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Short-time Work and Unemployment: Long-term Effects on Workers' Labor-market Outcomes, Time Use and Life Satisfaction

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Abstract

Many countries use job-retention schemes, such as short-time work (STW), to stabilize the labor market during economic downturns. While these schemes might prevent unemployment (UE) and its adverse effects on workers, STW could also deter workers from moving to more productive firms, thereby negatively affecting their labor market outcomes in the long run. We analyze the long-term effects of STW and UE on individual workers using survey data from the SOEP for 1984–2023, which allows us to examine a broad set of yearly measured outcome variables, including employment, weekly working hours, real hourly wages, time spent on unpaid care work and life satisfaction. For the empirical analysis, we employ a two-step procedure that includes propensity score matching and an event-study model with individual fixed effects. Our findings suggest that, in the German institutional context, STW had no significant negative effects on workers' labor market outcomes in the financial crisis of 2008/2009 and the economic crisis caused by the COVID-19 pandemic. This suggests that STW did not deter workers from switching to more productive firms. For the economic crisis following German reunification in the 1990s, however, we find that STW negatively affects workers' long-term outcomes, albeit less strongly than episodes of UE. These findings suggest that the stabilizing effect of STW strongly depends on the economic context.

Keywords: labor market shocks, job loss, short-time work, unemployment, event-study analysis

JEL codes: H31, E32, J13, J16, J22

1 Introduction

During economic crises, many workers experience unemployment (UE) episodes following job loss, which often lead to adverse long-term consequences for their labor market outcomes, as well as for their life satisfaction, emotional well-being, and health. An extensive body of literature documents these scarring effects of UE episodes (see e.g. Bertheau et al. (2023); Couch and Placzek (2010); Davis and von Wachter (2011); Eliason and Storrie (2006); Hijzen et al. (2010); Jacobson et al. (1993); Lachowska et al. (2020); Schmieder et al. (2023)).

For firms, laying off workers during economic distress may be costly as well, particularly if the crisis is temporary, because rehiring after the crisis can entail the loss of valuable firm-specific human capital. To avoid these detrimental effects, many countries in Europe have introduced job retention schemes such as short-time work (STW).[1] STW programs allow firms to retain their workers while the program pays a substantial fraction of their wages if workers reduce their working hours (sometimes even to zero) for a limited period. In the past two major global economic crises, including the financial crisis of 2008-2009 and the COVID-19 crisis in 2020-2021, many countries have used STW schemes to stabilize the labor market during economic downturns. While enabling firms to retain their staff during economic crises might reduce inefficiencies associated with job destruction and rehiring, STW schemes might also deter workers from quitting their jobs and moving to more productive firms, thereby introducing inefficiencies in the economy. Thus, the overall effects of STW schemes on firms and workers are not clear a priori.

Recently, a growing number of studies have examined the economic effects of STW schemes, particularly from the perspective of firms (see Cahuc (2024) for an extensive overview). Empirical studies for the period of the financial crisis of 2008-2009 in Germany, Italy, Switzerland, and France show that short-time work schemes can have positive effects on employment and on the survival of firms during times of crisis (see, e.g., Cahuc et al. (2021); Gehrke and Hochmuth (2021); Giupponi and Landais (2023); Kopp and Siegenthaler (2021)). Studies on the effects of STW episodes on workers' labor market outcomes or career trajectories have been relatively scarce to date. Few examples include Arranz et al. (2018) for Spain, Pavlopoulos and Chkalova (2022) for the Netherlands, Giupponi and Landais

[1] These countries for example include Germany, France, Italy, the Netherlands and the UK. See Giupponi et al. (2021) for a comparison of these schemes to the social insurance responses in times of crisis in the US system.

(2023) for Italy, Vogtenhuber et al. (2024) for Austria, and Tilly and Niedermayer (2016) as well as Brinkmann et al. (2024) for Germany.

In this study, we analyze the long-term effects of STW episodes on a broad set of workers' labor market outcomes, time spent on unpaid care work, and life satisfaction. Thereby, we aim to provide a comprehensive picture of the consequences of this type of event for individual workers. Our analysis focuses on Germany, a country with a long history of using STW as a job-retention policy. Germany repeatedly relied on STW during major crises, including the reunification period and especially its initial recession in 1993-1994, the financial crisis of 2008–2009, and the economic crisis caused by the COVID-19 pandemic (2020-2021). However, empirical evidence on the long-run consequences of STW for individual workers remains limited. In Germany, administrative data identifying STW participation at the individual level are available only for recent periods since 2020 (Brinkmann et al. (2024)) and are otherwise restricted to small regional samples (Tilly and Niedermayer (2016)).

We base the empirical analysis on survey data from the German Socio-Economic Panel (SOEP) (Goebel et al. (2019)). In this data, episodes of STW are observed for a representative sample of the German population over a long time period (1984 to 2023). An important advantage of these survey data over administrative records is that it enables the analysis of a broad set of outcomes. In addition to effects on subsequent employment, we can examine effects on hourly wages, weekly working hours, time spent on unpaid care work, and life satisfaction measures. This broader perspective allows us to assess how episodes of STW affect not only labor market outcomes but also how these are systematically linked to the private sphere.

The central challenge in the empirical analysis of the effects of STW episodes on individual workers is the lack of a clear counterfactual. In countries with existing STW schemes, there is a dual selection process of workers into episodes of STW: first, there is the selection of firms into using STW, and second, there is the selection of workers within the firm into the STW program (Cahuc (2024)). Depending on other institutional characteristics of the labor market, such as job-protection legislation, and wage-setting flexibility, the counterfactual scenario for workers who experience an STW episode might be either job loss (potentially followed by an episode of UE) or continued employment at the same firm. Existing studies find evidence for both positive and negative individual employment effects, depending on the context. For example, Tilly and Niedermayer (2016) compare workers entering STW during

the financial crisis of 2008-2009 to workers who transition into UE and find that short-time workers largely return to full-time employment with their current employer and do not experience long-run earnings or employment losses, whereas laid-off workers experience persistent income declines. While these results highlight the potential protective role of STW, the comparison with laid-off workers may overstate the program's benefits, since not all short-time workers would necessarily have been laid off in the absence of STW. Other studies construct counterfactuals using workers from matched firms that resemble firms that have used STW but did not themselves use STW. Pavlopoulos and Chkalova (2022) for example, analyze the use of STW in the Netherlands also during the financial crisis of 2008-2009 and find that the program reduced the probability of job separation, particularly in firms that reduced only a few hours for many workers. Similarly, Giupponi and Landais (2023) compare Italian short-time workers to workers in firms that are just at the cut-off of not being eligible for STW and show that employment probabilities initially evolve similarly across groups, but diverge over time due to differences in firm productivity. Workers from firms not eligible for STW ultimately experience higher earnings and working hours in the longer run. They further benchmark their findings against workers experiencing sudden UE, finding that the negative effects of STW are much more moderate. Closely related to our study is the work by Vogtenhuber et al. (2024), who compare workers participating in short-time work in Austria to similar workers who remained employed without entering STW. They find persistent post-program earnings losses that increase with the duration and recurrence of STW exposure and are particularly pronounced for white-collar workers and for men in blue-collar jobs with long STW spells.[2]

To identify the causal effect of STW, we employ a two-stage empirical approach that combines a fixed-effects event study with propensity score matching. In the first step, we match treated individuals with control individuals of the same age and with similar demographic characteristics and employment histories. In the second step, we estimate an event-study model with individual fixed effects to estimate the dynamic treatment effects of STW. To

[2]Two studies estimate the effects of STW by exploiting variations in eligibility rules by age, in the duration and generosity of STW benefits to identify causal effects of STW on workers' outcomes (Arranz et al. (2018); Brinkmann et al. (2024)). Arranz et al. (2018) for instance, study a reform that increased the generosity of the Spanish STW program during the Great Recession. They compare trajectories of short-time workers and non-short-time workers in a differences-in-differences setting, before and after the reform. They find that increasing the generosity of STW had little impact on employment in the short run but may have slightly negative effects in the medium run, potentially because more generous programs delay workers' reallocation toward more productive firms. Brinkmann et al. (2024) analyse a reform in Germany, extending potential STW duration from 6 to twelve months, finding differential employment effects but stronger wage growth in establishments that could use STW for longer periods.

benchmark these estimates of STW effects, we employ the same two-step approach to estimate the causal effect of an episode of UE following an involuntary job loss due to an employer dismissal or a plant closure. Since workers experiencing STW episodes differ substantially in observed characteristics from those experiencing UE episodes, the comparison of our estimated effects for STW and UE will be interpreted only descriptively.

Our estimation results indicate small but statistically significant negative effects of STW on the probability of employment in the years after the event. An STW episode reduces the probability of employment by 3 percentage points below baseline in the second year, and this probability gradually declines to roughly 8 percentage points below baseline in the fifth year after the event. Accordingly, we find small but statistically significant positive effects on the probability of receiving unemployment benefits several years after an STW episode. Conditional on employment, however, we do not find any negative effects on hourly wages or working hours.

These results are in sharp contrast to the estimated treatment effects of UE episodes. In line with the existing literature, we find that UE produces large instantaneous negative labor-market effects with persistent long-run consequences. Relative to baseline employment in the year prior to the event, an episode of unemployment reduces the probability of formal employment by roughly 40 percent in the event year and by about 15 percent even five years later. In contrast to STW, we find strong negative effects on wages and weekly working hours (conditional on employment) caused by episodes of UE.

We also find differences in the results regarding the effects on unpaid care work and life satisfaction. While STW causes a moderate increase in unpaid care work and a moderate decrease in life satisfaction, which recovers within the second year after the event, UE has stronger impacts on both outcomes. UE episodes lead to stronger and more persistent increases in hours spent on unpaid care work, and the detrimental effects on life satisfaction are more pronounced and persist for three periods after the event.

For both, UE and STW, we further investigate heterogeneous effects by gender, as previous literature has shown that UE episodes can affect men and women differently with respect to subsequent labor market outcomes and household behavior (Illing et al. (2021); Ivandić and Lassen (2023)). We find that overall labor market adjustment patterns are broadly similar for men and women. For example, wage losses following UE amount to roughly 11-15 percent for men and 10-15 percent for women relative to their respective baseline

earnings in the years after job loss. However, more pronounced gender differences emerge in the effects of unpaid care work. Following UE, housework increases substantially for both men and women, but the proportional response is much larger for men. For men, housework increases by about 56 percent relative to baseline in the first year after UE, whereas for women, the increase is roughly 19 percent. Time spent on childcare also differs by gender: fathers increase their time spent on childcare by roughly 44 percent relative to baseline in the first year after UE, whereas mothers show no systematic adjustment. For STW, adjustments in unpaid care work remain considerably smaller and do not differ by gender.

When analyzing heterogeneity across economic crises, we find that the moderate negative employment effects of STW are primarily driven by individuals who experienced STW during the reunification period (1989-2005). In contrast, when looking at the financial crisis and the COVID-19 period separately, the effects are generally close to zero. We show that the stronger negative employment and well-being effects of STW observed during the reunification period are driven primarily by workers in East Germany, consistent with the large-scale structural transformation of the labor market following German reunification. Descriptive evidence suggests that these differences across time periods are not attributable to variations in STW duration, measured as the number of weeks spent in STW. STW durations were actually longer in more recent crisis periods than during the reunification years. Instead, the results point toward differences in the composition of workers entering STW. Workers experiencing STW during the reunification period appear to be a more negatively selected group than those affected in later crises. Moreover, the reunification period likely represents a much more persistent restructuring shock, particularly in East Germany, whereas the financial crisis and the COVID-19 pandemic were potentially more temporary disruptions to the labor market. In such contexts, STW may have functioned more effectively as a stabilization policy, preserving employment relationships.

Based on our findings, we conclude that, in the German institutional context, STW had no significant negative effects on workers' labor market outcomes in the last two major economic crises, i.e. the financial crisis of 2008/2009 and the economic crisis caused by the COVID-19 pandemic. This suggests that STW did not, or at least not to a large extent, deter workers from switching to more productive firms. Therefore, STW can be considered successful in stabilizing the employment trajectories of affected workers. This conclusion

is particularly relevant when comparing the effects of STW with those of UE, which has persistent negative effects on labor market outcomes and life satisfaction for those impacted. To the extent that STW has prevented workers from experiencing UE, it can be regarded as an effective tool for avoiding the long-lasting negative labor market consequences that often follow job loss at leaest in the German context during the financial crisis of 2008-2009 and the recent COVID-19 pandemic.

The remainder of the paper is organised as follows. Section 2 describes the institutional background of STW. Section 3 presents the data sources and measurement of STW in our paper's context. Section 4 presents descriptives and our empirical strategy. Section 5 presents the main results for the full sample, while Sections 6 and 7 present the analysis on heterogeneities by gender and time period. Section 8 assess the robustness of our results and finally Sections 9 and 10 discuss mechanisms, implications, and avenues for future research.

2 Institutional background and theoretical considerations

The German STW scheme, known as *Kurzarbeit*, is a subsidy for reductions in working hours due to temporary shocks affecting firms. The scheme aims to prevent layoffs by allowing employers to reduce staff working hours during temporary economic downturns. Employees may have a share of their working hours reduced or be temporarily released from work (*"Kurzarbeit Null"*). Workers affected by STW receive 60% of their lost net wages (67% for those with at least one child).[3] Eligibility criteria for a firm to qualify for STW subsidies include a minimum ten percent reduction in the workforce and a corresponding wage loss of more than ten percent per affected employee. The statutory maximum duration for receiving short-time work benefits is twelve months. The Federal Ministry of Labor and Social Affairs (BMAS) may extend this period to up to 24 months by means of a legal ordinance.

Employers initiate STW by applying to the Federal Employment Agency. Some firms supplement the payment according to their collective agreements. The scheme operates through employer advance payments. Employers are then retrospectively reimbursed by

[3]Note that during the COVID-19 pandemic, a special regulation was introduced. In the early phase of the pandemic, and extended until June 30, 2022, an increased level of STW benefits was paid. From the fourth month of receipt onward, the benefit rate increased to 70 percent of net earnings (77 percent for individuals with children). From the seventh month onward, it amounted to 80 percent (87 percent for individuals with children), provided that the loss of working hours exceeded 50 percent in the respective calendar month.

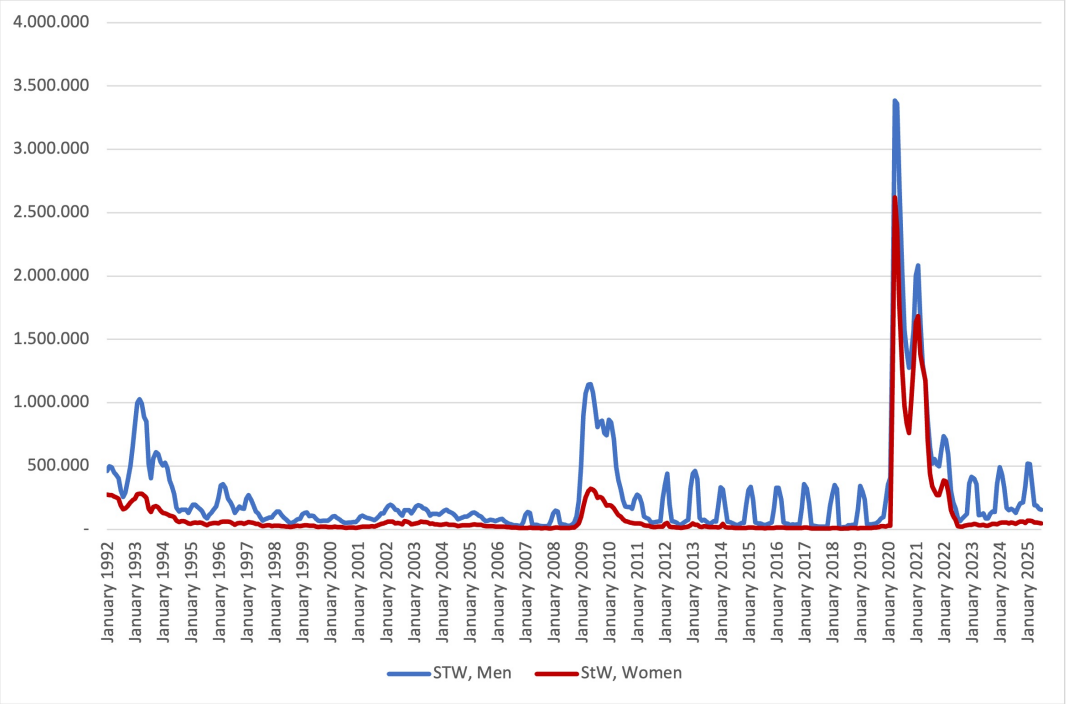
the Federal Employment Agency for their advanced STW payments and associated social security contributions. Employees receive payments through their employer and do not have any direct contact with employment agency offices (in contrast to individuals claiming UE benefits).

Since STW temporarily reduces an individual's working hours, sometimes even to zero, human capital accumulation can be affected, potentially with negative consequences for labor market trajectories. We thus hypothesize that STW negatively affects workers' subsequent labor market outcomes, albeit to a lesser extent than for workers affected by UE, since STW allows to keep their work contract and, thereby, firm-specific human capital. Another channel through which STW could negatively affect workers' labor market trajectories is a potential lock-in effect that prevents workers from switching to more productive firms. The magnitude of this potential effect likely depends on the nature of the underlying economic shock. When shocks are perceived as temporary and labor-market outside options remain relatively stable, preserving existing employment relationships through STW may be an efficient adjustment margin. In such settings, STW might help workers avoid costly job separations and smooth short-run fluctuations in labor demand. By contrast, when shocks reflect more persistent structural changes that permanently alter workers' outside options, maintaining existing job matches may be less beneficial. In these cases, the reallocation of workers across firms or sectors may become more important, and the employment-stabilizing role of STW may therefore be more limited.

Regarding unpaid care work, we hypothesize that time spent on these activities increases during STW. Whether there are long-term consequences in the time allocated to these activities is not clear a priori and likely depends on household context. Since the reduction of working hours or the full absence from work for workers affected by STW is typically shorter than for workers affected by UE, we expect effects on unpaid care work to be less pronounced in the case of STW than in the case of UE.

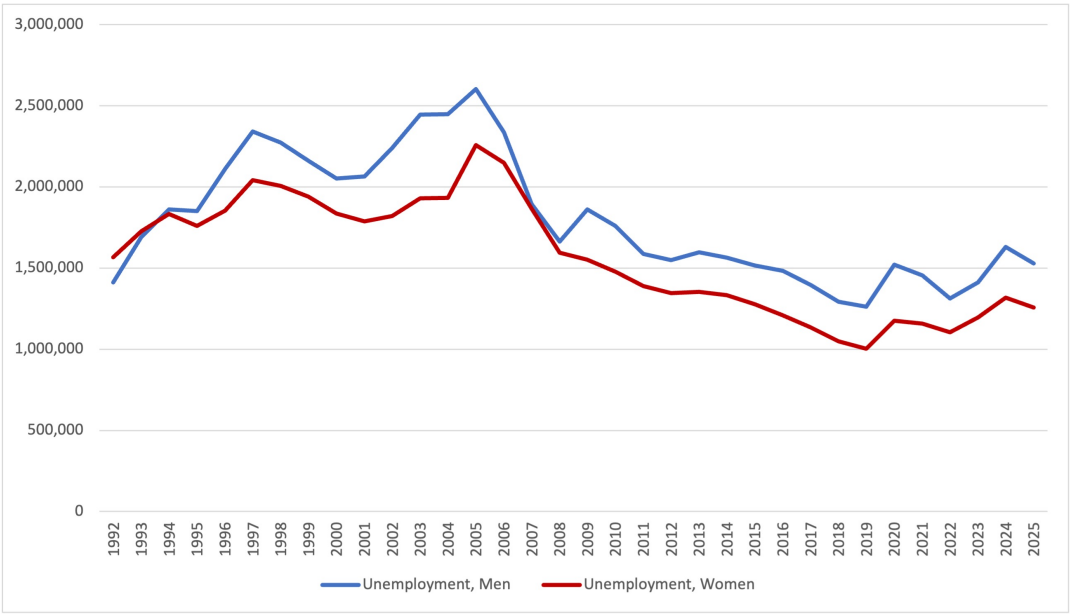
Because STW is explicitly designed to maintain job attachment while avoiding costly and time-intensive job search, we hypothesize that its effects on well-being, measured by life satisfaction, are less pronounced than the effects of UE. Moreover, STW might be particularly effective in preventing persistent labor-market detachment among groups facing higher search frictions. We therefore expect STW to generate smaller gender differentials in subsequent employment trajectories and well-being.

Figure 1: Number of men and women in STW per month in Germany, 1992–2023



Notes. Own display based on STW statistics provided by the Federal Employment Agency (Statistik der Bundesagentur für Arbeit), *Kurzarbeit*, Nuremberg, November 2025.

Figure 2: Number of men and women in UE per year, 1992–2023



Notes. Own display based on statistics from the Federal Statistical Office (2024).

Data from the Federal Employment Agency shows that STW is used predominantly in times of crisis in Germany. Figures 1 and 2 display that periods of economic stability are characterized by comparatively low take-up, whereas major macroeconomic shocks are associated with sharp but temporary spikes. The pronounced spikes in Figure 1 reflect distinct economic contexts: the early 1990s peak captures the adjustment period following reunification and the corresponding recession in 1993-1994; the increase in 2009–2010 coincides with the global financial crisis; and the strong surge in 2020–2021 reflects pandemic-related shutdowns and demand disruptions.[4]

From March 2020 to March 2021, an average of 3.6 million employees were on STW per month, corresponding to an employment equivalent of 1.6 million persons. Compared to previous crises, such as the financial crisis in 2009, the COVID-19 pandemic led to an unprecedented short-term increase in STW. While the financial crisis and the early 90's mainly affected export-oriented manufacturing, the pandemic shock extended to contact-intensive service sectors. As a result, a substantially higher number of women were affected by STW than in earlier downturns (see e.g. Alon et al. (2020); Hammerschmid et al. (2020)).

3 Data

For our main analysis, we use the German Socio Economic Panel (SOEP), an annual longitudinal household survey that has provided information on individuals representative of the population living in private households in Germany since 1984 (Goebel et al. (2019)). The survey currently covers approximately 30,000 individuals per year. Unlike UE spells, STW at the level of the individual is not systematically identifiable in German administrative labor market data and has only been recorded at the firm level since 2009.[5] By contrast, experiences of STW at the level of the individual have been measured in the SOEP for nearly four decades.

Combined with the longitudinal structure of the SOEP, this enables the analysis of subsequent labor market trajectories as well as outcomes outside the labor market, such as well

[4]Note again that policy adjustments during the pandemic included facilitated access to STW and extended benefit duration which further amplified take-up (see e.g. Hammerschmid et al. (2020)).

[5]In a manual digitization effort, the Federal Employment Agency identified individuals in firms that have made use of STW between March 2020 and December 2021 and estimated the probability that they participated in STW during this period. This enabled the analysis of STW exposure at the individual level for these two years (Brinkmann et al. (2024)).

being and unpaid care work, following employment shocks. The panel dimension further allows us to examine heterogeneity by time period, thereby extending the analysis beyond what is observable in administrative labor market records in Germany to date.

3.1 UE and STW measures in the SOEP

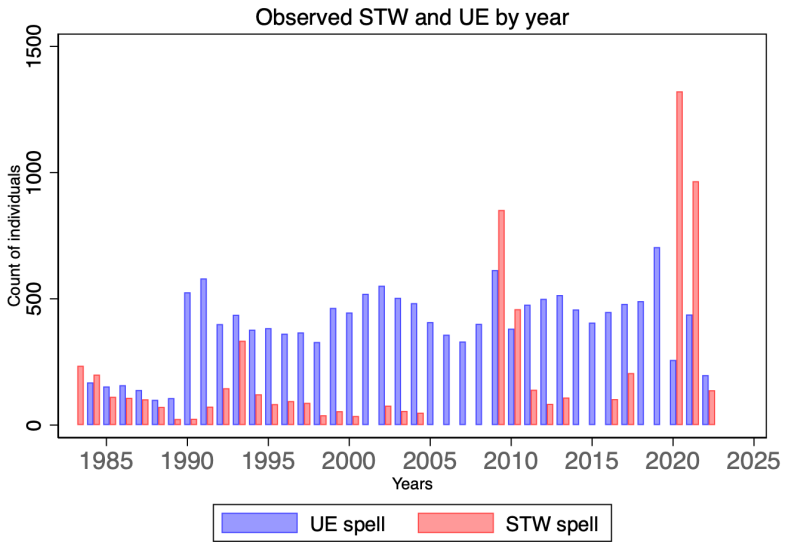
In the SOEP, STW exposure is measured retrospectively through a question asking whether respondents received STW benefits in the previous year. This question was included annually between 1984 and 2001 and subsequently reintroduced in several later waves (2003-2005, 2010-2011, and 2021-2023), corresponding to periods of major economic downturns that saw spikes in the use of STW schemes, including the financial crisis and the COVID-19 pandemic (see Section 2). In addition, selected waves (2010-2014 and 2017-2018) contain a retrospective calendar covering each month of the preceding year, in which respondents could indicate for every month whether they were in STW. This allows us to reconstruct annual STW exposure for nearly all years since 1984, with the exception of a small number of gaps (2001; 2005-2008; 2014-2015; and 2018-2019) for which no STW information was collected.

The SOEP further provides comprehensive information on employment histories, and labor market status is recorded at each survey wave. Since 1985, respondents report whether an employment spell ended in the previous year and, if so, the reason for termination. They select the reason from several categories, including the company closing down, termination by employer, own resignation, old age pension, or the end of a fixed term contract. While plant closures alone provide the most clearly exogenous source of separation, relying exclusively on this category would result in a substantially smaller sample. Following previous research analysing job loss with the SOEP data (see for example Everding and Marcus (2020); Hennecke and Pape (2022); Marcus (2013, 2014)), we consider individuals treated for involuntary UE when they report either a plant closure or an employer dismissal as the reason for job termination in the previous year.[6]

Figure 3 displays all involuntary UE and STW episodes identifiable in the SOEP as described above. Despite the limited survey sample size compared to the admin data, the timing of observed STW cases closely mirrors the patterns documented in administrative data (see Figure 1). Figure 4 further shows the number of observations over time, sepa-

[6]Note that our conclusions are unchanged, when restricting the sample to indiviuals who lost their job due to a plant closure only.

Figure 3: UE and STW in the SOEP, 1983-2022, individuals aged 18-65 years



Source. Own calculations based on SOEP core, v40.
Notes. Bars display the number of distinct treated individuals per survey year. Information on STW and UE is collected retrospectively for the previous year, therefore, the latest available observations refer to the experience of the event in 2022, measured in 2023. The STW question was introduced in 1984, providing information from 1983 onward. The question on reasons for job termination was introduced in 1985, providing information from 1984 onward.

rately for UE in the left panel and STW in the right panel, each broken down by gender and by region (East/West Germany). The gender distribution in the SOEP mirrors trends observed in the administrative data: women are disproportionately more affected by STW in the most recent period, namely during the COVID-19 pandemic, compared to earlier years. Regional patterns are also consistent with administrative evidence showing that STW already played a particularly important role during the reunification period in Germany and was therefore relatively more pronounced in East Germany at that time, compared to more recent periods (Institut Arbeit und Qualifikation (IAQ) (2025)).[7]

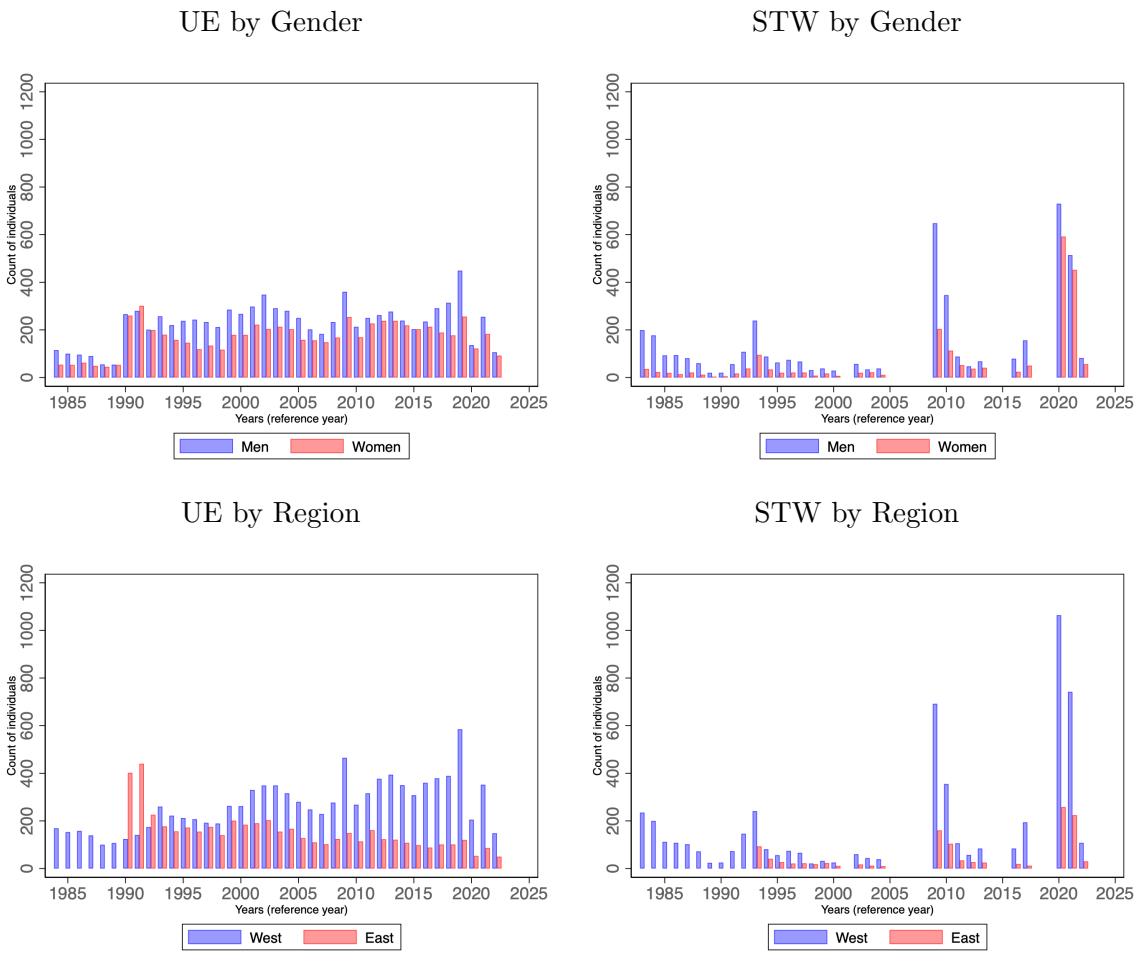
Taken together, these measures allow us to identify episodes of STW and involuntary UE over a long observation window.

3.2 Outcome variables

The SOEP provides information on labor market outcomes, unpaid care work, life satisfaction and detailed socio-economic characteristics. As our main outcomes of interest, we

[7]Note that we only observe people in East Germany starting in 1990 and we observe the first individuals experiencing STW in the East by 1993.

Figure 4: Treated Individuals: UE and STW, 1983 to 2022, Ages 18–65



Source. Own calculations based on SOEP core, v40.
Notes. Bars display the number of distinct treated individuals per survey year. Panels in the first row separate by gender; panels in the second row separate by region (East and West Germany). Note that we only observe people in East Germany starting in 1990 and we observe the first individuals experiencing STW in the East by 1993.

include the employment probability, the probability to receive unemployment benefits, gross hourly wages[8], and reported weekly working hours (conditional on employment). Additionally, and unique to the survey context, the dataset includes measures of unpaid care work, such as the number of hours a person reports spending on housework tasks and childcare on a normal weekday. This information is available for every year since 1984. We further analyze how episodes of involuntary UE and STW relate to individuals' well-being, given that workers remain employed and only experience a temporary cut in working hours. To capture subjective well-being, we make use of the measure of life satisfaction. This outcome has been collected since 1984 and is measured by a 0–10 Likert scale, where 0 indicates being completely dissatisfied and 10 indicates being completely satisfied.

4 Descriptive Statistics and Estimation Strategy

Our study aims to identify the causal effect of a period of involuntary STW or UE on workers' career outcomes, their time spent on unpaid care work, and well-being. For the analysis of UE, we focus on employer-initiated job losses, i.e., plant closures and dismissals by the employer. Our observation window spans the period from the mid-1980s to 2023. The data already covers the years prior to German reunification, which can be considered the first major external shock in our sample period. Two further large macroeconomic shocks, the financial crisis in 2008 and 2009, and the COVID-19 pandemic in 2020 and 2021, substantially increased the incidence of both UE and STW. In these periods, job loss and STW are arguably less endogenous than voluntary separations. Nevertheless, both involuntary UE and STW experiences still likely correlate with individual and firm characteristics that also affect subsequent outcomes. To address this concern, we impose sample restrictions and implement a two-step procedure that combines propensity score nearest-neighbor matching with a fixed-effects event-study design.

4.1 Sample Restrictions and Matching

Sample Restrictions for Treatment and Control groups: For this first step, we rely on selecting an appropriate pool of treatment and potential control group individuals through sample restrictions, followed by propensity score matching. Following De Chaisemartin and d'Haultfoeuille (2020), control individuals are those who do not ever experience

[8]Real gross hourly wages are calculated based on contractual work hours and current gross labor income in Euros. They are trimmed at the 1st and 99th percentile.

the respective employment shock, while observed in our sample.[9] For each treatment definition, individuals are considered treated between the year prior to the shock ($K = 0$) and the year in which they report having experienced STW or involuntary UE within the past year ($K = 1$). We restrict the treated sample to individuals who were employed in the year prior to the STW or UE spell, i.e. in $K = 0$. In cases where an individual experiences multiple shocks of the same type, we keep only the first observed UE or STW episode.

For every treated individual, we define a pool of potential control individuals consisting of individuals observed in the same calendar year, of the same age, and employed at time $K = 0$. For the pool of these potential control individuals, we assign that year as the pseudo-treatment year. Control individuals can therefore contribute multiple person $\times$ year observations if they qualify as potential matches for several treated individuals in different years. We retain all such person $\times$ year observations and subsequently construct an event-time structure for both treated and potential control individuals over the potential observation window $K \in [-5, 5]$ around the (pseudo-) event year. Panel attrition implies that the event-time panel is unbalanced, with some individuals observed throughout the entire $[-5, 5]$ window, whereas others are observed for only a subset of periods.

Matching procedure: With this pool of potential control individuals, we now perform one-to-one nearest-neighbor matching without replacement and impose a caliper of 0.01.[10] Matching is implemented separately for involuntary UE and STW. The resulting matched sample serves as the basis for the subsequent fixed-effects event-study analysis.

To compute the propensity score, we estimate a logistic regression at $K = 0$ in which the dependent variable indicates whether an individual experiences the respective employment shock. The propensity score estimation includes detailed pre-treatment labor and personal characteristics. Specifically, we include the highest education status (binary indicator on having a university degree), direct migration background, East/West residence, industry (11 groups)[11], firm size, marital status (binary indicator), children (binary indicator)[12],

[9] Note that individuals in the STW control group may experience involuntary UE at some point, and vice versa.

[10] The caliper requires that the absolute difference in estimated propensity scores between treated and control individuals does not exceed 0.01. Treated observations for which no control falls within this range are excluded from the matched sample.

[11] Industry is constructed from the two-digit NACE Rev.2 classification by grouping divisions into 11 broader categories. Observations with missing industry information are assigned to category 10, which therefore captures missing values rather than a respective industry group. As shown in Table 2, the distribution of industry categories is well balanced between treated and matched control individuals.

[12] This variable indicates whether at least one child aged 13 or younger resides in the household.

gender, age and calendar year, measured in $K = 0$. To closely capture pre-treatment labor market attachment, we compute averages of key employment variables over the five-year window $K \in [-5, 0]$. These include wages, tenure in the company, employment status, UE registration, and job changes (binary). For example, a person observed for five years prior, who was employed in only one of those years, will be matched based on that average. Similarly, a person observed for four years will have the average employment value calculated over the four years. In our preferred specification, we further include the exact time-varying information of employment and care work measures, namely employment-, and UE registration status, wage, weekly working hours, tenure, as well as reported hours spent on a weekday on housework and childcare in each of the three years prior to $K = 0$.

This three-year matching design mechanically implies a balanced sample in $K \in [-3, 0]$. Only individuals observed in each of these three years are eligible for matching, as we account for their specific labor market and care work information in these years. As a consequence, Table 1 shows identical numbers of observations for these event times within each treatment category. Outside the three-year window, the panel becomes unbalanced. The number of observations declines in these periods, because individuals are not necessarily observed for five full years before or after the event due to survey attrition or temporary non-response. Our final sample includes 3,628 matched individuals experiencing an involuntary UE spell and 1,861 individuals experiencing an episode of STW.

