Making the Cut: Close Elections and Local Welfare Policy *

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Abstract

Welfare benefit sanctions are a common tool in many social insurance systems. Using a regression discontinuity design based on close elections, we investigate how representatives' political alignment influences the implementation of a new sanction regime in the UK. Our results show that constituencies aligned with the Conservative-led central government experience significantly fewer sanctions. The RD estimate indicates a drop of 0.8 percentage points, implying 18 % lower sanction rates in central government controlled constituencies. These findings demonstrate that legislators can influence local implementation of national, rule-based policies. Such political influence undermines institutions that should be neutral to local partisan considerations.

JEL codes: P16, D72, D73

Keywords: political parties, close elections, policy implementation, fiscal consolidation, unemployment benefits

1 Introduction

Unemployment benefit sanctions are a common tool used by policymakers in many search-contingent unemployment insurance systems. Unemployment insurance and other social security transfers represent a significant share of day-to-day government spending[1]. It is well established in the existing literature that politicians and other partisan actors often manipulate discretionary expenditures in an attempt to boost their electoral chances[2]. However, national economic institutions and rule-based, non-discretionary policies are generally presumed to be neutral to special interests. How partisan alignment influences the implementation of national welfare policy, especially at the local level, remains vastly understudied. Our paper aims to close this gap—we examine how the party affiliation of an MP affects local implementation of non-discretionary welfare spending.
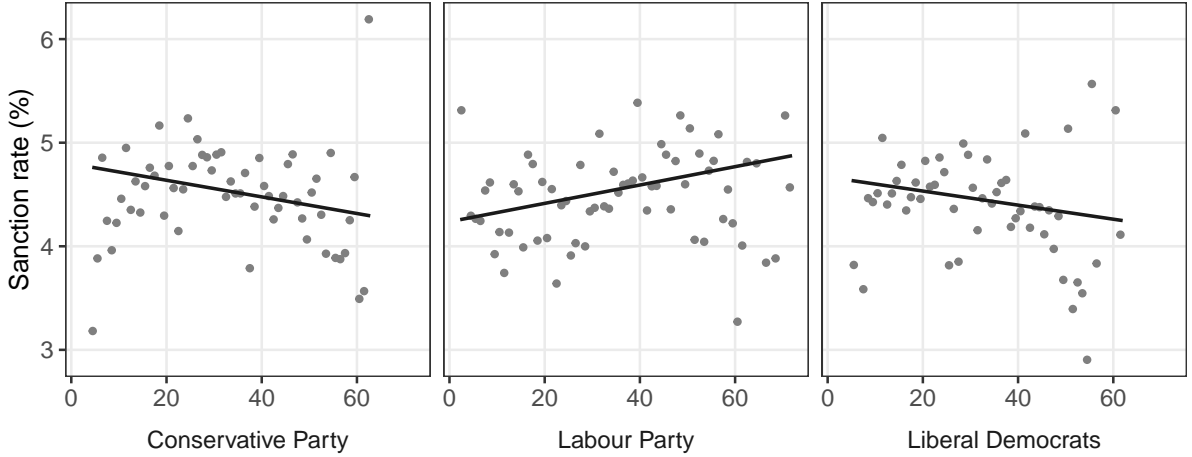
In the United Kingdom, the setting of our investigation, the unemployment benefit system is characterized by two salient facts in the aftermath of the global financial crisis and ensuing recession in the late 2000s. First, national reforms to unemployment benefit sanctioning policy drove a large increase in the overall average sanctioning rate. However, this masks substantial variation in reform-response across areas. Second, according to the National Audit Office, the large heterogeneity in sanctioning rates across comparable areas cannot be entirely explained by differences in jobseeker behavior.

We examine how the political alignment of a parliamentary seat influences the implementation of welfare reforms which aimed at reducing public spending in Westminster constituencies. In particular, we study the effect of representatives' alignment with the Conservative-led central government on local implementation of unemployment benefit sanctions. Although unemployment benefits are issued according to national legislation, there is considerable regional variation in the intensity of benefit sanctioning. Figure 1 illustrates that sanction rates are correlated with political party vote shares, with lower sanction rates in constituencies aligned with the Conservative–Liberal Democrat

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1. Social security transfers are 23 percent of 634.10 Billion GBP expenditures in the United Kingdom in 2010, the largest category ahead of health (18 %), pensions (18 %), and education spending (14 %)

2. https://www.reuters.com/world/uk/uks-sunak-criticised-claiming-he-diverted-funds-deprived-areas-2022-08-05/

Figure 1: Vote share in 2010 General Election vs unemployment benefit sanction rate

Notes: Binscatters of deciles of party vote share within a constituency in the 2010 general election and average Jobseeker's Allowance (JSA) sanction rate in the post-reform period (March 2012 – December 2015). The dots represent averages within running variable bins.

government. We examine whether these patterns are can be explained by local economic conditions or jobseeker effort, or if this variation is driven by politicians' demand for more lenient measures in their constituencies. We present novel causal evidence on the impact of political alignment on local sanction rates, and contribute to a deeper understanding of political influence on policy implementation.

Understanding partisan influence on allocation mechanisms and economic institutions is important for several reasons. First, the state provides unemployment benefits to help jobseekers smooth consumption. Partisan influence on the availability of social insurance, therefore, has direct welfare consequences for affected jobseekers. Second, it is crucial to understand to what extent local economic outcomes vary due to differences in jobseekers' search behavior versus differences in how their economic institutions operate. Third, this study can help explain some of the inequality in economic outcomes across areas, as the UK has particularly high regional inequality compared to other advanced economies. Finally, this research can inform policy design that is robust to such partisan pressures.

We use regression discontinuity design to estimate the causal effect of partisan alignment on the local implementation of the welfare reform. We leverage close races in UK's first-past-the-post parliamentary elections. Our design compares outcomes in electoral

districts where the Conservative-Liberal Democrat coalition narrowly won against the Labour Party, to districts where the Coalition was narrowly defeated by Labour.

Our results show that the implementation of the stricter sanctioning regime is significantly more *lenient* in the central government aligned constituencies. The estimates indicate a drop of 0.8 percentage points at the electoral threshold, on a baseline rate of 4.5 %. This implies 18 % lower sanction rates in Coalition controlled constituencies. The effect is driven by decreased number of sanctions, while the number of unemployment benefit claimants does not significantly differ between Coalition-held and Labour-held seats. The effect is concentrated in the years immediately after the 2012 welfare reform, with no difference in the sanction rates prior to the reform. The findings demonstrate that partisan alignment has a systematic influence on the implementation of national welfare policy.

One of the core assumption of most political models is that politicians seek to maximize their chances of re-election. Since welfare spending cuts are unpopular, legislators have an electoral incentive to mitigate the effects in their constituencies. Our findings are consistent with such electoral incentives: We find largest estimates for newly gained seats the were previously controlled by the Labour Party. We hypothesize that the ruling party has access to more influence channels, and thus more power over public employees and policy implementation. Since close elections take place in more competitive constituencies, the winners have particularly strong incentives to provide favorable public spending distributions and secure support in those districts.

Our paper contributes to several strands of literature. First, we provide novel causal evidence on the impact of political alignment on policy implementation. Our paper contributes to the literature studying the relationship between legislative representation and geographic distribution of public spending (see e.g., Gagliarducci et al. 2011; Golden and Min 2013). While this research has shown that representation and partisan alignment influence *discretionary* spending, the impacts on *non-discretionary* spending remain vastly understudied. Our focus on the period of welfare reforms in 2010's allows us to provide empirical evidence on the impact of political alignment on local implementation

of national welfare policy and spending cuts.

Second, our paper contributes to research studying political influence on policy implementation. Not much is known about how politicians influence outcomes not directly under their jurisdiction, such as welfare benefits and sanction policy, which is based on national legislation. Because the jobcentres administering unemployment benefits are under the jurisdiction of the Department for Work and Pensions, there is no official direct link between an MP and the job centres in their constituency. While previous research has examined the interaction between politicians and bureaucrats, focusing on political accountability and bureaucratic selection (e.g., Gallo and Lewis 2012; Colonnelli et al. 2020), we examine a different perspective: How does alignment with the central government affect legislators' ability to influence public employees? Our paper provides new insights into factors affecting politicians' ability to control bureaucrats and influence policy implementation.

Finally, our paper contributes to the literature on the political economy of welfare policies. Specifically, we connect to studies on political costs of fiscal consolidation policies (Alesina et al. 2012; Bansak et al. 2021; Hübscher et al. 2021; Gabriel et al. 2023), not only focusing the allocation of economic goods to political co-partisans, but also highlighting the allocation of "bads".

The rest of the paper is organized as follows. Section 2 reviews the related literature. Section 3 provides a description of the institutional setting and data. Section 4 describes the empirical strategy. Section 5 presents the results. Section 6 explores potential mechanisms. Section 7 concludes.

2 Related Literature

There is extensive evidence of legislators targeting discretionary spending to their electoral district. More representation for a district translates to more bargaining power, which can lead to more targeted spending (see e.g., Ansolabehere et al. 2002; Knight 2008; Dragu and Rodden 2011). Similarly, representation by the party in government can increase targeted spending as government membership can give the legislators' greater influence over

discretionary government spending (Baron and Ferejohn 1989; Albouy 2013).

In the UK context, Fouirnaies and Mutlu-Eren (2015) show that government parties in England target discretionary funds to local councils controlled by co-partisans, particularly in tight races and around election years. The UK Towns Fund, which allocated funds with the aim to improve towns' infrastructure, also skewed towards Conservative and marginal-Conservative seats in particular (Hanretty 2021).

The previous literature has focused almost exclusively on discretionary spending, which is malleable to special interests (e.g., Dahlberg and Johansson 2002; Brollo and Nannicini 2012; Albouy 2013). Our investigation adds to the literature by examining the impact of representation and political power on non-discretionary spending. A paper close to ours is Jennes and Persyn (2015), which studies how representation in the executive branch influences geographic variation in formula-based spending—income tax transfers, social security contributions and social expenditures—which is based on national legislation. Further research studying non-discretionary spending is focused on developing countries, where the influence of political factors is generally explained by corruption and weak institutions, a mechanism that is unlikely in our setting (see e.g., Banful 2011; Litschig 2012; Malik 2021).

Legislators' influence on formula-based spending is not well explained by the basic models of legislative bargaining. Another possibility is that MPs are able to influence the practical implementation of spending and spending cuts. A possible channel is through control over public employees. Therefore, our paper is also related to the literature examining interaction between politicians and bureaucrats. In general, the theoretical literature on delegation suggests that a principal prefers agents whose preferences are aligned with them (see e.g., Holmstrom 1980).[3] The theory of motivated agents suggests that bureaucrats have political preferences and derive utility from implementing a specific political agenda, and therefore matching the preferences of politicians and bureaucrats can enhance organizational efficiency (Besley and Ghatak 2005).

The emerging empirical literature confirms that politicians prefer to appoint politically

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3. For a review of the theoretical literature see e.g., Gailmard and Patty (2012).

aligned bureaucrats. Evidence for instance from Denmark (Christensen et al. 2014), Sweden (Dahlström and Holmgren 2019), and Germany (Bach and Veit 2018) points to politicization of the bureaucracy, suggesting that politically aligned bureaucrats are less likely to be replaced, and are more likely to be promoted to high public offices. Political alignment is also found to matter for lower ranking offices as well as personnel in public services (Brassiolo et al. 2020; Akhtari et al. 2022). Politician-bureaucrat alignment can also affect the intensive margin of public sector employment. Fiva et al. (2021) find that political shifts that cause top bureaucrats to become aligned with the local council lead to higher wage growth for the bureaucrats.

Political appointments can align politicians' and bureaucrats incentives and enhance accountability and effectiveness (Toral 2022). However, the preference for political alignment may also come at a cost as it may undermine the role of competence and lead to worse performance (Colonnelli et al. 2020; Gallo and Lewis 2012). Increased employee turnover can also cause disruptions in functioning of the bureaucracy and public service provision (Akhtari et al. 2022).

Our work also relates to a rich literature studying the effects of unemployment benefit sanctions on search behavior and subsequent outcomes like reemployment wages and stability. McVicar (2020) provides a recent summary of research in this area. However, our work aims to explore drivers of variation in sanction rates across localities. Our findings also relate to the empirical literature on political costs of fiscal consolidation. The previous literature is somewhat contradictory: While Alesina et al. (2012) find no evidence of an electoral backlash, recent literature shows that fiscal consolidation hurts governments' re-election chances (Hübscher et al. 2021), depresses voter turnout and increases extreme parties' vote share (Gabriel et al. 2023). Our findings help explain how the political costs of fiscal consolidation may be mitigated.

3 Institutional Background and Data

3.1 Welfare Reform & Unemployment Benefits in the UK

The context of our investigation is the UK 2010 parliamentary election and the 2012 welfare reform. This election is particularly noteworthy as it marked the end of the Labour era of government from 1997 until 2010. The Conservative-Liberal Democrat coalition government introduced a national welfare reform, which was enacted in March 2012. The reform sought to reduce public spending by restricting eligibility to benefits, reducing the financial value of benefits, and by increasing incentives to take up employment. The reform included changes to several benefits, including housing benefits, unemployment benefits, and child support, as well as a household benefit cap and a reduction in annual up-rating of value of benefits.

We focus on unemployment benefits. The Department for Work and Pensions (DWP) is a government department responsible for welfare policy and it administers unemployment benefits through Jobcentre Plus. There are about 800 jobcentres in the 2010–2015 election term. Jobcentre Plus used to be executive agency with its own chief executives and senior management structures, that reported to DWP. In October 2011 it was brought back under the direct control of the DWP and its single departmental board and executive team. This change could have made it easier for MPs to exert influence through the DWP. The reform also increased pressure to reduce personnel costs in the public administration, and personnel in the DWP has been declining since 2010—a situation which may create opportunities for political influence (see Figure D.6).

