Job Search and the Threat of Unemployment Benefit Sanctions

Thomas Walsh
European University Institute

University of Bath

13.02.23

Introduction

Unemployment Insurance:

- smoothes consumption while searching
- insurance-incentive tradeoffs

Sanctions policy tool to blunt the tradeoff:

- partial or complete stop of transfers for (e.g.) low search effort
- smoothing with less moral hazard
- jobseekers create worse matches (“market insurance")

Channels:

- direct: punishment via budget constraint
- **indirect: threat, deterrent via expectations**
  - much larger group!
  - e.g: if 10% ever sanctioned, 9X larger

Introduction

Policymakers tend to “toughen up" the UI regime after recessions (GFC, Covid)

- boost labour supply
- fiscal budget / austerity
- UK reform in 2012 *(other examples: UK, France, Germany 2022)*

Research Questions

Does sanction threat affect search behaviour

- speed of exit from unemployment

To what extent does sanction threat create worse matches?

- stability of matches formed

$\Rightarrow$ To answer, empirical strategy uses **UK Sanction Policy Reform in 2012**

- exploit **differential responses** across districts in sanctioning rate
- spatial heterogeneity in sanction response lends itself to **Difference-in-Differences** design

Snapshot of Results

Define: sanction threat as **sanctions-per-UI claimant** in the jobseeker's local area

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- reform increases hazard rate $\sim$ 20 percent (100% direct effect $\times$ 0.05-0.1)
- a 1ppt increase in sanction threat raises exit hazard by 0.5 ppts (baseline: 4.5)

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④ **treated districts have worse reemployment durations**

- $\sim$ 5-10 percent less likely to reach 12/24/36 months continuously employed

Existing Literature and This Work

- **A UI system matters for eqm job characteristics, match quality/suitability to skills**: Acemoglu (2001); Acemoglu and Shimer (1999, 2000); and Marimon and Zilibotti (1999).
- **...and UI duration for reemployment wages** Nekoei, Weber (2017) Card, Chetty, Weber (2007)
- **Direct Sanction Effect on Exit from unemployment** in most studies (typically northern EU) Abbring & Van Den Berg* (2003) Boockmann, Thomsen, Walter (2014) Arni, Lalive, van Ours (2013), van der Klaauw & van Ours (2011) Boone, Sadrieh, van Ours (2009), Lalive, Zweimueller, van Ours (2005) Lombardi (2019)*
- **...and inactivity** Petrongolo (2009)
- **some evidence local average sanction rates** also matter Lombardi, 2019

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To differentiate my work:

- focus on indirect effect
- approach identification from a new longitudinal angle (DID vs "timing of events")

UK unemployment insurance system

Institutional Details

Unemployment Insurance in UK

- search not duration or contribution contingent

Possible Reasons for a sanction:

- Failure to attend advisor meeting / work program
- Unavailable to work
- Ineligible search effort
- Refusing work, voluntarily leaving work
- Dismissal for misconduct

Features of UI/sanctions:

- sanction = UI payments stopped, typically for 4 weeks.
- About 70 GBP/week (80 EUR) , flat over time in real terms.
- Referal from jobcentre caseworker, imposed or cancelled by separate decision maker

Job Displacement, Earnings Losses, and Sanction Threat

Earning losses due to job displacement

Displacement event study regression:

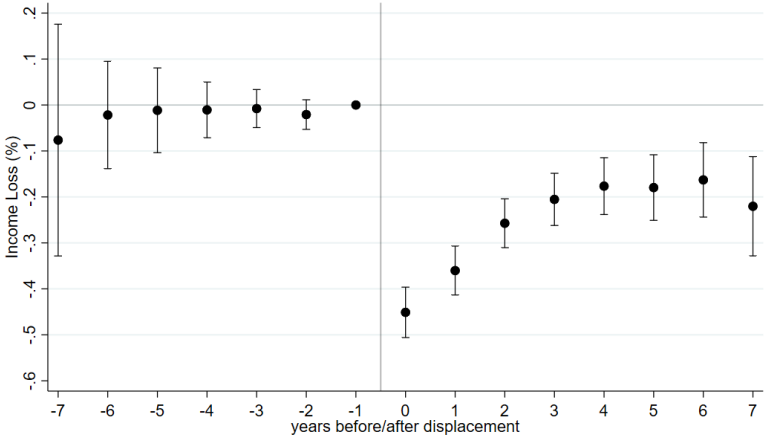
- Staggered Diff-in-Diff (job loss in different years)
- stacking estimator of Cengiz et al (QJE,2019), combines many 2×2 diff-in-diffs
- makes clean comparisons of displaced vs not-yet-displaced / *i.e. is stagger-robust*