Table 1: Number of Observations by Group and Event Time (3-Year Matching)

K	**Control (UE)**	**Treatment (UE)**	**Control (STW)**	**Treatment (STW)**
-5	2,636	2,545	1,396	1,396
-4	3,064	2,986	1,506	1,483
-3	3,628	3,628	1,861	1,861
-2	3,628	3,628	1,861	1,861
-1	3,628	3,628	1,861	1,861
0	3,628	3,628	1,861	1,861
1	3,176	3,610	1,483	1,790
2	2,867	3,255	1,337	1,538
3	2,543	2,970	1,127	1,314
4	2,256	2,686	891	834
5	1,980	2,427	833	779

Source. Own calculations based on the SOEP core, v40.

Our preferred specification therefore covers the period from 1985 to 2023 and includes the first observed involuntary UE or STW spell for men and women for whom a suitable match

in the control group is found between 1988 and 1989 (i.e., with $K = 0$ in 1988), which is the first time that sufficient pre-event information is available.[13] Figure A.1 reports the number of treated STW and UE observations over time in the matched sample. Compared to Figure 3, the number of STW observations in 2010 and 2021 is smaller in our final sample. This is because some individuals experience STW more than once, and only the first episode is kept in our sample. In addition, the final sample is smaller as the matching procedure excludes some individuals for whom no suitable control observation can be found among the pool of potential controls.[14]

Alternative samples: We further conduct robustness checks based on two alternative matching windows on the time-varying employment and care variables. First, we construct a larger but less balanced sample in which matching is based on exact time-varying employment and care variables from only one, rather than three years prior to $K = 0$ (see Table E.5). Second, we construct a smaller, more restrictive sample using five years of these pre-treatment time-varying observables (see Table D.3). The three-year window represents a compromise between sample size and pre-treatment comparability and therefore constitutes our preferred specification.

In an additional robustness exercise, we use the novel linked SOEP-CMI-ADIAB data, which combines survey information from the SOEP with administrative employment biographies from the Federal Employment Agency (Antoni et al. (2023)). This allows us to account for firm fixed effects. Treatment status is identified in the survey data, as it is not observable in the admin data for STW. For individuals who have consented to linkage since 2019, we observe complete administrative employment histories. Note that, therefore, the sample size is much smaller in this analysis, as it is restricted to individuals having consented to linkage since 2019, but we use only admin variables for the matching and hence,

[13] Note that as a first sample restriction, we drop individuals who were not employed in the year prior to the treatment event. This implies that the first survey year (1984) is excluded, as no information on the previous year is available. In addition, the preferred specification requires observing individuals' employment and care-work information for up to four years prior to the event to enable matching. The first potential treatment observations, therefore, correspond to individuals experiencing STW or UE between 1988 and 1989 (i.e., with $K = 0$ in 1988). East German workers entered the SOEP only in 1990. Consequently, the first UE and STW episodes we can consider for our estimation are those who experience either of the two employment shocks between 1993 and 1994 (i.e., with $K = 0$ in 1993), provided sufficient pre-event information is available.

[14] Note that in this preferred specification, 3,791 out of 7,419 unemployed individuals who were employed in $K = 0$ are excluded (about 51%) due to matching, as well as 896 out of 2,757 individuals in STW who were employed in $K = 0$ (about 32.5%). We also include a robustness check matching only on information from one year prior to $K = 0$, in which only one unemployed individual and no individuals in STW are excluded.

the sample is perfectly balanced in the 5 years before the event (see Table F.7). These administrative employment records enable us to track individuals in the administrative data until the end of 2023, so pre- and post-treatment employment trajectories are based on administrative records rather than self-reported survey information.

4.2 Descriptive Statistics

Table 2 reports descriptive statistics for treated individuals (where treatment is either STW or UE) and their respective matched control groups in the year prior to treatment, $K = 0$, for our main sample based on the three-year pre-treatment matching specification described in the previous section. The table confirms that the propensity score matching yields close balance between treated and control individuals across all observable characteristics included in the matching procedure. Demographic characteristics, labor market variables, and pre-treatment employment averages are nearly identical within treatment categories, supporting the validity of the matching design and ensuring comparability prior to the event. Importantly, subjective well being is not included in the matching procedure. Life satisfaction appears to be already significantly lower in $K = 0$ for individuals who subsequently experience UE or STW relative to their matched controls.

As Table 2 shows, treatment and control groups appear overall well balanced. However, clear structural differences emerge between the UE and STW samples. Individuals who are about to experience STW have substantially higher pre-treatment wages, longer tenure, and higher weekly working hours, and are more likely to be employed in larger firms than individuals who experience UE. In contrast, individuals facing UE display lower wages and shorter tenure on average, consistent with weaker pre-event labor market attachment. As expected, the share of women is almost at 50% for those who are experiencing involuntary UE, but just below 40% for the sample of individuals experiencing STW. STW individuals, in line with working longer hours, also spend less time on unpaid care work, namely housework (including washing, cooking, and cleaning) and child care.

Figures 5 to 7 provide a first descriptive overview of dynamic developments around the (pseudo-)event for selected outcomes. Figure 5 displays labor market outcomes. It shows that for individuals experiencing UE, employment rates, which are mechanically set to one in $K = 0$ due to our sample restriction, drop sharply in the first post-event year ($K = 1$) and recover only gradually thereafter. Accordingly, the reception of unemployment benefits

Table 2: Matched Control and Treatment Group Characteristics after Propensity Score Matching, measured in $K = 0$

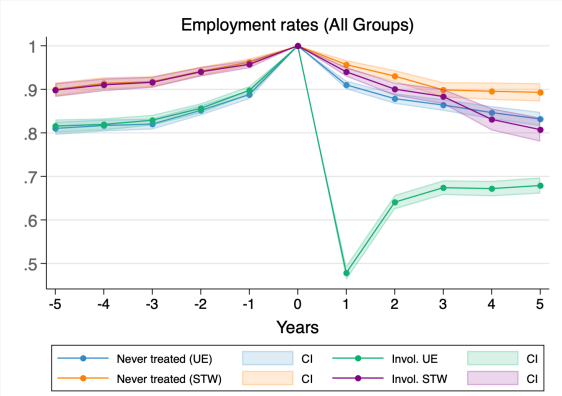
	UE			STW		
	Control	Treatment	p-value	Control	Treatment	p-value
	Mean (SD)	Mean (SD)		Mean (SD)	Mean (SD)	
Demographic characteristics						
Female	0.49 (0.50)	0.49 (0.50)	0.760	0.38 (0.49)	0.39 (0.49)	0.840
Age	41.13 (11.39)	41.13 (11.62)	0.989	42.40 (10.46)	42.60 (11.00)	0.567
Children	0.41 (0.49)	0.40 (0.49)	0.293	0.41 (0.49)	0.40 (0.49)	0.594
Married	0.61 (0.49)	0.62 (0.49)	0.847	0.66 (0.47)	0.65 (0.48)	0.557
Highest Education (CASMIN)	0.15 (0.36)	0.15 (0.36)	0.671	0.18 (0.38)	0.18 (0.39)	0.639
Migration Background	1.30 (0.56)	1.28 (0.55)	0.395	1.30 (0.58)	1.32 (0.54)	0.503
East Germany (Yes/No)	0.28 (0.45)	0.28 (0.45)	1.000	0.23 (0.42)	0.24 (0.43)	0.278
Mean Survey Year	2004.69 (9.56)	2004.51 (8.90)	0.400	2009.65 (10.65)	2009.93 (10.99)	0.418
Labor market information in $K = 0$						
Real Hourly Wage	10.70 (7.67)	10.65 (7.33)	0.801	15.78 (9.47)	15.52 (8.91)	0.387
Weekly Work Hours	30.60 (14.50)	30.66 (13.89)	0.850	35.09 (10.33)	34.91 (9.05)	0.566
Tenure	8.40 (9.04)	8.56 (9.50)	0.471	11.22 (9.90)	11.31 (10.06)	0.785
Industry (11 groups)	8.82 (2.38)	8.81 (2.43)	0.942	7.59 (3.08)	7.52 (3.19)	0.494
Firm Size	5.96 (3.34)	6.00 (3.14)	0.610	7.48 (2.78)	7.44 (2.72)	0.625
Unpaid care work						
Housework Hours per Weekday	1.44 (1.37)	1.43 (1.41)	0.604	1.22 (1.12)	1.22 (1.11)	0.953
Childcare Hours per Weekday	1.26 (2.88)	1.26 (2.82)	0.918	1.09 (2.29)	1.18 (2.62)	0.263
Well-being						
Life Satisfaction (current)	7.09 (1.68)	6.74 (1.77)	< 0.001	7.30 (1.62)	7.13 (1.61)	0.002
Employment Averaged Over Available Years in the Prior Five-Year Window						
Pre-Wage Avg	8.73 (5.94)	8.64 (6.34)	0.540	13.69 (7.71)	13.35 (7.92)	0.187
Pre-Tenure Avg	7.12 (7.08)	7.29 (8.33)	0.339	9.44 (8.02)	9.47 (8.90)	0.902
Pre-Work Hours Avg	26.78 (12.55)	26.85 (13.16)	0.816	32.97 (9.57)	32.52 (9.61)	0.150
Pre-Employment Status Avg	0.87 (0.19)	0.87 (0.22)	0.792	0.94 (0.12)	0.94 (0.15)	0.418
Pre-Registered UE Avg	0.05 (0.12)	0.04 (0.12)	0.517	0.02 (0.07)	0.02 (0.08)	0.772
Observations	3,628	3,628		1,861	1,861	

Source. Own calculations based on the SOEP core, v40.

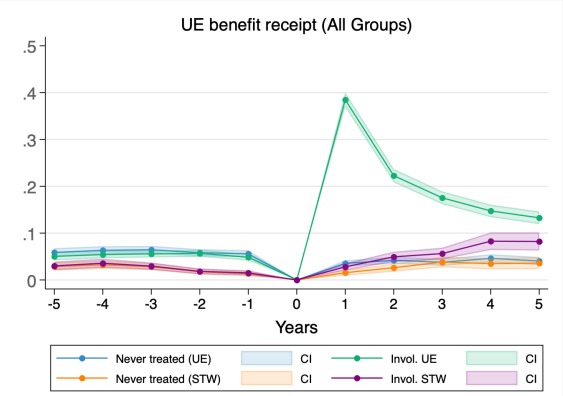
Notes. The table reports means and standard deviations (in parentheses) for treated and matched control individuals measured in $K = 0$, the year prior to the employment shock. Matching is performed separately for involuntary UE and STW using one-to-one nearest-neighbor propensity score matching without replacement and a caliper of 0.01. The variables labeled "Pre-" averages refer to averages computed over all available pre-treatment observations within the window $K \in [-5, 0]$. Because individuals are observed for varying lengths of time prior to treatment, these averages are calculated over the available pre-treatment years and therefore do not necessarily represent full five-year averages for all individuals. The matched sample corresponds to the three-year pre-treatment specification described in Section 4. P-values refer to tests of equality of means between treated and control individuals.

Figure 5: Raw means in control and treatment groups: Labor market outcomes

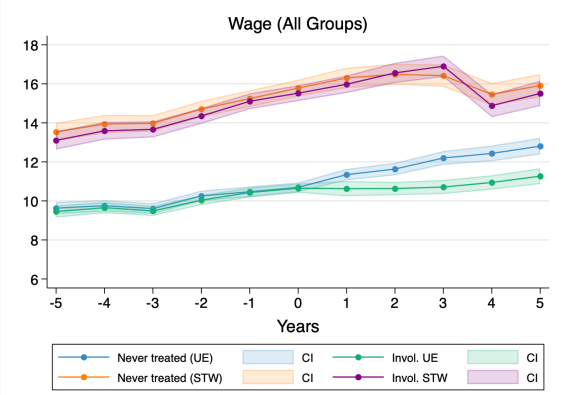
Employment



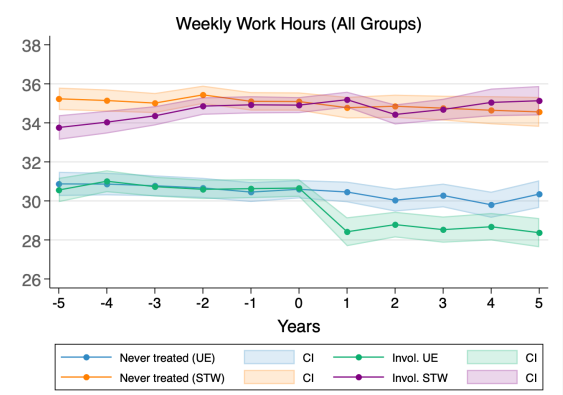
Reception of UE Benefits



Real Hourly Wage (conditional on employment)



Weekly Work Hours (conditional on employment)



Source. Own calculations based on the SOEP core, v40.
Notes. Plots show the raw means over time in our sample for the matched treatment and control individuals in each treatment category. Shaded areas represent 95% confidence intervals.

spikes in the first period after the event and then gradually declines in the subsequent periods. Wages stagnate relative to the control group's wage trajectory, while weekly working hours decline and remain persistently below those of non-treated individuals for several years.

For STW, employment probabilities are consistently higher in the pre-event period than in the UE sample, reflecting stronger labor market attachment among these individuals. Following the STW episode, employment declines only moderately and remains close to pre-treatment levels until $K = 3$. Nevertheless, relative to their matched control group, employment appears to remain on a slight downward trend in the post-treatment period beyond $K = 3$, and the reception of unemployment benefits increases accordingly. Reductions in wages and weekly working hours are comparatively small and largely temporary. In the STW development, wages and, to a lesser extent, hours continue to trend upward up to $K = 3$, followed by a visible change in levels around $K = 4$ and $K = 5$ for both the treatment and control groups. Due to the finite observation window of the data, which ends in 2023, the sample observed at $K = 4$ and $K = 5$ necessarily consists of earlier STW cohorts. These post-periods do not yet include individuals who experienced STW during the COVID-19 crisis, because for these cohorts, four or five years of post-event outcomes are not observed yet. As a result, the composition of the STW sample changes beyond $K = 3$, leading to discrete shifts in raw means for some outcomes even in the absence of treatment effects. To account for potential differences in the effect of STW across time periods, we examine heterogeneity by time period and, for the period around the COVID-19 crisis, focus only on mid-run effects of STW up to $K = 3$.

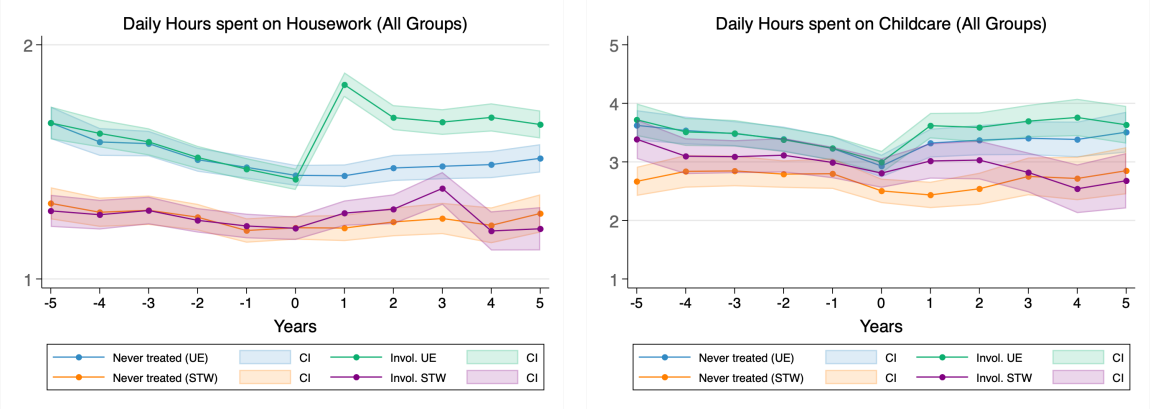
The raw means of time spent on unpaid care work around the (pseudo-)event are shown in Figure 6. Individuals experiencing UE exhibit a noticeable increase in time spent on housework and, to a lesser extent, in hours spent on childcare following the event. For STW, changes in unpaid care work appear comparatively small and short lived. For housework, it is noteworthy that levels increase for treated STW individuals before declining again in $K = 4$. We do not observe a similar pattern for the control group, suggesting that this may reflect a trend specific to individuals who experienced STW during the COVID-19 pandemic.

Before turning to the event-study estimation strategy, it is important to address potential anticipation effects. Table 2 shows that life satisfaction already differs in levels between

treatment and control groups in ($K = 0$). However, Figure 7 indicates that the trajectories of this outcome evolve similarly in control and treatment groups, roughly up to two years prior to treatment.

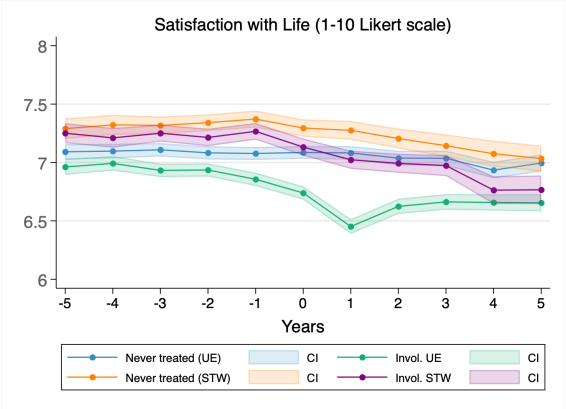
Figure 6: Raw means in control and treatment groups: Unpaid care work

Housework in hours per workday | Childcare in hours per workday



Source. Own calculations based on the SOEP core, v40.
Notes. Plots show the raw means over time for our sample for hours spent on housework. For childcare the sample is restricted to those having children. Shaded areas represent 95% confidence intervals.

Figure 7: Raw means in control and treatment groups: Life satisfaction



Source. Own calculations based on the SOEP core, v40.
Notes. Plots show the raw means over time in our sample. Shaded areas represent 95% confidence intervals.

A key advantage of using survey data to study these shocks is that it allows us to observe such anticipation dynamics, which are typically not detectable in administrative records. Consistent with prior studies using similar survey data that document anticipation effects before job losses (see, e.g., Clark et al. (2008); Kassenboehmer and Haisken-DeNew (2009); Wunder and Zeydanli (2021)), we normalize the event-study coefficients relative to the period two years prior to the labor market shock. This allows us to capture anticipation

effects relative to a period in which treated and control individuals still follow similar trajectories in well-being outcomes.

4.3 Estimation strategy

Having established the matched samples and their pre-treatment balance, we now turn to the second step of our identification strategy. While the propensity score matching ensures comparability between treated and control individuals along observable characteristics prior to the event, involuntary UE and STW may still be correlated with unobserved factors that affect subsequent outcomes. To address this concern, we combine the matched sample with a fixed-effects event-study design.

As described above, we construct a panel of treated and matched control individuals based on the three-year pre-treatment matching specification. The pre-event observations are defined relative to $K = 0$, the year prior to the employment shock, and the event window spans $K \in [-5, 5]$. In the main analysis, we now apply an event study approach and estimate the following specification:

$$y_{itc} = \sum_{k=-5}^{5} \delta_k \times I(t = c + k) \times treat_i + \alpha_i + \alpha_t + X_{it}\beta + \epsilon_{itc} \tag{1}$$

where y_{itc} denotes the outcome of individual i belonging to treatment cohort c in year t. The coefficients δ_k capture the dynamic effects of the employment shock at event time k relative to the omitted baseline period. The coefficient δ of the pre-treatment period $k = 0$ is fixed to zero. α_i and α_t denote individual and year fixed effects. In addition to individual and year fixed effects, we include a set of time-varying controls in X_{it}. Specifically, we control for age polynomials and an indicator for the presence of children under 14 years in the household.[15] For all outcomes, the omitted baseline category corresponds to $K = 0$, that is, the year prior to treatment. As described above, for life-satisfaction, however, we estimate the coefficients relative to $K = -1$, since this well-being measure already diverges between the control and treatment groups in the year before the respective shocks.

The regressions are estimated separately for each treatment definition (involuntary UE and STW), using their respective matched control samples. When studying heterogeneities,

[15] For outcomes related to time spent on childcare, the sample is restricted to individuals with children in the household, and the indicator for children is therefore omitted from the control vector in those regressions.

such as differences by gender or time period, we run the estimations separately for the relevant subsamples. In particular, for the period around the COVID-19 crisis, we focus on mid-run effects only up to $K = 3$, given the limited post-treatment years available in the data.

Our main specification relies on the three-year matching design described above. As robustness checks, we repeat the entire estimation procedure using alternative matching windows based on one-year and five-year pre-treatment time-varying observables. In an additional robustness exercise, we use the linked SOEP-CMI-ADIAB dataset. This allows us to identify individuals in STW in the survey data and link them to administrative employer employee records, conditional on consent to linkage since 2019. The administrative data provide complete employment histories and firm-specific information, enabling us to re-estimate the event-study specification with firm-level controls and measure employment outcomes using only variables available in the administrative data.

In settings with staggered treatment timing, treatment effects should first be estimated separately by cohort (time of treatment) and then aggregated along the event-time dimension to avoid contamination of estimates that can arise in standard two-way fixed-effects models (Sun and Abraham (2021)). To address this critique, as a final robustness check, we therefore implement the estimator proposed by Sun and Abraham (2021), which accounts for treatment-effect heterogeneity in dynamic treatment settings. The main results remain robust to this alternative specification and are reported in Section G.

5 Results

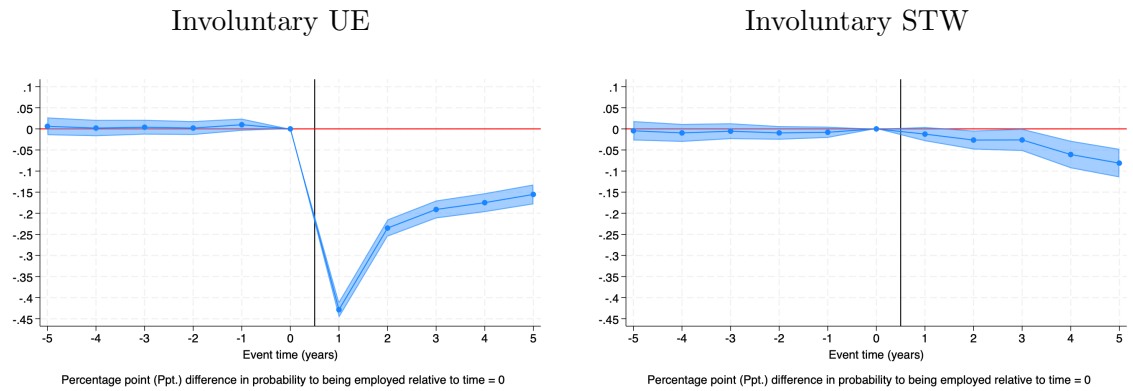
The main results are based on the preferred specification using our sample matched on observables in $K = 0$, including time-varying employment observables three years prior to treatment. The figures plot event-time coefficients relative to $K = 0$, i.e. the year before the respective treatment.

5.1 Labor market outcomes

Figures 8 and 9 report the effects on the probability of being formally employed and on registered UE, respectively. For individuals experiencing involuntary STW, pre-event coefficients are small and statistically insignificant for both outcomes, indicating no differential pre-trends relative to the matched control group. Following the STW episode, the probabil-

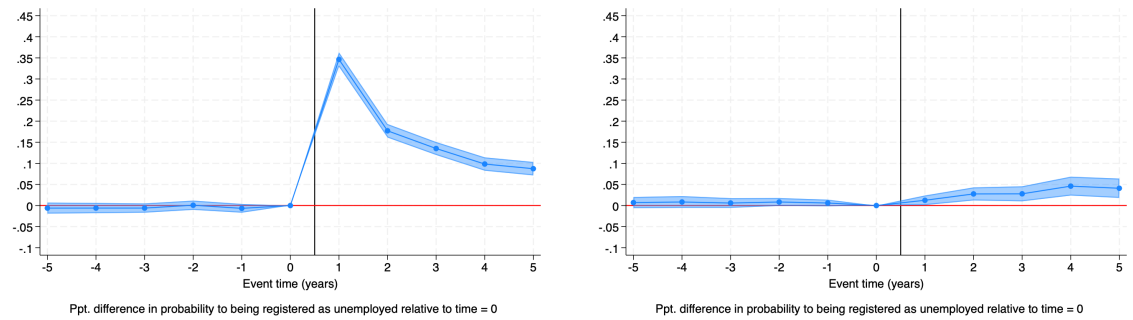
ity of being employed declines gradually. The estimated effect amounts to −2.6 percentage points in year two and is statistically significant at the 5 percent level. The magnitude increases over time, reaching −6.1 percentage points in year four and −8.1 percentage points in year five, both statistically significant at the 1 percent level. At the same time, registered UE increases after STW. The probability of being registered as unemployed (i.e. receiving UE benefits) rises by 1.2 percentage points in the first post-event year, statistically significant at the 5 percent level, by 2.8 percentage points in years two and three, and peaks at 4.6 percentage points in year four before remaining elevated at 4.1 percentage points in year five, statistically significant at the 1 percent level.

Figure 8: Employment



Source. Own calculations based on the SOEP core, v40.
Notes. Event-time coefficients from equation (1). Pre-event observations are based on matching three years prior to the event. Shaded areas represent 95% confidence intervals.

Figure 9: Reception of UE Benefits

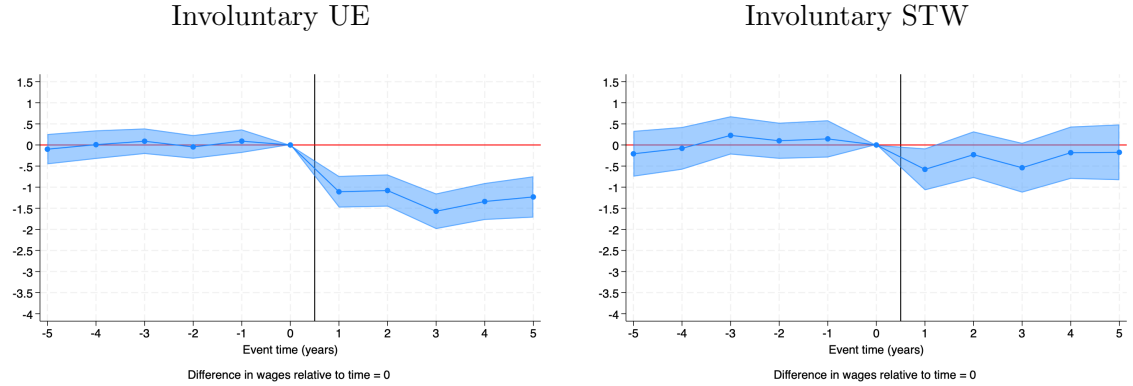


Source. Own calculations based on the SOEP core, v40.
Notes. Event-time coefficients from equation (1). Pre-event observations are based on matching three years prior to the event. Shaded areas represent 95% confidence intervals.

For individuals experiencing involuntary UE pre-event coefficients are likewise small and statistically insignificant. In the event year, registered UE increases sharply by 34.6 percentage points. The effect declines over time but remains substantial at 17.7 percentage points in year two and 8.7 percentage points in year five. Correspondingly, the probability of being formally employed falls by 42.9 percentage points in the event year. Although the magnitude decreases over time, employment remains 23.5 percentage points lower in year two and 15.5 percentage points lower five years after the event.

Figure 10 presents effects on real hourly wages, conditional on employment. For individuals experiencing STW, pre-event coefficients are small and statistically insignificant. In the first post-event year, wages decline by 0.58 Euro per hour, statistically significant at the 5 percent level. Relative to the baseline treatment-group mean wage of 15.52 (Table 2), this corresponds to a reduction of approximately 3.7 percent. Subsequent coefficients are negative but statistically insignificant and do not indicate a permanent negative effect on wage development for individuals experiencing STW relative to similar individuals who do not experience an STW episode. For individuals experiencing involuntary UE, wage losses are substantially larger. In the event year, wages decline by 1.11 Euros, statistically significant at the 1 percent level. Relative to the baseline mean wage of 10.65, this corresponds to a decrease of roughly 10.4 percent. Wage reductions remain sizable over time, amounting to −1.57 Euros in year three (about 14.7 percent), and −1.23 five years after the event (about 11.6 percent).

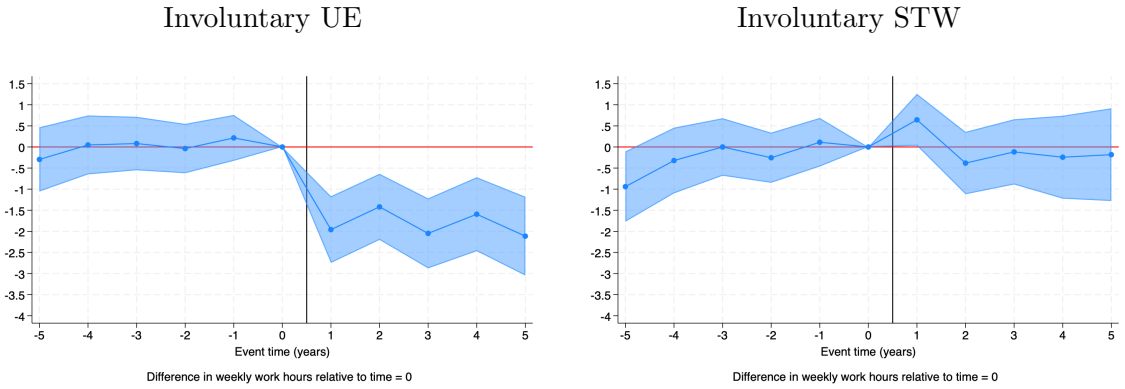
Figure 10: Real Hourly Wage (conditional on employment)



Source. Own calculations based on the SOEP core, v40.
Notes. Event-time coefficients from equation (1). Pre-event observations are based on matching three years prior to the event. Shaded areas represent 95% confidence intervals.

Figure 11 shows the estimated effects on weekly working hours, conditional on employment.

Figure 11: Weekly Working Hours (conditional on employment)



Source. Own calculations based on the SOEP core, v40.
Notes. Event-time coefficients from equation (1). Pre-event observations are based on matching three years prior to the event. Shaded areas represent 95% confidence intervals.

For STW, pre-event coefficients are not statistically different from zero, except for $K = -5$. In the first post-event year, weekly hours increase by 0.64 hours, statistically significant at the 5 percent level. Relative to the baseline treatment-group mean of 34.91 hours, this corresponds to an increase of approximately 1.8 percent. All other post-event coefficients are small in magnitude and statistically insignificant, indicating no persistent change in working hours following an STW episode. For UE, weekly working hours decline sharply in the event year by 1.96 hours, statistically significant at the 1 percent level. Relative to the baseline mean of 30.66 hours, this represents a reduction of approximately 6.4 percent. Reductions remain sizable and statistically significant throughout the post-event window, ranging between -1.42 hours (4.6 percent) and -2.11 hours (6.9 percent) over the five years following the event.

Taken together, the employment results indicate markedly different magnitudes across the two events. Involuntary UE, as has been vastly shown in the literature already, is associated with immediate and large declines in formal employment, sharp increases in registered UE, and persistent reductions in wages and weekly working hours. Although these effects attenuate over time, they remain economically sizable and statistically significant several years after the event. In contrast, involuntary STW is followed by comparatively small negative changes in employment probabilities and a slight increase in registered UE, as well as modest and short-lived adjustments in wages (downward) and working hours (upward). Comparing the effects of STW descriptively to what may be considered a "worst-case" benchmark, i.e. job loss, involuntary UE episodes result in substantially larger and more

persistent negative labor market effects than STW episodes when similar individuals are respectively used as the control group. As the treatment and control groups constitute distinct matched samples, these differences should be interpreted descriptively rather than as causally linked to differences in the policy instruments STW and UE.

5.2 Unpaid care work outcomes

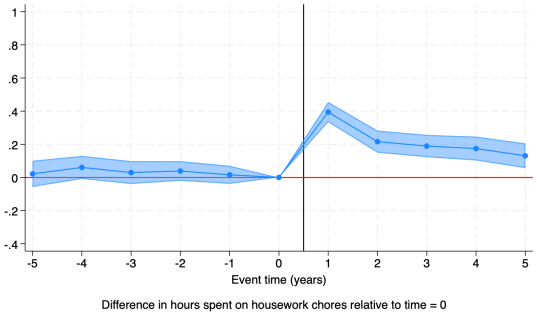
In this section, we examine outcomes outside the labor market, namely the effects of STW and involuntary UE on time spent on unpaid household care work. Figure 12 reports effects on housework hours per weekday. For individuals experiencing involuntary STW, pre-event coefficients are small and statistically insignificant. Following the event, housework hours increase moderately. In the first post-event year, housework rises by 0.08 hours per weekday, statistically significant at the 5 percent level. Relative to the baseline treatment-group mean of 1.22 hours (Table 2), this corresponds to an increase of approximately 6.6 percent. The largest increase occurs in year three, amounting to 0.14 hours per weekday and statistically significant at the 1 percent level, which represents an increase of roughly 11.5 percent relative to baseline. Estimates remain positive in years four and five but are smaller and not consistently statistically significant. For individuals experiencing involuntary UE, the increase in housework is substantially larger. In the event year, housework rises by 0.39 hours per weekday, statistically significant at the 1 percent level. Relative to the baseline treatment-group mean of 1.43 hours, this corresponds to an increase of approximately 27 percent. Although the magnitude declines over time, the increase remains economically sizable and statistically significant throughout the post-event window, amounting to 0.22 hours in year two (about 15 percent) and 0.13 hours five years after the event (about 9 percent).

Figure 13 presents effects on time spent on childcare in hours per weekday for the subsample of individuals having children. For STW, pre-event coefficients are mostly small and statistically insignificant. Post-event estimates fluctuate around zero and are statistically insignificant throughout the event window. This suggests that there is no effect on the amount spent on childcare.[16] For UE, childcare hours increase by 0.28 hours per weekday in the event year, statistically significant at the 5 percent level. In subsequent years, estimates are smaller in magnitude and statistically insignificant. By year five, the coefficient

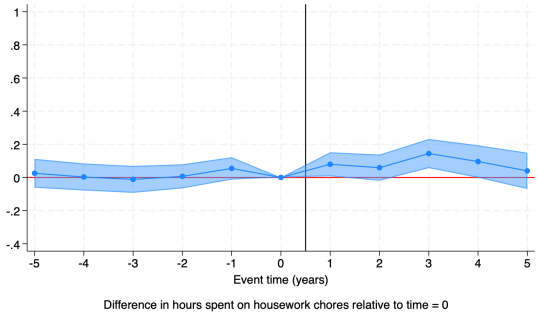
[16]Sample size becomes, of course, smaller for this subgroup, with only about 40 percent (around 744 individuals) of our sample of people experiencing STW having children, as indicated in Table 2.

Figure 12: Housework (Hours per Workday)

Involuntary UE



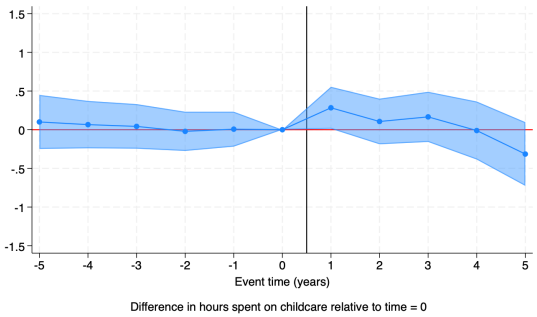
Involuntary STW



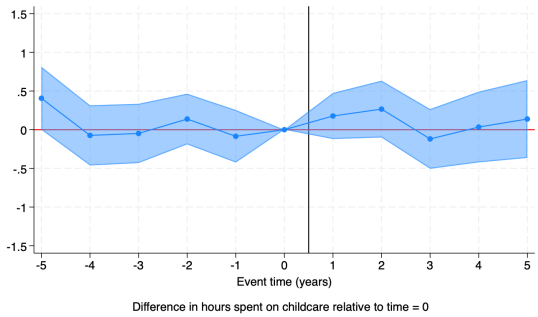
Source. Own calculations based on the SOEP core, v40.
Notes. Event-time coefficients from equation (1). Pre-event observations are based on matching three years prior to the event. Shaded areas represent 95% confidence intervals.

Figure 13: Childcare (Hours per Workday)

Involuntary UE



Involuntary STW



Source. Own calculations based on the SOEP core, v40.
Notes. Event-time coefficients from equation (1). Pre-event observations are based on matching three years prior to the event. Shaded areas represent 95% confidence intervals.

