Appendix

**Table 1: Amount of sanctions, by reasons and number of sanctions**

Reasons for a violation of duty: insufficient effort to find a job or continue a job and reduction of income or wealth, wasteful behaviour

Reasons for an omission of report: recipient does not show up for an appointment at the agency or misses checkups

For a detailed description of reasons for sanctions see §31, §32 SGB II

(1) Valid from April 2011; until March 2007 a 20% reduction was implemented.

The duration of a sanction is three months. For recipients under the age of 25 the sanction can be restricted to a six-weeks period.

|  |  |  |  |
| --- | --- | --- | --- |
| **Reasons of the sanction** | **Amount of sanction (first sanction)** | **Amount of sanction (second sanction in a one year period)** | **Amount of sanction (more than two sanctions in a one year period)** |
| Violation of duty, if recipient is 25 years or older | 30% of individual standard benefit | 60% of individual standard benefit | 100% of individual basic income support |
| Violation of duty, if recipient is under 25 years old | 100% of individual standard benefit; shelter and heating are still payed  | 100% of individual basic income support  | 100% of individual basic income support |
| Omission of report | 10% of individual standard benefit | 10%(1) of individual standard benefit | 10%(1) of individual standard benefit |

**Table 2: Covariate balancing: mean differences, standardized percentage bias, significance tests before and after matching**

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
|  |  | Treated | Control | % Bias | p>|t| |
| **Female** (Ref. Male) | Before | 0.471 | 0.558 | -16.9 | 0.005 |
|  | After | 0.471 | 0.519 | --9.0 | 0.281 |
| **Age from 25 onwards** (Ref. under 25) | Before | 0.875 | 0.912 | -11.7 | 0.035 |
|  | After | 0.875 | 0.900 | -7.2 | 0.402 |
| **Household size** (number) | Before | 2.602 | 2.292 | 19.4 | 0.000 |
|  | After | 2.602 | 2.464 | 8.6 | 0.325 |
| **Debts** (Ref.no debts) |  |  |  |  |  |
|  Less than 1,000 € | Before | 0.135 | 0.132 | 0.9 | 0.878 |
|  | After | 0.135 | 0.131 | 1.2 | 0.883 |
|  1,000 to less than 2,500€ | Before | 0.727 | 0.075 | -0.7 | 0.902 |
|  | After | 0.727 | 0.072 | 0.4 | 0.964 |
|  2,500 to less than 5,000€ | Before | 0.107 | 0.062 | 16.3 | 0.002 |
|  | After | 0.107 | 0.093 | 5.0 | 0.582 |
|  5,000 to less than 10,000€ | Before | 0.062 | 0.059 | 1.3 | 0.828 |
|  | After | 0.062 | 0.066 | -1.6 | 0.853 |
|  10,000 to less than 20,000€ | Before | 0.066 | 0.046 | 8.8 | 0.111 |
|  | After | 0.066 | 0.054 | 5.1 | 0.554 |
|  20,000 to less than 50,000€ | Before | 0.069 | 0.043 | 11.4 | 0.032 |
|  | After | 0.069 | 0.061 | 3.6 | 0.688 |
|  50,000€ and more | Before | 0.017 | 0.038 | -12.5 | 0.071 |
|  | After | 0.017 | 0.025 | -4.8 | 0.511 |
| **Savings** (Ref. No savings) |  |  |  |  |  |
|  Less than 1,000€ | Before | 0.228 | 0.299 | -16.0 | 0.010 |
|  | After | 0.228 | 0.255 | -6.2 | 0.448 |
|  1,000€ to less than 2,500€ | Before | 0.031 | 0.048 | -8.6 | 0.189 |
|  | After | 0.031 | 0.037 | -3.2 | 0.681 |
|  2,500€ and more | Before | 0.014 | 0.061 | -24.9 | 0.001 |
|  | After | 0.014 | 0.029 | -8.0 | 0.213 |
| **Child under 4** (Ref. no child under 4) | Before | 0.135 | 0.096 | 12.1 | 0.030 |
|  | After | 0.135 | 0.120 | 4.9 | 0.573 |
| **Partner** (Ref. no partner) | Before | 0.253 | 0.334 | -18.0 | 0.004 |
|  | After | 0.253 | 0.276 | -5.1 | 0.529 |
| **Immigrant background** (Ref. no immigrant background) | Before | 0.245 | 0.266 | -4.8 | 0.433 |
| After | 0.245 | 0.251 | -1.2 | 0.880 |
| **Mental Problems** (Ref. no at all) |  |  |  |  |
|  A little bit | Before | 0.194 | 0.212 | -6.9 | 0.257 |
|  | After | 0.194 | 0.203 | -2.2 | 0.788 |
|  Moderately | Before | 0.190 | 0.189 | 1.4 | 0.757 |
|  | After | 0.190 | 0.198 | 0.4 | 0.945 |
|  Quite a bit | Before | 0.125 | 0.133 | -2.4 | 0.691 |
|  | After | 0.125 | 0.125 | -0.2 | 0.981 |
|  Extremely | Before | 0.125 | 0.097 | 8.9 | 0.118 |
|  | After | 0.125 | 0.116 | 2.8 | 0.749 |
| **Single parent** (Ref. no single parent) | Before | 0.035 | 0.126 | -34.1 | 0.000 |
|  | After | 0.035 | 0.043 | -3.0 | 0.615 |
| **Duration of UB II receipt** (Ref: under two years) | Before | 0.384 | 0.348 | -7.5 | 0.208 |
|  between two and four years  | After | 0.384 | 0.447 | -13.0 | 0.126 |
|  over four years | Before | 0.522 | 0.558 | -7.1 | 0.234 |
|  | After | 0.522 | 0.450 | 14.6 | 0.079 |
| **Doctor visits** (number,last 3 months) | Before | 3.225 | 3.425 | -3.2 | 0.559 |
|  | After | 3.225 | 3.300 | -1.2 | 0.886 |
| **Number of close friends** (number) | Before | 5.163 | 5.210 | -1.3 | 0.822 |
|  | After | 5.163 | 5.084 | 2.2 | 0.796 |
| **Unemployment rate** (county level) | Before | 8.798 | 8.831 | -1.0 | 0.857 |
|  | After | 8.798 | 8.871 | -2.3 | 0.786 |
| **Wave** | Before | 3.980 | 5.843 | -89.5 | 0.000 |
|  | After | 3.980 | 3.979 | 0.0 | 1.000 |
| **School Education** (Ref. No school degree) |  |  |  |  |  |
|  Lower secondary education | Before | 0.021 | 0.017 | 2.6 | 0.650 |
|  | After | 0.021 | 0.019 | 1.4 | 0.865 |
|  Secondary Education | Before | 0.761 | 0.729 | 7.4 | 0.224 |
|  | After | 0.761 | 0.753 | 2.0 | 0.811 |
|  Higher Education | Before | 0.093 | 0.159 | -19.8 | 0.003 |
|  | After | 0.093 | 0.116 | -6.7 | 0.382 |
| **Professional Qualification** (Ref. No degree) |  |  |  |  |  |
|  Vocational Education | Before | 0.446 | 0.580 | -26.9 | 0.000 |
|  | After | 0.446 | 0.498 | -10.5 | 0.211 |
|  Tertiary Education | Before | 0.042 | 0.080 | -16.1 | 0.017 |
|  | After | 0.042 | 0.056 | -6.1 | 0.421 |