$$y_{ict}(r) - y_{ict}(-1) = \lambda_t + \sum_{r=-7}^{7} \alpha_r \mathbb{1}_{\{r\}} + \sum_{r=-7}^{7} \beta_r \left(\mathbb{1}_{\{r\}} \times \textit{displaced in year } c_{ic} \right) + \varepsilon_{it} \quad (1)$$

- outcome: earnings
- normalised to $r = -1$ in relative event time

▸ stacking regression

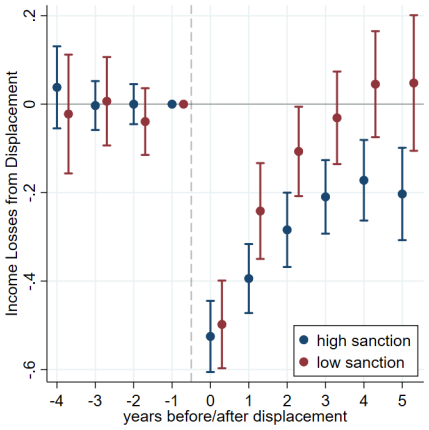
Figure 1: Earnings Losses from Job Loss



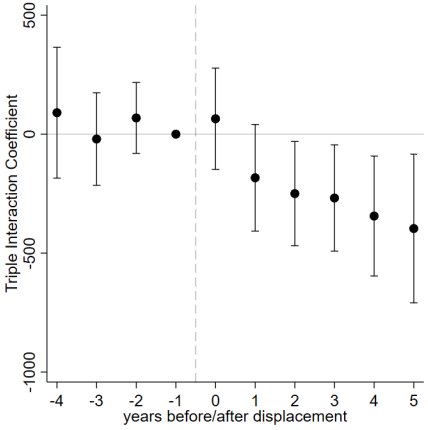
Sample: Ever-displaced only. Treated: lose job in year ***t***, control: not-yet-treated by ***t***.
Excludes zero earnings. Including zeroes leads to approx -40pct.

Figure 2: Earnings Losses by high/low sanction threat in early unemployment

(a) High and low sanction threat

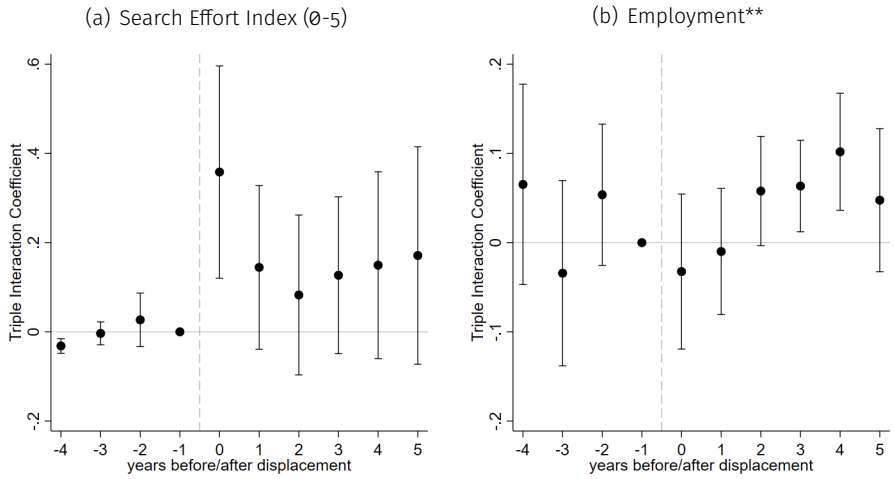


(b) Triple differences (high-low)



High sanction: average sanction rate in first 3 months of spell above/below average

