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Chapter 4: Accounting for pension wealth, the missing rich and under-coverage: A comprehensive wealth distribution for Germany

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Contents

1 Regional heterogeneity in the link between lifetime earnings and life expectancy

1.1 Introduction

Since reunification in 1989, life expectancy in Germany has increased significantly. According to the Federal Statistical Office (Statistisches Bundesamt, 2023), average remaining life expectancy at the age of 65 increased from 14.3 years in 1991-1993 to 17.6 years in 2020-2022 for men and from 18.0 years to 20.9 years for women. However, the likelihood of living a long life is not equal for all individuals and rather depends on their personal circumstances as there are clear mortality differentials by socioeconomic status (see, e.g., Von Gaudecker and Scholz, 2007; Haan et al., 2020) and and across geographical areas (see, e.g., Kibele, 2012; van Raalte et al., 2020; Rau and Schmertmann, 2020).

Many studies from across the globe convincingly show that higher income is associated with a longer lifespan (e.g., Cutler et al., 2006; Kinge et al., 2019; Dahl et al., 2021). A growing body of recent studies, moreover, shows that it is not just an individual's socioeconomic status that matters for when they die, but also the place where they live (e.g., Chetty et al., 2016; Deryugina and Molitor, 2021; Finkelstein et al., 2021). However, the interaction between an individual's socioeconomic status and their place of residence in the context of their importance for life expectancy is only poorly understood to date. For example, only very limited evidence (Chetty et al., 2016) exists on the degree to which the link between income and life expectancy varies between different geographical regions within a country. Furthermore, while a number of existing studies aim to identify place characteristics that are associated with longevity (e.g., Latzitis et al., 2011; Dwyer-Lindgren et al., 2017; Rau and Schmertmann, 2020; Finkelstein et al., 2021), there is a clear shortage of literature on the question of whether place health effects are heterogeneous for individuals of different socioeconomic status, as recently stressed by Deryugina and Molitor (2021). In addition, to the best of my knowledge, no study to date investigates whether place health effects and their interaction with socioeconomic status change over time.

I contribute to these underexplored research topics in the following ways. Using administrative data from the German Pension Insurance, I estimate remaining life expectancy at age 65 by lifetime earnings quintile[1] and geographic area (NUTS2).[2] This allows me, for the first time for Germany, to analyze regional differentials in life expectancy of individuals with similar (lifetime) earnings and consequently, to examine the degree of geographical and temporal variation in area-specific longevity gaps between individuals at the top and the bottom of the income distribution. Subsequently, I use these earnings- and region-specific life expectancy estimates together with a rich set of place characteristics obtained by combining different data sources (see Subsection 1.8.2 for an overview) to conduct a correlational analysis to investigate which place factors are associated with longevity. Specifically, I examine whether place matters differently for individuals' life expectancy depending

[1] Lifetime earnings refer to individuals' accumulated earnings over their entire career. Therefore, lifetime earnings can be seen as a more comprehensive earnings measure than, for example, cross-sectional annual earnings as they are more closely linked to individuals' life chances (see, e.g., Corneo, 2015; Tamborini et al., 2015).

[2] The NUTS classification (Nomenclature of territorial units for statistics) is a geographical system dividing the European Union into hierarchical levels for statistical purposes. In Germany, the NUTS2 level usually mirror the governmental regions referred to as "Regierungsbezirke". My study analyzes all 30 West German NUTS2 regions.

on their socioeconomic status and whether this interaction between place factors, socioeconomic status and life expectancy has changed over time.

I provide first evidence for substantial heterogeneity in the relationship between lifetime earnings and life expectancy across NUTS2 regions in Germany. My results suggest a general trend of increasing area-specific longevity gaps over time. Furthermore, regional inequality in longevity gaps across different areas declined over time. Place factors associated with longevity are better healthcare supply, lower air pollution levels, lower regional poverty levels and a higher prevalence of healthy behaviors (less smoking, lower obesity rates, higher exercise rates and healthier diet). However, there is no clear evidence for heterogeneity in place factors associated with longevity by socioeconomic status: place characteristics do not seem to influence the longevity of individuals at the top and the bottom of the lifetime earnings distribution in different directions or magnitudes. Additionally, I find suggestive evidence for some weak time trends regarding place factors associated with longevity for individuals at the bottom of the lifetime earnings distribution but not for individuals at the top. It appears possible that place used to matter more in Germany for individuals with low socioeconomic status but that over time the importance of place for their health has declined.

My study contributes to three strands of literature: first, the literature on mortality differentials by socioeconomic status and geographic area; second, the literature on place factors associated with longevity; and third, the literature on the heterogeneity of place health effects by socioeconomic status.

The study closest to mine is the paper by Chetty et al. (2016)) which, to the best of my knowledge, is the only other study estimating life expectancy differentiated by income and region. They provide evidence for substantial regional variation in the relationship between socioeconomic status and life expectancy in the United States (U.S.). With the exception of Montez et al. (2019) - who estimates life expectancy by educational level and region -, other related existing studies either estimate life expectancy by different socioeconomic status but not by geographical area (e.g., Kinge et al., 2019; Haan et al., 2020) or they calculate life expectancy estimates for different regions without differentiating between individuals of different socioeconomic status (e.g., Dwyer-Lindgren et al., 2017; Rau and Schmertmann, 2020; Hrzic et al., 2023). Studies on the relationship between income and life expectancy point towards an international trend of rising inequality in life expectancy by income (e.g., Cristia, 2009; van Raalte et al., 2018; Dahl et al., 2021). For Germany, a small number of studies specifically find rising longevity differentials between individuals with high and low lifetime earnings using administrative data (e.g., Kibele et al., 2013; Haan et al., 2020; Wenau et al., 2019). Furthermore, a number of studies for different countries show evidence for varying life expectancies across regions, for example Dwyer-Lindgren et al. (2017) for the U.S., Rashid et al. (2021) for the U.K., Bonnet and d'Albis (2020) for France and Janssen et al. (2016) for the Netherlands. Several existing studies for Germany provide evidence for mortality differentials across districts (e.g., Latzitis et al., 2011; Kibele, 2012; Kibele et al., 2015; Rau and Schmertmann, 2020). Additionally, van Raalte et al. (2020) and Redler et al. (2021) provide evidence for decreasing regional variation in mortality in Germany over time. Van Raalte et al. (2020) also show that regional inequality in life expectancy in Germany is relatively low compared to other countries like the U.S., the U.K. or France.

A number of previous studies for Germany examine the link between regional characteristics and longevity (e.g., Latzitis et al., 2011; Kibele, 2012; Rau and Schmertmann, 2020; Hrzic et al., 2023).Place factors in these existing studies mainly consist of healthcare indicators (e.g. number of physicians, hospital density), economic indicators (e.g. unemployment rate, average household income) and social conditions (e.g. share of welfare recipients, education, voter turnout). I expand on these analyses by further including direct measures for regional inequality, air pollution and health-related behaviors which have emerged as the standard in recent cutting-edge studies for the U.S. (Chetty et al., 2016; Dwyer-Lindgren et al., 2017; Deryugina and Molitor, 2021; Finkelstein et al., 2021).

Generally, studies on the heterogeneity of place factors associated with longevity by socioeconomic status are scarce. For Germany, only two studies to date have touched on this topic in a non-comprehensive manner. Both Kibele (2012) and Kibele (2014) find some evidence that the mortality risk of high income individuals is less affected by regional effects than it is the case for individuals at the bottom of the income distribution. However, in contrast to my study, these two papers only analyze one single regional factor Kibele (2014) or summarize regional characteristics into one deprivation index Kibele (2012). The few more closely related recent studies focus primarily on the U.S. (Chetty et al., 2016; Montez et al., 2019; Finkelstein et al., 2021) and find that geographic variation in life expectancy or mortality risk is higher for individuals of lower socioeconomic status, suggesting that place matters more for this group. According to Chetty et al. (2016), correlations between regional life expectancy and some place characteristics (e.g. access to healthcare) are significantly different between high-income and low-income individuals.

The paper is structured as follows: Section 1.2 describes the institutional setting and the data. In Section 1.3, I explain the methodological approach for estimating period- and region-specific life expectancy. Section 1.4 first presents evidence for heterogeneity in life expectancy differentiated by lifetime earnings across NUTS2 regions in Germany and then investigates whether place matters differently for individuals' life expectancy depending on their socioeconomic status and whether this interaction between place factors, socioeconomic status and life expectancy changed over time. Section 1.5 offers a discussion of the results. Section 1.6 presents limitations and potential extensions of this paper. Section 1.7 concludes the paper.

1.2 Institutional setting and data

1.2.1 The German public pension system

Since pension entitlements are subsequently used as a proxy for lifetime earnings, this subsection provides a brief overview over the key characteristics of the German public pension system.[3]

Most employees in Germany are mandatorily covered by the German Public Pension System, which is a pay-as-you-go system.[4] Over their career, individuals contribute a certain percentage of their annual gross earnings to accumulate pension entitlements which are called "earnings points". Earnings points essentially refer to an individual's relative position in the annual earnings distribution: if an individual's annual earnings are equal to the average annual earnings, then they earn exactly one earnings point for this specific year. If they earn 50% more than the average annual earnings, they acquire 1.5 earnings points. 50% less than the average annual earnings will bring them 0.5 earnings points. However, maximum annual pension

[3]For a more detailed description of the German public pension system see for example Rürup (2002) and Von Gaudecker and Scholz (2007).

[4]By law, civil servants are excluded from the German public pension system. In addition, most self-employed individuals are not obliged to contribute. Members of some occupations (e.g. architects, lawyers and physicians) also have the right to pay into their own pension funds instead.

contributions are capped at around twice the amount of average annual earnings, which means that an individual can only earn a maximum of around two earnings points in a single year.[5] After accumulating earnings points over their work life, retired individuals receive gross monthly pension payments which are equal to their number of accrued earnings point multiplied with the applicable pension value.[6] There are some options for early retirement before the regular retirement age of 65 for individuals of birth cohorts included in my study; however, in general, pension payments are proportional to pension contributions over the life cycle.[7] This means that accumulated pension entitlements are directly related to individual earnings over the life cycle and are therefore a commonly used proxy for lifetime earnings (see, e.g., Wenau et al., 2019; Haan et al., 2020).

1.2.2 Pension data

Information on lifetime earnings and mortality was obtained from high-quality administrative data provided by the German Pension Insurance. Specifically, I use the SK90 dataset covering all mandatorily covered German pensioners, which is available for the calendar years 1992-2015. This dataset contains information on pensioners' age, sex, place of residence (NUTS2), individual pension entitlements and year of death.[8] I focus on life expectancy at age 65 and only include individuals

[5] In 2015, the last calendar year available in my data, the threshold for pensionable earnings was 72,600 euros in western Germany, which referred to 2.07 earnings points.

[6] The pension value for each earnings point accumulated in western Germany in 2015 was 29.21 euros. As a result, individuals who retired with 50 earnings points, for example, received monthly pension payments of 1460.50 euros in 2015.

[7] Engels et al. (2017) and Lüthen (2016) provide an in-depth explanation of regulations and deductions for early retirement.

[8] Specifically, the data offers information on whether an individual's pension payment was terminated in the 12 months before the year's reporting date (November 30th). However, the only reason for an old-age pension to be terminated is an individual's death.

aged 65 or over for two reasons: comparability to other studies[9] and avoiding selection problems resulting from varying retirement ages, for example in the case of the early retirement of disabled individuals. At age 65 this potential selection problem does not occur since nearly all individuals are retired.[10] The restricted dataset includes individuals born between 1905 and 1949.

Similarly to related earlier studies (e.g., Von Gaudecker and Scholz, 2007; Kibele et al., 2013; Wenau et al., 2019), I solely focus on men. Unfortunately, the data does not allow for the investigation of the relationship between lifetime earnings and mortality for women. This is mainly the due to institutional reasons as, until 1967, women were legally allowed to leave the pension system when they married and collect the monetary value of their accumulated pension entitlements. Until 1995, they were able to re-join the pension system at a later point in life with the help of retroactive payments of contribution. In such cases, women´s pension entitlements are no longer closely related to their employment biographies and lifetime earnings. Furthermore, the SK90 dataset does not contain information on whether women re-entered the pension system or not. Additionally, because of the low female labor market participation rates for the cohorts analyzed in this study, female earnings points in general do not adequately reflect women´s socioeconomic status and their life chances.

Moreover, in my main analysis, I focus on West German men only. The reason for this limitation is that East Germans aged 65 years or over in the years between

[9] Related studies which also measure remaining life expectancy at age 65 are for example Kibele et al. (2013), Wenau et al. (2019) and Haan et al. (2020).

[10] During the years 1995-2015 the share of West German men retiring at age 66 or over only ranged between 1.3% (in 2008) and 3.4% (in 2010) of the total annual number of newly retired pensioners (Deutsche Rentenversicherung, 2022). Analysing already retired birth cohorts, Krickl and Hofmann (2013) show that only around 1.2% of individuals born between 1944 and 1946 retired at age 66 or older.

1995 and 2015 spent large parts of their working lives in the German Democratic Republic (GDR), meaning their earnings information, wage levels and therefore earnings points are not easily comparable to those of individuals from the same birth cohorts who lived and worked in Federal Republic of Germany (FDR).[11] Furthermore, the data does not cover earnings information for periods of self-employment or civil service, meaning that complete earnings biographies and, as such, lifetime earnings of self-employed individuals and civil servants are not available. In order to address this problem and obtain a representative sample of individuals with complete earnings information, I follow the approach of related studies (e.g., Von Gaudecker and Scholz, 2007; Wenau et al., 2019; Haan et al., 2020) and restrict the sample to individuals who accumulated at least 30 earnings points up to the age of 65. As a consequence, I automatically also drop individuals with very low lifetime earnings, which likely leads to a slight underestimation of the heterogeneity in life expectancy by lifetime earnings.[12]

In total, the restricted dataset offers around 65.04 million person-year observations for West German men. Among them are 3.31 million (5.09%) observed deaths.

1.3 Methodology

Methodologically, I closely follow the approach of Haan et al. (2020) to estimate the remaining life expectancy at age 65 for West German men using pension data. However, my study has two key differences from their approach. Firstly, I conduct

[11] For a more detailed description on institutional differences and why accumulated earnings points in the GDR and FDR do not reflect lifetime earnings in exactly the same manner, see for example Von Gaudecker and Scholz (2007).

[12] According to Haan et al. (2020), 20% of the unrestricted data are self-employed individuals and civil servants, while poor individuals with fewer than 30 earnings points make up around 5%. Therefore, the restricted dataset represents around 75% of all West German men aged 65 or over.

the analysis on a regional level (NUTS2).[13] And secondly, I estimate and analyze period life expectancy instead of cohort life expectancy. In contrast to cohort life expectancy, which uses information on mortality of the same cohort over time, period life expectancy makes use of mortality of different cohorts in a given year or period. The life expectancy measure used in this study - remaining period life expectancy at age 65 - therefore refers to the average length of life left for a hypothetical individual aged 65 experiencing age-mortality rates for the rest of their life equal to those in the cross-section in a given period. Period life expectancy is frequently used in recent studies on trends in inequality in longevity (e.g., Chetty et al., 2016; Currie and Schwandt, 2016; Kinge et al., 2019; Dahl et al., 2021) because of its nature as a summary measure of cross-sectional mortality rates during a given period. Furthermore, the use of period life expectancy allows for easier and less assumption-based matching with yearly data on a variety of regional indicators (see Subsection 1.4.2) than would be the case for cohort life expectancy.

Individuals are assigned to lifetime earnings quintiles based on their accumulated earnings points relative to all other West German male pensioners of the same five-year age group (seven age groups from age 65-69 to age 95-99) in each calendar year.[14] By using ranks (quintiles) instead of levels of lifetime earnings I prevent my estimates being affected by censoring of lifetime earnings for individuals at the top of the earnings distribution.[15] Furthermore, I group the person-year observations

[13] I refrain from conducting my study on a more detailed geographic level (e.g. NUTS3-level) for two main reasons. Fist, this would result in lower numbers of observation per region leading to lower precision of my quintile-specific life expectancy estimates and secondly, for reasons of limited data availability.

[14] Following Chetty et al. (2016), I refrain from calculating region-specific quintile cut-off points. Otherwise, it would complicate the comparison of life expectancies by lifetime earnings quintiles across different regions with varying lifetime earnings distributions.

[15] Bönke et al. (2015) find evidence for a right-censoring of annual earnings information at the top of the distribution using pension data. On average, annually around 7% of the recorded earnings of

for the years 1992-2015 into eight three-year time periods as I estimate period life expectancy for time periods instead of single years. Otherwise, the number of observations for less populated NUTS2 regions would be too small to estimate precise (lifetime earnings) quintile-specific life expectancy. I use observations for the time periods 1992-94 and 1995-97 for the estimation to increase precision of the age-mortality rates estimates. However, since I do not have access to many of the regional indicators for Subsection 1.4.2 (e.g. healthcare supply indicators) for the years before 1998, I do not estimate and report mortality rates and consequently life expectancy for these time periods.

In the first step, I estimate period- and region-specific conditional age-mortality rates by lifetime earnings quintiles for all the ages between 65 and 99. In the second step, I use these age-mortality rates to calculate earnings- and period-specific life expectancy at the age of 65 at NUTS2 level.

The probability of death within the next year is estimated using a logistic regression model accounting for heterogeneous age effects which may differ by lifetime earnings quintiles. I allow for region-specific effects of age, periods and earnings as well as their interactions. Additionally, I control for regional period-specific fixed effects, lifetime earnings fixed effects and the fixed effects of period-earnings interactions. The respective log-odds can be expressed as follows:

$$log\frac{(Pr(death_{iatqr}|survival\ until\ age\ a)}{1-Pr(death_{iatqr}|survival\ until\ age\ a)} = \beta_0 + \sum_{p=1}^{4}\beta_{rp}\,a^p + \sum_{p=1}^{4}\beta_{rpq}\,a^p + \mu_{rt} + \eta_{rq} + \nu_{rtq} \tag{1.1}$$

West German men are affected. By showing a high persistence of earnings levels and ranks over the life cycle, they imply that lifetime earnings based on pension data are also censored.

with q refering to individuals‘ i lifetime earnings quintile of time period t and a to the age of the respective individual from region r .[16]

In the next step, I use the parameter estimates to predict conditional age-specific mortality rates. Finally, I calculate for every NUTS2 region separately remaining life expectancy at age 65 by periods and lifetime earnings quintiles using the following formula:

$$\sum_{z=65}^{99} \Pi_{a=65}^{z}[1 - Pr(death_{atqr})] \tag{1.2}$$

where $Pr(death_{atqr})$ is the region-specific age-mortality rate for period t and lifetime earnings quintile q. This approach follows the underlying assumption that individuals' maximum age is 100 and therefore their probability of death at age 100 is equal to 1.

As depicted by Figure 1.7 in the Appendix, the generated NUTS2 level life expectancy estimates (averaged across quintiles) are very highly correlated (r=0.96) with life expectancy from external data provided by the European Statistical Office (Eurostat). I conclude from this external validation that my region- and quintile-specific life expectancy estimates are a valid and reliable measure for mortality of West German men at NUTS2 level.

1.4 Results

This section provides the main results of my analysis. It is divided into three parts. In Subsection 1.4.1, I investigate geographical variation and time trends of NUTS2-

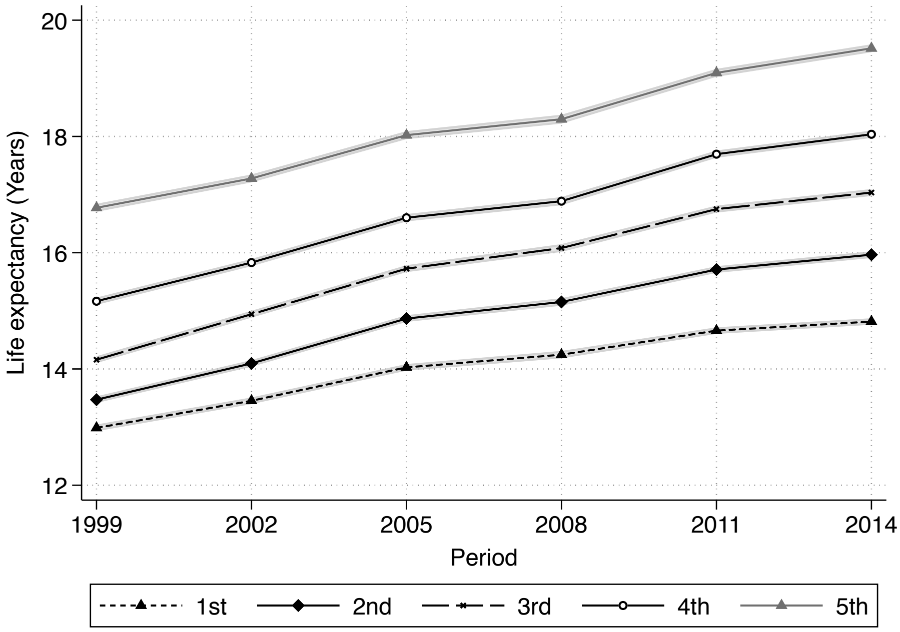
[16] I follow Haan et al. (2020) in their flexible specifications of age effects (4th order polynomial) for reasons of comparability and since they show that their model specification outperforms non-parametric models.

level life expectancy estimates. In Subsection 1.4.2, conducting a correlation analysis, I then analyze whether place matters differently for individuals' life expectancy depending on their socioeconomic status and whether this interaction between place factors, socioeconomic status and life expectancy changed over time. Finally, in Subsection 1.4.3 I further examine the robustness of my heterogeneity analysis using a regression and slope testing approach.

1.4.1 Regional variation in the link between lifetime earnings and life expectancy

There is an overall trend of rising average life expectancy for West German men over time, as Figure 1.1 shows. However, average life expectancy increased at different rates across the lifetime earnings distribution. Over the span of around 15 years, average improvements in absolute life expectancy were 1.8 years for the lifetime earnings poor (from 13.0 years to 14.8 years) and therefore lower than the increase of 2.7 years for the top lifetime earners (from 16.8 years to 19.5 years). As a result, the gap in average life expectancy between the bottom and top lifetime earnings quintile of West German male pensioners increased by 24% from 3.8 years in 1998-2000 to 4.7 years in the period 2013-2015.

However, there exists substantial heterogeneity in the relationship between lifetime earnings and life expectancy across regions in (West) Germany. Focusing on the time period 2013-2015, Figure 1.2 depicts life expectancy at age 65 both at the top and the bottom of the lifetime earnings distribution for the 30 West German NUTS2 regions. Life expectancy at the bottom of the distribution varied between

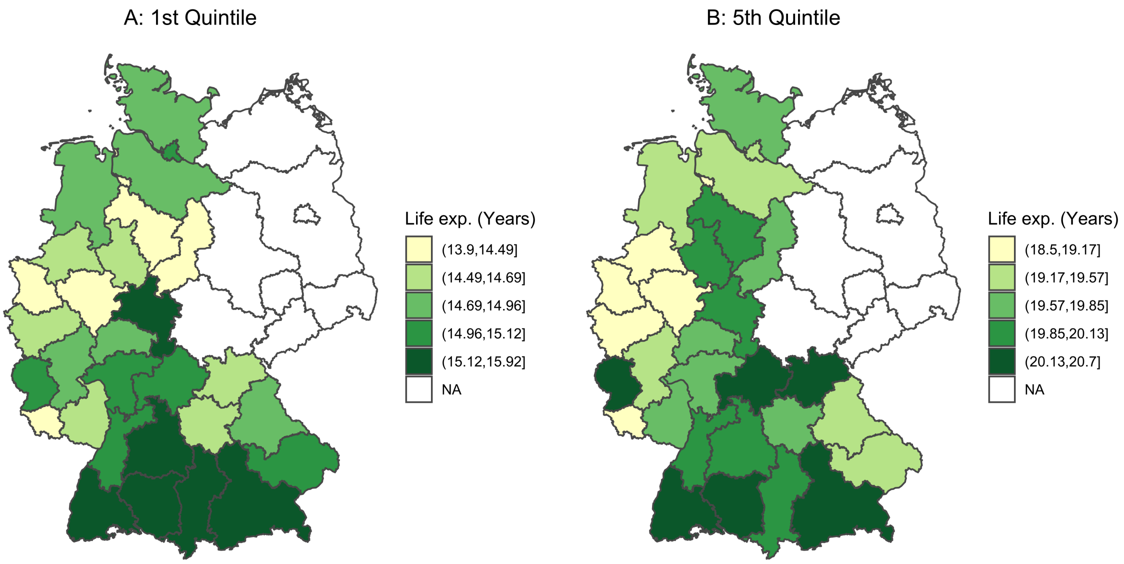


Notes: Life expectancy in remaining years at age 65. Grey areas indicate 95% confidence intervals estimated using the Delta method. *Source:* Own calculations based on SK90 data.

Figure 1.1: Life expectancy of West German men by periods and quintiles

13.9 years in Saarland and 15.9 years in Tübingen.[17] Among top lifetime earners, individuals in Bremen had the lowest life expectancy (18.5 years) while in Trier they were expected to live for around 20.7 additional years. Across all regions, the (population-weighted) standard deviation of life expectancy was 0.52 years for men at the bottom of the distribution versus 0.55 years for men in the top lifetime earnings quintile. This finding of a roughly equal variation across different regions in life expectancy for individuals in the bottom and top quintile is robust for all time periods apart from the years 1998-2000 where the variation is higher for individuals at the bottom of the lifetime earnings distribution. Another striking result is that there is a strong correlation (r=0.79) between the life expectancy of lifetime low earners and lifetime high earners in the same region and period of time.

[17]Table 1.2 in the Appendix lists life expectancies for all 30 NUTS2 regions for the 1st and the 5th quintile for each of the two time periods 1998-2000 and 2013-2015.

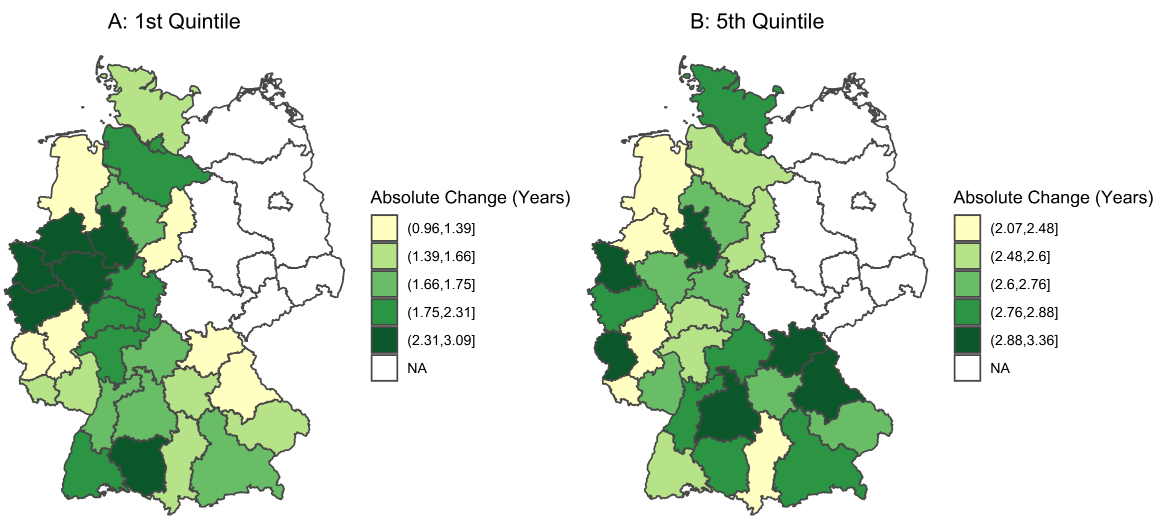


Notes: Life expectancy in remaining years at age 65. *Source:* Own calculations based on SK90 data.

Figure 1.2: Life expectancy by quintiles and NUTS2 regions, 2013-2015

Furthermore, Figure 1.2 suggests regional clustering of NUTS2 regions with regards to individuals' life expectancy by lifetime earnings. Four out of the eight NUTS2 regions with the lowest life expectancy for individuals in the 1st lifetime earnings quintile were clustered in the federal state of North Rhine-Westphalia, whereas the NUTS2 regions with the highest life expectancy for low-income individuals were located in Baden-Württemberg (three of the top five). Clustering for individuals in the top quintile looks fairly similar, with life expectancies within North Rhine-Westphalia ranking among the lowest while top lifetime earners in the federal states of Baden-Württemberg and Bavaria on average live longest (seven of the top ten regions are clustered in these two federal states). At the same time, my results show that life expectancies both at the top and the bottom of the lifetime earnings distribution can also vary substantially for NUTS2 regions within the same federal state. For example, in Bavaria, life expectancy varies between 19.5 years in Upper Palatinate ("Oberpfalz") and 20.3 years in Upper Bavaria ("Oberbayern") at the top and 14.6 years in Central Franconia ("Mittelfranken") and 15.8 years in Upper Bavaria at the bottom of the lifetime earnings distribution. In general, regions in the south of Germany tend to have higher life expectancies than regions in the north.

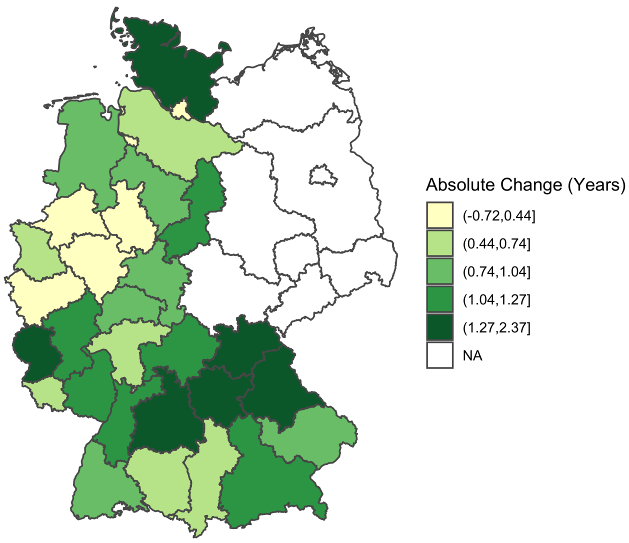
As depicted by Figure 1.8 in the Appendix, there is substantial regional variation in life expectancy differences between individuals in the 1st and the 5th lifetime earnings quintile. This longevity gap ranges from 4.1 years in Münster to 5.6 years in Trier and Upper Franconia ("Oberfranken") with a (population-weighted) standard deviation of 0.35 years. These results provide evidence for a substantial regional heterogeneity in the extent to which differences in individuals' socioeconomic status lead to different life expectancies.



Notes: Life expectancy in remaining years at age 65. The change in life expectancy refers to the absolute change (in years) between the periods 1998-2000 and 2013-2015. *Source:* Own calculations based on SK90 data.

Figure 1.3: Change in life expectancy by quintiles and NUTS2 regions, 1998-2000 vs. 2013-2015

Moreover, there is also geographic variation in life expectancy trends over time. Figure 1.3 maps the absolute change in life expectancy between the two time periods 1998-2000 and 2013-2015 by NUTS2 region for individuals at the top and bottom of the lifetime earnings distribution. Temporal trends in life expectancy varied more strongly in the 1st quintile (population-weighted standard deviation of 0.53), ranging between an increase of 0.96 years in Trier and 3.1 years in Münster.[18] Life expectancy improvements of top lifetime earners ranged between 2.1 years in Bremen and 3.4 years in Upper Palatinate, with a standard deviation of 0.25 years.



Notes: Life expectancy in remaining years at age 65. The longevity gap refers to the difference in life expectancy between individuals in the 1st and 5th quintiles of the respective NUTS2 region. The change in the longevity gap refers to the absolute change (in years) between the periods 1998-2000 and 2013-2015. *Source:* Own calculations based on SK90 data.

Figure 1.4: Change in the longevity gap by NUTS2 regions, 1998-2000 vs. 2013-2015

Similar to the national level (Figure 1.1), most NUTS2 regions follow the trend of increasing longevity gaps between individuals at the top and the bottom of the lifetime earnings distribution over time. In fact, this trend can be found for 29 of 30 NUTS2 regions in West Germany (Figure 1.4). The only exemption was Münster - the region with the highest increase in life expectancy for the bottom quintile - where the longevity gap decreased by 0.7 years. Among the 29 NUTS2 regions which experienced an increasing gap, the extent to which life expectancy

[18]Population weights for temporal trends refer to the average population weights of the two time periods 1998-2000 and 2013-2015.

between the lifetime earnings poor and rich diverged varied drastically across NUTS2 regions between 0.1 years in Arnsberg and 2.4 years in Upper Palatinate. Gaps in life expectancy between the bottom and top quintiles only increased slightly in regions in which individuals in the bottom quintile experienced the largest improvements in life expectancy. In contrast, regions which ranked highest regarding the life expectancy improvements for individuals in the top quintile tended to experience a strong increase in the longevity gap between the lifetime earnings poor and rich.[19] These temporal trends in NUTS2-level longevity gaps resulted in a decline in variation of those gaps across German NUTS2 regions over time. Specifically, the standard deviation decreased from 0.51 years in the period 1998-2000 to 0.35 years in 2013-2015.

1.4.2 Place factors associated with longevity

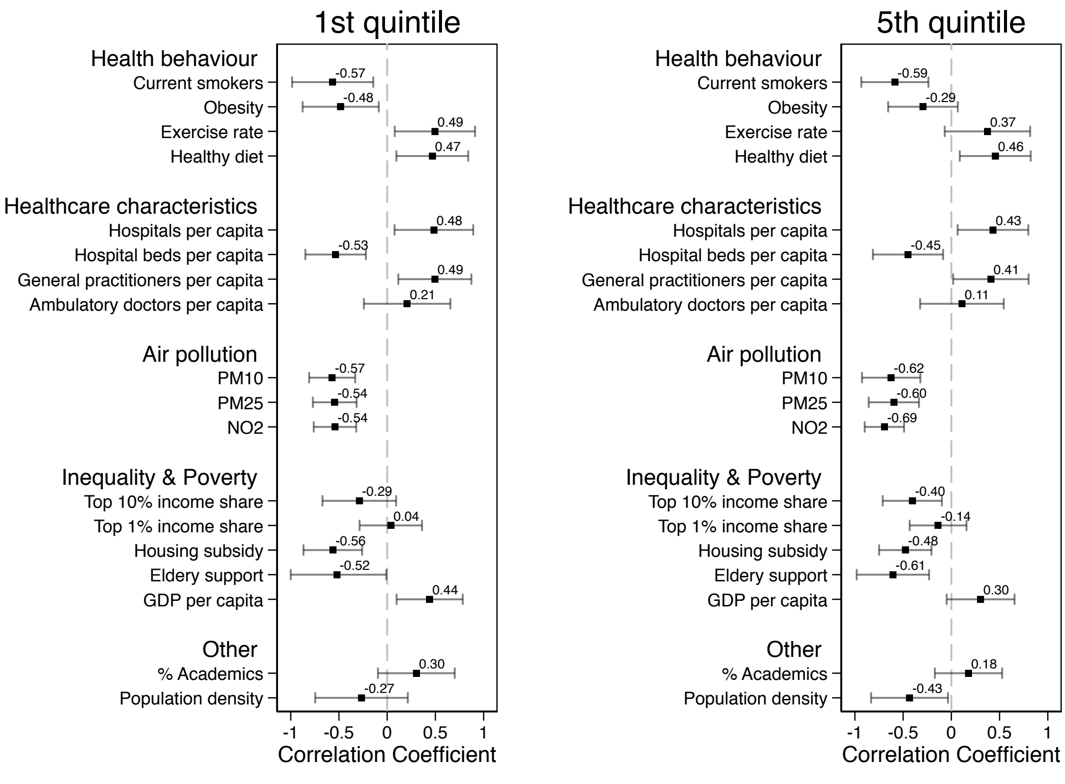
The results in Subsection 1.4.1 provide evidence for substantial heterogeneity in the relationship between lifetime earnings and life expectancy and how this relationship changed over time across NUTS2 regions in Germany. Against this background, my aim in this subsection is to identify factors related to this regional variation in the association between life expectancy and lifetime earnings and make an important contribution to our understanding of the heterogeneity of place factors associated with longevity for individuals of different socioeconomic status and how this interaction changed over time.

[19]My findings regarding temporal trends remain robust if I follow Chetty et al. (2016) in using the average period change in life expectancy between 1998-2000 and 2013-2015 instead of absolute changes. Figure 1.9 in the Appendix depicts the average period increase in life expectancy by lifetime earnings quintiles and shows that life expectancy of individuals in the 1st quintile increased on average by 0.13 years per period while the average per period increase for the 5th quintile was 0.18 years during the same period of time.

As pointed out by two recent influential studies (Deryugina and Molitor, 2021; Finkelstein et al., 2021), several empirical challenges exist for the identification of causal effects of the place of residence on longevity. One such challenge is, for example, non-random sorting of high-income individuals into areas with attractive living conditions like high-quality and quantity supply of healthcare or low pollution. As a result, it is no longer clear whether comparably high average longevity in such areas is a direct effect of favourable place characteristics or just the result of a high number of healthy, high-income individuals. Furthermore, non-random sorting itself can be the source of place effects due to peer influence on health-related behaviors, leading to regional differences in life expectancy as a result of both non-random sorting and the peer influences of individuals who live in this region. Other empirical challenges are due to potential unobserved confounders and reverse causality.

Finkelstein et al. (2021) only very recently presented the first empirical approach which is convincingly able to isolate the causal impacts of place characteristics on life expectancy by making use of a quasi-experimental design where they analyze movers coming from the same place who end up in different locations. However, following their approach with administrative data from the German Pension Insurance is not feasible. Due to strict data protection guidelines, it is not possible to follow individuals over time and therefore no panel structure is available that would allow for an investigation of whether individuals moved to another region. Instead, I follow Chetty et al. (2016) in their approach of conducting bivariate correlational analysis. As a result, my results cannot be interpreted as causal, but are rather of suggestive and descriptive nature. My main focus is to investigate the heterogeneity of place factors associated with life expectancy by socioeconomic status and whether the potential moderating influence of socioeconomic status for the link between life expectancy and place factors changed over time - two perspectives that so far have been vastly overlooked in the literature.

Following recent cutting-edge studies on the link between place characteristics and longevity (e.g., Chetty et al., 2016; Deryugina and Molitor, 2021; Finkelstein et al., 2021), I focus on the following place characteristics: health behaviors, healthcare, environment, inequality and poverty. I combine multiple different data sources to generate a comprehensive dataset on regional characteristics at NUTS2 level for the periods 1998-2000 and 2013-2015. Detailed definitions, data sources, and summary statistics for these characteristics are provided in Subsection 1.8.2.



Notes: Population-weighted Pearson correlations estimated between NUTS2 region place characteristics and remaining life expectancy at age 65. The error bars indicate 95% confidence intervals which are based on standard errors clustered by NUTS2 region. Detailed definitions of all variables can be found in Subsection 1.8.2. *Sources:* Life expectancies are based on own calculations using the SK90 dataset. The sources of the different place characteristics are presented in Subsection 1.8.2.

Figure 1.5: Correlations between life expectancy and place characteristics, 2013-2015

Figure 1.5 shows correlations of NUTS2-level life expectancy estimates for the 1st and 5th quintiles of the lifetime earnings distribution with regional area characteristics. The aim is to understand which factors are associated with longevity and whether there are differences for individuals at the top and the bottom of the distribution. I find evidence that, on average, longevity tends to be higher in areas where healthier behaviors are more prevalent. Specifically, the share of smokers at NUTS2 level is significantly negatively correlated with life expectancy - for both individuals at the top and the bottom of the lifetime earnings distribution. A region's obesity rate is also negatively correlated with regional life expectancy, although the correlation is only significant for low-income individuals. In addition, there is a strong positive and significant correlation (0.49) between regional exercise rates and the life expectancy of individuals with low lifetime earnings, while this correlation is somewhat smaller in size (0.37) and not statistically significant for individuals at the top of the distribution. Furthermore, a higher share of individuals following a healthy diet is positively correlated with longevity. In terms of regional healthcare supply, my results show a significant positive correlation for both general practitioner density and hospital density with life expectancy at age 65 for individuals in the 1st and 5th quintiles of the lifetime earnings distribution. Correlations for all ambulatory doctors per capita is positive but not significant. The correlation between life expectancy and the number of hospital beds per capita, however, does not have the expected sign: more hospital beds are associated with lower life expectancy. Interestingly, this correlation is also found by other studies (e.g., Deryugina and Molitor, 2021; Finkelstein et al., 2021) and hints at the limits of using correlational analysis when trying to identify place factors associated with longevity. In this case, for example, more hospital beds could be a response to poor health and therefore increased health care needs among residents. Furthermore, I find significant negative correlations between different types of air pollution and life expectancy. However, it does not seem to be the case that the impact for low-income individuals is significantly worse than for high-income individuals. Moreover, inequality was more negatively correlated for individuals in the top lifetime earnings quintile. Regional poverty indicators - namely, the number of individuals on housing subsidy and elderly support per 100 000 inhabitants - are negatively correlated with life expectancy for both individuals at the top and the bottom of the lifetime earnings distribution. I find evidence that GDP per capita is positively correlated with life expectancy. The correlation is higher and significantly positive for individuals at the bottom of the distribution while it is weaker and not statistically significant for the top lifetime earners. Other analyzed factors (population density and population share of academics) are not significantly correlated with life expectancy. These findings are essentially robust across the lifetime earnings distribution (see Figure 1.10 in the Appendix).

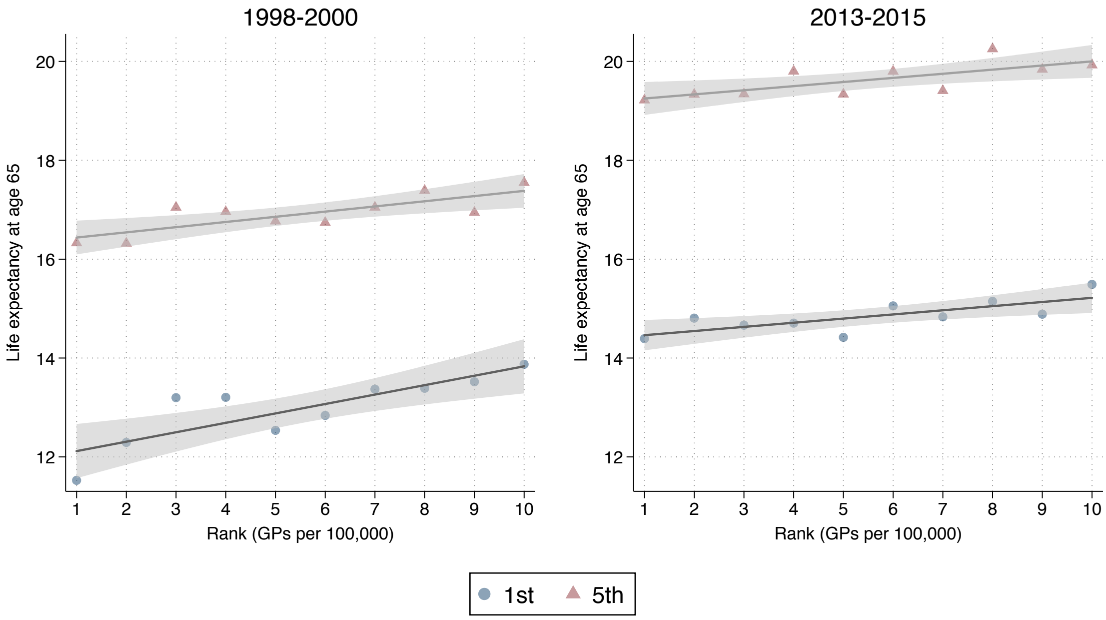
To explore whether these associations between place characteristics and life expectancy changed over time, I also analyzed correlations for the period 1998-2000 (see Figure 1.11 in the Appendix). There are no substantial differences with respect to the direction of the correlations or the heterogeneity between individuals at the top and the bottom of the distribution. However, it is striking that for the place characteristics related the ambulatory care (general practitioner density and ambulatory doctors) the correlations decreased in magnitude over time - specifically for the bottom quintile. The same holds true for the air pollution characteristics. Furthermore, correlations with smoking and a region's exercise rate are not significant in the period 1998-2000.

Finally, I also investigate correlations between changes in regional place characteristics and longevity gaps over time (Figure 1.12 in the Appendix). Of all the analyzed indicators, only changes over time in regional general practitioner density and NO_2 air pollution are significantly correlated with trends in regional longevity gaps. However, again, no conclusion regarding the direction of causality can be drawn from these results.

1.4.3 Robustness of heterogeneity analysis

In order to explore in further detail and verify the robustness of my heterogeneity analysis, I now present an alternative approach to look at the heterogeneity of place factors associated with longevity by socioeconomic status and potential changes over time. Following recent studies analysing the link between mortality and regional inequality in income (e.g., Currie and Schwandt, 2016; Redler et al., 2021), I implement a simple approach of bivariate regression analysis combined with slope testing. However, instead of solely focusing on inequality in income across different areas, I analyze a number of different place characteristics. First, for each period (1998-2000 and 2013-2015), I group the 30 NUTS2 regions into deciles according to, for example, their number of GPs, with the 1st decile referring to the three regions with the lowest number of GPs and the 10th decile containing the three regions with the highest GP density. Then, I calculate population-weighted average life expectancy by deciles and plot the ranks against the rank-specific average life expectancy.

Exemplarily, Figure 1.6 depicts the results for the number of GPs per 100 000 inhabitants. Regions with a higher number of general practitioners have on average higher life expectancy estimates both for individuals at the top and the bottom of the lifetime earnings distribution in 1998-2000 and 2013-2015. Table 1.1 reports



Notes: NUTS2 regions are grouped into deciles according to their number of general practitioners per capita. The grey areas around the fitted regression lines indicate the 95% confidence intervals. *Sources:* Life expectancies are based on own calculations using the SK90 dataset. Information on the number of GPs per NUTS2 region was obtained from the National Association of Statutory Health Insurance Physicians (KBV).

Figure 1.6: Life expectancy and general practitioners per capita by quintiles and periods

the slopes of the fitted regression lines (columns 1-4) and the p-values for the tests whether (1) slopes for the same quintile changed significantly over time (columns 5-6) and (2) slopes are significantly different for the 1st and 5th quintiles in the same period (columns 7-8). P-values higher than 0.05 mean that we cannot reject the null hypothesis of equal slopes, meaning that the association between a specific place characteristic and life expectancy for the 1st quintile did not significantly change over time or slopes between the 1st and 5th quintiles are not statistically different from each other suggesting no heterogeneity for individuals of different socioeconomic status.