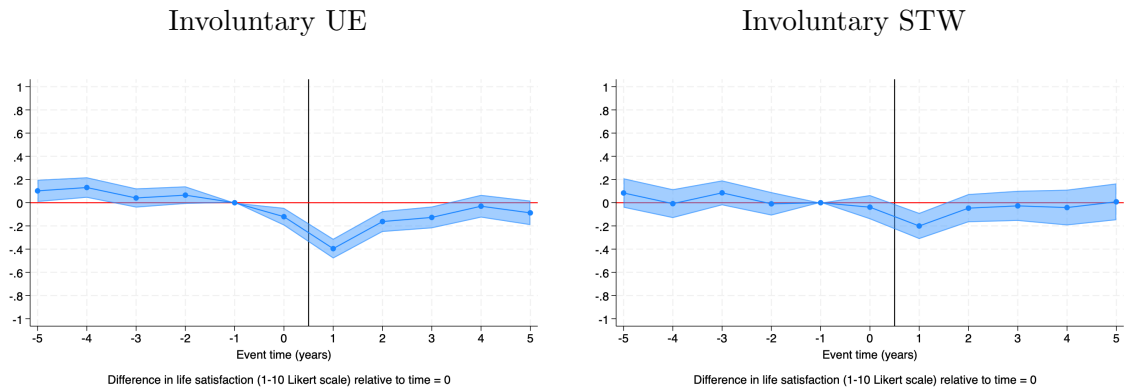
turns negative but remains statistically indistinguishable from zero.

To summarize, we find that UE episodes are associated with clear and persistent increases in housework and short-run increases in time spent on childcare, whereas STW episodes are followed by comparatively small and less systematic adjustments in unpaid care work.

5.3 Life Satisfaction

As discussed above, we observe clear anticipation patterns in life satisfaction, which declines already in the year immediately preceding the labor market shock. At the same time, treatment and control groups follow similar trajectories up to two years prior to the event. To account for these dynamics, we normalize the event-study coefficients relative to $K = -1$, the last pre-treatment period in which both groups evolve comparably in well-being. The estimates below, therefore, capture deviations from this baseline rather than from $K = 0$.

Figure 14: Life Satisfaction



Source. Own calculations based on the SOEP core, v40.
Notes. Event-time coefficients from equation (1). In these estimations we normalize the event-study coefficients relative to $K = -1$, so that the estimates capture deviations from this baseline rather than from $K = 0$. Pre-event observations are based on matching three years prior to the event. Shaded areas represent 95% confidence intervals.

Figure 14 reports effects on life satisfaction. While STW is followed by a moderate and short-lived drop without evidence of anticipation, UE generates a more pronounced short-run decline that gradually attenuates over time. For individuals experiencing involuntary UE, a decline in life satisfaction is already observable in $K = 0$, the observation period immediately preceding job loss. In this period, life satisfaction is 0.12 points lower than in $K = -1$, statistically significant at the 1 percent level. In the year after the job loss, the decline then increases substantially to 0.40 points, statistically significant at the 1 percent level. Relative to the baseline level of 6.86 at $K = -1$, this corresponds to a reduction of

approximately 5.8 percent. In year two, life satisfaction remains 0.16 points below baseline, statistically significant at the 1 percent level (about 2.4 percent), and in year three, the decline amounts to 0.13 points, statistically significant at the 1 percent level (about 1.8 percent). In later years, coefficients become smaller and statistically insignificant, indicating a gradual attenuation of the initial decline. In contrast, for individuals experiencing STW, pre-event coefficients, including in $K = 0$, are small and statistically insignificant, indicating no drop in life satisfaction prior to the event. In the event year, life satisfaction declines by 0.20 points, statistically significant at the 1 percent level. Relative to the baseline level of 7.27 at $K = -1$, this corresponds to a reduction of approximately 2.8 percent. In subsequent years, coefficients remain statistically indistinguishable from zero.

6 Heterogeneity by Gender

6.1 Descriptive Statistics by Gender

Like in many countries worldwide, the labor market in Germany is characterized by strong gender inequalities, such as gender gaps in pay, in working hours, in promotions, as well as gender gaps in unpaid care work. Against this background, we ask the question whether events like STW or UE differently affect men and women in terms of their long-term labor market outcomes, their time use as well as life satisfaction. We thus estimate, in the next step, the effects of STW and UE separately for men and women.

Tables B.1 and B.2 in the Appendix report baseline characteristics for men and women in the matched samples for involuntary UE and STW, measured in $K = 0$, the year prior to the employment shock. The tables follow the same structure as Table 2, but split the matched samples by gender.

Within gender, the matching procedure achieves close balance between treated and control individuals across demographic characteristics, labor market variables, unpaid care work, and pre-treatment employment averages. Differences are small and mostly statistically insignificant, indicating that the matching performs well separately for men and women.[17]

Across genders, clear differences emerge. Women have lower real hourly wages and work

[17]The number of observations is not perfectly identical between treated and control groups within gender because gender is included as one of several covariates in the propensity score matching rather than imposing exact gender matching. A small number of cross-gender matches, therefore, occur, but these cases are rare and do not seem to affect overall balance.

fewer weekly hours than men in both the UE and STW samples. They also exhibit shorter tenure and lower pre-treatment work hours on average, indicating weaker labor market attachment. Differences are particularly pronounced in unpaid care work. Women report substantially higher housework hours per weekday and considerably more childcare hours than men. These gaps are large in both the UE and STW samples, highlighting that men and women experience employment shocks under markedly different household and care arrangements.

Taken together, the descriptive statistics highlight substantial differences in labor market positions and unpaid care responsibilities between men and women prior to the employment shock. These differences reflect well-documented gender disparities in wages, working hours, and the allocation of unpaid work, and imply that men and women enter UE or short-time work from systematically different starting points.

6.2 Estimation results by Gender

Given these differences, we estimate treatment effects separately for men and women. The estimated effects capture the causal impact of the employment shock relative to the counterfactual of not experiencing the event for individuals of the same gender. Comparisons of effect sizes across genders are descriptive rather than causal, reflecting how employment shocks affect men and women, given their respective labor market conditions, care responsibilities, and baseline well-being.

Overall, employment responses to involuntary UE are remarkably similar across gender. Figure B.11 (Appendix) shows that in the year after the event, employment declines by very similar magnitudes for men and women and recovers gradually in subsequent years. In contrast, STW yields somewhat larger long-run reductions in women's employment. While employment losses for men remain modest, not exceeding about 7 percent of baseline employment, women experience larger declines, reaching around 13 percent below baseline levels by $K = 5$. However, estimates beyond $K = 3$ for women should be interpreted cautiously, as the composition of the treated group changes once observations from the COVID-19 STW period drop out of the sample.[18]

A similar pattern emerges for registered UE. Figure B.12 shows that in the year after the

[18] In the following section, we therefore examine effects separately by crisis period and restrict the COVID-19 analysis to the short- and medium-run window, excluding estimates beyond $K = 3$ for that subsample.

event registered UE rises by about 37 percentage points for men and 32 percentage points for women, and remains elevated for several years thereafter. For STW, increases are much smaller and broadly comparable in the early post-treatment periods. Larger differences emerge only in later years, where increases in registered UE reach about 5-6 percentage points for women compared to roughly 3-4 percentage points for men, again potentially reflecting compositional changes in the female STW sample.

Hourly wages, conditional on employment, display substantial declines following involuntary UE. As shown in Figure B.13, hourly wages fall by roughly 11-15 percent for men and about 10-15 percent for women within the first years following job loss, indicating broadly similar proportional wage losses across gender relative to their respective baselines. By contrast, wage effects following STW remain small. Only for men, wages decline modestly in the first post-treatment year (about 4 percent relative to baseline), statistically significant at the five percent level. They remain negative, but not significant, in the following years. For women, wage estimates fluctuate around zero and remain statistically insignificant throughout the post-treatment period.

Figure B.14 illustrates that working hours, conditional on employment, decline substantially after UE for both men and women. Relative to baseline working hours, reductions amount to roughly 7-8 percent for men and about 6-8 percent for women, indicating very similar proportional responses across gender. In contrast, STW does not lead to systematic changes in working hours for either group.

Patterns in unpaid care work differ somewhat across the two shocks and by gender. Figure B.15 shows that involuntary UE leads to clear increases in housework hours for both, men and women. For men, housework rises by about 0.38 hours per workday in $K = 1$, corresponding to roughly a 56 percent increase relative to the baseline level of 0.67 hours (Table B.1). The effect then declines but remains economically meaningful: increases are still around 0.27 hours in $K = 2$ and about 0.12 hours (roughly 6 percent above baseline) by $K = 5$. For women, housework increases by about 0.42 hours in $K = 1$, corresponding to roughly 19 percent relative to the baseline level of 2.20 hours. While the absolute increase is similar, proportional changes are substantially larger for men, given that women already spend much more time on these chores. For STW, adjustments in housework are considerably smaller. Among men, estimates are not different from zero in most time periods, except for becoming only marginally significant in $K = 3$, where

housework increases by about 0.11 hours (roughly 14 percent relative to the baseline of 0.78 hours; Table B.2). For women, the only significant increase also appears around $K = 3$, amounting to about 0.21 hours (roughly 11 percent relative to the baseline of 1.91 hours). Overall, UE is followed by a sizable and persistent rise in housework for both men and women, whereas STW is associated with smaller and less systematic adjustments.

Childcare responses differ across gender, especially following job loss. Figure B.16 shows that UE increases time spent on childcare for fathers but not for mothers. For men, time spent on childcare rises by about 0.67 hours per weekday in $K = 1$, corresponding to roughly a 44 percent increase relative to the baseline level of 1.51 hours (treated men with children in $K = 0$). This increase is short-lived and becomes statistically indistinguishable from zero already by $K = 2$. For women, childcare estimates remain close to zero throughout the post-treatment period, implying no detectable adjustment relative to the much higher baseline level of 4.49 hours (treated women with children in $K = 0$). For STW, effects on time spent on childcare remain small and insignificant for both men and women. Taken together, the results do not indicate systematic changes in time spent on childcare following STW relative to baseline levels of 1.76 hours for treated fathers and 4.59 hours for treated mothers with children in $K = 0$.

Finally, we look at the effects on well-being by gender. Figure B.17 shows that involuntary UE generates sizable short-run declines in life satisfaction for both men and women. For men, life satisfaction declines gradually several years prior to the event, whereas women exhibit a relatively flat pre-trend until $K = -1$, with the first clear decline occurring between $K = -1$ and $K = 0$. In the first post-event year, life satisfaction falls by about 0.38 points for men and 0.41 points for women. Relative to baseline levels of 6.81 (men) and 6.91 (women) in $K = -1$, these correspond to declines of roughly 5.5 percent and 6 percent, respectively. The effects attenuate thereafter, with later coefficients becoming small and statistically insignificant by $K = 3$ for men and $K = 4$ for women. For STW, well-being responses in life satisfaction remain smaller. Only for men, life satisfaction declines significantly in $K = 1$ (about 0.22 points, roughly 3 percent relative to the baseline of 7.23 in $K = -1$), while subsequent estimates are close to zero. For women, post-treatment coefficients remain small and statistically insignificant throughout, indicating no clear life satisfaction response to STW relative to women not experiencing STW.

Overall, we do not find marked gender differences in the effect of STW on long-term labor

market outcomes. We find a slightly more pronounced negative effect on the longer-term probability of employment for women than for men. However, conditional on employment, wage trajectories and the development of weekly working are not significantly impacted by an STW episode, neither for men nor for women. For UE, we find very similar detrimental effects on labor market outcomes for both men and women. Thus, we conclude that neither event amplifies the large gender differences observed in the German labor market. For unpaid care work, we find no long-term effects of an STW episode for either men or women. However, in relative terms, a UE episode affects unpaid care work more for men than for women, thereby slightly decreasing gender gaps in this dimension.

7 Do the effects differ by type of crisis?

Given the long time-period that STW is included in the SOEP questionnaire, we can assess whether the overall small effects of STW found in the main results differ over time and by type of crisis. Results are displayed in Section C in the Appendix. Figures C.20 to C.26 summarize the estimates across three periods: the reunification period (1985–2005), the financial crisis period (2005-2015), and the COVID-19 period (2015-2023).

During the reunification period (1985–2005), the negative employment effects of STW are most pronounced. Figure C.20 indicates that the decline in employment following STW shown in Figure 8 (section 5) is largely driven by this period, whereas in later periods, we find effects that are close to zero. This period is also the only one in which the short increase in weekly working hours, conditional on employment, in $K = 1$ is significant (C.23, upper panel on the right).

During the financial crisis, STW effects across outcomes are generally small and close to zero. Neither employment nor life satisfaction measures display strong responses, suggesting that STW largely stabilized labor market outcomes during this episode. If anything, we observe a small dip in life satisfaction at $K = 1$ during this period, which then quickly returns to the initial level.

During the COVID-19 crisis, some effects emerge in other dimensions, rather than employment. The moderate and short-lived negative wage effects shown in Figure 9 (section 5) are driven by a decline in wages at $K = 1$ following STW during this period. At the same time, non-employment outcomes show some slight changes: the significant increase in housework

at $K = 3$, visible for both men and women, appears to follow an upward trend in hours spent on housework since the COVID-19 crisis among individuals experiencing STW compared to those who did not. There is also, similar to the financial crisis, a short dip in life satisfaction at $K = 1$.

Consistent with the stronger negative STW effects observed during the reunification period, the employment consequences of UE are also largest in this period. In particular, the probability of being employed declines by almost 50 percentage points in $K = 1$ following UE during the reunification period, compared to roughly 40 percentage points during the financial crisis and about 30 percentage points during the COVID-19 crisis. Long-run effects are also most distinct in that early time period: employment remains around 20 percentage points lower in the reunification period, compared to approximately 15 percentage points in both the financial crisis and COVID-19 periods up to 5 years after the job loss. These patterns are mirrored in the evolution of unpaid housework, where increases following UE are strongest during the reunification period and smallest during the COVID-19 crisis. Life satisfaction responses also vary across periods. The dip in life satisfaction is most pronounced during the reunification period and becomes statistically insignificant during the COVID-19 crisis.

These heterogeneous effects by time period suggest that the stabilizing effect of STW strongly depends on the economic context. STW appears to have successfully stabilized the employment and wage trajectories of workers during the financial crisis and the COVID-19 pandemic. In contrast to workers who have lost their jobs during this time, workers with an STW event do not have worse employment and wage outcomes compared to workers without such an event. However, STW was less effective in stabilizing workers' labor market outcomes in the economic crisis after the reunification in the 1990s. Workers experiencing an STW event in this period exhibit lower employment probabilities and a higher prevalence of unemployment in the years following the STW episode. However, these detrimental effects remain much smaller in magnitude than those of workers who experienced UE after job loss during the same period.

8 Robustness

We conduct several robustness checks to assess the stability of the main results. First, we vary the length of the pre-treatment window used for matching. Using a longer five-

year matching window yields very similar estimates to our baseline specification, both in terms of magnitude and direction. In contrast, when matching is based on only one year of pre-treatment information, pre-trends are less consistently flat in some specifications. For this reason, we interpret these estimates with greater caution. Nevertheless, the estimated effects remain broadly aligned with the main results, suggesting that the qualitative conclusions are not sensitive to the precise matching window used. The corresponding results are reported in Appendix D and Appendix E.

Second, we replicate the analysis using the linked SOEP-ADIAB data to account for firm fixed effects. Based on a specification that includes firm fixed effects, we do not detect significant effects on individual employment trajectories. These findings further support the conclusion that STW largely preserves employment relationships rather than leading to systematic changes in firm structure or employment stability. The results are reported in Appendix F.

Third, we estimate dynamic treatment effects using the estimator proposed by Sun and Abraham (2021), which accounts for treatment-effect heterogeneity in staggered adoption settings. For most outcomes, the results closely resemble those obtained from the fixed-effects specification. In our setting, potential biases from staggered treatment timing are already mitigated to some extent because the matching procedure restricts potential control observations to individuals observed in the same age and calendar year as their treated counterparts before the fixed effects are applied, limiting problematic comparisons across cohorts. For employment, the Sun and Abraham (2021) estimator slightly adjusts the short-run post-treatment estimates and no longer indicates a small negative employment effect in the first three years following STW. This difference likely reflects the correction for heterogeneous treatment effects during the COVID-19 period. Importantly, when we split the analysis by time period, the same heterogeneity patterns across crises emerge as in the baseline specification. These results are presented in Appendix G.

9 Mechanisms and Discussion

The heterogeneity patterns by gender and across time periods suggest that the magnitude of STW effects depends less on individual characteristics and more on the broader economic environment in which STW became the policy instrument of choice. In this section, we explore several potential mechanisms that could explain the observed differences across

periods and the small gender differences.

We begin by analyzing whether the composition of workers experiencing UE and STW changes over time. Table 3 reports descriptive statistics for treated individuals across the three time periods. The table presents characteristics measured in $K = 0$, the year of the employment shock, and illustrates how the composition of treated individuals in both UE and STW evolves over time, and reveals several interesting patterns. First, the demographic composition of workers experiencing STW changes substantially across periods. While women represent only around 29 percent of STW recipients during the earlier periods, their share rises to about 50 percent during the COVID-19 period. At the same time, the share of workers residing in East Germany declines over time. Second, the labor market characteristics of individuals experiencing STW also shift. Workers affected by STW in more recent periods tend to have higher wages, shorter tenure, and are employed in smaller firms than workers experiencing STW during the reunification period. For example, the average real hourly wage among STW workers increases from 10.44 Euros during the reunification period to 18.21 Euros during the COVID-19 period. In line with this, we observe that workers have higher educational attainment and appear to be employed in distinct industries during this latest shock. Lastly, the differences between treated individuals in UE and STW are smallest during the early reunification period, making the counterfactual of unemployment particularly relevant for workers experiencing STW in that period.

Taken together, these patterns suggest that STW increasingly serves as a stabilizing instrument for workers in relatively secure employment relationships in later crises, whereas during the reunification period, it affected workers whose observable characteristics were closer to those facing unemployment. This compositional shift is consistent with the stronger STW effects observed during the reunification period and the more muted effects in later crises.

Next, we examine whether differences in the duration of STW across periods could explain the stronger employment and well-being effects observed in the earlier years. Figure A.2 reports the average number of weeks individuals spend in STW per year across the three time periods. Interestingly, the figure shows that the duration of STW exposure is actually longer in the more recent time periods than during the earlier reunification period. This pattern likely also reflects institutional changes over time, particularly policy responses during the financial crisis and later during the COVID-19 pandemic, that extended the

Table 3: Characteristics of Treated Individuals by Time Period, measured in $K = 0$

	UE (treated)			STW (treated)		
	1985–2005	2005–2015	2015–2023	1985–2005	2005–2015	2015–2023
	Mean (SD)	Mean (SD)	Mean (SD)	Mean (SD)	Mean (SD)	Mean (SD)
Demographic characteristics						
Female	0.45 (0.50)	0.53 (0.50)	0.56 (0.50)	0.29 (0.45)	0.29 (0.46)	0.50 (0.50)
Age	40.05 (11.45)	41.95 (11.71)	43.25 (11.55)	39.38 (10.72)	42.98 (10.55)	44.45 (10.98)
Children	0.39 (0.49)	0.38 (0.49)	0.46 (0.50)	0.45 (0.50)	0.34 (0.47)	0.41 (0.49)
Married	0.67 (0.47)	0.57 (0.50)	0.53 (0.50)	0.76 (0.43)	0.63 (0.48)	0.60 (0.49)
Highest Education (CASMIN)	0.13 (0.33)	0.16 (0.37)	0.20 (0.40)	0.09 (0.29)	0.20 (0.40)	0.23 (0.42)
Migration Background	1.29 (0.53)	1.24 (0.54)	1.34 (0.59)	1.42 (0.54)	1.19 (0.47)	1.32 (0.57)
East Germany (Yes/No)	0.32 (0.46)	0.25 (0.43)	0.23 (0.42)	0.30 (0.46)	0.24 (0.43)	0.21 (0.41)
Mean Survey Year	1997.34 (4.50)	2009.93 (3.32)	2017.67 (2.01)	1994.52 (3.84)	2009.47 (1.05)	2020.12 (0.36)
Labor market information in $K = 0$						
Real Hourly Wage	9.57 (5.59)	11.28 (8.20)	12.98 (9.50)	10.44 (4.63)	16.54 (8.27)	18.21 (9.93)
Weekly Work Hours	32.55 (12.82)	28.64 (14.90)	28.09 (14.42)	36.76 (7.29)	35.59 (8.87)	33.33 (9.87)
Tenure	8.94 (9.60)	8.92 (9.64)	6.63 (8.65)	11.62 (9.91)	12.88 (10.38)	10.22 (9.85)
Industry (11 groups)	10.00 (0.00)	8.49 (2.66)	5.21 (2.57)	10.00 (0.00)	9.72 (1.36)	4.67 (2.58)
Firm Size	6.55 (2.83)	5.33 (3.30)	5.40 (3.45)	8.20 (2.01)	7.39 (2.66)	6.98 (3.04)
Unpaid care work						
Housework Hours per Weekday	1.42 (1.59)	1.44 (1.22)	1.41 (1.13)	1.19 (1.41)	1.00 (0.99)	1.36 (0.92)
Childcare Hours per Weekday	1.00 (2.31)	1.52 (3.37)	1.71 (3.37)	0.92 (2.03)	0.86 (2.11)	1.53 (3.14)
Well-being						
Life Satisfaction (current)	6.63 (1.75)	6.71 (1.82)	7.15 (1.68)	6.67 (1.76)	6.91 (1.61)	7.56 (1.40)
Employment Averaged Over Available Years in the Prior Window						
Pre-Wage Avg	7.67 (4.81)	9.55 (7.49)	10.12 (7.84)	8.68 (4.03)	14.69 (7.35)	15.60 (8.81)
Pre-Tenure Avg	7.43 (8.30)	7.74 (8.65)	6.10 (7.70)	8.94 (8.26)	11.07 (9.31)	8.90 (8.96)
Pre-Work Hours Avg	29.19 (12.27)	24.50 (13.59)	23.45 (13.77)	35.06 (7.49)	33.26 (9.37)	30.46 (10.47)
Pre-Employment Status Avg	0.89 (0.20)	0.86 (0.23)	0.84 (0.24)	0.95 (0.14)	0.94 (0.15)	0.93 (0.15)
Pre-Registered UE Avg	0.04 (0.11)	0.05 (0.14)	0.06 (0.15)	0.02 (0.08)	0.03 (0.09)	0.02 (0.07)
Observations	1917	1175	654	541	479	841

Source. Own calculations based on the SOEP core, v40.

Notes. The table reports means and standard deviations (in parentheses) for individuals who experience either involuntary UE or STW, measured in $K = 0$, the year of the employment shock. Columns distinguish three time periods: the reunification period (1985–2005), the financial crisis period (2005–2015), and the COVID-19 period (2015–2023). The table illustrates how the composition of treated individuals differs across time periods for both UE and STW.

maximum duration for which firms could use STW (Brinkmann et al. (2024); Tilly and Niedermayer (2016)). At the same time, the event-study results indicate that the comparatively more negative employment and life satisfaction effects occur primarily in the earlier period. This comparison, therefore, does not support a simple duration-based explanation for the stronger early effects. Instead, it may indicate that the shorter STW duration during the reunification period reflects situations in which firms were potentially unable to stabilize employment relationships despite receiving STW subsidies.

Another potential mechanism relates to the nature of the reductions of weekly working hours associated with STW. In the most recent survey waves (since 2020), the SOEP provides additional information on the number of hours reduced while on STW. Descriptive patterns indicate that only about 20 percent of treated individuals report being fully absent from work (*"Kurzarbeit Null"*) during this period. Instead, many workers experience only partial reductions in working hours, with spikes at reductions of around 20, 50, or 100 percent. This pattern suggests that STW often operates as a short-run adjustment margin within ongoing employment relationships rather than as a de facto reduction to zero hours that would resemble job loss. In such a setting, even longer STW spells may generate only limited economic and psychological disruption, which is consistent with the comparatively small medium-run employment and well-being effects observed during the COVID-19 period. This interpretation also aligns with findings by Pavlopoulos and Chkalova (2022), who show that STW reduced unemployment, particularly in firms that used the scheme for many workers while implementing only small reductions in working hours.

Regional differences provide additional insights into the role of the broader economic environment. If the effectiveness of STW depends on workers' outside options in the labor market, the profound economic restructuring during the German reunification period may have generated particularly pronounced regional differences. Figures C.27 and C.28 therefore examine heterogeneity between East and West German workers across the three time periods for employment probability and life satisfaction. The results show that the most pronounced differences occur during the reunification period. In Figure C.27, STW is followed by more negative employment responses for individuals in East Germany during this early period, whereas estimates for the financial crisis and COVID-19 periods are close to zero and display little regional divergence. A similar pattern appears in Figure C.28 for life satisfaction, where the negative responses in the reunification period are again more visible

for individuals residing in East Germany.

Taken together, these patterns suggest that the broader economic environment shapes the consequences of STW spells both in actual employment outcomes and in well-being. The comparatively stronger effects during the reunification period are consistent with an environment characterized by greater labor market uncertainty and more limited outside options, particularly in East Germany. In contrast, during the financial crisis and the COVID-19 period, STW appears to function more as a stabilization policy, preserving employment relationships and limiting negative individual labor-market consequences and well-being shocks.

This interpretation is also consistent with evidence from Giupponi et al. (2022). Using data from the High-frequency Online Personal Panel Survey (HOPP) during the COVID-19 crisis, Giupponi et al. (2022) show that short-time work primarily protected labor market insiders, i.e. workers with higher incomes and stronger employment attachment, whereas unemployment insurance mainly supported labor market outsiders, such as younger workers at the beginning of their careers or individuals with lower education and fewer resources to smooth consumption.

10 Conclusion

In past economic crises, many countries have used STW or other job-retention schemes to stabilize the labor market. In Germany, STW was introduced 100 years ago and has a long history of use as a stabilizing labor-market policy. While STW stabilizes immediate employment outcomes by allowing firms to keep their workforce, and prevents workers from the potential detrimental effects of UE, STW could also deter workers from moving to more productive firms and thereby negatively impact their labor market outcomes in the long run. The long-term effects of STW episodes on workers' labor market outcomes are thus not clear a priori.

Based on our empirical analysis of the long-term effects of STW, we conclude that, in the German institutional context, STW has no significant negative effects on workers' labor market outcomes. The only exception is the economic crisis following German reunification, when STW actually adversely affected workers' labor market prospects. During the financial crisis of 2008/2009 and the economic crisis caused by the COVID-19 pandemic we do not

observe any negative effects of STW on labor market outcomes, including employment probabilities, hourly wages, or weekly working hours (conditional on employment). This suggests, that STW did not deter workers from switching to more productive firms, at least not when compared to workers who did not experience STW. Therefore, STW can be considered successful in stabilizing the employment trajectories of affected workers. This conclusion is particularly relevant when comparing the effects of STW with those of UE: Our analysis on the long-term effects of UE after job loss corroborates previous findings of significant long-term scarring effects in the labor market. Thus, to the extent that STW has prevented workers from experiencing UE, it can be regarded as an effective tool for avoiding the long-lasting negative labor market consequences that often follow job loss. The results of our analysis of life satisfaction measures point in the same direction: while STW leads to a drop in life satisfaction among affected workers, this effect is observed only in the first period after the event and is much smaller than the (longer-lasting) effects of UE.

Given the large gender disparities in the labor market, we investigate potentially heterogeneous effects of STW and UE for men and women. The analysis shows no marked gender differences in the effect of STW on long-term labor market outcomes. We find a slightly more pronounced negative effect on the longer-term probability of employment for women than for men. However, conditional on employment, wage trajectories and the development of weekly working hours are not significantly impacted by an STW episode, neither for men nor for women. For UE, we find very similar detrimental effects on labor market outcomes for both men and women. Thus, we conclude that neither event amplifies gender inequalities in the labor market. For unpaid care work, we find no long-term effects of an STW episode for either men or women. However, in relative terms, a UE episode affects unpaid care work more for men than for women. Based on these findings, future research should investigate the household dynamics following STW further. For example, it could be interesting to analyze the partners' reactions in terms of labor supply and unpaid care work to an episode of STW. Thereby, one could derive conclusions on the extent to which STW episodes actually affect the gender care gap observed in couple households, which is one important dimension of gender inequality.

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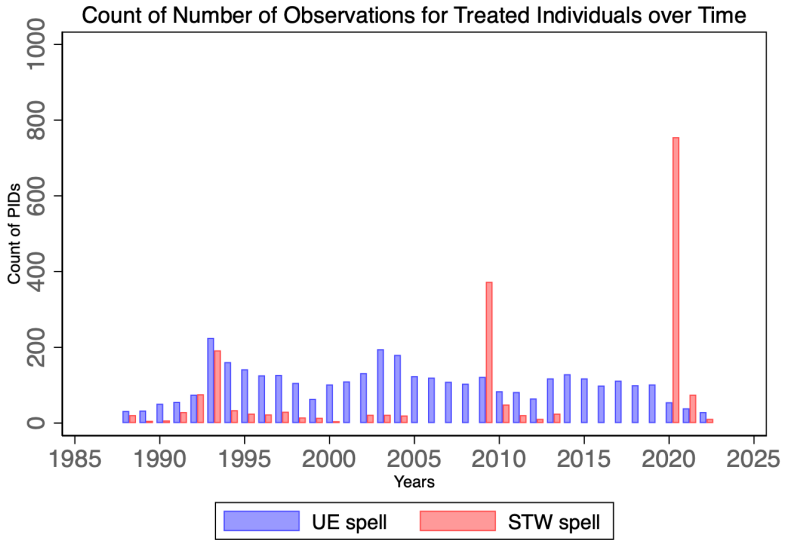
Appendices

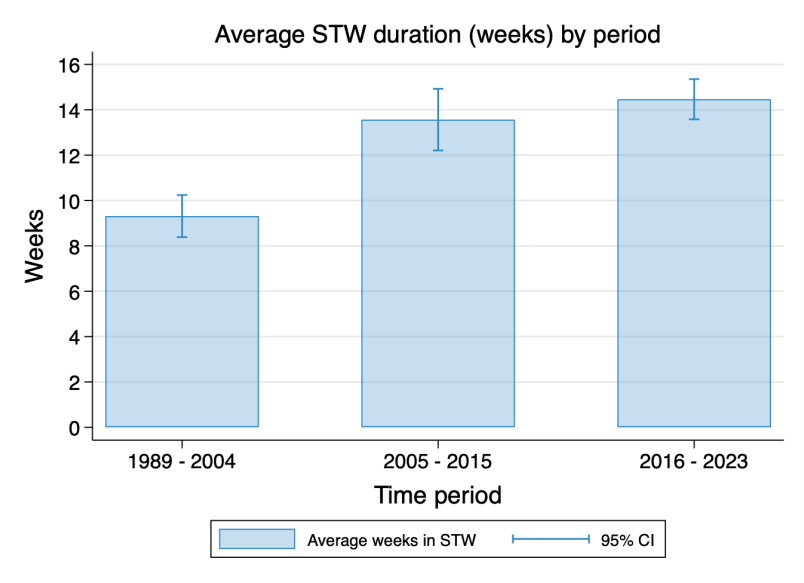
A Additional Descriptive Statistics

Figure A.1: UE and STW in the SOEP, 1989 to 2022 - Matching on 3 years



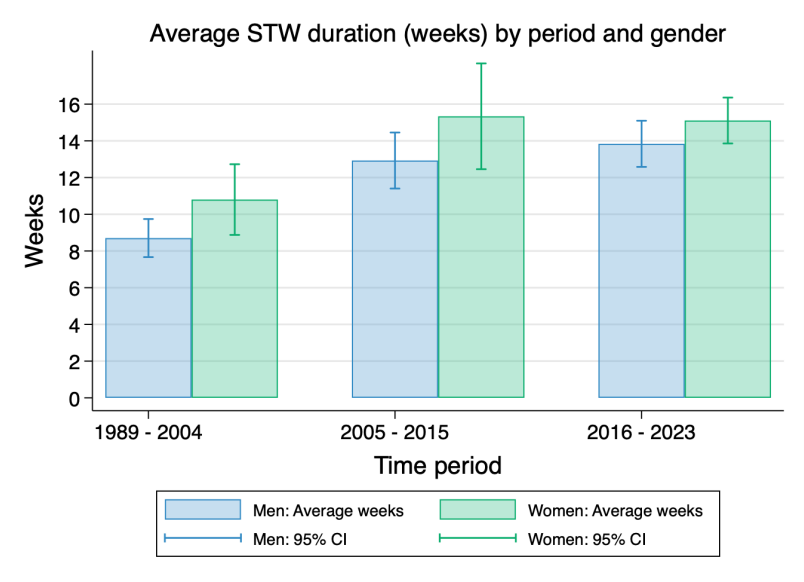
Source. Own calculations based on the SOEP core, v40.

Figure A.2: Average number of weeks in short-time work per year



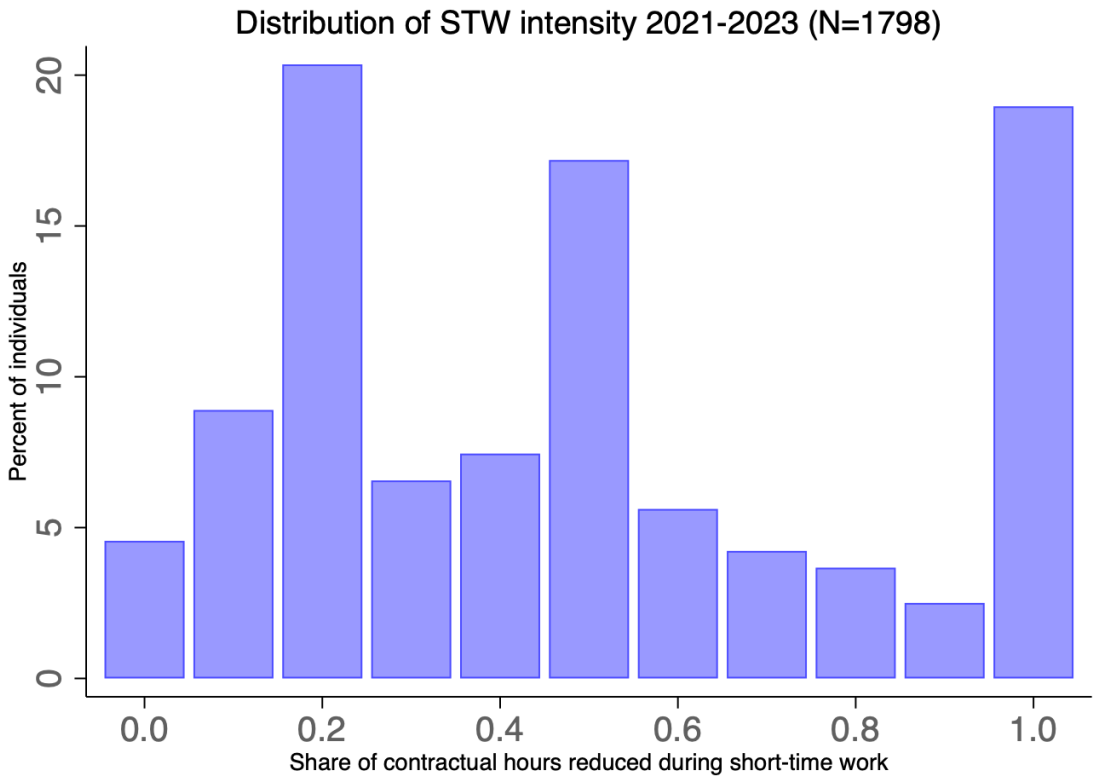
Source. Own calculations based on the SOEP core, v40.