Figure 3: Triple-Differences Estimates comparing displacements with high vs low sanctioning



less conservative sample restriction: employed in $r = -1$ only. **Employed at time of survey**

Policy Reform Details

UK Sanctions Regime and Reform

Effects of Sanction Reforms:

- changes to minimum sanctions
- large increases in sanctions-per-unemployed estimated +40% of baseline
- heterogeneity in response to reform across districts

Sources of Variation:

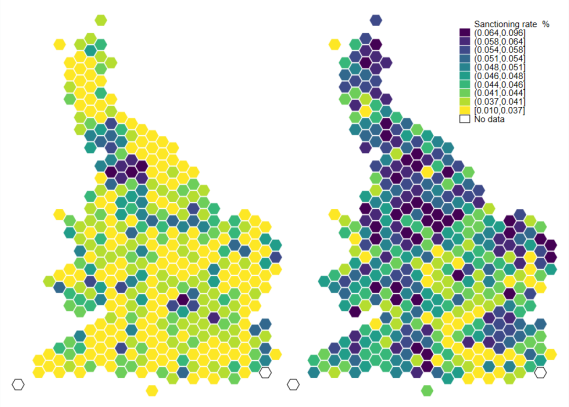
- degree of job centre discretion/autonomy
- use of sanction/exit targets

National Audit Office: “The NAO concludes it is likely that management focus and local work coach discretion have had a substantial influence on whether or not people are sanctioned (...) heterogeneity [in sanction rates across areas] not fully explained by jobseeker characteristics"

▸ minimum sanctions

Reform Effect on Sanction Intensity

Figure 4: Sanctioning Rates (%, pre vs post)



$$S_{gt} = \frac{number\ of\ sanctions\ issued_{gt}}{number\ of\ UI\ claimants_{gt}}$$

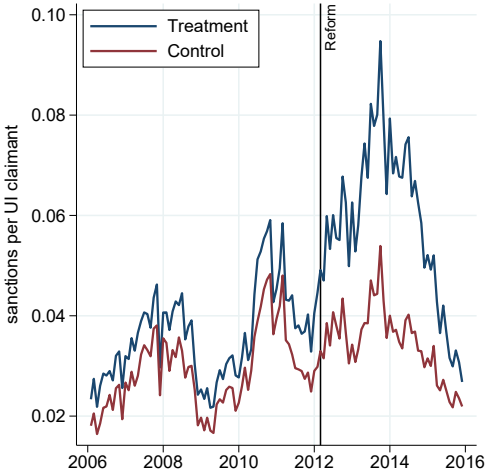
Defining treatment groups:

- approx movers v stayers
- very similar levels in t
- Treated: upper 25% in $t+1$
- Control: lower 25% in $t+1$
- very correlated with changes $(t, t+1)$

map | hexmap | London

Identifying Variation

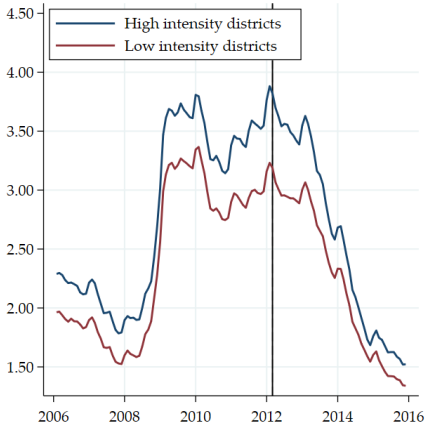
Figure 5: sanctioning rate (%)



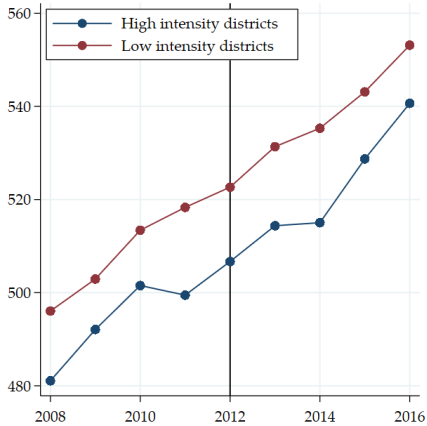
- Treatment defined in data-driven way
- *Threat*: local shocks drive policy actions
- examine other variables to look for shocks
- ▸ distributions in groups

