Changing the unemployment insurance duration: heterogeneous effects and an unbudging exit spike
Online appendix

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October 22, 2023

Revision 1.0: 2023-10-18

- Initial published version

Revision 1.1: 2023-10-23

- Added: decomposition of the effect on unemployment duration by week in unemployment, appendix Q
- Added: parametric estimates on the effects on the job-finding rates, appendix Q
- Renamed: added "– – and cumulative outcomes" to the title of appendix O for clarity

Contents

Appendix A The different benefits and transitions between benefits

Technically, Finland has three main unemployment benefits. Their official translations are earnings-related unemployment allowance, the basic unemployment allowance and the labour market subsidy. The earnings-related allowance is paid by the funds, while the basic allowance and the subsidy are administered by Kela.

The earnings-related allowance is based on prior wages, while both the basic allowance and the subsidy pay a flat rate, similar to other types of minimum income support by the government. This is why the earnings-related allowance is considered insurance (UI) in the main text, while both the basic allowance and the subsidy are considered unemployment assistance (UA) or welfare benefits.

All the benefits require that one is registered as a jobseeker and adheres to the various responsibilies of a jobseeker: they have to look for and be available for full-time jobs, accept job offers within certain guidelines, stay in contact with their caseworker, typically adhere to an employment plan, and possibly attend employment-promoting services.

If a registered jobseeker satisfies the recent employment condition, they are eligible for one of the allowances. Those who have been members in an unemployment fund during the qualifying employment period are eligible for the earnings-related allowance. Others are eligible for the basic allowance. Both allowances have the same maximum duration (in 2013, 100 weeks apart from old-age exemptions). If a person does not satisfy the recent employment condition or exhausts either allowance, they can apply for the subsidy.

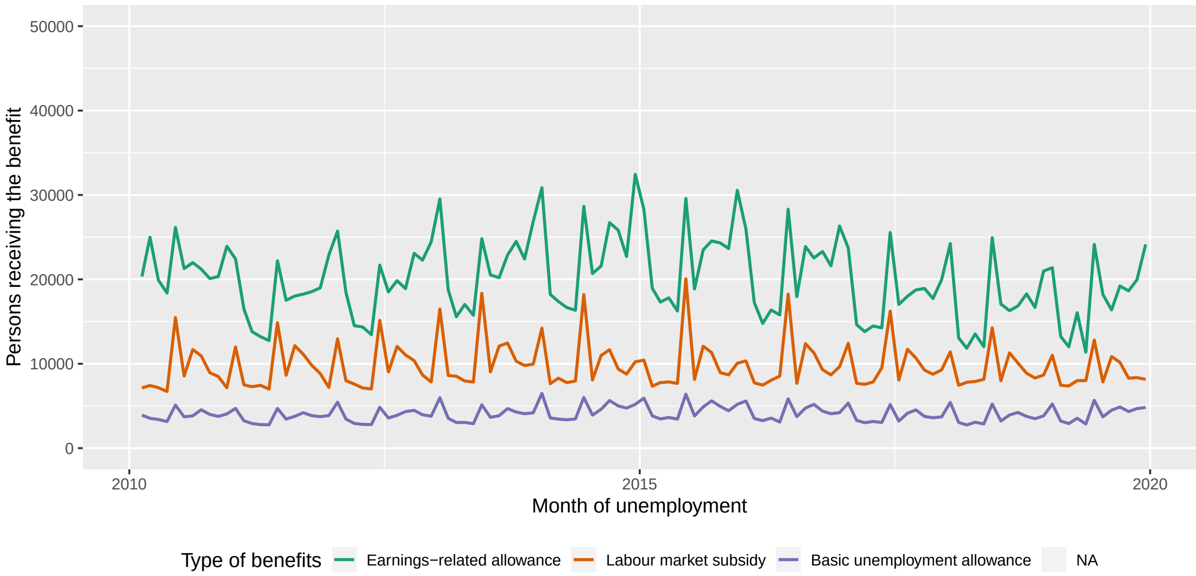
As figure 1 demonstrates, most fresh entrants into unemployment collect the earnings-related allowance, and very few collect the basic allowance. In other words, most persons who enter unemployment and satisfy the recent employment condition also satisfy the fund membership requirement. The normalized fund membership fees range from 3 to 10 euros per month in 2023 for a median-waged worker.

All three types of benefits can be paid at increased rates, for example during participation in employment-promoting services. The increase to the earnings-related allowance scales with prior earnings, while the flat-rate benefits offer only a flat-rate increase.

Technically, the earnings-related allowance consists of two parts: the same base part as the other benefits, plus an earnings-related part on top of that. The way the targeted reform in 2014 was implemented was that for the last 20 weeks of the old entitlement (weeks 81–100), the funds did not play the earnings-related part. Effectively, the recipients were being paid the flat-rate allowance. The main differences between this and a straight 20 weeks cut to insurance entitlement were that the benefit was being administered by funds instead of Kela for the 20 weeks, and that the rules being applied were for the two allowances, rather than the labour market subsidy.

There are some minor differences between the subsidy and the flat-rate allowance. While all the benefits are adjusted for simultaneous part-time employment, the subsidy has further means-testing for capital income, spousal and parental income. It can also be lowered for young persons living with their parents in some circumstances. In practice, these rules are rarely observed for the relevant samples (those transitioning from UI to the subsidy).

Figure 1: Observed entries into unemployment by month and benefit type



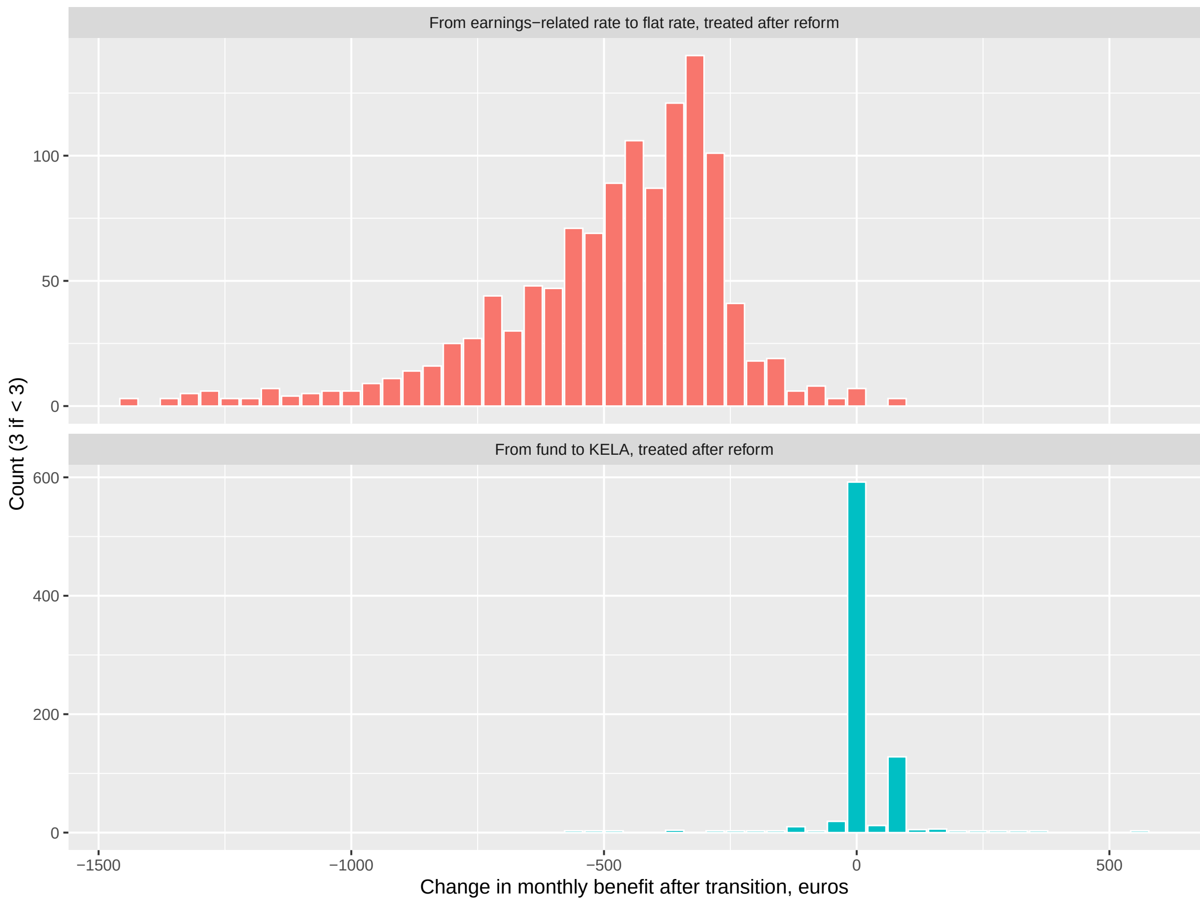
Includes new spells, as in the main text (a spell ends when no unemployment benefits are collected for a period of at least 30 days).

Figure 2 illustrates the distribution of observed individual-level changes in unemployment benefits for those targeted by the targeted reform at two stages. The first transition is while still being paid by the fund, but having exhausted the earnings-related part at 80 weeks (with no accompanying spike in exits). The second transition is when moving from fund-paid flat-rate allowance to Kela-paid subsidy at 100 weeks (this time, with the accompanying spike in exits).

For the overwhelming majority, there is no change in unemployment benefits when switching the agency during the targeted reform. In fact, for some individuals, the transition from funds to Kela at this point actually leads to an *increase*.[1] Despite this, the spike in exits occurred at the time of switching agencies, rather than the spike when benefits drop markedly.

[1]During participation in active labour market programs, persons can collect increased benefits, but the increase has a maximum duration. The maximum duration of the increase is specific to the benefit type. Thus, persons in ALMPs who transition from a flat-rate allowance (where the increase's maximum duration had run out) to the subsidy (where they had a new duration available for the increase) would see their benefits increase.

Figure 2: Observed changes in benefits at 80 and 100 weeks, after reform



Short-history individuals targeted by the targeted reform in 2014–2016.

Appendix B The major reforms to benefits in 2010's

The unemployment benefit law was changed 59 times from 2010 to 2019 alone. While many of these changes were quite technical or minor, many were also important. Kyyrä, Pesola, and Rissanen (2017) provide a review of the changes in the 2000s until 2017. Here, only a summarized list of major changes is presented to provide context.

How the changes in benefit levels and rules affected ongoing spells varied by reform: some only affected new entitlements starting after a year-turn, while others affected ongoing spells as well. All the insurance entitlement cuts only affected new entitlements earned after the cut came into force. The length of each entitlement was based on the first day of unemployment for which insurance benefits were actually collected for during the entitlement, regardless of payment date and the initial 5 or 7 days waiting period.

Table 1: Major reforms to unemployment benefits in 2010–2019.

Year	Major changes
2010	The recent employment condition unified to 34 weeks. Increases paid at start of unemployment and during ALMPs. Benefit types and levels simplified.
2012	A 100 euros/month increase to all base benefits.
2013	Spouse's income no longer affects the subsidy. Persons aged 24 or younger required to apply for a vocational education.
2014	Insurance entitlement cut from 100 weeks to 80 for those with less than 3 years of work history. The recent employment condition shortened to 26 weeks. Increase types reduced and their durations cut. An earnings disregard introduced: up to 300 euros of simultaneous wage income is disregarded when adjusting unemployment benefits.
2015	Level cuts to earnings-related benefits, affecting all new and ongoing spells. Many changes in rules for and availability of a hiring subsidy targeting long-term or difficult-to-employ unemployed. An earnings disregard introduced to housing benefits.
2017	Insurance entitlement cut from 80 weeks to 60 for those with less than 3 years of work history, and from 100 weeks to 80 for others (aged 57 or less). Benefit increase levels cut and waiting periods lengthened, affecting all new and ongoing spells. Another wide reform to the hiring subsidy.
2018	Activation model introduced: unemployed not showing sufficient activity could have their benefits reduced temporarily reduced by 4.5%.
2020	As a reaction to the COVID pandemic, waiting periods were effectively waived, the recent employment condition was halved, and collecting benefits did not use up the UI entitlement (for any spells between June and December, and for furloughs between March and December).

The changes in 2020 are notable because they also make interpreting the data from

2020 more difficult. In particular, the timing of new entitlements cannot be robustly inferred for this period. Thus, there are two reasons to end follow-ups of spells well before the COVID pandemic: the labour market disruptions caused by the pandemic, which might affect spells which had started unemployment at different times quite differently, and the inconsistency in the data.

Appendix C Additional evidence on frictions related to the agency switch

The main text illustrated, for the targeted reform's sample, how the number of calendar days per claim spike downwards at UI exhaustion time. Figures 3 and 4 show the same patterns for the universal reform and for an extended sample. This *extended sample* retains the same generic restrictions (persons aged 55 or higher, furloughs, voluntary quits and continued entitlements dropped; see section 3.1 in main text) as used for both reforms, but includes all new entitlements in 2010–2016 regardless of work history. In the main text, this sample was used to illustrate the time to next job after an exit with less noise. Since some benefit weeks have fairly low exit numbers, in smaller samples the visible patterns in time to next job can more easily be affected by outliers, although the patterns are still qualitatively similar (see below).

When calculating mean calendar days per claim, claims have been first groupped by payment date if the claimed dates also immediately follow one another. This is because the actual payments are commonly split in the data when an increase (for e.g. ALMP participation) only applies to a part of the payment. Figure 5 shows the patterns when this clustering is not done, using the targeted reform as an example. Days per claim now appear to be shorter at a few stages of ongoing spells. For example, the payments appear as split *in the benefit register* around the 80th week for the treatment group after the reform: one payment event for UI to the end of the 80th week, and another for the flat-rate payment after. However, the payment dates almost always coincide for these split payments, and they were typically also claimed together. The same is not true when one changes benefit agencies, when one has to file separate applications to different agencies, and there is also a significantly longer payment delay for new applications.

Figure 6 shows the mean ratio of FTE days to calendar weekdays (including any gaps between payments for a spell). Because partial unemployment consumes the entitlement at a slower rate, it is not surprising that partial unemployment is more prevalent for earlier FTE benefit weeks. However, this phenomenon is mild in absolute terms, as on all weeks the mean benefit days/calendar days ratio is still between 0.95 and 1. In particular, even in partial unemployment, benefits are still usually claimed for a calendar period of either a full month (the most common case) or 28 days.

The observed payment delay also spikes at UI exhaustion. The delay is defined as the lag from the last day claimed to the observed payment date. This delay is considerably longer for fresh applications, whether they are made for UI (at the start of a spell) or UA (after the end of insurance), as seen in figure 7. This delay is a feature of having to reapply, rather than a particular agency, as the delays normalize after the initial reapplication.

The main text used the aforementioned extended sample to illustrate the delays from exit to any job-finding within 30 days. Figures 8–9 show the same qualitative patterns

Figure 3: Calendar days per claim, universal reform sample

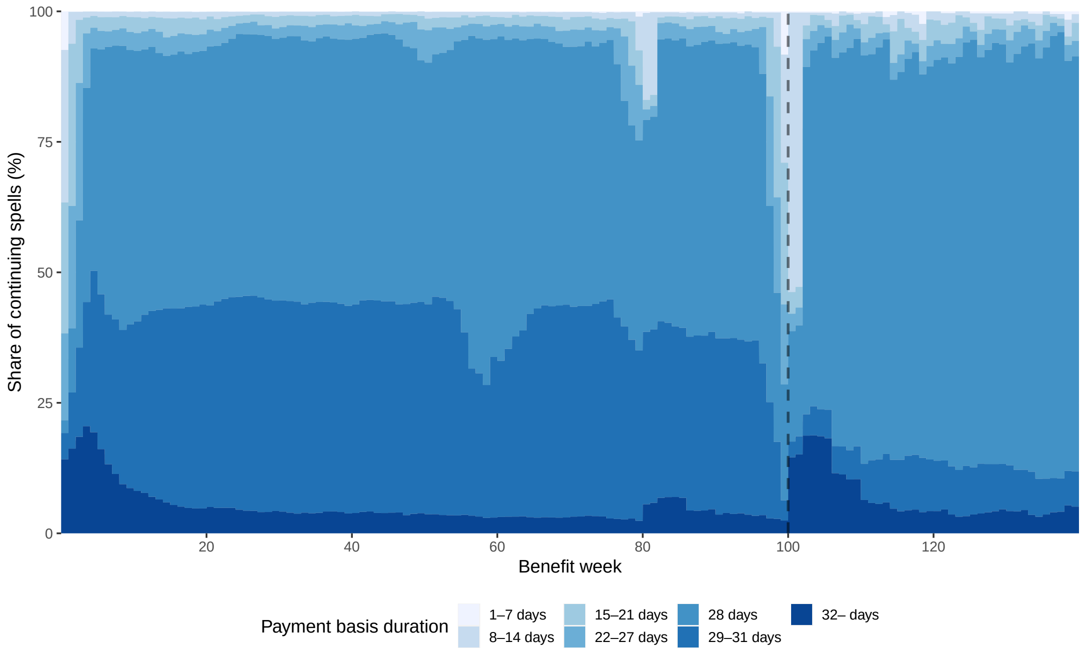


Figure 4: Calendar days per claim, extended sample

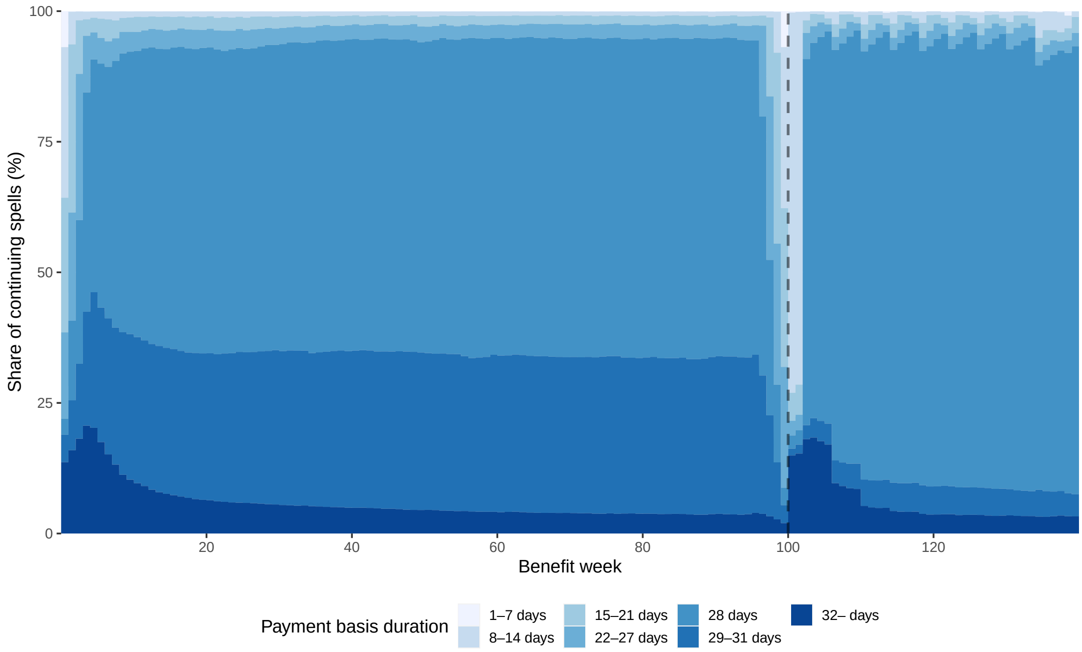
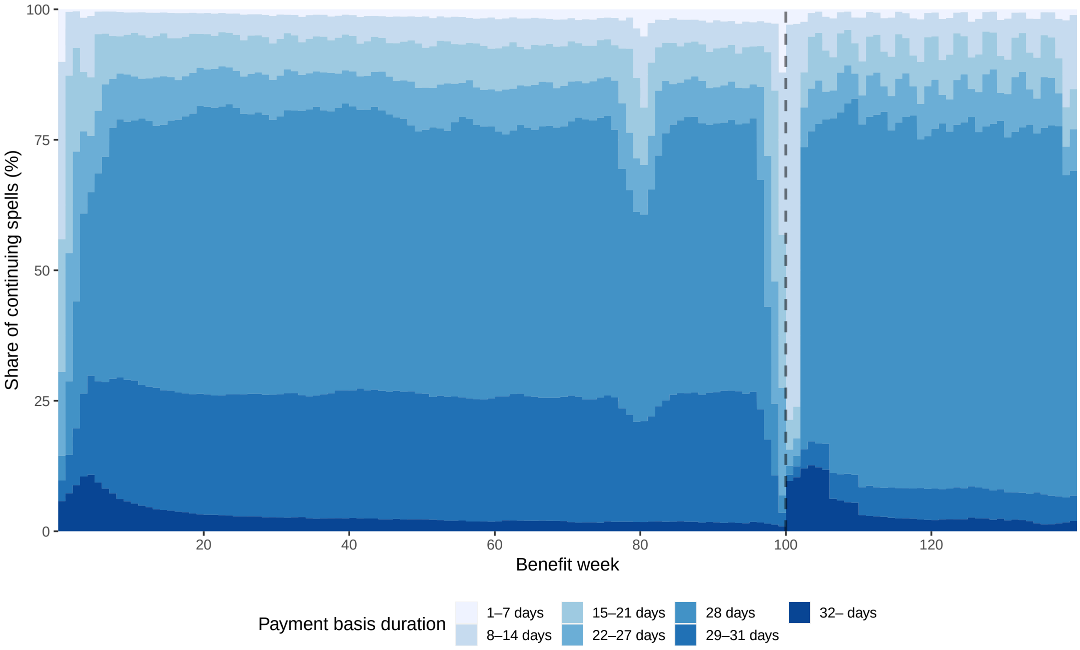


Figure 5: Calendar days per claim, targeted reform sample



No clustering by payment date is applied.

Figure 6: FTE days to calendar weekday ratio

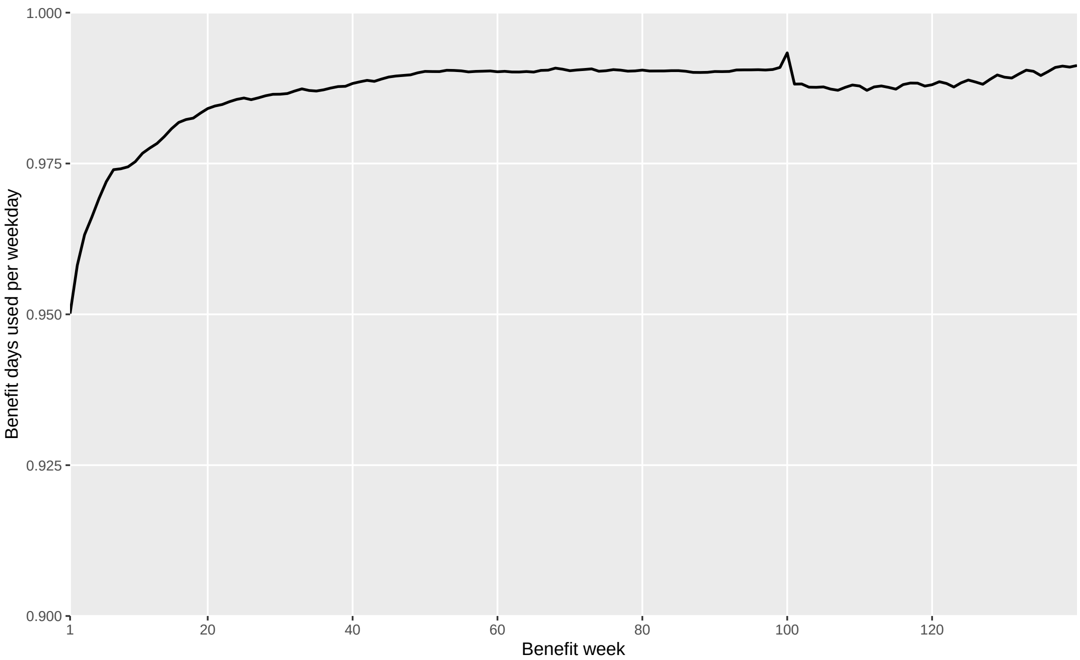
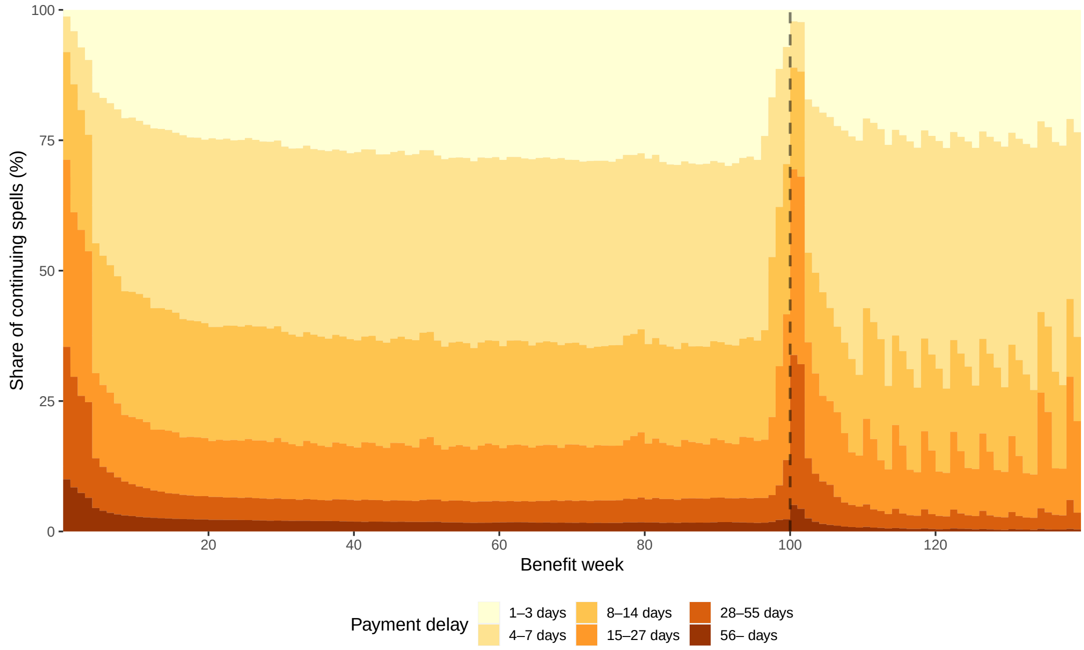


Figure 7: Payment delay in unemployment, extended sample



for the universal and targeted reform samples. Further, figure 10 extends the follow-up to potential job-findings within a full year, using the extended sample.

Figure 10 demonstrates that if the follow-up for job-finding is extended, the increase in delay to job at UI exhaustion is even more dramatic. However, for the adjustment, only jobs ongoing within 30 days after an exit and satisfying other job-finding criteria are used, since small frictions related to the benefit agency switch are unlikely to explain delays of several months from an exit to a job. The generic criteria for job-finding used in this paper have been chosen for comparability with Kyyrä and Pesola (2020b), with a few additional sanity checks. These criteria exclude a particularly high number of potential job-findings at both the earliest stages of employment and at the end of entitlement, discussed further in appendix section D.

The adjustment also takes into account the UI exhaustion rate. For each exit that warrants the adjustment, the average rate of FTE benefit days per eligible calendar day (weekday) is calculated for the preceding 90 months (or the duration of the spell, whichever is shorter). The weekdays strictly between the exit and the job-finding are then multiplied by this rate to yield the adjustment. This prevents spells in partial unemployment from getting disproportionally extended by the adjustment.

Figures 11–12 show the proposed adjustment to the hazard applied by group for the universal and the targeted reform. As the adjusted exit spike at benefit exhaustion is much smaller than the adjusted one, its movement or non-movement is also less noticeable across groups. However, the qualitative findings are similar for the effects of the reform at earlier stages of the spell: targeted reform had no significant effects, while the later universal

Figure 8: Time to job-finding after exit, universal reform

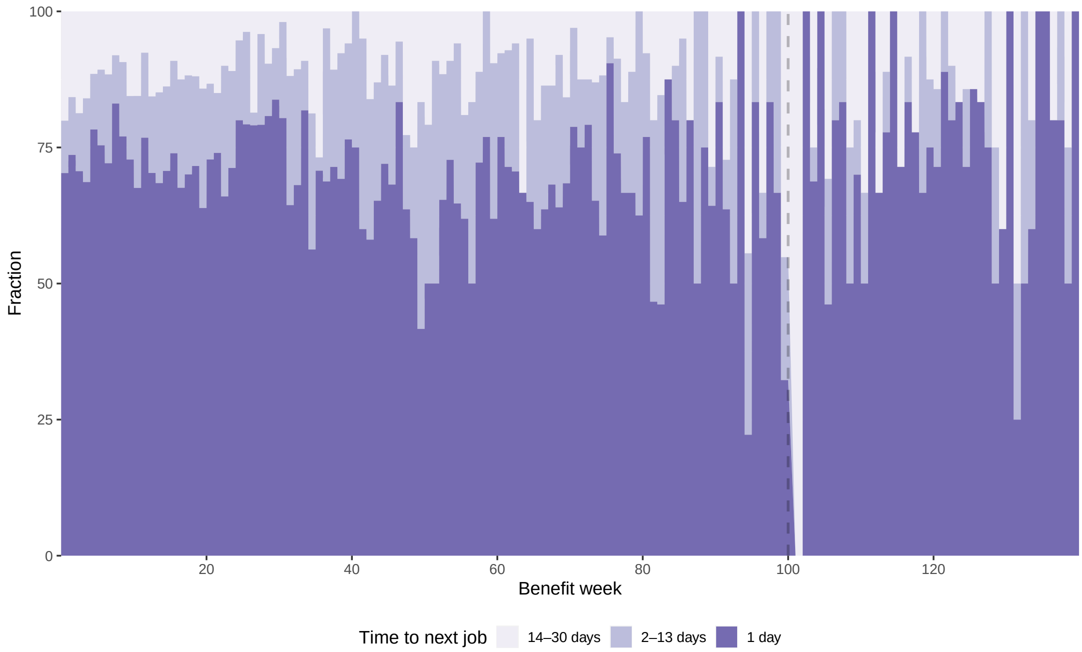


Figure 9: Time to job-finding after exit, targeted reform

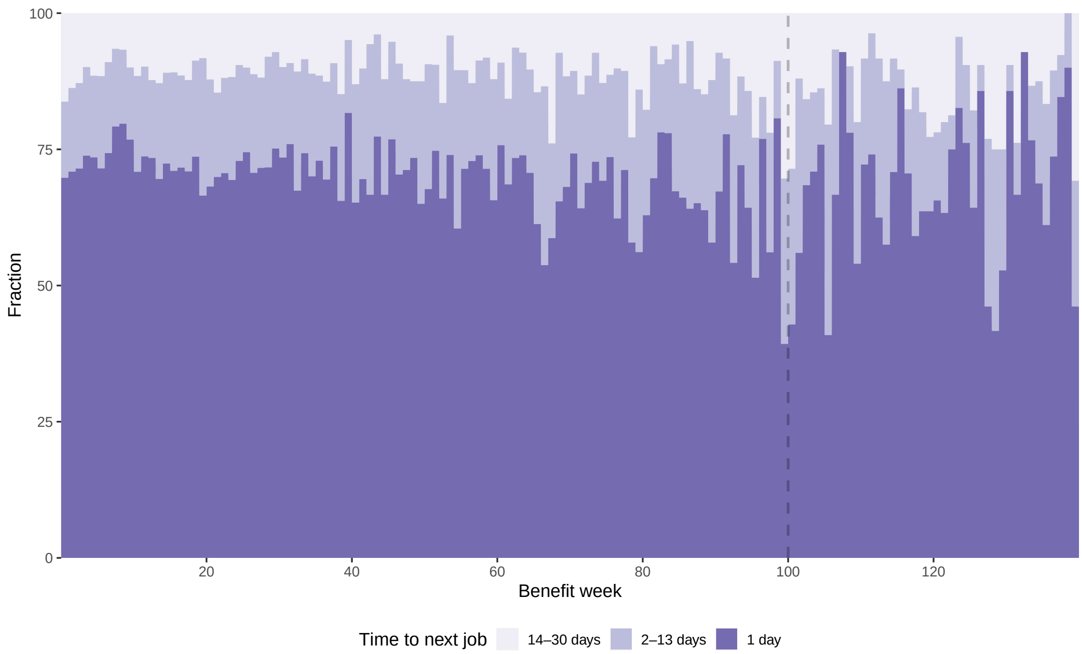
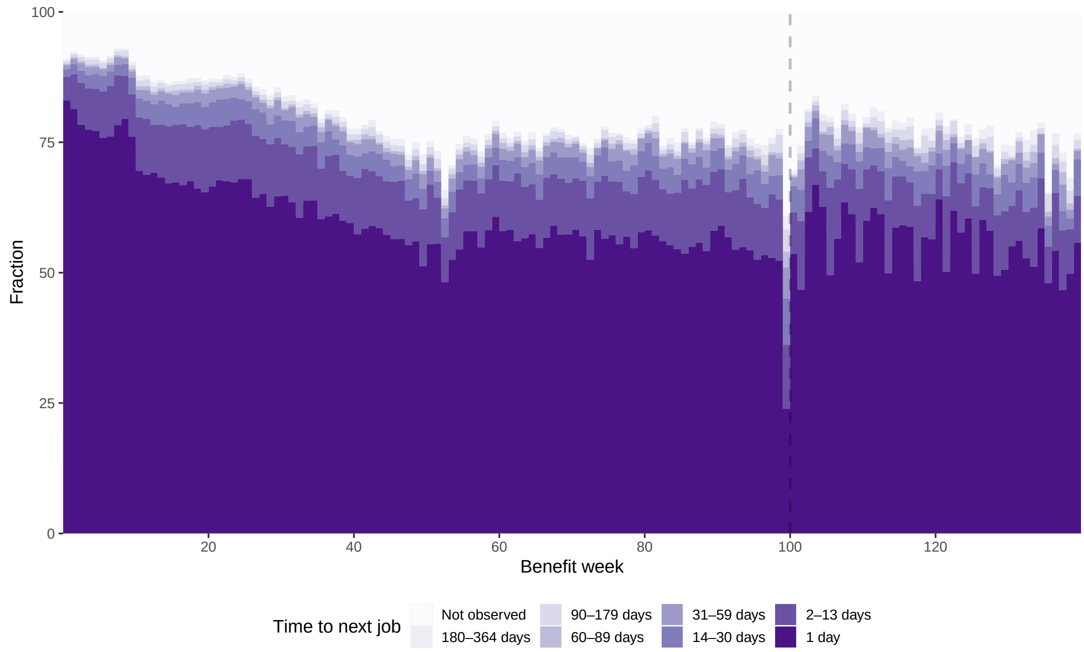


Figure 10: Time to jobs within a year after exit, extended sample



reform hastened job-findings in the early stages of unemployment.

To compare the effect of the adjustment itself, figures 13–14 show the adjustment for the universal reform sample and for the extended sample. The main text had the same figure for the targeted reform. In these cases, the adjustment does not completely remove the spike, although it still clearly flattens it. This suggests that the spike is not fully explained by the frictions as proxied by the delays to jobs, and that there are some more fundamental behavioral responses (or other frictions) related to UI expiration as well.

Figure 11: Adjusted hazard by group, universal reform sample

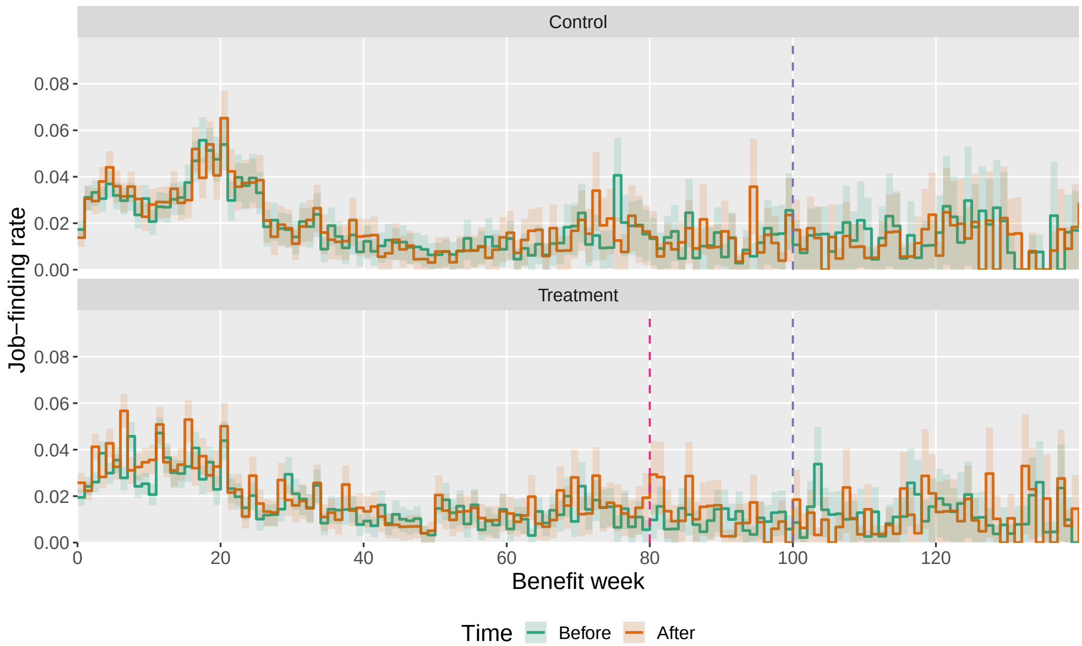


Figure 12: Adjusted hazard by group, targeted reform sample

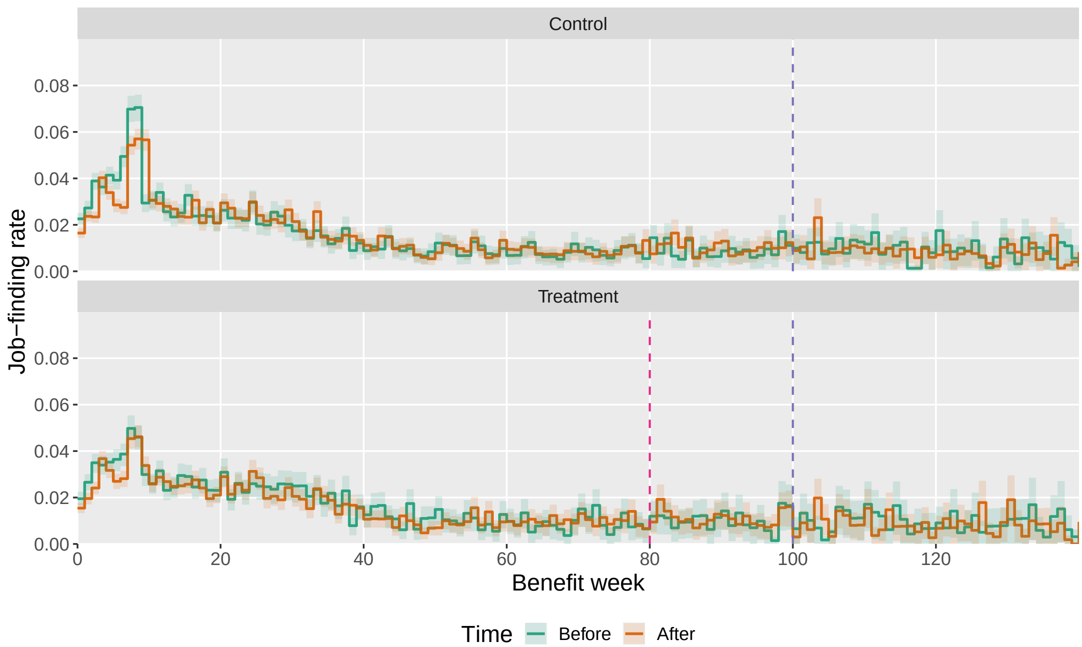
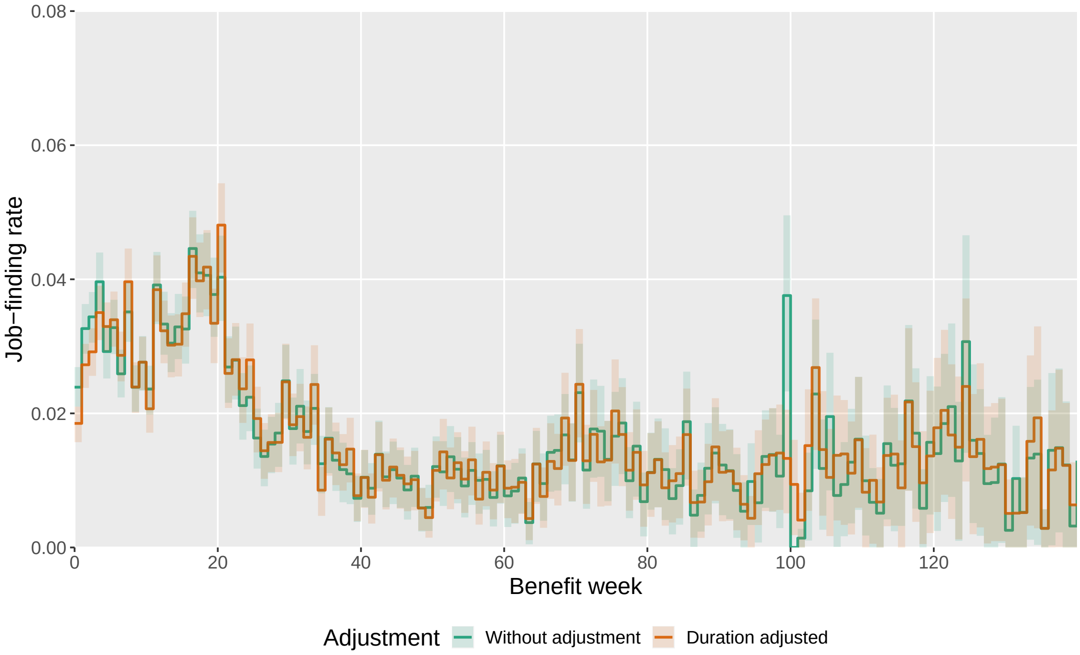
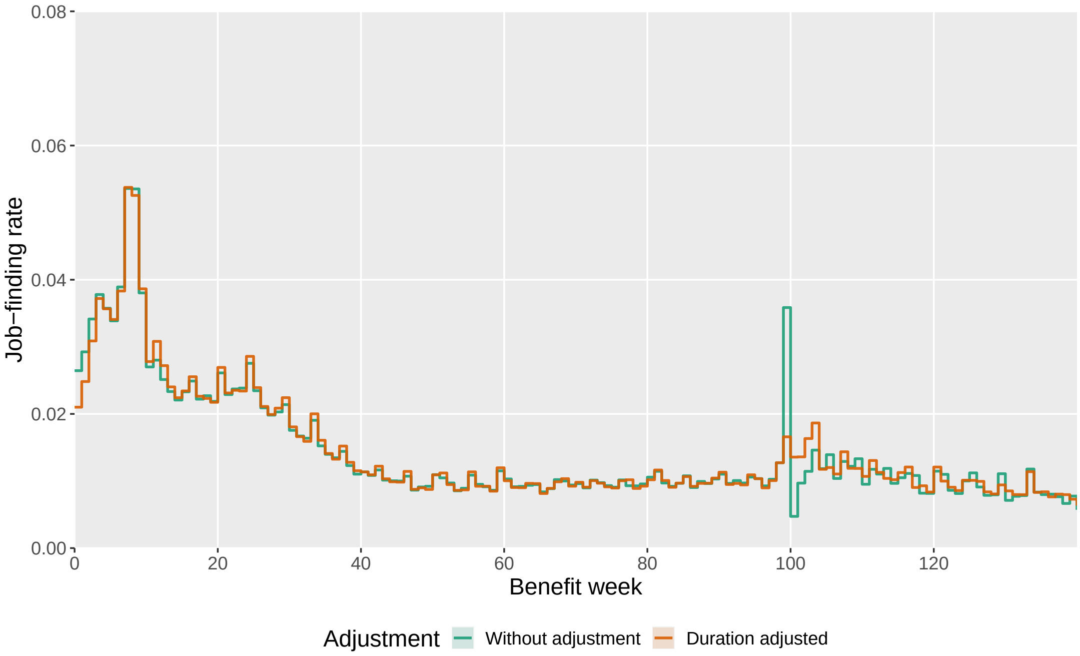


Figure 13: Job-finding hazard, adjusted by delay to job, universal reform



Adjustment to next job starting within 30 days after an exit described in the main text. Shaded areas correspond go bootstrapped 95% confidence intervals.

