Effects of a major and sudden relaxation of the benefit sanction regime on labour market outcomes of welfare recipients

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Abstract

We analyse the effect of a major reform of benefit sanction rules in the German welfare system. Following a ruling of the Federal Constitutional Court in November 2019, the severity of some benefit sanctions was lowered and the number of new sanctions issued substantially and suddenly dropped. We analyse the effects of this natural experiment on short-run labour market outcomes of welfare recipients before the onset of the COVID-19 pandemic in March 2020. We use administrative data and analyse average treatment effects conditional on survival up to the reform date. First and preliminary findings suggest a negative effect of the reform on the short-run conditional employment transition probability of welfare recipients.

Keywords: sanctions, welfare, unemployment, monitoring, job search.

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1 Introduction

Unemployment insurance (UI) and welfare systems are important safety nets. They provide jobseekers with financial support during unemployment and aid them with re-entering into the labour market. In return, jobseekers usually have to comply with a range of legal requirements, for example attending meetings with a caseworker or accepting job offers. Caseworkers monitor compliance with these requirements. If jobseekers refuse compliance, caseworkers may issue a sanction that reduces the unemployment benefit for a limited duration. Monitoring and sanctions are thus important elements of UI and welfare systems: they aim to ensure jobseekers' compliance with the legal requirements and thereby to combat well-known moral hazard problems of unemployment benefits.

In this study, we analyse the effect of a major reform of benefit sanction rules in the German welfare system. On 5 November 2019, the Federal Constitutional Court (FCC) ruled that certain sanction regulations are not in line with the German Constitution. The FCC ruling had major and sudden consequences for the application for benefit sanctions: first, the severity of sanctions decreased. Before the FCC ruling, certain sanctions could lead to a complete withdrawal of welfare benefits, including costs for accommodation and heating. After the FCC ruling, the maximum benefit reduction was limited to 30 percent of the basic cash benefit that welfare recipients receive for day-to-day expenditures and costs for accommodation and heating could not be lowered. This effectively reduced options of caseworkers to sanction repeated or follow-up infringements by more than 30 percent. Further, caseworkers received additional options to cushion severe consequences of sanctions for welfare recipients. Second, the risk to be sanctioned decreased. As seen in figure 1, there was a large and sudden drop in the number of new sanctions issued following the FCC ruling. This results partially from the legal changes and partially from a higher reluctance of caseworkers to issue sanctions, as we will explain in more detail below.

[Figure 1 about here]

Our aim is to study the effect of this reform on a range of welfare recipients' labour market outcomes. Our main focus lies on the duration until welfare recipients enter employment that is subject to social security contributions. We use administrative data that allows us to observe with daily precision entry into welfare receipt and uptake of employment. We focus on the duration of welfare receipt while being non-employed and apply an approach to study effects of a policy discontinuity on duration outcomes proposed by van den Berg et al. (2020). A challenge for the analysis is the onset of the COVID-19 pandemic shortly after the FCC ruling around March 2020. As seen in figure 1, the number of new sanctions further dropped thereafter. In consequence, we study short-run effects in the time window between November 2019 and March 2020. In the analysis, we censor durations by the end of March 2020.

In the analysis, we consider the effect of the reform for cohorts of welfare recipients entering welfare receipt in different calendar months in 2019. An important question is the choice of a suitable control group. Based on the distribution of observables at inflow and survival and hazard rates up to the reform date, we choose entrants in 2018 for the control group. To avoid issues resulting from seasonality in outcomes like uptake of jobs, we use for each calendar month cohort of the treatment group entering in 2019 the corresponding calendar month cohort in the control group in 2018. We estimate the effect conditioning on survival until the reform date in the treatment group, i.e. average treatment effect on the treated survivors (ATTS).

First and preliminary results show that the reform decreased the treatment group's transition probability to employment between November 2019 and end of March 2020. The ATTS lies between -6 to -9 percentage points across calendar month cohorts. Compared to the transition probability of the control group, this implies a notable decrease between 15 to 24 per cent. While the control group differs in terms of observables (and potentially unobservables) from the treatment group, a robustness check comparing the control group to entry cohorts in 2017 as a placebo treatment does mostly not show significant differences in transition probabilities. This raises our confidence that the difference we observe between treatment and control group reflect the effect of the reform.

Our study contributes to research on the effect of sanctions. From the perspective of job search theory, it has long been acknowledged that sanctions have two types of effects: the *ex-post* effect of imposing a sanction and the *ex-ante* effect of system with sanctions compared to a system without sanctions (Abbring et al., 2005). While the former affects those jobseekers who are sanctioned, the latter potentially affects a much larger group of jobseekers. Several studies show that sanctions ex post increase the exit hazard from unemployment or the exit hazard to employment.[1] Fewer studies show that sanctions also ex ante have such effects.[2] Only few studies analyse policy reforms of benefit sanction rules. The advantage of such analyses is that, first, policy reforms can serve as a natural experiment with plausibly exogenous variation in sanctions, which is often a challenge in empirical analyses relying on observational data. Second, they allow to study the total

[1] van den Berg et al. (2004), van der Klaauw and van Ours (2013), and Busk (2016) provide evidence for positive sanction effects on the transition rate to work among unemployed welfare recipients. For UI recipients, see, for example, Abbring et al. (2005), Lalive et al. (2005), Svarer (2011), van den Berg and Vikström (2014), Arni et al. (2013), and van den Berg et al. (2022).

[2] See Lalive et al. (2005), Arni et al. (2013), Arni et al. (2022), and Wolf (2024)

effect of sanctions, including ex-post and ex-ante effects. Two studies use policy reforms to study ex-ante effects. Arni and Schiprowski (2016) analyse a reform that increased the enforcement strictness for some sanctions in the Swiss unemployment insurance system. Amongst others, they find the reform significantly increased the exit rate to employment through individual job search effort of the unemployed. van den Berg and Vikström (2014) in their analysis of ex-post sanction effects in Sweden shed some light on the consequences of a reform that decreased sanction strictness, but did not substantially affect the number of sanctions issued. They find no difference in the transition rate to employment before and after the reform, which suggests that ex-ante effects are weak in the setting they study. Lombardi (2019) analyses the effect of two reforms in Sweden that increased sanction strictness. He finds that the reforms increased employment uptake of jobseekers. Walsh (2023) analyses the effects of a reform in the United Kingdom that increased the strictness of sanctions and the number of sanctions issued. He finds that the exit rate to employment significantly increased in districts with a strong increase in the sanction rate in response to the reform compared to districts that did not increase the sanction rate strongly. Both studies show in a decomposition exercise, that the ex-ante effect is quantitatively more important than the ex-post effect. Our study adds to this research by analysing a major and sudden benefit sanction reform in the German welfare system.[3]

2 Institutional setting: Changes of the sanction rules due to a judgment of the Federal Constitutional Court

In this section we are concerned with welfare benefit rules and in particular rules on benefit sanction that considerably changed due to a judgment of the German Federal Constitutional Court of 5th November 2019. So we discuss the rules of basic income support that were in force just prior and after the judgment; a more recent comprehensive welfare benefit reform that introduced the new "Citizen's Income" is not subject of this section.

In Germany the flat rate means-tested welfare benefit "unemployment benefit II" that was in place in period under consideration, aimed at guaranteeing a minimum income. It was available for households whose other income sources were insufficient to reach that threshold, which depended mostly on the exact composition of the household and

[3]Weber (2024) analyses the same reform in the German welfare system together with two subsequent policy reforms. He finds that job finding rates of welfare recipients who are entitled to benefits are lower after these reforms compared to a control group of welfare recipients who are not entitled to benefits. The analysis includes the periods of the COVID-19 pandemic, which may have differentially affected the treatment and control group. In contrast, our analysis focuses on the short period after the reform where the COVID-19 pandemic has not yet begun.

their costs of accommodation and heating. The latter can considerably vary over different regions so that there was no single threshold for the total welfare benefit in Germany. However, the base benefit level that was supposed to cover daily expenditures was fixed nationwide: An adult living alone received 424 Euro per month in 2019. In a couple household it was 382 Euro for each adult partner. For other people in particular children this base benefit level was lower.

The benefit system was characterised by rights and duties. On the one hand job centres were supposed to provide support for benefit recipients like job search assistance and various active labour market programmes. The aim was to help them to achieve earnings so that they could reduce or end their dependence on the welfare benefit. On the other hand the benefit recipients were supposed to provide efforts in order to achieve sufficient earnings. In turn they needed to attend meetings with their caseworker or come to appointments for a medical examination, to actively search for jobs, accept suitable job offers and were not supposed to voluntarily quit a job and become unemployed. Moreover, they had to participate in active labour market programmes and to take actions that were specified in their integration contract and that should help them to leave benefit receipt or at least to achieve earnings so that their unemployment benefit could be reduced. When these obligations were not met, benefit sanctions should be imposed.

All sanctions were imposed for a duration of three months. Let us first turn the sanction rules that were in force prior to the judgment of the German Federal Constitutional Court of 5th November 2019. The sanctions for missing an appointment led to a reduction of the welfare benefit of 10 per cent of the base benefit level, so 42,40 Euro per month for a single in 2019. This rule applied to all unemployment benefit II recipients regardless of their age. In contrast to sanctions for missing an appointment, for other infringements the sanction amount was higher and depended on whether or not a benefit recipient was already at least 25 years old and whether it was a first or a repeated infringement. For those aged at least 25 years a first such non-compliance like refusing a suitable job offer led to a cut of the welfare benefit by 30 per cent of the base benefit level. For a first repeated non-compliance within one year it was 60 per cent and for any further repeated non-compliance within one year no welfare benefit was paid, so also the benefit for costs of accommodation and heating was not paid. For unemployment benefit II recipients aged less than 25 years the sanction rules were stricter. For a first non-compliance other than missing an appointment the basic benefit was not paid. Any repeated infringement within a year led to no welfare benefit payment at all. However, past judgments of social courts in 2019 already implied that in multi-person households withdrawing the benefit to cover costs of accommodation and heating was no longer applied. Theoretically, this benefit could be withdrawn from a sanctioned person but it was then assigned to the other household members in a multi-person household.

The sanction rules prior to the judgment of the German Federal Constitutional Court of November 2019 allowed welfare recipient once they were informed by mail about a non-compliance to provide a good reason for the infringement. If no such good reason was provided, the sanction was supposed to be imposed according to the benefit rules. When the sanction amounted to more than 30 per cent of the basic cash benefit, case workers could provide non-cash benefits like food stamps to the sanctioned individual and had to do so for families with children. Various sanctions of missing an appointment could be in place at the same time having a cumulated effect on the benefit payment. Hence in a month, in which four such sanctions were in place, the reduction of the welfare benefit was 40 per cent of the basic cash benefit. If various sanctions due to other types of non-compliances were in place at the same time instead, the sanction amounts were not added up: E.g., if a sanction of 30 per cent and of 60 per cent of the basic cash benefit were in force at the same time for a welfare recipient only the 60 per cent reduction was applied; though sanctions due to missing appointment that were in force at the same time led to additional reductions of the benefit payment. Under certain circumstances the sanction period of three months could be reduced to six weeks for welfare recipients aged younger than 25 years. One potential reason for this could be that a person was not fully aware of the consequences of the non-compliance.