Figure 6: Equilibrium Labour Market Conditions

(a) Unemp-Population Ratio (%)

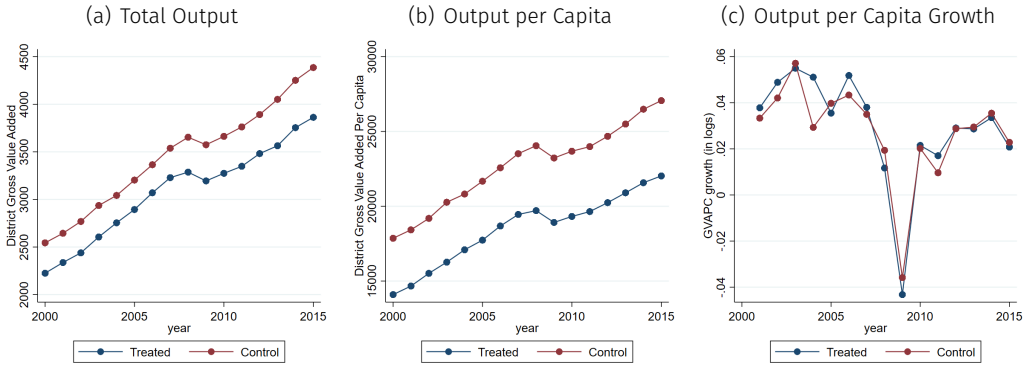


(b) Weekly Earnings



District Trends

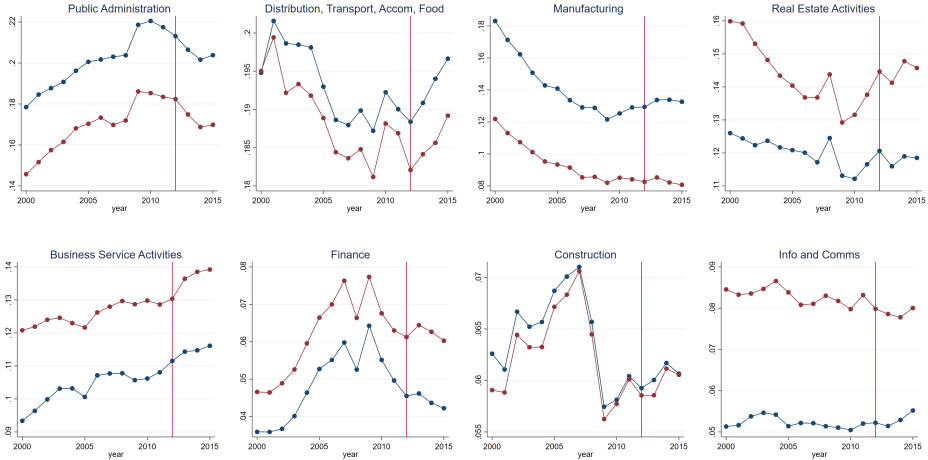
Figure 7: District-level Output (Real GVA)



*excludes Westminster and City of London due to high business concentration

District × Industry Trends

Figure 8: District-Industry Output Shares ($GVA_{ind,dist,year}/GVA_{dist,year}$)



Datasets

1. Working Life Histories extracted from UKHLS ("*Understanding Society*")

- longitudinal panel follows 40,000 households from 2009
- monthly history of labour market activity
- annual: income, hours, occupation, transfers, commute time, search effort
- (+) can potentially see items not tracked in admin data
- (-) loss of precision of exact month of transitions

Sample:

- 60,000 person-months in unemployment,
- 2009m1-2015m12
- restricted to ages 18-64
- median unemployment duration 9m, mean 12m, 68% below 1y

2. District-level variables and National macro variables sanction information etc.

→ Matched assuming no district changes between waves, can't see any very short-term moves.

