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**Working Paper**

## The dovish turnaround: Germany's social benefit reform and job findings

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# IAB-DISCUSSION PAPER

Articles on labour market issues

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## 7|2024 The Dovish Turnaround: Germany's Social Benefit Reform and Job Findings

Enzo Weber

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# The Dovish Turnaround: Germany's Social Benefit Reform and Job Findings

Enzo Weber (IAB, and University of Regensburg, Germany)

Mit der Reihe „IAB-Discussion Paper“ will das Forschungsinstitut der Bundesagentur für Arbeit den Dialog mit der externen Wissenschaft intensivieren. Durch die rasche Verbreitung von Forschungsergebnissen über das Internet soll noch vor Drucklegung Kritik angeregt und Qualität gesichert werden.

The “IAB Discussion Paper” is published by the research institute of the German Federal Employment Agency in order to intensify the dialogue with the scientific community. The prompt publication of the latest research results via the internet intends to stimulate criticism and to ensure research quality at an early stage before printing.

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## Abstract

On the labour markets, recent decades were characterised by structural supply-side reforms in many countries. Following its hawkish reforms from the 2000s, Germany has recently made a dovish turnaround. Conditions in basic income support for unemployed became more generous. Before, a temporary moratorium on sanctions had been imposed, providing a unique policy shift. We analyse the short-run consequences for job findings, building on large administrative data and a novel control group approach. The moratorium dampened job findings by four percent and the subsequent benefit reform by almost six percent in the first year. Other factors played a still larger role for the recent weakening of job findings.

## Zusammenfassung

Auf den Arbeitsmärkten waren die letzten Jahrzehnte in vielen Ländern von strukturellen Reformen auf der Angebotsseite gekennzeichnet. Nach den Hartz-Reformen in den 2000er Jahren hat Deutschland kürzlich eine Kehrtwende vollzogen. Die Bedingungen der Grundsicherung für Arbeitslose wurden großzügiger. Zuvor galt ein befristetes Sanktionsmoratorium. Wir analysieren die kurzfristigen Auswirkungen auf die Beschäftigungsaufnahmen und verwenden umfangreiche administrative Daten sowie eine neue Kontrollgruppe. Das Moratorium dämpfte die Jobaufnahmen aus der Grundsicherung um vier Prozent und die nachfolgende Bürgergeldreform um fast sechs Prozent im ersten Jahr. Andere Faktoren spielten für die jüngste Abschwächung der Arbeitsaufnahmen eine noch größere Rolle.

## JEL classification

JEL-Codes: J64, J2

## Keywords

Job Findings, Reforms, Sanctions, Social Benefits

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## 1 Introduction

On the labour markets, the recent decades have been characterised by “hawkish” structural supply-side reforms, especially in Europe. In the course of the euro crisis, this concerned Spain and Italy, amongst others (Gehrke/Weber 2018). Typically, benefits became less generous and requirements for job seekers more rigorous. For instance, the OECD “adequacy of guaranteed minimum income benefits” has fallen from 41 percent of median disposable income in 2001 to 34 percent in 2019.<sup>1</sup> As a measure of strictness in job search, the average score in the “Comparative Unemployment Benefit Conditions & Sanctions Dataset” (Knotz/Nelson 2019) has increased from 0.35 in 1980 to 0.50 in 2012.<sup>2</sup>

The German “Hartz” reforms had already been implemented in the mid-2000s. In times of mass unemployment, the focus was on deregulation, efficiency and activation (compare, e.g., Klinger/Rothe 2012). As a result, many studies find an increase in job findings and a decrease in structural unemployment (e.g. Fahr/Sunde 2009, Krause/Uhlig 2012, Krebs/Scheffel 2013, Klinger/Weber 2016, Hutter et al. 2022). The other side of the coin was real wage loss with an expanding low-wage sector (Gartner et al. 2023).

If unemployment insurance claims expire or do not exist at all, jobseekers in Germany receive basic income support from Jobcenters. This recently included around 1.8 million unemployed. The subsistence level for the whole household is guaranteed by means-tested benefits with fixed personal rates and housing support. The system was designed in the Hartz reforms that belonged to the period of hawkish labour market policies. Today, the situation has largely changed: the labour market has become much tighter, wage inequality reached record levels in the early 2010s, and a fundamental economic transformation shapes new job requirements. Against this backdrop, Germany has recently made a “dovish” turnaround – changing many regulations in the citizen’s benefit reform (Federal Ministry of Labour and Social Affairs 2023): Sanctions are confined to steps of 10, 20 and 30 percent.

- Higher limits for housing and assets as well as a deferment period of one year apply.
- Job integration is no longer prioritised over training, qualification incentives are increased.

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<sup>1</sup> For a jobless person without children, including housing benefits. See <https://www.oecd.org/els/soc/benefits-and-wages/>.

<sup>2</sup> On a scale from 0 to 1. Data gaps up to two years imputed, without Korea and the Netherlands due to longer gaps. The data cover the strictness of job-search and reporting requirements, the definition of suitable work, and unemployment benefit sanction rules.

- Mandatory integration agreements are replaced by legally less binding cooperation plans.
- Benefits increased by about twelve percent each in 2023 and 2024 (CPI inflation rates were 6.9 and 5.9 percent).

The idea of the reform was to create a level playing field, better professional development opportunities and qualification prospects. We consider the German case as a leading representative of such a dovish stance that may become more widespread after a period of hawkish reforms. Indeed, in the OECD “strictness of activation requirements index”, seven out of 28 countries saw a decline in 2022 compared to pre-pandemic levels. On the downside, such measures may weaken activation and incentives and hamper job findings. Notably, the dovish reforms in Germany came with the expectation that in the new era of labour shortages, unemployment would not return even if benefit systems were relaxed. This may be the case if improved earnings opportunities outweigh the outside options, such that search efforts and job take-up would adjust less to more generous conditions. Similarly, even if the Beveridge curve shifts outwards, a steep job creation curve – reflecting high tightness – could limit the increase of unemployment in the new equilibrium. On the other hand, the earlier reform effects in a slack labour market may even have been weaker due to downward wage rigidity (Abbritti/Fahr 2013) or a minor role of matching frictions if jobs are rationed (Michaillat 2012).

