of parental leave is one of the main policy instruments aimed at helping young families reconcile working life with family life.[85] This topic has become increasingly important and prominent in the public debate as female labour force participation rates have been growing over the past decades in many industrialized countries.[86] However, the advantages and disadvantages of this policy instrument have been critically discussed: while proponents of (more generous) parental leave entitlements assert positive consequences for the health and well-being of children and their mothers as well as for the general situation of women in the workplace (e.g., through lower unemployment rates related to childbirth and stronger incentives to return to the previous employer), opponents fear that parental leave mandates diminish market flexibility, thereby leading to inefficiencies and lower rates of employment growth (Ruhm 1998, 2000). The lack of an academic and societal consensus about the potential effects of parental leave on maternal labour market performance and on the development of new-borns is also partly reflected in the large cross-country variation in the generosity of parental leave entitlements in terms of duration of job protection and income replacement level. For instance, one of the shortest and less generous parental leave regulations can be found in the USA. The maternity and parental leave

[85] The Preamble of the European Community Council Directive 96/34/EC of 3 June 1996 on the framework agreement on parental leave states this very clearly: "The enclosed framework agreement represents an undertaking by Unicef, CEEP and the ETUC to set out minimum requirements on parental leave and time off from work on grounds of force majeure, as an important means of reconciling work and family life and promoting equal opportunities and treatment between men and women." (European Community 1996, Preamble).

[86] This increasing trend has been particularly strong in countries with comparatively low female labour force participation rates in the early 1980s, for instance, Austria, Germany, Italy, Spain (see also Table A 4-1 in the appendix). In the Scandinavian countries Denmark, Norway and Sweden, female labour force participation rates have increased between the year 1980 and 2009, but the development over time was not monotonic and seems to have flattened or even slightly decreased in the last decade.

entitlements according to the US federal Family and Medical Leave Act (FMLA) from 1993 grants a maximum duration of unpaid, job protected leave of 12 weeks which is low in comparison to other countries (Berger, Hill and Waldfogel 2005). Furthermore, the regulations in the FMLA apply to only about half of the female workforce; parental leave allowances for the other half are determined in individual employer regulations.[87] In contrast, most European countries grant much longer durations of job protected leave (some of which is even compulsory) and mothers or fathers on leave receive partial or full income compensation (for an overview, see Neyer 2003). Since the length of the granted parental leave is relevant for the return-to-work decision, these cross-country differences in legislation help to explain why new mothers in some countries return to work much sooner and spend less time at home with their child compared to mothers in other countries (e.g. Ruhm 2000 for a study on 16 European countries; Tanaka 2005 for an analysis of 18 OECD countries).

Overall, previous empirical studies in psychology and economics have produced mixed evidence regarding the impact of early maternal employment on child outcomes. If anything, the majority of studies seem to support the hypothesis that the labour force participation of mothers during their children's *first* year of life has potentially adverse effects on their subsequent development (see Ruhm 2008 and studies cited therein). In addition, there is some indication for heterogeneous effects across subgroups: children with a higher socio-economic background are potentially more likely to be *negatively* affected by maternal employment, while children from low income families might *benefit* if maternal employment improves the income situation of the household (Currie 2003). Hence, depending on the specific design of the laws regarding the length of granted leave, the income replacement level during the leave period as well as the medium- to long-term labour market consequences for the mother, parental leave mandates might affect child outcomes through time effects (more maternal time investments) and potentially through income effects (if household income is reduced due to foregone wage earnings of the mother in the short-run and potentially in the long-run).

The fundamental challenge of these empirical assessments is the non-trivial identification of the causal effect of early maternal employment on child development.

[87] A more detailed description and analysis of the US Parental Leave regulations and differences across single states is given by Han, Ruhm, Waldfogel (2009).

Maternal employment, fertility behaviour and the timing the labour market re-entry after childbirth are choice variables and might be driven by unobserved mother or child characteristics (e.g. ability, fertility and work preferences, role models, regional differences in availability and costs of alternative child care). If particular types of women (e.g., higher ability women) return to work sooner than others, differences in child outcomes between these groups of mothers might reflect differences in maternal characteristics and intergenerational transmission of ability rather than the causal effect of maternal employment. Reverse causality might be an additional challenge, since certain health conditions of a child are likely to impede its mother's return to work. As long as it is not clear which factors lead some women to return to work sooner than others, empirical analyses will only provide statistical associations and one must be careful not to draw conclusions about causality.

Until recently, the majority of studies on this topic tried to tackle the endogeneity problem of the maternal return-to-work decision (omitted variable bias) by including as many potentially relevant control variables as possible (e.g., pre-birth characteristics, the omission of which could confound the analysis), by estimating family fixed effects models and comparing sibling differences, by implementing propensity score matching or by employing instrumental variable techniques (among the first to follow this approach were Blau and Grossberg (1992); for an overview of previous studies and methods, see Currie 2003; Almond and Currie 2010; Hill, Waldfogel, Brooks-Gunn and Han 2005). However, as Currie (2003) notes, each of these empirical approaches has severe limitations and any inference and conclusions drawn from single studies have to be put in specific context and compared to results using other methods. None of these methods (except for the instrumental variable techniques if strong and compelling instruments are available), can convincingly solve the self-selection into maternal employment problem or the potential reverse causality from children's needs.

A very recent strand of the literature has tried to address the identification problem by employing quasi-experimental methods. These exploit exogenous changes in maternal employment caused by reforms in parental leave provisions in several countries with different institutional settings (Baker and Milligan (2010a, 2010b) focus on Canada, Carneiro, Løken and Salvanes (2010) on Norway, Dustmann and Schönberg

(2010) on Germany, Liu and Nordstrom Skans (2010) on Sweden, Würtz Rasmussen (2010) on Denmark).

This study complements this new development by analysing the effects of a substantial exogenous change in the duration of maternal time at home on medium-term cognitive child outcomes in Austria. The exogenous variation was induced by an unexpected and unanticipated policy reform that extended the maximum duration of job protected and paid parental leave by twelve months for all eligible mothers giving birth on July 1, 1990 or afterwards. Employed women having a child before this cut-off date were only eligible for job protected and paid parental leave until the child's first birthday. The reform had a strong impact on the time new mothers stayed at home with their children before returning to work since (a) female labour force participation in Austria in 1990 was already comparatively high[88], (b) most employed women generally satisfied the eligibility criteria, (c) take-up rates were extremely high and (d) most mothers exhausted the full duration of their leave entitlements (about 80 percent of mothers) (Lalive and Zweimüller 2009; Lalive, Schlosser, Steinhauer and Zweimüller 2010). However, although the reform caused mothers to substantially delay their return to work in the short-run, it did not adversely affect medium- or long-run employment and earnings of mothers (Lalive and Zweimüller 2009; Lalive, Schlosser, Steinhauer and Zweimüller 2010).

The aim of this chapter is to assess the reduced form or intention-to-treat effect of this twelve-months expansion of paid and job-protected parental leave for mothers on the cognitive skills of their children at age 15, measured by test scores from standardized assessments in mathematics, reading and scientific literacy from the international PISA study (Programme for International Student Assessment). The main empirical strategy is based on a Difference-in-Differences (DID) design which exploits the variation in the duration of parental leave created by the specific cut-off date of the reform. Specifically, differences in average test scores of children born shortly before and shortly after the reform (born in May/June 1990 versus July/August 1990 respectively) are compared with the test score differences in a control year in which there was no reform (children born in May/June 1987 versus July/August 1987). The inclusion of an additional pre-reform control year is motivated by the fact that outcome

[88] More than 70 percent among 20 to 40 year old women on average and even more than 80 percent for women with post-secondary education (see also Section 4.4.2).

comparisons across birth months within a given year could be confounded by season of birth or simple age effects (older children being more potentially advantaged at any given test date; see, for instance, Bedard and Dhuey 2006). However, as long as these underlying seasonality or age effects are constant over time, the DID approach will difference these out and estimate the true effect of the reform.

This study contributes to the existing literature in several ways: First, it adds to the few pieces of evidence on the causal relationship between maternal employment in the first years of the child's life and the child's cognitive development identified through exogenous reductions in maternal employment. Second, the analysis sheds light on this cognitive effect in a country where most non-parental child care of under three-year-olds is provided informally by grandparents or other relatives instead of formal day care centres (in contrast to countries like Sweden and Denmark where children participate in formal care-centres already at very young ages). Against this background, the reform most likely caused a replacement of informal care through maternal care, which might have different implications than switching from formal to maternal care (the possible role of different types of child care is discussed in more detail in Section 4.2.2). Third, the length of the paid extension, i.e. 12 months, is much larger than in the above mentioned comparable studies using quasi-experimental designs and might thus have a stronger impact on child outcomes. Moreover, only a few papers have assessed the effect of maternal care during the child's *second* year of life. Fourth, in contrast to the study for Germany by Dustmann and Schönberg (2010) which comes closest to the Austrian case in terms of cultural and institutional background, this analysis contains information on parental background and can thus distinguish between heterogeneous effects across subgroups. Fifth, to the best of my knowledge, this is the first empirical evaluation of the causal link between parental leave and child outcomes in Austria.

The empirical analysis produces several results: The overall effect of prolonged parental leave on test scores for the pooled sample of all children is close to zero and statistically insignificant. The subgroup analyses by maternal education and child gender yield inconclusive results. Although the main DID estimations indicate a strong and positive effect for children (especially for boys) of highly educated mothers a critical sensitivity analysis casts doubts on the robustness of these findings. Unfortunately, data limitations prevent a final assessment of whether these ambiguous results are caused by a violation of the common trend assumption.

The chapter is organized as follows: the next section introduces the underlying theoretical framework and reviews findings from previous empirical studies aiming at identifying the causal effect of early maternal employment on child outcomes in quasi-experimental approaches. This is followed by an overview of the institutional background in Section 4.3. The section includes details of the Austrian reform as well as a summary of findings from previous studies which evaluated this reform with respect to labour market and fertility outcomes. Section 4.4 explains the identification strategy and discusses critical assumptions and empirical challenges. Section 4.5 introduces the data set and describes the included outcome and control variables. The results of the main specification are presented and discussed in Section 0, while Section 4.7 contains several robustness checks and sensitivity analyses. Section 4.8 concludes with a critical and comparative summary of the empirical findings.

4.2 Parental Leave, Maternal Employment and Child Development

4.2.1 The role of maternal employment in the cognitive ability production function

Since the formation of human capital during childhood is a very complex process it is helpful to structure the discussion and analysis of potential effects of maternal employment on child outcomes along the lines of an underlying cognitive ability production function of the following type, where Y_{it} denotes a measure of cognitive ability of child *i* at age *t* (a similar formulation has been used, for instance, by Bernal 2008, Bernal and Keane 2011, and Dustmann and Schönberg 2010).

$$Y_{it} = Y_{it}(T_{it}, G_{it}, C_{it}, F_{it}, P_{it}, \omega_i) \tag{4.1}$$

According to this framework the cognitive ability of child *i* at age *t* is determined by several input factors, namely (T) maternal (parental) time investment up through age *t*, (G) inputs in the form of market-purchased goods and services other than non-parental child care (depending on income of parents; examples are quality of housing, additional educational material, nutrition, health expenditure), (C) time investment through non-parental caregivers (i.e. time in non-parental child care), (F) any direct effect of family composition, e.g. number of siblings (interaction between siblings; quantity-quality trade-off), birth order[89], time intervals between siblings[90], (P) public investments in children and child development (e.g., early child development programmes, public child care facilities and schools, state child health programmes and health insurance) and ($\boldsymbol{\omega}$) an idiosyncratic ability endowment, e.g., through intergenerational transmission of genes. As the function differentiates between different child ages it allows for varying effects of certain inputs at particular stages of child development.

Obviously, families cannot increase all input factors at the same time due to monetary and time budget constraints. In particular, when analysing the role of maternal employment within this framework, there is a clear trade-off between maternal time investment and maternal earned income which could be used to buy market-based inputs. Furthermore, it is possible that the reduction in maternal time inputs of working mothers can be at least partly compensated by the input of other goods (e.g., health investments, better nutrition) or by higher quality time investments of other caregivers (high income parents might be able to afford better quality child care). However, it should be noted that the time-income trade-off can be mitigated to the extent that mothers receive compensating maternal leave payments while being on parental leave.

The general provision or existence as well as the amount of such payments vary substantially across countries. In Austria at the beginning of the 1990s, mothers on parental leave received a monthly flat payment of approximately one third of the median female earnings (see Section 4.3.1). When assessing the importance of potential income losses related to leave periods after childbirth, it is crucial to consider not only short-term income losses due to absence from work during parental leave, but also potential long-term reductions in earnings which could result from human capital depreciation and/or increased difficulties in finding an employment after a prolonged leave period that exceeded the period of legal job protection.

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[89] Using a rich data set from Norway, Black, Devereux and Salvanes (2005) find that an increasing number of siblings leads to a reduction of average ‘child quality’ (i.e. educational attainment). However, the within-household analysis reveals that the negative ‘family size effect’ is distributed unevenly across siblings and that children of higher birth order are more adversely affected.

[90] A recent study from Sweden uses exogenously determined changes in spacing between births and finds that shorter spacing (within 24 months) has a detrimental effect on child outcomes (Pettersson-Lidbom and Skogman Thoursie 2009). Buckles and Munnich (2011) use instrumental variables to correct for the endogeneity of the time interval between births and find that test scores of older siblings are lower when spacing between siblings is smaller (preliminary findings based on US data from the NLSY79 Children and Young Adults Survey). In contrast to these results, an earlier study from the Netherlands finds no effect of the length of the time interval between births on cognitive child outcomes (Belmont, Stein and Zybert 1978).

Further refinements of the categories of input factors in the cognitive ability production function allow a more differentiated discussion help to explain potential heterogeneity across subgroups. For instance, Blau and Grossberg (1992) disaggregate parental time into ‘quantity of maternal time’ and ‘quality of parental time’ where higher quality of time also leads to better child outcomes. Interestingly, in their analysis ‘quality of parental time’ is proxied by a measure of verbal ability and mother’s and father’s education. A direct effect of higher education on quality of time and child care could be explained through better access to knowledge and application of methods to foster child development, for instance. In their analysis, Blau and Grossberg (1992) take quality of parental time as given; however, it is possible that working parents endogenously adjust the quality of time with their children in response to their working hours to compensate for foregone time with children. On the other hand, if parents are stressed and exhausted after work, they may have only limited capacities to spend as much ‘quality time’ with and pay as much attention to their children as they would like to or as might be beneficial for the children (Baum II 2003). This aspect of endogenous quality of parental time cannot be properly addressed in most empirical analyses due to the lack of measures of actual quality of parental time. Analogously, the unobservable *quality of non-parental child care*, i.e. the relative quality of time investment from alternative caregivers in comparison with maternal care, is also likely to affect child development (Bernal 2008; Bernal and Keane 2011). There is likely to be substantial variation in the quality of child care not only through differences across formal childcare centres, but also through quality differences across alternative forms of child care: formal (accredited) versus informal child care, informal child care by relatives versus non-relatives. It seems reasonable to believe that the availability and quality of certain types of child care is likely to play an important role as an intermediating factor for the effect of maternal employment on child outcomes as these determine the relative quality of maternal and non-maternal child care. If quality of non-parental child care is positively correlated with child care costs, then access to high quality day care centres is likely to be unequally distributed across families with different levels of income.

Certainly, a reduction in early maternal employment after childbirth through extended parental leave entitlements allows mothers to spend more time with their children than otherwise. Several potential mechanisms through which increased maternal time might positively affect child development have been put forward in the existing literature. One major channel put forward in most studies is prolonged breastfeeding, particularly during the child's first year of life, which is claimed to lead to better health outcomes of children (see critical discussion in Baker and Milligan 2008).[91] Increased maternal care time might furthermore positively influence health outcomes of children and thus their cognitive development through better monitoring ability of their health status and more timely doctor visits (see Berger, Hill and Waldfogel 2005 and studies cited therein), through more time for healthier meal preparation and house cleaning or lower risk of injuries and infectious disease (Morrill 2011). Early maternal employment, especially when exceeding 10 hours per week, might also negatively influence the attachment of mother and child and might lead to behavioural problems of the child (Brooks-Gunn, Han and Waldfogel 2002). Moreover, it is possible that exhausting market-based work leaves mothers not only with less time, but also with less energy for stimulating and nurturing child care to foster cognitive child development (lower quality home environment for cognitive development) (Ruhm 2004; Waldfogel, Han and Brooks-Gunn 2002). Conversely, a prolonged absence from work might raise the risk of social detachment and of depressions by mothers who stay at home, which in turn lowers the quality of maternal time and might have adverse effects for the children (Baum II 2003).

It should be noted that the above arguments and derived hypotheses from this simplified framework are implicitly holding the partner's labour supply and child care input constant. This assumption is less restrictive in countries where the bulk of parental child care is traditionally supplied by the mother and family life follows the 'male bread-winner model'. However, a more sophisticated analysis could furthermore take into account the joint labour force decision of the couple as well as the role of potentially endogenous paternal time investments.

4.2.2 Empirical evidence on the causal effect of maternal employment and cognitive child outcomes based on changes in parental leave legislations

While the relationship between maternal employment and cognitive child development has received a lot of attention in the literature, in recent years the research question has been revisited with the objective to identify the *causal effect* of maternal employment by exploiting exogenous variations in maternal employment generated by

[91] However, although Baker and Milligan (2008) find a significant impact of prolonged parental leave entitlements on breastfeeding duration in Canada, most of their results do not reveal any positive health effects for children.

changes in statutory parental leave entitlements. Table 4-1 provides a summary of these papers focusing on the main features of the respective reforms and those empirical results which are most relevant for the present study. All these studies have as a common starting point an unexpected extension of the granted parental leave duration which significantly increased the time mothers stayed at home before returning to work after childbirth. Furthermore, almost all studies are able to implement either a Difference-in-Difference or a Regression Discontinuity estimation strategy as most of the reforms were implemented with a sharp cut-off date that strictly determined mothers' eligibility to extended leave periods (only in Sweden the 90-days extension was gradually phased in over a period of three months). Against the background of the theoretical considerations regarding the potentially adverse effects of early maternal employment on child development (as well as on maternal health), it seems surprising that most of these studies find either no or only negligible effects of prolonged parental leave on cognitive child outcomes. The only exception is the analysis by Carneiro, Løken and Salvanes (2010) for Norway: based on a sample of mothers who were eligible for parental leave, the authors find significant positive effects of prolonged maternal time at home during the child's first year on medium-run schooling achievement, IQ measures and height. The effects seem to be even stronger for children of mothers with low educational attainment. Interestingly, the Norwegian study is unique in that it can distinguish between children of eligible and ineligible mothers. The authors demonstrate that results become *insignificant* as in the other studies when children of all mothers irrespective of their eligibility and affectedness are included in the estimation sample.[92]

The findings by Liu and Nordstrom Skans (2010) for Sweden stand in contrast to the empirical results for Norway: although the average effect for the pooled sample is statistically not significantly different from zero, it appears that children of better-educated mothers having at least some tertiary education have benefited from the reform and perform better in nationally administered tests. Hence, while in Norway the effect of maternal leave on child outcomes is positive for children from lower-educated

[92] When running estimations on the whole population of eligible and ineligible mothers, it will be the more difficult to detect the actual effect of the reform on child outcomes the larger the group of non-eligible mothers who do not change their return-to-work behaviour in response to the policy change. This is related to the fact that the effect will be averaged over the entire population and not over the actually affected subgroup.

mothers, results for Sweden imply that especially children from better-educated mothers have benefited from the reform.

In order to reconcile the seemingly contradictory evidence of zero effects and positive effects for separate subgroups it is important to pay attention to the essential differences between these studies which complicate straightforward comparisons. The analyses vary predominantly with respect to:

(1) *The affected age group of children*: Does the particular extension of parental leave allow women to stay at home longer during the first year of life (e.g., an extension from 8 to 12 months) or does it affect the period when the child is more than two years old? This point is highly relevant if the importance of maternal care varies over the different development stages of the child.

(2) *The length of the extension*: The analysed parental leave extensions vary between six weeks and 18 months. The granted length of the extension is likely to influence the additional time that mothers stay home. If there is a positive effect of maternal time on child outcomes and this effect is increasing with time input, then one would expect differential effects depending on leave duration.

(3) *The measure of cognitive development and age at its measurement*: While some studies focus on short-run effects measured before the first birthday (parent-reported assessments or psychological tests), others compare medium- or long-term outcomes up to age 29 (e.g., using completed educational attainment). On the one hand, it is possible that initial differences in early cognitive development might be mitigated, for instance, through special education or development programmes over the life course. On the other hand, it could be that particular inequalities in the very early stages of life persist over time or are even aggravated (cumulative effect of an early negative ‘shock’). Furthermore, different types of measures of cognitive development might generate different results if they a) capture different aspects or types of cognitive skills (verbal versus mathematical skills), b) are designed for a particular age (e.g., ‘Number Knowledge Test’ for four year-olds versus high school

grade point average), or c) differ in their precision or level of aggregation (e.g., educational attainment categories versus standardized test scores).

(4) *Different institutional environments*: There are strong differences across countries (and over time) in terms of prevailing non-parental child care arrangements (formal centre-based or informal care by relatives) which determines the type of care likely to be substituted by prolonged maternal care. In fact, there might be in addition an interaction effect of the institutional environment and of the initial parental leave length (i.e., if the pre-reform leave lasted only 3 months mothers might have used different types of child care than if the initial leave lasted already 18 months, which could be related to minimum age requirements in formal day-care facilities).

(5) *The type of the reforms and their indirect effects on other supposedly relevant determinants of child outcomes like income and fertility*: e.g., does the reform expand the duration of fully, partly and/or unpaid leave? These indirect effects could alter the (opportunity) costs of children and also enhance or change the 'quantity-quality' trade-off.

(6) *The precision of the data and the estimations*: Can eligible mothers be identified (only possible in the Norwegian study) or can children be linked to parents? Are the studies based on representative surveys or huge administrative datasets?

(7) *The estimation strategies*: the exact implementation of the DID and RD estimations differs across studies as do the control groups.