Economic Activity in UKHLS

Activity state by month:

- *"Which best describes your current situation?"*
  - Self-employed
  - In paid employment
  - **Unemployed**
  - Retired
  - On maternity leave
  - Caring for family/home
  - FT student
  - LT sick, disabled
  - Gov training scheme
  - Unpaid worker in family business
  - Apprenticeship
  - Something else
- transitions map well to "event studies" and national statistics (LFS)

transitions

Causal Estimates

Empirical Strategy

Difference-in-differences design

- use changes in controls to infer changes that would have happened in treatment group, absent treatment

Identifying variation in the data

- exploit heterogeneity in intensity responses to common reform
- T,C selected in a data-driven way

Identifying assumptions

A1. No Spillovers

- neither *across space* nor *through time*
- outcome depends only on *own contemporaneous treatment status*

A2. Common Trends.

- Absent treatment, Treated and Controls would have followed the same **changes** in outcomes
- DiD can tolerate a degree of endogeneous treatment
- can't handle contemporaneous reforms or asymmetric shocks

(A1+A2+panel data): ATT/average treatment effect *on the treated* is identified

▸ assumptions

Addressing Potential Threats to Identification

Local idiosyncratic shocks (reverse causality)

- Pre-trends cannot guarantee local shocks in 2012 didnt drive policy response
- Test economic conditions for diverging outcomes

Migration driven by policy

- **mixing**: high to low unemployment, T-C gap closes, biased towards zero
- **polarising**: high to higher S, low to lower S, estimates biased away from zero

Spillovers across space

- weekly jobseeker-advisor meetings, will be updated on current, local sanctioning rate
- T-C gap would close, estimates biased towards zero (again, if mixing spillovers)
- high degree of clustering of T/C poses a problem

Estimating Equation

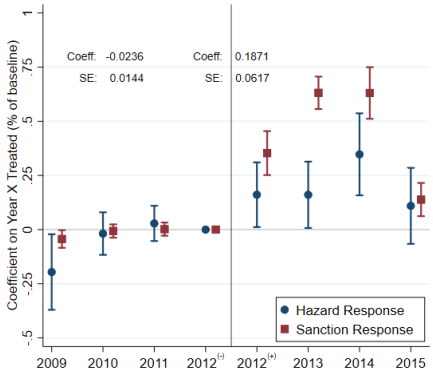
Canonical **2x2** simultaneous-reform Difference-in-differences:

$$\mathbb{1}\{\textit{exit}\}_{igt} = \overbrace{\lambda_t + \gamma_g + \theta_{T(i,t)}}^{\textit{common trends plus duration}} + \underbrace{\sum_{\ell=2009}^{2015} \beta_\ell \times (\mathbb{1}\{t = \ell\} \cdot \textit{Treated District}_g)}_{\textit{placebo effects/treatment effects}} + u_{igt} \quad (2)$$

- $\theta_{T(i,t)}$ duration-of-spell effects
- two-stage estimation. $(\lambda, \gamma, \theta)$ using untreatd (controls, pre-reform treated) obs (Gardner,'22)
- if treatment affects exit, it necessarily changes duration-of-spell

Figure 9: Difference-in-Differences Estimates

(a) Exit rate, total (% of baseline rate)



(b) Exit rate, into employment (% of baseline)

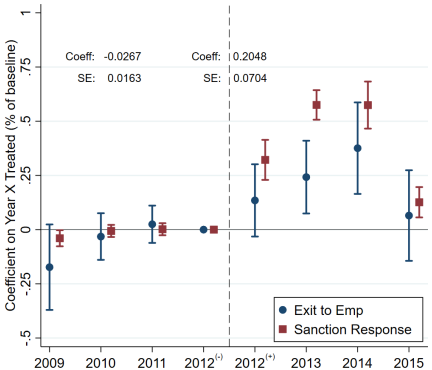
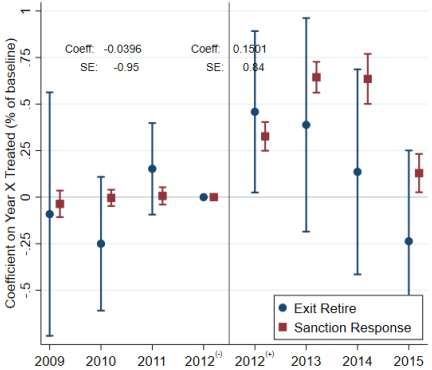