Figure 14: Job-finding hazard, adjusted by delay to job, extended sample



Adjustment to next job starting within 30 days after an exit described in the main text. The extended sample is a comparable sample of all new UI spells 2010–2016 with no work history restrictions.

Appendix D The impact of the job-finding criteria

The job-finding criteria are motivated by two concerns. First, as noted in the appendix W discussing the data on employment, available job start dates are likely to be more accurate than end dates. Second, the job-finding measure should ideally be roughly comparable with prior research, such as that by Kyyrä and Pesola (2020b).

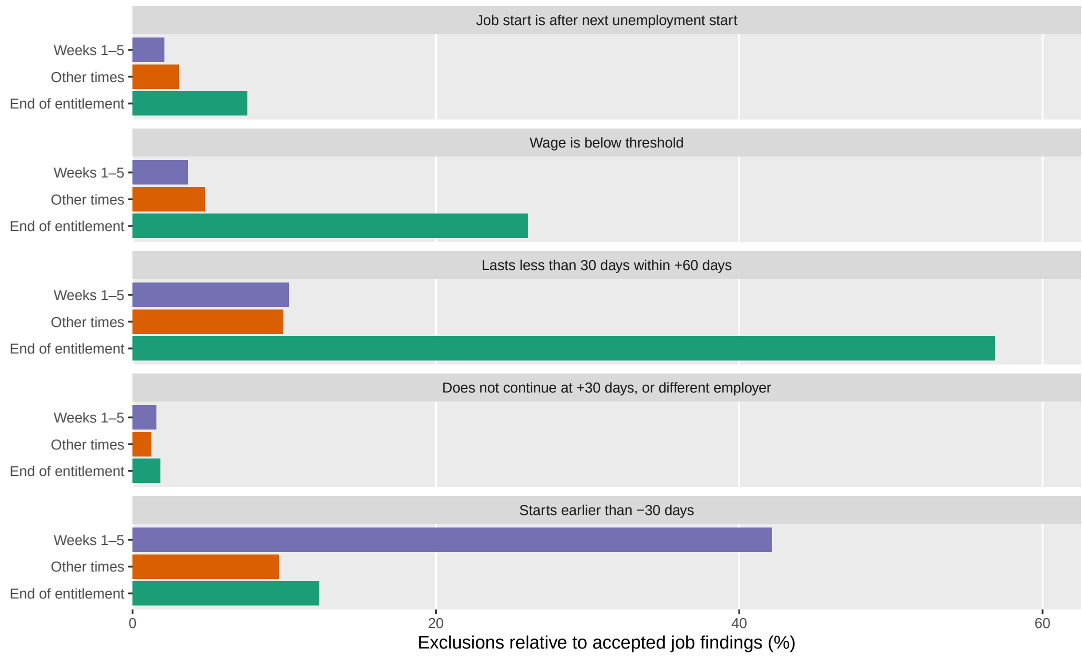
A job-finding after an exit from unemployment is defined as follows:

- Only an uncensored exit from unemployment can be followed by a job-finding.
- Job starts and durations are not counted past any later spell in unemployment.
- Estimated daily wage must be above a threshold. Finland does not have a universal minimum wage, as most jobs are instead covered by sector-specific collective agreements. Here, the minimum used is the legal "fallback" threshold required for jobs to count towards the recent employment condition when no collective agreements apply (1 331 euros per month in 2023 levels). The threshold varies by year based on wage inflation; the annual true threshold is used.
- The job must be ongoing at exit plus 30 days.
- The job start date must be no earlier than exit minus 30 days. The purpose of this rule is to avoid lingering phantom jobs in the data, as overlapping time in full-time unemployment directly suggests that at least some time in the observed employment spell is actually spent non-employed, i.e., the employer has probably not yet notified the pension funds about the separation.
- There are at least 30 days of qualifying employment for the same primary employer within 60 days after the exit. This rule allows for cases where the person's employment spell is broken by a few days' gap in the data, while still requiring a meaningful overall duration in the next job.

Jobs satisfying these criteria are also used to define the quality of the next job match for the main estimates. Durations in these jobs are further followed for up to 365 days from exit and mean monthly wages calculated over the employed duration.

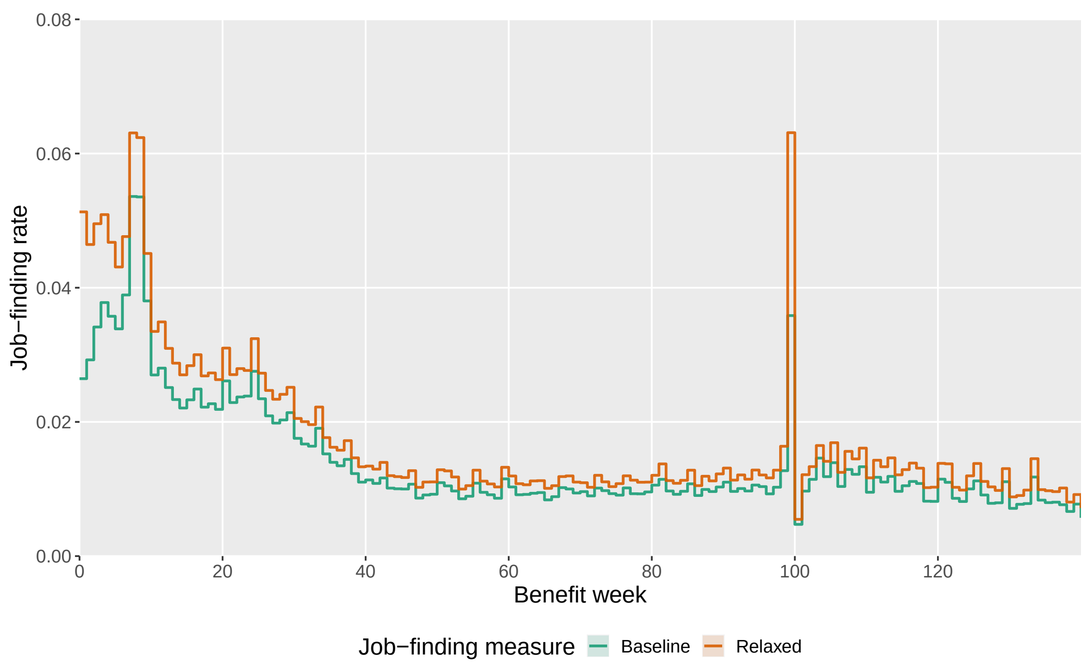
Figure 15 shows how the various exclusion criteria progressively drop jobs ongoing within 365 days after an exit, for different exit times. As can be seen, low estimated wages, short durations and long delays to jobs drop a particularly large fraction of potential job-findings after an exit at the end of the entitlement. On the other hand, for early exits, the requirement that the job start date cannot be earlier than the exit time minus 30 days drops a large share of potential recalls. Figure 16 shows an alternative hazard in the extreme case where all the exclusion criteria are dropped (any jobs ongoing within a year after an exit are classified as job-finding).

Figure 15: Exclusions by job-finding criteria



Exclusions are applied progressively, from top to bottom, on any jobs that are observed ongoing within 365 days after an exit from unemployment.

Figure 16: Alternative job-finding hazard



The alternative hazard is an extreme case, counting any job ongoing within a year after an exit as a job-finding. The baseline uses the criteria described in the text.

Appendix E Long-term outcomes for those exiting at the spike

In this appendix section, the additional data described in appendix Y is used to follow those exiting at UI exhaustion. The sample used had to be varied somewhat, depending on the focus of the follow-up and the available data. For example, focusing on year 2010 allows for a follow-up of up to 9 years, while collecting exits from around years 2010–2018 allows for different entitlement lengths and economic environments to be covered. Table 2 collects the samples used in one place.

Since those remaining in unemployment for roughly 2 years are a small, selected group of all unemployed, the spike exits are commonly compared to reasonable close groups. An exception is the subsection E.2, which specifically looks at subsequent labour market outcomes for all exits from UI, groupped by the used up benefit weeks at exit. Otherwise, spells that come close to the end of entitlement (reaching at least their individual maximum entitlement−5 weeks) are groupped into four categories. The categories are (1) those who leave unemployment shortly before (within 5 benefit weeks before exhaustion), (2) those who exit at exhaustion/spike (at the full entitlement used up exactly), (3) those who exit soon after (within 5 benefit weeks after exhaustion) and (4) those who leave later/censored. The rare cases who earn a new entitlement before censoring are dropped. As the focus is on the exit spike, the nominal entitlement for those in the targeted reform's target group is considered to be 100 weeks, i.e., the time they continue to collect benefits from unemployment funds. This is because the group do not exhibit a spike in exits at their effective UI expiration date, i.e., when their earnings-related benefits cease at 80 weeks.

In all cases, older individuals entitled (or predicted to be entitled) to additional days of UI (effectively unlimited UI until retirement, see appendix L) are excluded.

For annual incomes (available for 2010–2019) and daily incomes (available for 2021–early 2023), the status on a given year or day illustrated is determined by the largest income source. For daily employment status (2010–2019), the status on a given day is determined in the following order. Persons who are in subsidized job placements are classified to be in these placements. Individuals appearing employed and not in these placements are considered to be employed in the open labour market. Those who are not in placements and not employed and collect unemployment benefits are given the unemployed status.

Table 2: Samples used for spike follow-ups

Subsection	Figures	Age limits	Time 0	Selection criteria	Follow-up data
E.1			Exit from unemployment	Spell reached end of UI entitlement−5 weeks in...	Daily: unemployment benefits, employment
	17	≥ 57		...2010–05/2013, recent employment condition 34 weeks	
	18	≥ 57		...07/2013–2018, recent employment condition 26 weeks	
	19	< 57		...07/2013–2018, recent employment condition 26 weeks	
	20	≥ 57		...2010, and exits at UI exhaustion	
	21	< 57		...2010, and exits at UI exhaustion	
E.2	22, 23	< 57	Exit from unemployment	Exit from UI in 2010, spell did not start on furloughs	Daily: unemployment benefits, employment
E.3		< 57	End of entitlement	Spell reached end of UI entitlement−5 weeks in 2010	
	24, 25				Daily: unemployment benefits, employment data
	26				Annual: income data
E.4		< 57		Spell reached end of UI entitlement−5 weeks in...	
	27, 28		End of entitlement	...2010	Annual: income data
	29, 30, 31		Exit from unemployment	...Jan–June 2021	Daily: income data
	32		End of entitlement	...Jan–June 2021	Daily: income data
E.5	33	< 57	End of entitlement	Spell reached end of UI entitlement−5 weeks in January of 2010–2018	Daily: unemployment benefits, employment

E.1 Those aged 57 or higher leave for job placements

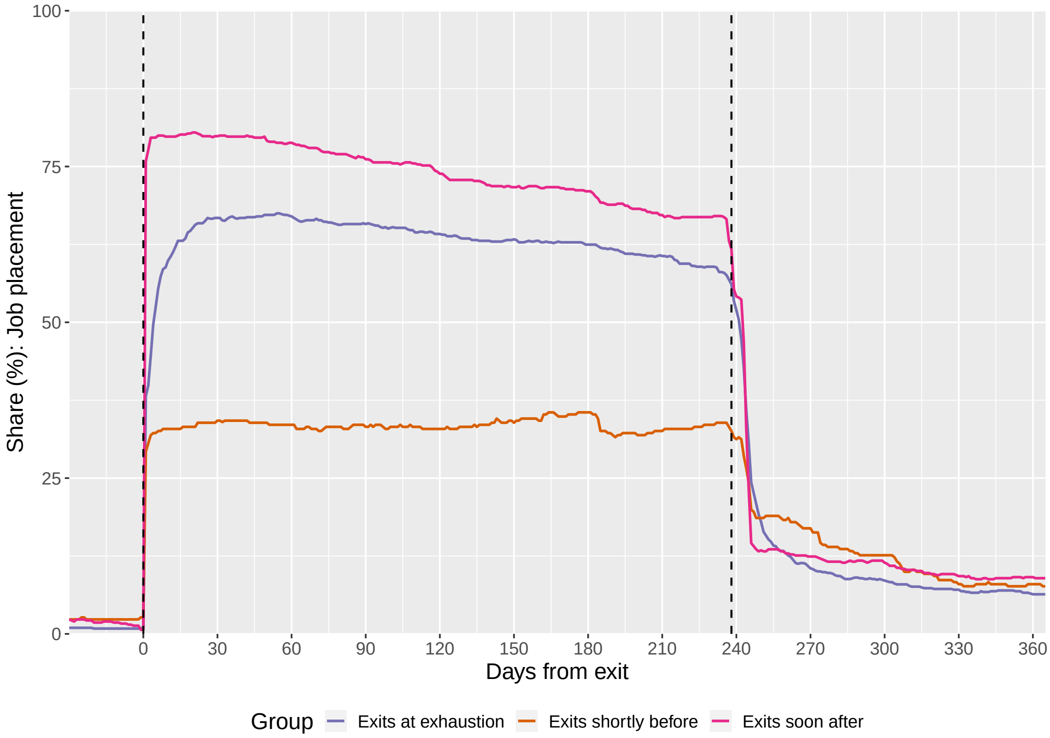
In the later subsections, the follow-ups are generally restricted to those aged less than 57 at UI exhaustion time. (If the spell does not actually reach the end of the entitlement, the exhaustion time is predicted.) The figures in this subsection justify this exclusion.

Those aged 57 or more have been guaranteed a subsidized job placement at UI expiration since 2010; see appendix L. This placement is arranged by the municipality of residence, and is stipulated to be long enough to satisfy a new employment condition (i.e., earn the person a new insurance period). While other similar subsidized job placements usually last for 1–2 years, the guaranteed placements are particularly likely to last for exactly the duration of the employment condition. After this period, the individuals typically return to unemployment. Since quitting a job voluntarily means a person is not entitled to benefits for 3 months, the placement durations are fixed in advance by the employers and employees. A plausible explanation for the pattern is that the municipalities are simply offering the bare minimum to satisfy the placement mandate to minimize expenses.

Figures 17 and 18 show the daily share of persons in job placements for those aged 57 or above after an exit from unemployment. The sample is split based on whether the applicable employment condition for another UI entitlement was 34 weeks (years 2010–2013) or 26 (years 2014–2019), illustrating how the placement duration varies to match the condition. As figure 19 shows, those aged 56 or less at UI expiration are far less likely to be in placements after an exit.

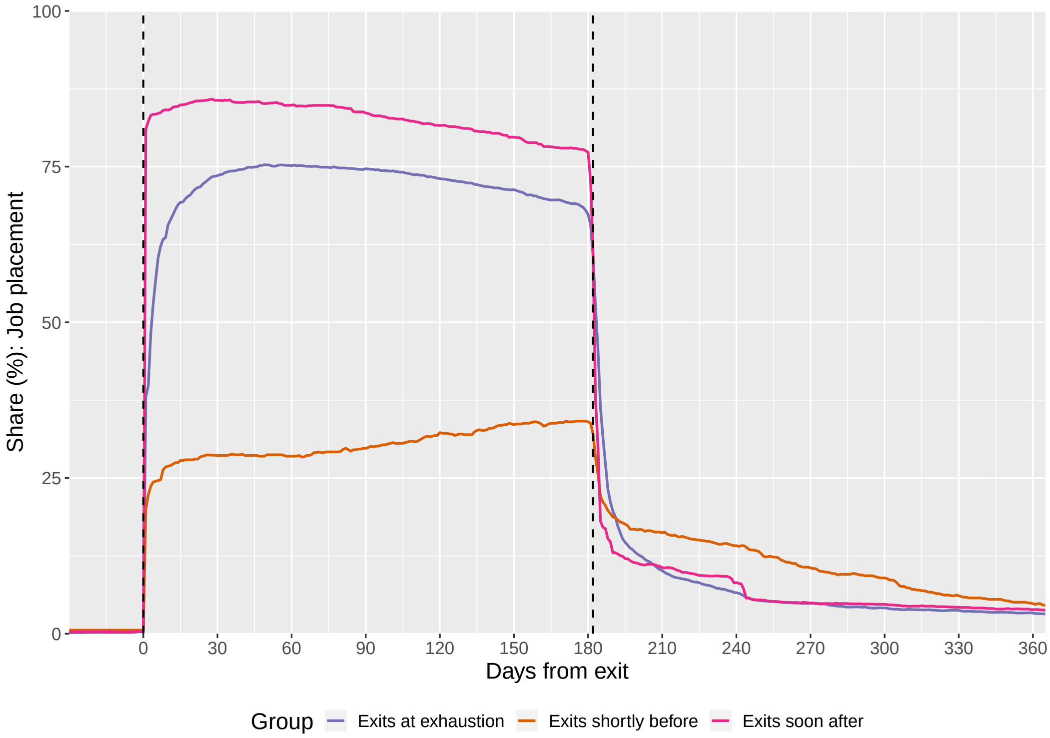
Figures 21 and 20 demonstrate the daily follow-ups for those who exit at the end of entitlement, separately for the two age-groups.

Figure 17: Persons in job placements, employment condition 34 weeks



Persons aged ≥ 57 at end of entitlement. The employment condition is the second dashed vertical line.

Figure 18: Persons in job placements, employment condition 26 weeks



Persons aged ≥ 57 at end of entitlement. The employment condition is the second dashed vertical line.

Figure 19: Persons in job placements, ages 56 and below

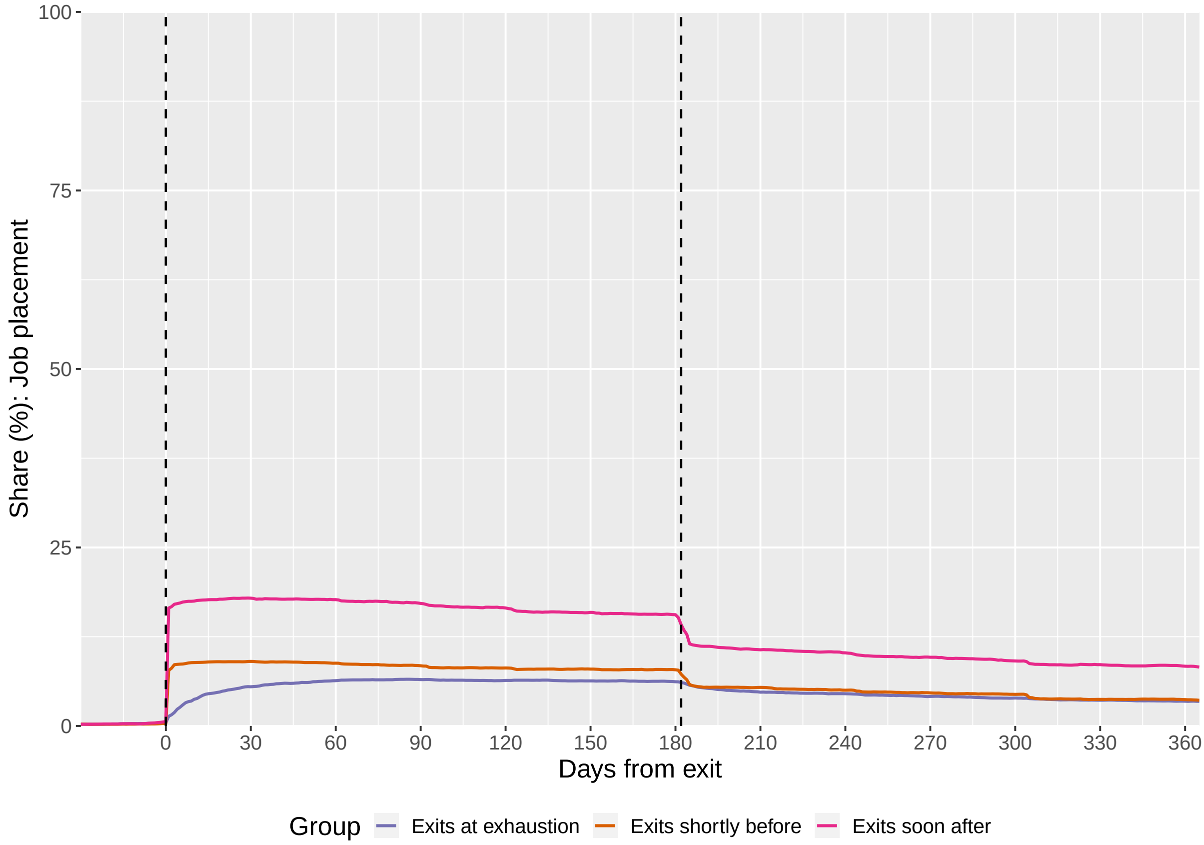


Figure 20: Daily status, exits at UI exhaustion, ages 57 and above

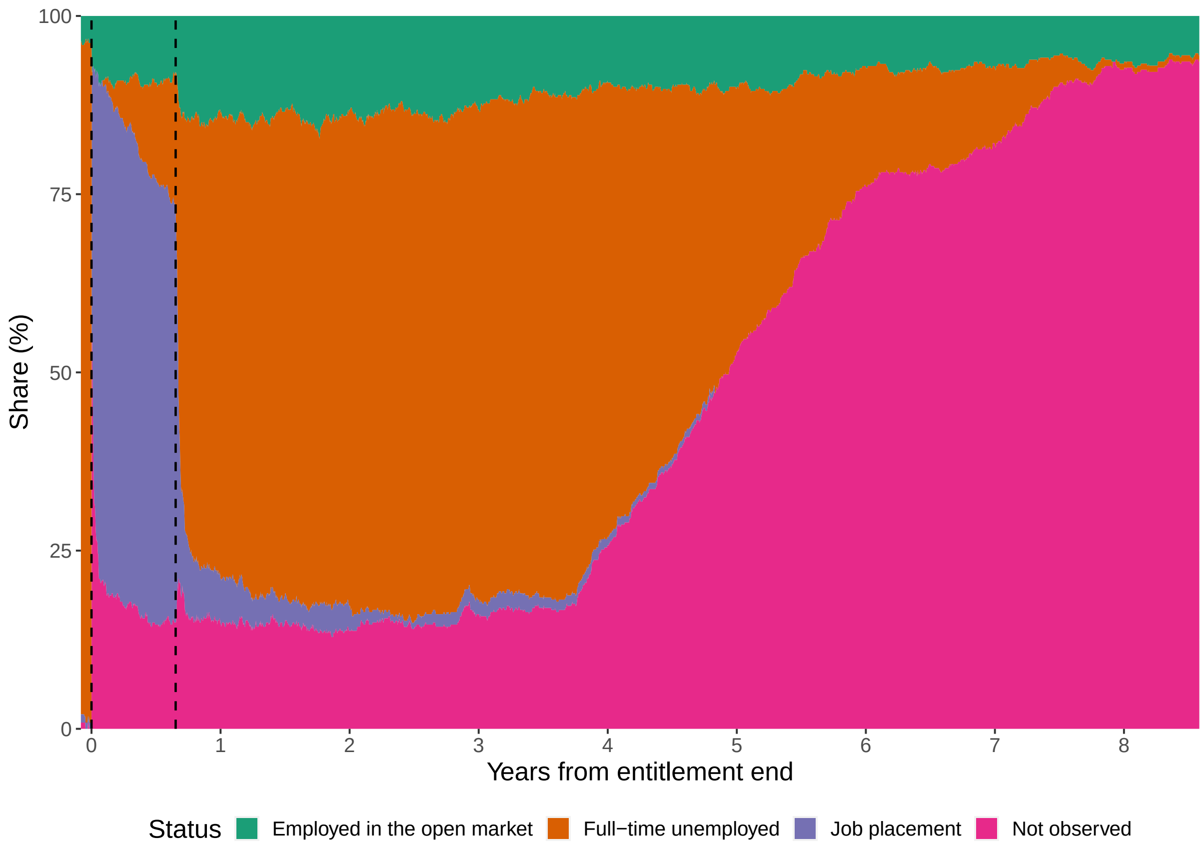
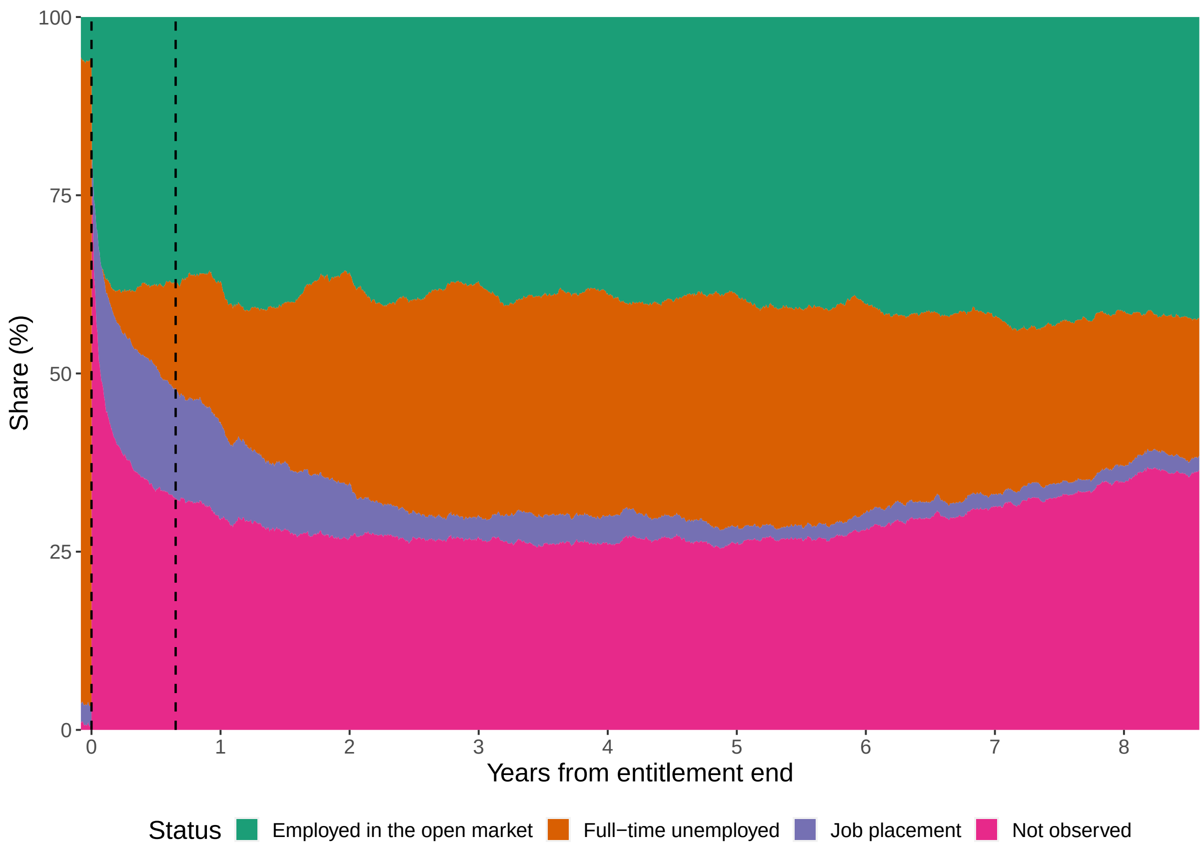


Figure 21: Daily status, exits at UI exhaustion, ages 56 and below



E.2 Longer unemployment is followed by lower employment

Figures 22 and 23 show the share of persons in employment on a given date after an exit from UI in 2010, excluding job placements. The exits are groupped by the number of consumed benefit weeks at exit, in 5-week bins, except for the maximum duration, which has its own bin.

The two figures are based on the same data, but the first one shows all 5-year bins coded by color to illustrate the consistency of the patterns. The second figure only plots the 25th, 50th, 75th quantiles of exit weeks and the maximum UI duration for better visual clarity.

The figures demonstrate that follow-up employment is persistently higher for exits with fewer UI weeks consumed. Most groups also have a fairly significant share falling back into unemployment at regular annual intervals.

Figure 22: Share employed after exit, by consumed benefit weeks at exit

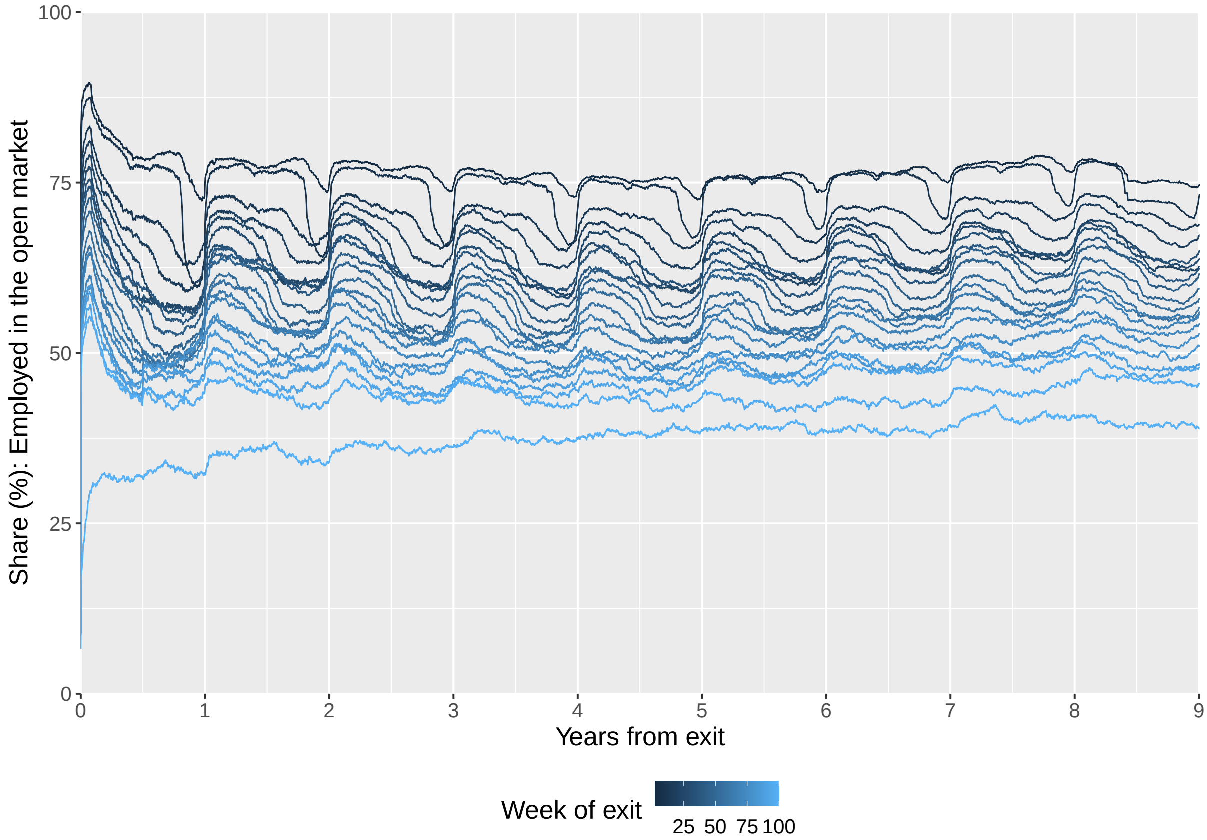
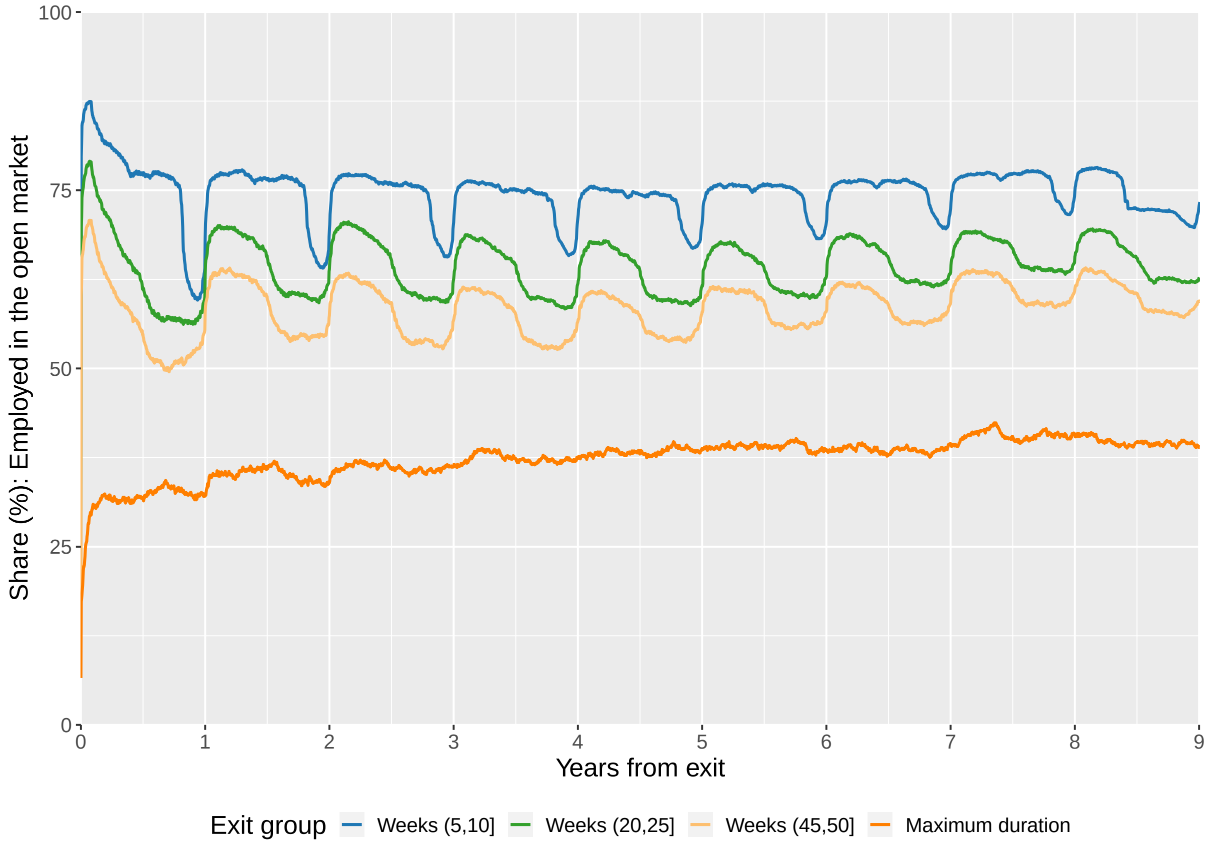


Figure 23: Share employed after exit, by consumed benefit weeks at exit



E.3 Exits shortly before exhaustion fare better

Figures 24 and 25 demonstrate cumulative wages and employment status from UI expiration date over the subsequent 8.5 years. As before, for those who exit shortly before actual expiration, the expiration date is predicted. Across the figures, those exiting just before exhaustion fare consistently better. On the other hand, over the follow-up, employment rates stay relatively low across all groups.

Figure 26 considers cumulative direct net contributions to the public finances. The contribution is defined as observed direct taxes minus transfers received. It excludes indirect taxes on goods and services, which accounted for roughly 32% of all taxes and tax-like payments in 2022. As mandatory insurance contributions nominally paid as the "employer's share" but based on individual wages are not included in the data, they have been roughly estimated and included as positive contributions. The balance is based on annual incomes, so the first year in each sum includes the unemployment benefits on the year when the entitlement ends. Only those exiting shortly before exhaustion have a positive contribution to the fiscal balance. In comparison, for those exiting after 0–50 weeks in unemployment, the corresponding cumulative direct contributions range from roughly +20 000 to +50 000 euros at the end of the 8th year after exits.

Figure 24: Cumulative wages, relative to UI expiration

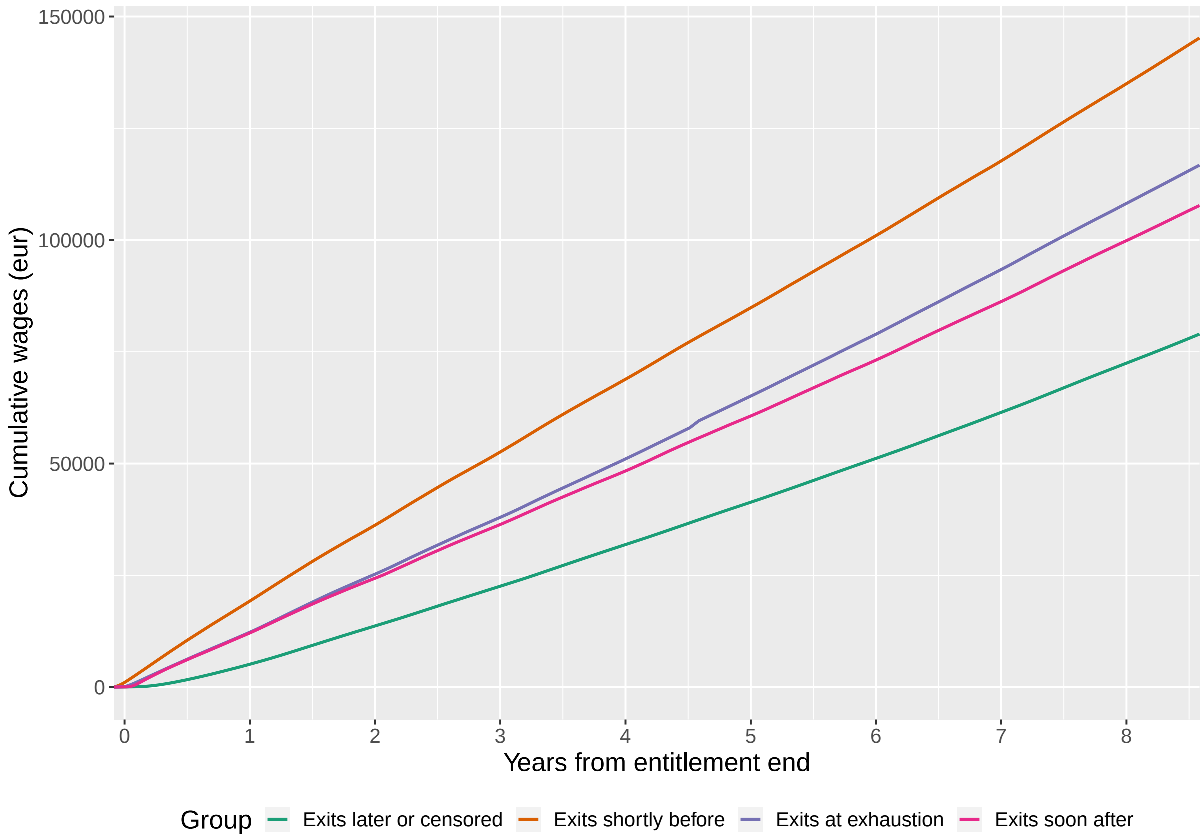


Figure 25: Share employed (open labour market), relative to UI expiration

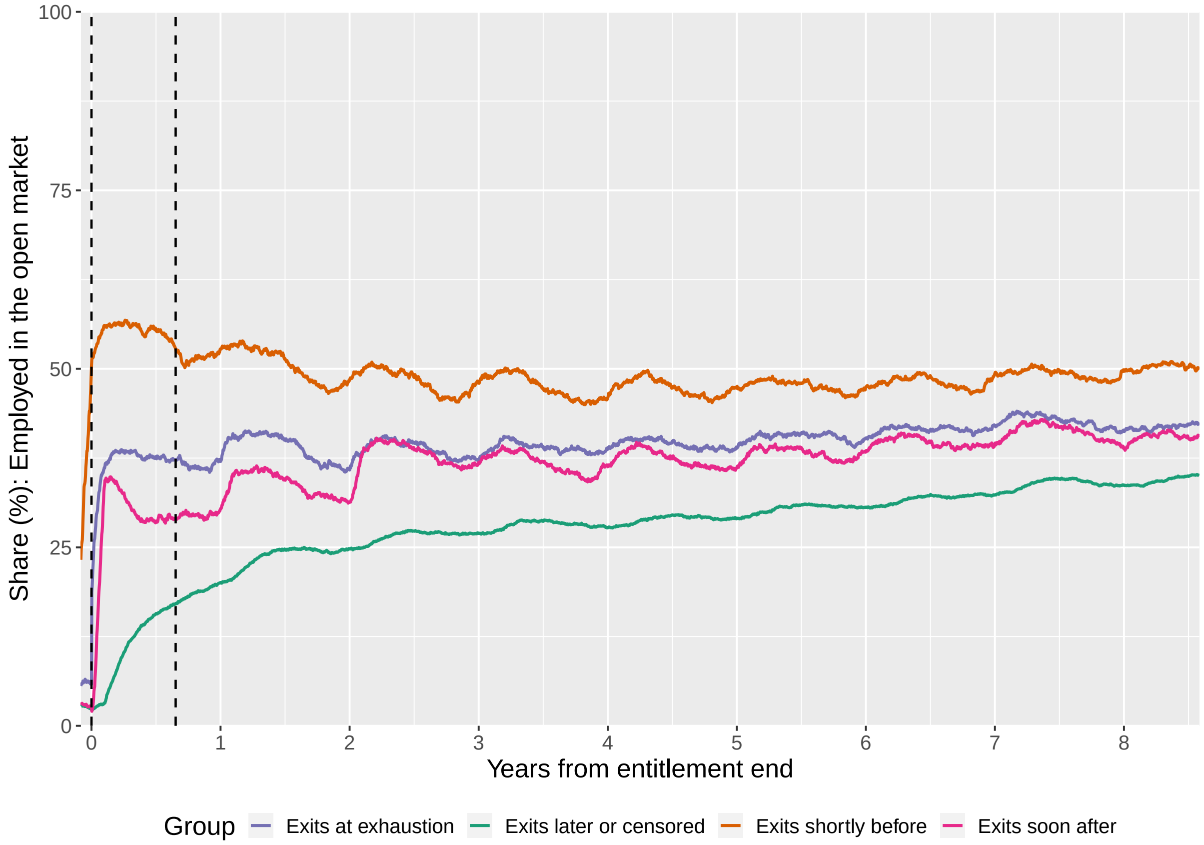
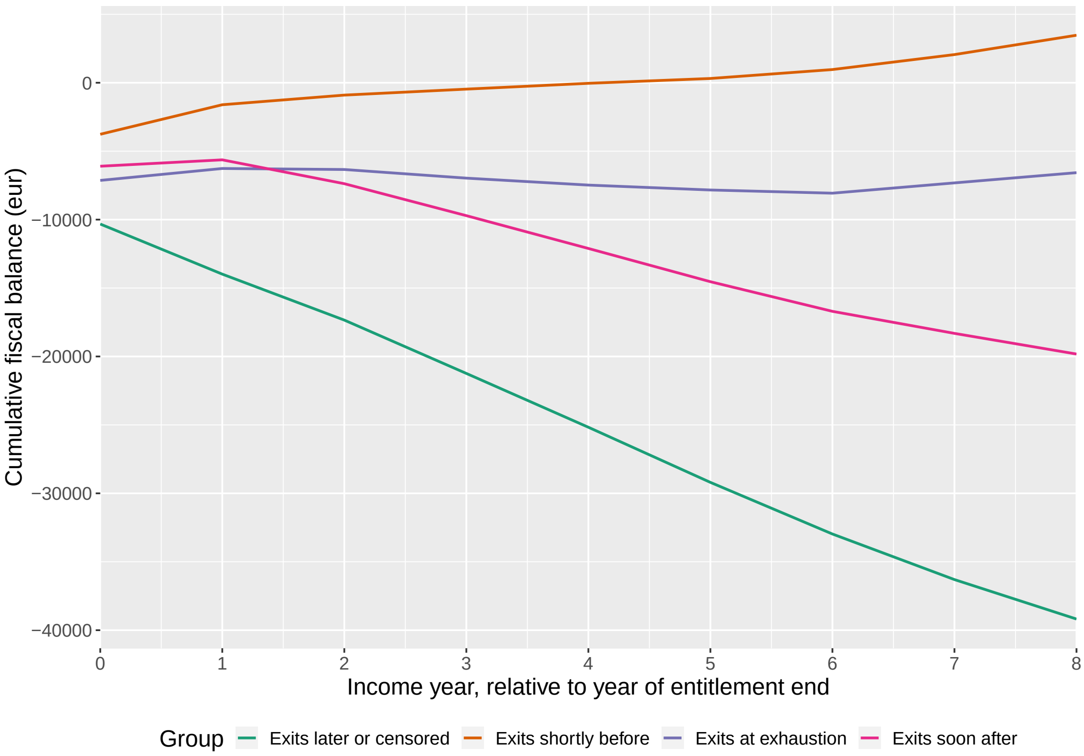


Figure 26: Cumulative direct net contributions to public finances



E.4 Exits at exhaustion are often towards pensions

Figures 27 and 28 show the primary source of income per year and group for 8 years after the end of the entitlement. Those exiting at the spike are much more likely to collect full-time pensions relatively soon after their exit. The majority of the short-term pensions are disability pensions. These outcomes are clearly rarer for other groups who come close to exhaustion but do not exit at the spike, exemplified here by those exiting shortly before exhaustion. (The contrasts for the spike exits vs. those who exit shortly after are qualitatively similar.)