Table 4-1: Studies using changes in parental leave legislations to identify causal effects of maternal employment on child outcomes

Publication (Authors and Year)	Country and Data Source	Year & Substance of PL reform	Assessed child outcome(s) (Short/Medium/Long run effects)	Main empirical method	Results on effect of reform on child outcomes	Heterogenous effects	Institutional background: provision of child care
Baker and Milligan (2010a)	*Canada:* National Longitudinal Study of Children and Youth (NLSCY); about 2,000 children per cohort	• December 31, 2000 • Maximum duration of maternity leave benefits raised from 25 to 50 weeks (out of which 10 and respectively 35 weeks can be claimed by either mother or father) • Pre-reform job protected maternity leave varied between 18 and 70 weeks across regions. Post-reform maternity leave duration increased to at least 52 weeks in all regions.	**SR effects:** • Children between 7 and 24 months • Parent-reported measures of temperament, motor and social development	Test of differences between average outcomes of birth cohorts born before and after the reform; (regressions based on six yearly values)	• Overall small and mostly insignificant effects on the development variables	• Not tested	• Centre-based care for children under 12 (24) months very low (4 % and 6 %) • **Mainly informal care** (about 39% and 41%)
Baker and Milligan (2010b)	*Canada:* National Longitudinal Study of Children and Youth (NLSCY); about 2,000 children per cohort	• See Baker and Milligan (2010a)	**SR effects:** • At ages 4 or 5 • Cognitive development (e.g., Peabody Picture Vocabulary or Number Knowledge Test) • Parent-reported behavioural development (e.g., hyperactivity)	See Baker and Milligan (2010a)	• No significant positive effects	• None (No differences by child gender or parental education)	See Baker and Milligan (2010a)

Carneiro, Løken and Salvanes (2010)	*Norway*: Administrative register data on schooling and family events and military records (linked child-parent data)	• July 1, 1977 • Introduction of paid PL for 18 weeks (4.5 months) with 100% income replacement as well as extension of unpaid PL from 3 to 12 months (on top of paid PL) [de facto increase in PL take-up from 8 to 12 months]	**MR & LR effects:** • Dropout rates from high school (measured at age 29) • College attendance (measured at age 29) • IQ (males aged 18-19) • Teenage pregnancy (females with birth before age 20) • Height (males aged 18-19)	Non-parametric RD (1977 cohort; local linear regression) and non-parametric RD-DID (cohorts 1977 and 1975)	• **Significant, positive effect** on high school graduation, college attendance and IQ (for males) for sample of eligible mothers • Insignificant effects when including ineligible mothers	• Yes, stronger positive effects for children from households with **lower maternal education** (less than 10 years of schooling) • No differences by child gender or pre-birth household income	• Extremely low enrolment rates of zero- to two-year-olds in public child care in 1977; **mainly informal** child care through relatives
Dustmann and Schönberg (2010)	*Germany*: Administrative data on public schools in three federal states (information on type of school/ track and graduation); social security data on educational attainment	• Three reforms: 1979, 1986, 1992 • May 1, 1979: Extension of paid+job protected PL (flat rate) from 2 to 6 months • January 1, 1986: Extension of paid+job protected PL (flat rate up to month 6; means-tested from month 7 to 10) from 6 to 10 months • January 1, 1992: Extension of unpaid job protected PL from 18 to 36 months (maternity leave payments up to month 18)	**MR & LR effects:** • 1979 reform: wages and educational attainment at age 28 or 29 • 1986 reform: Graduation from academic track (before age 20) • 1992 reform: Choice of school track at age 14 (8th grade) (most/medium/least academic track)	DID and TS-2SLS (RD and RD-DID as robustness check)	• **No significant effects** or only extremely small positive effects • Effect of expansion of 18 to 36 months even slightly negative	• Not tested	Enrolment in formal day care centres low (5% for under 18-months-olds); Child care **mainly informal through grandparents** or other relatives (29 %)

Liu and Nordstrom Skans (2010)	*Sweden*: Administrative register data	• August–October 1988 • Extension of paid PL benefits from 12 to 15 months • Gradual extension by 30 days in each of three consecutive months in 1988: 1st of August/September/October	**MR effects:** • Test scores from national tests during last year of compulsory school • Compulsory school grades (GPA scores) scores at age 16	OLS regression of child outcomes on legal number of PL months (according to birth month of child)	• Average effect on child outcomes is insignificant	• **Positive effect for well-educated mothers** (some tertiary education) • No differences between boys and girls	**Established public child care** system: 40-50% of children aged 1-2 in formal day care or family centres; only few children in informal care
Würtz Rasmussen (2010)	*Denmark*: Administrative register data (linked child-parent data); PISA 2000	• March 26, 1984 • Extension of paid PL from 14 to 20 weeks	**MR effects:** • High school enrolment • High school GPA • Reading test scores of 15-years old children (PISA test in 2000)	RD (DID as robustness check)	• No significant effects	• None (No differences by child gender or parental education)	Publicly subsidized **day care system even for very young** children available

Notes: PL – Parental leave; RD – Regression Discontinuity; DID – Difference-in-Difference; TS-2SLS – Two-Sample Two-Stage-Least-Squares; IV – Instrumental Variables; GPA – Grade Point Average; SR/MR/LR – short-run/medium-run/long-run effects

For instance, consider the seemingly contradictory findings for Norway and Sweden: one has to keep in mind that the Norwegian study focuses on a reform affecting leave taking between month 8 and 12 after childbirth and considers long-term schooling outcomes at ages 18 to 29 (e.g. high school drop-out rates). Furthermore, the predominant type of child care for children of the relevant age group was mainly informal (through relatives), suggesting a substitution effect of the reform from care by relatives to maternal care (at least relatively *more* maternal care compared to the situation before the reform in families combining both types of care). The Swedish reform affected children at a later stage of development, aged 12 to 15 months and the later observed child outcomes are test scores of national tests and grades in certain subjects during the last year of compulsory school (age 16). In contrast to the Norwegian scenario, almost half of Swedish children aged 1 to 2 during the time of the reform were in public day-care and informal child care arrangements were very uncommon. Hence, the positive effect for children of well-educated mothers in Sweden might be related to a substitution of public day care with maternal care and the possibility that well-educated mothers provide comparatively better quality care for their children than public institutions, which might not be the case for less educated mothers (Liu and Nordstrom Skans 2010).[93]

On the other hand, the authors of the study for Norway conjecture that the reform resulted in a replacement of informal care through maternal care with positive effects on all children, but seemingly more pronounced effects for children of lower educated mothers (Carneiro, Løken and Salvanes 2010). Moreover, their findings are in line with results from Bernal and Keane (2011) who show that only increased informal child care (i.e., non-centre based child care by grandparents or other relatives) has adverse effects on the cognitive development of children of single mothers in the USA, while an additional year of centre-based child care has no negative effects. Bernal and Keane (2011) relate their results to other empirical studies[94] providing evidence for negative effects of informal child care by grandparents on cognitive child development and summarize the two possible main channels for this adverse effect: (a) trained, well-

[93] Generally, parents can combine different types of child care and it is also possible that the reform helped parents to combine different types of child care more efficiently.

[94] Bernal and Keane (2011) refer to two studies from the UK from Hansen and Hawkes (2009) and Gregg, Washbrook, Propper and Burgess (2005). However, the evidence from Hansen and Hawkes (2009) regarding the effects of child care by grandparents is mixed and depends on the outcome (the study finds negative behavioural effects, especially for boys).

educated personnel in day centres could generate better cognitive stimulation for children and (b) there might be positive effects through interactions between children as well as increased educational activities fostering cognitive development. Similar results are found by Datta Gupta and Simonsen (2010) for Denmark who exploit regional differences in excess demand and waiting lists for child care: while participation in centre-based care with highly qualified personnel does not have detrimental effects on non-cognitive child outcomes in comparison with home-based care, family day care (in private homes by one child minder) has negative effects on non-cognitive outcomes, especially for boys from less educated mothers.

To sum up, the institutional background and the details of the reforms vary widely across countries and seem to play an important role for the effect of parental leave on child outcomes. In terms of institutional and cultural set-up, Austria comes closest to Germany, while the type of reform (paid leave extension between the child's first and second birthday) is more similar to the evaluated reform in Sweden where, however, participation rates of one- to two-year-olds in formal child care are very high. Nevertheless, the reform in Sweden led only to a three months extension of parental leave, while in Austria the extension comprised 12 months. This way the analysis in this chapter helps to shed more light on the influence of maternal employment beyond the child's first birthday when formal day care for this age group is generally not available. Furthermore, the Austrian reform is unique in that it involves an exceptionally long extension of paid parental leave.

4.2.3 Heterogenous effects

Some of the quasi-experimental studies mentioned in the previous section have also investigated in more detail whether maternal employment has differential impacts across population subgroups, focusing in particular on the level of the mother's educational attainment and child gender. As Table 4-1 indicates, there are no universally consistent patterns across these studies with respect to maternal education. While most papers do not find any differences in the impact between children of more versus less educated mothers, the Norwegian study finds more pronounced effects for children of lower educated mothers and the Swedish results indicate positive results for better educated mothers (as has been discussed in the previous section).

In terms of child gender, none of the four papers who run separate regressions for girls and boys detect any significant differences regarding the effect of prolonged maternal leave. In contrast, several previous studies from other strands of literature suggest that maternal employment has a stronger detrimental effect on boys as compared to girls, which is partly explained by the fact that boys seem to be more 'vulnerable to early stressors' and to react more adversely to non-maternal child care (Brooks-Gunn, Han and Waldfogel 2002).[95] These negative effects are also supported by papers assessing the effect on health outcomes (see next paragraph).[96]

4.2.4 Maternal employment and health outcomes of children

As mentioned before in Section 4.2.1, maternal employment might affect cognitive development of children also through its effect on health outcomes. There is a complementary literature on the relationship between early maternal employment and health outcomes of children. These studies face the same identification problem since a mother's return-to-work decision might be driven by unobserved factors like poor health of their children. Three recent studies specifically accounting for the non-random selection of mothers into employment by using quasi-experimental and instrumental variables approaches are Baker and Milligan (2008) for Canada as well as Gennetian, Hill, London and Lopoo (2010) and Morrill (2011) for the USA. While the Canadian study finds strong effects of prolonged parental leave on breastfeeding duration, there are generally no positive health effects on self-reported maternal or child health. In contrast to these findings are the two studies from the US: using age-based kindergarten eligibility for the youngest child as instrumental variable for maternal employment, Morrill (2011) finds that children of working mothers have a greater risk of suffering from adverse health events (overnight hospitalization, asthma episodes, injury and poisoning) and that the negative effect is twice as large for boys than for girls (the effect for the subsample of girls is even insignificant). Gennetian et al. (2010) correct for the endogenous work decision of the mother by using the experimental design of a welfare-

[95] In addition, there is evidence from Denmark that informal or less qualitative day care has particularly detrimental effects on boys (Datta Gupta and Simonsen 2010).

[96] General gender differences in schooling or particular types of cognitive skills (e.g., language versus mathematical skills) might be related to underlying genetic or cultural differences (Guiso, Monte, Sapienza, and Zingales 2008; Machin and Pekkarinen 2008). Furthermore, there is evidence for 'son preferences' leading to unequal treatment of sons and daughters with potentially long-term adverse effects for girls (although this phenomenon is generally associated with developing countries, Mammen (2011) shows that fathers in the USA spend more time with sons than with daughters).

to-work programme which increased the share of mothers working without affecting household income resources (income from the welfare programme was replaced by earned income from market based work). The results imply a negative effect of maternal employment on child health outcomes in low-income families measured at age five to nine; again, this adverse effect seems stronger for boys than for girls.

4.3 Institutional setting and background

This section describes in detail the content and timeline of the reform of the parental leave legislation in 1990 and gives an overview of the institutional background regarding the development of maternal employment as well as child care in Austria over time.

4.3.1 Parental Leave in Austria and the Reform in 1990

4.3.1.1 The development of parental leave in Austria

The history of parental leave in Austria dates back to 1957 when working women became entitled to an unpaid, but job protected leave of up to six months on top of the paid mandatory maternity leave of 12 weeks, making Austria the first country in Europe to introduce parental leave (Neyer 2003). Since that time childbirth related leave from work in Austria comprises two parts: a mandatory maternity leave and an optional parental leave.

4.3.1.2 Mandatory Maternity Leave

The length of the mandatory maternity leave was raised from 12 to 16 weeks in 1974 (Hoem, Prskawetz and Neyer 2001b). More specifically, according to the Maternity Protection Act (*Mutterschutzgesetz)* employed women are not allowed to work during the last 8 weeks before the expected birth date and 8 weeks after delivery (this period extends up to 12 weeks for multiple or premature births or Caesarean sections).[97] While being on leave women receive a ‘maternity pay’ (*Wochengeld*) which equals 100 percent of the average net earnings of the preceding 13 weeks. Furthermore, during pregnancy and until 4 months after delivery, women are subject to a special employment protection and cannot be dismissed by their employer. This period of

[97] Before 1974 the length of the mandatory maternity leave was only 12 weeks.

employment protection is further extended, if the mother takes parental leave immediately after the mandatory maternity leave (job protection until 4 weeks after returning to work).[98]

4.3.1.3 Parental leave before the reform in 1990

After the introduction of optional unpaid parental leave in 1957 – this was in fact maternal leave, since fathers were not entitled to take leave – important amendments to the law followed in the years 1961, 1974 and 1990. In 1961 the maximal duration of job protected parental leave was extended up to the child's first birthday and mothers on leave became entitled to a cash benefit as long as their household income was below a certain threshold. The level of benefits equalled the level of unemployment benefits for single mothers and half the level of unemployment benefits for married mothers.[99] To become eligible for parental leave payments women needed to be in employment subject to compulsory social insurance contributions for at least 52 weeks during the two years preceding the first birth (20 weeks during the most recent year for higher order births).[100] Furthermore, special regulations on work requirements applied for mothers becoming pregnant while being on parental leave: if their expected birth date lay within 14 weeks (3.5 months) after the expiry of the previous parental leave period then their parental leave entitlement was renewed (*automatic renewal*; see discussion in Hoem, Prskawetz and Neyer (2001a) and Lalive and Zweimüller (2009). However, this required women to become pregnant six and a half months after their last birth and which was even biologically difficult to realize and hence uncommon.

The work eligibility requirement for young mothers was subsequently reduced to 20 weeks during the last 12 months (the age criteria for 'young mothers' was at 20 in 1974 and was raised to 25 in 1989). 1974 saw important amendments to the law regarding cash benefits: the amount of cash benefits was uncoupled from household income by granting a flat rate benefit to all mothers on parental leave (single mothers and wives of no or low income earners received a 50 percent higher assistance). A subgroup of particularly disadvantaged mothers became entitled to special maternity

[98] See Law on Maternity Protection (Mutterschutzgesetz) as of March 2011, http://www.jusline.at/Mutterschutzgesetz_%28MSchG%29.html

[99] The information on the parental leave in this section draws mainly on the timeline of changes in parental leave legislation provided in Austrian Family Report 1999, Volume I., chapter 12.2.3, and Volume II., chapter 3.3.2 (BMUJF 1999a, 1999b), and in Hoem et al. (2001b, Appendix C).

[100] Periods during which women received unemployment benefits are counted towards these minimum work requirements.

leave payments (*Sondernotstandshilfe*) up to the child's third birthday (at first only single mothers, in 1990 married women in households with low or no income were added to this group). In 1982 farmers and self-employed mothers became eligible for up to 16 weeks of maternity flat rate transfer payments.

4.3.1.4 Parental leave taking of fathers

Fathers became eligible for the parental leave only as of January 1, 1990. However, their entitlement to parental leave was conditional on the mother meeting all eligibility criteria. The take-up rate of parental leave of fathers remained close to zero during the 1990s (from 0.2 percent in 1990 to 1 percent in 1997, see Table 4-2). These numbers demonstrate that parental leave taking was and is still almost exclusively relevant to working mothers.

4.3.1.5 The parental leave reform in 1990

The following empirical analysis will exploit a quasi-experiment that was created by the amendment to the parental leave legislation that came in effect on July 1, 1990 (*Karenzurlaubserweiterungsgesetz, June 27, 1990, BGBl. Nr. 408/1990*). The main aspect of this reform was the extension of the maximal duration of the optional paid and job protected leave from the child's first up to the child's second birthday (see scheme in Figure 4-1). According to the law, this extension was only granted to mothers of children who were born on or after the cut-off date of July 1, 1990. There were no 'transition rules' allowing mothers who gave birth before July 1, 1990 to benefit from the new regulations. This increase of 12 months of paid and job protected parental leave is much larger than any of the comparable reforms that took place in other countries and that have been evaluated in terms of child outcomes (see Table 4-1). An important side effect of this extension by 12 months was that it became easier for families to get automatic renewal of parental leave: the maximum period between previous birth and *conception* of a future child that would grant automatic renewal was extended by one year (from 6.5 to 18.5 months). As described in more detail further below (see Section 4.4.1), the reform was announced and implemented only shortly before it came into effect. This is why it was not possible for parents to adjust their fertility timing in order to take advantage of the more generous parental leave regime (i.e. there were no anticipatory fertility effects).

Figure 4-1: Parental leave entitlements before and after the reform on July 1, 1990

Other changes that came into effect with this reform were the option of part-time leave between the child's first and second birthday if both parents took their leave simultaneously or up the third birthday if only one parent went on leave or the parents took the leave in turns. However, only a tiny fraction of mothers made use of this new possibility (less than 2 percent of all women on parental leave were on part-time leave in the years 1992-1994, see BMUJF 1999b, Vol. 2, Table 3.46c on page 157). Furthermore, the parental leave subsidy (*Teilzeitbeihilfe*) for farmers and self-employed amounting to half the regular flat-rate parental-leave payment was extended to the child's second birthday. This reduced parental leave subsidy became also available to employed mothers who did not meet the minimal work requirements.

In 1990, the amount of the regular flat-rate parental leave payment was about 340 Euros per month, which corresponded to 31 (40) percent of gross (net) *median* female earnings (Lalive and Zweimüller 2009).[101] In 1996 the flat-rate benefits equalled about 35 percent of the *average* monthly net female earnings (BMUJF 1999b). Since the level of parental leave benefit was independent of previous earnings, the earnings replacement rate was much lower for mothers with high pre-birth earnings than for mothers with low pre-birth earnings. In other words, the associated income loss and opportunity costs of parental leave were higher for the former than for the latter group of mothers.

[101] According to a study by Fuchshuber (2006), which was conducted by order of the Austrian Federal Ministry for Health and Women to investigate potential career barriers and problems for female employees, some firms (in particular larger companies) tend to offer additional benefits for families (financial benefits or fringe benefits like child care facilities) or even special parental leave or job protection arrangements beyond the legally required minimum. Unfortunately, the report does not provide information on whether these benefits were already common in 1990.

Table 4-2: Statistics on the development of maternity and parental leave take-up between 1985 and 1997

Year	Number of women receiving mandatory maternity pay	Parental leave, 1st year	Parental leave, 2nd year	Parental leave (yearly average)	Parental leave (Dec 31)	Parental leave, Men	Parental leave, Women	Share of fathers on parental leave (%)	Number of births	Children aged 0-1	Children aged 1-2	Share of women on mand. maternity leave in comparison to all births (%)	Share of mothers on 1st year parental leave (%)
	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII*)	XIII **)
1985	60,505			37,601	38,440		37,601		82,970	87,906	89,176	72.9	51.3
1986	60,412			38,132	39,031		38,132		83,176	87,004	88,158	72.6	52.6
1987	60,500			38,354	39,493		38,354		81,996	86,625	87,357	73.8	53.1
1988	61,688			43,722	44,959		43,722		83,326	87,298	87,099	74.0	60.1
1989	62,477			44,357	45,876		44,357		84,095	88,534	88,063	74.3	60.1
1990	65,223			46,328	48,897	83	46,244	0.2	85,110	89,932	89,412	76.6	61.8
1991	71,637			59,868	83,039	328	59,540	0.5	90,499	92,573	90,971	79.2	
1992	73,256	n.a.	n.a.	106,195	115,680	781	105,414	0.7	91,123	94,803	93,881	80.4	
1993	75,913	58,660	59,044	117,703	120,514	920	116,783	0.8	91,684	94,797	95,610	82.8	74.3
1994	74,602	60,258	61,010	121,268	122,411	1,014	120,254	0.8	89,509	93,542	95,710	83.3	77.3
1995	73,045	57,210	63,510	120,271	120,611	1,044	119,227	0.9	86,441	89,554	94,118	84.5	76.7
1996	71,258	55,260	62,553	118,252	117,832	1,071	117,181	0.9	84,847	87,641	90,792	84.0	75.7
1997		59,455	50,949	112,239	115,110	1,067	111,170	1.0					

Source: Austrian Family Report 1999 Vol. 2 (BMUJF 1999b), p. 152-157; Notes: *) own calculations based on (I) and (IX); **) numbers adjusted in the Austrian Family Report for the fact that official parental leave lasts only for 10 months during the child's first year of life as it only starts after two months of mandatory maternity leave.

4.3.1.6 *Share of eligible mothers among all mothers*

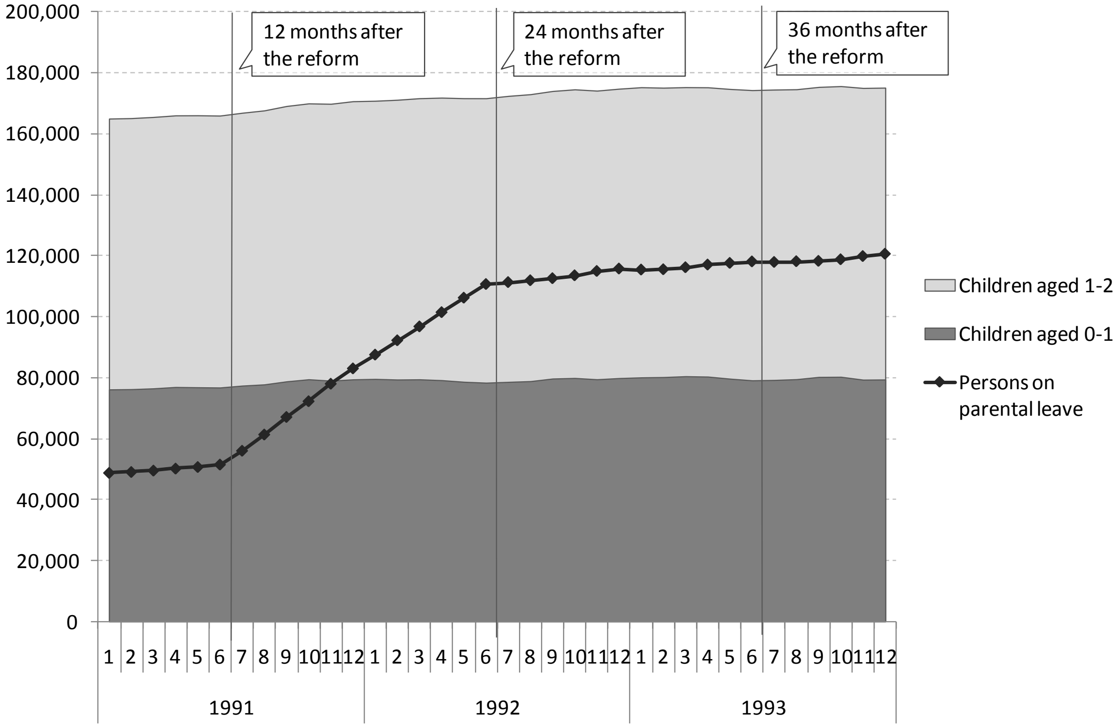
Due to the steadily increasing age at first birth (from 25 years in 1990 to 29 years in 2005; see Human Fertility Database) and high economic activity rates (73 and 70 percent for 20-24 and respectively 25-29 year old women in 1991) the share of mothers meeting the eligibility criteria and being entitled to parental leave payments was already relatively high in 1990. A rough upper bound estimate of the share of mothers who were eligible for parental leave in 1990 is given by the share of mothers on mandatory maternity leave among all mothers (see column XII in Table 4-2). According to this calculation at most 76.6 percent of all mothers were eligible for parental leave in 1990 (the corresponding figure in 1987 is 73.8 percent).[102]

4.3.1.7 *Take-up rates before and after the reform in 1990*

Already during the 1990s the take-up rates of eligible mothers were extremely high. Estimates range between 93 and 96 percent (Kreimer 2002). These numbers are also in line with the Austrian Family Report 1999, according to which the share of mothers returning to work immediately after the mandatory maternity leave and hence foregoing the option of parental leave was only about 5 percent (BMUJF 1999a). As regards the duration of take-up, only an extremely small fraction of eligible mothers returned to work before the end of their entitlement (Kreimer 2002). These high take-up rates as well as the fact that women made use of the full period can also be seen in Figure 4-2 that plots a time series of the monthly numbers of persons on parental leave and receiving parental leave pay between January 1991 and December 1993 (unfortunately, this data is not available for earlier years).

[102] This calculation is in line with the indicator used in the Family Report of 1999 (BMUJF 1999b).

Figure 4-2: Numbers on take-up of parental leave as well as number of children over time showing the effect of the reform of July 1, 1990.



Source**:** Persons on Parental Leave based on statistics from AMS (AMS, Public Employment Service Austria); Number of children: own calculations based on monthly birth records (Human Fertility Database). The number of children 0-1 (0-2) corresponds to the total of children born in the previous 10 (22) months.

Up to June 1991 the percentage of women on parental leave among all women who had given birth within the last year was about 65 percent (this number accounts for the fact that women appear in the statistics on parental leave for only 10 months). The first cohort of women who could remain on parental leave after the child's first birthday started their second leave year on July 1991 (12 months after the cut-off date). The figure demonstrates a steady inflow of mothers into the second leave year up to June 1992, which was the final, 24th month of leave for the first cohort of women who were eligible for 24 month of parental leave. Starting from July 1992 the strong inflow into the stock of persons on parental leave stopped and the share of mothers on parental leave among all mothers with up to two-year-old children stabilized again at around 65 percent (calculated using the adjusted numbers of birth in the preceding 2 years). After July 1992 the amount of women on parental leave continued to grow slightly due to the steadily rising number of eligible women (steady growth of female labour force participation and increasing age at first birth). Overall, the rise in the number of women on parental leave is approximately in line with the claim of high take-up rates of the extended leave duration: between June 1991 and June 1992 the monthly average number of women on parental leave doubles from about 52.000 to 111.000 (the latter is the sum of mothers on 'first year leave' *and* on 'second year leave'; the fact that the number more than doubles is related to the fact that there is a slight increase in number of children aged zero to two between these two points of time).

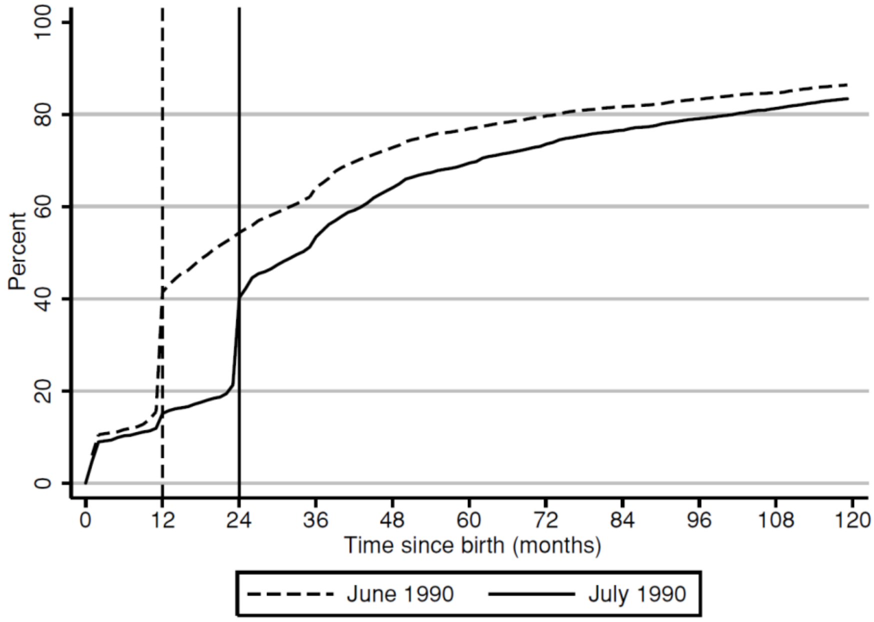
4.3.2 Effects on subsequent maternal labour market success and fertility

Previous studies investigating the effects of the 1990 parental leave extension have focused on outcomes and behavioural changes of new mothers, in particular their short- and medium-term labour market outcomes as well as their fertility behaviour.