Figure 10: Exit to retirement

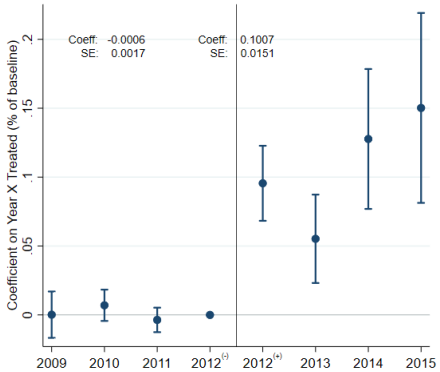
(a) Exit to retirement (% of baseline rate)



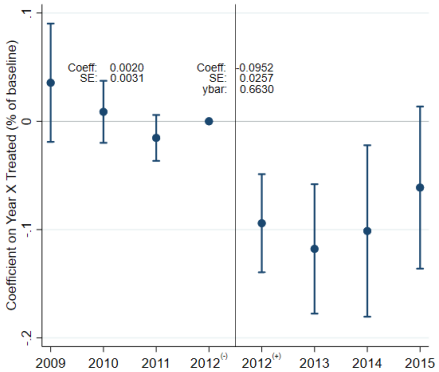
Post-reform spike one-and-done effect. Very low precision.

Figure 11: Reemployment Stability

(a) Cumulative Unemployment Spells[1]



(b) Pr(reemployment tenure $>$ **36** months)



[1]since start of sample

Estimated ATTs

Table 1: Regression Results: ATT estimates

	Exit rate			**Unemp.**	**Re-employ. duration**			**Sanction**
	total	employed	retired	N_u	>12m	>24m	>36m	
β (ppts)	0.00860*** (3.03)	0.00796*** (2.91)	0.00455 (0.84)	0.112*** (6.65)	-0.0396*** (-2.85)	-0.0410** (-2.45)	-0.0631*** (-3.70)	0.0170*** (14.92)
β (%)	0.191*** (3.03)	0.205*** (2.91)	0.150 (0.84)	0.101*** (6.65)	-0.0504*** (-2.85)	-0.0583** (-2.45)	-0.0952*** (-3.70)	0.418*** (14.92)
NT	59070	59070	12696	59070	59070	59070	59070	58672

Back of Envelope Calculation: Direct and Indirect effects of Sanctions

- worst case scenario: +20% is total effect
- assume: average sanction duration is 1 month
- incidence (flows) = prevalence (stock) of sanctions
- direct effect estimate +100%
- policy induces +4ppts in prevalence

$$\textit{Total effect} = (\Delta \textit{prevalence} \times \textit{hazard estimate from lit}) + \textit{Indirect Effect} \tag{3}$$

$$\textit{Indirect Effect} = 0.20 - 0.04 \times 1 \tag{4}$$

Conclusion

Main takeaways:

- Sanctioning policy acts on a **wide set of job-seekers**, not just the directly punished.
- Effects go **beyond exit rates**.
- **fast exits, less stable jobs with more unemployment risk**
  $\Rightarrow$ optimal UI design with endogeneous unemployment risk
- **implies a intensive-extensive margin tradeoff: duration of spell vs number of spells**
  ($\Rightarrow$ welfare implications)

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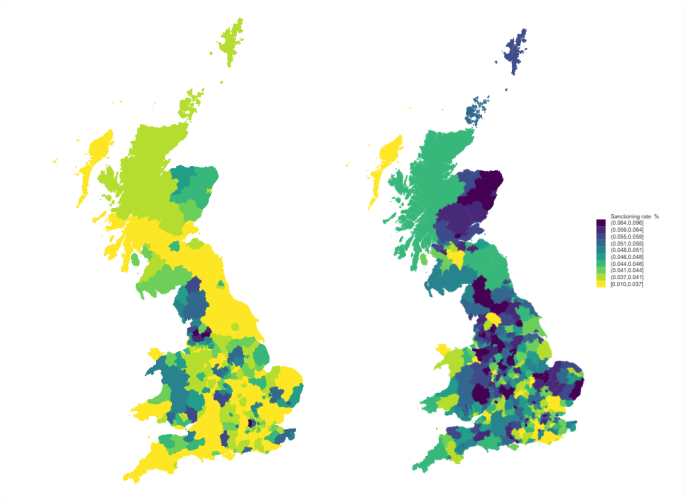
Thanks! thomas.walsh@eui.eu