Another overrepresented income group is having no observable or very low incomes. The available demographic data (household or family size or status in household) does not offer an immediate explanation for how this group actually finances their living. Having entrepreneurial or property income as the primary income source is also somewhat more common for those who exit at the spike.

For a higher-frequency analysis, figures 29–30 use the Incomes Register for classification. As this data only covers benefits from 2021, the population is now limited to persons reaching the 95th benefit week in the first half of 2021. Otherwise, the population is restricted and divided in the same way as before. This income data does not cover social assistance and more privacy-sensitive income categories such as disability pensions or sickness allowances, which probably explains why an even higher share of those exiting at the spike have no observed income.

Figures 31 and 32 compare the change in net income of those exiting at the spike in two ways. Figure 31 considers the income change in the third month after an *exit* for the relevant groups, i.e., those actually exiting around exhaustion. It illustrates that those exiting at the exhaustion typically take a large hit in their observable incomes, while those exiting at other times are typically able to quickly improve their incomes.

Figure 32 shows the income change in the first month after the *entitlement*, again for relevant groups, i.e., those actually reaching the expiration time. The graph demonstrates that all groups typically experience reductions in observed incomes as the earnings-related benefits expire, but those exiting at the spike often face particularly large ones.

In both figures, the income change is relative to the last month before the relevant event, and net monthly incomes are calculated based on observed employment income, benefits, taxes and distraint. Again, while the high-frequency Incomes Register data on benefits covers 90% of transfers on the population level, it specifically lacks social assistance, disability pensions and sickness allowances.

Figure 27: Largest income source by year, exits at exhaustion

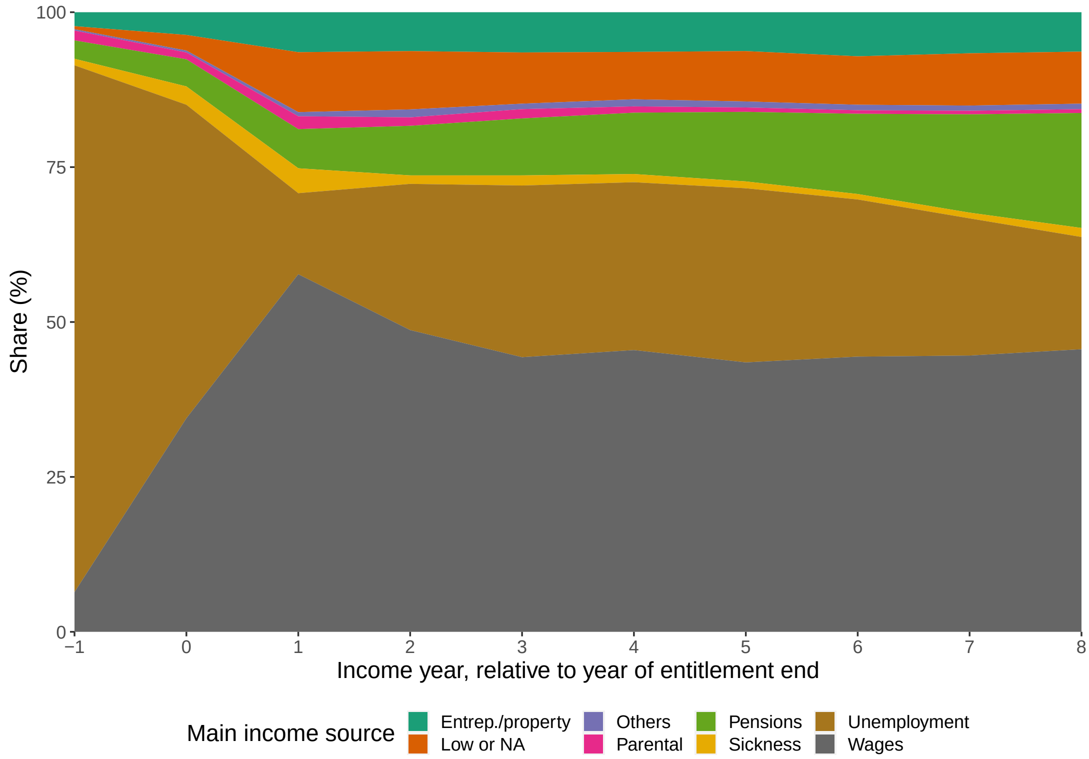


Figure 28: Largest income source by year, exits shortly before exhaustion

Figure 29: Largest income source by day, exits at exhaustion

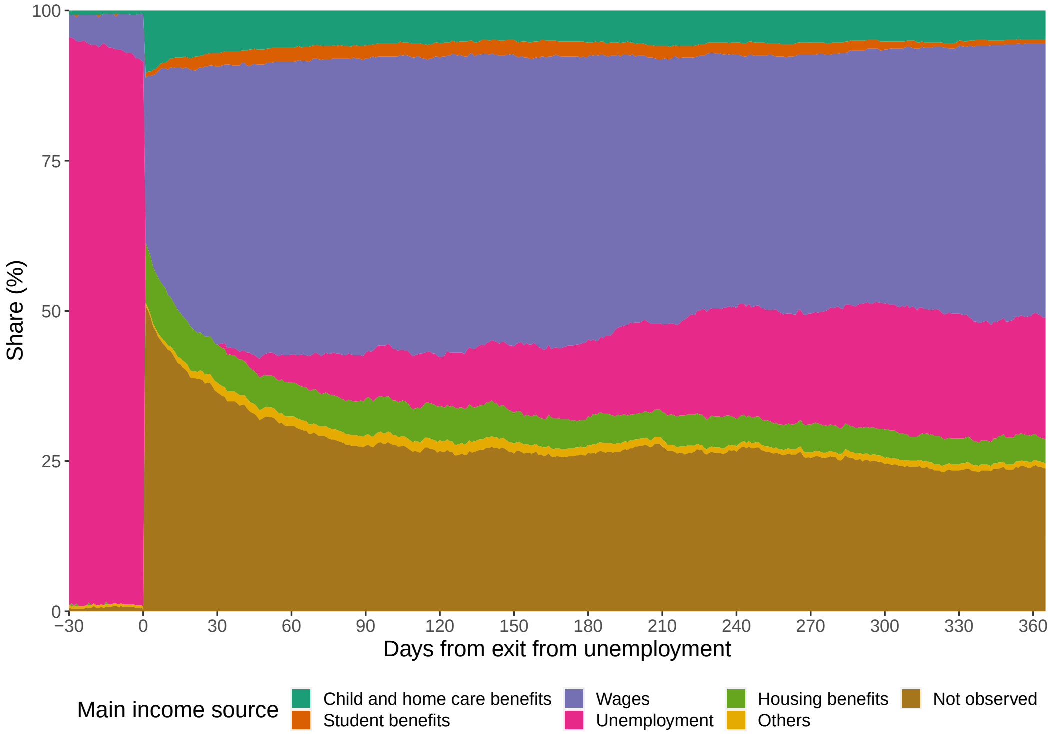


Figure 30: Largest income source by day, exits shortly before exhaustion

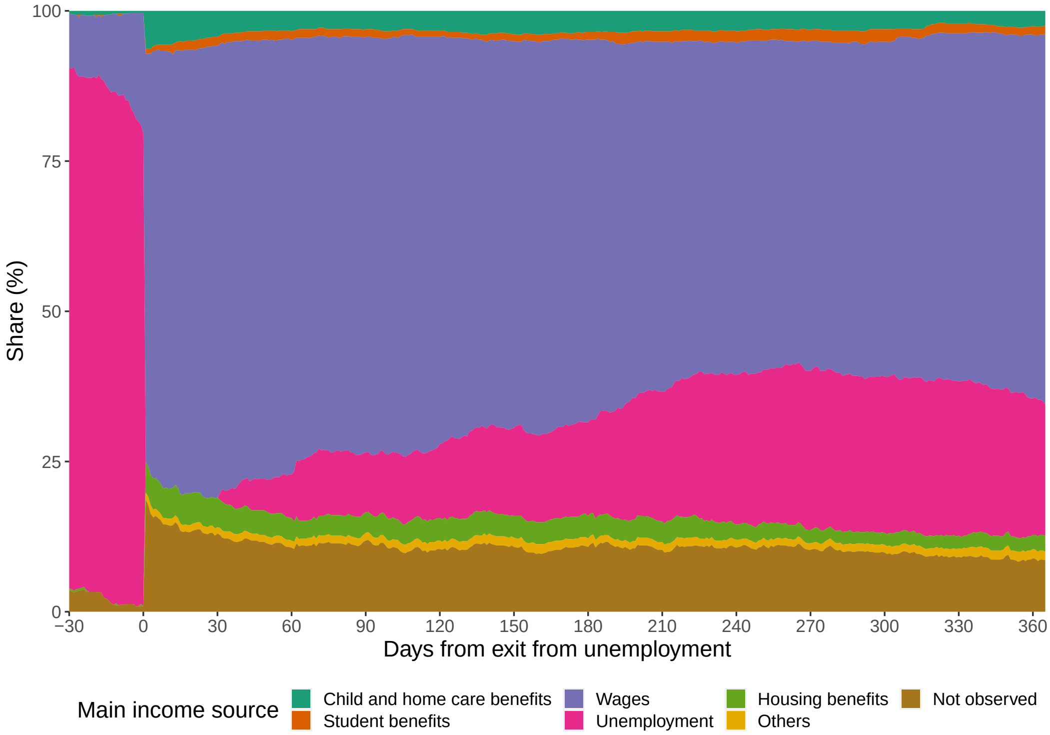


Figure 31: Income change, third month after an exit

Figure 32: Income change, first month after the end of the entitlement

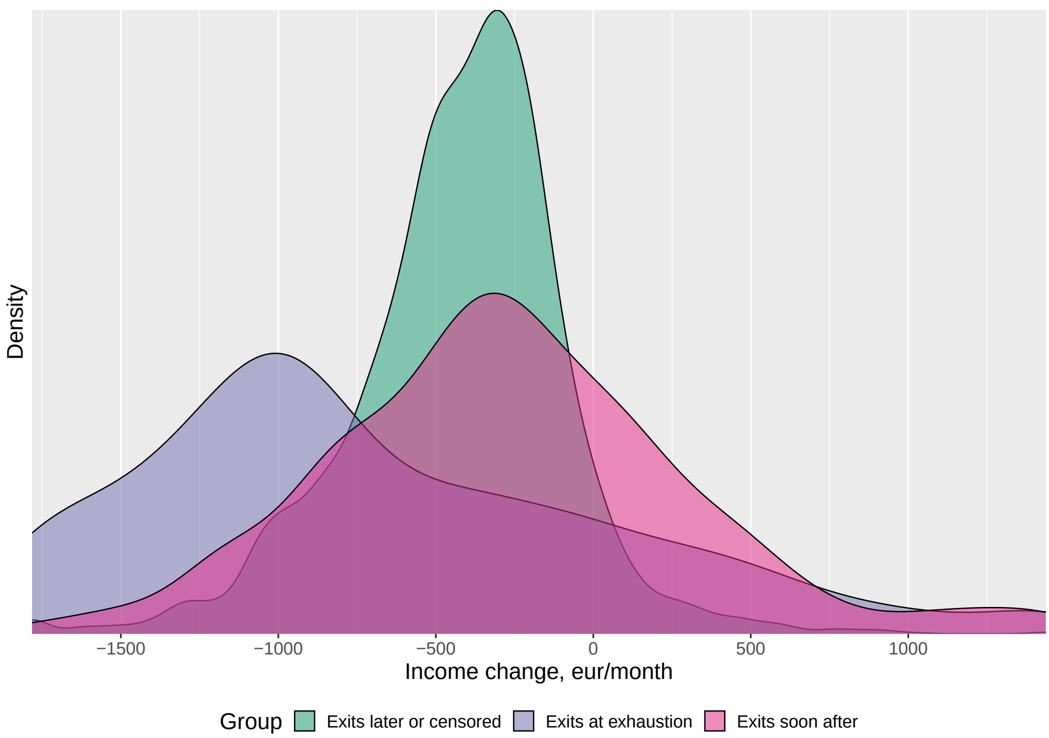
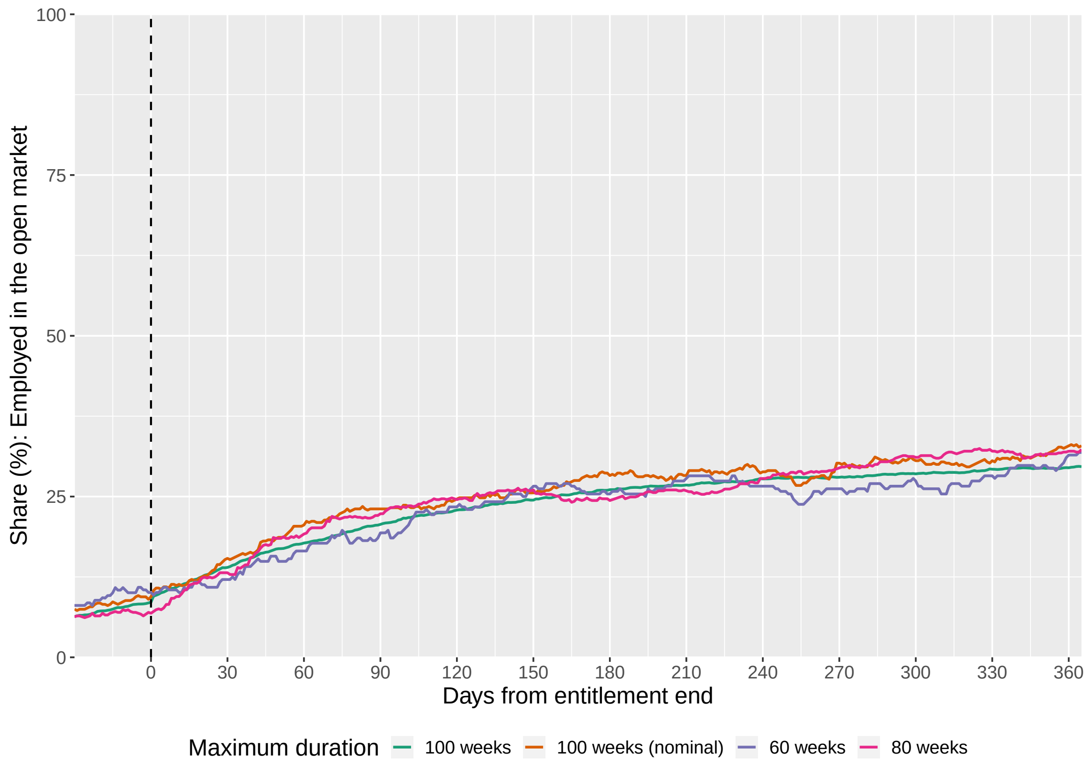


Figure 33: Share subsequently employed, by maximum entitlement



E.5 Similar employment rates after different entitlements

Figure 33 in this subsection again illustrates employment status for spells coming close to UI expiration between 2010 and 2018. This time, the cases are groupped by the entitlement: 100 weeks, 80 weeks or 60 weeks. The targeted reform's case where the funds continue to pay benefits until the 100 weeks, but for the last 20 weeks at only the flat rate of the assistance, is included as a separate case, labeled "100 weeks (nominal)". In this case, time is tracked from the agency switch time at 100 weeks, as there was not much going on at the 80 weeks mark.

Because some of the possible entitlements only have a small number of observations coming close to exhaustion well before 2020, these cases cannot be further split into groups by actual exit time. Thus, each group includes both exits and cases where unemployment continues well beyond the entitlement. While the size of the sample makes some of the curves appear quite noisy, the overall finding is that the subsequent employment rate is quite insensitive to the maximum entitlement – and, by implication, to the cumulative duration of the spell at the end of the entitlement. This is even more surprising when one considers that one of the groups (those with short histories) is quite different from the others, as their short work histories often mean significantly younger ages and lower prior wages.

Appendix F Predicting exits at the spike

In the previous appendix section, the focus was on patterns in post-exit outcomes for exits around the spike. In this section, the aim is to find out whether exiting at UI expiration time exactly can be predicted with past observables – and, ultimately, whether such predictability might help explain the bunching of exits at the spike.

To this end, a sample of long-term unemployed is again selected, as in the previous section: those who consume at least their maximum entitlement minus five. Data is collected for spells reaching the mark between 2010 and 2018, at the time of this mark. As earlier, those aged 57 or higher at the end of the entitlement, or eligible for additional days of UI, are excluded.

With the chosen sample, the prediction is for a conditional predictability of exiting at the spike, given a person has already been in unemployment for fairly long. If one were to predict exits at the spike across all unemployment spells, the prediction would very likely also pick up variables that simply predict longer-term unemployment overall.

The prediction approach is gradient boosting (XGBoost). Most of the hyperparameters are the defaults from the R `caret` package; the training is done on a 75% split with repeated cross-validation, using the area under ROC as as the metric. The following covariates were used as inputs:

- age
- number of distinct employment spells in the last 5 years and their mean duration, number of distinct employers in the last 5 years, time from last job, and the entire observable prior employment history
- number and mean duration of distinct unemployment benefit spells in the last 5 years (payment level data)
- months in unemployment in the last 22 years (annual data)
- the base wage used for benefits
- rate of FTE days consumption over the last 3 months (as a proxy for partial unemployment and its intensity)
- year
- education level
- 2-digit profession
- activity preceding unemployment
- unemployment fund
- residence permit, nationality (at 26 groups), and immigrant background
- the entitlement (60, 80, or 100 weeks)
- income data for the preceding year: property, entrepreneurial, social assistance, study grants, sickness allowance, parental/child home care allowances, and gross received transfers, all separately
- population density in postal code area
- the average number of jobseekers and open vacancies matching region and 4-digit profession over the preceding 9 months (the calculation of these measures is described in appendix Z)
- household size, age of the youngest child if any, status in household, and marital status
- gross debt and car and home ownership in the last year, and changes in debt and ownership status from 4 years ago
- survivors' and disability pensions (fixed term rehabilitation assistance and part-time disability pension as distinct categories)
- prior job placements (duration and count over the last 12 years and time from last placement)
- employer owner type, legal form and industry for the latest job
- primary language
- gender
- employment service segment, assigned by the PES office
- job seeking plans (count over the last 11 years, type of last plan, and time from last plan if any)
- sanctions imposed in labour market policy statements from the PES office over the last 12 years (number and time from last if any)

Across these variables, the algorithm identified the following important variables as the 11 most important ones: age (+), wage (+), work history (+), cumulative months of unemployment (−), population density (+), time from last jobseeking plan (+), having received disability pension (+), number of vacancies (+) and jobseekers (+) matching region and profession, and property income (+). (This subset also achieves almost the same prediction accuracy as using the all variables above.) The sign in the parentheses indicates whether higher values of the variable are individually associated with a higher (+) or lower (−) probability of exiting at exhaustion.

Across individual predictors, prior part-time disability pensions are particularly predictive: when observed, they translate into a more than 50% probability of an exit occurring at UI exhaustion. It is also worth noting that across persons coming close to UI exhaustion, those with more prior unemployment are less likely to exit at the spike than others; similarly, those who do exit at the spike are less likely to return to unemployment afterwards than the close counterparts.

That property income and disability pensions help predict exits at the spike aligns well with the findings on outcomes in the previous section. However, these predictors are still either fairly weak alone (for property income and other pensions than the part-time disability pension) or quite rare (only 0.6% in the sample received the part-time disability pension). Thus, at best, transitions to pensions or living off property or business income are only partial explanations for the observed exit spike.

Despite the extremely rich data, exits at the spike only show limited predictability overall. The prediction scores high on specificity (0.997 using the conventional 0.5 predicted probability cutoff), but low on sensitivity (0.116). Put differently, it appears that for a small subgroup of the sample, the timing of exits at the spike can be predicted fairly well, but most of the spike exits do not appear to be highly predictable.

Appendix G Potential technical explanations for the exit spike

Missing data. This hypothesis posits that some UA follow-up data is simply missing, causing an artificial exit spike to occur. This is unlikely. Data for unemployment benefits post the observed exit spike come directly from the administrative agency (Kela). Most individuals appear in these data at some point before and after the exit spike, and the data are consistent with other high-quality data such as the Incomes Register. In addition to the processed data, the raw original data was carefully examined to ensure that no data was erroneously dropped. While the data includes a large number of corrections and adjustments to prior payments, such cases are not appearing with higher frequency around the end of entitlement than anywhere else.

Misinformedness about entitlement. According to this hypothesis, the unemployed were not reacting to exhaustion because they did not except it. This is also improbable. The insurance funds inform the applicant of their entitlement duration several times during unemployment. With each payment, a notice is given to the claimant which includes both accumulated days of entitlement, and the maximum entitlement[2]. Additionally, the funds send additional decisions to beneficiaries when they exhaust the entitlement and can no longer claim unemployment benefits from funds. These decisions specifically mention the person may be eligible to apply for UA from Kela.

It is still possible that the entitlement cut came as a surprise to some individuals during the targeted reform. This is because the reform only applied to a minority of unemployed, which means there were less news articles and public discussion about it than the later reform. However, this is an implausible explanation for why the *spike* did not move. If the fall in benefits at UI exhaustion is what is creating the spike in the first place, this drop should have been noticed at *latest* after benefits actually fell. There were still 20 weeks in between the old entitlement and the new, but there are no significant changes in exits during this time. Additionally, even if misinformedness would explain the non-response in the spike, it is less likely to explain why the short-history group also responded weakly to the universal reform, which clearly affected other groups.

Different benefit rules. This hypothesis states that the spike occurs because some who were eligible to UI are not eligible to UA. Again, this is unlikely to be a major driver for the spike. As noted in appendix A, unemployment assistance and insurance do differ slightly in rules, even when the latter is being paid at the flat-rate amount. The assistance can be reduced for children living with their parents, or due to means-testing against the spouse's income or non-wage income. This is unlikely to explain much of the spike for two reasons.

In most cases, the additional rules reduce the amount being paid instead of making the

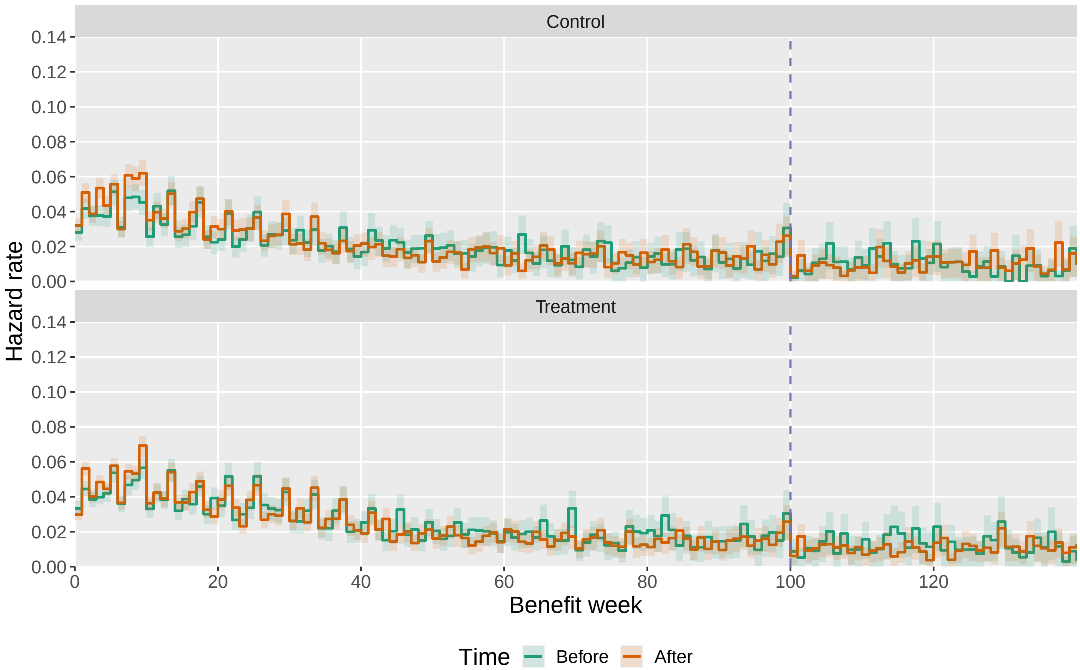
[2] For example, "6/400 benefit days" paid. One benefit week = five benefit days.

applicant ineligible for benefits, meaning the reductions are usually observable. The reductions are quite rare among the unemployed overall. They might still be more common among those with long UI spells, but even in this case, one would expect a continuous distribution of spousal or non-wage incomes (and a distribution of reductions). In the data, the reductions are also rare among those who are observed transitioning from UI to UA. Even if some of these reductions may themselves incentivize the unemployed to exit unemployment, at least some of such reductions should still be observable, since not everyone has a viable outside option. In particular, other relevant benefits such as the social assistance specifically require that the individual first applies for unemployment benefits. Finally, while observed business and property incomes are somewhat more commonly observed among those who exit at UI expiration (compared to those who exit shortly before or after), they are still a small minority of these exits.

Second, simply changing the benefit *type* does not appear to cause an exit spike. As explained in A, there are actually three types of benefits: the earnings-related insurance paid by funds and two types of unemployment assistance: the basic unemployment allowance and the labour market subsidy, both paid by Kela. The basic allowance shares most of the criteria of UI, including the maximum duration, but only pays the flat-rate level and is paid to those who do not fill the fund membership criteria. For the targeted reform, for weeks 80–100, the benefit paid to short-history individuals was *identical* in both criteria and levels to the basic allowance, except for the agency paying the benefit. There is no clear spike in spells starting on the basic allowance, presumably because there is no shift in the benefit agency at exhaustion; see figure 34.

Different stringency by payers. According to this hypothesis, the agency shift causes an exit spike because different agencies interpret the same rules differently. This explanation does not appear plausible. It is the PES offices that follow that the individual satisfies their obligations, and the offices regularly issue statements about each individual's eligibility to the payment agencies. (The available data has about 60 million such statements starting from the 1990's.) Discretion left to the payment agencies is constrained; thus, if a person was eligible to UI, their application for UA after exhausting the entitlement is unlikely to be rejected unless their behavior actually changes.

Figure 34: Exit hazard, basic unemployment allowance spells.



Indeterminate exit hazard from basic unemployment allowance. Non-parametric discrete time Kaplan-Meiser estimate. Sample restrictions are the same as used for the targeted reform, including work history.

Appendix H Exits to education

Overall, slightly fewer exits for the short-history group are towards observed job-finding, compared to other work histories. As the short-history group is also younger and slightly less educated than the other groups, it is natural to ask whether they might be leaving unemployment to continue their education when the opportunity arises. This would be one potential explanation for them also reaching the maximum duration less often, and being less responsive to entitlement cuts if they can treat education as a fallback option.

The magnitude of potential transitions to education could be assessed in three ways. While high-frequency data on attending education was not available, data from Statistics Finland for the main activity at the end of the years 1987–2019 was.[3] Second, data on collecting student grants from the government was available at the semester level. Based on this, a generous sub-hazard was calculated, classifying everyone who was considered a student at the end of the year when they exit unemployment (measure A) or collecting the student grant for a semester in 6 months after the exit (measure B) as transitioning to education. To construct a reasonable upper bound, this destination was allowed to override any potential job-finding status.

Figure 35 illustrates the education-specific exit hazard for both measures separately. The sample used covers UI spells starting between 2008 and 2016, excluding spells starting on furloughs, voluntary quits, and individuals aged 55 or above, as in the main text. While the hazard is clearly larger for those with short work histories in relative terms, in absolute terms such exits are very rare, even with the very generous definition.

As a third measure, for years 2021–2022, high-frequency data was available for student grants from the Finnish Incomes Register. In this case, comparable hazards would have very low power, as detailed benefit data is only available until end of 2021 (data from the Incomes Register for 2022–2023 also covers unemployment benefits, but lacks various key variables to determine new entitlements comparably). Thus, instead of hazards, figure 36 covers the *share of exits* in 2021–2022 that are potentially towards education, classifying student grants collected within 3 months of an exit as transitioning to education. Again, while slightly more of the observed exits might be into education for the short-history group, the overall rates are low, and the differences between groups are small. In particular, transitions to education are on average too rare to explain the gap in job-finding rates across groups, and probably too rare to plausibly explain why the short-history group does not appear to respond to entitlement cuts.

[3] This data also classifies being in labour market training as being a student. Since in these cases the person is commonly also collecting unemployment benefits and considered unemployed in the main text, such cases were checked separately and assigned a different status if necessary.

Figure 35: Exit hazard towards education in 2008-2016

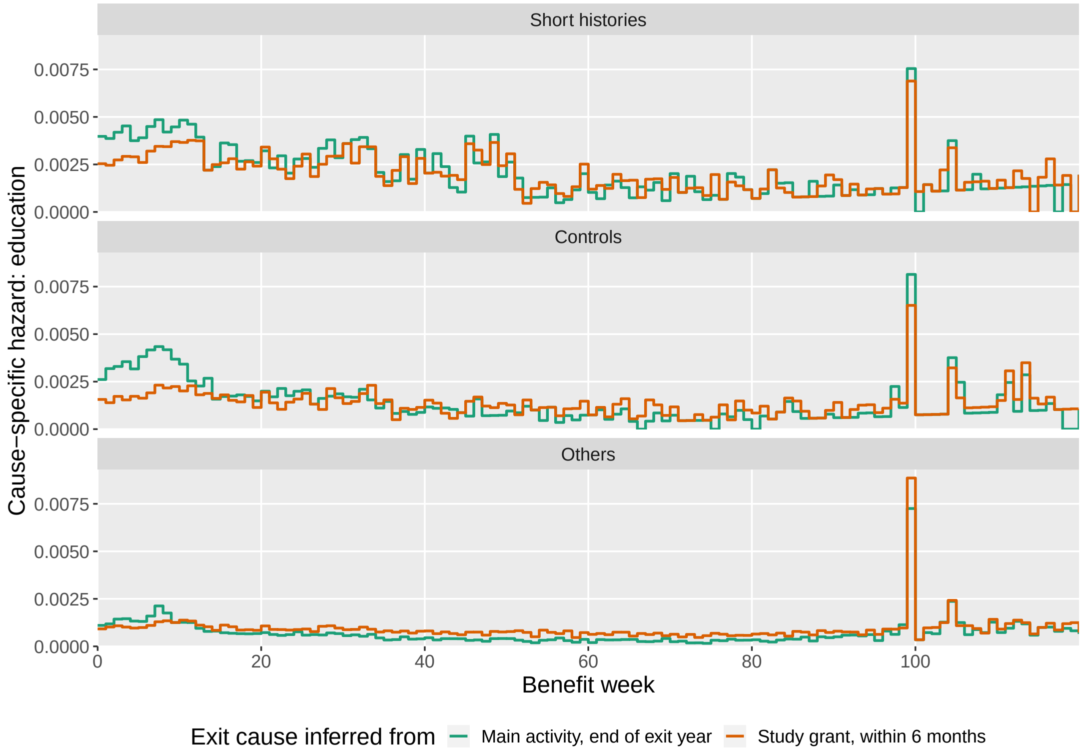
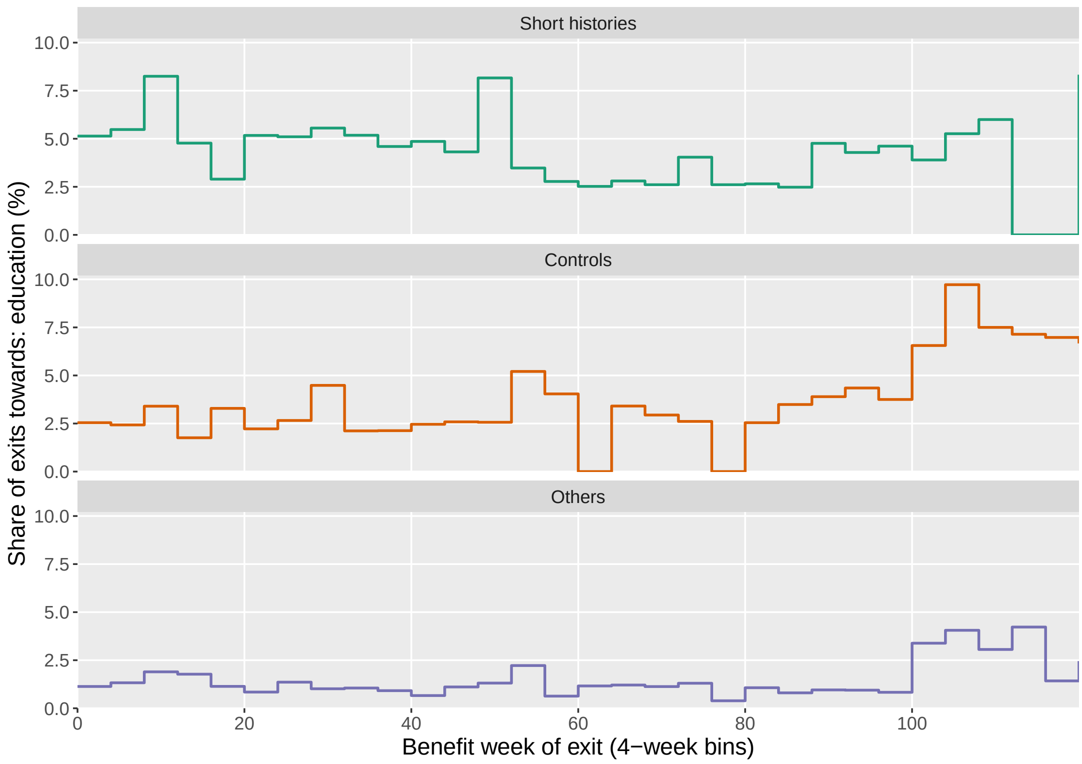


Figure 36: Shares of exits that are to education in 2021-2022



Note that the figure covers fractions of exits on a benefit week, and *not* cause-specific hazards.

Appendix I Potential alternative measures for unemployment

The Finnish context offers at least three possible ways to measure time in unemployment: time without a job (non-employment), time as a registered jobseeker, and time collecting benefits.

Card, Chetty, and Weber (2007) show that measuring unemployment by the time in registered unemployment can severely overestimate the exit spike in Austria. In Austria, persons are not required to remain register to remain eligible for benefits after their UI expires. Exits from the register may thus not indicate whether the person continues to collect benefits. In Finland, the same registration requirement applies for both benefits.

The approach taken by Card, Chetty, and Weber (2007) is to measure time in non-employment to estimate the job-finding hazard. However, using job data *alone* may severely underestimate the economically relevant exit rate for at least two reasons. First, two spells between jobs with similar durations may face very different times until UI exhaustion. This is because individuals do not always earn a new UI entitlement between spells of non-employment, might earn a new entitlement during a spell, and may consume the entitlement at different rates (e.g. during part-time employment). Using benefit data with direct information on the used up entitlement takes care of this issue. Second, spells that start with an exit from a job may end in meaningful transitions to non-job destinations, such as education (which may improve long-term employment outcomes) or self-employment, which is often not adequately captured by the available high-frequency employment data.

Kyyrä, Pesola, and Verho (2019) demonstrate empirically that the question is relevant: the spike in exits from the benefit system may be severely underestimated if one cannot track true time to UI exhaustion accurately. Fortunately, the Finnish payment data contains reliable, high-frequency data on the used up entitlement and new entitlements earned. Kyyrä, Pesola and Verho combine this data with detailed information on jobs to estimate the exit hazard and the job-finding rate. As this paper uses the same underlying data, the unadjusted hazards presented here are very close to the ones estimated in the paper by Kyyrä, Pesola and Verho.

Both the present paper and the earlier paper on the Finnish exit spike thus measure exits from the *benefit system* accurately. The remaining question is whether individuals may be forgoing benefits at specific times and for specific reasons, such as at the agency switch time, making the exit hazard a more biased measure of actual time in unemployment at these times. This is why the adjustment proposed in this paper further combines data on jobs and benefits. Notably, the potential bias to be corrected due to a one or two weeks' delay to the next job after an exit from the benefit system is still fairly small, compared to the potential bias introduced by relying on either the job data or the jobseeking register data alone.

One appealing – but ultimately untenable – option in the Finnish case would be to combine data on registered jobseeking, benefits and jobs. Using a person's status in the jobseeking register would likely lead to more biased estimates for at least three reasons. First, a person may continue to seek jobs through PES even while employed, so a status of "seeking for jobs" is not alone sufficient to determine unemployment. Second, even when considering the designated status in the register, persons are quite often classified ”employed” while also collecting benefits *and* having a valid labour market statement for benefits from the PES office. The reason for this discrepancy is unknown; nevertheless, it indicates that the employed/unemployed status in the register may be a misleading measure of true time in unemployment.

Third, there are no penalties for neglecting to notify the PES when one is no longer unemployed. Conversely, benefit fraud may carry stiff sanctions, and because of the wide coverage of administrative registers in Finland, the risk of getting caught trying to claim undue benefits is high. (Suspected fraudulent cases are rare, estimated at roughly 0.06%–0.08% of annual benefits by Kela, with qualitatively similar magnitudes reported by the Financial Security Authority for UI.)

To demonstrate the issues with the jobseeking data, figure 37 illustrates the differences across the jobseeking register, the benefit data, and the employment data around exit time for spells ending between July 2013 and 2020. The sample is subject to the same general criteria as the main estimation samples (furloughs, older cohorts and voluntary quits are excluded, but prior work history is unrestricted). In this case, ”not registered” means the person either does not appear in the register, or has a status other than ”unemployed”, ”furloughed”, ”on shortened work week”, ”on shortened work day”, ”in labour market training” or ”in employment promoting services”. Figure 38 shows the same follow-up, but this time groupping time on benefits in more detail according to the jobseeking register status.

A large fraction of those unemployed based on the benefit data appear to be employed according to the register *before* the exit, while a moderate fraction continue to appear as registered *after* the exit, even while also collecting wages. This phenomenon is not constrained to the end of the spells: similar shares are designated an ”employed” status in the register from the beginning of medium-length benefit spells.

Overall, benefit unemployment is likely to be the most reliable baseline measure of time on unemployment, even though it will miss those who are not employed and search for jobs but do not receive unemployment benefits. Future research might seek to combine benefit data with further high-frequency data sources to also examine transitions to other destinations than jobs, such as self-employment.