According to two articles of the German Basic Law the state has to protect human dignity and this has to be reflected by social policy and hence by the unemployment benefit II system that guarantees citizens in Germany a minimum income, provided that they cannot achieve this income by other means. In a court case that was concerned with a sanction of 60 per cent of the basic cash benefit due to a repeated infringement within one year the German Federal Constitutional Court decided in November 2019 that the sanction rules that were in place were partly not in line with the just mentioned rule of the German Basic Law. As long as a reform would not address this, transitional rules for benefit sanctions were set by the Federal Constitutional Court for welfare recipients aged at least 25 years. The German Federal Employment Agency also applied them to those unemployment benefit II recipients aged younger than 25 years (German Federal Employment Agency, 2019). So from December 2019 onwards the following changes applied:

- Benefit sanctions for other non-compliances than missing an appointment were always set to 30 per cent of the basic cash benefit, irrespective of whether it was a first or a repeated non-compliance within one year.

- If a welfare recipient's benefit was reduced due to various sanctions in the same month, then the total reduction of the benefit could not be higher than 30 per cent of the basic cash benefit.

- The duration of benefit sanctions was still three months. However, if due to some non-compliance a welfare recipient was sanctioned, but during the sanction period started to comply or convincingly and seriously agreed to comply in the future, the benefit sanction had to end by the end of the month, so that it did not have to last for three months.

- Finally, if a 30 per cent benefit sanction implied an extreme hardship for welfare recipients, case workers were allowed not to sanction for certain types of non-compliances like refusing a job offer or refusing to participate in an active labour market programme.

Hence, in December 2019 benefit sanctions became much less harsh compared with the months and years before. Also the frequency of benefit sanctions became lower, which at least partly might be a result of the new rules on agreeing to comply in the future and the possibility that case workers could avoid a sanctioning an infringement because the sanction would imply an extreme hardship for a welfare recipient.

The FCC ruling influenced the daily work of caseworkers beyond the direct consequences through the legal changes described above. Notably, not only the number of strong sanctions issued substantially dropped, but also the number of mild sanctions, as seen in figure 1. This latter drop is unlikely a result of the FCC ruling alone. Rather, caseworkers seem to have become fundamentally more reluctant to issue sanctions after the FCC ruling. A likely explanations is that, after the FCC ruling, there was a transition period where caseworkers needed to adapt their daily work routines. Interviews with caseworkers conducted by Bernhard et al. (2023) suggest that, particularly for caseworkers for whom sanctions were an important instrument in their daily routines, the ruling meant a fundamental shock to said routines. This may have been further exacerbated by the insecurity that some of the legal changes introduced. For example, 'extreme hardship' was only a vaguely defined legal term (Bernhard et al., 2023).

In summary, for welfare recipients the FCC ruling implied two major and sudden changes: first, the severity of sanctions decreased, as the maximum benefit reduction was limited and caseworkers received more options to cushion severe consequences of sanctions. Second, the sanction risk decreased, resulting in part directly from the legal changes and in part indirectly from a higher reluctance of caseworkers in issuing sanctions.

3 Data

We use administrative data from the Unemployment Benefit II Recipient History, which contains detailed information on all people living in households that receive welfare benefits. We link this to information from employer records in the Integrated Employment Biographies.

Our data contains a 50 per cent random sample of welfare recipients who start a period of welfare receipt between 1 January 2015 and 4 November 2019. Each period of welfare receipt of a person may contain multiple periods during which the person is non-employed. The latter is defined as not being in contributory employment or participating in certain active labour market policy measures. We refer to the spell of a person as the period during which the person receives welfare benefits while being non-employed.

Our sample for analysis focuses on those welfare recipients who are capable of working and aged between 18 and 55 years at the beginning of the welfare receipt period. As we explain in more detail below, with our methodological approach we compare entry cohorts of welfare recipients across different years. Particularly in the years 2015 and 2016, entry cohorts substantially differ in terms of observable characteristics from the 2019 entry cohort. This is probably due to the inflow of many refugees to Germany in this period. We therefore exclude from the analysis welfare recipients of nationality from one of the eight non-European countries with the most requests for political asylum in Germany in the period between 2012 and 2015.[4] Further, we only keep welfare recipients who are registered as jobseekers at the beginning of their spell. Finally, we keep only spells that start on the same day as the welfare receipt period. This means that we only retain one spell per person and welfare receipt period.

The main outcome is the transition rate from welfare and non-employment to employment that is subject to social security contributions (without vocational training). We define such an employment transition as uptake of employment within 93 days after the spell ends. In our main analysis, we censor the spell duration at the latest on 31 March 2020 for our treatment group, which is around the time when the COVID-19 pandemic in Germany began.

We consider further short-run outcomes during the welfare and non-employment spell: first, the transition to the first (mild or strong) sanction. Second, the participation in the first active labour market policy measure. As before, we censor the spell duration latest on 31 March 2020 for our treatment group.

[4]These countries are Afghanistan, Eritrea, Iraq, Iran, Nigeria, Pakistan, Somalia, and Syria.

4 Method

Our interest lies in the effect of the reform on several labour market outcomes of welfare recipients. We are particularly interested in their (short-run) probability to exit welfare receipt for contributory employment. Later, we will also consider (short-run) effects on a range of intermediate outcomes, such as participation in active labour market policy measures.

To estimate such effects, we cannot compare cohorts of welfare recipients with inflow after the reform with welfare recipients with inflow before the reform (potentially censored at the moment of the reform). As noted by van den Berg et al. (2020), if the treatment has an effect on the exit rate to employment, the estimate of the treatment effect at some elapsed duration $t > 0$ not only reflects the treatment effect itself, but also the effect of dynamic selection.

They therefore suggest an alternative approach, illustrated in figure 2. The main idea is to include only spells that begin before the reform and compare those surviving up to the reform date τ^* with earlier cohorts conditional on survival up to the same elapsed duration t_0. Under a version of the conditional independence assumption and no anticipation discussed below, van den Berg et al. (2020) show that the average treatment effect on the treated survivors (ATTS) is identified.

4.1 Details of the approach

Consider a population of welfare recipients flowing into the state of interest, which is welfare receipt while not being employed. We refer to this as the spell and t as the duration of the spell. We are interested in the effect of a binary treatment that starts at some time s during the spell. To each treatment s corresponds a random variable $T(s) \geq 0$, the potential outcome duration for treatment s. Our interest lies in contrasting the distribution of the potential outcome duration treated at s' and s. In practice, we do not observe $T(s)$, but only the elapsed duration S at which the welfare recipient is exposed to the reform as well as the observed outcome $T = T(S)$.

Denote by t_0 the process time at which the reform is implemented for an individual in the treatment group. τ denotes calendar time, τ_0 the calendar time of inflow of an individual, and τ^* the calendar time moment of the reform, i.e. 5 November 2019. Only cohorts with spell begin at calendar time $\tau_0 < \tau^*$ are included in the estimation. This leads to $S = \tau^* - \tau_0$. Denote by X observed characteristics and by V unobserved characteristics.

van den Berg et al. (2020) show that if two assumptions are satisfied, the ATTS at t_0 on the hazard rate at t_0 or on the conditional survival probability in some interval after t_0 is identified.

Assumption 1 (Assignment) $S \perp\!\!\!\perp \{T(s)\}|(X,V)$ and $S \perp\!\!\!\perp V|X$

Assumption 1 is a version of the conditional independence assumption (CIA). The first part means that conditional on observed and unobserved characteristics, treatment is independent of the potential outcomes. This contrasts with the traditional CIA, that only conditions on observed characteristics. This hence allows for endogenous selection into the treatment. The second part requires that different entry cohorts have the same distribution of unobservables given observables. We will discuss the validity of this assumption in the empirical part.

Assumption 2 (No anticipation) For all $s \epsilon (0,\infty)$ and for all $t \leq s$ and all X, V, $\Theta_{T(s)}(t|X,V) = \Theta_{T(\infty)}(t|X,V)$

where $\Theta_{T(s)}(t|X,V)$ denotes the cumulative hazard for treatment at $T(s)$ and similarly for the no-treatment case ($T(\infty)$). This means that before the treatment takes place, the welfare recipient's behaviour does not depend on the time remaining until the reform.

Taken together, the main idea is that if different entry cohorts are comparable in terms of observables and unobservables and there is no anticipation of the reform, then the 'weeding out' due to dynamic selection is equal in both treatment and control group up to the moment of the reform. In consequence, a comparison of outcomes of those surviving until the same elapsed process duration t_0 gives an unbiased estimate of the ATTS.

4.2 Empirical application

The reform we study was introduced with the ruling of the FCC on $\tau^* = 5$ November 2019. We define as the treatment group different monthly inflow cohorts entering welfare and non-employment 2019 before τ. We consider inflow cohorts in January, April, July, August, September, and October. This allows us to study the effect of the reform at different elapsed durations t_0. We study outcomes for the treatment group until 31 March 2020, the time around the COVID-19 pandemic began, and censor durations latest at this date. This corresponds to around 5 complete calendar months for the outcome window (including November 2019).

The crucial question is which entry cohort to select as the control group. To account for seasonal differences in transition rates to employment, we compare the treatment group to entry cohorts in the same month of each year 2015 to 2018. Equivalently to the treatment group, we focus on those surviving until their respective t_0 (5 November of the respective year), consider employment transitions in an outcome window until 31 March of the following respective year, and censor them thereafter.

5 Results

5.1 Selecting the control group

To select the control group, we compare observed covariates at inflow. In addition, we compare estimates of the survivor function and of the hazard rate until t_0. This allows us to gain a sense which control group is most similar to the treatment group not only in terms of observables, but also in terms of the dynamic selection on unobservables up to t_0.

Tables A1 to A6 in the appendix compares means of observed covariates for the treated 2019 cohorts to potential control cohorts who enter in the same calendar months in different years. Overall, those entering welfare and non-employment in 2018 seem to be most comparable to the entries in 2019. For example, the share of those with vocational degree is comparatively higher for entrants in 2015, 2016, and - to a lesser degree - in 2017, whereas the shares of the 2018 cohorts are more similar. Nevertheless, there remain significant differences between the 2018 and 2019 cohorts. Notably, the 2018 cohorts tend to have spend fewer days in employment in periods before welfare receipt and non-employment.

Figure A2 in the appendix compares estimates of the survivor function for each entry cohort. Overall, the left figure of each panel shows that each cohort's survivor function follows similar patterns and does not differ substantially from one another. The right figure of each panel compares the 2018 and 2019 cohorts, underlining similar estimates of the survivor functions for these groups. Figure A5 in the appendix largely confirms these results for estimates of monthly hazard rates. With the sole exception of the January 2016 cohort, all hazard rates follow largely the same pattern. As seen in the right figure in each panel, hazard rates between the 2018 and 2019 cohort are usually not statistically significant from one another in the time period up to the reform date.

Overall, this suggest the following conclusions: first, while the composition in terms of observable characteristics of inflow cohorts differs across years, these differences do not seem to lead to substantially different job finding rates. Second, the 2018 cohorts seem to be comparatively most similar to the 2019 cohorts. In the following, we will thus use the 2018 cohorts as the control group. The 2017 cohorts will be used as placebo treatment group.

