# Job Search and the Threat of Unemployment Benefit Sanctions

Thomas Walsh *

European University Institute

Job Market Paper. January 10, 2023
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### Abstract

How does the threat of punishment in the unemployment insurance system affect job search behaviours and subsequent labour market outcomes? This paper uses a difference-in-differences design, leveraging the differential response of districts to sanctioning policy reform in the United Kingdom during the early 2010s to examine the impact of unemployment benefit sanctioning *threat* on jobseeker exit from unemployment and future outcomes. Using working life histories constructed from panel survey data, results show that average district sanctioning rate increases exit speed from unemployment, driven mostly by transitions into employment. Treated districts experience more churn in the labour market – the unemployed start to experience more cumulative spells of unemployment compared to the control, and the probability that the next spell of continuous employment reaches one, two and three years falls. Overall, causal estimates are consistent with workers substituting market insurance for missing social insurance.

**Keywords:** Unemployment Insurance, Sanctions, Job Search Behaviour

**JEL classification codes:** J08, J64, J65

# 1 Introduction

Unemployment Insurance (UI) sanctions, partial or complete stops to transfer payments for noncompliance with UI conditions, can be a beneficial addition to the policymaker's active labour market policy toolkit. The social safety net can provide vital consumption smoothing while jobseekers search for their next job, but at the same time mitigate problems associated with moral hazard and low search effort, boosting labour supply. Sanctions can also help reduce loss of skills and human capital if they deter slow exits. However sanctioning policies can also come at a cost. Jobseekers can self-insure against reductions in UI not only along the offer-arrival margin (how hard they search for job offers) but also along the offer-acceptance margin – creating worse matches.

Sanctions affect the search behaviour of not just the small subset of punished jobseekers through their tighter budget constraint, but on the much larger group of all job seekers who fear future reductions in their transfers. This second group is potentially many times larger than the directly sanctioned. The deterrent effect on all those at risk of a sanction remains relatively understudied. Examining only the behaviour and outcomes of the directly sanctioned may give a misleading underestimate of the total sanction effect given the much broader population who are affected by the threat effect. Therefore, a better understanding of this indirect channel can play an important role in the evaluation and design of UI systems.

In this paper I examine how the threat effect of unemployment insurance sanctions affects job search behaviour and the consequences for matches formed. In particular, I investigate how the speed of exit from unemployment responds to average sanctioning rates within a jobseeker's local district. I also examine sanctioning threat on the types of transitions (into employment, into inactivity, into retirement), as well as stability of matches formed captured by cumulative unemployment spells of the pool of unemployed over time and probability that reemployment spells reach certain tenure milestones.

My empirical approach focuses on a national policy reform which induces heterogeneous increases in district-level sanctioning rates. Per claimant sanctioning rates vary significantly across districts and through time. Cross-sectional dispersion increases substantially following

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sanction system reform in the United Kingdom in the early 2010s. This setting lends itself to a Difference-in-Differences (DID) identification scheme, exploiting the *differential intensity response* of districts to centralised sanction reform in 2012. Figure 1 below reports sanctioning rates pooling districts based on their post-reform severity in sanctioning rates. The reforms saw increases in sanctioning activity both at the extensive margins (number of sanctions issued or number of individuals sanctioned) as well as the intensive margin (for a given infraction, the duration of transfer payment suspension typically increased).

The key contributions of this paper are the focus on the indirect threat channel of sanctions on jobkseeker outcomes, as well as to employ a novel identification strategy relying on longitudinal variation in sanction intensity in a jobseeker's local area.

My empirical work first documents a strong pattern of displacement earnings losses based on the intensity of local sanctioning a person experienced in the early months of their unemployment spell. Losses for those who experience above average sanctioning threat in the first 3 months experience income losses of around 20 percent compared to pre-displacement earnings, even after 5 years, while for the low threat group, losses are statistically indistinguishable from zero after 3 years. I show that these earnings losses are not associated with differential employment patterns, however high early-spell sanction threat is associated with higher search upon displacement.

These stylised facts are based only in correlations and motivate more rigourous causal examination of the data at higher frequency. My DID hazard regressions show that jobseekers in treated districts have on average almost 20 percent higher probability to exit unemployment in a given month (of 4.5 percent exit hazard at baseline), and this is nearly entirely driven by faster exits into employment. Exits into inactivity are rare events so results lack precision. In terms of stability of future employment, treated districts see a differential increase in *churning*, the average number of unemployment spells of jobseekers in treated districts increases substantially relative to the control group, while pre-reform they are indistinguishable in terms of unemployment history trends. The probability of achieving a continuous period of employment of medium duration (12, 24, 36 months) also falls by up to 9 percent in treated districts.

In combination these stylised facts and causal evidence point towards the use of offer arrival *and* offer acceptance margins of insurance – workers exit unemployment faster, but create

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worse, less stable matches, resulting is more frequent returns to unemployment. Typically when "toughening up" the sanctioning regime policymakers cite reasons of encouraging higher labour supply and "back to work" rhetoric after recessions, or fiscal cost-saving motives. My findings suggest such measures may in the end backfire.

I build a random search partial equilibrium model to examine the direct and indirect effects of sanctions on endogenous offer arrivals and acceptances, and the role each plays in medium-run earnings losses as seen in the data, and examine whether model implied dynamics can match the data.

**Related Literature**   This paper makes contributes to our understanding of unemployment benefit sanctions along several dimensions. Firstly, while there exists a large body of work examining the effects of sanctions on the sanctioned, not all of these examine the indirect or total effect of sanctioning on search behaviour. The threat effect is shown to be responsible for the majority of the total effect of sanctions in recent work. My work is complementary to these findings by approaching the same broad research question with an alternate identification scheme, and focusing on the indirect channel of sanctions.

The period of welfare reform in the UK has so far not been examined in the applied search literature. The UK experience is particularly important for its *severity*, *scale* and *state*. Toughening up the sanction reform is particularly likely to have bite in this context. The punishments in the sanctioning system were at the upper end of severity amongst studied countries (Austria, Germany, Netherlands, Sweden, Switzerland), and the UK already started from an extremely low base replacement rate. Figure (9) plots raw UI replacement rates as a percentage of average earnings in 2010, as well as a more broadly-defined replacement rate to account for other welfare spending such as housing assistance. Even with the wider definition of social insurance the UK ranks third last at 38 percent, behind OECD average of 57 percent.

Finally, the UK case is important in that the policy reform took place during a period in which the aggregate state of the economy was particularly poor, at the height of the fallout from the global financial crisis, and so this work can potentially talk to effects of UI sanctions in slack labour markets in which higher search effort may not generate many more job offers since

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vacancies are scarce in general. Indeed, in a frictional labour market, it is not obvious that the Pareto planner would want to induce higher search effort. When all workers search harder the total effect can be to simply congest the market more, or in the worst case, if many applications are from jobseekers in many queues simultaneously, firms will internalise the fact that most of their queue is in fact a "phantom queue".

**Direct Effect** A large literature examines the effect of sanctions on sanctioned sanctioned individuals. Worries about individual selection into sanctioning are a major concern in this literature. Many works in this literature rely on the "timing-of-events" identification scheme of Abbring and Van den Berg (2003). Existing work typically finds job seekers exit unemployment faster after having their UI cut (Van den Berg, Van der Klaauw and Van Ours (2004); Boockmann, Thomsen and Walter (2014)). Lalive, Van Ours and Zweimüller (2006) are able to study both the effect of sanction imposition on the sanctioned individual, and the advanced warning of an impending sanction on specific job seekers. The effect on reentry wages and job stability is less clear: Arni, Lalive and Van Ours (2013); Arni and Schiprowski (2019) look at both outcomes, and find reentry stability effects but no wage effects.

**Indirect Effect** Lombardi (2019) uses a policy reform in Sweden in 2013 as the basis for a natural experiment to examine not only the direct channel of sanctioning, but also the threat channel. He finds that the majority of the aggregate effect can be attributed to sanctioning threat. Boockmann et al. (2014) use what the literature has come to call a "judge fixed effect" or "leniency design". The individual sanctioning rate is instrumented with that of local employment office. The Intention-to-treat (ITT) estimate in this case would capture the idea of a background threat rate based on location. Delaney, Boyce, Daly, Mitchell and Moro (2020); Williams (2021) examine the relation between sanctioning threat rates and mental health and subjective well-being, finding a negative correlation between the two.

Relevant to the discussion of the efficacy of active labour market polcies and alternatives on the policymaker's menu of options, Belot, Kircher and Muller (2019) show that the simple intervention of showing UK job seekers past transitions made by similar job seekers can improve

the search breadth and interview prospects for the narrow searchers. This work suggests sanction pressure to exert more effort might not expand the breadth of applications if jobseeker beliefs or habits are constraining search choices.

**Roadmap**  in the next section I outline the broad features of the reform under consideration in this paper. Section 3 presents the datasets used and I outline variable construction and highlight some summary statistics. Section 4 presents stylised facts on job displacement earnings losses and sanction experience in the early phases of unemployment. In Section 5 the empirical framework and identification strategy are presented. Section 6 documents the main results of the paper with discussion and a battery of robustness checks, Section 7 builds a structural model to match facts and causal estimates. Section 8 concludes the paper.

## 2  Policy Environment and Reform

In May 2010 the UK elected a new Conservative-Liberal Democrat government, with a pro-austerity agenda and a focus on debt management by way of spending reduction. The fiscal budget of March 2012 outlined significant changes to the UI system, with an explicit view to "making work pay".

The new sanctioning regime was implemented at latest by October 2012, however the data shows administrative reaction to the announcement almost immediately, suggesting this was an implementation deadline rather than a starting date. Throughout the period under examination a Job Seekers Allowance[1] (JSA) sanction was a complete suspension of unemployment insurance transfer payments for a fixed period. While hardship funds are available to provide a subsistence level of household consumption, it is not clear whether households realise they are entitled to such funds if sanctioned.

JSA for a single-person household remained almost constant over the period in real terms (Rutherford (2013), Department for Work and Pensions Abstract of Statistics 2018, table [2]). Figure

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[1]Job Seeker's Allowance is the name given to unemployment insurance transfer payments in the UK welfare system

[2]https://www.gov.uk/government/statistics/abstract-of-statistics-2018

([8](#)) provides more detail on the evolution of JSA payments. Over the period 2009-2015 the average real JSA level is 77.23 GBP per week in 2018 prices for a single adult (69.35 nominal) and represents 11.4% of mean weekly earnings and 13.65 % of median weekly earnings. Excluding other social transfers, this is the lowest UI replacement rate in the OECD, and including other social assistance and housing asstiance this rises to third-last, ahead of only Greece (experiencing -5.5 percent GDP growth and the start of the Euro debt crisis) and Australia (which was largely insulated from the global financial crisis).

The 2012 reform increased the severity of the sanctioning regime in both the extensive (number of sanctions issued) and intensive margins (a given infraction entails a longer period of UI suspension). Table ([1](#)) details example reasons for a sanction and respective duration of sanction before and after the reform. Figure ([10](#)) breaks down total sanctions by reason (job search, advisor meeting, other).

While individual-level sanction data is not released by the UK DWP, estimates put average income loss due to a sanction at approximately £520 at current prices for the post-reform period 2013-14, with an average duration of around 8 weeks ([Tinson](#) ([2015](#))). In my dataset (outlined below) average pre-displacement earnings fall in the range of around 1500 per month.

# 3 Data and Descriptive Statistics

## 3.1 Data Sources

**UI Sanctions**  Sanctiong rates by Local Authority District (LAD) are taken directly from the UK Department for Work and Pensions (DWP) StatXplore portal. Data covers the period 2005 onwards, at the monthly frequency, listing total referrals for sanctions, sanctions imposed (punishments actually enacted), and number of individuals sanctioned.