This paper is the first to analyse the consequences of the citizen’s benefit reform for job findings. It contributes to the literature mainly occupied with hawkish reforms by adding evidence on a major policy reversal. Similarly, with its focus on the welfare system, it contributes complementary evidence to the literature on the design of unemployment insurance (e.g., van den Berg/Vikström (2014), Schmieder et al. 2016, Nekoei/Weber (2017)). Moreover, we make use of a unique period with a marked change of benefit conditionality for analysing the role of sanctions: while the reform came into force in January 2023, the government enacted a sanctions moratorium from July 2022 until the end of the year. No benefit reductions could be imposed in the event of a breach of the collaborative obligations such as rejecting a reasonable job offer (or a maximum of 10 percent was possible in the event of repeated missed appointments). Before, the Federal Constitutional Court had called for adjustments to the sanctions regime in basic income support in November 2019. Effectively, a maximum of 30 percent benefit reduction was set until a new legal regulation that was later implemented with the reform. Sanctions being intended to encourage unemployed to seek jobs, previous research at the individual level (e.g., van den Berg et al. 2022) indeed shows that they significantly increase the likelihood of taking up work.

The analysis builds on a full sample of administrative data. These reveal that job take-up from basic income support has fallen substantially. However, parallel to the institutional changes, other factors with possible adverse effects emerged. We account for the economic downturn in a labour market matching approach and use regional variation for estimating the influence of the integration of Ukrainian refugees. Furthermore, the paper puts forward a novel control group of unemployed not entitled to benefits and allows for spillover effects.

We find that the moratorium dampened job findings by four percent and the subsequent benefit reform by almost six percent in the first year. Before, the level had already been reduced by additional 2.6 percent with the reaction to the Constitutional Court decision.

The paper is structured as follows. The subsequent section presents the German data and accounts for labour market matching development. Section 3 elaborates on the identification of reform effects. The last section concludes.

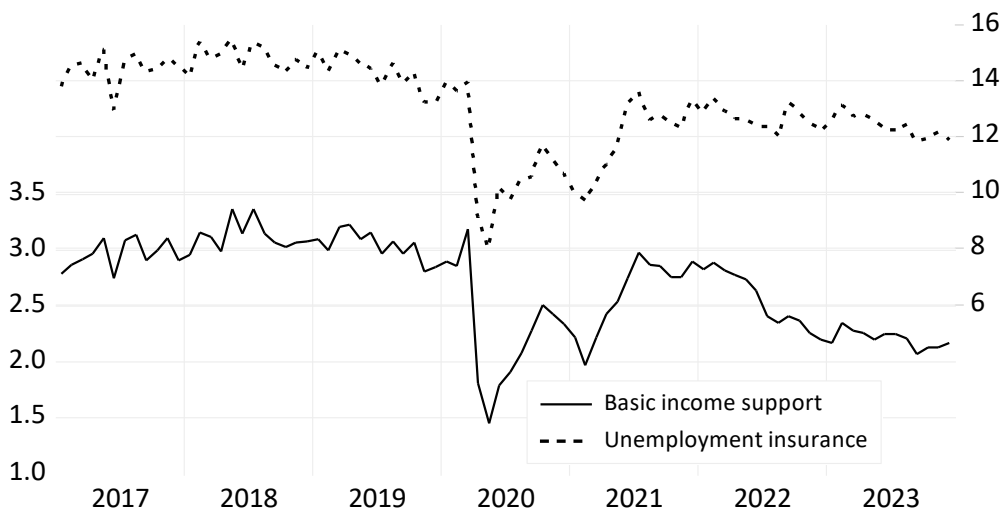
## 2 Data and labour market development

### Data

We use monthly administrative data from the German Federal Employment Agency (BA) until the end of 2023. This represents a full sample from the official labour market statistics. Persons count as unemployed if they registered at an employment agency or Jobcenter. A job finding is defined as a case where such a person leaves unemployment and starts an unsubsidised job. BA counts unemployment in the middle of a month, flows are defined between this date and the middle of the subsequent calendar month. Figure 1 shows the development of the job finding rate, i.e. the percentage of the unemployed switching to employment each month.

**Figure 1: Job finding rates of unemployed**

in percent



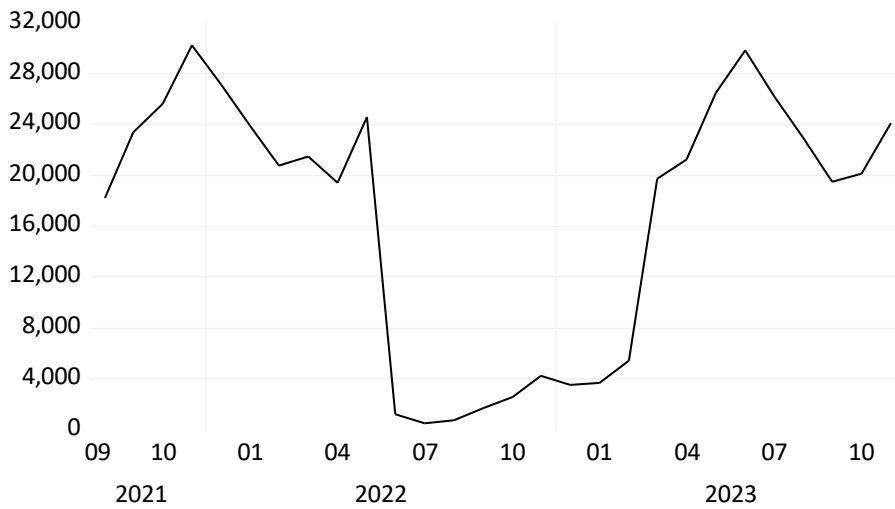
Note: Own calculations and seasonal adjustment (ARIMA X-12), without Ukrainian citizens.