Jobseeker's allowance (JSA) is an unemployment benefit for claimants who are not in full time employment and are actively seeking for work. Claimants have to fill in a Jobseeker's Agreement and go to a Job Centre every two weeks to certify that they are still seeking work. Between 2010–2015, JSA payments constituted about £4.4bn annually, or about 3 % of UK government benefit expenditure in Great Britain.

An unemployment benefit sanction means a loss of the benefit for a duration which depends on the offense. The welfare reform led to a new sanctioning regime for the JSA

from 22nd October 2012. The main changes are outlined in Table 1. The new sanction rules include three categories of sanctions, and the length of the sanction in a given level depends on the number of previous sanctions in the past year.

If a jobseeker does not fulfill all the requirements to keep their benefit, they are referred for a decision to be made on whether to impose a sanction. Sanction decisions are made by a separate DWP decision maker rather than the case worker. A sanction may not be applied if the jobseeker can present good reason for the infraction. However, what constitutes a "good reason" is one source of discretion contributing to variation in implementation of sanctions. Roughly half of all referrals lead to a decision to apply a sanction, and majority of sanctions (65 %) are imposed for low level infractions. Figure 2 plots the average number of JSA claimants, sanctions, and sanction rate over time. Although the number of claimants remains fairly stable during the election term, the rate of sanctions increases substantially post-reform.[4]
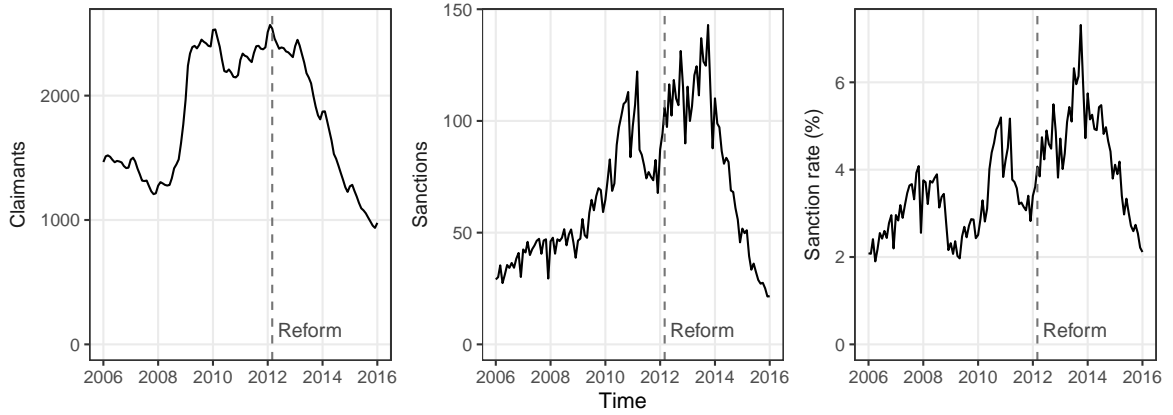
The reforms to sanctioning policy announced in March 2012 altered both the intensive and extensive margin of sanctions. Most notably, we see that dispersion of sanction rates across localities increases substantially post-reform, while dispersion across localities remained fairly stable in the years before the reform. Even the events of the Global Financial Crisis/Great Recession did not alter variance in sanction rates qualitatively, while it is only the 2012 reforms which trigger large growth in sanction intensity-inequality.

Table 1: Sanction policy changes following 2012 welfare reform

Infraction Level	Example Reasons	Old Sanction	New Sanction
Lower	Failure to attend advisor meeting Failure to attend work program	1 week	4 weeks, 13 weeks
Intermediate	Unavailable to work Ineligible search effort	disentitlement	4 weeks, 13 weeks
Higher	Refusing, voluntarily leaving work Dismissal for misconduct	1-26 weeks	13 weeks, 26 weeks, 156 weeks

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4. The reform also introduced a new benefit called Universal Credit, which will replace most of the means-tested working-age benefits, including Jobseeker's Allowance. It was first introduced in 2013 in four pilot local authorities for new claimants, but the subsequent roll-out was slow (see Figure D.1). Some of the decline in number of JSA claimants and sanctions in 2015 is explained by new claims being moved to Universal Credit.

Figure 2: Number of Jobseeker's Allowance (JSA) claimants and sanctions

Notes: Monthly averages across constituencies. The vertical dashed lines indicate the enactment of the Welfare Reform Act in March 2012.

Appendix Figure D.2 presents measures of dispersion in sanction rates. Our analysis quantifies the political economy of this increase in sanction rate dispersion.

3.2 Public Awareness: Internet Search Activity and News Media Coverage

The JSA sanction reforms of 2012 were salient in two main ways. Firstly, households direct their online attention towards JSA sanctions during the reform period in a similar way to during a recession.

Internet search activity volumes peaked between 2014 and 2015 for terms "Benefit Sanctions" and "JSA Sanctions", exactly the period when sanctioning rates are highest (see Fig. D.3). The figure suggests that JSA sanctions were the most salient type of benefit sanction in the period of interest. "Universal Credit Sanctions" is a phrase that only gains prominence in the later 2010s. The previous two terms achieve maximum search volumes in 2014–2015 comparable to maximum Universal Credit sanction interest immediately after the Covid-19 pandemic. Hence internet search interest in JSA sanctions in the reform period is comparable to interest in Universal Credit at the bottom of the business cycle in a period of potentially high layoffs.

Secondly, news mentions of "Benefit Sanctions" reach a peak in 2015 of over 2500 mentions across prominent tracked outlets, suggesting a fairly widespread coverage in the period examined. "JSA Sanctions" mentions in news articles also shows a marked increase

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in the years immediately after the 2012 reforms. Combined, these graphs present correlational evidence that both the demand for information on sanctions from households, and the supply of information about sanctions from news outlets responds to policy changes in 2012.

3.3 Parliamentary Elections

We use data on the 2010 Westminster general elections in England, Scotland and Wales. The elections are conducted using first-past-the-post voting in 632 single member constituencies.[5] The Conservative Party (center-right), Labour party (center-left) and Liberal Democrats (centrist) are the three major political parties to field candidates in all seats we analyze. Plaid Cymru and the Scottish National Party are both center-left in stance and the nationalist parties of Wales and Scotland respectively, fielding candidates only in certain seats.

All members of parliament are elected simultaneously for a five-year term. The 2010 election resulted in a significant swing for the Conservative Party and ended the Labour-era of government. The Conservative party formed a coalition government with the third largest party Liberal Democrats. The election results are presented in Table A.1. The Conservative Party won the majority of seats in England, while Labour won the majority in Scotland and Wales.

We limit our analysis primarily to contests between coalition partners Conservative and Liberal Democrats versus Labour, that is, where one of the coalition parties came first and Labour came second, or with Labour first and a coalition party second. We exclude cases such as Conservatives winning followed by Liberal Democrats and Labour in third place. Note, this selection does not exclude seats in Wales and Scotland to the extent the UK-wide parties occupy the first and second places.

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5. There are further 18 seats elected from Northern Ireland.

3.4 Data Sources

The data on JSA claimants and sanctions comes from the Department for Work and Pensions, with the monthly number of claimants provided by Nomis. For population data, we use the 2009 mid-year parliamentary constituency population estimates from the Office for National Statistics and from the National Records of Scotland. Information on economic activity comes from the 2009 Annual Population Survey, and data on income comes from the 2009 Annual Survey of Hours and Earnings, both provided by the Office for National Statistics.

Table A.2 presents summary statistics for constituencies won by either of the government parties or by the Labour Party. The Coalition-aligned constituencies have on average higher employment rates and earnings, and fewer JSA claimants. Summary statistics for all parliamentary constituencies are presented in Table A.3.

4 Empirical Strategy

Our empirical strategy centers on a regression discontinuity design, in which we compare constituencies where central-government-aligned parties narrowly won or were defeated. Let $Y_i(1)$ denote the potential outcome in case of treatment, i.e., the sanction rate in constituency i if a coalition party won the seat, and let $Y_i(0)$ denote the potential outcome in case of no treatment. We denote the treatment status, i.e., a coalition party winning the seat, as $D_i = \mathbb{1}\{m_i \geq c\}$, where m_i is the running variable and c is the election cut-off. We model treated and untreated potential outcomes as functions of the running variable m_i and the treatment status: $Y_i(0) = \alpha + f(m_i)$ and $Y_i(1) = Y_i(0) + \beta D_i$.

The key identifying assumption in this analysis is continuity of potential outcomes at the threshold—that absent treatment, districts immediately to the left and right of the cut-off would have had identical outcomes—and that being immediately on either side of the cut-off is as good as random (Hahn et al. 2001). The parameter of interest β^{RD} is

then given by

$$\beta^{RD} = \lim_{m \to c^+} E\big[Y_i(1); m\big] - \lim_{m \to c^-} E\big[Y_i(0); m\big] \tag{1}$$

We determine the running variable as the Coalition's distance to victory in head-to-head races with Labour, at the constituency-level, i.e., vote share of strongest coalition party minus Labour, and the cut-off is $c = 0$.

$$m_i = \begin{cases} \text{Conservative}_i - \text{Labour}_i & \text{if } 1^{st}/2^{nd} \text{ contested by Conservatives and Labour} \\ \text{Liberal Democrat}_i - \text{Labour}_i & \text{if } 1^{st}/2^{nd} \text{ contested by Liberal Democrats and Labour} \end{cases} \tag{2}$$

Hence a Labour-held seat would be represented by $m_i < 0$, while a seat held by one of the coalition parties would be represented by $m_i \geq 0$. We estimate the following equation, allowing for a vector of constituency-level controls:

$$Y_i = \alpha + \beta^{RD} D_i + f(m_i) + X_i'\Gamma + \varepsilon_i \tag{3}$$

where D_i is a dummy for a Coalition seat and and $f(m_i)$ is a polynomial of the Coalition win margin in constituency i, estimated separately on each side of the cut-off. X_i is a set of control variables, which includes log population, share of women, share of working age population, median earnings, and employment rate. To avoid including bad controls, we use population characteristics from 2009. The identification does not rely on conditioning on observables other than the running variable, and the additional covariates are included only to improve precision of the estimator. The causal parameter of interest is β^{RD} which captures the discontinuous effect of switching from a Coalition-held to Labour-held seat. We conduct estimation with the local polynomial approach, and we use the Calonico et al. (2014) approach for MSE-optimal bandwidth selection.

4.1 Validity of the RDD

The identification relies on the potential outcomes evolving smoothly at the cut-off. We examine the identifying assumption in several ways. We evaluate formally the continuity of the density of the running variable, evaluate discontinuities in pre-treatment covariates, examine sensitivity to bandwidth selection, and finally conclude with altering the threshold to placebo levels other than zero (Appendix C presents the robustness analyses).

Running Variable Density Continuity   We evaluate the smoothness of the density function around the cut-off informally by visual inspection and a formal McCrary-test of a discontinuity in density around the cut-off (McCrary 2008). Furthermore, we conduct manipulation testing using local polynomial density estimation as proposed by Cattaneo et al. (2020). Testing does not reject continuity of density around the $c = 0$ threshold, and we conclude that smoothness is satisfied locally (see Figures B.1 and B.1).

Pre-Treatment Covariate Balance   Figure B.2 shows that the pre-treatment covariates are smooth across the electoral threshold. The only exception to good balance is a marginally significant difference on employment rate. Although there are no corresponding effects on unemployment rate or economic activity rate, to ensure that there is in fact no sorting, we conduct additional balance tests on employment rates. Since the employment data comes from the labor force survey, which is collected on a quarterly basis, we can examine employment in alternative 12 month windows. We find no evidence of imbalance in employment, unemployment, or economic activity rates measured in slightly different windows (see Tables B.1 and B.2).

External Validity   The causal parameter we estimate is the Local Average Treatment Effect. The effective sample for our baseline estimates is 171 constituencies out of 632. While the outcome of close elections is as good as random, the set of constituencies that experience close elections may not itself be random. Table A.4 compares constituencies with and without close elections. Competitive constituencies differ somewhat—they have higher rates of JSA claimants and sanctions, and lower employment rates—but these

differences are modest compared to the much larger gaps observed between Conservative-
and Labour-leaning constituencies (see Table A.2). This suggests that the sample of close
races is not systematically skewed toward constituencies with extreme socio-economic
profiles.

In addition, electoral competitiveness does not appear highly persistent over time.
Comparing Westminster elections in 2005, 2010, and 2015 shows that only about 20
percent of constituencies had elections decided by a margin of 10 percentage points or
less in two consecutive years. Thus, competitive races occur in a relatively broad set of
constituencies over time, reinforcing the view that our LATE is informative beyond a
narrow or idiosyncratic subset of cases. Finally, Figure C.1 demonstrates that our results
are robust to a wide range of bandwidths: As the bandwidth increases, the point estimate
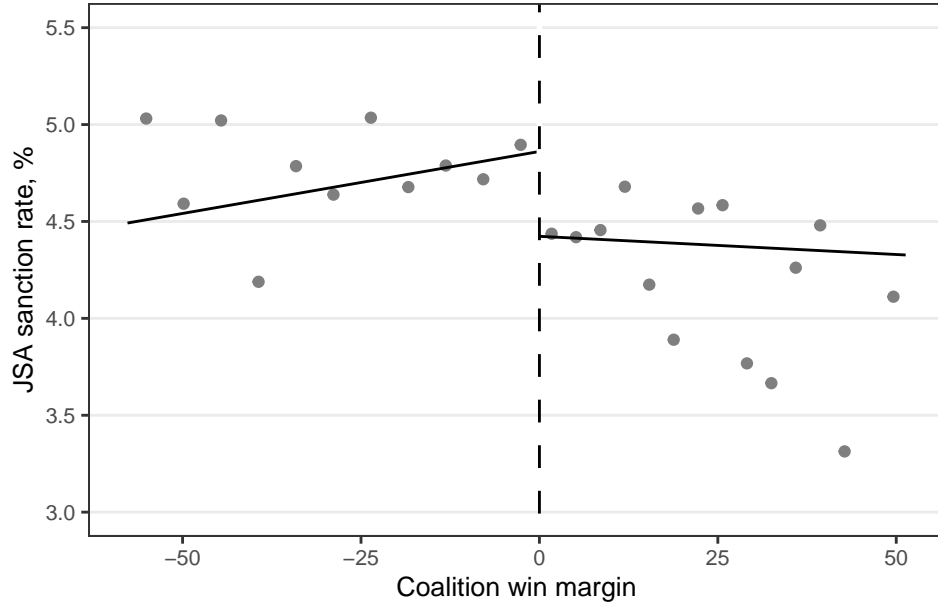decreases somewhat but remains statistically significant and qualitatively unchanged.