**Individual-level Panel Data**  Individual and household level variables are taken from the nationally representative UK Household Longitudinal Survey (UKHLS, "Understanding Society"). Around 40,000 Households from across the UK are surveyed on average once per year. Fieldwork

**Figure 1:** The Effect of Reform on Sanctioning rate for high and low intensity districts
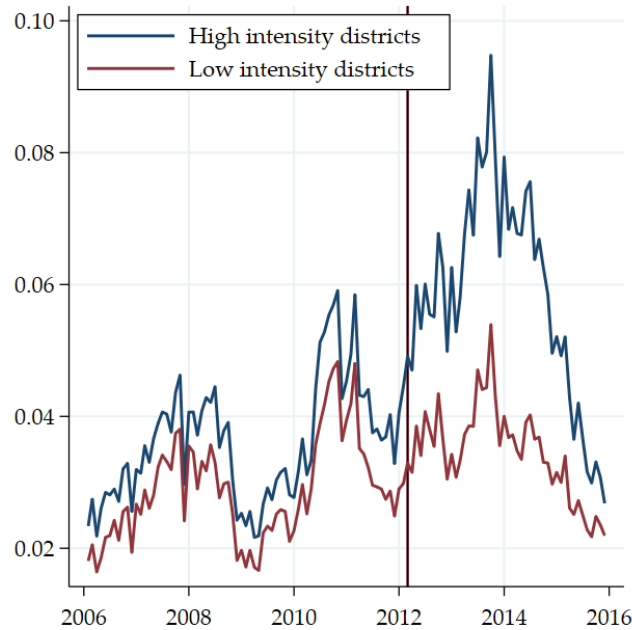


*Note:* Sanctioning rate is defined as the number of adverse sanctions imposed per JSA claimant, by district. Districts are pooled into two groups of high and low intensity districts, corresponding to the first and fourth quartile of post-reform sanction rates.

is completed in- person by trained interviewers as well as by online self-completion. Households are questioned throughout the year, so one can potentially create an unbalanced panel with a frequency higher than the balanced annual frequency.

The panel structure allows us to follow individuals trajectories through time, key for econometric identification of causal effect of sanctioning threat. In particular survey questions elicit respondents' employment status, job characteristics, and income streams. Especially important to this work are the survey modules eliciting respondents economic activities since the last wave, and the month of transition between spells. A special license access to Understanding Society is required in order to access district information of households.

**Other District and National Covariates** Additional district, regional, and national data are taken from NOMIS and ONS national statistics.

## 3.2 UKHLS Monthly Working Life Histories

Following Wright (2020) and Postel-Vinay and Sepahsalari (2019), I construct working life histories for respondents in the UKHLS sample, generating monthly time series based on respondents answers at the annual frequency. This allows me to observe spells of employment, unemployment and economic inactivity, and transitions between states, at the monthly frequency. Naturally this method is vulnerable to aggregation bias in that we cannot observe very short periods of unemployment of less than one month, and recall bias, in that survey participants are reporting on events some time (up to one year) after they have happened. Nevertheless, over the relevant sample period, information cannot be more than a year old. In the case of conflicting answers, precedence is given to the survey wave closest in time after the event.

This method extracts spell type, count, start and end dates, implied duration, and implied transition date. Where a spell transition is implied at survey seams due to changes in reported state, missing transition dates are imputed to the survey date, thus we observe some spell transitions with a degree of noise, although it is bounded. Nevertheless, Upward and Wright (2019) and Postel-Vinay and Sepahsalari (2019) show aggregates constructed from the UKHLS/BHPS samples using this method yields fairly good comparisons with national statistics such as the Labour Force Survey. I repeat this exercise and leave transition rates to the appendix (Figure 14)

**The Ins and Outs of UK unemployment** This yields 17,032 periods of unemployment across 12,377 individuals aged 18-64 in the relevant time period. While the median individual in the sample only experiences one spell of unemployment, the distribution is fairly right-skewed, and the distribution of spell durations even more so. The top 20 percent of respondents account for 40 percent of spells, and over 50 percent of total months in unemployment. The median spell lasts 9 months while the mean last 12.44 months. 68 percent of spells fall below the mean duration. Figure (15) plots flow rates $\lambda_t(a \rightarrow b)$ across states.

**Sanction Threat** I construct sanctioning threat as the ratio between the number of sanctions issued to the number of Job Seekers Allowance (JSA) claimants within district (group) $g$ in month

$t$.

$$\text{Sanctioning Threat: } S_{gt} = \frac{\text{number of sanctions}_{gt}}{\text{number of JSA claimants}_{gt}} \tag{1}$$

One may worry that such a ratio is already polluted by a threat effect of sanctions on search behaviour in the denominator. As an added robustness I use alternative proxies for sanctioning threat, using pre-reform average number of claimants, and lagged claimants as denominator.

**Matching**  Individuals working histories are matched to localities based on the household level UKHLS information reported in each survey wave (approximately annually). Individuals are freely able to transition into and out of households, however I match respondents to reported LAD based on the year in which they answer the survey. This potentially misses transitions which occur at other times of the year, or short departures and returns within a year. Nevertheless, the rate of reported mobility across districts is low.

I keep all individuals of working age from 18 to 64 years (prime age 25-54 as robustness). The UKHLS transitions from the former British Household Panel Survey (BHPS) starting in 2008, so 2009 is the first wave of the expanded panel. I limit my analysis to the period 2009 to 2015 due to further reforms to the broader welfare system, including the roll-out of Universal Credit (UC) - an all-in-one social transfer. While UC also features sanctions, at present data on the reason for sanction linked to job search specifically is not available.

## 4   Stylised Facts on Job Displacement and Sanctions

In this section I establish stylised facts related to income losses on job displacement and the association with early-in-spell sanctioning threat experienced.

Firstly, I estimate long-run earning losses associated with job loss using the stacking estimator of Cengiz, Dube, Lindner and Zipperer (2019). Upward and Wright (2019) perform a similar exercise with BHPS data between 1991 to 2007. They find medium-run earnings losses at 5 years of around 15 percent. A displaced worker belongs to cohort $c$ is $i$ becomes unemployed in year

$c$. Relative event-time $r$ is therefore simply $t - c$. The stacking regression is one solution to the problem of staggered treatment, stacking displaced cohorts, aligned in relative-to-event time but not calendar time. The sample consists of only ever-displaced individuals, so selection into treatment is less problematic in this case. A displaced worker will feature in the stacked dataset exactly once for each displacement as treated, but will contribute information as a control in several times. The estimating equation is therefore:

$$Y_{ict}(r) = \lambda_t + \gamma_{ic} + \sum_r \alpha_r T_r + \sum_r \beta_r (T_r \cdot D_{ic}) + \varepsilon_{it} \quad \text{for } r \in \{-Q, ..., +P\} \tag{2}$$

With normalisation of outcome to $Y_{ict}(r) - Y_{ict}(-1)$ so that relative year $-1$ is the baseline of comparison. The $T_r$ are relative period dummies and $D_{ic}$ is a binary displacement-cohort dummy.

The sequence of coefficients $\{\alpha_r\}$ tracks the evolution of earnings for the not-yet-displaced through time, and $\{\beta_r\}$ measures the displacement effect.

**Triple Differences**  Expanding on the model above, I first run the event study regression twice, splitting the sample according to whether a displaced worker experiences a high or low sanctioning threat in (up to) their first 3 months of unemployment. The first panel of the above figure plots the two event studies estimated in separate regressions, while the second panel estimates a single triples differences regression with the necessary interaction terms.

The triple difference regressions show that sanctioning threat experienced in the early months of unemployment has a strong association with medium-run earnings losses. Displaced workers who lose their job in a district-month experiencing above-average sanctioning threat will on average have medium-run earnings 20 percent below pre-job loss levels, conditional on reemployment (excluding zeroes). The other group which does not experience such a strong threat does not experience income losses which are significantly different from zero after 3 years. Initial sanctioning rate can explain a large share of earnings losses experienced by displaced workers at longer horizons, though confidence intervals are fairly large.

These regressions cannot claim any causality since initial sanctioning threat is not randomly assigned (even after absorbing permanent differences across individuals, and aggregate time
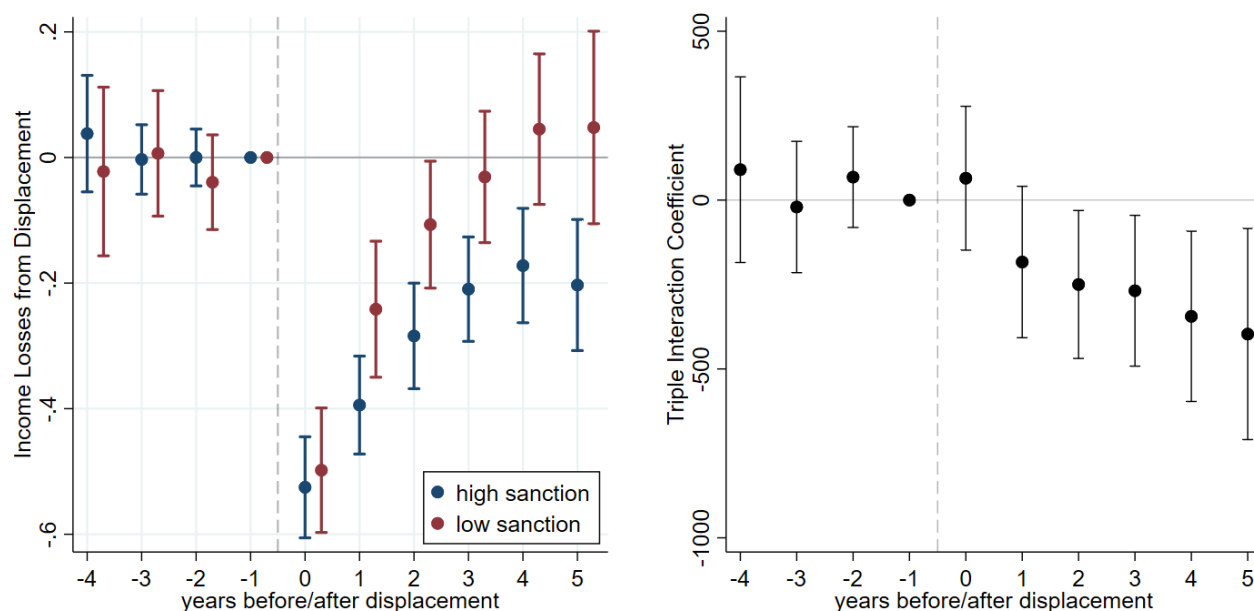
**Figure 2:** Medium-Run Earnings Losses from Job Displacement
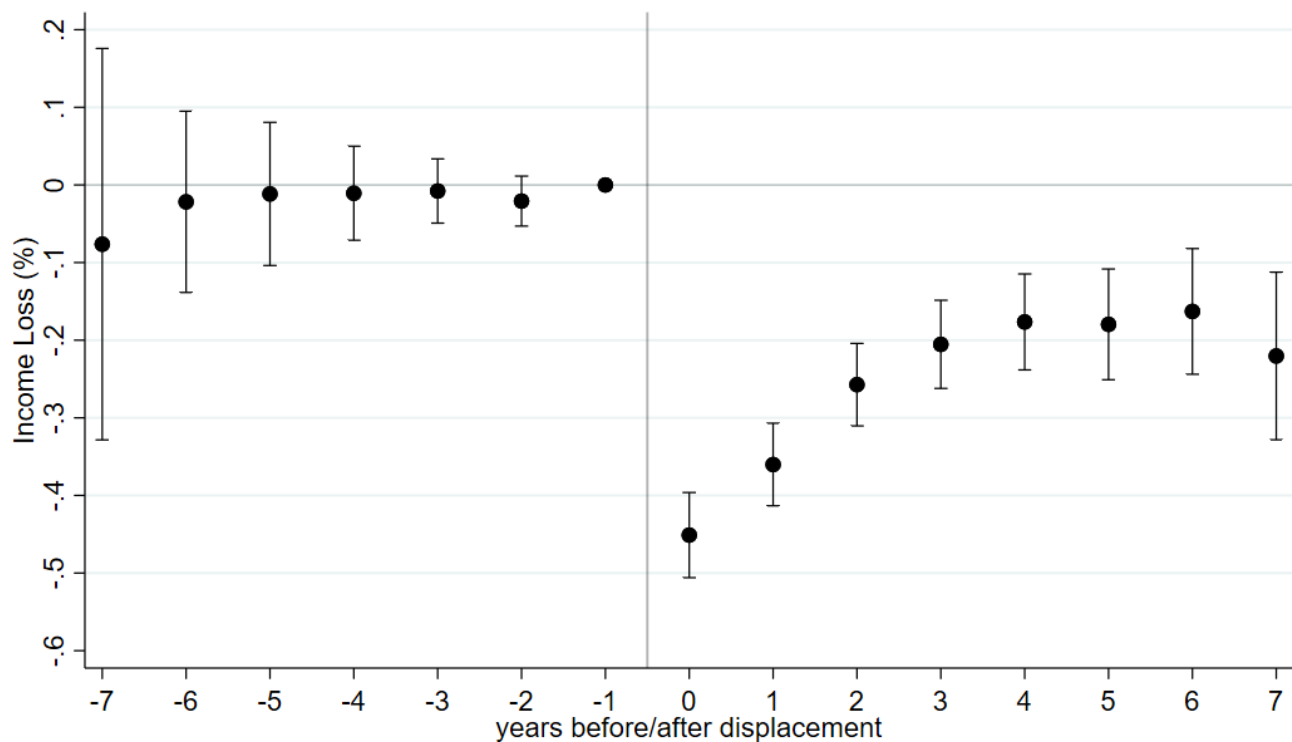


*Note: Coefficients from a stacking difference-in-differences regression. Error bars represent 95% confidence intervals, standard errors clustered at the person level. Treatment is defined as displacement in a given calendar year. Control units for that specific displacement cohort are defined as those workers not yet displaced. Estimation uses full UKHLS panel available from 2009 to 2018.*

trends), nevertheless are provocative in the correlations found. I bolster this correlational evidence further, showing these earnings losses are not driven by differential employment patterns across the two groups over time, but there is significant variation in a search effort proxy variable, consistent with the hypothesis that workers search harder under greater sanctioning pressure, but also alter their acceptance strategies to accept worse jobs in terms of pay.