Source: BA Statistics. © IAB

With Covid there was a slump. But what matters for our research questions is that after an interim recovery, the transition rates from basic income support fell by about 20 percent in 2022 – or one third compared to 2019. In fact, the greatest reduction in job findings after the pandemic occurs in July 2022, the first month of the moratorium. It is also notable that there was no such shift in unemployment insurance, for which no sanctions moratorium applied (and in any case no comparable sanction regulation). Figure 2 offers a cross-check in this regard: Indeed, the new sanctions drop abruptly to minimal values in July 2022.



**Figure 2: New sanctions in basic income support**

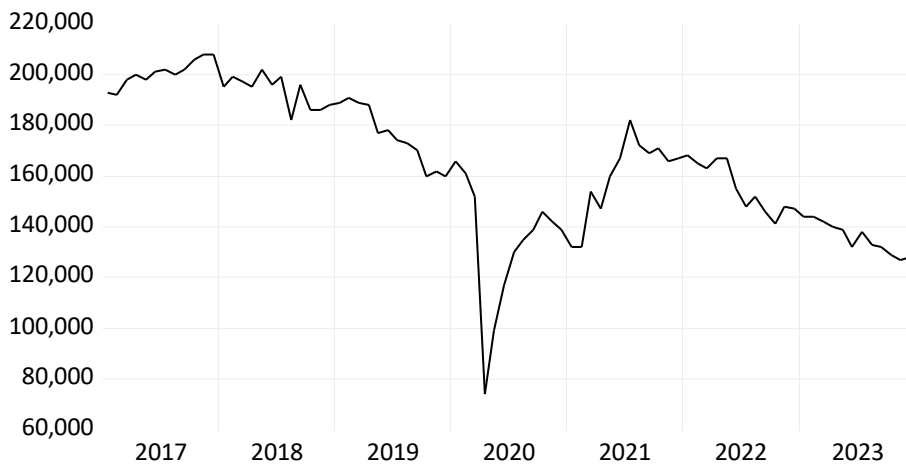


Note: without Ukrainian citizens.

Source: BA Statistics. © IAB

In the spring of the same year, the energy crisis began and the labour market weakened. Figure 3 shows that significantly fewer new vacancies were registered from mid-2022 onwards. In fact, the level was recently lower than in the second Covid lockdown. This made it more difficult for the unemployed to find jobs.

**Figure 3: Newly registered vacancies**



Source: BA Statistics, official seasonal adjustment. © IAB

## Controlling for labour market development

In order to account for labour market development, we begin with a search and matching approach. The transitions of job seekers in basic income support into employment  $F$  are explained in a matching function by the number of unemployed  $U$  in basic income support and the number of vacancies  $V$ , which together can form “matches”, i.e.,  $F = MV^\alpha U^\beta$  (Pissarides 2000). This transfers the principle of a standard production function in which the matches represent the output. The more vacancies there are, the more unemployed get into jobs.

The term  $M$  in the matching function indicates the efficiency with which the matching takes place given vacancies and unemployment. The matching efficiency also depends on the intensity of the search on the labour market and the activation or willingness to take up jobs (cf. Hutter et al. 2022, Merkl/Sauerbier 2023), which can be shifted by institutional changes. Accordingly, we model it as  $M = M(S, R, \varepsilon)$ , where  $S$  is a shift indicator for the sanctions moratorium from July 2022 until the end of the year,  $R$  is an indicator for the citizen’s benefit reform from January 2023 and  $\varepsilon$  captures further factors.<sup>3</sup> Such approaches have also been used in the literature, for example, for the macro evaluation of the Hartz reforms (Fahr/Sunde 2009, Klinger/Rothe 2012).

In order to adequately capture the effect of the labour market downturn, a proper specification of labour demand is key. In stock-flow approaches, it often turns out that the inflow of vacancies is more relevant than the stock. That is also the case here: The empirical specification shows that the new vacancies  $v$  from the current and the previous month are highly significant. In contrast, the second lag ( $t$ -value -0.27) and the vacancy stock ( $t$ -value -1.48) provide no additional explanatory power.

Formally, the empirical matching function is:

$$\ln F_t/U_{t-1} = c_0 + c_1 t + \alpha_0 \ln v_t + \alpha_1 \ln v_{t-1} + \beta \ln U_{t-1} + \gamma_1 S_t + \gamma_2 R_t + \delta seas_t + \varepsilon_t, \quad (1)$$

It is common to include a time trend to cover unobserved secular developments. Then,  $\ln M_t = c_0 + c_1 t + \gamma_1 S_t + \gamma_2 R_t + \varepsilon_t$ . We consider unemployment with one lag since, due to the BA counting mechanism,  $U_{t-1}$  measures the number of unemployed at the beginning of the month where job findings are recorded (e.g., Hutter et al. 2022). Accordingly,  $F_t/U_{t-1}$  is the job finding rate.  $seas$  denotes a matrix of monthly seasonal dummies.

Equation (1) is estimated by OLS using monthly data for basic income support since January 2017. This avoids a break as after 2016 unemployment benefit recipients who received top-up unemployment benefit II were reclassified from basic income support to unemployment insurance. Residual diagnostics are satisfying with no evidence against normality from the Jarque-Bera test ( $p$ -value 0.836) and no evidence against homoscedasticity from the White test ( $p$ -value 0.286). HAC standard errors (Newey/West 1987) are used in view of some limited and unsystematic autocorrelation.