5 Results

Baseline Results We present graphical evidence for our main result in Figure (3).
The figure presents unemployment benefit sanction rate as a function of the Coalition's
winning margin with a global linear fit on each side of the electoral threshold. Although
there is considerable dispersion in the tails of the win margin, the figure shows a noticeable
drop in the sanction rate close to the cut-off for constituencies won by one of the coalition
parties.[6]

The regression results confirm the graphical evidence. The local polynomial RD es-
timates are presented in Table 2. We report estimates from conventional local linear and
quadratic RDD specifications using the MSE optimal bandwidth proposed by Calonico
et al. (2014) as well as half of the optimal bandwidth (columns (3) and (6)). We also
report robust bias-corrected p-values and confidence intervals. Since our sample is relat-
ively small, we include pre-treatment covariates to increase precision (columns (2), (3),
(5), (6)). The robust p-value in column (1) indicates that the estimate is marginally
significant also without additional controls. The RD estimates indicate a drop of 0.7–0.9

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6. Figure D.4 shows the corresponding RD plot with a fourth order polynomial fit.

Figure 3: Effect of Coalition win on JSA sanction rate

Notes: The chart presents binned averages of JSA sanction rate (March 2012 – December 2015). Solid lines are linear fit lines from a global polynomial RD specification with demographic and socioeconomic controls. The running variable is win margin of the strongest Coalition party against Labour. Observations to the right of the threshold are constituencies under a Coalition MP.

percentage points at the cut-off, on a baseline rate of 4.5 %. This implies on average 18 % lower sanction rates in Coalition controlled constituencies than in Labour controlled constituencies. The estimates are robust to different polynomial specifications and a smaller bandwidth.

Estimates by Year Economic behavior of households may also change discontinuously at the election threshold—one concern is that workers, jobseekers, and firm managers internalize that they live in a Coalition-held seat, and act accordingly. For example, they may expect that under a Conservative MP, if there is any influence on local policy-making, Conservative-influenced institutions would be more severe, and so they change job-search and labor supply decisions, which in turn affects local vacancy creation.

If we assume household and firm responses to local political control do not change over time, we can account for time-invariant characteristics which may also jump at the cut-off by estimating the RD regression separately by year. We exploit the change in

Table 2: RDD results for the effect of Coalition win on JSA sanction rate

	Linear			Quadratic		
	(1)	(2)	(3)	(4)	(5)	(6)
Coalition win	-0.659*	-0.800**	-0.913*	-0.765*	-0.849**	-0.955*
	(0.475)	(0.400)	(0.496)	(0.523)	(0.423)	(0.538)
Robust p-value	0.099	0.025	0.053	0.099	0.030	0.077
Robust 95 % CI	[-1.71, 0.15]	[-1.68, -0.11]	[-1.94, 0.01]	[-1.89, 0.16]	[-1.75, -0.09]	[-2.01, 0.1]
Bandwidth	12.20	12.80	6.40	20.40	24.12	12.06
N	171	171	81	253	263	159
Controls	-	✓	✓	-	✓	✓

Notes: *p<0.1; **p<0.05; ***p<0.01. The dependent variable is JSA sanction rate (March 2012 – December 2015). Estimates in columns (1)-(3) are from local linear and estimates in column (4)-(6) from quadratic regressions. Estimates in column (1)-(2) and (4)-(5) are from specifications using MSE-optimal bandwidth, and estimates in columns (3) and (6) are calculated with half of the bandwidth. In all regressions a triangular kernel is used. Controls include log population, share of women, share of working age, median earnings, and employment rate. Standard errors in parentheses. CCT robust bias-corrected p-values and 95 % CIs are reported.

national sanctions policy in 2012, two years after elections, to disentangle asymmetric reform implementation in coalition-aligned seats versus changes in jobseeker and other economic behaviors. Such behavioral discontinuities should already appear in 2010 and 2011 data, while policy discontinuities are only activated with reforms in 2012.[7]
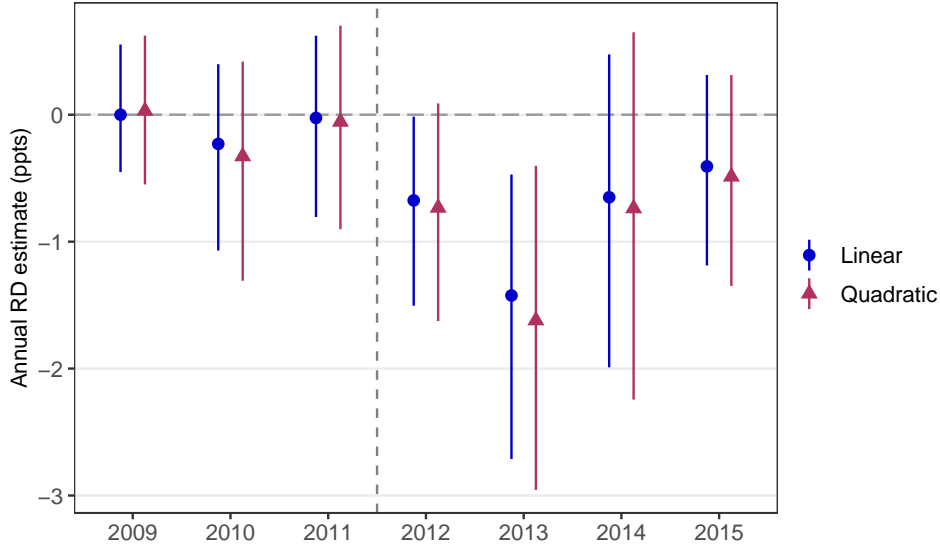
Decomposing our main result by year, we find no discontinuities in years 2010 and 2011, before the national sanctioning reform, while RD estimates become statistically significant in 2012 onward (Figure 4).[8] As a placebo test, we also test for effects in 2009, i.e., one year prior to the election, and show that the Coalition win in 2010 had no effect on the placebo outcome. This results shows that our findings are not driven by Coalition MPs being more successful in areas with lower sanction rates already before the election. The biggest effect takes place in 2013, the year following the reform. At this time, sanction rates were at their highest, which meant that there was most scope for more lenient implementation. Shielding one's constituents was also most valuable when the sanctioning rates were highest.

The intuition of the year-by-year estimates is very similar to the difference-in-discontinuities design. As a further robustness test, we also formally estimate a difference-in-discontinuities model following Grembi et al. (2016). This approach identifies the treat-

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7. Cf. the difference-in-discontinuities estimator employed by Grembi et al. (2016) in the context of municipal fiscal rules with a sharp cut-off at 5,000 inhabitants per municipality.

8. The estimates are also presented in Table E.1.

Figure 4: RDD results for the effect of Coalition win on JSA sanction rate by year
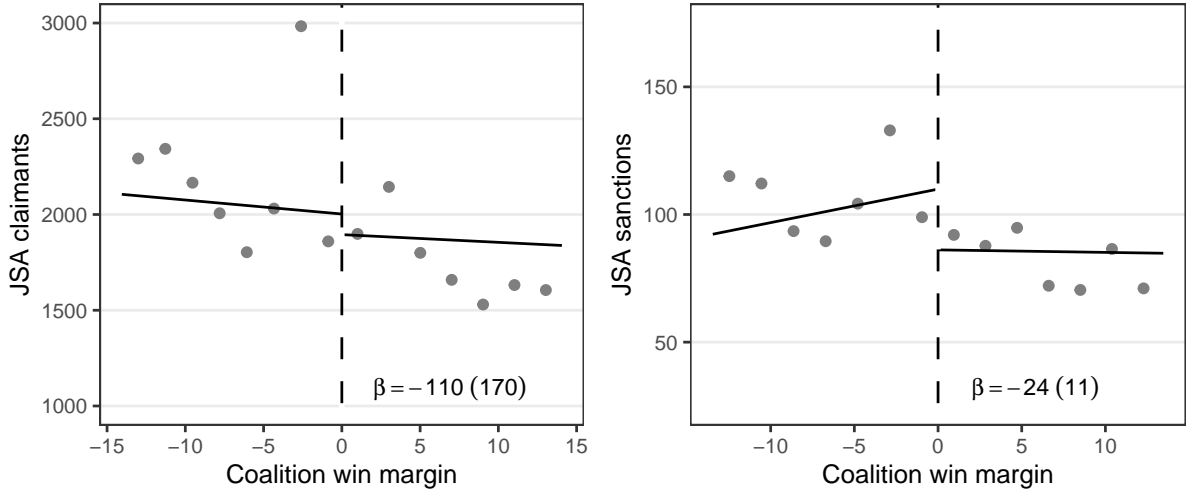

Notes: The chart presents annual RD estimates for the effect of Coalition alignment on average annual sanction rate. Each dot represents a separate RD regression. Demographic and socioeconomic controls included. Vertical bars represent CCT robust bias-corrected 95 % confidence intervals. Dashed vertical line splits the years into pre- and post-reform periods. Blue dots and red triangles represent linear and quadratic polynomial fits, respectively.

ment effect of a change in sanction policy in constituencies closely won by the Coalition. The estimates, which are presented in Figure C.3, show a similar pattern as those in Figure 4.

Examining separately the number of claimants and number of sanctions reveals that the effect on sanction rates indeed comes from fewer sanctions being imposed, while the number of claimants is not affected (Figure 5). These findings indicate that the effect is not driven by exit from unemployment or eligible jobseekers being discouraged from claiming benefits. The results therefore show that the implementation of the harsher sanctioning regime differs significantly between seats closely won by one of the coalition parties or the Labour Party. In particular, the implementation is significantly more lenient in constituencies won by the Coalition.

Our interpretation is that the government-aligned MPs were able to influence the local implementation of the new, harsher sanctioning regime, leading to an uneven allocation of sanctions that favored their constituencies. Since close elections take place in competitive constituencies, it is likely that the effect is driven by electoral incentives. There is

Figure 5: RDD results for the effect of Coalition win on number of JSA claimants and JSA sanctions

Notes: The chart presents binned averages of number of JSA claimants (left panel) and JSA sanctions (right panel) (March 2012 – December 2015) and fit lines from a linear RD specification with demographic and socioeconomic controls. β is the RD estimate. Standard errors in parentheses.
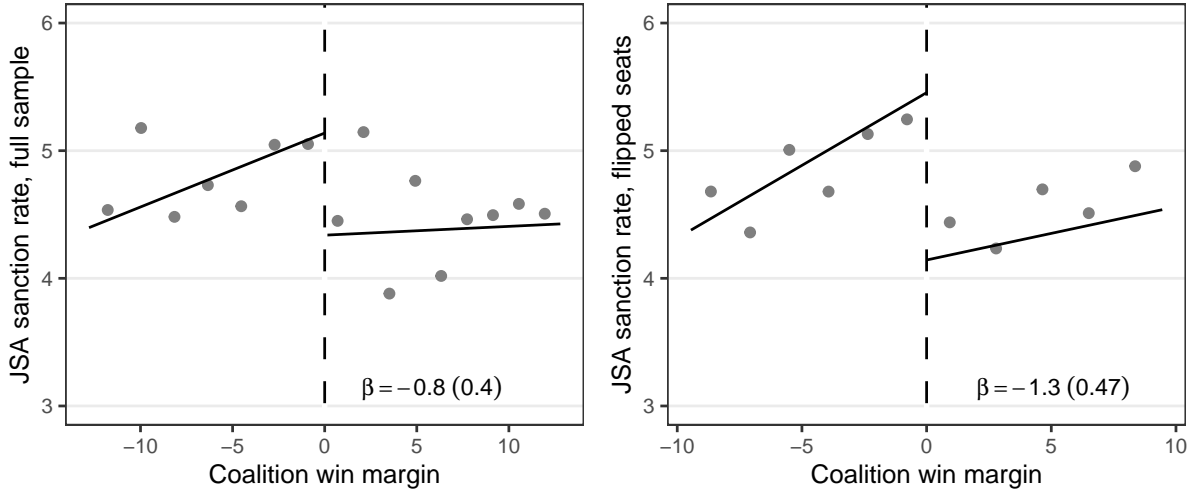
anecdotal evidence that Job Centre staff were given sanction targets and made claiming benefits more difficult on purpose.[9] This indicates that Job Centers and sanctions were politicized, which supports our interpretation that the observed partisan effect is due to MPs exerting influence—directly or indirectly—on public employees in the social welfare administration. At the time, benefit sanctions were also prominent in the media, and there was for example a large volume of negative media coverage relating to salient cases of jobseeker deaths after being sanctioned.[10]

Flipped Seats An important dimension of heterogeneity we test is whether sanction policy is more lenient in seats which flip from Labour to Coalition control in the 2010 election. Figure 6 presents RD estimates for the full sample and for seats held by Labour in 2005. We find significantly stronger effects for the newly gained seats, with effect size about 50 % larger than when using the full sample. This finding is consistent with the

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9. See, e.g., https://www.theguardian.com/society/2013/mar/21/jobcentre-set-targets-benefit-sanctions.

10. See Figure D.3, and e.g., https://www.theguardian.com/society/2013/mar/22/jobseekers-story-benefit-sanctions and https://www.theguardian.com/society/2014/oct/23/benefit-sanctions-investigated-mps.