Figure 37: Status around exit from the benefit system

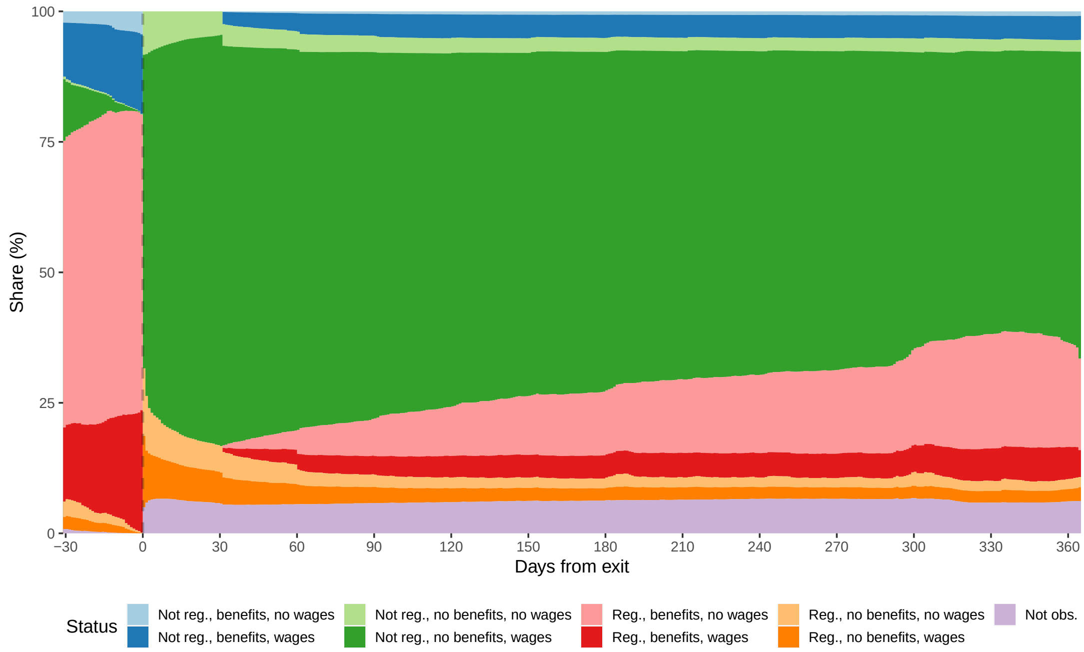
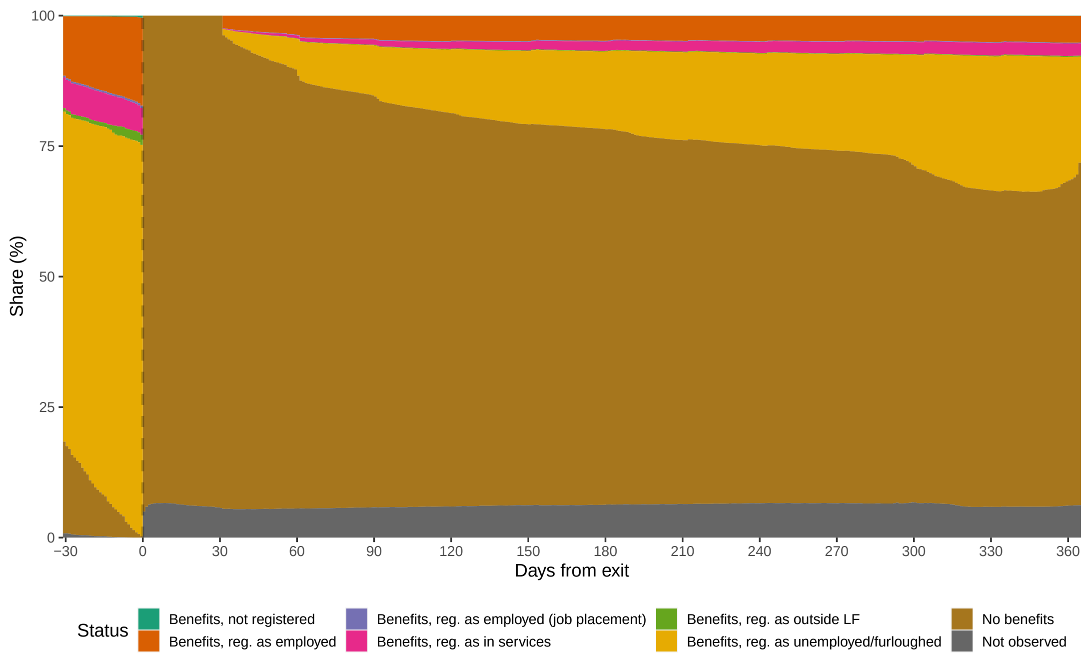


Figure 38: Jobseeking register status around exits



Appendix J Evolution of characteristics by unemployment duration

This section covers the evolution of some key observable characteristics for the population continuing in unemployment. To obtain sufficient power for long durations of unemployment, the sample used for these figures covers fresh UI entitlements starting 2010–2016, and follows the general sample restrictions (furloughs, voluntary quits and older cohorts dropped), but has no restrictions on work history. To avoid noise associated with small numbers of exits on some weeks, the figures show the variables for the *survivors*, i.e., across spells ongoing past a given week. The variables and shares with largest changes in figures 39 to 42 are the share of spells entering unemployment from other activities than unemployment, time from previous observed job, and two foreign nationality measures, all of which trend upwards. Aligning with the earlier evidence on the low predictability of exits at the spike, there are only minor bumps in the composition at UI exhaustion time.

Most covariates, such as prior wage, age, gender, duration of last job, having a fixed term contract, regional labour market tightness or local unemployment rate (fixed at the start of each spell), work history, recent employment, education and profession exhibit much smaller changes across the remaining survivor populations. As an example, the skill level composition, approximated by the ISCO skill classification of observed professions, is illustrated in figure 43. The composition is surprisingly stable over time.

Figure 39: Share of ongoing spells: preceding activity other than job

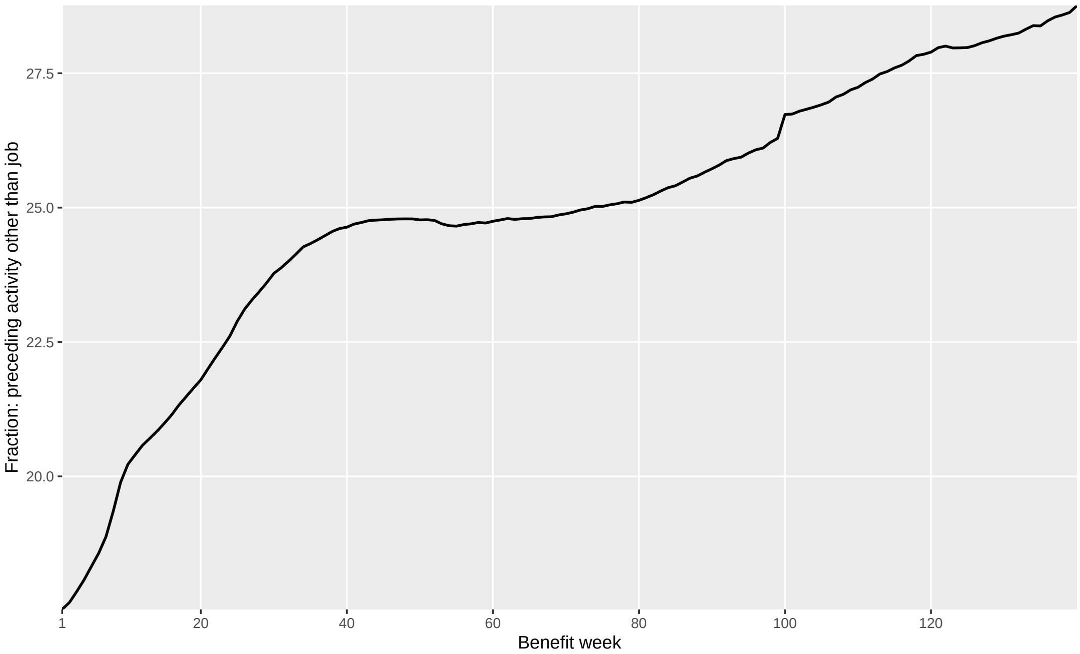
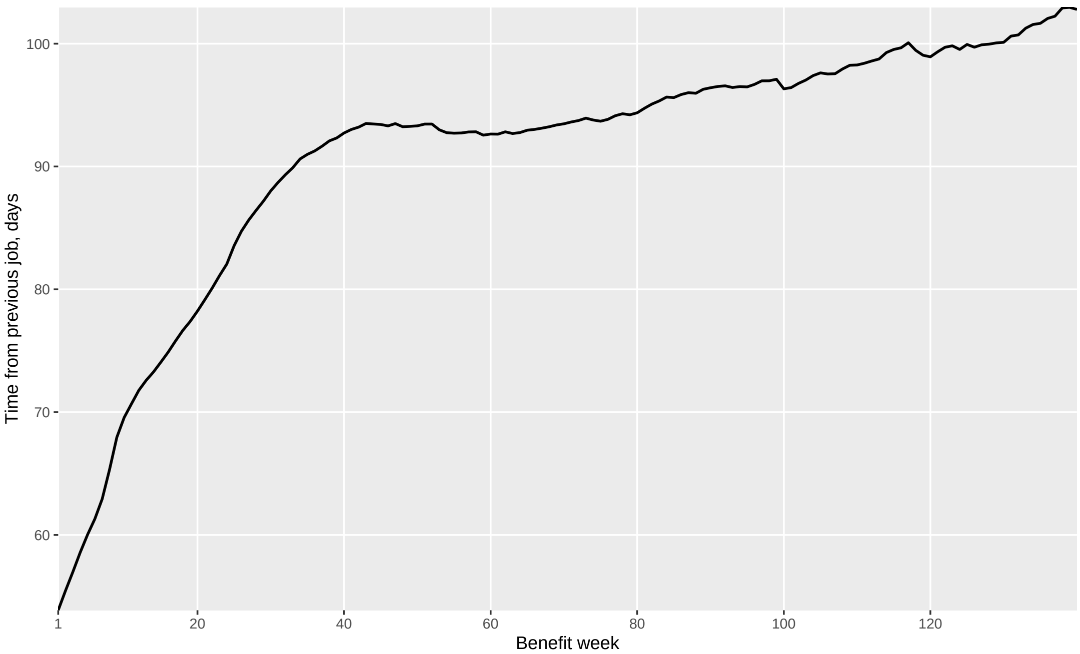


Figure 40: Mean time from previous job for ongoing spells



Time from previous job is fixed at start of spell.

Figure 41: Share of ongoing spells: nationality other than FI, RU, EE

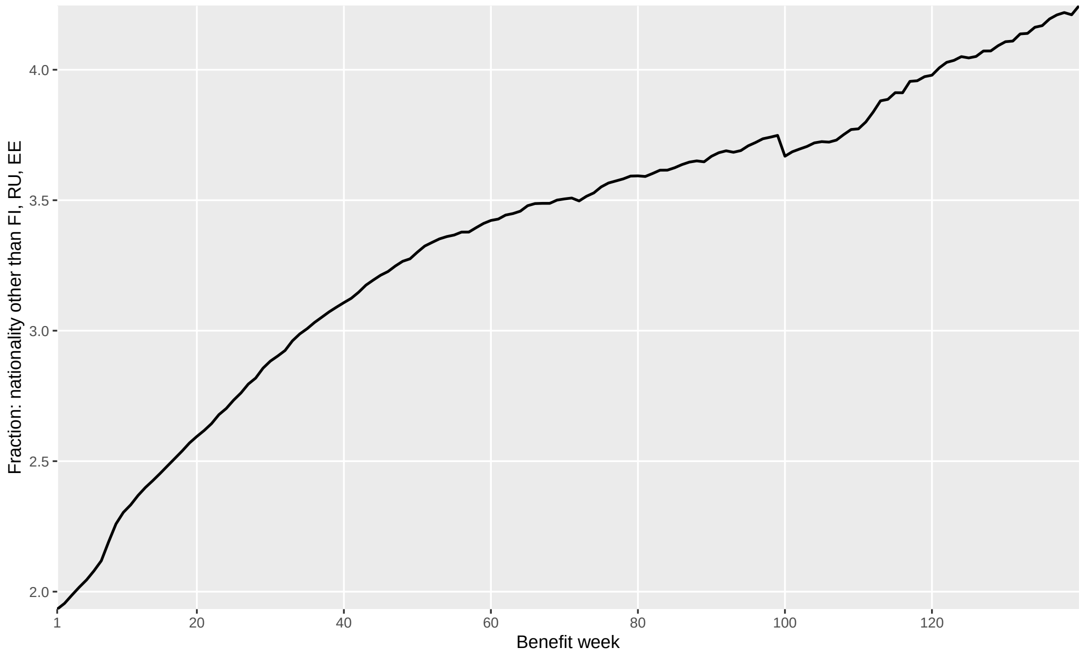


Figure 42: Share of ongoing spells: Russian or Estonian

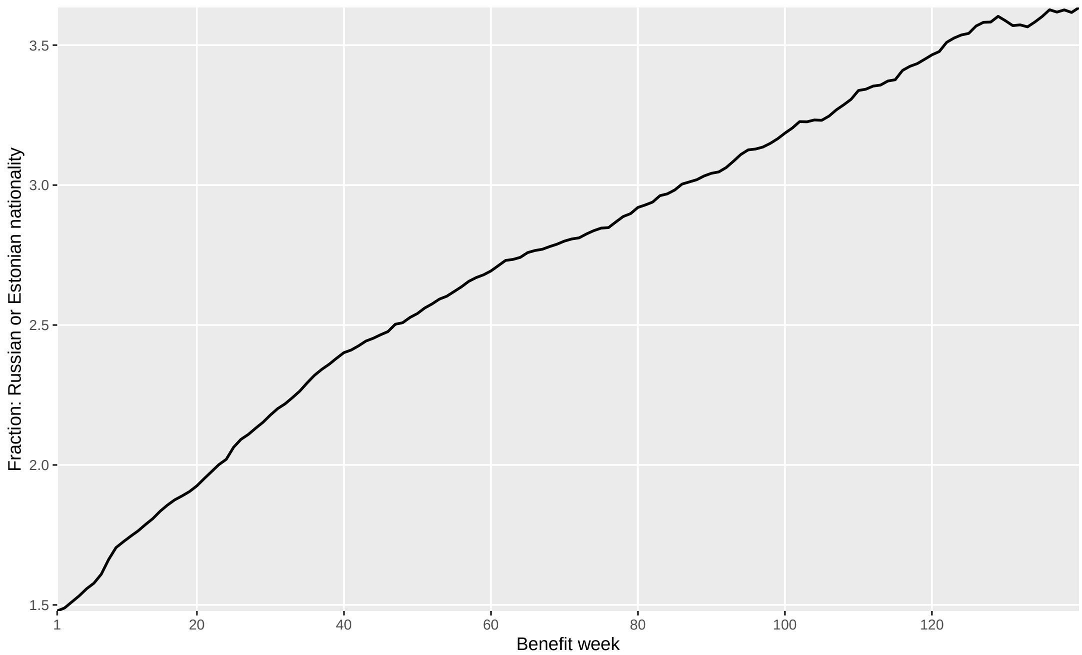
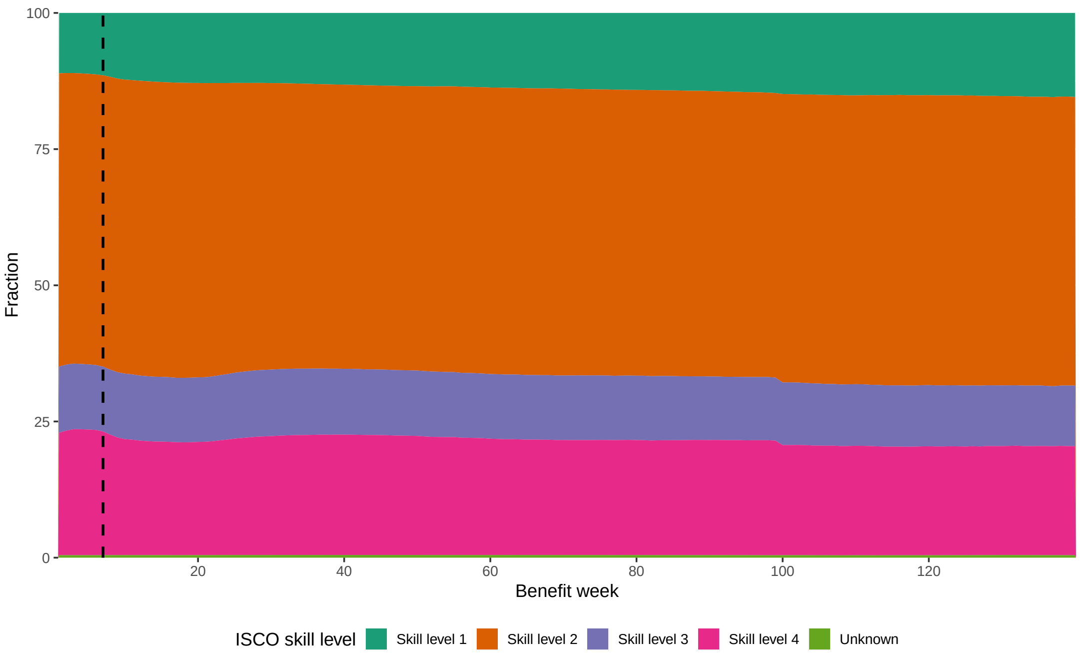


Figure 43: ISCO skill level composition of ongoing spells



ISCO skill level as by ILO, except that persons with unknown or no profession are assigned skill level 1.

Appendix K Alternative setups: targeted setup repeated for 2017

Two setups were used to examine how the short-history group responded to the later universal reform. Unfortunately, the fact that the short-history individuals are only a relatively small minority of all UI recipients meant that they could not be examined as part of the main setup used for the universal reform. There were relatively low numbers of relevant spells starting at the turn of the year. Using them for estimation would have resulted in limited power and unsuccessful weighting for small subgroups, while one of the placebo tests implies that the year-turn setup may not be sufficiently robust without weights.

To achieve reasonable power, both of the setups used for this purpose used the full year 2017 as the post-reform period. To avoid the follow-ups from overlapping with the COVID-19 period for some spells, each unemployment spell was followed for a maximum of 2 years and 7 weeks instead of 2 years and 10 months. As noted in the main text, the same follow-up was used as an alternative for the targeted reform, resulting in qualitatively similar but smaller mean durations and mean responses in that case.

The first setup used was a relatively straightforward replication of the main setup used for the targeted reform. The main differences to the main setup were:

- The main setup uses a total of four years worth of initiated spells: 2012–2013 as the pre-period and 2014–2015 as the post-period. The replication uses spells started in either 2013 (pre-period) or 2017 (post-period).
- The nature of the treatment changes. In the main setup, only the treatment group experiences the entitlement cut, from 100 weeks to 80. In the replication, the entitlement is cut from 100 weeks to 60 for the treatment group, and from 100 weeks to 80 for the control group.
- The pre- and post-periods are much further apart.
- The administrative implementation of the reform was different. In 2014, the time when the short-history unemployed had to change benefit agencies stayed at 100 weeks, although their UI was exhausted at 80 weeks. Since 2017, the agency switch has again coincided with the end of the UI entitlement and the sharp drop in unemployment benefits.

The DiD estimate for the duration of unemployment from this setup was +3.0 weeks (bootstrapped s.e. 0.81), indicating a much weak weaker response by those with short histories, even though they experienced a larger entitlement cut.

A distant base year was chosen because year 2013 is the last year where the short-history status could be inferred for all new UI spells. Using this base year allowed the measurement error to prior work history – and, by implication, the duration of the entitlement –, to be significantly reduced also for the post-period. As with the main setup, this was achieved by first dropping wrongly classified spells from the pre-period, then weighting the post-period groups to their pre-period counterparts by observables.

Comparing distant years is still less than ideal, so another setup was used, with 2016 as the base year and year 2017 as the post-reform period. This time, the other main differences to the main setup were:

- A different control group was used: those with 5–19 years of work history, instead of 3.5–5. This choice was made to reduce classification error, which is much smaller further away from the 3 years threshold, as data from 2013 could not be used to mitigate the error through other means in this case. Almost all of the remaining observable error remains with the treatment group (at roughly 18% for this group), potentially biasing the DiD result.

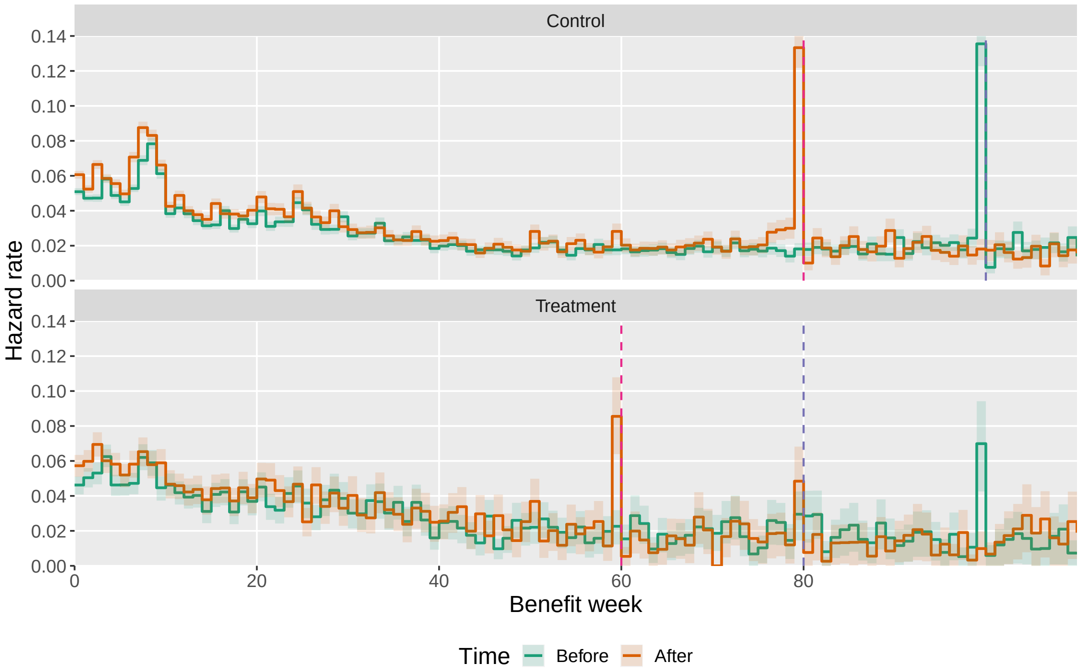
- In this replication, the entitlement is cut from 80 weeks to 60 for the treatment group, and from 100 weeks to 80 for the control group. The reduction is thus the same in absolute terms, although slightly larger in relative terms for the treated (25% for the treated, 20% for the controls).

- In this setup, the administrative implementation of UI was exceptional (agency switch and UI exhaustion time separated) for the treated spells starting in 2016, but normal (agency switch and UI exhaustion coincide) in all other cases.

- The exceptional implementation was phased out in 2019. This phase-out was also extraordinary:
  - The reform stopped applying from 2019.
  - Any short-history unemployed person who had initially entered unemployment in 2014–2016, had not earned a new one by 2019, and had not collected 100 weeks of benefits paid by funds by 2019, would be paid the regular earnings-related UI up to the 100 weeks mark.
  - This even applied to individuals who had already dropped to the flat-rate assistance paid by funds before 2019, i.e., had passed the 80 weeks threshold but not the 100 weeks one. They would return to regular UI.
  - The phase-out directly affected about 23% of those individuals in the pre-reform treatment sample who passed the 80 weeks threshold at some point. The phase-out caused their effective entitlement to be longer than 100 weeks.

The DiD estimate for the duration of unemployment from the second setup was +1.23 weeks (bootstrapped s.e. 0.58). The before–after difference for the treatment group

Figure 44: Exit hazard, targeted reform analysis repeated for 2017



Non-parametric estimate, exits binned to weeks. The shaded areas correspond to a bootstrapped 95% confidence interval.

was a reduction of −2.0 weeks. The before–after difference itself does not have a causal interpretation, as it could also reflect other changes in the labour market or the legislation from 2016 to 2017.

While this second alternative setup suffers from multiple issues, its overall results suggest that the treatment group might be somewhat responsive to the entitlement changes. If this were true, it is also possible that the weak overall response to the targeted reform might be partly explained by the reform's peculiar implementation, and partly by its target group.

In 2017, the treatment group's exit spike clearly jumps from 100 weeks to 60 weeks, i.e., from the old agency switch time to the new one. Despite this large jump, only a small share of the reduction in mean duration, can be directly attributed to the movement of the spike alone. There is now also a smaller spike in the post-period at 80 weeks, which in this case appears to be driven by misclassified spells, who now face both the agency switch and UI expiration at 80 weeks. Figure 44 shows the non-parametric estimates of the hazard rates using the latter setup in this appendix.

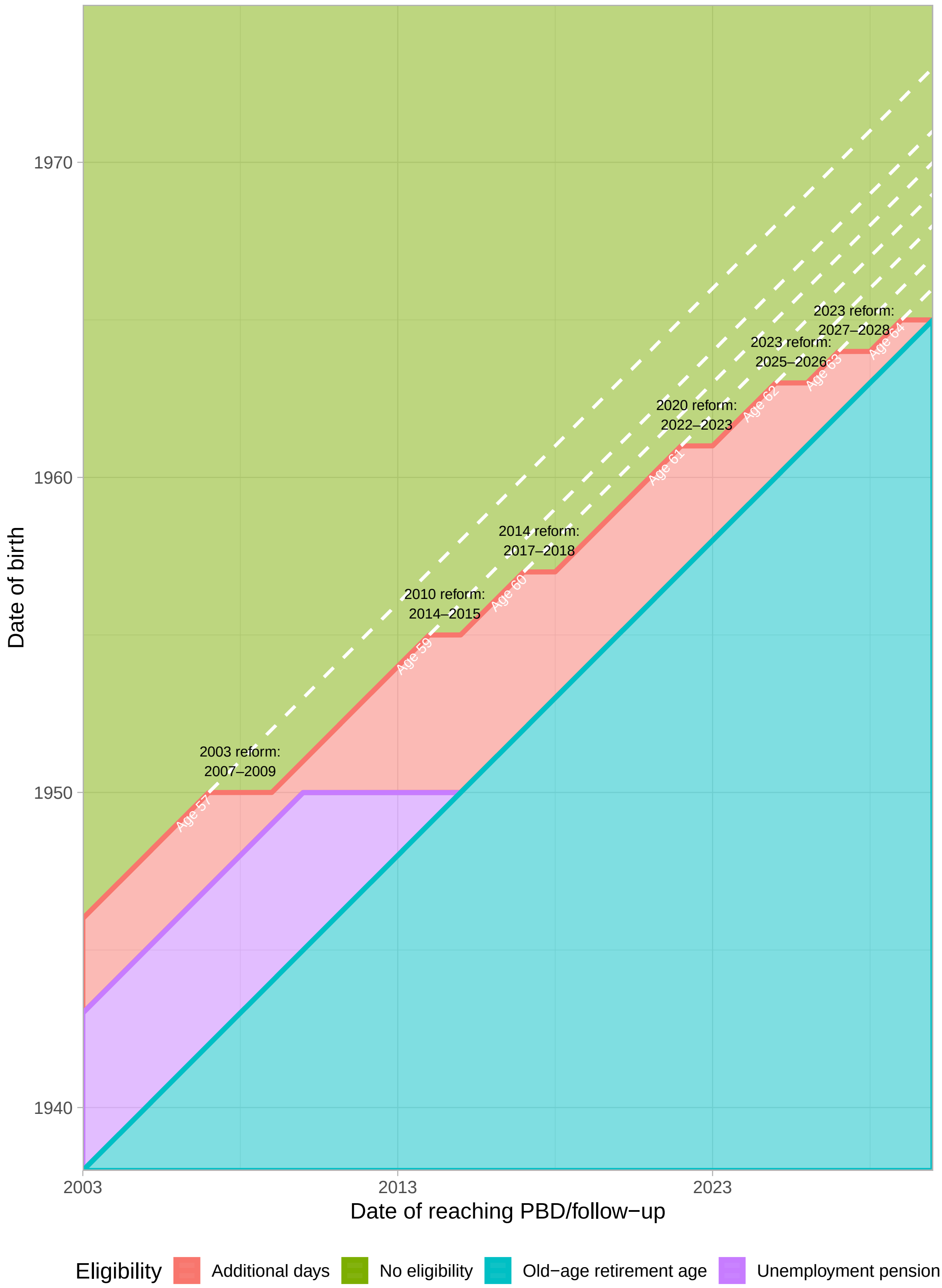
Appendix L Alternative setups: the 58 year old in 2017

As noted in the main text, the universal reform affected almost everyone, except for those aged 58 or higher, who kept the old 100 weeks entitlement. Finland has a long history of age-specific policies for the older unemployed, since at least 1971. These policies were generally speaking expanded until 1997 and have been gradually scaled down since.

At the time of the universal reform, there were two major age-specific policies in place. First, most of those aged 59 or more when their spell started in 2017 were entitled to a system of *additional days*, which effectively means unlimited UI entitlement until retirement age. The effects of this system has been studied by e.g. Kyyrä and Pesola (2020a). Figure 45 demonstrates how persons with a given birthdate have been gradually phased out from the additional days schemes (and a preceding scheme of unemployment pension). Second, those aged 57 or more when reaching their UI expiration date were effectively guaranteed a subsidized job placement when the entitlement ended.

The schemes motivated the following setup. Those aged 58 at the start of their spell were defined a control group, who did not have their entitlement cut. Those aged 57 at the start of their spell were considered a treated, as their entitlements were cut in 2017 by 20 weeks. Otherwise, the same sample restrictions applied as for the estimation samples in the main text. Entitlements started in years 2016 and 2017 were used to obtain a meaningful sample size, which meant that the follow-up times were cut from 2.84 years to 2.16 years to avoid clashing with the COVID pandemic period. The DiD estimate for the effect of the universal reform was a -2.5 weeks reduction in mean unemployment time (bootstrapped s.e. 1.6), close to the estimates for the universal reform in the next appendix when a comparable followup is used.

Figure 45: Old-age policies in unemployment insurance over time



Each horizontal line represents an individual born on the date given by the y coordinate on the vertical axis. Each horizontal point indicates whether the person is, upon reaching the regular end of insurance entitlement on the date given by the x coordinate, eligible to either a policy of extended unemployment insurance entitlement or pensions. The dashed diagonal lines represent phaseouts in eligibility.

Appendix M Visualizing the follow-ups

Figures 46 to 49 illustrate the *actualized* longer followups (2 years and 10 months for unemployment, plus 1 year for the re-employment job if any). Figures 50 and 51 show the macroeconomic environment around the time, highlighting the potential follow-up durations for the targeted reform after the reform as an example.

Figure 46: Spells followed per week before the targeted reform.

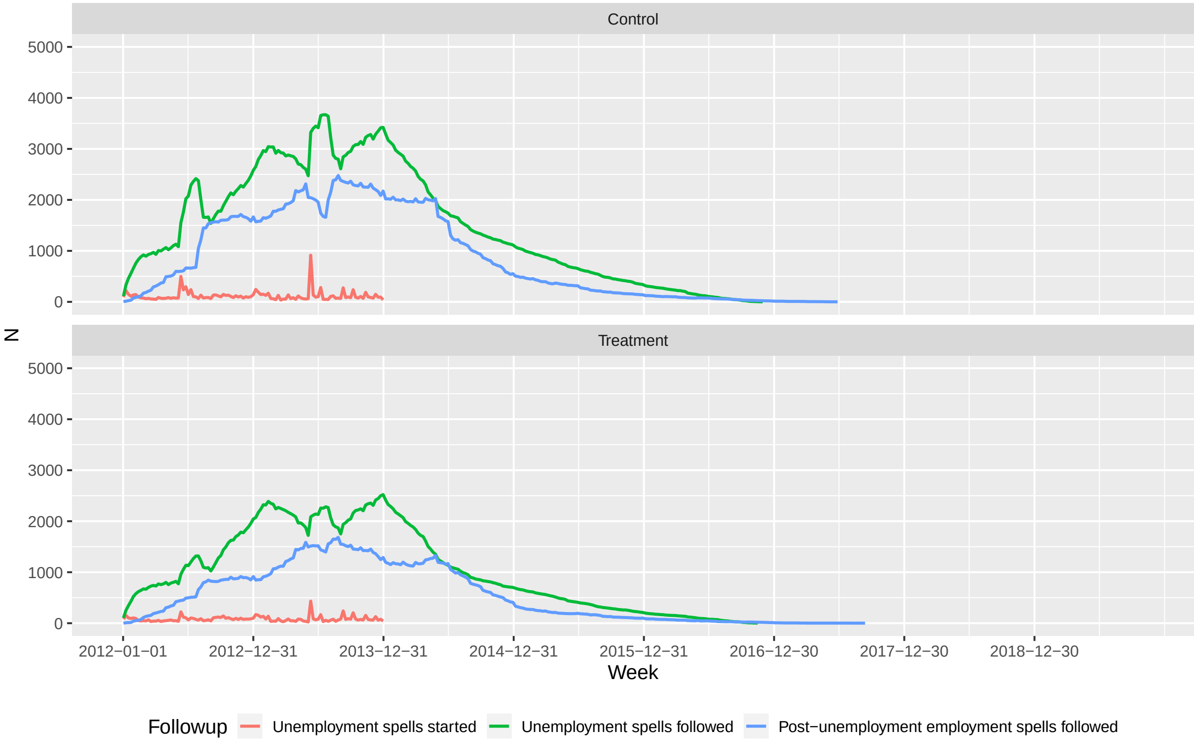


Figure 47: Spells followed per week after the targeted reform.

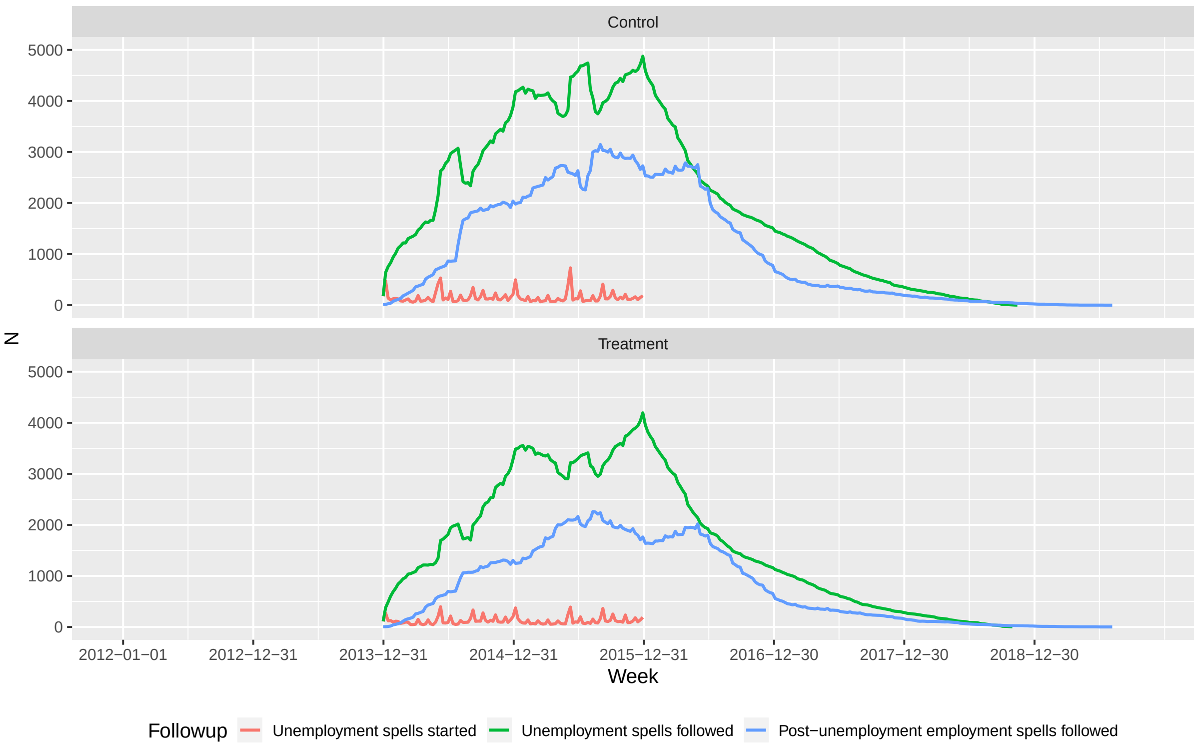


Figure 48: Spells followed per week before the universal reform.

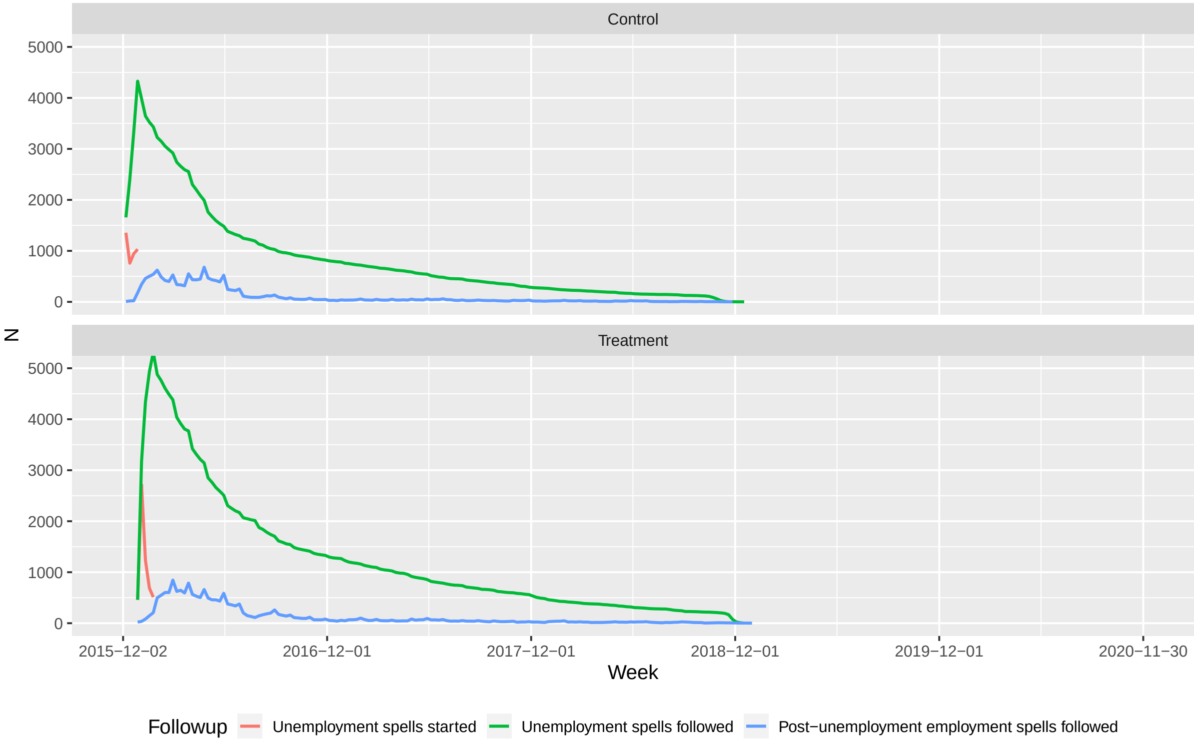


Figure 49: Spells followed per week after the universal reform.

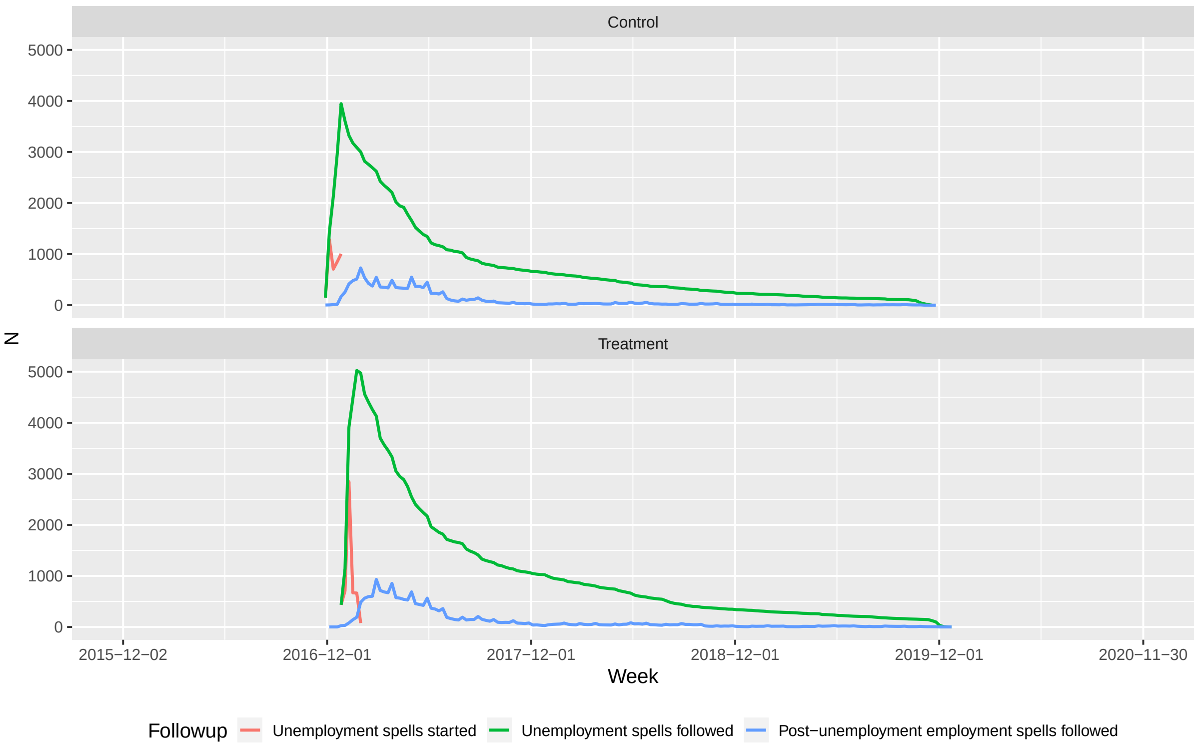
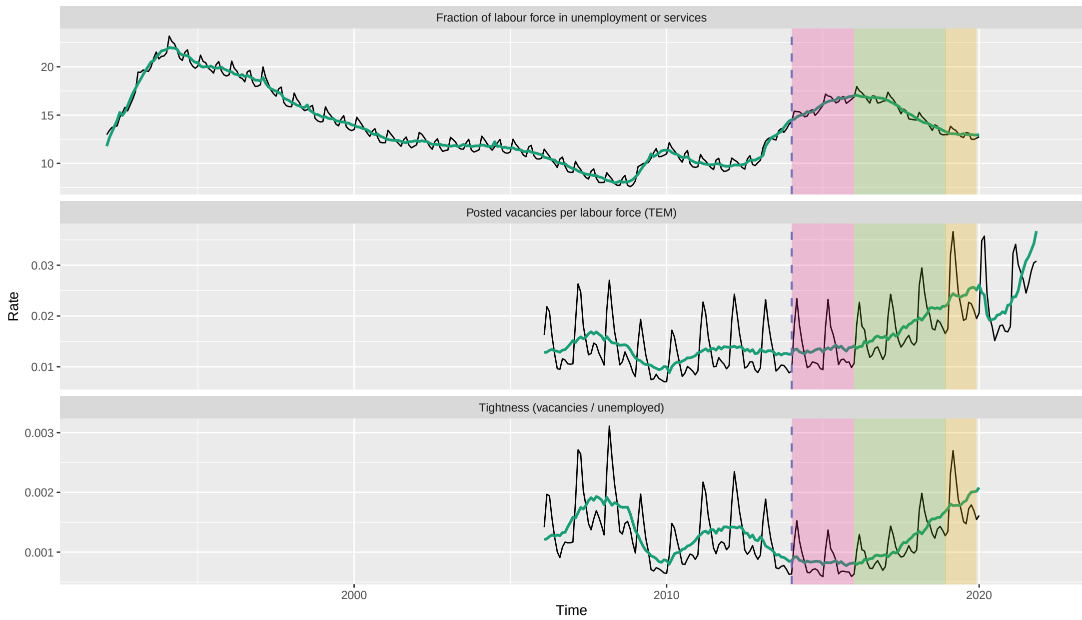


Figure 50: Labour market tightness during the follow-ups.



The shaded areas correspond to the timeframes for starts of follow-ups of the targeted reform, the maximum follow-up for unemployment, and the maximum follow-up for employment (left to right).

Figure 51: GDP and unemployment during the follow-ups.