A closer look at the p-values for the slope test for whether the association between life expectancy and place characteristics changed over time (columns 5 and 6) shows that p-values are considerably lower for the bottom quintile. Moreover, for two

Table 1.1: Life expectancy and place characteristics - slopes of regression lines

	Slopes				p-values			
	1998-2000		2013-2015		over time		1st vs. 5th	
Characteristics	**1st** (1)	**5th** (2)	**1st** (3)	**5th** (4)	$1st\Delta^{1999}_{2014}$ (5)	$5th\Delta^{1999}_{2014}$ (6)	$1999\Delta^{1st}_{5th}$ (7)	$2014\Delta^{1st}_{5th}$ (8)
Current Smokers	-0.051	-0.031	-0.103***	-0.102***	0.552	0.219	0.834	0.971
	(0.082)	(0.047)	(0.027)	(0.029)				
Obesity rate	-	-	-0.079*	-0.051*	-	-	-	0.537
			(0.036)	(0.024)				
Exercise rate	0.022	0.031	0.072	0.065*	0.542	0.489	0.908	0.905
	(0.067)	(0.035)	(0.043)	(0.033)				
Healthy diet	-	-	0.092***	0.103***	-	-	-	0.741
			(0.026)	(0.021)				
Number of hospitals	0.149**	0.100**	0.074	0.070	0.270	0.605	0.420	0.955
	(0.051)	(0.032)	(0.043)	(0.047)				
Hospital beds	-0.160**	-0.087*	-0.096***	-0.089***	0.373	0.978	0.352	0.831
	(0.065)	(0.038)	(0.024)	(0.025)				
General practitioners	0.191***	0.105***	0.084***	0.083**	0.053	0.587	0.122	0.989
	(0.045)	(0.028)	(0.025)	(0.027)				
Ambulatory doctors	0.102*	0.069*	0.028	0.033	0.252	0.474	0.600	0.934
	(0.052)	(0.035)	(0.034)	(0.035)				
PM_{10}	-0.191***	-0.095**	-0.073**	-0.112***	0.066	0.699	0.139	0.356
	(0.051)	(0.035)	(0.031)	(0.027)				
PM_{25}	-0.191***	-0.080**	-0.075*	-0.104***	0.054	0.605	0.058	0.546
	(0.042)	(0.035)	(0.037)	(0.030)				
NO_2	-0.201***	-0.122***	-0.075*	-0.127***	0.023	0.886	0.081	0.252
	(0.036)	(0.022)	(0.035)	(0.027)				
Top 10% income share	-0.162***	-0.078**	-0.068*	-0.064**	0.089	0.732	0.120	0.935
	(0.041)	(0.030)	(0.032)	(0.026)				
Top 1% income share	-0.018	0.012	0.008	-0.027	0.754	0.541	0.690	0.632
	(0.066)	(0.032)	(0.048)	(0.053)				
Housing subsidy	-	-	-0.079*	-0.084	-	-	-	0.933
			(0.037)	(0.047)				
Elderly support	-	-	-0.104**	-0.105**	-	-	-	0.983
			(0.035)	(0.038)				
GDP per capita	0.057	0.040	0.062	0.060	0.945	0.720	0.820	0.978
	(0.061)	(0.039)	(0.034)	(0.040)				
% Academics	-	-	0.006	0.010	-	-	-	0.939
			(0.038)	(0.039)				
Population density	-0.180***	-0.076*	-0.049	-0.062	0.020	0.807	0.070	0.813
	(0.037)	(0.039)	(0.034)	(0.040)				

Notes: Columns 1-4 report the slopes of the fitted regression lines for different periods and quintiles. Standard errors for regression coefficients are reported in parentheses. ***, ** and * denote significance at the 1, 5 and 10% levels respectively. Columns 5-6 show the p-values for the null hypothesis that the quintile-specific slopes are equal in both periods. Columns 7-8 report the p-values for the null hypothesis that the slopes are equal for both quinitiles. Detailed definitions of all variables can be found in Subsection 1.8.2. *Sources:* Life expectancies are based on own calculations using the SK90 dataset. The sources of the different place characteristics are presented in Subsection 1.8.2.

place characteristics (NO_2 air pollution and population density), I can even reject the null hypothesis of equal slopes in 1998-2000 and 2013-2015. For both of these characteristics the negative slope decreases significantly in magnitude over time.

For other indicators (e.g. PM_{10} and PM_{25} air pollution and number of GPs) the p-values are very close to the 0.05 threshold. Consequently, my results suggest some evidence for time trends in place factors associated with longevity for individuals at the bottom of the lifetime earnings distribution. For individuals at the top of the distribution no evidence for time trends can be found (column 6). High p-values in columns 7 and 8 suggest that there is no significant heterogeneity in place factors associated with longevity between individuals at the top and the bottom of the lifetime earnings distribution.

All in all, results from the slope testing approach are in line with the findings from the correlational analysis presented in Subsection 1.4.2.

1.5 Discussion

One of the main contributions of this study is to present first estimates of regional life expectancy at NUTS2 level for Germany by lifetime earnings quintiles. In the period 2013-2015, life expectancy varied substantially by 2.0 years between the regions with the highest and lowest life expectancy for individuals at the bottom and by 2.2 years for individuals at the top of the distribution. However, not only does inequality in life expectancy exist for individuals in the same lifetime earnings quintile across West German NUTS2 regions, but longevity gaps also occur for individuals within the same geographical area who are on different ends of the lifetime earnings distribution. My finding of increasing region-specific longevity gaps over time which are primarily driven by high increases in top lifetime earners' life expectancy is in line with the results for all West German men by Haan et al. (2020), who do not differentiate by region. For the U.S., Chetty et al. (2016) also find similar results when analysing the relationship between cross-sectional earnings and life expectancy. According to my results, while within-region longevity gaps are mostly rising, there is a decline in variation of those gaps across different NUTS2 regions over time. Other recent studies also show declining regional disparities in mortality rates (Redler et al., 2021) and life expectancy (van Raalte et al., 2020) across German regions.

The study by Chetty et al. (2016) for the U.S. is the only study to date that also analyzes geographical variation in life expectancy differentiated by earnings. While it is important to note that methodological differences in factors such as earnings measures (cross-sectional vs. lifetime earnings), observation periods, age of analyzed individuals, the area level of observation (30 German NUT2 regions vs. 595 U.S. commuting zones) and differences in our divisions of the earnings distribution (quintiles vs. quartiles) hamper direct comparisons between my results and the findings presented by Chetty et al. (2016), some key similarities and differences become clear. Similarly to my results for Germany, Chetty et al. (2016) find evidence for substantial variation in the relationship between socioeconomic status and life expectancy across U.S. geographical areas. Another similarity to my results for Germany is that they present evidence for geographical clustering of regions with the highest and lowest life expectancies both at the bottom and the top of the lifetime earnings distribution in the U.S. Furthermore, they also find that in the U.S. not just levels of life expectancy, but also temporal trends vary significantly across regions. In contrast to my findings, however, Chetty et al. (2016) show that, in the U.S., life expectancy across all commuting zones varies more strongly among individuals in the bottom income quartile than for individuals in the top income quartile. In contrast, my results for Germany show no difference in life expectancy variation between the 1st and 5th quintiles. Unfortunately, the above-mentioned methodological differences between my study and the study by Chetty et al. (2016) make it very difficult to assess whether the finding by van Raalte et al. (2020) that state-level inequality in overall life expectancy in the U.S. exceeds inequality in Germany also holds true when differentiating by (lifetime) earnings. It would be interesting to analyze more comprehensively how the regional variation I found for Germany - which I assessed as being substantial - actually holds up in international comparisons. To answer this question, more studies for different countries are needed on the question to what extent life expectancy by socioeconomic status varies geographically.

In general, my findings on which regional characteristics are associated with longevity fit in well with previous findings for Germany. For example, existing studies for Germany (e.g., Latzitis et al., 2011; Kibele, 2012; Rau and Schmertmann, 2020; Redler et al., 2021) also provide evidence for an association between high mortality and regional deprivation indicators (e.g. unemployment rate, share of welfare beneficiaries). Previous findings regarding the role of regional healthcare supply indicators for longevity are rather inconclusive. Latzitis et al. (2011) and Rau and Schmertmann (2020), for example, find no clear association between regional life expectancy and the number of physicians per 100 000 inhabitants. However, this discrepancy between their results and mine could potentially be due to differences in the studies' designs.[20] Furthermore, the significant negative correlations I find between the prevalence of unhealthy behaviors and life expectancy are in line with

[20]While I focus on old-age mortality (remaining life expectancy at age 65), Latzitis et al. (2011) and Rau and Schmertmann (2020) include individuals of all age groups for their life expectancy at birth estimates. Since utilization of healthcare services in Germany is particularly high for old individuals (see, e.g., Thode et al., 2005; Rattay et al., 2013) , it is not unreasonable to expect stronger correlations between healthcare supply indicators and life expectancy for individuals aged 65 or older. Other methodological differences are, for example, that they conduct district-level analyses and do not differentiate life expectancies by income.

existing studies showing a negative effect of lifestyle risk factors (smoking, drinking, obesity) on health and life expectancy in Germany (e.g., Li et al., 2014; Janssen et al., 2021). Similarly, the significant negative correlation between a NUTS2 region's level of air pollution and regional life expectancy in Germany is not surprising given the well-established harmful effects of air pollution on population health found by earlier studies (e.g., Cohen et al., 2017; Margaryan, 2021; Pestel and Wozny, 2021).

Strikingly, moreover, my findings on which place factors are linked to longevity for Germany fit considerably well with results from related studies for the U.S. Studies by Dwyer-Lindgren et al. (2017) and Deryugina and Molitor (2021) both present evidence for the importance of differences in lifestyle risk factors (smoking, obesity, lack of exercise), poverty indicators and physician density for explaining regional variation in life expectancy. Furthermore, in line with my results for Germany, Deryugina and Molitor (2021) suggest a negative link between regional air pollution and life expectancy. According to Chetty et al. (2016), health behaviors are the main source of regional variation in longevity. Other than that, they do not find strong correlations for other place factors such as access to medical care or regional environment. However, their indicators are different to the ones used in my study and other studies for the U.S.[21] Finkelstein et al. (2021) show that on average places with favourable life expectancy effects tend to have healthcare of higher quality and quantity, less air pollution and higher prevalence of healthier behaviors.

One of my key findings is the lack of heterogeneity in both direction and magnitude of the correlations between place factors and life expectancy for individuals at

[21] Compared to my study or other studies for the U.S. (e.g., Deryugina and Molitor, 2021; Finkelstein et al., 2021), Chetty et al. (2016) use different indicators for air pollution (seggregation instead of actual air pollution data) and healthcare supply (access to healthcare indicators such as percentage share of the uninsured instead of physician and hospital density indicators) which could potentially explain some of the differences in the correlations.

the top and the bottom of the lifetime earnings distribution in Germany. Initially, this result appears at least somewhat surprising given the fact that a large body of existing international studies suggests socioeconomic gradients for factors such as health-related behaviors (e.g., Lantz et al., 1998; Cutler et al., 2006; Pampel et al., 2010), the utilization of healthcare (e.g., d'Uva and Jones, 2009; Godøy and Huitfeldt, 2020; Lueckmann et al., 2021) or exposure to air pollution (e.g., O'Neill et al., 2003; Neidell, 2004; Hajat et al., 2015; Boing et al., 2022). Additionally, both Kibele (2012) and Kibele (2014) find some evidence that the mortality risk of high income individuals is less affected by regional context than is the case for individuals at the bottom of the income distribution - however, due to methodological differences their results are only comparable to mine to a limited extent.[22] Furthermore, in contrast to my findings for Germany, previous studies for the U.S. (Chetty et al., 2016; Finkelstein et al., 2021) have found that geographic variation in life expectancy is higher for low-income individuals, suggesting that place matters more for this group. Indeed, according to Chetty et al. (2016), while some important correlates of life expectancy are very similar for individuals at the top and the bottom of the income distribution—such as smoking, exercise, and obesity rates—, correlations with other place characteristics (e.g. access to healthcare, social capital, share of immigrants, local government expenditure) are significantly different for these two groups. Similarly, Montez et al. (2019) provide suggestive evidence that place effects are heterogeneous with respect to individuals of different socioeconomic backgrounds by showing that

[22]For example, differences exist regarding the observational period, with Kibele (2012) conducting a pooled analysis for the years 1998, 2001 and 2004 and Kibele (2014) for the years between 2002-2004, while I report more recent results up to 2015. Furthermore, I estimate area- and lifetime earnings-specific life expectancies while Kibele (2012) and Kibele (2014) analyze individual-level mortality risks. Moreover, while I analyze the moderating influence of socioeconomic status for the link between life expectancy and a variety of particular place characteristics, Kibele (2012) pool together different place factors into one single regional context score while Kibele (2014) only focuses on one specific regional factor (unemployment rate).

there is little variation in state-level life expectancy for individuals with at least one year of college while variation is substantially larger for those without a high school degree. Although they focus on education instead of income as a way of determining individuals' socioeconomic status, their results are still relevant as a comparison to my findings for Germany due to the strong link between education and lifetime earnings (see, e.g., Bönke et al., 2015; Tamborini et al., 2015; Bhuller et al., 2017). As an explanation for the socioeconomic heterogeneity in importance of place for life expectancy, Montez et al. (2017) and Montez et al. (2019) argue that in the U.S. higher education seems to act as a personal "firewall" against regional circumstances. One potential reason as to why in the U.S. place factors seem to matter more strongly for the life expectancy of individuals of low socioeconomic status is the comparably generous German welfare state and universal healthcare system. This may provide a better safety net for individuals at the bottom of the earnings distribution, acting as their „firewall" against regional factors in a similar fashion to higher education in the U.S.

Finally, I explore whether the associations between place characteristics and life expectancy changed over time - separately for individuals of different socioeconomic status. To the best of my knowledge, no other study has explicitly focused on this question before. For individuals at the top of the lifetime earnings distribution no evidence for changes over time is found. For individuals at the bottom, the results are ambiguous. For some characteristics, clearly no changes occurred, while for others there is suggestive evidence for some time trends (e.g. air pollution and population density). Specifically, it appears possible that in earlier time periods - similar to what we can see in the U.S. today (Chetty et al., 2016; Finkelstein et al., 2021) - place used to matter more in Germany for individuals with low socioeconomic status. However, over time, the importance of regional context for life expectancy in Germany has declined. This idea is supported by my finding that regional variation in life expectancy was higher for individuals at the bottom than for individuals at the top of the lifetime earnings distribution in the period 1998-2000 and roughly equal between these groups for the time periods thereafter. Furthermore, such a development over time would explain the divergence of my findings for the period 2013-2015 from those of Kibele (2012) and Kibele (2014) - who find that place matters more for individuals at the bottom of the distribution in their analyses for the years between 1998 and 2004. However, further research on this topic is necessary in order to draw major conclusions; my results should rather be seen as a starting point and guide for future work on whether place health effects and their interaction with socioeconomic status change over time.

1.6 Qualifications and extensions

This section reflects on limitations regarding the data and methods used. Furthermore, it discusses potential extensions of this paper and further directions for future studies building on this work.

First, it is important to note that my study contains a number of data-driven limitations. For example, for reasons explained in detail in Subsection 1.2.2, I am not able to include women and East German individuals in my analysis. As a result, since the administrative data from the German Pension Insurance does not contain information on self-employed individuals and civil servants, my sample comprises a very homogeneous group of West German men who regularly contributed to the public pension system. In order to obtain a consistent sample of individuals with complete earnings biographies, I restrict my sample to individuals who accumulated at least 30 earnings points. This is an established approach (see, e.g., Von Gaudecker and Scholz, 2007; Wenau et al., 2019; Haan et al., 2020) to exclude individuals with long periods of self-employment and civil service, who are not directly identifiable in the data available to me. Although further investigations on that matter by Haan et al. (2020) suggest that the overall effect of this restriction when it comes to excluding individuals at the bottom of the earnings distribution is only limited, it definitely excludes individuals with very low lifetime earnings.[23] Consequently, it is likely that the true extent of inequality in lifetime earnings is underestimated in my study due to underrepresentation of the bottom of the lifetime earnings distribution. Another reason why the lifetime earnings poor might be not covered comprehensively is that I only include individuals aged 65 or over to avoid selection problems resulting from varying retirement ages. My findings and the ones of earlier studies (e.g., Von Gaudecker and Scholz, 2007; Haan et al., 2020) on differential mortality by lifetime earnings suggest, however, that on average poorer individuals face a higher risk of death before the age of 65 than individuals who are higher up in the lifetime earnings distribution. Therefore, it is likely that a proportionally higher share of individuals who would end up in the bottom of the lifetime earnings distribution at the age of 65 die before reaching that age than is the case for individuals with higher (lifetime) earnings. Additionally, it is important to stress that my findings regarding place factors associated with longevity cannot be interpreted as causal but are rather of suggestive and descriptive nature. The administrative dataset used in this study only offers a very limited amount of individual-level information and

[23]According to Haan et al. (2020), restricting the sample to individuals with at least 30 earnings points reduces the sample by around 25%. Around 20% are self-employed individuals and civil servants, while poor individuals with fewer than 30 earnings points make up around 5%. Furthermore, Haan et al. (2020) show in a robustness test that changing the sample restriction to include individuals with at least 20 earnings points does not substantially alter their results.

does not follow a panel data structure due to its lack of an individual identifier variable. As a consequence, empirical problems such as selection bias due to selective sorting into regions and reverse causality cannot be addressed sufficiently. Against this background, I follow other related studies facing similar limitations (e.g., Chetty et al., 2016; Rau and Schmertmann, 2020) in explicitly relying on rather descriptive and simple approaches (correlation analysis and bivariate regression with slope testing).[24] Future studies aiming to identify causal place health effects for Germany could attempt to replicate the quasi-experimental approach presented by Finkelstein et al. (2021) for the U.S., where they compare individuals who used to live in the same area but later moved to different destination regions. In this context, a promising novel data source for Germany is the SOEP-RV linkage project (Lüthen et al., 2022), which combines both the advantages of complete earnings biographies from pension records and a rich set of individual-level socioeconomic information (e.g. health status, health-related behaviors, information on relocation). Additionally, as the German equivalent of the U.S. Medicare claims data used by Finkelstein et al. (2021), claims data from German statutory health insurance providers might be used for causal analysis of place health effects. One could argue that another limitation of this study is the geographical level analyzed. NUTS2 regions in (West) Germany are rather heterogeneous regarding their area size (420 km^2 in Bremen vs. 17,529 km^2 in Upper Bavaria) and their population size (536,722 in Trier vs. 5,197,679 in Düsseldorf in the year 2021). It is to be expected that place characteristics vary quite strongly, especially within some of the larger regions and that a lot of potential regional variation is therefore lost when analysing NUTS2

[24] I refrain from using multivariate regression approaches as they also cannot sufficiently address empirical issues (selection bias, reverse causality, collinearity). Against this background, they are dubbed as "naïve regression approaches" in the recent influential study by Deryugina and Molitor (2021).

regions (30 in West Germany) in comparison to NUTS3 regions (324 districts in West Germany). For example, living conditions such as exposure to air pollution or access to healthcare services can vary strongly between urban and rural areas within the same NUTS2 region. Consideration of lower geographical areas (e.g. districts or municipalities) would allow for more detailed analysis of actual living conditions. However, there are two main reasons why I refrain from conducting my study on a lower geographical level. First, the 95% confidence intervals of my life expectancy estimates (differentiated by NUTS2 region and lifetime earnings quintile) are already considerably wide (see Table 1.2 in the Appendix). If I were to conduct my analysis for my limited sample (West German men aged 65 or above who regularly contributed to the public pension system) and on a lower regional level (e.g. at the district level), the precision of my life expectancy estimates differentiated by lifetime earnings quintiles would be reduced drastically as a result of lower numbers of observation per region. As a consequence, no reliable conclusions on longevity gaps between individuals at the top and the bottom of the lifetime earnings distribution within the same region or variation in life expectancies across different regions could be drawn. The second reason is - to the best of my knowledge - the lack of (openly accessible) data availability for some place characteristics at a lower regional level - especially for earlier time periods (e.g. ambulatory physicians, air pollution).

Another shortcoming of the administrative pension data used is that it only contains information on the current place of residence but offers no information on where people used to live in the past and on the time of relocation. Thus, I cannot rule out the possibility that some individuals spent the majority of their lives living in different regions (where they were exposed to different place characteristics) from the ones I can attribute to them. While there is some general evidence for an increased likelihood for residential changes around the age of retirement in Germany (see, e.g., Winke, 2017; Friedrich and Ringel, 2019), the extent to which individuals in my specific sample relocate remains unclear. Only having access to information on the place of residence at old-age (after retirement) makes it impossible to examine potential place health effects associated with different places of residence at different periods of life. For example, a number of existing studies show that children growing up in more deprived areas end up to having worse health outcomes as adults compared with individuals who grew up in better-off communities (e.g., Hayward and Gorman, 2004; Lippert et al., 2017; Reuben et al., 2020). Moreover, factors linked to life expectancy could well be different for individuals of different ages, as recently also pointed out by Deryugina and Molitor (2021) and Finkelstein et al. (2021). In this context, future studies could also expand on my work and investigate whether the moderating role of socioeconomic status for the interaction between place characteristics and life expectancy is more pronounced for younger individuals than it appears to be the case for older individuals.

Future studies could also aim to examine an even more comprehensive set of place characteristics than the one investigated in my study. For example, it would be interesting to investigate whether there is variation across different areas regarding health norms or health literacy, which could cause differences in the utilization of preventative care (e.g. cancer screenings). On a similar note, Deryugina and Molitor (2021) highlight the potential importance of regional peer effects for influencing individuals' health-related behaviors. While one could argue that some of my indicators (e.g. share of smokers, share of individuals following a healthy diet) might reflect this matter, to my knowledge no direct measures on health norms or health literacy are (openly) available for different German regions in high quality and for long periods of time to date.

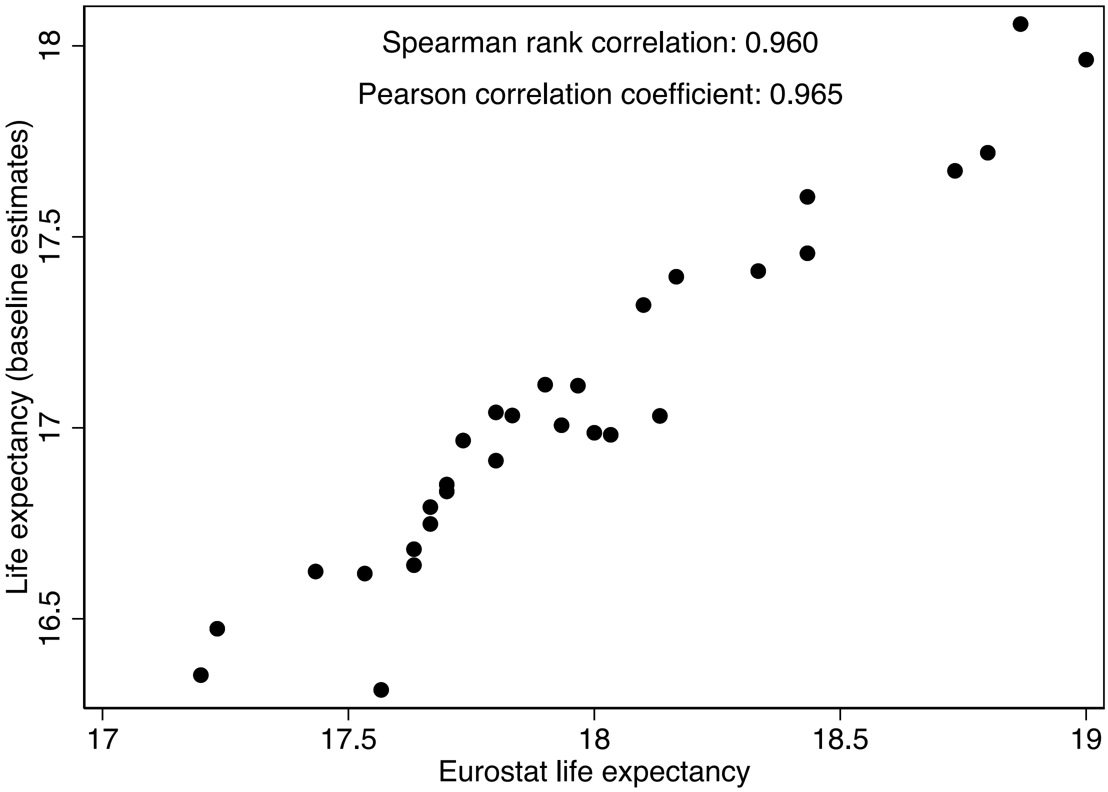
1.7 Conclusion

In this paper, I provide first evidence for substantial geographical variation in life expectancy differentiated by lifetime earnings across German NUTS2 regions. My results suggest a general trend of increasing area-specific longevity gaps over time which is primarily driven by high increases in top lifetime earners' life expectancy. Over the same time period, there is a decline in variation of those longevity gaps across different NUTS2 regions over time. Additionally, my findings point towards regional clustering of areas with particularly high or low life expectancy both for individuals in the 1st and the 5th quintiles of the lifetime earnings distribution. According to my analysis, place factors associated with longevity are better healthcare supply, lower air pollution, lower regional poverty and a higher prevalence of healthy behaviors (less smoking, lower obesity rates, higher exercise rates and healthier diet). However, correlations between place characteristics and life expectancy do not seem differ between individuals at the top and the bottom of the lifetime earnings distribution both in terms of directions or magnitudes. Additionally, I find suggestive evidence for some weak time trends regarding place factors associated with longevity for individuals at the bottom of the lifetime earnings distribution but not for individuals at the top. Against this background, it appears possible that place used to matter more in Germany for individuals with low socioeconomic status but over time the importance of regional context for their health has declined.

My findings are of importance for the ongoing public discussion about indexing the age of retirement to increases in overall life expectancy. Given the substantial heterogeneity in life expectancy by lifetime earnings and across different regions, linking the age of retirement to increases in average life expectancy could further increase inequalities between individuals of different socioeconomic status and regions. To tackle the redistributive problem of high earners or individuals from regions with relatively high life expectancy receiving their pensions for a longer period of time, a policy of increasing the age of retirement would have to take into account individuals' lifetime earnings and the place of residence. Furthermore, my study suggests that broad policies aiming to, for example, improve healthcare supply or air pollution automatically might not be enough to decrease inequality in life expectancy between the income poor and rich given the homogeneity of magnitude and direction in the correlations between place factors and their respective life expectancies. Moreover, caution is advised when drawing policy implications from my results on place factors associated with longevity since they are of descriptive and suggestive nature rather than causal. Therefore, there is a strong need for future research to focus on causal identification of place health effects for Germany.

1.8 Appendix

1.8.1 Additional tables and figures



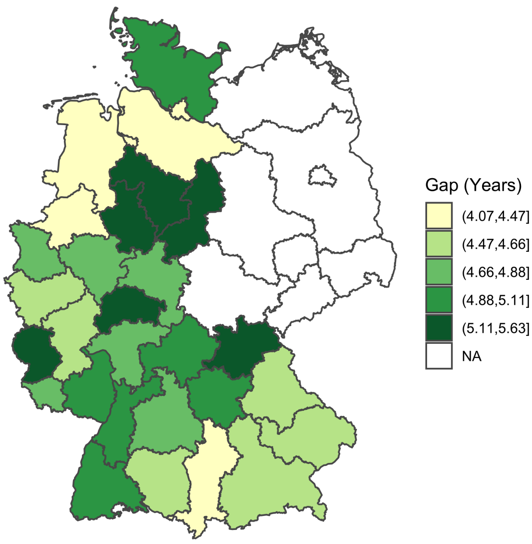
Notes: Eurostat life expectancy estimates are not available by lifetime earnings quintile. Therefore, for reasons of comparability, I use my quintile-specific estimates and calculate population-weighted average (across quintiles) life expectancy at age 65 for each NUTS2 region for the period 2013-2015. In the next step, I conduct correlational analysis using remaining life expectancy estimates at age 65 for West German men provided by the European Statistical Office (Eurostat) for the years 2013-2015. The fact that civil servants and self-employed individuals are not included in my analysis due to data limitations (as explained in further detail in Subsection 1.2.2) could potentially explain why my life expectancy estimates are systematically lower than the Eurostat estimates, which are representative for the entire population of West German men aged 65. Indeed, Haan and Schaller (2021) show that civil servants and self-employed individuals have a comparably high life expectancy. *Sources:* Baseline life expectancies are based on own calculations using the SK90 dataset. Furthermore, I use life expectancy estimates provided by the European Statistical Office (Eurostat).

Figure 1.7: Life expectancy correlations with Eurostat data

Table 1.2: Life expectancy at age 65 by NUTS2 region and period

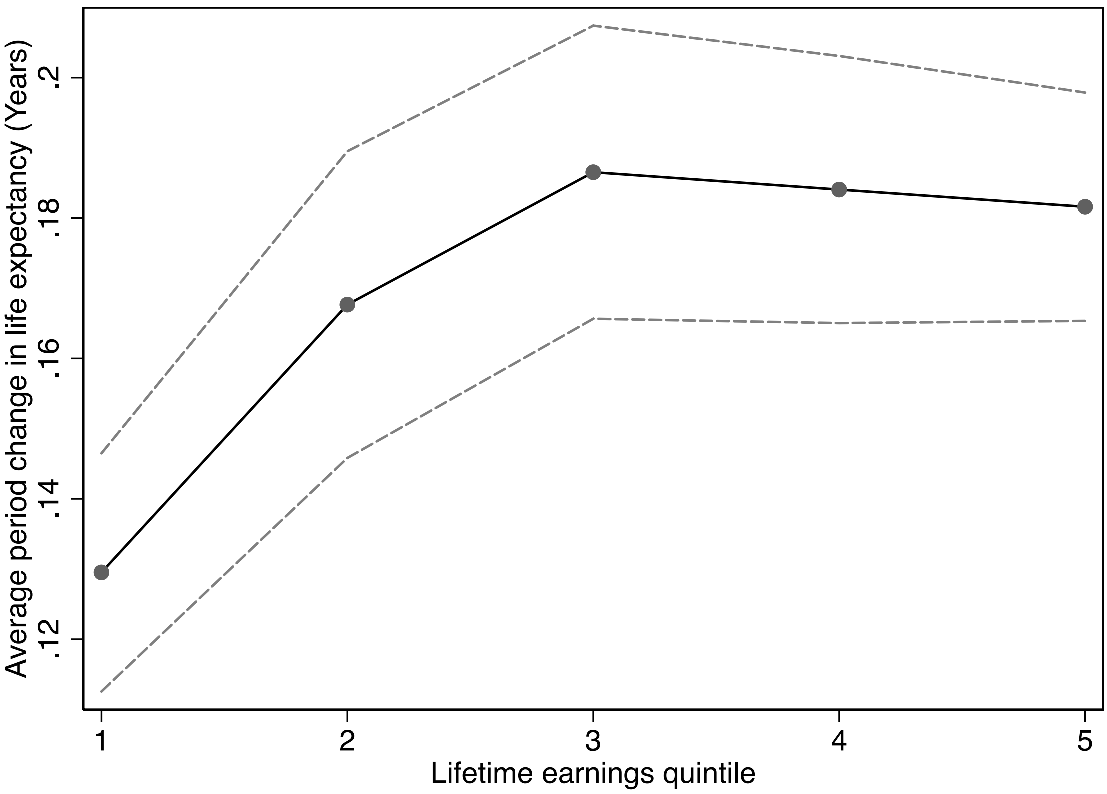
	1998-2000		2013-2015	
NUTS2 region	1st quintile (1)	5th quintile (2)	1st quintile (3)	5th quintile (4)
Schleswig-Holstein	13.2 (13.0, 13.5)	16.8 (16.5, 17.1)	14.7 (14.4, 15.0)	19.6 (19.3, 19.9)
Hamburg	12.7 (12.3, 13.1)	16.7 (16.4, 17.0)	15.0 (14.6, 15.4)	19.2 (18.9, 19.6)
Braunschweig	13.0 (12.6, 13.3)	17.0 (16.6, 17.5)	14.3 (14.0, 14.6)	19.6 (19.1, 20.0)
Hannover	12.7 (12.4, 13.0)	17.2 (16.8, 17.6)	14.4 (14.1, 14.7)	19.9 (19.5, 20.2)
Lüneburg	12.9 (12.6, 13.3)	16.7 (16.2, 17.2)	14.7 (14.4, 15.1)	19.2 (18.7, 19.6)
Weser-Ems	13.5 (13.3, 13.8)	17.0 (16.5, 17.5)	14.9 (14.6, 15.1)	19.3 (18.9, 19.8)
Bremen	12.5 (11.9, 13.1)	16.5 (15.9, 17.0)	14.2 (13.6, 14.8)	18.5 (17.9, 19.1)
Düsseldorf	11.6 (11.4, 11.9)	16.0 (15.8, 16.2)	14.1 (13.9, 14.3)	18.9 (18.7, 19.1)
Köln	12.0 (11.8, 12.2)	16.2 (16.0, 16.5)	14.5 (14.3, 14.8)	19.1 (18.9, 19.3)
Münster	11.4 (11.1, 11.7)	16.2 (15.9, 16.6)	14.5 (14.2, 14.8)	18.6 (18.3, 18.9)
Detmold	11.9 (11.6, 12.2)	16.9 (16.5, 17.4)	14.7 (14.4, 15.0)	19.9 (19.5, 20.4)
Arnsberg	11.4 (11.2, 11.7)	16.1 (15.9, 16.4)	14.1 (13.8, 14.3)	18.9 (18.6, 19.1)
Darmstadt	13.0 (12.7, 13.3)	17.2 (16.9, 17.4)	15.0 (14.7, 15.3)	19.7 (19.4, 19.9)
Gießen	12.9 (12.5, 13.3)	17.3 (16.6, 18.0)	14.7 (14.3, 15.1)	19.8 (19.2, 20.5)
Kassel	13.5 (13.1, 13.8)	17.4 (16.8, 18.0)	15.2 (14.9, 15.6)	20.1 (19.5, 20.7)
Koblenz	13.9 (13.6, 14.3)	17.2 (16.6, 17.7)	14.9 (14.6, 15.3)	19.5 (18.9, 20.0)
Trier	14.1 (13.6, 14.6)	17.7 (16.4, 19.0)	15.0 (14.5, 15.5)	20.7 (19.5, 21.9)
Rheinhessen-Pfalz	13.0 (12.7, 13.3)	16.9 (16.5, 17.3)	14.7 (14.3, 15.0)	19.6 (19.2, 20.0)
Stuttgart	13.6 (13.3, 13.9)	17.0 (16.7, 17.2)	15.3 (15.1, 15.6)	20.0 (19.7, 20.2)
Karlsruhe	13.3 (13.0, 13.6)	17.2 (16.9, 17.5)	15.0 (14.7, 15.3)	20.0 (19.7, 20.3)
Freiburg	13.6 (13.3, 13.9)	17.9 (17.4, 18.3)	15.4 (15.1, 15.7)	20.4 (20.0, 20.8)
Tübingen	13.6 (13.2, 13.9)	17.7 (17.3, 18.2)	15.9 (15.5, 16.3)	20.6 (20.1, 21.0)
Oberbayern	14.1 (13.9, 14.4)	17.5 (17.2, 17.8)	15.8 (15.6, 16.1)	20.3 (20.0, 20.6)
Niederbayern	13.5 (13.1, 13.8)	17.0 (16.1, 17.8)	15.0 (14.7, 15.4)	19.6 (18.8, 20.4)
Oberpfalz	13.9 (13.5, 14.3)	16.1 (15.3, 17.0)	14.9 (14.5, 15.2)	19.5 (18.7, 20.4)
Oberfranken	13.3 (12.9, 13.6)	17.0 (16.3, 17.7)	14.6 (14.3, 15.0)	20.2 (19.5, 20.9)
Mittelfranken	13.2 (12.9, 13.6)	17.1 (16.6, 17.5)	14.6 (14.3, 14.9)	19.7 (19.3, 20.2)
Unterfranken	13.4 (13.1, 13.7)	17.4 (16.8, 18.0)	15.1 (14.8, 15.4)	20.2 (19.6, 20.8)
Schwaben	14.0 (13.6, 14.4)	17.5 (17.0, 18.1)	15.6 (15.3, 16.0)	19.9 (19.4, 20.4)
Saarland	12.3 (11.9, 12.8)	16.6 (16.1, 17.1)	13.9 (13.5, 14.4)	18.8 (18.3, 19.3)

Notes: Life expectancies in remaining years at age 65 are shown for each NUTS2 region for the 1st and 5th quintile for the two time periods 1998-2000 and 2013-2015. 95% confidence intervals are shown in parentheses. *Sources:* Life expectancies are based on own calculations using the SK90 dataset.



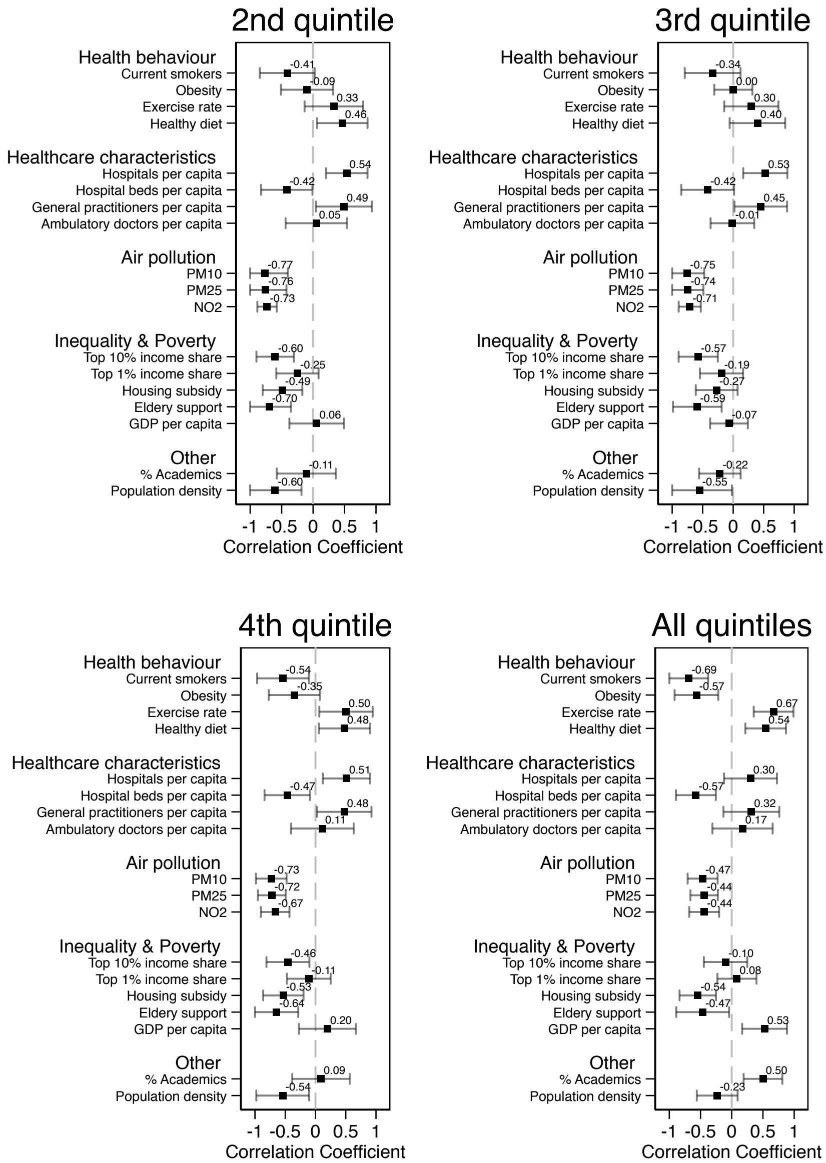
Notes: Life expectancy in remaining years at age 65. The longevity gap refers to the difference in life expectancy between individuals in the 1st and 5th quintiles of the respective NUTS2 region. *Source:* Own calculations based on SK90 data.

Figure 1.8: Longevity gap by NUTS2 regions, 2013-2015



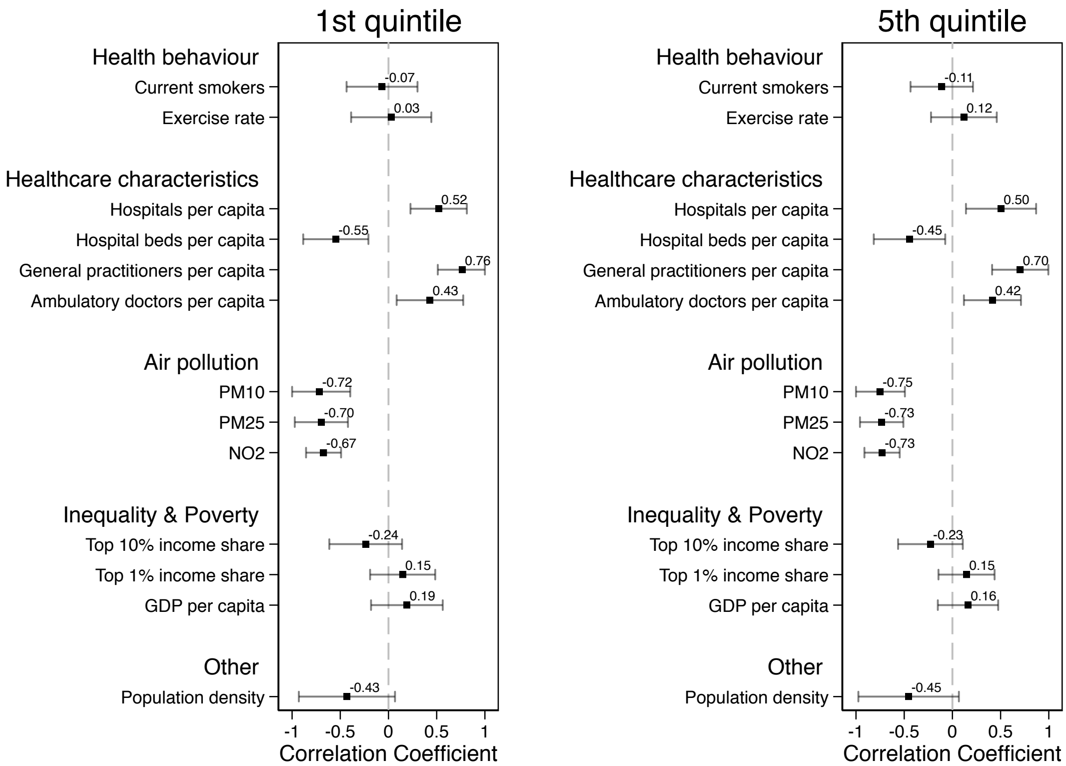
Notes: Average period change estimated using an OLS regression of life expectancy (remaining years at age 65) on time periods by lifetime earnings quintile. The dashed lines show the 95% confidence intervals. *Source:* Own calculations using the SK90 dataset.

Figure 1.9: Average period change in life expectancy between 1998-2000 and 2013-2015



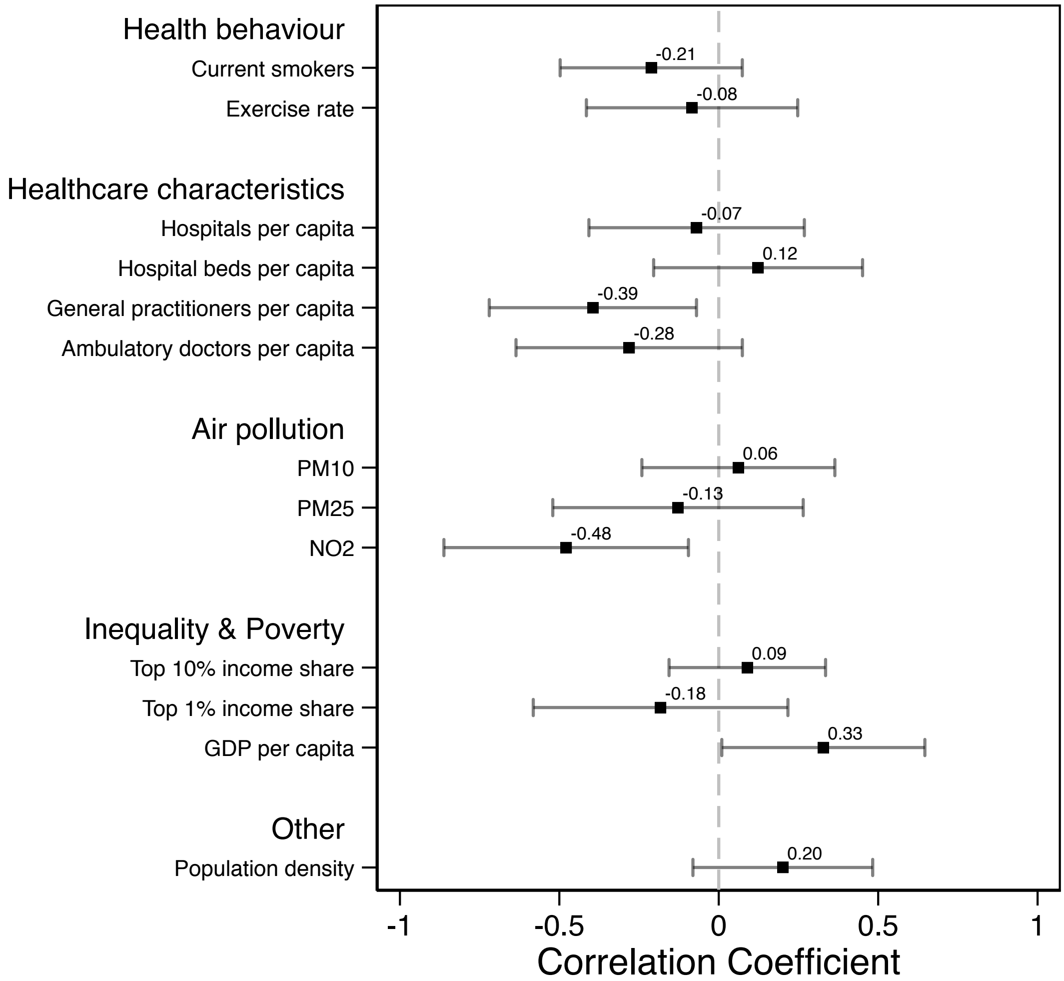
Notes: Population-weighted Pearson correlations estimated between NUTS2 region place characteristics and remaining life expectancy at age 65. The error bars indicate 95% confidence intervals which are based on standard errors clustered by NUTS2 region. "All quintiles" refers to the population-weighted average (across quintiles) life expectancy at age 65 for each NUTS2 region for the period 2013-2015. Detailed definitions of all variables can be found in Subsection 1.8.2. *Sources:* Life expectancies are based on own calculations using the SK90 dataset. The sources of the different place characteristics are presented in Subsection 1.8.2.

Figure 1.10: Correlations between life expectancy and place characteristics across the distribution, 2013-2015



Notes: Population-weighted Pearson correlations estimated between NUTS2 region place characteristics and remaining life expectancy at age 65. The error bars indicate 95% confidence intervals which are based on standard errors clustered by NUTS2 region. Detailed definitions of all variables can be found in Subsection 1.8.2. *Sources:* Life expectancies are based on own calculations using the SK90 dataset. The sources of the different place characteristics are presented in Subsection 1.8.2.