4.3.2.1 Short-term and medium-term effects on maternal employment

The fact that the duration of the leave entitlement before and after the reform in Austria was actually binding and thereby exogenously determining the minimum length of leave take-up has been demonstrated by Lalive and Zweimüller (2009) and Lalive, Schlosser, Steinhauer and Zweimüller (2010). This result can be illustrated with Figure 4-3 from Lalive and Zweimüller (2009): it depicts the proportion of pre- versus post-reform mothers having returned to work at any time after giving birth to their first child. There are about ten percent of mothers who return to work immediately after the end of the mandatory maternity leave, i.e. two months after childbirth, and this pattern holds generally true before and after the reform. Both the dotted line for pre-reform mothers (June births) and the solid line for post-reform mothers (July births) reveal a jump of roughly 20 percentage points at the time when the legal leave entitlement expires (after 12 months and 24 months respectively). This reaction at the end of the parental leave shows that the duration of job-protected and paid leave is binding and determining the work decisions of a large group of mothers. Nevertheless, the majority of mothers stay out of the labour force and do not return to work immediately after the parental leave has elapsed. The effect of the reform becomes visible when considering the share of women having returned to work after 23 months: while only about 20 percent of the post-reform mothers have returned to work after 23 months, the majority (around 55 percent) of women having given birth under the old legislation have returned to work after 23 months; hence, the reduction in short-term re-entry (within 23 months) equals approximately 35 percentage points. After 24 months, i.e. after the end of the two year parental leave, post-reform mothers are still over 10 percent less likely than pre-reform mothers to have returned to work. After three years 62 out of 100 pre-reform mothers have returned to work, whereas the same is only true for 52 out of 100 post-reform mothers. Although this gap in return-to-work rates between the two groups narrows slowly over time, post-reform mothers remain slightly less likely to have ever returned to work even after ten years (the difference amounts to three percentage points). Irrespective of the duration of the parental leave a substantial fraction of mothers do not return to work within ten years after giving birth (around 18 percent of post-reform mothers; the number for pre-reform mothers is only slightly lower).

Figure 4-3: Share of mothers returning to work after birth



Source: Figure 5.B. taken from Lalive and Zweimüller (2009, p. 1387)

Despite the substantial changes in return-to-work behaviour, there are no medium-term effects on alternative labour market outcomes like average number of months in employment or earnings per month (Lalive and Zweimüller 2009). Even though in the short-run post-reform mothers work significantly fewer months on average and have lower earnings than pre-reform mothers, there are no significant differences in these outcomes after ten years.

Overall, the reform has had a significant impact on the time mothers take parental leave after birth. The average duration of parental leave take-up and receiving benefits (after mandatory maternity leave) increased from 10 to 20 months (Lalive, Schlosser, Steinhauer and Zweimüller 2010). Lalive and Zweimüller (2009, p. 1399-

1340) conclude furthermore that "most mothers exhaust the full duration of their leaves (sic); that return to work is substantially delayed even after PL [parental leave] has been exhausted".

While Lalive and Zweimüller's (2009) analysis is limited to first births, results generally hold for higher order births as well. As Lalive et al. (2010) demonstrate, the parental leave extension had no significant medium-term effects (after five years) on employment probability, months worked per year, daily earnings or annual incomes of mothers with first or higher order births.

Last but not least, it should be noted that mothers who gave birth after the extension of the parental leave experienced an income loss of about 3,200 Euros which, is caused almost exclusively by foregone earnings during the prolonged leave period and not by differential earnings developments after the expiration of the parental leave entitlements (Lalive et al. 2010).

4.3.2.2 Effects on fertility behaviour (timing and completed fertility)

Theoretical considerations as well as empirical evidence on the fertility effects of the reform are presented in the paper by Lalive and Zweimüller (2009). In their terminology, the overall impact of the reform on fertility was a combination of *current-child effect* and *future-child effect*. The current-child effect relates to changes in fertility behaviour which, in combination with the 12 months leave extension, made it biologically more feasible for couples to take advantage of the prolonged automatic renewal period in case of a new pregnancy (the automatic renewal period increased from 15.5 to 27.5 months). The authors use the term future-child effect to refer to changes in fertility due to the reduced costs associated with giving birth under the new legislation with extended job protection and benefits (the more generous parental leave regulations affect the costs-and-benefits of future births as such). Their empirical analysis of the current-child effect is based on a comparison of short- and medium-term outcomes of treated and control mothers (giving birth to their first child in July 1990 or June 1990 respectively), who do not differ in pre-birth characteristics and who face similar labour market conditions but are subject to more or less generous parental leave regulations. The empirical approach basically resembles a fuzzy Regression Discontinuity Design, since *eligibility* to the extended leave period is based on a random assignment according to the cut-off date of childbirth, July 1, 1990 (the authors perform several sensitivity checks and provide supporting material and facts for this and other underlying identifying assumptions). The estimation of the *future child effect* (the role of extended parental leave on future births) is based on a comparison of short-term outcomes from women giving birth in June 1990 versus June 1987 (both groups are subject to the same parental leave regulations of one year for the current child, but differ with respect to the length of parental leave granted for higher order births within the next three years).

According to the analysis of Lalive and Zweimüller (2009), the reform had a strong and significant impact on fertility outcomes in the short (within three years after the previous birth) and medium-run (after ten years). The extended parental leave caused an increase in the probability of having a second child within three years after the previous birth by five percentage points (15 percent) due the *current child effect* and by seven percentage points (21 percent) due to the *future child effect*. These short run effects also translate into medium-run effects: after ten years post-reform mothers are three percent more likely to have given birth to another child than pre-reform mothers. A more detailed analysis of the timing effects reveals further interesting changes: the probability of giving birth within the first 16 months actually decreased. The overall positive effect in the three-year period is hence driven by the strong increase of the birth probability between month 17 and 28. In sum, while there is a significant decrease in the share of siblings born within a very short time interval (less than 16 months apart from each), the increase of the share of siblings whose age gap is between 17 to 28 months is even larger leading to an overall reduction in spacing between births. Lalive and Zweimüller (2009) conclude from their findings that the 1990 reform not only affected the spacing between subsequent births (tempo or timing effect), but is likely to have actually increased completed fertility (quantum effect). Their results are robust to sensitivity and placebo checks (controlling for pre-birth characteristics; excluding births one week before and after the reform; running a placebo experiment using June 1987 and July 1987).

However, another study by Šťastná and Sobotka (2009) who use data on individual, parity specific birth records and who also investigate the effect of the 1990 parental leave extension on subsequent fertility behaviour, do not find any medium-term effects on total number of children within ten years after the reform. Nevertheless, this latter study confirms the findings by Lalive and Zweimüller (2009) on changes in spacing between first and second births. It furthermore demonstrates that these changes apply to birth intervals between second and third births as well. More specifically, Šť'astná and Sobotka (2009) show that the extended leave period reduces the occurrence of very short birth intervals of 15 to 20 months and increases the share of births occurring after 21 to 26 months following the last birth. The difference between the two studies lies mainly in the different types of data and samples: while Lalive and Zweimüller (2009) extract the relevant fertility information from administrative Social Security records and is thus limited to a certain subpopulation of women (who ever were employed in jobs subject to social insurance contributions; public sector workers as well as self-employed and farmers are not included), the analysis by Šť'astná and Sobotka (2009) involves information on all live births by women residing in Austria and is thus comprehensive. However, since the reform in 1990 could by definition only affect working women, it is likely that the latter analysis provides less precise estimates (since it is based on working (=eligible) and non-working (=non-eligible) women). On the other hand, information on birth events constructed from the Social Security database could be also imperfect, since births occurring before the first job are not captured.

One of the first studies to analyse the effect of the reform in 1990 on subsequent fertility was conducted by Hoem, Prskawetz and Neyer (2001a, 2001b). Using the Austrian Family and Fertility Survey from 1995/1996 to analyse trends and changes in *third births* in Austria from 1960 to 1996, they find that the July 1990 reform had a *tempo* effect on third births (at least in the short run), in the sense that the general trend towards postponement of third births was temporarily reversed. Since their analysis does not go beyond the year 1996 it is impossible to draw any conclusions regarding the persistence of this effect over time. Hoem, Prskawetz and Neyer (2001a, 2001b) attribute this narrowed spacing between second and third births to the implicit 'speed premium' (automatic renewal period; waiver of employment requirements) of the new parental leave legislation.

4.3.2.3 Heterogeneity of effects across population subgroups

To test for differential reactions to the reform across subgroups, Lalive and Zweimüller (2009) run separate analyses for high and low wage earners (women earning above or below the median daily income one year before the birth) on the one hand and for white- and blue-collar occupations on the other hand. Both groups, high-and low-wage mothers react to the extended leave period with higher fertility rates in the short term (within three years). However, mothers with low pre-birth wages react almost twice as strongly as mothers with high pre-birth wages: the probability of having an additional child within three years after giving birth increases by seven versus four percent respectively. Interestingly, a more detailed timing analysis reveals that high-wage mothers respond by significantly reducing their very short-term fertility (within 16 months after the previous birth), whereas this short-term postponement effect is much smaller for low-wage mothers. These group differences become even more pronounced when looking at the long-term fertility effect: while low-wage mothers have an excess fertility of five percent after ten years, there is no significant change in long-term fertility for high-wage mothers. Hence, high-wage mothers seem to have reacted to the extended leave period mainly by changing the spacing between births, while low-wage mothers have also increased the total number of births in the next ten years. Lalive and Zweimüller (2009) hypothesise that financial support during the parental leave period is supposedly more relevant to low income families by mitigating potential financial distress, whereas the job protection guarantee might be more relevant for career-oriented women with higher wages and more specialized skills and higher costs of job lost. In contrast to these stark differences in fertility behaviour, there seem to be no differential effects on short- and medium-term labour market outcomes apart from a slightly stronger short-run earnings reduction for low-wage women.

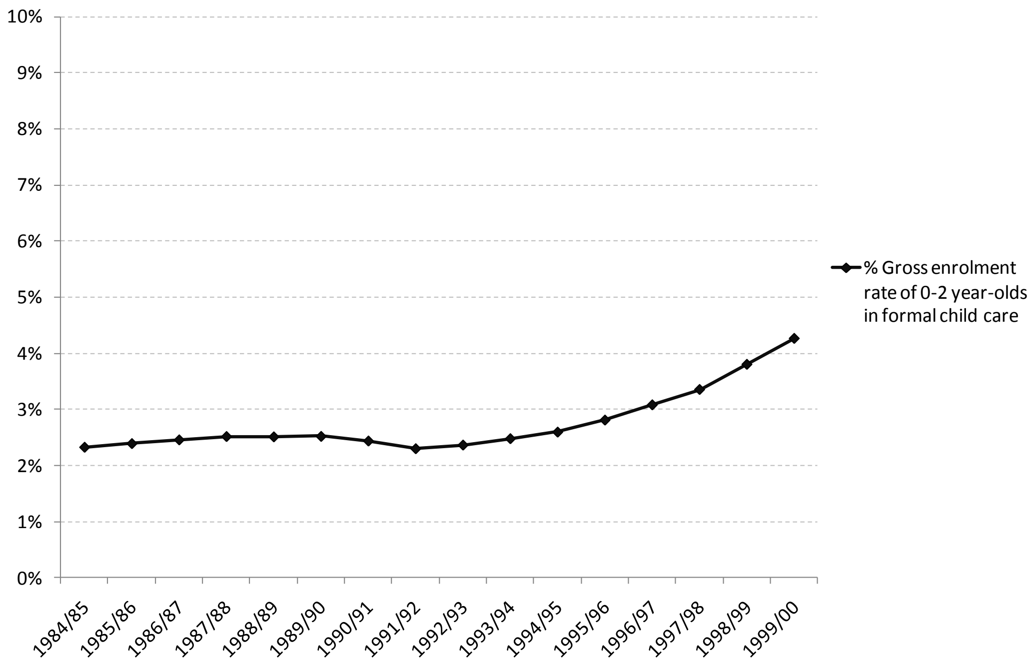
A different picture emerges when assessing differences between white- and blue-collar workers: both groups react quite similarly to the more generous leave regulations in terms of their short- and long-term fertility outcomes, even though white-collar mothers tend to adjust their timing more than blue-collar mothers. Under the more generous parental leave regime a larger fraction of white-collar women has their second child already within 28 months after the last birth (within the prolonged automatic renewal period of now 28 months). However, white-collar workers tend to react more strongly to the reform than blue-collar workers in terms of labour market outcomes. Especially in the short-run, treated white-collar workers have lower earnings and are less likely to return to work than white-collar control mothers. And although in the long-run there are generally no effects for either of the occupation groups, white-collar mothers have a slightly lower probability of having returned to work within ten years after giving birth (there are no differences in terms of long-term employment or earnings outcomes).

4.3.3 The availability and usage of formal and informal child care facilities

4.3.3.1 Child care for children from 0-2 and 3-6 years of age

The potential effect of maternal employment on child development depends crucially on the type and quality of care children are exposed to during the working hours of their mothers. Furthermore, the availability of centre-based or other forms of non-parental care (including care by relatives) for pre-school children generally facilitates the reconciliation of work and family life of parents and especially mothers and might therefore affect female labour force participation. This is why it is important to understand the extent of the availability and usage of child care in Austria around the time of the reform. Unfortunately, comprehensive child care information and in particular separate information by different types of child care – formal and informal – in Austria for the year 1990 or before is extremely scarce.

Figure 4-4: Gross enrolment rates of zero- to two-year-olds in formal child care (% of children in respective age group)

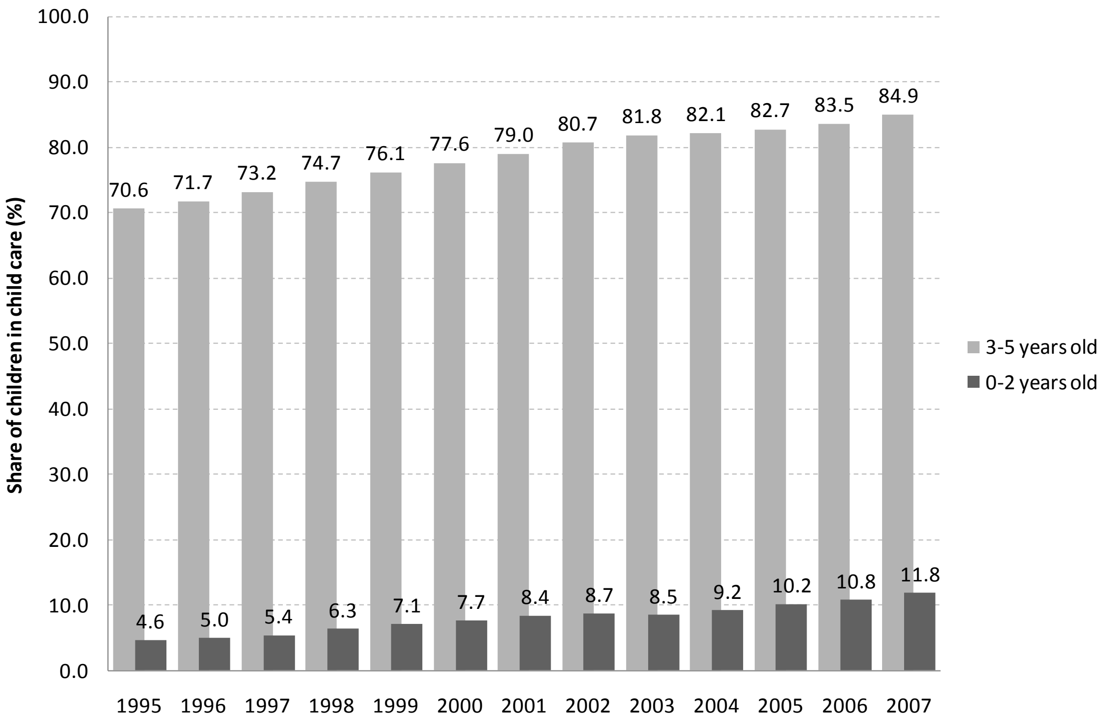


Notes: Gross enrolment rates of zero- to two-year-olds based on own calculations, dividing absolute numbers on children enrolled in formal child care by the sum of all births from the previous two years. Data sources: Statistik Austria (2010) and Human Fertility Database.

Figure 4-4 depicts the development of the enrolment rate of children younger than three years of age using an indicator that was calculated based on information on the number of births in the previous two years and enrolment numbers of zero- to two-year-olds in kindergarten (children enter formal care and school typically in September). According to this measure only about 2.5 percent of this age group participated in formal child care.[103]

Furthermore, there are official statistics on the provision of and participation in formal pre-school child care starting from 1995. While the numbers for zero- to two-year-olds are low, enrolment rates of children aged three to six in formal childcare (Kindergarten) are much higher and rose over time (from around 70 percent in 1995 up to 85 percent in 2007, see Figure 4-5).[104]

Figure 4-5: Share of children in institutional day care over time (average for Austria)



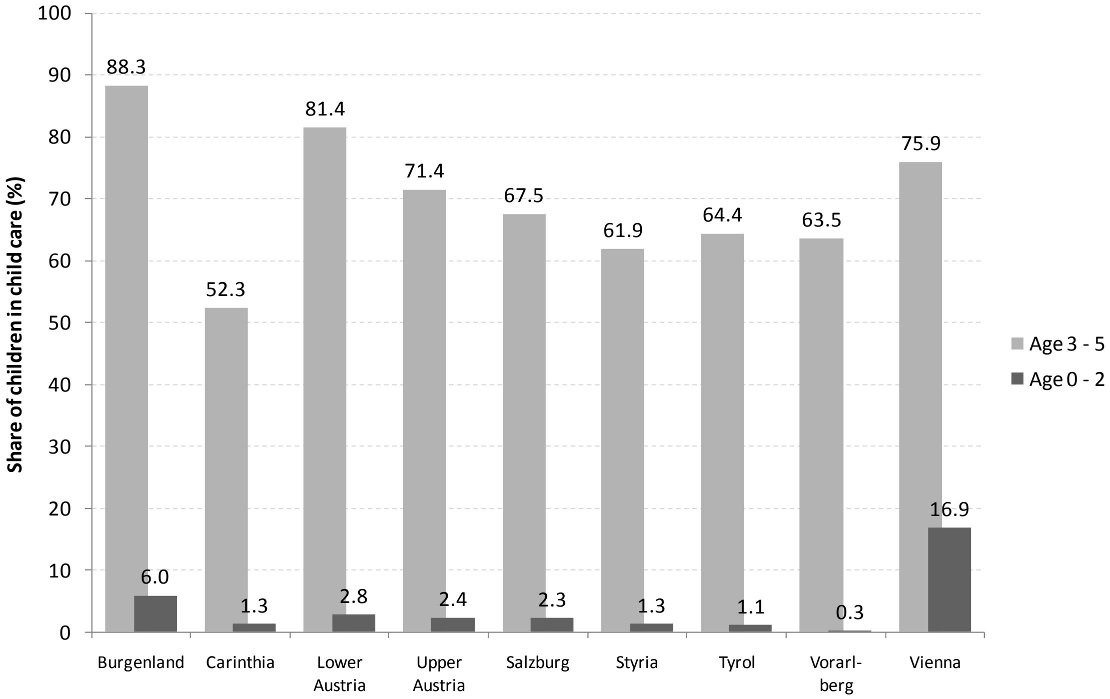
Source: Data from Statistik Austria (2010).

[103] These numbers tend to be biased slightly downwards, since the denominator does not take into account infant mortality. However, the positive trend in the figure is not driven by changes in infant mortality.
[104] Unfortunately these official statistics on enrolment in pre-school day care centers are only available starting from 1995.

Nevertheless, these rates are still below the 90 percent target rate set by the European Union for 2010 (Stanzel-Tischler and Breit 2009). The supply and usage of institutional child care for children under three years is even lower, but has traditionally been extremely low in Austria: in 1995 only 4.6 percent of children in this age group were registered in day care centres. Given the high Austrian female labour force participation rate it is not surprising that a survey in 1995 revealed excess demand for institutional day care for under three-year-olds (Dörfler 2004). Alternative enrolment figures of child care ratios stem from the micro census 1995 which collected information on overall external child care (institutional facilities as well as child minders or independent child groups). According to the micro census data, six percent of children under the age of three were in non-family day care, out of which about one third (36 percent) was provided through non-institutional child care arrangements (BSMUJ 1999a, p. 534). An OECD report (OECD 1989) provides numbers on enrolment rates in formal pre-primary education at age two in 1986/1987, i.e. before the parental leave reform: in Austria only 1.2 percent of the two-year-olds were enrolled in institutional care-centres, while in Norway and Finland the corresponding rates were 20.8 and 21.1 percent respectively (however, these numbers do not include other, less education oriented institutions like day mothers, crèches, day-care centres as well as informal care).

Apart from differences across age groups there is also a substantial variation in the proportion of children in institutional day care across regions in 1995 (see Figure 4-6). The region with the highest share of under three-year-olds in day care centres, namely 16.9 percent, is Vienna, the Austrian capital and the only city in the country with more than one million inhabitants (about 1.5 million according to the 1991 Census). The corresponding numbers of other regions of the country are much smaller and the day care shares ranged between 0.3 and 6.0 percent. Even though the overall ratio of children in formal care has increased over time this regional pattern has remained unchanged.

Figure 4-6: Share of children in institutional formal day care across regions in the year 1995



Source: Data from Statistik Austria (2010).

A special section on child care in the micro census from 2002 furthermore reveals that 9.8 percent of children under the age of three were enrolled in formal child care facilities (BSMG 2003). Within this group the highest fraction (40.4 percent) of children attended public pre-kindergarten child care (Kindergarten/Krippen), 21.9 percent attended private kindergarten, 23.7 percent were looked after by day mothers and 8.3 percent attended play or child groups. 70.6 percent of these children attended the child care facilities on five days a week, 29.3 percent on four days.

Unfortunately, information and data on use of informal (unregulated and not registered) child care provided by relatives, friends, neighbours, babysitters in Austria is very sparse. Nevertheless, this form of child care seems to play a non-negligible role in Austria, as 20 percent of children under the age of three are cared for by unpaid child-minders in a typical week in 2008 (OECD Family database 2010 (OECD 2010, Table PF3.3.A)).

Against this background of limited availability and low enrolment rates of zero- to two-year-olds in formal non-family day care in Austria even in the years 2002 to 2008, it seems unlikely that the reform in 1990 and the prolonged duration of the parental leave implied a substitution from formal to maternal child care. Instead, the numbers tend to support the idea that the reform generated a substitution from individual informal day care arrangements (by relatives, friends, neighbours, etc.) to maternal care among those mothers who extended their leave period from one to two years as a reaction to the reform.

4.3.3.2 Time use of parents

An analysis of the interrelationship of own and partner's labour force participation and child care activities on the Austrian Time Use Survey from 1992 produces three interesting and relevant findings (Neuwirth 2004). First of all, and not surprisingly, increased market working time is associated with less time spent on child care activities. However, there is only an imperfect substitution between working and child care time: one additional hour of own labour market activity reduces maternal child care by only about 10 to 20 minutes. Furthermore, daily time devoted to child care rises with educational level ceteris paribus, i.e. holding market working time constant: mothers with university degrees spend on average 50 minutes per day more on child care activities than mothers with compulsory education. Time spent on child care is also higher for under three-year-olds than children aged four to six.

4.3.4 Possible effects of the Austrian reform on child outcomes (possible mechanisms)

Combining these identified reform effects with the theoretical considerations outlined in Section 2.1 there seem to be two potential channels through which the parental leave extension in Austria could have affected child development: first, a 'quality channel' which works through the potentially superior quality of maternal care as opposed to alternative forms of child care, and second, the 'fertility channel', which works through changes in the fertility behaviour, i.e. the number of children and the spacing between births.

To summarize, the parental leave reform in 1990 significantly raised the time new mothers stayed at home after childbirth between the child's first and second birthday, but it did not affect medium- to long-run income and labour market outcomes of the average mother (thus, it is unlikely that the reform exerted a negative income effect on child outcomes driven by medium- to long-term income losses; if anything, the leave extension generated a relatively small and only short-term income loss caused by foregone earnings during the additional leave months).[105] At the time of the reform the prevailing form of child care for children under two years of age was almost exclusively informal care provided by grandparents or other persons. Under the hypothesis that maternal care is superior to informal care for very young children, one would expect that the prolonged parental leave period had a positive impact on child outcomes. This could be especially true for better educated mothers if they are able to provide higher quality and more 'productive' maternal care, for instance, through better access to knowledge on how to foster cognitive development of children (Grossman (2006) provides an overview and a critical discussion of the theoretical models as well as the empirical evidence regarding the effect of education on nonmarket outcomes, in which he distinguishes between *productive* and *allocative* efficiency, both of which are increasing in human capital and education).

However, the reform also altered the fertility behaviour of parents, most unambiguously the time interval between adjacent births: although the incidence of extremely short birth intervals declined, the average birth interval between first and second as well as second and third child decreased due to the incentives generated by the automatic renewal period. If shorter spacing between births reduces the time and material resources that are allocated to each child, this effect could have negative implications for child development and cognitive outcomes. On the contrary, if the relation between spacing and child outcomes is non-linear, a positive effect of the reduction in extremely short birth intervals could outweigh a negative effect from the average reduction in spacing. There is also some empirical evidence that the reform might have increased the total number of births per woman. If this is the case, this effect could work in the opposite direction and diminish or reverse a potentially positive time effect.[106]

[105] However, for those women who would *not* have returned to work after the child's first birthday under the old regime anyways, the prolonged parental leave payments might have implied a *gain* in short term income as these mothers would not have earned *any* income during the second year. Furthermore, it is difficult to assess to what extent this short-term income loss from working mothers translates into a potentially negative income shock for the child: if non-parental child care is costly, a fraction of the mother's earned income will be spent on child care and it is not clear whether the remaining amount of maternal income is actually larger or smaller than the parental leave payments. Only if the reform leads to a significant short-term income loss, there might be negative effects on child outcomes which could work against any positive time effect.