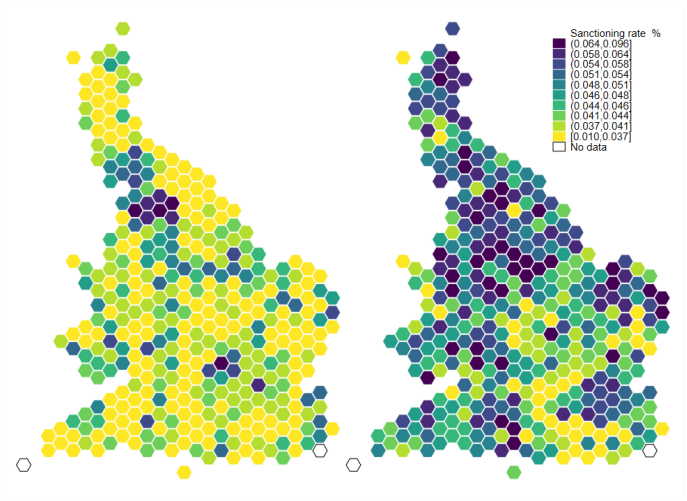
Appendix

Mapping Sanction Rates, 2010/12 vs. 2012/14



back

Mapping Sanction Rates, 2010/12 vs. 2012/14



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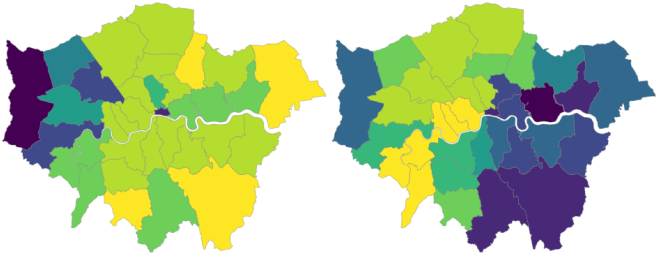
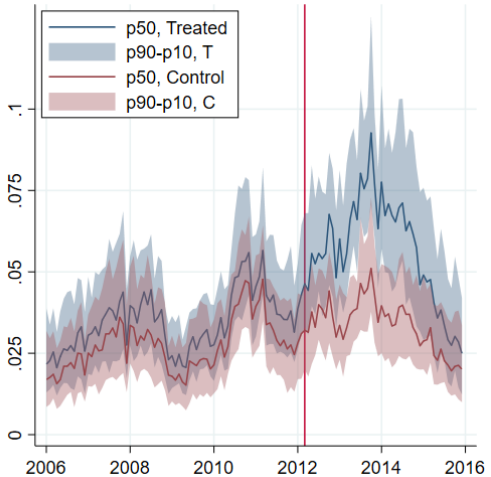
Mapping Sanction Rates, 2010/12 vs. 2012/14; London



back

Distribution on Sanction Rates within Group



pictures page

Intensive Margin of Sanction Reform

Infraction Level	**Example Reasons**	Sanction in weeks	
		Before	*After*
Lower	Failure to attend advisor meeting Failure to attend work program	1	4,13
Intermediate	Unavailable to work Ineligible search effort	0	4, 13
Higher	Refusing, voluntarily leaving work Dismissal for misconduct	1-26	4, 26, 156

Table 2: **Structure of Sanctions**

back

Identifying assumptions

Potential outcomes: $Y_{it}(\mathbf{D})$, where $\mathbf{D}$

A1. No Spillovers neither across space nor through time

$$Y_{it}(\mathbf{D}) = Y_{it}(D_{gt} \in \{0, 1\})$$

A2. Common Trends.
Absent treatment, Treated and Controls would have followed the same **changes** in outcomes