5.2 Non-parametric estimation of effects on the employment transition probability

Employment

To analyse how the ruling of the FCC affects the short-run employment chances, we compare the 2019 monthly inflow cohorts as treatment group to the respective 2018 monthly inflow cohort as a control group. We focus on differences in the transition probability after t_0 conditional on survival up to t_0. We do not control for differences in observed variables, but subsume them into the unobservable variables (van den Berg et al., 2020).

Figure 3 panel (a) shows life table estimates of the conditional employment transition probability for the treatment and control group. As we condition on survival up to the reform date, the transition probabilities increase as we move closer to the reform date. This reflects dynamic selection, with cohorts that enter earlier in the year being negatively selected in terms of their employment probability in November of the year, when the reform took place for the treatment group.

[Figure 3 about here]

The estimates in figure 3 panel (a) show that the treated cohorts have a significantly lower transition probability to employment compared to the control cohorts. Effects are substantial: in absolute terms, the transition probability decreases between 6 p.p. for the April cohort to 9 p.p. for the September cohort. In relative terms, this means a drop of the transition probability between 15 per cent for the July to 24 per cent for the January cohort.

Control and treatment group are however not perfectly comparable regarding observed covariates and therefore likely also not regarding unobservables. To gain a sense how important these differences are, we compare the 2018 entrants as control group to the 2017 entrants as a placebo treatment group.

Figure 3 panel (b) shows the results of this placebo check. In contrast to panel (a), we find largely no statistically significant differences between placebo treatment and control cohorts. Only the April 2017 entrants have a significantly higher transition probability compared to the April 2018 entrants. Largely, this placebo check raises our confidence, that the effect estimate shown in panel (a) are not merely due to observable and unobservable selection, but at least partially reflects the effect of the reform.

Sanctions

Next, we estimate reform effects on the conditional transition probability to the first sanction during the welfare and non-employment spell. We again condition on survivors at the reform date and censor durations before the beginning of the COVID-19 pandemic.

Figure 5 displays the results. Panel (a) shows results for the transition probability to the first strong sanction. The regulation of strong sanctions were the main focus of the reform. We see that the transition probability to a first strong sanction is more than halved for each entry cohort. Panel (b) shows results for the transition probability to the first mild sanction. Despite these sanctions not being the main focus of the reform, we also see a large drop in the transition probability with all treated cohorts, except the January cohorts. Placebo checks (not shown) suggest that these differences are due to the reform.

[Figure 5 about here]

These results show, as expected, that the reform strongly decreased welfare recipients' probability to receive a strong sanction, but also to receive a mild sanction, mirroring the drop in new sanctions shown in figure 1. This drop may partially explain the negative reform effect on employment transitions.

5.3 Semi-parametric estimation of effects on the transition rate to employment

In this section, we estimate treatment effects on the transition rate to employment relying on semi-parametric Cox proportional hazards models. Such models leave the baseline transition rate unspecified. Instead of conditioning on survival until the reform date, we include a time-varying indicator that is set to one after the reform is implemented. Again, we censor duration outcomes before the beginning of the COVID-19 pandemic. We do not account for unobserved heterogeneity in these models.

Note that we focus on the daily transition rate to employment rather than the transition probability in a five month interval after the reform. As noted by van den Berg et al. (2020), after the moment the reform had an effect on this transition rate, estimates reflect not only the effect of the reform itself but also dynamic selection.

Table 2 shows the results. As seen in column (1), for all inflow cohorts analyzed, we find a statistically significant and substantial decrease in the transition rate to employment after the reform was implemented. Coefficient estimates are in the ballpark of -0.5. Including control variables in column (2) hardly changes these estimates. This raises our confidence that differences between the treatment and the control group are not merely due to differences in composition with regard to observed covariates.

[Table 2 about here]

Table 3 repeats this exercise with the entrants into welfare in 2017 as the placebo treatment group. Largely, we do not find statistically significant and strong differences. An exception is, as before, the April cohort. We will have to look in more detail into what explains the differences for this cohort.

[Table 3 about here]

6 Conclusion

We analyse the effect of a major and sudden change in the benefit sanction rules in the German welfare system on welfare recipient's short-run labour market outcomes before the onset of the COVID-19 pandemic. We apply an approach proposed by van den Berg et al. (2020) to estimate the average treatment effect on the treated survivors at the date of the reform. We focus on a cohort of entrants into welfare in different calendar months in 2019 as the treatment group and compare them to cohorts of entrants into welfare in different calendar months in 2018 as the control group. First and preliminary findings show that the conditional employment transition probability of the treatment group after the reform and before the onset of the COVID-19 pandemic lies significantly lower compared to the control group. This result suggests that the reform decreased the short-run employment chances of welfare recipients.

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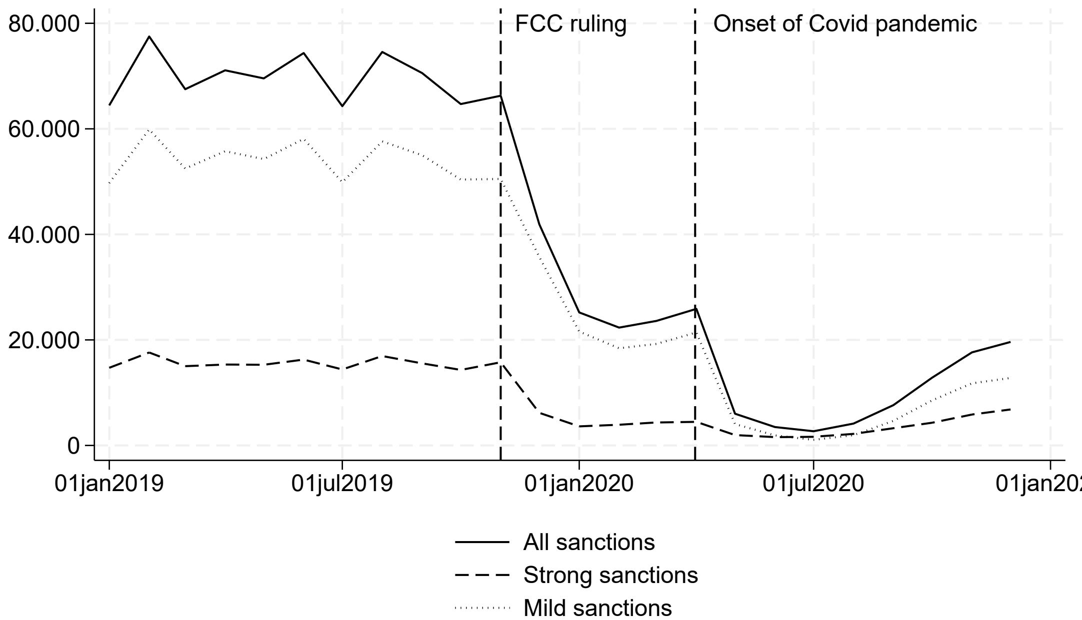
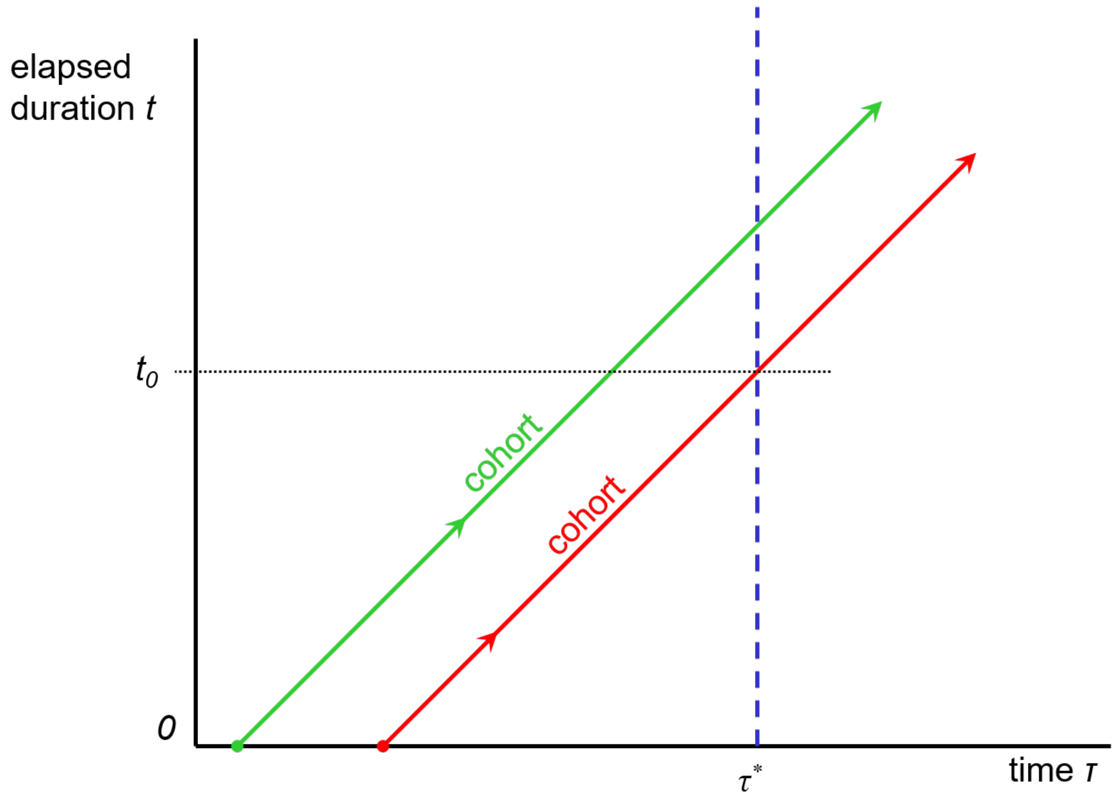
Tables and figures

Figure 1: Number of new sanctions per month



Notes: the left vertical line marks the date of the Federal Constitutional Court (FCC) ruling of 5 November of 2019. The right vertical line marks the onset of the COVID-19 pandemic mid to end of March 2020.
Source: Statistics Department of the Federal Employment Agency, own calculations.

Figure 2: Illustration of the methodological approach



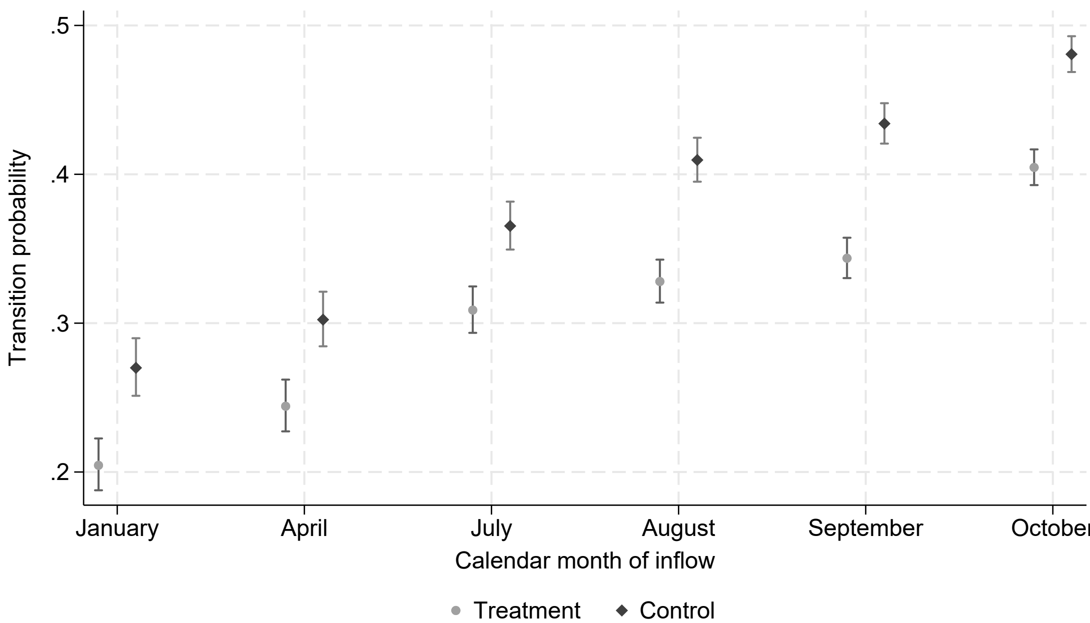
Source: van den Berg et al. (2020).