[106] The same could be true, if the reform exerts a significant negative income shock for families (see discussion in footnote 105).

Moreover, the reactions to the reform differed across population subgroups: high-wage mothers reacted less strongly to the extended leave period in terms of fertility behaviour than low-wage mothers (see Section 4.3.2.3). For the group of high-wage mothers there is no significant rise in overall fertility (no potentially negative quantity-quality trade-off), there is a much more pronounced decrease in extremely short birth intervals of 16 months (which should have a positive impact on child outcomes) and a weaker increase in short term fertility (within three years). In contrast, low-wage mothers have an excess fertility in the short and in the long-run by about seven and five percent respectively and there is no reduction in the incidence of extremely short birth intervals. These fertility effects are more likely to offset any positive time effect of the prolonged leave duration on child outcomes of low-wage mothers.

Overall, it seems that any positive effect from increased maternal time should be more easily detectable among children of high-wage mothers who altered their fertility behaviour only very modestly. For the children of low-wage mothers the effect of the parental leave reform is a mixture of counteracting changes of maternal care, increased family size and changed spacing between births.

4.4 Empirical approach

This section describes the empirical method used to investigate the link between the extended parental leave and cognitive child outcomes. It discusses the corresponding identifying assumptions as well as several refinements and potential problems with the approach.

4.4.1 Difference-in-Difference estimator and identifying assumptions

Given the unexpected and strict implementation of the prolonged parental leave period for all children born on July 1, 1990 or later, it is possible to use a Difference-in-Differences (DID) analysis to identify the effect of extended maternal care on child outcomes (like in the analysis of Germany by Dustmann and Schönberg 2010). The DID regression specifications are the following:

$$y_i = \alpha + \beta_1 Post\,June + \beta_2 y2006 + \beta_3 Post\,June * y2006 + \theta_m birth\,month + \varepsilon_i \qquad (4.2)$$

$$y_i = \alpha + \beta_1 Post\,June + \beta_2 y2006 + \beta_3 Post\,June * y2006 + \theta_m birth\,month + \mu X + \varepsilon_i \qquad (4.3)$$

y_i is the measure for cognitive child outcome (i.e. test scores from standardized tests from the two years 2006 and 2003 covering the cohorts of children born in 1990 and 1987, respectively, see Section 4.5 for data description), *Post June* is a dummy variable taking on the value 1 for all children whose birthday is on or after July 1 (July – December) and the coefficient β_1 captures all possible permanent and general differences between children born in the first and the second half of a given year; *y2006* is a dummy variable for the year 2006 that controls for the common trend between the two test years 2006 and 2003; the interaction effect between *Post June* and *y2006* identifies all children whose mothers were affected by the reform and eligible to a longer parental leave – β_3 is the coefficient of interest and measures the treatment effect; to account for possible season of birth effects as well as age effects the regressions include a set of *birth month* dummy variables.[107] To control for possible differences in sample composition over time the more refined specification (4.3) contains additionally a set of parental and other background characteristics (*X*).

If the assignment into treated (=post reform; 24 months PL) and control group (=pre reform; 12 months PL) is 'as good as random', a simple representation of the estimated treatment effect $\hat{\beta}_3$ (estimated by OLS) is

$$\hat{\beta}_3 = (\bar{y}_{2006,post} - \bar{y}_{2006,pre}) - (\bar{y}_{2003,post} - \bar{y}_{2003,pre}). \quad (4.4)$$

This is the difference in average cognitive outcomes (test scores) of children born after versus before the reform (whose mothers were eligible to 24 versus 12 months of paid parental leave respectively) less the difference in outcomes of children born before and after July 1, 2003 who were not subject to the reform.

The advantage of this DID approach is that potentially confounding systematic differences between children born before and after July 1 which could otherwise exert a bias are differenced out: First, the test scores used in the analysis stem from tests that took place within a certain month (e.g. April) and children born in January 1990 will be about 12 months older at the time of the test than children born in December 1990. If age in itself has a positive effect on outcomes, any potentially positive effects of the reform will be downward biased, since post-reform children are always younger than pre-reform children. Second, there might be systematic season of birth effects affecting

[107] Figure A 4-1 in the appendix reveals a seasonal pattern in the number of births (on average there are more births per day in July, August and September than in the rest of the year). However, these seasonal trends are constant over the years and should thus be accounted for in the DID approach.

the composition of children and their parents over the year. If certain types of couples are more likely to have babies in particular months of the year this might also impact upon the distribution of test scores across birth months.

One of the most crucial identifying assumptions for this approach is that assignment into treatment was indeed ‘as good as random’, i.e. that mothers could not self-select into treatment or control group. This basically requires that mother could not manipulate the date of childbirth around the cut-off date of July 1, 1990. Lalive and Zweimüller (1999) provide several arguments and evidence in support of the assumption of random assignment: first, an assessment of newspaper reports about a potential reform of parental leave duration revealed that the public discussion did not start before November 11, 1989 and that it was not clear until April 5 whether and when such a reform would be implemented. This timing of policy decision and implementation makes anticipatory adjustments to fertility plans highly unlikely, especially when taking into account that successful conception and date of childbirths cannot be perfectly controlled and planned by parents. Furthermore, as also argued by Würtz Rasmussen (2010), it is biologically infeasible to postpone the date of delivery (it is easier to give birth at an earlier date). The exceptions are births by Caesarean sections. However, an analysis of number of births during the days shortly before and after the reform did not indicate a higher density of births on July 1 or the days after (Lalive and Zweimüller 2009). Another assumption is the *common trend* assumption which requires that seasonal patterns or age effects are constant across years.

However, the common trend assumption might be problematic if there are changes over time. Certainly, this assumption becomes less restrictive if one limits the sample to children born extremely close to the cut-off date as these children are very similar in age as well as in season of birth (like in a Regression Discontinuity analysis).[108] Another advantage of narrowing the window of birth months before and after the reform would be that these children are more likely to face identical kindergarten and schooling regulations and rules, e.g., the Austrian school year typically runs from September to August (see also Section 4.4.3). Furthermore, their mothers were exposed to similar macroeconomic conditions and labour market developments (this argument in favour for a narrow sample around the cut-off date is given by Lalive

[108] This relates to the general idea behind a Regression Discontinuity Approach which relies on the similarity of composition of treatment and control group in terms of observed and unobserved characteristics who only differ with respect to the assignment into treatment.

et al. 2010). However, this strategy would require a very large data set. Unfortunately, the available test scores data base does not satisfy this condition.

Hence, given the data at hand, there is a *trade-off* between limiting the analysis on children who are as similar as possible (which would also reduce the likelihood of violating the common trend assumption) and having a sufficiently large sample size. Therefore, each specification will be estimated several times while successively narrowing down the window of birth months (starting from the birth months March to October and narrowing it down to May to August).

Since the data neither allow identifying children whose mothers were actually eligible for the more generous parental leave entitlements nor contain information on actual duration of leave taking of mothers, the estimated effect will represent the intention-to-treat effect of the reform (i.e. the reduced form effect of being eligible to 24 instead of 12 months of parental leave; see explanation and discussion by Baker and Milligan (2008, 2010) and Dustmann and Schönberg (2010)). More specifically, the estimated intention-to-treat effect is the average causal effect of the *assignment* of treatment on the outcome. The estimation procedure will simply compare outcomes of students that were randomly assigned to the treatment group or the control group (i.e., 24 or 12 months of parental leave), without accounting for the fact that neither all children in the treatment group actually received the treatment nor all children in the control group did not receive the treatment of prolonged parental leave. This intention-to-treat effect will be a lower bound estimate of the effect of prolonged parental leave and maternal care on child outcomes (compliers: mothers who adjust the length of the parental leave in response to the extended entitlements), since it is estimated on the full sample *including* children of mothers who did not change their behaviour because of the reform (non-complying mothers; see Angrist, Imbens and Rubin 1996). This latter group consists of always-takers (mothers always staying at home irrespective of the parental leave regime), i.e. non-working mothers as well as working mothers, who stop working post-birth for much longer than the granted parental leave period irrespective of the actual legislation, and of never-takers (mothers who return to work very early irrespective of the generosity of the system), i.e. non-eligible working mothers, e.g. self-employed, or working mothers who would return to work immediately after the compulsory maternity leave period independent of the actual leave provision. Furthermore, the intention-to-treat estimate could be a combination of different channels through which the reform might have affected child outcomes (see discussion of channels in Section 4.3.4). If information on *actual* maternal leave taking duration had been available in the data, the reform cut-off date could have been used as an instrumental variable in order to estimate the local average treatment effect of the reform.

4.4.2 Refinement and subgroup analysis

One way to get closer to the actual effect of the reform would be to restrict the sample to children whose mothers were actually eligible for parental leave and hence affected by the reform. Although direct information on the working status of the mother is not available in the data, it is possible to infer average labour force participation rates from educational attainments of mothers by splitting the sample into sub-groups of mothers with comparatively high or low labour force participation rates and hence a higher and a lower likelihood of parental leave eligibility (a similar approach is adopted in Baker and Milligan 2008).

Table 4-3 provides information on employment rates of mothers aged 18 to 39 with a baby younger than one year. The ratios were calculated using data from the Austrian Census 1991 (May 15, 1991). The employment measure includes women who are currently absent from work due to maternity or parental leave in the numerator (ratio of employed women to all women in the respective age group). Note that the Census 1991 was conducted before the first cohort of post-reform mothers started their second year of parental leave (by July 1991).

According to these figures women having completed post-secondary or tertiary education (having employment ratios of above 80 percent) will be categorized as 'High labour force participation group (High LFP)', and those with lower ratios will be subsumed as 'Low labour force participation group (Low FLP)'. The latter group comprises women whose highest educational degree is higher secondary school or less. On average, these groups have an employment ratio of about 84 percent (High LFP group) and 70 percent (Low LFP group).[109] When including women born abroad in the calculations the numbers become slightly lower, but the overall ranking remains the same (on average 81 (67) percent for the High (Low) LFP group; this might be related

[109] When including the 'unemployed' in the measure, the average ratios of 'economically active' become 89 and 78 percent, respectively.

to lower labour force participation rates or higher unemployment rates among foreign-born individuals). Unfortunately the census data does not allow a distinction between employed and self-employed or unpaid workers, and thus these numbers probably slightly exaggerate the share of mothers with employment contracts granting parental leave eligibility (possibly especially so for the less educated).

Table 4-3: Female employment ratio by highest education completed, Census 1991

		Born in Austria		Born anywhere	
ISCED	**Highest education completed**	**Employment ratio**	**N**	**Employment ratio**	**N**
1. Group of mothers with lower labour force participation					
2	Compulsory secondary school	60.8%	2,020	57.2%	2,528
3C	Intermediate technical & vocational secondary school (short form)	67.7%	136	67.2%	137
3B	Upper secondary	75.3%	3,828	74.6%	3,970
3A	Higher general secondary school	65.9%	437	60.8%	523
	Total	*70.0%*	*6,421*	*67.3%*	*7,158*
2. Group of mothers with higher labour force participation					
4	Post-secondary (not tertiary) (Intermediate or higher technical & vocational secondary school)	81.7%	699	79.7%	744
5B	Post-secondary college (tertiary)	90.7%	333	90.0%	341
5A/6	University, Polytechnic (tertiary)	81.6%	305	76.8%	358
	Total	*83.9%*	*1,337*	*81.4%*	*1,443*

Notes: Subsample of all mothers aged 18 to 39 years with a child younger than one year (Austria, Census date May 15, 1991). The 'employment ratio' is calculated as the ratio of persons working for an employer, self-employed persons, unpaid workers engaged in the production of economic goods, and persons who have a job but were temporarily absent for some reason (e.g. maternity or parental leave) divided by the total number of people in this age group. Employment measure does not include unemployed individuals, since the focus of the following analysis is on working mothers. Including unemployed women in the employment measure changes the ratios only slightly (they become larger). The educational classification is according to ISCED 1997. Source: Census Austria 1991 downloaded from the Integrated Public Use Microdata Series International (Minnesota Population Center 2011); own calculations.

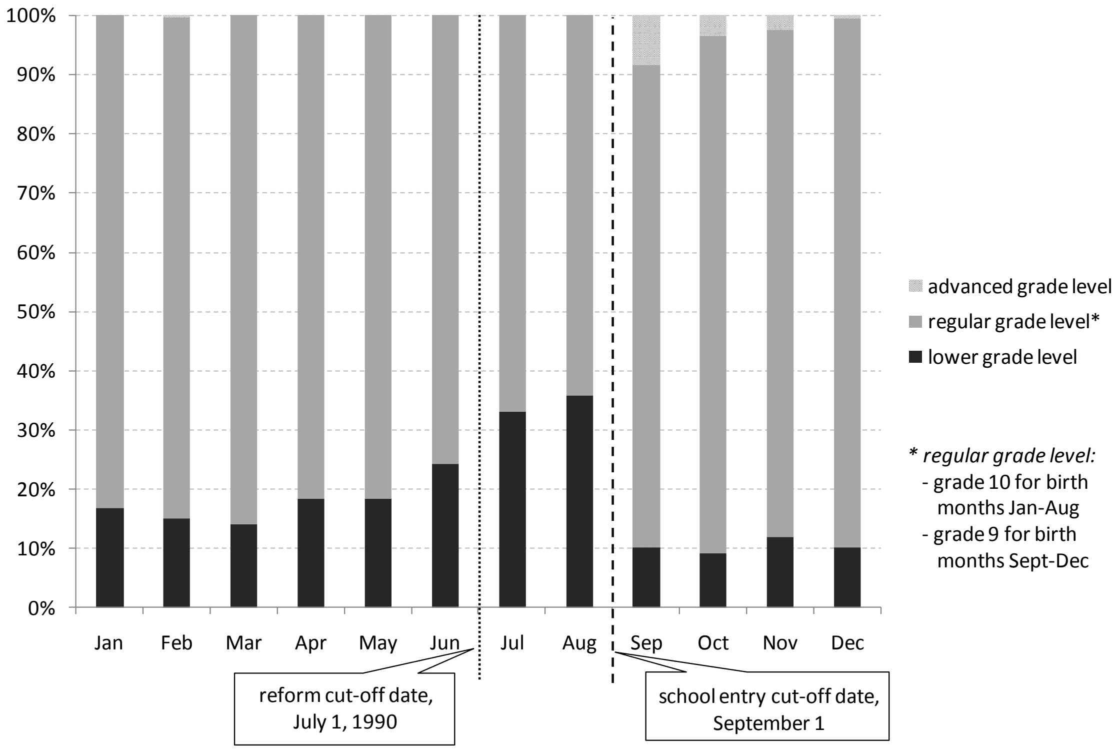
A division between these two groups of mothers according to educational attainment will not only differentiate between supposable eligibility statuses (women with higher education being more likely to be affected by the reform because of higher employment rates). In addition, if more educated mothers are more career-oriented and have stronger incentives to return to work immediately after the end of the granted period of job protection their ‘return-to-work’ decisions might follow the official rules more strictly (the period of parental leave entitlements might be more binding for women with a stronger attachment to the labour force). If this is the case, an estimation based on children from this subgroup of mothers will reflect the extension of the specific effect more clearly (mothers with lower attachment to the labour force might stay away from work for a longer period irrespective of the granted parental leave period). Moreover, a higher level of education might have a stronger and positive impact on child development in itself, if mothers with higher education are able to provide better quality time and care at home than less educated mothers (or informal child care).

4.4.3 Potentially confounding effects: School-entry cut-off age

One additional challenge for the empirical analysis is the proximity of the reform cut-off date July 1, 1990 to the Austrian school entry cut-off date which is the 1st of September of each year. More specifically, all children having their sixth birthday before September 1 of each year are obliged to start school in the same year (school starts typically at the beginning of September) unless they are officially considered as not yet ready for school given their cognitive skills (level of maturity according to professional opinion). However, until 1999, enforcement of this school entry cut-off date was not strict, conceding some discretionary power to *parents* regarding the decision whether their child was ‘ready’ for school or whether it should start one year later. Overall, the fraction of children enrolling late tends to be particularly high in the birth months immediately preceding the cut-off date. Furthermore, children born in July and August who enter school at the regular age will always be the youngest in their class. Simple age effects (biological, cognitive development) as well as more complex psychological peer effects might increase the likelihood that these children perform relatively worse than others and might thus be at higher risk of repeating a grade (recent empirical accounts of such relative age effects within classrooms are given by the following studies: Bedard and Dhuey (2006), Black, Devereux and Salvanes (2011), Mühlenweg (2010), Schneeweis and Zweimüller (2009) and Sprietsma (2010)).[110]

These two effects – late enrolment and grade repetition – lead to a very unequal distribution of the share of children enrolled in their *regular* grade level given their birth date across all birth months. Figure 4-7 plots the fractions of 15 year old children in Austria (for the reform cohort 1990) who were enrolled in a regular, advanced or lower grade level by month of birth. The categorization into regular, advanced and lower level is based on a comparison of their actual grade level with the grade level they should be in according to their birth date (based on the PISA 2006 data; for data description see Section 4.5.1). More specifically, in this particular case all children born before September should be enrolled in grade 10, while all children born between September and December should be enrolled in grade 9.

Figure 4-7: Grade progression by birth month



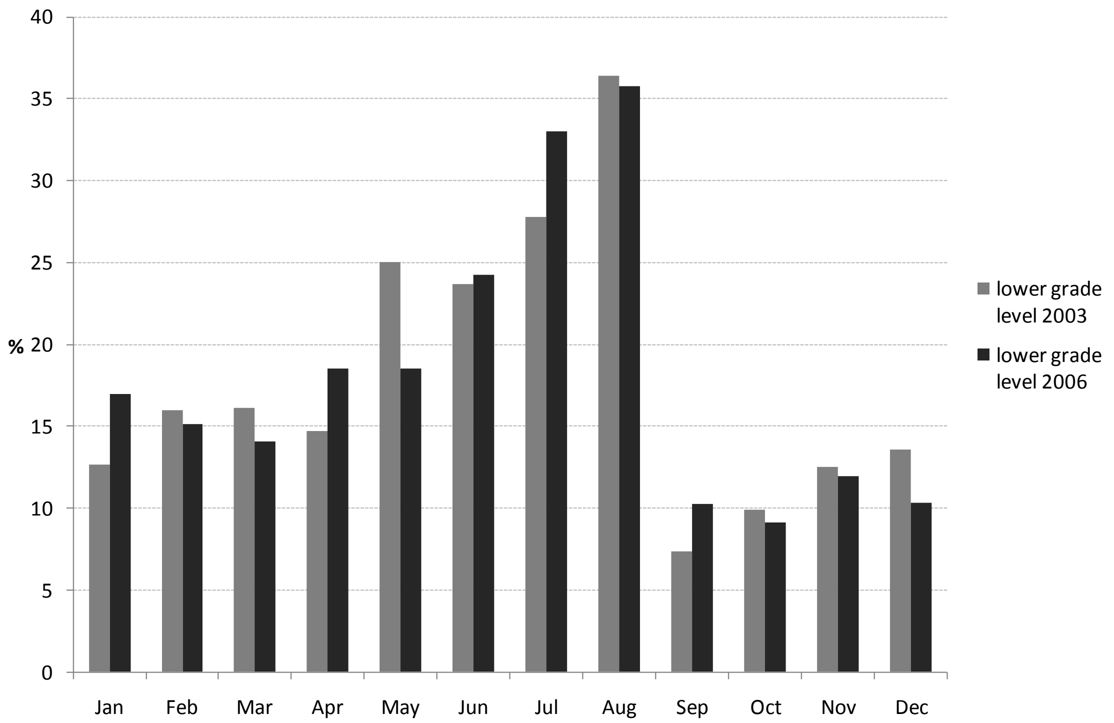
Source: PISA 2006; own calculations based on full sample included in the analysis and using student weights calculated by the data providers (N= 4,372; see sample description in Section 4.5.2).

[110] The cited literature also discusses other potential relative age effect channels. On the one hand, relatively younger children might benefit from the fact that they cover certain material at a younger age than their older classmates. On the other hand, there could be a negative effect if younger children imitate risky behaviour from their older peers.

The figure reveals that the fraction of children in lower than regular grade levels exceeds 30 percent in the months July and August (33 and 36 percent respectively), whereas in June and May the fraction drops to 24 and 19 percent. Under the plausible assumption that children in lower grade levels will perform comparatively worse in the PISA tests than children in higher grade levels – since they have completed fewer years of formal education and covered less material – the *average* test scores of children born in August and July will be lower than for children born earlier in the year. The same is especially true for children born between September and December 1990: among these children about 80 percent are in grade level 9 and hence their average test scores will be lower than those in the preceding birth months.

The comparison of the distribution of children in lower than regular grades across birth months for the cohorts 1990 and 1987 (PISA data 2006 and 2003) in Figure 4-8 reveals that this phenomenon is relatively consistent over time. This is an important prerequisite for the DID estimation strategy.

Figure 4-8: Distribution of students in lower than regular grade level (cohorts 1990 and 1987)



Source: PISA 2006 and 2003; own calculations based on full sample included in the analysis and using student weights calculated by the data providers.

The compositional changes in grade level attendance across birth months and especially to the right of the reform cut-off date seems to call for a limitation of the analysis to the birth months June and July only. Unfortunately though, as already mentioned earlier, this strategy is not possible due to the small sample size. To test to what extent the empirical results are sensitive to this school entry cut-off date, as a further robustness check the following analysis is also run only including children born before September (but still gradually extending the sample to the left of the cut-off).[111]

An alternative sensitivity test will restrict the sample to children who are in the regular grade only. However, since current grade level of a student might be endogenous to his cognitive skills, these results can only provide suggestive evidence and have to interpreted with caution.

4.5 Data on cognitive outcomes of the 1990 birth cohort in Austria

An analysis of the effects of the 1990 parental leave extension on medium-term cognitive child outcomes requires micro data on scholastic achievements from students born shortly before and after the reform in 1990. However, nationally representative data on standardized cognitive child outcomes for single birth cohorts which also contains information on socio-demographic characteristics of the child as well as its parents are generally not available in Austria. The only exception are data from international skill assessment studies, in particular, the Programme for International Student Assessment (PISA), Trends in International Mathematics and Science Study (TIMSS) or the Progress in International Reading Literacy Study (PIRLS).

4.5.1 The PISA data

Among these international studies there is fortunately one which captures exactly the relevant birth cohort: the PISA study 2006 was based on students born between January and December 1990. The following analysis will use data from the Austrian subsample of the PISA studies in 2006 and 2003, covering the birth cohorts

[111] In Austria, the compulsory schooling law requires children to complete at least nine years of formal education (school). Hence, some students who should have been observed in grade 10 (born before September), might have already dropped out of school. According to the Austrian survey conductors about 5 percent of the relevant cohort have already left school and are not included in the test (Schreiner, Breit, Schwantner and Grafendorfer 2007). This is another reason why the sample of children born before September (might have already dropped out of school) and after September 1 (still in compulsory schooling) differ and why one needs to account for these differences in the analysis.

1990 and 1987 respectively (see motivation for and advantages of the DID approach in Section 4.4.1).

There are several important features of the PISA data which make it especially suitable for the analysis: first of all, the PISA data provides results from *standardized tests* of cognitive skills in terms of reading, mathematical and scientific literacy. The focus of PISA is less a pure assessment of curriculum based knowledge, but more an evaluation of general skills needed for adult life and of the ability to apply knowledge to real-life problems. Second, the tests are administered to a nationally representative sample of 15-year old students independent of their current grade level in school. In contrast, other international studies like TIMSS and PIRLS assess students in particular grade levels, e.g. 4^{th} and 8^{th} grade, and are thus not representative for a particular birth cohort. Comparisons of outcomes across birth months would be biased if, for example, the propensity of grade retention or early or late school entry differs between children born closer or further away from the school entry cut-off date (see discussion in Section 4.4.3). Third, the PISA data files contain important student-reported background information on the student (e.g., gender, birth year and month, nationality, attitudes), the student's parents (educational achievement, nationality, occupational information) and the school (e.g., school programme, location, school size, resources).

However, it should be mentioned that the Austrian PISA data has also several disadvantages: although the overall sample size in 2006 includes about 4,927 students, the relevant sample when comparing children born in particular months of the year becomes quite small (there are only about 350 children per birth month). Furthermore, there is no retrospective information on maternal labour market participation at the time of birth which prevents a clear identification of mothers who were truly eligible for parental leave. Moreover, information on exact birth dates is not available in the data due to strict data security and protection rules. The lack of information on exact birth dates in conjunction with the rather small sample size prevents any refinement of the analysis beyond the month level (e.g. children born shortly before or after the cut-off date cannot be excluded to test for robustness to potential sorting across the cut-off date).