The regression results show the following: The sanctions moratorium indicator reflects a reduction in job finding rates by 0.103 ( $t$ -value -5.84). Logically, a substantial part is also

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<sup>3</sup> Due to the statistical counting in the middle of the month, the indicators equal 0.5 in the first month, 1 afterwards and 0.5 in the calendar month succeeding the period. Using pure 0/1-indicators produces very similar results. We prefer an indicator to the variable in Figure 2 because the sanctions themselves are endogenous: when they are imposed, it implies that unemployed have behaved in a way that lowers the job finding rate. In any case, Figure 2 shows that in the moratorium the variable is practically identical to an indicator.

explained by the slowdown in the vacancy variables. The reform indicator has a negative coefficient of -0.087 ( $t$ -value -3.79). In comparison, the matching function (1) can be estimated for the jobseekers in unemployment insurance that was not directly affected by the sanctions moratorium and the reform. In this case, there are actually no negative effects for the two indicators ( $t$ -values 1.07 and 1.09). For the time being, this puts forward an instructive empirical pattern. However, basic income support and unemployment insurance may have been subject to further influences that differed between them, beyond the institutional changes.

### 3 Identifying reform effects

#### Confounding factors?

Equations such as (1) can be subject to further confounding factors. For example, there may be changes in hiring intensity or the opportunity cost of employment (Chodorow-Reich / Karabarbounis 2016) that are neither captured by new vacancies nor by trend or autoregressive terms. Notably, in addition to the start of the sanctions moratorium and the weakening of the labour market, there were two significant developments in the relevant period: Firstly, the statutory minimum wage was increased by 14.8 percent to 12 euros per hour in October 2022. This could have impaired job creation and therefore the hiring of unemployed, and more so in basic income support than in unemployment insurance, where higher wages are achieved. However, the matching function already controls for the new vacancies. Thus, a labour-cost channel via job creation would not bias the estimates. Below we also consider vacancies at the low-skilled requirement level, where the minimum wage is by far most relevant (Börschlein et al. 2022). Besides, one could take into account the last strong minimum wage intervention, its introduction in 2015: while this is not necessarily applicable to the later situation, at least the job finding rate at that time had shown no reaction at all.

Secondly, the Ukrainian refugees were integrated into basic income support from June 2022. Because of this special effect of around 190,000 people, Ukrainian citizens were excluded from the entire data used. However, it is conceivable that the greater burden on the Jobcenters (in contrast to the employment agencies responsible for matching in unemployment insurance) impaired matching efficiency. In fact, previous quasi-experimental evidence (Hainmüller et al. 2016) shows that moving from a caseload of 1:100 to 1:40 improved job finding rates by six percent. In the present case, the change in the caseload is much smaller, since Ukrainians only make up eleven percent of the unemployed in basic income support and to a limited extent additional staff was deployed for their integration. In addition, Hainmüller et al. (2016) found that significant impact channels were more registration of vacancies and stronger monitoring and sanctioning. However, the new vacancies are already controlled in the matching function and sanctions were hardly possible anyway because of the moratorium. On the other hand, whilst less than ten percent of all the job take-ups from basic income support are due to placement through Jobcenters (following both the administrative data and survey data in Merkl/Sauerbier 2023), contact intensity will also foster the jobseekers' own search activities. Furthermore, refugees are a group that required special efforts and besides the unemployed, many more non-

jobseeking Ukrainian citizens had to be registered. Similarly, organising the upcoming citizen's benefit reform needed human resources.

In sum, we would expect that the refugee integration dampened the job finding rates, but by clearly less than six percent. For a direct check of this assertion, we exploit the regional variation in the distribution of Ukrainians in basic income support. For that purpose, we make use of 50 functional labour market regions that delineate homogenous labour markets based on commuter flows (Kropp/Schwengler 2016). From 2022:06 the share of Ukrainian war refugees among the unemployed  $Ukr$  is calculated as the share of Ukrainian citizens minus the minor pre-war share (0.004 at the end of 2021). It amounts to 0.11 in total with a sizeable standard deviation of 0.08 between the regions. Using this share as a measure of treatment intensity, we set up the matching function as a monthly panel model for regions  $i = 1, \dots, 50$  with cross-section and time fixed effects ( $\theta_i$  and  $\mu_t$ ):

$$\ln F_{it}/U_{it-1} = c_0 + \alpha_0 \ln v_{it} + \alpha_1 \ln v_{it-1} + \beta \ln U_{it-1} + \gamma D_t^U Ukr_{it} + c_1 \theta_i + c_2 \mu_t + \varepsilon_{it}, \quad (2)$$

where  $D^U$  is a treatment dummy for the period from 2022:06 onwards and  $Ukr$  represents a continuous treatment variable (compare Callaway et al. 2021). The model is estimated again since 2017:01 with seasonally adjusted data and allowing for period heteroscedasticity. The coefficient  $\gamma$  of the interaction term results as -0.272 ( $t$ -value -2.30), saying that 0.01 more in the refugee share stands for 0.272 percent less in the job finding rate. Approximating the total effect via extrapolation, the overall share of 0.11 would correspond to -3 percent, which lies in the expected range and would reduce the indicator effects from (1) accordingly. When we estimate model (2) for unemployment insurance covered by the employment agencies that were not in charge of the refugees, indeed we find no impact ( $\gamma = -0.047$ ,  $t$ -value -0.64).

### A control group approach

To comprehensively control for latent factors, it would be optimal to observe a group of unemployed similarly exposed to the labour market environment but not treated by the institutional changes. This would be given for unemployed who are in basic income support – and thus concerned by poorer placement conditions due to the workload in Jobcenters or the weakening of the labour market – but not entitled to benefits – such that suspended benefit reductions or more generous benefit conditions would not affect their incentives. In fact, this can result from two constellations:

- Although a household is in basic income support, a person is still not entitled if he or she receives other primary benefits or has an income that meets personal needs (compare Statistics of the Federal Employment Agency 2015). That includes multiple possible cases and combinations, such as maintenance payments, early pensions, asylum seeker benefits, training assistance, children's allowance, minor earned income etc. According to special analyses by BA Statistics, there was a generally stable pattern of these reasons over the recent years. From this group we consider those who are registered as unemployed (and are therefore able to work, actively searching and available for jobs).
- Furthermore, unemployment in basic income support without benefits can arise due to the different logic of unemployment and benefit status: The former is given for persons who are registered for job placement and handled in the operative system of the Jobcenters. This status is recorded in the statistics of the “persons able to work” at any point in time. The

benefit status is mirrored by the benefit statistics and includes any changes of new and existing claims applied with delay from an ex post perspective. While in most cases, this coincides with the unemployed status, there is always a number of unemployed served by the Jobcenters but not entitled to benefits. Unemployed who temporarily do not receive benefits due to a sanction are not part of this group.