Table 1: Number of subjects by entry year and month

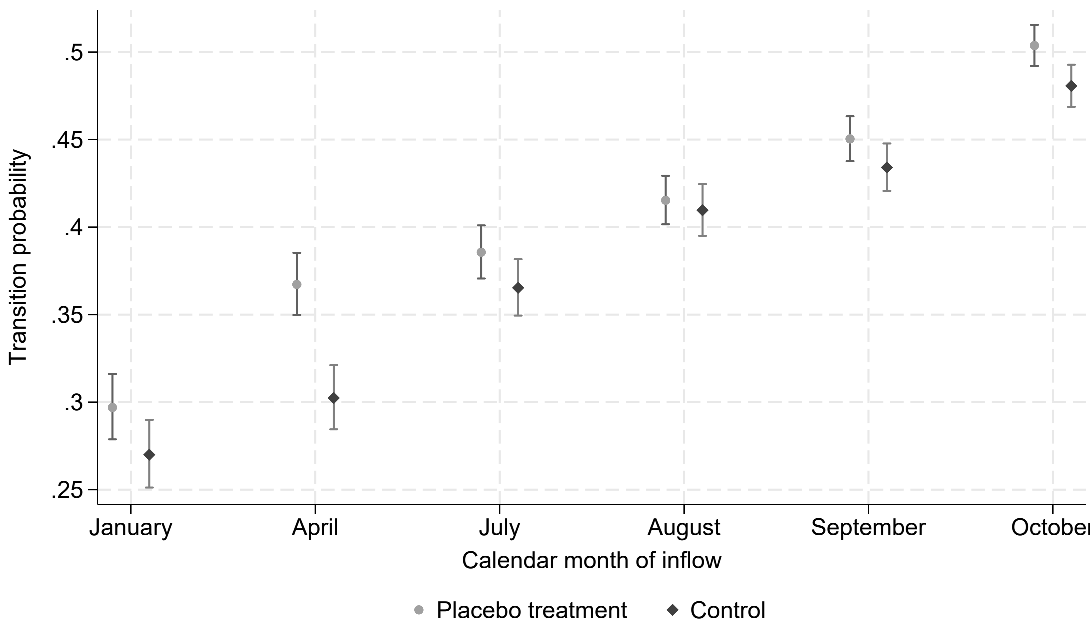
	2015	2016	2017	2018	2019	Total
January	20,536	17,951	16,465	14,117	14,296	83,365
April	16,408	15,472	13,524	11,966	11,611	68,981
July	15,467	14,567	12,717	11,209	10,911	64,871
August	17,322	15,852	13,651	11,799	11,554	70,178
September	14,987	14,069	12,543	11,290	10,946	63,835
October	16,722	14,323	12,455	11,965	12,043	67,508
Total	101,442	92,234	81,355	72,346	71,361	418,738

Source: administrative data of the Statistics Department of the Federal Employment Agency, own calculations.

Figure 3: Lifetable estimates of the conditional employment transition probability of the treatment and control group



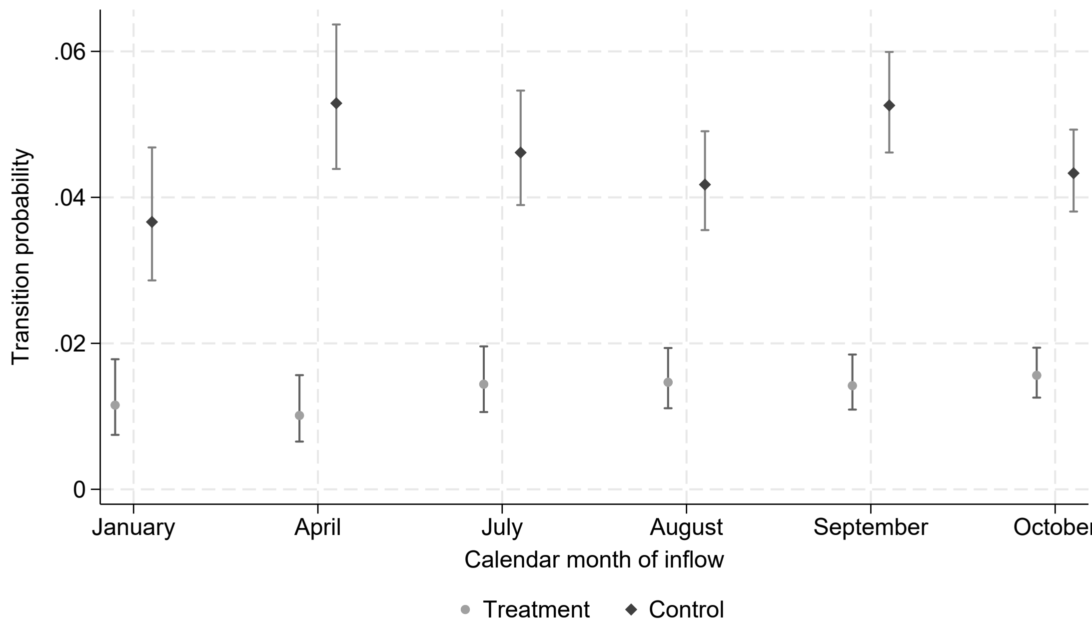
(a) Treatment effect



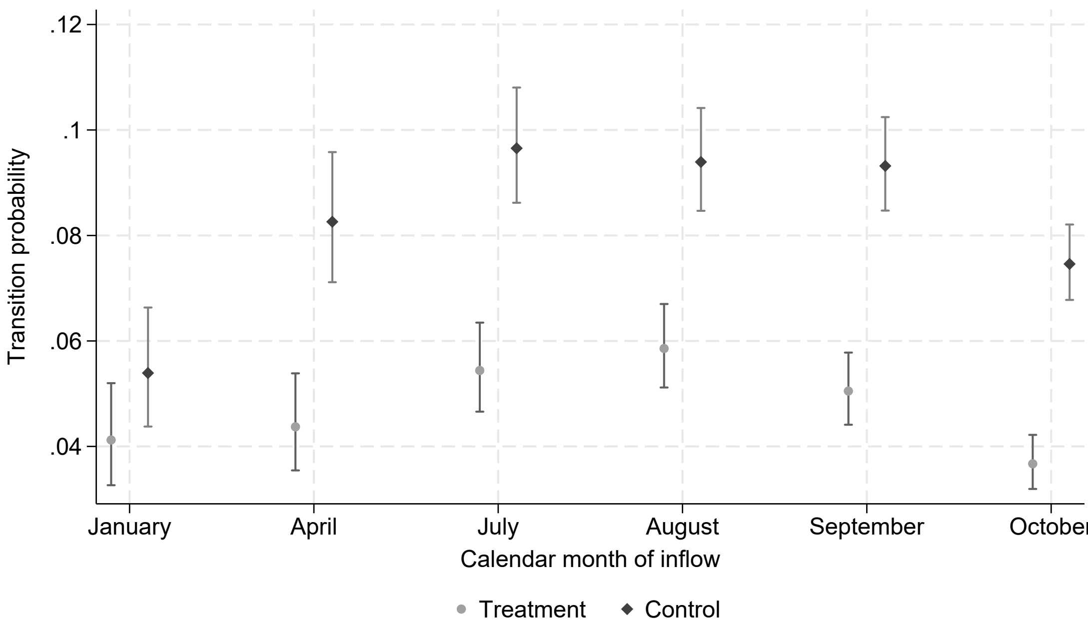
(b) Placebo check

Notes: panel (a) shows 95 percent confidence intervals and estimates of the conditional employment transition probability between 5 November 2019 and 31 March 2020 of the treatment group entering welfare receipt in 2019 and surviving up to t_0. For the control group entering welfare receipt in 2018 and surviving up to t_0 it shows conditional employment transition probability between 5 November 2018 and 31 March 2019. Panel (b) shows analogous estimates for the placebo treatment entering welfare receipt in 2017 and the control group entering welfare receipt in 2018. The elapsed durations in days from the first day of the inflow month are 308 for January, 218 for April, 127 for July, 96 for August, 65 for September, and 35 for October.
Source: administrative data of the Statistics Department of the Federal Employment Agency, own calculations.

Figure 5: Lifetable estimates of the conditional first sanction transition probability of the treatment and control group



(a) Strong sanction



(b) Mild sanction

Notes: the panels show 95 percent confidence intervals and estimates of the conditional first sanction transition probability between 5 November 2019 and 31 March 2020 of the treatment group entering welfare receipt in 2019 and surviving up to t_0. For the control group entering welfare receipt in 2018 and surviving up to t_0 it shows conditional first sanction transition probability between 5 November 2018 and 31 March 2019. Panel (a) shows results for the first strong sanction, panel (b) for the first mild sanction. The elapsed durations in days from the first day of the inflow month are 308 for January, 218 for April, 127 for July, 96 for August, 65 for September, and 35 for October.
Source: administrative data of the Statistics Department of the Federal Employment Agency, own calculations.

Table 2: Treatment effects on the transition rate to employment

	(1) No controls	(2) Controls
Panel a: January		
Treatment	-0.560***	-0.623***
	(0.0690)	(0.0691)
Subjects	28,413	28,413
Panel b: April		
Treatment	-0.499***	-0.486***
	(0.0598)	(0.0598)
Subjects	23,577	23,577
Panel c: July		
Treatment	-0.439***	-0.425***
	(0.0446)	(0.0446)
Subjects	22,120	22,120
Panel d: August		
Treatment	-0.491***	-0.492***
	(0.0386)	(0.0386)
Subjects	23,353	23,353
Panel e: September		
Treatment	-0.516***	-0.524***
	(0.0346)	(0.0347)
Subjects	22,236	22,236
Panel f: October		
Treatment	-0.457***	-0.450***
	(0.0281)	(0.0281)
Subjects	24,008	24,008

Notes: coefficients and standard error estimates of the treatment effect from a Cox proportional hazards model. The dependent variable is the transition rate to employment. Durations are censored before the beginning of the COVID-19 pandemic. Treatment is a time-varying indicator set to one after the reform. The panels show the results for different inflow cohorts. The treatment group consists of different cohorts with inflow in 2019, the control group consists of different cohorts with inflow in 2018. Column (1) shows estimates without control variables. Column (2) shows estimates controlling for gender, age, vocational and school education, German nationality, children in different age groups, East Germany, communal jobcenter, and days in contributory employment in periods before inflow into welfare. $*p < 0.1, **p < 0.05, ***p < 0.01$

Source: administrative data of the Statistics Department of the Federal Employment Agency, own calculations.