Figure A.3: Average number of weeks in short-time work per year, by gender



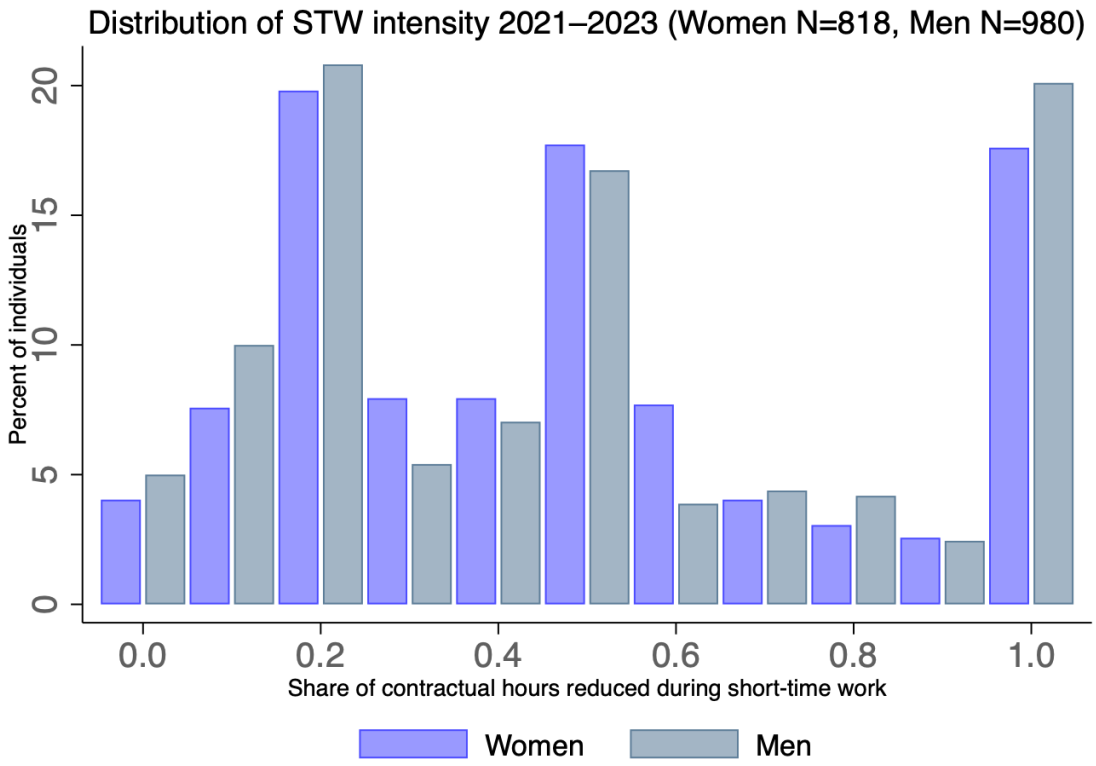
Source. Own calculations based on the SOEP core, v40.

Figure A.4: Distribution of short-time work intensity in the SOEP, 2021–2023



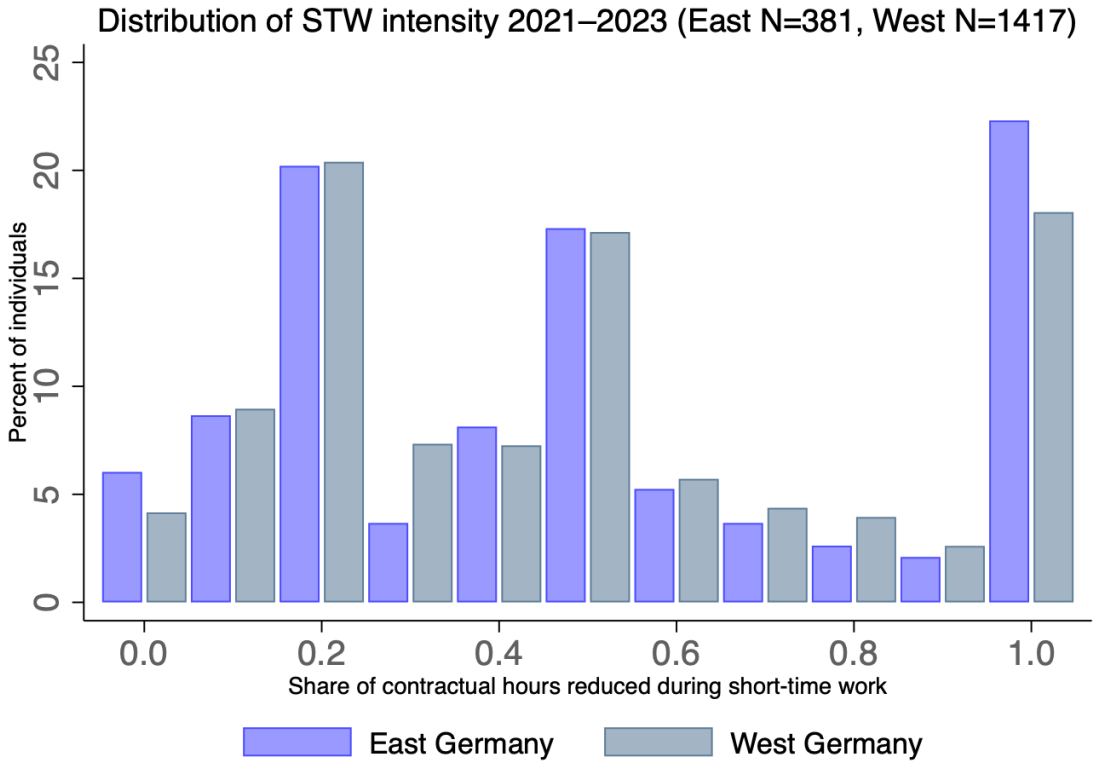
Source. Own calculations based on the SOEP core, v40.
Notes. The figure shows the distribution of the share of contractual weekly hours reduced during STW, defined as the number of reduced hours divided by the number of contractual hours. Ratios above 1 are set to missing. A value of 1 indicates that contracted work hours were completely reduced (Kurzarbeit Null) during the STW period.

Figure A.5: Distribution of short-time work intensity in the SOEP, 2021–2023, by gender



Source. Own calculations based on the SOEP core, v40.
Notes. The figure shows the distribution of the share of contractual weekly hours reduced during STW, defined as the number of reduced hours divided by the number of contractual hours. Ratios above 1 are set to missing. A value of 1 indicates that contracted work hours were completely reduced (Kurzarbeit Null) during the STW period.

Figure A.6: Distribution of short-time work intensity in the SOEP, 2021–2023, by region



Source. Own calculations based on the SOEP core, v40.
Notes. The figure shows the distribution of the share of contractual weekly hours reduced during STW, defined as the number of reduced hours divided by the number of contractual hours. Ratios above 1 are set to missing. A value of 1 indicates that contracted work hours were completely reduced (Kurzarbeit Null) during the STW period.

B Heteregoneity by gender

Table B.1: Matched Control and Treatment Group Characteristics by Gender: Involuntary UE, measured in $K = 0$

	Men			Women		
	No UE	UE	p-value	No UE	UE	p-value
	Mean (SD)	Mean (SD)		Mean (SD)	Mean (SD)	
Demographic characteristics						
Female	0.00 (0.00)	0.00 (0.00)		1.00 (0.00)	1.00 (0.00)	
Age	41.29 (11.61)	40.97 (12.12)	0.425	40.96 (11.15)	41.29 (11.08)	0.375
Children	0.40 (0.49)	0.39 (0.49)	0.716	0.42 (0.49)	0.40 (0.49)	0.256
Married	0.63 (0.48)	0.63 (0.48)	0.950	0.60 (0.49)	0.61 (0.49)	0.728
Highest Education (CASMIN)	0.16 (0.37)	0.15 (0.36)	0.281	0.14 (0.35)	0.15 (0.36)	0.596
Migration Background	1.31 (0.57)	1.31 (0.56)	0.665	1.28 (0.54)	1.26 (0.53)	0.441
East Germany (Yes/No)	0.28 (0.45)	0.30 (0.46)	0.182	0.28 (0.45)	0.26 (0.44)	0.170
Mean Survey Year	2004.11 (9.38)	2003.74 (8.72)	0.216	2005.30 (9.70)	2005.30 (9.02)	0.998
Labor market information in $K = 0$						
Real Hourly Wage	11.65 (8.23)	11.82 (8.03)	0.535	9.69 (6.90)	9.45 (6.30)	0.265
Weekly Work Hours	33.49 (14.30)	34.41 (12.95)	0.039	27.57 (14.08)	26.79 (13.76)	0.093
Tenure	9.39 (9.76)	9.66 (10.40)	0.414	7.37 (8.11)	7.42 (8.33)	0.843
Industry (11 groups)	8.68 (2.66)	8.83 (2.52)	0.073	8.96 (2.05)	8.79 (2.34)	0.023
Firm Size	6.40 (3.24)	6.35 (2.98)	0.590	5.49 (3.37)	5.64 (3.26)	0.200
Unpaid care work						
Housework Hours per Weekday	0.74 (0.85)	0.67 (0.81)	0.024	2.18 (1.43)	2.20 (1.48)	0.699
Childcare Hours per Weekday	0.62 (1.64)	0.63 (1.41)	0.939	1.93 (3.64)	1.90 (3.65)	0.822
Well-being						
Life Satisfaction (current)	7.06 (1.68)	6.72 (1.75)	<0.001	7.11 (1.68)	6.76 (1.79)	<0.001
Employment Averaged Over Available Years in the Prior Five-Year Window						
Pre-Wage Avg	9.78 (6.38)	9.89 (6.85)	0.638	7.63 (5.23)	7.36 (5.47)	0.133
Pre-Tenure Avg	8.18 (7.61)	8.41 (9.16)	0.400	6.01 (6.29)	6.14 (7.20)	0.572
Pre-Work Hours Avg	30.33 (11.91)	31.19 (11.82)	0.027	23.07 (12.12)	22.38 (12.97)	0.098
Pre-Employment Status Avg	0.91 (0.16)	0.91 (0.18)	0.951	0.83 (0.20)	0.83 (0.24)	0.711
Pre-Registered UE Avg	0.04 (0.11)	0.04 (0.12)	0.866	0.05 (0.13)	0.05 (0.13)	0.459
Observations	1855	1842		1773	1786	

Source. Own calculations based on the SOEP core, v40.

Notes. The table reports means and standard deviations (in parentheses) separately for men and women in the matched sample, measured in $K = 0$, the year prior to the employment shock. Matching is performed separately for involuntary UE using one-to-one nearest-neighbor propensity score matching without replacement and a caliper of 0.01. The variables labeled "Pre-" averages refer to averages computed over all available pre-treatment observations within the window $K \in [-5, 0]$. Because individuals are observed for varying lengths of time prior to treatment, these averages are calculated over the available pre-treatment years and therefore do not necessarily represent full five-year averages for all individuals. The matched sample corresponds to the three-year pre-treatment specification described in Section 4. P-values refer to tests of equality of means between treated and control individuals within gender.

Table B.2: Matched Control and Treatment Group Characteristics by Gender: STW, measured in $K = 0$

	Men			Women		
	No STW	STW	p-value	No STW	STW	p-value
	Mean (SD)	Mean (SD)		Mean (SD)	Mean (SD)	
Demographic characteristics						
Female	0.00 (0.00)	0.00 (0.00)		1.00 (0.00)	1.00 (0.00)	
Age	41.83 (10.89)	42.63 (11.09)	0.083	43.31 (9.67)	42.55 (10.86)	0.163
Children	0.44 (0.50)	0.41 (0.49)	0.158	0.36 (0.48)	0.39 (0.49)	0.333
Married	0.69 (0.46)	0.68 (0.47)	0.562	0.62 (0.49)	0.61 (0.49)	0.841
Highest Education (CASMIN)	0.17 (0.38)	0.17 (0.38)	0.823	0.19 (0.39)	0.20 (0.40)	0.647
Migration Background	1.33 (0.60)	1.32 (0.54)	0.852	1.27 (0.55)	1.31 (0.56)	0.177
East Germany (Yes/No)	0.20 (0.40)	0.24 (0.43)	0.020	0.27 (0.44)	0.24 (0.43)	0.261
Mean Survey Year	2008.70 (10.97)	2008.37 (10.96)	0.470	2011.18 (9.93)	2012.43 (10.56)	0.021
Labor market information in $K = 0$						
Real Hourly Wage	16.31 (9.81)	16.73 (9.38)	0.296	14.93 (8.84)	13.59 (7.73)	0.002
Weekly Work Hours	36.85 (9.64)	36.90 (7.85)	0.888	32.25 (10.76)	31.73 (9.90)	0.343
Tenure	11.53 (10.09)	12.29 (10.35)	0.075	10.72 (9.57)	9.75 (9.38)	0.052
Industry (11 groups)	7.50 (3.23)	7.71 (3.29)	0.119	7.73 (2.82)	7.21 (3.01)	<0.001
Firm Size	7.67 (2.67)	7.73 (2.55)	0.569	7.18 (2.93)	6.97 (2.92)	0.177
Unpaid care work						
Housework Hours per Weekday	0.83 (0.91)	0.78 (0.79)	0.177	1.84 (1.15)	1.91 (1.19)	0.312
Childcare Hours per Weekday	0.81 (1.55)	0.76 (1.49)	0.416	1.54 (3.08)	1.85 (3.69)	0.084
Well-being						
Life Satisfaction (current)	7.31 (1.63)	7.11 (1.58)	0.003	7.27 (1.61)	7.17 (1.67)	0.249
Employment Averaged Over Available Years in the Prior Five-Year Window						
Pre-Wage Avg	14.50 (8.14)	14.74 (8.25)	0.477	12.39 (6.76)	11.14 (6.81)	<0.001
Pre-Tenure Avg	9.75 (8.05)	10.44 (9.27)	0.055	8.94 (7.96)	7.92 (8.04)	0.016
Pre-Work Hours Avg	35.29 (8.25)	35.34 (7.49)	0.899	29.21 (10.33)	28.02 (10.82)	0.034
Pre-Employment Status Avg	0.95 (0.11)	0.96 (0.11)	0.125	0.92 (0.14)	0.90 (0.19)	0.018
Pre-Registered UE Avg	0.02 (0.07)	0.02 (0.08)	0.568	0.02 (0.07)	0.02 (0.08)	0.812
Observations	1150	1144		711	717	

Source. Own calculations based on the SOEP core, v40.

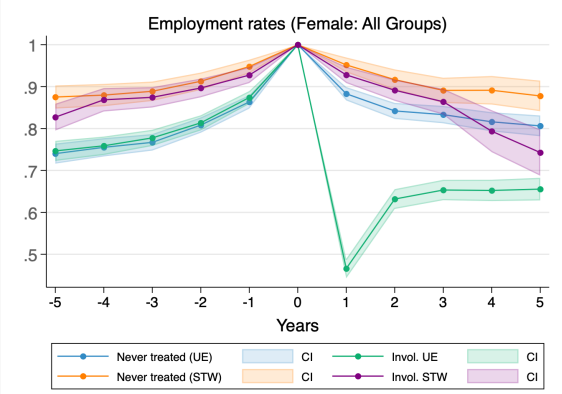
Notes. The table reports means and standard deviations (in parentheses) separately for men and women in the matched sample, measured in $K = 0$, the year prior to the employment shock. Matching is performed separately by gender for STW using one-to-one nearest-neighbor propensity score matching without replacement and a caliper of 0.01. The variables labeled "Pre-" averages refer to averages computed over all available pre-treatment observations within the window $K \in [-5, 0]$. Because individuals are observed for varying lengths of time prior to treatment, these averages are calculated over the available pre-treatment years and therefore do not necessarily represent full five-year averages for all individuals. The matched sample corresponds to the three-year pre-treatment specification described in Section 4. P-values refer to tests of equality of means between treated and control individuals within gender.

Figure B.7: Raw means in control and treatment groups: Employment outcomes by gender

Employment

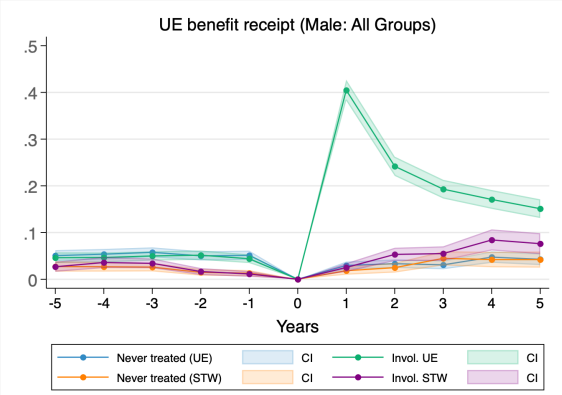
Men

Women

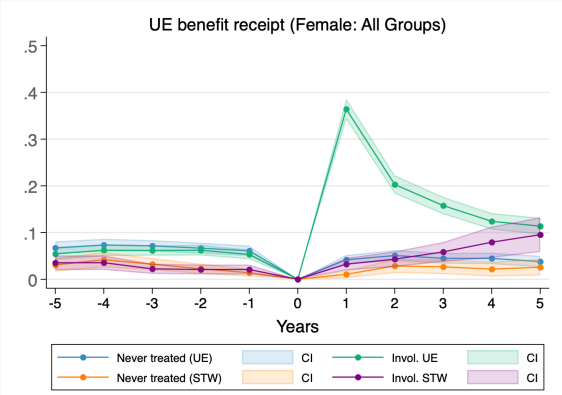


UE benefit reception

Men

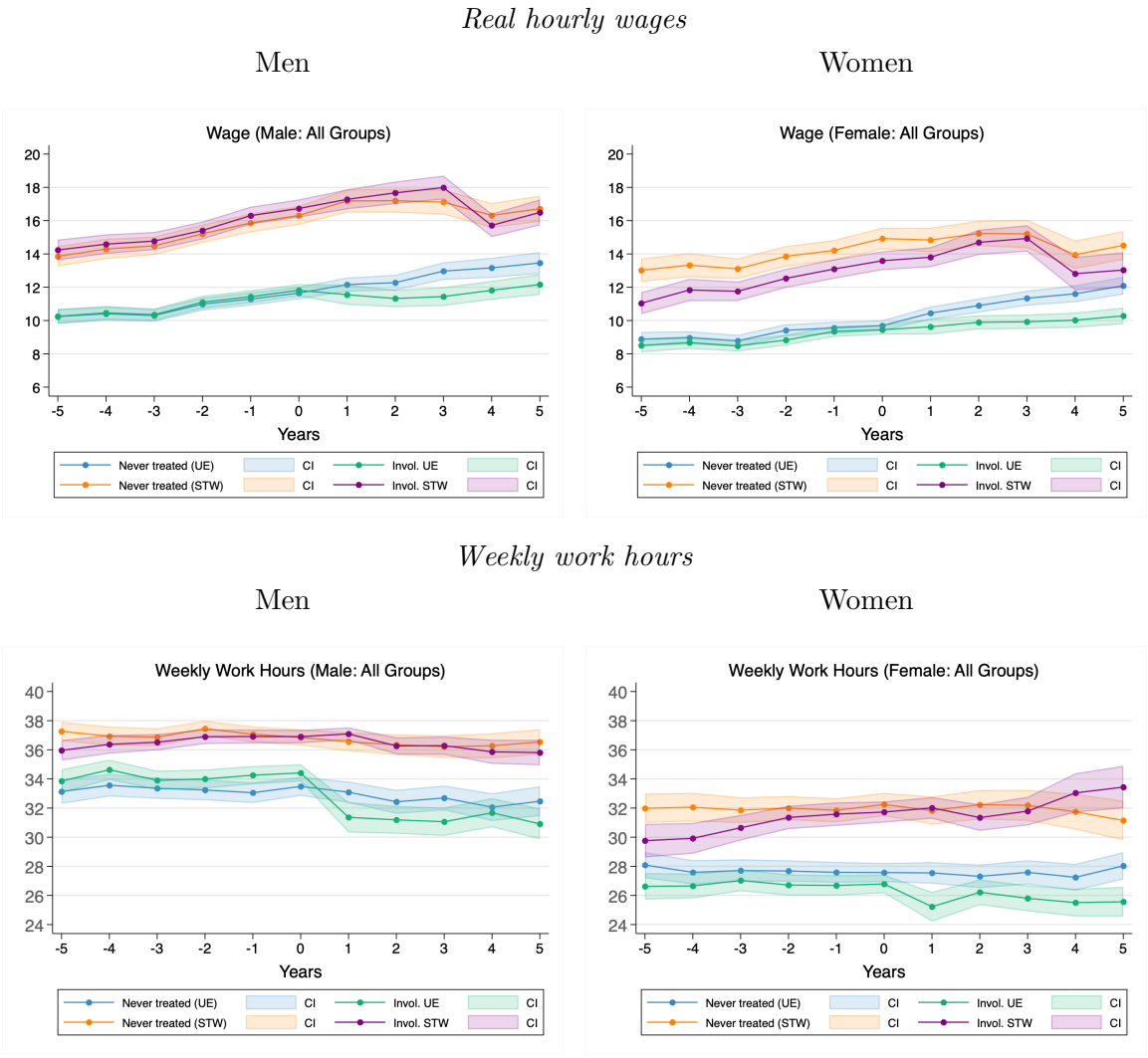


Women



Source. Own calculations based on the SOEP core, v40.
Notes. Plots show raw means over time for matched treatment and control individuals. Shaded areas represent 95% confidence intervals.

Figure B.8: Raw means in control and treatment groups: Earnings and working hours by gender

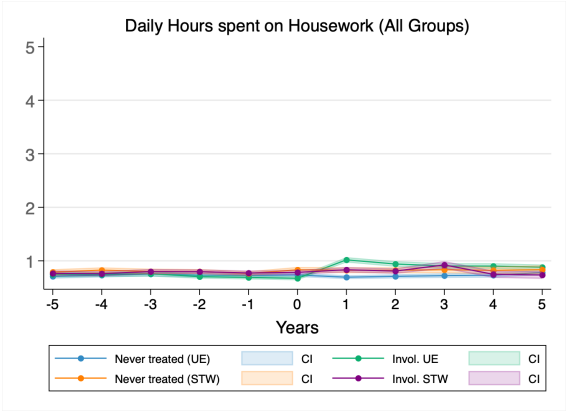


Source. Own calculations based on the SOEP core, v40.
Notes. Plots show raw means over time for matched treatment and control individuals. Shaded areas represent 95% confidence intervals.

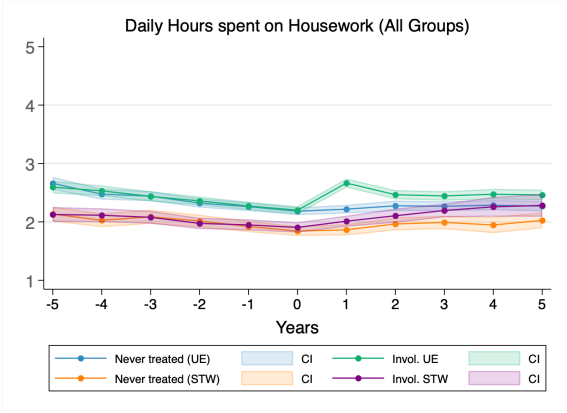
Figure B.9: Raw means in control and treatment groups: Unpaid care work by gender

Housework hours per workday

Men

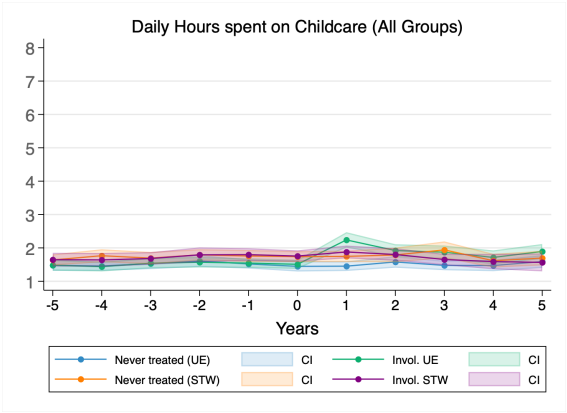


Women

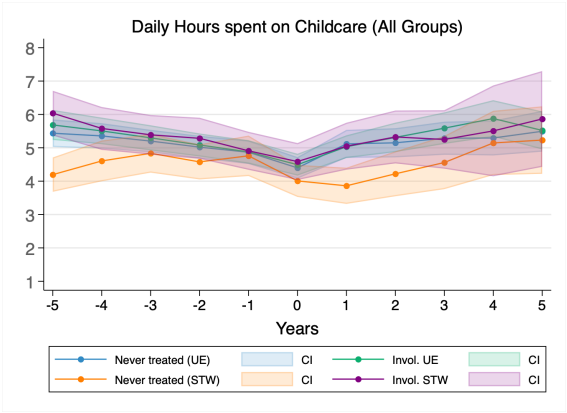


Childcare hours per workday

Men

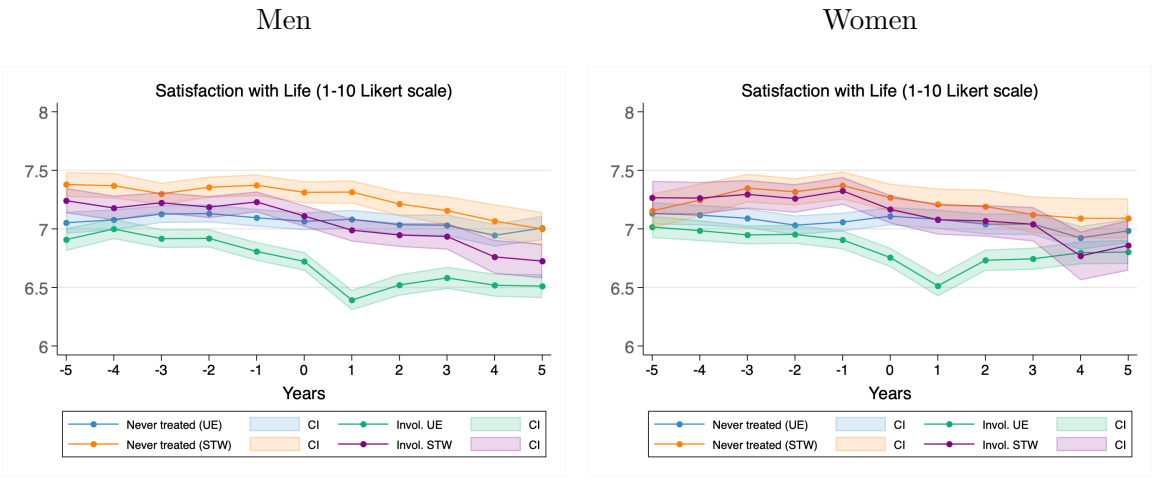


Women



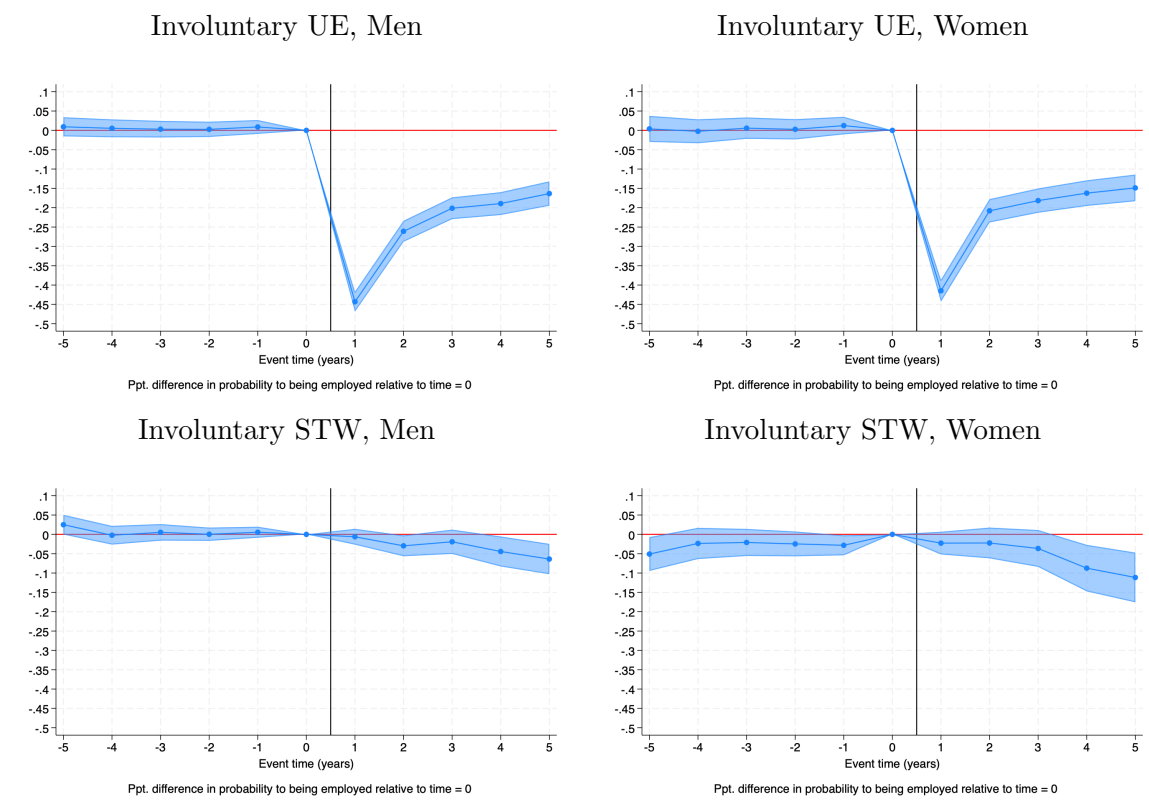
Source. Own calculations based on the SOEP core, v40.
Notes. Plots show raw means over time. For childcare, the sample is restricted to individuals with children. Shaded areas represent 95% confidence intervals.

Figure B.10: Raw means in control and treatment groups: Well-being, life satisfaction, by gender



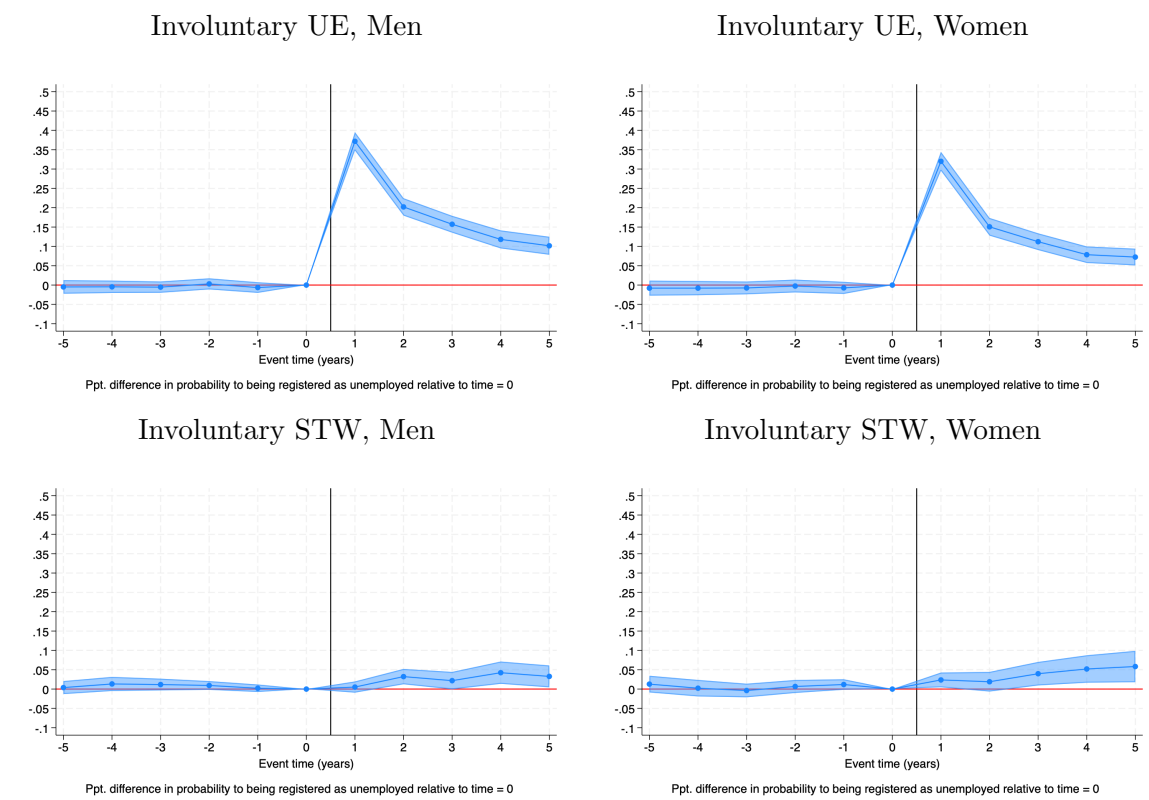
Source. Own calculations based on the SOEP core, v40.
Notes. Plots show raw means over time. We normalize the event-study coefficients relative to $K = -1$, the last pre-treatment period in which both groups evolve comparably in well-being outcomes. The estimates therefore capture deviations from this baseline rather than from $K = 0$. Shaded areas represent 95% confidence intervals.

Figure B.11: Employment



Source. Own calculations based on the SOEP core, v40.
Notes. Plots show the event-time coefficients obtained from estimating equation (1). The estimates are based on a panel where the pre-event observations are based on matching three years prior to the event. Shaded areas represent 95% confidence intervals.

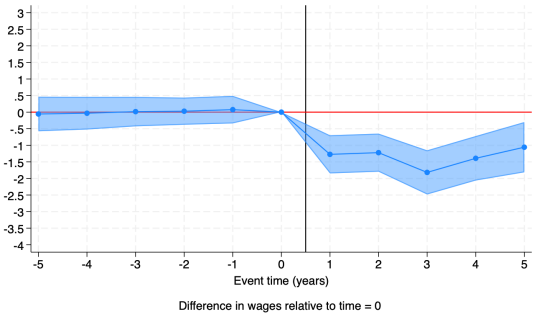
Figure B.12: Reception of UE Benefits



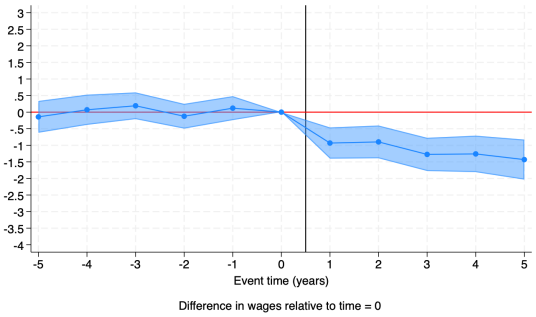
Source. Own calculations based on the SOEP core, v40.
Notes. Plots show the event-time coefficients obtained from estimating equation (1). The estimates are based on a panel where the pre-event observations are based on matching three years prior to the event. Shaded areas represent 95% confidence intervals.