Figure 6: RDD results for the effect of Coalition win on JSA sanction rate – Full sample and flipped Labour to Coalition seats
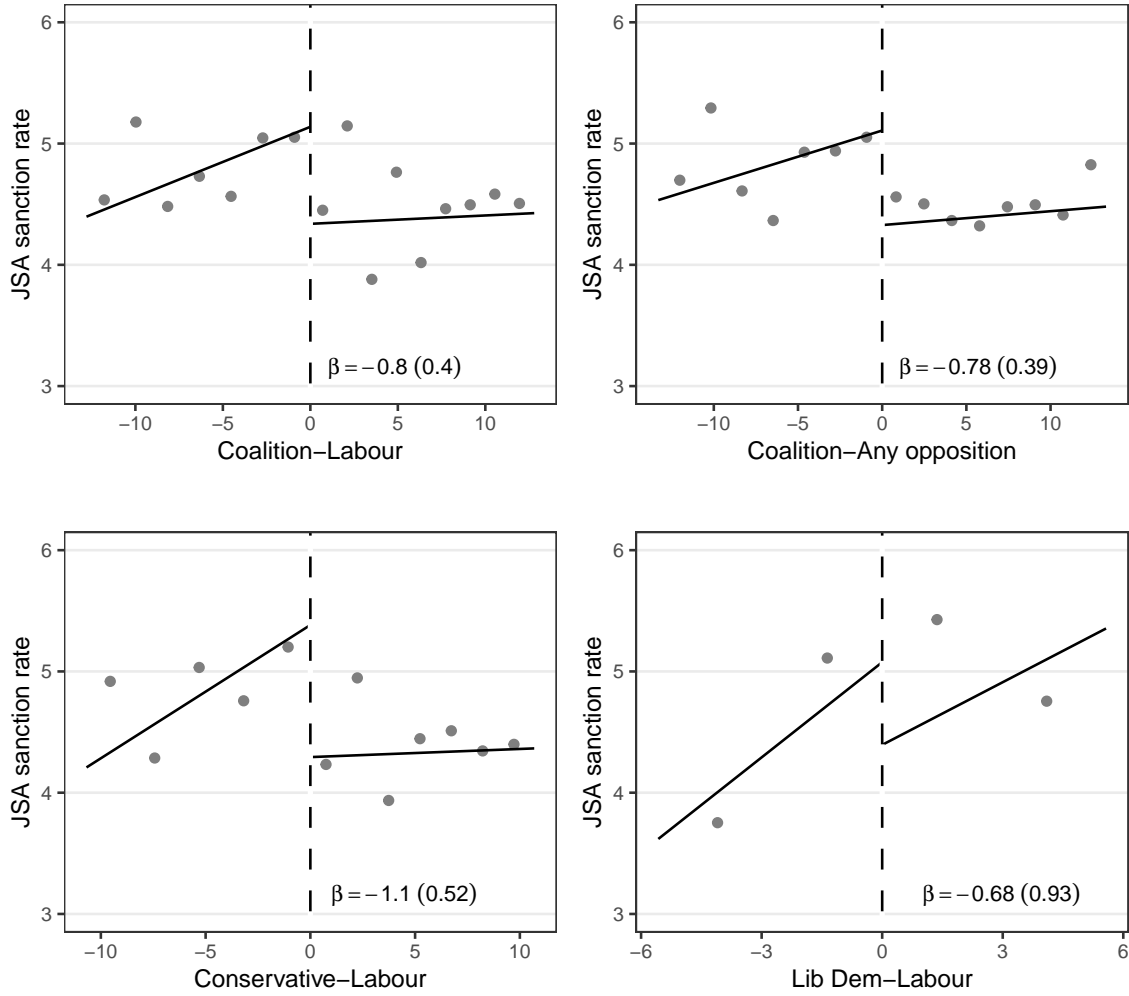

Notes: The chart presents binned averages of JSA sanction rate (March 2012 – December 2015) and linear fit lines from a linear RD specification with demographic and socioeconomic controls. Left panel presents estimates from full sample and right panel using only seats won by Labour in 2005. β is the RD estimate. Standard errors in parentheses.

interpretation that the more lenient implementation of the sanction regime is motivated by electoral concerns, and particularly by an attempt to solidify voter support in the previously Labour-held constituencies. Decomposing the results again by year shows the same pattern as when using the full sample (Figure D.5). However, the estimates suggest that the effect was more immediate, as well as more persistent.[11]

Alternative Head-to-Head Races We also test whether the effect on sanctioning policy varies between the coalition parties or depending on the runner-up party. Figure 7 presents RD estimates from different head-to-head electoral races with the running variable modified accordingly. The upper left panel shows the baseline estimate of the Coalition win against Labour. Including races between the Coalition and any other opposition party has negligible influence on the estimate. Instead, the results show that the negative effect on the sanction rate is mostly driven by constituencies with close races between Conservative and Labour. The point estimate of Liberal Democrat win against

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11. The estimates are also presented in Tables E.2, and E.3. We test for manipulation of the running variable in this alternative sample and do not find discontinuity at the cutoff (see Figure B.3). Panel A of Table B.3 also shows that the pre-treatment covariates are balanced.

Figure 7: RDD results for the effect of Coalition, Conservative Party, or Liberal Democratic Party win on JSA sanction rate

Notes: The chart presents binned averages of JSA sanction rate (March 2012 – December 2015) and fit lines from a linear RD specification with demographic and socioeconomic controls within the optimal Calonico et al. (2014) bandwidths. Running variable in the upper left panel is Coalition's win margin against Labour Party, in the upper right panel Coalition's win margin against any opposition party, in the lower left panel Conservative's win margin against Labour, and in the lower right panel Liberal Democrat's win margin against Labour. β is the RD estimate. Standard errors in parentheses.

Labour also suggests a negative effect on sanctions, but the estimate is imprecise due to low number of close races.[12]

In sum, the findings show that close electoral races especially between the two main parties contesting UK elections—Conservative and Labour—result in unequal implementation of the welfare reforms, while including close races involving the coalition partner Liberal Democrats or other opposition parties attenuates the effect. The results are therefore consistent with the interpretation that the distortion is motivated by electoral concerns, and that the Conservative Party, the leader of the coalition, was most effective at influencing the sanction policy implementation.
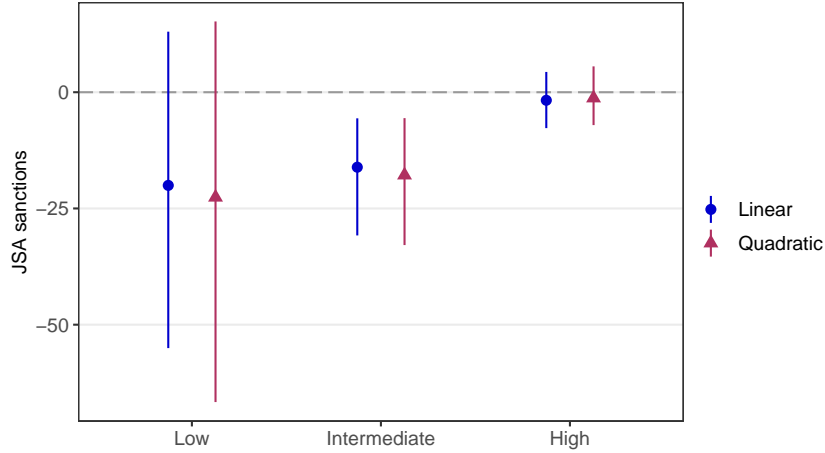
6 Mechanisms

Our results show that party control has a systematic effect on local implementation of national welfare policy. The findings are consistent with more lenient implementation due to electoral incentives: We find largest estimates for newly gained seats and the most competitive races. Instead, we do not find evidence for a behavioral response whereby job search, firm behavior, or case worker behavior would change at the time of the election. A possible mechanism for our findings is that MPs exert influence on the bureaucracy—for example through their party, through the DWP, or job centers in their district. We argue that the most consistent explanation for what drives the partisan effect is that MPs aligned with the central government have more channels to influence bureaucrats and policy implementation than opposition MPs. We next provide fours sets of results supporting our hypothesis.

Different Sanction Levels Differential implementation of the sanction regime implies that the differences should be most obvious in decisions involving more discretion. As decisions about intermediate sanctions involve most room for discretion, implementing intermediate level sanction more harshly or more leniently should be the easiest way to

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12. The estimates are also presented in Appendix Table E.4. We test for manipulation of the running variable in these alternative head-to-head races and do not find evidence of discontinuity at the cutoff (see Figure B.3). Table B.3 shows that pre-treatment covariates are balanced in these alternative samples.

Figure 8: RDD results for the Effect of Coalition win on JSA sanctions at different sanction levels
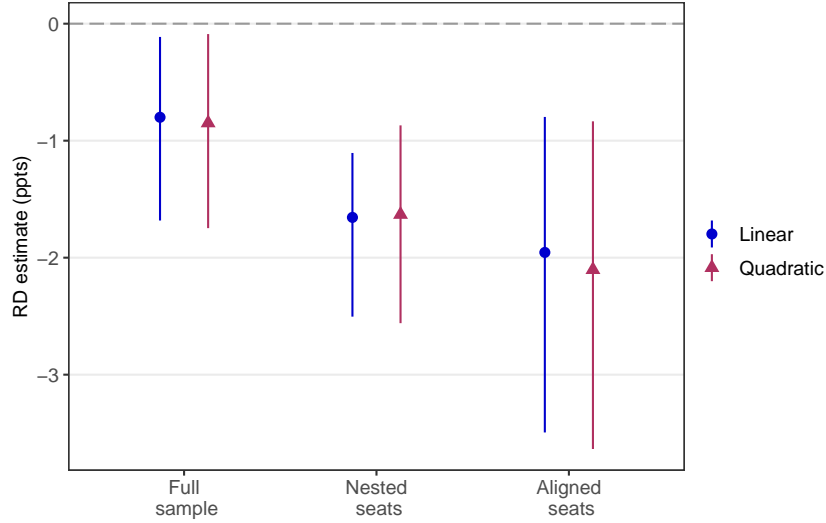

Notes: The chart presents RD estimates for the effect of Coalition alignment on number of JSA sanctions in different sanction categories. Each estimate represents a separate RD regression. Blue dots and red triangles represent linear and quadratic polynomial fits, respectively. Demographic and socioeconomic controls are included. Vertical bars represent CCT robust bias-corrected 95 % confidence intervals.

influence number of sanctions.

Estimating the RD separately for number of sanctions according to sanction category reveals that intermediate level sanctions decrease significantly (Figure 8 and Table E.5). The point estimates on lower level sanctions also suggest a similar decrease in number of sanction, but the estimates are not statistically significant. Higher level sanctions were not affected. Intermediate sanctions are applied due to insufficient search effort or being unavailable for work. A person may be considered unavailable for work if they have "unreasonable" restrictions for example on nature of employment, hours, pay, duration of employment, or location. Imposing sanctions for such infringements is likely to have most room for discretion. Partisan influence especially on intermediate sanctions contributes significantly to the overall variation in implementing sanctions, as significantly fewer intermediate sanctions are imposed in government-aligned constituencies.

Alignment between District Council and Constituency MP   One way MPs might influence public employees and policy implementation is through local politicians. A testable implication is that the effect on sanction rate should vary depending on alignment between a local council and a parliamentary constituency. Westminster constituencies

Figure 9: RDD results for the effect of Coalition win on JSA sanction rate – Full sample, nested and aligned seats with council districts

Notes: The chart presents RD estimates for the effect of Coalition alignment on JSA sanction rate. Each estimate represents a separate local linear RD regression. Blue dots and red triangles represent linear and quadratic polynomial fits, respectively. Vertical bars represent CCT robust bias-corrected 95 % confidence intervals.

are typically the smaller geographic unit with most nested entirely within council districts (local authority district, LAD). We examine the alignment-across-multiple-levels hypothesis by examining constituencies nested entirely within the same LAD.

Figure 9 plots the full sample RD results as well as estimates using the sample of Westminster constituencies which are completely nested within a single council district, and the sample of constituencies that are also aligned with the party that controls the LAD council in 2012 at the start of the reforms (see also Table E.6). The results indicate that the partisan effect on sanctions is conditioned by alignment with a local council. Compared to the full sample estimate, the effect is larger in constituencies that are nested within one LAD. This suggests that MPs are more efficient at influencing policy implementation when dealing with fewer local governments. The point estimates for the politically aligned seats are even larger, although the confidence intervals are wide due to small sample size. The findings support the hypothesis that MPs have greater influence over local policy implementation when local council is controlled by the same party.

Table 3: OLS results for the estimates of Coalition win on JSA sanction rate conditional on MP seniority

	(1)	(2)	(3)	(4)
Coalition win	-0.532	-0.847**	-0.490	-0.780**
	(0.334)	(0.345)	(0.345)	(0.354)
Coalition win margin	0.034	0.046	0.034	0.047
	(0.034)	(0.033)	(0.034)	(0.033)
Coalition win margin × Coalition win	-0.025	-0.015	-0.023	-0.018
	(0.044)	(0.042)	(0.046)	(0.045)
Minister			-0.442	-0.700
			(0.513)	(0.597)
N	185	171	185	171
R^2	0.02	0.18	0.03	0.19
Controls	-	✓	-	✓

Notes: *p<0.1; **p<0.05; ***p<0.01. The dependent variable is JSA sanction rate (March 2012 – December 2015). All estimates are from an OLS regression estimated on a sample restricted to within the MSE-optimal bandwidth. Controls include log population, share of women, share of working age, median earnings, and employment rate. Heteroskedasticity-robust standard errors in parentheses.

Seniority Another factor that should be associated with MPs influence is their seniority. As a measure of seniority, we consider whether an MP is also a government minister. Ministers are likely to have better connections both within the government and within the bureaucracy.

More senior MPs, especially the government ministers, may have more influence on policy implementation. We examine this hypothesis by limiting the set of treated constituencies to only those held by the so-called frontbenchers: the Cabinet and all other ministers. Specifically, we define an MP as a frontbencher if he or she held a minister post at any point during the 2010–2015 term.