**Table 3: The goods and activities included in the deprivation index**

|  |  |
| --- | --- |
| Deprivation Index | Including |
|  | Habitation: Sufficient number of rooms, no clammy walls or floors inside, bathroom inside, toilet inside, garden/balcony/terraceFood/clothing: Sufficient number of winter clothing, buy new clothing now and then, one warm meal per dayConsumption: car, tv, dvd-player, computer with internet, washing machineFinance: Save a fixed amount each month, replace old furniture, possibility to pay for: unexpected spending, over-the-counter medicine, rent on time, Gas/water/electricity on timeSocial Participation: A week-long holiday each year, (invite friends for a meal, go to restaurant, go to cinema/theatre/concert once a month) |

Source: PASS, see Berg et al. (2019)

; N (controls) =11946, N (treated)=484

**Figure 1: The percentage of deprived goods for non-sanctioned and sanctioned benefit recipients**

Source: PASS and process data from the German Employment Agency 2006-2017, own calculations; Notes: bootstrapped standard errors, 100 replications; N (controls) =7662, N (treated) =289; deprivation index includes 23 goods and activities

; N (controls) =11946, N (treated)=484

**Figure 2: The level of the equivalence-weighted net household income for non-sanctioned and sanctioned benefit recipients**

**recipients**

Source: PASS and process data from the German Employment Agency 2006-2017, own calculations; Notes: bootstrapped standard errors, 100 replications; N (controls) =7662, N (treated) =289

; N (controls) =11946, N (treated)=484

**Figure 3: The effect of sanctions on the number of deprived goods by household composition**

Source: PASS and process data from the German Employment Agency 2006-2017, own calculations; Notes: bootstrapped standard errors, 100 replications; All households: N (controls) =7662, N (treated) =289; Single Households: N (controls) =3926, N (treated) =125; Single Parent: : N (controls) =1683, N (treated) =88