Indeed, we find no evidence that unemployed not entitled to benefits would gain less from public employment services. The proportion of people in this group who take up jobs due to placement through the Jobcenters is even larger than overall in basic income support.

To our knowledge, we are the first to tap the potential of this group for an empirical investigation. Therefore, we explore the characteristics in more detail. The group comprises around 60,000 people (again excluding Ukrainian citizens). While this is small relative to the overall labour market, having a full sample of administrative data at our disposal, we can build on a sufficient sample size. Table 1 demonstrates that the structure is comparable to aggregate unemployment in basic income support. There is only a certain overrepresentation of young people, but we will provide formal checks excluding composition effects below. Moreover, in order to ensure that specific labour market developments will affect both groups alike, besides the – practically identical – skill level, the occupational dimension is key. In that regard, we consider the target occupations over 36 two-digit categories in 2022 (obtained from BA Statistics). The correlation of the shares between the two groups amounts to 0.992.

**Table 1: Structure of unemployed in basic income support and of unemployed not entitled to benefits 2022**

Share in	Women	Foreigners	Low-skilled	Young (<25)	Older (>55)	Unmarried
Basic income support	0.451	0.359	0.668	0.076	0.193	0.553
Not entitled to benefits	0.459	0.431	0.677	0.172	0.134	0.558
Δ Difference	-0.005	-0.008	-0.009	-0.009	+0.007	-0.003

Note: Own calculations, without Ukrainian citizens. “Δ Difference”: difference of shares among unemployed in basic income support and unemployed not entitled to benefits, mean 2022:07-2023:09 minus mean 2017:01-2022:06, seasonally adjusted.

Source: BA Statistics, own calculations. © IAB

Figure 4 shows the job finding rate in basic income support relative to the job finding rate of the unemployed not entitled to benefits. Until Covid-19, the ratio is well described by a fixed level plus noise. The economic downturn 2018/19 generated no cyclicity, serial correlation is negligible (with  $Q(3)$   $p$ -value 0.544). Apparently, outside the pandemic<sup>4</sup> (dashed line) the job finding rates of both groups followed common trends. Figure 4 also clarifies that the Court decision at the end of 2019 (after which the sanctions immediately more than halved) was

<sup>4</sup> Besides the state of emergency due to lockdowns, sector and school closures, there were far-reaching ad-hoc adjustments in basic income support during the pandemic. Amongst many other temporary changes in social and labour law, the means test was effectively suspended, housing costs were reimbursed also beyond the existing limits, sanctions were largely suspended, the continued benefit approval was granted automatically and provisional approval of benefits for self-employed was considered binding.

followed by a drop to a lower level. The same level was reached again after the pandemic (and in fact also between the two lockdowns), so common trends continued.

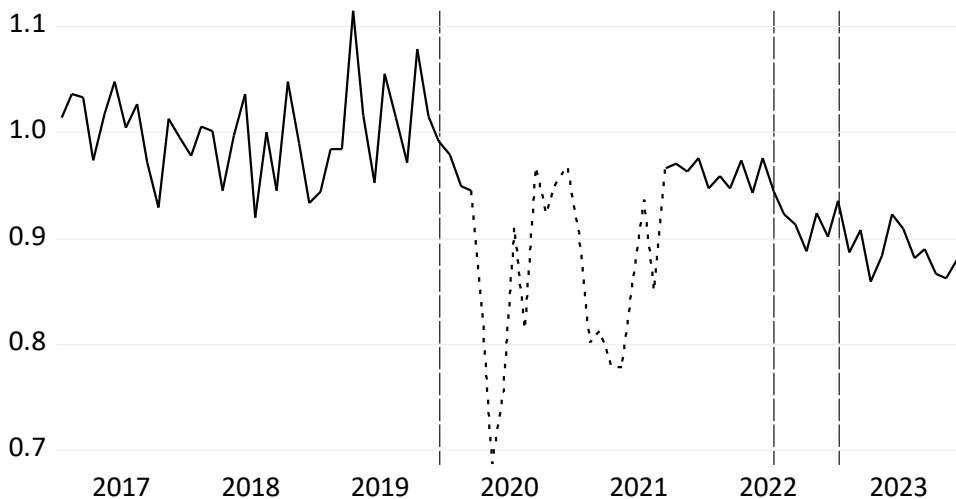
Apparently, this supports the key assumption behind a difference-in-difference approach. The unemployed in basic income support represent the treatment group and the unemployed not entitled to benefits the control group. The outcome is given by the log job finding rates. The common trends assumption states that no differences in the development of the two groups would have occurred without the policy interventions. In addition to the visual inspection, we will provide further checks after the baseline estimation. Furthermore, the difference-in-difference identification relies on the no-interference assumption (Rubin 1978). In that regard, the influence of the treatment group behaviour on the control group will be quantified below.

The treatment dates are 2022:07 and 2023:01, beyond the Court decision (indicator C from 2019:12). Thus, the chronological sequence is as follows:

1. The Court decision is permanently relevant since 2019:12.
2. The moratorium begins in 2022:07 and ends in 2022:12.
3. The reform comes into effect in 2023:01.

Thus, while the moratorium ends with the reform, they both come in addition to the previous change of the sanctions regime. Figure 4 suggests a drop right at the start of the moratorium.<sup>5</sup> After its expiry, with the implementation of the reform the lower level persists and even falls further. .

**Figure 4: Ratio of job finding rate of unemployed in basic income support and job finding rate of unemployed not entitled to benefits**



Note: Own calculations and seasonal adjustment via log-linear month dummies as in (3), without Ukrainian citizens, 2017:01-2019:11=1. Vertical lines mark the Federal Constitutional Court decision, the sanctions moratorium and the citizen's benefit reform.