Figure B.13: Wage, conditional on employment

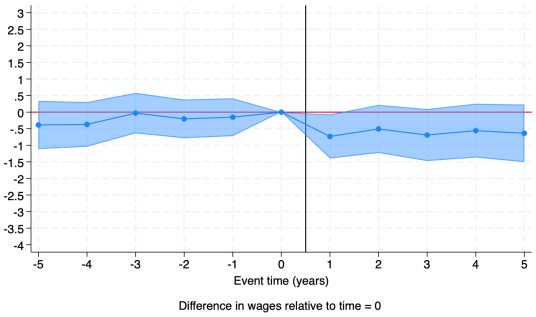
Involuntary UE, Men



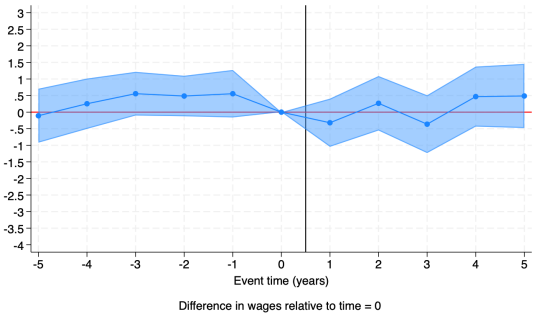
Involuntary UE, Women



Involuntary STW, Men

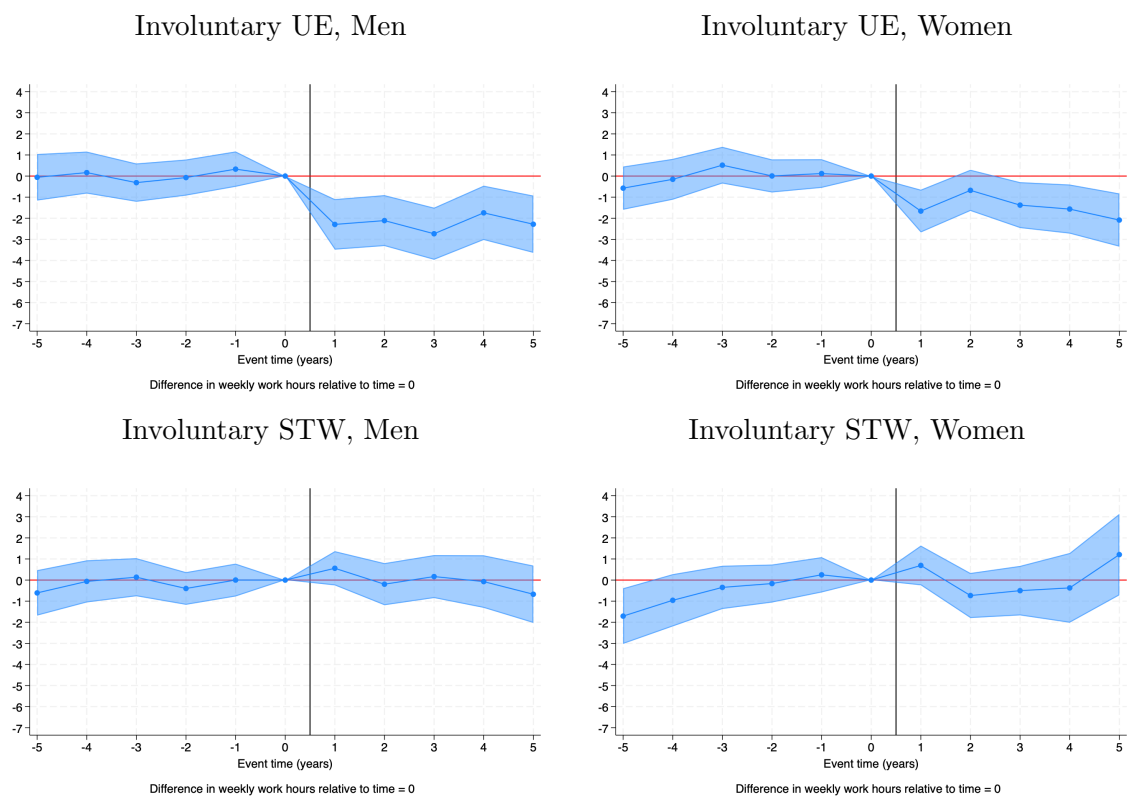


Involuntary STW, Women



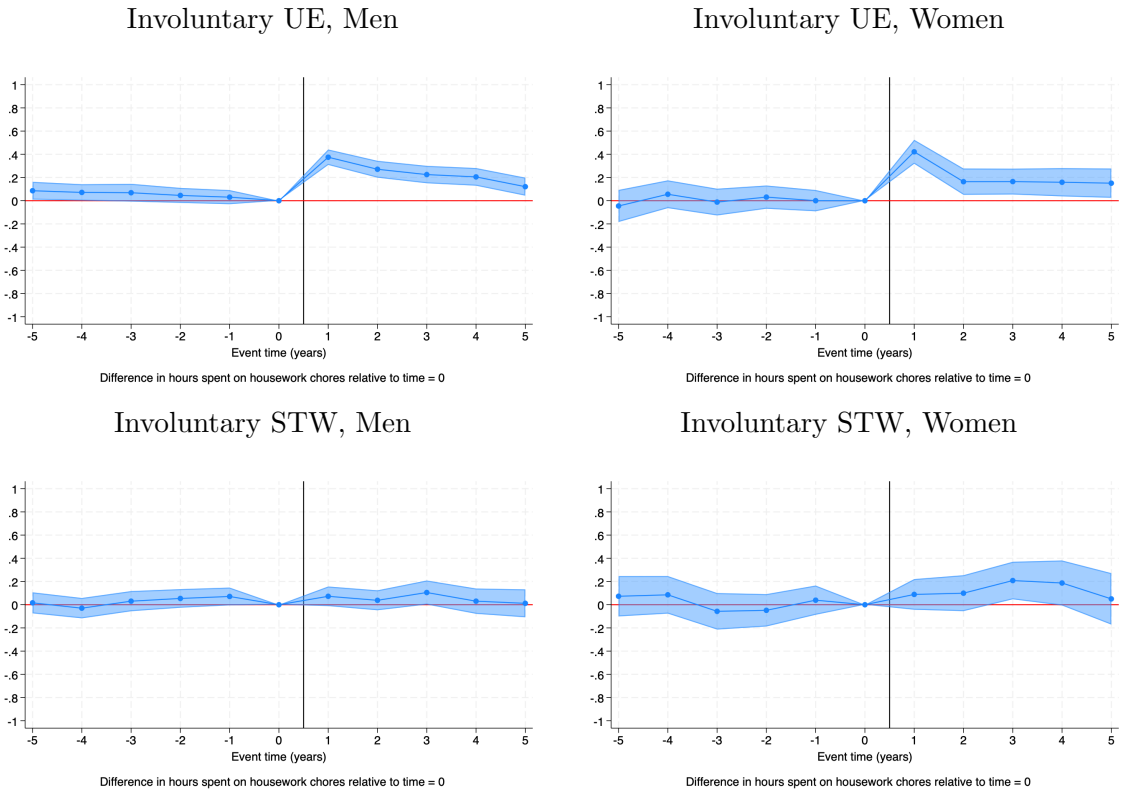
Source. Own calculations based on the SOEP core, v40.
Notes. Plots show the event-time coefficients obtained from estimating equation (1). The estimates are based on a panel where the pre-event observations are based on matching three years prior to the event. Shaded areas represent 95% confidence intervals.

Figure B.14: Weekly work hours, conditional on employment



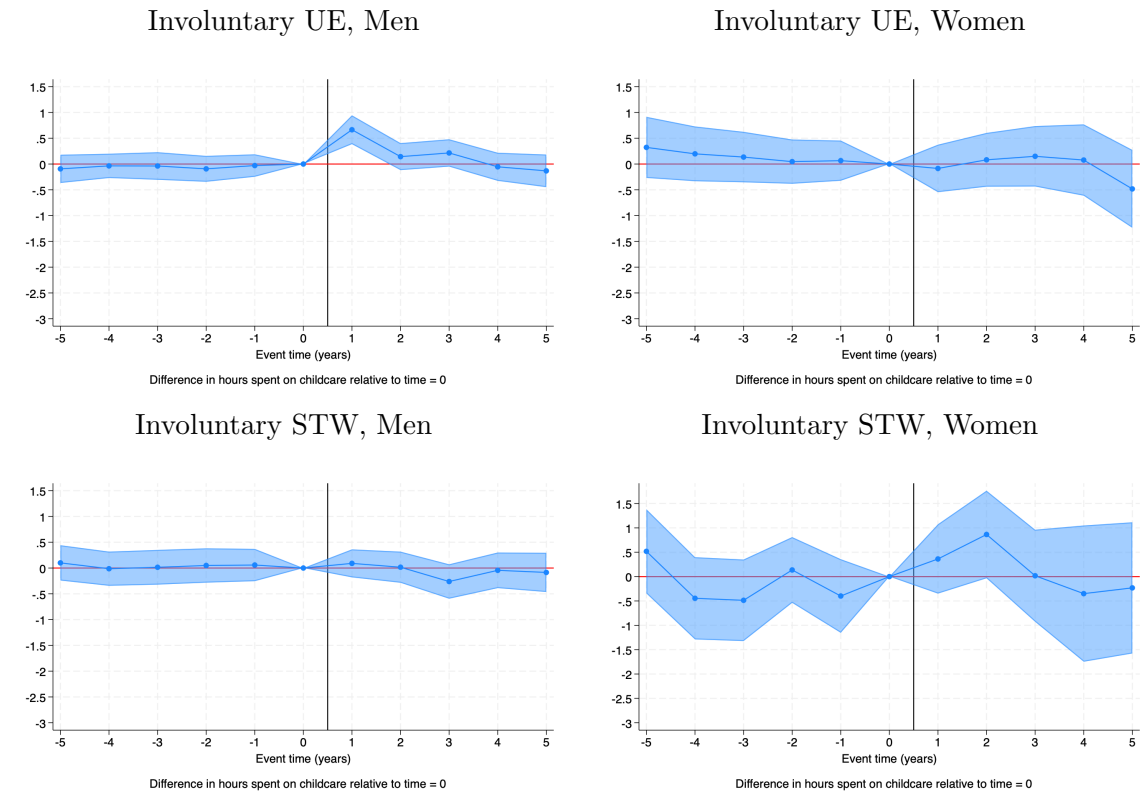
Source. Own calculations based on the SOEP core, v40.
Notes. Plots show the event-time coefficients obtained from estimating equation (1). The estimates are based on a panel where the pre-event observations are based on matching three years prior to the event. Shaded areas represent 95% confidence intervals.

Figure B.15: Housework in hours per workday



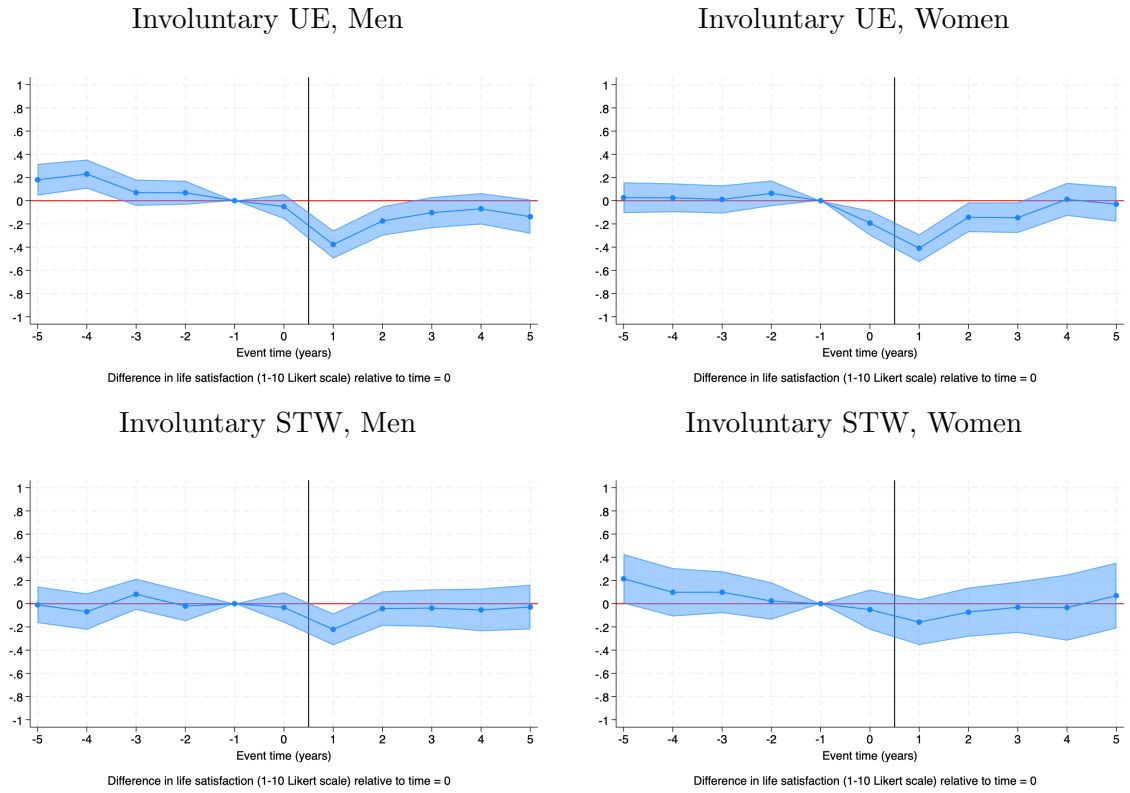
Source. Own calculations based on the SOEP core, v40.
Notes. Plots show the event-time coefficients obtained from estimating equation (1). The estimates are based on a panel where the pre-event observations are based on matching three years prior to the event. Shaded areas represent 95% confidence intervals.

Figure B.16: Childcare in hours per workday



Source. Own calculations based on the SOEP core, v40.
Notes. Plots show the event-time coefficients obtained from estimating equation (1). The estimates are based on a panel where the pre-event observations are based on matching three years prior to the event. Shaded areas represent 95% confidence intervals.

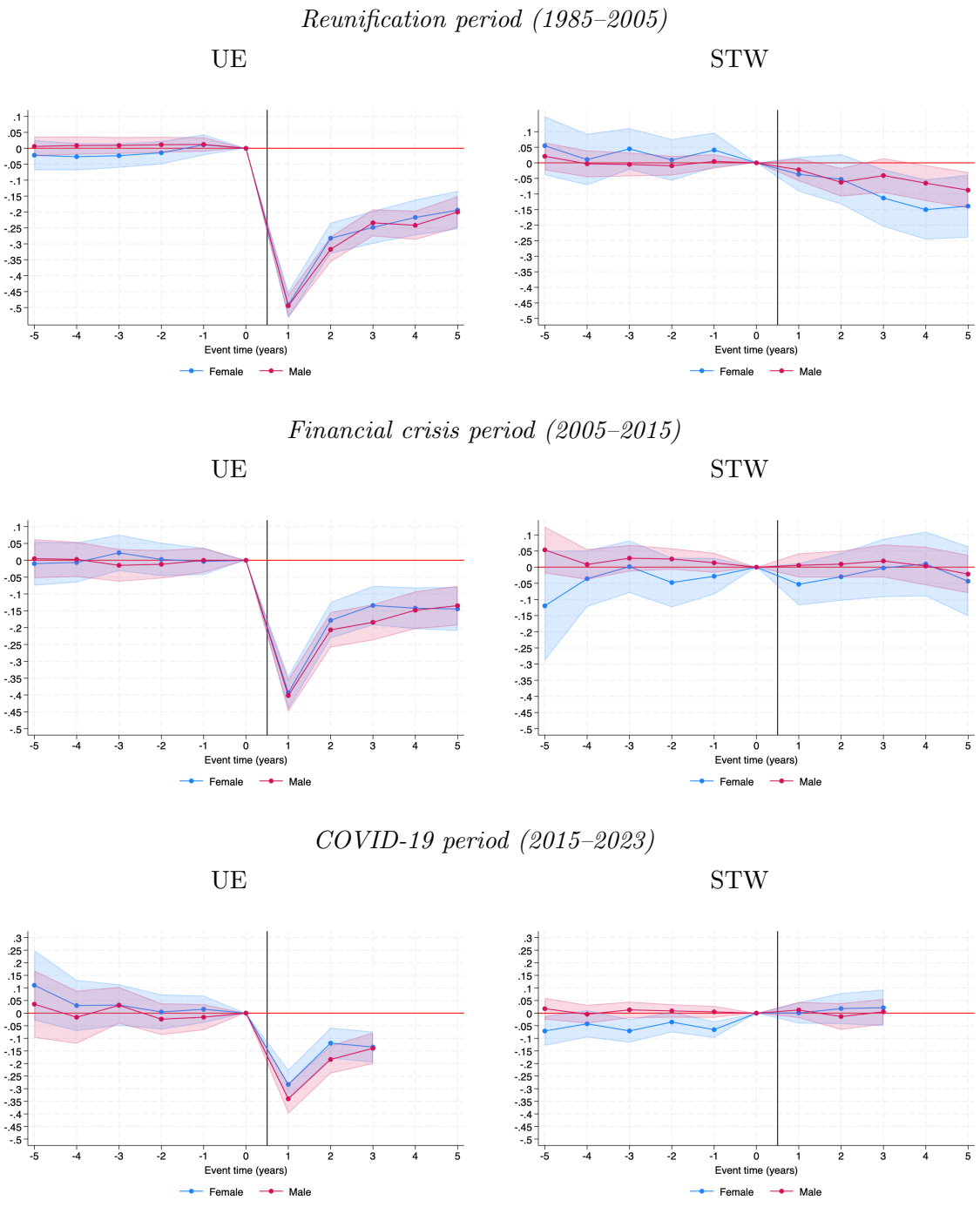
Figure B.17: Life satisfaction



Source. Own calculations based on the SOEP core, v40.
Notes. Plots show the event-time coefficients obtained from estimating equation (1). We normalize the event-study coefficients relative to $K = -1$, the last pre-treatment period in which both groups evolve comparably in well-being outcomes. The estimates therefore capture deviations from this baseline rather than from $K = 0$. Pre-event observations are based on matching three years prior to the event. Shaded areas represent 95% confidence intervals.

B.0.1 Heterogeneity by Gender and time period

Figure B.18: Employment

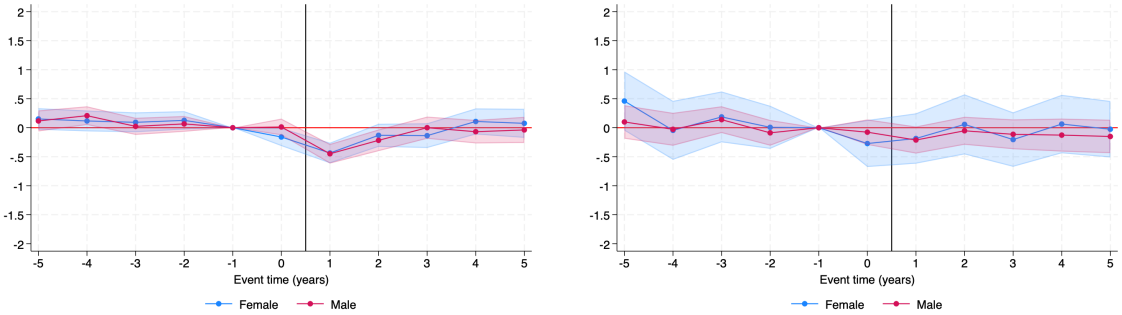


Source. Own calculations based on the SOEP core, v40.
Notes. Plots show event-time coefficients relative to time 0 from estimating equation (1). The estimates are based on matching three years prior to $K = 0$. Shaded areas represent 95% confidence intervals.

Figure B.19: Well-being, life satisfaction

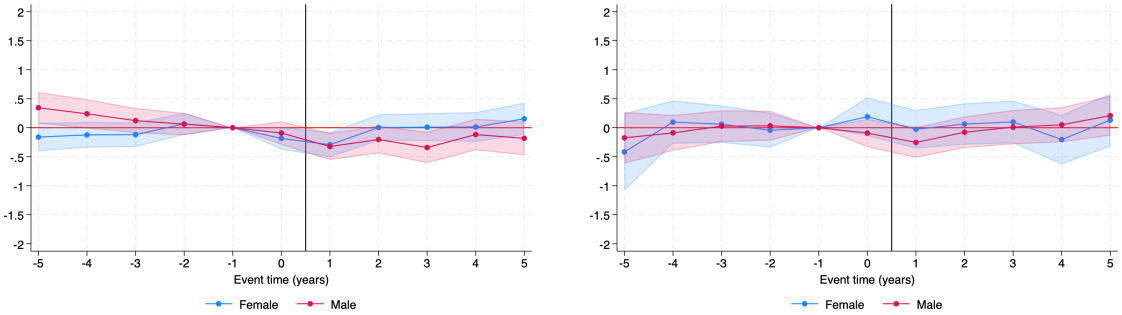
Reunification period (1985–2005)

UE STW



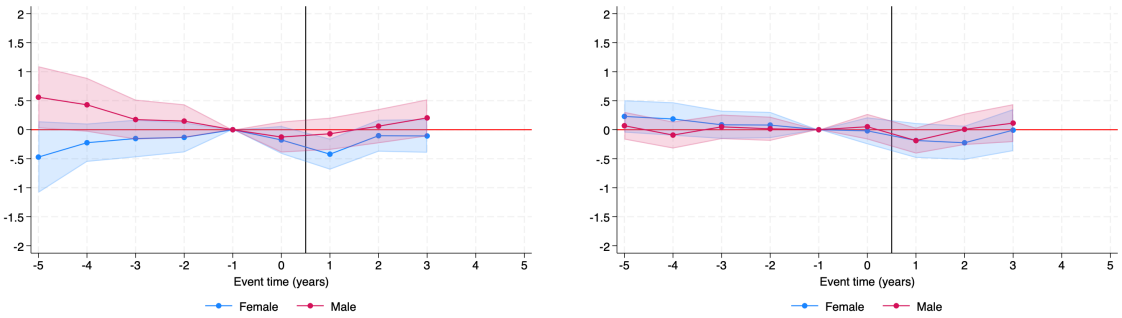
Financial crisis period (2005–2015)

UE STW



COVID-19 period (2015–2023)

UE STW



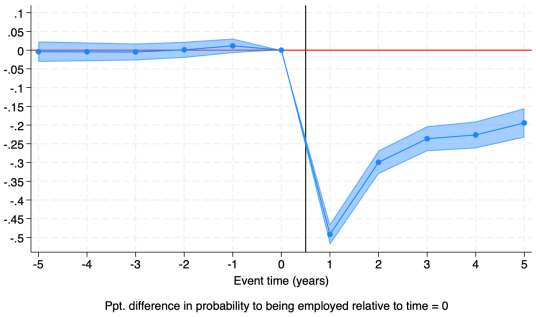
Source. Own calculations based on the SOEP core, v40.
Notes. Plots show event-time coefficients obtained from estimating equation (1). We normalize the event-study coefficients relative to $K = -1$, the last pre-treatment period in which both groups evolve comparably in well-being outcomes. The estimates therefore capture deviations from this baseline rather than from $K = 0$. The estimates are based on matching three years prior to $K = 0$. Shaded areas represent 95% confidence intervals.

C Heterogeneity by time period

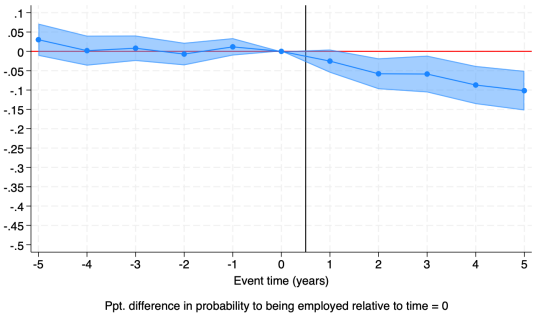
Figure C.20: Employment

Reunification period (1985–2005)

UE

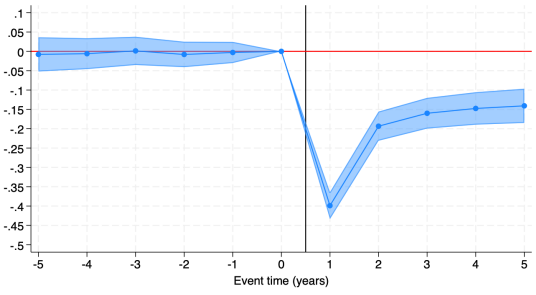


STW



Financial crisis period (2005–2015)

UE



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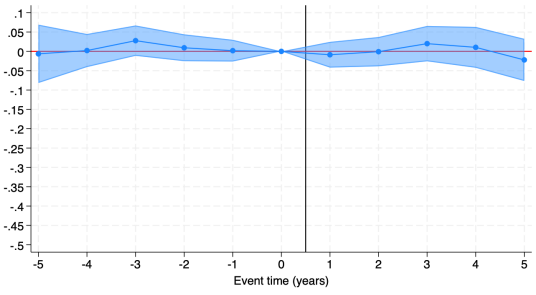
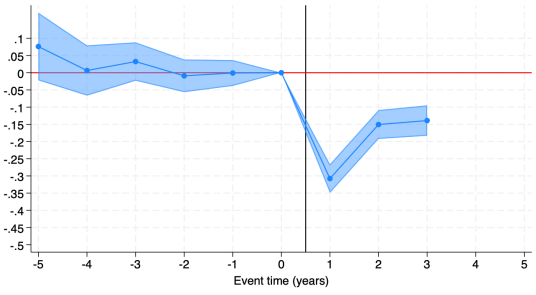


COVID-19 period (2015–2023)

UE



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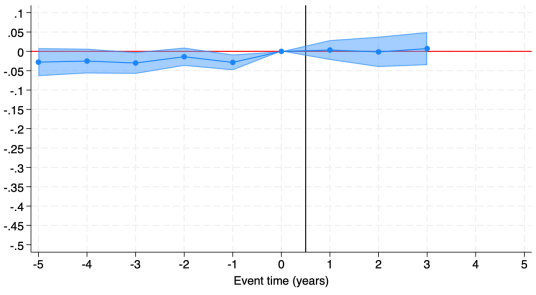
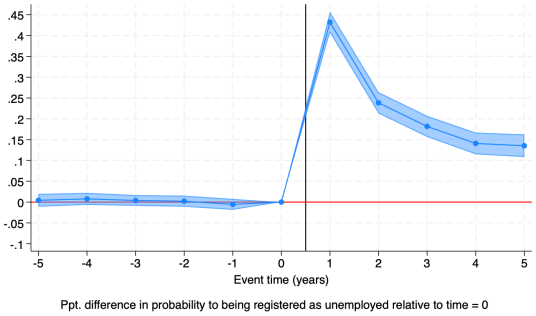


Source. Own calculations based on the SOEP core, v40.
Notes. Plots show event-time coefficients relative to time 0 from estimating equation (1). The estimates are based on matching three years prior to $K = 0$. Shaded areas represent 95% confidence intervals.

Figure C.21: Reception of UE Benefits

Reunification period (1985–2005)

UE



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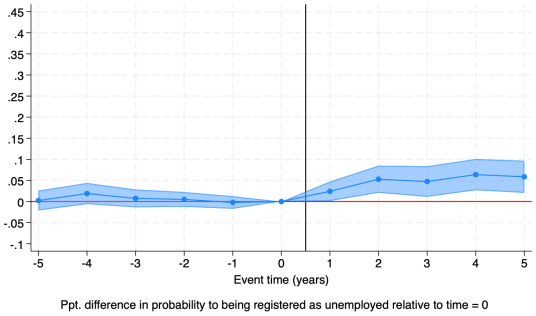
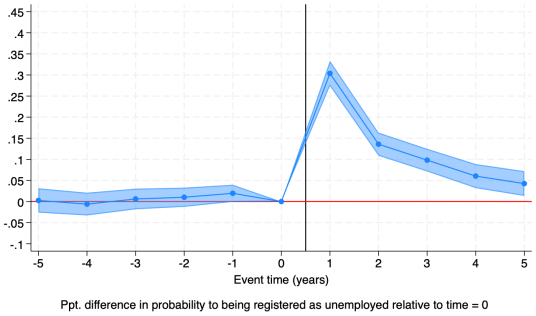


Financial crisis period (2005–2015)

UE



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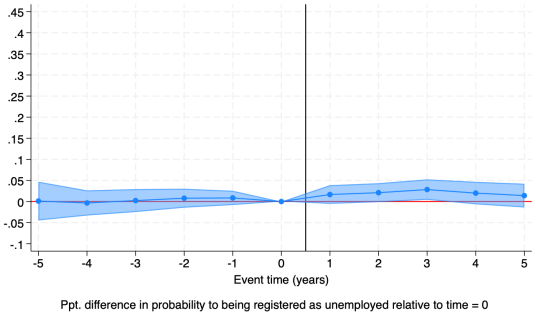
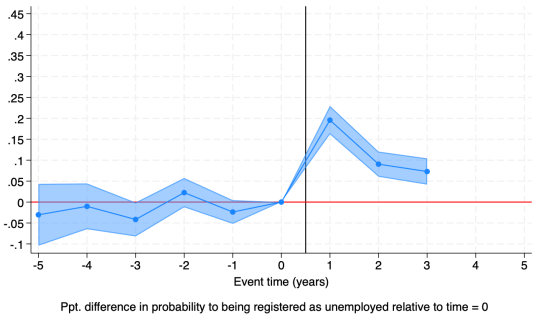


COVID-19 period (2015–2023)

UE



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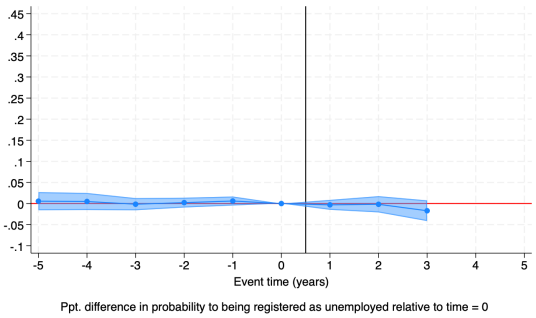
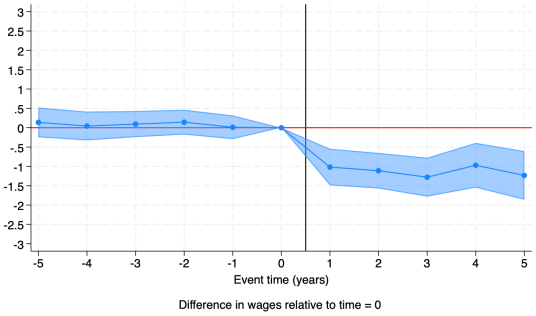


Source. Own calculations based on the SOEP core, v40.
Notes. Plots show event-time coefficients relative to time 0 from estimating equation (1). The estimates are based on matching three years prior to $K = 0$. Shaded areas represent 95% confidence intervals.

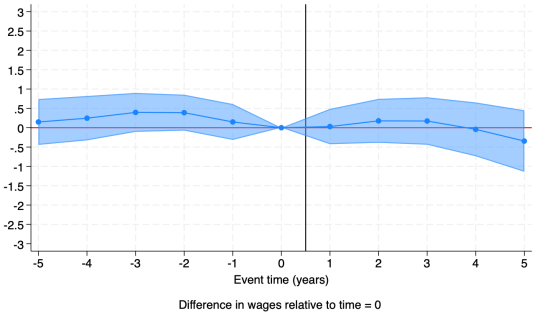
Figure C.22: Real hourly wage, conditional on employment

Reunification period (1985–2005)

UE

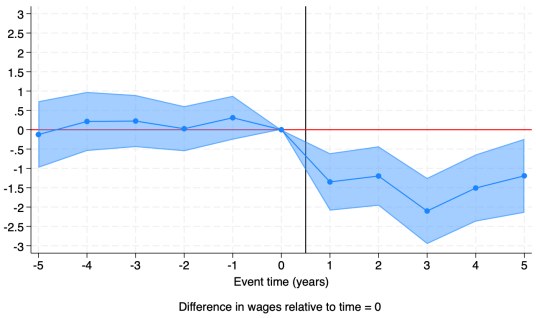


STW



Financial crisis period (2005–2015)

UE



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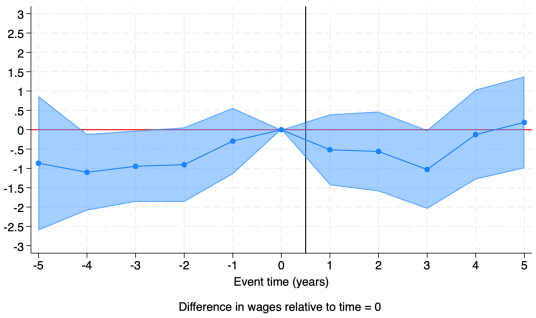
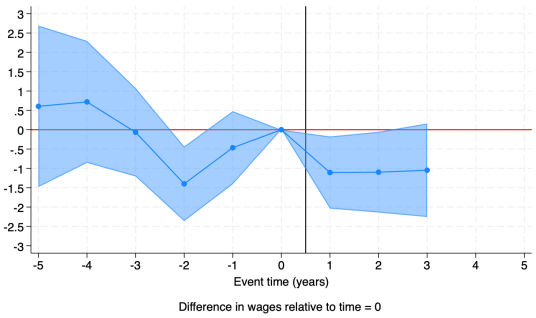


COVID-19 period (2015–2023)

UE



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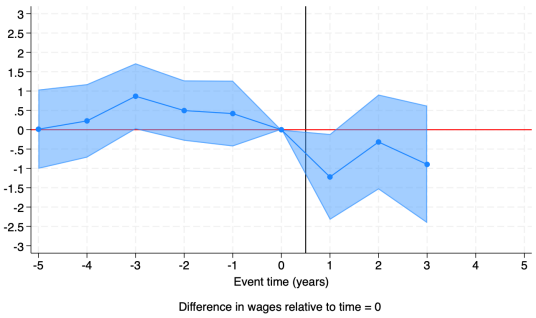
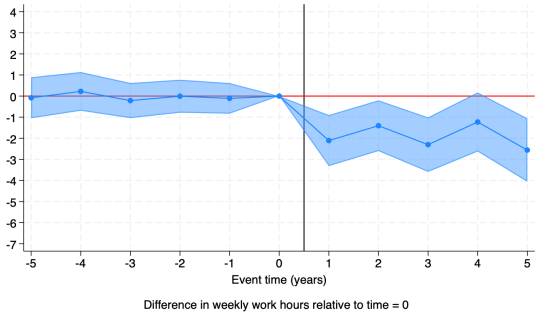


Source. Own calculations based on the SOEP core, v40.
Notes. Plots show event-time coefficients relative to time 0 from estimating equation (1). The estimates are based on matching three years prior to $K = 0$. Shaded areas represent 95% confidence intervals.

Figure C.23: Weekly work hours, conditional on employment

Reunification period (1985–2005)

UE



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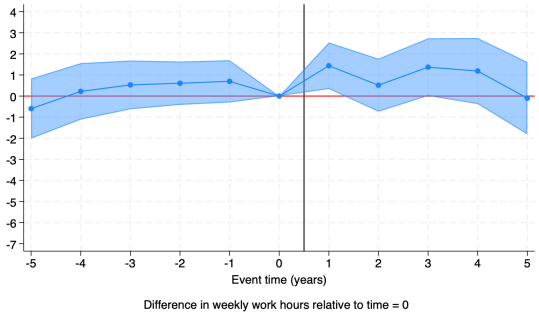
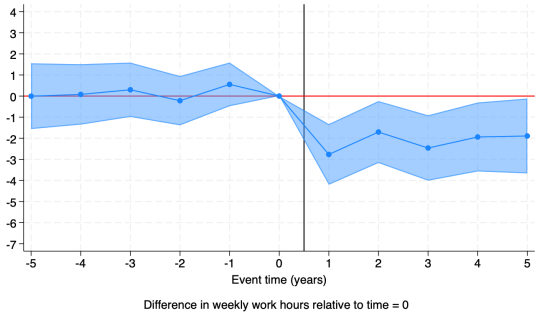


Financial crisis period (2005–2015)

UE



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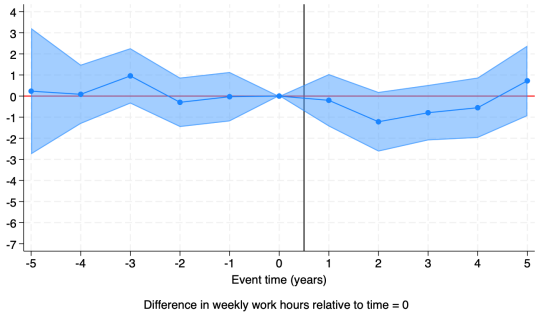
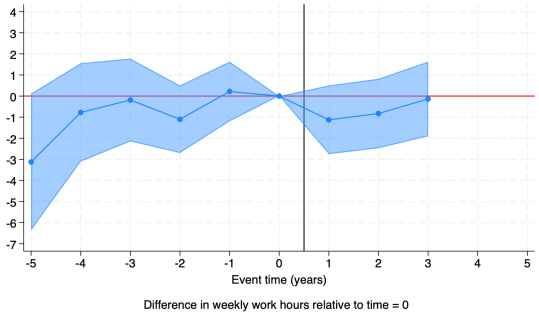
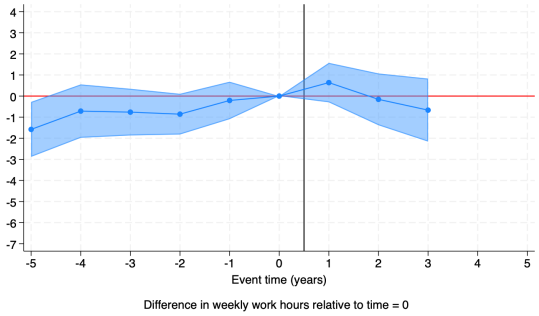


COVID-19 period (2015–2023)

UE

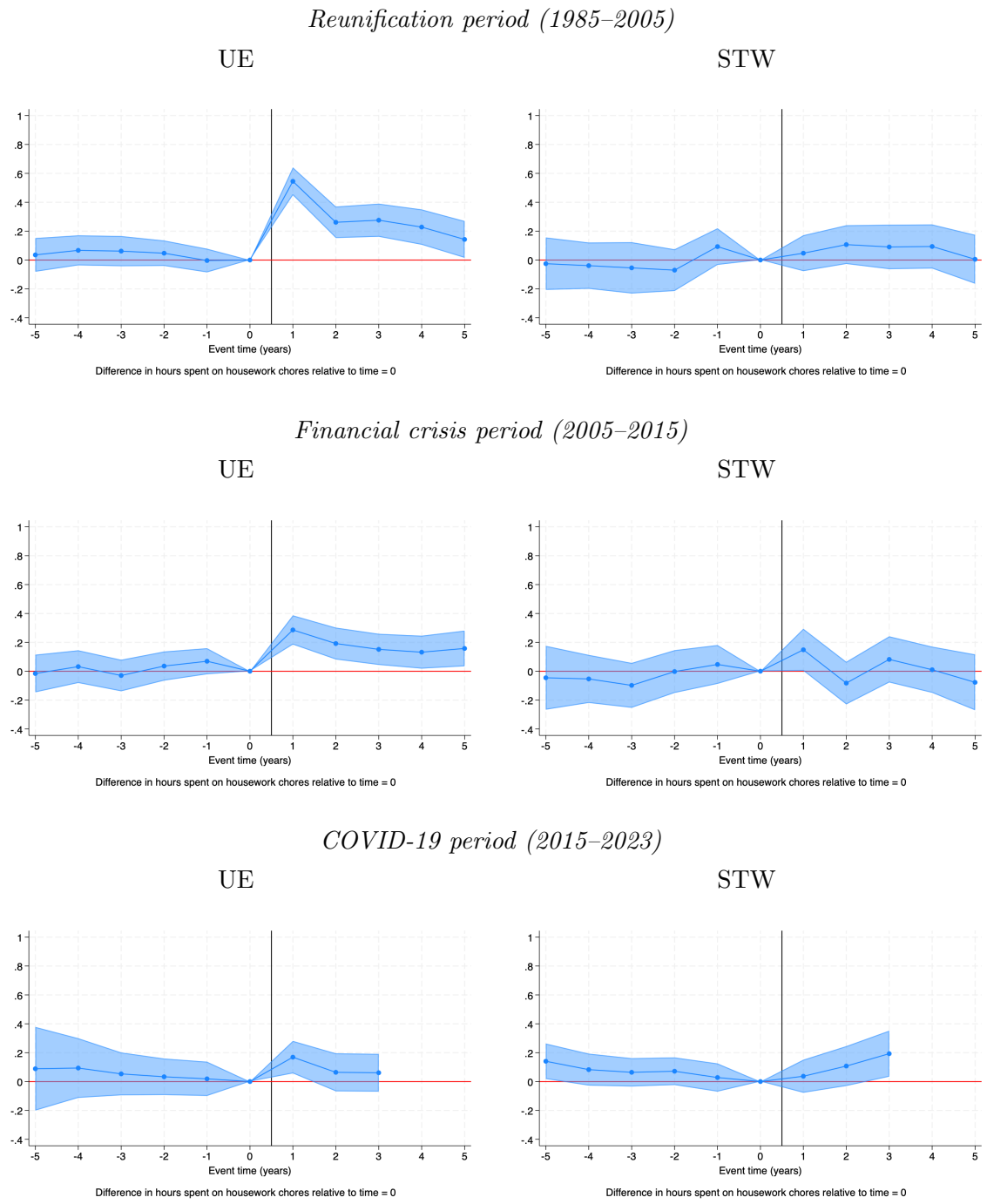


STW



Source. Own calculations based on the SOEP core, v40.
Notes. Plots show event-time coefficients relative to time 0 from estimating equation (1). The estimates are based on matching three years prior to $K = 0$. Shaded areas represent 95% confidence intervals.