Figure 1.11: Correlations between life expectancy and place characteristics, 1998-2000



Notes: Population-weighted Pearson correlations between a NUTS2 regions' relative change (in percent) in place characteristics and absolute change (in years) in remaining life expectancy at age 65 between the periods 1998-2000 and 2013-2015. The error bars indicate 95% confidence intervals which are based on standard errors clustered by NUTS2 region. Detailed definitions of all variables can be found in Subsection 1.8.2. *Sources:* Life expectancies are based on own calculations using the SK90 dataset. The sources of the different place characteristics are presented in Subsection 1.8.2.

Figure 1.12: Correlations between change in life expectancy and change in place characteristics, 1998-2000 vs. 2013-2015

1.8.2 Data and definitions of place characteristics

In this subsection, I describe the data sources and the definitions of the place characteristics which I use for correlational analysis (Figure 1.5, Figure 1.11 (Appendix), and Figure 1.12 (Appendix)) and slope testing analysis (Figure 1.6 and Table 1.1). In general, I match life expectancy estimates for a specific period with place characteristics of the median year for the respective period. Specifically, life expectancy estimates for the period 1998-2000 are correlated with place characteristics in 1999 and for the period 2013-2015 I use place characteristics for the year 2014. I present summary statistics for all of these measures in Table 1.3.

Healthcare characteristics

For this study, I compiled a novel dataset on regional healthcare supply infrastructure in Germany by combining data from different sources which is not openly available and prepared on request specifically for this study. These sources are namely the hospital statistics of the German Federal Statistical Office and statistical information from the federal registry of physicians ("Bundesarztregister") provided by the National Association of Statutory Health Insurance Physicians (KBV, "Kassenärztliche Bundesvereinigung").The key strength of this generated dataset is that it contains information on a wide range of health supply indicators on NUTS2 level over a long period of time for both inpatient (1995-2015) and outpatient care (1999-2015) in Germany.

Hospital indicators

NUTS2-level data on German hospital infrastructure indicators for the years 1995-2015 was obtained from a special evaluation of the hospital statistics of the German

Federal Statistical Office. The survey underlying the hospital statistic is conducted on a yearly basis and includes all German hospitals. Specifically, I use information on the **total number of hospitals** and **hospital beds**. For reasons of comparability between regions of different population size, I express the above-mentioned indicators in units per 100,000 inhabitants.

Ambulatory care indicators

Data on ambulatory healthcare infrastructure was obtained from the National Association of Statutory Health Insurance Physicians (KBV). On request, the KBV provided me with statistical information on their federal registry of physicians on NUTS2 level for the years 1999-2015. The federal registry of physicians contains information on all physicians and psychotherapists who are recognised service providers under statutory health insurance (SHI-physicians) and who practice ambulatory care. There is no official statistic on the number of ambulatory physicians who are not SHI-physicians. However, a good proxy is the difference in the number of ambulatory physicians reported by the German Medical Association (150,106 in the year 2015) and the number SHI-physicians stated in the federal registry of physicians (144,769 in 2015). According to these numbers, in 2015 the federal registry of physicians covered around 96% of all ambulatory physicians. In order to avoid counting twice physicians who are potentially both part of the hospital statistics and the federal registry of physicians, I exclude authorised doctors. Authorised doctors ("ermächtigte Ärzte") are hospital doctors who are authorised to provide ambulatory care and are recognised service providers under statutory health insurance. Additionally, the data also provides information on the number of general practitioners ("GPs").

Finally, the **number of general practitioners** and the **total number of ambulatory doctors** are expressed in units per 100,000 inhabitants.

Health behavior characteristics

Data on health behavior is obtained from the Socio-Economic Panel (SOEP). The SOEP is a longitudinal representative household survey questioning around 30,000 respondents annually (Goebel et al., 2019). It includes a rich set of socioeconomic characteristics such as information on an individual's place of residence (NUTS2 level) as well as on their health-related behaviors. I use weighted data using SOEP weighting factors. Specifically, I use SOEP data to obtain the following indicators:

- **Current smokers:** In the years 1998, 1999, 2001 and every two years since 2002, individuals were asked whether or not they smoke cigarettes, pipes or cigars. Based on this information, I generate a dummy variable indicating whether a person is currently a smoker or not. In my analysis I do not distinguish between cigarette, pipe and cigar smokers. Finally, a smoking prevalence variable is constructed which indicates the fraction (in percent) of surveyed individuals who currently smoke in each NUTS2 region.

- **Obesity rate:** Since 2002, an individual's body-mass-index (BMI) is calculated in the SOEP every two years using questions on an individual's height and body weight. Using this BMI information, I construct a dichotomous variable indicating whether an individual is obese or not. According to the official definition of the World Health Organisation (World Health Organization, 2022), an individual is considered obese if they have a BMI of 30 or higher. Finally, I calculate the fraction (in percent) of surveyed individuals who are obese in each NUTS2 region.

- **Exercise rate:** Respondents in the SOEP are asked how often they play sports, exercise, walk or swim. Based on their answers, individuals are grouped into four categories: *"Almost never or never", "Several times a year", "At least once a month" and "At least once a week"*. I generate a dummy variable indicating whether a respondent exercises at least once a month or not. In the next step, I compute the fraction (in percent) of surveyed individuals who exercise at least once a month in each NUTS2 region. Since for the period 2013-2015, information is not available in 2014 but only in 2013 and 2015, I use the average exercise rates of these two available years instead.

- **Healthy diet rate:** Information on the share of individuals interested in maintaining a healthy diet is derived from the following question which was included in the SOEP in 2010, 2012 and 2014: "How much attention do you pay to maintaining a healthy diet?". The four available answers were: *"A lot"; "Some", "A little" and "None"*. In the first step, I generate a dummy variable indicating whether an individual pays "a lot" or at least "some" attention to maintaining a healthy diet or not. In the next step, I use this newly generated dummy variable to calculate the share (in percent) of surveyed individuals who are paying attention to maintaining a healthy diet for each NUTS2 region.

Air pollution

Air pollution data is based on the EU Joint Research Commission's (JRC) EDGAR v5 database (Crippa et al., 2020). The data is aggregated to NUTS2 regions by Naqvi (2021) by mapping the information on different emission types (0.1×0.1 degree grid-level data) on to NUTS2 boundaries for Germany for the years 1995-2015. In my study, I include information on **three pollutants** (PM_{10}, PM_{25} and NO_2) which are associated with severe negative health impacts (see, e.g., Deryugina et al., 2019; Margaryan, 2021). In order to compare regions of different size, I calculate emissions in tons per square kilometre.

Inequality and Poverty

Inequality indicators

Information on income inequality (**Top 1 % and Top 10 % income share**) is provided by the "The German Regional Inequality Database (GRID), 1895-2018" (Bartels, 2023). For the period 1998-2000 I have to use information for 1998 instead of 1999 because, as of 2022, the dataset does not cover the year 1999. For the years following 1998, data is available every three years between 1998 and 2013 and then annually since 2013. Originally, data is provided at the district level. I derive data for NUTS2 regions by calculating population-weighted averages of their respective districts.

Poverty indicators

The poverty indicators used in this study are provided by the INKAR database of the Federal Office for Building and Regional Planning (BBR). Since the INKAR database does not report data for the NUTS2 regions of the federal states Lower Saxony and Rhineland-Palatinate, I use district-level data and calculate the place indicators at NUTS2 level as population-weighted averages of their districts.

- **Housing subsidy:** Share of households that receive housing benefits ("Wohngeld"). Housing subsidy refers to contributions by the government to households' housing costs to ensure that low income individuals can afford suitable housing. Data at NUTS2 level is not available for the period 1998-2000.

- **Elderly support:** Share of individuals aged 65 or older that receives financial support ("Grundsicherung im Alter") to secure the basic necessities of life. Data at NUTS2 level is not available for the period 1998-2000.

- **GDP per capita:** Refers to the gross domestic product (GDP) per capita in 1000 euros. Due to data limitations, I have to use information for the year 2000 instead of 1999.

Other indicators

- **Academics:** Data on the share (in percent) of employees with an academic degree (Bachelor, State examination, Diploma, Master, PhD) is provided by the INKAR database of the Federal Office for Building and Regional Planning (BBR). Only employees subject to social insurance contributions are included, meaning no civil servants or self-employed individuals are considered. Since the INKAR database does not report data for the NUTS2 regions of the federal states Lower Saxony and Rhineland-Palatinate, I use district-level data and calculate the place indicators at NUTS2 level as population-weighted averages of their districts.

- **Population density:** Refers to a NUTS2 region's average number of inhabitants per square kilometre. Data is obtained from the "Regionaldatenbank Deutschland" of the Federal Statistical Office.

Table 1.3: Summary statistics of place characteristics

	1998-2000		**2013-2015**	
Characteristics	Mean (1)	S.D. (2)	Mean (3)	S.D. (4)
Current Smokers	31.77	4.67	26.16	3.31
Obesity rate	-	-	18.62	2.63
Exercise rate	33.75	5.26	51.49	4.49
Healthy diet	-	-	45.86	4.50
Number of hospitals	2.89	0.57	2.50	0.51
Hospital beds	691.05	84.59	611.16	86.52
General practitioners	64.25	6.52	65.99	4.37
Ambulatory doctors	131.57	15.89	154.25	15.82
PM_{10}	1287.69	1173.27	1027.59	918.12
PM_{25}	809.11	690.82	594.97	532.33
NO_2	456.52	160.75	415.94	145.10
Top 10% income share	31.28	3.70	33.39	1.65
Top 1% income share	9.54	1.26	10.13	0.83
Housing subsidy	-	-	11.30	2.62
Elderly support	-	-	4.82	1.42
GDP per capita	27.47	5.86	37.71	7.31
% Academics	-	-	12.85	3.85
Population density	432.45	413.71	415.68	396.04

Notes: This table presents population-weighted means and standard deviations for the place characteristics used in the correlation and regression analysis of this study. Detailed definitions of all variables can be found in Subsection 1.8.2. *Sources:* The sources of the different place characteristics are presented in Subsection 1.8.2.

2 The gender gap in lifetime earnings: The role of parenthood

2.1 Introduction

While most research on the gender pay gap has focused on differences in cross-sectional data, gender inequalities can add up over the life course as previous work experience, career pathways and earnings determine future labor market outcomes. Hence, a purely cross-sectional analysis cannot account for the biographical dimension of gender inequalities. However, due to high data requirements, there is only scarce empirical evidence on gender lifetime earnings gaps (e.g., Boll et al., 2017; Guvenen et al., 2021, 2022). In addition, these studies are often limited by their use of administrative data and subsequent lack of family-related information (e.g. number of children, marital status). Since, on average, the labor market participation of women is lower than that of men at both the intensive and extensive market due to family-related factors such as childcare (see, e.g., Goldin, 2014; Kleven et al., 2019), an analysis of the household context is necessary for a more comprehensive understanding of the underlying drivers of gender differentials in lifetime earnings.

This study uses the Socio-Economic Panel (SOEP) to shed light on the role of women's family backgrounds in gender differences, from both a cross-sectional and a lifetime perspective. Using an Oaxaca Blinder decomposition, we show that the gaps can largely be explained by both the extensive and intensive margins of labor.

On average, women have less work experience and work fewer hours, which has a strong negative effect on women's earnings.

To further take advantage of the detailed socioeconomic and family background information in the SOEP survey compared to administrative data sources, we use a dynamic microsimulation model to obtain full employment biographies, and subsequently lifetime earnings data. This approach leads to a more comprehensive sample than the ones of earlier studies (Bönke et al., 2015; Boll et al., 2017) for Germany as we are, for the first time, able to include self-employed individuals, civil servants and women with longer unemployment/inactivity spells. Our estimates show that women accumulate on average around 51.5% less than men in terms of lifetime earnings up to age 60. The unadjusted gender gap in lifetime earnings correlates largely with the number of children and ranges from 17.3% for childless women to 68.0% for women with three children or more.

To investigate which part of the observed gender gap in lifetime earnings can be associated with differences in the distribution of characteristics (e.g. work experience, level of education) across gender and which part is due to differences in labor market returns to characteristics, we estimate women's counterfactual lifetime earnings. We find that around 80% of the observed lifetime earnings gap can be explained by different characteristics across men and women, leading to an adjusted gender lifetime earnings gap of 10%. Contrary to the unadjusted gap, motherhood does not play a crucial role for the adjusted gender lifetime earnings gap. The adjusted gender gaps in lifetime earnings for childless women and women with three or more children only differ by around 2 percentage points (pp).

Our paper is related to three different strands of literature. First and most generally, it contributes to the extensive literature on the gender gap in pay and its drivers.

Existing studies show that a large extent of the pay gap can be attributed to fewer hours worked and higher discontinuity of female employment biographies (e.g., Bertrand et al., 2010; Blau and Kahn, 2017).[1] The persistence of this gender earnings inequality is mainly due to different effects of parenthood on men's and women's labor market behavior, and consequently their earnings (see, e.g., Waldfogel, 1998; Angelov et al., 2016; Kleven and Landais, 2017). In line with previous studies (e.g., Goldin, 2014; Juhn and McCue, 2017; Gallen et al., 2019), we confirm that gender differences in annual earnings increase during the period of family formation, peak around age 40 and slowly decrease until retirement, leading to an inverse U-shape of the gender annual earnings gap over the work life.

Studies for Germany show that the cross-sectional earnings gap between mothers and non-mothers are largely driven by domestic work and childcare duties (e.g., Beblo and Wolf, 2002; Ejrnæs and Kunze, 2013). Strikingly, child penalties on women's pay are high in Germany compared to other countries (see, e.g., Kleven et al., 2019). This is often attributed to longer maternal leave entitlement and a higher rate of part-time work for women in Germany (see, e.g., Harkness and Waldfogel, 2003; Gangl and Ziefle, 2009). However, more recent studies also stress the influence of relative conservative gender norms in Germany in this context (e.g., Kleven et al., 2019, 2020).

Second, our study adds to the scarce literature on lifetime earnings and specifically to what extent these accumulated earnings differ by gender. Lifetime earnings refer to the sum of individuals' accumulated earnings over their entire work life.[2] As

[1] Past studies in this field focused on gender differences in human-capital accumulation and discrimination as the main drivers of gender inequalities in labor markets. Altonji and Blank (1999) give an overview of the early literature in this field.

[2] Due to their close link to individuals' life chances, lifetime earnings are often seen as the more comprehensive earnings measure in comparison to, for example, cross-sectional annual earnings (see, e.g., Corneo, 2015; Tamborini et al., 2015).

mentioned before, mostly due to high data requirements, the literature on the gender pay gap and its evolution has primarily focused on cross-sectional hourly wages, annual earnings or earnings over a short time period. Using administrative data for the U.S., Guvenen et al. (2021) show that the fraction of women among lifetime top earners is significantly lower than that of men for birth cohorts 1956 to 1958. On average, lifetime top earners in the U.S. tend to be individuals who experience high earnings growth over the first half of their life cycle – the period when the gender gap increases the most, likely due to family-related reasons. In a later study, Guvenen et al. (2022) provide evidence that the large gender lifetime earnings gap is narrowing over time, with women's median lifetime earnings increasing while men's median lifetime earnings decreases for younger birth cohorts.

Using administrative data from the German Pension Register (VSKT), Bönke et al. (2015) find evidence that intragenerational lifetime earnings inequality for West German men born between 1935 and 1969 has increased, largely due to losses in the bottom of the lifetime earnings distribution. They also supplement their work with additional results on West German women. However due to data restrictions, their data only includes women with stable employment biographies. Therefore, the VSKT data is not representative for most women mainly due to the high rate of inactivity amongst women of older cohorts and should not be used for estimating the gender lifetime earnings gap in Germany. Closest to our paper is the study by Boll et al. (2017) analyzing the gender lifetime earnings gap in Germany. Using the administrative Sample of Integrated Labour Market Biographies (SIAB), they estimate an unadjusted gender lifetime earnings gap of 46% for West German birth cohorts 1950 to 1964. They show that the gender gap widens significantly during the age of family formation and that gender differences in work experience and hours worked explains around two-thirds of this overall gender lifetime earnings gap. However, SIAB data does not offer any information about individuals' family background. Hence, to the best of our knowledge, our study is the first to extensively examine the influence of parenthood in the context of gender differentials in lifetime earnings.

Third, our study contributes from a methodological point of view to the literature on the implementation of dynamic microsimulation models for the simulation of missing information (e.g., Zucchelli et al., 2012; Li and O'Donoghue, 2013; Levell and Shaw, 2016). A dynamic microsimulation approach refers to a regression-based simulation which predicts the transition probabilities of different units (e.g. individuals or households) for moving from one state to another between two different points in time. Therefore, in contrast to studies using a splicing approach (e.g., Westermeier et al., 2012; Grabka and Goebel, 2017) where sequences of existing biographies are stitched together to construct full life-cycle data, the microsimulation approach typically "ages" the data year by year (Li and O'Donoghue, 2013). We apply a dynamic microsimulation model to SOEP survey data to obtain complete earnings biographies, which facilitates lifetime earnings analyses. Combining simulation models with survey data is a well-established method to deal with missing observations and panel attrition, which often impede using survey data to conduct long-term analyses (see, e.g., Brown et al., 2009; Coronado et al., 2011). For Germany, for example, there are existing studies simulating employment biographies using SOEP data (e.g., Geyer and Steiner, 2014; Bonin et al., 2015; Hänisch and Klos, 2016).

The next section introduces our dataset and starts by analyzing cross-sectional gender differences in hourly wages and annual earnings over the work life by using an Oaxaca Blinder decomposition. Section 2.3 describes our microsimulation approach to obtain full work biographies and presents our estimates for the unadjusted and adjusted gender lifetime earnings gap. Section 2.4 discusses limitations and potential extensions of this paper. Section 2.5 concludes.

2.2 Cross-sectional analysis

The cross-sectional analysis allows us to explore how gender gaps in hourly wages and annual earnings develop with increasing age and to investigate if short-term differences already follow certain patterns across gender. This first step is crucial to subsequently better understand how gender inequalities in labor market characteristics and earnings add up or equalize over the entire work life.

2.2.1 Data and methodology

Our study is based on the German Socio-Economic Panel (SOEP). The SOEP is a representative annual panel survey questioning about 30,000 individuals across 15,000 households since 1984. In contrast to administrative data, the SOEP includes a rich set of socioeconomic variables, detailed labor market information and household background including information on the partner and children.[3]

We restrict our cross-sectional analysis to birth cohorts 1940 to 1979. These are the same birth cohorts used for the underlying regressions of our microsimulation model in Section 2.3. We observe these cohorts at least once between the ages of 38 and 44 in the SOEP. This age restriction is crucial as it is the age frame when individuals' cross-sectional earnings show the highest correlation with lifetime earnings (Björklund, 1993; Bönke et al., 2015) and is therefore needed to successfully simulate life-cycle

[3] See Goebel et al. (2019) for a detailed overview of the SOEP.

profiles in Section 2.3. Further, we focus on West German individuals since those born in East Germany were only included in the SOEP after German reunification in 1990. The poor comparability of the Federal Republic of Germany and the German Democratic Republic with respect to labor market institutions and economic systems does not allow us to simulate missing information for East Germans before 1990.

Section 2.2 focuses on the evolution of cross-sectional hourly wages and annual earnings with increasing age over the work life. This approach sheds light on two main components of the gender gap in lifetime earnings; the gender gap in hourly wages shows the differences in the compensation between women and men for one hour of their work, while the gap in annual earnings reveals dissimilarities driven by the variation in working hours.

We use an Oaxaca Blinder decomposition (see Oaxaca, 1973; Blinder, 1973) to investigate how much of the difference in the observed gender gap is driven by different characteristics between men and women and how much can be attributed to different returns to characteristics within the labor market.[4] Using this decomposition approach the gender gap G in the labor market outcome variable L (here: logarithmic hourly wage and logarithmic annual earnings) is defined as:

$$G_x = E(L_{mx}) - E(L_{fx}) \tag{2.1}$$

Therefore, G is the gender differential between the means of outcome L for men (m) and women (f) at age x. We can then divide the gender gap into two parts. First, the *endowment part*, which is the component of the gender gap which is due to differences in the distribution of characteristics between men and women.

[4] A more detailed description of this methodological approach can be found in Subsection 2.6.1 in the Appendix.

And second, the *coefficient part*, which accounts for differences in returns to characteristics. Hence, the coefficient part shows the gender driven difference of the labor market's willingness to pay for the same characteristics obtained by either men or women. However, note that the coefficient part may also include gender differences that remain unexplained in our model due to data and model restrictions. We run the following regression model separately by sex (s) and age (x) for the labor outcome L [5]:

$$L_{s,i,x} = \alpha_{s,i,x} + \beta_{s,i,x} Z_{s,i,x} + \epsilon_{s,i,x}, \quad E(\epsilon_{s,x}) = 0, \quad s \in \{F, M\}, x \in [20, 60] \tag{2.2}$$

where Z is a vector of control variables including work experience measured as number of working years, full-time or part-time work, work sector, highest education level, marital status and number of children. In addition, we control for cohort and time effects.[6]

2.2.2 Hourly wage

Overall, employed men have significantly higher hourly wages than employed women (see Table 2.3 in the Appendix). At the beginning of their work life at age 20, men earn on average 9.37 euros per hour while women's average wage is only 7.97 euros per hour. In line with results found by the Federal Statistical Office (Statistisches Bundesamt, 2017), the average hourly wages of men in our sample then almost triples over the work life to 26.13 euros per hour at age 60. In contrast,

[5]For comparability, we only control for variables that we can also use in our analysis of the lifetime gender gap in Section 2.3.

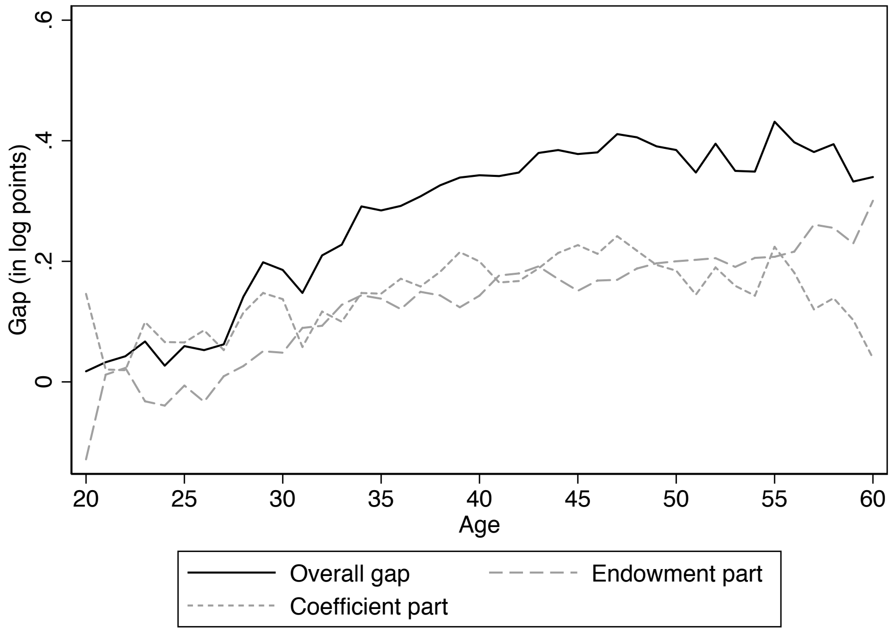
[6]Our pooled sample includes birth cohorts 1940 to 1979. Therefore, we include cohort dummies into our estimation model. We do not find any consistent cohort effects in our analysis. Figure 2.10 in the Appendix also shows that gender gaps in labor market outcomes are generally stable over time in our sample of working women.

women's hourly wages only increase to 17.48 euros, already showing significant gender differences in wage growth over the work life.

The solid line in Figure 2.1 shows the evolution of the gender gap in hourly wages in log points from age 20 to 60. Notably, the gender gap remains stable over the early years of work life. At age 25, men's hourly wages are only 0.059 log points higher than women's and the difference is still insignificant (see also Table 2.1). However, during the time of family formation and childcare, this gap drastically widens up to a highly significant difference of 0.378 log points at age 45.[7] Afterwards, the growth of the gender gap in hourly wages slows down and remains relatively stable with a peak at age 55. This finding is consistent for all cohorts (see Figure 2.11 in the Appendix). In line with our findings, previous studies also documented a widening of the gender wage gap over the life cycle (e.g., Anderson et al., 2002; Angelov et al., 2016; Tyrowicz et al., 2018).

The results of the Oaxaca Blinder decomposition are displayed by the grey lines in Figure 2.1 and also in Table 2.1. Visibly, the widening of the gender gap in hourly wages over the work life is driven by the increase in the endowment part, while the coefficient part of the gender gap shapes its overall trend. At younger ages, the different distribution of characteristics does not play a role yet. Therefore, at the beginning of work life all wage differences between men and women are due to different returns to labor market characteristics. Main differences in characteristics such as work experience or family background widen only later in life; after age 25, the high and significant coefficients for work experience in Table 2.1 show that the increase of the endowment part is mainly driven by women's lower gain of work experience with increasing age. By the age of 60, men have accumulated on

[7]A gender gap of 0.059 log points corresponds to a wage differential of $(e^{0.059} - 1) * 100 = 6.08\%$, while a gap of 0.378 log points corresponds to a wage differential of $(e^{0.378} - 1) * 100 = 45.94\%$.



Notes: Only employed individuals are considered. Cohorts 1940-1979, weighted sample. *Source:* Own calculations based on SOEP v35.

Figure 2.1: Gender gap in hourly wages

average 37.32 years of full-time and 1.09 years of part-time work experience, whereas women have accumulated on average only 19.65 years of full-time and 13.32 years of part-time work experience (see Table 2.3 in the Appendix). Our results show that these large differences in work experience are crucial to explaining the gender gap in hourly wages. By the end of work life, differences in work experience account for 0.309 log points of the overall gender wage gap of 0.340 log points. Hence, around 90% of the overall gender gap of 40.5% in hourly wages can be explained by differences in work experience.

In contrast to the stable growth of the endowment part, the evolution of the coefficient part follows a slight inverse U-shape. At age 20, the gender gap cannot be explained through differences of characteristics across genders, but the coefficient part amounts to 0.126 log points. This means that even if women and men had the same labor market characteristics, men's wages would be 0.126 log points (13.4%) higher than women's wages at this age. The coefficient part of the gender gap then peaks at 0.247 log points (28.0%) at age 45 and then declines again to a difference of 0.042 log points (4.3%) just before retirement.[8] In contrast to the endowment part, none of the variable groups have a constant significant influence on the overall gender gap, including the constant itself.[9] Therefore, not one individual effect dominates the coefficient part of the overall gender gap, but the coefficient part is instead a combination of many individual influences including those not controlled for in this regression model.

In summary, the gender gap in hourly wages is determined by two factors: first, women have in sum less favorable labor market characteristics compared to men, and second, even if they have the same characteristics, the labor market rewards women worse than men. The influence of differences in characteristics grows significantly with age, mainly through increasing differences in accumulated work experience across gender. Of the observed gender gap of 40.5% (0.340 log points) at age 60, different characteristics account for 87% (0.297 log points). This leads to an adjusted gender gap in hourly wages of 5.3%.

[8]Table 2.4 and Table 2.5 in the Appendix display the separate regression results for men and women which provide the basis for the difference in coefficients displayed in the Oaxaca Blinder regression.

[9]The constant of the coefficient part also includes the effects of gender differences in unobserved predictors (Jann, 2008), e.g., different occupational choices or differences in employers.

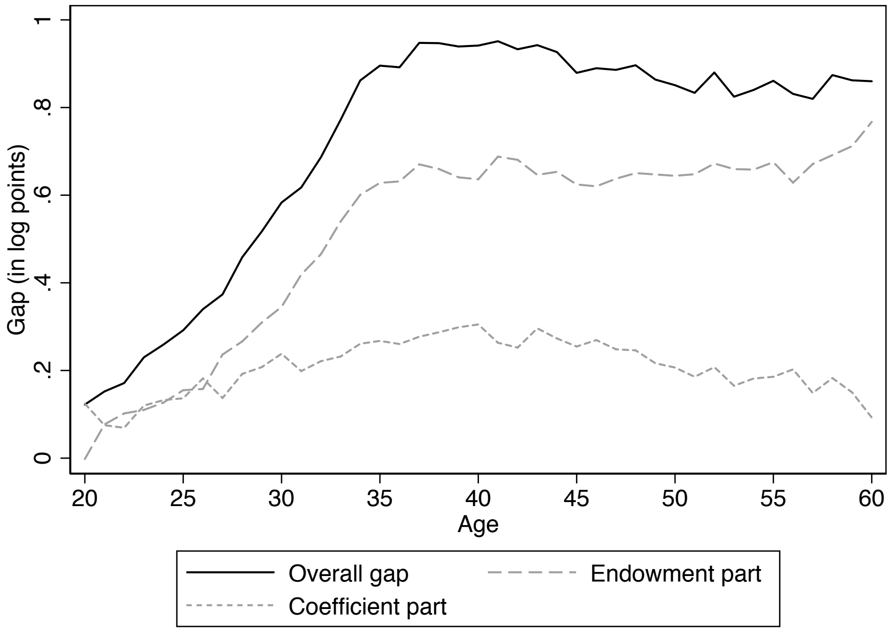
Table 2.1: Oaxaca Blinder decomposition of hourly wage gender gap

	(1) Age 20	(2) Age 25	(3) Age 30	(4) Age 35	(5) Age 40	(6) Age 45	(7) Age 50	(8) Age 55	(9) Age 60
Overall									
Men	1.963***	2.563***	2.771***	2.912***	2.980***	3.008***	3.019***	3.054***	3.003***
	(0.040)	(0.021)	(0.015)	(0.012)	(0.012)	(0.013)	(0.016)	(0.022)	(0.026)
Women	1.945***	2.503***	2.586***	2.628***	2.637***	2.630***	2.634***	2.622***	2.663***
	(0.033)	(0.024)	(0.019)	(0.017)	(0.016)	(0.017)	(0.017)	(0.020)	(0.031)
Difference	0.018	0.059	0.186***	0.284***	0.343***	0.378***	0.385***	0.432***	0.340***
	(0.052)	(0.031)	(0.024)	(0.021)	(0.020)	(0.021)	(0.024)	(0.030)	(0.041)
Endowment	-0.108**	-0.031	0.033*	0.107***	0.107***	0.131***	0.196***	0.200***	0.297***
	(0.040)	(0.018)	(0.015)	(0.018)	(0.020)	(0.022)	(0.026)	(0.028)	(0.043)
Coefficient	0.126*	0.091**	0.152***	0.177***	0.235***	0.247***	0.189***	0.231***	0.042
	(0.050)	(0.033)	(0.028)	(0.026)	(0.029)	(0.028)	(0.036)	(0.040)	(0.060)
Endowment									
Children	0.003	-0.000	0.001	-0.002	-0.004	-0.009*	-0.015*	-0.014	-0.013
	(0.005)	(0.003)	(0.001)	(0.002)	(0.003)	(0.004)	(0.007)	(0.011)	(0.013)
Married	0.001	-0.001	0.004	0.002	0.003	0.005*	0.000	-0.002	0.010
	(0.004)	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.008)
Experience	-0.068*	0.024*	0.084***	0.167***	0.207***	0.224***	0.228***	0.264***	0.309***
	(0.030)	(0.011)	(0.013)	(0.019)	(0.022)	(0.028)	(0.026)	(0.032)	(0.049)
Part time	0.002	-0.041**	-0.027	-0.067***	-0.096***	-0.066***	-0.026	-0.056*	-0.030
	(0.008)	(0.013)	(0.015)	(0.019)	(0.019)	(0.019)	(0.019)	(0.024)	(0.024)
Education	-0.008	-0.019**	-0.009	0.021**	0.019**	0.022***	0.031***	0.030***	0.045***
	(0.007)	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)	(0.007)	(0.008)	(0.011)
Cohort	-0.001	-0.002	-0.000	0.001	0.003	0.001	0.002	0.003	-0.001
	(0.005)	(0.003)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
Sector	-0.037	0.009	-0.019*	-0.017**	-0.025***	-0.045***	-0.024***	-0.024*	-0.022*
	(0.029)	(0.011)	(0.008)	(0.006)	(0.007)	(0.007)	(0.007)	(0.010)	(0.011)
Coefficient									
Children	0.003	0.030	0.095***	0.063	0.002	0.007	-0.075	-0.048	0.080
	(0.005)	(0.020)	(0.025)	(0.034)	(0.038)	(0.046)	(0.044)	(0.053)	(0.088)
Married	-0.006	0.021	0.041	0.052	0.090*	-0.008	0.100*	0.047	0.020
	(0.010)	(0.027)	(0.031)	(0.034)	(0.038)	(0.041)	(0.045)	(0.056)	(0.074)
Experience	0.243***	0.207	0.067	-0.094	-0.058	0.005	-0.150	0.196	-0.973
	(0.063)	(0.129)	(0.128)	(0.127)	(0.112)	(0.170)	(0.226)	(0.280)	(0.995)
Part time	0.013	0.031	0.008	-0.021	0.032	0.032	0.030	0.145***	0.036
	(0.027)	(0.037)	(0.023)	(0.019)	(0.021)	(0.021)	(0.025)	(0.036)	(0.054)
Education	0.329	-0.144	-0.210	-0.105	-0.564*	0.183	0.568	0.131	0.722
	(0.195)	(0.174)	(0.141)	(0.230)	(0.256)	(0.237)	(0.295)	(0.840)	(0.648)
Cohort	0.083	0.023	0.002	-0.007	0.091	-0.019	0.030	-0.030	0.002
	(0.044)	(0.085)	(0.036)	(0.125)	(0.049)	(0.034)	(0.041)	(0.038)	(0.040)
Sector	0.222	-0.445**	-0.436**	-0.053	-0.066	-0.139	-0.037	-0.085	-0.153
	(0.193)	(0.139)	(0.160)	(0.116)	(0.147)	(0.142)	(0.124)	(0.188)	(0.192)
Constant	-0.762**	0.368	0.587*	0.343	0.708*	0.186	-0.276	-0.125	0.308
	(0.276)	(0.283)	(0.255)	(0.308)	(0.322)	(0.317)	(0.383)	(0.887)	-1.090
N	765	1782	3053	4323	5356	5592	4304	2866	1758

Notes: Standard errors in parentheses; The stars refer to the following significance level: $*p < 0.05$, $**p < 0.01$, $***p < 0.001$. The different drivers are summarized as followed: “Children”: Number of children; “Married”: Dummy variable on marital status, “Experience”: Total years of working full time, part time or being inactive (also squared); “Part time”: Dummy variable indicating full time or part time work; “Education”: Dummy variables indicating highest level of educational attainment, “Sector": Occupational sector; “Cohort”: Cohort dummies. Cohorts 1940-1979, weighted sample. *Source:* Own calculations based on SOEP v35.

2.2.3 Annual earnings

In addition to earning less per hour, women also work on average fewer hours than men do. Therefore, the gender gap in annual earnings might be even wider than the gap in hourly wages due to gender differences in the intensive margin of work.



Notes: Only employed individuals are considered. Does not include values of zero annual earnings. Cohorts 1940-1979, weighted sample. *Source:* Own calculations based on SOEP v35.

Figure 2.2: Gender gap in annual earnings

Figure 2.2 shows the overall gender gap in annual earnings, the part of the gap due to different characteristic across gender (endowment part) and the part of the gender gap due to differences in coefficients (coefficient part). Visibly, the gender gap in annual earnings is significantly higher than the gender gap in hourly wages. At the peak of the gap at age 40 (0.829 log points corresponding to 129.1%), men's average annual earnings are more than twice as high than women's. Similar to the gender gap in hourly wages, the gender gap in annual earnings increases rapidly until age 35 and remains on a constant high level during the years of child rearing. Afterwards, it only declines slightly until retirement. This finding is in line with earlier studies for the U.S. providing evidence for a similar course of the cross-sectional gender gap in annual earnings over the work life (Goldin, 2014; Juhn and McCue, 2017).

When decomposing the overall gender gap in annual earnings, we find that the larger gap (in comparison to the gap in hourly wages) is driven by the significantly higher endowment part. While the gender gap due to differences in coefficients is only slightly higher than in the model for hourly wages, the endowment part has more than tripled.[10] This result underlines the importance of differences in the intensive labor margin across gender.

Table 2.2 shows that the endowment part of the gender gap in annual earnings is also driven by the lesser work experience women have accumulated over their life cycle. Moreover, the lower number of hours worked by women per year at all ages influences the gender gap to an even greater extent. These findings are in line with previous studies (e.g., Bertrand et al., 2010; Gallen et al., 2019).

At age 35, women's annual earnings are on average 0.327 log points lower than men's due to their lower number of work hours.[11] In addition, women's earnings are on average 0.203 log points lower than men's due to the lesser work experience they have accumulated up to this age. This means that at this point around half of the overall gap can be explained by the distribution of working hours and around a quarter can be explained by the different distribution of work experience. The

[10] Please note that since this subsection focuses on the intensive margin of work, we now include the total hours worked per year for this model in contrast to the binary variable (part-time/full-time) used when we were analyzing the gender gap in hourly wages. Consequently, this leads to an even more significant endowment part for the analysis of annual earnings as the total number of work hours is a key driver in the earnings difference across gender.

[11] It is crucial to note that our model does not control for endogenous choice. Hence, we do not differentiate whether women choose to work fewer hours or if they have trouble finding adequate employment. See, for example, Harnisch et al. (2018) and Beckmannshagen and Schröder (2022) for studies on working hours mismatches in Germany.

effect of work experience steadily increases over the life cycle and peaks at age 60 with 0.351 log points. In contrast, differences in the level of education or family background play a smaller role.

The coefficient part of the gender gap in annual earnings is positive throughout the life cycle. This means that, besides worse characteristics, women also face less beneficial coefficients in their wage regression (see Table 2.2, and Tables 2.6 and 2.7 in the Appendix). This is especially pronounced between ages 30 and 45. There are two potential explanations: First, employers could fear a higher risk of work absence by women due to pregnancy and child rearing, and therefore already include the higher risk of absence in the paid wages of women (see, e.g., Correll et al., 2007). Second, women might opt for less financially rewarding positions in return for higher work flexibility after having children (see, e.g., Goldin, 2014). However, interestingly, for individuals aged 60 the coefficient part of the gender gap is very small in magnitude and no longer statistically significant, indicating that at this point the gender gap in annual earnings is almost entirely driven by differences in endowments.

Table 2.2: Oaxaca Blinder decomposition of the annual earnings gender gap

	(1) Age 20	(2) Age 25	(3) Age 30	(4) Age 35	(5) Age 40	(6) Age 45	(7) Age 50	(8) Age 55	(9) Age 60
Overall									
Men	9.462***	10.155***	10.460***	10.623***	10.695***	10.713***	10.717***	10.693***	10.542***
	(0.041)	(0.024)	(0.017)	(0.012)	(0.013)	(0.015)	(0.017)	(0.022)	(0.030)
Women	9.424***	9.950***	9.923***	9.854***	9.867***	9.915***	9.904***	9.868***	9.775***
	(0.039)	(0.026)	(0.026)	(0.024)	(0.021)	(0.021)	(0.024)	(0.028)	(0.040)
Difference	0.038	0.205***	0.537***	0.769***	0.829***	0.797***	0.812***	0.825***	0.766***
	(0.057)	(0.036)	(0.031)	(0.027)	(0.024)	(0.025)	(0.030)	(0.036)	(0.050)
Endowment	-0.081	0.102***	0.318***	0.538***	0.555***	0.529***	0.609***	0.657***	0.747***
	(0.046)	(0.026)	(0.028)	(0.025)	(0.025)	(0.028)	(0.034)	(0.036)	(0.050)
Coefficient	0.119*	0.103**	0.219***	0.231***	0.274***	0.269***	0.203***	0.168***	0.019
	(0.046)	(0.036)	(0.036)	(0.028)	(0.031)	(0.033)	(0.042)	(0.042)	(0.053)
Endowment									
Children	-0.000	0.001	0.001	-0.001	-0.003	-0.010*	-0.018*	-0.020	-0.012
	(0.005)	(0.003)	(0.002)	(0.002)	(0.003)	(0.005)	(0.007)	(0.011)	(0.012)
Married	0.001	0.001	0.005	0.001	0.001	0.001	-0.002	-0.004	0.003
	(0.004)	(0.003)	(0.003)	(0.001)	(0.002)	(0.003)	(0.002)	(0.003)	(0.008)
Experience	-0.058	0.033*	0.129***	0.203***	0.244***	0.273***	0.306***	0.334***	0.351***
	(0.030)	(0.014)	(0.015)	(0.018)	(0.022)	(0.026)	(0.028)	(0.033)	(0.040)
Hours worked	0.023	0.082***	0.214***	0.327***	0.313***	0.282***	0.310***	0.331***	0.371***
	(0.018)	(0.015)	(0.024)	(0.022)	(0.021)	(0.023)	(0.025)	(0.029)	(0.034)
Education	-0.003	-0.021**	-0.011	0.023**	0.020**	0.023***	0.032***	0.033***	0.048***
	(0.006)	(0.007)	(0.007)	(0.007)	(0.006)	(0.007)	(0.007)	(0.008)	(0.012)
Cohort	-0.001	-0.002	0.000	0.001	0.002	0.000	0.002	0.003	-0.002
	(0.005)	(0.003)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.003)	(0.002)
Sector	-0.043	0.008	-0.019*	-0.016*	-0.021**	-0.040***	-0.021**	-0.020	-0.012
	(0.026)	(0.011)	(0.009)	(0.006)	(0.007)	(0.008)	(0.008)	(0.010)	(0.011)
Coefficient									
Children	0.000	0.037*	0.155***	0.069	-0.005	0.025	-0.093	-0.074	0.069
	(0.004)	(0.015)	(0.028)	(0.037)	(0.040)	(0.049)	(0.048)	(0.057)	(0.081)
Married	-0.004	0.033	0.040	0.049	0.092*	-0.012	0.113*	0.075	0.060
	(0.010)	(0.025)	(0.033)	(0.036)	(0.040)	(0.048)	(0.048)	(0.055)	(0.074)
Experience	0.224***	0.146	0.045	-0.139	-0.159	-0.150	-0.169	0.378	-0.964
	(0.063)	(0.139)	(0.146)	(0.156)	(0.121)	(0.177)	(0.269)	(0.423)	(0.966)
Hours worked	-0.348	-0.260	-0.625***	-0.811***	-0.801***	-0.783***	-0.891***	-1.152***	-0.621***
	(0.222)	(0.184)	(0.160)	(0.105)	(0.100)	(0.143)	(0.144)	(0.145)	(0.151)
Education	0.356*	-0.082	-0.240	-0.165	-0.595*	0.229	0.720*	0.932	0.809
	(0.173)	(0.182)	(0.151)	(0.245)	(0.263)	(0.310)	(0.332)	(0.700)	(0.699)
Cohort	0.067	0.002	-0.038	0.028	0.134**	-0.005	0.039	-0.030	0.003
	(0.044)	(0.051)	(0.039)	(0.124)	(0.046)	(0.036)	(0.044)	(0.040)	(0.040)
Sector	0.166	-0.295*	-0.396*	-0.126	-0.055	-0.091	-0.008	-0.120	-0.098
	(0.180)	(0.144)	(0.166)	(0.130)	(0.159)	(0.150)	(0.142)	(0.213)	(0.217)
Constant	-0.344	0.521	1.278***	1.326***	1.663***	1.056*	0.492	0.159	0.759
	(0.330)	(0.304)	(0.310)	(0.358)	(0.343)	(0.413)	(0.465)	(0.834)	-1.112
N	765	1782	3053	4323	5356	5592	4304	2866	1758

Notes: Standard errors in parentheses; The stars refer to the following significance level: $*p < 0.05$, $**p < 0.01$, $***p < 0.001$. The different drivers are summarized as followed: “Children”: Number of children; “Married”: Dummy variable on marital status, “Experience”: Total years of working full time, part time or being inactive (also squared); “Hours worked”: Hours worked per year; “Education”: Dummy variables indicating highest level of educational attainment, “Sector": Occupational sector; “Cohort”: Cohort dummies. Cohorts 1940-1979, weighted sample. *Source:* Own calculations based on SOEP v35.

2.3 Microsimulation and lifetime analysis

The previous section gave an analysis of the cross-sectional gender gaps in hourly wages and annual earnings, their development with increasing age and drivers. In this section, we investigate how gender earnings differentials might accumulate or balance out over the complete work life by looking at lifetime earnings. This allows us to shed light on the biographical dimension of the gender earnings gap.

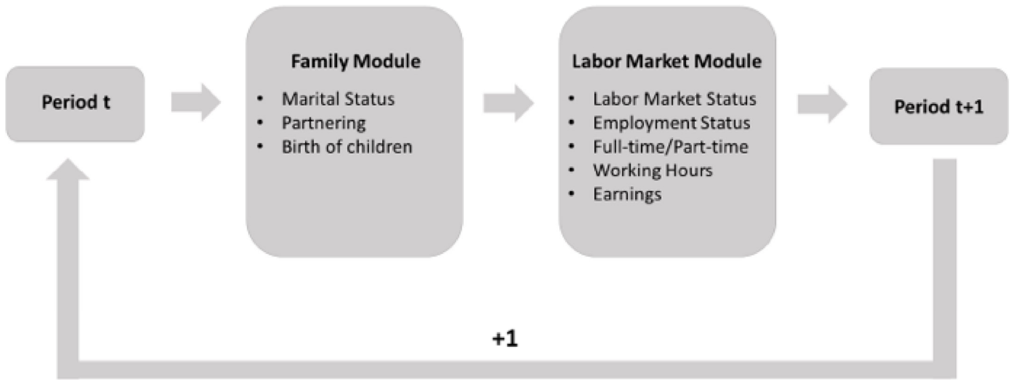
2.3.1 Data and methodology

We continue to use the SOEP as it offers panel data containing not only detailed labor market but also family background information, which administrative data cannot offer. However, the SOEP suffers from panel mortality. Only around 10% of the participants have been observed for at least 20 years or more, with an average participation period of 9.36 years (see Figure 2.12 in the Appendix). To investigate lifetime earnings for a larger sample, we implement a dynamic microsimulation approach to fill in the missing data of non-observed years during an individual's work live. This approach yields complete earnings data for the observation period which we can combine with the rich set of socioeconomic characteristics and family information in the SOEP.