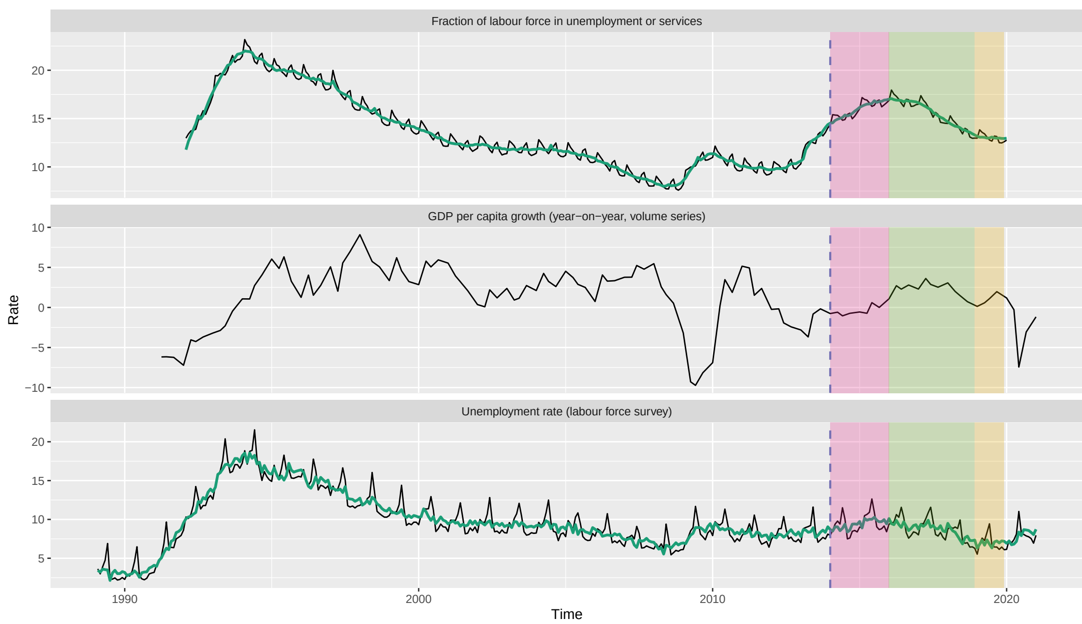
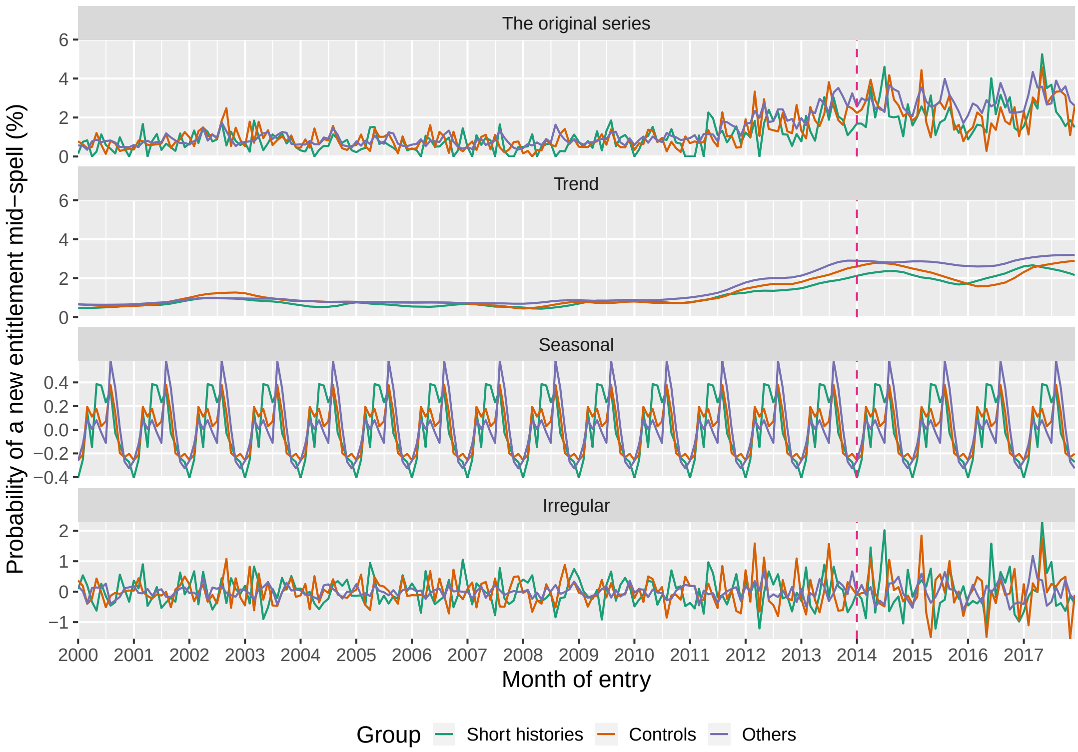


Figure 52: Trends in new entitlements earned mid-spell



Appendix N New entitlements mid-spell

In the main estimation samples, individuals who earned a new entitlement during otherwise uninterrupted unemployment (i.e., through partial unemployment) were simply excluded from the sample, amounting to roughly 3–4% of the otherwise eligible sample being dropped. Figure 52 shows the probability of earning a new entitlement over an otherwise uninterrupted spell for spells starting in 2000-2018 across two groups. The probabilities develop roughly similarly across the groups until the end of 2015, probably reflecting both the loosening of the recent employment condition and overall trends in part-time work.

The change in the probability of earning a new entitlement mid-spell was also directly checked for. After the targeted reform, the probability for the treatment group to earn a new entitlement increased by 0.9 percentage points more than for the control group; a likelihood ratio test for a logistic regression yields a p-value of .008. This implies that the treated were more likely to earn new entitlements mid-spell, but that the quantitative change was quite small. After the universal reform, the corresponding difference-in-differences measure for the probability of a mid-spell entitlement was -0.2 percentage points and a p-value of 0.60 for the likelihood ratio test, implying no significant differences in the likelihood.

There are two main problems with including these spells in the main sample. First, it is not clear whether to count the new entitlement as a new spell or as continuing the old one. A new spell would appear peculiar as the person has clearly not left unemployment, and for both spells, the link between elapsed benefit weeks and time in continued unemployment would be broken. Considering the new entitlement as a continuation of the old spell would, on the other hand, break the link between elapsed benefit weeks over spell and time to exhaustion. Additionally, the new entitlement might actually move the spell from, say, before/treatment group to the after/control group, depending on when the entitlement starts and how much work history has been accrued.

The second issue is that earning a new entitlement is both predictable and itself a potential outcome, because the reforms can cause new entitlements to become (relatively) more or less valuable. The predictability comes from the fact that a person with an open-ended or long-term part-time job contract can reasonably expect they will earn a new entitlement long before UI exhaustion. The relative value of the new entitlement is changed for the treatment group in the targeted reform in particular. For those with short histories who entered unemployment before the reform, a new entitlement becomes less valuable if it starts after 2014, as it is often a shorter one than before. For the treated who enter after the reform, the new entitlement may actually be a longer one than their current one if they also accrue enough new work history to switch them into the control group.

Tables 3 and 4 collect the observed entitlement changes over a follow-up of 3.84 years (whether during the same spell or after a period outside benefit unemployment). Figures 53 and 54 illustrate the timing of ongoing spells by entitlement from the start of the initial entitlement. Both the tables and the figures show that the new earned entitlements are quite often of a different duration than the original one.

Even noting the above concerns, it makes sense to directly check for the potential impact of including the mid-spell entitlements in the sample. The chosen approach for this was to simply append any new entitlements earned mid-spell to the original spell. Repeating the main estimation procedure for this setup yielded point estimates of -3.0 weeks for the universal reform and $+0.16$ weeks for the targeted reform; both results are close to the main estimates.

Figure 53: Ongoing spell by entitlement, after/treated, targeted reform

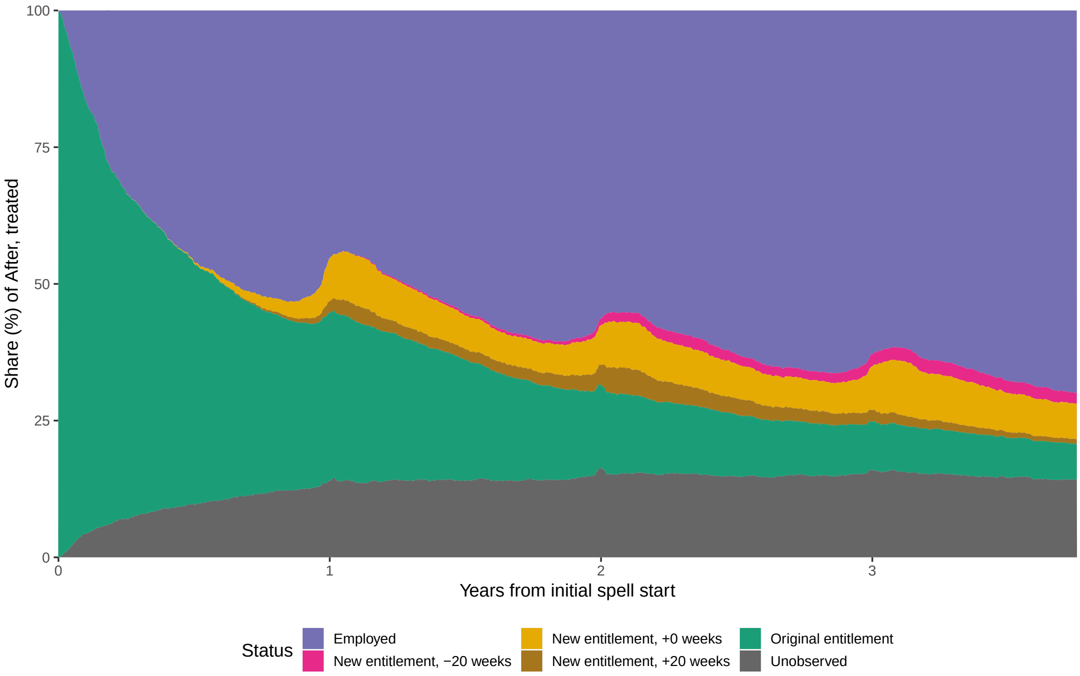


Figure 54: Ongoing spell by entitlement, controls/before, universal reform

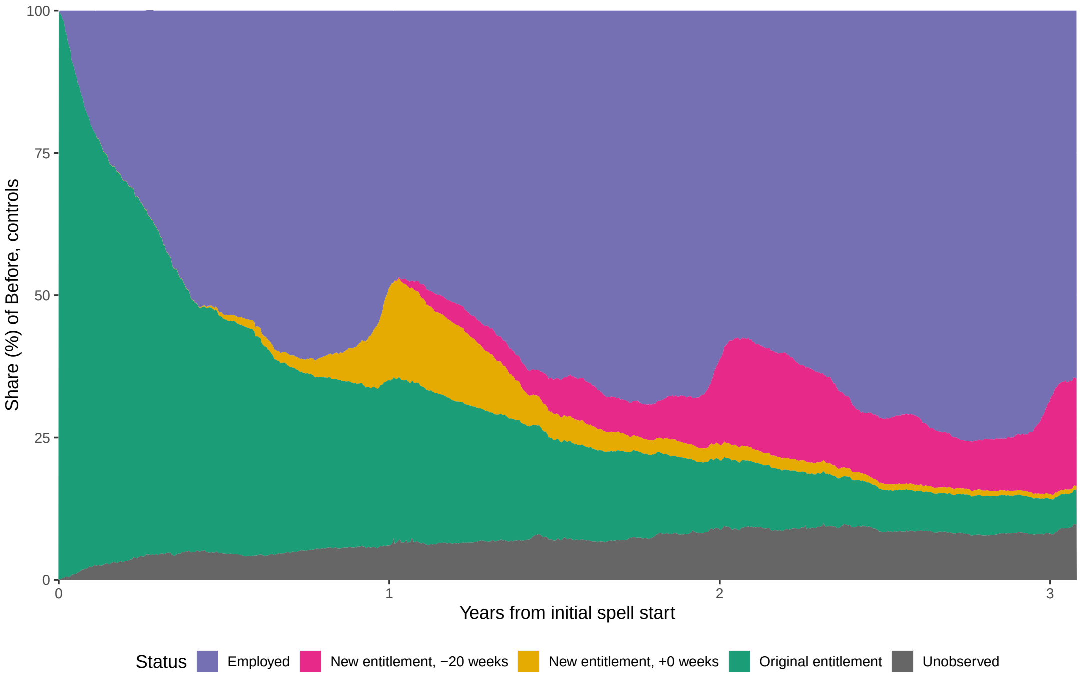


Table 3: New entitlements earned over follow-up, targeted reform

Time	Treatment	-20 weeks	-40 weeks	+0 weeks	+20 weeks
Before	Control	2.144% (after 3.5 years)		50.674% (after 1.7 years)	
Before	Treatment	23.749% (after 1.8 years)	0.454% (after 3.5 years)	35.103% (after 2.0 years)	
After	Control	29.317% (after 2.6 years)		32.945% (after 1.2 years)	
After	Treatment	8.056% (after 2.4 years)		35.228% (after 1.8 years)	12.755% (after 1.5 years)

Table 4: New entitlements earned over follow-up, universal reform

Time	Treatment	New entitlement, -20 weeks	New entitlement, +0 weeks
Before	Control	42.8% (after 1.88 years)	22.9% (after 0.87 years)
Before	Treatment	32.2% (after 1.81 years)	11.1% (after 0.76 years)
After	Control	48.2% (after 1.42 years)	
After	Treatment		39.2% (after 1.51 years)

Appendix O Alternative setups: continued entitlements and cumulative outcomes

For the estimates in the main text, only spells starting with fresh entitlements were used. Thus, about 15–17% of otherwise eligible spells were dropped, as they had not residual entitlement left from an earlier spell. The main motivation for this was simply to keep the main estimates comparable and easy to interpret: the hazard rates and the mean response both simultaneously track time in unemployment and time until exhaustion, starting from 0. The drawback of this approach is that responses in either the probability of re-entering unemployment or in the mean duration of re-entries might go unobserved.

Three alternative approaches are considered to assess continued entitlements. First, figures 55 and 56 plot the hazards including continued entitlements for the targeted reform. The same entitlements are included as for the main estimation sample, but now including re-entry spells (as separate spells) until end of 2017 and with a spell-level follow-up of 2.16 years. The figures reiterate the main findings that the targeted reform did not have large changes and did not move the exit spike. However, there is a slight increase around 80 weeks for the post-reform treatment group when re-entries are included, suggesting that unemployed who re-enter are more responsive to the drop in benefits.

Second, the mean responses were re-estimated for *cumulative* time in unemployment per entitlement for both reforms over the 2.84 year follow-up. The setup is otherwise identical to the one used in the main text, but having a gap of 30 days or more between benefits no longer counts as an exit. To keep the setup roughly comparable to the baseline approach, spells with new entitlements earned during the follow-up are still dropped unless there are at least 4 months between the corresponding periods of unemployment. A spell is now only considered to have ended instead of being censored if it was not ongoing within 30 days of the end of the follow-up.

The empirical results are collected in tables 5 and 6. There is now a small effect for the targeted reform, with 1.1 weeks shorter durations on average, but this remains statistically insignificant. The corresponding estimate for the universal reform is that cumulative durations shortened by 4.0 weeks. Since the pre-reform mean duration was also longer at 41.6 weeks of cumulative unemployment, the relative decrease is 9.5% in cumulative duration for the universal reform, compared to a relative decrease of 9.8% when only the first spell per entitlement was considered.

Figure 55: Hazard with and without continued entitlements, targeted reform

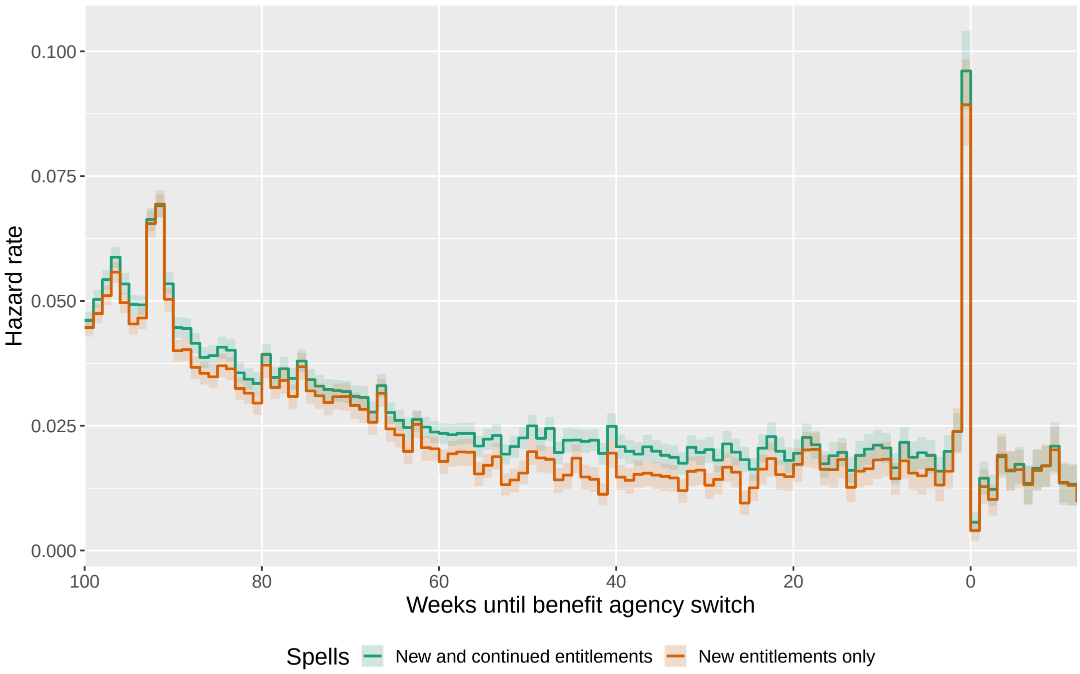


Figure 56: Exit hazards by group, targeted reform, continued entitlements included

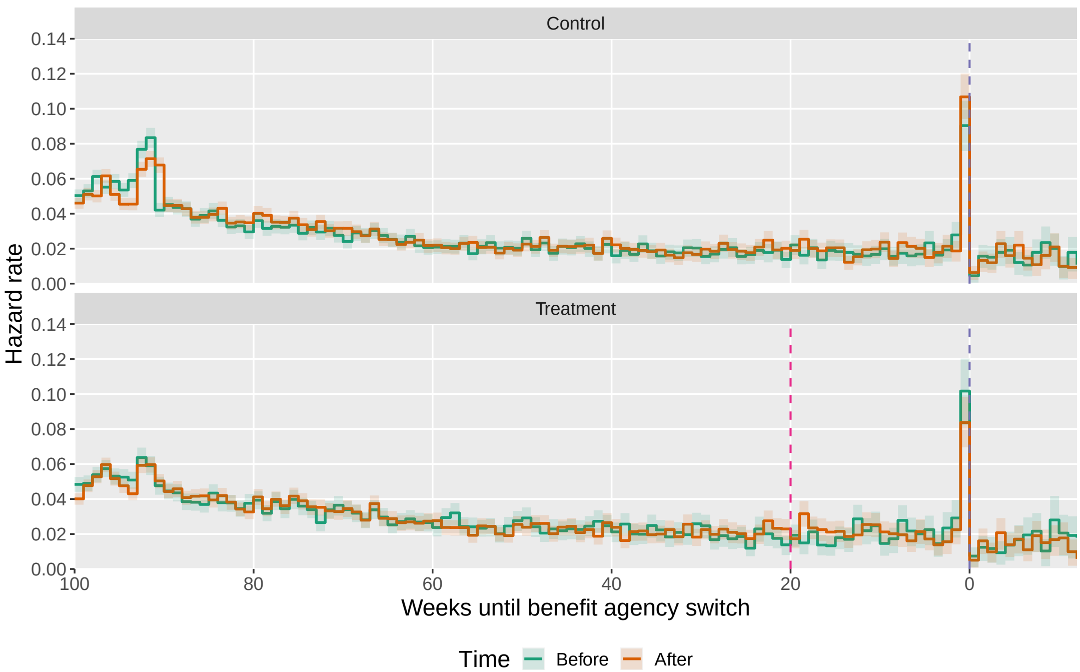


Table 5: Effects of the targeted reform on cumulative unemployment duration and re-employment.

Outcome	Unweighted, without controls	Unweighted, with controls	Weighted, without controls	Weighted, with controls	Pre-reform mean	N	ESS
Elapsed benefit weeks	-0.4964 (0.8288)	-0.5252 (0.8033)	-1.2323 (0.8589)	-1.0757 (0.8760)	38.5	40,599	35,421
Re-employment probability	-0.0121 (0.0093)	-0.0088 (0.0090)	-0.0024 (0.0101)	-0.0052 (0.0095)	0.67	40,599	35,421
Duration in next job	2.1301 (2.5411)	0.4409 (2.4901)	3.4679 (2.8132)	2.7574 (2.5985)	258	30,226	26,453
Wage in next job	5.4801 (16.2133)	24.2912 (14.3355)	23.7972 (15.6486)	28.0814 (15.1434)	1798	30,226	26,453

All unemployment spells during the follow-up chained together. Effects for re-employment probability are the bootstrapped marginal effects. Effects on wage and duration of job are for the re-employed. Wages are monthly wages indexed to 2005. Pre-reform means are for the treatment group. ESS = effective sample size.

Table 6: Effects of the universal reform on cumulative unemployment duration and re-employment.

Outcome	Unweighted, without controls	Unweighted, with controls	Weighted, without controls	Weighted, with controls	Pre-reform mean	N	ESS
Elapsed benefit weeks	-4.781 (1.338)	-4.529 (1.264)	-4.016 (1.360)	-3.983 (1.299)	41.6	15,409	14,110
Re-employment probability	0.046 (0.015)	0.043 (0.014)	0.036 (0.016)	0.035 (0.015)	0.71	15,409	14,110

All unemployment spells during the follow-up chained together.

As the third approach, cumulative unemployment, employment and wages per *person* were tracked from the start of their entitlements. These results are only suggestive, as they do not account for the fact that many individuals in both groups change their treatment status and earn entitlements of different lengths, as was shown in appendix N.

One interpretation is that, given that most of the unemployed return to unemployment at some point, the treatment can be defined by the *timing* of the entitlement cuts: those in the "after, treated" group are treated by shorter entitlements early on, while others are typically treated with similar cuts at some later point. Figures 57–60 show the overall individual-level daily employment rate and cumulative wages over 3.84 years after the start of a spell.

The figures for the targeted reform suggest that prior work history, rather than the entitlement, is driving differences in cumulative wages, while timing of the spell is driving long-term employment status. For the universal reform, the employment status appears to be significantly better for several years for the treatment group actually treated (spell started after reform) vs. treatment group not treated (spell started before reform). Although the employment statuses eventually converge, this might be explained by the fact that the before-treatment group is often still being treated with a short entitlement if re-entering unemployment, only later, and this later treatment may be suppressing the average time spent in unemployment.

Figure 57: Cumulative wages, targeted reform

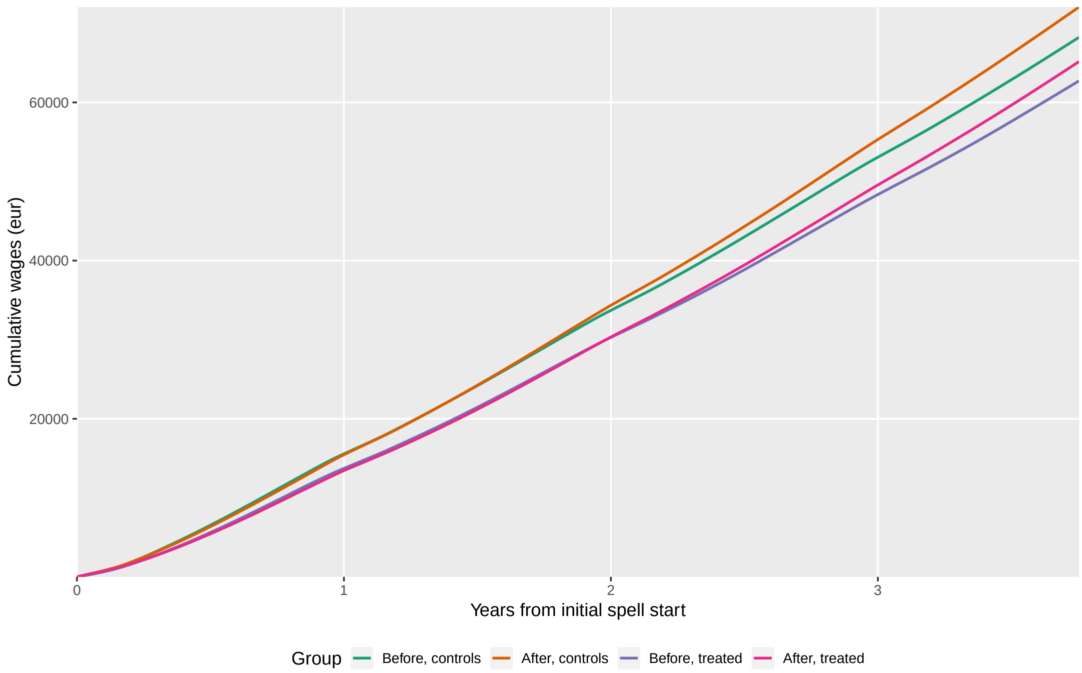
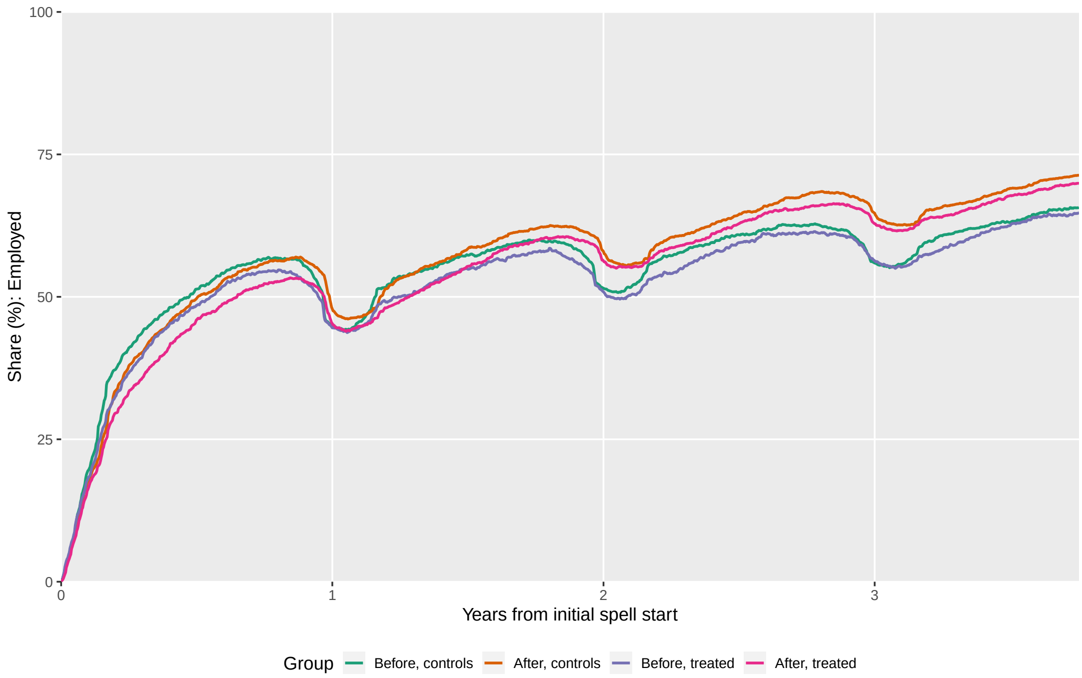


Figure 58: Share employed, targeted reform



A person is classified as employed when they earn wages and are not collecting unemployment benefits.

Figure 59: Cumulative wages, universal reform

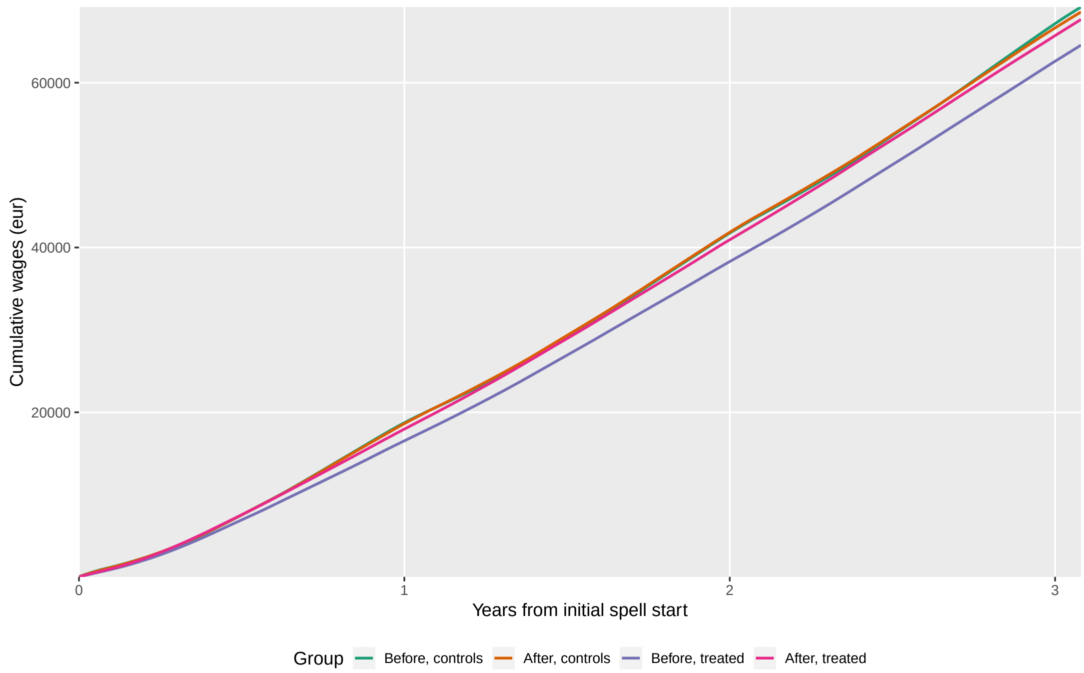
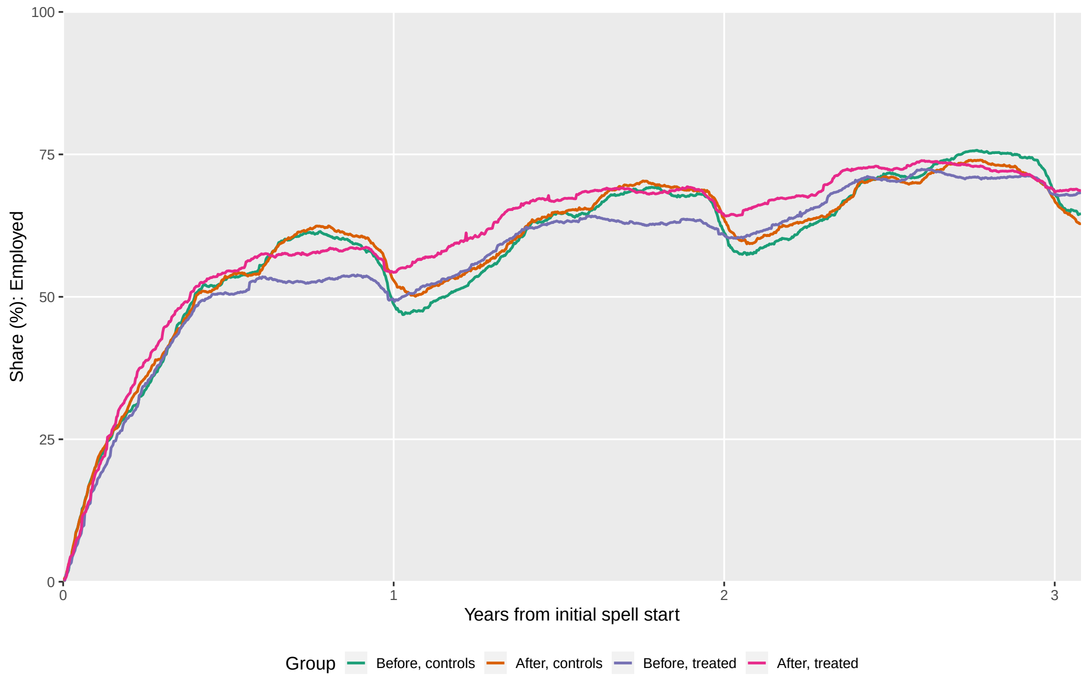


Figure 60: Share employed, universal reform



A person is classified as employed when they earn wages and are not collecting unemployment benefits.

Appendix P Trends in partial unemployment

Simultaneous to the targeted reform, incentives for partial unemployment were increased in 2014. A person on benefits could earn up to 300 euros, the earnings disregard, in wages without a reduction in benefits. Each euro above the 300 euros reduces benefits by 50 cents, as long as working hours stay below 80% of a full-time job and wages and benefits together do not exceed the person's prior earnings. Both this change and the entitlement cut can make partial unemployment more attractive, since partial unemployment only consumes the entitlement at the full-time equivalent rate and may eventually lead to a new entitlement.

Partial unemployment is common at the level of individuals and spells, but a relatively minor phenomenon as a share of aggregate unemployment. One reasonable way to define the intensity of unemployment is to calculate it as $1 - \frac{\text{hours worked}}{\text{full-time hours}}$, which is 1 if no-one works during unemployment and 0.25 if everyone works 75% of a full-time week during unemployment. As hours worked are not observed, a reasonable proxy is $1 - \frac{\text{wage unemployed}}{\text{previous wage}}$.

Note that this definition is close to, but not identical to the ratio of FTE benefit weeks to any benefit weeks. There is a difference because FTE *benefit weeks* are defined (in law) from the perspective of the benefits as $\frac{\text{benefits paid}}{\text{benefits if unemployed}} \times$ benefit weeks, and each euro in wages only reduces benefits by 50 cents and only after subtracting the earnings disregard. Thus, the intensity measure is lower than the FTE ratio during part-time unemployment. Additionally, the FTE ratio may fluctuate for the same person over time even when the amount of hours worked does not change, due to changes in benefit rules during partial unemployment.

The aggregate intensity measure has fluctuated between 0.9 and 0.96 from 2000 to 2019 for UI recipients.[4] The measure was low until the financial crisis in 2009, then slowly decreased to prior levels, and increased again during the COVID pandemic. Whether these changes are driven by the business cycle, structural change, or changes in benefit incentives is beyond the scope of this paper; regardless, viewed this way, the direct impact of partial unemployment on aggregate unemployment appears to be minor.

Note that the aggregate intensity measure can be decomposed as $1 - s_{\text{spells}} \times s_{\text{days}} \times (1 - I_{\text{partials}})$, where s_{spells} is the share of benefit days in spells with at least one day in partial unemployment (varying between 25% and 40% across groups and time in 2000–2019), s_{days} is the share of days in partial unemployment in such spells (35%–55%), and I_{partials} is the intensity measure for only the days in partial unemployment (55%–70%).

Figures 61–64 demonstrate the trends for the aggregate measure and its composite parts across *new* UI spells over 2000-2018 for persons with short work histories, slightly longer work histories, and other histories. Spells starting on furloughs or after voluntary

[4]Part-time wages during unemployment below the earnings disregard are unobserved for certain funds and time periods. For these cases, the wages have been imputed if the payment data indicated the person was in partial unemployment for given days.

quits and spells of persons aged 55 or higher are again dropped. As high-frequency data on unemployment assistance was not available between 2000-2009, the figures only cover the UI part of each spell. Note that to be consistent with the other figures on trends, the measures are tracked by start time of spells, rather than for spells ongoing in a given month. The aggregate intensity measure for the short history group only shows a significant divergence from the other groups after 2017. This divergence appears to be driven mostly by the share of spells with partial benefit days, which continued to increase for other groups.

Finally, figure 65 demonstrates the share of partial unemployment over spell for the targeted reform. Part-time work appears to be more prevalent during the early stages of unemployment, when unemployment is measured by FTE benefit week. This is expected even if partial unemployment is evenly distributed across *calendar* time in unemployment, because being in partial unemployment mechanically slows down the rate at which FTE benefit weeks are consumed.

Figure 61: Intensity of unemployment

Intensity is 100% if no-one is working during unemployment. If everyone is earning 75% of their prior wage during unemployment, intensity is 25%.

Figure 62: Weighted share of spells with any partial unemployment

The weights are the durations per spell in non-FTE benefit days, i.e., weekdays for which unemployment benefits were claimed.

Figure 63: For spells with partial unemployment, share of benefit days in partial unemployment

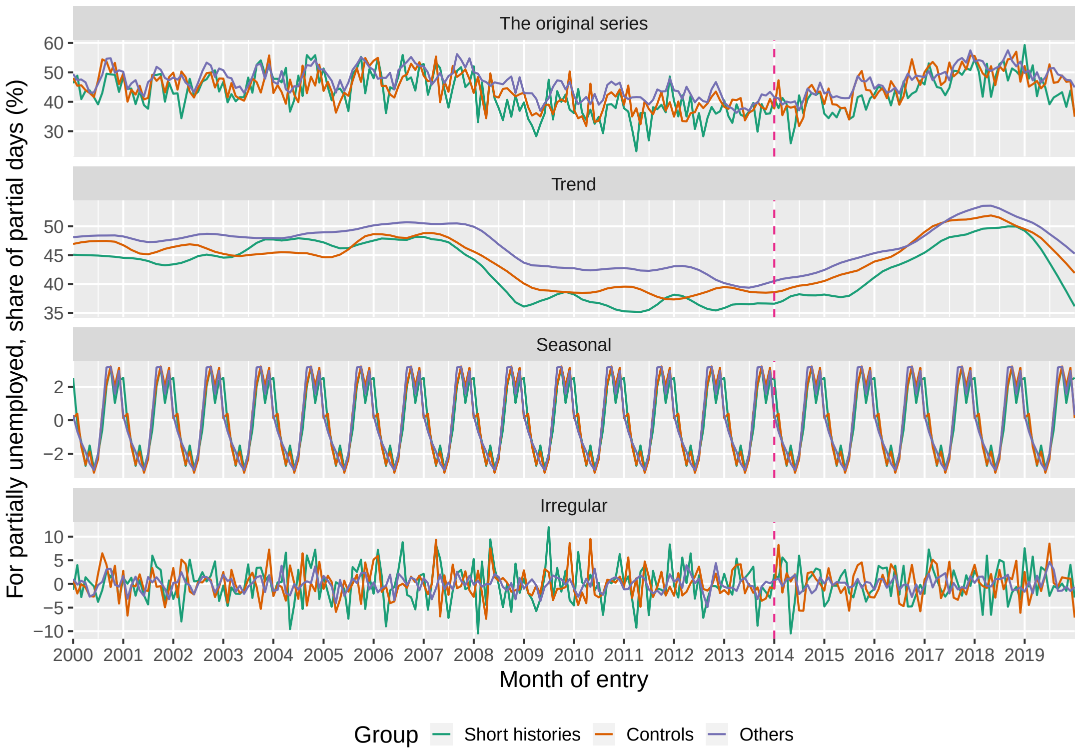


Figure 64: Intensity of unemployment for days in partial unemployment

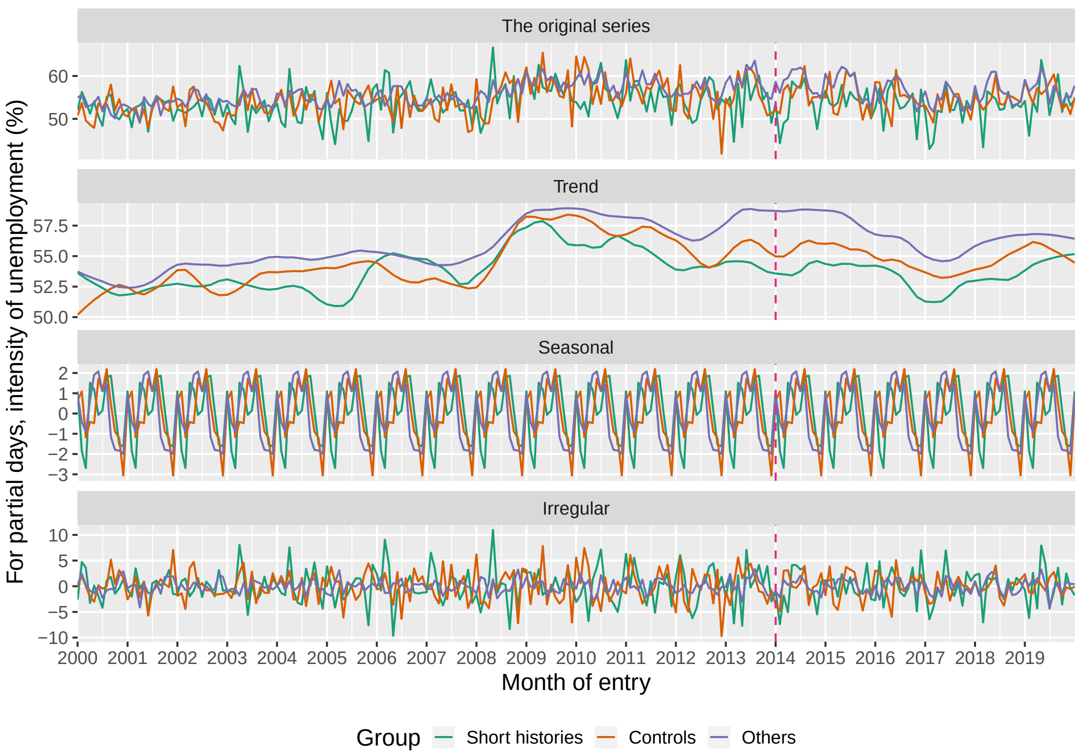
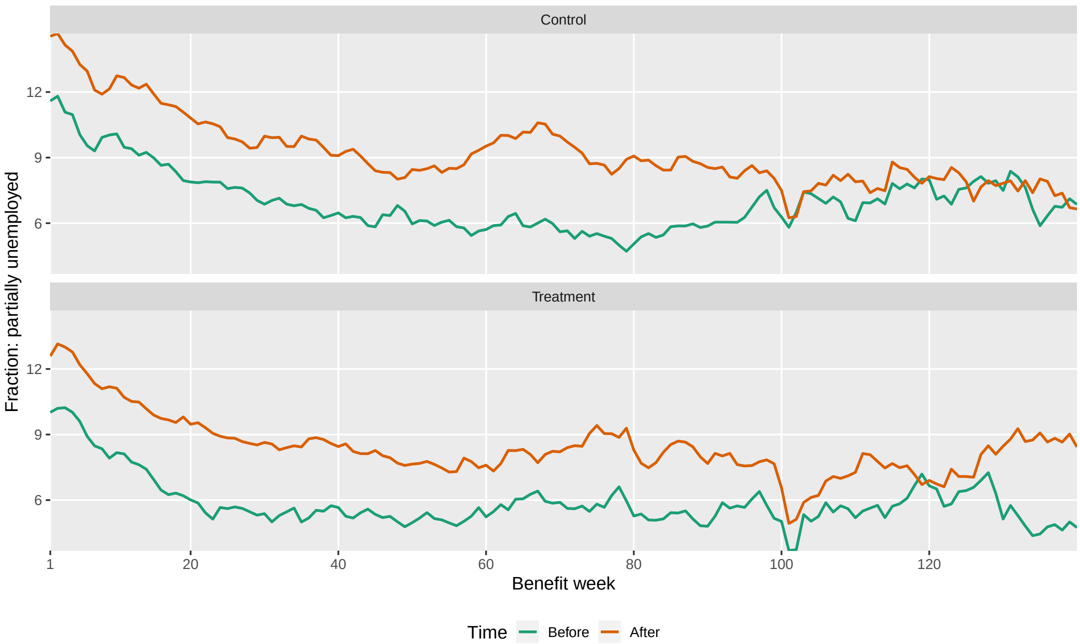


Figure 65: Share in partial unemployment over spell, targeted reform



Appendix Q The hazard response tables and covariate values

Tables 7 and 8 present the main hazard estimates for the two reforms. Tables 9 and 10 list the covariate values used for calculating the baseline hazard for these tables and the figures in the main text. (The values were found by selecting modes or medians iteratively to correspond to an actual group of workers.)