The Programme for International Student Assessment was initiated and is coordinated by the Organisation for Economic Co-operation and Development

(OECD)[112]. The international micro data files are publicly available free of charge from the OECD web-site which also offers detailed information on the data, variables and survey methodology (see http://www.pisa.oecd.org/). In Austria, the PISA 2003 and 2006 tests were managed and carried out by the Federal Institute for Education Research and Innovation & Development of the Austrian School System (Bifie – Bundestinstitut für Bildungsforschung, Innovation & Entwicklung des österreichischen Schulwesens).[113]

The students participating in the Austrian PISA test were sampled according to a two-stage stratified sampling design: the first stage involved a random selection of schools from the universe of all schools having 15 year old students (probability-proportional-to-size sampling, where size referred to number of 15-year-olds within school; the stratification also accounted for different school types and programmes); in the second stage a maximum of 35 15-year-olds were selected within each school to participate in the test (Breit and Schreiner 2007). The test itself was designed as a two hours paper-and-pencil assessment including multiple-choice and open answer questions plus a 30 minutes background questionnaire (OECD 2009a).

4.5.2 Sample selection and variables

Several observations were dropped from the original data in order to increase the cohesiveness of the data for the analysis. A detailed overview of how many observations were lost due to these sample restrictions is presented in Table A 4-2 in the appendix to this chapter. Overall, the sample of children born between May and August 1990 (1987) in the PISA data 2006 (2003) was reduced from 1,640 (1,574) to 1,480 (1,379) observations. The final sample used in the regressions thus represents about 90 percent (86 percent) of the original sample. First, students whose mothers are highly unlikely to have been eligible for parental leave or affected by the reform are excluded: this relates to children who were not born in Austria and whose mother was thus unlikely to work in Austria at the time of the reform in 1990. However, all those children who were born in Austria even though their mother and/or father were born abroad are kept in the sample. Furthermore, a few observations had missing information on maternal education (parental education is reported by the student). These

[112] Up to date, PISA tests were carried out in the years 2000, 2003, 2006 and 2009 and the number of countries participating in the test has increased from 43 countries (OECD and non-OECD) in 2000 to 65 countries in 2009.
[113] Further details on the implementation of PISA in Austria are available at http://www.bifie.at/pisa.

observations were dropped for two reasons: first, missing answers on maternal education could indicate absence of mothers due to death, separation, etc. which is problematic given the aim to evaluate the importance of *maternal* employment or care on child outcomes. Second, the level of maternal education is a crucial variable according to which the sample will be divided into two groups in the subsequent analysis and missing information on maternal educational attainment prohibits the allocation of the student to a particular group. A few students *in schools for children with special needs* were also excluded from the sample to increase consistency across students and across years, since these children have a completely different curriculum and these children were administered special test items (Schreiner, Breit, Schwantner and Grafendorfer 2007).

The variables that are used in the analysis are described in Table 4-4. The main outcome variables measuring cognitive skills are test scores in mathematics, reading and science.[114] The test scores in each subject are rescaled by the OECD so that the mean across all participating countries is 500 points and the standard deviation is 100 points.[115]

Two other potential outcome variables are indicator variables on whether a student is in a lower than the regular grade level given their birth month and whether the student is enrolled in a school track which gives access to university or college education (academic track). In Austria, students are allocated to different educational tracks after the fourth grade (i.e. at age 10 or 11), which is relatively early compared to other European countries (Schneeweis and Zweimüller 2009). Starting with grade level nine, a further differentiation into specific tracks takes place; the PISA sample thus covers several different school types representing high and low track schools, which are also associated with different levels of PISA test outcomes. A list of these national

[114] Strictly speaking, the PISA data does not contain standard test scores, but so-called ‘plausible values’ (five plausible values per subject) which are estimates of the overall proficiency of a student given his/her particular objective test outcomes, drawn from an underlying latent ability distribution of the student. As recommended in the PISA Data Analysis Manual (OECD 2009b), the following analysis is based on one of these plausible values (plausible value 1) for each subject (as has been done in another study by Schneeweis and Winter-Ebmer 2007). Also in line with the OECD recommendations all presented results are weighted using the provided student level weights. Standard errors are clustered by school programme, school location and gender to account for within group correlation of outcomes.

[115] More specifically, to ease comparison of results across survey years, the test results were rescaled such that the reading and mathematics reporting scales of 2006 are equal to those in 2003. The test results in science are rescaled such that the mean is 500 and the standard deviation is 100 for the 30 OECD countries that participated in PISA 2006 (see OECD 2009a, PISA 2006 Technical Report, pp. 157-158). However, any general differences in test scores across years and subjects will be accounted for in the analysis by including controls for years and by running separate regressions for each subject.

school programmes and how these are categorized into ‘academic track’ schools can be found at the bottom of Table 4-4. Again, since these different school programmes are associated with different achievement levels and since they have different orientations (vocational training versus academic education) enrolment into a particular school type is an endogenous outcome.

Table 4-4: Description of variables

Variable Name	Variable Description
School outcomes	
Mathematics score	Mathematics test score (plausible value 1)
Reading score	Reading test score (plausible value 1)
Science score	Science test score (plausible value 1)
Retained	= 1, if observed grade level is below grade level student should be according to this birth month (regular grade level is 10 for children born before September, and 9 for children born in September or later) (= 0, otherwise)
Academic track	= 1, if enrolled in school which enables student to attend university after graduation (= 0, otherwise)
Male	= 1, if gender is male; = 0, if gender is female
Age in years	Age in years at the time of the test
School location (dummy variables)	
City	= 1, if school is located in ‘city’, i.e. ‘100,000 to 1,000,000 inhabitants’ (= 0, otherwise)
Metropolitan area	= 1, if school is located in ‘metropolitan area’, ‘more than 1,000,000 inhabitants’ (= 0, otherwise)
Mother’s and father’s educational attainment (dummy variables):	
Lower secondary	Lower secondary or less [→ ‘low LFP’ group]
Upper secondary	Upper secondary [→ ‘low LFP’ group]
Tertiary	Tertiary or Post-Secondary [→ ‘high LFP’ group]
Educ. father: miss.	Educational attainment of father is unknown or missing
Migration background of family (dummy variables)	
Family type 2	= 1, if family speaks German at home, but at least one of the parents was born abroad (= 0, otherwise)
Family type 3	= 1, if both parents were born in Austria and home language is German (= 0, otherwise)
National school programme (Austria) (dummy variables):	
Vocational (low track)	Vocational (low track) (Hauptschule, Polytechnische Schule)
Apprenticeship	Apprenticeship training (Berufsschule)
BMS	Medium vocational school (Berufsbildende Mittlere Schule)
BHS	Higher vocational school (Berufsbildende Höhere Schule) [→ ‘academic track’]
AHS	Gymnasium, Higher general school (Allgemeinbildende Höhere Schule) [→ ‘academic track’]

Further variables of interest are the student’s gender and age (age will be implicitly accounted for by controlling for birth month), dummy variables indicating whether the school is located in an urban area (i.e. metropolitan area or large city; the base category are smaller towns with less than 100,000 inhabitants and villages), information on mother’s and father’s highest completed level of education, and control variables for migration status of the family (whether the family speaks German at home and whether mother and father were born in Austria; the base category are non-German speaking families). All these variables have been selected to be included in the analysis since they are comparatively unlikely to be endogenous to child characteristics (i.e., these variables were chosen as to present pre-birth characteristics of the family). This endogeneity problem is the reason why no information on current employment or current occupation of the mother is incorporated in the analysis (*current* refers to date of the respective PISA test).

4.5.3 Descriptive statistics and first raw comparisons

The mean values of the outcome variables and control variables of the PISA 2006 and 2003 data are presented in Table 4-5, separately for children born in May and June (before the reform in 1990) and for children born in July and August (after the reform in 1990).

As regards the other demographic characteristics, school location, mother’s and father’s education as well as migration background there are no significant systematic differences between pre and post reform children in 2006, which supports the important assumption that the reform was unexpected and that there was not systematic self-selection of particular types of parents or families around the cut-off date. In 2003 there seems to be a higher density of mothers with tertiary education among children born in July or August than born in May or June.

Table 4-5: Mean comparisons of outcomes and characteristics of students born in May/June versus July/August in the reform year 1990 and the control year 1987

	PISA 2006 (birth cohort 1990)				PISA 2003 (birth cohort 1987)			
	Pre reform	Post reform			Pre reform	Pre reform		
Birth months	**May-June**	**July-Aug.**			**May-June**	**July-Aug.**		
	Mean (1)	Mean (2)	Diff. (2)-(1)	Std. error	Mean (1)	Mean (2)	Diff. (2)-(1)	Std. error
Mathematics score *(Std. deviation: 92.8)*	521.20	519.80	-1.45	5.23	515.90	515.10	-0.74	5.92
Reading score *(Std. deviation: 101.1)*	507.20	500.70	-6.46	5.74	501.10	501.70	0.62	5.73
Science score *(Std. deviation: 90.6)*	526.90	523.70	-3.18	5.09	504.40	502.50	-1.87	5.65
Retained	0.21	0.34	0.13**	0.03	0.25	0.33	0.08**	0.03
Academic track	0.54	0.52	-0.02	0.03	0.46	0.51	0.04	0.03
Male	0.52	0.50	-0.02	0.03	0.50	0.49	-0.01	0.03
Age in years	15.92	15.75	-0.17**	0.00	15.93	15.76	-0.17**	0.00
City	0.41	0.41	-0.01	0.03	0.34	0.35	0.01	0.03
Metropolitan area	0.14	0.13	-0.01	0.02	0.16	0.14	-0.02	0.02
Mother's education:								
Lower secondary	0.09	0.09	0.00	0.02	0.13	0.09	-0.04**	0.02
Upper secondary	0.54	0.56	0.02	0.03	0.59	0.56	-0.03	0.03
Tertiary	0.37	0.35	-0.02	0.03	0.28	0.34	0.07**	0.03
Father's education								
Lower secondary	0.06	0.06	0.00	0.01	0.09	0.07	-0.02	0.02
Upper secondary	0.47	0.46	-0.01	0.03	0.51	0.48	-0.03	0.03
Tertiary	0.45	0.46	0.02	0.03	0.37	0.41	0.05	0.03
Educ. father: miss.	0.02	0.02	0.00	0.01	0.03	0.04	0.01	0.01
Family type 2	0.09	0.07	-0.02	0.02	0.06	0.06	0.00	0.01
Family type 3	0.85	0.86	0.01	0.02	0.90	0.91	0.01	0.02
School programme:								
Vocational (low track)	0.05	0.07	0.02	0.02	0.05	0.07	0.02	0.02
Apprenticeship	0.26	0.23	-0.04	0.02	0.26	0.24	-0.02	0.03
BMS	0.13	0.16	0.03*	0.02	0.20	0.17	-0.03	0.02
BHS	0.33	0.30	-0.03	0.03	0.31	0.32	0.01	0.03
AHS	0.23	0.24	0.01	0.02	0.18	0.21	0.03	0.02
Number of observations	*716*	*764*			*680*	*680*		

Notes: Estimations weighted by individual inverse probability weights provided in the PISA data set. The standard deviations refer to the 2006 data using the pooled sample from May to August. Significance levels are denoted by: ** p<0.05, * p<0.1.

4.6 Empirical results of the effect of the reform on cognitive skills

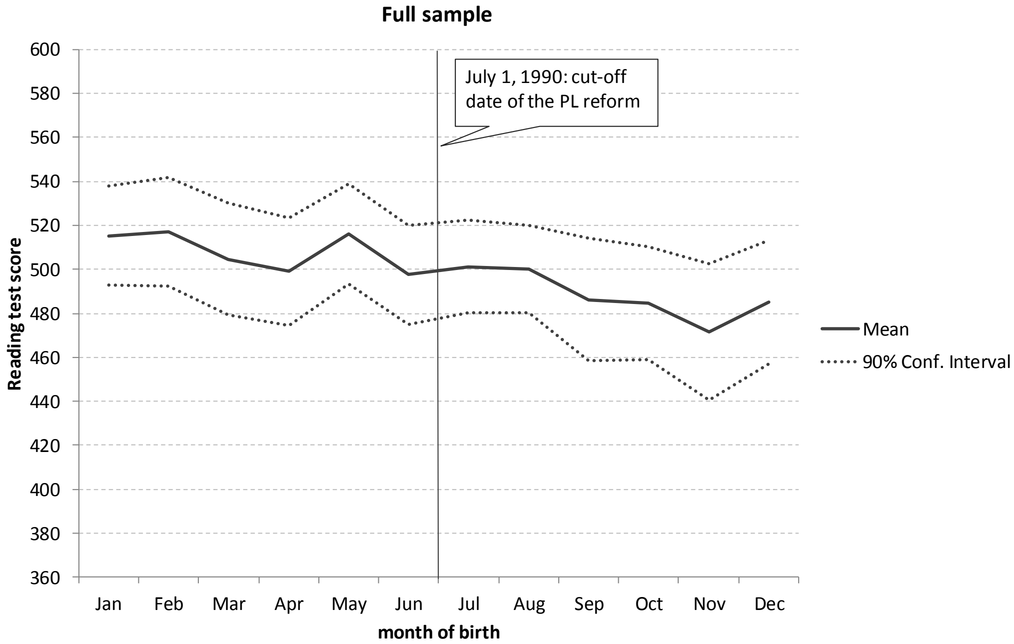
After showing some simple graphs on the distribution of the average test scores across birth months, this section presents and discusses the results from the Difference-in-Differences estimations.

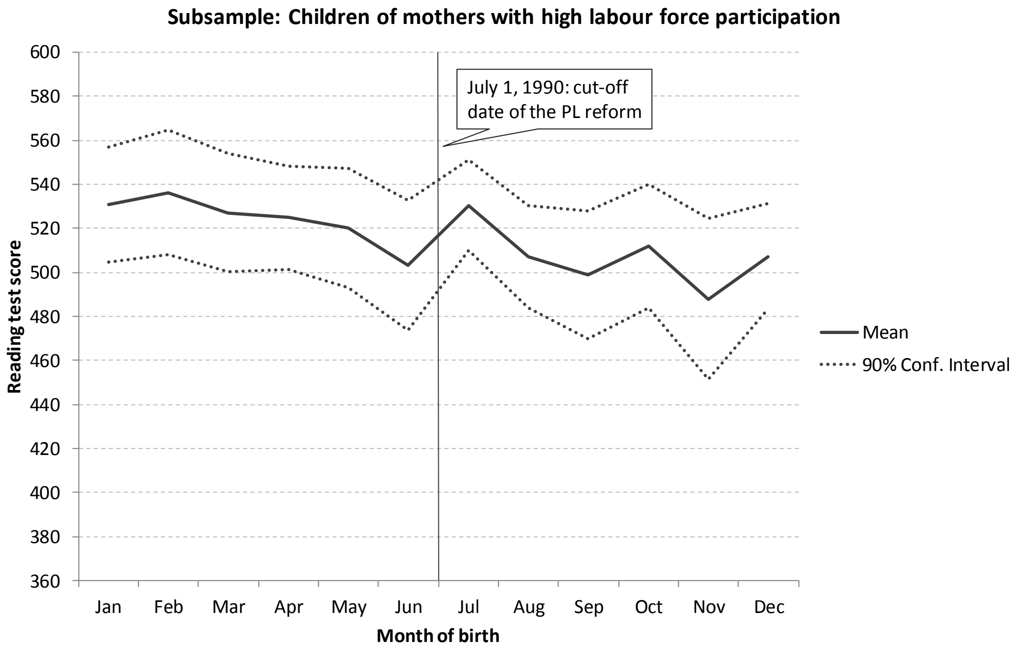
4.6.1 Graphical Results

Figure 4-9 helps to get a first impression and better understanding of the distribution of test scores across birth months. The graphical analysis is a simple tool to check whether there is a jump in test scores for children born after the parental leave extension (using reading test scores for this example). It seems that there is a general negative trend in average reading test scores across birth months that would be in line with possible age effects as discussed earlier. However, comparing the test outcomes immediately before and after the reform (i.e., June and July children) the absence of any marked differences suggests no positive reform effect. Nevertheless, this flat passage could imply a positive effect if it is the result of a positive reform effect neutralizing a negative age effect. Furthermore, the graph illustrates that children born between September and December 1990 perform on average worse in the tests. This is related to the fact that these children are in lower grade levels due to the school-entry cut-off regulations (children born in September or later enter school one year later).

In contrast, the picture looks different when the sample is limited to those children whose mothers were more likely to be affected by the reform due to a higher labour force participation rate (Figure 4-9, lower graph). Although the overall negative age effect remains, test scores of July children are much higher than for June or even May children and it seems as if the distribution was slightly upward shifted for post-reform children. Average test results of August children are lower than those for July children which would be in line with a negative age and school-entry effect (the share of August children who are in grade level nine instead of ten is higher than in July).

Figure 4-9: Average reading test scores across birth months (full and subsample)





Notes: The graphs plot the estimated birth month coefficients of a regression of test scores on a set of birth month dummy variables without additional controls (weighted using the individual students weights provided in the PISA data; the base month excluded from the regression is 'June') based on the full sample (upper graph) and on the subsample of children with mothers with higher labour force participation likelihood (lower graph). The 90 percent-confidence interval is based on estimated standard errors that were clustered by school programme, school location, and gender.

To test whether the test score gap between June and July children is significantly different from zero, a simple estimation was carried out based on the PISA 2006 before proceeding to the DID estimations. Test scores from the three different subjects were regressed on a set of birth months with June as the excluded base category, and other control variables on parental background. The estimated 'born in July' coefficients are reported in Table 4-6 for separate regressions by subject, maternal labour force participation and child gender. The first two columns in Table 4-6 show the results from the full sample of mothers. As the previous graphs suggested the average test scores do not differ significantly between pre- and post-reform children in the full sample and this applies for all three tested subjects. Furthermore, the estimated coefficients change only marginally after controls for parental background are included in the regression (although standard errors become slightly smaller). Comparing the results by gender (columns 1 and 2, middle and bottom panel) reveals that there might be gender differences as the estimated coefficients for boys tend to be positive (but insignificant), while the estimated coefficients for girls tend to be negative. Restricting the sample to the subsample of mothers who were more likely affected by the reform (columns 3 and 4), confirms the graphical impression of a positive effect: the estimated effects become much larger and are significantly different from zero (at the five percent level) for reading and science. Again, the effects for boys seem to be larger than for girls. The separate analyses show that the differences remain significantly different from zero only for the male subsample. For completeness, the two columns on the right show the respective results for children from mothers with lower labour force participation rates. Most of the coefficients have a negative sign implying a decline in test scores after the reform; however, none of these effects is significantly different from zero.

Given the previous discussion of the potentially confounding age and school-entry effects, it is important to refine the analysis and to perform sensitivity checks in order to be able to draw more well-grounded and meaningful conclusions. This will be done in the next sections.

Table 4-6: Simple OLS regressions on the differences between June and July children using only data from the PISA test 2006 (including birth months May-August)

	(1) Full sample	(2) Full sample	(4) High LFP mother	(5) High LFP mother	(7) Low LFP mother	(8) Low LFP mother
			MALES + FEMALES			
Mathematics	3.043	3.289	15.687	15.276	-3.377	-2.920
	(7.002)	(6.208)	(11.040)	(11.074)	(8.358)	(7.519)
Reading	3.308	3.987	28.946**	28.559**	-10.111	-8.923
	(6.936)	(6.022)	(13.485)	(13.730)	(8.677)	(7.396)
Science	3.850	3.832	23.700**	22.113**	-6.424	-5.802
	(6.315)	(5.428)	(10.398)	(10.223)	(8.082)	(6.894)
Observations	1,480	1,480	523	523	957	957
			MALES			
Mathematics	11.734	8.615	27.893	21.853	2.882	1.934
	(8.957)	(8.014)	(17.287)	(14.846)	(8.820)	(9.099)
Reading	12.491	10.470	50.349**	44.261*	-7.795	-5.048
	(9.167)	(7.871)	(22.448)	(21.999)	(10.809)	(9.806)
Science	7.720	4.677	40.772**	34.568**	-9.886	-10.464
	(8.559)	(7.312)	(15.513)	(14.392)	(8.722)	(7.863)
Observations	752	752	265	265	487	487
			FEMALES			
Mathematics	-5.771	-2.289	3.492	5.848	-9.627	-4.493
	(10.041)	(9.823)	(14.652)	(15.520)	(14.027)	(13.150)
Reading	-6.051	-2.955	8.174	11.779	-12.219	-8.156
	(9.755)	(8.702)	(15.309)	(15.431)	(13.713)	(11.104)
Science	-0.118	2.751	7.136	7.288	-2.750	2.238
	(9.269)	(8.554)	(14.316)	(14.530)	(14.140)	(12.586)
Observations	728	728	258	258	470	470
Controls for parental background	-	✓	-	✓	-	✓

Notes: The presented numbers are the estimated effects of ‘being born in July’ as opposed to ‘being born in May’ using the 2006 PISA sample. The results in the top panel stem from the pooled sample of males and females, the middle panel from the male and the bottom panel from the female sample. All regressions include children born between May 1 and August 31 and control for birth months (dummy variables) and gender (top panel). The control variables on parental background include dummy variables for mother’s and father’s educational attainment, school location, and migration background. Robust standard errors in parentheses (clustered by school programme, school location, and gender). Estimations weighted by individual inverse probability weights provided in the PISA data set. *** p<0.01, ** p<0.05, * p<0.1. Source: PISA data set (OECD), own calculations.

4.6.2 Difference-in-Differences estimates of the effect of the parental leave reform on test scores

The results of the main DID estimates of the effect of the parental leave extension on PISA test scores are presented in Table 4-7 for the full sample (all mothers) and separately for mothers with higher and lower labour force participation rates. The estimation window includes children born two months before and after the reform (born between May and August). For each of these samples the first column shows the results without any control variables (columns 1, 3 and 5), while the second column contains results after controlling for background variables to account for potential changes in sample composition across years (and possibly across months). Even this more refined estimation approach seems to confirm the findings from the simple graphs shown earlier: the treatment effects of the reform (being born post June in the year 1990) are close to zero in the full sample (columns 1 and 2). Adding controls changes the coefficient slightly, but given the size of the standard errors these differences are not significant.[116] The average effect on reading test scores is a little bit larger and negative, but still not significantly different from zero.

In contrast to these neutral findings are the results from the regressions based on the subsample of high LFP mothers. The average effect on all three test subjects is significantly positive and of remarkable size; according to these results the extended parental leave period raised test scores by about 22 percent of a standard deviation (a little bit less in mathematics).[117] It is likely that these differences in results between the full sample and the sample of high LFP mothers can be related to the different levels of eligibility of the reform in conjunction with possibly higher quality care of better educated mothers (relative to informal care arrangements). Furthermore, the results for the subgroup of high LFP mothers are more likely to show the pure *time and care* effect of the parental leave extension, since the reform did not affect overall fertility or labour market success of mothers with higher earnings (which is proxied here by higher levels of education and higher labour force participation rates).

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[116] Standard errors were clustered by school programme, school location and gender to account for the fact that test scores of students in the same programme, location and gender are likely to be correlated and not independent of each other.

[117] As a further refinement, a set of dummy variables for the different school programs (which are highly correlated with different average test scores) was added to the regressions. As expected, this inclusion helped to increase the precision of the estimates as the standard errors became smaller. The treatment effects became larger and more significant. However, due to the endogeneity of these variables these results are only reported in the appendix, Table A 4-3, column set (A).

In comparison, the effects for the low LFP group are generally negative and even statistically significant for the reading test scores. Although the fraction of affected mothers in this group is smaller, it seems that these children have not benefited from the reform. This could be potentially attributed to the increased levels of fertility and less available resources (time and market goods) per child (i.e. a possible ‘quantity-quality’ trade-off), to shorten time intervals between births or maybe to a lower quality of maternal time in the sense of ability to foster cognitive child development.

To shed more light on potential gender differences regarding the effects of maternal employment on cognitive development of children found in other studies, the same analysis is repeated for boys and girls separately (Table 4-8 presents only the coefficients of the treatment effects of the reform).

Even though sample sizes become rather small in this subgroup analysis, the estimates seem to indicate that boys react more strongly to the reform than girls, especially when looking at the group of mothers with higher LFP rates. While both post-reform girls and boys have higher test scores on average, the coefficient for the boys is almost three times as large as that for girls. Furthermore, the effect remains only significantly different from zero for the male subgroup as the coefficients are too imprecisely estimated in the female subgroup (the effect on reading and science test scores for males corresponds to about 0.3 and 0.4 standard deviations). These results would be in line with potential differences in needs and in development between girls and boys at very young ages causing boys to benefit comparatively more from maternal care between the age of one and two. As before, the results for the pooled sample are rather small and insignificant and the results for the group of mothers with lower LFP rates have a negative sign.