To implement our dynamic microsimulation model successfully, we need to add the following restrictions to our cross-sectional sample: First, our lifetime earnings investigation focuses on birth cohorts 1964 to 1972 only. This approach gives us the opportunity to observe the cohorts starting at age 20 until at least age 45. This restriction is important as we know in the German context that only lifetime earnings up to age 45 and older are sufficient proxies for complete lifetime earnings up to age 60 (Bönke et al., 2015). Second, we exclude individuals who were only observed prior to turning 30 since labor market patterns of individuals in their twenties are very unstable and could yield a life-cycle bias (see, e.g., Haider and Solon, 2006; Brenner, 2010; Bönke et al., 2015). Further, the probability of observing the highest educational attainment accurately increases significantly with age 30 and older (Autorengruppe Bildungsberichterstattung, 2018) and observing the true educational attainment is crucial as education levels and earnings patterns over the work life are highly correlated (see, e.g., Bhuller et al., 2011; Bönke et al., 2015; Brunello et al., 2017). Third, we also exclude individuals without at least two consecutive observation years in the SOEP. Otherwise, no panel information is available and a distinction between individual short- and long-term labor shocks would not be possible. After eliminating those observations, we are left with a sample of 3,315 women and 3,212 men across birth cohorts 1964 to 1972 (see Table 2.8 in the Appendix) for the dynamic microsimulation.

2.3.1.1 Dynamic microsimulation model

We apply a dynamic microsimulation model to fill in missing information in non-observed years based on the individual's employment biography and socioeconomic characteristics. The general idea and structure of our microsimulation approach follows the approach proposed by Levell and Shaw (2016). To exploit our data to its fullest extent, we use both forward- and backward-looking simulations. The simulation starts either at an individual's first or last observed year in the data. As shown in Figure 2.3, we impute the missing variables in time $t+1$ or $t-1$ by running the regressions for our dynamic microsimulation in two consecutive steps: First, missing observations of marital status, fertility (i.e. number of children) and partners are simulated in the Family Module (Module 1). Second, the obtained information from Module 1 is used in addition to other provided data to simulate individuals' labor market information in the Labor Market Module (Module 2). Completing both modules yields the successful imputation of all relevant information in time $t+1$ or $t-1$. Afterwards, the process moves forward to the simulation of the next years, i.e. $t+2$ or $t-2$, $t+3$ or $t-3$, and so on. The simulation ends after reaching 1984 in the backward looking and 2017 in the forward-looking process. We obtain a full dataset without any missing earnings or family information between 1984 and 2017.



Source: Own diagram.

Figure 2.3: Dynamic microsimulation model

In addition, investigating complete lifetime patterns for our sample requires us to extend our simulation for 15 additional years until 2032, when the youngest birth cohort 1972 turns 60. The prediction of employment biographies after 2017 is based on regression parameters of observed individuals from older cohorts, while we assume that general labor market characteristics (e.g., unemployment rate) remain stable after 2017. We also account for differences in trends using cohort and age fixed effects in our regressions. Nevertheless, this prediction comes naturally with a certain level of uncertainty due to the assumption that trends remain stable – an assumption that neglects, for example, labor market effects related to the COVID-19 pandemic. The simulation ends when all missing information between 1984 and 2032 is simulated.

Within each module, the simulation of variables is based on estimating transition probabilities between two years, e.g., if marital status changes from year t to $t+1$. The estimation of a change of a variable j between two periods is then implemented by using a random process (see, e.g., Neufeld, 2000; Plümper and Troeger, 2007; Zucchelli et al., 2012): For each individual observation i we simulate the transition probability from time t to $t+1$ or $t-1$ and then draw a random number N_{it} from a uniform $[0,1]$ distribution. If the calculated transition probability P_{it} is larger than the drawn random number N_{it} ($P_{it} > N_{it}$), a transition occurs. In contrast, no transition takes place if $P_{it} \leq N_{it}$. Therefore, high transition likelihoods do not always induce actual transitions and even low transition probabilities may still lead to transitions. This approach helps to account for the uncertainty that comes with a simulation. Additionally, we use a Monte Carlo simulation approach to test the robustness of our results (see Figures 2.14 and 2.15 in the Appendix). The results of the Monte Carlo simulation confirm the high robustness of our simulation outcomes.

Next, we will give brief summaries about both simulation modules. Detailed information on all the regression models of every simulation step can be found in Table 2.9 in the Appendix.

2.3.1.2 Module 1: Family module

Empirical evidence shows that family background strongly influences women's labor market behavior (e.g., Kleven and Landais, 2017). Therefore, we need information on individual's family background before simulating earnings for non-observed years. All individuals in our sample completed entry questionnaires including questions on marital status and, if applicable, birth years of children before entering the survey; this allows us to reconstruct full family histories. Consequently, missing data occurs exclusively after individuals left the survey. This eliminates the necessity of the backward looking simulation component in this module. In addition, we also observe most women at older ages so only around 20% of child information must be simulated.

The Family Module then consists of two steps: predicting marital status, including a partnering module when necessary, and predicting births of children for individuals with missing information. First, we run logistic regressions separately by gender s (Female or Male) and marital status m (Single or Partnered) in year t to predict the individual transition probability $p^{married}$ to change the marital status from year t to the missing year $t+1$:

$$p^{married}_{m,s,t+1} = \beta_0 + \beta_1 X_{m,s,t} + \epsilon_{m,s,t}, \quad E(\epsilon_{m,s,t}) = 0, m \in \{S, P\}, s \in \{F, M\}, t \in [1984, 2017] \tag{2.3}$$

The regression consists of a set of explanatory variables X_t including socioeconomic characteristics (e.g., education, age, migration background) and labor market behavior (e.g., employment status). In addition, we control for the number of years that an individual's marital status has remained unchanged until year t. Table 2.9 in the Appendix gives a detailed overview about all covariates included in each regression-based simulation step.

Recall that if $P_{it} \leq N_{it}$, the marital status stays the same and if $P_{it} > N_{it}$, the marital status changes. Therefore, this simulation step has four possible outcomes: First, a person who is single in year t can remain single in $t+1$. Second, married individuals can stay married. Here we assume that their partners remain the same. Third, married individuals in period t can get divorced and become single in $t+1$.[12] And fourth, singles in year t can get married in $t+1$. In this last case, we run a Partner Module to assign a partner.[13] This allows us to account for partners' characteristics when simulating family and labor market decisions. Using Mahalanobis distance matching (Mahalanobis, 1936) we identify five "best" partners based on age, education and region for each observation. We then randomly assign one of the five potential partners to the individual. Our matching procedure is not unique, i.e., one individual can serve multiple times as a "donor" for partner characteristics. In this way, we ensure a sufficient pool of potential partners.

Next, we simulate whether a woman will give birth to a child in the next non-observed period $t+1$ by marital status m :

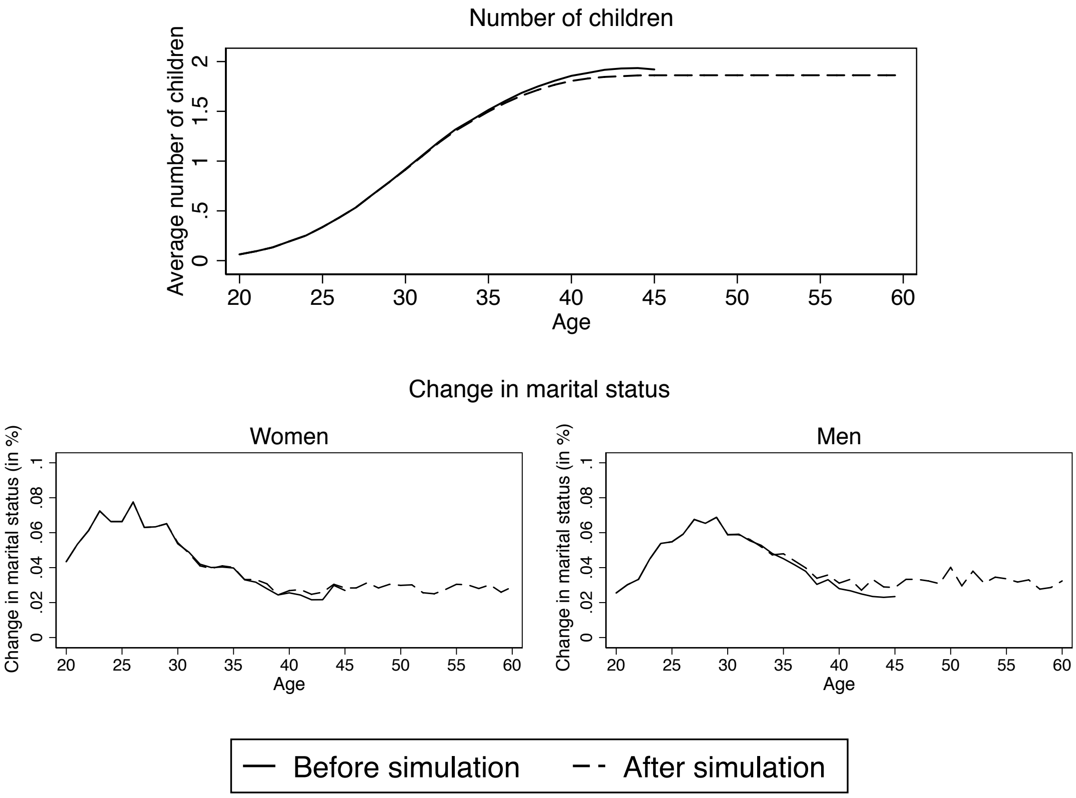
$$p^{birth}_{m,t+1} = \beta_0 + \beta_1 X_{m,t} + \epsilon_{m,t}, \quad E(\epsilon_{m,t}) = 0, m \in \{S,P\}, t \in [1984, 2017]. \tag{2.4}$$

Again, X_t represents a set of explanatory variables including socioeconomic characteristics like information on existing children and labor market information. The simulation is similar to the approach described in the simulation of the marital status. Afterwards, the information on an individual's number of children is updated accordingly. In contrast to our marriage simulation, births are only simulated for women. Children are then attached to men depending on women's family background.

[12] In this case we assume that the children stay with the mother. Empirical evidence by the Statistisches Bundesamt (2018a) supports this assumption: The share of single fathers in the period since 1997 is only 10 to 13%.

[13] For a few married individuals in our data, we cannot observe partner information since the partner did not participate in the survey, e.g., because they refused. In those cases, we also run the Partner Module as a preparation step before starting the Family Module.

Since we estimate transition likelihoods for $t+1$ by using information available in period t, the likelihood of a change of the marital status or a childbirth in $t+1$ do not influence the transition probability of one another. Therefore, the order in which we implement fertility and marital transitions is irrelevant and does not alter our results.



Notes: Panel A shows the average number of children of women by age before and after the simulation. Panel B demonstrates the share of individuals in our sample changing their marital status before and after the simulation. *Source:* Own calculations based on SOEP v35.

Figure 2.4: Family information before and after simulation

Completing the Family Module for years 1984 to 2032 results in a sample with full information on family characteristics. Figure 2.4 shows that our simulated data (dashed line) replicates the initial distributions before the simulation (solid line) very accurately. In Panel A, the women's average number of children increases strongly until age 35. Then, the growth rate slows down and comes to a natural stop between ages 40 and 45 due to biological reasons. Panel B displays the percentage change in marital status by age. Obviously, both men and women follow the same trend over the life cycle. Most changes in marital status happen in the beginning of life.

2.3.1.3 Module 2: Labor market module

The Labor Market Module generates complete information on an individual's employment biography through five stages: labor market participation, employment status, type of work arrangement (full-time or part-time), annual working hours and annual earnings. In this module, we use both forward and backward simulation as the introductory survey questionnaires do not allow us to construct sufficient work histories. Our model description will focus on the forward-looking simulation component. However, the backward-looking part of the simulation follows the same methodology.

In general, the logic and structure of this module is very similar to our approach in the Family Module. We start with the estimation of $p^{lmp}_{(m,t+1)}$, the probability for an individual of marital status m to change the labor market participation lmp from year t to year $t+1$. The labor market participation dummy variable is equal to 1 if individuals are unemployed or employed and equal to 0 if they are not attached to the labor market (e.g., due to parental or sick leave). We run the estimation separately by gender s and marital status m:

$$p^{lmp}_{s,m,t+1} = \beta_0 + \beta_1 p^{lmp}_{s,m,t} + \beta_2 p^{lmp}_{s,m,t-1} + \beta_3 X_{s,m,t} + \epsilon_{s,m,t},$$

$$E(\epsilon_{s,m,t}) = 0,\ s \in \{F, M\},\ m \in \{S, P\},\ t \in [1984, 2017]. \tag{2.5}$$

$X_{(s,m,t)}$ is again a vector of control variables with socioeconomic characteristics like marital status, partner's earnings and their own labor market information. Further, we include lagged dependent variables to account for path dependencies over the work life while still modelling a dynamic data generating process.[14] If individuals are recorded as not participating in year $t+1$, we directly record their earnings as zero for $t+1$ and do not include them in the subsequent steps. For individuals who are active in the labor market, we next run a regression to estimate the probability to change their employment status $p^{emp}_{(s,m,e,t+1)}$ (employed/unemployed) from year t to year $t+1$. The following model is run separately by gender s, marital status m and employment status e:

$$p^{emp}_{s,m,e,t+1} = \beta_0 + \beta_1 p^{emp}_{s,m,e,t} + \beta_2 p^{emp}_{s,m,e,t-1} + \beta_3 X_{s,m,e,t} + \epsilon_{s,m,e,t},$$

$$E(\epsilon_{s,m,e,t}) = 0,\ s \in \{F,M\},\ m \in \{S,P\},\ e \in \{0,1\},\ t \in [1984,2017]. \tag{2.6}$$

Once more, the regression contains a set of explanatory variables $X_{(s,m,e,t)}$ including information on family and the socioeconomic background. Also included in the control vector is the work history of individuals. To this end, we measure work experience by years of full-time work, part-time work and years without any work until year t to account for the different levels of labor market experience.

Individuals recorded as unemployed in year $t+1$ after this first regression step receive zero earnings in $t+1$ and are excluded from further estimations. For all employed individuals, the dynamic microsimulation moves forward with a logistic regression simulating if individuals worked full- or part-time in year $t+1$. In the next

[14] For this estimation strategy, we are only able to include individuals that have at least two observation years in the SOEP. Including additional lags would result in a reduced sample size since it would impose stricter sample restrictions (surveyed for at least three years in the SOEP).

step, we estimate the probability of changing full-time or part-time arrangements from year t to year $t+1$:

$$p^{wt}_{(s,m,t+1)} = \beta_0 + \beta_1 p^{wt}_{s,m,t} + \beta_2 p^{wt}_{s,m,t-1} + \beta_3 X_{s,m,t} + \epsilon_{s,m,t},$$

$$E(\epsilon_{s,m,t}) = 0,\ s \in \{F, M\},\ m \in \{S, P\},\ t \in [1984, 2017]. \tag{2.7}$$

Again, $X_{(s,m,t)}$ includes the usual control variables in addition to the labor market history. We can now move on to estimate the precise number of annual working hours in $t+1$ separately for part-time and full-time workers. We use an OLS regression model following the same logic as the earnings regression model as introduced in Equation (2.8).[15]

Finally, we use an earnings regression to estimate the annual earnings $y_{(s,m,t+1)}$ by gender s and marital status m:

$$y_{(s,m,t+1)} = \beta_0 + \beta_1 y_{s,m,t} + \beta_2 y_{s,m,t-1} + \beta_3 X_{s,m,t} + \epsilon_{s,m,t},$$

$$E(\epsilon_{s,m,t}) = 0,\ s \in \{F, M\},\ m \in \{S, P\},\ t \in [1984, 2017]. \tag{2.8}$$

$X_{(s,m,t)}$ now includes information about the work history in years of full-time work, part-time work or unemployment, working hours in t and, if applicable, partner and child information. All earnings are price-adjusted and presented in 2015 euros. The simulation then moves to the next year, e.g., $t+2$ or $t-2$. After completing all five steps of the Labor Market Module between 1984 and 2017, all individuals have complete employment and earnings information for previously unobserved years.

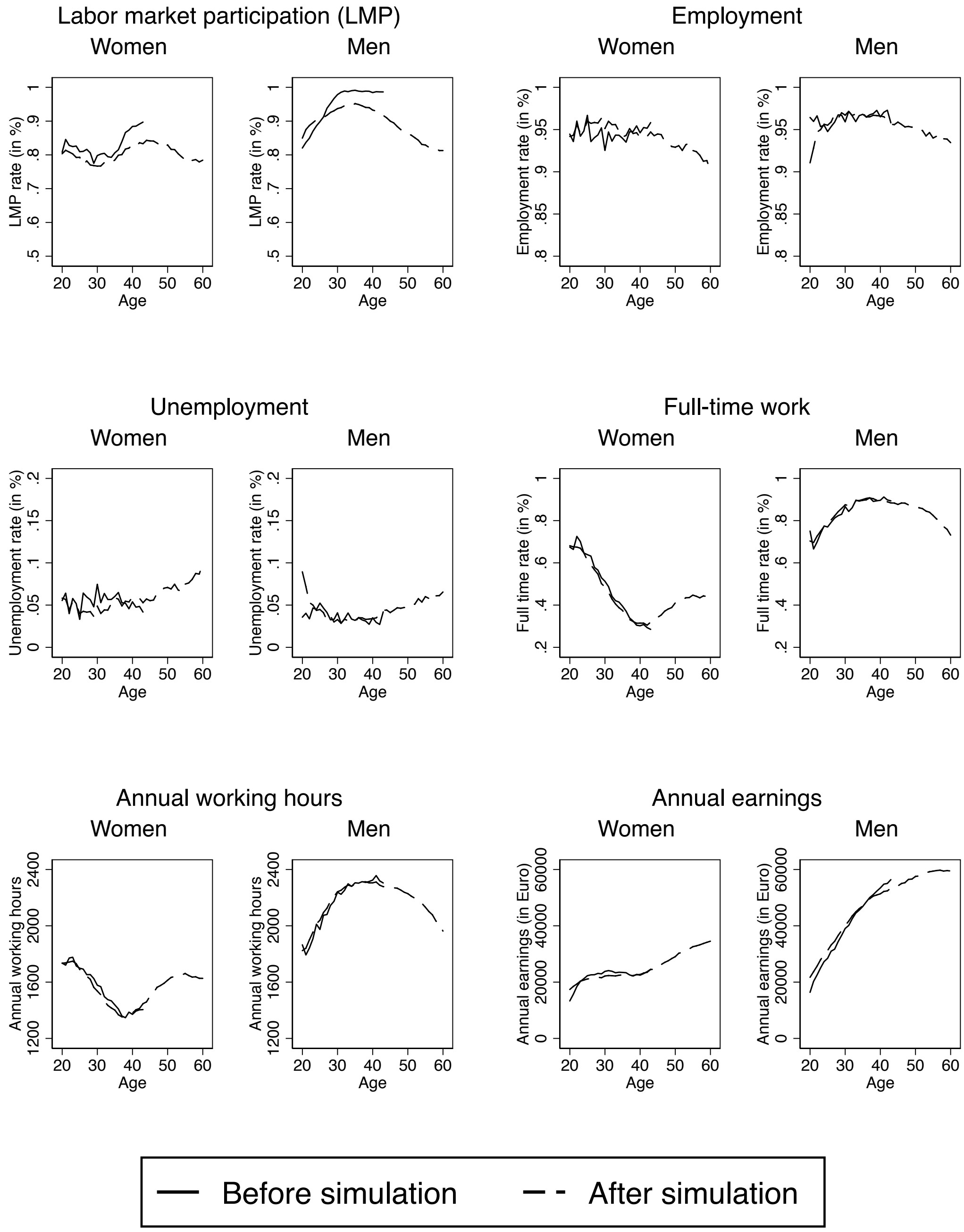
[15]Again, see Table 2.9 in the Appendix for more detailed information.

Afterwards, we continue the simulation until 2032 to obtain complete biographical data up to age 60.

Figure 2.5 shows that our simulated data (dashed line) replicates the original SOEP data (solid line) well, particularly for Panel D (Full-time work), Panel E (Working hours) and Panel F (Earnings). Panel A (Labor Market Participation), Panel B (Employment) and Panel C (Unemployment) show small deviations. Most of these differences occur in the beginning of the work life. These differences do not necessarily diminish the quality of our microsimulation for the following two reasons: First, our sample restriction to individuals observed at least once at age 30 or older leads to fewer observations in individuals' early twenties. As a result, our SOEP sample before the simulation is not very reliable for this age range due to a small sample size, and therefore comparisons may be misleading. Second, as depicted in Figure 2.5, earnings are on average relatively low at the beginning of an individuals work life and they increase over their careers. Consequently, earnings at young age only account for a small share of lifetime earnings.

After the completion of both modules of our dynamic microsimulation model, we obtain all relevant labor market and household information for birth cohorts 1964 to 1972 from age 20 to 60 to proceed with our lifetime analysis.[16] Overall, the simulated data mirrors the data patterns before simulation and our simulation results are robust. Additional robustness checks based on a Monte Carlo simulation approach and the simulation of pseudo-missings can be found in the Appendix.

[16]Our sample after the microsimulation is significantly different from our original SOEP sample. Therefore, we cannot use the longitudinal weights initially provided by the SOEP. To maintain representativeness, we therefore use census data (Mikrozensus) to reweight our sample with regard to cohort, age, family and labor market information. The Mikrozensus is considered highly representative for Germany, covering about 1% of the entire German population through mandatory participation.



Notes: Only employed individuals are considered. Does not include values of zero annual earnings. Cohorts 1940-1979, weighted sample. *Source:* Own calculations based on SOEP v35.

Figure 2.5: Labor market information before and after simulation

2.3.2 Lifetime analysis

Although we have already shown that women face lower hourly wages and annual earnings, and are less active on the labor market, the cross-sectional analysis only shows a snapshot of an individual's employment biography. A cross-sectional analysis does not reveal how these different factor add up over the life cycle. For a better understanding of when and how in life the gender gap develops, we investigate differences in accumulated earnings over the life cycle for birth cohorts 1964 to 1972 using their complete biography data from age 20 to 60 obtained from our microsimulation. To analyze the accumulation of earnings over the work life we follow Bönke et al. (2015) and use the "up-to-age-X" (UAX) concept. UAX earnings refer to accumulated price-adjusted (in 2015 euros) gross annual earnings up to a certain age X. In line with the study by Bönke et al. (2015), we define lifetime earnings as UA60 earnings.

2.3.2.1 Gender gap in lifetime earnings

To analyze the gender gap in lifetime earnings, we now focus on nonlogarithmic incomes rather than logarithmic incomes as used in the Oaxaca Blinder decomposition in Section 2.2.[17] Using logarithmic incomes would lead to the exclusion of zero earnings and, thus, periods of inactivity.[18] Since especially women accumulate periods of inactivity over life through motherhood and child rearing, those parts

[17]As stated in Section 2.2, the Oaxaca Blinder decomposition is based on an OLS regression model using log hourly wage and log annual earnings.

[18]The inverse hyperbolic sign (ihs) transformation represents an alternative concept. In contrast to the logarithmic transformation, it is also defined for negative and zero values (see, e.g., Burbidge et al., 1988; Pence, 2006). Due to these advantages, it is primarily used in the literature on wealth distributions (e.g., Pence, 2006; Grabka et al., 2015; Sierminska et al., 2018). However, we refrain from using this transformation as it is not easily interpretable and not a very commonly used concept in the literature on gender earnings gaps.

of their employment biographies without any earnings play a crucial role for the gender lifetime earnings gap and need to be included in this analysis.

The gender gap G in the labor market outcome variable L (here: hourly wages, annual earnings, UAX earnings) in percent for men m and women f at age x is now defined as:

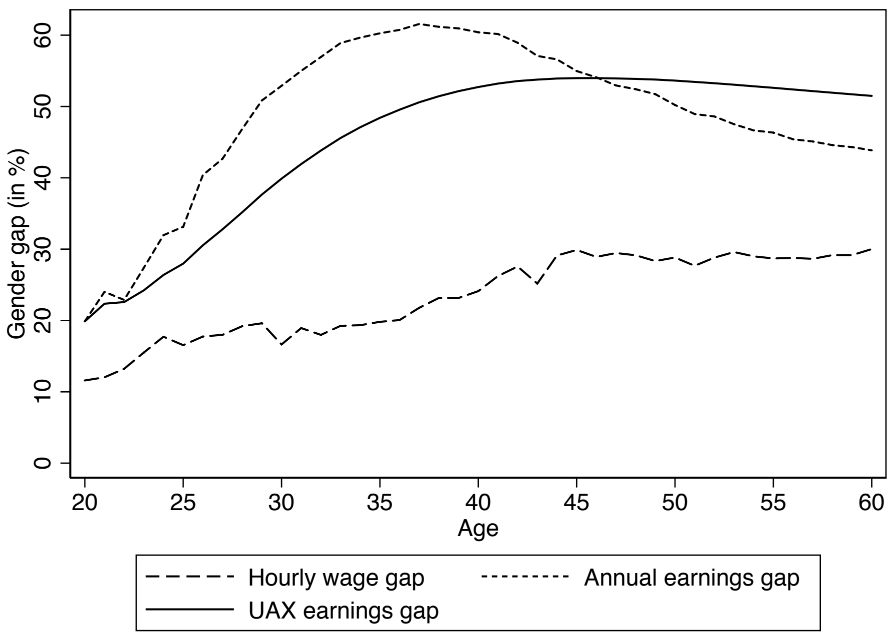
$$G_x = [(\overline{L}_{m,x} - \overline{L}_{f,x})/\overline{L}_{m,x}] \times 100 \,. \tag{2.9}$$

Based on our new sample obtained from the microsimulation, Figure 2.6 shows the gender gaps in hourly wages, annual earnings and UAX earnings for ages 20 to 60 for birth cohorts 1964 through 1972. As expected, despite the same trend, we see several differences when we compare the gender gaps in hourly wages and annual earnings using this microsimulation sample to our results based on the cross-sectional sample discussed in Section 2.2.

At early ages, the gender gap in hourly wages rather low but then increases steadily until retirement. However, we can observe differences in levels which are driven by the more confined cohort restriction in our microsimulation sample and the varying definition of the gender gap (logarithmic vs. non-logarithmic income). Comparing the gender gaps in annual earnings reveals more pronounced differences between the cross-sectional and lifetime approach. First, the inversely U-shaped gender gap in annual earnings in Figure 2.6 is significantly larger than the gender gap shown in Figure 2.2. This difference is largely driven by the inclusion of inactive labor periods with zero earnings in this lifetime analysis, while we excluded those in our cross-sectional analysis in Section 2.2.[19] Including periods with zero earnings leads

[19] See Figure 2.16 for a direct comparison of the gender gap in annual earnings when including or excluding individuals with zero earnings.

to a decline in women's average earnings, and thus to an increase in the gender gap. Naturally, this difference is especially pronounced in the years of family formation when women, on average, have longer spells of labor market inactivity due to child rearing. Second, in contrast to the gender gap estimated using the cross-sectional sample, Figure 2.6 shows a pronounced decline of the gender gap in annual earnings between ages 40 and 60. Again, this difference is driven by the different composition of our two samples. While the cross-sectional sample includes all birth cohorts 1940 to 1979, the lifetime sample is restricted to younger cohorts. Due to the higher labor market participation rates for women of younger cohorts, the gender gap in annual earnings declines again before retirement once we restrict our sample to younger cohorts, because more women reenter the labor market after times of inactivity during family formation.



Notes: Individuals with zero UAX earnings are included in the calculation. For annual earnings, employed and unemployed individuals are considered. For hourly wages, only employed individuals are considered. Cohorts 1964-1972. *Source:* Own calculations based on SOEP v35.

Figure 2.6: Gender gaps in wages, annual earnings and UAX earnings over the life cycle

Finally, the solid line in Figure 2.6 shows the gender gap in UAX earnings as the sum of the annual earnings up to age X. Ultimately, the UA60 earnings coincide with our definition of lifetime earnings. Hence, the higher the age X, the closer UAX earnings are to lifetime earnings. At the beginning of the work life, women earn on average 20% less than men do. The difference in earnings accumulates over the life course and increases to a gender gap in UA40 earnings of 52.7%. After that, the gap remains stable, which results in a gender gap in lifetime earnings of 51.5% (UA60). At this point in life, women have earned on average around 732,000 euros — slightly less than half of the average income that men were able to accumulate (1,510,000 euros).[20]

The evolution of the gender gap in UAX earnings is by construction driven by the gender gap in the annual earnings curve. UAX earnings are less volatile since the marginal effect of adding an additional year of annual earnings to the UAX earnings decreases with increasing age. Hence, the gender gaps in annual and UAX earnings both experience large growth until age 40, but when the gender gap in annual earnings declines again, the UAX gender gap remains at its high level.

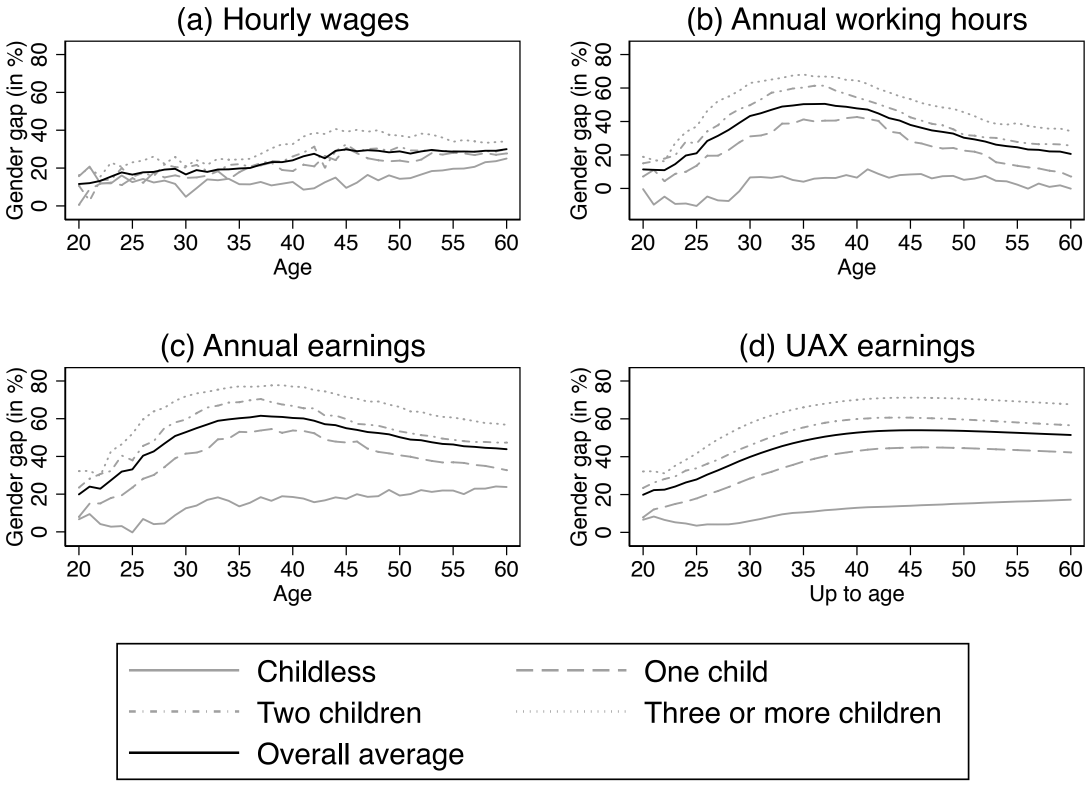
The profound difference in lifetime earnings is largely the result of differences in the extensive and intensive margin of labor supply of women over their lives. One can discuss how labor supply is influenced by own decisions or forced by personal and social circumstances. Previous studies have shown a strong relationship between gender gaps in income and children (e.g., Angelov et al., 2016; Kleven and Landais, 2017; Adda et al., 2017). This can be partially explained by the close connection between women's labor market decisions and the number of children they have (Kühhirt and Ludwig, 2012; Ejrnæs and Kunze, 2013). In line with these studies,

[20] Compare Figure 2.17 and Figure 2.18 for the distribution of annual earnings and UAX earnings by men and women over the work life.

we also find that mothers face higher earning losses with every additional child, while fatherhood does not seem to affect men's earnings. Hence, observed earnings differences between childless women and men are smallest and grow wider with every additional child (see Figure 2.17 in the Appendix). This observation also holds true when we analyze the evolution of UAX earnings by number of children (Figure 2.18 in the Appendix).

Figure 2.7 shows the gender gap in hourly wages (Panel A), the gender gap in hours worked (Panel B), the gender gap in annual earnings (Panel C) and the gender gap in UAX earnings (Panel D) over the life cycle by number of children. In the beginning, the gender gap in hourly wages shows only small gender differences for men and women with and without children but widens over the life cycle. In Section 2.2, we have shown that this is mainly explained by the lesser work experience women with children gain over their life courses. The gender gap in annual earnings clearly differs by the number of children throughout the entire life cycle (see Figure 2.7, Panel C), exacerbating the gap in hourly wages mainly due to mother's lower intensive margin of work (see Figure 2.7, Panel B).

The gender gap in lifetime earnings also increases with the number of children. While childless men and women experience a gender gap of 17.3%, the gap is significantly higher for men and women with three or more children (68.0% at age 60). The significant widening of the gender gap between UA20 and UA35 earnings thereby coincides with the increase in the cross-sectional gender gaps in annual hours worked, and consequently annual earnings. These results are in line with existing studies finding evidence for motherhood penalties and fatherhood premiums (e.g., Budig and England, 2001; Killewald and Gough, 2013; Killewald and García-Manglano, 2016). Therefore, descriptive evidence clearly hints that motherhood might be a key driver of gender earnings inequality over the life cycle.



Notes: Number of children refers to the total number at age 60. Gender gaps in accumulated earnings are earnings up to a given age. Individuals with zero annual and UAX earnings are included in the calculation. *Source:* Own calculations based on SOEP v35.

Figure 2.7: Gender gaps over the life cycle by children

2.3.2.2 Counterfactual analysis

In the last step, we want to determine which part of the observed gender gap in lifetime earnings can be associated with differences in the distribution of characteristics across gender and which part is associated with differences in returns to characteristics. To investigate this issue further, we will predict counterfactual lifetime earnings for women in the following two steps.

First, we take the earnings regression results from our microsimulation model, estimated for male M and female F individuals separately:

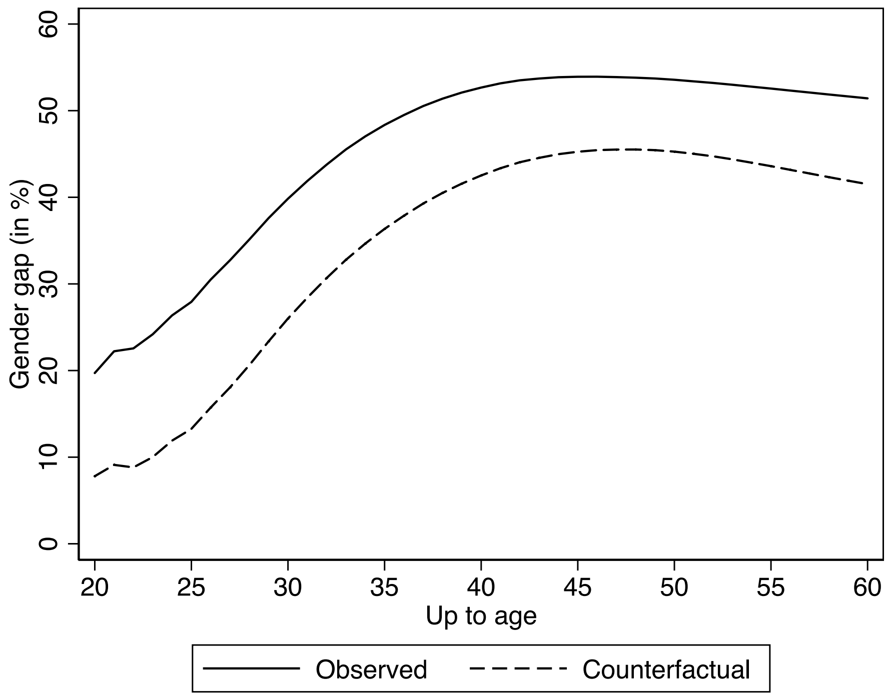
$$\hat{y}_{s,t+1} = \hat{\beta}_{0,s} + \hat{\beta}_{1,s} y_{s,t} + \hat{\beta}_{2,s} y_{s,t-1} + \hat{\beta}_{3,s} X_{s,t}, \quad s \in \{F, M\} \text{ and } t \in [1984, 2017] \tag{2.10}$$

Second, we then estimate women's counterfactual annual earnings $\hat{y}_f^C$ by using the coefficients obtained from the male regression model in the women's Mincer earnings regression:

$$\hat{y}_{f,t+1} = \hat{\beta}_{0,m} + \hat{\beta}_{1,m} \hat{y}_{f,t} + \hat{\beta}_{2,m} \hat{y}_{f,t-1} + \hat{\beta}_{3,m} \hat{X}_{f,t}, \quad t \in [1984, 2017] \tag{2.11}$$

Women's counterfactual annual earnings in year t then represent the salary women would have earned if their characteristics were rewarded the same as men's. Adding up the counterfactual annual earnings for each woman over the life course then yields women's counterfactual UAX earnings. As a result, all differences displayed in the counterfactual gender lifetime earnings gap are solely based on different characteristics for men and women and not by different returns to characteristics.

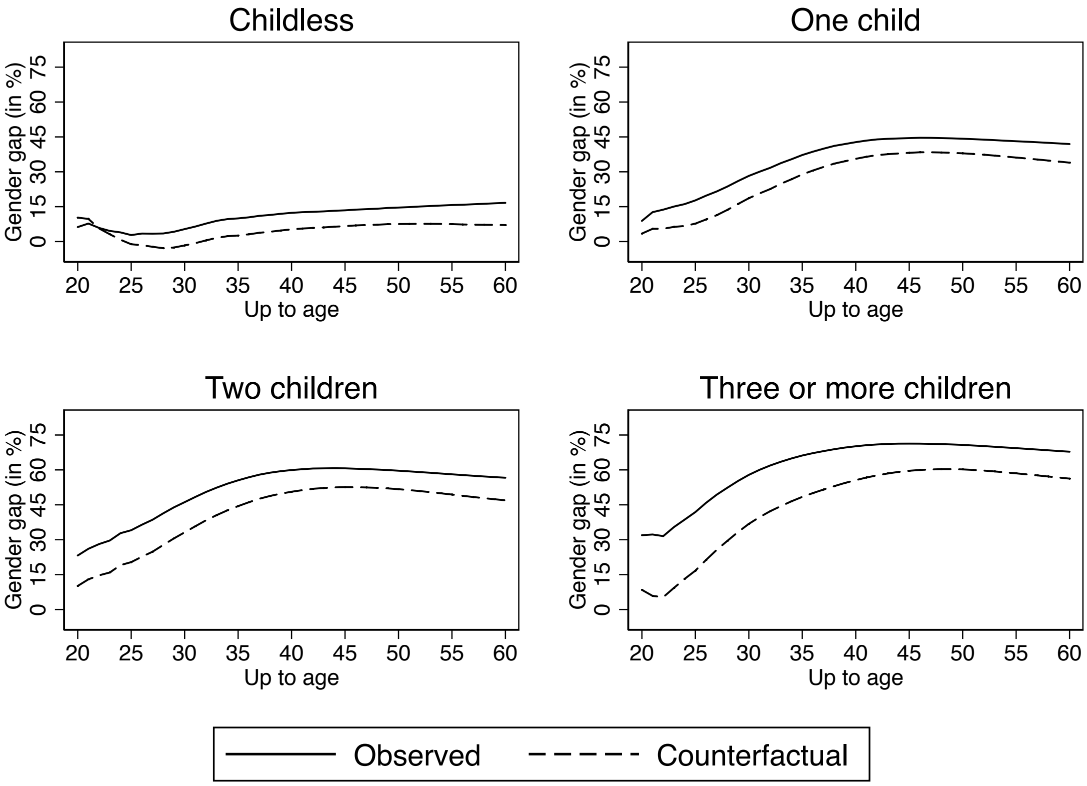
Figure 2.8 compares the observed and counterfactual gender gaps in UAX earnings. That means the difference between the truly observed and the counterfactual gender gap can be interpreted as the unexplained part of the gender gap in UAX earnings (adjusted gender gap). In the beginning of the work life, the difference between both gaps shown in Figure 2.8 is 12.1 pp. Therefore, in early years, approximately half of the gender gap in UAX earnings is due to a different allocation of characteristics and half is due to a different reward or payment of characteristics. The adjusted gender gap then increases to about 14.8% for UA30 earnings and declines afterwards to 10% for lifetime earnings (UA60). Thus, until the years of family formation, the



Notes: Estimated and counterfactual gender gap in UAX earnings. Gender gaps in accumulated earnings are earnings up to a given age. Individuals with zero UAX earnings are included in the calculation. *Source:* Own calculations based on SOEP v35.

Figure 2.8: Counterfactual estimation of the lifetime earnings gap

unexplained difference between women's and men's pay grows, whereas it declines towards retirement. Overall, 80% of the observed gender lifetime earnings gap of 51.5% at age 60 can be explained by a different distribution of labor market characteristics of men and women. Consequently, one fifth of the observed gender lifetime earnings gap of 51.5% at age 60 is due to a less favorable reward for women's labor market characteristics, leading to an overall adjusted gender lifetime earnings gap of around 10%. The evolution of the adjusted gender gap indicates that rewards are least favorable for women in the first half of their work life. As this is the main time for family formation, this might be due to either a sorting of women into worse positions to gain more flexibility or the labor market rewarding women less favorably during this time due to the higher risk of inactivity periods.



Notes: Estimated and counterfactual gender gaps in UAX earnings. Gender gaps in accumulated earnings are earnings up to a given age. Individuals with zero UAX earnings are included in the calculation. *Source:* Own calculations based on SOEP v35.

Figure 2.9: Counterfactual estimation of the lifetime earnings gap by number of children

Next, we want to investigate how motherhood influences the adjusted gender gap in lifetime earnings. Hence, Figure 2.9 compares the observed and counterfactual gender gaps by the number of children. As already shown in Figure 2.7 (Panel D), the observed gender gap in lifetime earnings is lowest for childless women and increases strongly with the number of children women have. But how much of the observed gender gap in lifetime earnings of women with and without children can be explained by a different distribution of characteristics, and what is the influence of the role of motherhood on the adjusted gender gap in lifetime earnings?

Using German data, this paper shows for the first time that in stark contrast to the observed gender gap in UAX earnings, the adjusted gender gap only slightly differs by the number of children women and men have. The difference between childless women and women with three or more children amounts to only 3 pp, with mothers of three or more children facing the highest adjusted gender gap in lifetime earnings with 11.4%. Hence, the large differences in the observed gender gaps of women with and without children are mainly driven by the different accumulation of characteristics rather than an additional unexplained penalty of motherhood.

Overall, we show that the difference in the gender gap in lifetime earnings by motherhood is largely driven by different characteristics women accumulate over their work life. Our results in Section 2.2 and Figure 2.7 (Panel B) indicated that these differences are primarily due to fewer working hours and less work experience which women with children accumulate over their work life. Nevertheless, at the end of the work life women on average face an adjusted gender gap in lifetime earnings of around 10%.

2.4 Qualification and extensions

In this section, I discuss limitations of this study's underlying assumptions and its data source. Additionally, I outline potential extensions for future versions of this paper or further studies building on this work.

As outlined in Section 2.3, this study is not based on data containing complete earning biographies and the simulation of missing information on an individual's career path is an essential part of our analysis. The dynamic microsimulation model implemented in this study relies on a number of assumptions. For example, in order to obtain complete earnings biographies for cohorts that have yet to turn 60 years old, we simulate career trajectories until 2032. This "out-of-sample forecast" for the years 2018-2032 is based on regression parameters of older cohorts while accounting for differences in trends by using cohort- and year-fixed effects. While we are confident that we account for broad labor market trends like increasing female labor market participation over time, we also assume that general labor market characteristics (e.g. unemployment rate) remain generally stable after 2017. As a result, our model neglects the recent unexpected labor market disturbances caused by the COVID-19 pandemic or the economic consequences (e.g. energy crises) of the Russian war on Ukraine. Therefore, our calculated lifetime earnings for the cohorts 1964-72 should be seen as an upper-bound estimate not accounting for the higher risk of unemployment and short-time work arrangements ("Kurzarbeit") that has subsequently actually arisen.

Against this background, despite our extensive efforts to prove robustness and reliability of our results, the simulation of missing information leads to some degree of uncertainty. Therefore, using data offering complete earnings biographies and family information would theoretically be preferable for the analysis of the gender lifetime earnings gap and its link to parenthood. However, until very recently, no such data source for Germany existed. Another option would have been to rely on administrative data from the Sample of Integrated Labour Market Biographies (SIAB) which offers more complete employment biographies but lacks family information. Recent approaches trying to approximate child births (Müller and Strauch, 2017) and to identify couples (Goldschmidt et al., 2017; Bächmann et al., 2021) come with their own shortcomings which would have potentially biased our results. In contrast, the recent SOEP-RV linkage project (Lüthen et al., 2022) combines both the advantages of complete earnings biographies from pension records and the rich set of socioeconomic and family information from the SOEP, making it the "gold standard" for future research on the link between lifetime earnings and parenthood. Comparing our results for the gender lifetime earnings gap to similar investigations based on SOEP-RV data would offer a suitable robustness check which could further add to the credibility of our results.

Another qualification of our study is that while we explore drivers of gender gaps in hourly wages and annual earnings in the cross-section by implementing a decomposition approach (Section 2.2), we do not investigate different channels through which gender differences in lifetime earnings evolve over the life cycle in detail. To additionally focus on the causal identification and extensive analysis of different channels through which children drive the gender lifetime earnings gap is beyond the scope of a single paper. However, there are many opportunities for future research to explore how exactly different channels of parenthood drive (gender) differences in accumulated earnings over the life cycle. For example, one could follow recent studies (e.g., Kleven et al., 2019) implementing an event study approach or analyze the effects of parenthood timing (i.e. the age when parenthood occurs) on human capital accumulation and earnings biographies (e.g., Miller, 2011; Adda et al., 2017).

Additionally, in order to present a more comprehensive measure of individuals' available resources and life chances, future extensions could include analysing disposable (instead of gross) lifetime earnings accounting for the tax and benefit system and household-level instead of individual-level lifetime earnings. Initial work on this topic can be found in Bönke and Glaubitz (2022). Finally, future research on the lifetime earnings gap should aim to also include East German individuals. Jessen (2022) shows that long-run child penalties are considerably lower for East German women than for women from West Germany. He argues that norms due to differences in cultural upbringing play an important in explaining these regional differences in the effect of parenthood on earnings. Against this background, it would be interesting to investigate differences in the gender lifetime earnings gap between East and West Germany.

2.5 Conclusion

This paper underlines the importance of accounting for the biographical dimension when analyzing gender inequalities. First, our results show that cross-sectional gender differences are persistent over the work life. Comparing multiple dimensions of cross-sectional gender differences, we find that the gender gap in hourly wages is substantially smaller (less than half the size) than the gender gap in annual earnings. Using an Oaxaca Blinder decomposition, we show that the gender gap in annual earnings can largely be explained by the extensive and intensive margin of labor, with women accumulating less work experience and working fewer hours.