Figure C.24: Housework in hours per workday

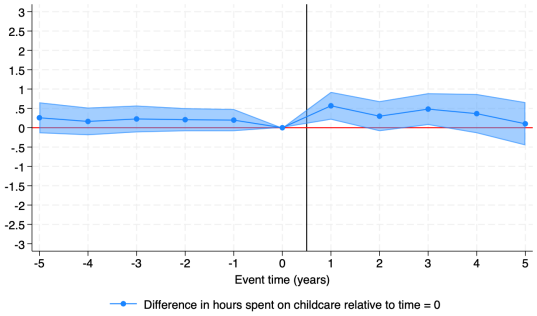


Source. Own calculations based on the SOEP core, v40.
Notes. Plots show event-time coefficients relative to time 0 from estimating equation (1). The estimates are based on matching three years prior to $K = 0$. Shaded areas represent 95% confidence intervals.

Figure C.25: Childcare in hours per workday

Reunification period (1985–2005)

UE



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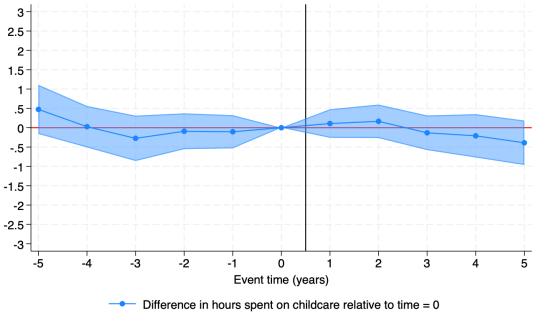
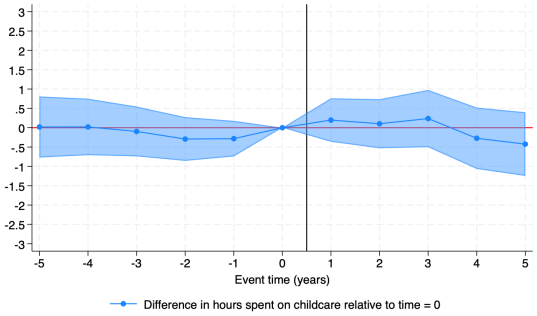


Financial crisis period (2005–2015)

UE



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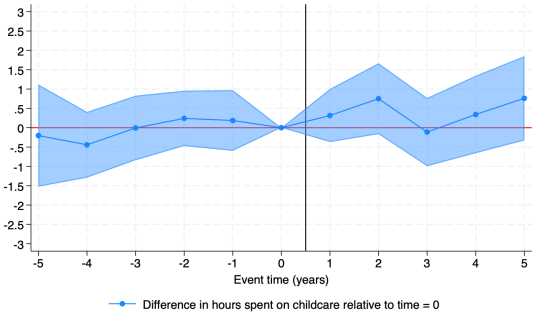
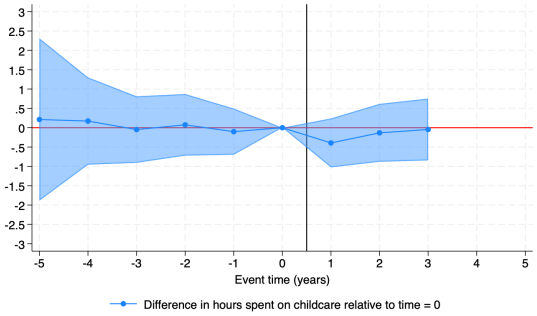


COVID-19 period (2015–2023)

UE



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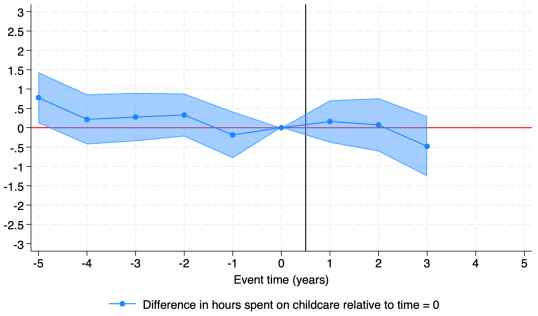
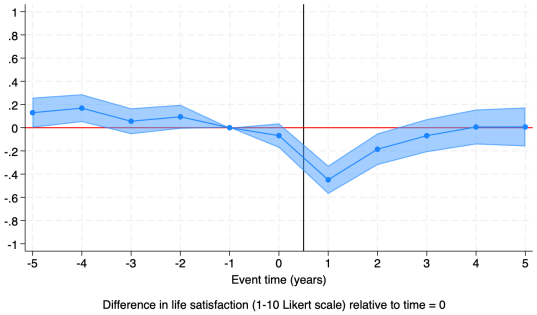


Source. Own calculations based on the SOEP core, v40.
Notes. Plots show event-time coefficients relative to time 0 from estimating equation (1). The estimates are based on matching three years prior to $K = 0$. Shaded areas represent 95% confidence intervals.

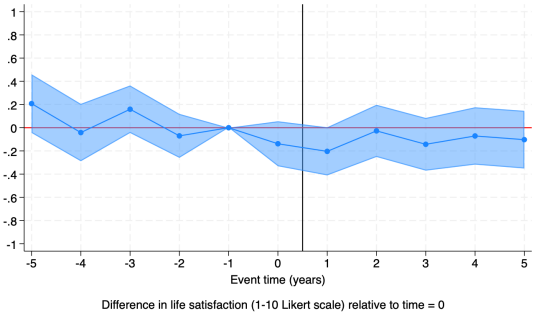
Figure C.26: Well-being, life satisfaction

Reunification period (1985–2005)

UE

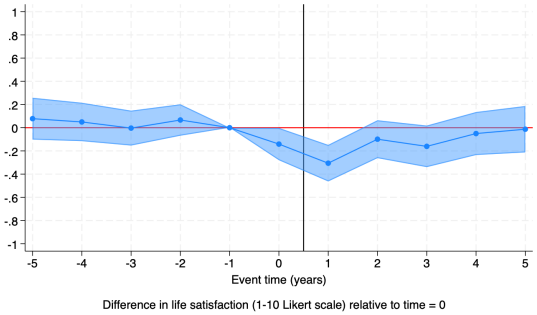


STW

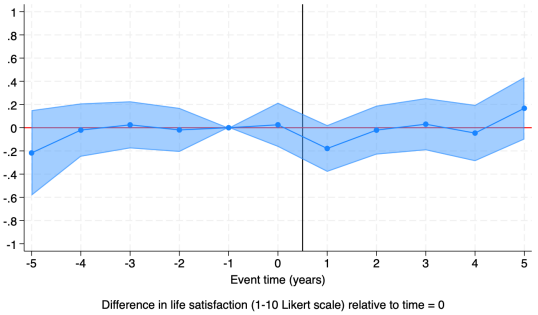


Financial crisis period (2005–2015)

UE

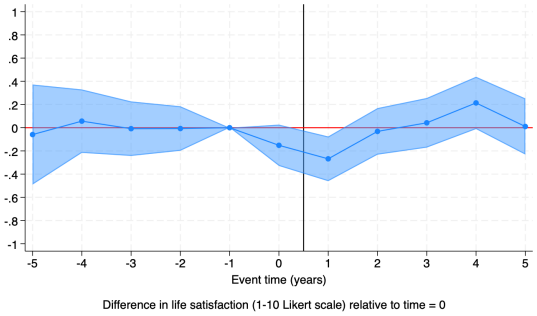


STW

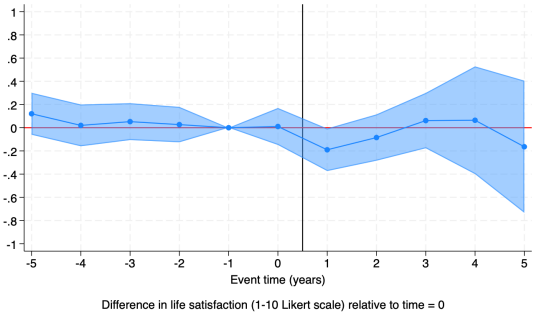


COVID-19 period (2015–2023)

UE



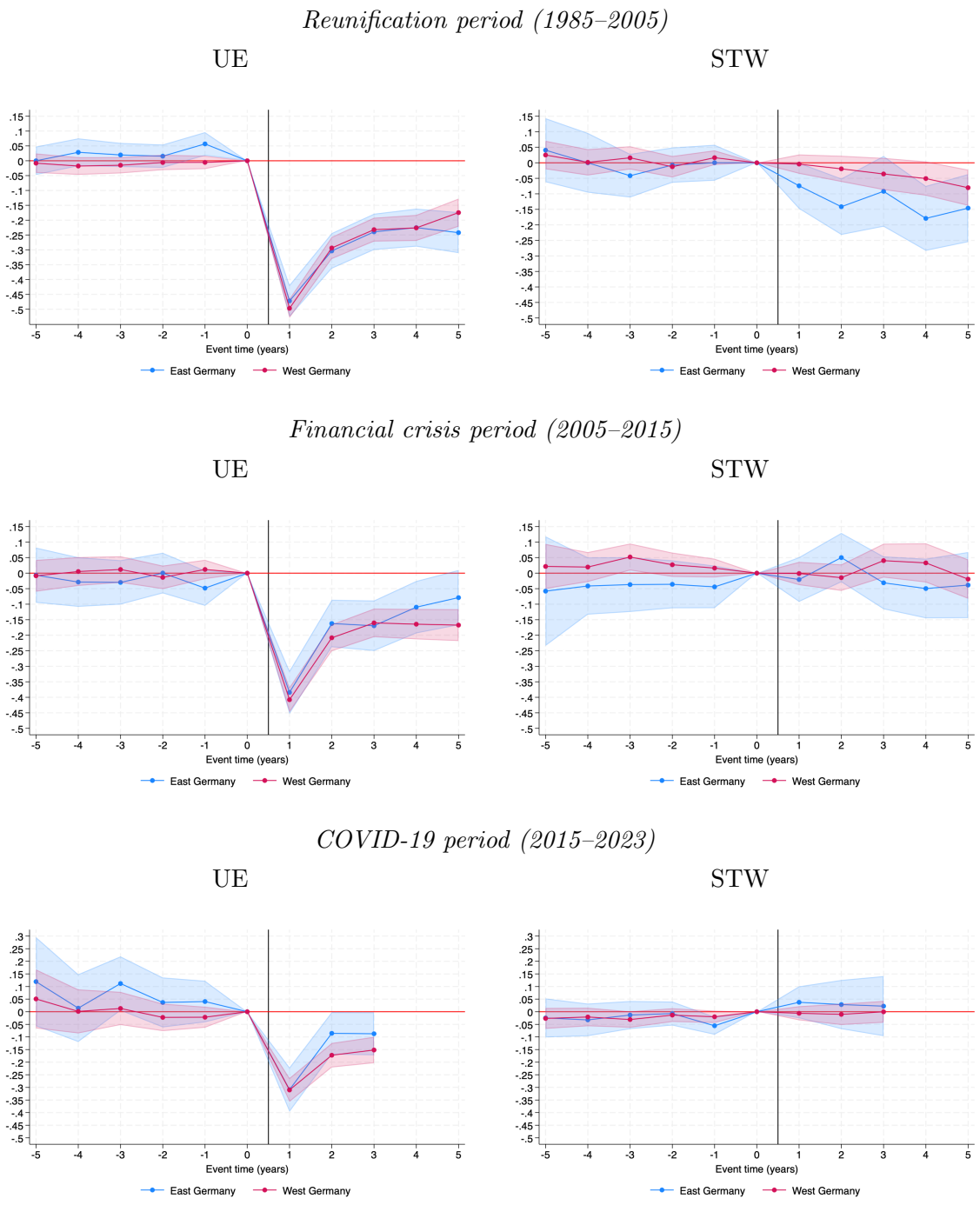
STW



Source. Own calculations based on the SOEP core, v40.
Notes. Plots show event-time coefficients obtained from estimating equation (1). We normalize the event-study coefficients relative to $K = -1$, the last pre-treatment period in which both groups evolve comparably in well-being outcomes. The estimates therefore capture deviations from this baseline rather than from $K = 0$. The estimates are based on matching three years prior to $K = 0$. Shaded areas represent 95% confidence intervals.

C.1 Heterogeneity by Region

Figure C.27: Employment

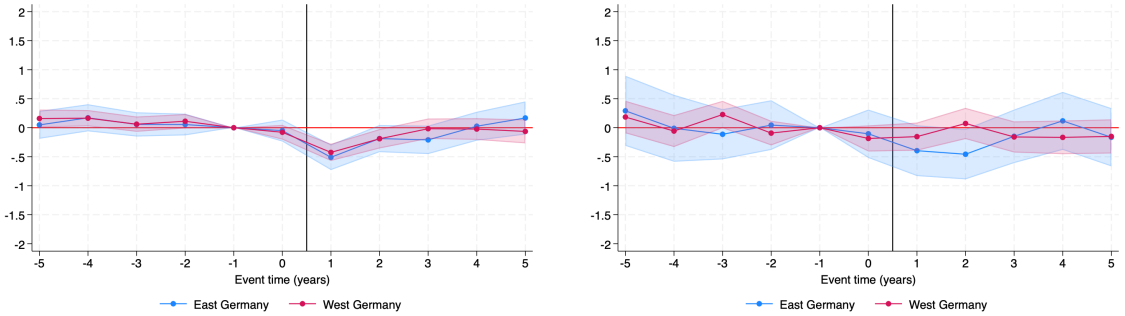


Source. Own calculations based on the SOEP core, v40.
Notes. Plots show event-time coefficients relative to time 0 from estimating equation (1). The estimates are based on matching three years prior to $K = 0$. Shaded areas represent 95% confidence intervals.

Figure C.28: Well-being, life satisfaction

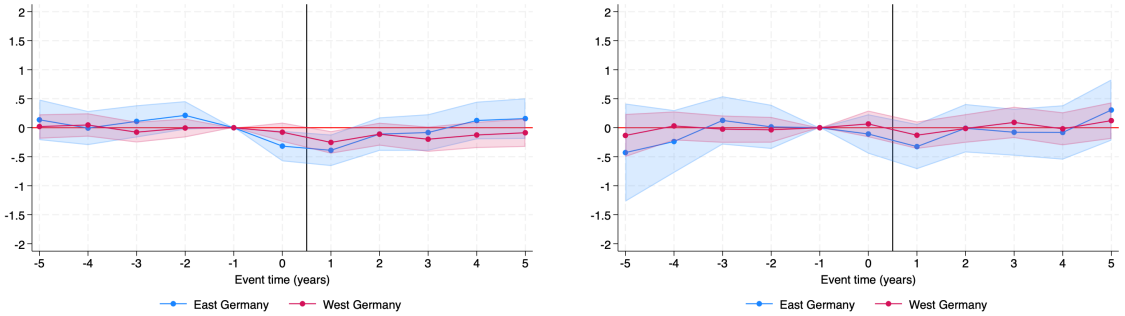
Reunification period (1985–2005)

UE STW



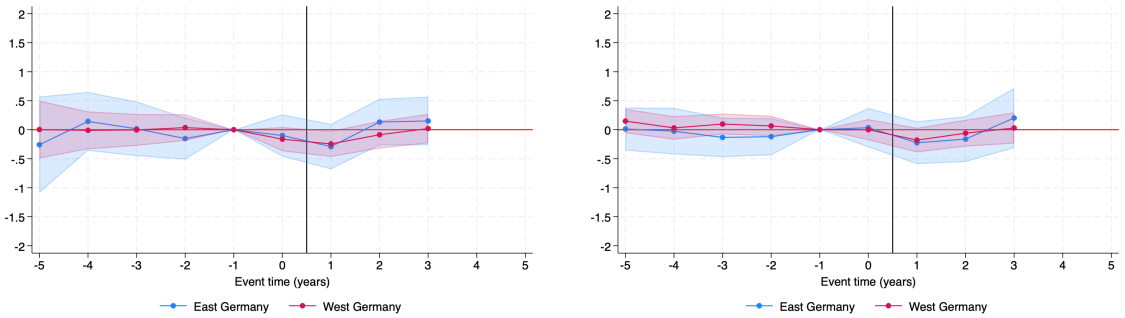
Financial crisis period (2005–2015)

UE STW



COVID-19 period (2015–2023)

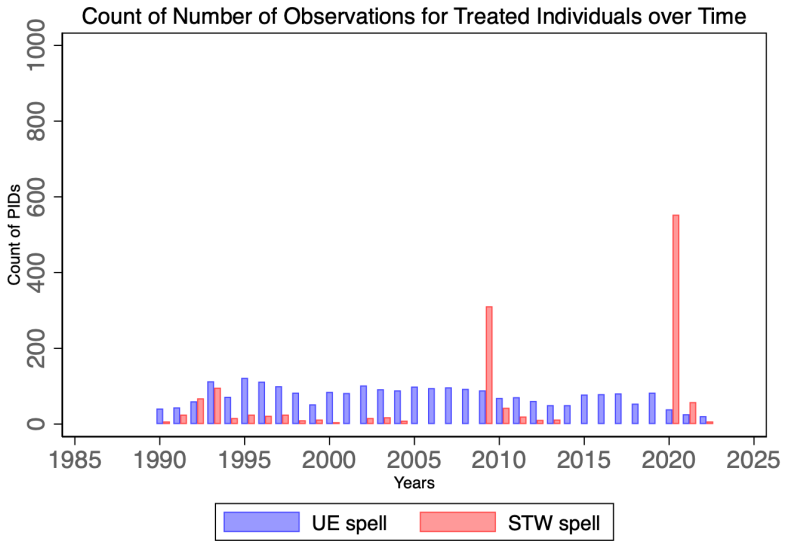
UE STW



Source. Own calculations based on the SOEP core, v40.
Notes. Plots show event-time coefficients obtained from estimating equation (1). We normalize the event-study coefficients relative to $K = -1$, the last pre-treatment period in which both groups evolve comparably in well-being outcomes. The estimates therefore capture deviations from this baseline rather than from $K = 0$. The estimates are based on matching three years prior to $K = 0$. Shaded areas represent 95% confidence intervals.

D Robustness - Matching on five years prior

Figure D.29: UE and STW in the SOEP, 1990 to 2022 - Matching on 5 years



Source. Own calculations based on the SOEP core, v40.

Table D.3: Number of Observations by Group and Event Time (5-Year Matching)

K	Control (UE)	Treatment (UE)	Control (STW)	Treatment (STW)
-5	2,484	2,484	1,369	1,369
-4	2,484	2,484	1,369	1,369
-3	2,484	2,484	1,369	1,369
-2	2,484	2,484	1,369	1,369
-1	2,484	2,484	1,369	1,369
0	2,484	2,484	1,369	1,369
1	2,215	2,473	1,142	1,316
2	2,004	2,227	985	1,130
3	1,824	2,050	805	969
4	1,630	1,845	625	606
5	1,428	1,659	585	564

Source. own calculations based on the SOEP core, v40

Table D.4: Matched Control and Treatment Group Characteristics after Propensity Score Matching, measured in $K = 0$

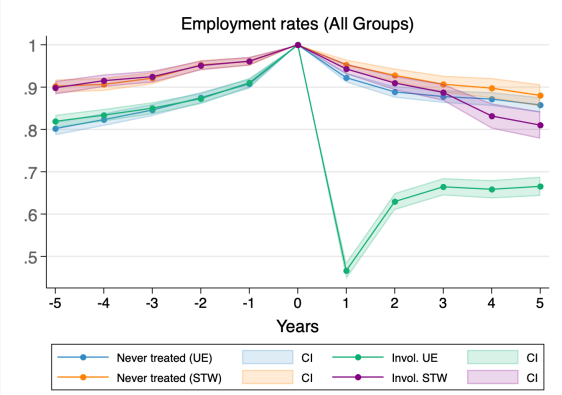
	UE			STW		
	Control	Treatment	p-value	Control	Treatment	p-value
	Mean (SD)	Mean (SD)		Mean (SD)	Mean (SD)	
Demographic characteristics						
Female	0.51 (0.50)	0.50 (0.50)	0.281	0.39 (0.49)	0.38 (0.49)	0.724
Age	42.41 (10.64)	42.66 (11.06)	0.418	41.83 (10.89)	42.63 (11.09)	0.083
Children	0.41 (0.49)	0.39 (0.49)	0.271	0.44 (0.50)	0.41 (0.49)	0.158
Married	0.64 (0.48)	0.65 (0.48)	0.358	0.69 (0.46)	0.68 (0.47)	0.562
Highest Education (CASMIN)	0.15 (0.35)	0.16 (0.36)	0.324	0.17 (0.38)	0.17 (0.38)	0.823
Migration Background	1.29 (0.55)	1.27 (0.53)	0.132	1.33 (0.60)	1.32 (0.54)	0.852
East Germany (Yes/No)	0.26 (0.44)	0.26 (0.44)	0.948	0.20 (0.40)	0.24 (0.43)	0.020
Mean Survey Year	2004.52 (9.26)	2004.76 (8.62)	0.338	2008.70 (10.97)	2008.37 (10.96)	0.470
Labor market information in $K = 0$						
Real Hourly Wage	11.17 (7.83)	11.16 (7.49)	0.969	16.31 (9.81)	16.73 (9.38)	0.296
Weekly Work Hours	30.17 (14.28)	30.26 (14.04)	0.832	36.85 (9.64)	36.90 (7.85)	0.888
Tenure	9.57 (9.33)	9.57 (9.94)	0.999	11.53 (10.09)	12.29 (10.35)	0.075
Industry (11 groups)	8.94 (2.27)	8.89 (2.39)	0.502	7.50 (3.23)	7.71 (3.29)	0.119
Firm Size	6.08 (3.36)	6.00 (3.15)	0.389	7.67 (2.67)	7.73 (2.55)	0.569
Unpaid care work						
Housework Hours per Weekday	1.50 (1.43)	1.46 (1.47)	0.363	0.83 (0.91)	0.78 (0.79)	0.177
Childcare Hours per Weekday	1.31 (3.02)	1.26 (2.83)	0.500	0.81 (1.55)	0.76 (1.49)	0.416
Well-being						
Life Satisfaction (current)	7.06 (1.66)	6.71 (1.77)	< 0.001	7.31 (1.63)	7.11 (1.58)	0.003
Employment Averaged Over Available Years in the Prior Five-Year Window						
Pre-Wage Avg	9.36 (5.97)	9.23 (6.50)	0.474	14.50 (8.14)	14.74 (8.25)	0.477
Pre-Tenure Avg	8.11 (7.23)	8.08 (8.67)	0.904	9.75 (8.05)	10.44 (9.27)	0.055
Pre-Work Hours Avg	27.00 (12.64)	26.76 (13.20)	0.512	35.29 (8.25)	35.34 (7.49)	0.899
Pre-Employment Status Avg	0.88 (0.19)	0.88 (0.21)	0.742	0.95 (0.11)	0.96 (0.11)	0.125
Pre-Registered UE Avg	0.04 (0.11)	0.04 (0.12)	0.937	0.02 (0.07)	0.02 (0.08)	0.568
Observations	2484	2484		1369	1369	

Source. own calculations based on the SOEP core, v40.

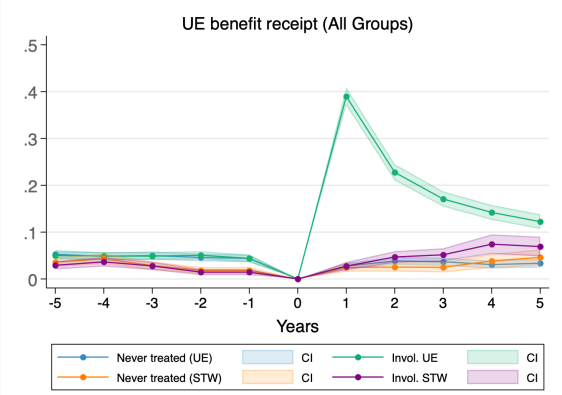
Notes. The table reports means and standard deviations (in parentheses) for treated and matched control individuals measured in $K = 0$, the year prior to the employment shock. Matching is performed separately for involuntary UE and STW using one-to-one nearest-neighbor propensity score matching without replacement and a caliper of 0.01. The variables labeled "Pre-" averages refer to averages computed over all available pre-treatment observations within the window $K \in [-5, 0]$. Because individuals are observed for varying lengths of time prior to treatment, these averages are calculated over the available pre-treatment years and therefore do not necessarily represent full five-year averages for all individuals. The matched sample corresponds to the five-year pre-treatment specification described in Section 4. P-values refer to tests of equality of means between treated and control individuals.

Figure D.30: Raw means in control and treatment groups: Labor market outcomes

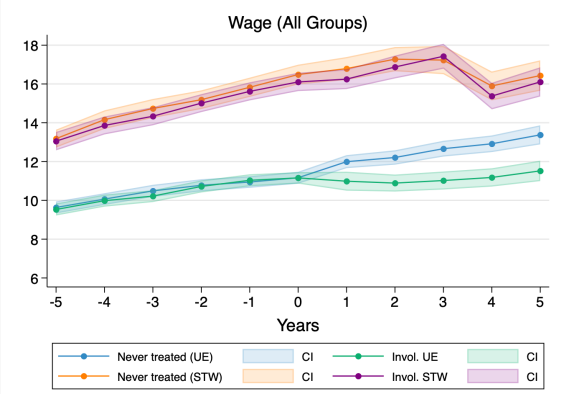
Employment



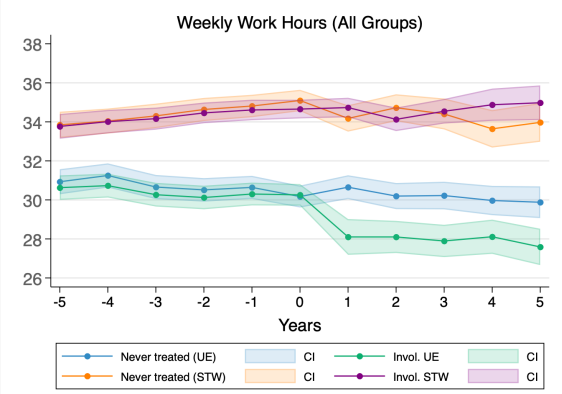
Reception of UE Benefits



Real Hourly Wage (conditional on employment)



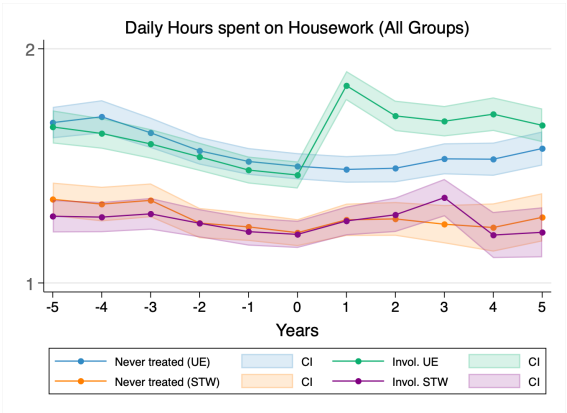
Weekly Work Hours (conditional on employment)



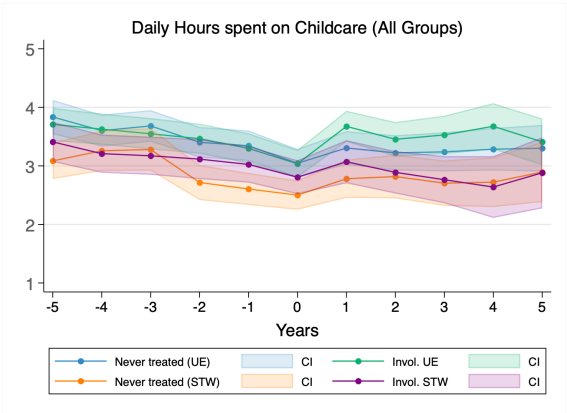
Source. own calculations based on the SOEP core, v40.
Notes. Plots show the raw means over time in our sample for the matched treatment and control individuals in each treatment category. Shaded areas represent 95% confidence intervals.

Figure D.31: Raw means in control and treatment groups: Unpaid care work

Housework in hours per workday

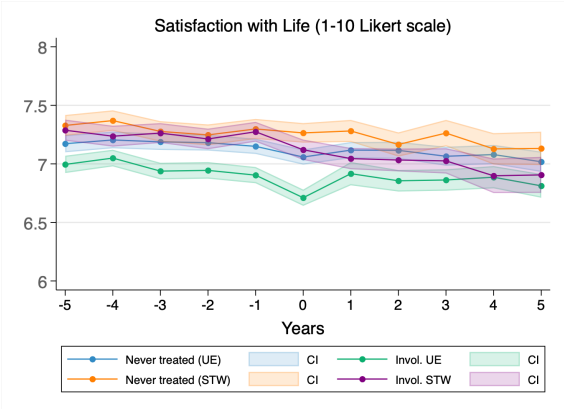


Childcare in hours per workday



Source. own calculations based on the SOEP core, v40.
Notes. Plots show the raw means over time for our sample for hours spent on housework. For childcare the sample is restricted to those having children. Shaded areas represent 95% confidence intervals.

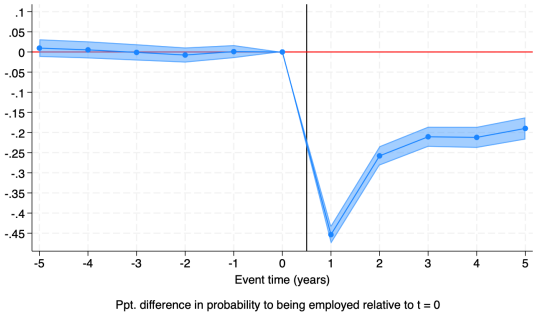
Figure D.32: Raw means in control and treatment groups: Well-being, life satisfaction



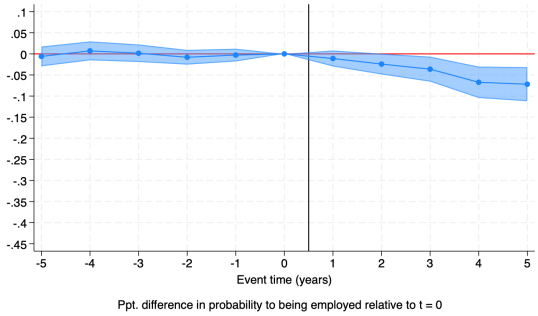
Source. own calculations based on the SOEP core, v40.
Notes. Plots show the raw means over time in our sample. Shaded areas represent 95% confidence intervals.

Figure D.33: Employment

Involuntary UE



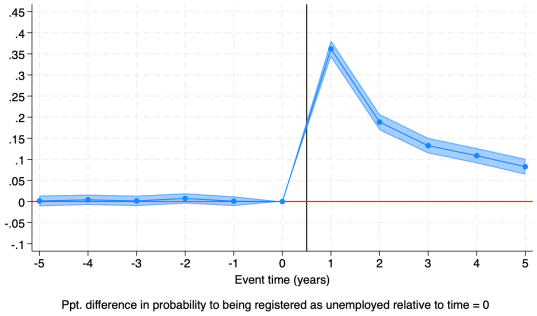
Involuntary STW



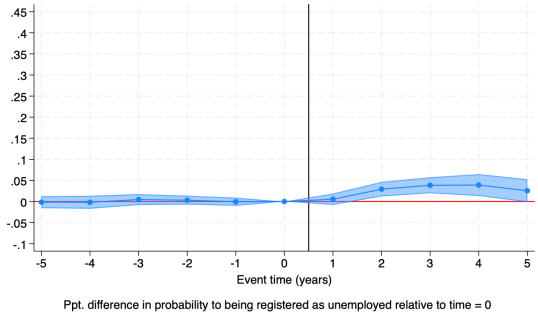
Source. own calculations based on the SOEP core, v40.
Notes. Plots show the event-time coefficients obtained from estimating equation (1). The estimates are based on a panel where the pre-event observations are based on matching 5 years prior to the event. Shaded areas represent 95% confidence intervals.

Figure D.34: Reception of UE Benefits

Involuntary UE



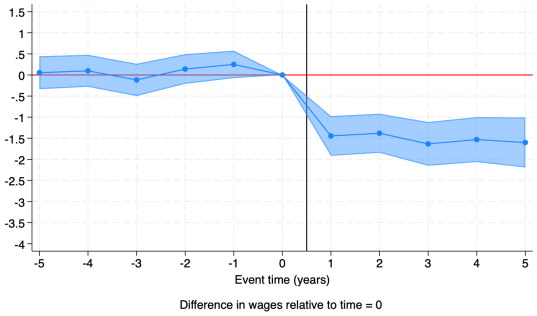
Involuntary STW



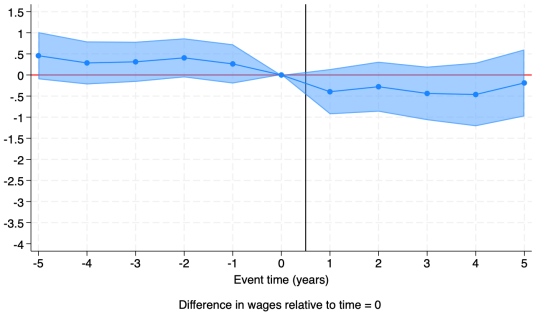
Source. own calculations based on the SOEP core, v40.
Notes. Plots show the event-time coefficients obtained from estimating equation (1). The estimates are based on a panel where the pre-event observations are based on matching 5 years prior to the event. Shaded areas represent 95% confidence intervals.

Figure D.35: Real hourly wage, conditional on employment

Involuntary UE



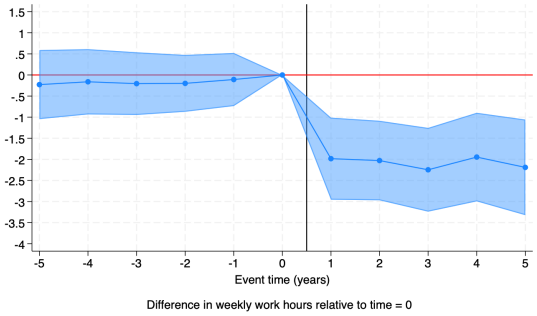
Involuntary STW



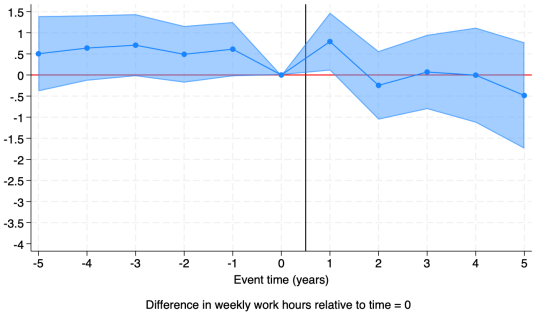
Source. own calculations based on the SOEP core, v40.
Notes. Plots show the event-time coefficients obtained from estimating equation (1). The estimates are based on a panel where the pre-event observations are based on matching 5 years prior to the event. Shaded areas represent 95% confidence intervals.