Source: BA Statistics. © IAB.

<sup>5</sup> Recall that due to the mid-month statistical count, half of July lies before the moratorium. Thus, an immediate reaction corresponds to a halfway drop in July.

For pursuing a difference-in-difference approach, one may use a panel model for the two unemployment groups with group and time fixed effects. Equivalently, we can sum up the evidence by regressing the log-difference of the job finding rates on a constant and the indicators:

$$\ln F_t/U_{t-1} - \ln F_t^{ne}/U_{t-1}^{ne} = c + \gamma_0 C_t + \gamma_1 S_t + \gamma_2 R_t + \delta seas_t + \vartheta X_t + \epsilon_t, \quad (3)$$

where *ne* stands for “not entitled” and *X* holds explanatory variables used below. Again, the sample starts in 2017, excluding the Covid period 2020:04 to 2021:08. While there is no residual autocorrelation in  $\epsilon$  (Q(3) *p*-value 0.402), potential heteroscedasticity (White test *p*-value 0.062) is addressed by the HAC estimator. The moratorium coefficient results as -0.052 (*t*-value -5.79) and the reform coefficient as -0.077 (*t*-value -9.35). The shift following the Court decision amounts to -0.035 (*t*-value -3.85).

In Table 2, we conduct several checks by specifying *X* in (3) in order to corroborate the common-trends assumption. Relevant variables stem from matching function (1) and can be expected to cancel out if they affect both groups alike. First, we add the two vacancy variables from (1), which would be relevant if the groups are influenced differently by the labour market development. However, these are insignificant. The same holds for the time trend from (1). Additionally, stochastic dynamics can be modelled with ARMA-terms in  $\epsilon_t$ , but residual autocorrelation is low. Heterogeneity in the pool of unemployed could affect the job finding rates if the composition changes. Therefore, as e.g. Gehrke/Weber (2018), we control for the shares of older (>55 years), younger (<25 years), female, foreign and low-skilled unemployed (obtained from BA Statistics and seasonally adjusted). Concretely, we add the group differences of these shares to the regression. Again, these turn out to be insignificant. The bottom row in Table 1 shows that the group difference of the shares changed only slightly in the treatment period. Thus, composition effects are not driving the results. We reach the same conclusion when reweighting the job finding rates of the persons in the control group so that they match exactly the shares in the treatment group from Table 1: the regressions results remain virtually identical. We also checked heterogeneity of labour demand by separately including the vacancies in temporary agency employment, which is particularly sensitive to the economic situation, and the vacancies at the low-skilled requirement level particularly relevant for basic income support (cf. Table 1). However, both played no important role empirically. This seems plausible given the similar structure in terms of skill level and occupation of the two unemployment groups. All these results are encouraging in that they support the assumption that the two job finding rates would have continued their common trends without the institutional changes.

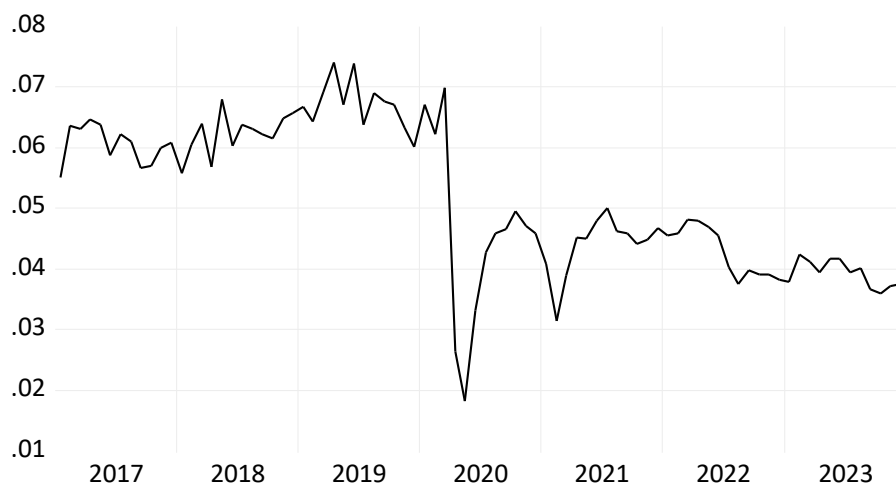
**Table 2: Robustness checks**

Conducted checks	New vacancies	Linear trend	AR(1)	Composition controls	Temp. agency vacancies	Low-skilled vacancies
<i>F</i> -test <i>p</i> -value	0.345	0.941	0.831	0.363	0.422	0.127

Note: OLS regression (3) with additional terms in *X*. New vacancies as in (1), temporary agency and low-skilled vacancies in addition. Composition controls: shares of older (>55 years), younger (<25 years), female, foreign and low-skilled unemployed. Source: BA Statistics, own calculations.

The reform package also contains additional support for training, e.g., financial incentives, implemented in July 2023. In the short run, higher take-up of training measures could, in turn, reduce transitions into jobs. However, there is clear evidence that after such lock-in effects, training increases employment probabilities (e.g., Lechner et al. 2011, Kruppe/Lang 2018). Therefore, in Figure 5 we consider the transition rate of unemployed in basic income support into education and labour market measures. So far, the data show no upward shift. This makes it unlikely that the negative effects on job findings we measure are due to a trade-off against training.

**Figure 5: Transition rate of unemployed in basic income support into education and labour market measures**



Note: Own calculations and seasonal adjustment (ARIMA X-12), without Ukrainian citizens.  
Source: BA Statistics. © IAB.