To quantify the impact of these changes at different stages of unemployment on the mean times, the mean was recalculated as follows. First, the estimation intervals were groupped so that "moving the spike" covered intervals 77–80, 81–84, 97–100 and 101–104, i.e., weeks immediately before and after the new and old exhaustion time. All other intervals were considered their own group. For each of these groups at a time, a counterfactual hazard for the "after, treated" group was recalculated after changing the corresponding interaction term to zero for that week. The means were then approximated from the hazard by the treatment status and whether the spell started before or after the reform. (The cumulative hazard approximation with no changes match the observed means with an accuracy of roughly 0.05 weeks.) Finally, a counteractual difference-in-differences estimate was calculated and compared to the actual DiD estimate.

For the universal reform, 19% of the reduction in mean duration can be attributed to quicker exits in weeks 1–4, 32% in weeks 5–10 and 29% in weeks 13–26. The contribution from the spike group was 19%. Other week groups had smaller positive or negative contributions.

Table 11 approximates the relative contribution of the changes in the hazard within a given interval towards the average effect on unemployment duration. The purpose of the table is to indicate which changes in the hazard are economically important in terms of the aggregate effect on unemployment. While the changes in the initial stages of unemployment appear small in relative terms, they end up being significant on average, because many more unemployed are surviving at these stages than later on.

To calculate the decomposition, for each interval in the table, counterfactual group-specific mean durations were recalculated from the hazards after setting the reform effect on the hazard to 0 for that interval. "Spike" collects weeks 77–84 and weeks 97–104, to group together the movement of the spike. The difference in differences was then recalculated from these group-specific means. The relative effect in the table is the opposite number of the ratio of this counterfactual effect to the effect when no hazards are changed (which is very close to the difference in difference estimated directly from the duration data). The decomposition is only approximate, as the counterfactual ignores dynamic selection: if the effect on weeks 1–4 was truly forced to be zero, the composition of the unemployed surviving to week 5 would also have been different.

Table 7: Results from the hazard model for the targeted reform

Weeks	After	Interaction	exp(After)	exp(Interaction)	Baseline hazard
Weeks 1–4	-0.0263 (0.078)	0.0036 (0.034)	0.97 (0.83–1.13)	1.00 (0.94–1.07)	0.078 (0.073–0.084)
Weeks 5–10	-0.0788 (0.077)	0.0483 (0.029)	0.92 (0.79–1.07)	1.05 (0.99–1.11)	0.086 (0.081–0.091)
Weeks 11–12	0.0248 (0.086)	0.0161 (0.066)	1.03 (0.87–1.22)	1.02 (0.89–1.15)	0.077 (0.068–0.088)
Weeks 13–26	0.0598 (0.079)	0.0182 (0.033)	1.06 (0.91–1.24)	1.02 (0.96–1.09)	0.067 (0.063–0.072)
Weeks 27–34	0.0991 (0.082)	-0.0825 (0.057)	1.10 (0.94–1.30)	0.92 (0.82–1.03)	0.059 (0.053–0.066)
Weeks 35–48	0.0778 (0.086)	-0.0334 (0.063)	1.08 (0.91–1.28)	0.97 (0.86–1.09)	0.043 (0.038–0.048)
Weeks 49–59	0.1861 (0.092)	-0.1928 (0.088)	1.20 (1.01–1.44)	0.82 (0.69–0.98)	0.034 (0.029–0.040)
Weeks 60–76	0.1092 (0.093)	0.1536 (0.082)	1.12 (0.93–1.34)	1.17 (0.99–1.37)	0.037 (0.031–0.043)
Weeks 77–80	0.2099 (0.133)	0.0226 (0.173)	1.23 (0.94–1.59)	1.02 (0.73–1.44)	0.036 (0.026–0.051)
Weeks 81–84	0.1567 (0.128)	0.2738 (0.172)	1.17 (0.91–1.50)	1.32 (0.94–1.84)	0.047 (0.033–0.066)
Weeks 85–96	0.1944 (0.103)	-0.1255 (0.114)	1.21 (0.99–1.48)	0.88 (0.71–1.10)	0.036 (0.029–0.045)
Weeks 97–100	0.0670 (0.113)	-0.0560 (0.139)	1.07 (0.86–1.34)	0.95 (0.71–1.23)	0.069 (0.052–0.090)
Weeks 101–104	0.2826 (0.183)	-0.0866 (0.274)	1.33 (0.92–1.89)	0.92 (0.53–1.55)	0.024 (0.014–0.040)
Weeks 105–120	0.0759 (0.106)	-0.1673 (0.119)	1.08 (0.88–1.33)	0.85 (0.67–1.06)	0.032 (0.025–0.040)

The second and third columns are the raw coefficients from the model, with bootstrapped standard errors in parentheses. The fourth and fifth columns hold the transforms of the coefficients to hazard ratios, with bootstrapped confidence intervals in parentheses. The last column is a representative daily baseline hazard for the treated before the reform.

Table 8: Results from the hazard model for the universal reform

Weeks	After	Interaction	exp(After)	exp(Interaction)	Baseline hazard
Weeks 1–4	-0.0453 (0.038)	0.1606 (0.051)	0.96 (0.89–1.03)	1.17 (1.06–1.30)	0.049 (0.0443–0.054)
Weeks 5–10	0.0453 (0.043)	0.1987 (0.054)	1.05 (0.97–1.14)	1.22 (1.09–1.35)	0.046 (0.0413–0.051)
Weeks 11–12	0.0702 (0.088)	0.1131 (0.108)	1.07 (0.90–1.28)	1.12 (0.90–1.38)	0.046 (0.0374–0.057)
Weeks 13–26	0.0638 (0.034)	0.0778 (0.047)	1.07 (1.00–1.14)	1.08 (0.99–1.19)	0.039 (0.0360–0.043)
Weeks 27–34	0.0772 (0.081)	-0.0781 (0.103)	1.08 (0.92–1.27)	0.92 (0.76–1.13)	0.026 (0.0211–0.032)
Weeks 35–48	0.1979 (0.082)	-0.0721 (0.105)	1.22 (1.04–1.44)	0.93 (0.76–1.14)	0.017 (0.0141–0.021)
Weeks 49–59	-0.1425 (0.123)	0.3199 (0.146)	0.87 (0.68–1.11)	1.38 (1.03–1.83)	0.018 (0.0136–0.024)
Weeks 60–76	0.2328 (0.087)	-0.0497 (0.107)	1.26 (1.07–1.50)	0.95 (0.77–1.17)	0.023 (0.0183–0.028)
Weeks 77–80	-0.0377 (0.198)	1.5854 (0.253)	0.96 (0.66–1.43)	4.88 (2.92–7.86)	0.050 (0.0299–0.081)
Weeks 81–84	-0.5231 (0.283)	0.8698 (0.339)	0.59 (0.34–1.04)	2.39 (1.21–4.58)	0.022 (0.0110–0.042)
Weeks 85–96	0.1525 (0.142)	0.0149 (0.190)	1.16 (0.87–1.53)	1.02 (0.70–1.48)	0.020 (0.0138–0.029)
Weeks 97–100	-0.1070 (0.157)	-1.0369 (0.254)	0.90 (0.66–1.23)	0.35 (0.22–0.59)	0.015 (0.0094–0.025)
Weeks 101–104	-0.0379 (0.363)	-0.0037 (0.447)	0.96 (0.48–1.98)	1.00 (0.41–2.39)	0.015 (0.0061–0.035)
Weeks 105–120	-0.0882 (0.152)	0.0522 (0.188)	0.92 (0.67–1.22)	1.05 (0.73–1.53)	0.020 (0.0136–0.028)
Weeks 121–	0.1384 (0.148)	0.0519 (0.186)	1.15 (0.86–1.54)	1.05 (0.73–1.52)	0.022 (0.0155–0.032)

For notes, see Table 7.

Table 9: Covariate values used for calculating the baseline hazard, targeted reform

Variable	Value
Profession (ISCO level 1)	Service and sales workers
Gender	Woman
Month at start (groupped)	6–7
Year at start	2014
Type of unemployment at start	Full-time unemployed
Days from last job	1
Work history (years)	2.4
Age	20
Recent employment (weeks)	102.0
Number of dependent children	0
Local unemployment rate (pct. points over nat. avg)	1.3
Highest income quint. share of resid. in postal code area	7.6
Jobseekers/vacancies in region matching 4-digit profession	4.5

Table 10: Covariate values used for calculating the baseline hazard, universal reform

Variable	Value
Profession (ISCO level 1)	Craft and related trades workers
Work history (years)	13
Age	34
Number of dependent children	0
Highest income quint. share of resid. in postal code area	10
Vacancies/jobseekers in region matching 4-digit profession	0
Duration of last spell, in weeks	No previous spell observed
Residence permit if any	No data (usually a Finnish citizen)
Prior total wages (euros, indexed to 2005)	445 516

See appendix Z for variable definitions.

Table 11: Decomposition of the effects on average duration

Stage of unemployment	Relative removal effect
Weeks 1–4	18.79%
Weeks 5–10	32.45%
Weeks 11–12	7.00%
Weeks 13–26	29.24%
Weeks 27–34	−8.00%
Weeks 35–48	−5.93%
Weeks 49–59	10.69%
Weeks 60–76	−1.88%
Weeks 85–96	−1.49%
Spike	19.02%
Weeks 105–120	0.06%

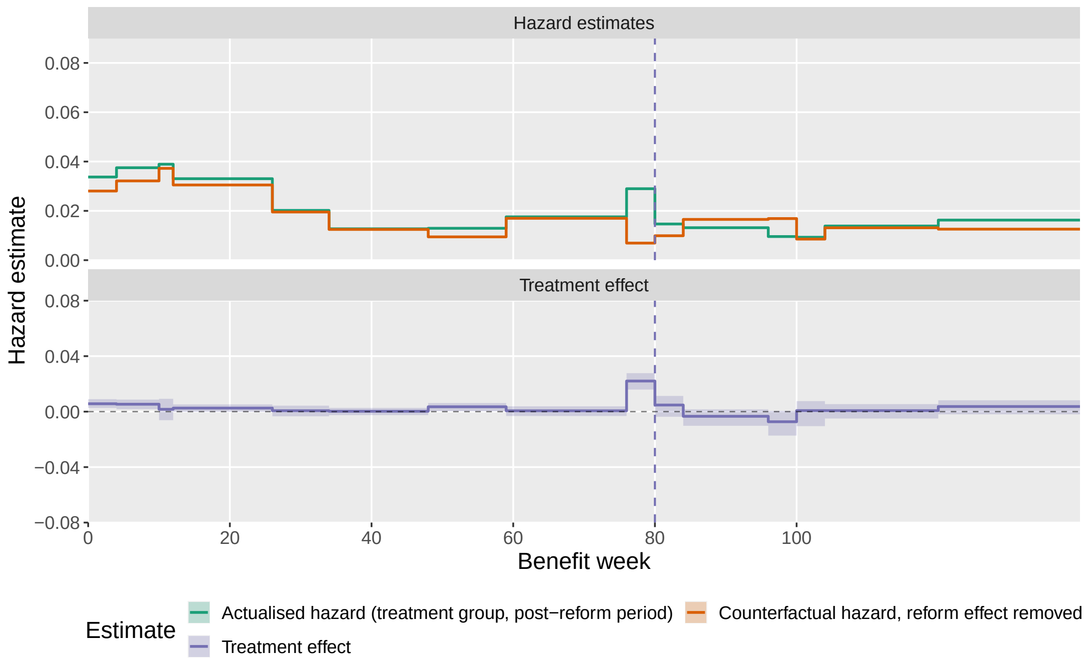
Table 12: Results on the job-finding rate for the universal reform

Weeks	After	Interaction	exp(After)	exp(Interaction)	Baseline hazard
Weeks 1–4	-0.0742 (0.050)	0.1844 (0.065)	0.93 (0.84–1.02)	1.20 (1.06–1.36)	0.0337 (0.0313–0.036)
Weeks 5–10	0.0943 (0.051)	0.1538 (0.063)	1.10 (0.99–1.21)	1.17 (1.03–1.32)	0.0375 (0.0351–0.040)
Weeks 11–12	0.0718 (0.104)	0.0441 (0.127)	1.07 (0.88–1.32)	1.05 (0.81–1.34)	0.0389 (0.0351–0.043)
Weeks 13–26	0.0653 (0.038)	0.0801 (0.053)	1.07 (0.99–1.15)	1.08 (0.97–1.20)	0.0330 (0.0308–0.035)
Weeks 27–34	-0.0205 (0.096)	0.0327 (0.121)	0.98 (0.81–1.18)	1.03 (0.82–1.31)	0.0202 (0.0182–0.022)
Weeks 35–48	0.0601 (0.100)	0.0204 (0.123)	1.06 (0.87–1.29)	1.02 (0.80–1.29)	0.0127 (0.0114–0.014)
Weeks 49–59	-0.2256 (0.149)	0.3142 (0.174)	0.80 (0.59–1.06)	1.37 (0.98–1.94)	0.0129 (0.0113–0.015)
Weeks 60–76	0.2071 (0.101)	0.0344 (0.127)	1.23 (1.01–1.50)	1.04 (0.81–1.33)	0.0176 (0.0157–0.020)
Weeks 77–80	-0.2502 (0.222)	1.4314 (0.302)	0.78 (0.51–1.21)	4.18 (2.28–7.46)	0.0290 (0.0241–0.034)
Weeks 81–84	-0.1759 (0.333)	0.3925 (0.414)	0.84 (0.43–1.59)	1.48 (0.67–3.38)	0.0147 (0.0102–0.019)
Weeks 85–96	0.3658 (0.178)	-0.2279 (0.222)	1.44 (1.01–2.03)	0.80 (0.52–1.24)	0.0132 (0.0107–0.016)
Weeks 97–100	-0.0759 (0.246)	-0.5660 (0.399)	0.93 (0.57–1.50)	0.57 (0.27–1.28)	0.0096 (0.0057–0.013)
Weeks 101–104	-0.1427 (1.025)	0.0838 (1.094)	0.87 (0.13–7.09)	1.09 (0.12–8.64)	0.0093 (0.0050–0.013)
Weeks 105–120	-0.1622 (0.195)	0.0536 (0.244)	0.85 (0.58–1.25)	1.06 (0.65–1.68)	0.0139 (0.0113–0.017)

The second and third columns are the raw coefficients from the model, with bootstrapped standard errors in parentheses. The fourth and fifth columns hold the transforms of the coefficients to hazard ratios, with bootstrapped confidence intervals in parentheses. The last column is a representative daily baseline hazard for the treated before the reform.

Table 12 collects the results of the universal reform on the job-finding (sub)hazard. Figure 66 illustrates the changes, similarly to the figure in the main text. The most significant results on the job-finding sub-hazard are qualitatively similar to the unrestricted hazard in relative terms: job-finding increases significantly in the early stages of unemployment, and the spike shifts from the old entitlement at 100 weeks to the new one at 80 weeks. The effects are likely to be conservative due to the relatively strict job-finding criteria discussed in appendix D.

Figure 66: Estimated job-finding rate and the treatment effect, universal reform.



The estimated (sub)hazards are for a treated individual in the post-period. The counterfactual corresponds to an estimate where the $Treatment \times After$ parameter is set to zero. The plotted treatment effect is the difference between the two hazards. The shaded areas correspond to bootstrapped 95% confidence intervals for this difference.

Appendix R Temporary unemployment in the summer

The Finnish labour market has a number of special rules for primary school teachers. One particular curiosity is that tenured teachers on certain long-term leaves of absence can temporarily return to their jobs during the summer holidays.[5] Individuals hired as their substitutes are only hired into 9-month fixed-term contracts, and become unemployed for the summer. This leads to a noticeable surge in unemployment each year, with up to thousands of entries in a single week around start of June (see figure 69). A slightly less specific version of this group was also discussed by Kyyrä and Pesola (2020b).

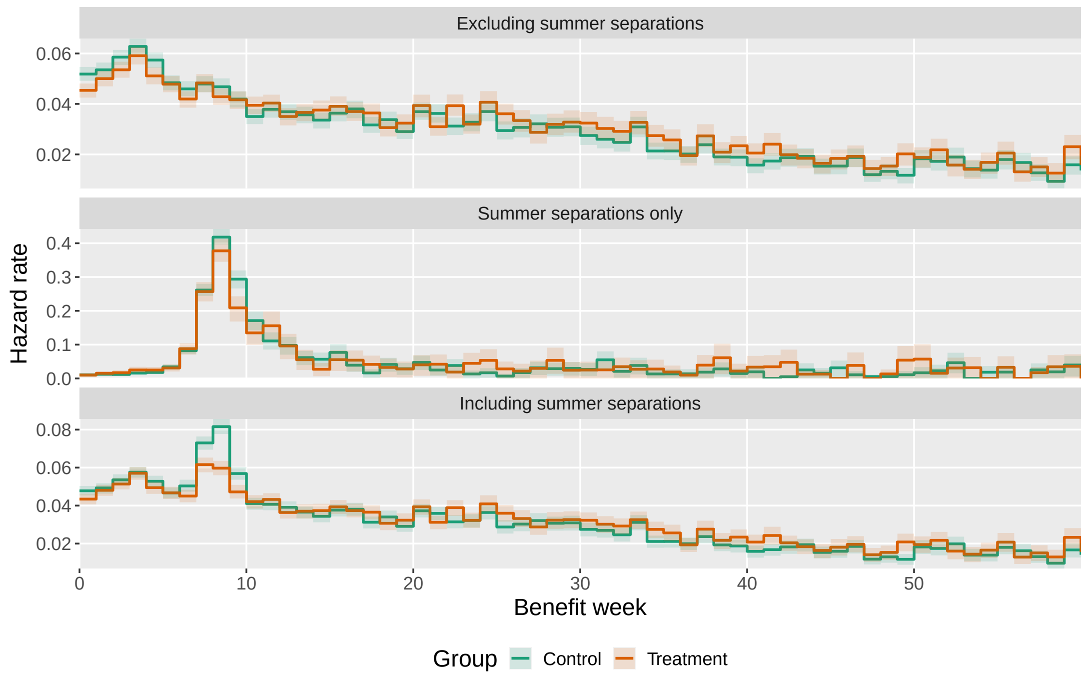
Most of the substitutes do not exit unemployment during the summer, but overwhelmingly return to a new 9-month fixed-term contract at the end of summer, often to their previous employer. They can also usually be recognized in the data by a combination of their profession, time of entry into unemployment, duration of their previous job, and having lost their job due to a fixed-term contract ending.

In many respects, their situation resembles that of the furloughed. The maximum entitlement is unlikely to affect this group, as they usually exit after a fixed term of unemployed and typically earn a new entitlement over the next year.

For the targeted reform, figure 67 compares the exit hazards when these special summer separations are included, excluded or the only group; note that the vertical scale had to be varied by group to fit in the massive exit spike around weeks 8–10. Figure 68 shows the hazard when these separations are dropped from the sample. The early hazard rate becomes much smoother and lower, and is also more similar across the groups. Removing these separations from the sample has no significant impact on the estimated effects on average durations.

[5] Of the Nordic countries, at least Finland, Sweden, Norway and Denmark have specific provisions in their labour market institutions for teachers and their holidays. For a discussion from the point of view of the teacher's union, see https://www.oaj.fi/ajankohtaista/uutiset-ja-tiedotteet/2021/maaraaikaisten-opettajien-kesaajan-palkka-puhututtaa/.

Figure 67: Exit hazard with and without summer separations



"Summer separations" are identified by a combination of ISCO-4 level profession, entry into unemployment (between 23rd of May and 15th of June), duration of the last job (between 9 and 11 months), and reason for the termination of last job (a fixed-term contract ended).

Figure 68: By-group exit hazard with summer separations excluded

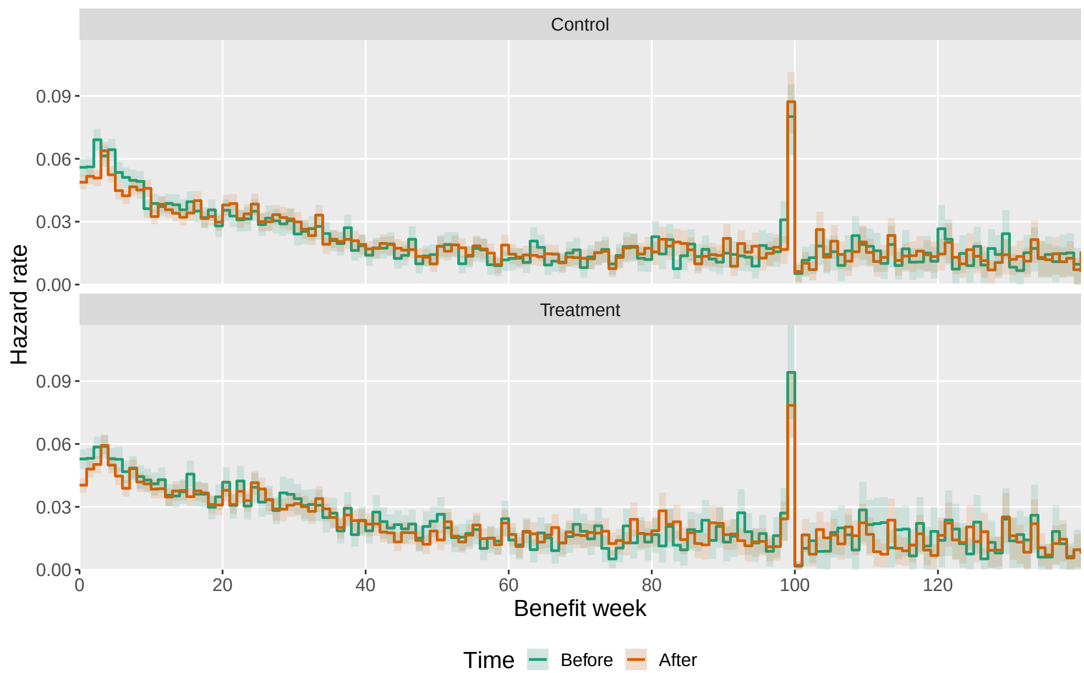
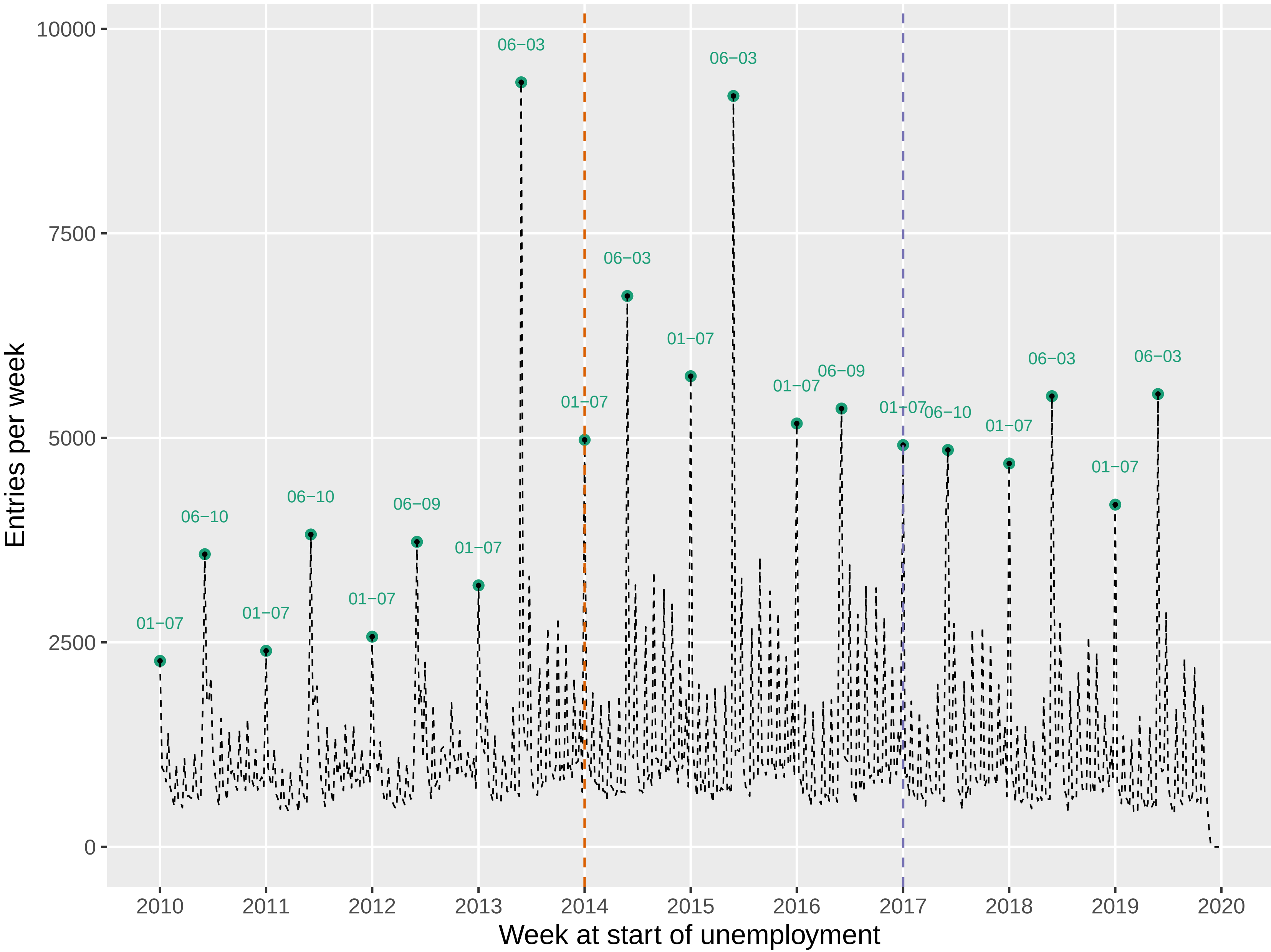
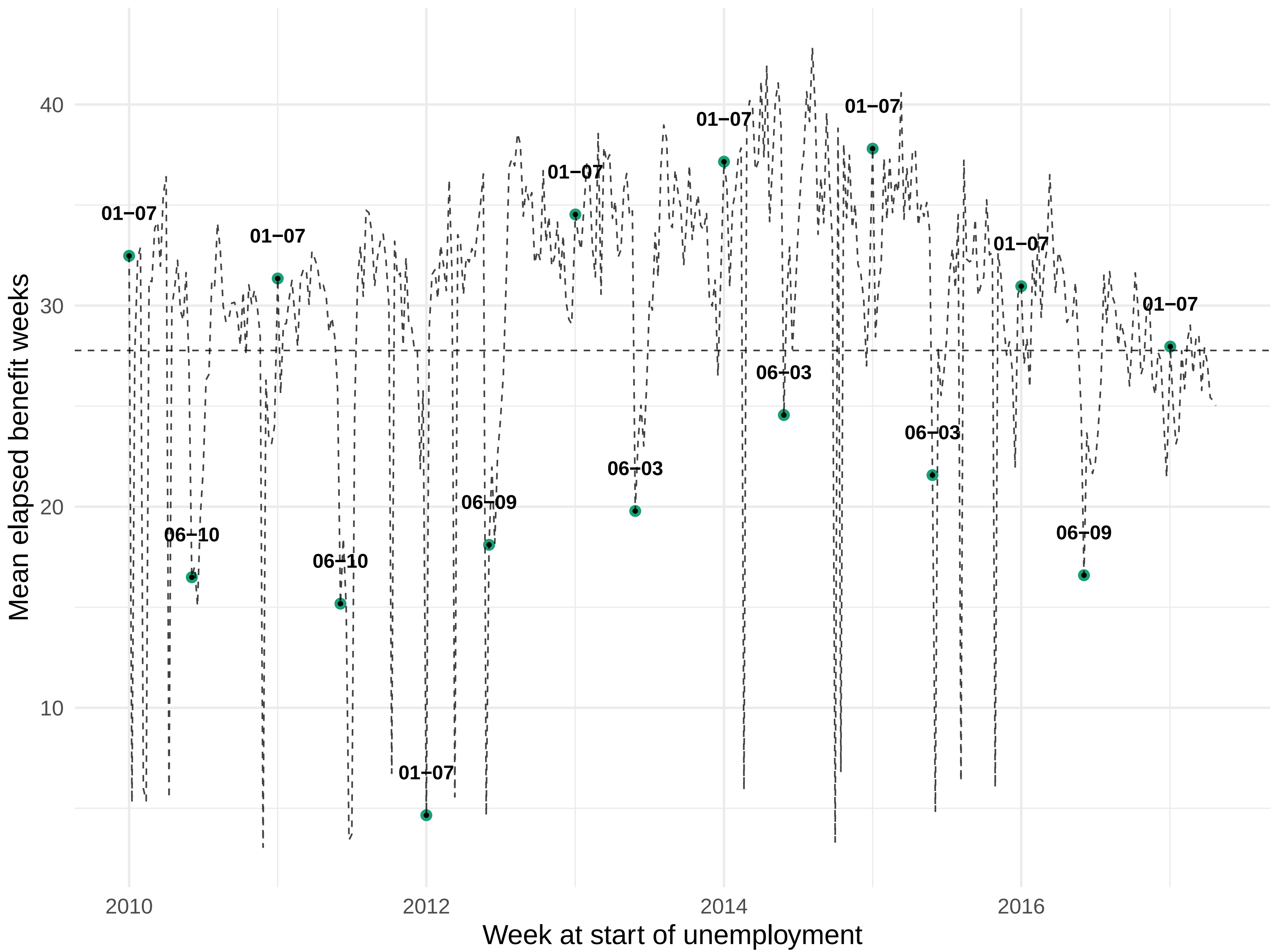


Figure 69: Weekly inflow into unemployment benefits by group



Inflows into unemployment insurance, with three peak entry dates for each year highlighted. Includes all work histories, but otherwise the same sample restrictions and definitions as for the main setups were applied.

Figure 70: Unemployment duration by entry week



Unemployment duration (over 3 years) by week of entry, with three peak entries per year highlighted. Includes all work histories, but otherwise the same sample restrictions and definitions as for the main setups were applied. The horizontal dashed line is the overall mean duration.

Appendix S Changes to benefit levels and effects of the 2010–2013 increase

A large number of changes to benefit rules occurred over the 2010's. To explore the static effects of these changes consistently in the context of the targeted reform, one rule was changed at a time from rules fixed at the 2012 situation, and the benefit then recalculated. Any potential behavioral responses were ignored. It is worth noting that as a general rule of thumb, increases in benefit generosity affect everyone, while reductions only affect spells starting after the reductions came into force. Thus, many of the rules also affected benefits for spells starting in 2012.

The benefits were first calculated as per the true rules of the payment's time to verify the calculation methodology. In most cases, the difference between recalculated and observed daily benefits was extremely minor (for more than 95% of the cases, it was 1 eurocent or less). Finally, to assess the impacts of national changes in base wages, the effects of the simplest possible wage adjustments to the base year were tested, namely indexing the wages to the levels observed for our sample in 2012.

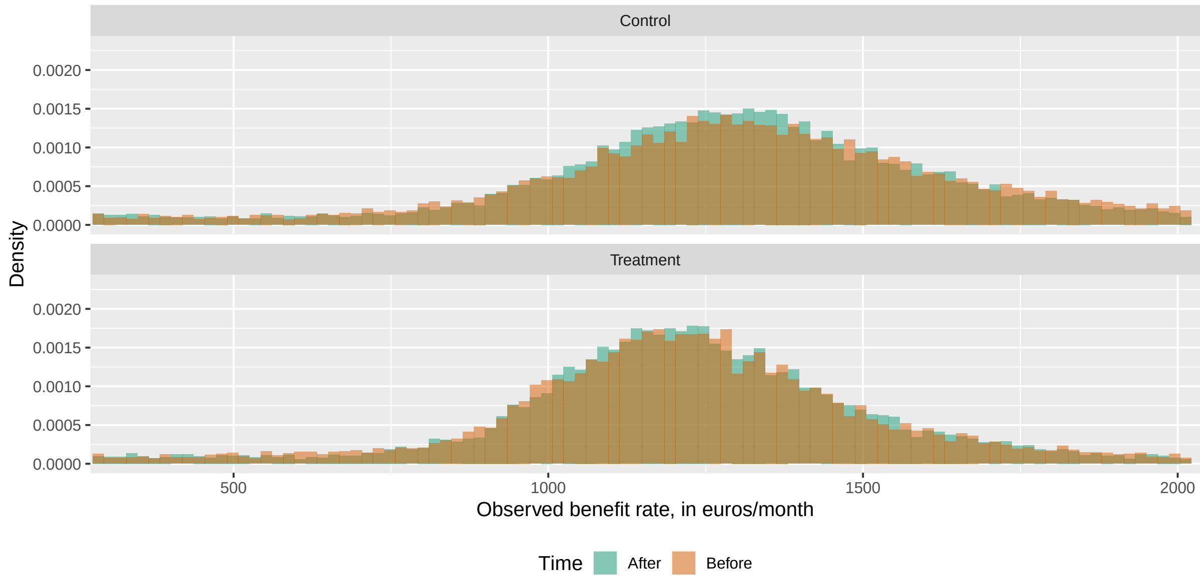
Table 13 collects the findings for each rule change. Overall, it appears that most sources of variation had little effect over the medians or means of the overall benefit (average daily benefits calculated over each spell's duration). In particular, the median overall effect on benefit levels of the removal of the initial benefit increase for the control group, discussed next in more detail, was around 1.2 %.

Table 13: Effects of different benefit rule changes on benefits, from year 2012 rules and levels

Measure	Treatment	Time	Q1	Q5	Q25	Median	Q75	Q95	Q99	Mean	Median of relative change (%)
The removal of the initial benefit increase	Treatment	Before	0	0	0	0	0	0	0	0	0
The removal of the initial benefit increase	Treatment	After	-207.483	-62.673	0	0	0	0	0	-10.321	0
The removal of the initial benefit increase	Control	Before	0	0	0	0	0	0	0	0	0
The removal of the initial benefit increase	Control	After	-393.321	-243.059	-59.121	-15.214	-0.914	0	0	-51.673	-1.190
Changes in the base wage rate	Treatment	Before	-33.099	-13.796	0	0	19.741	52.187	76.804	9.901	0
Changes in the base wage rate	Treatment	After	-39.997	-21.157	-0.084	13.537	31.121	57.197	78.893	15.668	1.139
Changes in the base wage rate	Control	Before	-2.026	0	0	0	43.337	80.869	112.441	23.222	0
Changes in the base wage rate	Control	After	-15.869	0	26.981	44.545	62.865	95.501	126.184	46.004	3.409
The earnings disregard	Treatment	Before	0	0	0	0	0	0	44.145	1.285	0
The earnings disregard	Treatment	After	0	0	0	0	0	92.789	150.000	13.197	0
The earnings disregard	Control	Before	0	0	0	0	0	0	47.328	1.378	0
The earnings disregard	Control	After	0	0	0	0	0.899	106.301	150.000	15.047	0
The change in the maximum rate during partial employment	Treatment	Before	0	0	0	0	0	0	37.244	1.080	0
The change in the maximum rate during partial employment	Treatment	After	0	0	0	0	0	85.280	171.678	10.949	0
The change in the maximum rate during partial employment	Control	Before	0	0	0	0	0	0	44.557	1.157	0
The change in the maximum rate during partial employment	Control	After	0	0	0	0	0	103.043	189.761	13.326	0
General benefit level changes	Treatment	Before	0	0	0	11.632	13.370	19.777	40.149	9.329	0.884
General benefit level changes	Treatment	After	0	9.362	15.372	16.527	17.391	27.950	51.228	17.692	1.359
General benefit level changes	Control	Before	0	0	0	10.320	13.991	21.479	43.364	9.424	0.764
General benefit level changes	Control	After	0	7.128	15.372	17.028	21.443	43.323	54.029	19.019	1.340
Flat-rate benefits for last 100 days	Treatment	Before	0	0	0	0	0	0	0	-0.013	0
Flat-rate benefits for last 100 days	Treatment	After	-135.679	-65.692	0	0	0	0	0	-7.181	0
Flat-rate benefits for last 100 days	Control	Before	0	0	0	0	0	0	0	0	0
Flat-rate benefits for last 100 days	Control	After	-17.970	0	0	0	0	0	0	-0.842	0
The change in the increased rates	Treatment	Before	0	0	0	0	0	7.881	39.864	1.455	0
The change in the increased rates	Treatment	After	0	0	0	0	0	10.123	52.864	2.026	0
The change in the increased rates	Control	Before	0	0	0	0	0	13.585	52.017	2.089	0
The change in the increased rates	Control	After	-0.260	0	0	0	0	7.673	59.208	2.096	0
The change in the wage threshold	Treatment	Before	0	0	0	0	0	0	0	0	0
The change in the wage threshold	Treatment	After	-80.821	0	0	0	0	0	0	-1.181	0
The change in the wage threshold	Control	Before	0	0	0	0	0	0	0	0	0
The change in the wage threshold	Control	After	-81.882	0	0	0	0	0	0	-1.944	0

Q1–Q99 refer to the effect of a rule change, or the daily rate, in the 1st to 99th quantiles in the group respectively.

Figure 71: Observed monthly benefit rates.



The numbers refer to individuals' mean benefit rates, which in turn are calculated for each individual as their mean monthly benefit over their entire spell. Counts below 3 have been replaced by 3 to maintain privacy.

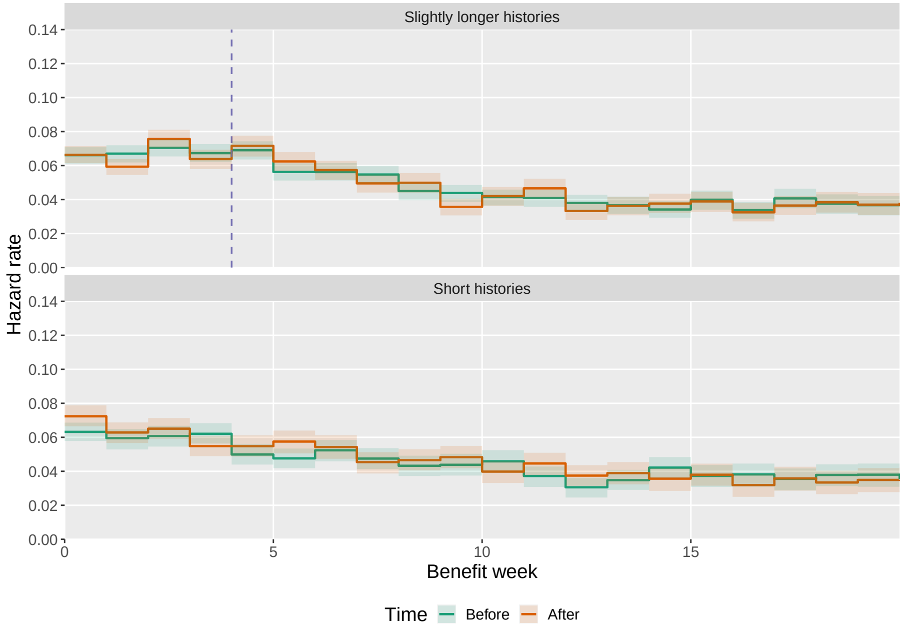
As discussed in the main text, from 2010 to 2013, individuals with more than 3 years of work history were eligible to 4 weeks of increased benefits at the start of unemployment. This increase had only been available for the control group of the targeted reform, and was phased out in the post-period.

To examine the effects of this phase-out on time in unemployment, two difference-in-differences setups were used. In both setups, the DiD estimate for the effect of the increase on mean time in unemployment was close to zero.

The first one is a straightforward replication of the setup used for the targeted reform for years 2008–2011, simply flipping the control and treatment groups. Now, the controls were those with less than 1.5–2.5 years of work history, who did not get an increase. The treated were those with 3.5–5 years of history, who got an increase in the post-period. As data for UA was not available before 2010, only the UI part of the spells were used. Figure 72 shows that there is very little change in the nonparametrically estimated hazard over the first 20 weeks.

The second setup uses a very different sample. Another initial increase type that was still available in 2014 was a longer increase for those with at least 20 years of work history who had been laid off. The duration of this increase was reduced from 20 weeks to 18, while the payment level was slightly increased, but for the earliest weeks in unemployment, those receiving this increase formed a reasonable control group. The treatment group was now defined as those with 15 to 18 years of work history who either collected the old 4-week increase (before 2014) or got no increase (in 2014). In this case, the control group

Figure 72: Early exit hazard, introduction of the early benefit increase



Short histories: 1.5–2.5 years of work history. Slightly longer histories: 3.5–5 years of work history. Before: 2008–2009, after: 2010–2011.

was so small due to the selection criteria that the weekly hazards become very noisy; however, the estimate of the effect on cumulative exit probability for the first 10 weeks of unemployment is close to zero.

Put together, the available evidence suggests that the initial benefit increase probably did not have a very large impact on exit rates from unemployment, due to its short duration compared to the overall insurance entitlement in Finland.

Appendix T Weighting and balance

To deal with potential changes over time in the composition of the groups, a wide selection of weighting and matching approaches were considered. To aid selection, two random set-aside placebo samples that were not used for subsequent placebo tests *or* the main setups were drawn. Within these samples, available covariates that were prognostically important, i.e., helped predict unemployment durations in test regressions according to the Akaike information criterion, were considered for weighting. Both the covariates and the balancing approaches considered were ranked based on (a) no bias introduced in outcomes for the random placebo samples, (b) overall improved balance in observed covariates (i.e., balancing some covariates should not reduce observed balance in other prognostically important variables), (c) effective sample size and (d) computational cost, in that order of priority.

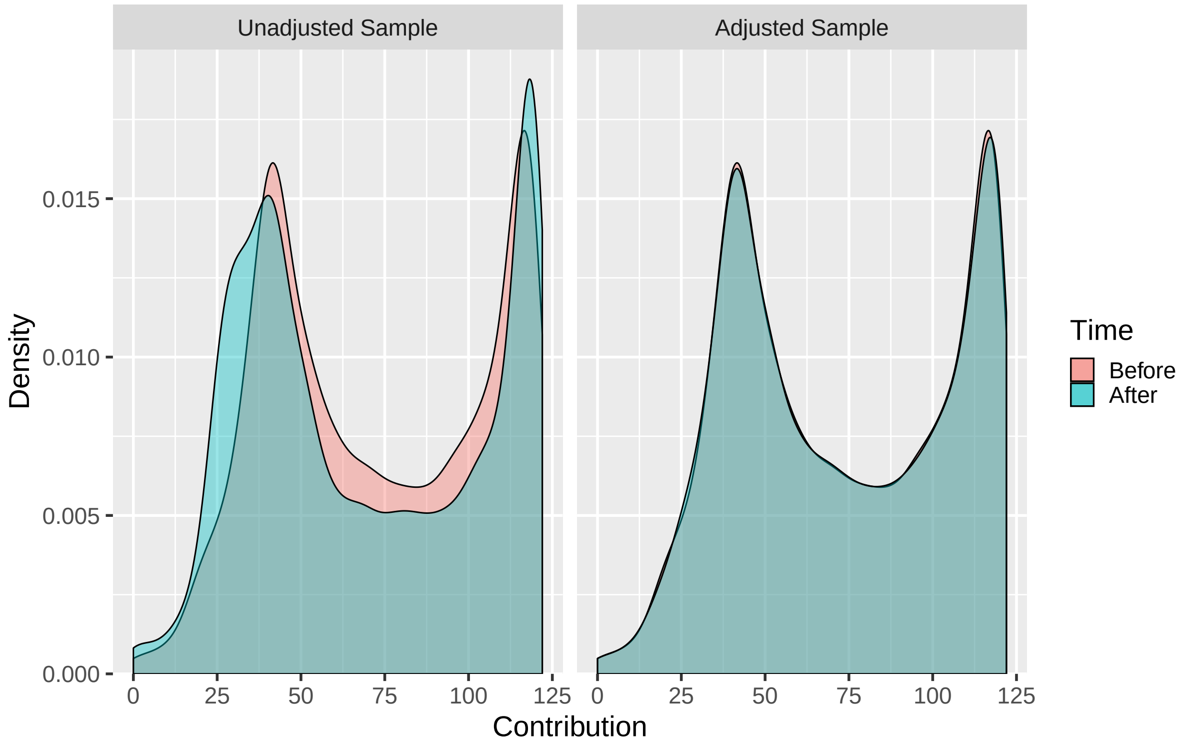
The methods tested included 1-to-1 and 1-to-many propensity score matching, with and without exact matching on contributions towards the recent employment condition, coarsened exact matching, parametric and nonparametric covariate balancing propensity score, Super Learner, energy weighting, generalized boosted models and empirical calibration weights. Among these candidates, empirical calibration weights and entropy balancing weights were ranked the highest, with traditional propensity scores coming close behind. The main results are not sensitive on the choice between these three candidates; ultimately, entropy balancing weights yielded slightly better effective sample sizes and were also computationally the most efficient.