To examine heterogeneity by seniority, we estimate an OLS regression within the MSE-optimal bandwidth obtained from the baseline RD regression including covariates. First, columns (1) and (2) in Table 3 present estimates for Coalition win on sanction rate. The specifications are analogous to the baseline RD, and they yield point estimates that are are very similar to the RD estimates. Columns (3) and (4) present estimates for heterogeneity with respect to seniority. The term *Minister* can only take value 1 for winning Coalition MPs and is therefore equivalent to an interaction term with the

treatment dummy. Although not statistically significant (ministers make up less than 10 % of the estimation sample), the point estimates are large in magnitude and suggest that the drop in the sanction rate is considerably larger in constituencies where the MP is also a minister.

Survey on Targets for Job Centre Plus Caseworkers   Finally, we present evidence for the existence of informal influence on sanctions decisions. The Public and Commercial Services Union (PCS), the union representation of civil service workers, conducted a survey in January 2014 of its JCP members working in the DWP. 67,000 of its members are directly involved in administering JSA and ESA and the sanctioning process.

The results found "*23% of advisers said they had been set or told that there was a target for sanctions referrals*". Beyond explicit targets, a substantial share (81%) also reported being "*set an expectation for sanction referrals*". PCS written evidence to the British Parliament claims internal DWP disciplinary measures could be brought against those caseworkers thought to be under-performing. These measures could have consequences for caseworkers pay and promotions within the organization, for example causing workers to not receive pay rises (PCS 2014).

The official stance from the DWP is at odds with the PCS written evidence in Parliamentary testimony and the results of the PCS membership survey, on both of explicit or implied sanctions targets. Our findings on partisan leniency are consistent with informal pressure on non-aligned districts. They can thus offer an explanation of why some workers were pressured to impose more sanctions, although there was no general policy of sanction targets at the regional or national level. In fact, in the hypothetical case of a general sanction target, either nationwide or across a large share of districts, we might not observe the partisan lenience effect estimated in our RD regressions.

7 Conclusions

This paper studies the impact of political alignment on local implementation of national welfare policy. In particular, we examine how political control influences the practical im-

plementation of a new unemployment benefit sanctioning regime. We employ regression discontinuity design to exploit exogenous variation in having a central government-aligned MP on local economic policy institutions. Our identification strategy solves the endogeneity problem whereby areas leaning more towards or against the central government are endogenous to local characteristics—different places vote for different parties.

The welfare reform introduced by the Conservative-led government implied stricter monitoring of welfare recipients and a harsher sanctioning regime. However, our results show that its implementation is significantly more lenient in the Coalition-held constituencies. We find that constituencies with a marginally aligned MP have unemployment benefit sanction rates which are on average 18 % lower than in marginally unaligned areas after the enactment of the reform.

Legislators have an electoral incentive to provide a more favorable distribution of public spending and spending cuts to their constituencies. The mechanism we suggest is that government-aligned legislators have wider access to various influence channels, and are able exert more influence over public employees, thereby resulting in an uneven allocation of punitive welfare measures. Although our results do not constitute proof of this exact mechanism, they do show that party control has a systematic effect on local sanctioning policy. Such partisan influence on the availability of welfare benefits has direct welfare consequences for affected jobseekers.

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Making the Cut:
Close Elections and Local Welfare Policy

Online Appendix

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A Summary Statistics

Table A.1: UK 2010 House of commons elections results for England, Scotland and Wales

Party	Vote share	Seats	Runner-up	Candidates
Conservative Party	36.89	306	190	631
Labour Party	29.66	258	159	631
Liberal Democrats	23.56	57	242	631
Scottish National Party	1.69	6	29	59
Plaid Cymru	0.57	3	6	40
Green Party	0.97	1	0	331
Other	6.65	1	6	1093

Notes: Vote shares are calculated over total votes. Seat obtained by "Other" refers to the Speaker.

Table A.2: Summary statistics for constituencies won by coalition parties or Labour

	Coalition			Labour			
	N	Mean	SD	N	Mean	SD	Difference
JSA saction rate, %, post-refrom	363	4.36	1.00	258	4.73	0.99	-0.36 (0.08)***
JSA sanctions	363	57.15	31.57	258	123.45	51.31	-66.3 (3.6)***
JSA claimants	363	1264.41	630.25	258	2516.71	930.63	-1252.3 (66.7)***
Conservative vote share, %	363	45.15	9.89	258	22.86	9.03	22.29 (0.77)***
Libdem vote share, %	363	26.69	11.10	258	18.67	7.11	8.02 (0.73)***
Labour vote share, %	363	20.17	10.30	258	46.47	7.73	-26.3 (0.72)***
Win margin, ppts	363	17.95	11.33	258	19.28	12.94	-1.33 (1)
Number of parties	363	5.42	0.69	258	5.40	0.68	0.02 (0.06)
Flipped seat	363	0.26	0.44	258	0.02	0.14	0.24 (0.02)***
MP standing down	363	0.20	0.40	258	0.21	0.41	-0.01 (0.03)
Population	363	96053.43	10837.25	258	95860.94	13233.66	192.49 (1001.02)
Female population, %	363	51.01	0.71	258	50.96	0.89	0.06 (0.07)
Working age population, %	363	63.89	3.23	258	66.04	3.43	-2.15 (0.27)***
Economic activity rate, %	363	79.01	4.27	258	73.56	5.23	5.45 (0.4)***
Employment rate, %	363	74.02	4.94	258	66.31	6.09	7.71 (0.46)***
Unemployment rate, %	187	7.90	2.23	240	10.27	3.10	-2.37 (0.26)***
Median earnings	324	22451.23	3889.92	244	20699.91	3321.38	1751.32 (303.16)***

Notes: Summary statistics for UK parliamentary constituencies in England, Scotland and Wales. Standard errors adjusted for clustering at constituency level. Coalition refers to constituencies won by Conservative Party or Liberal Democrats. JSA statistics are monthly averages across constituencies from January 2010 to December 2015. Post-reform JSA sanction rate refers to the period from March 2012 to December 2015. Flipped seats is share of seats where party control changed in the 2010 election. MPs standing down is share of seats where the MP did not seek re-election in 2010. Earnings is gross annual pay.

Table A.3: Summary statistics for parliamentary constituencies

	N	Mean	SD
JSA saction rate, %, post-reform	632	4.51	1.02
JSA sanctions	632	84.08	52.22
JSA claimants	632	1772.55	983.41
Conservative vote share, %	631	35.68	14.57
Libdem vote share, %	632	23.14	10.47
Labour vote share, %	632	30.92	15.94
Conservative win	632	0.48	0.50
Libdem win	632	0.09	0.29
Labour win	632	0.41	0.49
Win margin, ppts	632	18.44	12.14
Number of parties	632	5.41	0.70
Flipped seat	632	0.16	0.37
MP standing down	632	0.20	0.40
Population	632	95675.88	12309.07
Female population, %	632	50.99	0.79
Working age population, %	632	64.77	3.48
Economic activity rate, %	632	76.76	5.38
Employment rate, %	632	70.86	6.60
Unemployment rate, %	437	9.16	3.01
Median earnings	579	21660.14	3751.71

Summary statistics for UK parliamentary constituencies in England, Scotland and Wales. JSA statistics are monthly means across constituencies from January 2010 to December 2015. Post-reform JSA sanction rate refers to the period from March 2012 to December 2015. Flipped seats is share of seats where party control changed in the 2010 election. MPs standing down is share of seats where the MP did not seek re-election in 2010. Earnings is gross annual pay.
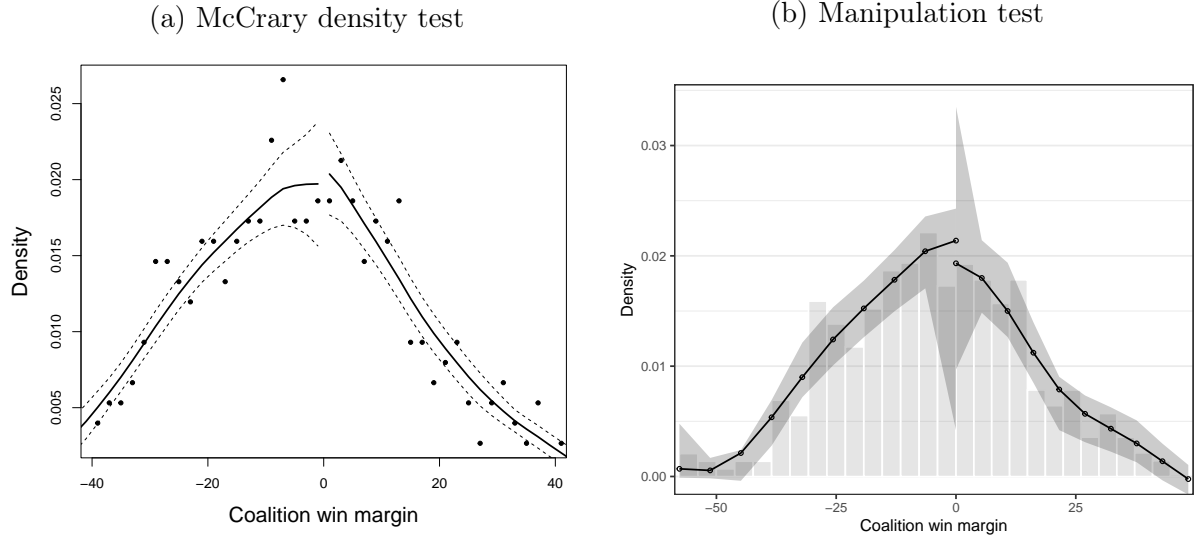
Table A.4: Summary statistics for constituencies with or without close elections

	Coalition			Labour			
	N	Mean	SD	N	Mean	SD	Difference
JSA saction rate, %, post-refrom	185	4.62	1.09	447	4.47	0.99	0.15 (0.09)
JSA sanctions	185	94.07	40.80	447	79.95	55.80	14.11 (3.99)***
JSA claimants	185	1959.13	743.80	447	1695.33	1058.11	263.8 (74.1)***
Conservative vote share, %	185	33.78	8.87	446	36.46	16.31	-2.68 (1.01)***
Libdem vote share, %	185	20.59	8.09	447	24.20	11.15	-3.61 (0.79)***
Labour vote share, %	185	37.16	4.39	447	28.34	18.12	8.82 (0.92)***
Win margin, ppts	185	6.51	3.75	447	23.37	10.92	-16.86 (0.59)***
Number of parties	185	5.50	0.66	447	5.36	0.71	0.14 (0.06)**
Flipped seat	185	0.38	0.49	447	0.07	0.25	0.31 (0.04)***
MP standing down	185	0.26	0.44	447	0.18	0.38	0.09 (0.04)**
Population	185	95098.23	11935.80	447	95914.94	12465.59	-816.71 (1056.55)
Female population, %	185	50.91	0.68	447	51.03	0.82	-0.12 (0.06)*
Working age population, %	185	65.56	3.43	447	64.44	3.45	1.12 (0.3)***
Economic activity rate, %	185	76.30	4.84	447	76.95	5.59	-0.65 (0.44)
Employment rate, %	185	69.89	5.58	447	71.26	6.95	-1.36 (0.53)***
Unemployment rate, %	153	9.16	2.82	284	9.16	3.11	0 (0.29)
Median earnings	171	21239.46	3617.94	408	21836.45	3796.86	-596.99 (334.26)*

Summary statistics for UK parliamentary constituencies in England, Scotland and Wales. Close refers to constituencies within the bandwidth of 12.8 ppts Coalition voteshare distance. Standard errors adjusted for clustering at constituency level. JSA statistics are monthly averages across constituencies from January 2010 to December 2015. Post-reform JSA sanction rate refers to the period from March 2012 to December 2015. Flipped seats is share of seats where party control changed in the 2010 election. MPs standing down is share of seats where the MP did not seek re-election in 2010. Earnings is gross annual pay.