**Figure 3:** High and Low Sanction Threat Experience and Displacement Earnings Loss



*Note: Coefficients from difference-in-differences regressions (L) and Triple differences (R). Error bars represent 95% confidence intervals, standard errors clustered at the individual level. Treatment is defined as displacement in a given calendar year. Control units for that specific displacement cohort are defined as those workers not yet displaced. Sample includes displaced cohorts from 2009 to 2015 inclusive.*

**Figure 4:** Search and Employment Triple Difference Estimates
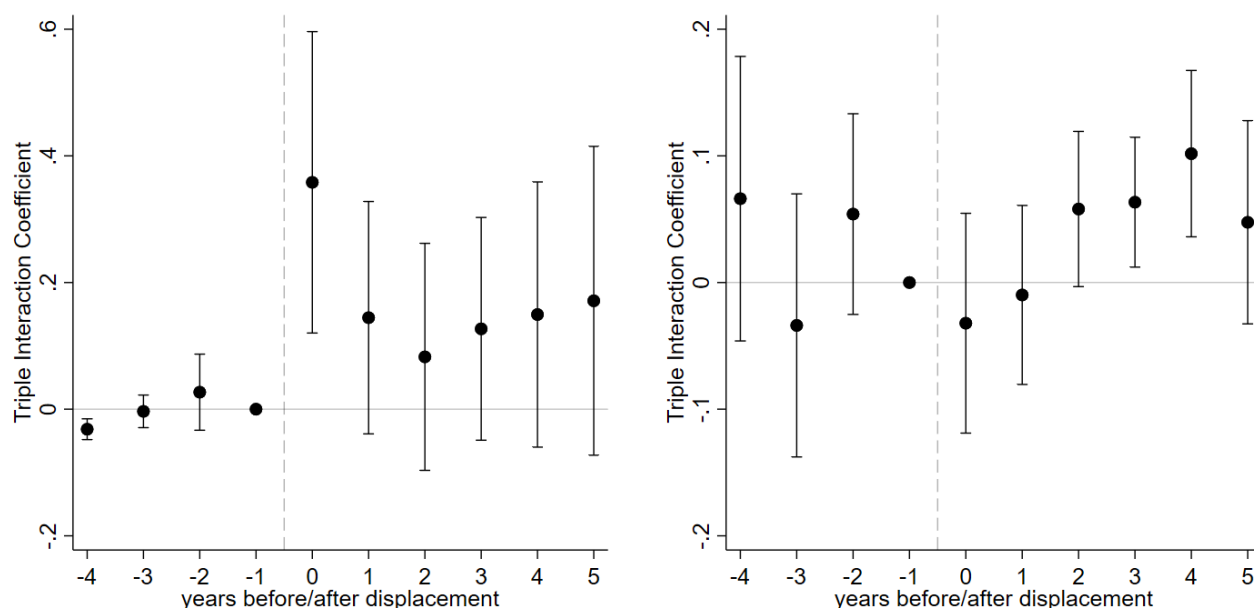
# 5   Empirical Strategy

My empirical strategy centres around a difference-in-differences design with a binary treatment and simultaneous roll-out to all groups, comparing the path of outcomes for a treated group of districts relative to the evolution in outcomes for a control group to account for changes that would have otherwise happened. Selection into treatment is clearly a concern in this setting in which districts are not randomly assigned treatment, however a DID design can tolerate certain degrees of endogeneous allocation of treatment, for example, treated districts tend to be poorer and have higher unemployment. However districts do not exhibit differential *changes* in these covariates, which is key to identification which uses changes in the control groups outcomes to estimate the unobserved counterfactual dynamics in the treatment group had they not received treatment.

Exploiting the 2012 reform to UK sanctioning policy, I define a treated district as one in which the district-month-average sanctioning intensity is in the top quartile in period immediately after the reform (2012m3-2014m12), while the control group are those districts in which average intensity is in the lowest quartile.

$$D_{gt} = \begin{cases} 1 & \text{if: } \mathbb{1}_g[S_{g,post} \in Q_4] \times P_t \\ 0 & \text{if: } \mathbb{1}_g[S_{g,post} \in Q_1, Q_4] \end{cases} \tag{3}$$

Since sanctioning intensity is a continuous variable itself, one can perform a DID regression with $S_{gt}$, the continuous sanctioning rate in percent, as the outcome on $D_{gt}$ to examine whether or not treatment and control groups exhibited diverging trends in sanctioning intensity before the reform (in addition to tests on the pre-reform coefficients in the outcome equation).

## 5.1   Identifying Assumptions

Under a common trends assumption (CTA) and stable unit treatment value assumption (SUTVA), DID estimates are consistent for the average treatment effect on the treated (ATT) (see appendix for full exposition):

**Common Trends Assumption**  The CTA states that untreated potential outcomes $Y(0)$ have a linear-additive structure made up of a common aggregate time trend $\lambda_t$ common to all, permanent differences across groups are captured by $\gamma_g$ and the influence of duration $T$ is captured by $\theta_T$ for within-spell variables such as exit hazard rate. Let $Y(0)$ denotes the untreated potential outcome.

$$E\left[Y_{igt}(0) \,\middle|\, g, t, T\right] = \lambda_t + \gamma_g + \theta_T \tag{4}$$

In the $2 \times 2$ context, letting $t + 1$ denote the treated period, this structure implies we can infer the missing counterfactual object $E[Y_{t+1}(0)|D = 1]$:

$$E\left[Y_{t+1}(0) - Y_t(0)|D = 1\right] = E\left[Y_{t+1}(0) - Y_t(0)|D = 0\right] \tag{5}$$

This assumption is challenged to the extent to which estimates for placebo treatment effects in the pre-reform period differ significantly from zero (implying the above three-way fixed effect structure $\lambda_t + \gamma_g + \theta_T$ is not sufficient to capture all dynamics in observed outcomes in the pre-reform period. It is not possible to evaluate parallel trends in the post-reform period.

**Stable Unit Treatment Value Assumption**  states that only contemporaneous own treatment value $D_{g,t}$ matters for outcomes in $(g, t)$, so potential outcomes are indexed by $D_{gt}$ only:

$$Y_{igt}(\mathbf{D}) = Y_{igt}(D_{gt} = d) \quad D_{gt} \in \{0, 1\} \tag{6}$$

Where $\mathbf{D}$ is the vector of all groups treatment statuses. This is a restrictive assumption in the sense that it precludes any sort of spillovers across districts $Y_{igt}(D_{gt}, D_{g't})$ or through time $Y_{igt}(D_{g,t}, D_{g,t-1}, ...)$ (or both). While the DID literature is making advances in dealing with spatial spillovers[3], for now I proceed assuming no spillovers across districts, however acknowledge cross-district spillovers in sanctioning threat stands as a potential threat to my identification scheme.

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[3]for example Butts (2021) and Clarke (2017)

## 5.2 Estimation

The canonical $2 \times 2$ difference-in-differences specification, featuring simultaneous treatment rollout to all treat groups and pooled placebo effect $\alpha$ and pooled ATT $\beta$, takes the following form, where $P_t = 1(t > 2012m3)$ is the "post-reform" indicator and $D_g = 1$ for treated districts, irrespective of when, and is estimated by OLS.

$$Y_{igt} = \lambda_t + \gamma_g + \theta_{T(i,t)} + \alpha[(1 - P_t) \cdot D_g] + \beta[P_t \cdot D_g] + u_{igt} \tag{7}$$

When the dependent variable is binary or a count, the linearity of the estimator may be less desirable for well known reasons, nevertheless I defend the choice of estimator to invoke common or parallel trends in the outcome variable *in levels*, $Y$, and not parrellel trends in an index variable inside a non-linear function $G(Y^*)$. Wooldridge (2021) and Athey and Imbens (2006) explore non-linear difference-in-difference estimation.

A pooled placebo test of parallel trends in the pre-reform period tests $H_0 : \hat{\alpha} = 0$. The dynamic event study specification splits pre and post periods by year to examine the evolution of treatment effects over time and to look at pre-trends. The baseline period to which all coefficients are normalised is January to March 2012, labelled $2012^{(-)}$. Outside of this period, and the remainder of 2012, labelled $2012^{(+)}$, I pool months into their respective calendar years in order to increase power but still display dynamics over time.

$$Y_{igt} = \lambda_t + \gamma_g + \theta_{T(i,t)} + \sum_{\ell=2009}^{2011} \alpha_\ell \cdot \mathbb{1}(t = \ell) \cdot D_g + \sum_{\ell=2012^{(+)}}^{2015} \beta_\ell \cdot \mathbb{1}(t = \ell) \cdot D_g + u_{igt} \tag{8}$$

In hazard regression models presented below, duration dependence in the probability of exit is modelled non-parametrically by adding duration-of-spell fixed effects, $\theta_{T(i,t)}$. Since duration is potentially affected by treatment, I estimate the duration, group and time effects using untreated observations only in a two-step estimation.

Since all treated groups are treated at the same time, and I employ a binary treatment indicator, problems associated with negative weights or invalid comparisons across newly-treated

to already-treated groups is not an issue in this context and an estimator from a new class of robust DID estimators is not necessary. De Chaisemartin and D'Haultfoeuille (2022) provide a survey of recent advances in this area.

# 6  Results and Discussion

My empirical investigation shows treated jobseekers in high sanction threat districts typically exit unemployment faster, and this is mostly driven by exits back into employment, however exits into non-employment are already rare at baseline, and so regression analysis typically lacks power to detect small changes in already low base rates. There is suggestive evidence of a spike in early retirements.

As regards reemployment outcomes, sanctioning threat is shown to increase unemployment churning – cycling back and fourth between employment and unemployment. When looking at the duration of the next period of unemployment, workers who reenter employment in treated districts are less likely to be in employment after 12, 24, and 36 months. These results are consistent with job seekers using job quality margins to insure themselves against UI losses, and therefore market insurance could explain the displacement losses differential between high and low early sanction threat experiences.

To complement regression analysis, I also examine other labour market variables from national statistics to bolster the case that treated and control units do follow parallel trends. Figures (22 and **??**) examine labour market variables local unemployment-population ratio and nominal weekly earnings. No departure from parrellel trends is obvious on visual inspection.

Overall, my results suggest that policymakers face to some degree a tradeoff between the frequency and duration of unemployment spells. Shortening spells backfires in the sense it increases the likelihood of future returns to unemployment.

**Sanctioning Intensity and Exit Hazard Rate Response to Reform**   The baseline event study hazard regression follows equation (8) above. $Y_{igt}$ is a binary indicator taking value 1 if the

jobseeker exits unemployment in month $t$, and zero otherwise.

$$
Y_{igt} = \begin{cases} 1 & \text{if: } \textit{person i exits unemployment in month t} \\ 0 & \textit{otherwise} \end{cases} \tag{9}
$$

The red squares track the impact of the reform on the difference between sanctioning rates in treated and control districts. Differential treatment intensity, after controlling for district and time fixed effects, are well centred on zero with small errors in the pre-reform period, consistent with no pre-reform differential trends in sanctioning. Sanctioning intensity, $S_{gt}$, rises by up to 2ppts above pre-reform levels in the treated group, equivalent to an an increase of almost 75 percent.

The probability of exiting unemployment, $\hat{E}[exit_{igt}|g,t,T,D]$ accounting for group, time, and duration effects,shows no strong pre-trend in the pre-reform period. A placebo test of coefficient $\alpha_{pre} = 0$ in the pooled regression (7) cannot reject the null, though a fairly low p-value is primarily driven by a small pool of unemployed in 2009 around the survey start, and an even smaller number of exits in 2009. Standardising coefficients to pre-reform baseline exit rates (4.6 percent probability to exit unemployment in a given month), the placebo coefficient is -0.0236 percent of baseline (se 0.0144). The average treatment effect on the treated (ATT) estimate is an economically meaningful 0.1871 percent increase in the probability of exiting unemployment in a given month over baseline (0.0086 ppts unscaled ATT estimate).