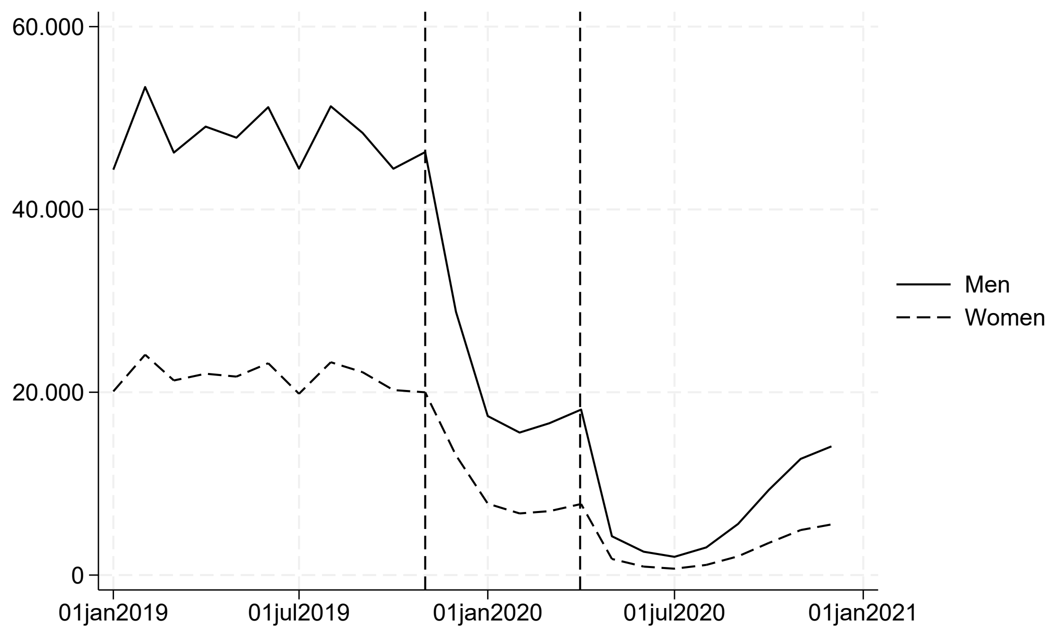
Table 3: Placebo treatment effects on the transition rate to employment

	(1) No controls	(2) Controls
Panel a: January		
Placebo treatment	0.0979	0.0824
	(0.0574)	(0.0574)
Subjects	30,582	30,582
Panel b: April		
Placebo treatment	0.248***	0.231***
	(0.0484)	(0.0484)
Subjects	25,490	25,490
Panel c: July		
Placebo treatment	0.0563	0.0780*
	(0.0381)	(0.0382)
Subjects	23,926	23,926
Panel d: August		
Placebo treatment	0.0126	0.0280
	(0.0328)	(0.0328)
Subjects	25,450	25,450
Panel e: September		
Placebo treatment	0.0486	0.0617*
	(0.0289)	(0.0290)
Subjects	23,833	23,833
Panel f: October		
Placebo treatment	0.0649**	0.0742**
	(0.0245)	(0.0246)
Subjects	24,420	24,420

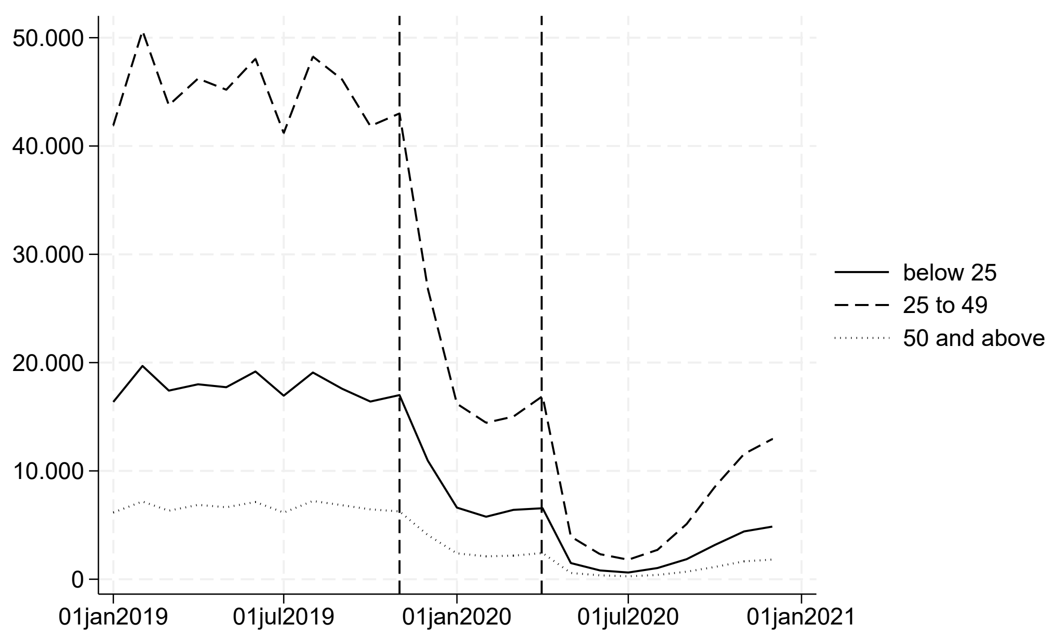
Notes: coefficients and standard error estimates of the treatment effect from a Cox proportional hazards model. The dependent variable is the transition rate to employment. Durations are censored before the beginning of the COVID-19 pandemic. Placebo treatment is a time-varying indicator set to one after the placebo reform date. The panels show the results for different inflow cohorts. The placebo treatment group consists of different cohorts with inflow in 20197, the control group consists of different cohorts with inflow in 2018. Column (1) shows estimates without control variables. Column (2) shows estimates controlling for gender, age, vocational and school education, German nationality, children in different age groups, East Germany, communal jobcenter, and days in contributory employment in periods before inflow into welfare. $*p < 0.1, **p < 0.05, ***p < 0.01$
Source: administrative data of the Statistics Department of the Federal Employment Agency, own calculations.

Appendix

Figure A1: Number of new sanctions per month by gender and age



(a) Gender



(b) Age in years

Notes: the left vertical line marks the date of the Federal Constitutional Court (FCC) ruling of 5 November of 2019. The right vertical line marks the onset of the COVID-19 pandemic mid to end of March 2020. *Source:* Statistics Department of the Federal Employment Agency, own calculations.

Table A1: Mean of January 2019 cohort and t-test of mean difference compared to January cohorts in earlier years

	Mean	Difference 2018	2017	2016	2015
Female	0.350	-0.004	-0.005	-0.008	-0.003
Age	35.231	-0.258**	-0.418***	-0.607***	-0.498***
Highest vocational degree: none	0.335	-0.032***	-0.059***	-0.049***	-0.058***
Highest vocational degree: vocational degree	0.558	0.009	0.031***	0.047***	0.064***
Highest vocational degree: university degree	0.093	0.003	0.001	-0.000	-0.007**
Highest vocational degree: missing	0.014	0.020***	0.027***	0.002	0.001
Highest school degree: none	0.045	-0.001	-0.008***	-0.012***	-0.013***
Highest school degree: lower secondary (Hauptschule)	0.130	-0.013***	-0.013***	-0.031***	-0.048***
Highest school degree: lower secondary (Realschule)	0.555	0.002	0.014**	0.041***	0.071***
Highest school degree: upper secondary (Hochschulreife)	0.230	0.005	0.004	0.000	-0.009**
Highest school degree: missing	0.040	0.006***	0.003	0.002	-0.001
German nationality	0.746	0.000	0.005	0.023***	0.037***
Any child aged 0-2 years	0.062	0.004	0.007**	0.002	0.001
Any child aged 3-17 years	0.233	-0.008	-0.001	-0.020***	-0.010**
Any child aged 18-24 years	0.074	-0.001	0.003	-0.003	0.001
East Germany	0.256	0.002	0.001	0.010**	0.018***
Communal jobcenter	0.266	0.000	0.007	0.005	-0.002
Days in cont. employment past year	122.740	-12.447***	-11.658***	-16.223***	-12.146***
Days in cont. employment past 5 years	543.046	-20.676***	-11.590**	-11.675**	-4.878
Days in cont. employment past 10 years	907.986	-16.313	-4.884	-10.819	-11.356

Notes: the first column shows the mean for the treatment cohort. The remaining columns display the mean difference along with asterisks indicating statistically significant mean differences from a t-test with $*p < 0.1, **p < 0.05, ***p < 0.01$.

Source: administrative data of the Statistics Department of the Federal Employment Agency, own calculations.

Table A2: Mean of April 2019 cohort and t-test of mean difference compared to April cohorts in earlier years

	Mean	Difference 2018	2017	2016	2015
Female	0.374	0.010	0.007	0.003	0.011*
Age	34.437	-0.301**	-0.633***	-0.853***	-0.647***
Highest vocational degree: none	0.346	-0.019***	-0.065***	-0.041***	-0.045***
Highest vocational degree: vocational degree	0.500	0.009	0.019***	0.032***	0.039***
Highest vocational degree: university degree	0.141	0.002	0.008*	0.002	0.001
Highest vocational degree: missing	0.013	0.008***	0.038***	0.007***	0.005***
Highest school degree: none	0.046	-0.002	-0.006**	-0.012***	-0.010***
Highest school degree: lower secondary (Hauptschule)	0.122	-0.009**	-0.018***	-0.022***	-0.036***
Highest school degree: lower secondary (Realschule)	0.490	-0.000	0.012*	0.032***	0.047***
Highest school degree: upper secondary (Hochschulreife)	0.299	0.010	0.009	0.001	-0.006
Highest school degree: missing	0.042	0.002	0.003	0.001	0.005**
German nationality	0.755	0.003	0.013**	0.022***	0.027***
Any child aged 0-2 years	0.064	-0.006*	-0.001	-0.010***	-0.005*
Any child aged 3-17 years	0.215	0.002	-0.012**	-0.013***	-0.009*
Any child aged 18-24 years	0.075	-0.002	-0.005	-0.004	-0.001
East Germany	0.255	-0.009	0.006	0.007	0.020***
Communal jobcenter	0.248	0.010*	0.009*	0.009*	0.004
Days in cont. employment past year	100.298	-2.392	-4.527***	-9.177***	-12.434***
Days in cont. employment past 5 years	490.236	-7.569	-6.195	-14.312**	-14.746**
Days in cont. employment past 10 years	823.474	0.520	1.535	-21.995**	-10.485

Notes: the first column shows the mean for the treatment cohort. The remaining columns display the mean difference along with asterisks indicating statistically significant mean differences from a t-test with $*p < 0.1, **p < 0.05, ***p < 0.01$.

Source: administrative data of the Statistics Department of the Federal Employment Agency, own calculations.