As noted in the main text concerning the estimates, a number of prognostic covariates were considered but ultimately rejected from the weighting process for the targeted reform. This was because they had a large impact on effective sample size, while causing little change in the point estimates. They were thus included only in the regression stage. For the universal reform, the same variables were used for both weighting and regression controls. Additionally, in the case of region, a small reduction in observable balance was considered acceptable to obtain a better match in other respects. (For both reforms, regional labour market tightness was controlled for on region-profession interactions; see appendix Z.) For transparency, balance is illustrated for both the covariates selected for weighting and a number of other variables.

Figures 73–90 demonstrate the acquired balance, complementing the similar figures in the main text.

For the targeted reform, control and treatment groups were balanced *separately* to their pre-reform counterpart. The target was to replicate the balance that existed before the change to recent employment condition and without the error in measuring overall work history. That balance includes small but systematic differences between groups with slightly more and slightly less work experience. Trying to balance these groups to be

Figure 73: Balance in the employment condition, targeted reform, controls.



similar to *each other* would, in this case, *increase* selectivity, as it would weigh atypical individuals more strongly – for example, persons who have longer overall work histories but who happen to be as young or only have as much recent employment as the average shorter-history individual.

Figure 74: Balance in continuous and binary variables, targeted reform, controls.

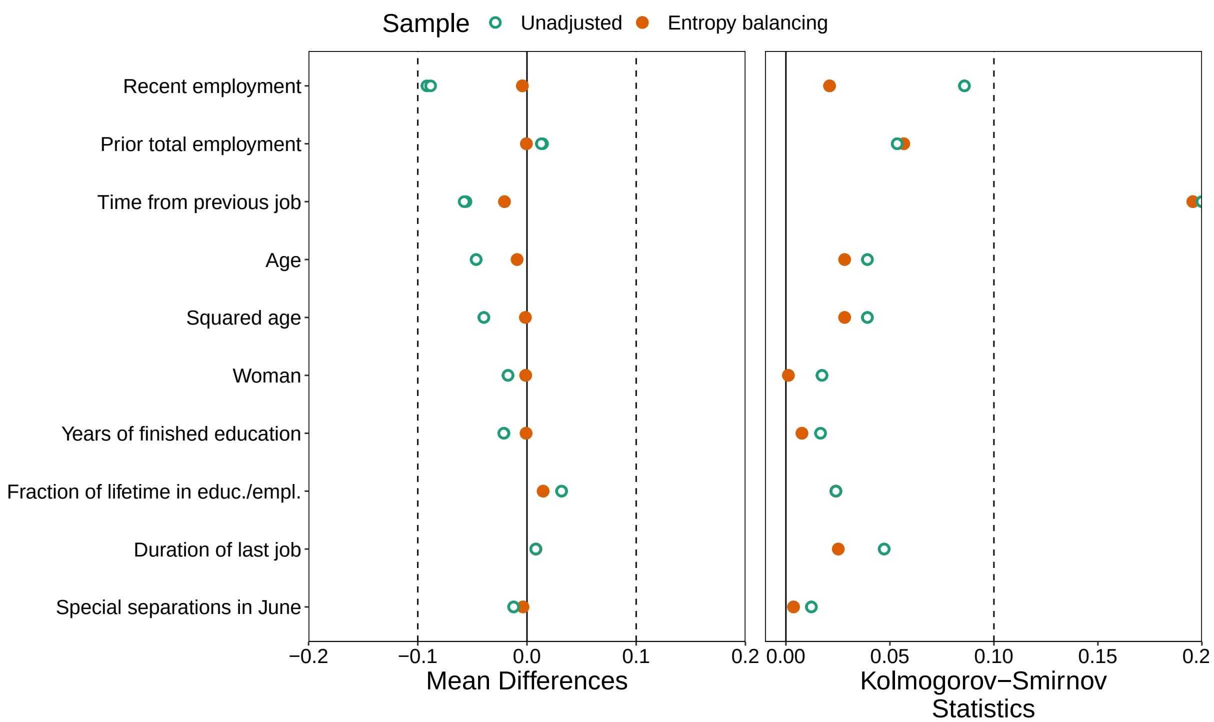


Figure 75: Balance in level of education, targeted reform, controls

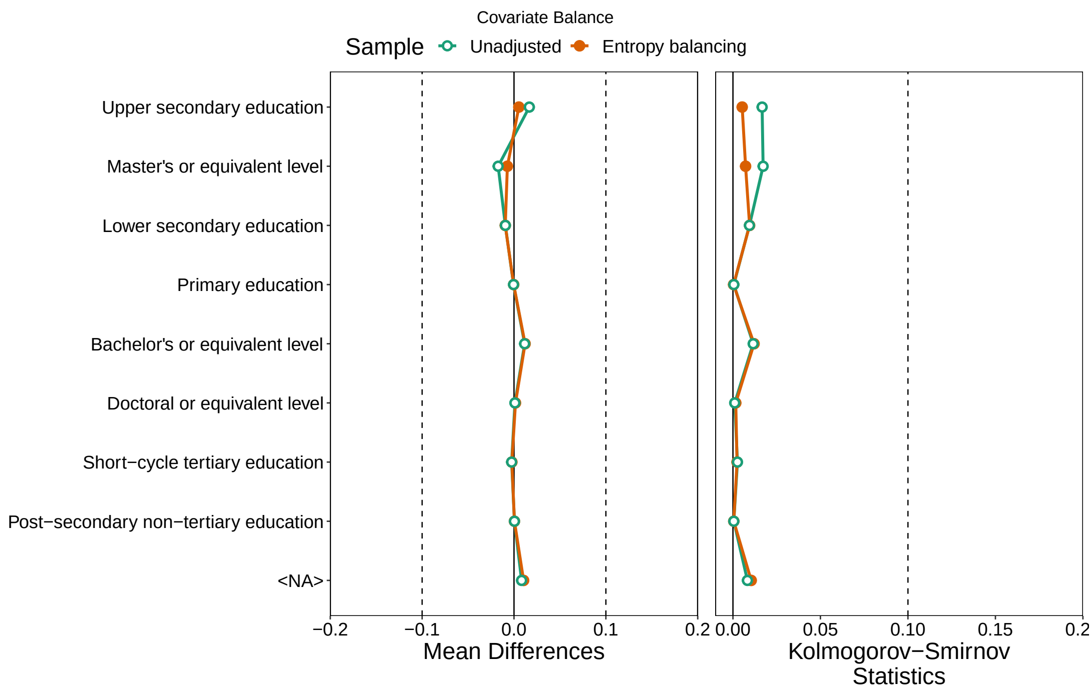


Figure 76: Balance in region, targeted reform, controls

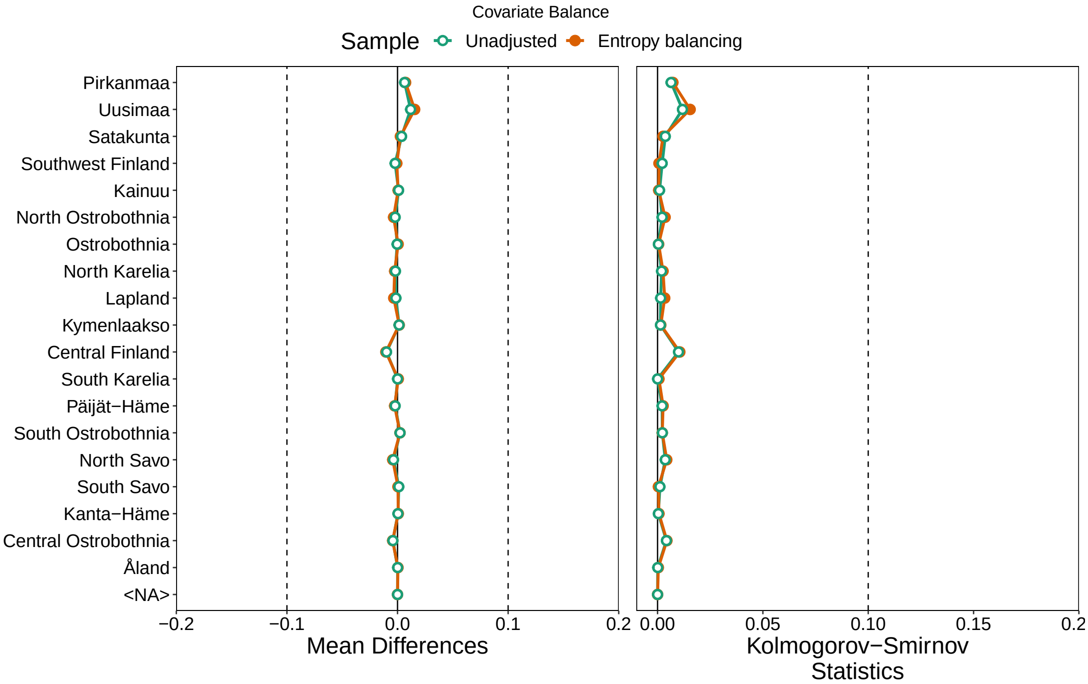


Figure 77: Balance in residence permit, targeted reform, controls

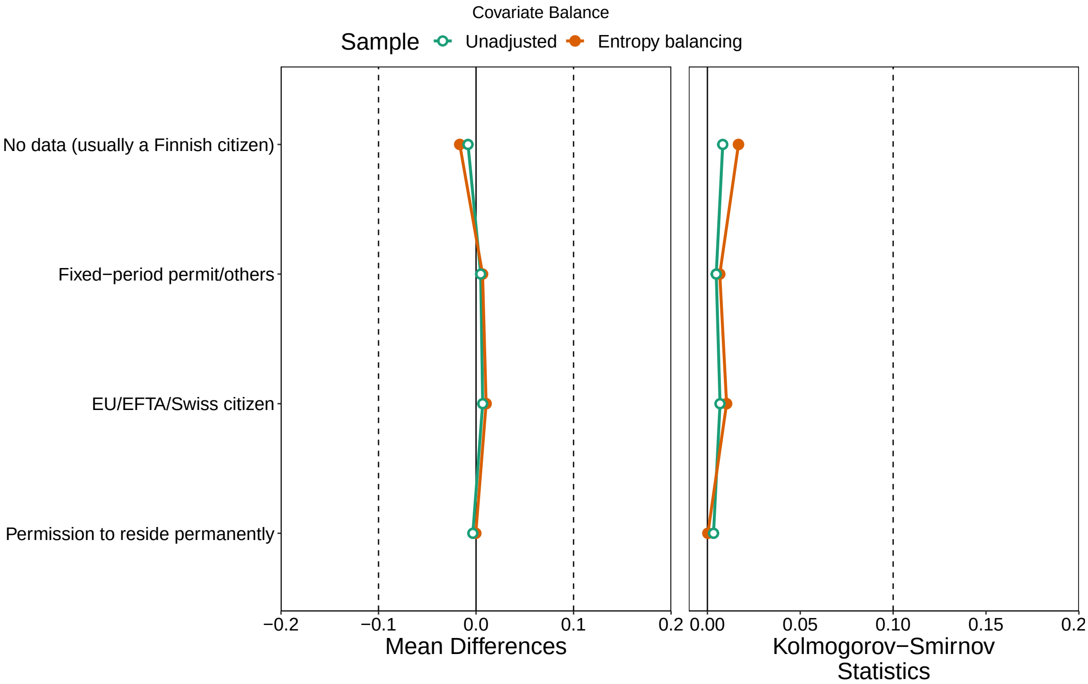


Figure 78: Balance in initial type of unemployment, targeted reform, controls

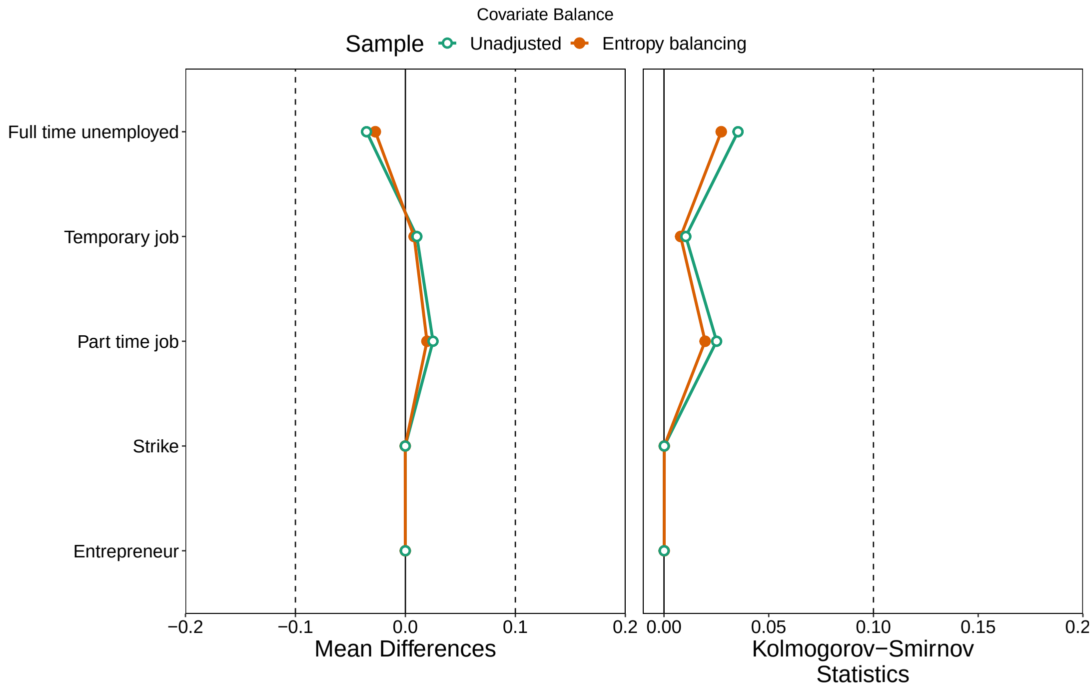
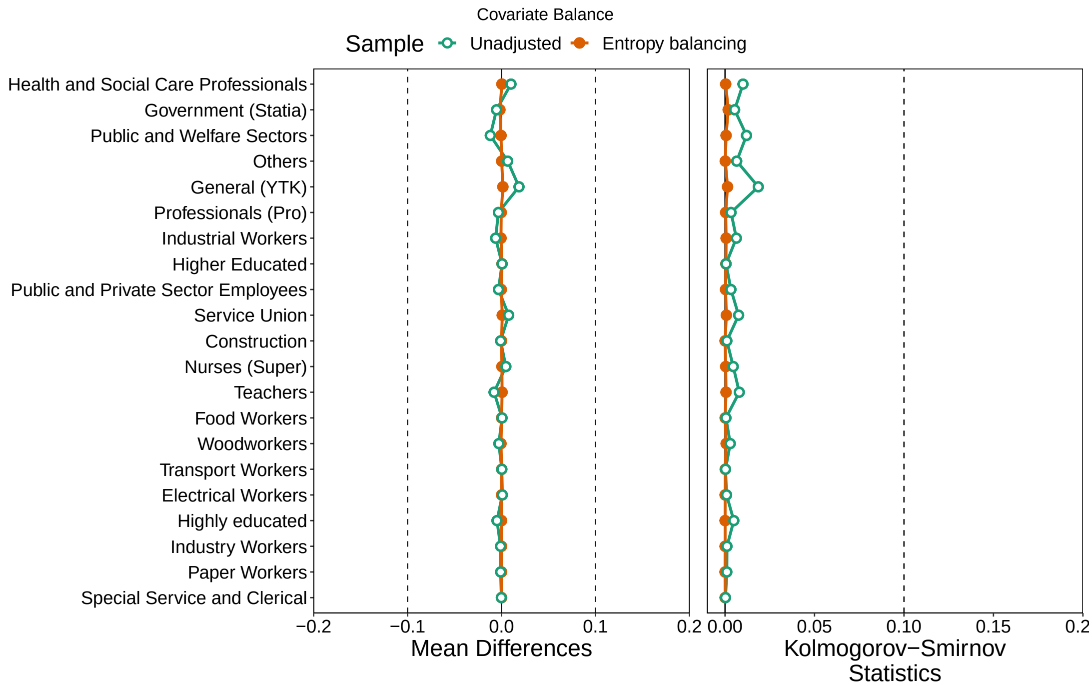


Figure 79: Balance in unemployment fund, targeted reform, controls



Unemployment fund can be considered a reasonable proxy for industry and profession. Due to a change in the statistical classification, some of the profession data for 2013 is suspect.

Figure 80: Balance in level of education, targeted reform, treated

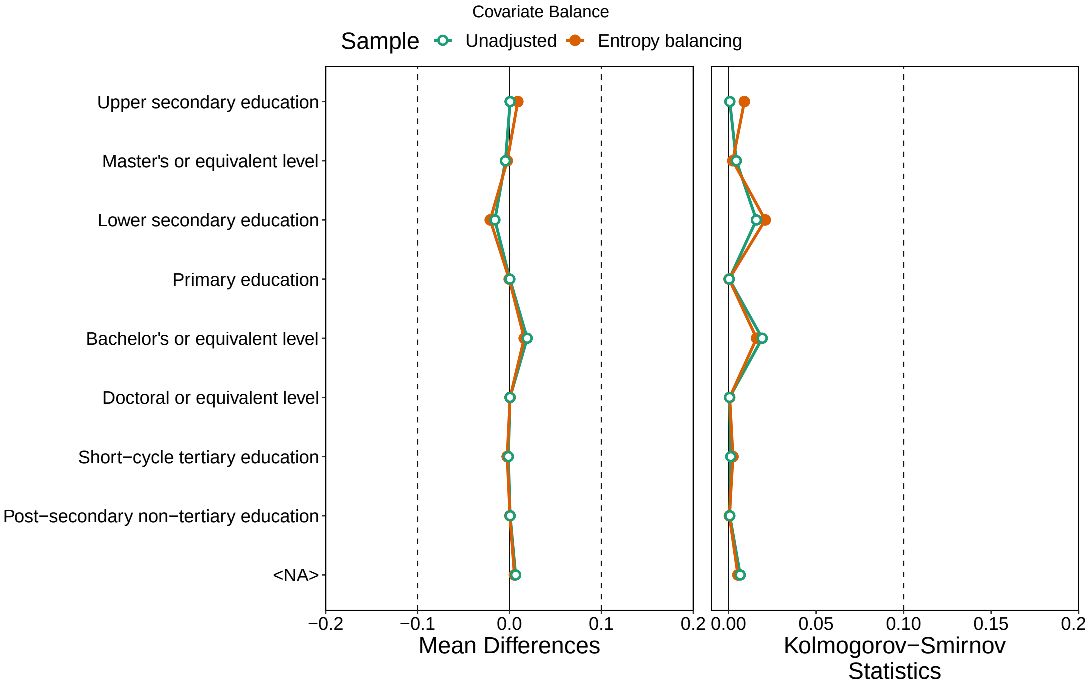


Figure 81: Balance in region, targeted reform, treated

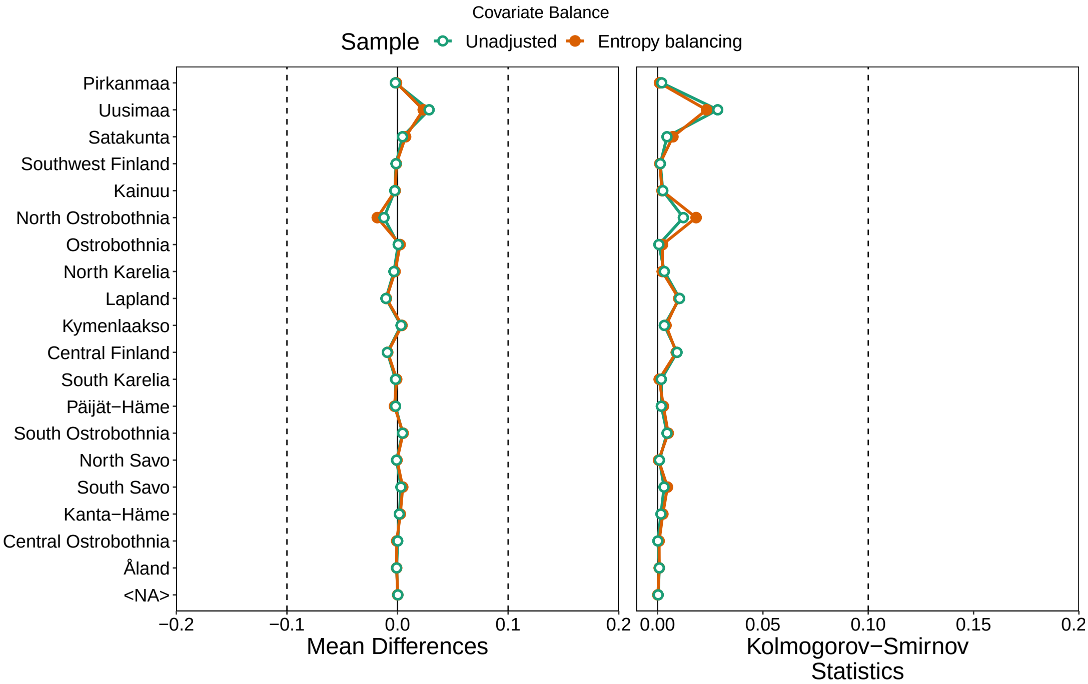


Figure 82: Balance in residence permit, targeted reform, treated

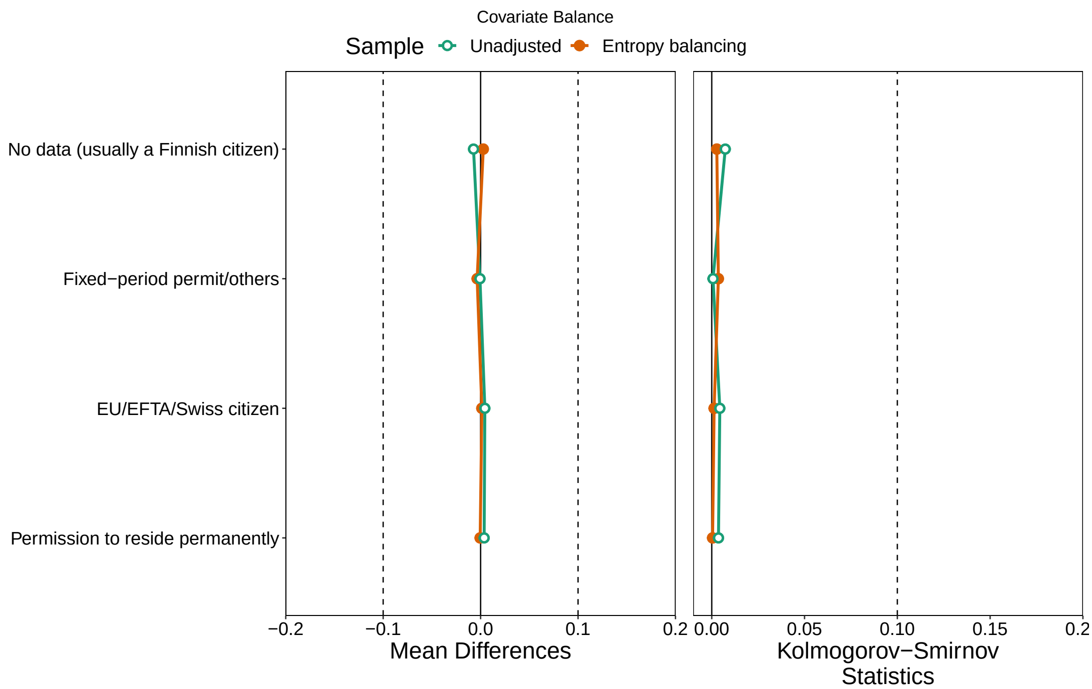


Figure 83: Balance in initial type of unemployment, targeted reform, treated

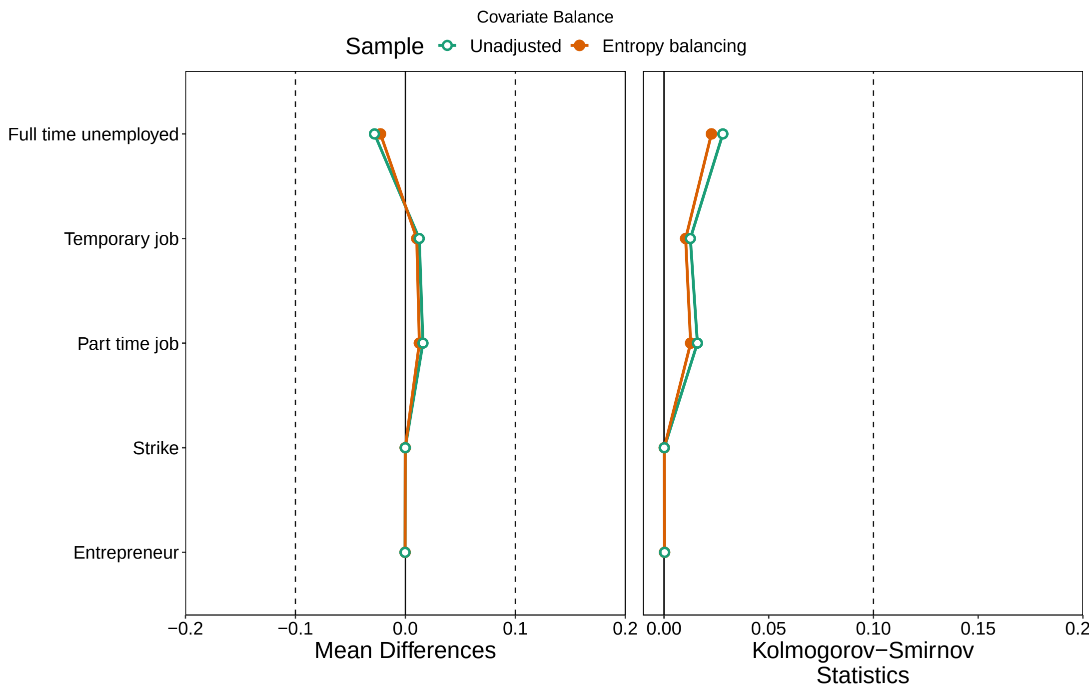
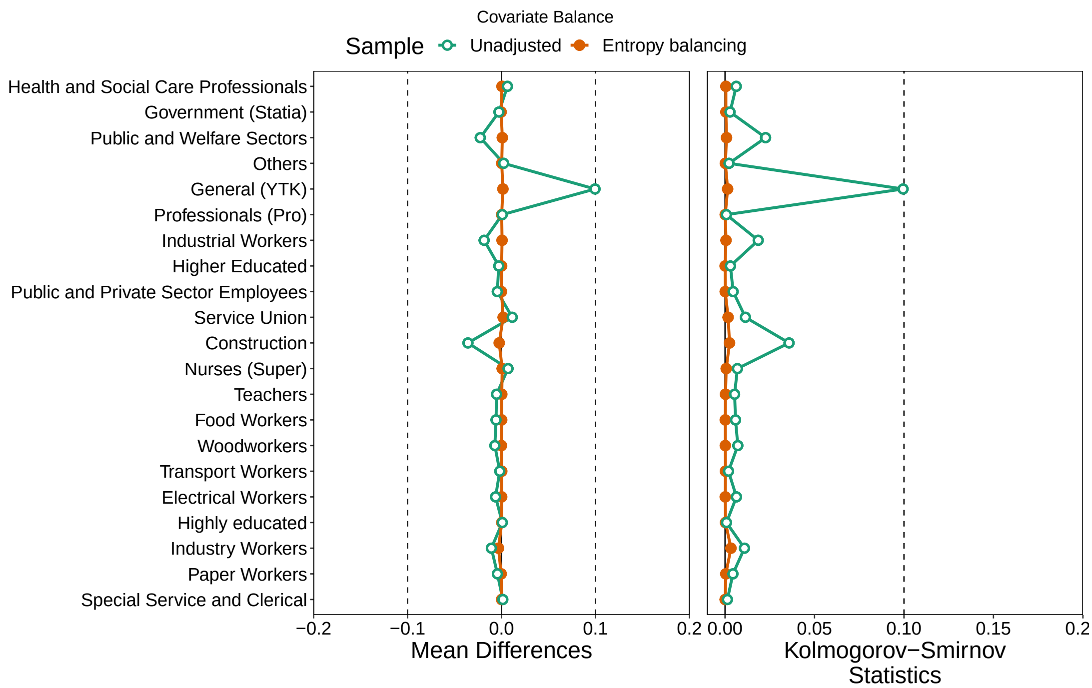


Figure 84: Balance in unemployment fund, targeted reform, treated



Unemployment fund can be considered a reasonable proxy for industry and profession. Due to a change in the statistical classification, some of the profession data for 2013 is suspect.

Figure 85: Balance in level of education, universal reform

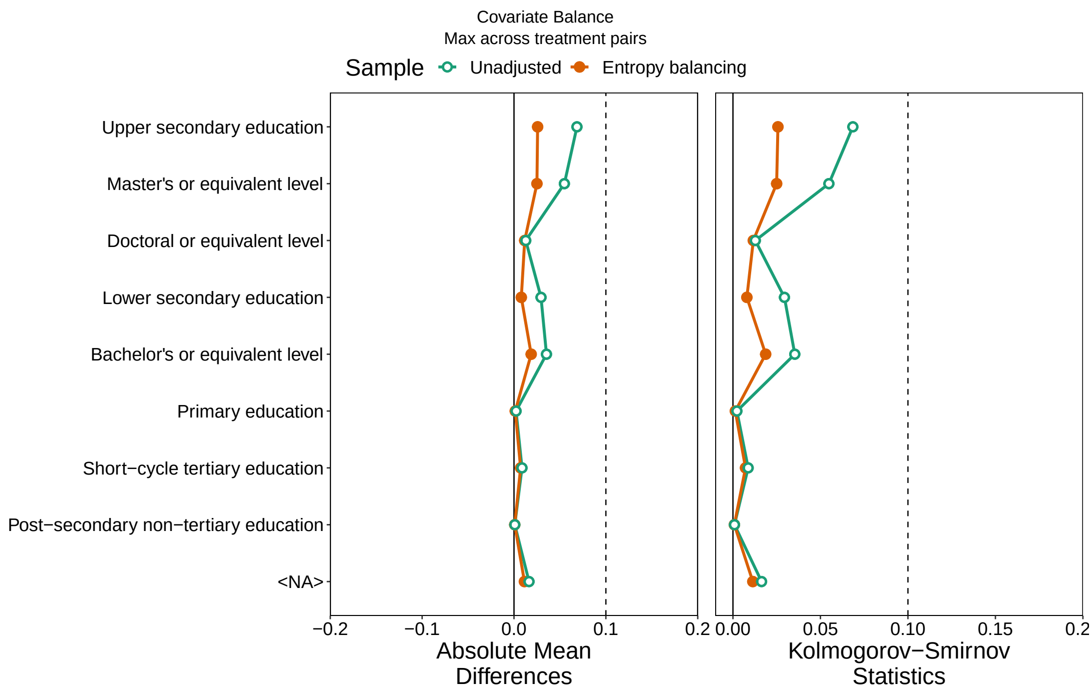


Figure 86: Balance in region, universal reform

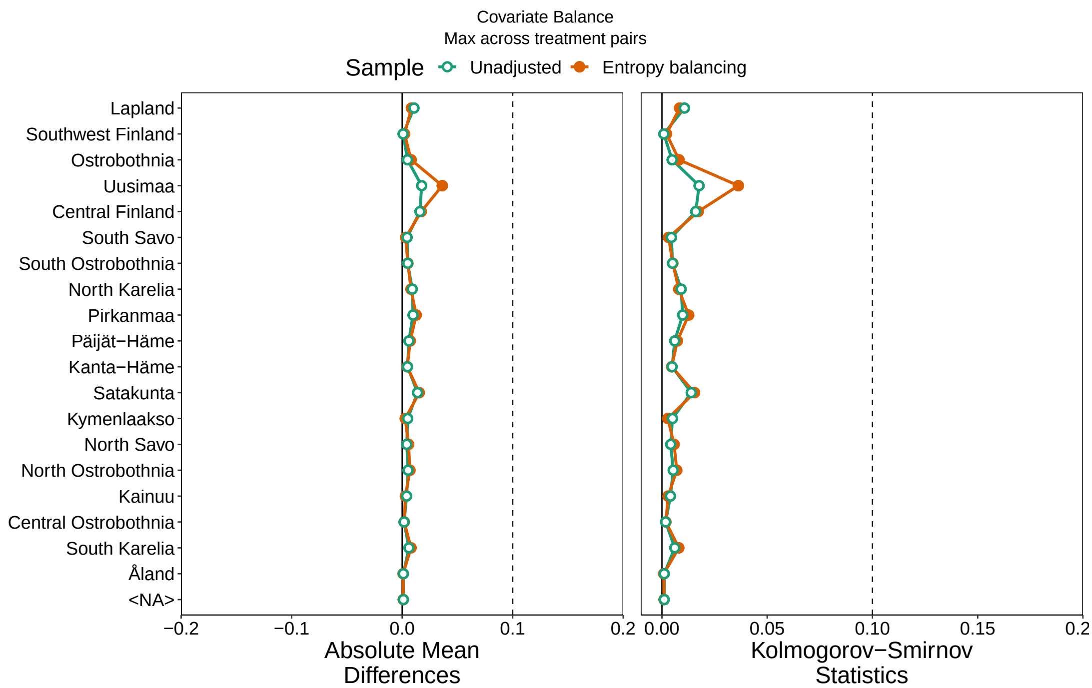


Figure 87: Balance in residence permit, universal reform

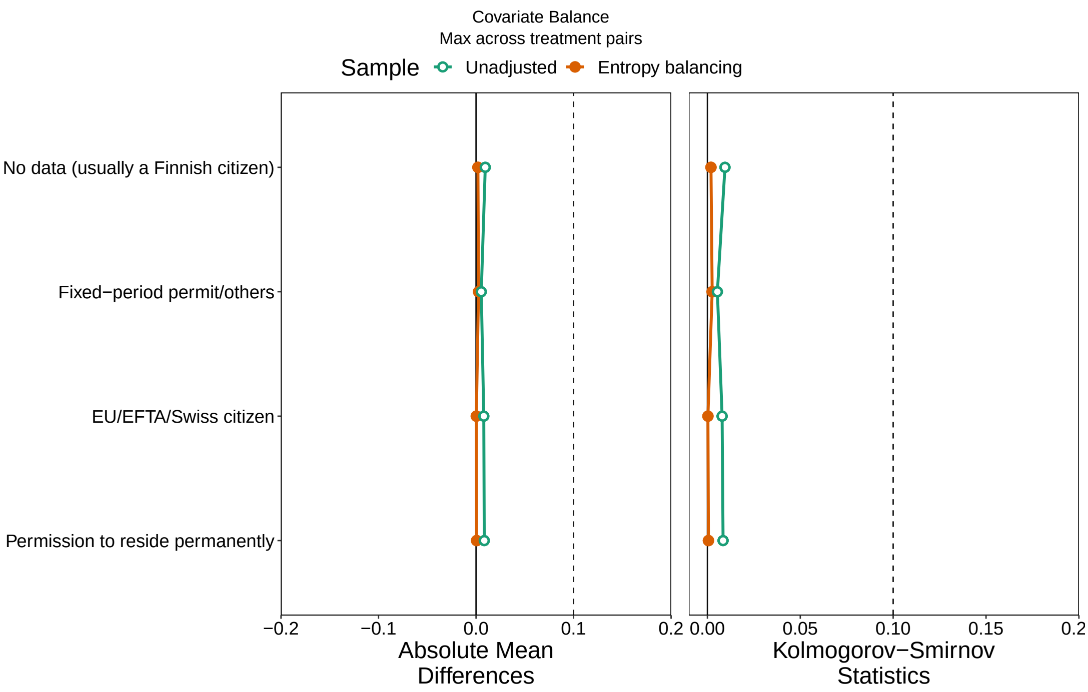


Figure 88: Balance in initial type of unemployment, universal reform

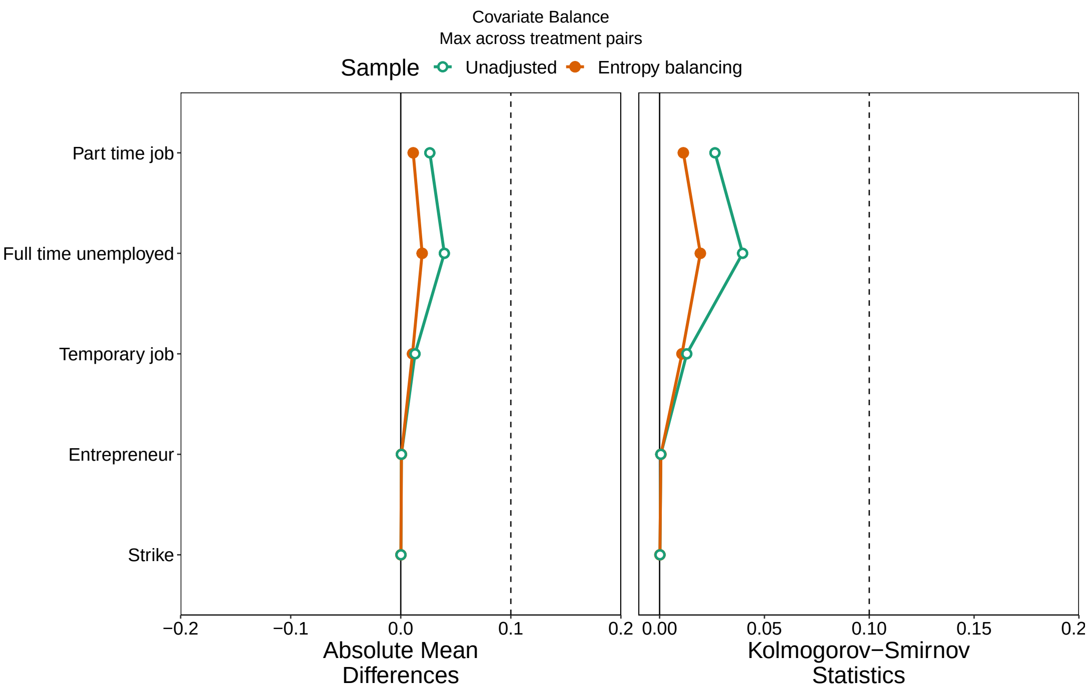


Figure 89: Balance in unemployment fund, universal reform

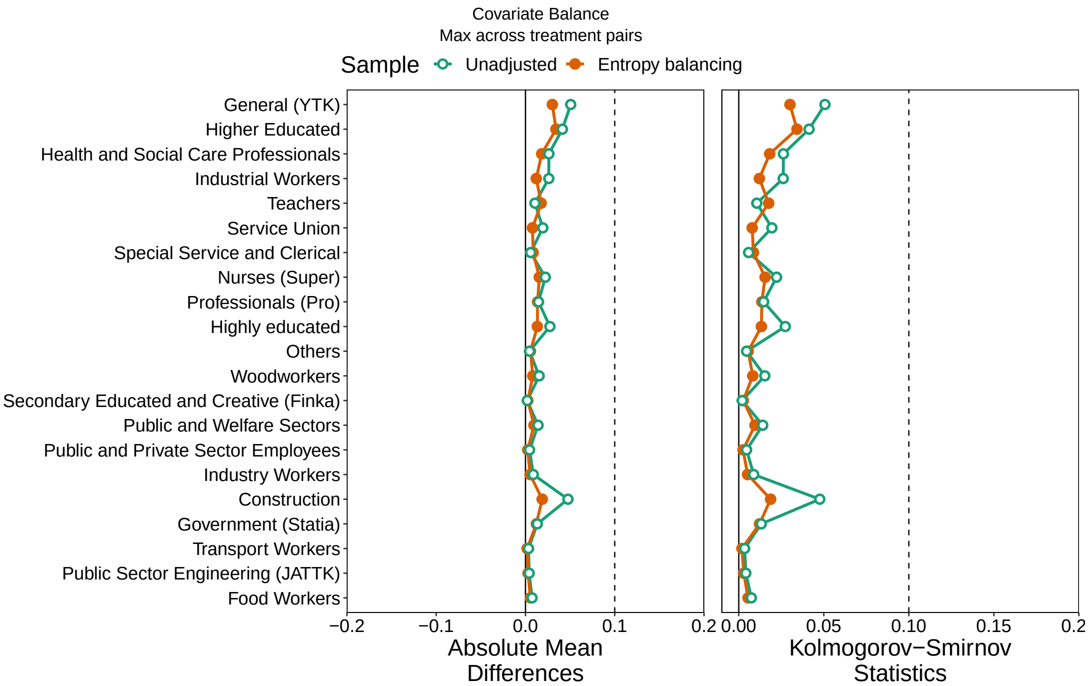


Figure 90: Balance in one-digit profession, universal reform

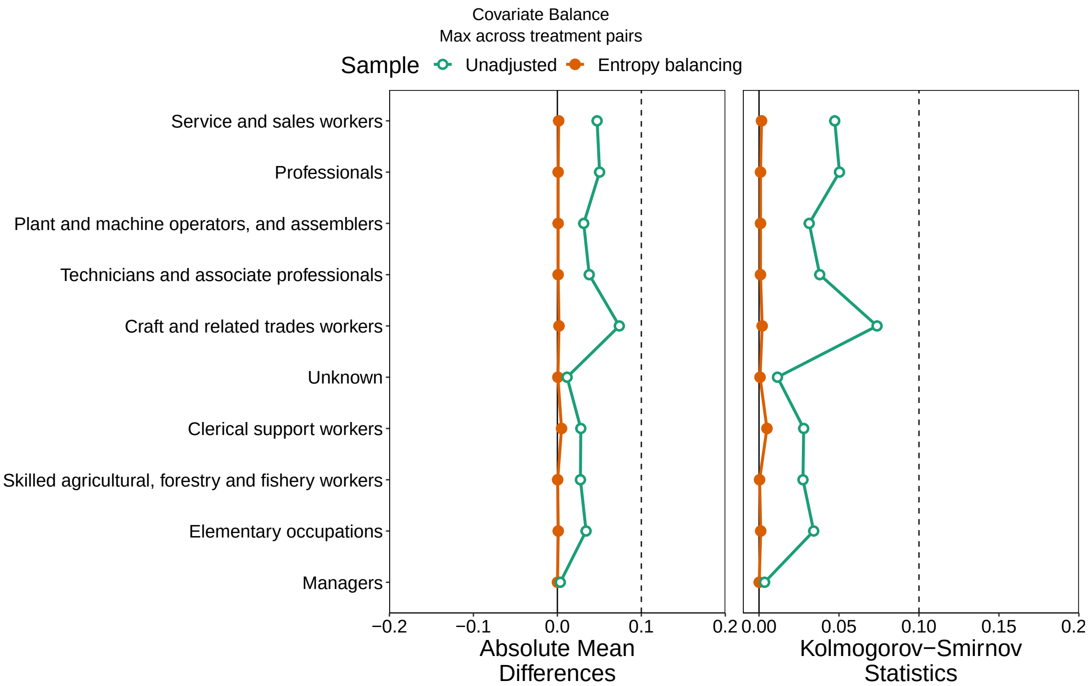
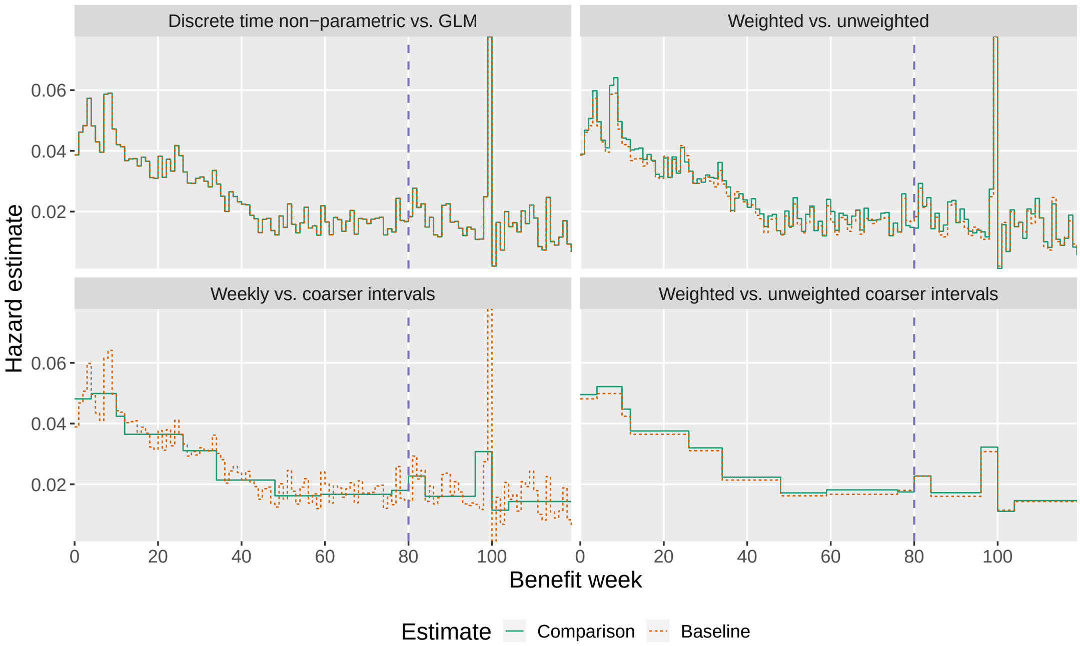


Figure 91: Semiparametric vs. non-parametric estimate of exit hazard



All hazards are for the treatment group of the targeted reform in the post-period. The top-left panel compares the non-parametric hazard estimate with data binned into weeks to a semiparametric estimate on week-length intervals, where the latter is estimated using Poisson equivalence.
The top-right panel compares the same semiparametric estimate with and without the balancing weights used for the main hazard estimates. The bottom-left panel shows how switching from week-length intervals to coarser ones affects the hazard estimate. The bottom-right panel shows the estimates on these longer intervals with and without weights.