Moreover, we run placebo tests by adding indicators for pseudo-treatment periods to  $X$  and re-estimating regression (3). The first block in Table 3 treats the time around the start of the moratorium. It shows that there was no pre-treatment effect or trend starting in the months right before July 2022. It also reveals that there was only a single one-time shift exactly at the treatment date. The second block does the same for the time around the introduction of the reform. Again, we find no pre-treatment effects, which also speaks against relevant anticipation effects of the reform. Notably, these would not change the reform coefficient itself, but overestimate the coefficient of the moratorium that was in place immediately before the reform. However, there are no negative shifts building up over time when approaching the introduction of the reform. This could also be due to the timing of the moratorium: Additional behavioural changes in anticipation of a dovish reform become obsolete if breaches of duty are not currently sanctioned anyway. The first months of 2023 require special care also because even after the end of the moratorium, sanctions remained at low levels until March 2023 (Figure 2). To the extent that this was an after-effect of the moratorium, the consequences for job findings during this period may be wrongly picked up by the reform indicator. However, the month dummies are empirically irrelevant. The following two columns of Table 3 show that both a pre-Covid and a post-Covid shift are insignificant. The last column checks an additional shift in July 2023, when



specific parts of the reform (cooperation plans, qualification incentives) became operative. However, the extra effect is practically zero.

**Table 3: Placebo tests with pseudo-treatment periods**

Period	22:05	22:06	22:07	22:08	22:11	22:12	23:01	23:02	23:03	18:07- 22:06	21:09- 22:06	23:07- 23:12
t-value	-0.68	-0.22	0.06	0.00	0.23	0.11	1.04	-0.07	0.56	-0.61	-0.40	-0.01

Note: OLS regression (3) with additional indicators for pseudo-treatment periods in blocks.

Source: BA Statistics, own calculations.

### Spillover effects

Our main concern with the no-interference assumption is that while the unemployed not entitled to benefits were not directly affected by the institutional changes, indirect spillover effects (e.g., Levine 1993) could occur via changing labour market congestion: As long as labour demand is not perfectly elastic, the job chances may increase if the other unemployed search and compete less intensely. While one would correctly estimate the effect on the *difference* of the job finding rates, it would not be entirely attributable to the treatment group (e.g., Lalive et al. 2015).

For assessing such replacement, one can build on a logical extension of the matching function: We formulate equation (1) for the unemployed not entitled to benefits (the control group) and add lagged unemployment in basic income support (the treatment group) as an explanatory variable. This cross-term captures competition effects on the job chances. Since the treated group is large relative to the control group, such an approach at the aggregate level has good chances to detect a spillover with sufficient precision (compare Cahuc et al. 2022). Estimating the matching function again since 2017, we get a cross effect of -0.408 (t-value -2.01). A concern may be that the matching of both groups of unemployed depends on common latent factors. Then, higher unemployment in the cross-term would also pick up worsened general matching conditions and thus be negatively correlated with job findings. Consequently, the coefficient of the cross-term would overestimate the pure replacement effect. One can investigate that with an approximation, using  $\varepsilon_t$  as a conventional measure of time-varying matching efficiency: we obtain the residual from (1) for basic income support and include it as an estimate of a latent factor into the matching function for the control group. Then, the coefficient of the cross-term shrinks (in absolute value) to -0.341. Using the first principal component of both residuals as a factor gives nearly the same result.

This spillover effect is larger than what, e.g., Lalive et al. (2015) find for an unemployment insurance extension in Austria. This is plausible given that in our case a small control group faces competition from a much larger treatment group. In sum, for every percent the job finding rate of the treatment group falls due to the institutional changes, that of the control group would rise by about 0.34 percent. Then, the estimated indicator coefficients from (3) represent 1.34 times the effects on the treatment group. It follows that the latter amount to -0.040 for the moratorium and -0.057 for the reform. These effects come in addition to the shift after the Court decision of -0.026. The reform effect corresponds to the estimate from the matching function (1) for the reform indicator, -0.087, less the refugee effect estimated in (2), -0.03.

To identify replacement via the treatment, one would need experimental variation or specific labour markets where the institutional changes did not apply (compare Crépon et al. 2013). This is not the case here. However, in order to underpin the existence of replacement effects, we exploit the limited variation in regional data. The idea is that if treated unemployed reduce their search efforts, the job findings of unemployed not entitled to benefits will develop the better in the treatment period the more exposed they were to competition from the treatment group in their region. This exposure is measured by the regional ratio of the number of unemployed in basic income support and the unemployed not entitled to benefits for the last pre-treatment month 2022:06 (seasonally adjusted). The average ratio is 22.9. The panel model for the 50 labour market regions reads:

$$\ln F_{it}^{ne}/U_{it-1}^{ne} - \ln F_{it}/U_{it-1} = c_0 + \gamma D_t E_i + c_1 \theta_i + c_2 \mu_t + \varepsilon_{it} , \quad (4)$$

where  $D$  is a treatment dummy for the period from 2022:07 onwards and exposure  $E$  represents the continuous treatment variable. In (4), we focus on the difference with the job finding rate of the unemployed in basic income support in order to ensure that any time-varying regional distinctions cancel out.<sup>6</sup> Accordingly, variables such as the regional vacancy inflow or the share of Ukrainian citizens used in (2) turn out to be clearly insignificant. The model is estimated since 2021:09 with the regional number of unemployed as weights. The coefficient  $\gamma$  of the interaction term results as 0.002 ( $t$ -value 2.53). Thus, an additional unit in the ratio increased the job finding rate in the treatment period by 0.2 percent. This verifies significant replacement effects in the treatment period. Of course, the regional variation is not sufficient to tell how the outcome would differ without any replacement.

Finally, we make use of the regional data for an additional check of the control group approach. On that account, we re-estimate equation (3) as a panel with region fixed effects. The coefficients  $\gamma_0$ ,  $\gamma_1$  and  $\gamma_2$  are very close to the estimates above (-0.036, -0.051, -0.077). The fact that effects within the regions and in the aggregate coincide excludes the possibility that differences in regional distribution of the two unemployment groups drive the results.

## 4 Conclusion

This paper is the first to investigate the consequences of the recent citizen's benefit reform in Germany and a previous sanctions moratorium for job findings. For dovish labour market policies, we can draw several conclusions.