We then applied a dynamic microsimulation model to obtain full lifetime earnings data including family background information. Using our simulated data, we observe a gender gap in lifetime earnings of 51.5%. Further, we show that the unadjusted gender gap in lifetime earnings increases with the number of children women have. While childless women face an average gender gap in lifetime earnings of 17.3%, mothers with three or more children experience a gap of 68.0%. Furthermore, we used the coefficients from the male earnings regression simulation model to estimate women's counterfactual earnings. As a result, all differences remaining were solely based on different characteristics of men and women and not by different returns to characteristics. The difference between the truly observed gender gap and the counterfactual gap then yielded the adjusted gender gap in lifetime earnings of 10%. This means that women earn on average 10% less than men over their lifetime due to a different reward for their characteristics in comparison to men. We find that in stark contrast to the observed gender gap in UAX earnings, the adjusted gender gap only differs slightly by the number of children women and men have.

The documented gender inequalities in lifetime earnings are high and therefore concerning for a variety of social and economic reasons. For example, fewer financial opportunities for women, and especially mothers, might create unhealthy dependency structures within households (see, e.g., Kalmuss and Straus, 1982). Furthermore, lower lifetime earnings result in significantly lower pensions and consequently a higher risk of poverty among elderly women (see, e.g., Fasang et al., 2013; Grabka et al., 2017). Against this background, it is of high importance to create the right conditions for women to have the opportunity and incentive to increase their labor market participation. One promising suggestion on how to increase work incentives for women in Germany is, for example, a reform of *Ehegattensplitting*, the joint taxation of married couples or civil partners (see, e.g., Bach et al., 2017). Furthermore, the influential study by Olivetti and Petrongolo (2017) stresses the importance of the availability of childcare in this context. For Germany specifically, there is evidence that more extensive provision of adequate childcare would potentially positively influence mothers' labor market participation (e.g., Bauernschuster and Schlotter, 2015; Müller and Wrohlich, 2020). More broadly, fundamental changes in norms regarding the household division of labor are necessary as women still conduct the majority of housework and care-related tasks (see, e.g., Samtleben, 2019). Additionally, employers should offer more flexible work arrangements in order to foster the compatibility of work and family. Indeed, recent studies indicate that such factors might have the potential to foster an increase in women's labor market participation as a considerable share of women who are currently working part-time have the (unrealized) desire to increase their working hours (e.g., Harnisch et al., 2018; Beckmannshagen and Schröder, 2022).

2.6 Appendix

2.6.1 Supplementary material for cross-sectional analyses

Oaxaca Blinder decomposition The Oaxaca Blinder decomposition was simultaneously introduced by Oaxaca (1973) and Blinder (1973) and divides the gender differential in labor market outcomes (here: hourly wage or annual earnings) into an *endowment part* and a *coefficient part*. The endowment part of the gender differential accounts for the part of the gap which can be attributed to differences in the allocation of characteristics (e.g., working hours, highest level of education) between men and women. In contrast, the coefficient part captures the gender differences in labor market returns to characteristics, and therefore in their coefficients. In other words, it states the gender differences of what the labor market is willing to pay for the same characteristics. This part is also called the raw or adjusted gender wage/earnings differential. This adjusted gap, however, also contains the effects of gender differences in unobserved predictors (Jann, 2008). The Oaxaca-Blinder decomposition approach enables us to analyze whether the gender gap in wages/earnings is mainly driven by the different distributions of productivity characteristics or by different rewards for these characteristics by gender.

The gender gap G_x is defined as the difference between the means of the labor market outcomes L at age x of men m and women f:

$$G_x = E(L_{mx}) - E(L_{fx}) \tag{2.12}$$

L_s for either sex (s) is based on the linear model

$$L_{sx} = Z'_{sx}\beta_{sx} + \epsilon_{sx}, \quad E(\epsilon_{sx}) = 0, \; S \in \{f, m\}, \tag{2.13}$$

where the vector Z includes all relevant characteristics, β is the estimation vector and ϵ is the error term. Inserting Equation (2.13) into Equation (2.12), the earnings differential can also be written as:

$$G_x = E(Z_{mx})'\beta_{mx} - E(Z_{fx})'\beta_{fx}. \tag{2.14}$$

For the decomposition of the results, a non-discriminatory coefficient vector is needed, called β^*. Following Neumark (1988), the vector is determined as a pooled regression over both sexes. The gender gap can then be rewritten as:

$$G_x = \underbrace{[E(Z_{mx}) - E(Z_{fx})]'\beta_x^*}_{\text{Endowment part}} + \underbrace{[E(Z_{mx})'(\beta_{mx} - \beta_x^*) + E(Z_{fx})'(\beta_x^* - \beta_{fx})]}_{\text{Coefficient part}} \tag{2.15}$$

where the first part of Equation (2.15) is the endowment part and the second part is the coefficient component of the gender gap in the labor market outcome.

Supplementary tables and figures

Table 2.3: Descriptive statistics - means by age

Men	Age								
	20	25	30	35	40	45	50	55	60
Annual earnings	15748.13 (10972.17)	27727.89 (13306.99)	37925.13 (18571.57)	45217.80 (24095.07)	51615.70 (31182.61)	54204.14 (38951.27)	54747.55 (35380.01)	53969.63 (33505.90)	51535.02 (50496.35)
Hourly wage	9.37 (7.72)	15.13 (18.11)	18.12 (20.76)	20.72 (16.32)	23.06 (16.25)	23.95 (17.91)	24.24 (14.47)	25.83 (31.96)	26.13 (28.33)
Hours worked per week	34.55 (13.42)	38.29 (14.48)	42.81 (12.47)	43.49 (11.38)	44.39 (11.01)	44.10 (10.61)	43.50 (11.17)	42.65 (12.13)	39.34 (14.38)
Years in full-time work	1.20 (1.28)	4.75 (2.60)	8.54 (3.77)	12.97 (4.37)	17.71 (4.85)	22.58 (5.31)	27.43 (5.71)	32.69 (5.81)	37.32 (5.74)
Years in part-time work	0.14 (0.47)	0.33 (0.98)	0.55 (1.56)	0.56 (1.64)	0.61 (1.93)	0.65 (2.10)	0.75 (2.44)	0.68 (2.46)	1.09 (2.90)
Years in unemployment	0.13 (0.38)	0.31 (0.70)	0.39 (0.98)	0.43 (1.21)	0.45 (1.37)	0.47 (1.64)	0.51 (1.85)	0.49 (1.78)	0.45 (1.65)
Years of education	8.97 (3.86)	10.61 (3.03)	11.84 (3.25)	12.44 (3.17)	12.62 (3.03)	12.65 (2.96)	12.67 (2.92)	12.57 (2.84)	12.73 (2.92)
Women	Age								
	20	25	30	35	40	45	50	55	60
Annual earnings	12773.34 (8683.31)	21115.69 (12332.56)	22975.43 (16720.75)	21925.18 (19512.82)	22944.75 (18626.65)	24975.61 (20497.00)	26705.30 (21713.56)	26475.69 (25559.13)	24659.61 (21236.77)
Hourly wage	7.97 (6.58)	12.87 (9.59)	15.19 (12.19)	15.63 (13.18)	16.23 (12.02)	16.23 (10.88)	16.82 (12.49)	16.54 (13.18)	17.48 (14.99)
Hours worked per week	31.75 (13.02)	32.28 (14.34)	29.91 (15.60)	26.68 (15.26)	27.38 (14.29)	28.87 (13.99)	30.09 (13.98)	29.42 (13.64)	26.97 (14.49)
Years in full-time work	1.20 (1.23)	4.36 (2.71)	6.73 (4.12)	8.04 (5.23)	9.63 (6.46)	11.61 (7.99)	14.00 (9.69)	16.70 (11.75)	19.65 (13.99)
Years in part-time work	0.21 (0.55)	0.82 (1.61)	1.91 (2.60)	3.81 (3.76)	5.69 (4.87)	7.60 (6.16)	9.45 (7.75)	11.66 (9.77)	13.32 (11.79)
Years in unemployment	0.17 (0.40)	0.28 (0.73)	0.40 (0.95)	0.50 (1.19)	0.56 (1.52)	0.58 (1.60)	0.64 (1.77)	0.71 (1.99)	0.57 (1.86)
Years of education	9.17 (3.91)	11.17 (2.98)	12.07 (3.31)	12.42 (3.00)	12.48 (2.89)	12.39 (2.91)	12.34 (2.78)	12.11 (2.59)	12.02 (2.71)

Notes: Only employed individuals with hourly wages and annual earnings greater than zero were included. Cohorts 1940-1979, weighted sample. Annual earnings and hourly wages are price-adjusted and presented in 2015 euros. Standard errors in parentheses. *Source:* Own calculations based on SOEP v35.

Table 2.4: Regression results for hourly wages - women

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Age	20	25	30	35	40	45	50	55	60
One child	-0.019	-0.061	-0.159***	-0.023	-0.003	0.075**	0.068*	0.104**	-0.058
	(0.158)	(0.055)	(0.046)	(0.036)	(0.038)	(0.034)	(0.040)	(0.048)	(0.070)
Two children	-0.461	-0.234**	-0.154***	-0.058	-0.000	0.073**	0.138***	0.088*	-0.014
	(0.501)	(0.107)	(0.058)	(0.041)	(0.039)	(0.036)	(0.040)	(0.048)	(0.071)
3 or more children		-0.171	-0.111	-0.167***	-0.030	0.036	0.129***	0.103*	-0.051
		(0.195)	(0.093)	(0.055)	(0.050)	(0.043)	(0.048)	(0.058)	(0.083)
Married	0.033	-0.038	0.054	0.008	0.004	0.068**	-0.052*	-0.031	0.055
	(0.100)	(0.036)	(0.034)	(0.030)	(0.029)	(0.027)	(0.029)	(0.035)	(0.048)
Years FT	0.445***	0.055**	0.059***	0.028***	0.035***	0.026***	0.027***	0.014**	0.030***
	(0.061)	(0.022)	(0.014)	(0.009)	(0.007)	(0.005)	(0.005)	(0.006)	(0.007)
Years FT (sq)	-0.054***	-0.003	-0.002***	0.000	-0.001*	-0.000	-0.000	-0.000	-0.000**
	(0.016)	(0.002)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Years PT	-0.023	-0.039	-0.028	-0.019*	-0.013	-0.020***	-0.002	-0.001	-0.011
	(0.103)	(0.029)	(0.017)	(0.011)	(0.008)	(0.006)	(0.006)	(0.006)	(0.008)
Years PT (sq)	0.012	0.001	0.003*	0.002**	0.001**	0.001***	0.000*	0.000	0.000**
	(0.028)	(0.004)	(0.002)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Years UE	-0.689***	-0.101**	-0.174***	0.013	-0.062***	-0.096***	-0.076***	-0.058***	-0.044*
	(0.209)	(0.043)	(0.039)	(0.021)	(0.018)	(0.018)	(0.018)	(0.017)	(0.024)
Years UE (sq)	0.236*	-0.000	0.034***	-0.005**	0.004**	0.007***	0.004**	0.001	0.001
	(0.140)	(0.006)	(0.009)	(0.003)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)
Part-time	0.244***	0.272***	0.110***	0.182***	0.161***	0.102***	0.023	-0.068*	0.069
	(0.081)	(0.049)	(0.042)	(0.032)	(0.031)	(0.027)	(0.031)	(0.039)	(0.054)
Education	-0.068***	0.007	-0.033**	-0.063***	0.018	0.008	-0.023	0.012	-0.052
	(0.024)	(0.017)	(0.015)	(0.015)	(0.023)	(0.015)	(0.021)	(0.029)	(0.036)
Education (sq)	0.004*	0.001	0.003***	0.006***	0.002***	0.003***	0.004***	0.002**	0.005***
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Constant	2.241***	1.714***	1.233***	1.700***	1.825***	1.508***	1.033***	1.614***	1.898***
	(0.770)	(0.266)	(0.211)	(0.222)	(0.199)	(0.161)	(0.313)	(0.373)	(0.463)
Obs.	382	882	1307	1859	2493	2653	2043	1320	778
R-squared	0.323	0.127	0.187	0.240	0.192	0.219	0.205	0.213	0.248
Cohort-FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Sector-FE	YES	YES	YES	YES	YES	YES	YES	YES	YES

Notes: Standard errors in parentheses; The stars refer to the following significance level: $*p < 0.1$, $**p < 0.05$, $***p < 0.01$. *Source:* Own calculations based on SOEP v35.

Table 2.5: Regression results for hourly wages - men

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Age	20	25	30	35	40	45	50	55	60
One child	0.160	0.089	-0.017	-0.017	0.007	0.029	-0.043	0.026	0.053
	(0.349)	(0.055)	(0.031)	(0.024)	(0.022)	(0.024)	(0.029)	(0.040)	(0.053)
Two children	-0.952	0.134*	0.065*	0.069***	0.033	0.103***	-0.000	0.034	-0.010
	(0.751)	(0.079)	(0.035)	(0.024)	(0.022)	(0.023)	(0.027)	(0.037)	(0.049)
3 or more children	-0.006	0.139*	-0.024	0.015	0.050*	0.049*	-0.013	0.075	0.203***
	(0.173)	(0.075)	(0.045)	(0.033)	(0.028)	(0.029)	(0.035)	(0.049)	(0.067)
Married	-0.026	0.015	0.164***	0.085***	0.101***	0.069***	0.084***	0.020	0.105**
	(0.166)	(0.041)	(0.027)	(0.023)	(0.021)	(0.023)	(0.026)	(0.035)	(0.048)
Years FT	0.737***	0.164***	0.105***	0.058***	0.053***	0.045***	0.032***	0.040***	-0.030
	(0.061)	(0.023)	(0.013)	(0.008)	(0.007)	(0.008)	(0.009)	(0.012)	(0.036)
Years FT (sq)	-0.108***	-0.012***	-0.006***	-0.002***	-0.001***	-0.001***	-0.001***	-0.001**	0.001
	(0.015)	(0.002)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Years PT	0.225	-0.197***	-0.074***	-0.020*	-0.056***	-0.038***	-0.069***	-0.070***	-0.057***
	(0.181)	(0.036)	(0.018)	(0.012)	(0.010)	(0.011)	(0.010)	(0.014)	(0.017)
Years PT (sq)	-0.079	0.021***	0.006***	0.001	0.003***	0.003***	0.003***	0.003***	0.001*
	(0.071)	(0.005)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Years UE	-0.279	-0.108**	-0.178***	-0.113***	-0.117***	-0.105***	-0.094***	-0.069***	-0.094***
	(0.195)	(0.050)	(0.029)	(0.015)	(0.013)	(0.012)	(0.014)	(0.019)	(0.036)
Years UE (sq)	0.099	-0.005	0.023***	0.004***	0.006***	0.006***	0.004***	0.002**	0.006
	(0.101)	(0.014)	(0.006)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.004)
Part-time	0.271***	0.422***	0.257***	0.173***	0.336***	0.251***	0.189***	0.389***	0.201***
	(0.075)	(0.054)	(0.043)	(0.034)	(0.031)	(0.031)	(0.039)	(0.045)	(0.054)
Education	-0.031	-0.038***	-0.071***	-0.039***	-0.051***	0.040*	0.060***	0.002	0.020
	(0.030)	(0.013)	(0.010)	(0.010)	(0.013)	(0.022)	(0.023)	(0.055)	(0.067)
Education (sq)	0.003	0.003***	0.005***	0.003***	0.004***	0.001*	0.000	0.002	0.002
	(0.003)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.002)	(0.002)
Constant	1.996***	1.591***	2.263***	2.331***	2.519***	1.703***	1.673***	1.875***	1.318
	(0.504)	(0.163)	(0.183)	(0.172)	(0.173)	(0.187)	(0.231)	(0.437)	(0.926)
Obs.	383	900	1746	2464	2863	2939	2261	1546	980
R-squared	0.449	0.231	0.185	0.229	0.277	0.283	0.252	0.184	0.208
Cohort-FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Sector-FE	YES	YES	YES	YES	YES	YES	YES	YES	YES

Notes: Standard errors in parentheses; The stars refer to the following significance level:
$*p < 0.1, **p < 0.05, ***p < 0.01$.

Source: Own calculations based on SOEP v35.

Table 2.6: Regression results for annual earnings - women

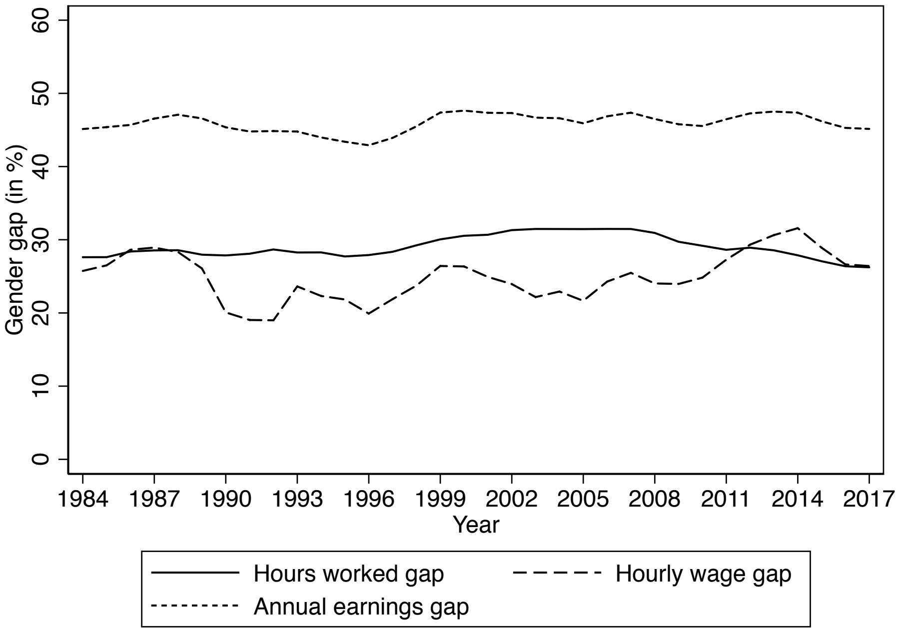
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Age	20	25	30	35	40	45	50	55	60
One child	0.113	-0.033	-0.130***	-0.008	-0.005	0.101***	0.059	-0.086	-0.044
	(0.150)	(0.051)	(0.045)	(0.036)	(0.037)	(0.034)	(0.039)	(0.047)	(0.071)
Two children	-0.485	-0.234**	-0.205*****	-0.044	0.007	0.092***	0.131***	0.066	-0.024
	(0.486)	(0.100)	(0.059)	(0.041)	(0.039)	(0.035)	(0.039)	(0.048)	(0.071)
3 or more children		-0.056	0.174*	-0.160***	-0.011	0.081*	0.132***	0.079	-0.034
		(0.183)	(0.093)	(0.056)	(0.049)	(0.043)	(0.047)	(0.058)	(0.083)
Married	0.040	-0.062*	0.038	0.044	-0.004	0.102***	-0.036***	-0.013	0.048**
	(0.096)	(0.034)	(0.034)	(0.031)	(0.029)	(0.026)	(0.028)	(0.035)	(0.048)
Years FT	0.466***	0.098***	0.065***	0.031***	0.036***	0.025***	0.023***	0.009*	0.031***
	(0.060)	(0.021)	(0.014)	(0.009)	(0.007)	(0.005)	(0.005)	(0.006)	(0.007)
Years FT (sq)	-0.059***	-0.006***	-0.003***	-0.000	-0.001**	-0.000	-0.000	0.000	-0.000**
	(0.015)	(0.002)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Years PT	-0.099	-0.033	-0.005	-0.001	-0.002	-0.010*	0.002	-0.001	-0.011
	(0.103)	(0.027)	(0.016)	(0.011)	(0.008)	(0.006)	(0.006)	(0.006)	(0.008)
Years PT (sq)	0.034	0.002	0.001	0.001	0.001	0.001***	0.000	0.000	0.000**
	(0.028)	(0.004)	(0.002)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Years UE	-0.524***	-0.076*	-0.174***	-0.010	-0.075***	-0.097***	-0.079***	-0.050***	-0.047*
	(0.197)	(0.040)	(0.039)	(0.021)	(0.018)	(0.018)	(0.018)	(0.017)	(0.024)
Years UE (sq)	0.141	-0.002	0.033***	-0.006**	0.005***	0.007***	0.004**	0.001	0.001
	(0.134)	(0.006)	(0.009)	(0.003)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)
Weekly hours	0.066***	0.054***	0.081***	0.093***	0.091***	0.087***	0.105***	0.113***	0.093***
	(0.009)	(0.004)	(0.004)	(0.003)	(0.003)	(0.002)	(0.003)	(0.004)	(0.006)
Weekly hours (sq)	-0.001***	-0.000***	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Education	-0.046**	0.002	-0.034**	-0.071***	0.013	0.007	-0.024	0.012	-0.051
	(0.023)	(0.016)	(0.015)	(0.015)	(0.023)	(0.015)	(0.020)	(0.028)	(0.036)
Education (sq)	0.002	0.001	0.003***	0.006***	0.003***	0.003***	0.004***	0.002**	0.005***
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Constant	7.583***	8.273***	8.012***	7.637***	7.141***	7.141***	7.140***	6.986***	7.550***
	(0.197)	(0.137)	(0.130)	(0.126)	(0.169)	(0.119)	(0.153)	(0.213)	(0.277)
Obs.	382	882	1307	1859	2493	2653	2043	1320	778
R-squared	0.573	0.540	0.627	0.663	0.578	0.599	0.674	0.681	0.660
Cohort-FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Sector-FE	YES	YES	YES	YES	YES	YES	YES	YES	YES

Notes: Standard errors in parentheses; The stars refer to the following significance level: $*p < 0.1$, $**p < 0.05$, $***p < 0.01$. *Source:* Own calculations based on SOEP v35.

Table 2.7: Regression results for annual earnings - men

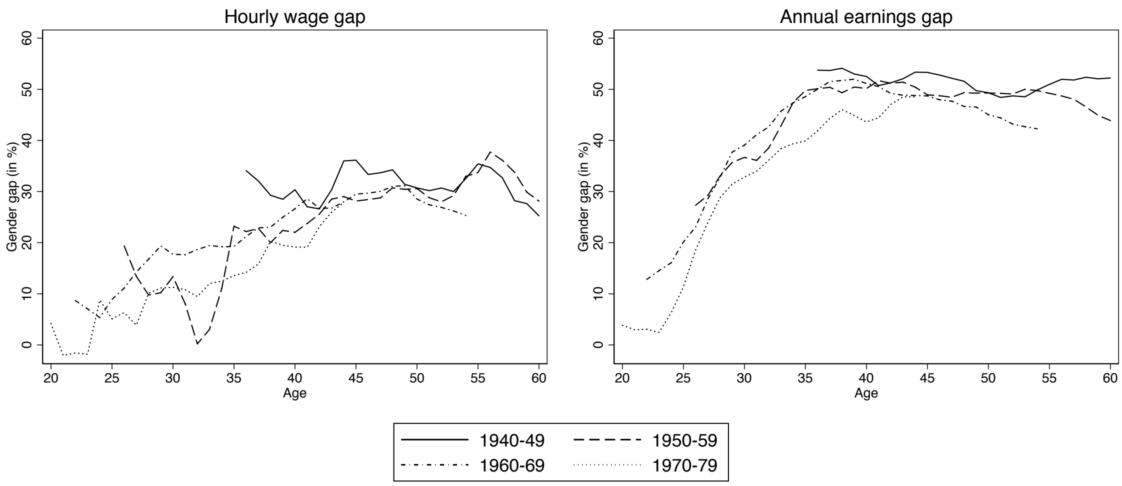
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Age	20	25	30	35	40	45	50	55	60
One child	0.189	0.061	-0.008	-0.014	0.014	0.028	-0.042	-0.003	0.073
	(0.332)	(0.052)	(0.028)	(0.023)	(0.021)	(0.024)	(0.028)	(0.038)	(0.051)
Two children	-1.006	0.139*	0.066**	0.067***	0.026	0.091***	-0.008	0.019	0.015
	(0.714)	(0.075)	(0.032)	(0.023)	(0.021)	(0.022)	(0.026)	(0.036)	(0.047)
3 or more children	-0.008	0.128*	-0.026	0.025	0.050*	0.051*	0.013	0.100**	0.157**
	(0.164)	(0.071)	(0.041)	(0.032)	(0.026)	(0.028)	(0.034)	(0.047)	(0.065)
Married	0.017	0.021	0.121***	0.078***	0.089***	0.054**	0.091***	0.028	0.099**
	(0.158)	(0.039)	(0.025)	(0.022)	(0.020)	(0.022)	(0.025)	(0.034)	(0.046)
Years FT	0.731***	0.179***	0.094***	0.058***	0.045***	0.043***	0.031***	0.052***	-0.033
	(0.058)	(0.022)	(0.012)	(0.008)	(0.006)	(0.008)	(0.009)	(0.012)	(0.035)
Years FT (sq)	-0.106***	-0.013***	-0.005***	-0.002***	-0.001***	-0.001***	-0.001**	-0.001***	0.001
	(0.014)	(0.002)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Years PT	0.095	-0.191***	-0.044***	-0.020*	-0.060***	-0.042***	-0.057***	-0.079***	-0.056***
	(0.174)	(0.034)	(0.016)	(0.011)	(0.010)	(0.011)	(0.010)	(0.013)	(0.015)
Years PT (sq)	-0.051	0.024***	0.003	0.000	0.003***	0.003***	0.003***	0.003***	0.001*
	(0.067)	(0.005)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Years UE	-0.204	-0.107**	-0.188***	-0.110***	-0.118***	-0.101***	-0.091***	-0.109***	-0.122***
	(0.186)	(0.047)	(0.026)	(0.014)	(0.012)	(0.012)	(0.014)	(0.018)	(0.035)
Years UE (sq)	0.040	-0.005	0.023***	0.004***	0.006***	0.005***	0.004***	0.004***	0.008**
	(0.097)	(0.014)	(0.005)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.004)
Weekly hours	0.044***	0.049***	0.070***	0.052***	0.037***	0.054***	0.077***	0.053***	0.071***
	(0.009)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)	(0.005)
Weekly hours (sq)	-0.000***	-0.000***	-0.001***	-0.000***	-0.000***	-0.000***	-0.001***	-0.000***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Education	-0.018	-0.040***	-0.076***	-0.041***	-0.057***	0.039*	0.053**	0.091*	0.019
	(0.028)	(0.012)	(0.009)	(0.009)	(0.012)	(0.021)	(0.023)	(0.053)	(0.065)
Education (sq)	0.003	0.004***	0.005***	0.004***	0.004***	0.002**	0.001	-0.001	0.002
	(0.002)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.002)	(0.002)
Constant	7.710***	8.367***	8.434***	8.715***	9.163***	7.965***	7.506***	7.527***	8.427***
	(0.195)	(0.113)	(0.099)	(0.103)	(0.115)	(0.174)	(0.201)	(0.394)	(0.730)
Obs.	383	900	1746	2464	2863	2939	2261	1546	980
R-squared	0.542	0.539	0.481	0.394	0.409	0.417	0.437	0.400	0.522
Cohort-FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Sector-FE	YES	YES	YES	YES	YES	YES	YES	YES	YES

Notes: Standard errors in parentheses; The stars refer to the following significance level: $*p < 0.1$, $**p < 0.05$, $***p < 0.01$. *Source:* Own calculations based on SOEP v35.



Notes: Only employed individuals are considered. Does not include values of zero annual earnings. Cohorts 1940-1979, weighted sample. *Source:* Own calculations based on SOEP v35.

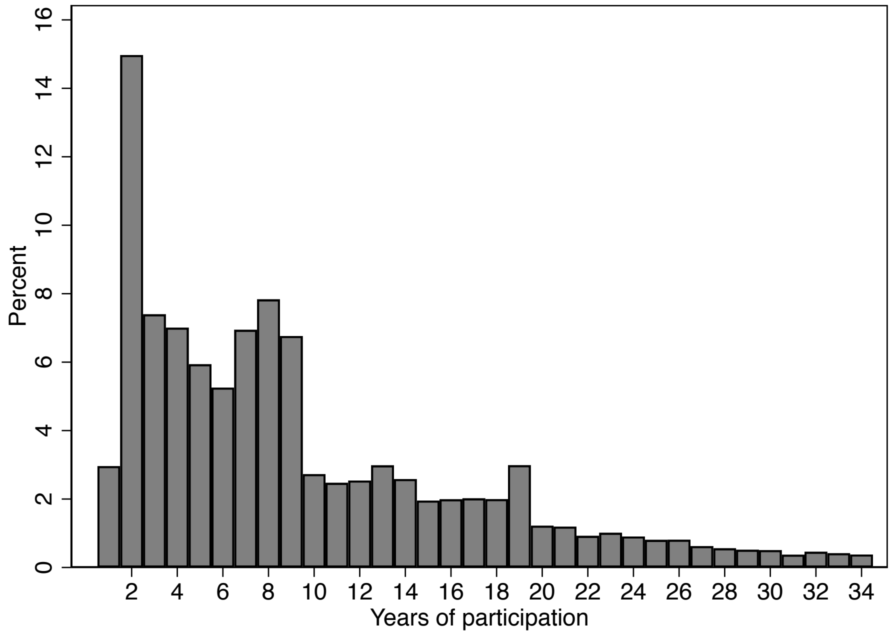
Figure 2.10: Gender gaps in labor market outcomes by survey year



Notes: Only employed individuals are considered. Does not include values of zero hourly wages or annual earnings. Cohorts 1940-1979, weighted sample. *Source:* Own calculations based on SOEP v35.

Figure 2.11: Gender gaps in hourly wages and annual earnings by cohort

2.6.2 Supplementary material for lifetime analyses



Notes: Refers to participation years of the SOEP sample used in 2.2 of this paper. Restrictions for the microsimulation in Section 2.3 are not applied here. *Source:* Own calculations based on SOEP v35.

Figure 2.12: Distribution of participation years in the SOEP

Table 2.8: Distribution of cohorts by gender

Birth Cohort	Number of Men	Number of Women	Total
1964	382	324	706
1965	373	383	756
1966	404	425	829
1967	385	401	786
1968	378	385	763
1969	388	387	775
1970	311	364	675
1971	303	342	645
1972	288	304	592
Total	3212	3315	6527

Source: Own calculations based on SOEP v35.

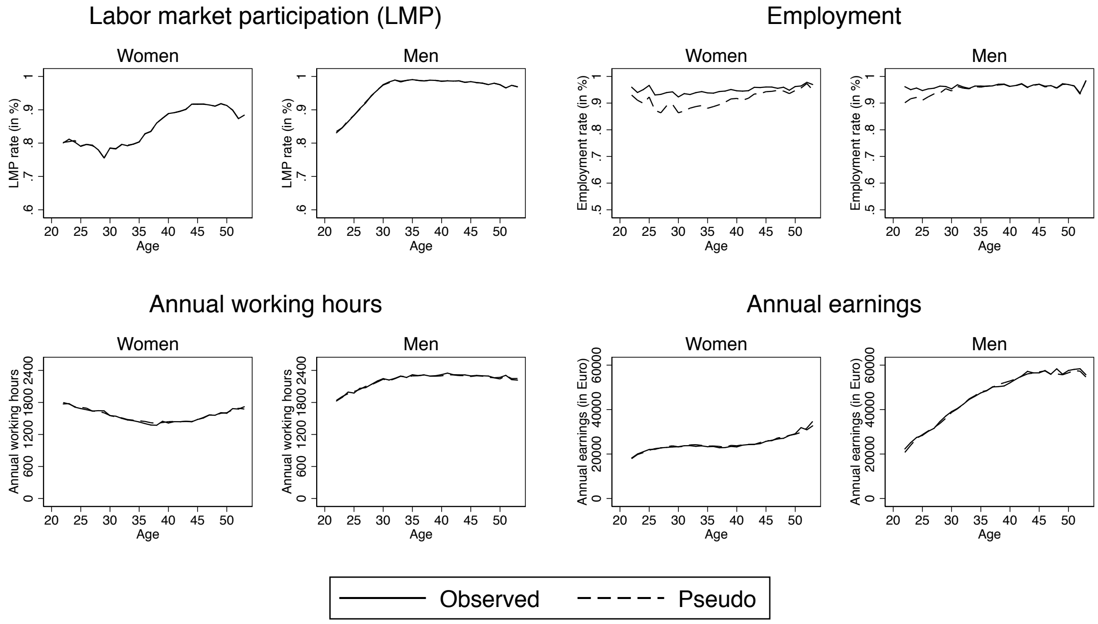
Table 2.9: Overview regression models of the dynamic microsimulation

Dependent Variables	Explanatory Variables
Child birth in t+1 (Logit)	Number of children, age of youngest child, earnings; Additionally, for married women: partner's age, highest level of education and earnings; Run separately for married women and single women
Change in marital status (married/single) in t+1 (Logit)	Marriage duration term interacted with age, number of children; Additionally, for women: age of youngest child; Additionally, for married individuals: Partner's age and highest level of education; Run separately for men and women for each respective marital status
Change in labor force status in t+1 (Logit)	Labor force status in t and t-1, labor market history (years in full-time, part-time, unemployment), number of children (not for unmarried men); Additionally, for women: number of years since birth of last child; Additionally, for married individuals: partner's labor force status and earnings in t; Run separately for men and women for each respective marital status
Change in employment status (working/ unemployed) in t+1 (Logit)	Employment status in t-1, labor market history (years in full-time, part- time, unemployment), number of children (not for unmarried men); Additionally, for women: number of years since birth of last child; Additionally, for married individuals: partner's employment status and earnings of the in t; Run separately for men and women for each possible combination of marital and employment status in t
Transition in employment or unemployment in t+1 after not participating in the labor market in t (Logit)	Employment status in t-1, labor market history (years in full-time, part-time, unemployment), number of children (not for unmarried men); Additionally, for women: number of years since birth of last child; Additionally, for married individuals: partner's employment status and earnings in t; Run separately for men and women for each respective marital status (requirement: participating in the labor market in t+1)
Transition full-time work/ part-time work in t+1 (Logit)	Labor force status in t-1, dummy variable indicating full-time or part- time work in t-1, labor market history (years in full-time, part-time, unemployment), number of children (not for unmarried men); Additionally, for women: number of years since birth of last child; Additionally, for married individuals: partner's employment status and earnings of the partner in t; Run separately for men and women for each possible combination of marital and full-time/ part-time status in t
Transition in full- time work/ part-time work in t+1 after not working in t (Logit)	Labor force status in t-1, dummy variable indicating full-time or part-time work in t-1, labor market history (years in full-time, part-time, unemployment), number of children (not for unmarried men)-, Additionally, for women: number of years since birth of last child; Additionally, for married individuals: employment status and earnings of the partner in t; Run separately for men and women for each respective marital status (requirement: working in t + 1)
Number of working hours in t (OLS)	Annual hours worked in t-1 and t-2, annual earnings in t-1, dummy variable indicating full-time or part-time work in and labor market status t-1, number of children (not for unmarried men); Additionally, for married individuals: earnings of the partner in t-1; Run separately for men and women for each respective marital and work (full-time/part-time) status
Annual earnings in t (OLS)	Annual earnings in t-1 and t-2, annual hours worked in t, t-1 and t-2, labor market history (years in full-time, part-time, unemployment), dummy indicating marital status; Run separately for men and women

Notes: Explanatory variables which are included in every model: highest level of education and year of birth interacted with (quadratic) age, place of residency before 1989 (East or West Germany), immigration background (yes or no). This table depicts forward-looking simulations. Backward-looking simulations function analogously. *Source:* Own calculations based on SOEP v35.

Robustness: Microsimulation

Pseudo missings To test the robustness of our simulation model further, we use the concept of pseudo missings. To that end, we set truly observed information for some part of the sample missing (pseudo missings) and predict their now missing observations again by using our dynamic microsimulation and the regression coefficients previously obtained. As we need a starting point of at least two observations for our models due to the lagged terms, we use the first two truly observed years for everyone before starting to create pseudo missings. Figure 2.13 shows the differences between the simulated pseudo missings (dashed line) and the truly observed information (solid line) for labor force status, employment status, annual working hours and annual earnings. In most graphs, the level of accuracy of the model is so high that it is hard to even tell the solid and dashed line apart. For labor market status, the model predicts 99.9% of all pseudo missings correctly. And even for employment status, where there appear to be bigger differences between pseudo missing and observations at a first glance, overall 97.7% of all cases are simulated correctly. These results further support the robustness of our simulation model.



Notes: The graphs comparing truly observed and simulated pseudo information for annual working hours and annual earnings only focus on employed individuals. *Source:* Own calculations based on SOEP v35.

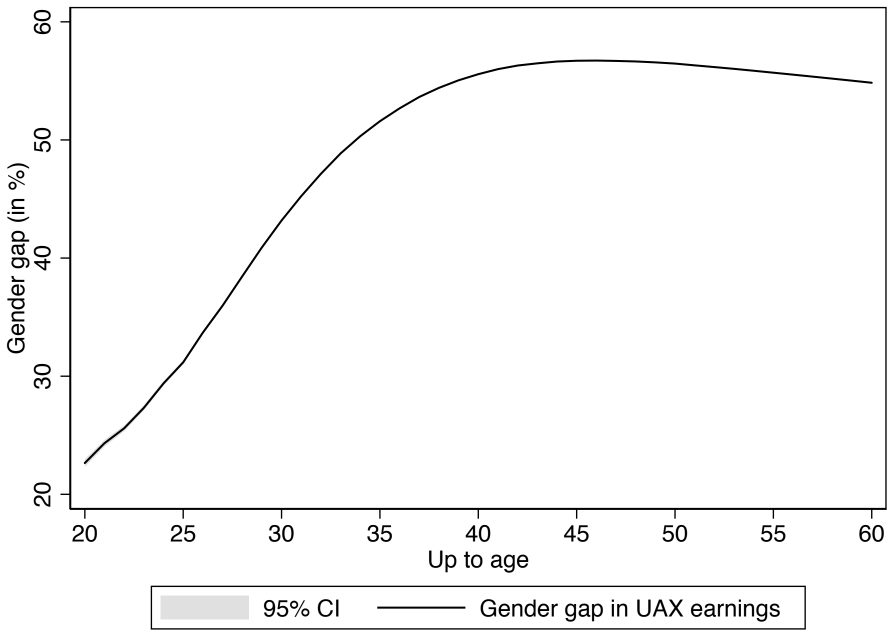
Figure 2.13: Pseudo missings for labor market outcomes

Monte Carlo simulation Another way to validate the robustness of our dynamic microsimulation model is to make use of the underlying random process described in Subsection 2.3.1.1. We implement a Monte Carlo simulation approach by simulating each individual's employment biographies 100 times. By doing so, due to the underlying random process determining transitions in labor market outcome variables between $t-1$ and t, we simulate up to 100 different employment biographies for each individual. However, due to limited computational capacities we only simulate the employment variables (labor market status, employment status, full-time/part-time work, annual working hours and annual earnings) and keep the family information (number of children and marital status) constant for each of the 100 iterations. In the next step, we calculate lifetime earnings for each of the 100 simulated career paths per individual and compute the average lifetime earnings and the resulting

UAX earnings gender gap in the population for each of the 100 runs. By deriving the 95% confidence intervals we can analyze whether average lifetime earnings vary significantly for different underlying random processes or whether they are robust. The results are presented in Figures 2.14 and 2.15. Figure 2.14 shows that lifetime earnings by cohorts are very robust. However, lifetime earnings of women vary more strongly than men's. Figure 2.15 provides evidence for a very narrow 95% confidence interval for the gender gap in UAX earnings. Consequently, the results of the Monte Carlo simulation confirm the high robustness of our simulation outcomes.

Source: Own calculations based on SOEP v35.

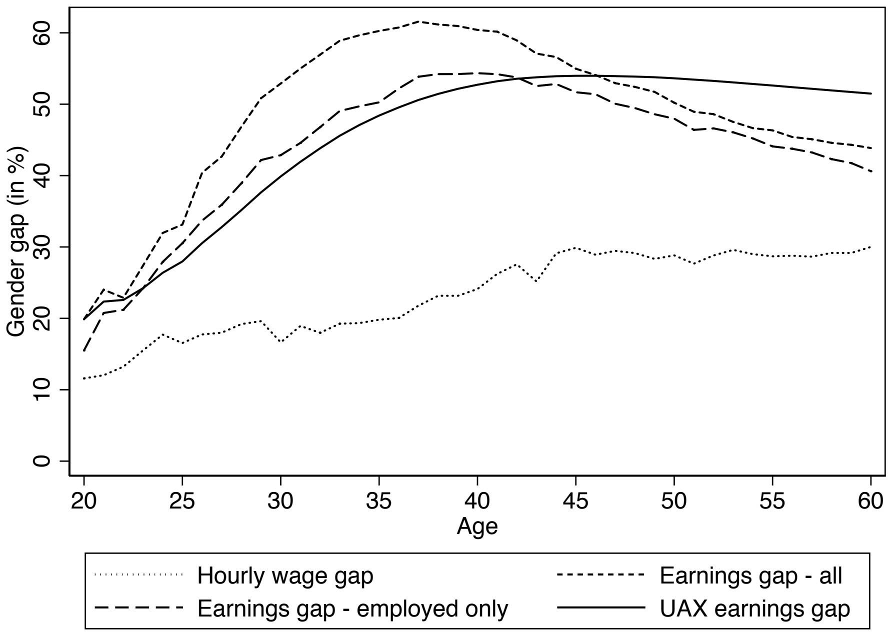
Figure 2.14: Monte Carlo simulation for earnings



Source: Own calculations based on SOEP v35.

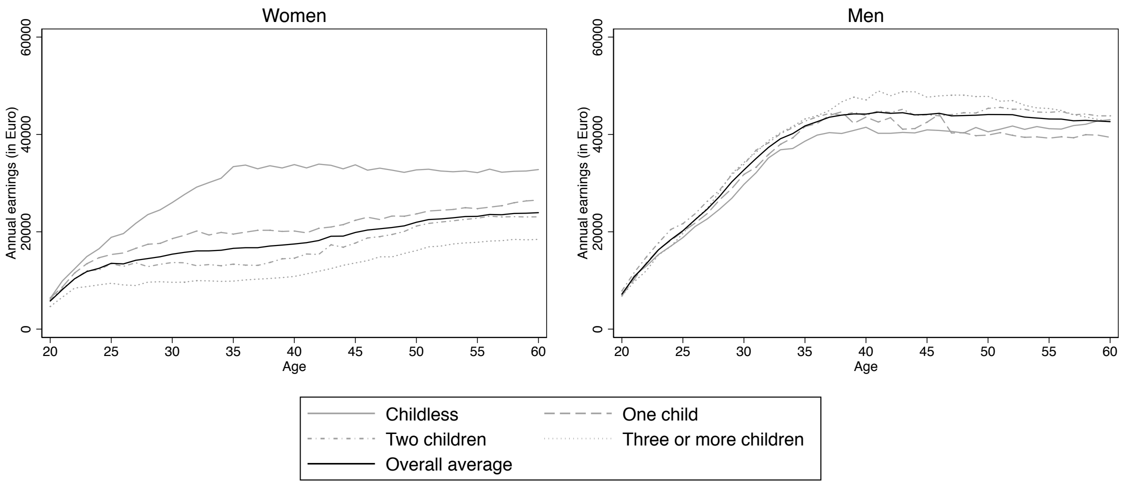
Figure 2.15: Monte Carlo simulation for the gender gap in UAX earnings

Additional Figures



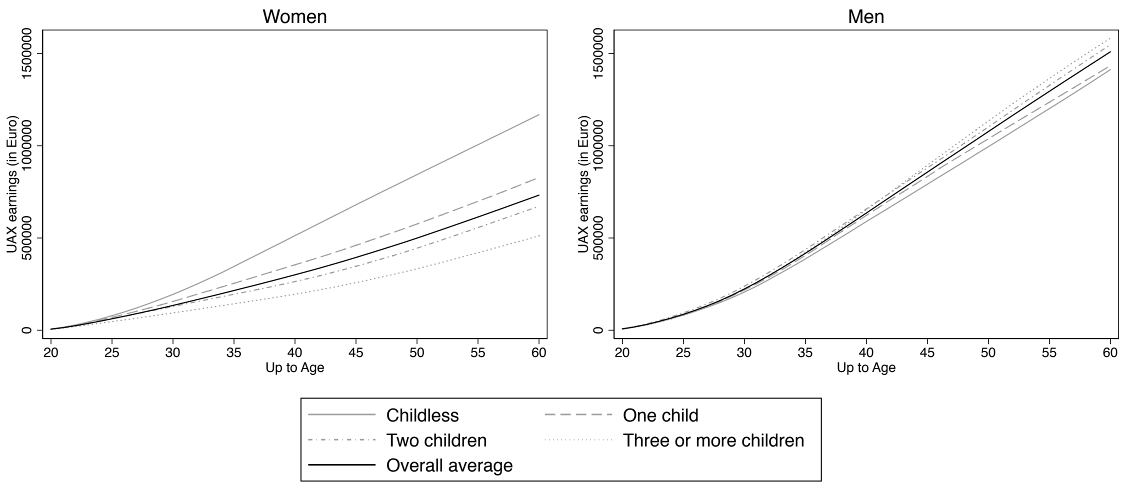
Notes: Individuals with zero UAX earnings are included in the calculation. For annual earnings gap, all employed and unemployed individuals are considered. Cohorts 1964-1972. *Source:* Own calculations based on SOEP v35.

Figure 2.16: Gender gaps in earnings by different concepts



Notes: Employed and unemployed individuals are considered. Number of children refers to the total number at age 50. Cohorts 1964-1972. *Source:* Own calculations based on SOEP v35.

Figure 2.17: Annual earnings by gender and number of children



Notes: Employed and unemployed individuals are considered. Number of children refers to the total number at age 50. Cohorts 1964-1972. *Source:* Own calculations based on SOEP v35.

Figure 2.18: UAX earnings by gender and number of children

3 Is there a desired added worker effect? Evidence from involuntary job losses

3.1 Introduction

In the early 2020s, labor markets around the world have been facing great challenges. Globally, economies have been suffering from the consequences of the COVID-19 pandemic and the Russian invasion of Ukraine. As other European countries, Germany has experienced a spike in energy prices and high inflation. This economic climate has presented various challenges for many firms, leading to an increased risk of layoffs and bankruptcies. Furthermore, recent advancements in artificial intelligence and automation technologies have started the Fourth Industrial Revolution ("industry 4.0"), which is transforming whole industries and predicted to lead to the replacement of entire occupations and the creation of new jobs (Dauth et al., 2021). Against this background, the recent upsurge of economic literature examining the effects of firm closures or, more generally, job losses (Illing et al., 2021; Jarosch, 2023; Schmieder et al., 2023) appears to be very timely.