; N (controls) =11946, N (treated)=484

## Method

Difference-in-Differences (DiD) - Propensity Score Matching (PSM) combines the strengths of two approaches and can account for selection on observables (PSM) and unobserved time-constant individual heterogeneity (DiD; (Heckman *et al.,* 1997)).[[1]](#endnote-1) The DiD can be explained within a potential outcome framework that distinguishes a treatment group (*D=1*) that experiences a sanction and a control group (*D=0*) that does not. Both groups need to be at risk of experiencing a sanction; therefore, only basic income support recipients are under investigation. For both groups, two potential outcomes are defined$ (Y^{0 }, Y^{1}$) at each time point, but only one outcome is observed, whereas the other outcome remains an unobserved counterfactual. The effect of sanctioning is identified by comparing the change in deprivation $E\left(Y\_{t+1}^{1}- Y\_{t}^{0 }\right|D=1)$of the treatment group between periods *t* and *t+1* to the counterfactual trend in deprivation $E(Y\_{t+1}^{0}- Y\_{t}^{0}|D=1)$ they would have experienced if no treatment had taken place. This counterfactual trend is approximated by the actual change in deprivation $E\left(Y\_{t+1}^{0}-Y\_{t}^{0}\right|D=0)$ of the control group according to the crucial “common trend assumption” $E\left(D=1\right)= E(Y\_{t+1}^{0}-Y\_{t}^{0}|D=0)$. Unobserved individual fixed effects are eliminated. An advantage, compared to a simple fixed-effects estimator, is that the between-comparison with the trend of a control group additionally removes any common period effect that affects the treatment and control group in identical ways; any ageing effects are also removed. The common trend assumption is essential for an unbiased DiD estimator. With the available data, we can test the common trend assumption from t-1 to t0.[[2]](#endnote-2) The test shows that the common trend assumption is fulfilled (Figure 3).[[3]](#endnote-3) The second key assumption is SUTVA (the [potential outcome] observation on one unit should be unaffected by the particular assignment of treatments to the other units). Since we use a random sample of independent households, we do not expect the sanction of a person in a household to affect the deprivation of untreated households.

To reduce heterogeneity and to create more similar control and treatment groups before implementing DiD, a PSM is performed. The one-dimensional propensity score P(D=1|X) measures the probability of being sanctioned (making the sanction transition versus not) conditional on a high dimensional vector of control variable X. The construction of “statistical twins” ensures that as similar treatment and control groups as possible exist before calculating the DiD estimator. We use the possibility of PSM to include variables that reduce not only individual heterogeneity but also heterogeneity at the household level.[[4]](#endnote-4) The calculation of the propensity score is the first step of PSM and is based on a logistic regression explaining the determinants of a transition into a sanction. The common support condition of the PSM guarantees that only persons with suitable control cases are considered. After calculating the propensity score, PSM algorithms form “statistical twins” that have similar propensity scores.[[5]](#endnote-5) After comparing the algorithms available, we decided on a epanechnikov kernel matching with a 0.06 bandwidth, restricting on the cases under common support by dropping treatment observations whose pscore is higher than the maximum or less than the minimum pscore of the controls, because it showed the best balancing between the treatment and control groups (Caliendo and Kopeinig, 2008). To test balancing, we conduct t-tests and compare standardized biases (Table 2 in the appendix). We tested several different combinations and measurements of our matching variables and chose the model that minimizes the mean standardized bias across the entire sample. We were able to reduce the mean standardized bias substantially to a level of 2.1%. Although not all variables[[6]](#endnote-6) meet the boundary value of a standardized bias reduction below 5%, the t-tests for all variables examined show no significant differences between the control and treatment groups after matching.

The changes in the level of deprivation of the treated and matched controls are then compared to estimate the average treatment effect on the treated (ATT), which is the deprivation effect of being sanctioned on those who actually were sanctioned.

$$ATT^{DID-PSM}= \frac{1}{N\_{D\_{1}}}\sum\_{i\in D\_{1∩S}}^{}\left[\left(Y\_{i,t+1}^{1}-Y\_{i,t}^{0}\right)-\sum\_{j\in D\_{0∩S}}^{}W\_{ij}\left(Y\_{j,t+1}^{0}- Y\_{j,t}^{0}\right)\right]$$

*D1 (D0)* represents the treatment (control) group, and S represents the area of common covariate support.

1. The description of the method is based on (Gebel and Voßemer, 2014). [↑](#endnote-ref-1)
2. Unfortunately, it is not possible to use more pretreatment waves to test the common trend assumption because the PASS survey data limits our case numbers. More than one pretreatment wave is not feasible because the number of cases decreases too much. The sharp decrease in the number of cases is because sanctions have been imposed less frequently in recent years and that more successive waves need to be filled, which is problematic due to panel attrition. For example, the number of cases treated decreases from 484 to 289 if we include t-1, compared to the case where we only analyse the change from t0 to t1. [↑](#endnote-ref-2)
3. We test the common trend assumption on the basis of the matched sample to ensure that the number of cases is constant and the common trend assumption is correct for the sample used later. [↑](#endnote-ref-3)
4. See Measures section. [↑](#endnote-ref-4)
5. The PSM analyses are conducted using the Stata ado psmatch2 (Leuven and Sianesi, 2003). [↑](#endnote-ref-5)
6. Sex, age, size of household, partner, Duration of UB II receipt do not meet the boundary value of a standardized bias reduction below 5%. [↑](#endnote-ref-6)