First, an almost complete suspension of sanctions has stronger effects than restricting their extent (as in the Court decision), which seems plausible. The moratorium effect means that four out of a hundred jobs were no longer taken up every month. Second, from this estimate one can infer relevant ex-ante effects of sanctions: if one assumes, according to treatment effects measured in the literature (e.g. van den Berg et al. 2022), that a job finding rate of initially 2.7

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<sup>6</sup> This is equivalent to pooling the two unemployment groups and estimating a triple interaction effect of the treatment dummy, exposure and a group dummy, while controlling for region-time, group-time and group-region fixed effects.

percent (the basic level in Figure 1) is halved in 20,000 cases (the decline in Figure 2), the rate calculated for all 1.6 million unemployed in basic income support is reduced by 0.7 percent. This is significantly less than the estimated moratorium effect: for the aggregate, the job finding rate of the sanctioned people themselves is of limited importance. Thus, we support the hypothesis that the mere possibility of sanctions influences behaviour (cf. Arni et al. 2022). Evidently, such effects pertain to a much larger group than actual sanctions.

While these estimates exploit a unique policy shift, effects of a six-month moratorium would not necessarily carry over to a permanent regime without sanctions. Abolishing sanctions altogether could change the character of benefit systems and the behaviour of individuals beyond what can be observed during a moratorium as a temporary intervention in an existing environment. For instance, according to Collischon et al. (2023), 44 percent of those in basic income support were not aware of the moratorium. To the extent that this would change in a permanent regime, the effects on job findings would be stronger.

Third, in the short run we establish clear negative effects of the dovish benefit reform on job findings, too. Indeed, it consists of multiple components all inclined to potentially put a drag on job findings. As for sanctions, while they picked up after the moratorium, the new regime was less strict than before: The sanctions due to job rejection or neglect of integration agreement, i.e., most directly connected to the job finding margin, more than halved (-56 percent for June-September 2023 compared to the first half of 2022) – whereas the most common reason for sanctions is given by missed appointments. The average benefit reduction due to the sanctions decreased substantially, too (-43 percent). By the same token, the stock of existing sanctions did not recover (-57 percent) because new sanctions became considerably shorter – usually one month.

The dovish reform effect does not explain the majority of the recent job finding weakness in basic income support, but is still sizeable: the negative estimate of almost six percent amounts to about half of the positive effects of more than ten percent typically found in the literature for the Hartz reforms (Fahr/Sunde 2009, Klinger/Rothe 2012, Klinger/Weber 2016). Indeed, the hawkish reforms – including the abolishment of income-dependent social assistance, lower social benefits, shorter unemployment insurance claims, stricter sanctions and definition of reasonable work, mandatory integration agreements, reorganisation of placement services, and deregulation of labour law – were still more comprehensive than the dovish turnaround.

However, when comparing the dovish reform to the situation before the Federal Constitutional Court restricted the sanctions regime from the Hartz reforms, job take-up is dampened in sum by 8.3 percent. Of course, the reform does not bring the labour market more than halfway back to the 2000s, due to higher tightness and lower separation rates (Klinger/Weber 2016, Hutter et al. 2022, Hartung et al. 2022). But still, the effect stands for a plus of about 0.9 percentage points in the steady-state unemployment rate.<sup>7</sup> We conclude that in order to contain prevalent hysteresis risks, it is important to strengthen job findings again. On the other hand, with increasing pressure, people are more likely to accept jobs with little prospects and low pay (Arni et al. 2022, Wolf 2024) or to turn away from job placement altogether if there is severe interference with their

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<sup>7</sup> Calculated as  $u = \frac{s}{s+f}$ , calibrating the job finding rate  $f = 0.027$  as above and the separation rate  $s = 0.0035$  (generated from the inflow into unemployment in basic income support excluding education and labour market policy measures).

living conditions. Comparable effects on job quality are known for unemployment insurance, e.g. van den Berg/Vikström (2014) and Nekoei/Weber (2017). Moreover, a significant part of the effect of the Hartz reforms was based on a trade-off between quantity and quality of jobs (Gartner et al. 2023).

In this context, our study complements the literature on hawkish structural reform effects by analysing the effects of a dovish turnaround on the job finding margin. The results demonstrate that the opportunity costs of less strict regulations persist also in times when mass unemployment has given way to labour shortages. This represents a key challenge for many welfare states. Indeed, it underlines the necessity to design benefit schemes combining stronger job take-up with a focus on training and professional development. Such a strategy could involve better financial incentives for expanding jobs, supported by starting subsidies (OECD 2023, Bruckmeier et al. 2021, Weber 2023), options for longer – instead of higher – sanctions (van den Berg et al. 2022), which can be lifted at any time if cooperation is resumed, inflation adjustment of benefits, which promptly compensates for losses in purchasing power but avoids disproportionate increases (Weber 2023), investments in case management capacity to address individual reasons for unemployment (Beste et al. 2023, Hainmueller et al. 2016), as well as dual qualification that brings together work experience and training. In this regard, short-term budget cuts may run the risk of jeopardising the achievement of longer-term reform goals.

Accordingly, augmenting the job finding results of the underlying paper, future research should consider the job quality dimension of the citizen's benefit reform once relevant data is available. Due to decreased pressure on job seekers, hiring wages and job tenure might have improved. However, separations could also have increased as a result of a lower willingness to compromise (Hartung et al. 2023) and a lower propensity to retain workers in a less tight labour market (Klinger/Weber 2020). Further analyses in the medium term are important because the reform goals of higher job quality and sustainability may only be achieved over time. The full reform effect may not have materialised within the first year. Nevertheless, the transitions into education and labour market measures have not yet risen, hysteresis is likely to increase as unemployment lengthens, and matching frictions will become more significant in economically stronger times. Moreover, while we focused on direct effects on matching efficiency, feedback on job creation could be evaluated (Krause/Uhlig 2012, Hochmuth et al. 2021, Hutter et al. 2022). Besides the unemployed, additional analyses could address the effects on the labour market outcomes of further recipients of basic income support, just as the risk of dropping out of the labour market. Finally, detailed evaluations shall address the single components of the citizen's benefit reform.

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