Similarly sanction intensity has an ATT coefficient of 0.0170 (s.e. 0.0011). A back of the envelope calculation suggests that a 1 percentage point increase in sanctioning threat, induced by the policy reform, increases exit hazard rates by 0.5 percentage points in treated districts.

**Figure 5:** Effect of Policy Reform on Sanctioning Intensity and Probability to Exit Unemployment Spell (in percent)



(a) Exit

(b) Exit into Employment
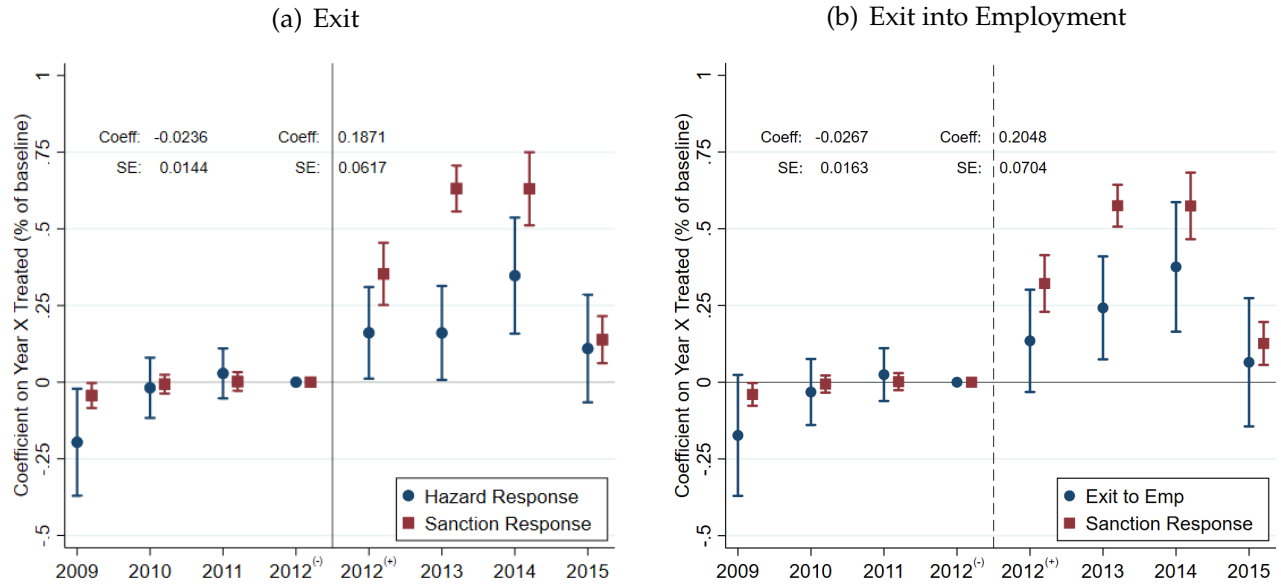
*Note: Coefficients from difference-in-differences regression. Dependent variable: unemployment spell exit indicator (L) and exit into employment indicator (R). Error bars represent 95% confidence intervals, standard errors clustered at the district level. Coefficients are normalised to the period 2012m1:m3=0, and rescaled by the mean of pre-reform dependent variable in treated districts.*

**Exits into Employment, Non-employment, and Retirement** The overwhelming majority of exits in my sample are exits into employment (UE flows). I further examine exit into non-employment and early retirement for the over-50s. Given UN transitions are already rare at baseline, it is perhaps unsurprising point estimates are very uncertain, even in the pre-reform period. While I find no effect, this is partially due to extremely low power. I find suggestive but insignificant coefficients for the immediate period after the reform for early retirements.

**Churn Rate and Cumulative Unemployment Spell** one of the principal contributions of this paper is to present new evidence that sanctioning threat drives churning in the labour market. I call repeated transitions back into unemployment (and re-employment) churning. In my churning event study regressions I define the outcome variable as a simple counter for distinct unemployment spells which must be separated by employment. Let $\lambda_t$ denote activity transition dummies of person $i$ in district $g$ time $s$: $N_{igt}^{U} = \sum_{s=t_0}^{t} \mathbb{1}_s(EU) \in \{1, 2, .., 5\}$.
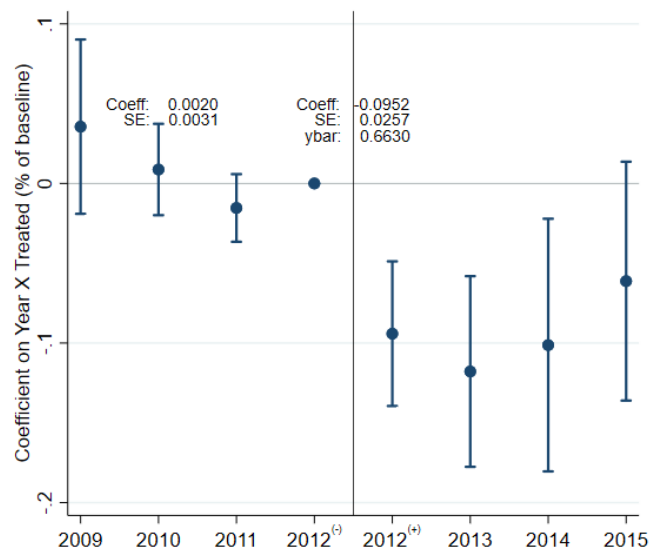
19

**Figure 6:** Cumulative number of unemployment spells, $N_U$
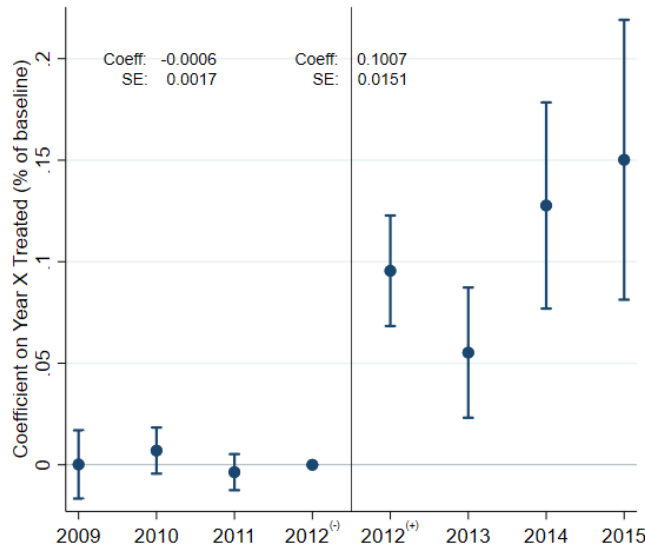


*Note: Coefficients from difference-in-differences regression. Error bars represent 95% confidence intervals, standard errors clustered at the district level. Coefficients are normalised to the period 2012m1:m3=0, and rescaled by the mean of pre-reform exit rates in treated districts.*

**Next Employment Spell Duration** To further validate the churning hypothesis, I examine the duration of future employment directly after exit. I allow workers to change job and employer, however they must be continuously in employment throughout the spell. I find the probability of having a reemployment duration in treated districts (controlling for composition of unemployment spell duration) that lasts longer than 12, 24, and 36 months falls by 5.04, 5.83, and 9.52 percent of baseline respectively, consistent with faster exits from unemployment into lower quality jobs with lower stability. While one might be concerned this effect is mechanical due to the end point of the survey data, a very similar result holds if the dependent variable is reversed to $T_E < n$, i.e. returns to employment are more likely in treated districts *to be less than n months* in total duration.

**Figure 7:** Next employment duration > 36 months



*Note: Coefficients from difference-in-differences regression. Error bars represent 95% confidence intervals, standard errors clustered at the district level.*

**Selection and Composition Effects in Returns**   Some caution is required when interpreting the results for post-unemployment outcomes. The pool of workers who return to work and when they return will not be random. We would expect returns to employment to be positively selected. In terms of reemployment outcomes, we would then expect this selected group to have better outcomes than the total population. As such one could view my estimates as a lowerbound, given they are for adverse outcomes, the effect of positive selection would be to push the estimated effect upwards towards zero.

A second point of concern is that the marginal re-entrants in treated and untreated districts are now different. One could imagine under a stricter sanctioning regime, more and more marginal workers are forced back into employment. A more marginal worker will also face higher unemployment risk. I do not identify these channels (selection in the extensive margin of returns to employment, the timing margin, and diverging pools of returning workers)

## 6.1 Addressing Potential Threats to Identification

**High-frequency Migration**    The survey frequency is not high enough to identify short spells of temporary migration for less than roughly 12 months (actual survey wave gaps will vary). Nevertheless, if migration patters are from areas of high unemployment to low unemployment, and therefore high sanctions to low sanctions, some treated units would appear as controls, thus introducing a type of "mixing migration" (where treated and controls are mistaken for each other). If this is the case, intuitively the treated and control pools become more similar and thus bias treatment effect estimates towards zero. If on the otherhand migration follows a kind of "polarising migration" pattern in which jobseekers in treated areas move to even higher intensity districts, and those in control districts move to more lenient areas, then the dosage of sanction threat received would appear to be smaller than is actually the case, biasing the estimate away from zero.

**Spatial Spillovers Across Districts**    While I do not address spatial spillovers directly, I argue this is not a threat to identification for two reasons. Firstly, again, to the extent that spillovers shuffle treated and controls (some of the units labelled as controls will receive some indirect spillover treatment), then my results can be seen as a lowerbound estimate for the true effect. Secondly, and qualitatively, since jobseekers have regularly contact with caseworkers in employment offices to monitor their activity, it is highly likely the caseworker will inform the jobseeker of local economics conditions, both in terms of vacancies but also on recent, local sanctioning activity.

**Idiosyncratic Local Shocks and Reverse Causality**    One principal concern with my identification scheme is the presence of unobserved shocks at the district level which trigger an endogeneous response from employment offices to sanction at higher rates in districts I have labelled as treated, but were simply hit by asymmetric shocks. While no evidence can prove the absence of idiosyncratic shocks driving treatment, I can show that district-level variables do no exhibit this kind of divergence around the time of the reform, suggesting local labour market shocks are not the driver of sanction response heterogeneity to the central reform.

Figure 23 below presents measures of local economicy activity at the district level. Treated and

Control groups do not show evidence of asymetric shocks around the time of the reform in 2012. 23 plots Gross Value Added (GVA) and GVA per capita (GVAPC). Breaking these district-level results down further by industry shares within districts, I also construct average industry shares across broad industry groups. Qualitatively there is no "spiking" pattern we would expect to see in the presence of uneven shocks in 24

## 6.2   Treatment Effect Heterogeneity by Demographics

[Work in Progress]

# 7   A Search Model with Sanctions

To interpret my empirical findings, I build a partial equilibrium random search model to examine how households adjust search behaviour in response sanctions, both the direct and indirect channels.

Households choose search effort as well as job-offer acceptances, and so the model allows households to adjust along job-offer arrival and job-acceptance margins. Jobs are heterogeneous in their pay and their stability in order to capture earnings losses and unemployment churning seen in the data.

The probability a jobseeker is sanctioned is inversely related to their search effort, but detection by the employment office is imperfect so an unemployed agent with low effort may avoid a sanction, and conversely a high-effort searcher may still receive a sanction.

**Employment**   Workers are employed in jobs with wage $w$ and riskiness $\sigma$. The worker returns to unemployment with probability $\sigma$ based on the characteristics of her current job. In unemployment she faces a clean search record (will receive $b$ with certainty in the first period unemployed).

$$W(w, \sigma) = \left\{ u(w) + \beta \left[ (1 - \sigma) W(w, \sigma) + \sigma U(b) \right] \right\} \tag{10}$$

**Unemployment, Search, Detection**  The unemployed searcher's problem has state variable $y$ denoting consumption in the period. Households are hand-to-mouth and cannot borrow or save. If unsanctioned, the jobseeker consumes $b$, and if sanctioned consumes $\varepsilon$ representing home production.

$$y = \begin{cases} b & \textit{transfer payment if receiving unemployment benefits} \\ \\ \varepsilon & \textit{home production if sanctioned} \end{cases} \tag{11}$$

Households optimally choose costly effort to improve offer arrival rates as well as avoid sanctions. Sanctions are triggered with probability $\pi(e)$ where $\pi'(e) < 0$. Job offers are drawn with probability $\lambda(e)$ with $\lambda'(e) \geq 0$ and $\lambda(e) \in [0, 1]$

$$U(y) = \max_{e} \left\{ u(y) - v(e) + \beta \left[ R(e) + \lambda(e) \int_{W \geq R(e)} [W(w, \sigma) - R(e)] dF(w, \sigma) \right] \right\} \tag{12}$$

Reservation utility, given parameters:

$$R(e) = \pi(e)U(\varepsilon) + (1 - \pi(e))U(b) \tag{13}$$

**Job Offer Distribution**  A job is a wage-stability pair $(w, \sigma)$ drawn from exogeneous cumulative distribution function $F(w, \sigma)$. With a slight abuse of notation to denote $1 - F(R(e))$ as the share of jobs that would be accepted if offered, the household balances disutility of searching hard versus the returns to effort acting through higher probability to draw an offer, as well as lower sanctioning risk.