Table 4-7: Difference-in-Differences estimation results (boys and girls)

	(1) Full sample	(2) Full sample	(3) High LFP mother	(4) High LFP mother	(5) Low LFP mother	(6) Low LFP mother
Mathematics						
Treatment effect	-0.406	2.000	17.158*	16.067*	-7.106	-5.021
	(6.855)	(6.698)	(9.685)	(9.177)	(9.554)	(8.829)
Post June	0.182	-2.541	-21.675*	-19.536*	8.308	5.954
	(7.008)	(6.768)	(10.889)	(9.904)	(8.577)	(7.985)
Born May	-2.335	-4.190	-11.698	-14.114	1.044	0.195
	(5.856)	(5.599)	(8.948)	(8.600)	(5.996)	(5.712)
Born July	-3.804	-2.946	8.361	5.116	-9.964	-8.397
	(5.193)	(4.754)	(8.805)	(8.625)	(6.691)	(5.151)
Year 2006	4.993	3.943	-6.723	-6.249	8.143	8.345
	(6.436)	(5.844)	(10.352)	(8.933)	(6.810)	(6.386)
Constant	503.812***	418.476***	528.176***	457.109***	494.827***	407.570***
	(12.363)	(18.962)	(14.228)	(23.328)	(12.398)	(20.766)
Reading						
Treatment effect	-7.362	-4.138	22.710*	21.220*	-19.811*	-17.144*
	(8.589)	(8.093)	(12.390)	(11.672)	(10.442)	(9.450)
Post June	4.555	1.302	-24.609*	-22.431**	15.431*	13.508*
	(7.596)	(7.279)	(12.399)	(11.074)	(7.930)	(7.473)
Born May	6.910	4.674	0.601	-2.670	8.447	8.182
	(4.948)	(4.701)	(10.120)	(8.886)	(5.723)	(5.763)
Born July	-1.769	-0.869	16.953	13.304	-11.251	-9.860
	(5.743)	(5.295)	(10.178)	(10.022)	(7.927)	(6.011)
Year 2006	6.476	4.842	-13.517	-12.230	12.997*	12.764*
	(6.958)	(6.423)	(11.796)	(11.183)	(7.518)	(7.140)
Constant	515.920***	408.674***	545.670***	447.464***	505.305***	399.338***
	(13.032)	(22.197)	(15.666)	(27.492)	(13.621)	(24.844)
Science						
Treatment effect	-1.009	2.103	23.713**	22.951**	-11.102	-8.454
	(7.623)	(7.411)	(10.329)	(10.020)	(9.939)	(9.041)
Post June	0.686	-2.749	-22.041**	-20.678**	8.964	6.426
	(7.004)	(6.814)	(10.603)	(9.660)	(8.579)	(8.011)
Born May	3.855	1.607	0.473	-2.685	4.232	3.384
	(5.944)	(5.677)	(7.782)	(6.878)	(6.900)	(6.757)
Born July	-1.083	-0.068	13.061	9.648	-8.223	-6.618
	(5.294)	(4.953)	(8.716)	(8.708)	(7.492)	(5.770)
Year 2006	22.148***	20.497***	5.236	5.398	27.570***	27.240***
	(6.812)	(6.154)	(10.186)	(8.797)	(7.330)	(6.902)
Constant	495.036***	388.403***	520.461***	419.676***	485.895***	380.347***
	(12.660)	(18.083)	(14.611)	(24.012)	(12.929)	(18.672)
Observations	2,840	2,840	943	943	1,897	1,897
Controls						
Parental background	-	✓	-	✓	-	✓

Notes: All regressions control for gender. The control variables on parental background include dummy variables for mother's and father's educational attainment, school location, and migration background. Robust standard errors in parentheses (clustered by school programme, school location, and gender). Estimations weighted by individual inverse probability weights provided in the PISA data set. *** p<0.01, ** p<0.05, * p<0.1. Source: PISA data set (OECD), own calculations.

Table 4-8: Difference-in-Differences estimation results by gender.

	(1) Full sample	(2) Full sample	(3) High LFP mother	(4) High LFP mother	(5) Low LFP mother	(6) Low LFP mother
			MALES			
Mathematics	0.092	0.264	17.935	15.832	-8.420	-9.027
	(7.906)	(8.087)	(12.438)	(12.282)	(13.177)	(11.769)
Reading	-7.309	-6.808	34.009**	33.118**	-27.664*	-26.634**
	(9.794)	(9.750)	(15.154)	(14.985)	(14.516)	(12.868)
Science	-2.063	-1.232	41.033***	40.396***	-23.698	-23.251*
	(9.177)	(9.208)	(10.875)	(11.447)	(15.409)	(13.379)
Observations	1,426	1,426	482	482	944	944
			FEMALES			
Mathematics	-0.493	4.104	18.164	16.001	-5.617	-2.023
	(11.066)	(10.743)	(15.450)	(15.184)	(14.318)	(13.281)
Reading	-7.416	-1.890	13.368	13.905	-12.800	-8.914
	(13.684)	(12.585)	(19.744)	(19.082)	(15.466)	(13.978)
Science	0.561	5.762	7.778	6.330	1.670	5.818
	(12.139)	(11.878)	(16.723)	(15.817)	(14.109)	(13.126)
Observations	1,414	1,414	461	461	953	953
Controls						
Parental background	-	✓	-	✓	-	✓

Notes: The upper panel includes only male, the lower panel only female students. All regressions include dummy variable controls for survey year, birth months and for all children born post June. The control variables on parental background include dummy variables for mother's and father's educational attainment, school location, and migration background. Robust standard errors in parentheses (clustered by school programme, school location, and gender). Estimations weighted by individual inverse probability weights provided in the PISA data set. *** p<0.01, ** p<0.05, * p<0.1. Source: PISA data set (OECD), own calculations.

Given the trade-off between a larger sample size which could help to increase precision of the estimates on the one hand and the need to keep the estimation window as narrow as possible to reduce the influence of possibly confounding factors (the school-entry effect due to the proximity to the school entry cut-off date, age-at-test effect, seasonality), the regressions in Table 4-7 are repeated for wider samples which are gradually reduced to the more narrow window of four months (two pre- and two post-reform months) presented above (the widest sample includes children born between March and October).[118]

[118] Results using the narrowest window possible – i.e. children born in June or July– are reported in Table A 4-3, column set (B) in the appendix. These estimates are even larger and have a higher level of

The results for the symmetric extension of the sample (four/three/two months before and after the reform) are shown in Table 4-9 and are reported separately by maternal labour force participation subgroups (columns (3) and (6) correspond to the results from the two previous tables, Table 4-7 and Table 4-8). Looking at the first three columns for mothers with higher education it becomes evident that increasing the sample size symmetrically to the left and to the right of the cut-off leads to smaller coefficients; in other words, a narrower window around the cut-off date leads to a comparison of more similar students resulting in larger effects. As a result and although the coefficients remain positive, the estimates in the top panel for the pooled male and female sample based on the wider window become insignificant. Nevertheless, the estimated effects for boys remain highly significant and positive, while those for girls get much smaller and even switch signs. Similarly, the results for the low LFP subgroup become less negative (or more positive) the more months are included in the analysis. Nevertheless, most of the results remain negative albeit often insignificant; the strongest effects are still found for boys.[119]

Table 4-10 summarizes further checks on whether these results are influenced by children born after the school-entry cut-off age (after September 1) and who are thus in lower grade levels than the other students. This time, additional months were added only to the left of the reform date, i.e. before July, while holding the number of months to the right constant (July and August). However, although the estimated coefficients are slightly larger (for the high LFP subgroup), the general picture remains the same.

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significance than the estimates based on the four months sample, which could be driven by the omission of August children who are more likely to be in lower grade levels (and thus have lower test scores on average).

[119] Since the share of children aged 0-2 in childcare during the 1990s in Vienna was much higher than in the rest of Austria, the regressions from Table 4-9 were re-estimated excluding students from Vienna. These estimations could serve as a rough check on whether the mechanism of the reform was mainly through replacing informal care by maternal care (unfortunately, the data does not contain information on the place of birth, but only on location of the school). The results in the appendix, Table A 4-5, seem to support this hypothesis: the estimated effects for children from the group of mothers with higher LFP rates are slightly larger (especially for the regressions including birth months May to August). On the other hand, the negative coefficients for the male subsample from the group with lower LFP rates become much smaller. This would be in line with the argument that in Vienna, the reform generated more of a trade-off between formal versus maternal child care and that this replacement might have had more severe effects for children (especially sons) of mothers with lower levels of education. Excluding this region from the sample should consequently reduce the pronounced negative effect of the reform.

Table 4-9: DID estimates based on symmetrically extended estimation samples (up to four pre- and post-reform birth months)

	High LFP mothers			Low LFP mothers		
	(1) Mar-Oct	(2) Apr-Sep	(3) May-Aug	(4) Mar-Oct	(5) Apr-Sep	(6) May-Aug
	MALES + FEMALES			MALES + FEMALES		
Mathematics	5.678	9.944	16.360*	2.248	0.526	-5.634
	(7.713)	(7.315)	(9.274)	(6.483)	(7.313)	(9.140)
Reading	9.222	13.141	20.604*	-8.113	-9.716	-16.309*
	(9.455)	(9.811)	(11.724)	(6.831)	(7.656)	(9.245)
Science	11.661	15.918*	23.068**	-3.119	-5.116	-8.805
	(8.650)	(8.938)	(10.076)	(7.087)	(7.706)	(9.140)
Observations	1,887	1,425	943	3,772	2,840	1,897
	MALES			MALES		
Mathematics	13.328	13.487	15.832	-2.974	-3.577	-9.027
	(11.192)	(9.667)	(12.282)	(8.868)	(10.290)	(11.769)
Reading	27.162**	28.860**	33.118**	-16.769	-20.926*	-26.634**
	(11.567)	(12.027)	(14.985)	(10.094)	(12.214)	(12.868)
Science	32.614***	34.360***	40.396***	-16.917*	-18.967	-23.251*
	(10.420)	(9.864)	(11.447)	(9.709)	(11.123)	(13.379)
Observations	953	716	482	1,866	1,397	944
	FEMALES			FEMALES		
Mathematics	-0.186	5.977	16.001	7.340	4.872	-2.023
	(10.628)	(11.017)	(15.184)	(8.960)	(10.040)	(13.281)
Reading	-9.740	-1.316	13.905	3.965	2.321	-8.914
	(14.472)	(14.614)	(19.082)	(9.056)	(9.913)	(13.978)
Science	-8.190	-2.603	6.330	11.648	9.335	5.818
	(11.611)	(12.598)	(15.817)	(10.141)	(11.132)	(13.126)
Observations	934	709	461	1,906	1,443	953

Notes: The reported estimated treatment effects stem from separate estimations of different specifications. All regressions include dummy variables for month of birth, a year dummy for 2006, a dummy variable for all children born after June. Estimations from columns 1, 2, 4, and 5 include a further dummy variable for all children born between September and December to account for the school entry cut-off date in Austria and an interaction effect of this dummy variable with the year 2006 variable to account for potential general trends in school entry or repetition norms. The control variables on parental background include dummy variables for father's educational attainment, school location, and migration background of the family. Robust standard errors in parentheses (clustered by school programme, school location, and gender). Estimations weighted by individual inverse probability weights provided in the PISA data set. *** p<0.01, ** p<0.05, * p<0.1. Source: PISA data set (OECD), own calculations.

Table 4-10: DID estimates based on sample adding more pre-reform birth months while holding the number of post-reform birth months constant

	High LFP mothers			Low LFP mothers		
	(1) Mar-Aug	(2) Apr-Aug	(3) May-Aug	(4) Mar-Aug	(5) Apr-Aug	(6) May-Aug
	MALES + FEMALES			**MALES + FEMALES**		
Mathematics	6.037	10.427	16.360*	2.381	0.767	-5.634
	(7.720)	(7.383)	(9.274)	(6.534)	(7.356)	(9.140)
Reading	9.609	13.807	20.604*	-8.117	-9.529	-16.309*
	(9.380)	(9.694)	(11.724)	(6.964)	(7.756)	(9.245)
Science	12.066	16.600*	23.068**	-3.046	-4.961	-8.805
	(8.565)	(8.852)	(10.076)	(7.184)	(7.759)	(9.140)
Observations	1,407	1,178	943	2,830	2,367	1,897
	MALES			**MALES**		
Mathematics	13.612	13.594	15.832	-2.555	-3.134	-9.027
	(11.056)	(9.807)	(12.282)	(8.798)	(10.209)	(11.769)
Reading	27.569**	28.932**	33.118**	-16.415	-20.235	-26.634**
	(11.609)	(12.154)	(14.985)	(10.110)	(12.242)	(12.868)
Science	33.445***	35.290***	40.396***	-16.873*	-18.723	-23.251*
	(10.514)	(10.098)	(11.447)	(9.680)	(11.060)	(13.379)
Observations	718	594	482	1,424	1,174	944
	FEMALES			**FEMALES**		
Mathematics	0.947	6.821	16.001	7.340	4.731	-2.023
	(10.881)	(11.234)	(15.184)	(9.294)	(10.293)	(13.281)
Reading	-8.025	0.064	13.905	4.100	2.223	-8.914
	(14.513)	(14.782)	(19.082)	(9.328)	(10.085)	(13.978)
Science	-6.790	-1.662	6.330	12.025	9.341	5.818
	(11.657)	(12.657)	(15.817)	(10.495)	(11.267)	(13.126)
Observations	689	584	461	1,406	1,193	953

Notes: The reported estimated treatment effects stem from separate estimations of different specifications. All regressions include dummy variables for month of birth, a year dummy for 2006, a dummy variable for all children born after June. The control variables on parental background include dummy variables for father's educational attainment, school location, and migration background of the family. Robust standard errors in parentheses (clustered by school programme, school location, and gender). Estimations weighted by individual inverse probability weights provided in the PISA data set. *** p<0.01, ** p<0.05, * p<0.1. Source: PISA data set (OECD), own calculations.

4.6.3 Difference-in-Difference-in-Differences estimations (DDD estimations)

The DID estimation strategy assumes that the age-related differences across birth months (season-of-birth, age-at-test, school-entry effect) are constant over time or at least over the period of three years that lies between the two PISA waves (the ‘treatment cohort’ tested in 2006 and the ‘control cohort’ tested in 2003). As a further check and control for possibly confounding changes in relative birth month outcomes, an additional control group will be included in the analysis that participated in the PISA test in both relevant years, but was not affected by the parental leave reform in 2006. Austria’s neighbouring country Germany has a very similar schooling and tracking system (Schneeweis and Zweimüller 2009) and both countries are also very close in terms of their child care institutions as well as cultural values and attitudes towards the role of families and mothers (Neyer 2003). Furthermore, the PISA test language in both countries is German and thus Germany seems to be a suitable candidate to act as additional control group in the analysis.

By including a further control group to the analysis, the effect of the parental leave reform on child outcomes will be estimated using the following triple difference estimation specification, which adds another country variable, as well as interaction effects between this country variable and ‘Post June’ births and the year dummy variable 2006 and the triple interaction term of the ‘Post June’, ‘Year 2006’ and ‘Austria’ dummy variables.

$$\begin{aligned} y_i = {} & \alpha + \beta_1 Post\,June + \beta_2\, y2006 + \beta_3 Post\,June * y2006 + \beta_4 Austria + \\ & \beta_5 Austria * y2006 + \beta_6 Austria * Post\,June + \beta_7 Post\,June * y2006 * \\ & Austria + \theta_m birth\,month + \mu X + \varepsilon_i \end{aligned} \tag{4.5}$$

The OLS estimate of the treatment effect $\hat{\beta}_7$ now becomes (AUT – stands for Austria (treatment country, in which the reform takes place); GER – stands for Germany (control country)):

$$\begin{aligned} \hat{\beta}_7 = {} & \left[\left(\bar{y}_{2006,AUT,post} - \bar{y}_{2006,AUT,pre}\right) - \left(\bar{y}_{2006,GER,post} - \bar{y}_{2006,GER,pre}\right)\right] - \\ & \left[\left(\bar{y}_{2003,AUT,post} - \bar{y}_{2003,AUT,pre}\right) - \left(\bar{y}_{2003,GER,post} - \bar{y}_{2003,GER,pre}\right)\right]. \end{aligned} \tag{4.6}$$

Table 4-11 displays the estimated treatment effect of the parental leave reform in Austria based on the DDD regressions. Generally the DDD results correspond to the DID estimates. Children of mothers with higher labour force participation, seem to have benefited from the parental leave extension – the estimated coefficients are positive for all children, but only significant and larger for boys (the estimated coefficients for males are slightly larger than the DID estimates corresponding to approximately between 0.4 and 0.7 standard deviations). Although the average estimated effects of the pooled sample of males and females are slightly larger than the DID estimates, they are slightly less significant for the May to August sample. Hence, although the additional observations from Germany help to almost double the sample size, they do not help to increase the precision of the estimation. In contrast, a comparison of the standard errors across the wider and narrower samples (March-October versus May-August) shows that the wider sample helps to reduce the standard errors slightly, but the estimated effect also decreases when children further away from the cut-off are included in the analysis.

The three columns on the right for the low LFP group of mothers show negative effects for the pooled male and female sample which are only significantly different from zero for the reading test scores. However, splitting the sample into male and female students reveals that this negative effect seems to be mainly driven by the boys. In contrast to the previous DID results, the DDD estimates of the male subsample now become much larger and more significant. The estimates for the girls are generally not very large (albeit positive) and insignificant.

To check again whether the estimators are influenced by the group of students born after August who regularly attend a lower class than children born earlier in the year, the DDD estimations are repeated including (a) only July and August and (b) only July as ‘post-reform’ birth months in the regression sample (the results are reported in the appendix, Table A 4-6 and Table 4-7 respectively). The results based on the sample including July and August to the right of the cut-off date are very similar to the results in Table 4-11. However, once July is the sole ‘post reform’ birth month in the analysis, the results change slightly: for the high LFP group coefficients increase in size and become more significant in the pooled and in the male sample (only the mathematics test score for boys becomes insignificant; the coefficients in the female sample become larger, but remain insignificant). As regards the effects for children whose mothers have lower LFP rates, the coefficients become smaller (in absolute terms, i.e. less negative), but the results for the male subgroup becomes more significant. This strong negative finding for boys of mothers with lower education is indeed surprising (especially since the effects for the girls are close to zero).

Table 4-11: DDD estimations including German students as further control group

	High LFP mothers			Low LFP mothers		
	(1) Mar-Oct	(2) Apr-Sep	(3) May-Aug	(4) Mar-Oct	(5) Apr-Sep	(6) May-Aug
	MALES + FEMALES			**MALES + FEMALES**		
Mathematics	19.210*	22.205*	22.765	-6.174	-9.851	-17.131
	(9.922)	(11.276)	(14.804)	(10.027)	(11.674)	(14.101)
Reading	18.640	20.605	25.079	-23.189*	-26.549*	-30.991*
	(13.503)	(15.201)	(17.315)	(12.286)	(13.369)	(15.499)
Science	29.104**	33.547**	37.808**	-14.867	-16.967	-20.919
	(11.817)	(13.318)	(15.267)	(11.326)	(13.141)	(14.809)
Observations	4,228	3,212	2,158	6,519	4,968	3,325
	MALES			**MALES**		
Mathematics	28.476**	32.697**	38.109**	-33.133**	-34.056*	-37.219**
	(12.966)	(13.427)	(18.223)	(15.778)	(17.211)	(16.667)
Reading	32.198*	36.062*	40.010*	-55.182***	-56.815**	-57.088**
	(16.737)	(18.282)	(22.573)	(19.308)	(21.203)	(22.137)
Science	48.607***	56.602***	66.932***	-51.510***	-53.240***	-53.071***
	(10.574)	(12.055)	(15.812)	(16.249)	(18.578)	(18.600)
Observations	2,157	1,634	1,113	3,209	2,438	1,636
	FEMALES			**FEMALES**		
Mathematics	11.050	11.602	9.412	16.207	10.129	2.579
	(16.350)	(18.952)	(23.564)	(11.318)	(15.010)	(21.739)
Reading	0.896	3.155	13.632	7.913	1.553	-8.546
	(18.381)	(21.168)	(21.205)	(12.644)	(16.166)	(23.939)
Science	9.298	9.523	10.392	16.910	14.040	9.852
	(18.943)	(21.459)	(23.252)	(13.299)	(17.327)	(22.128)
Observations	2,071	1,578	1,045	3,310	2,530	1,689

Notes: The reported estimated treatment effects are from separate estimations of different specifications. All regressions include dummy variables for month of birth, year and country fixed effects, a dummy variable for all children born after June, interaction effects between year and the 'post June' dummy, year and country, country and 'post June'. Estimations from columns 1, 2, 4, and 5 include a further dummy variable for all children born between September and December to account for the school entry cut-off date in Austria (and a year interaction). The control variables on parental background include dummy variables for father's educational attainment, school location, and migration background of the family. Robust standard errors in parentheses (clustered by school track (more/less academic)[120], school location, and gender). Estimations weighted by individual inverse probability weights provided in the PISA data set. *** p<0.01, ** p<0.05, * p<0.1. Source: PISA data set (OECD), own calculations.

[120] Since Austrian and German school types are not fully comparable, an alternative, comparable school type measure was constructed which differentiates between schools that do or do not provide access to university or college after graduation (academic track versus non-academic track).

4.7 Further robustness checks

This section presents the findings from several alternative estimations, placebo tests and sensitivity checks in order to test the robustness of the previous results. To reduce the extensiveness of the tables, only results from the regressions by gender subgroup will be presented: the question is whether the previously found strong effects for boys prove robust.

4.7.1 DID using German students as alternative control group

A first alternative estimation strategy is to use only data from the PISA test 2006 and to implement a DID estimator using German students as a control group (instead of Austrian students from the pre-reform year 2003). This estimation strategy replaces the assumption of a common birth month trend across years by the assumption of a common birth month trend across regions. Again, the results for the children of the high LFP group of mothers correspond to the previous findings (see Table 4-12; although the coefficients are slightly larger than in the original DID regressions and the effects on test scores in mathematics for boys become significant). For the group of mothers with lower LFP rates the effects remain negative, but insignificant for boys. However, the coefficients for the female subgroup of less educated mothers increase in size and in the specifications using the extended sample (i.e. children born after the Austrian school-entry cut-off date) the coefficients become even significantly positive. Nevertheless, these positive effects are much smaller and less significant when using only the standard four months window (May-August). In contrast to Austria, the school-entry cut-off date in Germany is determined separately in each of the 16 federal states and thus varies across the country.[121] Still, in many federal states the cut-off date is June 30 (Keil 2005), which would coincide with the reform cut-off date. This could potentially lead to a violation of the common trend assumption if this causes a drop in average test scores around this threshold in the German sample. While the DDD estimates accounted for this potential drop by differencing across years, this might be a problem in the DID regression set up (by artificially raising the 'treatment effect').

[121] Optimally, one could have limited the German sample to those regions, which are culturally closest to Austria (i.e. Bavaria, which shares a common border with Austria). Unfortunately, the international PISA data files do not contain regional identifiers.

Table 4-12: Robustness check. DID estimations using only PISA 2006 data and Germany as a control group.

	High LFP mothers			Low LFP mothers		
	(1) Mar-Oct	(2) Apr-Sep	(3) May-Aug	(4) Mar-Oct	(5) Apr-Sep	(6) May-Aug
		MALES			**MALES**	
Mathematics	22.887*	31.897**	34.045**	6.172	1.653	-10.321
	(11.024)	(11.212)	(14.802)	(13.336)	(15.374)	(15.686)
Reading	31.732*	40.370**	44.189*	-2.666	-7.632	-15.874
	(16.611)	(17.236)	(22.632)	(16.095)	(17.192)	(17.525)
Science	34.776**	44.846***	51.963***	-4.358	-10.952	-21.228
	(13.091)	(12.187)	(16.236)	(11.271)	(12.525)	(12.549)
Observations	1,202	911	618	1,621	1,226	826
		FEMALES			**FEMALES**	
Mathematics	15.409	15.938	21.866	22.334***	17.954**	11.546
	(16.665)	(17.766)	(18.373)	(5.781)	(8.156)	(11.338)
Reading	5.420	4.990	12.259	14.473**	9.908	8.082
	(14.225)	(15.269)	(15.076)	(6.885)	(9.419)	(12.153)
Science	8.694	9.160	13.475	20.077**	18.477*	19.073*
	(14.292)	(15.279)	(15.227)	(7.969)	(9.619)	(10.982)
Observations	1,151	888	596	1,675	1,261	843

Notes: The reported estimated treatment effects are from separate estimations of different specifications. All regressions include dummy variables for month of birth, a dummy variable for all children born after June. Estimations from columns (1)–(4) include a further dummy variable for all children born between September and December to account for the school entry cut-off date in Austria. The control variables on parental background include dummy variables for father's educational attainment, school location, and migration background of the family. Robust standard errors in parentheses (clustered by school programme, school location, and gender). Estimations weighted by individual inverse probability weights provided in the PISA data set. *** p<0.01, ** p<0.05, * p<0.1. Source: PISA data set (OECD), own calculations.

4.7.2 Placebo tests

In this subsection the results of three placebo tests are presented: First, a DID regression using exclusively the German subsample and treating July 1990 as pseudo reform month in Germany (column set (A) in Table 4-13), second, a DID regression using only data from the non-reform year 2003 for Austria and Germany, treating July 1987 in Austria as pseudo reform month (column set (B) in Table 4-13), third, using the original DID estimation strategy based on Austria and the years 2006 and 2003, but using alternative months as pseudo cut-off dates of the reform, namely May 1990 and June 1990 (Table 4-14).