B Validity

Figure B.1: Running variable density continuity

(a) McCrary density test
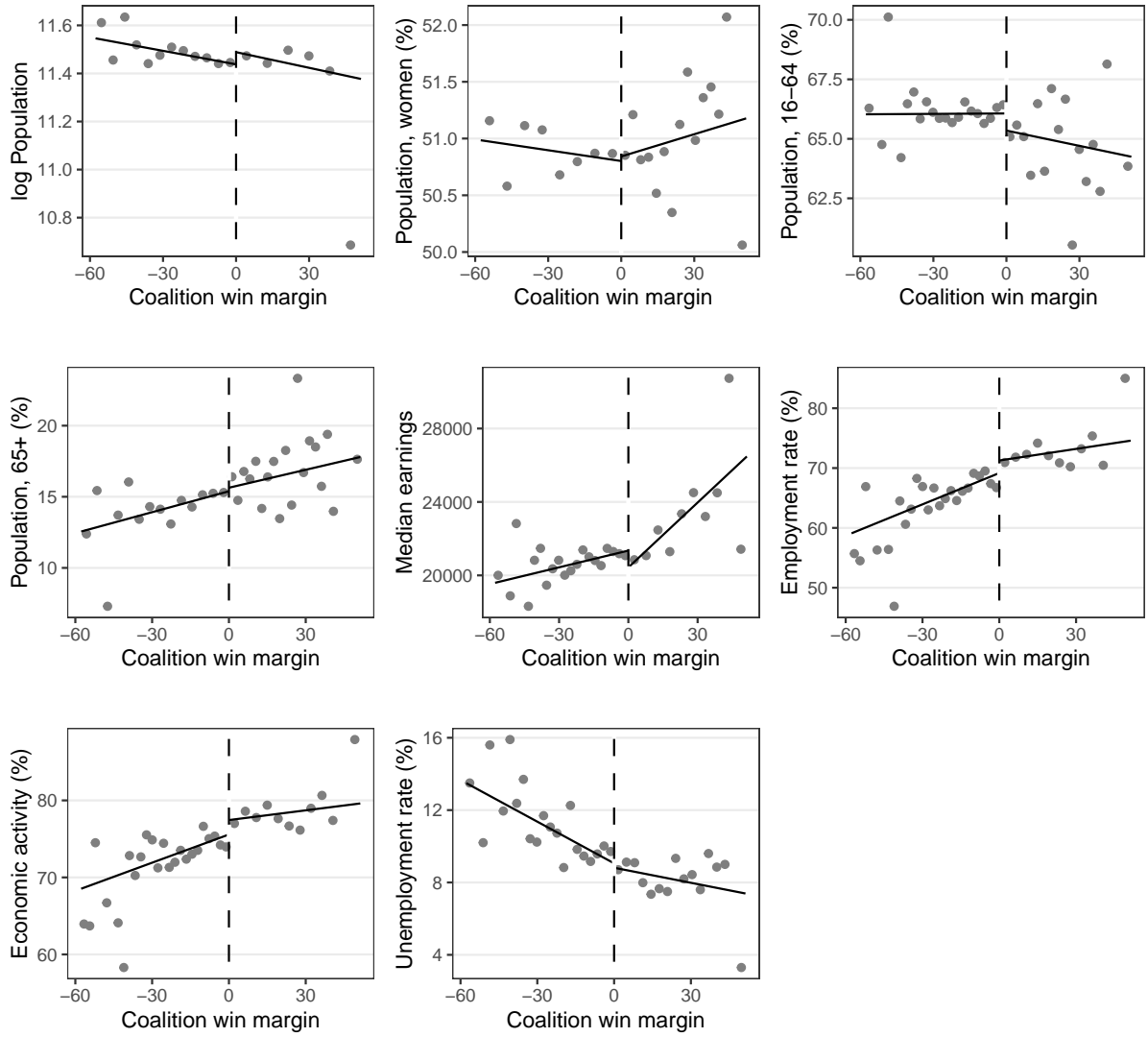
(b) Manipulation test

Notes: Figure (a) presents the McCrary test (McCrary 2008). Dots represent binned average outcomes and dashed lines represent the 95 % confidence intervals. Estimated log difference in heights at the cut-off is 0.05 with a standard error 0.25. Figure (b) presents the manipulation test proposed by Cattaneo et al. (2020). Dots represent robust bias-corrected estimated densities and shaded area represents the 95 % confidence intervals. The histogram represents the density of running variable, Coalition vote margin to victory (ppts). Bias-corrected estimated difference in densities at the cut-off is 0.007 with a standard error 0.008.

Table B.1: RDD results for the effect of Coalition win on pre-treatment covariates

	Population (1)	Women (2)	Working age (3)	Elderly (4)	Earnings (5)	Employment (6)	Activity (7)	Unemployment (8)
Coalition win	0.032	0.161	-1.204	0.679	-833.252	3.493*	2.677	-0.967
	(0.047)	(0.344)	(1.492)	(1.199)	(1189.551)	(2.318)	(2.298)	(0.987)
Robust p-value	0.606	0.625	0.371	0.524	0.443	0.094	0.237	0.260
Robust 95 % CI	[-0.07, 0.12]	[-0.51, 0.84]	[-4.26, 1.59]	[-1.59, 3.12]	[-3243.03, 1419.93]	[-0.66, 8.43]	[-1.78, 7.22]	[-3.05, 0.82]
Bandwidth	11.88	11.52	11.91	10.26	14.62	12.51	11.83	9.74
N	170	162	170	146	188	178	169	117
Mean(Y)	11.46	50.99	64.77	16.5	21660.14	70.86	76.76	9.16

*p<0.1; **p<0.05; ***p<0.01. The dependent variable in column (1) is log population, in column (2) share of women (%), in column (3) share of working age population (%), in column (4) share of population over the age of 65 (%), in column (5) gross annual median earnings, in column (6) employment rate (%), in column (7) economic activity rate (%), and in column (8) unemployment rate (%). Estimates come from local linear regressions using MSE-optimal bandwidth and a triangular kernel. Standard errors in parentheses. CCT robust bias-corrected p-values and 95 % CIs are reported. Mean(Y) is unconditional sample average.

Figure B.2: Balance of covariates by Coalition win margin

Notes: The chart presents binned averages of pre-treatment covariates with linear fit lines. Corresponding RD estimates presented in Table B.1.

Table B.2: RDD results for the effect of Coalition win on pre-treatment economic activity rates

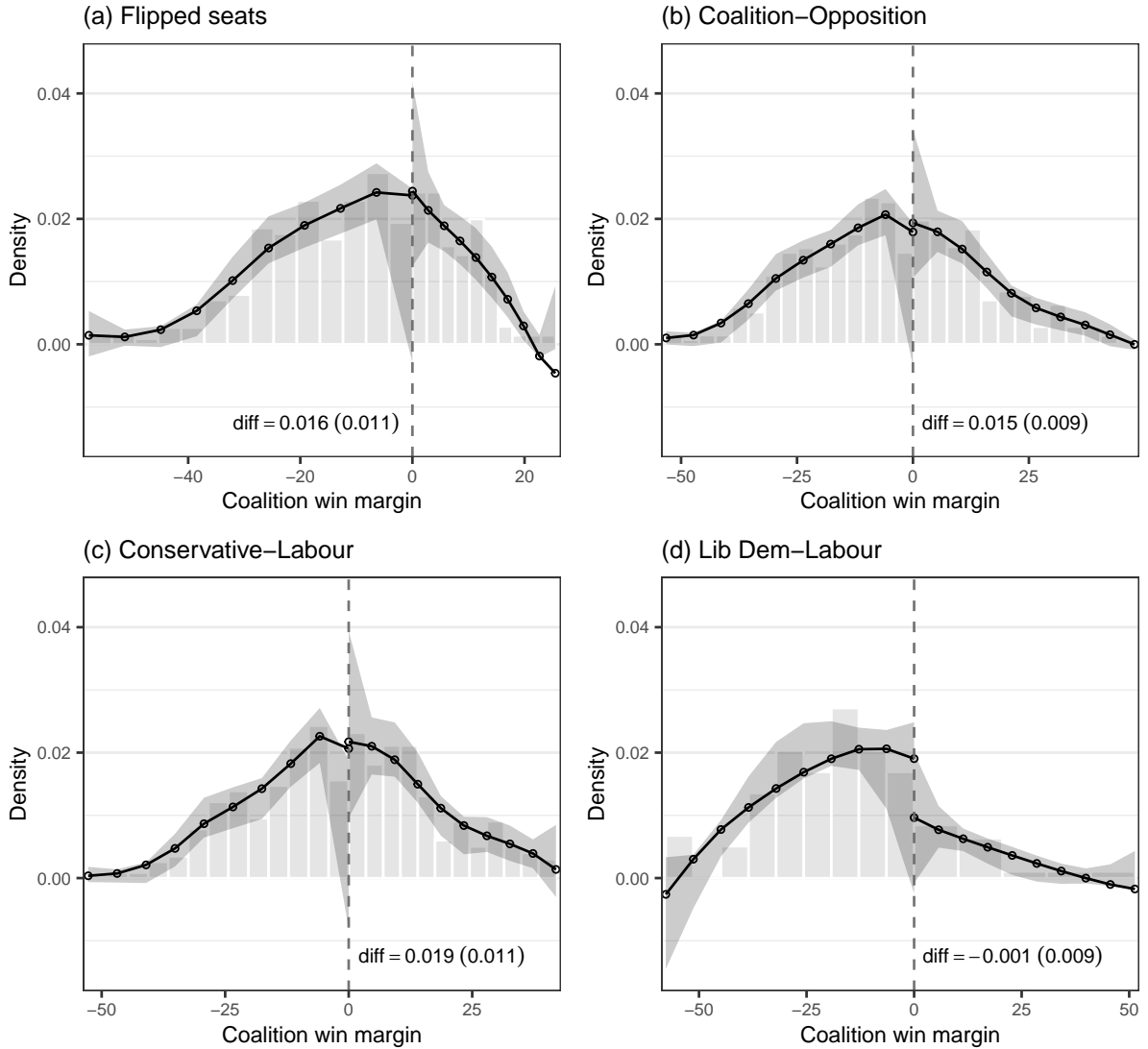|	October 2008-September 2009			April 2009-March 2010		
	Employment (1)	Activity (2)	Unemployment (3)	Employment (4)	Activity (5)	Unemployment (6)
Coalition win	2.718	2.298	-0.292	2.962	2.010	-1.051
	(2.387)	(2.195)	(1.319)	(2.228)	(2.222)	(1.348)
Robust p-value	0.196	0.286	0.738	0.150	0.425	0.304
Robust 95 % CI	[-1.59, 7.76]	[-1.96, 6.64]	[-3.03, 2.14]	[-1.16, 7.58]	[-2.58, 6.13]	[-4.03, 1.26]
Bandwidth	12.78	11.67	11.36	15.08	13.31	10.69
N	185	166	130	209	192	123
Mean(Y)	71.19	76.85	8.8	70.46	76.5	9.34

*p<0.1; **p<0.05; ***p<0.01. The dependent variable in columns (1) and (4) is employment rate (%), in columns (2) and (5) economic activity rate (%), and in columns (3) and (6) unemployment rate (%). Estimates come from local linear regressions using MSE-optimal bandwidth and a triangular kernel. Standard errors in parentheses. CCT robust bias-corrected p-values and 95 % CIs are reported. Mean(Y) is unconditional sample average.

Table B.3: RDD results for the effect of Coalition win on pre-treatment covariates in alternative estimation samples

	Population (1)	Women (2)	Working age (3)	Elderly (4)	Earnings (5)	Employment (6)	Activity (7)	Unemployment (8)
Panel A: Flipped seats								
Coalition win	0.053	0.383	-1.636	0.608	436.091	3.318	2.331	0.616
	(0.054)	(0.383)	(1.653)	(1.360)	(1223.974)	(3.760)	(3.600)	(1.236)
Robust p-value	0.407	0.306	0.320	0.662	0.705	0.464	0.593	0.379
Robust 95 % CI	[-0.06, 0.15]	[-0.36, 1.14]	[-4.88, 1.6]	[-2.07, 3.26]	[-1934.92, 2862.97]	[-4.62, 10.12]	[-5.13, 8.98]	[-1.34, 3.51]
Bandwidth	8.36	12.01	11.55	9.45	6.57	6.56	6.62	4.78
N	102	140	134	114	69	75	75	46
Panel B: Coalition-Opposition								
Coalition win	0.033	0.162	-1.328	0.821	-963.815	3.544*	2.609	-1.015
	(0.047)	(0.344)	(1.498)	(1.202)	(1181.379)	(2.326)	(2.319)	(0.986)
Robust p-value	0.573	0.615	0.328	0.449	0.364	0.084	0.251	0.250
Robust 95 % CI	[-0.07, 0.12]	[-0.5, 0.85]	[-4.4, 1.47]	[-1.45, 3.26]	[-3388.85, 1242.07]	[-0.54, 8.57]	[-1.88, 7.21]	[-3.07, 0.8]
Bandwidth	12.11	11.31	11.68	10.19	14.64	12.08	11.42	9.73
N	175	164	170	149	194	175	165	120
Panel C: Conservative-Labour								
Coalition win	0.017	-0.007	-0.802	0.969	-1274.072	1.999	1.398	-0.465
	(0.072)	(0.340)	(1.197)	(1.485)	(1935.731)	(2.292)	(2.147)	(1.344)
Robust p-value	0.951	0.956	0.449	0.442	0.423	0.434	0.658	0.687
Robust 95 % CI	[-0.14, 0.14]	[-0.69, 0.65]	[-3.25, 1.44]	[-1.77, 4.05]	[-5346.18, 2241.74]	[-2.7, 6.29]	[-3.26, 5.16]	[-3.18, 2.09]
Bandwidth	10.62	10.57	15.78	10.21	12.46	13.51	12.08	10.35
N	124	123	176	120	135	159	141	100
Panel D: Liberal Democrat-Labour								
Coalition win	0.097	0.202	-0.308	-2.444	-839.736	1.500	1.360	5.239
	(0.122)	(0.933)	(5.030)	(3.580)	(3570.072)	(8.138)	(6.996)	(7.624)
Robust p-value	0.452	0.683	0.992	0.431	0.654	0.901	0.876	0.280
Robust 95 % CI	[-0.15, 0.33]	[-1.45, 2.21]	[-9.81, 9.91]	[-9.84, 4.2]	[-8597.25, 5397.18]	[-14.93, 16.97]	[-12.62, 14.8]	[-6.71, 23.18]
Bandwidth	9.26	8.07	9.67	9.61	7.43	10.55	10.72	5.28
N	25	21	25	25	19	26	26	14