For affected households, an involuntary job loss constitutes a negative shock to their household income. This income shock is particularly large if the partner affected by the job loss is the household's main earner. One focus of the economic literature has been to examine how households react to such income shocks and, in particular, whether or not the other partner tries to compensate for the loss of income by expanding their labor supply. The international evidence for these “added worker effects”, as they are known in the literature (Lundberg, 1985), is mixed and their existence and magnitude likely depends on the design and generosity of national unemployment insurance schemes (Bredtmann et al., 2018). For Germany, recent studies find little evidence for a significant added worker effect (Fackler and Weigt, 2020; Illing et al., 2021).

Theoretically, there are at least two potential explanations for the absence of an added worker effect. First, individuals might not want or need to increase their labor supply in response to their partners’ job loss. This can be due to different reasons: There might be a lack of incentive due to the generosity of the benefit system or there might be a lack of capacity or flexibility due to strong intra-household specialization. For example, one partner might mainly focus on breadwinning while the other partner shifts their time to housework and care work. In such cases, it might be more cumbersome to change these roles within the household than it is for the partner who has lost their job to find a new one.

An alternative potential explanation for an absence of the added worker effect is that while partners would in fact prefer to adjust their labor supply, they are not able to realize their preferred labor supply adjustment. At the intensive margin, for example, employees and their employers might not find a compromise regarding the extent of working hours adjustments. One can imagine a scenario wherein an employee might only want to increase their working hours from 20 hours per week to 25 hours per week but the employer only offers a full-time position (40 hours) or a part-time position (20 hours). Furthermore, timing might be a factor: an individual might want to increase hours immediately after their partner’s job loss, but the employer might need a considerable amount of time to restructure processes at work. By then, the partner could already be re-employed. Similar issues might arise at the extensive margin: an inactive partner might want to take up employment but might be unable to immediately find a suitable job due to a lack of experience or search frictions. Thus, while there may be a preference for an extension of labor supply, people may fail to realize it.

However, existing empirical studies on the added worker effect solely focus on actual working hours (e.g., Fackler and Weigt, 2020; Illing et al., 2021). As is common in labor economics, authors rely on the axiom of *revealed* labor supply preferences, assuming that actual working hours fully reflect an individual's labor supply preferences. A potential mismatch between the actual working hours and the individual's *stated* labor supply preferences is hereby overlooked. The idea that the response in observed working hours to a partner's job loss may not necessarily reflect the desired change in working hours was first proposed by Maloney (1987). However, to the best of our knowledge, no empirical study to date has extensively analyzed the effect of a partner's job loss on an individual's desired working hours.[1]

Using data from the German Socio-Economic Panel (SOEP, see Goebel et al., 2019) which offers information on both actual and desired labor supply, we examine reactions in actual and desired working hours to a partner's involuntary job loss. Thus—while there might not be an actual added worker effect—we shed light on the question of whether a *desired* added worker effect exists. In our main analysis, we focus on exploring the effect of men's job loss on the actual and desired working

[1] Triebe (2015) touches on the effect of job loss on partners' stated preferences for an extension of labor supply in one of her sub-analyses. However, her analysis is very limited. As she compares desired working hours post-treatment with actual working hours pre-treatment, the analysis neglects the potential pre-existence of hours mismatches and therefore does not warrant a plausible interpretation of the effect of partners' job loss on desired working hours.

hours of their female partners. The reason for this procedure is that women are more often working part time and thus have a higher potential for an extension of labor supply.[2] Accordingly, our main contribution lies in discovering whether or not there is a mismatch between women's responses in actual labor supply and their stated labor supply preferences after their male partners involuntarily lose their jobs. In doing so, we also analyze whether household income shocks constitute one root of mismatches between desired and actual working hours, a topic which has received more attention through the grown literature on labor market imperfections (Manning, 2013; Faberman et al., 2020).

We implement an event study analysis to compare the labor supply preferences of women whose male partners are affected by an involuntary job loss to those of women whose households do not experience such a shock to household income but are very similar in other individual and household characteristics. We distinguish between couples in which the female partner was in employment when her male partner lost his job and couples in which the female partner was not employed at the time of her partner's job loss. For the first group of couples, we consider actual and desired working hours. For the latter, we explore the women's probability of taking up employment, their stated intent to do so and their job search effort. We also run an extensive set of sensitivity analyses to assess the robustness of our empirical strategy, investigate effect heterogeneity, and examine the reactions of strongly affected households.

We do not find evidence for a significant desired added worker effect on the extensive and intensive margin. In fact, actual and desired working hours largely remain

[2]See Section 3.2 for a more extensive discussion. The full analysis is additionally conducted for men's (desired) labor market responses to their female partners' involuntary job loss. Results are presented in the Appendix. Due to a limited number of observations, we cannot include same-sex couples.

the same for the treatment and the control group. Neither do we find a significantly increased realized or intended reaction to take up employment among female partners who are not employed at the time of their partners' job loss. These findings are robust for several sub-groups and for different econometric specifications. Thus, we provide evidence that the absence of the added worker effect reflects the labor supply preferences of women and is not due to labor market frictions preventing women from adjusting working hours to their changed preferences.

Sensitivity analyses hint that shock intensity (magnitude and persistence) is limited and therefore the reactions to the shocks are small. Many of the male partners affected by a job loss find new positions relatively quickly. For this short time period out of employment the unemployment insurance in Germany offers a high replacement rate. As a result, the female partner's need to extend labor supply might not be particularly high. When focusing on households which experience more intense shocks, we find small and partially significant positive effects on the female partners' desired and actual working hours. However, these effects are very small and of similar magnitude for desired and actual working hours. Thus, even in households that are particularly affected, we find no evidence that income shocks are a driver of mismatches between desired and actual working hours.

Our study contributes to two different strands of literature: first, the literature on the added worker effect and secondly, the literature on differences between desired and actual working hours.

Following the seminal work by Lundberg (1985), a number of influential studies for different countries - e.g. Austria (Halla et al., 2020), the Netherlands (De Nardi et al., 2021), Norway (Blundell et al., 2015), the U.S. (Stephens, 2002) and a cross-country study for 28 European countries (Bredtmann et al., 2018) - analyze the added worker effect and suggest that the design of a country's unemployment insurance and benefit system shape the labor market reactions of spouses to their partners job loss.

For Germany, there are several studies analysing the added worker effect based on SOEP data. Despite finding substantial and persistent earnings losses of displaced workers, Fackler and Weigt (2020) find no evidence for a significant added worker effect when analysing partners' earnings after displacement. Triebe (2015) provides evidence for an added worker effect for married but not for unmarried couples. Another study based on SOEP data by Ehlert (2012) finds no evidence for a significant increase in working hours for West German women in response to their husbands' job loss. Illing et al. (2021) are the first authors to analyze the added worker effect using German administrative data. However, similarly to earlier studies based on survey data, they find no evidence for an added worker effect in Germany—regardless of the gender of the displaced worker. In fact, Illing et al. (2021) show that the opposite is the case: both for men and women, displacement leads to modest declines in their partners' earnings over the following years.

The absence of a meaningful added worker effect for Germany is often explained by the generous tax and transfer system mitigating the income shock resulting from unemployment to such a large extent that partners' incentives to increase working hours are low (e.g., Ehlert, 2012; Fackler and Weigt, 2020). However, the vast majority of existing studies focus on the partner's realized labor supply responses and interpret these as fully reflecting labor supply preferences.[3] In doing so, they

[3] Similarly to our paper, existing studies on the added worker effect primarily focus on labor supply responses at the intensive margin. However, some of them additionally analyze the extensive margin, i.e. whether previously unemployed individuals join the labor force as a response to their partners' job loss (e.g., Kohara, 2010; Triebe, 2015; Halla et al., 2020). While international evidence is mixed, a limited number of studies even show a decline in partners' labor market

neglect the possibility that partners could in fact prefer to increase their labor supply but are unable to immediately realize the preferred increase in working hours. In this case, while desired working hours would increase, actual working hours would remain unchanged.

As we know from existing studies, mismatches between desired and actual hours have severe consequences on individuals' health (Bassanini and Caroli, 2015; Bell et al., 2012), well-being (Başlevent and Kirmanoğlu, 2014; Wooden et al., 2009), and the income distribution (Beckmannshagen and Schröder, 2022). While the consequences are wide-ranging, the causes of mismatch between desired and actual working hours are manifold: mismatches can arise due to a limited number of jobs available, job openings with non-negotiable working hours due to employers' higher bargaining power, search frictions or other market imperfections (see, e.g., Altonji and Paxson, 1992; Bloemen, 2008; Chetty et al., 2011; Lachowska et al., 2023).[4] Adding to this strand of literature, one aim of this paper is to analyze whether shocks to household income are another driver of hours mismatches.

The remainder of the paper is structured as follows: Section 3.2 describes our data source and the focal variables for the analyses. Section 3.3 explains the applied empirical strategy. Section 3.4 presents our main results as well as a set of sensitivity analyses. In Section 3.5 we discuss the results in the context of the existing literature while Section 3.6 reviews limitations and potential extensions of this study. Section 3.7 concludes the paper.

participation or job search efforts—the so-called "discouraged worker effect" (e.g., Lundberg, 1985; Hardoy and Schøne, 2014)—which is in line with the findings of Illing et al. (2021) for Germany.

[4] For a detailed theoretical underpinning of the economic rationale behind desired working hours and mismatches between desired and actual working hours, see Beckmannshagen and Schröder (2022), Appendix A.

3.2 Data

3.2.1 SOEP data and focal variables

Our empirical analysis is based on data from the German Socio-Economic Panel (SOEP). The SOEP is a representative household panel survey which has been conducted on a yearly basis since 1984 and, as of 2019, comprises around 30,000 respondents living in 15,000 households (Goebel et al., 2019). The SOEP has a number of characteristics that are vital for our analysis. First, being the only available data source for Germany offering longitudinal information on both desired and actual working hours, the SOEP allows for a comprehensive investigation of a potential heterogeneous effect of involuntary job losses on partners' actual and desired working hours. Secondly, it offers a rich set of socioeconomic information on individuals' labor market activities and preferences, allowing for detailed analysis and high-quality matching. Importantly, information is not only available for the household head but also for their partners. Therefore, due to its panel structure, the SOEP allows us to follow individuals and their partners over time, which is key for our analysis of actual working hours and working hours preferences before and after treatment.

The key variable to identify treated couples is the generated `pgjobend` variable which contains the reason why an individual's employment was terminated. The variable always refers to the time between the interviews of consecutive survey waves. Based on the `pgjobend` variable we compute a treatment indicator variable which categorises people as treated if the given reason for an employment termination is either a dismissal by the employer or a plant closure. In line with the existing literature on causal effects of involuntary job losses (e.g., Chan and Huff Stevens, 2001; Kohara, 2010; Hennecke and Pape, 2022), we define both plant closures and dismissals as causes of involuntary job loss, which allows us to analyze the affected households' labor supply responses in a sufficiently large sample.[5]

In our main analysis we consider couples in which the male partner experienced a job loss due to dismissal or plant closure while at the same time the female partner was also employed. For these couples, our outcome variables are the following:

- Men's employment status at the time of the interview
- Men's gross yearly labor income
- Women's actual working hours
- Women's desired working hours[6]
- Women's gross yearly labor income
- Household net yearly income
- Women's hours mismatch[7]

While the first two outcome variables capture the first stage of our analysis, i.e. an income shock induced by a job loss, the following four outcome variables aim at measuring the women's reaction and the effect on household net income. By

[5]Our results remain robust when we only analyze households affected by plant closures (see Figure 3.4). Plant closures and mass layoffs are viewed as the most exogenous sources of job losses and are used in recent studies on the added worker effect drawing on large administrative data (e.g., Halla et al., 2020; Illing et al., 2021). However, the SOEP does not offer any information on mass layoff events and only focusing on plant closures would significantly reduce our sample size to 150 couples, which would not allow us to conduct sensitivity or sub-group analyses.

[6]See Figure 3.16 in the Appendix for the exact wording of the survey question capturing desired working hours. Also, Beckmannshagen and Schröder (2022) Appendix B provides strong evidence that the desired working hours variable is empirically meaningful, i.e. longer desired hours today predict an extension of actual working hours in the future.

[7]The hours mismatch, Δh, is defined as the difference between desired and actual working hours.

examining women's working hours we focus on the intensive margin of labor supply of women already in employment. By contrast, in a sub-analysis we consider couples in which the male partner experienced a involuntary job loss while at that time the female partner was not employed. For these couples our outcome variables are the following:

- Women's employment status at the time of the interview
- Women's intent to return to work[8]
- Whether women actively searched for a job in the last two weeks
- Whether women would start a job within the next two weeks if they were offered a position

By considering these variables, we capture intended or realized reactions on the extensive margin of employment, i.e., whether women who were not employed before their partners' job loss exhibit a higher tendency to take up employment after their partners' job loss.

Our observation period covers the years 1997 to 2019, with job losses experienced between 1999 and 2017. Since we conduct an event study it is important to be able to observe our key variables two years prior and after treatment. The SOEP did not survey desired working hours in 1996, which is why the earliest event year that we study is 1999. We choose 2017 as our last event year because we do not want to analyze labor market outcomes that happened after the onset of the COVID-19 pandemic in 2020.[9]

[8]The intent to return to work is surveyed with a 4-point Likert scale. For the analysis we recoded it as a binary variable, with "not at all" and "rather unlikely" recoded as 0, while "rather likely" and "for certain" recoded as 1.

[9]The pandemic and the regulatory reactions present a multifaceted shock to the labor market and—for example due to closed childcare facilities—to the division of time and labor within households.

3.2.2 Treatment and control group

In our main analysis, we focus on exploring the effect of men's job loss on the actual and desired working hours of their female partners. Women are particularly affected by hours mismatches (Beckmannshagen and Schröder, 2022). Thus, exploring whether shocks to household income are a driving factor of women's hours mismatches is of great interest. Furthermore, the vast majority of male workers work full time (91%) with an average of 42 actual working hours per week. Thus, there is simply little scope for a labor supply extension among men. In contrast, only 48% of female workers are employed in full-time jobs and the average number of actual weekly working hours is 31 hours.[10] As a result, on average, there is more potential for women to extend their labor supply after their partners' job loss, making it particularly interesting to concentrate primarily on the labor supply adjustments of women. Nonetheless, we also conduct our analysis for the male partners' labor supply reaction to their female partners' job losses. The results are presented in Figure 3.8 in the Appendix. As expected, we find no effects on desired and actual working hours of men after their partners' job loss.

For our empirical analysis, we divide couples into two groups: a treatment group and a control group. The treatment group refers to couples for whom the male partner is affected by involuntary job loss due to plant closure or dismissal. Couples in which the male partner was never affected by an involuntary job loss are assigned to the *unmatched* control group. Between the years 1999 and 2017, a total of 5,082 men surveyed in the SOEP experienced a job loss due to plant closure or dismissal.

Disentangling the different effects of an involuntary job loss and other pandemic-related measures is not feasible in our setting.

[10]The reported average full time rates and actual working hours for men and women are obtained from the unrestricted sample (time period 1999-2017) and refer only to individuals in regular employment.

Table 3.1: Sample restrictions and observations

Sample restriction	Treated obs.	Control obs.
Men affected by involuntary job loss	5,082	176,111
Employed in two years prior to job loss	3,617	122,972
Not civil servants or self-employed	2,813	74,188
In stable relationship	1,339	38,751
Age 24-55 at the time of job loss	1,040	29,832
Both partners' employment status observed	1,029	29,540
No missings in all matching variables	1,003	29,017
Only keeping the first experienced job loss	796	29,017
Partner employed before job loss	562	20,938
Outcome variables non-missing	444	17,070
After matching	430	430
Partner **not** employed before job loss	234	8,079
After matching	172	172

Notes: Table shows number of couples in treatment and control group after the stepwise implementation of sample restrictions. *Source*: SOEP v37.

However, as depicted by Table 3.1, imposing a variety of sample restrictions reduces the number of treated couples substantially. We only include men in stable employment relationships, meaning they were employed in the two years prior to their job loss (t-2 and t-1).[11] Additionally, for reasons of comparability between the treatment and control group, we exclude civil servants and individuals in self-employment from the control group as they cannot be affected by plant closures or dismissals in the same way as regular employees. Furthermore, since our aim is to investigate the (desired) added worker effect within existing couples, we only consider couples in stable relationships. Our definition of stable relationships includes both married and cohabiting unmarried couples who remain together for the entire observation period from t-2 to t+2.[12] Men affected by job loss have to be between the age of 25

[11] Following Illing et al. (2021), we include both men that were working on a full-time and men working on a part-time basis prior to their job loss in order to account for recent labor market trends of rising male part-time rates.

[12] Although the SOEP data structure would theoretically allow us to follow both partners after a separation, we refrain from including couples separating during the five-year observation period.

and 55 in the year prior treatment. We choose these age limits since we primarily want to analyze individuals who have finished their education and fully entered the work force while at the same time also excluding old-age workers to avoid transitions into early retirement. This age restriction also applies to their female partners. Some individuals face multiple involuntary job losses in their careers. In our main specification, we only consider their first observed job loss in the time period from 1999 to 2017 as we do not want to consider the same individuals twice at two different points in time. Also, we want to minimize the potential that couples have experienced similar shocks to their household income before and thus react differently due to anticipation.[13] Our final pre-matching sample comprises of 796 couples in the treatment group[14] and 29,017 potential couple-period observations in the control group.

By imposing these restrictions to both the treatment and control group, naturally both groups are at least to a certain extent comparable before job loss (see Table 3.2). For example, they are of similar age and have a similar family situation (number of children, marital status). However, as depicted by Table 3.2 they also differ in a number of important characteristics on individual (e.g. tenure, gross labor income),

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We do this in order to limit the risk that our investigations of labor supply responses to partners' job loss are biased by simultaneous labor supply adjustments around separation, which can be quite strong (see, e.g., Johnson and Skinner, 1986; Özcan and Breen, 2012; Brüggmann, 2020). While we acknowledge the proven link between job loss and separation (see, e.g., Charles and Stephens, 2004; Eliason, 2012), its implication for our analysis is limited since we only exclude 34 couples which otherwise meet all requirements to be part of the treatment group.

[13] In a robustness check we instead consider only the latest experienced involuntary job loss in the observation period. As shown in Figure 3.9 in the Appendix, this does not change our main results.

[14] The involuntary job losses that we consider in our event study are fairly evenly distributed over time. An exception is the period from 2002 to 2005, which has particularly high numbers of involuntary job losses in our sample. During this period Germany was often referred to as the "sick man of Europe" with a recession in 2003 and high unemployment rates (Dustmann et al., 2014).

establishment (firm size), and partner level (partners' full time rate, partners' gross labor income).

As our empirical approach relies on the comparison of couples in the treatment and control group to estimate the causal effect of involuntary job loss on partners' actual and desired labor supply, it is necessary to ascertain a high degree of comparability between both groups. We therefore apply a matching procedure to match treated couples to suited controls.

3.3 Empirical approach

We match control units from the pool of never-treated to treatment units based on a broad set of sociodemographic characteristics and labor market variables in the pre-shock period to obtain a control group that is closely comparable to our treatment group. More specifically, we match 1:1 and combine propensity score matching and exact matching. We match year-by-year and then stack treatment and control pairs from each year to ensure that we compare treatment and control group in the same year.[15] Further, since the gender-specific labor supply decisions and gender norms in East Germany vary widely from West Germany (see, e.g., Jessen, 2022), we only compare West (East) German couples with West (East) German couples. Lastly, while we also consider cohabiting unmarried couples if they are in a stable relationship, labor supply decisions of married couples might differ from those of unmarried couples, for example due to joint taxation (Bick and Fuchs-Schündeln, 2017). We thus only compare (un)married couples with (un)married couples. Consequently,

[15] Our approach is similar to Schmieder et al. (2023) and aims to avoid the Goodman-Bacon (2021) critique of event study designs with treatment in multiple periods.

the exact matching variables are the calendar year in which the job loss occurred, marital status, and place of residence (East Germany or West Germany).

Propensity scores are estimated based on the following set of variables which refer to the male partners in the pre-shock period unless stated otherwise: age, education, gross labor income, gross labor income two years before job loss, tenure with firm, firm size, a dummy indicating full-time position, a dummy indicating full-time position two years before job loss, female partners' education, female partners' gross labor income, female partners' gross labor income two years before male partners' job loss, a dummy indicating full-time position for the female partners, a dummy indicating full-time position for the female partners two years before job loss, dummy variables indicating presence of children in the age groups under 3, 3 to 5, 6 to 12, 12 to 18, a dummy indicating a person in need of care in the household, household net income, household net income two years before the male partners' job loss.

We select these variables because they likely influence household time use and labor supply. We only keep matched couples for whom common support is given.[16] As shown in Table 3.2 the matching procedure results in 430 matched couples in the control group whose sociodemographic characteristics are very similar to the characteristics of the 430 treated couples with no statistically significant differences.[17]

[16]We use the Stata command `kmatch` (Jann, 2017) and specify the `comsup` option, which ensures that common support is given based on the minima and maxima comparison (Caliendo and Kopeinig, 2008).

[17]We conducted t-tests for all displayed variables and none of the differences are statistically significant. The smallest p-value of 0.23 was obtained for gross income.

Table 3.2: Sample means, controls vs. treated

	Controls (raw)		Controls (matched)		Treated	
	Mean	sd	Mean	sd	Mean	sd
Married	0.87	0.33	0.84	0.36	0.84	0.36
East	0.24	0.42	0.36	0.48	0.36	0.48
Age	43.34	7.19	42.21	7.41	42.24	7.61
No. of Children	0.93	0.99	0.86	0.97	0.87	0.92
Need of care	0.01	0.12	0.02	0.13	0.01	0.12
Tenure	14.63	9.44	8.91	7.46	8.78	9.01
Firm size over 200	0.61	0.49	0.31	0.46	0.28	0.45
Primary educ.	0.30	0.46	0.34	0.47	0.37	0.48
Full time	0.96	0.19	0.96	0.19	0.95	0.21
Gross labor inc.	53,191	25,033	39,258	21,005	37,267	27,047
Partner primary educ.	0.21	0.41	0.22	0.42	0.22	0.42
Partner full time	0.43	0.50	0.57	0.50	0.55	0.50
Partner gross labor inc.	26,858	19,044	25,280	18,709	25,270	17,568
Household net inc.	59,233	22,087	52,531	22,037	50,995	22,945
No. of observations	18,199		430		430	

Notes: Displayed are descriptive statistics of socioeconomic characteristics for the unmatched pool of potential control individuals, the matched control group, and the treatment group after matching. Primary education refers to having obtained the basic school qualification which is reached after 9 years of schooling in Germany. *Source*: SOEP v37.

We analyze different outcomes of the matched groups in an event study framework. More specifically, we run OLS regressions with individual and year fixed effects as well as a set of dummies for the pre- and post-shock relative periods and their interactions with the treatment dummy. In this setting, the coefficients of the post-shock interaction dummies measure the period-specific average treatment effect of the involuntary job loss on the treated (ATT). The regression equation takes the form

$$Y_{it} = \sum_{k=-2,k\neq-1}^{2} \gamma_k P_{it}^k + \sum_{k=-2,k\neq-1}^{2} \delta_k P_{it}^k \times T_i + \nu_i + \tau_t + \epsilon_{it}, \tag{3.1}$$

with Y_{it} being the outcome of interest for person i in year t (e.g. a woman's desired working hours), $\left\{P_{it}^k\right\}_{k=-2,k\neq-1}^{2}$ being a set of relative period-dummies running from

-2 to 2 but excluding the reference period $k = -1$, with a shock occurring between period $k = -1$ and period k=0 if the person is in the treatment group. T_i is the respective treatment group dummy, ν_i are individual fixed effects, τ_t are year fixed effects, and ϵ_{it} is an idiosyncratic error. The coefficients of interest are the δ_k which can be interpreted as ATTs.

For our empirical strategy it is essential to assume that the involuntary job loss comes as a shock that is exogenous to our main outcome variables, i.e. the female partner's labor supply preferences.[18] Thus, the central identifying assumption for allowing a causal interpretation of the coefficients is the common trend assumption (see, e.g., Goodman-Bacon, 2021). In our application the common trend assumption states that in the absence of the male partner's involuntary job loss, differences in outcomes of the treatment and control groups would remain constant. If this assumption holds, one can interpret the differences in outcomes between treatment and control group, i.e. the depicted coefficients as causal effects. To show that the common trend assumption holds, we show pre-trends, i.e. differences for all outcome variables prior to the treatment in our result graphs.

3.4 Results

The presentation of our results is structured in three parts. First, we show our main analyses that focus on women's intensive margin labor supply adjustments after their partners experienced a job loss. Thus, in these analyses we only consider

[18]In the context of the partner's labor supply preferences, it is very likely that our outcome of interest—desired working hours of female partners—is uncorrelated to their male partner's dismissal. We therefore consider both job loss due to plant closure and due to dismissal as an exogenous shock to household income. In a robustness check we consider plant closures and dismissals separately and the outcomes do not significantly differ, which strengthens our joint approach (see Figure 3.4).

women who are in employment when their partners' job loss occurred. Secondly, we examine extensive margin adjustments; that is, we examine whether women who were not employed when their partners lost their jobs have a higher tendency to take up employment after the job loss occurred. Lastly, we conduct a comprehensive set of tests to assess the robustness of our results.

3.4.1 Intensive margin adjustments

Figure 3.1 comprises six panels with the main outcome variables of interest. Panel A and B show how the male partner was affected by a job loss and thus present the first stage to our analyses. Panel A depicts the employment rate of male partners. By construction, all male partners in the treatment and control group were in employment in the two years prior to the job loss. While all men in the treatment group experience a job loss between period -1 and 0, the employment rate of treated men drops by 45 percentage points (pp) compared to the control group in period 0. Thereafter, there is a partial recovery and the effect is around -25 pp in period 1 and 2. Accordingly, many of the men affected by a job loss find a new employment relatively quickly.

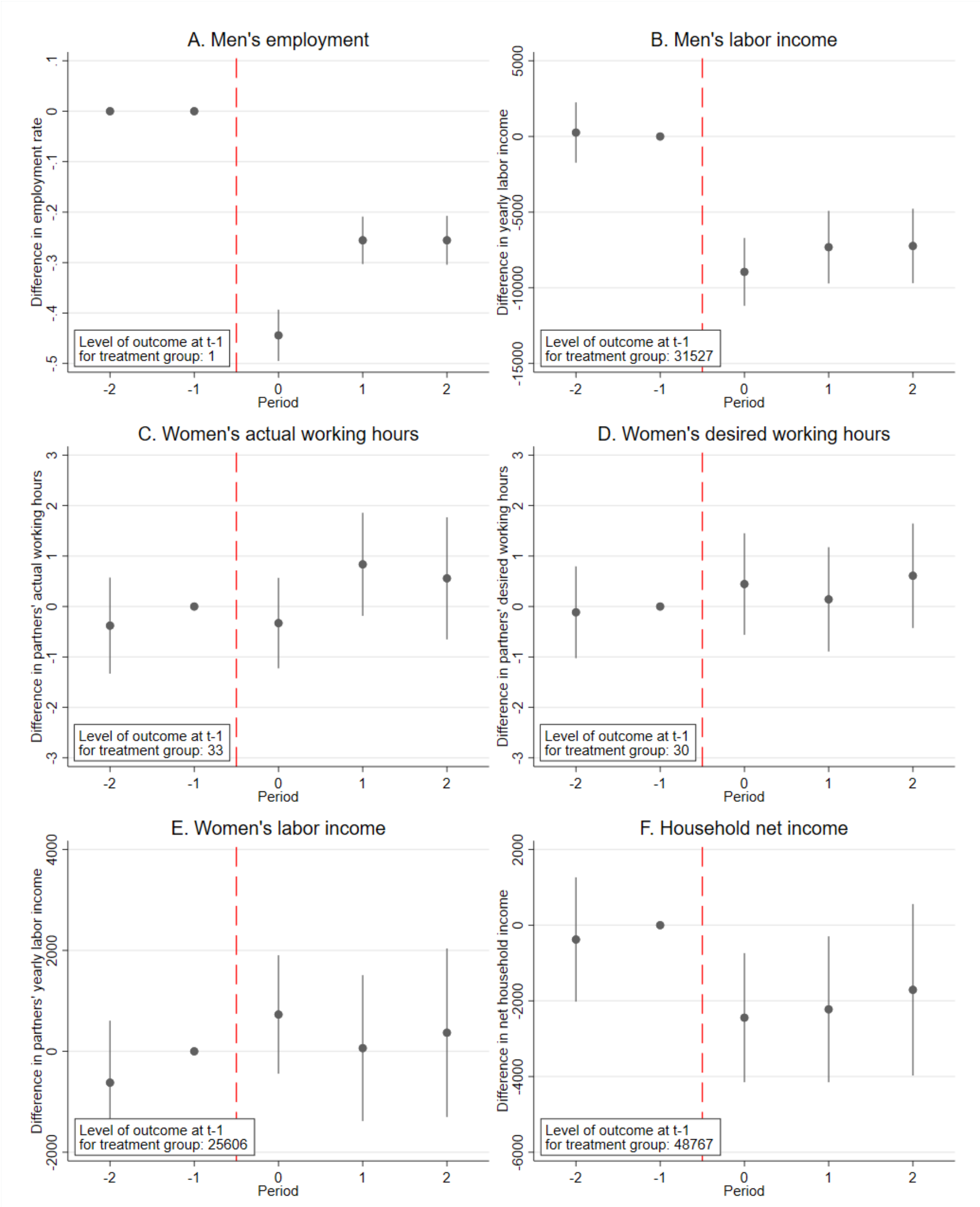
Panel B shows how men's yearly gross labor income is affected by a job loss. In period 0, it drops by around 9,000 euros compared to the labor income of the control group. Thus, the unexpected job loss clearly affects men's labor income with an average relative reduction of about 28% in period 0. Again, one can observe a slight recovery in the periods thereafter: in period 2 the effect is at around -7,500 euros. However, as depicted by Panel A, a substantial proportion of the affected men take up a new position relatively quickly and are back in employment at the time of the survey in period 0, which is also reflected in their incomes.

Panel C shows how the female partners' actual working hours react to the job loss of their male partners. There is no immediate reaction to the job loss in period 0. The point estimates for period 1 and 2 indicate a slight increase in actual working hours in these periods; however, none of the effects is statistically significant at the 95% level. Consequently, we do not find a significant adjustment on the intensive margin of labor supply and therefore no evidence of an added worker effect.

However, the stated labor supply preferences of women might still have changed after their male partners' job loss. Panel D therefore depicts how women's desired working hours develop after the shock to household income. While the point estimates indicate a very small increase in desired working hours in all post-shock periods, none of the estimates is statistically significant.

In Panel E we examine women's labor income after their partners' job loss. Similarly to women's labor supply, we do not find significant changes in women's labor income after their male partners' job loss.

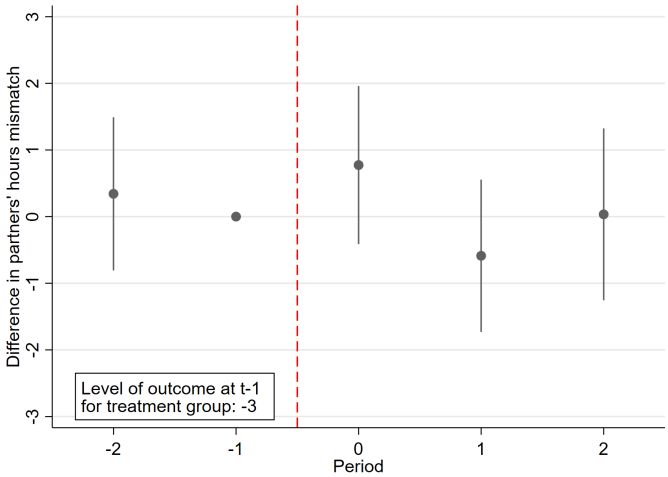
Lastly, in Panel F, we analyze how the men's involuntary job loss affects household net income. In the periods after the shock, household net income drops on average by about 2,000 euros, which amounts to a decline of 4% relative to the pre-shock household net income. Thus, the effect of the male partners' job loss on household net income is considerably smaller (both in absolute and relative terms) than its effect on men's individual gross labor income (Panel B). Furthermore, in general, the effect of men's job loss on household net income is only temporary as we only find a significant drop in periods 0 and 1. In period 2, the point estimate is smaller and not statistically significant. Both the small magnitude and the short-term nature of the drop in household net income might explain the absence of larger adjustments in the female partners' labor supply or labor supply preferences.



Notes: Shows period-specific coefficients according to Equation (3.1). Bars give robust 95% confidence intervals of the respective coefficients. All incomes are price-adjusted and presented in 2019 Euros. Number of individual observations: 860 (430 treated, 430 control units). *Source*: SOEP v37.

Figure 3.1: Main outcomes after men's job loss

As mismatches between desired and actual hours can have serious consequences on earnings and well-being, we are particularly interested in whether a shock to household income due to a partner's job loss causes such mismatches. Therefore, in Figure 3.2 the effect of male partners' job loss on women's hours mismatches is examined. Similar to the findings for desired and actual working hours, there is no clear pattern of changing hours mismatches after the male partners' job loss. While point estimates indicate some minor fluctuations after the shock, none of the coefficients is statistically significant at the 95% confidence level. Thus, there is no indication that a partner's job loss can be considered as one of the drivers of women's hours mismatches.

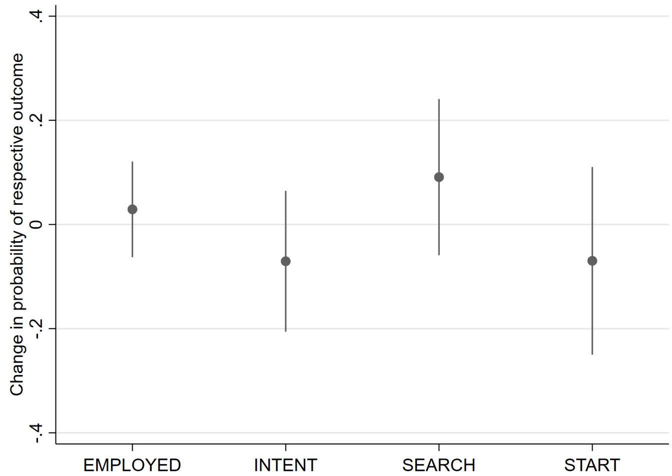


Notes: Shows period-specific coefficients according to Equation (3.1). Bars give robust 95% confidence intervals of the respective coefficients. Number of individual observations: 860 (430 treated, 430 control units). *Source*: SOEP v37.

Figure 3.2: Women's hours mismatch after men's job loss

3.4.2 Extensive margin adjustments

For female partners who are not in employment at the time of the interview before their male partner experiences a job loss, desired or actual working hours are not observed. For these women we therefore consider other outcome variables that capture their probability of actually taking up employment or their intent to do. As our sample contains only 172 couples after matching for which this is the case, we deviate from our period-specific event study framework and instead compare our outcomes of interest in the pre-shock period with the period after the job loss occurs in a standard differences-in-differences approach.[19]



Notes: Shows coefficients of a diff-in-diff regression of extensive margin outcomes according to Equation (3.2). Bars give robust 95% confidence intervals of the respective coefficients. EMPLOYED stands for the effect on employment probability, INTENT for the effect on a dummy variable capturing the stated intent to (re-)enter employment, SEARCH for the effect of actively searching for a job within two weeks prior to the survey, while START stands for the effect on a dummy capturing whether the respondent would be able to start a new position within two weeks. Number of individual observations for EMPLOYED: 344 (172 treated, 172 control units). For INTENT, SEARCH, and START the number of observations is slightly lower because we can only analyze these outcomes for women who are still unemployed in the period after the job loss. *Source*: SOEP v37.

Figure 3.3: Extensive margin outcomes

Figure 3.3 depicts the outcomes for women who were out of employment when their male partners experienced a job loss. The first coefficient presents the actual extensive employment margin; that is, it shows whether women who are out of

[19]See Equation (3.2) in the Appendix for the detailed regression equation that we estimate.

employment when their partners experience the job loss have a higher probability of taking up employment than women who are out of employment and whose partners do not experience a job loss. As shown, women's probability of taking up employment does not increase significantly.

Women's employment probability remaining unaffected by their partners job loss does not rule out that women in affected households adjusted their labor supply preferences in terms of their intent to take up employment or their job search behavior. However, the second, third, and fourth coefficient depicted in Figure 3.3 show that neither women's stated intent to enter employment, the active job search behavior, nor the willingness to start a new position within two weeks significantly changed after their partners' job loss. Thus, also for the extensive margin of employment, we do not find any significant effect of the male partners' job loss on women's actual employment probability nor on variables capturing their stated labor supply preferences.

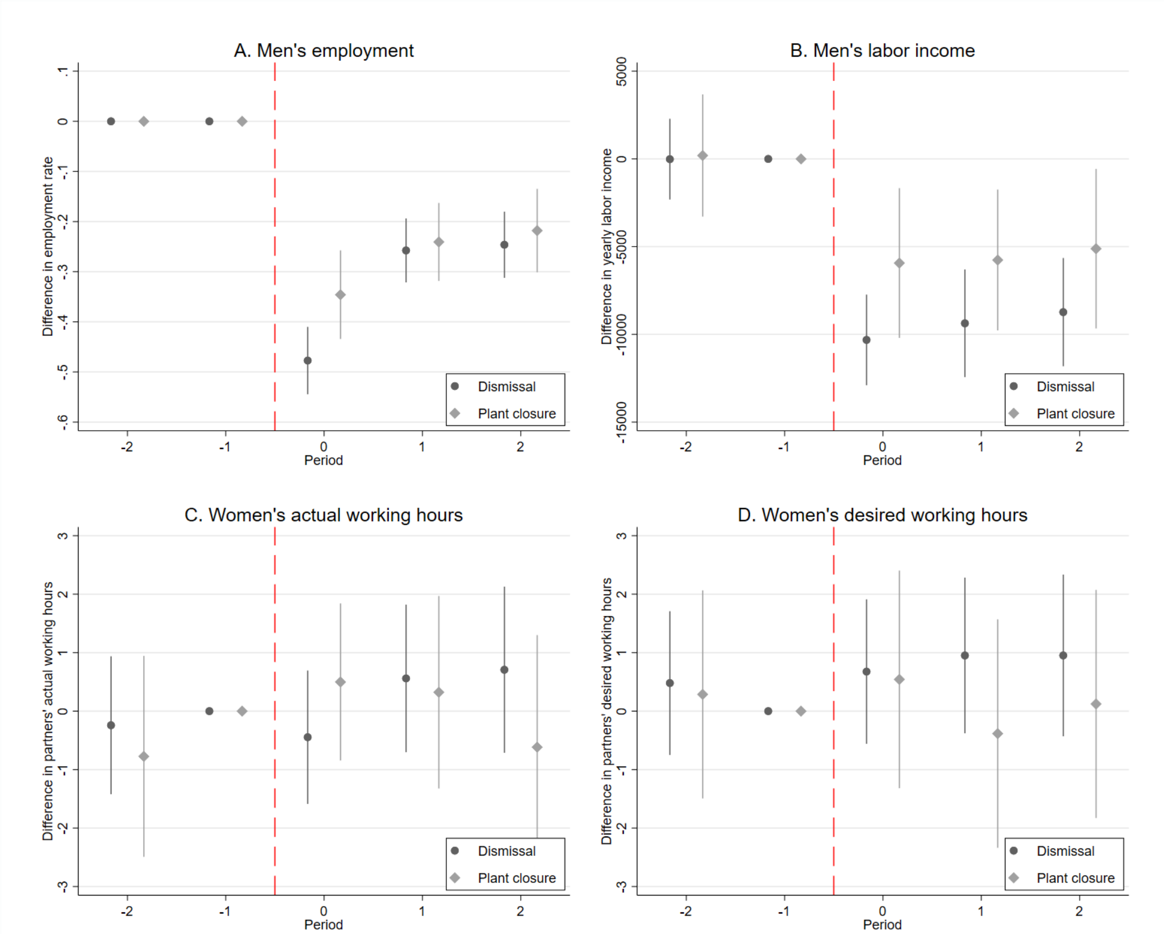
3.4.3 Robustness checks

As we find no significant effects of a partner's involuntary job loss on women's labor supply preferences, we conduct a variety of sensitivity analyses to assess the robustness of the null effects and examine whether the results change for some particularly affected couples. The sensitivity analyses can be classified into three broad categories. First, we investigate the robustness of our empirical strategy by using an alternative matching approach, an alternative estimator proposed by Callaway and Sant'Anna (2021) and differentiating between different types of involuntary job loss (plant closure vs. dismissal). Secondly, we explore the heterogeneity of women's (desired) labor market responses to their partners' job loss for a number of different sub-groups (e.g. women with vs. without children, women working full-time vs. part-time jobs before their partners' job loss). And lastly, we investigate whether the "shock intensity" (e.g. duration of men's unemployment, relative loss in household income) matters for women's (desired) working hours adjustments following their partners' involuntary job loss.

Robustness of empirical strategy First, we provide an alternative to our baseline estimates, which are based on exact matching and propensity score matching. Figure 3.10 in the Appendix presents estimates based on exact matching and the commonly used Mahalanobis distance matching approach (Mahalanobis, 1936) using the same matching variables. The results based on this alternative matching approach are fairly similar to our baseline estimates and no significant desired added worker effect can be found.

Furthermore, the recent literature on dynamic treatment effects has emphasized the importance of taking into account time-specific heterogeneous treatment effects when estimating ATTs in standard two-way fixed effects models and pooling events at different points in time (Goodman-Bacon, 2021). To ensure that heterogeneous treatment effects along event timing do not bias our main estimates, we re-run our main analysis using the estimator proposed by Callaway and Sant'Anna (2021). The aggregate group-time ATTs estimated based on Callaway and Sant'Anna (2021) do not substantially alter our results (see Figure 3.11 in the Appendix). This is not surprising given the fact that we follow Schmieder et al. (2023) in their strategy of stacking matched treatment-control groups for each year of job loss, which already has strong similarities to the estimator proposed by Callaway and Sant'Anna (2021).

Additionally, we differentiate between two different types of involuntary job loss: layoff due to plant closure and dismissal. There is an extensive literature on the



Notes: Shows period-specific coefficients according to Equation (3.1) differentiated for couples affected by dismissal or plant closure. Bars give robust 95% confidence intervals of the respective coefficients. Gross labor income is price-adjusted and presented in 2019 euros. Number of individual observations: Plant closure: 266 (133 treated, 133 control units); dismissal: 528 (264 treated, 264 control units). Note that the overall number is lower than in our main specification because the underlying logit estimations of our year-by-year propensity score matching procedure do not converge in all years due to the small sample size after splitting the sample. *Source*: SOEP v37.

Figure 3.4: Different types of involuntary job loss—dismissals vs. plant closures

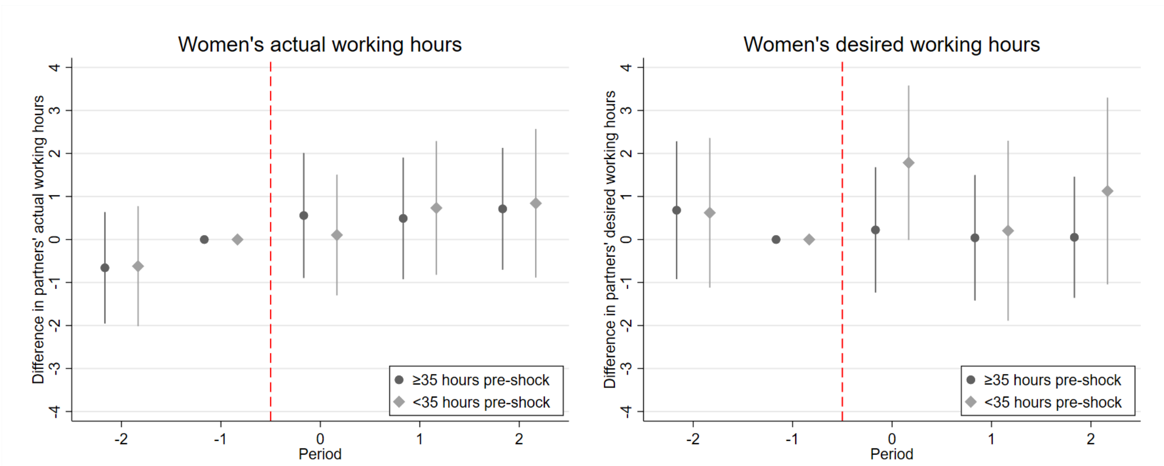
effects of unexpected job loss focusing solely on individuals affected by plant closures or mass layoffs as these job loss events are seen as credibly exogenous (e.g., Marcus, 2013; Schmieder et al., 2023). However, as already explained in Subsection 3.2.1, we also include dismissals as another type of involuntary job loss. In order to make sure that also including dismissals does not bias our main results, Figure 3.4 shows outcomes after differentiating between couples for which the male partner was dismissed and couples for which the male partner was affected by plant closure. As we can see in Figure 3.4 (Panel A), the drop in the employment rate between period -1 and period 0 is more severe for dismissed workers (-48 pp) than for individuals who lost their job due to plant closure (-34 pp). This large drop in the employment rate is accompanied by an income loss of around 10,000 euros for dismissed workers. At the same time, income loss of men affected by plant closure between period -1 and period 0 is around 5,500 euros. While employment rates converge very quickly, differences in income losses seem to be more persistent for these two groups. Regarding the female partners' labor supply responses, we find no clear pattern for actual working hours. In contrast, for desired working hours, the point estimates for women with dismissed partners are higher, which could be the due to the persistently higher income losses that these couples experience. However, most importantly, no significant differences in actual and desired labor supply responses can be found for these two types of involuntary job loss.

Heterogeneity by sub-groups In our baseline analysis, which is based on the full sample, we do not find any evidence for a significant female labor supply response to male partners' involuntary job loss—both with regards to their actual and desired working hours. In order to gain a deeper understanding of whether this lacking response is universal or whether there are more responsive groups, we perform a number of different sensitivity checks to analyze heterogeneity in labor supply responses for different sub-groups.

First, we investigate whether married women react differently than unmarried women. In our main specification we consider all cohabiting couples in stable relationships, regardless of their marital status. However, theoretically, there are different ways in which marital status could influence labor supply responses. For example, unmarried women might not respond as strongly to their partners' job loss as married women since they might not see themselves as forming an economic unit with their partner. Indeed, evidence on a lower degree of income pooling in unmarried compared to married couples (e.g., Hiekel et al., 2014; Evans and Gray, 2021) can be interpreted as a sign of higher degrees of individualism and independence. In line with this argument, Triebe (2015) finds evidence for an added worker effect for married but not for unmarried couples. On the other hand, higher marginal effective and participation tax rates due to joint taxation of married couples could prevent wives from increasing their labor supply at the extensive and intensive margin—especially once their partners are re-employed (see, e.g., Bick and Fuchs-Schündeln, 2017). To assess whether marital status impacts our results, we exclude unmarried couples from the analysis, which reduces our sample size by 16%. As depicted by Figure 3.12 in the Appendix, no substantial differences to our baseline results occur when only analysing married couples.