Figure D.36: Weekly work hours, conditional on employment

Involuntary UE



Involuntary STW

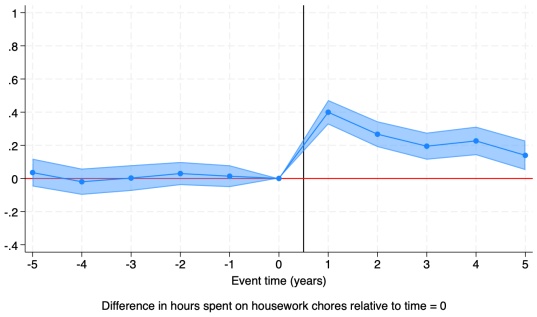


Source. own calculations based on the SOEP core, v40.
Notes. Plots show the event-time coefficients obtained from estimating equation (1). The estimates are based on a panel where the pre-event observations are based on matching 5 years prior to the event. Shaded areas represent 95% confidence intervals.

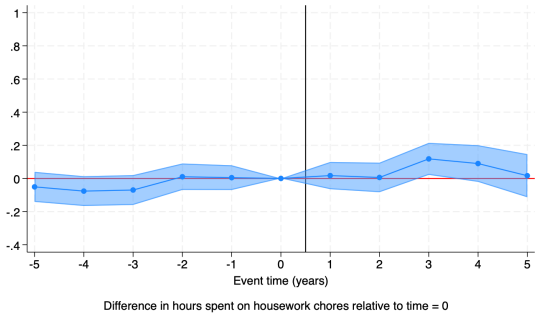
D.1 Unpaid care work outcomes

Figure D.37: Housework in hours per workday

Involuntary UE



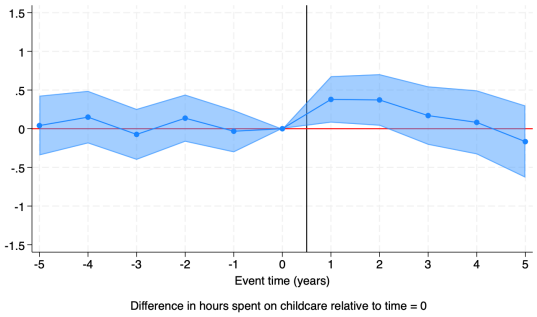
Involuntary STW



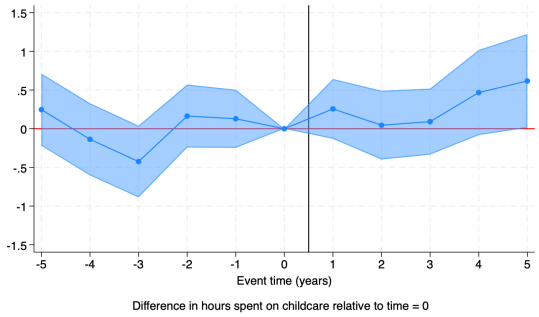
Source. own calculations based on the SOEP core, v40.
Notes. Plots show the event-time coefficients obtained from estimating equation (1). The estimates are based on a panel where the pre-event observations are based on matching 5 years prior to the event. Shaded areas represent 95% confidence intervals.

Figure D.38: Childcare in hours per workday

Involuntary UE



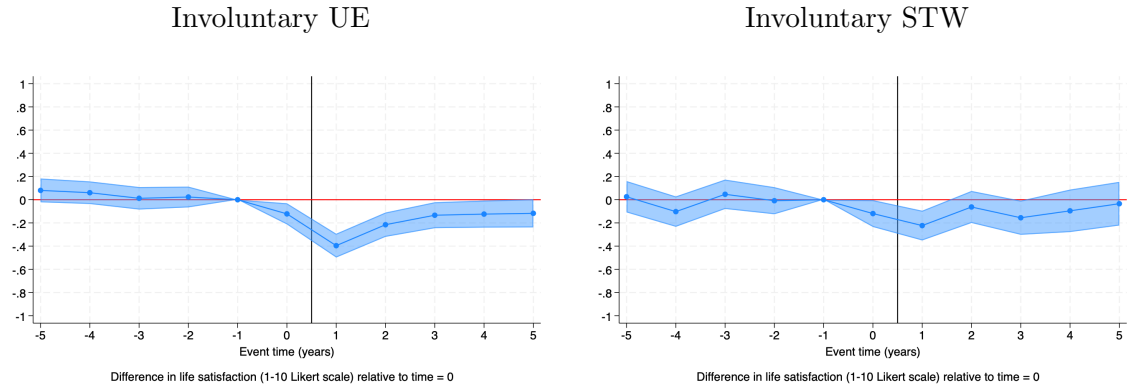
Involuntary STW



Source. own calculations based on the SOEP core, v40.
Notes. Plots show the event-time coefficients obtained from estimating equation (1). The estimates are based on a panel where the pre-event observations are based on matching 5 years prior to the event. Shaded areas represent 95% confidence intervals.

D.2 Life satisfaction outcome

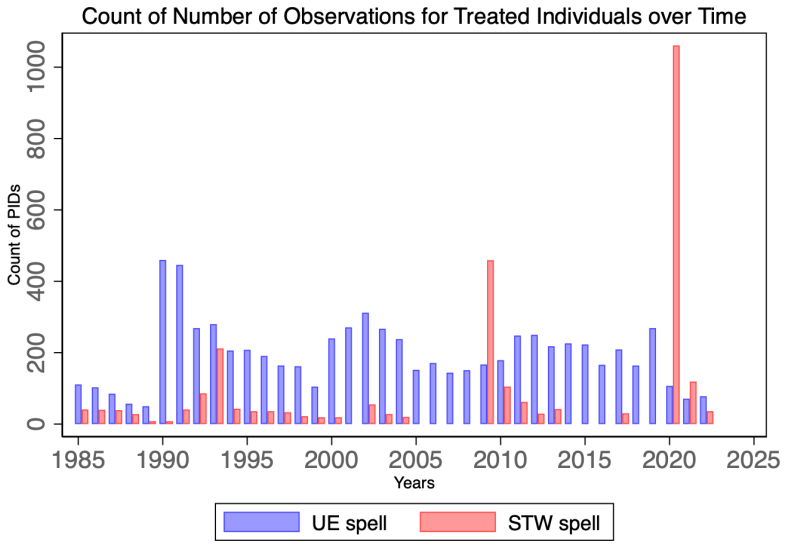
Figure D.39: Life satisfaction



Source. own calculations based on the SOEP core, v40.
Notes. Plots show the event-time coefficients obtained from estimating equation (1). We normalize the event-study coefficients relative to $K = -1$, the last pre-treatment period in which both groups evolve comparably in well-being outcomes. The estimates therefore capture deviations from this baseline rather than from $K = 0$. The estimates are based on a panel where the pre-event observations are based on matching 5 years prior to the event. Shaded areas represent 95% confidence intervals.

E Robustness - Matching on one year prior

Figure E.40: UE and STW in the SOEP, 1985 to 2022 - Matching on one year



Source. own calculations based on the SOEP core, v40.

Table E.5: Number of Observations by Group and Event Time (1-Year Matching)

K	Control (UE)	Treatment (UE)	Control (STW)	Treatment (STW)
-5	3,164	2,711	1,375	1,499
-4	3,635	3,193	1,489	1,600
-3	4,261	3,817	1,761	1,987
-2	4,934	4,769	1,994	2,126
-1	5,811	5,840	2,213	2,319
0	7,418	7,418	2,757	2,757
1	6,000	7,363	2,034	2,619
2	5,171	6,413	1,751	2,159
3	4,514	5,823	1,474	1,832
4	3,948	5,304	1,168	1,190
5	3,493	4,794	1,083	1,104

Source. own calculations based on the SOEP core, v40

Table E.6: Matched Control and Treatment Group Characteristics after Propensity Score Matching, measured in $K = 0$

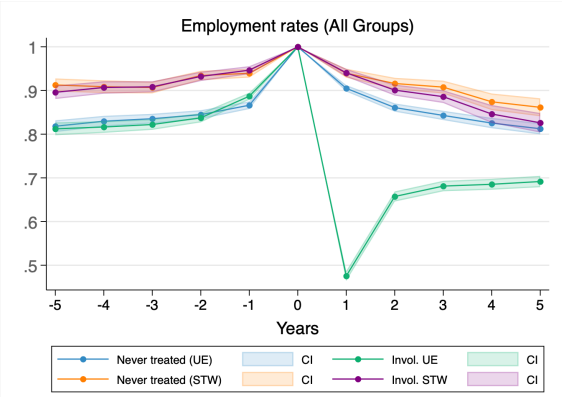
	UE			STW		
	Control	Treatment	p-value	Control	Treatment	p-value
	Mean (SD)	Mean (SD)		Mean (SD)	Mean (SD)	
Demographic characteristics						
Female	0.48 (0.50)	0.47 (0.50)	0.107	0.36 (0.48)	0.37 (0.48)	0.557
Age	38.90 (12.18)	38.71 (11.99)	0.349	41.27 (11.13)	41.08 (11.28)	0.541
Children	0.43 (0.50)	0.44 (0.50)	0.655	0.39 (0.49)	0.41 (0.49)	0.105
Married	0.59 (0.49)	0.59 (0.49)	0.653	0.64 (0.48)	0.62 (0.48)	0.372
Highest Education (CASMIN)	0.16 (0.36)	0.15 (0.36)	0.165	0.17 (0.38)	0.19 (0.39)	0.080
Migration Background	1.30 (0.54)	1.30 (0.55)	0.426	1.33 (0.57)	1.36 (0.56)	0.042
East Germany (Yes/No)	0.35 (0.48)	0.34 (0.47)	0.153	0.22 (0.41)	0.22 (0.41)	0.720
Mean Survey Year	2002.88 (10.70)	2003.03 (10.15)	0.371	2009.31 (11.28)	2009.64 (11.52)	0.287
Labor market information in $K = 0$						
Real Hourly Wage	9.37 (7.81)	9.24 (7.09)	0.307	15.23 (9.94)	14.85 (9.17)	0.143
Weekly Work Hours	31.62 (14.43)	31.76 (13.74)	0.533	35.17 (10.53)	34.46 (10.05)	0.010
Tenure	7.24 (8.47)	7.08 (8.74)	0.272	10.19 (10.17)	9.78 (9.81)	0.133
Industry (11 groups)	9.01 (2.22)	8.93 (2.33)	0.025	7.99 (2.96)	7.78 (3.09)	0.011
Firm Size	6.02 (3.40)	6.04 (3.16)	0.745	7.27 (2.94)	7.18 (2.91)	0.214
Unpaid care work						
Housework Hours per Weekday	1.45 (1.50)	1.41 (1.40)	0.097	1.20 (1.20)	1.23 (1.11)	0.420
Childcare Hours per Weekday	1.16 (2.75)	1.22 (2.63)	0.140	1.07 (2.39)	1.19 (2.55)	0.079
Well-being						
Life Satisfaction (current)	7.07 (1.73)	6.77 (1.87)	< 0.001	7.34 (1.61)	7.25 (1.65)	0.058
Employment Averaged Over Available Years in the Prior One-Year Window						
Pre-Wage Avg	7.95 (5.90)	7.80 (6.21)	0.133	13.29 (8.14)	13.02 (8.22)	0.209
Pre-Tenure Avg	6.55 (6.82)	6.41 (7.92)	0.249	8.77 (8.45)	8.51 (8.74)	0.267
Pre-Work Hours Avg	28.91 (13.01)	28.92 (13.52)	0.961	32.98 (10.11)	32.32 (10.43)	0.017
Pre-Employment Status Avg	0.90 (0.18)	0.90 (0.20)	0.514	0.94 (0.13)	0.94 (0.15)	0.871
Pre-Registered UE Avg	0.04 (0.11)	0.03 (0.11)	0.122	0.02 (0.08)	0.02 (0.08)	0.378
Observations	7418	7418		2757	2757	

Source. own calculations based on the SOEP core, v40.

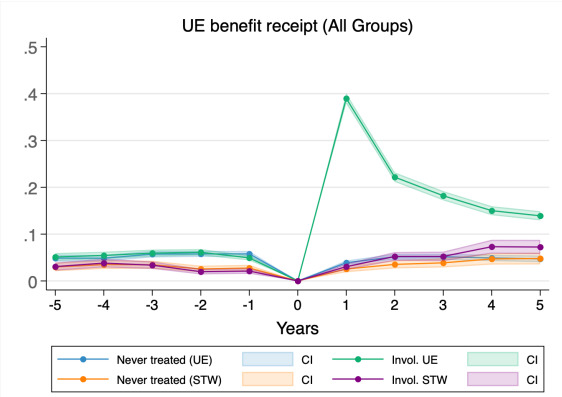
Notes. The table reports means and standard deviations (in parentheses) for treated and matched control individuals measured in $K = 0$, the year prior to the employment shock. Matching is performed separately for involuntary UE and STW using one-to-one nearest-neighbor propensity score matching without replacement and a caliper of 0.01. The variables labeled "Pre-" averages refer to averages computed over all available pre-treatment observations within the window $K \in [-1, 0]$. Because individuals are observed for varying lengths of time prior to treatment, these averages are calculated over the available pre-treatment years and therefore do not necessarily represent full averages for all individuals. The matched sample corresponds to the one-year pre-treatment specification described in Section 4. P-values refer to tests of equality of means between treated and control individuals.

Figure E.41: Raw means in control and treatment groups: Labor market outcomes

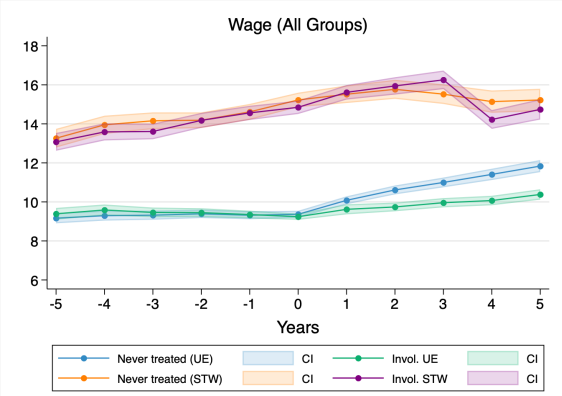
Employment



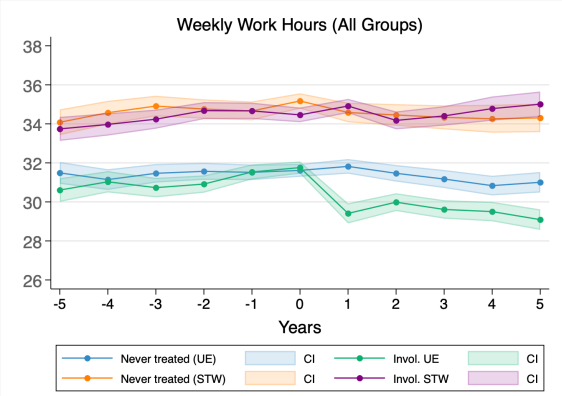
Reception of UE Benefits



Real Hourly Wage (conditional on employment)



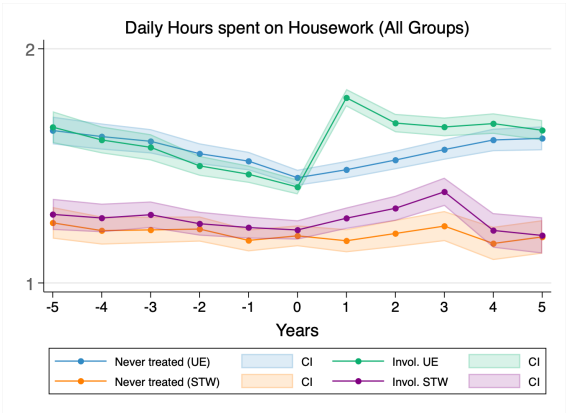
Weekly Work Hours (conditional on employment)



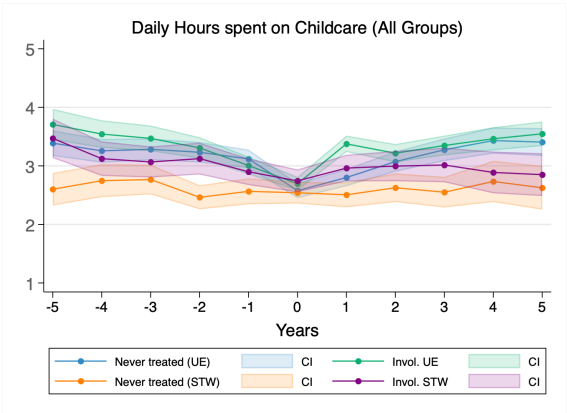
Source. own calculations based on the SOEP core, v40.
Notes. Plots show the raw means over time in our sample for the matched treatment and control individuals in each treatment category. Shaded areas represent 95% confidence intervals.

Figure E.42: Raw means in control and treatment groups: Unpaid care work

Housework in hours per workday

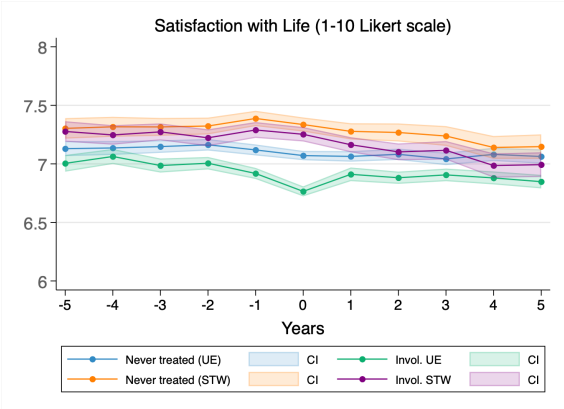


Childcare in hours per workday



Source. own calculations based on the SOEP core, v40.
Notes. Plots show the raw means over time for our sample for hours spent on housework. For childcare the sample is restricted to those having children. Shaded areas represent 95% confidence intervals.

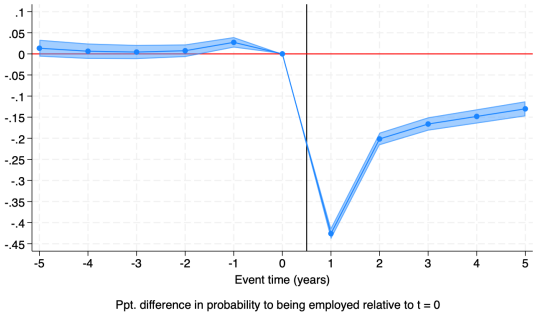
Figure E.43: Raw means in control and treatment groups: Well-being, life satisfaction



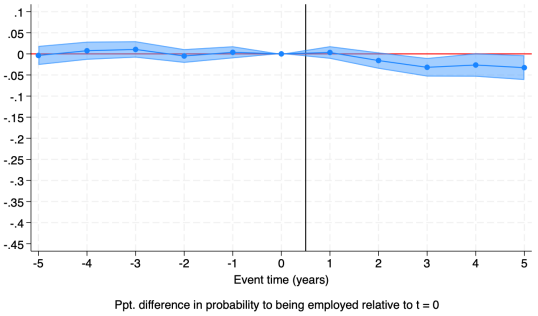
Source. own calculations based on the SOEP core, v40.
Notes. Plots show the raw means over time in our sample. Shaded areas represent 95% confidence intervals.

Figure E.44: Employment

Involuntary UE



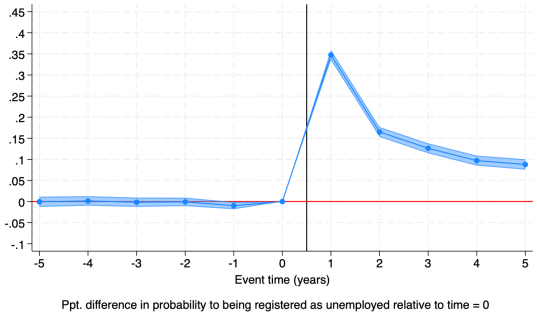
Involuntary STW



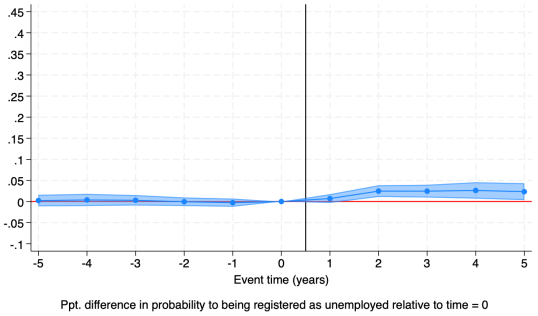
Source. own calculations based on the SOEP core, v40.
Notes. Plots show the event-time coefficients obtained from estimating equation (1). The estimates are based on a panel where the pre-event observations are based on matching one year prior to the event. Shaded areas represent 95% confidence intervals.

Figure E.45: Reception of UE Benefits

Involuntary UE



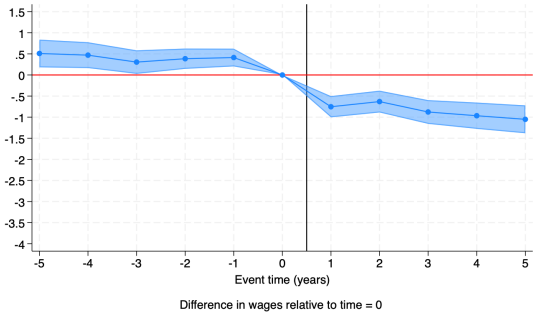
Involuntary STW



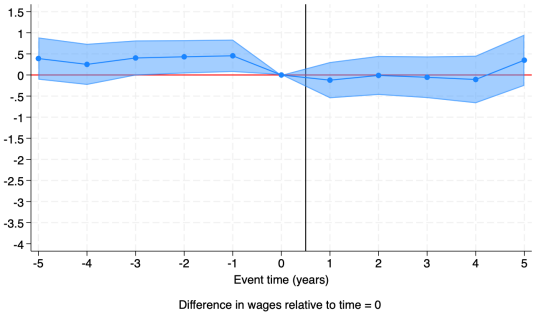
Source. own calculations based on the SOEP core, v40.
Notes. Plots show the event-time coefficients obtained from estimating equation (1). The estimates are based on a panel where the pre-event observations are based on matching one year prior to the event. Shaded areas represent 95% confidence intervals.

Figure E.46: Real hourly wage, conditional on employment

Involuntary UE



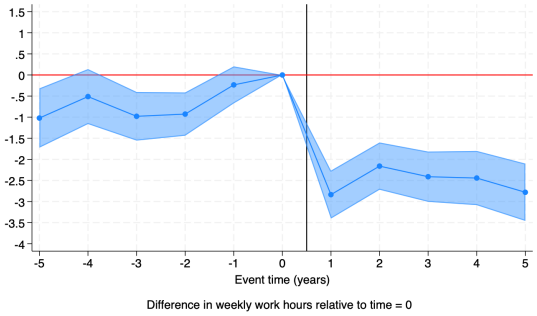
Involuntary STW



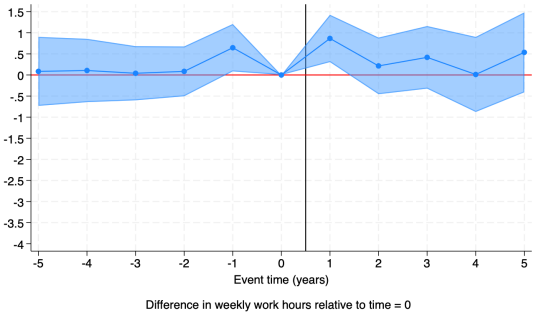
Source. own calculations based on the SOEP core, v40.
Notes. Plots show the event-time coefficients obtained from estimating equation (1). The estimates are based on a panel where the pre-event observations are based on matching one year prior to the event. Shaded areas represent 95% confidence intervals.

Figure E.47: Weekly work hours, conditional on employment

Involuntary UE



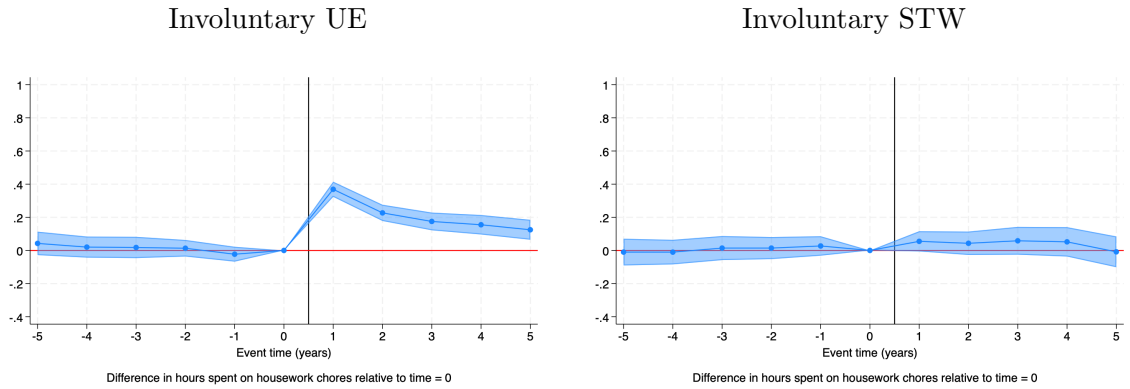
Involuntary STW



Source. own calculations based on the SOEP core, v40.
Notes. Plots show the event-time coefficients obtained from estimating equation (1). The estimates are based on a panel where the pre-event observations are based on matching one year prior to the event. Shaded areas represent 95% confidence intervals.

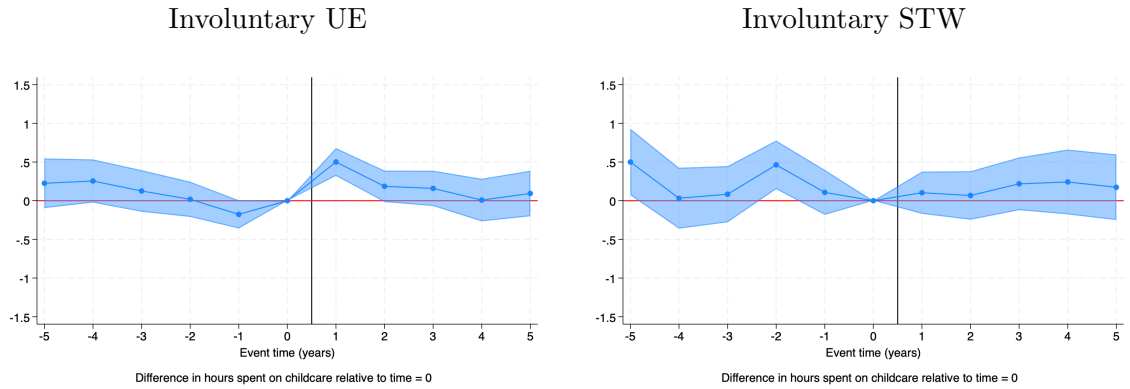
E.1 Unpaid care work outcomes

Figure E.48: Housework in hours per workday



Source. own calculations based on the SOEP core, v40.
Notes. Plots show the event-time coefficients obtained from estimating equation (1). The estimates are based on a panel where the pre-event observations are based on matching one year prior to the event. Shaded areas represent 95% confidence intervals.

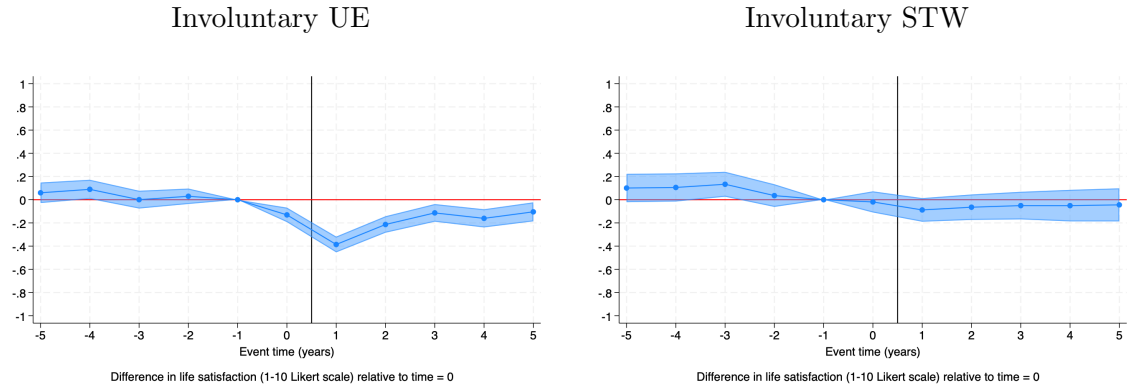
Figure E.49: Childcare in hours per workday



Source. own calculations based on the SOEP core, v40.
Notes. Plots show the event-time coefficients obtained from estimating equation (1). The estimates are based on a panel where the pre-event observations are based on matching one year prior to the event. Shaded areas represent 95% confidence intervals.

E.2 Life satisfaction outcome

Figure E.50: Life satisfaction



Source. own calculations based on the SOEP core, v40.
Notes. Plots show the event-time coefficients obtained from estimating equation (1). We normalize the event-study coefficients relative to $K = -1$, the last pre-treatment period in which both groups evolve comparably in well-being outcomes. The estimates therefore capture deviations from this baseline rather than from $K = 0$. The estimates are based on a panel where the pre-event observations are based on matching one year prior to the event. Shaded areas represent 95% confidence intervals.

F Robustness - Matching on five years prior - IAB only

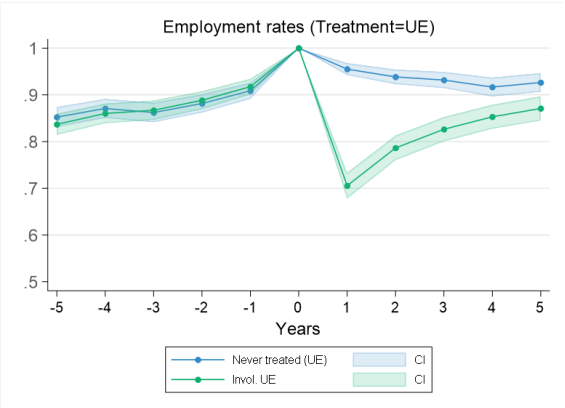
Table F.7: Number of Observations by Matching Period and Group

K	Control (UE)	Treatment (UE)	Control (STW)	Treatment (STW)
		5-Year Matching, IAB only		
-5	1,030	1,030	953	953
-4	1,030	1,030	953	953
-3	1,030	1,030	953	953
-2	1,030	1,030	953	953
-1	1,030	1,030	953	953
0	1,030	1,030	953	953
1	979	1,029	915	953
2	893	927	390	297
3	804	812	385	296
4	708	728	376	288
5	639	643	346	281

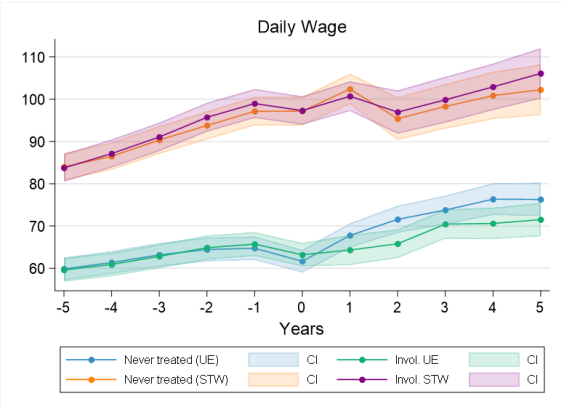
Source. own calculations based on the SOEP core, v40

Figure F.51: Raw means in control and treatment groups

Employment



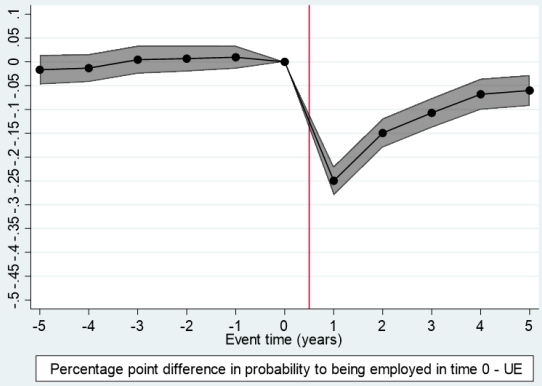
Daily Wages



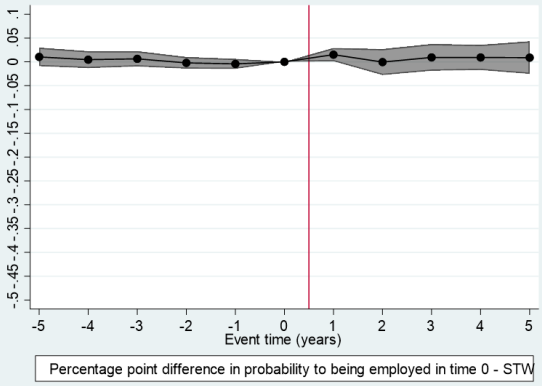
Source. own calculations based on the SOEP core, v38 and SOEP-ADIAB
Notes. Plots show the event-time coefficients obtained from estimating equation (1) with additional firm fixed effects. The estimates are based on a panel where the pre-event observations are constructed using matching over the five years prior to treatment using only IAB administrative data. Shaded areas represent 95% confidence intervals.

Figure F.52: Employment

Involuntary UE



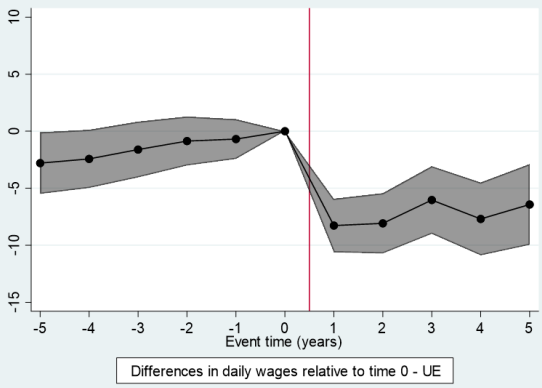
Involuntary STW



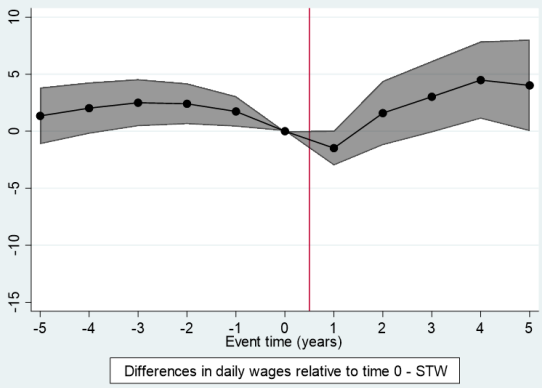
Source. own calculations based on the SOEP core, v38 and SOEP-ADIAB
Notes. Plots show the event-time coefficients obtained from estimating equation (1) with additional firm fixed effects. The estimates are based on a panel where the pre-event observations are constructed using matching over the five years prior to treatment using only IAB administrative data. Shaded areas represent 95% confidence intervals.

Figure F.53: Daily wage

Involuntary UE



Involuntary STW

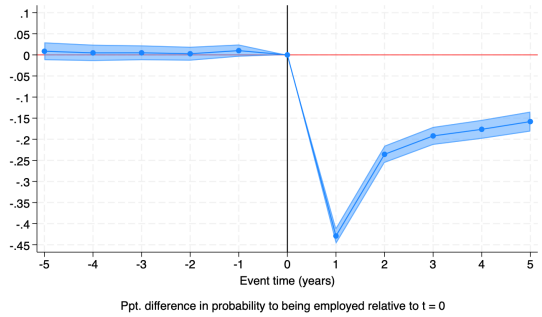


Source. own calculations based on the SOEP core, v38 and SOEP-ADIAB
Notes. Plots show the event-time coefficients obtained from estimating equation (1) with additional firm fixed effects. The estimates are based on a panel where the pre-event observations are constructed using matching over the five years prior to treatment using only IAB administrative data. Shaded areas represent 95% confidence intervals.