Table A3: Mean of July 2019 cohort and t-test of mean difference compared to July cohorts in earlier years

	Mean	Difference 2018	2017	2016	2015
Female	0.385	0.010	0.010	0.027***	0.018***
Age	34.200	-0.404***	-0.608***	-1.322***	-1.360***
Highest vocational degree: none	0.345	-0.022***	-0.062***	-0.053***	-0.045***
Highest vocational degree: vocational degree	0.537	0.025***	0.026***	0.035***	0.050***
Highest vocational degree: university degree	0.101	-0.004	-0.001	-0.004	-0.005
Highest vocational degree: missing	0.017	0.000	0.037***	0.022***	0.001
Highest school degree: none	0.044	-0.001	0.002	-0.006**	-0.012***
Highest school degree: lower secondary (Hauptschule)	0.134	-0.015***	-0.021***	-0.026***	-0.033***
Highest school degree: lower secondary (Realschule)	0.508	0.016**	0.023***	0.037***	0.050***
Highest school degree: upper secondary (Hochschulreife)	0.270	-0.002	-0.008	-0.005	-0.007
Highest school degree: missing	0.043	0.003	0.003	0.000	0.001
German nationality	0.755	0.006	0.009*	0.035***	0.039***
Any child aged 0-2 years	0.061	-0.003	-0.002	0.003	-0.003
Any child aged 3-17 years	0.230	-0.007	-0.002	-0.007	-0.013**
Any child aged 18-24 years	0.079	0.001	0.002	-0.001	0.000
East Germany	0.239	0.019***	0.012**	0.023***	0.006
Communal jobcenter	0.260	-0.013**	0.007	0.004	-0.003
Days in cont. employment past year	95.115	-1.874	-8.916***	-13.869***	-15.138***
Days in cont. employment past 5 years	499.919	-22.778***	-33.578***	-59.524***	-38.404***
Days in cont. employment past 10 years	849.995	-34.019***	-49.806***	-100.321***	-69.128***

Notes: the first column shows the mean for the treatment cohort. The remaining columns display the mean difference along with asterisks indicating statistically significant mean differences from a t-test with $*p < 0.1, **p < 0.05, ***p < 0.01$.

Source: administrative data of the Statistics Department of the Federal Employment Agency, own calculations.

Table A4: Mean of August 2019 cohort and t-test of mean difference compared to August cohorts in earlier years

	Mean	Difference 2018	2017	2016	2015
Female	0.394	0.001	0.009	0.031***	0.043***
Age	33.789	-0.079	-0.326**	-1.271***	-1.475***
Highest vocational degree: none	0.354	-0.010*	-0.054***	-0.047***	-0.050***
Highest vocational degree: vocational degree	0.521	0.010	0.008	0.036***	0.054***
Highest vocational degree: university degree	0.110	-0.002	0.001	-0.012***	-0.008**
Highest vocational degree: missing	0.015	0.003*	0.045***	0.023***	0.004**
Highest school degree: none	0.048	-0.000	0.001	-0.008***	-0.011***
Highest school degree: lower secondary (Hauptschule)	0.135	-0.009**	-0.017***	-0.025***	-0.031***
Highest school degree: lower secondary (Realschule)	0.501	0.010	0.005	0.027***	0.039***
Highest school degree: upper secondary (Hochschulreife)	0.274	-0.003	-0.002	0.006	0.001
Highest school degree: missing	0.042	0.003	0.013***	-0.000	0.002
German nationality	0.757	0.003	-0.002	0.029***	0.043***
Any child aged 0-2 years	0.062	0.001	-0.002	-0.002	0.001
Any child aged 3-17 years	0.249	-0.009	-0.009*	-0.014***	-0.016***
Any child aged 18-24 years	0.086	0.006	0.001	0.003	-0.001
East Germany	0.243	-0.003	-0.006	0.010*	0.040***
Communal jobcenter	0.256	-0.001	-0.002	-0.004	-0.003
Days in cont. employment past year	93.306	0.928	-6.570***	-12.382***	-15.114***
Days in cont. employment past 5 years	478.978	-11.423*	-29.059***	-46.527***	-53.792***
Days in cont. employment past 10 years	812.254	-11.405	-45.672***	-76.261***	-94.649***

Notes: the first column shows the mean for the treatment cohort. The remaining columns display the mean difference along with asterisks indicating statistically significant mean differences from a t-test with $*p < 0.1, **p < 0.05, ***p < 0.01$.

Source: administrative data of the Statistics Department of the Federal Employment Agency, own calculations.

Table A5: Mean of September 2019 cohort and t-test of mean difference compared to September cohorts in earlier years

	Mean	Difference 2018	2017	2016	2015
Female	0.381	0.007	0.011*	0.006	0.020***
Age	33.997	-0.046	-0.048	-0.569***	-0.542***
Highest vocational degree: none	0.356	-0.005	-0.060***	-0.066***	-0.047***
Highest vocational degree: vocational degree	0.513	0.008	0.012*	0.044***	0.048***
Highest vocational degree: university degree	0.115	-0.005	0.003	-0.003	-0.003
Highest vocational degree: missing	0.017	0.002	0.045***	0.025***	0.002
Highest school degree: none	0.053	0.001	-0.002	-0.010***	-0.014***
Highest school degree: lower secondary (Hauptschule)	0.145	-0.014***	-0.026***	-0.030***	-0.044***
Highest school degree: lower secondary (Realschule)	0.488	0.018***	0.020***	0.042***	0.062***
Highest school degree: upper secondary (Hochschulreife)	0.270	-0.009	-0.003	-0.008	-0.013**
Highest school degree: missing	0.043	0.003	0.012***	0.007**	0.009***
German nationality	0.734	0.006	-0.008	0.023***	0.026***
Any child aged 0-2 years	0.059	0.004	0.009***	0.002	0.002
Any child aged 3-17 years	0.241	-0.008	-0.001	-0.012**	-0.004
Any child aged 18-24 years	0.087	-0.001	-0.001	-0.002	-0.002
East Germany	0.242	0.006	0.006	0.011**	0.020***
Communal jobcenter	0.259	-0.004	0.005	-0.002	0.006
Days in cont. employment past year	96.650	0.029	-6.556***	-8.294***	-11.485***
Days in cont. employment past 5 years	498.575	-26.806***	-37.699***	-30.696***	-38.809***
Days in cont. employment past 10 years	836.411	-35.330***	-52.359***	-45.101***	-61.221***

Notes: the first column shows the mean for the treatment cohort. The remaining columns display the mean difference along with asterisks indicating statistically significant mean differences from a t-test with $*p < 0.1, **p < 0.05, ***p < 0.01$.

Source: administrative data of the Statistics Department of the Federal Employment Agency, own calculations.

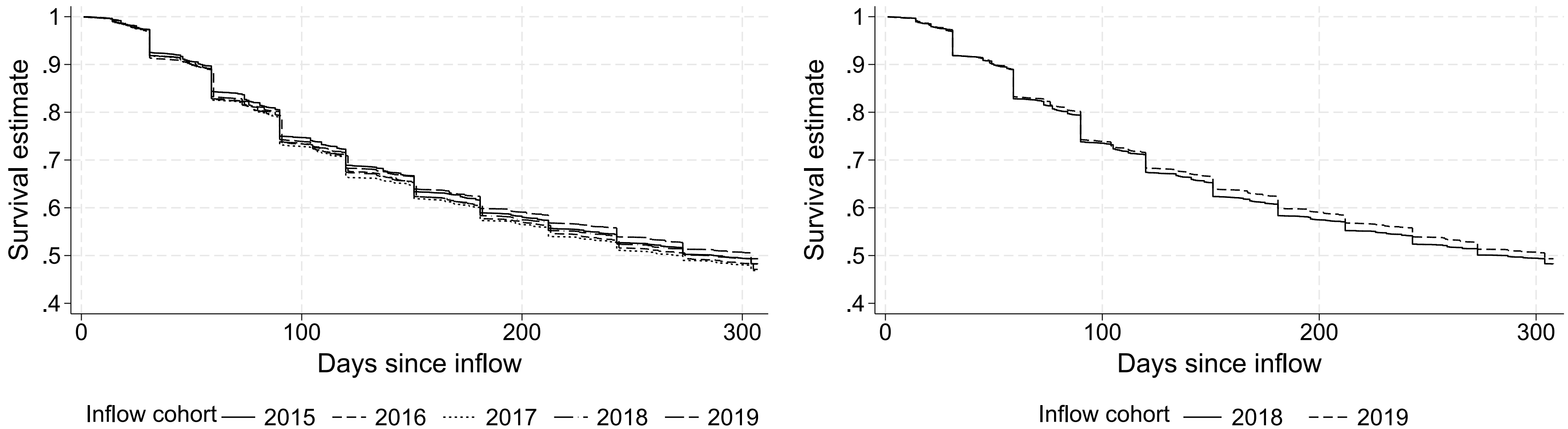
Table A6: Mean of October 2019 cohort and t-test of mean difference compared to October cohorts in earlier years

	Mean	Difference 2018	2017	2016	2015
Female	0.374	0.010	0.012*	0.010*	0.018***
Age	34.035	-0.112	-0.139	-0.417***	-0.607***
Highest vocational degree: none	0.349	-0.012**	-0.068***	-0.070***	-0.047***
Highest vocational degree: vocational degree	0.498	0.002	0.013**	0.026***	0.031***
Highest vocational degree: university degree	0.138	0.006	0.011**	0.014***	0.012***
Highest vocational degree: missing	0.014	0.004**	0.044***	0.029***	0.004**
Highest school degree: none	0.048	-0.000	-0.002	-0.008***	-0.014***
Highest school degree: lower secondary (Hauptschule)	0.131	-0.009**	-0.022***	-0.027***	-0.036***
Highest school degree: lower secondary (Realschule)	0.481	-0.006	0.002	0.024***	0.037***
Highest school degree: upper secondary (Hochschulreife)	0.296	0.013**	0.016***	0.007	0.004
Highest school degree: missing	0.043	0.002	0.007***	0.004*	0.008***
German nationality	0.754	0.003	-0.004	0.013**	0.018***
Any child aged 0-2 years	0.064	-0.007**	-0.002	-0.003	-0.008***
Any child aged 3-17 years	0.216	-0.002	-0.005	-0.010**	-0.007
Any child aged 18-24 years	0.073	-0.002	-0.000	-0.002	-0.001
East Germany	0.245	0.012**	0.019***	0.031***	0.025***
Communal jobcenter	0.258	-0.001	0.004	0.005	-0.005
Days in cont. employment past year	94.214	2.380	-3.905***	-2.860**	-8.438***
Days in cont. employment past 5 years	493.890	-24.403***	-34.225***	-32.251***	-39.853***
Days in cont. employment past 10 years	837.056	-43.907***	-46.251***	-54.016***	-69.677***