Table 4-13: Placebo tests using the German subsample

	(A) DID using Germany only (2006 and 2003)		(B) DID using 2003 only; (Austria and Germany)	
	High LFP mothers	**Low LFP mothers**	**High LFP mothers**	**Low LFP mothers**
	(1) May-Aug	(2) May-Aug	(3) May-Aug	(4) May-Aug
	MALES		**MALES**	
Mathematics	-18.912	28.261**	-5.108	27.613**
	(13.572)	(9.814)	(19.843)	(9.970)
Reading	-4.714	28.811	4.011	40.045***
	(13.985)	(16.458)	(18.768)	(11.639)
Science	-24.595*	28.987**	-15.874	31.872**
	(13.076)	(12.100)	(18.373)	(11.878)
Observations	631	692	495	810
	FEMALES		**FEMALES**	
Mathematics	4.981	-3.058	15.117	7.042
	(16.789)	(10.460)	(21.769)	(14.299)
Reading	-1.027	-0.012	-0.965	13.276
	(11.016)	(8.516)	(21.035)	(16.328)
Science	-6.044	-3.224	4.422	8.101
	(13.003)	(11.323)	(21.984)	(14.966)
Observations	584	736	495	846

Notes: Each cell reports the treatment effect estimated using different specifications. All regressions include dummy variables for month of birth and a dummy variable for all children born after June as well as controls for father's educational attainment, school location, and migration background of the family. The regressions in the left two columns include furthermore a year dummy as well as year-post June interaction effect. The two columns on the right contain a country variable for Austria and an Austria-post June interaction effect. Robust standard errors in parentheses (clustered by school programme, school location, and gender). Estimations weighted by individual inverse probability weights provided in the PISA data set. *** p<0.01, ** p<0.05, * p<0.1. Source: PISA data set (OECD), own calculations.

The results of the first test based on PISA data for Germany only are mostly insignificant for children of mothers with higher education as they should be given the pseudo reform assumption of the test (Table 4-13). Only the coefficient for the science test scores for the male sample is negative and significant at the 10 percent significance level. What is more worrying are the significantly positive effects of the pseudo treatment on maths and science test scores for sons of mothers with lower LFP rates (the coefficients for the girls are very small and insignificant). These results indicate that for this particular group there is a positive trend in Germany over time and this could possibly lead to an artificial inflation of the negative coefficients in the DDD regression framework. The second test, using Austrian and German PISA data from the non-reform year 2003 again shows no significant effects for the group of children with high LFP mothers. This result gives more credibility to the positive effects for boys found in the DID regressions using Austrian and German data from 2006 (see Table 4-12). Furthermore, these results do not seem to support the hypothesized problem of the German school-entry cut-off dates. On the other hand, the results for boys of the low LFP group are disturbing: there seems to be an increase in test scores for this particular group in this regression comparing Austria with Germany which again could bring about the stronger negative results in the DDD framework. Hence, as long as the high LFP group is concerned these two placebo tests using German data tend to support the validity of the previous results and the use of the German data as additional control group. Regarding the results for children from the lower LFP group it seems that there might be confounding trends in Germany, which casts doubts on the reliability of the DDD estimates for this subgroup.

The third placebo test using alternative pseudo reform dates is presented in Table 4-14. The three columns in column set (A) come from DID regressions using May 1, 1990 as pseudo reform date and compare the test scores of children born two months prior and after the pseudo reform cut-off date (born March-April versus May-June). None of the estimated effects is significantly different for the male subsample (irrespective of maternal labour force participation rates). As regards the female subsample, all coefficients except for one are not significantly different from zero (the exception is a significantly negative effect for the reading score in the group of mothers with high LFP rates). Thus, overall, but especially regarding the male subsample, it seems that the placebo test supports the idea that the DID effects found in the original regressions are not driven by statistical outliers or seasonal/yearly patterns or trends.

Table 4-14: Placebo tests using other months as pseudo reform dates

	(A) Pseudo cut-off is *May 1, 1990* DID 2006+2003			(B) Pseudo cut-off is *June 1, 1990* DID 2006+2003		
	Full sample	**High LFP mothers**	**Low LFP mothers**	**Full sample**	**High LFP mothers**	**Low LFP mothers**
	(1)	(2)	(3)	(4)	(5)	(6)
	Mar-Jun	Mar-Jun	Mar-Jun	May-Jun	May-Jun	May-Jun
		MALES			**MALES**	
Mathematics	4.007	-5.903	10.418	-39.341***	-69.630***	-29.584*
	(10.081)	(14.688)	(12.885)	(12.065)	(19.648)	(15.088)
Reading	7.702	-14.833	19.133	-33.873**	-69.286**	-23.999
	(11.491)	(17.469)	(14.598)	(14.916)	(26.523)	(18.220)
Science	1.185	-15.328	10.248	-38.284**	-58.028**	-34.799*
	(11.928)	(13.059)	(15.292)	(17.216)	(23.587)	(19.251)
Observations	1,432	462	970	716	226	490
		FEMALES			**FEMALES**	
Mathematics	1.437	-26.332	18.209	-16.537	-33.782	-12.179
	(11.353)	(15.491)	(16.229)	(15.080)	(25.184)	(15.695)
Reading	2.017	-39.974**	25.166	-13.607	-23.318	-13.127
	(13.365)	(15.263)	(19.199)	(13.753)	(25.679)	(13.450)
Science	-1.225	-23.605	12.143	0.846	-9.598	3.672
	(9.896)	(13.953)	(12.958)	(14.874)	(31.326)	(14.183)
Observations	1,361	450	911	680	222	458

Notes: Each cell reports the treatment effect estimated using different specifications. All regressions include a year dummy (2006) and control for mother's and father's educational attainment, school location, and migration background of the family. The three left columns include furthermore control variables for birth months, a dummy variable for all children born after the pseudo cut-off date May 1, 2006 (May and June) as well as year-post April interaction effect. The three columns on the right contain a dummy variable for all children born after the pseudo cut-off date June 1, 2006 and an interaction of this dummy with the year dummy 2006. Robust standard errors in parentheses (clustered by school programme, school location, and gender). Estimations weighted by individual inverse probability weights provided in the PISA data set. *** p<0.01, ** p<0.05, * p<0.1. Source: PISA data set (OECD), own calculations.

However, turning to the three columns on the right which treat June 2006 (the month prior to the actual reform) as pseudo reform date and including only one pre and post pseudo-reform months (May and June) the picture looks less promising. Although the placebo test would require all coefficients to be insignificant, the estimated pseudo-reform effects are significantly negative and quite large for the male subgroup (although the samples become extremely small) and negative but insignificant for the girls. This finding is puzzling and the question is, whether June 2003 or June 2006 are outliers in the data who drive the original effects (if there is a negative June effect, than the estimated positive treatment effect of the reform on July 1 for sons of higher LFP mothers might be driven by this negative pre-reform effect). The significant June effects are worrying and require a further repetition of the original DID estimates *excluding* children born in June to test to what extent the June effect seems to be driving the result (see next section).

Generally, it could also be that June 2003 is an *outlier* and it would be helpful to test this possibility by repeating the DID analysis using another control year with PISA test scores from Austria instead of PISA 2003. Alternative robustness checks could have involved data from the Austrian PISA tests 2000 and 2009 (i.e., birth cohorts 1984 and 1993). However, several problems with data from these two years made them unsuitable for this purpose.[122] First of all, the sample of schools included in the sample frame in the year 2000 was biased, since students enrolled in 'combined school and work' vocational programmes were systematically underrepresented. Furthermore, substantial revisions of the student background questionnaire between 2000 and 2003 make it impossible to construct comparable and consistent categories of parental education. As regards the 2009 data,

> *"a dispute between teacher unions and the education minister in Austria led to the announcement of a boycott of PISA which was withdrawn after the first week of testing. The boycott required the OECD to remove identifiable cases from the dataset. Although the Austrian dataset met the PISA 2009 technical standards after the removal of these cases, the negative atmosphere in relation to education assessments affected the conditions under which the assessment was administered and could have adversely affected student motivation to respond to the PISA tasks"* (see footnote 122).

An even bigger problem that impedes the inclusion of the 2009 data into the analysis comes from the fact that the cohort of included children (born in 1993), were the first cohort to be affected by stricter school entry rules (entry into first grade) that came into effect at the beginning of the school year 1999/2000. An amendment to the school law caused an increase in compliance with the school-entry cut-off date regulations. As a consequence the rate of children entering school late became much smaller. It is possible that these stricter school entry regulations led to an increase of repetition rates for children, who would have otherwise started school one year later. These changes and consequences affected mainly children born shortly before the cut-off date of September 1, i.e. born in August or July. Thus, there is a different trend over

[122] See also the information on 'Anomalies in PISA data' available on the OECD web-site, http://www.pisa.oecd.org/document/53/0,3746,en_32252351_32235731_38262901_1_1_1_1,00.html.

time regarding the relative composition of July and August children in terms of age at school entry and subsequent school experiences in comparison with children born in other months of the year.

4.7.3 DID estimates excluding June children

As explained above, it is possible that the estimated treatment effect found in the earlier regressions is driven by an unexpectedly negative trend in test scores of June children (while these could be driven either by June 2003 test scores being unnaturally high *or* June 2006 test scores being unnaturally low). This is why the previous DID estimations are repeated without including June children in the estimation sample. Certainly, given the rather small size of the estimation sample, dropping observations from an entire birth month affects the precision of the estimates. Furthermore, the post-reform results – including the potentially problematic August and September children due to the school-entry effect – receive relatively more weight. Table 4-15 shows the main results for the different sample specifications (symmetric extension to the left and to the right of the reform cut-off, as well as including only July and/or August as post-reform months) and also for the DID estimates using only 2006 and German students instead of Austrian students from 2003 as control group (the two columns on the right of the table).

Once June is excluded from the sample, the estimated coefficients for children from the group of mothers with higher maternal LFP rates become much smaller in size and mostly insignificant. In fact, the estimated treatment effects become negative for girls across all subjects and for boys with respect to mathematics test scores. Nevertheless, the estimated treatment effects on reading and science for the male subsample remain positive and are partly significantly different from zero on the ten percent level (column two and four). The estimated treatment effects for children whose mothers have a lower LFP rate become more negative. In contrary, the re-estimated DID estimates based on the 2006 data with Germany as control region remain significantly positive for boys of mothers with higher LFP rates (columns eleven and twelve).

Table 4-15: DID estimates excluding children born in June from the regressions

	DID estimates (Austria 2006/2003)										DID 2006 estimates (Austria/Germany)	
	High LFP mothers					Low LFP mothers					High LFP mothers	Low LFP mothers
	(1) Apr-Sep	(2) Apr-Aug	(3) May-Aug	(4) Apr-Jul	(5) May-Jul	(6) Apr-Sep	(7) Apr-Aug	(8) May-Aug	(9) Apr-Jul	(10) May-Jul	(11) May-Aug	(12) May-Aug
MALES												
Mathematics	-3.562	-3.053	-11.802	3.795	-5.300	-7.246	-6.937	-22.481	-6.216	-20.677	41.468*	-18.153
	(12.295)	(12.512)	(18.361)	(15.792)	(20.934)	(12.541)	(12.471)	(13.925)	(12.390)	(15.499)	(22.332)	(23.099)
Reading	11.644	12.055	6.050	30.448	25.624	-23.291	-22.913	-37.972**	-15.547	-29.394*	50.809*	-33.143
	(12.504)	(12.905)	(17.999)	(17.910)	(22.745)	(14.776)	(14.860)	(13.744)	(13.800)	(14.822)	(27.692)	(25.717)
Science	18.735	20.302*	17.775	29.408*	26.449	-24.883*	-24.807*	-39.587**	-27.274**	-40.945**	55.520**	-33.025*
	(11.360)	(11.718)	(16.363)	(14.842)	(19.116)	(13.158)	(13.097)	(15.462)	(12.149)	(16.484)	(21.224)	(18.487)
Observations	618	496	384	359	247	1,154	931	701	707	477	476	633
FEMALES												
Mathematics	-7.214	-6.956	-5.076	-11.251	-10.699	5.338	5.207	-7.976	3.350	-10.154	7.875	13.223
	(13.109)	(13.306)	(20.421)	(31.011)	(34.851)	(9.369)	(9.538)	(12.049)	(15.356)	(18.596)	(20.875)	(9.925)
Reading	-14.864	-13.951	-1.744	-16.931	-5.742	5.035	4.971	-15.054	0.264	-22.413	-6.757	-5.276
	(15.295)	(15.410)	(24.619)	(25.117)	(33.059)	(10.851)	(10.917)	(16.558)	(17.410)	(23.475)	(17.222)	(15.172)
Science	-10.327	-9.636	-1.957	-14.274	-7.914	12.026	12.053	7.861	12.846	6.944	-10.257	17.605
	(16.195)	(16.227)	(25.967)	(24.400)	(32.539)	(11.488)	(11.535)	(14.290)	(18.790)	(22.166)	(16.709)	(11.550)
Observations	604	479	356	354	231	1,201	951	711	691	451	461	655

Notes: Each cell reports the estimated treatment effects from separate DID regressions controlling for the standard set of background variables as in the original regressions in Section 4.6. Children born in June are excluded from the estimation sample. Robust standard errors in parentheses (clustered by school programme, school location, and gender). Estimations weighted by individual inverse probability weights provided in the PISA data set. *** p<0.01, ** p<0.05, * p<0.1. Source: PISA data set (OECD), own calculations.

To sum up, omitting June children from the estimation reduces the estimates. The previously positive treatment effects for boys from better educated mothers become insignificant in most specifications. Since these regressions gave relatively more weight to the post-reform birth months, the remaining question is to what extent the findings (including and excluding June) are affected by the proximity to the school-entry cut-off date and the corresponding higher fraction of students in lower grade levels in the post-reform months. This will be analysed in the following section.

4.7.4 DID estimates of other schooling outcomes and the role of retained students

To better understand the underlying mechanisms of the previous findings as well as the potentially confounding role of the school-entry cut-off age effect two further analyses are conducted. The first set of regressions repeats the DID estimations using alternative schooling outcomes, namely whether a student is in a lower than regular grade level given his birth month and whether a student is enrolled in the academic track. The research question motivating these regressions is whether the reform had an effect on these schooling outcomes as well. If so, the estimated treatment effect of the reform based on test scores might partly be driven by these mechanisms and the consequential grade composition of students for each birth month (more or less regular or retained students in the post-reform months July and August). Unfortunately, the data do not allow disentangling the reasons *why* students are in a lower than regular grade level. This could be either due to deferred school entry or to grade repetition.

The regression results for the effect of the extended maternal leave period on the two alternative schooling outcomes are reported in Table 4-16. Overall, the effects for both outcomes are small and in most of the cases not significantly different from zero. Nevertheless, there seem to be differences between the two groups of mothers: for the group of *mothers with lower LFP rates* (column 3) the results suggest that the extended leave period increased the likelihood that treated children are significantly more likely to be in a lower than regular grade level. This effect is significantly positive in the pooled male and female sample, but seems to be even more pronounced for girls than for boys. In line with these findings the coefficients of the reform on the propensity to be enrolled in the academic track are all negative. However, none of these estimates is significant.

In contrast, the results for children whose *mothers have higher LFP* rates are close to zero and are all not significantly different from zero. In the pooled male and female sample (column 2, top panel) the signs of the coefficients point to a potentially negative effect of the parental leave reform on the likelihood to be retained and a positive effect on being enrolled in an academic track school (none of the results for this group of mothers is significant though).

Table 4-16: Probability of being in lower than regular grade level (grade retention) or being enrolled in the academic track (linear probability models)

		DID 2006 and 2003 Two months window May – August	
	(1) Full sample	(2) High LFP mothers	(3) Low LFP mothers
		MALES + FEMALES	
Retained	0.055 (0.035)	-0.022 (0.059)	0.089** (0.041)
Academic track	-0.042 (0.034)	0.004 (0.059)	-0.067 (0.043)
Observations	2,840	943	1,897
		MALES	
Retained	0.019 (0.051)	-0.031 (0.090)	0.042 (0.049)
Academic track	-0.044 (0.036)	-0.005 (0.062)	-0.064 (0.047)
Observations	1,426	482	944
		FEMALES	
Retained	0.089** (0.040)	0.002 (0.077)	0.137** (0.062)
Academic track	-0.047 (0.057)	0.019 (0.091)	-0.065 (0.073)
Observations	1,414	461	953

Notes: Each cell reports the estimated treatment effects from separate DID regressions controlling for the standard set of background variables as in the original regressions in Section 4.6. The dependent variables are two dummy variables indicating whether a student is enrolled in a lower than regular grade level given his birth date and whether the student is enrolled in the academic track. Robust standard errors in parentheses (clustered by school programme, school location, and gender). Estimations weighted by individual inverse probability weights provided in the PISA data set. *** p<0.01, ** p<0.05, * p<0.1. Source: PISA data set (OECD), own calculations.

There also appear to be some differences between boys and girls which however seem to be driven by the subgroup of girls from mothers with lower LFP rates. For the boys, all of the estimated effects are statistically significant and close to zero. The same is true for daughters of mothers with higher LFP rates (no significant effects). However, in the full sample (all mothers) of girls the results seem to suggest a positive and statistically significant effect of the reform on the likelihood to be retained. This result seems to be driven by the group with low LFP mothers though.

To sum up, although the estimated coefficients are suggestive, they are all close to zero and mainly not significant. Most importantly, for the group of children from mothers with higher LFP rates there seems to be no effect of the reform on the two outcomes. In the full sample estimation for females as well as for the subgroup of mothers with lower LFP rates, there seems to be an effect on the likelihood to be in a lower than regular grade level. This could explain the respective negative findings in the test score regressions.

The next test repeats the DID estimations for the test score outcomes from Section 4.6.2, but limits the estimation sample to students who are not retained but currently enrolled in the regular grade level according to their birth month and the school-entry cut-off regulations. This will generally increase the average test scores in each birth month, since students in lower grades have lower test scores on average as they have less years of formal schooling and have covered less material in school. In addition, this will probably reduce the variance of test scores in each birth month (especially in the months close to the school-entry cut-off where there are more children on lower grade levels).

The first six columns in Table 4-17 are based on regressions *including* the potentially problematic month of June, while the six columns on the right side of the table are based on regressions *excluding* children born in June. Certainly, if the likelihood of a student's retention is in itself affected by the reform and thus an *outcome* variable of the reform, it is problematic to split the sample according to this variable. However, the previous results have shown that there is no significant reform effect on the probability of grade retention for children of mothers with higher LFP rates and on boys; the exception are daughters of mothers with lower LFP – and, hence, the results for this subgroup have to be interpreted cautiously.

The results in the first six columns from Table 4-17 can be compared to the results from Table 4-9 as these are estimated using the same estimation windows (samples with gradually narrowed birth month estimation windows). The general pattern of the estimated treatment effects corresponds well to the one using the full sample. However, the positive treatment effects for sons from mothers with higher LFP rates are slightly larger and slightly more significant, while the negative effect on sons of mothers with lower LFP rates become slightly weaker and less significant. The results for the female subsamples change only marginally, but remain insignificant. What is more interesting: repeating this set of estimations *excluding* children born in June does not affect these findings dramatically. The effect of the parental leave extension remains positive and large (and is significantly different from zero in many specifications) for sons of mothers with higher LFP rates. The estimated effects for the girls are not significantly different from zero through all specifications and for all subjects. On the other hand, the negative effect of the reform on children from mothers with lower LPF rates becomes slightly larger and partly significant; however, only in the May to August specification do the boys' test scores on reading and science seem to be significantly affected by the reform.

Hence, although the parental leave reform has only negligible and insignificant effects on the likelihood of being retained for the subsample of children with mothers with higher LFP rates (as shown in Table 4-16), the restriction of the sample to 'regular' students seems to reduce the sensitivity of the results with respect to the exclusion of June. However, if anything, boys with higher LFP mothers seem to be less likely to be retained as a result of the reform (even though the coefficient is not significant), meaning that restricting the sample to 'regular' students should actually work in the other direction (making the reform effect less positive). Overall, even though it could be critical to restrict the sample to 'regular' students, it helps to get an impression of how grade retention of students close to the school-entry cut-off date affect the DID results (namely through a possible downward bias).

Table 4-17: DID estimates using only students in ‘regular’ grades (including and excluding June births)

	Regressions based on regular students, *including June*						Regressions based on regular students, *excluding June*					
	High LFP mothers			**Low LFP mothers**			**High LFP mothers**			**Low LFP mothers**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Mar-Oct	Apr-Sep	May-Aug	Mar-Oct	Apr-Sep	May-Aug	Mar-Oct	Apr-Sep	May-Aug	Mar-Oct	Apr-Sep	May-Aug
MALES												
Mathematics	22.924	25.806*	34.498*	3.848	3.164	-1.665	12.712	10.751	14.553	1.114	-1.890	-18.257
	(14.680)	(12.610)	(17.440)	(8.758)	(10.706)	(13.062)	(16.200)	(14.773)	(22.068)	(8.979)	(12.115)	(11.744)
Reading	37.448***	40.073***	46.034**	-13.098	-17.583	-22.845	27.349**	26.243**	26.485	-14.438	-21.957	-37.660**
	(8.680)	(8.678)	(16.907)	(12.289)	(14.401)	(15.197)	(10.483)	(10.880)	(20.688)	(12.810)	(16.652)	(13.297)
Science	49.459***	54.125***	66.762***	-9.099	-11.633	-16.041	40.195***	41.450***	51.701**	-12.028	-17.283	-33.328**
	(11.867)	(10.108)	(15.647)	(9.836)	(12.252)	(15.431)	(13.594)	(12.075)	(21.050)	(9.551)	(12.645)	(12.222)
Observations	704	504	305	1,496	1,076	707	637	437	238	1,308	888	519
FEMALES												
Mathematics	-2.633	4.970	12.703	7.594	2.813	3.047	-12.112	-4.003	-1.171	6.050	-1.977	-7.861
	(17.865)	(17.758)	(21.009)	(8.225)	(9.050)	(12.725)	(20.275)	(20.296)	(25.229)	(8.215)	(8.525)	(11.914)
Reading	-10.685	-1.671	13.178	2.323	-0.630	-2.665	-20.881	-12.360	3.265	1.105	-3.845	-12.066
	(20.900)	(20.847)	(25.449)	(9.911)	(10.597)	(15.513)	(21.133)	(20.729)	(27.753)	(9.930)	(10.229)	(17.009)
Science	-4.932	1.055	9.103	10.972	5.710	9.090	-10.736	-4.429	4.575	11.294	3.445	7.326
	(19.124)	(20.332)	(22.878)	(11.378)	(12.151)	(13.979)	(21.303)	(23.818)	(31.441)	(11.835)	(13.492)	(17.153)
Observations	746	555	351	1,576	1,162	740	661	470	266	1,379	965	543

Notes: Each cell reports the estimated treatment effects from separate DID regressions controlling for the standard set of background variables as in the original regressions in Section 4.6. The estimation sample includes only children in regular grade levels (according to their birth months). Robust standard errors in parentheses (clustered by school programme, school location, and gender). Estimations weighted by individual inverse probability weights provided in the PISA data set. *** p<0.01, ** p<0.05, * p<0.1. Source: PISA data set (OECD), own calculations.

4.8 Conclusions

The objective of the empirical analysis presented in this chapter was to investigate whether a substantial extension of the paid and job-protected Austrian parental leave mandate – from the child's first to the child's second birthday – had any medium-term effects on the cognitive development of children. What makes the Austrian parental leave reform particularly suitable for a causal Difference-in-Difference (DID) analysis is that it was implemented with a strict and unanticipated cut-off date: only those mothers who gave birth to their child on or after July 1, 1990 became eligible for the more generous 24 months parental leave duration. As a consequence of this unexpected cut-off date the allocation of mothers and their children into treatment (24 months parental leave entitlements) and control (12 months parental leave) group was 'as good as random'. This exogenous discontinuity in parental leave duration helps to control for the problem of the otherwise endogenous return-to-work decision. Another advantage of this particular Austrian reform is that it did not seem to have any effects on medium or long-term labour market outcomes of mothers and only a small positive effect on fertility (as shown by previous studies).

To assess the effect of the parental leave extension on child outcomes the empirical analysis made use of mathematics, reading and science test scores from the standardized PISA test (using the cohort born in the year of the reform 1990 as well as a control birth cohort which was not subject to a reform, 1987; the corresponding PISA tests were conducted in the years 2006 and 2003). Since the data do only contain information on maternal and paternal educational attainment, but neither on parental leave eligibility nor on actual leave taking, the estimated effects resemble intention-to-treat estimates (net reduced form effects).