Next, we examine the role of children in the household for the effect of partners' job loss on female labor supply. Raising children is one of the major determinants of within-household division of time and thus also of labor supply decisions. Accordingly, the presence of children in different age groups has major implications for the added worker effect (Halla et al., 2020; Cammeraat et al., 2023). Therefore, in Figure 3.13 in the Appendix, we differentiate between the labor supply responses of women with children and women without children living in the same household. While point estimates indicate small positive adjustments of actual and desired working hours among women without children, we find no significant effect on actual or desired hours for women with children or for women without children. As the children's age might be an important factor for the mother's time budget, we also separately examine mothers of children under or over 12 years old. With smaller groups under investigation, we lose precision and find insignificant point estimates very close to zero and no meaningful differences between the different groups.

Furthermore, we want to investigate whether the (desired) added worker effect is stronger for women working fewer hours before their partners' job loss. Naturally, the potential scope for the added worker effect is larger among women working part-time jobs than among women working full-time jobs. We therefore divide our sample of women who are employed in the year before their partners' involuntary job loss into two groups: 1) women working at least 35 hours, and 2) women working fewer than 35 hours. The average number of actual weekly working hours for women working fewer than 35 hours is 23 hours in t-1 while it is considerably larger for the group of women working 35 hours or more (37 hours). As depicted by Figure 3.5, responses in actual working hours are not statistically significant and roughly equal in size for both groups. However, we find an increase in desired working hours for women working fewer than 35 hours per week before their partners' involuntary job loss in period 0. Desired working hours increase by around two additional hours, an 8% increase in comparison to their pre-shock desired working hours (24 per week). With a p-value of 0.051 the effect is on the verge of statistical significance. As a result, there is a clear mismatch between the responses in actual and desired working hours in period 0. The increase in desired working hours vanishes in period 1 and period 2 and is therefore only temporary. In contrast, we find no evidence for an increase in desired working hours for women who worked 35 or more hours before their partners' job loss.



Notes: Shows period-specific coefficients according to Equation (3.1) differentiated for women with different levels of pre-shock working hours. Bars give robust 95% confidence intervals of the respective coefficients. Number of individual observations: 304 women with <35 hours pre-shock (152 treated, 152 control units); 408 women with ≥35 hours pre-shock (204 treated, 204 control units). Note that the overall number is lower than in our main specification because the underlying logit estimations of our year-by-year propensity score matching procedure do not converge in all years due to the small sample size after splitting the sample. *Source*: SOEP v37.

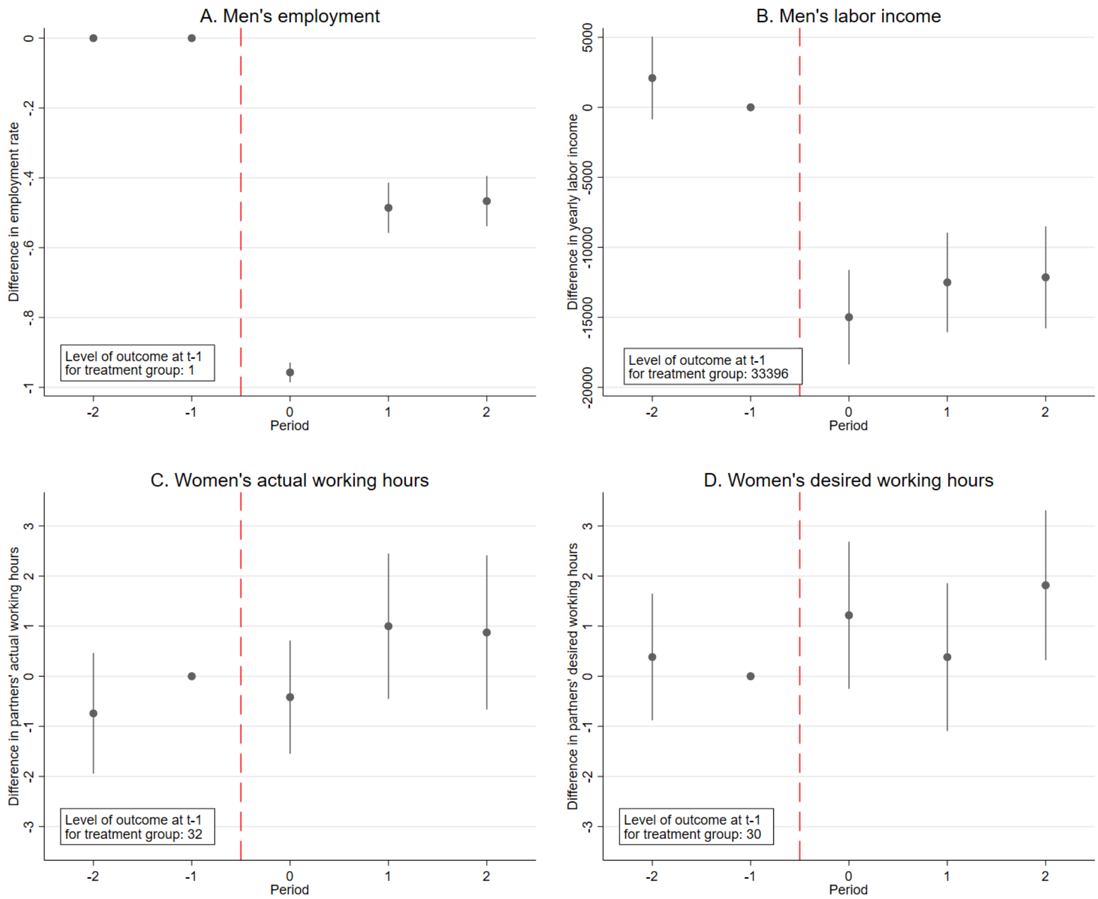
Figure 3.5: Different levels of pre-shock working hours

Accounting for shock intensity One of the potential reasons for the absence of stronger labor supply responses could be the limited intensity and persistence of the income shock that households experience after the male partner's job loss. As depicted in Panel A of Figure 3.1 and described in Subsection 3.4.1, the majority of men are back in employment relatively quickly and average effects on household income are modest (around -4%).

The following set of sub-analyses examines whether labor supply reactions of female partners differ for couples for whom the experienced shock was particularly severe. First, we repeat our main analyses but restrict our sample to only include couples in which the male partner was still out of employment when the interview in period 0 was conducted. Figure 3.6 shows results for this sub-group analysis. Panels A and B show that—as expected—the shock was more intense for this group. For men affected by an involuntary job loss, the employment probability in period 0 is reduced by 95%[20] while yearly gross income on average drops by 15,000 euros, which amounts to 45% of pre-shock income. Thus, the negative income shock is substantially larger than it is in our baseline analysis where yearly gross income on average drops by around 9,000 euros (see Panel B of Figure 3.1). Panel C displays actual hours of the female partner. As for the main analysis, we do not find any significant effect on actual hours. However, the point estimates indicate a slight increase of one hour in period 1 and period 2. Due to a lack of precision after reducing the sample by more than half, the estimates are not statistically significant. Panel D shows the female partners' desired working hours. In all post-shock periods point estimates are positive, indicating a slight increase in desired working hours. However, the effect is only statistically significant at the 95% level in period 2 and indicates an increase of close to 2 hours per week or 6.7%.

Secondly, we take into account the relative income loss that couples experience due to the involuntary job loss. We restrict the sample to contain only couples in the top half of relative income loss from period -1 to 0. Results are presented in Figure 3.14 in the Appendix. The results are generally very similar to the results of sub-group analyses for couples in which the male partner was still unemployed in period 0. Point estimates for both actual and desired hours indicate small increases in post-shock periods but are insignificant.

[20]Note that the ATT is not 100% because some men in the control group might also move out of employment for other reasons than an involuntary job loss.



Notes: Shows period-specific coefficients according to Equation (3.1) for a restricted sample of only couples in which the men were still unemployed in period 0. Gross labor income is price-adjusted and presented in 2019 euros. Bars give robust 95% confidence intervals of the respective coefficients. Number of individual observations: 420 (210 treated, 210 control units). *Source*: SOEP v37.

Figure 3.6: Larger shock intensity: longer unemployment spells

Lastly, we restrict the sample to contain only couples,in which the male partner worked in full-time employment prior to losing his job. As 95% of men in the treatment group work in full-time employment, this only slightly reduces the sample. Due to the large overlap with the baseline sample, the results which are displayed in Figure 3.15 in the Appendix mirror our main results, as would be expected.

Overall, examining sub-groups for whom the experienced shock was particularly severe yields more insights with respect to the dynamics of female labor supply preferences after the male partners' job loss. There is evidence that women's desired working hours tend to increase after a job loss that puts their male partners out of employment for a longer period of time. However, due to the small sample size these results should be interpreted rather cautiously.

3.5 Discussion

The main focus of our study is to analyze both the actual and the desired labor supply response of women to their male partners' involuntary job loss. Primarily, we investigate changes at the intensive margin, i.e. changes in actual and desired working hours. In line with existing studies for Germany (e.g., Fackler and Weigt, 2020; Illing et al., 2021), we find no evidence for an added worker effect in actual labor supply. Moreover, we provide novel evidence for the absence of a significant effect on desired working hours. We thus show that the absence of the added worker effect generally reflects the stated labor supply preferences of women and cannot be attributed to labor market frictions preventing women from adjusting working hours according to their changed preferences.

Several potential reasons why women do not seem to desire an increase of their labor supply are conceivable. One reason could be that the loss in household net income is not that severe or sufficiently long-lasting for women to adjust their labor supply preferences. In line with recent studies for Germany (e.g., Illing et al., 2021; Jarosch, 2023; Schmieder et al., 2023) we find evidence for, on average, significant and persistent gross earnings losses for individuals affected by job loss (see Figure 3.1, Panel B). However, reduction in household net income (after taxes and benefits) is much less severe and no longer statistically significant just two years after the job loss occurs (see Figure 3.1, Panel F). Thus, the German tax and transfer system plays a substantial role in mitigating the income shock, which is in line with findings by Ehlert (2012) and Fackler and Weigt (2020).[21] Fackler and Weigt (2020) show that redistributive measures of the tax and transfer system reduce the household income gap between couples affected by job loss and their non-affected counterparts by around 93% in the first year after job loss and by approximately 72% in the longer run (five years after job loss occurs).

In addition to the rather generous unemployment insurance system, many of the male partners affected by an involuntary job loss appear to find new jobs relatively quickly. In fact, the effect on men's employment implies that the employment rate in the treatment group is only reduced by 25% compared to the control group in period 1 (see Figure 3.1, Panel A). Thus, for the majority of the affected men, the time between job loss and re-employment appears rather short.[22] As a result, it is likely that in most cases the male partners' temporary unemployment does not lead to a persistent change in the intra-household division of housework. While Foster and Stratton (2018) show that significant labor market events can indeed affect the division of time spent on housework between partners, recent results for Germany by Hennecke and Pape (2022) cast doubt on the persistence of these effects. Hennecke and Pape (2022) show that a father's job loss significantly increases paternal childcare and housework in the short-run; however, effects reverse shortly

[21] For example, under the current regulations, former employees who were employed for at least 12 months receive unemployment benefits of 60% (67% in case of parenthood) of their prior gross earnings. The duration of entitlement depends on the duration of the prior employment and on age. For individuals aged less than 50, the maximum duration of entitlement is one year. After this benefit has expired, individuals will only receive a basic payment at subsistence level, which takes into account income and wealth of all household members. See Schmieder and Trenkle (2020) for a more detailed description of the German unemployment insurance system.

[22] Note that the majority of our observation period is characterized by very good labor market conditions in Germany that are often described as the "German labor market miracle" (Burda and Hunt, 2011). Illing et al. (2021), who use German administrative data on involuntary job losses occurring between 2002 to 2012, also find a high propensity of swift re-employment.

after re-employment and no strong evidence for persistent changes in bargaining powers or gender role attitudes exists. Against this background, the lack of a response in women's actual and desired working hours could also be the result of both factors: first, the anticipation that short-term changes in the intra-household division of housework and childcare are not persistent and second, the actual reversal to the prior intra-household division of these tasks once their partner finds a new job. Generally, gender roles in Germany are on average more traditional than in most other Western societies (Kleven et al., 2019). These traditional gender roles in Germany might be a crucial determinant for the absence of women's labor supply response as women on average carry out most household and childcare-related tasks (Samtleben, 2019; Schäper et al., 2023).

However, as seen in Figure 3.5, when we consider only couples in which women worked less than full time and thus had the capacity for an extension of their labor supply, we find small indications for a temporary increase of desired working hours while actual working hours remain unchanged. Also, we obtained similar results when considering only couples in which the male partner was still unemployed in period 0 (see Figure 3.6). Thus, while the magnitude is small (between 1 and 2 hours), there exist certain cases in which women prefer an extension of working hours but are unable to adjust their actual working hours accordingly after their partners' job loss. This is consistent with Knaus and Otterbach (2019) and Euwals (2001) who find that adjusting working hours within an existing job is difficult for many employees, in particular for women. In addition, if adverse macroeconomic or regional labor market conditions lead to the male partner's dismissal or plant closure, these conditions may correlate with the female partner's perceived and actual chances of adjusting their working hours or finding a job. In fact, Halla et al.

(2020) and Illing et al. (2021) argue that correlated shocks affecting both partners working in similar regions and industries are a potential explanation for the absence of an added worker effect. This idea is closely related to the literature on the so-called discouraged worker effect (e.g., Benati, 2001; Van Ham et al., 2001).

As we only find small differences in women's desired and actual labor supply responses limited to specific sub-groups, we can draw from our analyses that household income shocks due to involuntary job loss are not a main driver of mismatches between desired and actual working hours. At the same time, these mismatches seem to be a pervasive characteristic of the German labor market that is well documented (Knaus and Otterbach, 2019; Beckmannshagen and Schröder, 2022). It is left for future research to systematically discover the drivers of mismatches between desired and actual working hours.

3.6 Qualifications and extensions

This section critically reflects on potential limitations of our study, most of which are due to limited sample size. Furthermore, it discusses room for future research.

For our analysis, we draw on SOEP survey data since it offers information on employees' desired working hours that is not available in a comparable panel structure in other data sources for Germany.[23] However, the number of surveyed individuals in the SOEP is small in comparison to the number of observations offered by many

[23]While desired working hours are also surveyed in the "Mikrozensus" — an official, annual survey using a representative sample of one percent of the German population and households — this data source does not offer the panel structure needed for our analysis. Due to its rotating panel design, the survey only offers panel data on individuals and households for a maximum of four years, which would further shorten our observation period. Furthermore, individuals and households are no longer surveyed if they change their place of residence, which would seriously bias our results since there is evidence for a strong relationship between job loss and regional mobility (e.g., Fackler and Rippe, 2017; Huttunen et al., 2018).

administrative data sources. As a result, we decided to not only analyze job losses due to plant closures but also those due to dismissals in order to obtain a larger sample size which would also allow us to examine different sub-samples for our sensitivity analyses and robustness checks. While plant closures are the most exogenous source for job losses and are therefore preferably used in studies based on large administrative data (Illing et al., 2021; Schmieder et al., 2023), dismissals are also widely used in related studies (e.g., Chan and Huff Stevens, 2001; Kohara, 2010; Hennecke and Pape, 2022) but are potentially more prone to being endogenous. For example, in our setting, an endogeneity problem would arise if an unobserved variable affects both treatment assignment and the outcome variable. Relationship problems is one such unobserved factor that could potentially both influence the likelihood of being dismissed and our main outcome variables (actual and desired working hours). Serious relationship problems could lead to lower work productivity, which might lead to someone getting dismissed, and at the same time could lead to adjustments in partners' (desired) working hours in preparation for an anticipated break-up (see, e.g., Johnson and Skinner, 1986; Özcan and Breen, 2012; Brüggmann, 2020). This specific potential source of endogeneity is indirectly addressed by only analysing "stable couples" who stay together during the five-year observation period (see Subsection 3.2.2 for further details). More generally, we address the concern that including dismissals might lead to endogeneity problems and therefore biased estimates in one of our robustness checks. Figure 3.4 shows that our baseline results remain largely unchanged if we limit our sample to job losses due to plant closures.

Another limitation of our analysis in comparison to related studies for Germany (e.g., Illing et al., 2021) is the relatively short observation period of two years after the job loss occurred. Since the replacement rates of the unemployment insurance in

Germany are usually high (up to 67%) during the first year of unemployment, one could expect that (desired) added worker effects only really start to come into effect one year after job loss. Older individuals, however, might be entitled to even longer unemployment benefit payments.[24] Therefore, given the German institutional setting, theoretically it is not unlikely that potential effects do not occur immediately but may need more than our two-year observation period after job loss occurs to come into effect.[25] However, extending the observation period would result in a lower number of couples for whom we have complete information due to panel attrition. That said, since our data indicates that the majority of men re-enter the labor market within the two years after job loss, extending the observation period would not necessarily facilitate many new insights.

Furthermore, our study would benefit from more detailed sensitivity analyses aiming to examine specific scenarios in which mismatches between desired and actual labor supply response to partners' job loss might be particularly severe. For example, related studies provide evidence that the household context - especially the presence of children in the household - is an important factor for women's labor market responses to their partners' job loss (e.g., Halla et al., 2020; Cammeraat et al., 2023) and working hours' mismatches in general (e.g., Beckmannshagen and Schröder, 2022). Due to limited sample size, we only differentiate between women with children and women without children in the household and between women

[24]The design of the unemployment insurance legislation in Germany changed multiple times during our observation period (e.g. the German Hartz reforms in the mid-2000s). Due to limited sample size, we cannot analyze in detail whether treatment effects vary over time. However, we address the potential problem of time-specific heterogeneous treatment effects by following the matching approach by Schmieder et al. (2023) and conducting a robustness check where we use the estimator proposed by Callaway and Sant'Anna (2021). For a detailed overview of unemployment insurance durations see, for example, Hartung et al. (2022).

[25]If individuals are still unemployed once their unemployment insurance benefits expire, they receive subsistence benefits which are means-tested and are reduced for partner earnings and other income sources.

with children aged younger or older than 12 years. However, it would be interesting to more specifically focus on women with particularly high levels of care duties - for example, women with even younger children (e.g. aged 6 or younger) and women caring for elderly relatives, partners or children with disabilities.

Additionally, future studies could investigate whether the desired added worker effect occurs predominantly in regions, industries or periods within the business cycle which are characterised by strong labor market frictions or monopsony power. For example, Beckmannshagen and Schröder (2022) show that working hours mismatches are especially prevalent in the service sector, where union power is weak. Unfortunately, due to our already limited sample size, we cannot conduct such specific sub-analyses without losing the statistical power necessary to derive insightful conclusions.

Similarly, the limited number of couples for whom the female partner is out of employment in the year their partners' job loss occurs hinders in-depth investigation regarding the existence of a desired added worker effect at the extensive margin. While we conduct some baseline analyses using a standard differences-in-differences approach, we cannot examine specific sub-samples to perform sensitivity analyses.

3.7 Conclusion

In this paper, we conduct an in-depth examination of the adjustments in actual labor supply and stated labor supply preferences of women after their partners suffered an involuntary job loss. In doing so, we shed light on the question of whether the absence of added worker effects is in line with women's labor supply preferences or whether it is due to an inability to realize their labor supply preferences.

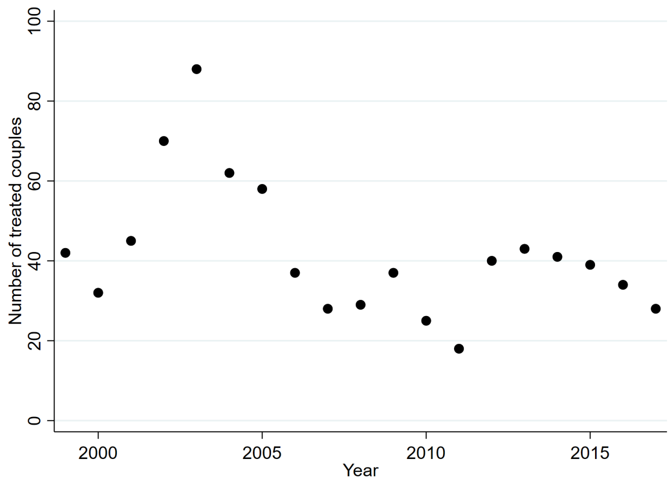
Our event study analysis shows that neither the actual working hours nor the desired working hours of women change significantly after their partners' job loss. Thus, we provide evidence that the absence of the added worker effect in Germany is in line with the labor supply preferences of women and cannot be explained by labor market frictions preventing them from adjusting working hours according to their changed preferences. Instead, our results indicate that the household income shock caused by the involuntary job loss is only temporary as the majority of affected men find new jobs in the same year in which the job loss occurs. At the same time, in the short run, the German unemployment insurance system is rather generous and offers high replacement rates. The interplay of these two factors—many of the affected men finding new jobs relatively quickly, and insurance through the tax and transfer system during the unemployment period— may alleviate the pressure on the female partner to quickly adjust labor supply and thus likely presents a reason for the absence of desired and actual added worker effects.

In general, our results imply the persistence of the intra-household division of paid and unpaid work—even if households face exogenous (income) shocks. This suggests that short-term changes in partners' time availability do not suffice to achieve a more gender equal intra-household division of labor. To shed long-term habits as well as overcome workplace and societal expectations, substantial and permanent changes in the institutional setting (e.g. in the financial incentives through a reform of the joint taxation of married couples in Germany) and norms might be necessary.

Nonetheless, our sub-analyses provide suggestive evidence that under certain circumstances (e.g. high shock intensity, low level of pre-shock working hours), women wish to slightly extend their labor supply in the short run but are unable to do so. Against this background, by indicating the possible existence of a desired added worker effect, this study can be considered as a starting point for future research on this topic. For example, future studies could aim to examine whether the effect is more pronounced in local labor markets, industries or periods within the business cycle which are characterised by strong labor market frictions or monopsony power. Furthermore, it would be intriguing to analyze the mismatch between desired and actual labor supply responses to partners' job loss for countries other than Germany with different tax and benefit systems, gender norms, and labor market conditions.

3.8 Appendix

Descriptive statistics



Notes: Shows yearly number of couples in which the male partner experiences an involuntary job loss. Refers to the total number of couples regardless of the employment status of the female partner. *Source*: SOEP v37.

Figure 3.7: Number of treated couples by year

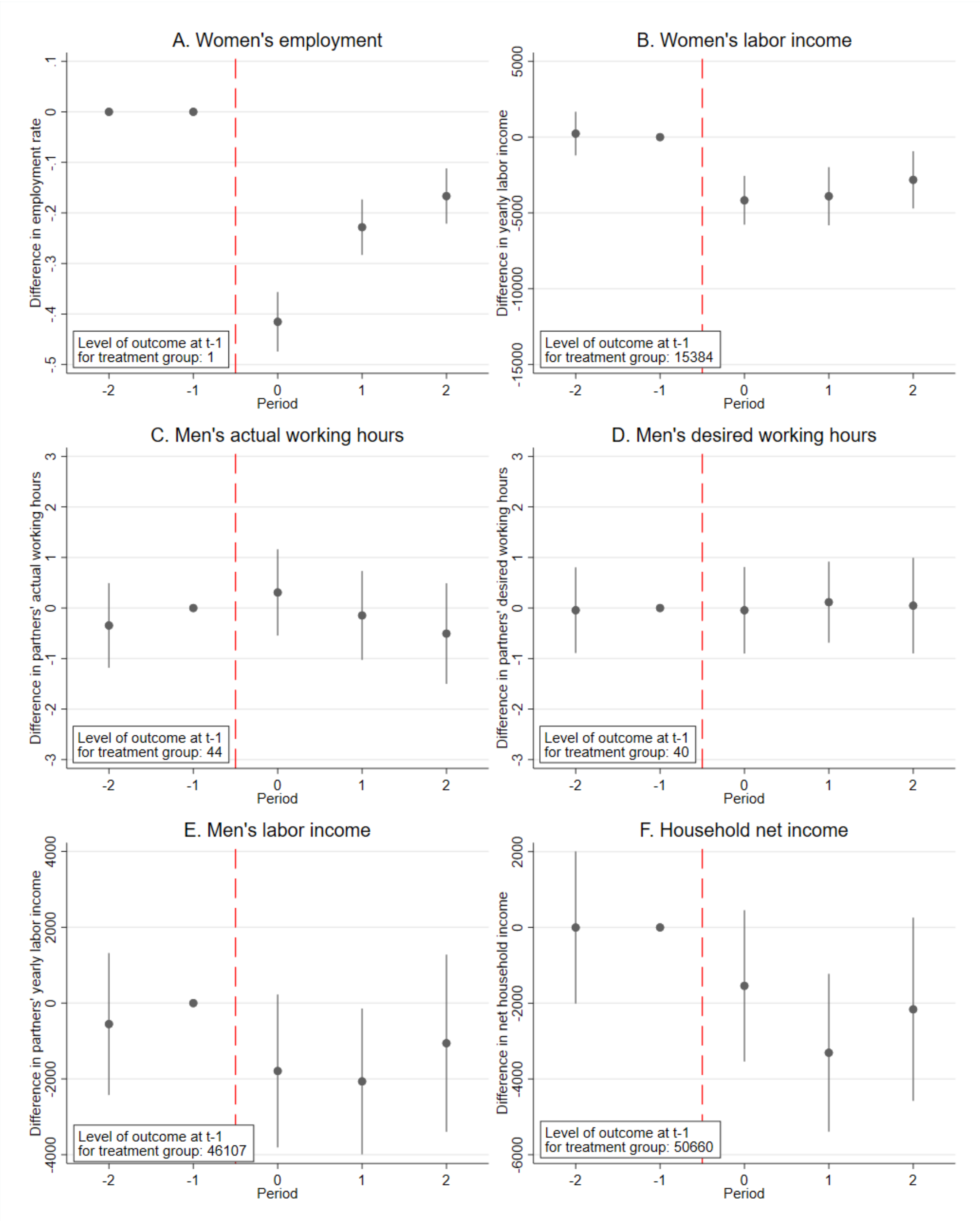
Regression Equation for Extensive Margin Outcomes The difference-in-difference approach applied for extensive margin outcomes follows

$$Y_{i,t} = \alpha\, B_{i,t} + \beta\, B_{i,t} \times T_i + \nu_i + \tau_t + \varepsilon_{i,t}, \tag{3.2}$$

where $B_{i,t}$ is a dummy variable that is 1 in any post-shock period, T_i is the respective treatment group dummy, ν_i is an individual fixed effect, τ_t is the year dummy, and $\varepsilon_{i,t}$ is an idiosyncratic error. In this setting the β coefficient of the interaction term between treatment indicator and the pre-/post-shock dummy is the coefficient of interest.

Men's labor supply reactions to female partners' involuntary job loss Our main analyses focus on women's labor supply preferences after their male partner is affected by an involuntary job loss. One of the main reasons for concentrating on women's labor supply preferences is the high share of male workers already working in full-time positions. Thus, there is little scope for male partners to increase labor supply and compensate for lost income due to their partners' job loss. However, for reasons of transparency and comprehensiveness, we also conducted a full analysis based on male labor supply preferences after female partners' involuntary job loss.

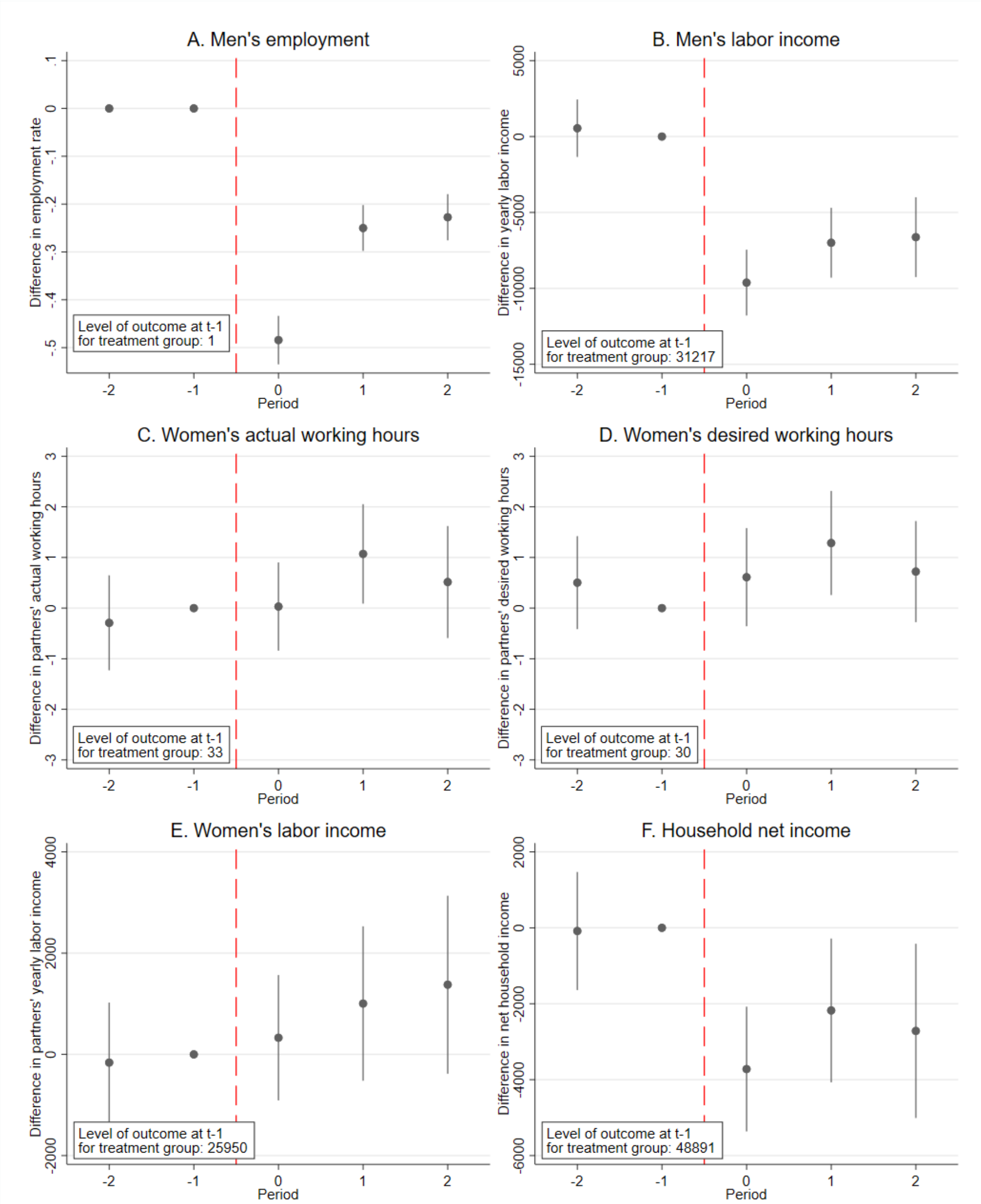
Figure 3.8 shows the results of the main analyses considering households in which the female partner was affected by an involuntary job loss. Panel A shows that the employment rate among affected women drops by about 40% in period 0, while the effect is around -20% in periods 1 and 2. Accordingly, similarly to the findings for men affected by an involuntary job loss (see Figure 3.1), large parts of the affected population seem to find a new job rather quickly. Panel B depicts the effect on women's gross labor income. On average, it drops by almost 5,000 euros in period 0 and partially recovers in the following periods. In terms of absolute values, the income shock for women is much smaller than for men. However, we also have to consider that women's pre-shock incomes are substantially lower 15,400 euros compared to 31,527 euros for men). When analysing the male partners' labor supply (Panel C and D), we find no significant effects for actual or desired working hours. Most strikingly, the estimate for desired hours is very precisely zero. Thus, as expected, our results indicate that male labor supply remains unchanged after female partners' involuntary job loss. As depicted in Panel F, the shock to household net income is small and only significantly different from zero in period 1.



Notes: Shows period-specific coefficients according to Equation (3.1). All incomes are price-adjusted and presented in 2019 euros. Bars give robust 95% confidence intervals of the respective coefficients. Number of individual observations: 780 (390 treated, 390 control units). *Source*: SOEP v37.

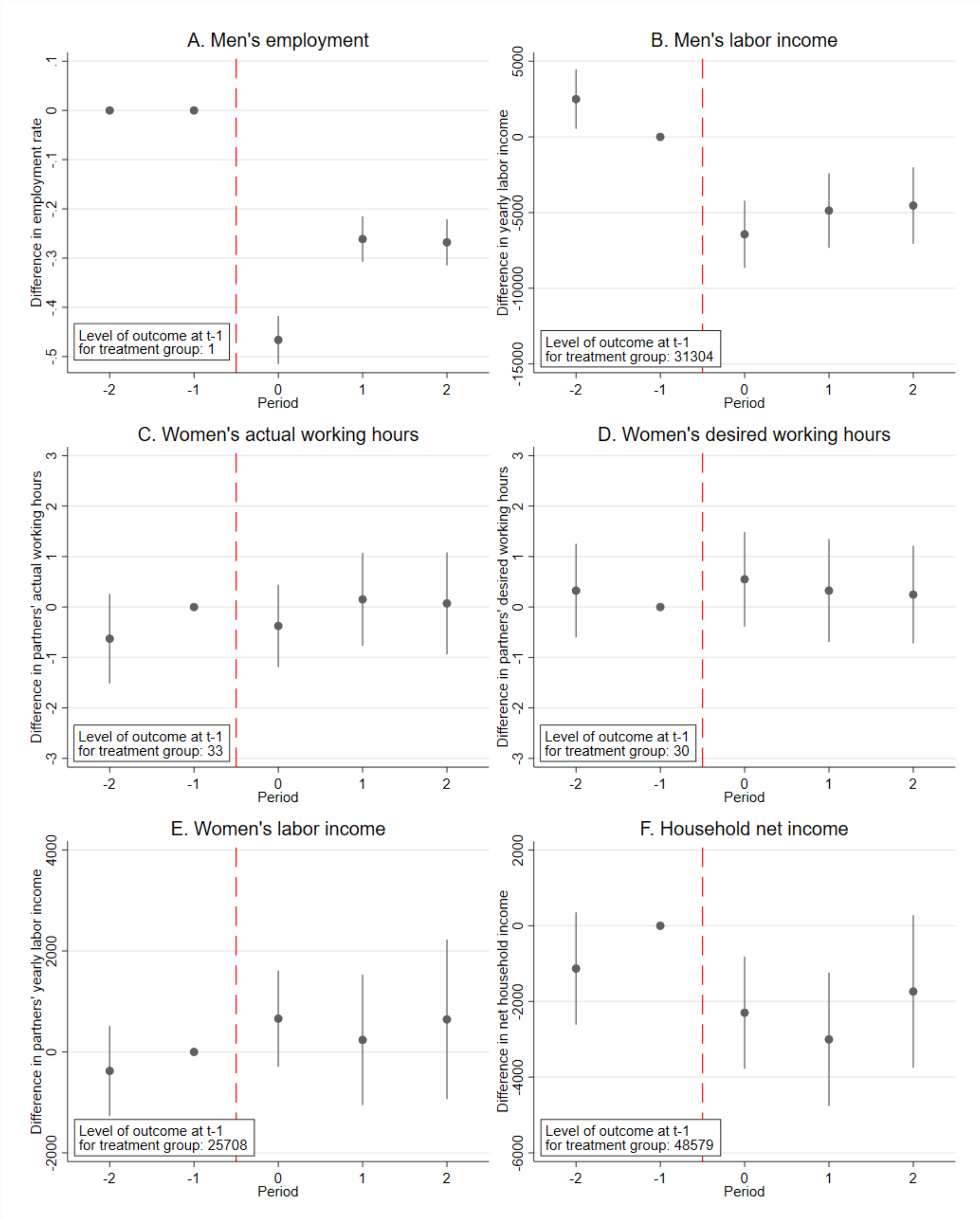
Figure 3.8: Main outcomes for men after female partners' job loss

Additional Figures



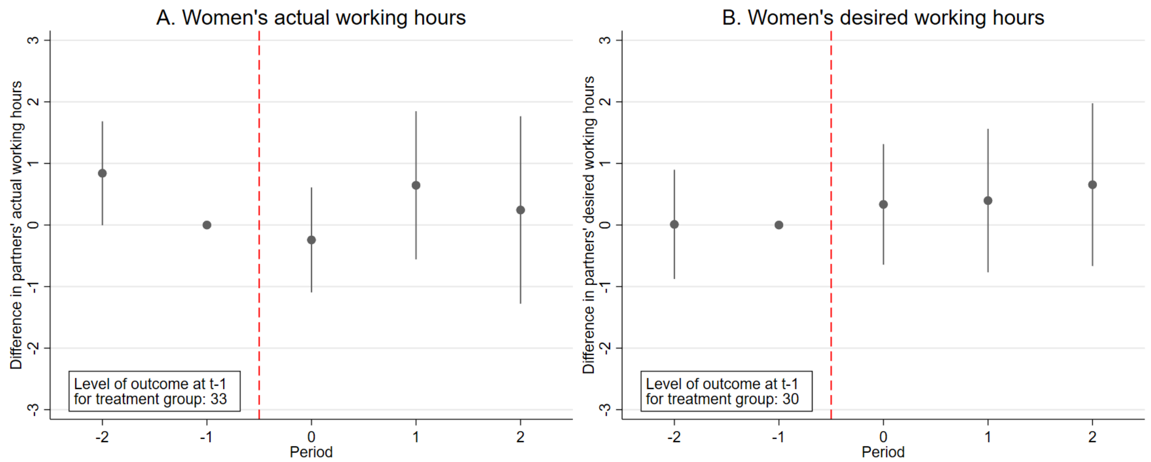
Notes: Shows period-specific coefficients according to Equation (3.1). All incomes are price-adjusted and presented in 2019 euros. Bars give robust 95% confidence intervals of the respective coefficients. Number of individual observations: 888 (444 treated, 444 control units). *Source*: SOEP v37.

Figure 3.9: Main outcomes when considering only the last experienced job loss



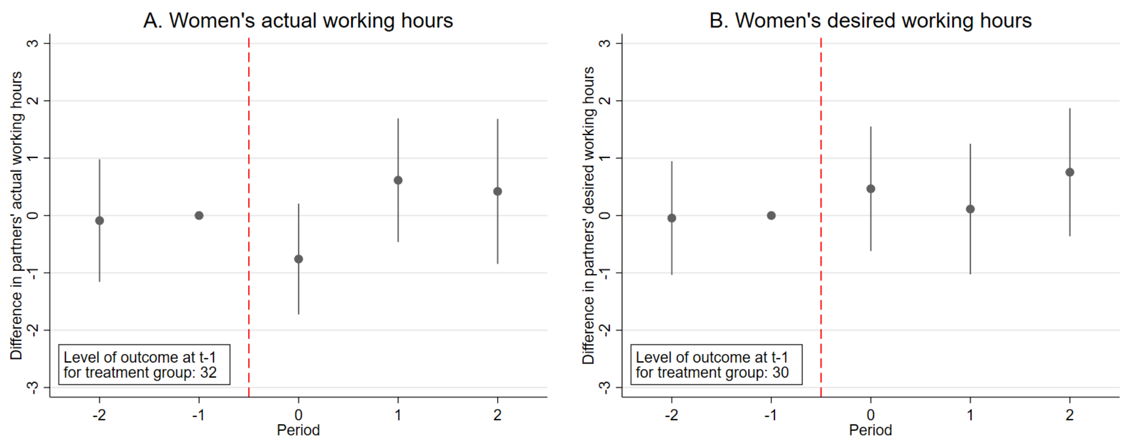
Notes: Shows period-specific coefficients according to Equation (3.1). All incomes are price-adjusted and presented in 2019 euros. Bars give robust 95% confidence intervals of the respective coefficients. Number of individual observations: 888 (444 treated, 444 control units). *Source*: SOEP v37.

Figure 3.10: Estimates based on Mahalanobis distance matching



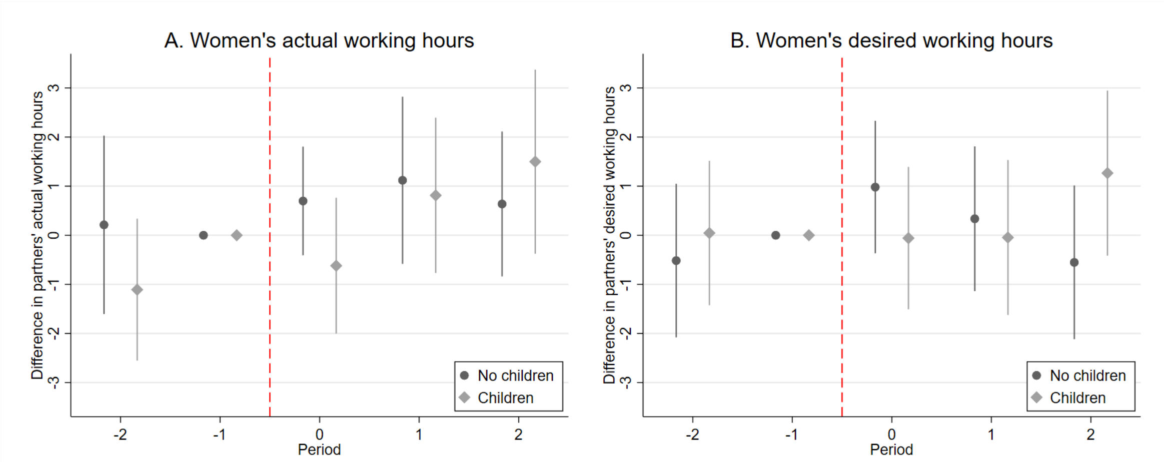
Notes: Shows period-specific coefficients according to Equation (3.1). Bars give robust 95% confidence intervals of the respective coefficients. Number of individual observations: 860 (430 treated, 430 control units). *Source*: SOEP v37.

Figure 3.11: Results of Callaway and Sant'Anna (2021) estimator



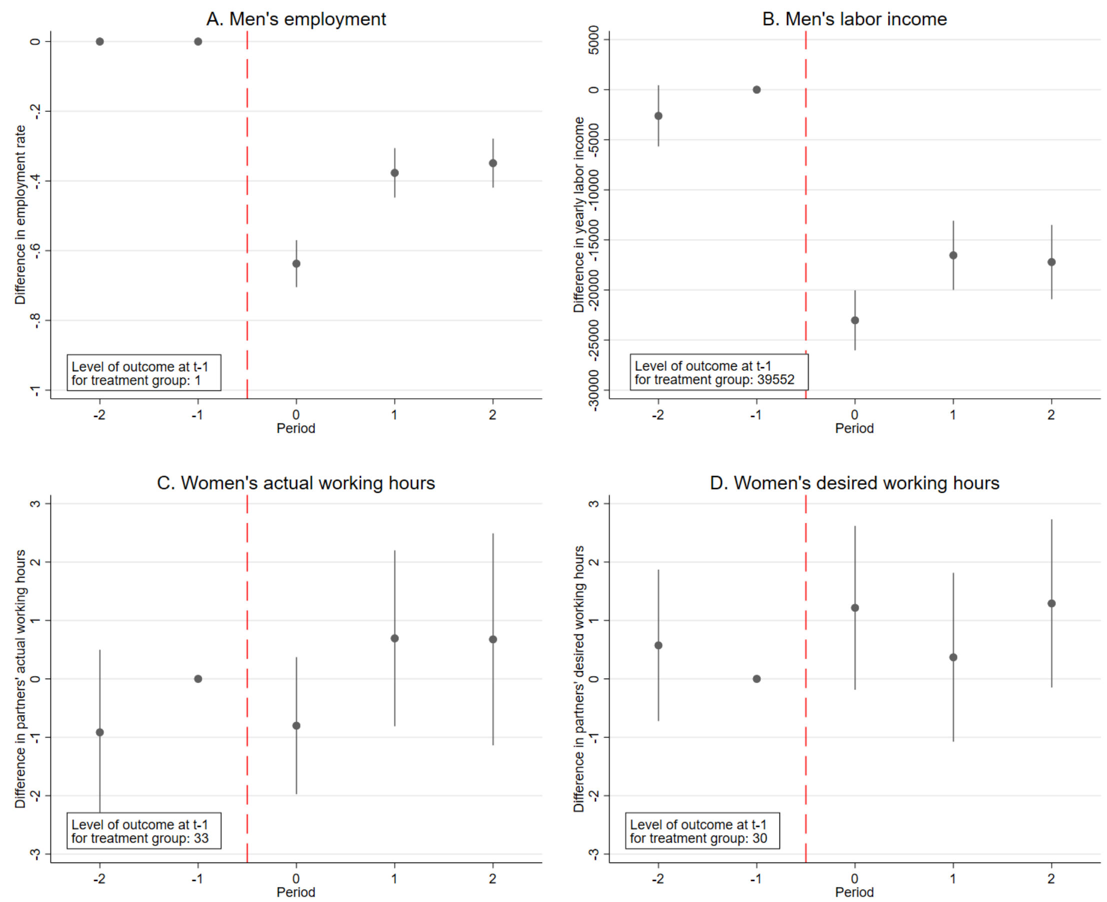
Notes: Shows period-specific coefficients according to Equation (3.1). Bars give robust 95% confidence intervals of the respective coefficients. Number of individual observations: 726 (363 treated, 363 control units). *Source*: SOEP v37.