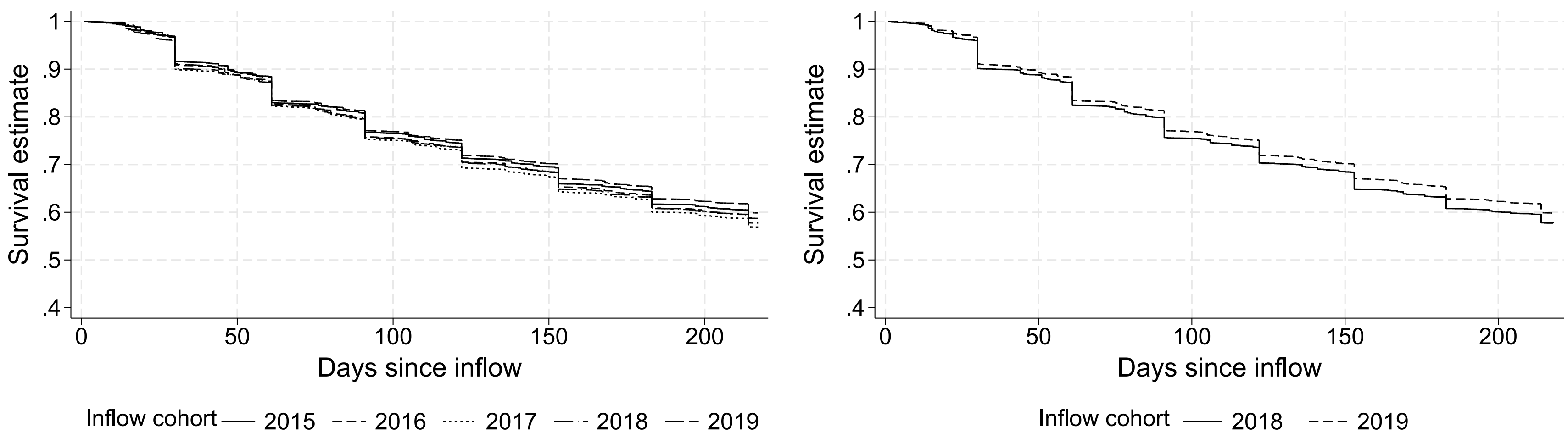
Notes: the first column shows the mean for the treatment cohort. The remaining columns display the mean difference along with asterisks indicating statistically significant mean differences from a t-test with $*p < 0.1, **p < 0.05, ***p < 0.01$.

Source: administrative data of the Statistics Department of the Federal Employment Agency, own calculations.

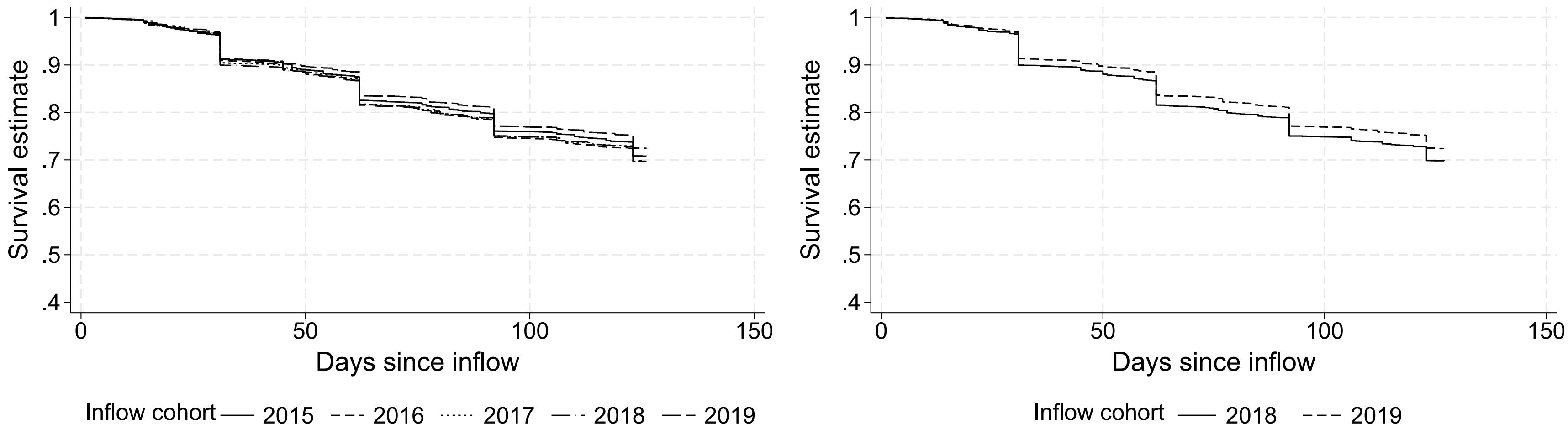
Figure A2: Estimates of the employment survivor function by inflow cohort