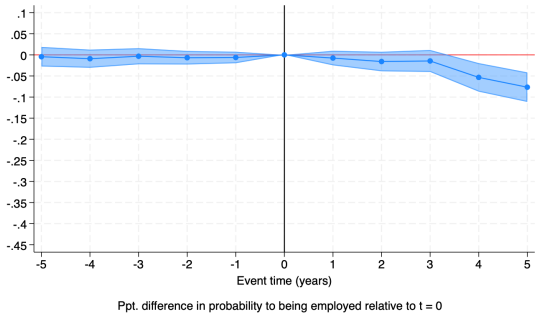
G Robustness - Sun and Abrahams

Figure G.54: Employment

Involuntary UE



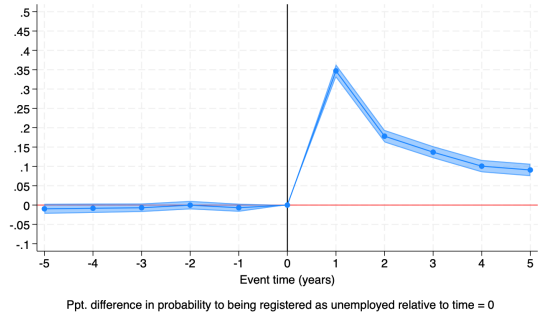
Involuntary STW



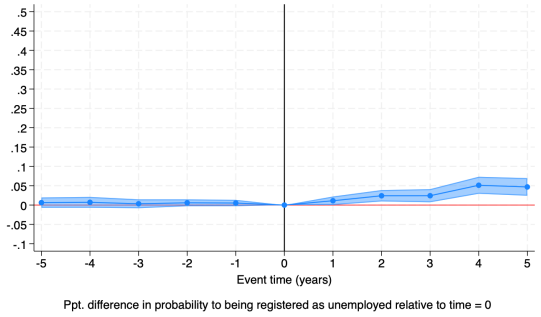
Source. own calculations based on the SOEP core, v40.
Notes. Plots show event-time coefficients estimated using the method proposed by Sun and Abraham (2021). This estimator constructs cohort-specific treatment effects for each event time and then aggregates them across cohorts. Unlike the conventional two-way fixed effects event study in equation (1), this approach avoids biased comparisons that can arise under staggered treatment timing when treatment effects differ across cohorts. The estimates are based on a panel where the pre-event observations are based on matching three years prior to the event. Shaded areas represent 95% confidence intervals.

Figure G.55: Reception of UE Benefits

Involuntary UE



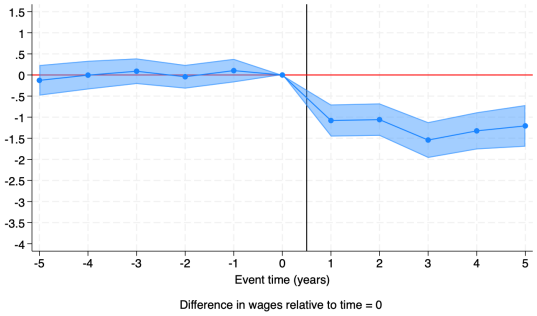
Involuntary STW



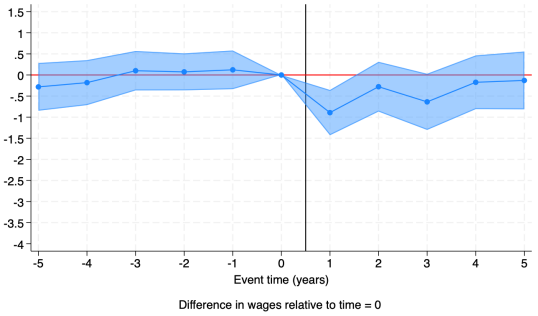
Source. own calculations based on the SOEP core, v40.
Notes. Plots show event-time coefficients estimated using the method proposed by Sun and Abraham (2021). This estimator constructs cohort-specific treatment effects for each event time and then aggregates them across cohorts. Unlike the conventional two-way fixed effects event study in equation (1), this approach avoids biased comparisons that can arise under staggered treatment timing when treatment effects differ across cohorts. The estimates are based on a panel where the pre-event observations are based on matching three years prior to the event. Shaded areas represent 95% confidence intervals.

Figure G.56: Real hourly wage, conditional on employment

Involuntary UE



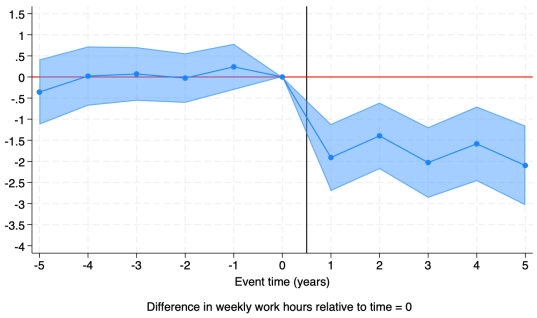
Involuntary STW



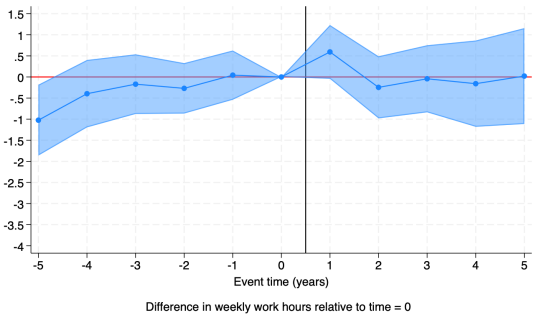
Source. own calculations based on the SOEP core, v40.
Notes. Plots show event-time coefficients estimated using the method proposed by Sun and Abraham (2021). This estimator constructs cohort-specific treatment effects for each event time and then aggregates them across cohorts. Unlike the conventional two-way fixed effects event study in equation (1), this approach avoids biased comparisons that can arise under staggered treatment timing when treatment effects differ across cohorts. The estimates are based on a panel where the pre-event observations are based on matching three years prior to the event. Shaded areas represent 95% confidence intervals.

Figure G.57: Weekly work hours, conditional on employment

Involuntary UE



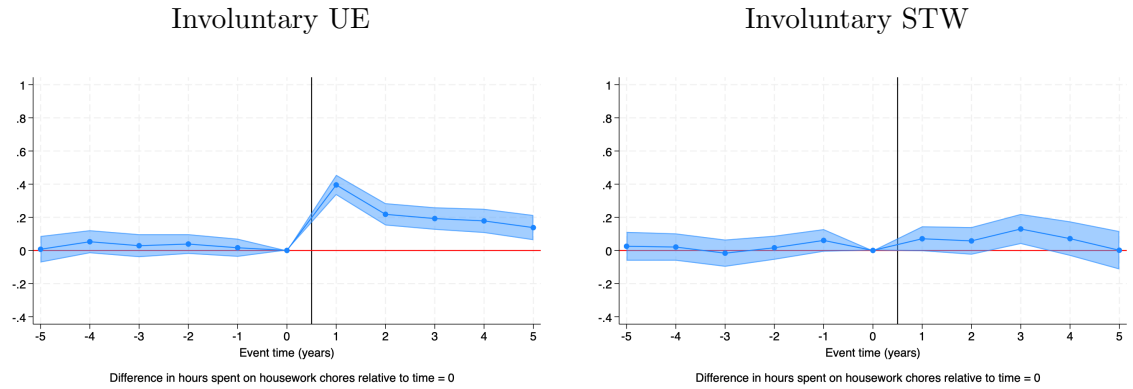
Involuntary STW



Source. own calculations based on the SOEP core, v40.
Notes. Plots show event-time coefficients estimated using the method proposed by Sun and Abraham (2021). This estimator constructs cohort-specific treatment effects for each event time and then aggregates them across cohorts. Unlike the conventional two-way fixed effects event study in equation (1), this approach avoids biased comparisons that can arise under staggered treatment timing when treatment effects differ across cohorts. The estimates are based on a panel where the pre-event observations are based on matching three years prior to the event. Shaded areas represent 95% confidence intervals.

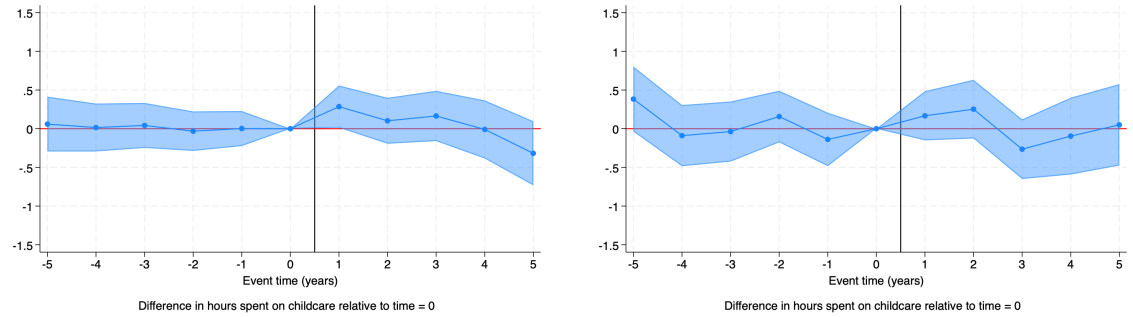
G.1 Unpaid care work outcomes

Figure G.58: Housework in hours per workday



Source. own calculations based on the SOEP core, v40.
Notes. Plots show event-time coefficients estimated using the method proposed by Sun and Abraham (2021). This estimator constructs cohort-specific treatment effects for each event time and then aggregates them across cohorts. Unlike the conventional two-way fixed effects event study in equation (1), this approach avoids biased comparisons that can arise under staggered treatment timing when treatment effects differ across cohorts. The estimates are based on a panel where the pre-event observations are based on matching three years prior to the event. Shaded areas represent 95% confidence intervals.

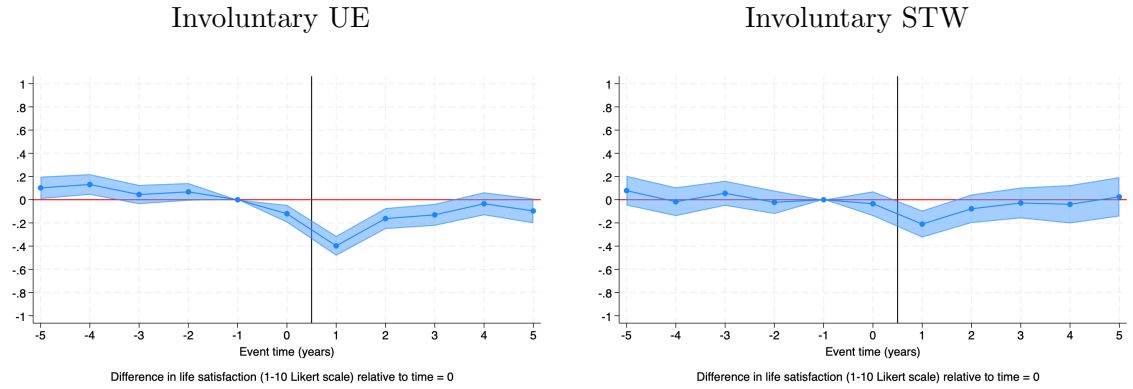
Figure G.59: Childcare in hours per workday



Source. own calculations based on the SOEP core, v40.
Notes. Plots show event-time coefficients estimated using the method proposed by Sun and Abraham (2021). This estimator constructs cohort-specific treatment effects for each event time and then aggregates them across cohorts. Unlike the conventional two-way fixed effects event study in equation (1), this approach avoids biased comparisons that can arise under staggered treatment timing when treatment effects differ across cohorts. The estimates are based on a panel where the pre-event observations are based on matching three years prior to the event. Shaded areas represent 95% confidence intervals.

G.2 Life satisfaction outcome

Figure G.60: Life satisfaction



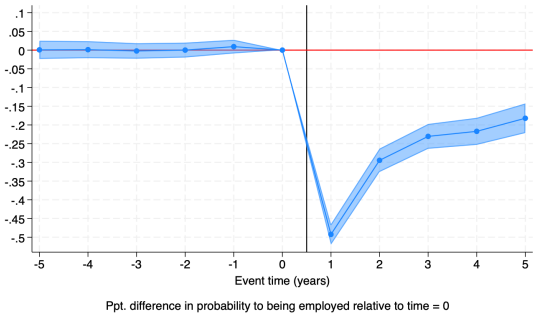
Source. own calculations based on the SOEP core, v40.
Notes. Plots show event-time coefficients estimated using the method proposed by Sun and Abraham (2021). This estimator constructs cohort-specific treatment effects for each event time and then aggregates them across cohorts. Unlike the conventional two-way fixed effects event study in equation (1), this approach avoids biased comparisons that can arise under staggered treatment timing when treatment effects differ across cohorts. We normalize the event-study coefficients relative to $K = -1$, the last pre-treatment period in which both groups evolve comparably in well-being outcomes. The estimates therefore capture deviations from this baseline rather than from $K = 0$. The estimates are based on a panel where the pre-event observations are based on matching three years prior to the event. Shaded areas represent 95% confidence intervals.

G.3 Heterogeneity by time period

Figure G.61: Employment

Reunification period (1985–2005)

UE



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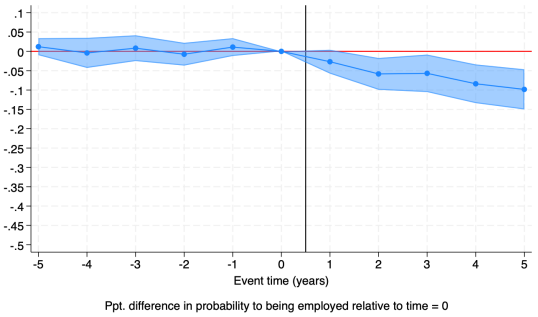
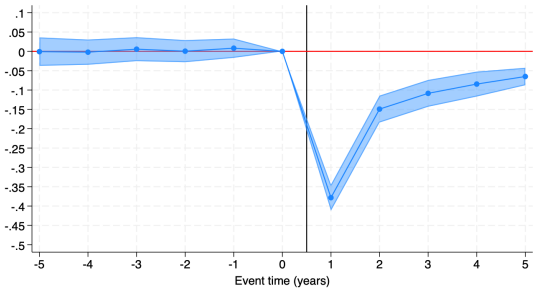


Financial crisis period (2005–2015)

UE



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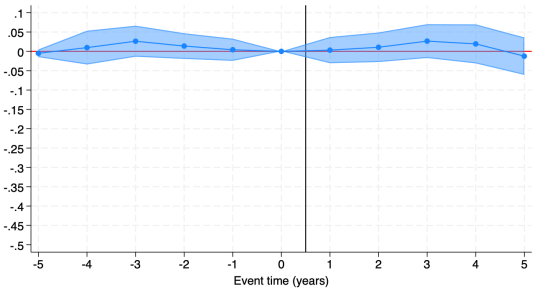
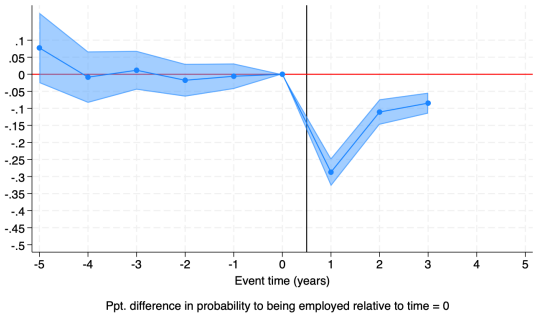


COVID-19 period (2015–2023)

UE



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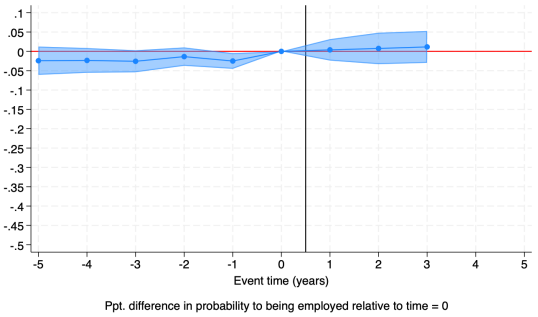
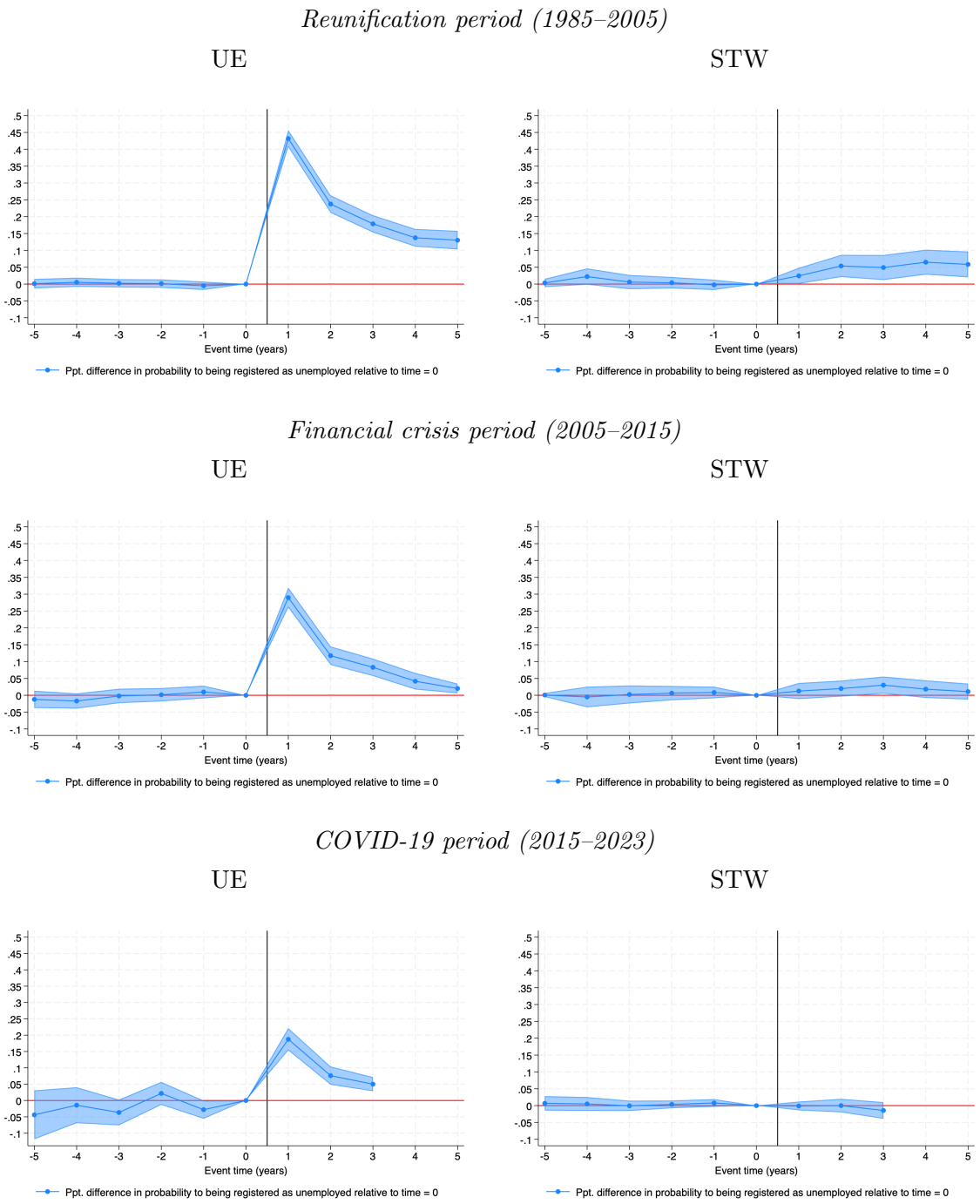


Source. own calculations based on the SOEP core, v40.
Notes. Plots show event-time coefficients estimated using the method proposed by Sun and Abraham (2021). This estimator constructs cohort-specific treatment effects for each event time and then aggregates them across cohorts. Unlike the conventional two-way fixed effects event study in equation (1), this approach avoids biased comparisons that can arise under staggered treatment timing when treatment effects differ across cohorts. The estimates are based on a panel where the pre-event observations are based on matching three years prior to the event. Shaded areas represent 95% confidence intervals.

Figure G.62: Reception of UE Benefits

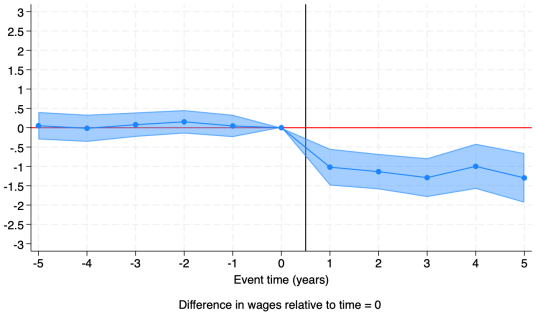


Source. own calculations based on the SOEP core, v40.
Notes. Plots show event-time coefficients estimated using the method proposed by Sun and Abraham (2021). This estimator constructs cohort-specific treatment effects for each event time and then aggregates them across cohorts. Unlike the conventional two-way fixed effects event study in equation (1), this approach avoids biased comparisons that can arise under staggered treatment timing when treatment effects differ across cohorts. The estimates are based on a panel where the pre-event observations are based on matching three years prior to the event. Shaded areas represent 95% confidence intervals.

Figure G.63: Real hourly wage, conditional on employment

Reunification period (1985–2005)

UE



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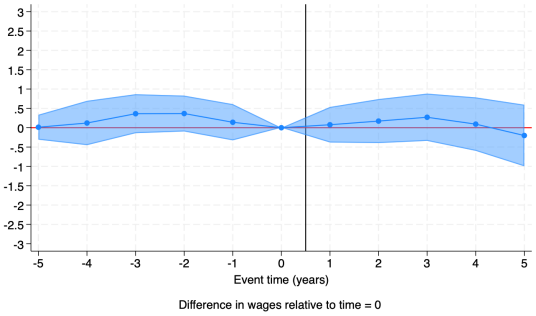
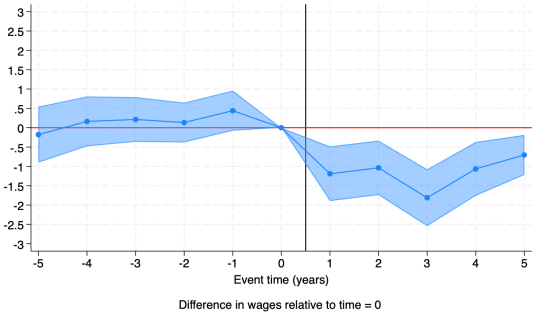


Financial crisis period (2005–2015)

UE



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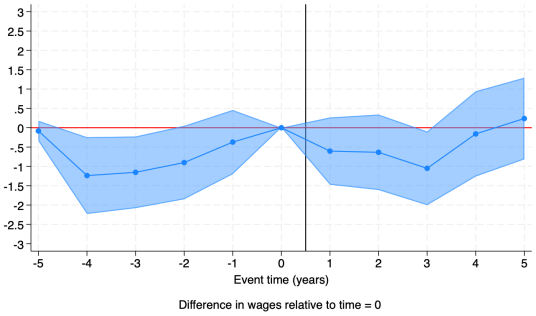
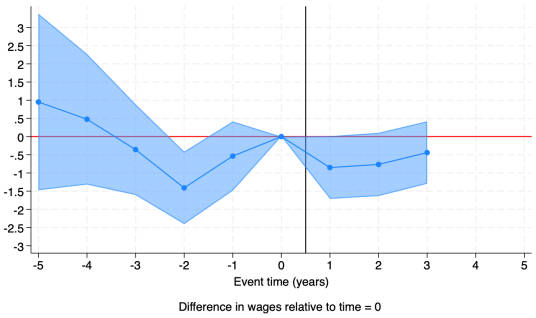


COVID-19 period (2015–2023)

UE



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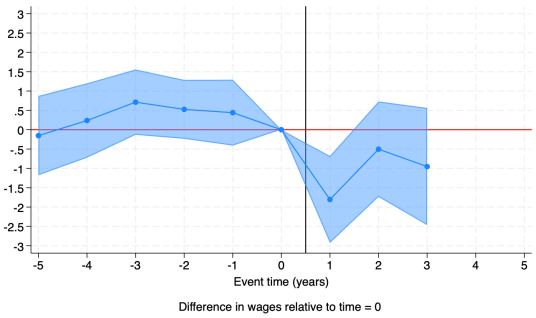
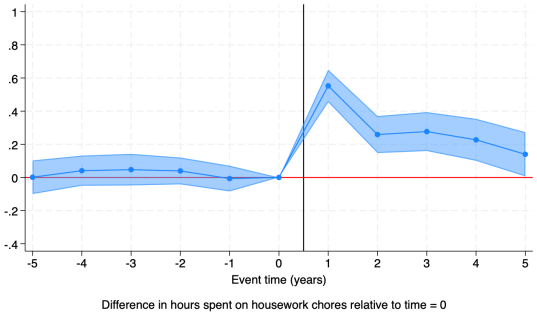


Source. own calculations based on the SOEP core, v40.
Notes. Plots show event-time coefficients estimated using the method proposed by Sun and Abraham (2021). This estimator constructs cohort-specific treatment effects for each event time and then aggregates them across cohorts. Unlike the conventional two-way fixed effects event study in equation (1), this approach avoids biased comparisons that can arise under staggered treatment timing when treatment effects differ across cohorts. The estimates are based on a panel where the pre-event observations are based on matching three years prior to the event. Shaded areas represent 95% confidence intervals.

Figure G.64: Housework in hours per workday

Reunification period (1985–2005)

UE



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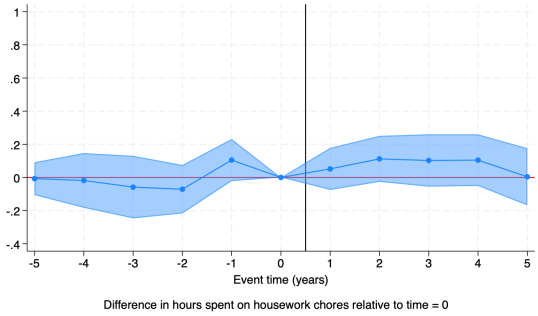
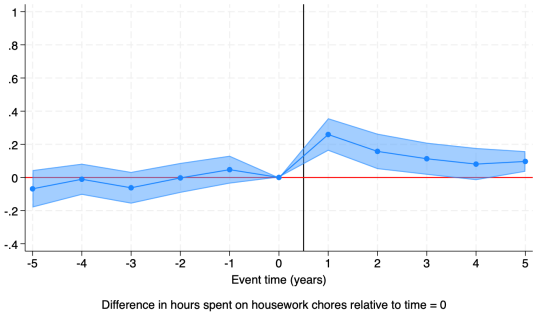


Financial crisis period (2005–2015)

UE



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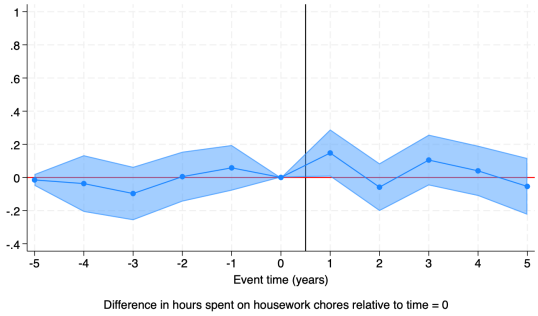
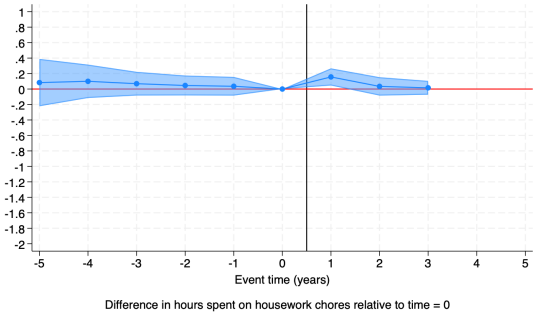


COVID-19 period (2015–2023)

UE



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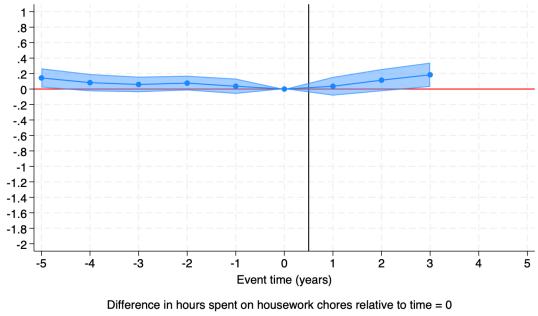
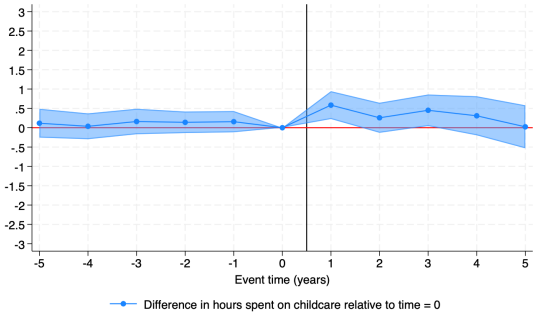


Source. own calculations based on the SOEP core, v40.
Notes. Plots show event-time coefficients estimated using the method proposed by Sun and Abraham (2021). This estimator constructs cohort-specific treatment effects for each event time and then aggregates them across cohorts. Unlike the conventional two-way fixed effects event study in equation (1), this approach avoids biased comparisons that can arise under staggered treatment timing when treatment effects differ across cohorts. The estimates are based on a panel where the pre-event observations are based on matching three years prior to the event. Shaded areas represent 95% confidence intervals.

Figure G.65: Childcare in hours per workday

Reunification period (1985–2005)

UE



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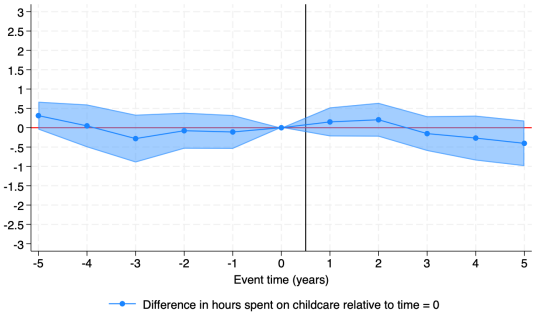
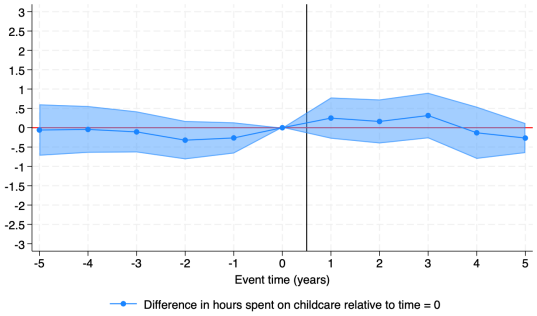


Financial crisis period (2005–2015)

UE



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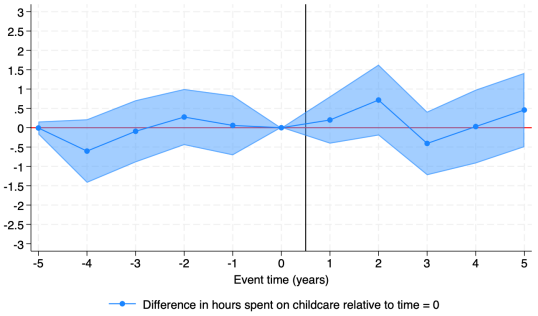
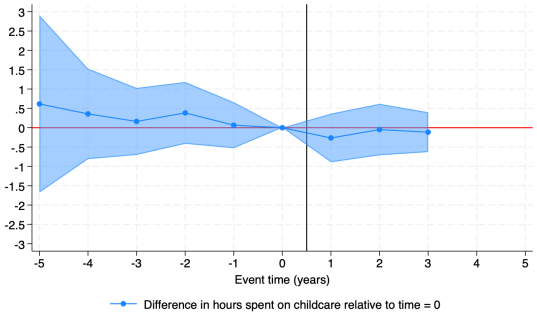


COVID-19 period (2015–2023)

UE



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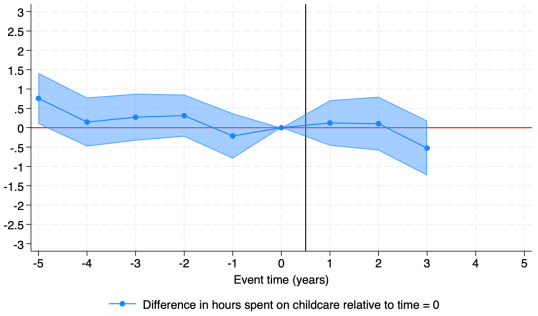
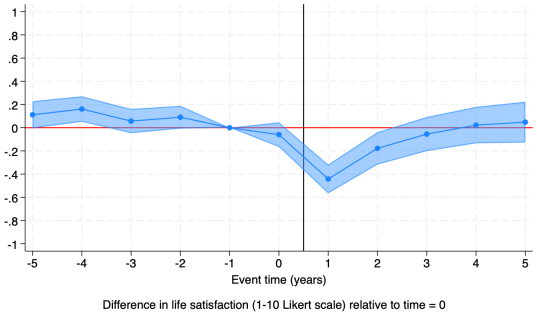


Source. own calculations based on the SOEP core, v40.
Notes. Plots show event-time coefficients estimated using the method proposed by Sun and Abraham (2021). This estimator constructs cohort-specific treatment effects for each event time and then aggregates them across cohorts. Unlike the conventional two-way fixed effects event study in equation (1), this approach avoids biased comparisons that can arise under staggered treatment timing when treatment effects differ across cohorts. The estimates are based on a panel where the pre-event observations are based on matching three years prior to the event. Shaded areas represent 95% confidence intervals.

Figure G.66: Well-being, life satisfaction

Reunification period (1985–2005)

UE



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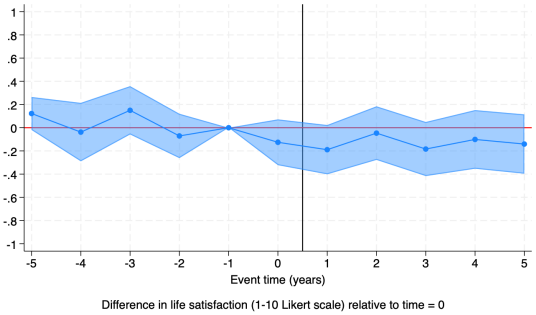
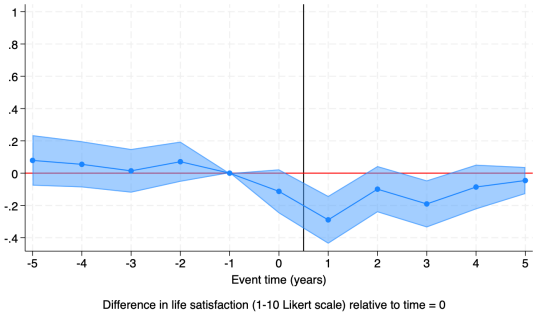


Financial crisis period (2005–2015)

UE



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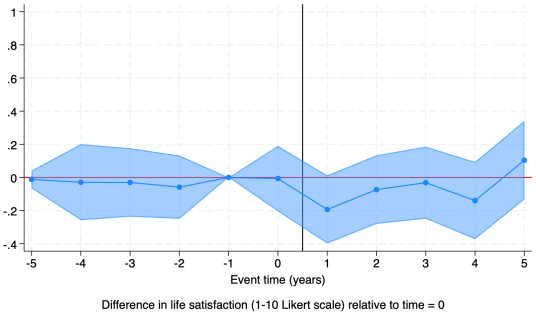
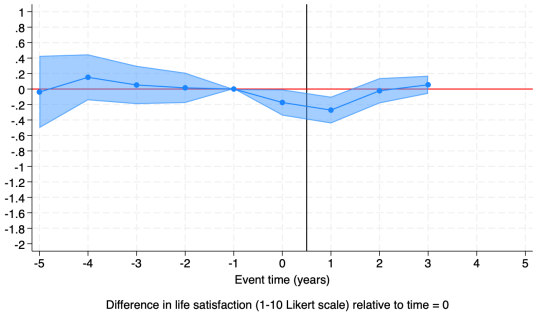


COVID-19 period (2015–2023)

UE



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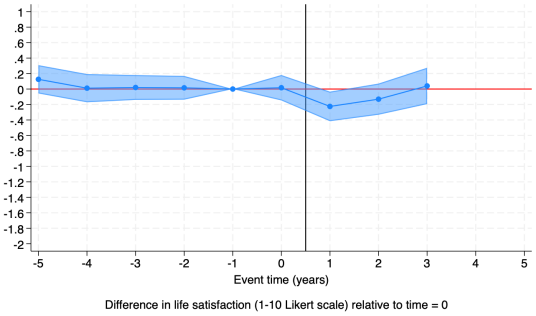

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