**HH optimality**  satisfies:

$$v'(e) = \beta \left[ \underbrace{\pi'(e)[U(\varepsilon) - U(b)](1 - \lambda(e)(1 - F(R)))}_{\textit{reduction in sanction risk if still unemployed}} + \underbrace{\lambda'(e)(1 - F(R))\mathbb{E}[W(w, \sigma) - R(e)|W \geq R(e)]}_{\textit{boosts arrival rate, increases reservation utility}} \right] \tag{14}$$

The presence of UI sanctions $\pi(e > 0) > 0$ in this model has an ambiguous effect on effort compared to a model without $\pi(e) = 0$. While a non-zero probability of sanction makes effort

appealing in order to reduce sanction risk (first term) the second term, the expected value of an accepted offer, will fall due to lower reservation utility coming from more weight put on the sanctioned state in (13).

**Direct and Indirect Effects of Sanctions**  I define the direct effect of a sanction as the partial derivative of outcome $x$ with respect to received unemployment insurance, $b$. I define the indirect effect as the derivative with respect to the shock $\eta$, where $\eta$ is a small shock to the probability of being sanctioned, altering, for example, the reservation job:

$$R(e) = \{\pi(e) + \eta\}U(\varepsilon) + (1 - \{\pi(e) + \eta\})U(b) \tag{15}$$

[Work in progress]

# 8  Conclusion

In this paper I show that the benefits of running a tough unemployment insurance sanctioning system - faster matching - come at the cost of generating unstable matches, so to an extent the policymaker faces a tradeoff between the number and duration of unemployment spells. Tightening the sanction regime for reasons of pressuring jobseekers back to work may in the end backfire by generating unstable matches which separate easily.

Ex ante it is not clear what the welfare implications of the tougher sanctioning regime are: many short matches will probably preclude the development of human capital, but at the same time, shorter spells stave off stigma and other duration dependence effects.

This potential tradeoff matters at the micro level - how best to insure workers when they can enter "bad job traps". It also remains an open question to what extent the severity of the sanctioning regime matters in the aggregate. Knowing workers become more desperate under sanction pressure, firms will alter which types of jobs they create. This is likely to have implications for productivity via match quality.

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# Appendices

## A Figures

**Figure 8:** Evolution of Weekly Jobseekers Allowance
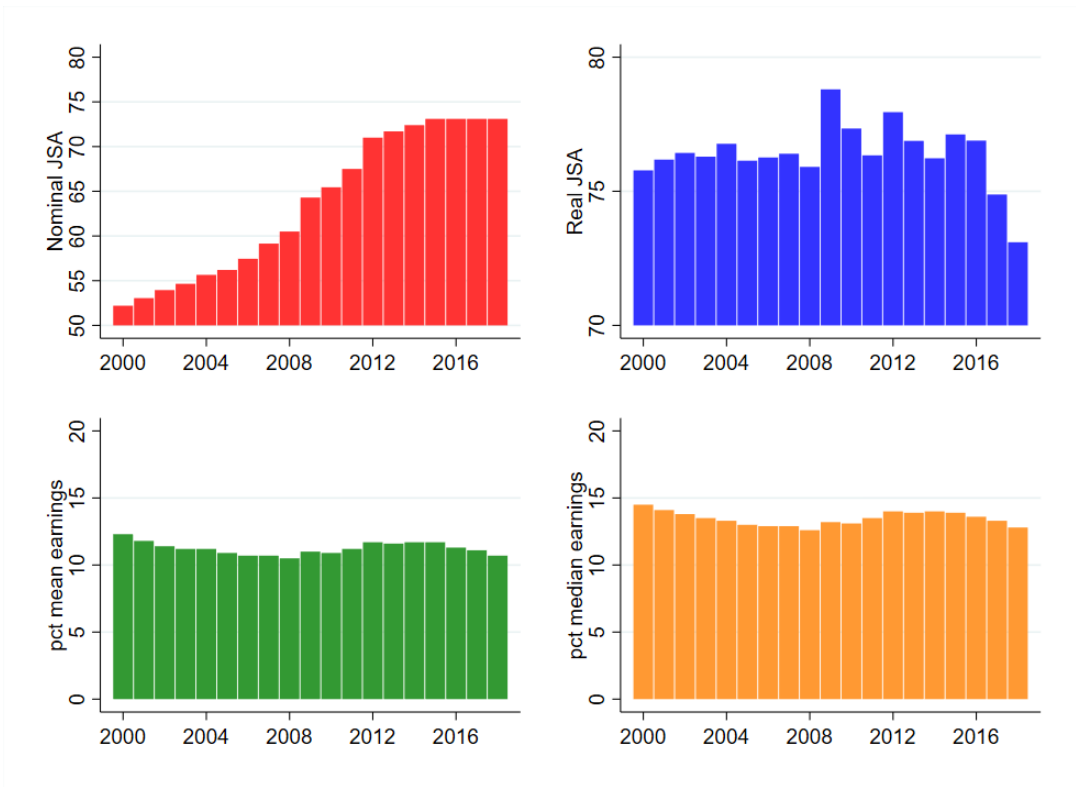


*Note: nominal JSA refers to weekly JSA in current prices, unadjusted for rising prices. Real JSA refers to weekly JSA adjusted for inflation in 2018 prices. pct mean earnings refers to JSA as a proportion of mean earnings, and similarly for median earnings. Source: ONS, DWP.*

**Figure 9:** International Comparison of Replacement rates in 2010 (percent of average wage)
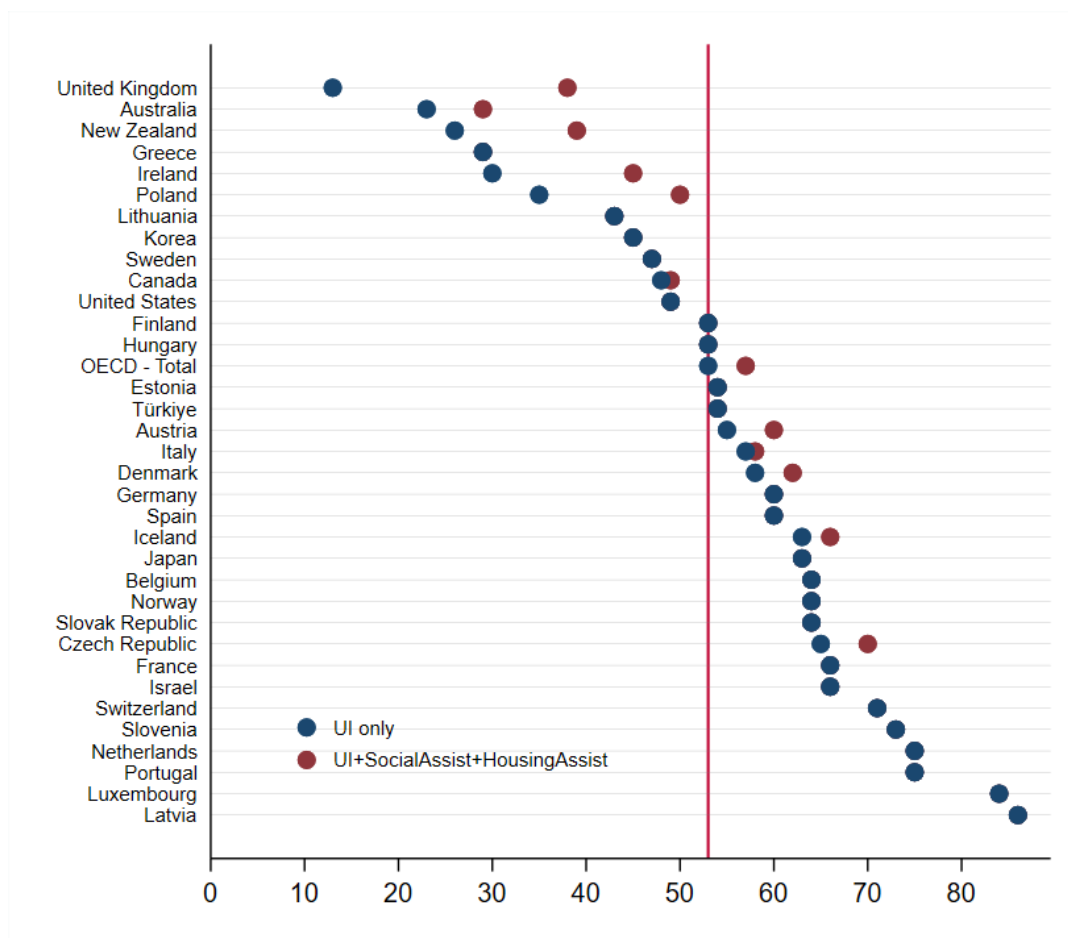


*Note: Replacement rates in OECD countries. Blue dots represent unemployment insurance payments as a share of mean earnings, while the red dot factors in other transfers such as other forms of social assistance and housing assistance. Source: OECD. .*