Notes: *p<0.1; **p<0.05; ***p<0.01. The dependent variable in column (1) is log population, in column (2) share of women (%), in column (3) share of working age population (%), in column (4) share of population over the age of 65 (%), in column (5) gross annual median earnings, in column (6) employment rate (%), in column (7) economic activity rate (%), and in column (8) unemployment rate (%). Estimates come from local linear regressions using MSE-optimal bandwidth and a triangular kernel. Standard errors in parentheses. CCT robust bias-corrected p-values and 95 % CIs are reported. Panel A uses sample of seats won by Labour in 2005, Panel B uses sample of seats contested by one of the coalition parties and any opposition party, Panel C uses a sample of seats contested by Conservatives and Labour, and Panel D uses a sample of seats contested by Liberal Democrats and Labour.
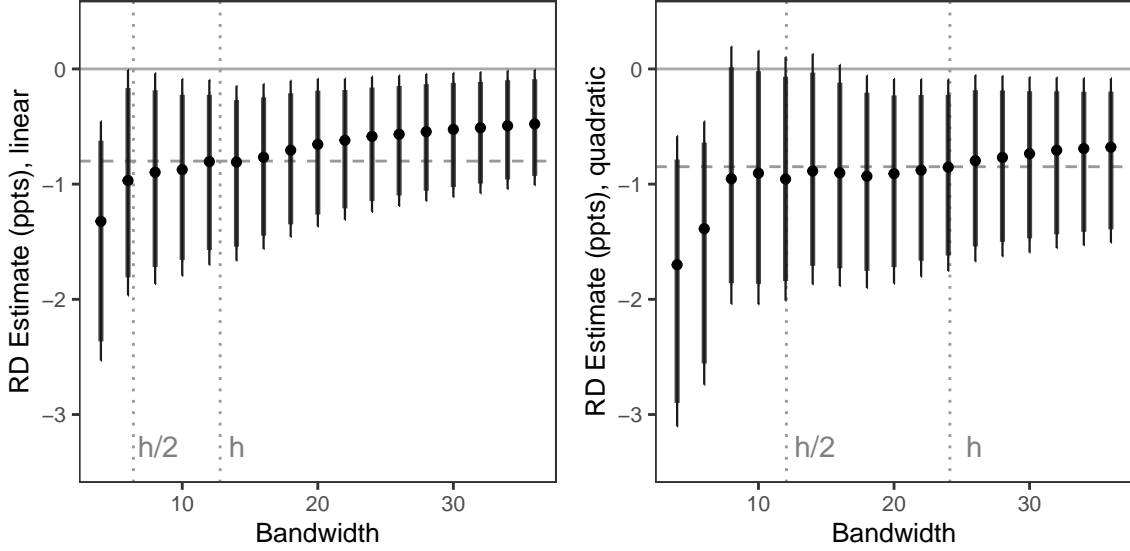
Figure B.3: Manipulation test of running variable in alternative estimation samples

(a) Flipped seats

(b) Coalition–Opposition

(c) Conservative–Labour

(d) Lib Dem–Labour

Notes: Margin to victory is calculated as vote share difference between (a) one of the Coalition parties and Labour in flipped seats, (b) in constituencies contested by one of the Coalition parties and any opposition party, (c) the Conservative party and the Labour party, (d) the Liberal Democrats and the Labour party. Dots represent robust bias-corrected estimated densities and shaded area represents the 95 % confidence intervals. Bias-corrected differences in estimated differences in densities and standard errors presented.

C Robustness

Figure C.1: Regression discontinuity robustness – Bandwidth sensitivity for linear and quadratic polynomials

Notes: The chart presents RD estimates for the effect of Coalition alignment on JSA sanction rate. Each dot represents a separate RD estimate from local linear (left panel) or local quadratic (right panel) regression, with demographic and socioeconomic controls and a triangular kernel. Each panel reports RD estimates adjusting only the bandwidth used to fit polynomials. Thick bars represent CCT robust bias-corrected 90 % CIs, and thin bars represent 95 % CIs, respectively. Vertical dotted lines represent the MSE-optimal bandwidth, h, as well as $h/2$. RD regressions maintain constant h/b ratio between bandwidth for the RD estimator and bias correction. Dashed horizontal lines represent the baseline estimates.

Sensitivity to Bandwidth Choice   Employing local linear non-parametric regression (Hahn et al. 2001), estimates may be sensitive to the choice of bandwidth, h, employed in determining how to weight observations based on their distance from the cut-off. For example, on the positive side of the threshold, outcome y^+ would be modeled as:
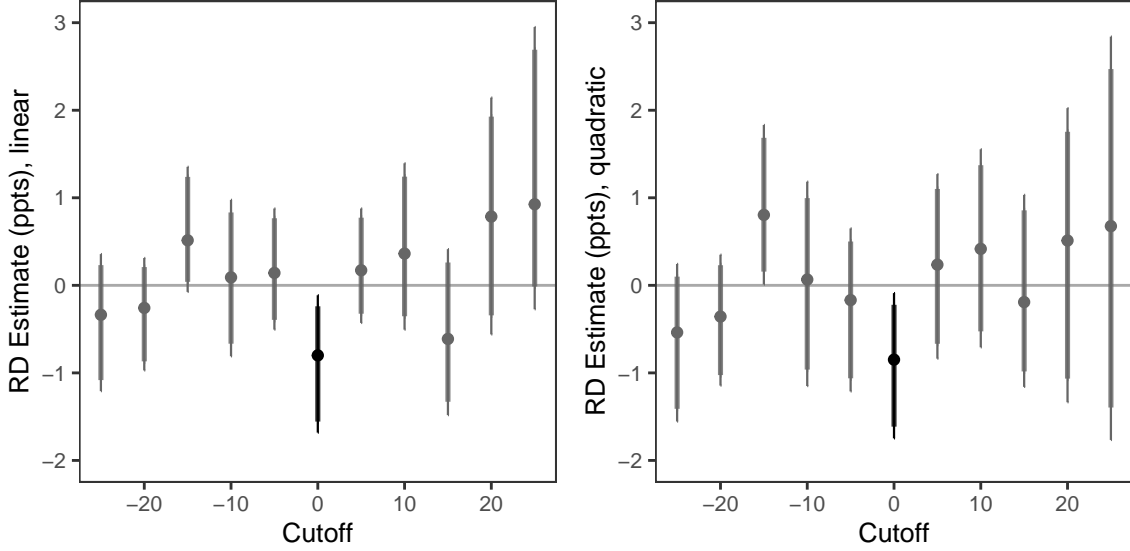
$$\hat{y}^+ = \arg\min_{a,b} \sum_i \left(Y_i - a - b(m_i - c)\right)^2 K\left(\frac{m_i - c}{h}\right) 1(m_i > c) \tag{4}$$

We examine various alternative bandwidths, centered on the optimal choice as determined by the methodology of Calonico et al. (2014).

Figure C.1 shows RD estimates using a range of different bandwidths for the estimator. The estimate remains stable and statistically significant for a wide range of estimation windows. As the bandwidth narrows and the sample size decreases, the estimates become somewhat less precise.

Placebo Cut-Offs   To examine the robustness of our main result we also employ placebo cut-offs at the left and right of the true discontinuity: $\{c \pm 5, \pm 10, ..., \pm 25\}$, for linear and quadratic polynomials. Overall, baseline results appear to be robust—cutting

Figure C.2: Regression discontinuity robustness – Placebo cut-offs

Notes: Each dot represents a separate RD estimate from local linear (left panel) or local quadratic (right panel) regression, with demographic and socioeconomic controls and a triangular kernel. Each panel reports repeated RD regressions with linear or quadratic fits, adjusting only the cut-off. Placebos are presented in grey, while baseline estimates are presented in black. Thick bars represent CCT robust bias-corrected 90 % CIs, and thin bars represent 95 % CIs, respectively.

the running variable at arbitrary thresholds elsewhere on the support does not generate estimates which are statistically different from zero (Figure C.2). However, there is a fair degree of variability in the placebo point estimates, possibly due to errors in model fit being particularly bad in areas where a polynomial fit is a bad approximation.
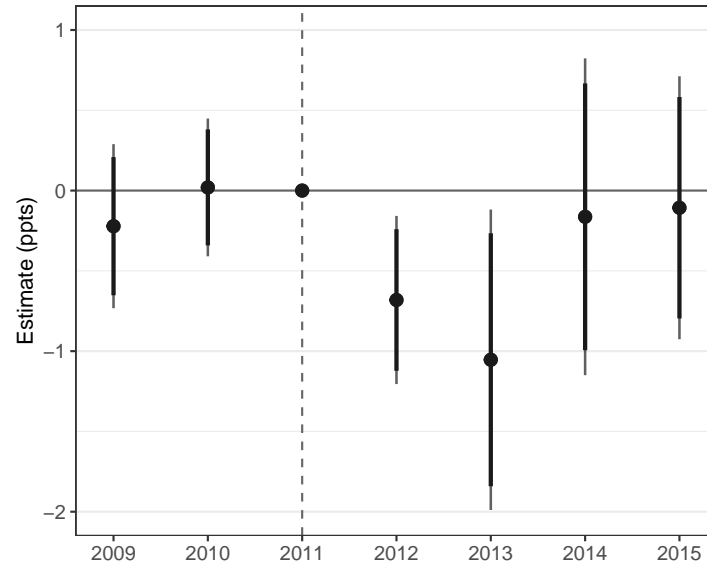
Difference-in-Discontinuities   The year-by-year RD estimates (see Figure 4) show that significant partisan effects on unemployment sanctions appear only with the change in national sanctions policy in 2012. We find no discontinuities in years 2010 and 2011. Estimating the RD model by year is intuitively very similar to the difference-in-discontinuities design. As a further robustness test, we also formally estimate a difference-in-discontinuities model.

Following Grembi et al. (2016), we restrict the sample to constituencies with $m_i \in [0 - h, 0 + h]$ and estimate the following regression:

$$Y_{it} = \delta_0 + \delta_1 m_i + D_i(\gamma_0 + \gamma_1 m_i) + \sum_t \mathbb{1}\{\text{Year} = t\}[\tau_t + \gamma_t m_i + D_i(\beta_t + \lambda_t m_i)] + \varepsilon_{it} \quad (5)$$

where D_i is a dummy for a Coalition seat, m_i is the Coalition win margin in constituency i. The main difference to the regression in Grembi et al. (2016) is that instead of a pooled model with a time indicator $\mathbb{1}\{\text{Year} \geq 2012\}$, we consider a dynamic model with a year indicator $\mathbb{1}\{\text{Year} = t\}$, using 2011 as the reference year. The causal parameters of interest are β_t which capture the treatment effect of change in sanction policy in constituencies aligned with the central government. For the bandwidth h, we use the CCT optimal bandwidth from our baseline RD model (Table 2, column (1)).

9

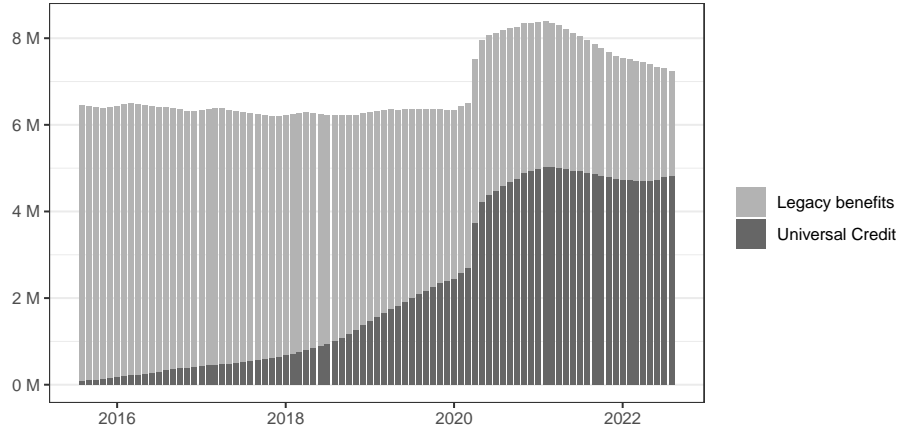Figure C.3: Difference-in-discontinuities results for the effect of Coalition win on JSA sanction rates

Notes: Each dot represents an estimate of β_t from Equation 5. 2011 is the reference year, and the Welfare Reform was enacted in March 2012. Thick bars represent 90 % CIs, and thin bars represent 95 % CIs, respectively.

Figure C.3 presents the diff-in-disc estimates. Similarly to the year-by-year RD estimates, we find significant decrease in the sanction rates in central government aligned districts following the 2012 Welfare Reform. The biggest effect takes place in 2013. We find no evidence of discontinuities in the election year 2010, or in 2009.
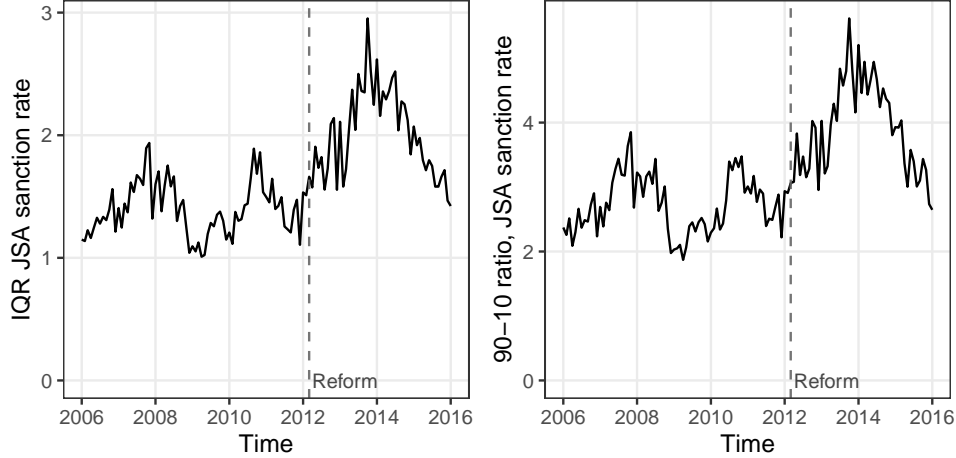
D Additional Figures

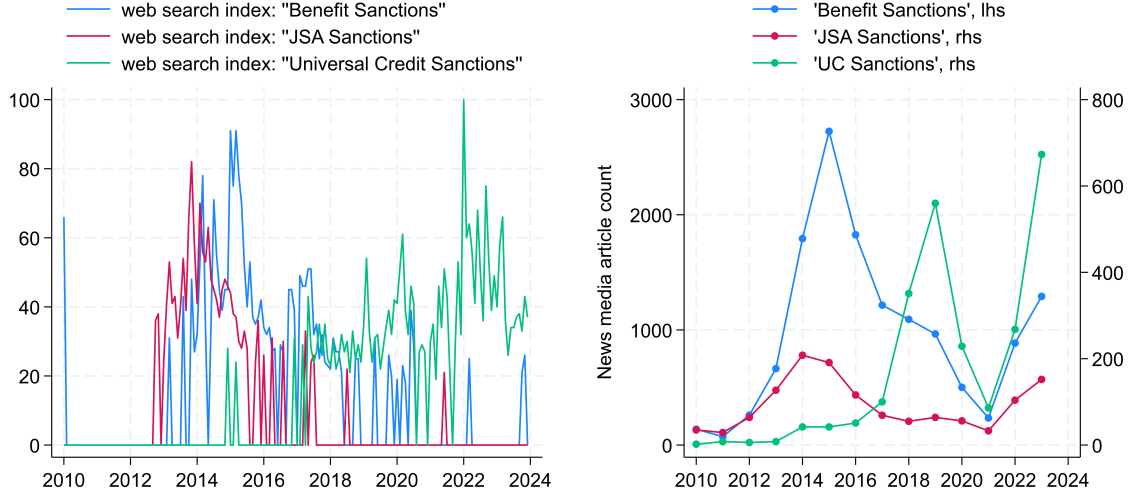Figure D.1: Number of households on Universal Credit and on legacy benefits

Notes: By the end of 2018 claiming Universal Credit was possible in all jobcentres in Great Britain, and the DWP expects that all households claiming legacy benefits will be transitioned to Universal Credit by the end of 2024 (House of Commons Library 2023).