Appendix U Choosing the hazard intervals

In the parametric estimate of the hazard, the modeling assumption is that the exit rate is constant over intervals. Using one-week intervals would have left too little power to identify most of the effects. The intervals were chosen starting from one-week intervals and dropping intervals that had the least impact on the average error in the cumulative hazard estimate until reasonable power was obtained. To zoom in on potentially important changes, intervals around 4 weeks (the initial benefit increase in 2010–2013) and on both sides of 80 weeks (the new entitlement) and 100 weeks (the old entitlement) marks were added.

Figure 91 compares the semiparametric and a weekly non-parametric estimate of the hazard, and also shows that the weights used end up having a relatively minor impact on the hazard estimate.

Appendix V The role of the employment offices

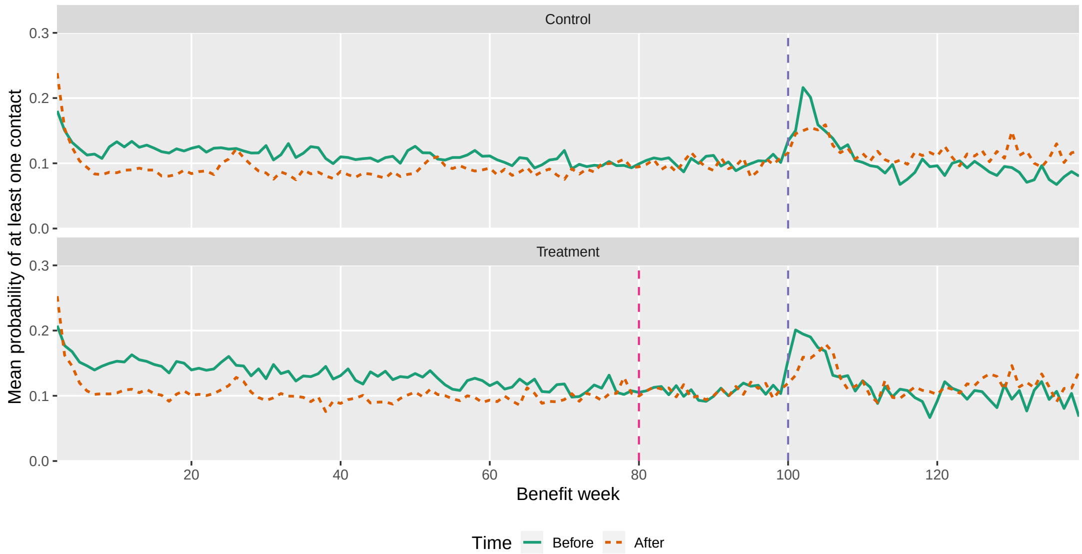
A potential channel which might push people out of the unemployment benefit system are sanctions and threats of sanctions imposed by the public employment offices. Persons who refuse to accept job offers or employment-promoting services, cannot be contacted, or otherwise neglect their responsibilities can have their benefits reduced or be given a sanction period of no benefits. These sanctions are directly observed through the labour market policy statements issued by the offices, observed in the available data. For persons receiving the earnings-related benefits, sanctions imposed for reasons other than simply finding a job or no longer looking for a full-time job (i.e., a regular exit) are quite rare.

Individuals might still be incentivized or helped to transition from unemployment through contacts and guidance from the caseworkers. Regular contacts and interviews are also recorded in the data. For the target group of the targeted reform, there is a noticeable spike in contacts around the exit spike at the 100 benefit weeks mark, as shown in figure 92. However, this spike occurs right *after* the 100 weeks mark, for those who *remain* in unemployment, so it cannot directly be driving the exit spike (the exits at 100 weeks and are not contacted more frequently than others).

Job offers relayed by caseworkers and offers for job placements were also checked from the available data. Offers during the insurance entitlemen happened with relatively low frequencies, and do not exhibit a marked increase before the end of entitlement.

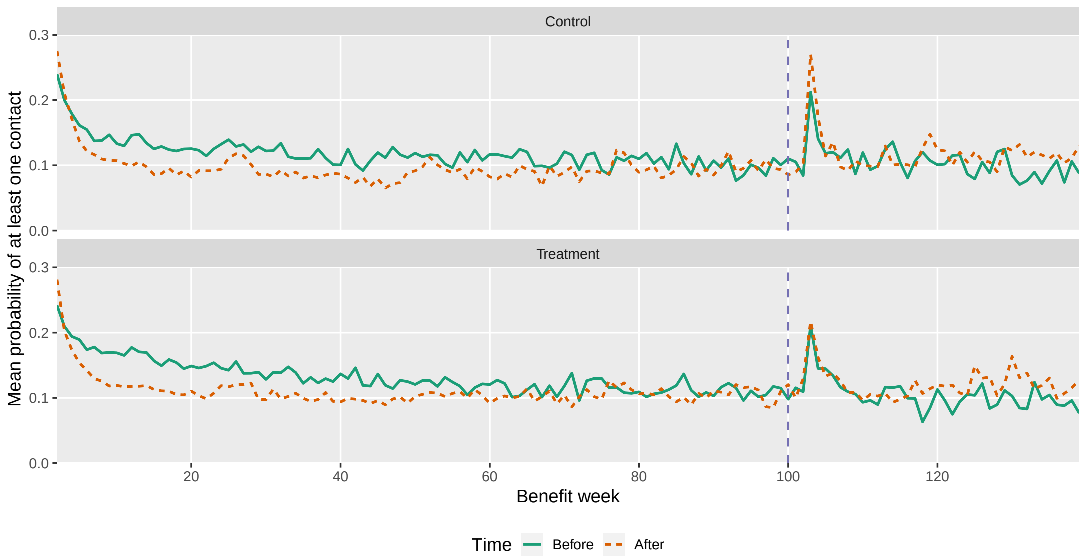
In principle, if the unemployed anticipate the contacts in case they continue in unemployment, they might be exiting unemployment to avoid the pressure from the caseworkers. However, a similar contact spike appears for those who collect the basic unemployment allowance (figure 93), who do not exhibit a clear exit spike (figure 34). Thus, increased contacts appear to be an unlikely driver for the spike.

Figure 92: Contacts from PES caseworkers, UI spells.



Mean probability of at least one contact per FTE benefit week for the sample used for the targeted reform. Contact dates are matched to FTE benefit weeks by combining the contact data and benefit payment data.

Figure 93: Contacts from PES caseworkers, basic unemployment allowance spells.



Mean probability of at least one contact per FTE benefit week for those receiving basic unemployment allowance. Sample restrictions are similar to the ones used for the targeted reform.

Appendix W Measurement error in prior employment

Over the 2010's, the law on unemployment benefits stipulated three different ways of calculating a person's prior employment. First, for qualifying for certain benefits targeted at those with very long careers or the oldest individuals, pensions records for wages are to be used. These are mostly of little relevance to this paper.

For the recent employment condition, the individual must work at least 18 hours a week, with a wage satisfying the applicable collective agreement. If no collective agreements apply, a wage threshold is defined directly in the law (1 331 euros per month in 2023 levels). Neither condition could be directly verified from the annual wage and job contract data available, so they were approximated by weekly days worked and with an estimated daily wage satisfying at least the fallback wage threshold in the law.

By default, the recent employment condition is checked over the preceding 28 months. The review period can, however, be extended for a large number of reasons, such as studies, child homecare, military service and sickness. Insofar as these extensions could be determined from the available data, they were used to extend the estimated review periods, but the data on all of the conditions was imperfect. If a person has been entitled to either unemployment allowance before, the review period cannot extend beyond the start of the latest entitlement.

The third measure, prior work history, is the most important for this paper, and is used for two main purposes: to determine eligibility for an increase in benefits for the first 4 weeks of unemployment (2010–2013), and to determine the maximum entitlement (2014–present). This measure covers each day of past employment, regardless of wages and hours worked. According to the government bills detailing how the prior history should be calculated[6], it should primarily be checked through proofs of employment from employers, presented by the insurance applicant, although other sources such as pensions records could "also" be used. In practice, communication with experts at the Federation of Unemployment Funds in Finland confirmed that, apart from the funds' own records, a primary source for this measure were the employment records by the Finnish Centre of Pensions, the same data used in this paper, and only exceptionally were proofs of employment requested. The way the funds construct this measure was carefully checked with the Federation's workers, but no clear reason for the observable discrepancies were found.

While job start dates and annual wages are deemed accurate, job end dates and estimated monthly wages might have more error. First, not all contracts stipulate a constant wage and hours for each month. Second, there are likely cases where employers only inform the pensions agency about a contract termination with some delay, judging by the fact that many spells of full-time unemployment directly overlap with the job data.

[6] HE 113/2016, p. 12; HE 90/2013, p. 27; HE 179/2009, p. 24; HE 48/2005, p. 28

Direct overlaps between employment and full-time unemployment have been directly pruned for *recent employment* and *future employment*, but not *work history*. Similarly, high-frequency data about wages in part-time unemployment was used when distributing annual wages to days in employment. Each choice was motivated by how they affected the relevant observable measurement error in the unemployment data. As noted above, for total work history in particular, the unemployment funds routinely use the same pensions contributions data, as other documentation from distant past might not be readily available. For recent employment, they can use their own payments records and request more fine-tuned data directly from employers and employees.

The measurement errors in each of the measures can be indirectly assessed through three ways. First, regarding the recent employment condition, the benefit that a person receives is indicative of whether or not they satisfy the condition. Cases where a person started a new entitlement during otherwise uninterrupted unemployment (i.e., through part-time employment) were specifically used to tune the parameters to measure the employment condition. Figures 94 and 95 compare the estimated recent employment weeks to the benefit actually received at the start of spell. The figures show that while there is a clear correlation between measured employment and benefit type, a sizable fraction of the unemployed are receiving the "wrong" benefit (receiving the allowances before they should be eligible, or receiving the subsidy after they should be entitled to an allowance).

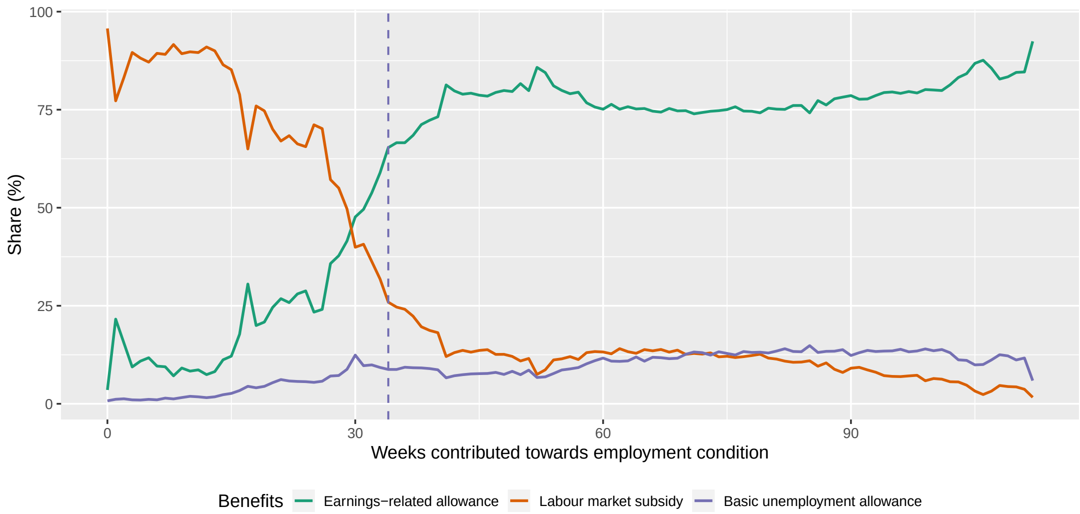
For the work history measure, the mismeasurement can be assessed, first, by looking at whether a person was paid the 4-week increase at the start of entitlements in 2010–2013, as this increase was only available for those with sufficient history.[7] Error using this data is shown in figure 96 for new entitlements in 2010–2013. Mismeasurement is defined as the share of persons who should be eligible to the increase, but ends up receiving it, or vice versa. A slightly extended sample was also used here which relaxed some restrictions (such as furloughs being dropped), to check whether the sample selection criteria might be correlated with the mismeasurement. Recall, however, that because mismeasured cases here could be detected *from the start of a spell*, they were dropped when balancing the sample, and the post-reform sample was then weighted to mimic the pre-reform counterpart with no error.

For years 2014–2016, the mismeasurement could only be observed for the highly selective subset who actually continued in unemployment for at least 80 weeks. Figure 97 illustrates the degree of this error and how the weights used affected it.

To place the groups in context, figure 98 shows the overall distribution of prior employment across those receiving unemployment benefits. As is evident from the figure,

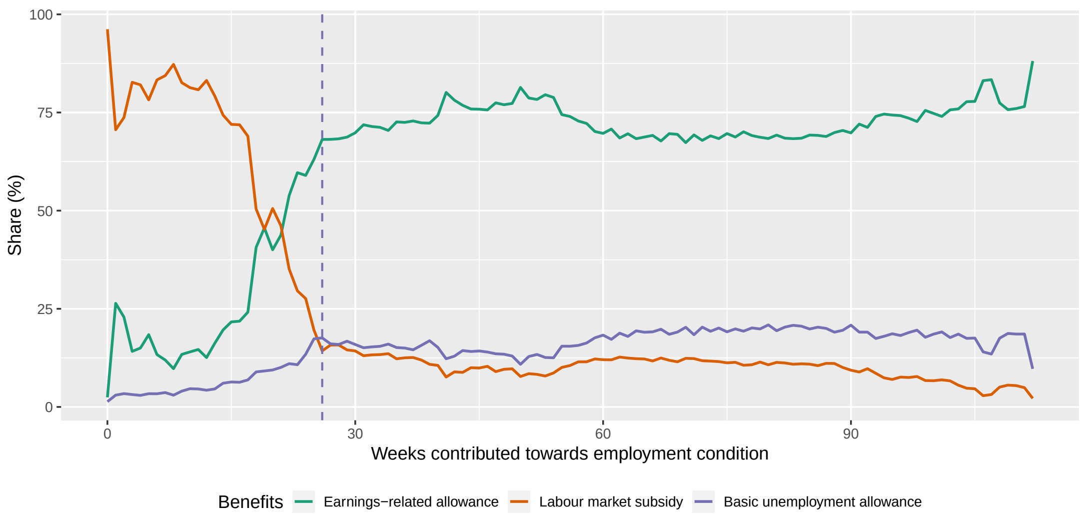
[7] The increase did not require that the applicant apply for it specifically; the funds routinely checked the history at the start of each spell. If a person was receiving an increase due to a "long working career" or during ALMPs, these increases took precedence; the rare cases where these were paid at the start of the spell were excluded.

Figure 94: Type of benefits by observed employment, 2010–2013.



New spells starting in 2010–2013 vs. employment condition at the start of the spells. The dashed vertical line is the formal employment condition required for receiving the allowances at the time.

Figure 95: Type of benefits by observed employment, 2014–2019.



New spells starting in 2014–2019 vs. employment condition at the start of the spells. The dashed vertical line is the formal employment condition required for receiving the allowances at the time.

both the treated *and* the control group chosen for the targeted reform are in the lowest quartile of observed employment.

Finally, figures 100 and 99 illustrate the data coverage. The data provided by the Finnish Centre of Pensions covered all the individuals who were found in the unemployment benefit or the employment office jobseeking registers at any point in the 2010's. As most working-age individuals experience at least some temporary unemployment on occasion, this translates to a significant fraction of all employees and wages being covered (figure 100). Further, conditional on appearing in the dataset overall, it appears likely that most periods of employment are being covered (figure 99).

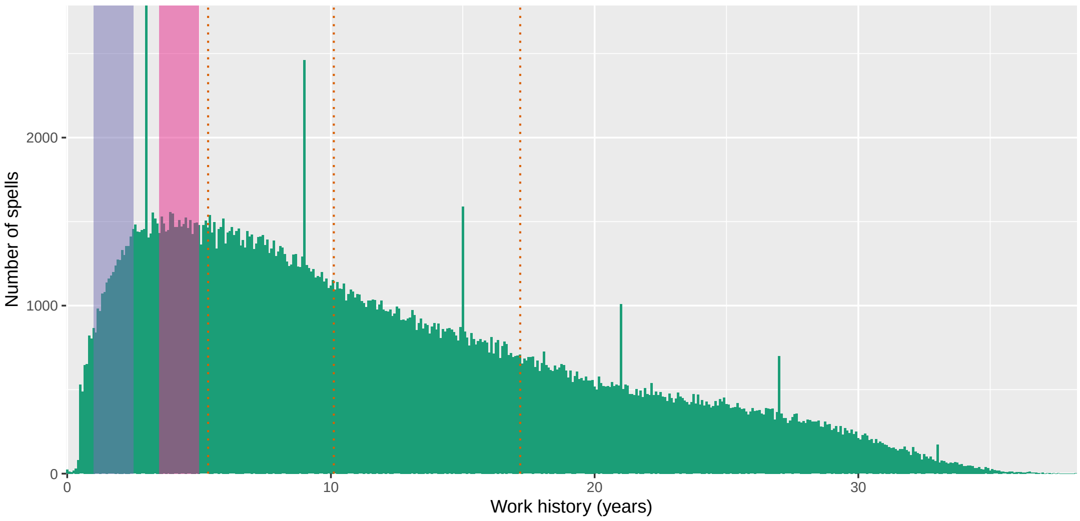
Figure 96: Observed mismeasurement for work history, 2010–2013.

The mismeasurement share is reported as a function of measured work history. The shaded area corresponds to the actual bandwidth of observed employment used to define the treatment and control groups for the targeted reform.

Figure 97: Observed mismeasurement for work history, 2014–2016.

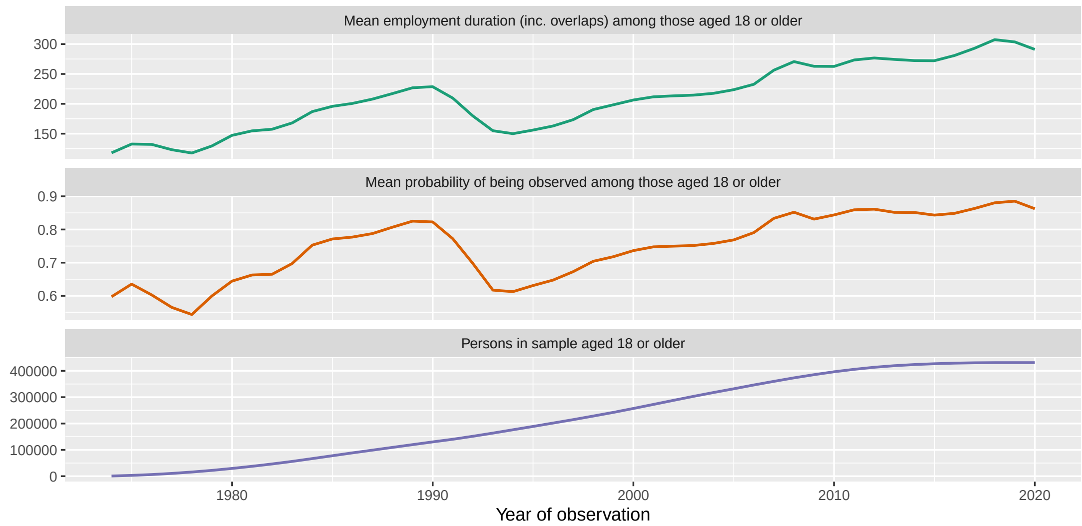
The mismeasurement share is reported as a function of measured work history. The shaded areas correspond to the actual bandwidths of observed employment used to define the treatment and control groups for the targeted reform.

Figure 98: Distribution of observed prior employment.



Prior employment estimated at the start of the spell. The dashed lines correspond to the 25th, the 50th and the 75th quantile. The shared areas are the bandwidths used to define the treatment and control groups for the targeted reform.

Figure 99: Employment observed for those in the data



Observed days of employment, probability of being observed, and persons in sample by year of observation. All the numbers are for those aged at least 18 in a given year *and* appearing in the dataset.

Figure 100: Dataset available vs. all the unemployed

Comparison of the entire available dataset (all those unemployed) versus all the employed per annum.

Appendix X Recent employment versus work history

While recent employment and work history are correlated by construction, this correlation is far from perfect. Figure 101 shows a heatmap of the multivariate distribution of observed work history and observed recent employment for UI entitlements started between 2010 and 2016. The most common observed recent employment value is the maximum across different work histories, even though those with less histories also often have less recent employment. Figure 102 shows a similar heatmap for the generalized[8] median duration by a (work history, recent employment) pair, showing that neither variable is related to unemployment duration in a straightforward, linear way. It is worth noting that both variables differ from the duration of the last job, which only counts one job.

Since both samples were already carefully selected, in particular based on total work history, trying to split them based on recent employment as well might easily lead to a non-representative sample. In particular, there is no guarantee that the identifying assumptions would hold across arbitrary partitions of the data. Thus, no attempt was made to directly examine whether those with less recent employment respond more or less weakly than others to entitlement cuts. However, the fact that mean durations differ by both recent and total past employment does suggest that responsiveness may also vary across both of these variables.

The best available direct evidence for the responsiveness by recent employment comes from previous research in other countries, such as the study by Le Barbanchon (2016), who finds that those with less recent employment respond more strongly to entitlement cuts. The question remains directly relevant for policy: a governmental working group in 2021 discussed changing the current system, where entitlement duration varies by work history, to one where the entitlement duration would instead vary by recent employment.[9]

It is also worth noting that persons with only a few years of work history typically have not yet received UI. Those that do might not be representative of other short-history individuals who don't, and they are typically experiencing their first UI spell. Figure 103 shows the share of work history cohorts who have received either UI or any unemployment benefits by the end of 2022. The population in this case is the entire Finnish population aged 18–65 by end of 2022, and work histories are calculated between 1987 and 2022.

Finally, the measure of recent employment in this paper aligns with the formal eligibility criteria for unemployment benefits. This is the measure that most urgently needed balancing for the targeted reform, which directly loosened said criterion. Additionally, a governmental working group has explicitly discussed staggering the entitlement by the contributions according to this particular measure, making it directly policy relevant. This measure allows for substantial extensions of the review period, i.e., what is consid-

[8] The median duration is calculated as a mean of three values around the median to maintain privacy.
[9] http://urn.fi/URN:ISBN:978-952-00-9879-7

Figure 101: Distribution of work history and recent employment

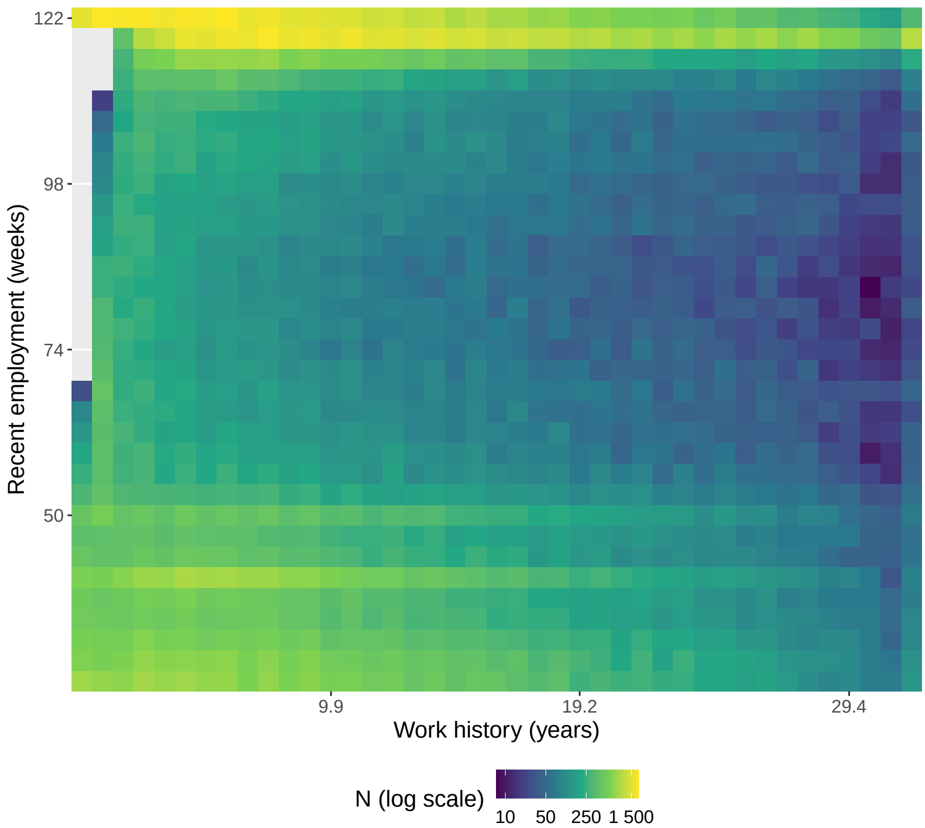


Figure 102: Unemployment duration by work history and recent employment

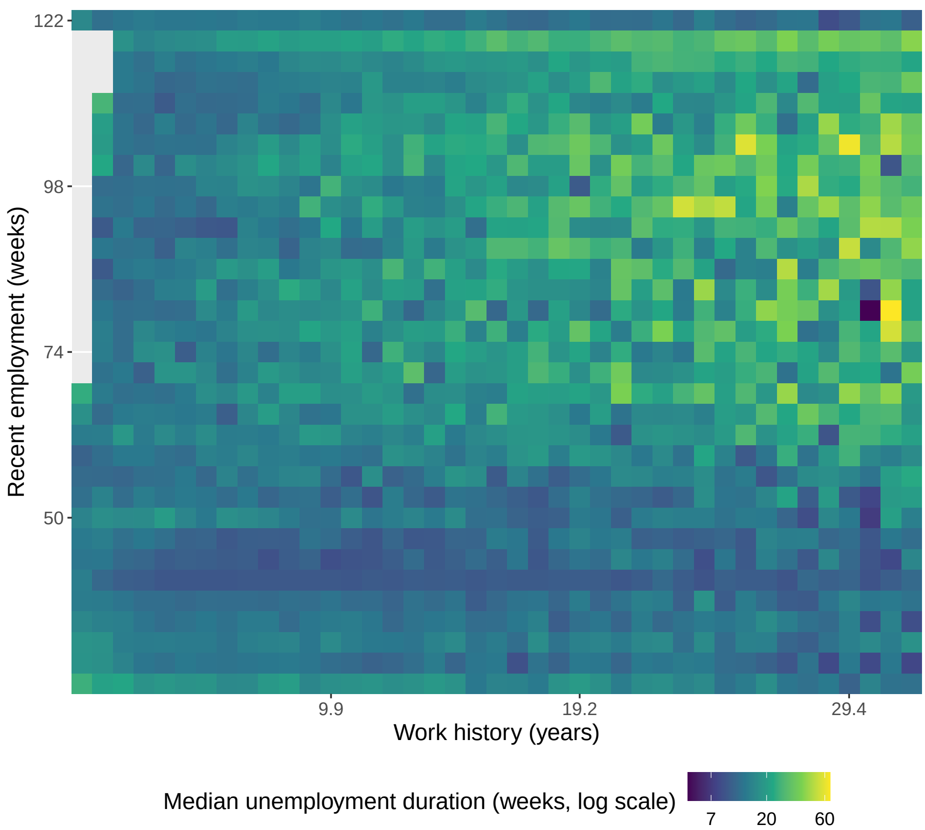


Figure 103: Prior employment and unemployment benefits

The shaded areas correspond to the work history bandwidths used to define the treatment and control groups for the targeted reform.

ered "recent": employment from up to 9 years ago can be included if the person has a valid reason for their absence from employment. A best effort has been made, given the available data, to take these absences into account when estimating recent employment in this paper. Thus, the measure may be quite different from other ways of calculating time worked recently.

Appendix Y Additional datasets

In addition to the data described in the main text, a number of additional datasets were available. These datasets had a number of limitations, in particular for their time coverage or frequency, so they were only used for the analysis in the appendix. Table 14 collects both the "base" datasets and expanded ones.

Table 14: Description of datasets

Dataset/source	Covers	Frequency	Time	Notes
Kela-UA benefits (Social Security Institution)	Each payment of UA (labour market subsidy, basic unemployment allowance)	One day	2010–2021	Base dataset
Fiva-UI benefits (Financial Security Institution)	Each payment of UI (earnings-related unemployment allowance)	One day	1999–2021	Base dataset
ETK-employment (Finnish Centre for Pensions)	For the unemployed: insured job contracts: start and end	One day	1948–2022	Base dataset; coverage on insurance mandate
ETK-wages	For the unemployed: wages per contract	Annual	1948–2022	Base dataset; coverage on insurance mandate
ETK-absences	For the unemployed: periods of non-employment (parental and child home care allowances, rehabilitation allowances, dates of educational attainment, adult education allowances, and a subset of sickness and accident allowances)	Annual	2005–2022	Base dataset; valid absences from employment for extending the review period
TEM-jobseeking register (Ministry of Economic Affairs and Employment)	Registered jobseekers: birthdates, gender, attained education, profession, nationality, residence permit, language skills, labour market training, jobseeking status, jobseeking plans, labour policy statements, contacts with PES offices, job offers, residence; vacancies and job placements	Daily (when in register)	1991–2019	Base dataset (–2019), auxiliary (2020–2021)
FOLK Income (Statistics Finland)	Incomes: wages, business, property, pensions, social assistance, unemployment, sickness allowance, housing benefits, study grants, child home care and parental allowances	Annual	1987–2019	Auxiliary dataset
FOLK employment periods (Statistics Finland)	Finnish employment contracts, including employer characteristics	Daily	1987–2019	Auxiliary dataset; mostly based on ETK data until 2018
FOLK Basic (Statistics Finland)	Demographic data, including birth year, marital status, student grants, main activity at end of year, household and family characteristics, debt	Daily	1987–2019	Auxiliary dataset
FOLK pension periods (Statistics Finland)	Pension start dates and types of pensions	Daily	1995–2019	Auxiliary dataset
Incomes Register: wages (through Statistics Finland)	Wage earnings, employer IDs, taxes and distraint on wages	Payment period (monthly)	2019–04/2023	Auxiliary dataset
Incomes Register: benefits (through Statistics Finland)	90% of benefits, taxes and distraint on benefits; excludes social assistance and some pensions	Payment period	2021–04/2023	Auxiliary dataset

All datasets could be linked at the person level *except for* ETK-employment and ETK-wages, which could only be linked to the other base datasets for technical and privacy-related reasons. This means that for any analysis using the auxiliary datasets, the employment data is slightly different, as the auxiliary employment data could only be linked to wages at the (person, year) level instead of the (person, year, employer, contract ID) level. Additionally, the auxiliary employment data only covers employment from 1987.

Appendix Z Additional descriptive statistics and definitions

In the main text, the sample was described through descriptive statistics for the unweighted targeted reform in 2014. Table 15 shows the same statistics after weighting. Tables 16 and 17 repeat these statistics for the universal reform in 2017, unweighted and weighted.

In both tables, benefit weeks in ALMPs refers to FTE benefit weeks for which the person collected benefits while participating in active labour market programs. Number of children is for underage children, as benefits can be increased if the person has dependent children; it is truncated at a maximum of 3 in the benefit data.

FTE benefit levels are reported over FTE months by dividing each payment by corresponding FTE weeks and multiplying by 4.3. These numbers are inflation-adjusted to 2019 levels. "Last job" refers to the last spell in employment during which (a) the primary employer did not change and (b) the person was paid a wage above the same minimum threshold as used for re-employment.

Labour market tightness was calculated from raw data on registered jobseekers and open vacancies from the Ministry of Economic Affairs and Employment. The raw inverse tightness is, for each spell, the average of open vacancies per jobseeker over the preceding 9 months matching the person's 4-digit profession and region. Persons with no observed profession or unknown profession were given the regional average. This grid was chosen after searching through all combinations of six regional classification levels, five profession classification levels and month lags between 4 and 24, and normalized and unnormalized levels and logs, and choosing the combination yielding the best prediction of unemployment duration in a training dataset according to the MSE.

Years of completed education were estimated by combining data on the person's finished educations prior to entitlement from the jobseeking register to mean durations of a given education.

Postal code area data was calculated by combining residence information in the benefit data to the public Paavo postal code area statistical database by Statistics Finland.

Entries in June for specific professions refer to the phenomenon of temporary summer unemployment for (mostly) teachers, discussed in appendix R.

Table 15: Means for the targeted reform after weighting.

Variable	Treatment, before	Treatment, after	Control, before	Control, after
Duration in full-time equivalent benefit weeks	27.2	28.8	28.6	30.4
Duration in calendar days	201	212	212	224
Spell continues past insurance entitlement	6.42%	10.36%	8.01%	8.19%
Spell continues past pre-reform entitlement	6.42%	6.73%	8.01%	8.19%
Age	24.9	24.9	29.5	29.5
Woman	49.2%	49.4%	61.4%	61.2%
Received unemployment benefits since 2005	71.4%	72.5%	79.6%	80.4%
Received UI since 2005	10.8%	14.9%	47.0%	45.2%
Received unemployment assistance (UA) since 2005	68.7%	68.6%	66.3%	69.2%
UI spells in last 1.9 years if any, N	1.5	1.5	1.5	1.5
UA spells in last 1.9 years if any, N	1.4	1.4	1.3	1.3
Wage basis for benefits (indexed to 2019), euros/mo	2232	2210	2390	2357
Number of children	0.31	0.25	0.64	0.58
Benefit weeks in ALMPs	4.57	4.94	5.02	5.37
Benefit weeks in partial unemployment	2.11	3.15	2.67	3.70
Total unemp. benefit payments, euros	8298	8597	9377	9777
Initial payment, euros/month	1204	1248	1465	1323
Average payment, euros/month	1201	1233	1338	1302
Prior employment, years	1.77	1.78	4.23	4.19
Nationality other than Finnish / residence permit	11.47%	11.13%	9.99%	11.63%
Duration of last job, years	0.74	0.74	1.03	1.04
Time from previous employment to spell, days	30.7	28.0	42.4	39.1
Contribution weeks towards the employment condition	58.1	58.2	73.0	72.9
Estimated years of completed education	12.9	13.0	14.2	14.2
Inverse of regional labor market tightness	0.36	0.35	0.47	0.41
Postal code area inv. pop. density (pct of national weighed avg.)	65.5%	68.3%	76.7%	81.8%
Postal code area unemp. rate (pct points over national weighed avg.)	1.18%	1.13%	1.12%	1.10%
Re-enters unemployment after spell	53.6%	52.7%	53.4%	53.2%
Entry in June after fixed-term contact in specific professions	6.26%	5.65%	10.31%	9.94%
Days from exit to next spell (if any)	153	146	164	155
Average payment by Kela, euros/month*	786	767	794	787
Last payment paid by fund, euros/month**		750		769
Last earnings-related payment, euros/month**		1228		1227
First flat-rate payment paid by fund, euros/month**		743		768
Initial payment paid by Kela, euros/month**		765		779
N	7495	12811	10680	16291

$*$ = among those who transfer to Kela after 100 benefit weeks

$**$ = among those directly affected by the reform (flat-rate benefits paid by funds after 80 weeks)

Table 16: Means for the universal reform.

Variable	Treatment, before	Treatment, after	Control, before	Control, after
Duration in full-time equivalent benefit weeks	31.4	26.7	27.9	26.5
Duration in calendar days	233	200	208	198
Spell continues past insurance entitlement	8.03%	8.41%	6.17%	5.65%
Spell continues past pre-reform entitlement	8.03%	5.91%	6.17%	5.65%
Age	35.7	35.8	36.1	36.4
Woman	59.5%	60.6%	48.1%	50.6%
Received unemployment benefits since 2005	79.6%	83.1%	84.3%	84.5%
Received UI since 2005	61.3%	65.4%	69.6%	69.8%
Received unemployment assistance (UA) since 2005	50.7%	56.6%	51.8%	55.9%
UI spells in last 1.9 years if any, N	1.6	1.7	1.8	1.8
UA spells in last 1.9 years if any, N	1.3	1.3	1.3	1.3
Wage basis for benefits (indexed to 2019), euros/mo	2589	2562	2700	2630
Number of children	0.83	0.80	0.87	0.86
Benefit weeks in ALMPs	4.84	3.68	3.56	3.82
Benefit weeks in partial unemployment	4.26	3.94	3.35	3.38
Total unemp. benefit payments, euros	10801	8743	9739	9065
Initial payment, euros/month	1627	1436	1447	1422
Average payment, euros/month	1439	1371	1431	1407
Prior employment, years	10.01	9.95	10.26	10.25
Nationality other than Finnish / residence permit	6.55%	6.48%	6.53%	7.36%
Duration of last job, years	1.9	1.7	1.7	1.7
Time from previous employment to spell, days	27.7	26.7	31.0	37.3
Contribution weeks towards the employment condition	80.0	74.9	67.8	67.3
Estimated years of completed education	14.9	15.1	14.3	14.5
Inverse of regional labor market tightness	0.37	0.41	0.51	0.57
Postal code area inv. pop. density (pct of national weighed avg.)	87.1%	86.9%	77.6%	74.8%
Postal code area unemp. rate (pct points over national weighed avg.)	1.06%	1.05%	1.06%	1.07%
Average payment by Kela, euros/month$*$	782	773	773	778
Last payment paid by fund, euros/month$*$	1381	1347	1400	1332
N	5606	5398	4392	3997

$*$ = among those who transfer to Kela after 100 benefit weeks

Table 17: Means for the universal reform after weighting.

Variable	Treatment, before	Treatment, after	Control, before	Control, after
Duration in full-time equivalent benefit weeks	31.5	26.7	28.2	26.6
Duration in calendar days	234	200	212	200
Spell continues past insurance entitlement	8.19%	8.41%	6.76%	5.76%
Spell continues past pre-reform entitlement	8.19%	5.91%	6.76%	5.76%
Age	35.8	35.8	35.8	35.8
Woman	61.0%	60.6%	56.4%	56.1%
Received unemployment benefits since 2005	83.3%	83.1%	84.0%	83.2%
Received UI since 2005	65.1%	65.4%	67.4%	67.1%
Received unemployment assistance (UA) since 2005	53.1%	56.6%	53.6%	56.5%
UI spells in last 1.9 years if any, N	1.6	1.7	1.9	1.9
UA spells in last 1.9 years if any, N	1.3	1.3	1.3	1.4
Wage basis for benefits (indexed to 2019), euros/mo	2538	2562	2602	2607
Number of children	0.80	0.80	0.81	0.81
Benefit weeks in ALMPs	4.79	3.68	3.66	3.87
Benefit weeks in partial unemployment	4.32	3.94	3.79	3.71
Total unemp. benefit payments, euros	10675	8743	9611	9019
Initial payment, euros/month	1584	1436	1399	1404
Average payment, euros/month	1415	1371	1381	1387
Prior employment, years	9.99	9.95	9.88	9.88
Nationality other than Finnish / residence permit	6.43%	6.48%	6.38%	6.22%
Duration of last job, years	1.8	1.7	1.7	1.7
Time from previous employment to spell, days	26.0	26.7	33.8	38.7
Contribution weeks towards the employment condition	77.5	74.9	68.6	68.8
Estimated years of completed education	14.9	15.1	14.8	14.9
Inverse of regional labor market tightness	0.41	0.41	0.41	0.41
Postal code area inv. pop. density (pct of national weighed avg.)	87.2%	86.9%	84.1%	79.2%
Postal code area unemp. rate (pct points over national weighed avg.)	1.06%	1.05%	1.07%	1.07%
Average payment by Kela, euros/month∗	778	773	770	776
Last payment paid by fund, euros/month∗	1357	1347	1374	1329
N	5606	5398	4392	3997

∗ = among those who transfer to Kela after 100 benefit weeks

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