**Figure 10:** Reason for Sanction

(a) Count in Thousands

(b) Share of Total
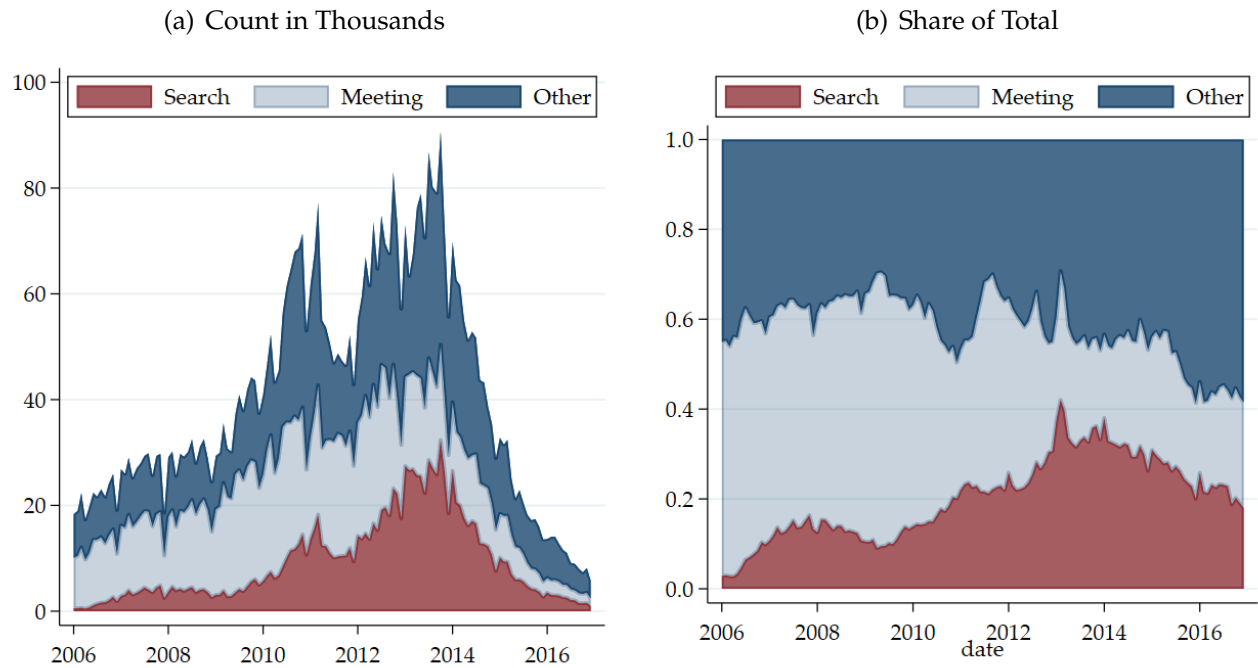


*Note: Aggregate counts and shares by reason for sanction issued.*

**Figure 11:** Changes in sanctioning 2010-2014 by district
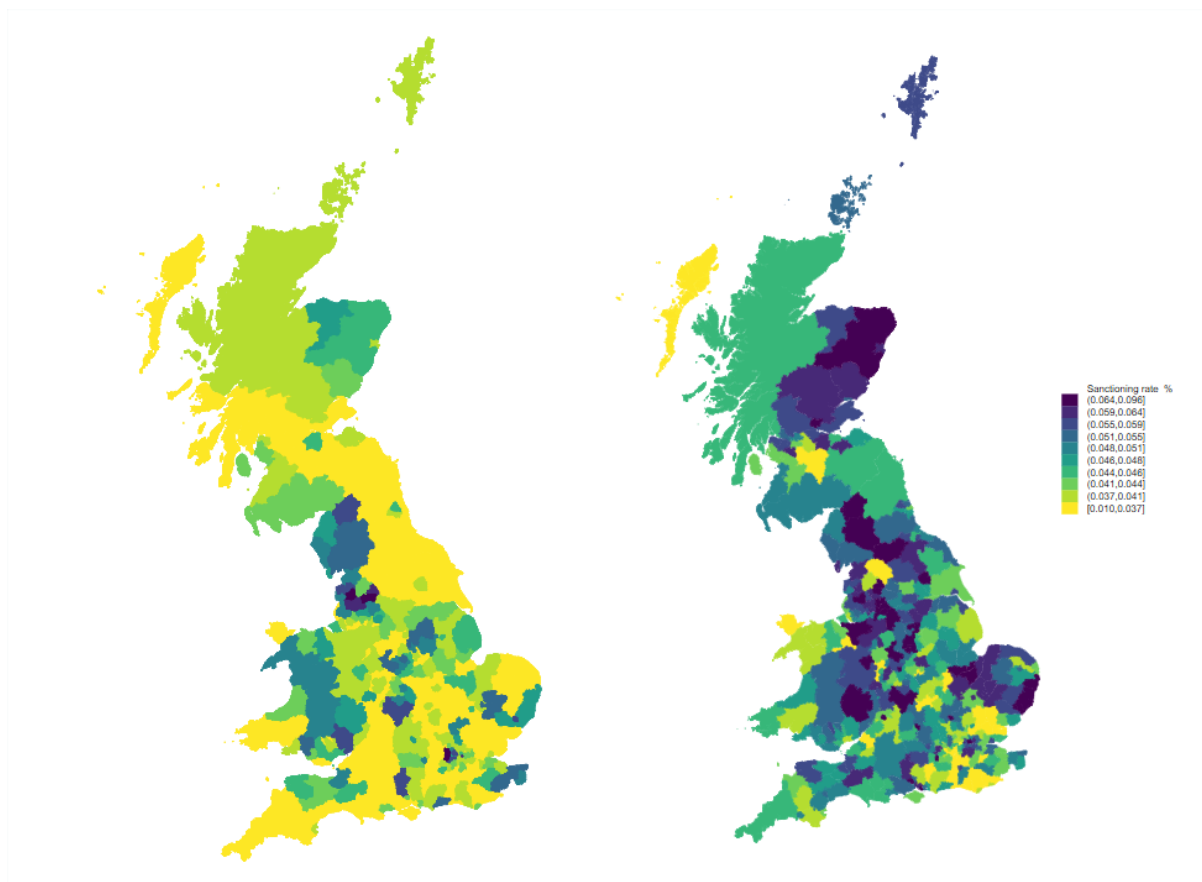


**Figure 12:** Changes in sanctioning 2010-2014, London districts
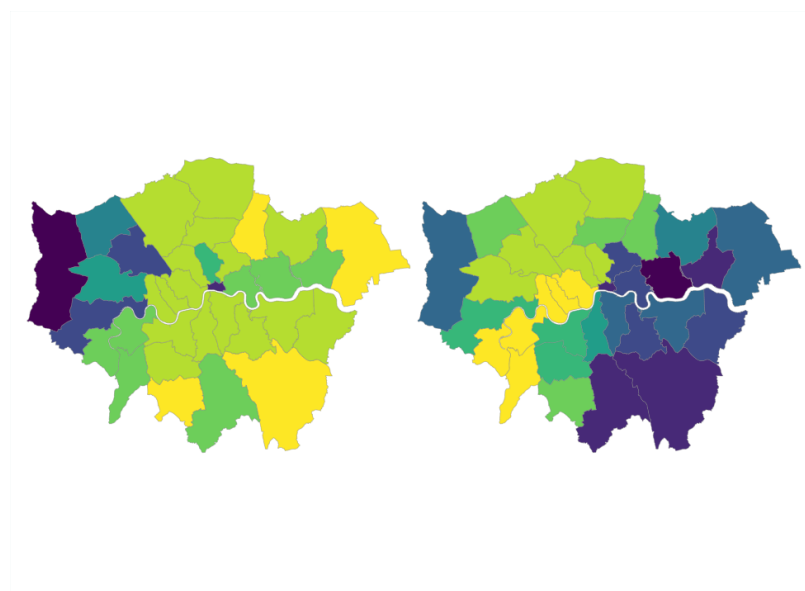


*Note: coloured based on decile of sanctioning intensity, scale adapted from post-reform period. LHS plots mean intensity 2010m1-2012m2 and RHS plots mean intensity 2012m3-2014m12*

**Figure 13:** Equiareal Hexplot: Changes in Sanctioning 2010m1-2012m2 v 2012m3-2014m12
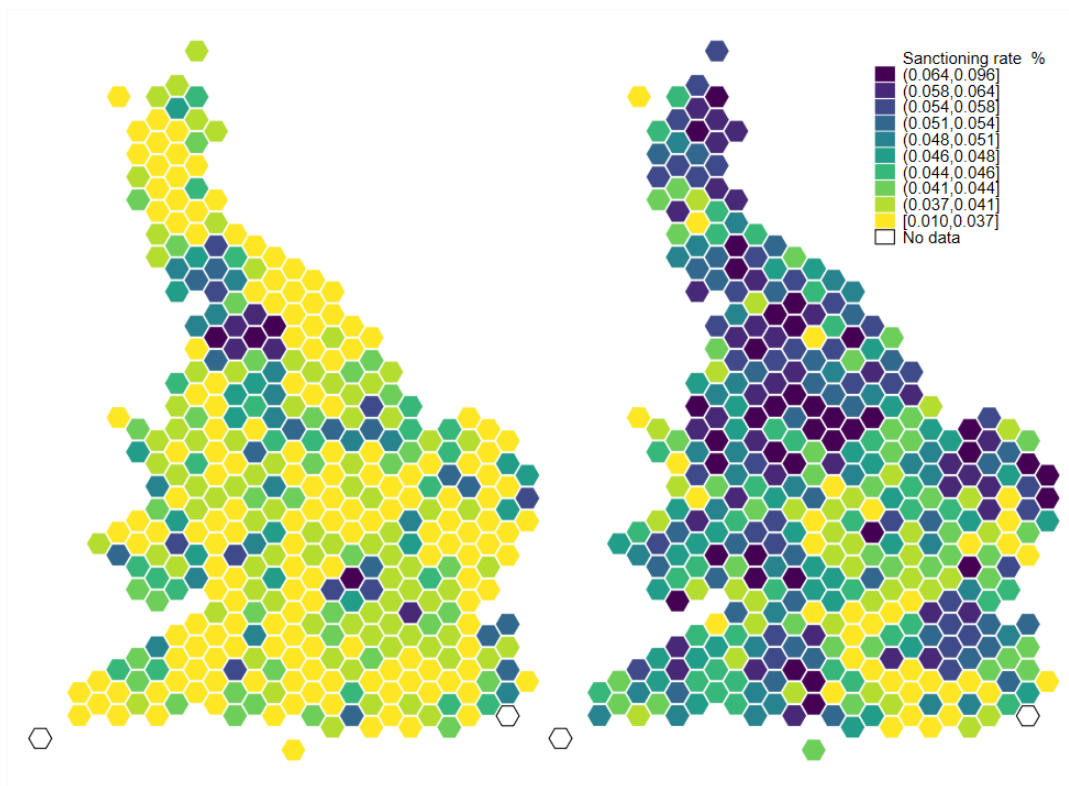


*Note: coloured based on decile of sanctioning intensity, scale adapted from post-reform period. LHS plots*

*mean intensity 2010m1-2012m2 and RHS plots mean intensity 2012m3-2014m12*

**Figure 14:** Labour Market Transition and Unemployment Rates – UKHLS microdata and aggregate National Statistics (Labour Force Survey)



(a) Separation Rate, $Pr(U'|E)$

(b) Job Finding Rate, $Pr(E'|U)$

(c) Unemployment Rate (percent )
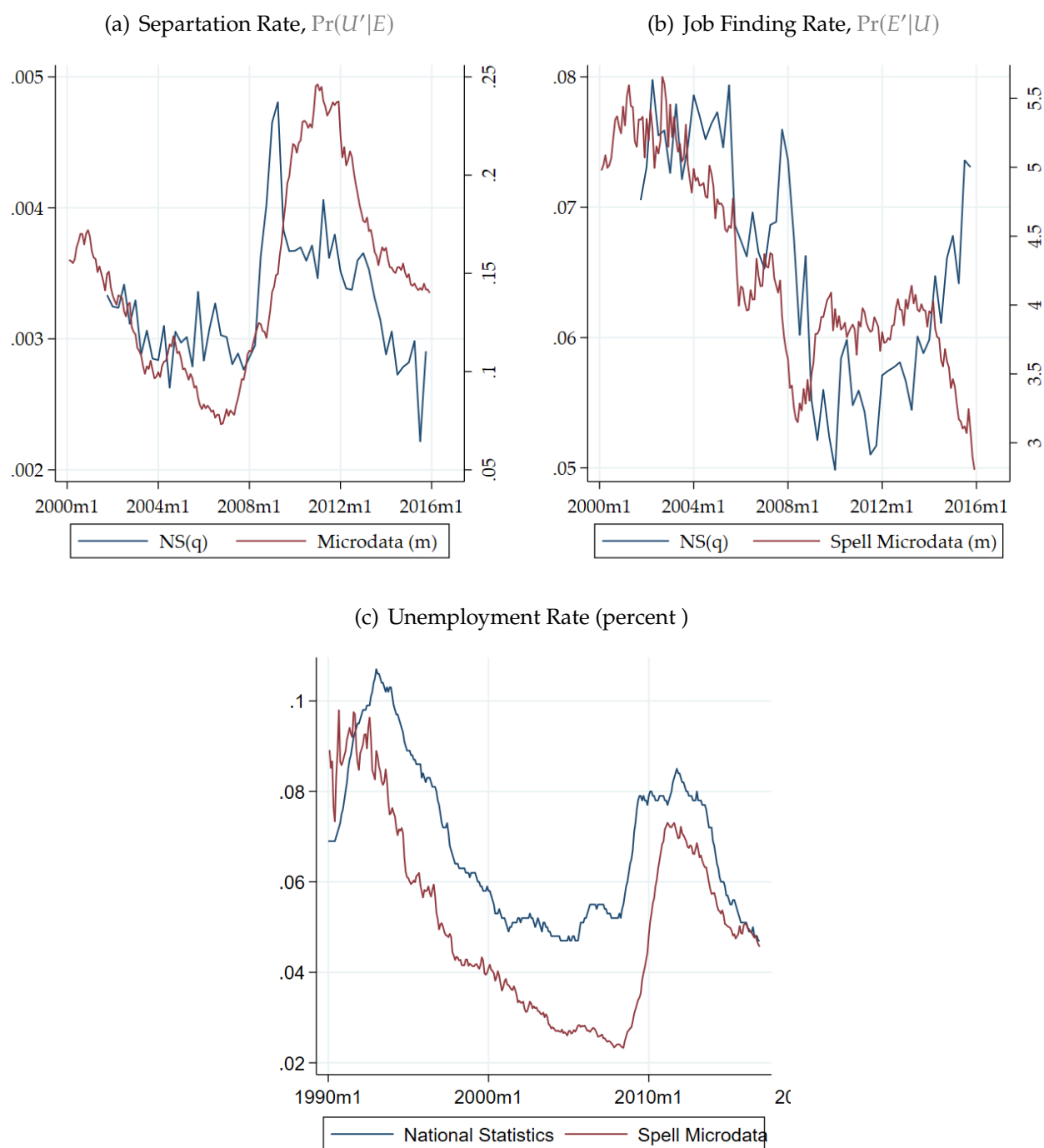
*Note: Transition rates in red calculated using UKHLS individual panel data, and smoothed using a centred MA(13). National statistics (Labour Force Survey) in blue. Unemployment rate in microdata calculated as $\frac{U}{U+E}$ where E includes self-reported self-employed*