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

(A1+A2+panel data): ATT/average treatment effect *on the treated* is identified

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

back

Stacking Estimator

- Suppose treatment in each t is a different intervention (losing a job in 1998 $\neq$ in 2002)
- combine many small 2×2s

$$D_{98}(g,t) = \left(\begin{array}{c|cccccc} t & 97 & 98 & 99 & 00 & 01 & 02 \\ \hline g_{97} & 1 & 1 & 1 & 1 & 1 & 1 \\ g_{98} & 0 & 1 & 1 & 1 & 1 & 1 \\ g_{99} & 0 & 0 & 1 & 1 & & \\ g_{00} & 0 & 0 & 0 & 1 & & \\ g_{01} & 0 & 0 & 0 & 0 & 1 & \\ g_{02} & 0 & 0 & 0 & 0 & 0 & 1 \end{array}\right) \qquad D_{00}(g,t) = \left(\begin{array}{c|cccccc} t & 97 & 98 & 99 & 00 & 01 & 02 \\ \hline g_{97} & 1 & 1 & 1 & 1 & 1 & 1 \\ g_{98} & 0 & 1 & 1 & 1 & 1 & 1 \\ g_{99} & 0 & 0 & 1 & 1 & & \\ g_{00} & 0 & 0 & 0 & 1 & & \\ g_{01} & 0 & 0 & 0 & 0 & 1 & \\ g_{02} & 0 & 0 & 0 & 0 & 0 & 1 \end{array}\right) \tag{5}$$

- Can drop never-treated (NT)
- Top row doesn't have a control (always-treated, AT)
- Last row also left out (last-treated, LT)

back

Aligned on Event Time

Aligning and averaging the 2x2DiDs

$$D(g,t) = \begin{array}{c|cccc|ccccc} r & -4 & -3 & -2 & -1 & =0 & +1 & +2 & +3 & +4 \\ \hline g_{98} & & & & 0 & 1 & 1 & 1 & 1 & 1 \\ g_{99} & & & 0 & 0 & 1 & 1 & 1 & 1 & \times \\ g_{00} & & 0 & 0 & 0 & 1 & 1 & 1 & \times & \times \\ g_{01} & 0 & 0 & 0 & 0 & 1 & 1 & \times & \times & \times \end{array} \tag{6}$$

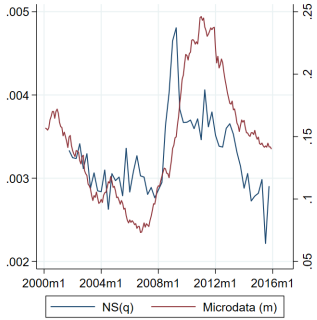
- $R = 8$ relative periods from $T = 6$
- can see why event-studies have widening error bands

back

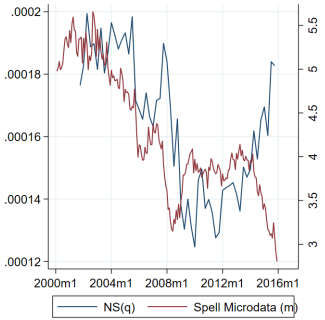
Understanding Society vs Labour Force Survey

Figure 12: Transition Rates

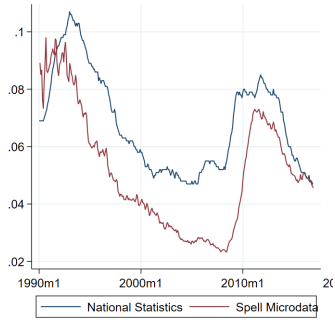
(a) Separtation Rate



(b) Job Finding Rate



(c) Unemployment Rate



back

1. “Job Search and the Threat of Unemployment Benefit Sanctions"
job search and unemployment / job quality / policy effectiveness

2. “Sectoral Volatility and the Investment Channel of Monetary Policy"
dispesion of productivity shocks / policy effectiveness

3. “Government Spending Multipliers in Firm-level Production Networks"
distribution of procurement / policy effectiveness / budget efficiency

4. “Making the Cut: Close Elections and Local Welfare Policies"
political origins of policy asymmetry in UK

Governance of Institutions and Systems

- Institutions, Incentives and Welfare
- Public Finance
- Political Economy

Innovation and Productivity

(Social Determinants of Health)

- information, taxes and incentives)