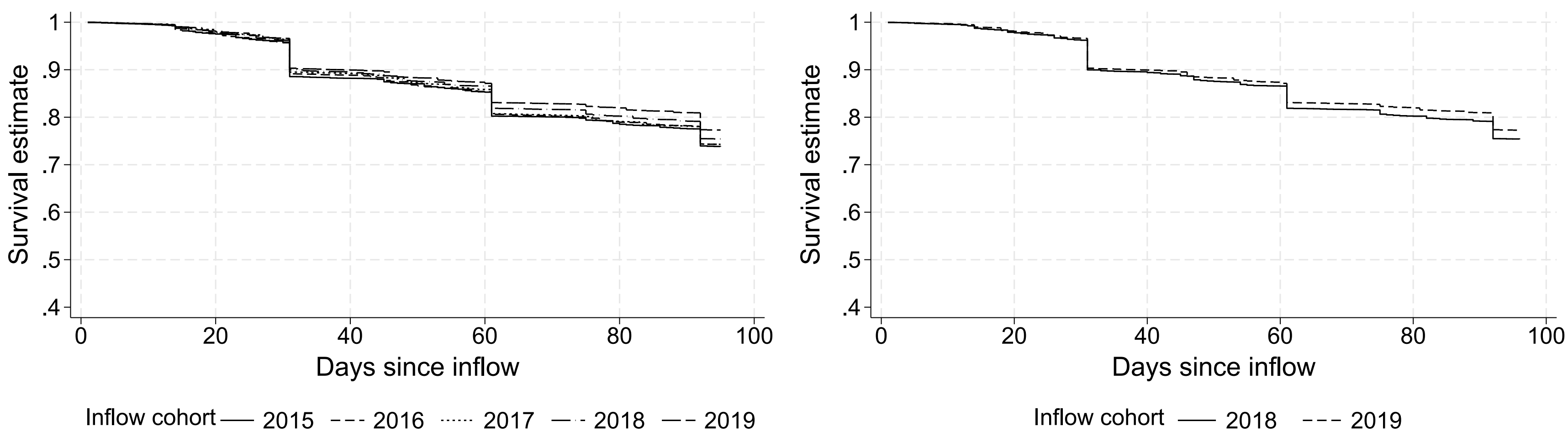
(a) January



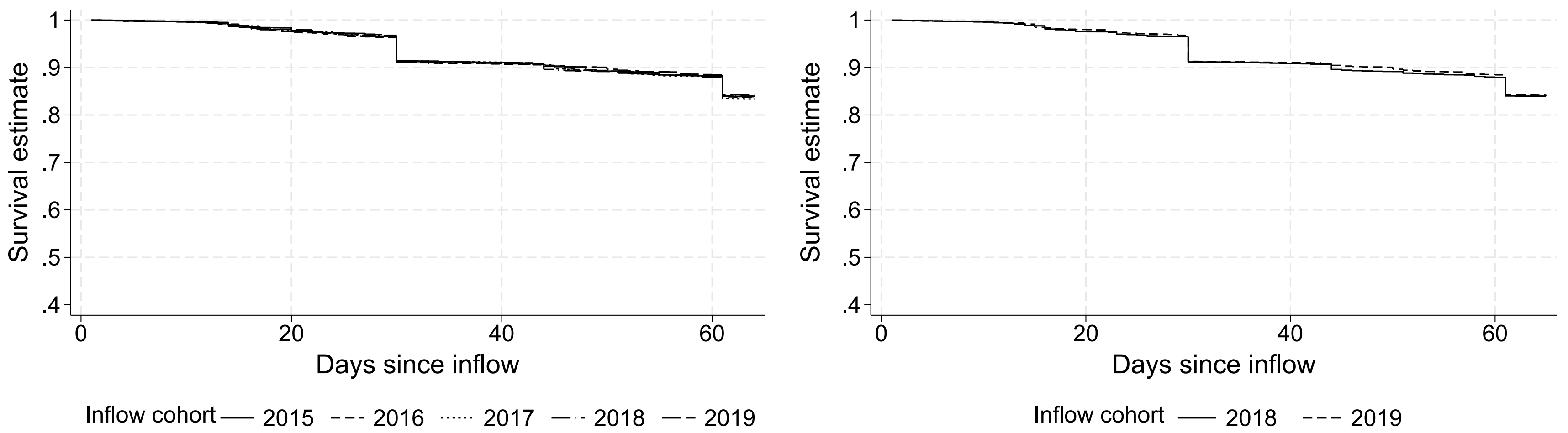
(b) April



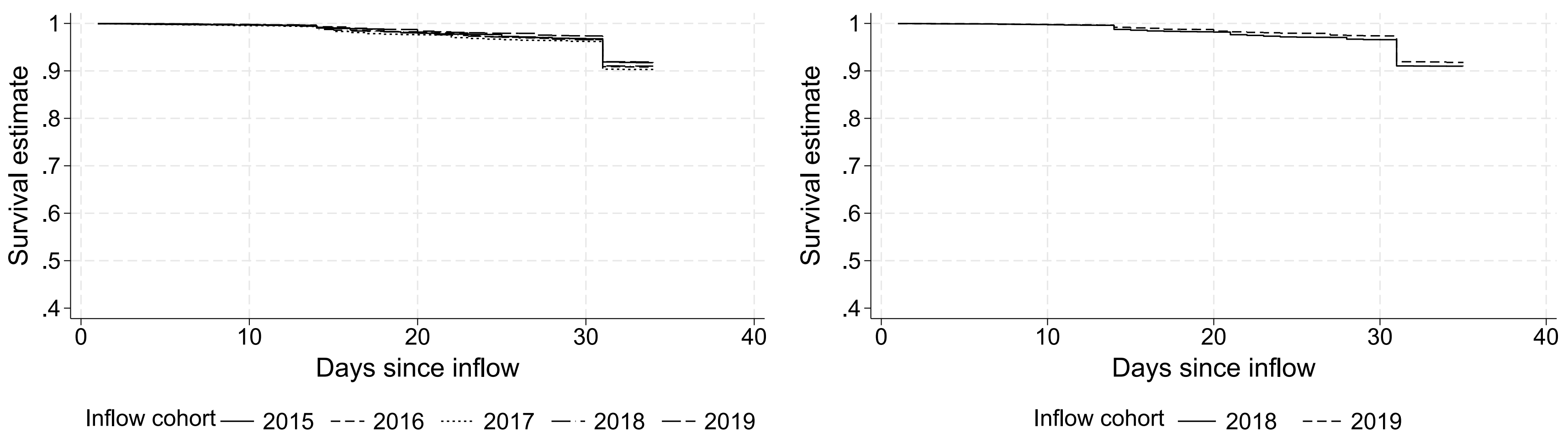
(a) July



(b) August



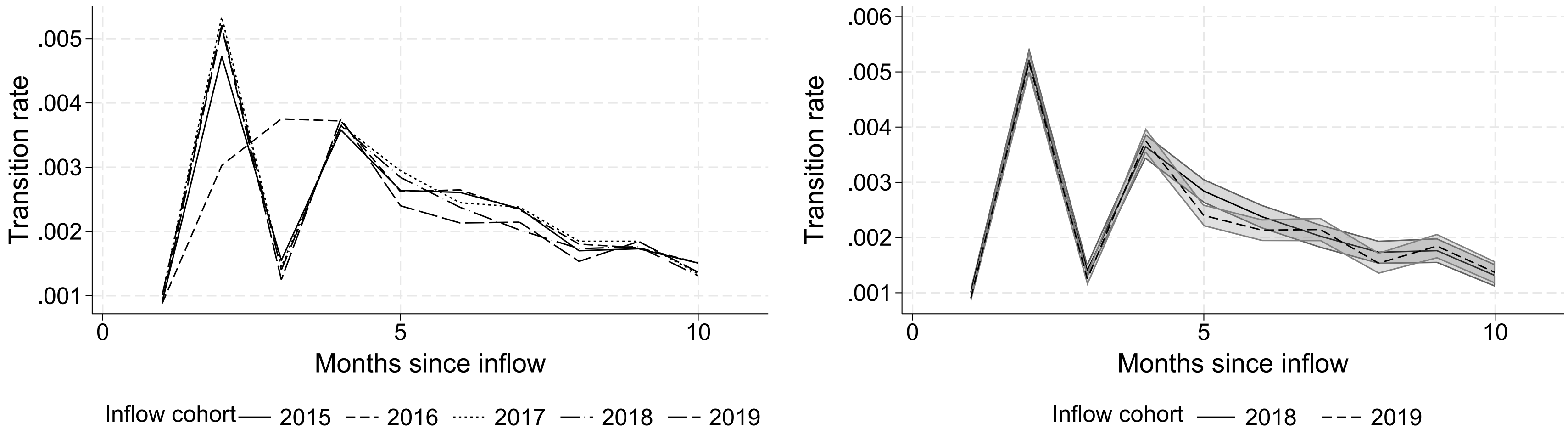
(a) September



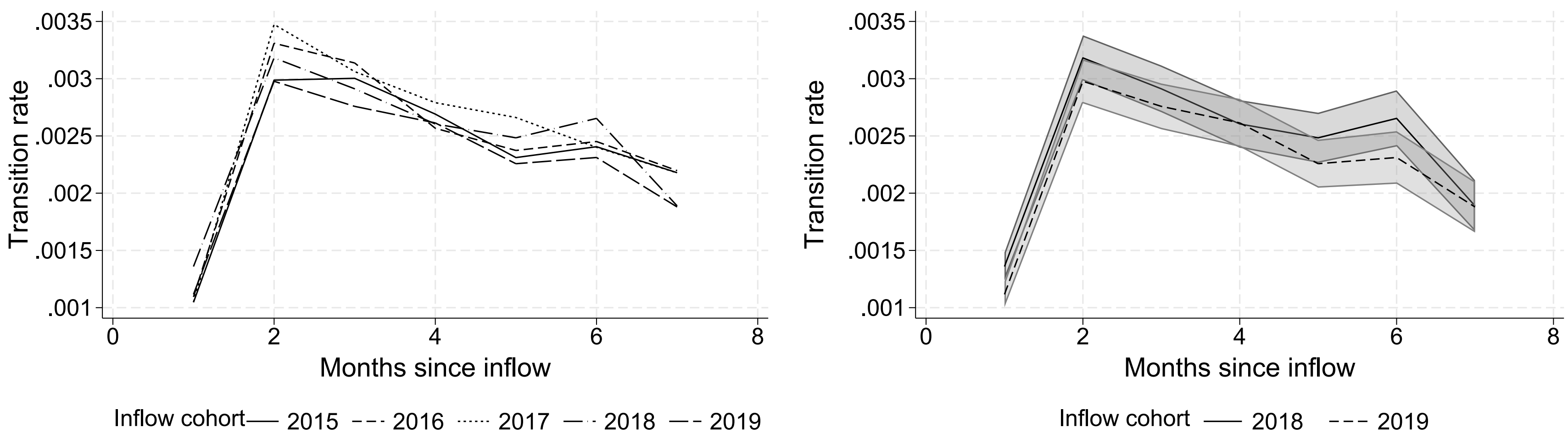
(b) October

Notes: The panels show lifetable estimates of the employment survivor function for the inflow cohorts in calendar months of different years. Survivor functions are displayed until the reform date for the respective inflow cohort.
Source: Statistics Department of the Federal Employment Agency, own calculations.

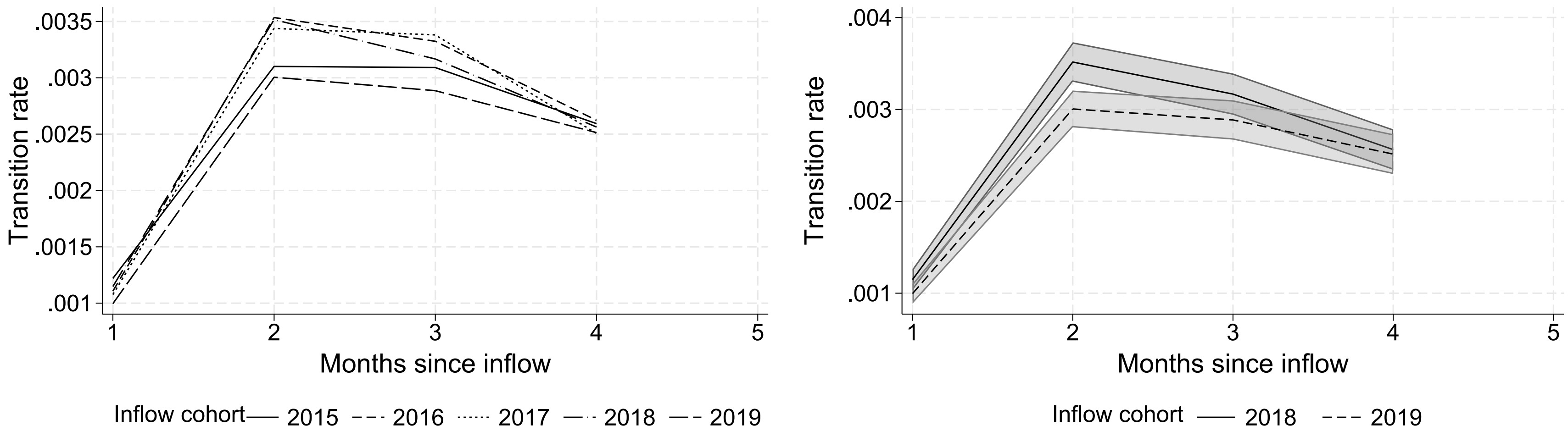
Figure A5: Estimates of the monthly transition rate to employment by inflow cohort



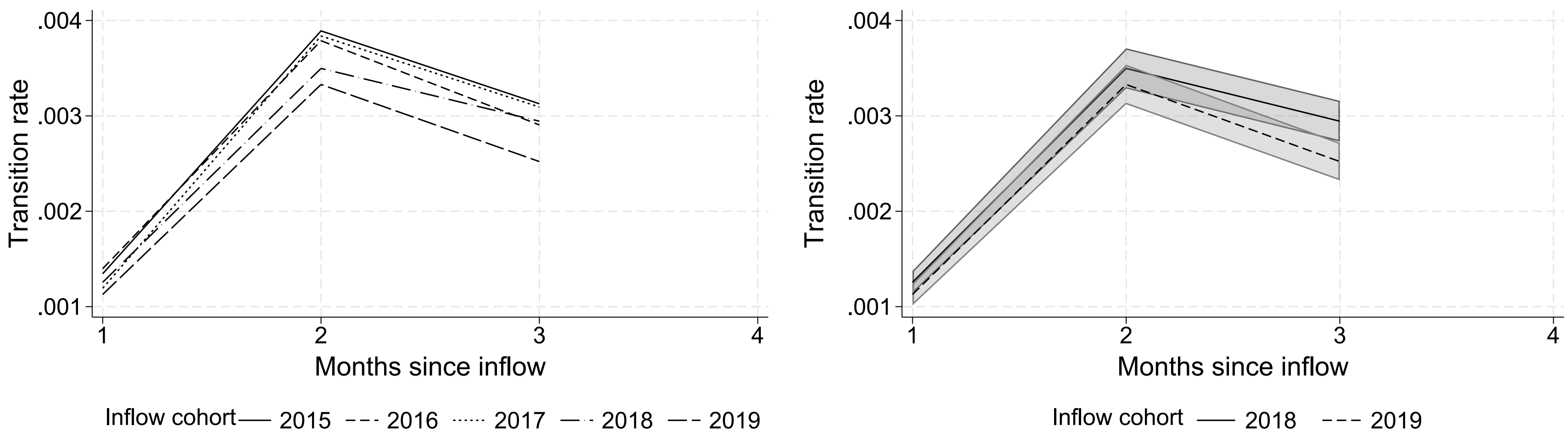
(a) January



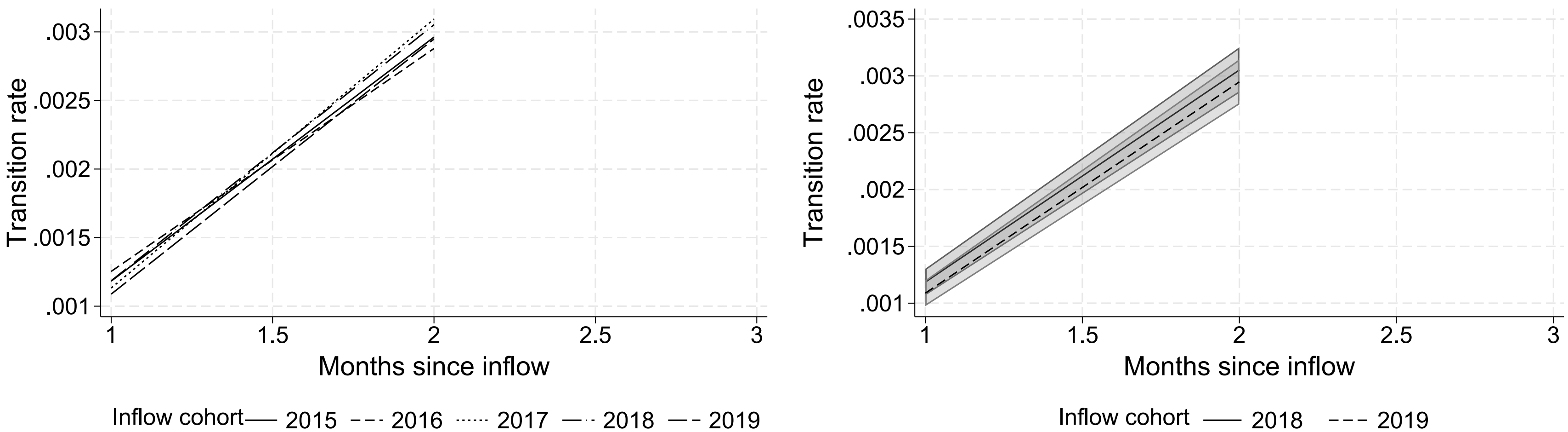
(b) April



(a) July



(b) August



(a) September

Notes: The panels show lifetable estimates of the transition rate to employment calculated in 30 day intervals for the inflow cohorts in calendar months of different years. Transition rates are displayed until the reform date for the respective inflow cohort.
Source: Statistics Department of the Federal Employment Agency, own calculations.