**Figure 15:** Transition rates, normalised (2007m1=1)
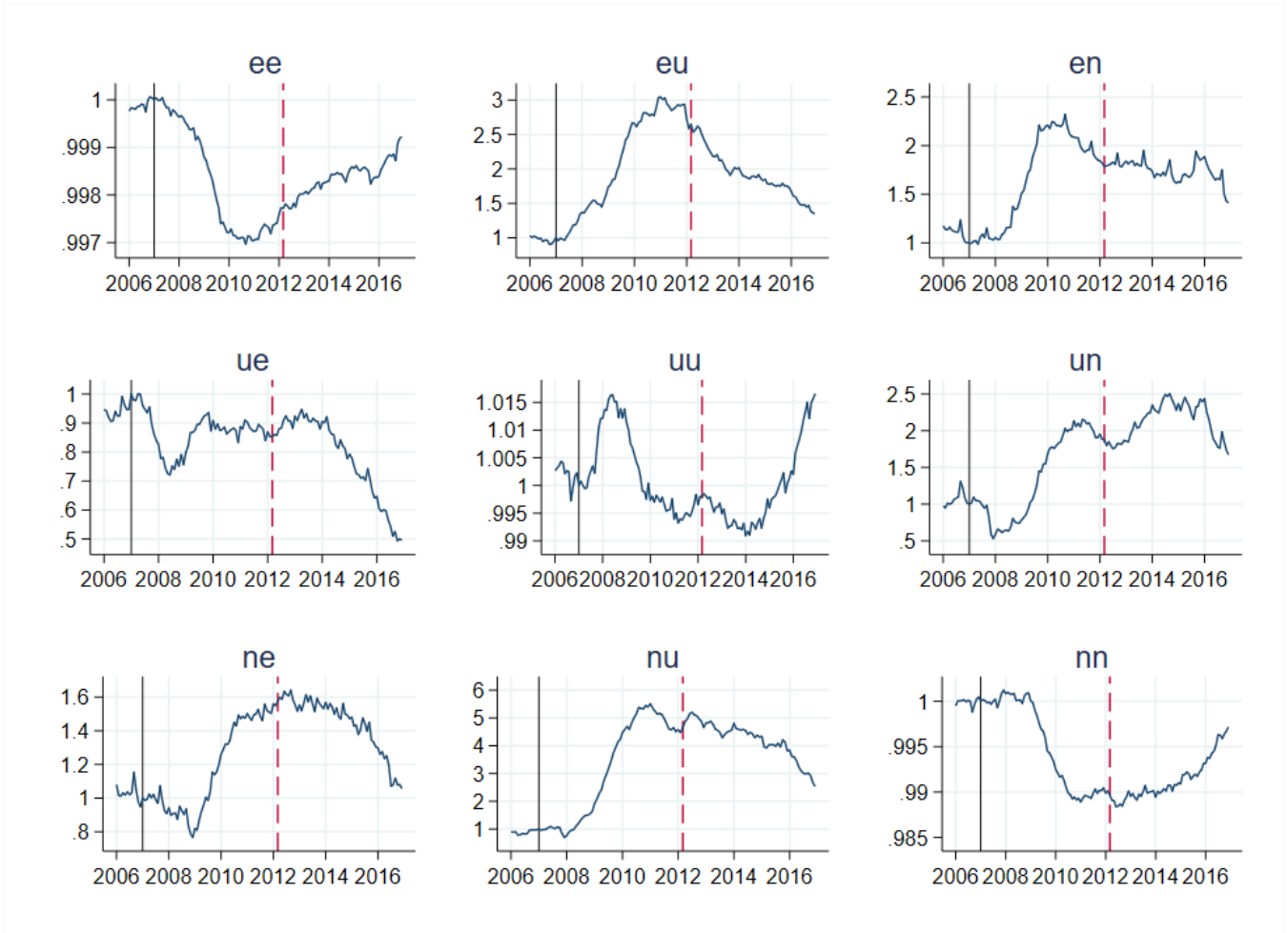


*Note: Transition rates calculated using UKHLS individual panel data, and smoothed using a centred MA(13). Rates are normalised to January 2007, marked by the solid black line. The red dashed line marks the sanctioning reform. eu should be red as $e_{t-1} \rightarrow u_t$*

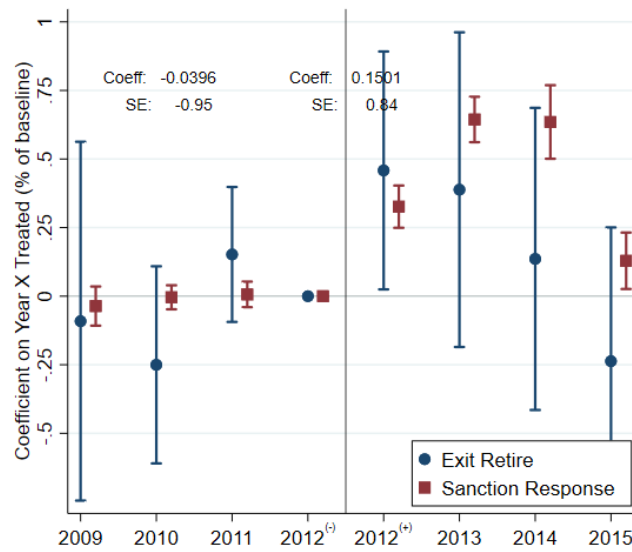**Figure 16:** Baseline DID hazard Event Study: Exit (ppts)
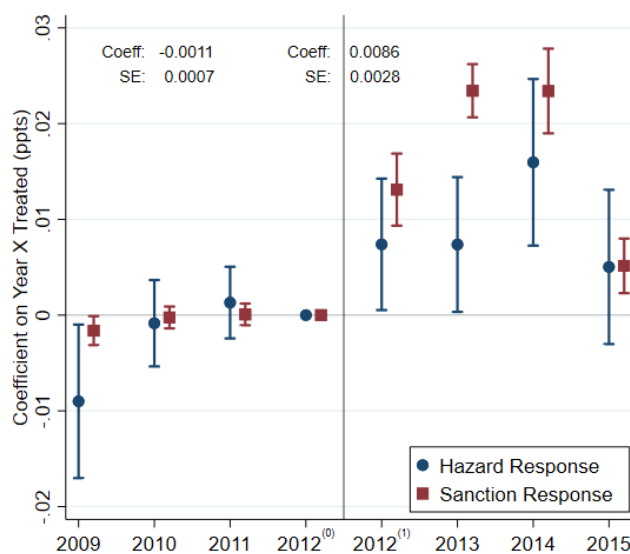


*Note: Coefficients from difference-in-differences regression. Error bars represent 95% confidence intervals, standard errors clustered at the district level.*

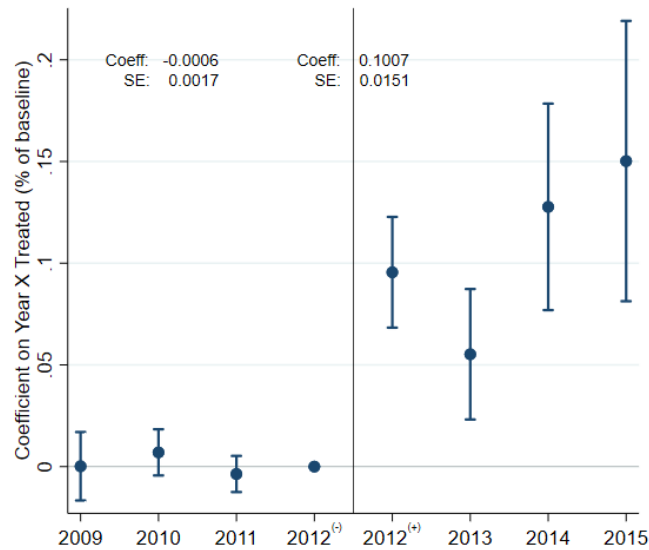**Figure 17:** Exit into employment (ppts)
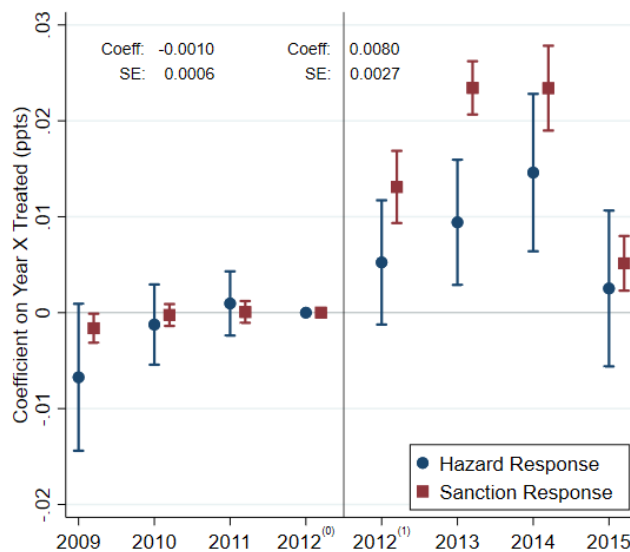


*Note: Coefficients from difference-in-differences regression. Error bars represent 95% confidence intervals, standard errors clustered at the district level.*

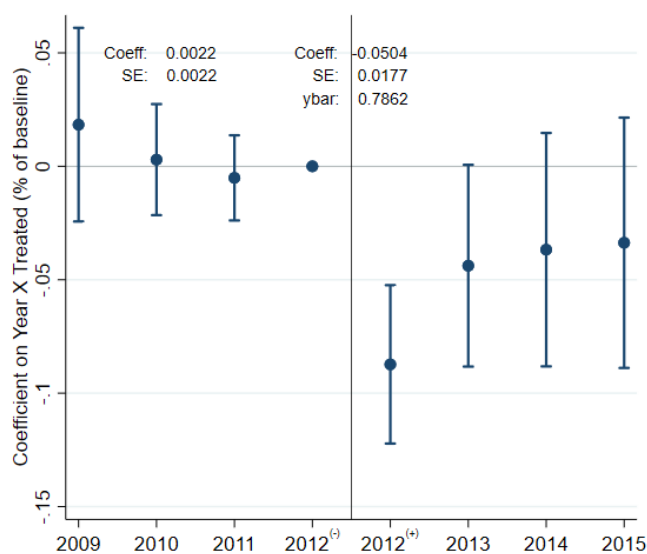**Figure 18:** Exit into retirement (ppts)



*Note: Coefficients from difference-in-differences regression. Error bars represent 95% confidence intervals, standard errors clustered at the district level.*

**Figure 19:** Cumulative number of unemployment spells (count)



*Note: Coefficients from difference-in-differences regression. Error bars represent 95% confidence intervals, standard errors clustered at the district level.*

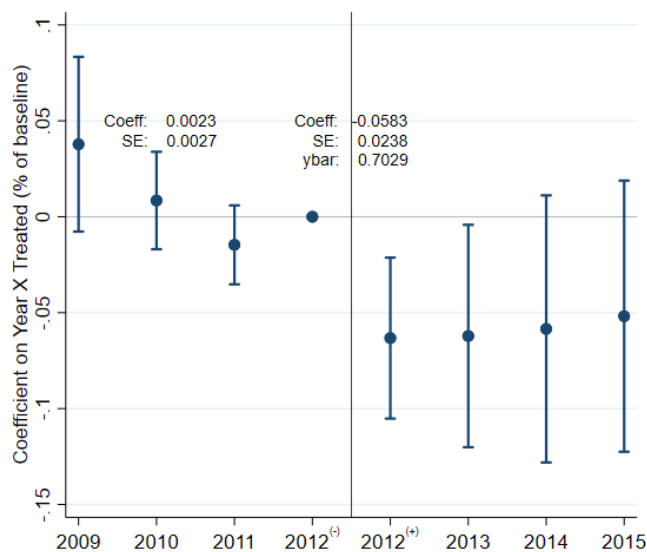**Figure 20:** Employment duration > 12, percent of baseline



*Note: Coefficients from difference-in-differences regression. Error bars represent 95% confidence intervals, standard errors clustered at the district level.*