Figure 3.12: Results for married women



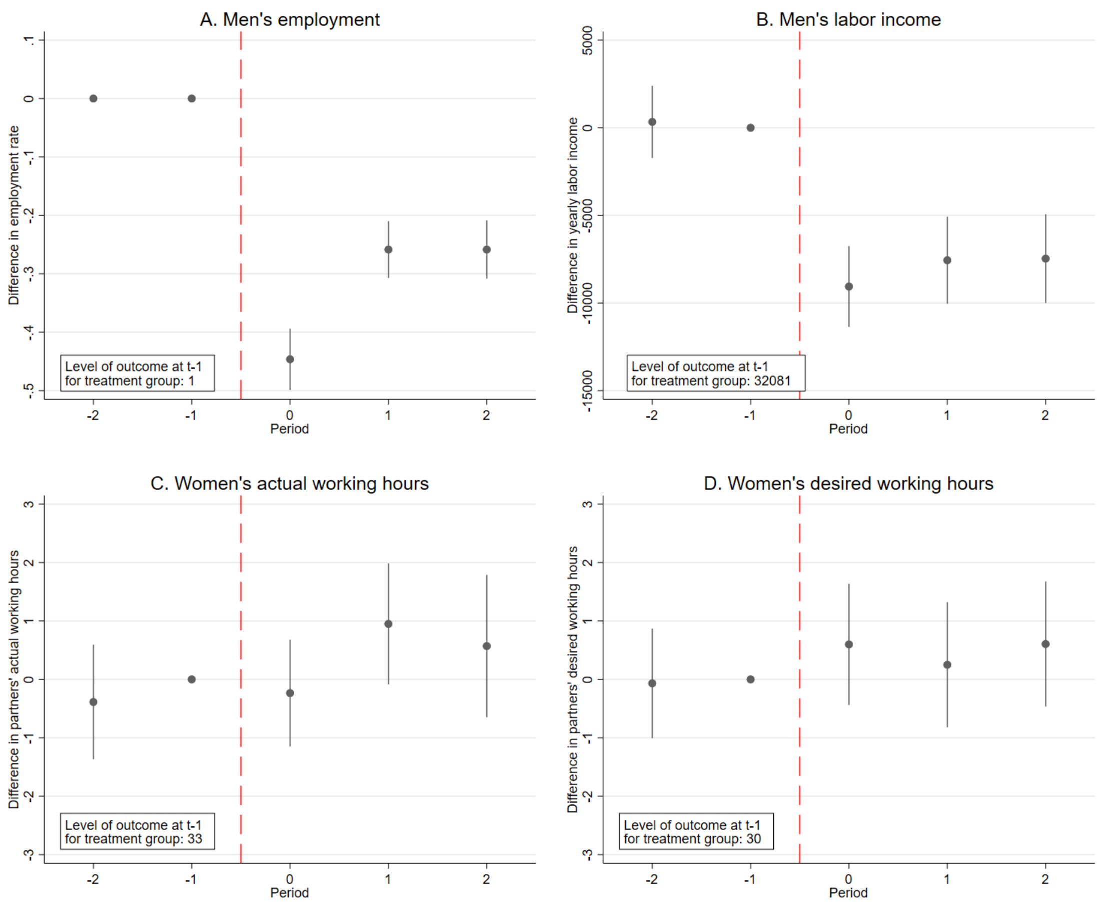
Notes: Shows period-specific coefficients according to Equation (3.1). Bars give robust 95% confidence intervals of the respective coefficients. Number of individual observations: No children: 318 (159 treated, 159 control units), children: 422 (211 treated, 211 control units). Note that the overall number is lower than in our main specification because the underlying logit estimations of our year-by-year propensity score matching procedure do not converge in all years due to the small sample size after splitting the sample. *Source*: SOEP v37.

Figure 3.13: Results for households with children vs. households without children



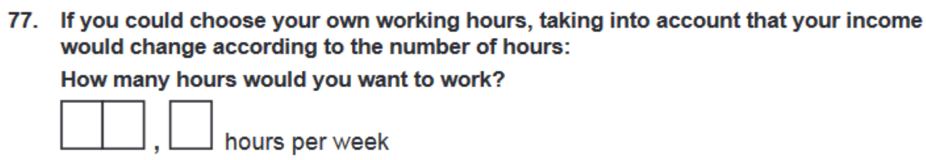
Notes: Shows period-specific coefficients according to Equation (3.1) after restricting the sample to only couples in the top half of the distribution of relative income loss from period -1 to 0. Gross labor income is price-adjusted and presented in 2019 euros. Bars give robust 95% confidence intervals of the respective coefficients. Number of individual observations: 430 (215 treated, 215 control units). *Source*: SOEP v37.

Figure 3.14: Results for top half of income loss



Notes: Shows period-specific coefficients according to Equation (3.1) after restricting the sample to only couples in which the male partner works in full-time employment in period -1. Gross labor income is price-adjusted and presented in 2019 euros. Bars give robust 95% confidence intervals of the respective coefficients. Number of individual observations: 820 (410 treated, 410 control units). *Source*: SOEP v37.

Figure 3.15: Results when men were in full-time employment before job loss


77. If you could choose your own working hours, taking into account that your income would change according to the number of hours:

How many hours would you want to work?

hours per week

Notes: Contains the question on desired working hours from the 2017 questionnaire. The questions was asked in the same way from 1997 onward.

Figure 3.16: Survey question on desired working hours in the SOEP

4 Accounting for pension wealth, the missing rich and under-coverage: A comprehensive wealth distribution for Germany[1]

4.1 Introduction

Comparing wealth between countries and within countries over time requires thorough conceptual harmonization. A country's institutional setting can create or reduce savings and investment incentives and thus affect the accumulation of wealth. A generous social security system or publicly funded higher education, for example, lower incentives to save. Adding the net present value of pension wealth to marketable wealth – to compute to what we refer to as augmented wealth – is an important step toward making wealth distributions more comparable between groups with differing levels of pension coverage or between countries with different institutional settings. Yet, because of the high data requirements to simulate social security pensions, only a few studies to date have estimated the net present value of pension wealth, for example, in Germany (Bönke et al., 2019) and the United States (Bönke et al., 2020).

[1]This is a post-peer-review, pre-copyedit version of an article published in *Economics Letters*. The final authenticated version is available online at: https://doi.org/10.1016/j.econlet.2023.111299.

This study calculates a comprehensive wealth distribution for Germany with the aim of informing German and international debate on the distribution of wealth including pension wealth. We estimate the net present value of pension wealth in Germany in 2012 and 2017 based on the individual's work history to date (accrual value) recorded in Socio-Economic Panel (SOEP) data. We build on earlier work by Frick and Grabka (2010, 2013) and Bönke et al. (2019) who estimate German public pension wealth (typically pay-as-you-go) and occupational pension wealth (pay-as-you-go or funded) for the year 2012. The current market value of private pensions is recorded in SOEP.

To ensure international comparability, we also use state-of-the-art methods to deal with two well-known shortcomings of survey data: First, to address the under-coverage of assets like financial assets and businesses, we uprate the survey data to macroeconomic aggregates (Batty et al., 2019; Garbinti et al., 2021; Albers et al., 2022). Second, to address the underrepresentation of the rich, we top-correct the survey data with rich lists (Bricker et al., 2019) and apply the generalized Pareto interpolation method developed by Blanchet et al. (2022).

We find that including pension wealth increases the wealth-income ratio of German households from around 570% to 850% in 2017. Pension wealth is the most important wealth component for the bottom 50% of German households and represents around 70% of their augmented wealth portfolio, whereas it represents around 45% for the middle 40% (P50-90) and just 3% for the top 1%. The varying importance of pension wealth across the wealth distribution has an equalizing impact: The wealth share of the bottom 50% increases from 2% to 9%, while that of the top 1% declines from 30% to 20%.

Whether or not to include pension wealth is debated. Some argue that including pension wealth calls for including *all* promises of future government transfers. For example, Saez and Zucman (2016) state that "it is not clear where to stop, and such computations are inherently fragile because of the lack of observable market prices for these types of assets". Figure 4.4 in the Appendix illustrates the sensitivity of our pension wealth estimate with respect to the chosen discount rate. We see the inclusion of pension assets as the first step towards a fully comprehensive wealth concept, which would include other future social security entitlements like the monetary value of long-term care insurance. We argue that future pension entitlements are the most immediate complement to standard wealth because they will flow with certainty after reaching retirement age. Claims on future health benefits are less straightforward to predict as their realization depends on the individual becoming sick and their magnitude depends on the (covered) cost of treatment. In general, their consumption value is very difficult to monetize. Providing an estimate for the distribution of pension wealth in Germany, we leave it to the individual researcher whether or not to include this estimate.

The remainder of the paper is structured as follows: Section 4.2 describes our data source and methodology. Section 4.3 presents our main results while Section 4.4 reviews limitations and potential extensions of this study. Section 4.5 concludes the paper.

4.2 Data and methodology

4.2.1 Data

Our main data source is the SOEP, which is a representative survey of German households (Goebel et al., 2019). Along with information on socio-demographics, education, and labor market status, the SOEP questionnaire contains a module about individual and household wealth, which has been included in the questionnaire every five years since 2002. To measure total wealth, we draw on household balance sheets (HBS) published annually by the Federal Statistical Office (Destatis). To address the underrepresentation of the very rich, we use the rich list of the German business magazine *Manager Magazin*. The 2017 version contains 1001 entries, which roughly equals 0.01% of German households (assuming an average of four family members per entry). Individual and joint household survival rates are calculated from demographic tables from the Federal Statistical Office (Statistisches Bundesamt, 2018b, 2020).

4.2.2 Estimating pension wealth

The current value of private pensions is recorded in the SOEP and included in financial wealth. To conceptualize private wealth, we estimate the current value of pay-as-you-go public (employee and civil servant) pensions and occupational pensions. For both public and occupational pensions, entitlements are accumulated over the entire course of working life and are proportional to lifetime earnings (equivalence principle).

The SOEP collects in-depth information on individual pension entitlements in Germany for both the retired and non-retired population. For the retired population, entitlements are the sum of pensions from the survey year until death. For the non-retired, entitlements are the sum of pensions from retirement age to death based on accumulated remuneration points up to the survey year.[2]

Using the accrual approach, pension wealth is the present value (PV) of the expected payments from entitlements accumulated up to the survey year (Bönke et al. (2019), Wolff (2015)) . Pension wealth PV_t^p in year $t = 2012, 2017$ is the present value of future pension payments discounted at the commonly assumed rate $r = 3\%$[3]:

$$PV_t^p = \sum_{t=0}^{T-a} s_{a,t} \times \frac{1}{(1+r)^t} \times E_t^p \tag{4.1}$$

where $s_{a,t}$ denotes the probability of a person aged a surviving until year t, $T-a$ is the remaining maximum life span, and E_t^p is the pension payment from pension scheme p (regular employee, civil servant, or occupational pension). We look at gross pension wealth before taxes and social security contributions and conduct the estimation of expected present value of widower pensions analogously to the direct entitlements outlined in Equation (4.1) but based on joint household survival functions (Bönke et al., 2019). We refrain from speculating about future taxes and contributions to estimate net pension wealth for two reasons. First, the direction of their impact is quite clear: Because of tax allowances for low-income pensioners and tax progressivity, net pension wealth would reveal an even greater equalizing impact on the wealth distribution than gross pension wealth. Second, not including

[2]Pensions may be based on an individual's own contributions during working life or their derived rights, i.e., widow(er) pensions for married couples.

[3]Mechanically, pension wealth decreases with the assumed discount rate. Bönke et al. (2019) show for a reasonable range from 2 to 5 % that an discount rate increase by one percentage point decreases median pension wealth by around 20%. Figure 4.4 in the Appendix shows that choosing r=1% results in a wealth-income ratio of ca. 390% in 2017, r=3% of 280% and r=5% of 215%.

future taxes and contributions also insures conceptual consistency and comparability regarding the other considered wealth components.

4.2.3 Uprating and top-correction of survey data

Survey data like the SOEP are known to have two shortcomings for internationally comparable inequality measures. First, survey data only capture a fraction of total business and financial assets (mostly held by the rich) while capturing the majority of total real estate wealth (mostly held by the middle class) (see Table 4.1). To close this gap, we uprate all survey data to macroeconomic aggregates (see Subsection 4.2.3). Survey data are also known to miss the very wealthy, thus creating a downward bias for income and wealth inequality measures (Bartels and Metzing, 2019; Schröder et al., 2020).

We follow the uprating procedure of Albers et al. (2022) and Batty et al. (2019). First, we start with unadjusted survey data and compute each percentile's share in the survey aggregate of each asset. Second, we top-correct the distribution by adding the asset-specific share held by the top 0.01% recorded in the MM-list to the asset-specific shares of the top percentile and reduce the asset-specific shares held by the bottom 99 percentiles proportionately. In a third step, we uprate the distribution by multiplying the asset-specific shares by the respective macroeconomic aggregate. Note that this procedure implicitly assumes that the uprating factor is constant across the distribution.

Comparing the survey data aggregates to macroeconomic aggregates from the HBS reveals different degrees of undercoverage, which results in different uprating factors. The upper panel of Table 4.1 shows the ratio of the SOEP aggregate and the HBS aggregate by asset type. Real estate wealth, consumer debt, and housing debt are covered reasonably well: The SOEP aggregate sums to around 80% of the HBS aggregate. In contrast, less than a third of HBS financial and business assets are captured in the SOEP.

Despite some households and individuals changing ranks, rank correlations between the unadjusted and the uprated wealth distribution are as high as 98% in both years (Table 4.1).

Table 4.1: Uprate factors and rank correlations

	Ratio		Uprate factor	
	2012	2017	2012	2017
Real estate wealth	80.2	68.8	1.25	1.45
Housing debt	85.5	86.0	1.17	1.16
Consumer debt	84.0	81.2	1.19	1.23
Financial assets	35.0	33.6	2.86	2.98
Business assets	26.1	30.1	3.83	3.32
Spearman rank correlations				
Individual-level			0.98	0.98
Household-level			0.98	0.98

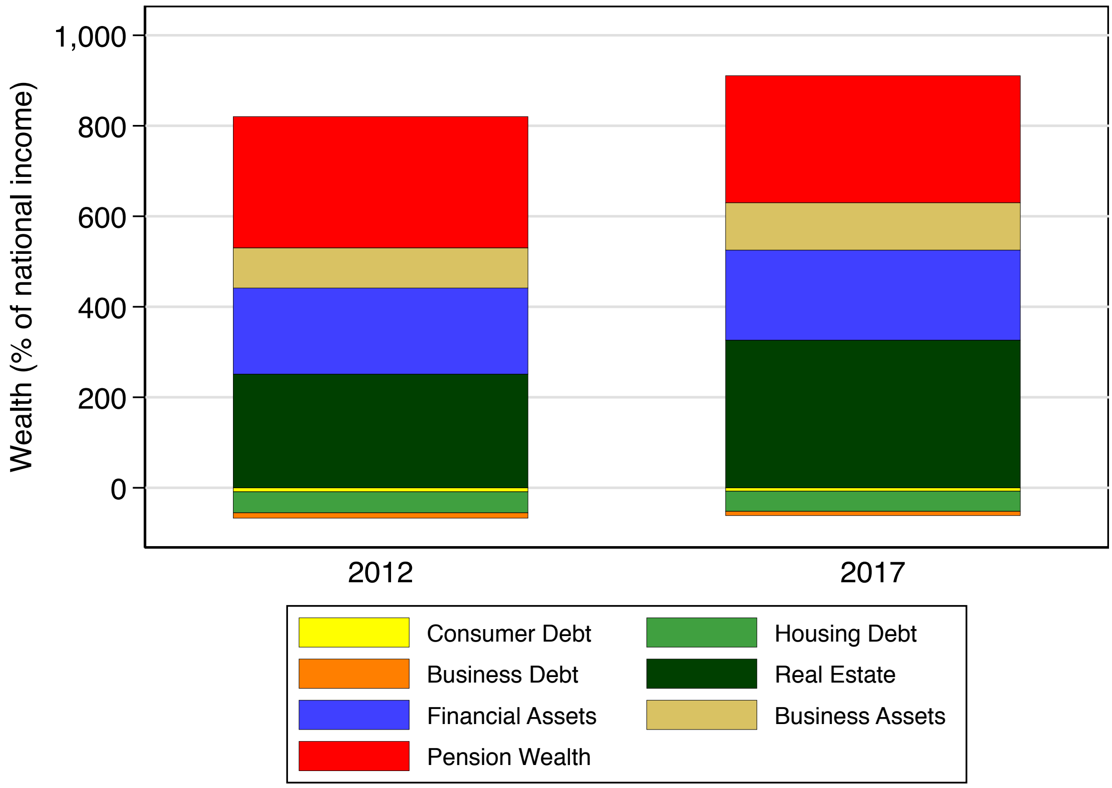
Notes: Lines 1-5 display the ratios of the SOEP wealth aggregates to HBS aggregates as well as the implicit uprate factors. Spearman's rank correlations between individuals' ranks in the net wealth distribution before and after the uprating procedure. *Sources:* SOEPv36 and total wealth from Albers et al. (2022) who use HBS and revised aggregates for business assets and real estate.

4.3 Results

4.3.1 Wealth-income ratio including pension wealth

Figure 4.1 displays the wealth-income ratio in Germany by asset type in 2012 and 2017. We are the first to include an encompassing measure of pension wealth, which in 2017 adds up to around 280% of national income in Germany. Between

2012 and 2017, the house price boom in Germany led to a remarkable expansion of real estate wealth, while the importance of other asset types remained comparably stable. In international comparison, Germany's wealth-income ratio is high, even when not accounting for pension wealth (around 570%). In 2017, wealth-income ratios (only including funded pension wealth) stood at 500% in Sweden, 540% in the United States, 570% in France, and 640% in the United Kingdom (Albers et al., 2022). Including pension wealth brings the German wealth-income ratio from around 570% to 850% in 2017.



Sources: Total wealth (excl. pension wealth) is from Albers et al. (2022). Pension wealth is calculated based on SOEPv36 top-corrected and uprated assuming a discount rate r=3%. For alternative discount rates see Figure 4.4 in the Appendix. National income is from the World Inequality Database (wid.world).

Figure 4.1: Wealth-income ratio, 2012 vs. 2017

4.3.2 Wealth composition of different wealth groups

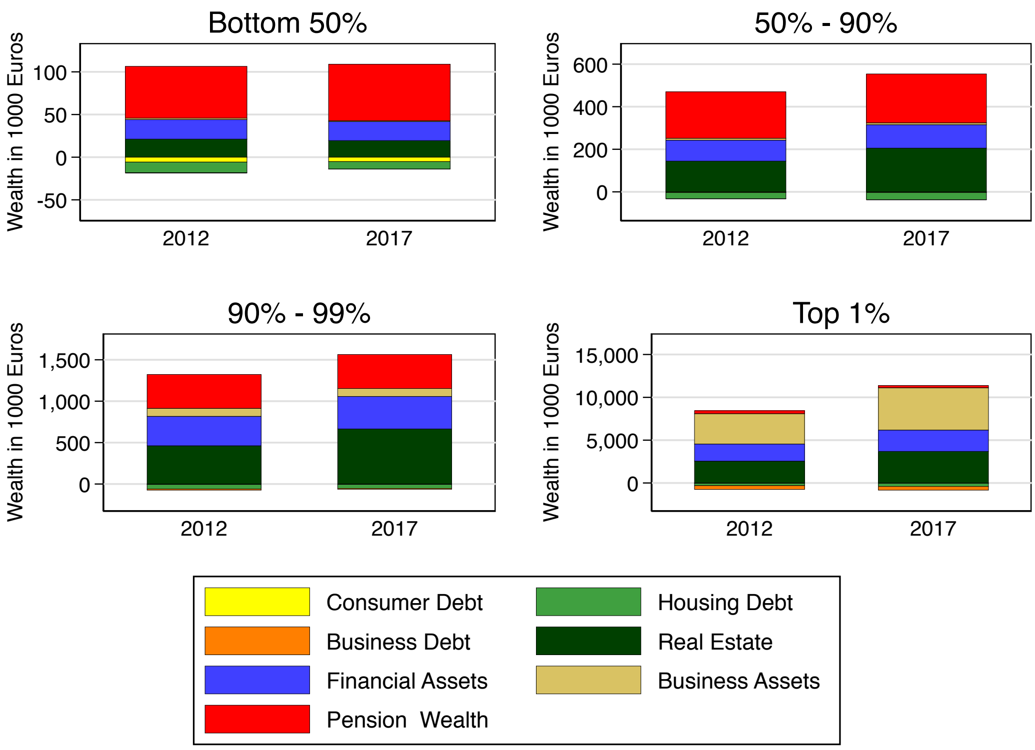
After having discussed the role of pension wealth for the aggregate composition of wealth in Germany, we now group households into four wealth groups that emerged as the new standard in recent wealth distribution studies (e.g., Garbinti et al., 2021; Kuhn et al., 2020): bottom 50%, middle class (P50-90), upper middle class (P90-99) and top 1%. Figure 4.2 shows the composition of average wealth including pension wealth in 2012 and 2017 by wealth group. The trends in Figure 4.2 replicate previous findings of a middle-class benefiting from house price growth (Albers et al., 2022).

Bottom 50%: In 2017, the average augmented wealth of the bottom 50% amounts to around 95,000 euros. Pension wealth is the most important wealth component for these households, representing around 70% of their augmented wealth portfolio. Their average augmented wealth growth between 2012 and 2017 was comparably low (+8%).

Middle class (50-90%): For the German middle class, pension wealth and housing are the most important wealth components. In 2017, their pension wealth accounts for around 45% and net housing wealth for around 33% of their average augmented wealth. Largely driven by rising real estate wealth, the average augmented wealth of the middle class increased by 18% between 2012 and 2017.

Upper middle class (90-99%): When moving up the augmented wealth distribution to the upper middle class, we find that both financial assets and business assets are increasingly important. In contrast, while still forming a substantial part of the overall portfolio,pension wealth declines in relative weight to around 27%. Housing still represents the most important asset for the German upper middle class (41% of augmented wealth in 2017). Average augmented wealth increased by 20% between 2012 and 2017.

Top 1%: Business assets are the dominant asset class for the top 1%, amounting to around 43% of their average augmented wealth in 2017. Housing (31%) and financial assets (23%) are also important. In contrast to the other wealth groups, the relative weight of pension wealth is negligible for the top 1% . With an increase of 37%, the top 1% faced the highest growth in average augmented wealth among all wealth groups between 2012 and 2017. Their remarkable wealth growth is a result of both capital gains and savings (Albers et al., 2022), but is not related to pension wealth.



Notes: Composition of household wealth in 2015 euros. *Source*: SOEPv36 top-corrected and uprated, own calculations.

Figure 4.2: Heterogeneity of portfolios for the bottom, middle, and top, 2012 vs. 2017

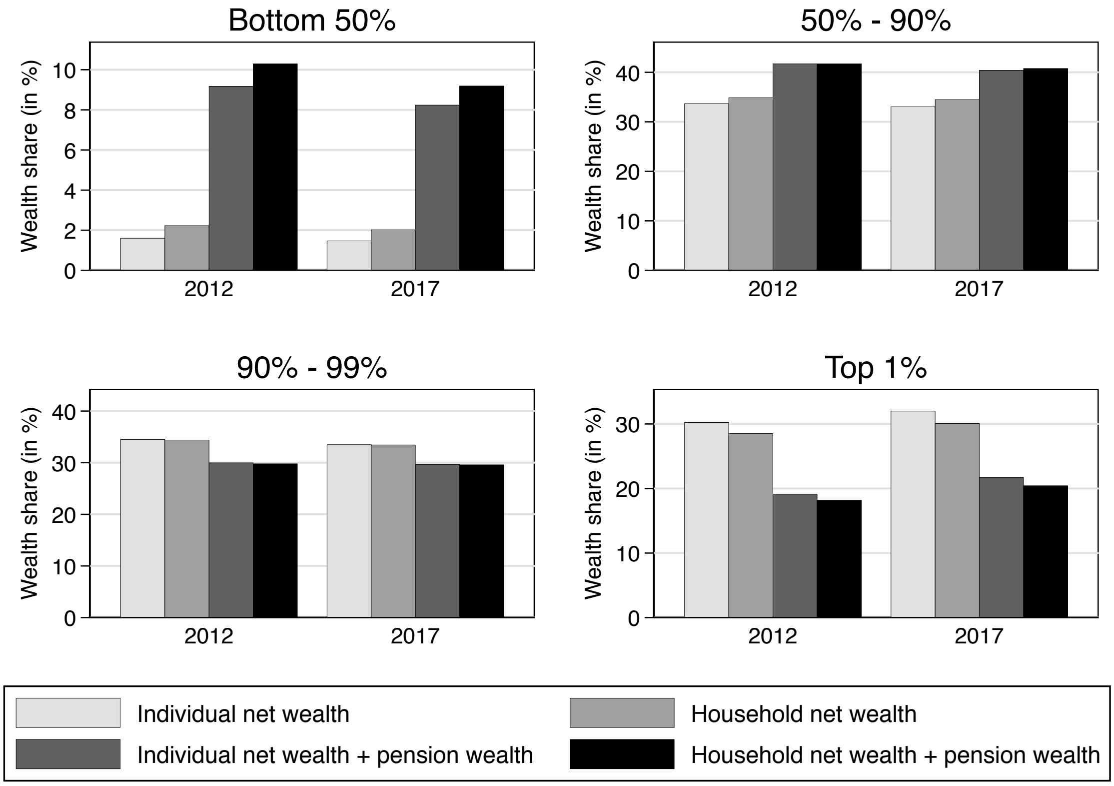
4.3.3 The distribution of wealth including pension wealth

How does the inclusion of pension wealth affect the wealth shares of the different wealth groups? The SOEP enables us to report both household and individual wealth shares.

Figure 4.3 compares individual and household wealth shares of four wealth groups in 2012 and 2017 using different wealth concepts (net wealth vs. net wealth + pension wealth). Two findings are worth noting.

First, a comparison of households to individuals shows that the wealth share of the bottom 50% increases at the expense of the top 1%, which loses in relative terms. This phenomenon occurs because individuals living together do not perfectly sort by their individual wealth. In other words, poor individuals live with richer individuals, which reduces household wealth at the top and increases household wealth at the bottom of the distribution. This pattern holds for both net wealth and augmented wealth.

Second, the bottom wealth share increases substantially when adding pension wealth. Due to the fact that pension wealth is the dominant factor in the wealth portfolio of the bottom 50% of households (see Figure 4.2), their wealth share in 2017 increases from 2% when only considering net wealth to 9% when also accounting for their pension wealth. In contrast, the wealth share of the top 1% declines from 30% to 20% due to the inclusion of pension wealth. Schröder et al. (2020) find a similar individual top 1% wealth share of around 35% using SOEP data with a top-wealth sample and MM list in 2019.



Notes: Net wealth is the sum of business wealth, real estate wealth, and financial assets minus consumer debt, housing debt and business debt. *Source:* SOEPv36 top-corrected and uprated, own calculations.

Figure 4.3: Shares of individual and household net wealth, 2012 vs. 2017

4.4 Qualifications and extensions

This section reflects on critical assumptions on which our results are based. Moreover, it examines potential extensions of our paper and future uses of the novel data on which it is based.

In our study, we construct a comprehensive wealth distribution including pension entitlements for Germany. We require several assumptions for our wealth calculations. For example, when uprating our survey data to macroeconomic aggregates, our procedure assumes that the uprating factor is constant across the distribution.

As already noted by Albers et al. (2022), there are arguments to be made that the uprating factor should in fact rather increase moving up the wealth distribution as under-representation of wealthy households and under-reporting of wealth is probably more extensive at the top. To address this, we would have to adjust the percentile shares for different asset types in a way that they are higher for wealthier households and lower for households at the bottom of the wealth distribution. However, for this approach, even more assumptions are needed with respect to quintile-specific under-reporting and under-representation intensity. In comparison, conducting a proportional approach relies on a much more conservative assumption and leads to our estimates representing a lower bound of wealth inequality. Additionally, this strategy allows for better comparability of our results to the ones of Albers et al. (2022), which also assume a constant uprating factor.

Furthermore, when calculating pension wealth, we assume age-specific survival rates to be homogeneous across the wealth distribution. By doing so, we neglect potential differential mortality for individuals at different positions in the wealth distribution. While there is a lack of studies on differential mortality by wealth, recent studies for Germany show a strong positive link between lifetime earnings and life expectancy (Haan et al., 2020; Glaubitz, 2023). Against this background, it is highly likely that, on average, life expectancy increases when moving up the wealth distribution. However, here, we cannot address this problem as there are not any reliable age-specific survival rates differentiated by wealth available for Germany.[4] As a result, our findings on wealth inequality should again be interpreted as a lower bound estimate. Accounting for differential mortality would likely increase wealth

[4]We refrain from making our own estimations of survival rates as the sample size of the SOEP is not large enough to estimate reliable and precise age-specific survival rates for different sub-groups (by position in the wealth distribution and gender). Furthermore, panel mortality could also bias our results (Schnell and Trappmann, 2006).

inequality as wealthy individuals, on average, likely live longer and therefore receive their pension payments for a longer period of time than individuals at the bottom of the wealth distribution.

There are several ways in which future research can build on our work. For example, our wealth distribution for Germany can be continuously updated once new SOEP waves including the module on individual and household wealth are released (once every five years). This would offer important insights on how total wealth and its distribution changes over time and could inform German policy debates on the distribution of wealth including pension wealth. Future updates would benefit from the novel sampling strategy for surveying high net-worth individuals in the SOEP (Schröder et al., 2020), which aims to address the underrepresentation of this group. Additionally, future extensions could aim to identify main drivers of changes in (pension) wealth and its distribution over time. For example, one could explore the role of increasing life expectancies, changes in income inequality or institutional changes (e.g. adjustments to pension values or reforms).

The generated data set offers a wide range of information both at the individual and household level - not only on wealth but also for a rich set of socioeconomic variables (e.g. health, employment, family situation). Using this novel data source, future work could, for example, analyze wealth mobility over time, i.e. the extent to which individuals or households move up or down in the (augmented) wealth distribution over time and explore common drivers (e.g. health shocks, bequests, divorce, job-related changes). Furthermore, our generated data set could be utilized to add to recent evidence on the gender gap in (augmented) wealth in Germany (Cordova et al., 2022; Bartels et al., 2023) by enabling investigations into how this gap and its drivers change over time.

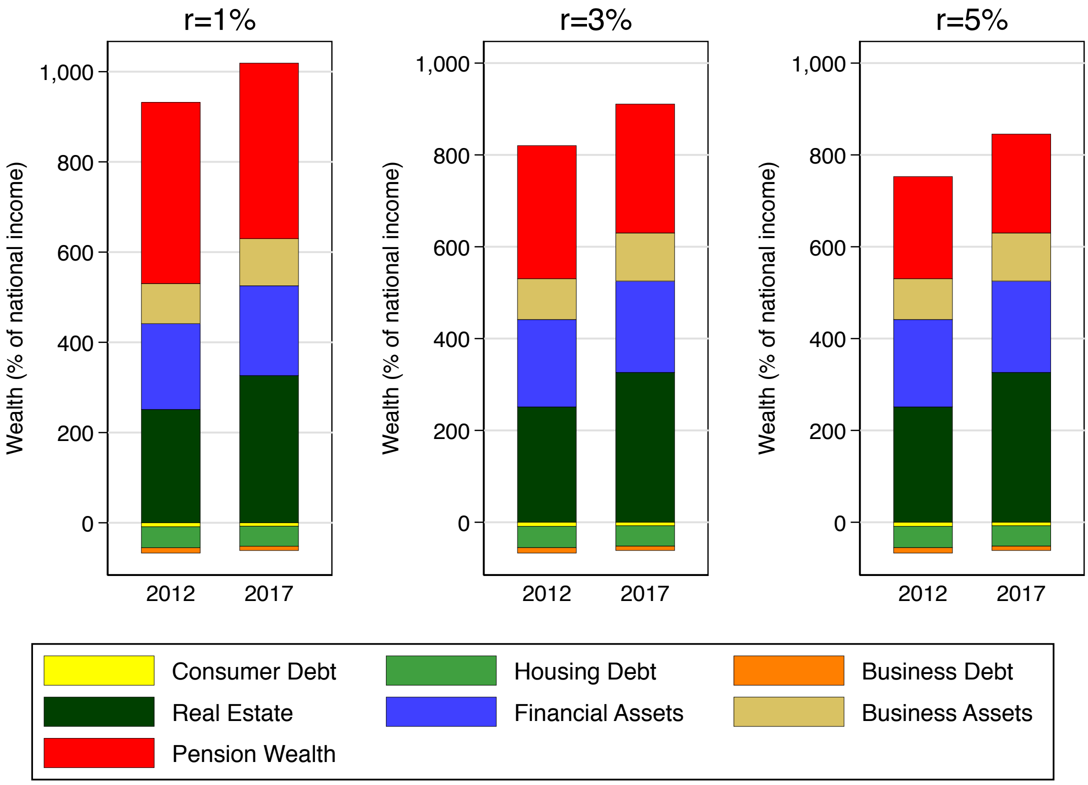
4.5 Conclusion

In this paper, we have provided a comprehensive wealth distribution for Germany to inform the policy debate on the distribution of wealth including pension wealth. We found that including pension wealth increases the wealth-income ratio of German households from 570% to 850%. The relevance of pensions compared to national income not only indicates the importance and generosity of a country's pension system; it is also informative about the current obligations and sustainability of welfare states in times of demographic change and declining growth rates.

Because of the varying importance of pensions across the wealth distribution, pension wealth plays an equalizing role: The wealth share of the bottom 50% increases from 2% to 9% when including pension wealth, whereas that of the top 1% declines from 30% to 20%.

Adding pension wealth to net wealth is an important step towards making wealth distributions more comparable between groups with differing levels of public pension coverage (e.g. self employed vs. employees) or between countries with different institutional settings for at least two reasons. First, future pension benefits, next to standard wealth, determine consumption opportunities during retirement. Second, these benefits provide a safety net that helps mitigate financial risks like outliving one's savings. Hence, public pensions schemes have immediate incentives for private wealth accumulation and not including pension wealth may bias comparisons and underestimate capabilities.

4.6 Appendix



Sources: Total wealth (excl. pension wealth) is from Albers et al. (2022). Pension wealth is calculated based on SOEPv36 top-corrected and uprated. National income is from the World Inequality Database (wid.world).

Figure 4.4: Wealth-income ratio for different discount rates, 2012 vs. 2017

Table 4.2: Components of the wealth-income ratio

	Ratio (%)		Total (1,000 euros)	
	2012	2017	2012	2017
Consumer debt	-9	-8	-200	-204
Housing debt	-47	-45	-1053	-1202
Business debt	-12	-10	-269	-256
Real estate	251	326	5,688	8,819
Financial assets	190	199	4,309	5,382
Business assets	89	105	2,013	2,831
Pension wealth	290	281	6,570	7,593
Augmented wealth	752	848	17,058	22,961

Notes: Ratios in percent of the national income. Total wealth components in current billion euros. *Sources:* SOEPv36 top-corrected and uprated, own calculations. National income is from the World Inequality Database (wid.world).

Table 4.3: Wealth thresholds for the augmented wealth distribution

	Net wealth		Augmented wealth	
Quantile	2012	2017	2012	2017
P50	69,842	89,551	221,695	255,266
P90	510,865	643,874	833,175	964,389
P99	2,134,640	2,946,011	2,586,895	3,564,573

Notes: Thresholds for the household net wealth and the augmented wealth distribution in 2015 euros. *Source:* SOEPv36 top-corrected and uprated, own calculations.

Table 4.4: Portfolios across wealth groups in 2017, 1,000 euros

	Bottom 50%	50%-90%	90%-99%	Top 1%
Consumer debt	-5.2	-3.0	-4.5	-31.8
Housing debt	-8.8	-33.8	-50.0	-378.0
Business debt	-0.1	-0.9	-9.6	-434.8
Real estate	19.4	205.3	664.9	3685.1
Financial assets	22.3	109.9	392.2	2494.2
Business assets	0.9	8.8	96.5	4936.0
Pension wealth	66.4	230.1	410.3	271.5
Augmented wealth	94.9	516.4	1499.8	10542.2

Notes: Composition of household wealth in 2015 euros. Wealth is reported in units of 1,000 euros. *Source:* SOEPv36 top-corrected and uprated, own calculations.

Table 4.5: Portfolios across wealth groups in 2012, 1,000 euros

	Bottom 50%	50%-90%	90%-99%	Top 1%
Consumer debt	-5.7	-3.2	-5.0	-32.6
Housing debt	-12.5	-28.7	-55.5	-271.1
Business debt	-0.3	-1.4	-14.5	-451.3
Real estate	21.2	144.5	461.6	2547.1
Financial assets	22.9	98.0	355.2	1998.3
Business assets	1.8	9.1	96.5	3546.6
Pension wealth	60.7	218.5	409.8	352.5
Augmented wealth	88.1	436.8	1248.1	7689.5

Notes: Composition of household wealth in 2015 euros. Wealth is reported in units of 1,000 euros. *Source:* SOEPv36 top-corrected and uprated, own calculations.

Table 4.6: Shares of individual and household net wealth, 2012 vs. 2017

	Bottom 50%	50%-90%	90%-99%	Top 1%
2017				
Individual net wealth	1.5	33.0	33.5	32.0
Household net wealth	2.0	34.5	33.4	30.1
Individual net wealth + pension wealth	8.2	40.4	29.6	21.8
Household net wealth + pension wealth	9.2	40.8	29.6	20.4
2012				
Individual net wealth	1.6	33.7	34.5	30.2
Household net wealth	2.2	34.9	34.4	28.5
Individual net wealth + pension wealth	9.2	41.7	30.0	19.1
Household net wealth + pension wealth	10.3	41.7	29.8	18.2

Notes: Net wealth is the sum of business wealth, real estate wealth, and financial assets minus consumer debt, housing debt, and business debt. Wealth shares in percent. *Source*: SOEPv36 top-corrected and uprated, own calculations.

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English Summary

This dissertation comprises four empirical chapters which contribute to the fields of inequality research and labor economics.

The first chapter examines the interaction between socioeconomic status, place of residence and life expectancy, which so far remains poorly understood. I contribute to deepening this understanding by, firstly, using administrative data from the German Pension Insurance to provide novel estimates for remaining life expectancy at age 65 by lifetime earnings quintiles and geographic areas (NUTS2). I show evidence for substantial heterogeneity in the link between lifetime earnings and life expectancy across NUTS2 regions in Germany. Subsequently, I use these life expectancy estimates together with a rich set of place characteristics to conduct a correlational analysis investigating which place factors are associated with longevity. Specifically, I examine whether place matters differently for individuals' life expectancy depending on their socioeconomic status and whether this interaction between place factors, socioeconomic status and life expectancy has changed over time. Place factors associated with longevity are better healthcare supply, lower air pollution levels, lower regional poverty levels and a higher prevalence of healthy behaviors. Strikingly, the correlations between place factors and life expectancy appear to be homogeneous rather than heterogeneous in magnitude and direction for individuals at the top and the bottom of the lifetime earnings distribution. Furthermore, I find suggestive evidence that the importance of place for the life expectancy of low income individuals may have decreased over time.

While the second chapter of this thesis also investigates lifetime earnings, here we refrain from using administrative data. Instead, we use data from the Socio-economic Panel (SOEP) together with a dynamic microsimulation approach to facilitate lifetime analysis for a more comprehensive sample (including women, self-employed individuals and civil servants). The aim of this chapter is to advance understanding of the persisting gender earnings gap in Germany. First, we briefly investigate gender inequality in wages and annual earnings in the cross-section, which is mainly driven by gender differences in hours worked and accumulated work experience. Subsequently, we focus on the simulation of complete earnings biographies from SOEP data, which in the next step facilitates the investigation of the gender gap in lifetime earnings. We find evidence for an average gender lifetime earnings gap of 51.5% for birth cohorts 1964-1972. We show that this unadjusted gender lifetime earnings gap increases strongly with the number of children, ranging from 17.3% for childless women to 68.0% for women with three or more children. However, using a counterfactual analysis we find that the adjusted gender lifetime earnings gap only differs slightly by women's family background.

In the third chapter, we examine the effect of an individual's involuntary job loss on their partner's actual and desired labor supply response. Thus—while existing research has found little to no evidence for an added worker effect in Germany—we shed light on the question of whether a desired added worker effect exists. Using data from the SOEP, we study individuals' changes in actual and desired working hours after their partners' involuntary job loss in an event study design. Our results show that neither desired nor actual working hours change significantly. These findings are robust for several sub-groups and for different econometric specifications. Therefore, we provide first evidence that the absence of the added worker effect is in line with individuals' stated labor supply preferences and is not the result of an inability to realize desired working hours.

In the fourth chapter, we construct a comprehensive wealth distribution for Germany in order to inform the national and international debate on the distribution of wealth including pension entitlements. We estimate the net present value of pension wealth in Germany in 2012 and 2017 using SOEP data. To ensure international comparability, we also implement state-of-the-art methods to deal with two well-documented shortcomings of survey data. First, to address the undercoverage of assets such as financial and business assets, we uprate the survey data to macroeconomic aggregates. Second, in order to address the underrepresentation of the rich, we top-correct the survey data using rich lists. We show that including pension wealth increases German households' wealth-income ratio from 570% to 850% in 2017. Furthermore, we provide evidence that pension wealth has an equalizing role by showing that the wealth share of the bottom 50% increases from 2% to 9% once pension wealth is included, while the wealth share of the top 1% declines from 30% to 20%.

Deutsche Zusammenfassung

Diese Dissertation besteht aus vier empirischen Kapiteln, die Beiträge zur Ungleichheitsforschung und zur Arbeitsmarktökonomie leisten.

Das erste Kapitel untersucht die Interaktion zwischen sozioökonomischem Status, regionalen Charakteristiken des Wohnortes und Lebenserwartung, die bisher nur unzureichend erforscht wurde. Ich trage zum Verständnis dieser Interaktion bei, indem ich in einem ersten Schritt mittels administrativer Daten der Deutschen Rentenversicherung neue Schätzungen für die Restlebenserwartung im Alter von 65 Jahren nach Position in der Lebenseinkommensverteilung (Quintile) und geografischen Regionen (NUTS2) generiere. Ich zeige, dass der Zusammenhang zwischen Lebenseinkommen und Lebenserwartung zwischen den einzelnen NUTS2-Regionen Deutschlands sehr heterogen ausgeprägt ist. Anschließend verwende ich diese Schätzungen der Lebenserwartung zusammen mit einer Vielzahl von regionalen Charakteristiken, um eine Korrelationsanalyse durchzuführen. Zu den regionalen Charakteristiken, die mit einer höheren Lebenserwartung in Verbindung gebracht werden können, gehören eine bessere Gesundheitsversorgung, eine geringere Luftverschmutzung, ein niedrigeres regionales Armutsniveau und eine höhere Prävalenz von gesunden Verhaltensweisen. Auffallend ist, dass die Korrelationen zwischen regionalen Charakteristiken und der Lebenserwartung bei Personen am oberen und unteren Ende der Lebenseinkommensverteilung in Größe und Richtung eher homogen als heterogen sind. Darüber hinaus finde ich Hinweise darauf, dass die Bedeutung regionaler Charakteristiken für die Lebenserwartung von Personen mit niedrigen Lebenseinkommen im Laufe der Zeit abgenommen hat.

Zusammenfassung

Das zweite Kapitel dieser Arbeit untersucht ebenfalls Lebenseinkommen, jedoch verzichten wir hier auf die Verwendung administrativer Daten. Stattdessen verwenden wir Umfragedaten des Sozio-oekonomischen Panel (SOEP) in Verbindung mit einem dynamischen Mikrosimulationsansatz, um eine Lebenszeitanalyse für eine umfassendere Stichprobe (einschließlich Frauen, Selbstständige und Beamte) zu ermöglichen. Ziel dieses Kapitels ist es, die anhaltenden geschlechtsspezifischen Einkommensunterschiede in Deutschland besser zu verstehen. In einem ersten Schritt untersuchen wir die geschlechtsspezifischen Unterschiede in Löhnen und Jahreseinkommen im Querschnitt. Hier sind hauptsächlich Unterschiede in den geleisteten Arbeitsstunden und der akkumulierten Berufserfahrung die Haupttreiber der geschlechtsspezifischen Unterschiede. Anschließend konzentrieren wir uns auf die Simulation vollständiger Erwerbsbiographien mittels SOEP-Daten, welche im nächsten Schritt die Untersuchung der geschlechtsspezifischen Unterschiede in den Lebenseinkommen ermöglicht. Wir zeigen, dass die Lücke in den Lebenseinkommen zwischen Männern und Frauen („Gender Lifetime Earnings Gap") im Durchschnitt rund 51,5% für die Geburtskohorten 1964-1972 beträgt. Wir zeigen zudem, dass diese (unbereinigte) geschlechtsspezifische Lebenseinkommenslücke stark von der Anzahl der Kinder abhängt - von 17,3% bei kinderlosen Frauen bis zu 68,0% bei Frauen mit drei oder mehr Kindern. Anhand einer kontrafaktischen Analyse stellen wir jedoch fest, dass der bereinigte geschlechtsspezifische Lebenseinkommensunterschied nur geringfügig vom familiären Hintergrund der Frauen abhängt.

Im dritten Kapitel untersuchen wir die Auswirkungen des unfreiwilligen Arbeitsplatzverlustes einer Person auf das tatsächliche und gewünschte Arbeitsangebot des Partners. Während bisherige Studien wenige bis gar keine Belege für einen „added worker effect“ in Deutschland gefunden hat, untersuchen wir, ob es einen „desired added worker effect“ gibt, d.h. einen Anstieg des gewünschten Arbeitsangebots, welcher sich aber gegebenenfalls nicht in einer tatsächlichen Änderung widerspiegelt. Anhand von Daten aus dem SOEP untersuchen wir die Veränderungen der tatsächlichen und gewünschten Arbeitszeiten von Personen nach dem unfreiwilligen Verlust des Arbeitsplatzes ihres Partners mittels eines „Event Study“-Ansatzes. Unsere Ergebnisse zeigen, dass sich weder die gewünschte noch die tatsächliche Arbeitszeit signifikant ändert. Diese Ergebnisse sind für mehrere Untergruppen und für verschiedene ökonometrische Spezifikationen robust. Wir liefern also erste Belege dafür, dass das Ausbleiben eines „added worker effects“ mit den Arbeitsangebotspräferenzen der Individuen übereinstimmt und nicht das Ergebnis davon ist, dass Individuen ihre gewünschten Arbeitszeiten nicht realisieren können.

Im vierten Kapitel konstruieren wir eine umfassende Vermögensverteilung für Deutschland, um die nationale und internationale Debatte über die Vermögensverteilung (einschließlich der Rentenvermögen) zu informieren. Dafür schätzen wir u.a. den Gegenwartswert des Rentenvermögens in Deutschland für die Jahre 2012 und 2017 auf Basis von SOEP-Daten. Um die internationale Vergleichbarkeit zu gewährleisten, werden die SOEP-Daten zudem in zweifacher Weise angepasst. Zum einen werden die SOEP-Daten auf makroökonomische Aggregate hochgerechnet, um die Untererfassung von Finanzvermögen und Unternehmensvermögen zu adressieren. Außerdem werden die SOEP-Befragungsdaten anhand von Reichenlisten korrigiert, da sehr reiche Haushalte in den Befragungsdaten untererfasst sind. Durch die Einbeziehung des Rentenvermögens steigt das Verhältnis der Vermögen zum deutschen Nationaleinkommen von 570% auf 850%. Darüber hinaus zeigen wir, dass die Einbeziehung der Rentenvermögen die Vermögensungleichheit reduziert.

So steigt dadurch der Vermögensanteil der ärmeren 50% der Bevölkerung von 2% auf 9%, während der Vermögensanteil der Top-Vermögenden (1%) von 30% auf 20% sinkt.

Erklärung

Erklärung gemäß §4 Abs. 2

Hiermit erkläre ich, dass ich mich noch keinem Promotionsverfahren unterzogen oder um Zulassung zu einem solchen beworben habe, und die Dissertation in der gleichen oder einer anderen Fassung bzw. Überarbeitung einer anderen Fakultät, einem Prüfungsausschuss oder einem Fachvertreter an einer anderen Hochschule nicht bereits zur Überprüfung vorgelegen hat.

(Ort, Datum, Unterschrift)

Erklärung gemäß §10 Abs. 3

Ich habe meine Dissertation soweit im Folgenden nicht anders vermerkt selbständig verfasst.

Folgende Hilfsmittel wurden benutzt

- Statistik und Mathematik: Stata, Excel, R
- Schriftsatz und Formatierung: LaTeX

(Ort, Datum, Unterschrift)