Figure D.2: Dispersion in Jobseeker's Allowance (JSA) sanction rates

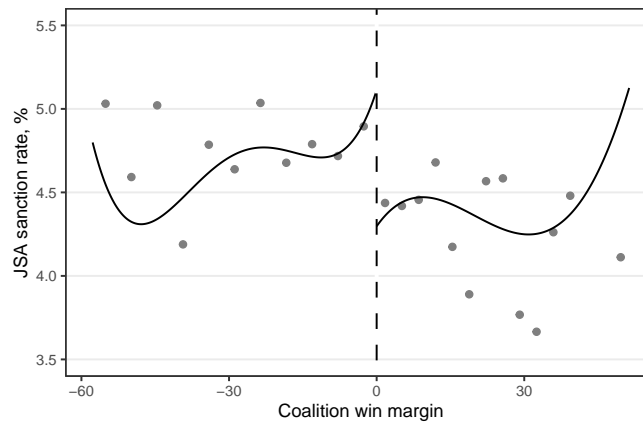*Notes*: Interquartile range (left-side panel) is computed as the $p75 - p25$, by month. Similarly, for the $p90 - p10$ ratio (right-side panel). The vertical dotted lines indicate the enactment of the Welfare Reform Act in March 2012.

Figure D.3: Internet Search Activity and News Media Coverage

Notes: Internet search index (left panel) obtained from Google. 100 = highest search intensity across all three terms. News media coverage (right panel) obtained from Nexis news mentions database.

Figure D.4: Regression Discontinuity plot – Global polynomial fit

Notes: The chart presents a fourth-order global polynomial regression discontinuity fit on each side of the electoral cut-off. Dots are binned average sanction rates over the post-reform period from March 2012 to December 2015

Figure D.5: RDD results for the effect of Coalition win on JSA sanction rates by year: Full sample and flipped Labour to Coalition seats

Notes: The chart presents annual RD estimates for the effect of Coalition alignment on average annual sanction rate. Each dot represents a separate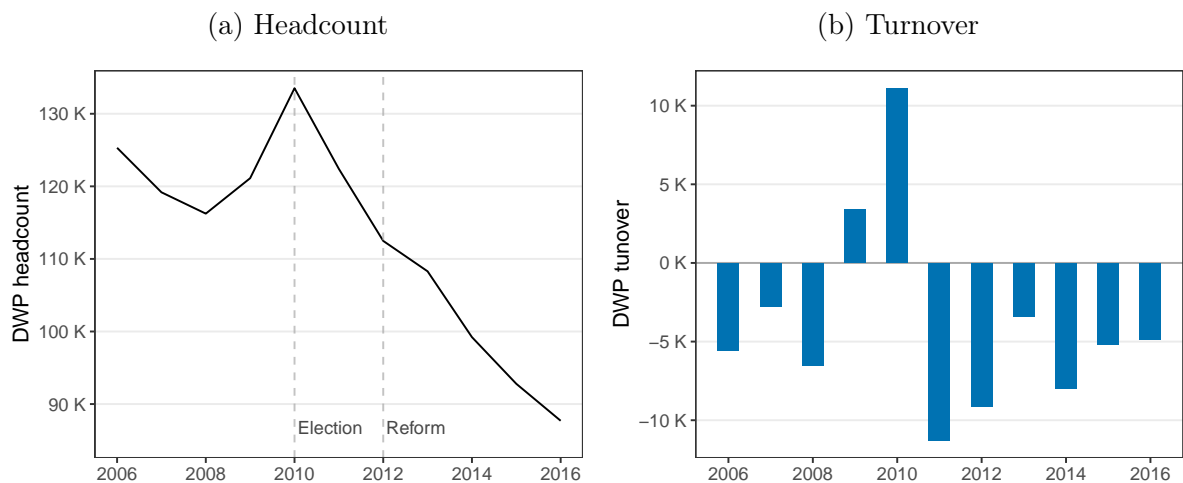 local linear RD regression. Demographic and socioeconomic controls included. Dashed vertical line splits the years into pre- and post-reform periods. Blue dots represents estimates from sample of only seats held by Labour in 2005, and grey triangles represents estimates from full sample. All regressions use MSE-optimal bandwidth and triangular kernel. Vertical bars represent CCT robust bias-corrected 95 % confidence intervals.

Figure D.6: Department for Work and Pensions headcount and turnover

(a) Headcount

(b) Turnover

Notes: Civil service headcount and difference between entrants and leavers in the Department for Work and Pensions during 2006–2016. The vertical dashed lines indicate time of the elections and enactment of the Welfare Reform Act. Data source: Office for National Statistics.

E Additional Tables

Table E.1: RDD results for the effect of Coalition win on JSA sanction rate by year

	2009 (1)	2010 (2)	2011 (3)	2012 (4)	2013 (5)	2014 (6)	2015 (7)
Coalition win	0.000	-0.230	-0.025	-0.675**	-1.424***	-0.650	-0.407
	(0.256)	(0.374)	(0.364)	(0.380)	(0.572)	(0.629)	(0.383)
Robust p-value	0.843	0.370	0.801	0.046	0.005	0.228	0.253
Robust 95 % CI	[-0.45, 0.55]	[-1.07, 0.4]	[-0.81, 0.62]	[-1.5, -0.01]	[-2.71, -0.47]	[-1.99, 0.48]	[-1.19, 0.31]
Bandwidth	17.10	9.58	11.55	14.35	11.41	13.54	12.39
N	205	130	152	186	151	178	162
Controls	✓	✓	✓	✓	✓	✓	✓
Mean(Y)	2.44	3.88	3.65	4.34	5.43	4.96	3.14

*p<0.1; **p<0.05; ***p<0.01. The dependent variable is JSA sanction rate in a given year. The table reports both conventional and bias-corrected estimates from local linear RD specifications. All regressions use MSE-optimal bandwidths and a triangular kernel. Controls include log population, share of women, share of working age, median earnings, and employment rate. Standard errors in parentheses. CCT robust bias-corrected p-values and 95 % CIs are reported. Mean(Y) is an unconditional sample average.

Table E.2: RDD results for the effect of Coalition win for flipped Labour to Coalition seats

	Linear			Quadratic		
	(1)	(2)	(3)	(4)	(5)	(6)
Coalition win	-1.076**	-1.311***	-1.569***	-1.223**	-1.510***	-2.140***
	(0.549)	(0.475)	(0.591)	(0.653)	(0.637)	(0.813)
Robust p-value	0.029	0.004	0.003	0.049	0.008	0.004
Robust 95 % CI	[-2.28, -0.12]	[-2.31, -0.44]	[-2.89, -0.57]	[-2.56, -0.01]	[-2.93, -0.43]	[-3.94, -0.76]
Bandwidth	8.14	9.45	4.73	8.43	7.40	3.70
N	97	106	52	103	83	42
Controls	-	✓	✓	-	✓	✓

*p<0.1; **p<0.05; ***p<0.01. The dependent variable is JSA sanction rate in the post-reform period from March 2012 to December 2015. The sample includes only seats won by Labour in 2005. All regressions use MSE-optimal bandwidths and a triangular kernel. Controls include log population, share of women, share of working age, median earnings, and employment rate. Standard errors in parentheses. CCT robust bias-corrected p-values and 95 % CIs are reported.

Table E.3: RDD results for the effect of Coalition win on JSA sanction rate for flipped Labour to Coalition seats by year

	2009 (1)	2010 (2)	2011 (3)	2012 (4)	2013 (5)	2014 (6)	2015 (7)
Coalition win	-0.265	-0.551*	-0.568	-0.920**	-1.886***	-1.480*	-0.933**
	(0.287)	(0.299)	(0.378)	(0.398)	(0.558)	(0.760)	(0.397)
Robust p-value	0.377	0.066	0.139	0.044	0.002	0.074	0.021
Robust 95 % CI	[-0.95, 0.36]	[-1.3, 0.04]	[-1.52, 0.21]	[-1.81, -0.02]	[-3.3, -0.73]	[-3.37, 0.16]	[-1.94, -0.16]
Bandwidth	6.49	7.52	5.32	7.43	10.21	7.59	6.91
N	69	84	61	83	114	86	75
Controls	✓	✓	✓	✓	✓	✓	✓
Mean(Y)	2.57	4.04	3.76	4.55	5.66	5.15	3.17

*p<0.1; **p<0.05; ***p<0.01. The dependent variable is JSA sanction rate in a given year. The sample includes only seats won by Labour in 2005. All regressions use MSE-optimal bandwidths and a triangular kernel. Controls include log population, share of women, share of working age, median earnings, and employment rate. Standard errors in parentheses. CCT robust bias-corrected p-values and 95 % CIs are reported. Mean(Y) is an unconditional sample average.

Table E.4: RDD results for the effect of winning in alternative head-to-head races on JSA sanction rate

	Coalition—Labour (1)	Coalition—Any opposition (2)	Conservative—Labour (3)	Lib Dem—Labour (4)
Coalition win	-0.800**	-0.780**	-1.089**	-0.681
	(0.400)	(0.393)	(0.520)	(0.927)
Robust p-value	0.025	0.028	0.019	0.371
Robust 95 % CI	[-1.68, -0.11]	[-1.64, -0.09]	[-2.24, -0.21]	[-2.65, 0.99]
Bandwidth	12.80	13.30	10.69	5.57
N	171	181	114	16
Controls	✓	✓	✓	✓
Mean(Y)	4.74	4.78	4.73	4.84

*p<0.1; **p<0.05; ***p<0.01. The dependent variable is JSA sanction rate in the post-reform period from March 2012 to December 2015. All regressions use MSE-optimal bandwidths and a triangular kernel. Controls include log population, share of women, share of working age, median earnings, and employment rate. Standard errors in parentheses. CCT robust bias-corrected p-values and 95 % CIs are reported. The mean outcome is calculated as $E[Y_i| -h \leq m_i < 0]$, where h is the MSE-optimal bandwidth.

Table E.5: RDD results for the effect of Coalition win on JSA sanctions at different sanction levels

	Linear			Quadratic		
	Low	Intermediate	High	Low	Intermediate	High
	(1)	(2)	(3)	(4)	(5)	(6)
Coalition win	-20.038	-16.125***	-1.732	-22.600	-17.834***	-1.258
	(17.363)	(6.425)	(3.083)	(20.885)	(6.966)	(3.222)
Robust p-value	0.226	0.005	0.586	0.218	0.006	0.815
Robust 95 % CI	[-55.04, 13.02]	[-30.8, -5.62]	[-7.72, 4.37]	[-66.65, 15.22]	[-32.88, -5.57]	[-7.07, 5.56]
Bandwidth	14.34	11.41	13.65	18.94	18.19	23.67
N	186	151	178	223	215	255
Controls	✓	✓	✓	✓	✓	✓
Mean(Y)	109.09	32.97	23.7	109.09	32.97	23.7

*p<0.1; **p<0.05; ***p<0.01. Dependent variable is number of sanctions (March 2012 – December 2015) in a given category. All regressions use a triangular kernel and MSE-optimal bandwidths, and controls for log population, share of women, share of working age, median earnings, and employment rate. Standard errors in parentheses. CCT robust bias-corrected p-values and 95 % CIs are reported.

Table E.6: RDD results for the effect of Coalition win on JSA sanction rate: Full sample, nested and aligned seats with LAD councils

	Linear			Quadratic		
	Full sample	Nested	Aligned	Full sample	Nested	Aligned
	(1)	(2)	(3)	(4)	(5)	(6)
Coalition win	-0.800**	-1.656***	-1.955***	-0.849**	-1.631***	-2.103***
	(0.400)	(0.357)	(0.688)	(0.423)	(0.431)	(0.714)
Robust p-value	0.025	0.000	0.002	0.030	0.000	0.002
Robust 95 % CI	[-1.68, -0.11]	[-2.5, -1.11]	[-3.49, -0.8]	[-1.75, -0.09]	[-2.56, -0.87]	[-3.64, -0.83]
Bandwidth	12.80	8.20	8.53	24.12	14.00	16.14
N	171	86	44	263	135	82
Controls	✓	✓	✓	✓	✓	✓

*p<0.1; **p<0.05; ***p<0.01. The dependent variable is JSA sanction rate (March 2012 – December 2015). Columns (1) and (4) show the full sample estimates, columns (2) and (5) use a subsample of seats nested within one LAD district, columns (3) and (6) use a subsample of seats aligned with LAD party in 2012. Estimates in columns (1)-(3) are from local linear and estimates in columns (4)-(6) from quadratic regressions. All regressions use MSE-optimal bandwidth and a triangular kernel. Controls include log population, share of women, share of working age, median earnings, and employment rate. Standard errors in parentheses. CCT robust bias-corrected p-values and 95 % CIs are reported.