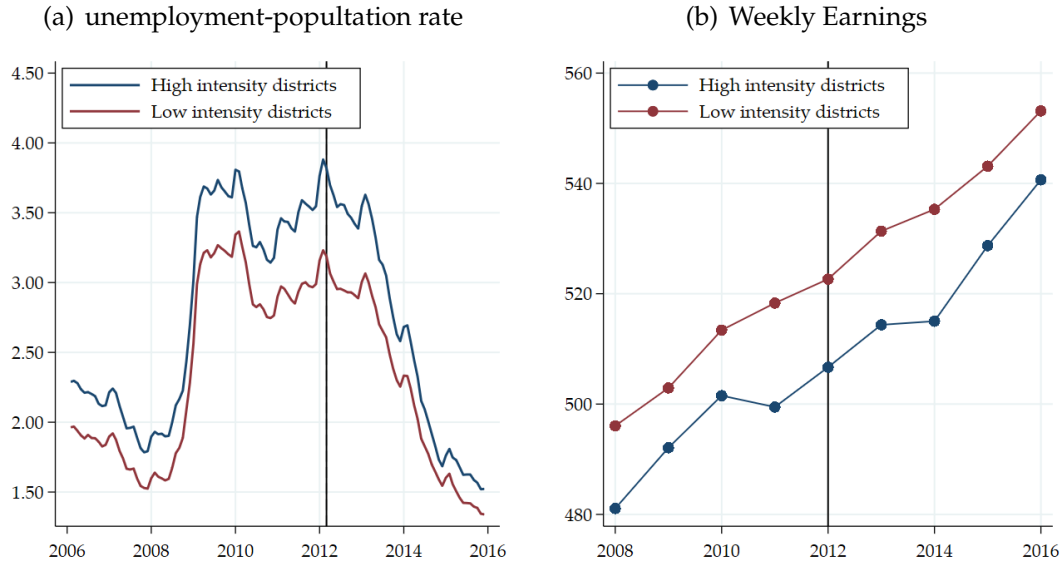**Figure 21:** Employment duration > 24, percent of baseline



*Note: Coefficients from difference-in-differences regression. Error bars represent 95% confidence intervals, standard errors clustered at the district level.*

**Figure 22:** Trends in local labour markets by treatment status

(a) unemployment-popultation rate

(b) Weekly Earnings



*Note: time series evolution of unemployment-popultation rates and weekly earnings in treated and untreated groups.*

**Figure 23:** Trends in local labour markets by treatment status
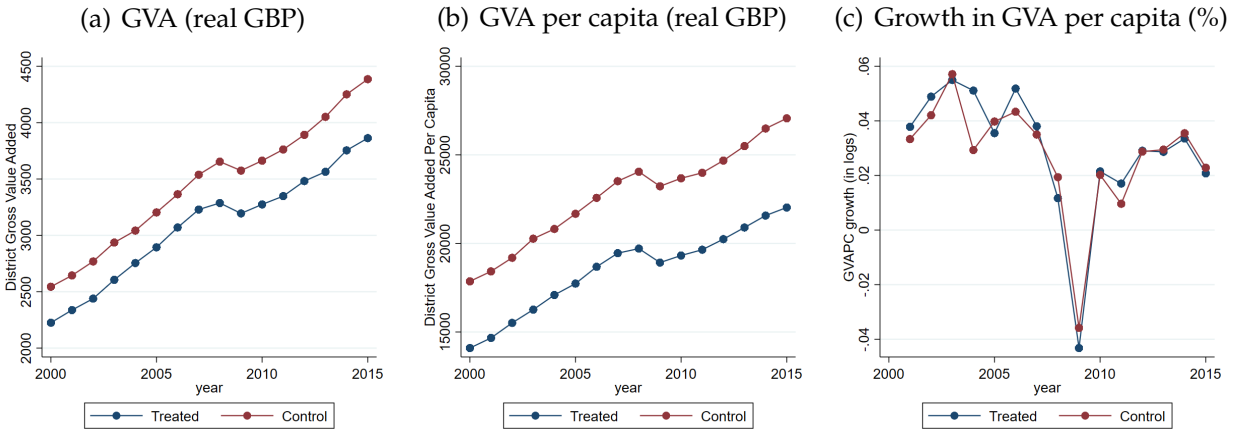
(a) GVA (real GBP)

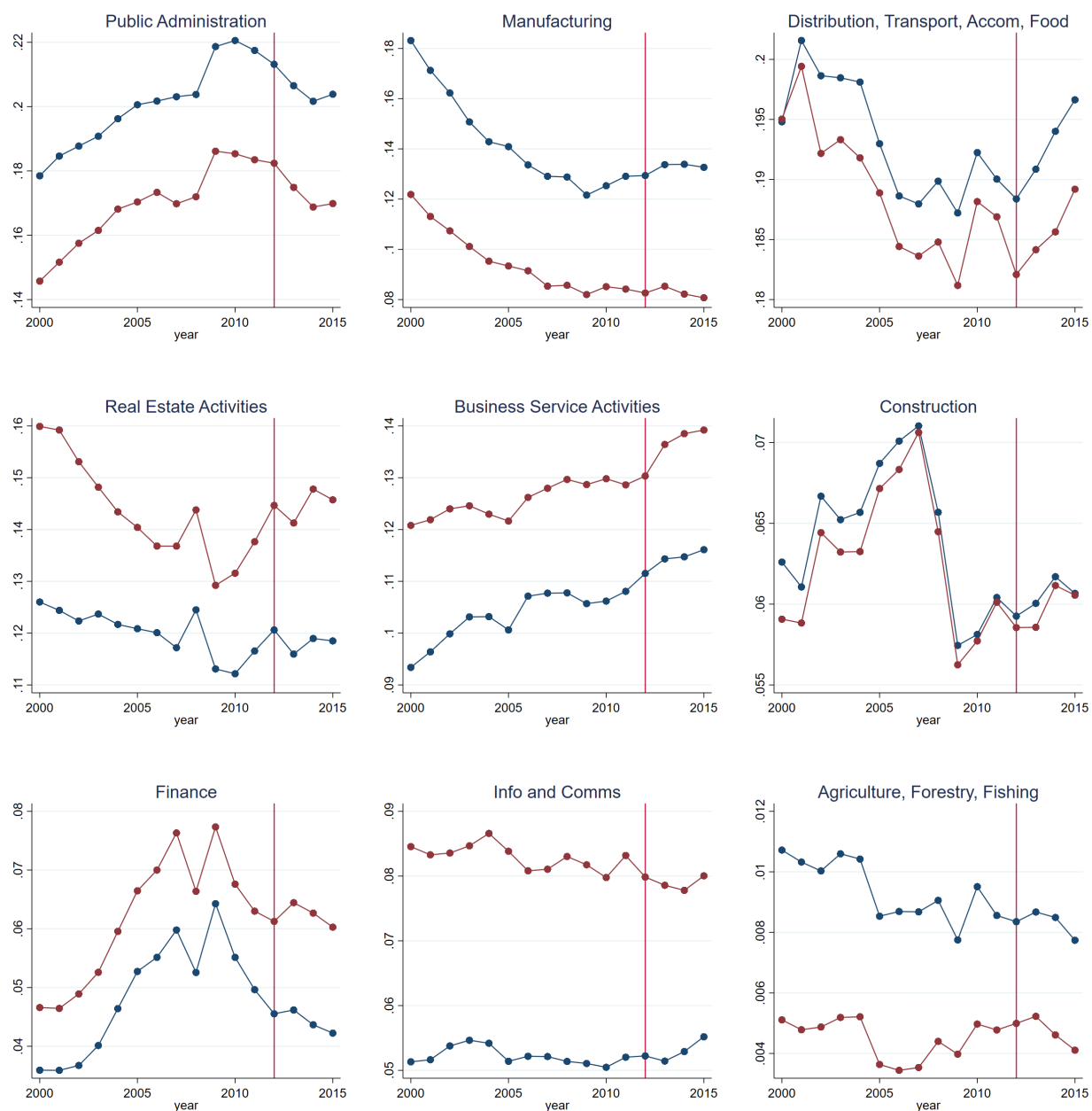(b) GVA per capita (real GBP)

(c) Growth in GVA per capita (%)



*Note: time series evolution of unemployment-popultation rates and weekly earnings in treated and untreated groups.*

**Figure 24:** Trends in Average District-level Industry GVA-Shares by treatment status



*Note: time series evolution of industry GVA shares in treated and untreated groups.*

# B Tables

**Table 1:** Intensive Margin of Sanctions within Infractions

| Infraction Level | Example Reasons | Old Sanction | New Sanction |
|---|---|---|---|
| Lower | Failure to attend advisor meeting<br>Failure to attend work program | 1 week | 4 weeks, 13 weeks |
| Intermediate | Unavailable to work<br>Ineligible search effort | No Sanction | 4 weeks, 13 weeks |
| Higher | Refusing, voluntarily leaving work<br>Dismissal for misconduct | 1-26 weeks | 4 weeks, 26 weeks, 156 weeks |

**Table 2:** Description of Key Variables

| Variable | Type | Description |
|---|---|---|
| *Unemployment* | | |
| start, end date | date | reported transition dates into and out of unemployment (month) |
| duration | integer | duration of unemployment spell in months (end - start) |
| 1(exit) | binary | 1 if spell ends in month $t$, 0 otherwise |
| 1(exit employed) | binary | 1 if spell ends in month $t$ and employed $t+1$, |
| 1(exit non employed) | binary | 1 if spell ends in month $t$ and not employed $t+1$ |
| $N^u$ | integer | cumulative count of unemployment spells (count EU transitions) |
| | | |
| *Employment* | | |
| Income | cont. | real monthly labour earnings |
| duration | integer | duration of continuous employment across any jobs, employers |
| | | |
| Sanction Threat | cont. | ratio of sanctions to UI claimants in district $g$ month $t$ |

**Table 3:** Regression Results: Placebo and ATT estimates

| | Exit | | | Unemployment | | Re-employment | | | Sanction |
|---|---|---|---|---|---|---|---|---|---|
| | total | employed | retired | $N_u$ | $N_u \geq 3$ | >12 | >24 | >36 | |
| $\alpha$ pl. (ppts) | -0.00109 (-1.64) | -0.00104 (-1.63) | -0.00120 (-0.95) | -0.000707 (-0.37) | 0.000120** (2.03) | 0.00175 (1.02) | 0.00160 (0.86) | 0.00135 (0.66) | -0.000423* (-1.89) |
| $\beta$ ATT (ppts) | 0.00860*** (3.03) | 0.00796*** (2.91) | 0.00455 (0.84) | 0.112*** (6.65) | 0.00835*** (2.92) | -0.0396*** (-2.85) | -0.0410** (-2.45) | -0.0631*** (-3.70) | 0.0170*** (14.92) |
| $\beta$ ATT (pct) | 0.191*** (3.03) | 0.205*** (2.91) | 0.150 (0.84) | 0.101*** (6.65) | 15.92*** (2.92) | -0.0504*** (-2.85) | -0.0583** (-2.45) | -0.0952*** (-3.70) | 0.418*** (14.92) |
| N | 59070 | 59070 | 12696 | 59070 | 59070 | 59070 | 59070 | 59070 | 58672 |

*Note: each column represents the output from a separate regression following eqn. (7). Dependent var in the column header. ATT(ppts) presents the raw coefficient, while ATT(pct) adjusts for baseline pre-reform averages in the treated group, pl. represents placebo coefficient. Standard Errors are clustered at the district level.*
[multiply dep var by 100, max 4 dps, replace tstats with ses, add row for effect for 1ppt increase]

# C What does a $2 \times 2$ DID estimate?

Let $Y_{igt}$ be the observable data. Let $(Y_{igt}(0), Y_{igt}(1))$ be untreated and treated potential outcomes respectively (assuming SUTVA, there are two potential outcomes indexed by own-treatment only). Let $\Delta_{it}$ denote unit level treatment effect, $\Delta_{it} = Y_{igt}(1) - Y_{igt}(0)$

$$\beta^{DD} = E[y_{it+1} - y_{it}|D_i = 1] - E[y_{it+1} - y_{it}|D_i = 0] \quad \text{Difference in Differences} \tag{16}$$

$$= E\left[y_{it+1}(1) - y_{it}(0)|D_i = 1\right] - E\left[y_{it+1}(0) - y_{it}(0)|D_i = 0\right] \tag{17}$$

Under a parallel trends assumption: same evolution of $Y(0)$, second difference term can be substituted:

$$E\left[y_{it+1}(0) - y_{it}(0)|D_i = 1\right] = E\left[y_{it+1}(0) - y_{it}(0)|D_i = 0\right] \tag{18}$$

Plug back in:

$$\beta^{DD} = E\left[y_{it+1}(1) - y_{it}(0)|D_i = 1\right] - E\left[y_{it+1}(0) - y_{it}(0)|D_i = 1\right] \tag{19}$$

$$= E\left[y_{it+1}(1) - y_{it+1}(0)|D_i = 1\right] \tag{20}$$

$$= E\left[\Delta_{it+1}|D_i = 1\right] \quad \text{Average Treatment Effect on Treated} \tag{21}$$