The results of the DID analyses and several robustness checks reveal that there seem to be heterogeneous effects of the parental leave reform on PISA test scores across estimation samples and subgroups. When using the *full sample*, the estimates suggest that the 12 months parental leave extension did not have any statistically significant causal medium-run effects on cognitive skills. This finding for Austria is in line with the results of most of the studies using changes in parental leave mandates to identify the causal effect of early maternal employment on child outcomes (in particular, Canada

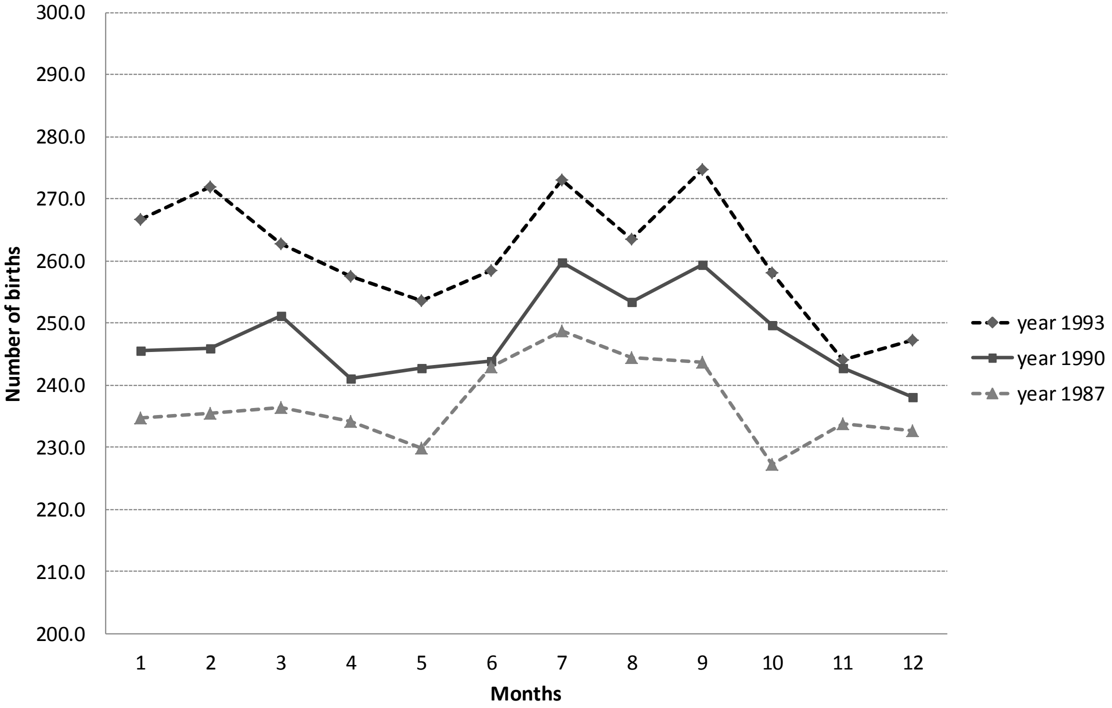
(Baker and Milligan 2010a, b), Denmark (Würtz Rasmussen 2010), Germany (Dustman and Schönberg 2010) and Sweden (Liu and Nordstrom Skans 2010)).

However, when splitting the sample into two groups of mothers with higher and lower labour force participation rates respectively (i.e., higher and lower educational attainment), interesting findings emerge: for the children of mothers with higher labour force participation rates and who are thus more likely to have been eligible and affected by the reform, there appears to be a significantly positive effect of the parental leave extension on the PISA test scores at age 15. This positive result seems to be especially driven by the large and significant effect on boys. Such a positive reform effect among the group of more eligible mothers would generally correspond to the findings by Carneiro, Løken and Salvanes (2010) for Norway: while their analysis using the full sample produces insignificant results, the estimated effects using the subsample of eligible mothers (which they can identify in the data) reveals significantly positive effects of a prolonged parental leave period up to the child’s first birthday. Furthermore, Liu and Nordstrom Skans (2010) also find positive effects for their assessed parental leave extension in Sweden when restricting the sample to children of mothers with higher education. The other studies do either not test or simply do not find any differences according to maternal parental leave eligibility or educational status. The same is true for gender differences: while there exists some evidence of heterogeneous *health* outcomes of girls and boys with respect to early maternal employment, the previous evaluation studies of parental leave on *cognitive* child outcomes have not tested or not found corresponding gender effects. Against this background, the presumably stronger effect of the Austrian parental leave extension on boys points to an interesting aspect which should be explored in more detail in future research.

Although these empirical results are robust to various sensitivity checks – including Difference-in-Difference-in-Differences estimations using Germany as an additional control region which was not affected by the 1990 parental leave reform – the findings seem to be sensitive to the exclusion of children born in the pre-reform month June 1990. Hence, although the results suggest a significantly positive and causal reform effect for children of mothers with higher labour force participation rates one has to be cautious about drawing any final policy conclusions. Nevertheless, as already highlighted and demonstrated by Carneiro, Løken and Salvanes (2010) for Norway it seems to be very important for the analysis to differentiate between mothers who are eligible or ineligible for parental leave. Furthermore, the estimated reform effects are only informative about the *net impact* of the parental leave extension on cognitive child outcomes at age 15 (reduced form estimations). This is true for the present as well as for the majority of the cited evaluation studies (which all lack relevant information on eligibility, actual leave taking and maternal time investments in child rearing). Hence, it remains an important task for future research to investigate in more depth the transmission channels through which parental leave mandates might affect child outcomes. Besides the role played by *maternal* care for children another important transmission channel might work through – potentially reduced – *paternal* time with children if fathers adjust their time allocation between family and work in response to the extended leave duration of mothers. Such a reduction in paternal child care – which might be particularly relevant in low-income families – could actually offset any positive effects of additional maternal time.

4.9 Appendix to Chapter 4

Figure A 4-1: Seasonal birth pattern (average number of live births per day across months)



Notes: Birth data refer to the resident population of Austria (permanent residents), irrespective of citizenship, and do not include births registered abroad. Migrants having stayed in Austria less than 3 months are not counted in the resident population. Source: Human Fertility Database (monthly birth numbers). Daily number of births based on own calculations.

Table A 4-1: Female labour force participation rates in selected countries (% of female population ages 15-64)

	1980	1985	1990	1995	2000	2005	2009
Australia	52.0	54.4	61.9	64.2	65.5	68.2	70.1
Austria	49.9	50.8	55.3	62.2	62.3	65.5	68.4
Denmark	71.7	74.8	77.6	73.3	75.4	75.8	76.5
France	55.2	56.5	57.7	60.5	62.3	64.8	65.5
Germany	52.0	52.3	57.8	61.5	63.5	68.0	71.2
Italy	39.7	39.9	43.6	42.5	46.2	50.4	51.8
Netherlands	48.2	48.4	52.4	58.2	65.6	70.0	73.7
Norway	61.8	67.1	69.9	72.2	76.2	74.5	75.7
Spain	33.1	34.5	41.4	45.8	51.8	58.2	63.1
Sweden	75.0	78.9	81.9	77.3	74.8	76.4	77.0
Switzerland	64.9	66.0	68.2	69.1	71.7	74.3	76.4
United Kingdom	56.3	61.0	66.1	65.9	67.7	68.7	69.4
United States	59.8	63.9	67.5	69.4	70.4	68.6	68.1

Notes: Labour force participation rate is defined as the proportion of the economically active population aged 15-64. Source: World Bank (2011). Gender Statistics Database.

Table A 4-2: Overview of reduction of original sample size due to sample restriction and item-non-response

	2006		2003	
	Children born		*Children born*	
	January-December	**May-August**	January-December	**May-August**
Original sample size	4,906	**1,640**	4,597	**1,574**
... after excluding students not born in Austria	4,456	**1,503**	4,126	**1,410**
... after excluding students in schools for children with special needs	4,439	**1,499**	4,102	**1,401**
... after excluding students with missing educational information of mother	4,372	**1,480**	4,034	**1,379**
Final sample with non-missing information of included control variables		**1,480**		**1,360**
This corresponds to % of original sample		***90.2%***		***86.4%***

Notes: The table informs about the reduction of the original PISA 2006 and 2003 samples due to sample restrictions and missing observations. Source: PISA data set (OECD), own calculations.

Table A 4-3: DID estimates, further specifications

	(A) **Additional controls for school programme** Two months window (May-Aug)			(B) **One month window (Jun-Jul)** Main specification (standard control variables)		
	(1) Full sample	(2) High LFP mothers	(3) Low LFP mothers	(4) Full sample	(5) High LFP mothers	(6) Low LFP mothers
		MALES + FEMALES			**MALES + FEMALES**	
Mathematics	5.802	20.335***	-1.843	15.827	46.545***	3.851
	(4.643)	(6.742)	(6.559)	(11.000)	(17.205)	(14.990)
Reading	-0.463	24.799**	-13.269*	11.118	55.814***	-8.905
	(6.264)	(10.191)	(6.643)	(11.624)	(18.600)	(14.871)
Science	5.841	26.334***	-5.070	11.370	45.131***	-3.056
	(5.559)	(7.816)	(6.793)	(11.633)	(15.713)	(15.429)
		MALES			**MALES**	
Mathematics	6.482	18.951**	-1.565	22.142*	59.477***	3.493
	(6.176)	(8.810)	(9.276)	(12.254)	(13.847)	(18.967)
Reading	-1.770	32.890**	-19.939*	22.062	89.072***	-7.507
	(8.348)	(14.420)	(9.674)	(15.176)	(24.605)	(21.125)
Science	4.479	41.727***	-16.264	19.684	80.092***	-8.818
	(7.316)	(9.158)	(10.110)	(15.128)	(17.602)	(22.173)
		FEMALES			**FEMALES**	
Mathematics	6.005	17.721	-1.131	8.832	29.720	1.123
	(6.815)	(12.052)	(9.133)	(17.555)	(32.105)	(24.157)
Reading	0.230	15.905	-6.717	0.361	25.030	-8.991
	(9.373)	(16.923)	(8.592)	(16.869)	(25.715)	(22.423)
Science	7.763	7.868	7.344	3.288	6.676	3.341
	(8.234)	(12.725)	(9.874)	(17.108)	(23.328)	(24.125)
Controls						
Parental background	✓	✓	✓	✓	✓	✓
School programme	✓	✓	✓	-	-	-

Notes: The upper panel includes only male, the lower panel only female students. All regressions include dummy variable controls for survey year, birth months and for all children born post June. The control variables on parental background include dummy variables for mother's and father's educational attainment, school location, migration background and for the five different school types. The sample size for the pooled samples (top panel; row 1 and 3) are 2,840 and 1,386 respectively. Robust standard errors in parentheses (clustered by school programme, school location, and gender). Estimations weighted by individual inverse probability weights provided in the PISA data set. *** p<0.01, ** p<0.05, * p<0.1. Source: PISA data set (OECD), own calculations.

Table A 4-4: DID estimates, including only one 'post-reform' month (July)

	High LFP mothers			Low LFP mothers		
	(1)	(2)	(3)	(4)	(5)	(6)
	Mar-Jul	Apr-Jul	May-Jul	Mar-Jul	Apr-Jul	May-Jul
	MALES + FEMALES			**MALES + FEMALES**		
Mathematics	8.229	12.587	18.436	1.140	-0.269	-6.783
	(15.021)	(15.374)	(16.278)	(9.949)	(10.906)	(13.139)
Reading	17.776	22.329	28.804*	-7.819	-9.188	-16.252
	(13.783)	(14.203)	(15.804)	(10.120)	(10.705)	(12.468)
Science	15.428	20.104	26.311*	-5.060	-6.768	-10.773
	(12.239)	(12.719)	(13.670)	(10.978)	(11.397)	(13.389)
Observations	1,145	916	681	2,346	1,883	1,413
	MALES			**MALES**		
Mathematics	20.046	20.516	22.530	-2.083	-2.593	-8.195
	(13.042)	(13.605)	(15.253)	(10.263)	(11.900)	(14.752)
Reading	45.181***	47.766**	52.335**	-9.156	-12.951	-18.941
	(16.062)	(17.502)	(19.672)	(11.375)	(12.760)	(15.171)
Science	41.839***	44.333***	49.019***	-19.719*	-21.487*	-25.685
	(12.292)	(13.026)	(14.065)	(11.359)	(12.091)	(15.982)
Observations	581	457	345	1,200	950	720
	FEMALES			**FEMALES**		
Mathematics	-3.534	1.535	10.342	5.569	3.115	-3.839
	(28.181)	(29.468)	(31.116)	(15.939)	(17.161)	(20.301)
Reading	-11.181	-3.387	10.113	-0.699	-2.565	-15.003
	(22.966)	(23.740)	(26.898)	(16.582)	(17.788)	(21.782)
Science	-11.819	-7.059	0.125	12.778	10.421	6.205
	(20.282)	(21.443)	(23.573)	(18.620)	(19.504)	(21.794)
Observations	564	459	336	1,146	933	693

Notes: The reported estimated treatment effects stem from separate estimations of different specifications based on the Austrian PISA data 2006 and 2003. All regressions include dummy variables for month of birth, a year dummy for 2006, a dummy variable for all children born after June. The control variables on parental background include dummy variables for father's educational attainment, school location, and migration background of the family. Robust standard errors in parentheses (clustered by school programme, school location, and gender). Estimations weighted by individual inverse probability weights provided in the PISA data set. *** p<0.01, ** p<0.05, * p<0.1. Source: PISA data set (OECD), own calculations.

Table A 4-5: DID estimations excluding observations from Vienna.

	High LFP mothers			Low LFP mothers		
	(1) Mar-Oct	(2) Apr-Sep	(3) May-Aug	(4) Mar-Oct	(5) Apr-Sep	(6) May-Aug
		MALES			**MALES**	
Mathematics	15.760	17.056	15.945	3.062	6.102	1.903
	(12.834)	(11.867)	(15.894)	(9.063)	(9.876)	(10.871)
Reading	32.536**	36.820**	42.002**	-12.455	-11.839	-15.642
	(13.646)	(14.137)	(16.713)	(10.876)	(12.445)	(11.604)
Science	35.190***	40.442***	44.329***	-9.637	-7.091	-9.195
	(12.157)	(12.663)	(14.160)	(9.581)	(9.339)	(10.815)
Observations	784	586	391	1,590	1,205	824
		FEMALES			**FEMALES**	
Mathematics	6.716	11.041	24.195	2.743	0.766	-2.255
	(10.694)	(11.801)	(16.923)	(9.447)	(10.843)	(14.919)
Reading	-1.750	3.669	20.118	5.595	2.452	0.509
	(15.200)	(15.160)	(20.835)	(9.983)	(11.019)	(15.014)
Science	-4.031	-0.906	9.784	8.535	5.756	6.486
	(12.914)	(13.848)	(17.708)	(10.845)	(12.040)	(14.731)
Observations	785	602	396	1,695	1,273	845

Notes: The reported estimated treatment effects stem from separate estimations of different specifications based on the Austrian PISA data 2006 and 2003. The sample excludes children living in Vienna. All regressions include dummy variables for month of birth, a year dummy for 2006, a dummy variable for all children born after June. The control variables on parental background include dummy variables for father's educational attainment, school location, and migration background of the family. Robust standard errors in parentheses (clustered by school programme, school location, and gender). Estimations weighted by individual inverse probability weights provided in the PISA data set. *** p<0.01, ** p<0.05, * p<0.1. Source: PISA data set (OECD), own calculations.

Table A 4-6: DDD estimates, including only two 'post-reform' months (July and August)

	High LFP mothers			Low LFP mothers		
	(1)	(2)	(3)	(4)	(5)	(6)
	Mar-Aug	Apr-Aug	May-Aug	Mar-Aug	Apr-Aug	May-Aug
	MALES + FEMALES			**MALES + FEMALES**		
Mathematics	18.991*	21.884*	22.765	-5.564	-9.276	-17.131
	(9.963)	(11.259)	(14.804)	(10.109)	(11.773)	(14.101)
Reading	18.470	20.374	25.079	-22.205*	-25.883*	-30.991*
	(13.437)	(15.170)	(17.315)	(12.420)	(13.402)	(15.499)
Science	28.977**	33.443**	37.808**	-14.057	-16.326	-20.919
	(11.869)	(13.328)	(15.267)	(11.478)	(13.253)	(14.809)
Observations	3,143	2,645	2,158	4,877	4,111	3,325
	MALES			**MALES**		
Mathematics	28.337**	32.937**	38.109**	-32.050*	-33.276*	-37.219**
	(13.109)	(13.506)	(18.223)	(16.075)	(17.255)	(16.667)
Reading	32.141*	36.247*	40.010*	-53.978**	-55.936**	-57.088**
	(16.556)	(18.176)	(22.573)	(19.885)	(21.458)	(22.137)
Science	48.457***	56.952***	66.932***	-50.191***	-52.185**	-53.071***
	(10.736)	(11.991)	(15.812)	(16.658)	(18.681)	(18.600)
Observations	1,615	1,350	1,113	2,410	2,014	1,636
	FEMALES			**FEMALES**		
Mathematics	11.277	11.569	9.412	16.768	11.147	2.579
	(16.250)	(18.794)	(23.564)	(11.624)	(15.431)	(21.739)
Reading	1.167	3.640	13.632	8.589	2.491	-8.546
	(18.502)	(21.409)	(21.205)	(12.821)	(16.398)	(23.939)
Science	9.526	9.598	10.392	17.281	14.894	9.852
	(19.197)	(21.652)	(23.252)	(13.708)	(17.689)	(22.128)
Observations	1,528	1,295	1,045	2,467	2,097	1,689

Notes: The reported estimated treatment effects stem from separate estimations of different specifications. All regressions include dummy variables for month of birth, year and country fixed effects, a dummy variable for all children born after June, interaction effects between year and the 'post June' dummy, year and country, country and 'post June'. The control variables on parental background include dummy variables for father's educational attainment, school location, and migration background of the family. Robust standard errors in parentheses (clustered by school track (more/less academic), school location, and gender). Estimations weighted by individual inverse probability weights provided in the PISA data set. *** p<0.01, ** p<0.05, * p<0.1. Source: PISA data set (OECD), own calculations.

Table A 4-7: DDD estimates, including only one 'post-reform' month (July)

	High LFP mothers			Low LFP mothers		
	(1) Mar-Jul	(2) Apr-Jul	(3) May-Jul	(4) Mar-Jul	(5) Apr-Jul	(6) May-Jul
		MALES + FEMALES			**MALES + FEMALES**	
Mathematics	23.787*	27.064*	28.006	-12.450	-16.021	-24.164
	(13.928)	(14.826)	(17.540)	(12.830)	(14.956)	(17.880)
Reading	29.370*	31.610*	35.799*	-20.369	-23.877	-29.145
	(16.311)	(17.557)	(18.973)	(14.704)	(16.108)	(18.571)
Science	36.203**	41.148**	45.302***	-19.369	-21.599	-26.645
	(14.061)	(15.300)	(16.807)	(14.772)	(16.903)	(19.038)
Observations	2,566	2,068	1,581	4,017	3,251	2,465
		MALES			**MALES**	
Mathematics	26.028	30.699	35.408	-37.074***	-37.652**	-41.197***
	(21.021)	(20.366)	(23.135)	(12.042)	(14.257)	(14.221)
Reading	43.741*	48.882*	52.053*	-51.579***	-52.982***	-53.417***
	(22.900)	(24.392)	(27.165)	(16.004)	(17.679)	(18.416)
Science	54.234***	63.334***	73.115***	-57.985***	-59.688***	-60.479***
	(18.995)	(18.917)	(20.420)	(10.752)	(13.951)	(15.451)
Observations	1,306	1,041	804	2,002	1,606	1,228
		FEMALES			**FEMALES**	
Mathematics	21.529	22.319	21.237	10.292	4.967	-4.348
	(21.740)	(24.856)	(30.430)	(20.344)	(23.908)	(30.504)
Reading	6.228	8.959	18.564	3.392	-2.553	-14.082
	(18.633)	(19.764)	(21.419)	(23.260)	(27.553)	(34.500)
Science	16.580	17.040	18.002	15.769	13.669	7.646
	(19.553)	(22.626)	(26.049)	(24.241)	(28.202)	(32.892)
Observations	1,260	1,027	777	2,015	1,645	1,237

Notes: The reported estimated treatment effects stem from separate estimations of different specifications. All regressions include dummy variables for month of birth, year and country fixed effects, a dummy variable for all children born after June, interaction effects between year and the 'post June' dummy, year and country, country and 'post June'. The control variables on parental background include dummy variables for father's educational attainment, school location, and migration background of the family. Robust standard errors in parentheses (clustered by school track (more/less academic), school location, and gender). Estimations weighted by individual inverse probability weights provided in the PISA data set. *** p<0.01, ** p<0.05, * p<0.1. Source: PISA data set (OECD), own calculations.

5 Conclusions

This thesis has empirically investigated three distinct research questions in the fields of subjective well-being and human capital formation. The main objective was to shed light on the causal effect of specific determinants of job and life satisfaction as well as of cognitive skills. To this end, each of the three chapters took advantage of specific exogenous events which allowed the implementation of quasi-experimental research designs.

The first research question of the thesis (Chapter 2) posed the question whether the observed positive gap in job satisfaction between public and private sector employees in Post-Soviet Ukraine reflect rents by public sector workers or whether this alleged satisfaction premium is simply caused by a non-random self-selection of inherently 'happy types' into this sector. To solve this question the empirical analysis implemented a new identification strategy using instrumental variable techniques taking advantage of the rich retrospective information provided in the Ukrainian Longitudinal Monitoring Survey (ULMS). Based on the quasi-natural experiment of the large-scale mass privatization accompanying the transition process, industry and industry-region specific privatization probabilities were constructed and assigned to individuals based on their pre-determined Soviet industry(-region) affiliations. Since private ownership was not allowed during the Soviet Union, since choices with respect to jobs as well as place of living were limited at that time and since the incentive and remuneration system differed substantially from typical market economies, it seems plausible that there was no self-selection of particular personality types according to whether a job would become a state or a private sector job only later. The findings based on this empirical strategy show that the observed and significantly positive public sector satisfaction premium indeed reflect rents enjoyed by public sector workers in Ukraine. In other words, the higher job satisfaction in the state sector is driven by genuine differences in average job characteristics between the two sectors (implying causality). The analysis also reveals that part of these genuine differences in jobs can be attributed to social and financial fringe benefits which are much more prevalent in the public than in the private sector. However, after correcting for self-selection and accounting for various job (dis)amenities and fringe benefits, there remains a sizable satisfaction premium the source of which needs to be addressed in future research.

The second empirical study (Chapter 3) was devoted to the analysis of the long-term effects of the 1986 nuclear disaster of Chernobyl on life satisfaction and mental health in Ukraine in the years 2003 to 2007. The explosion of one block of the nuclear power plant in Chernobyl on April 26, 1986 caused radioactive fallout and contamination in regions scattered across Ukraine, as well as Belarus and Russia. The severity of the accident is reflected in the fact that, wind and rainfall caused a dispersion of radiation also in more distant areas of Northern and Western Europe. Due to the unforeseeable and unexpected nature of the disaster as well as the uneven geographic spread of the radioactive fallout in 1986 this study has argued that the catastrophe affected a random part of the Ukrainian population and thus represents a 'natural experiment'. To assess the long-run effects of the nuclear accident the study employed a self-reported measure of 'being affected by the catastrophe' as well as objective measures of 1986 regional effective exposure doses which were assigned to survey respondents according to their place of living in 1986. The empirical results of both types of measures based on the ULMS data as well as another large Ukrainian micro data set (repeated cross-sections of the Ukrainian Household Budget Survey, 2004-2008) reveal that there is a significantly negative causal effect of the Chernobyl catastrophe on subjective well-being as well as mental health even after 17 to 21 years. These results are robust to instrumental variable regressions (2SLS) in which the self-reported measures are instrumented with the objective radiation data as well as to several sensitivity checks. In fact, the 2SLS estimates suggest that the naive regressions using the potentially endogenous self-reported measures of 'affectedness' represent a lower bound estimate of the true underlying effect. These lower bound estimates were used to calculate the corresponding average individual compensating income differential which amounts to the tremendous sum of around ten percent of Ukraine's GDP. In addition, the empirical analysis also assessed whether the negative subjective well-being and mental health effects triggered behavioural reactions by respondents. Indeed, the findings provide evidence that persons who were affected by the Chernobyl catastrophe report a lower subjective life expectancy and are less likely to engage in risky health behaviour (smoking). To study fatalistic or precautionary behavioural responses in greater detail is left for future research.

The third research question which was assessed in Chapter 4 asked to what extent a generous paid and job-protected parental leave system has a positive or negative spill-over effect on cognitive outcomes of children at age 15. The empirical challenge of such an analysis is to overcome the potential endogeneity problem related to the actual duration of the mother's parental leave (parents are free to choose the time when a mother or a father returns to work after childbirth). The identification strategy to solve this problem was to exploit an Austrian reform which increased the officially granted paid and job protected parental leave period from 12 to 24 months for all mothers whose children were born on or after the reform cut-off date July 1, 1990. This cut-off date generated an 'as good as random' allocation of mothers and children into treatment (post-reform, maximum lengths of 24 months of paid and job protected parental leave) and control group (pre-reform, maximum parental leave duration of 12 months). To account for possible seasonality and age effects, the estimation strategy followed a Difference-in-Difference (DID) approach using an additional pre-reform cohort of children born in 1987. Using information on the children's birth months to identify maternal eligibility to the more generous parental leave mandates, the resulting estimates represent reduced form or intention-to-treat effects. The results of the DID estimates of the 12 months parental leave extension on standardized PISA test scores vary across subgroups. Using the *full sample* there are no statistically significant medium-run effects of the more generous parental leave regime on cognitive skills. This finding is in line with the results of most of the studies using changes in parental leave legislation to identify the causal effect of early maternal employment on child outcomes (in particular, Canada, Denmark, Germany and Sweden). When dividing the sample according to maternal labour force participation rates (proxied by educational attainment) the results suggest that there is a significantly positive effect of the parental leave extension for children (especially boys) of mothers with higher labour force participation rates (who are also more likely to be affected by the reform). This result proves robust to all sensitivity checks except for a test excluding the pre-reform month of June. Against this background and given that the estimated effect only represent net reduced form effects not allowing to identify any particular transmission channels, it seems too early to derive any conclusive policy suggestions. Given that most of the other evaluation studies face the same problem as they lack information on actual leave taking or time spent on child care, it remains a challenging task for future research to pin down and to analyse the actual mechanisms through which parental leave and early maternal employment influence the development of cognitive and non-cognitive skills and to explore possible effect heterogeneity according to the gender of the child.

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