Unemployment, sanctions and mental health: the relationship between benefit sanctions and antidepressant prescribing

Abstract

International social security systems increasingly place work-related conditions on individuals claiming out-of-work benefits, and enforce requirements through the use of benefit sanctions. The literature on the impacts of benefit sanctions considers both labour market and wider social effects, which this study contributes to through a focus on mental health. It considers the period of Coalition government (2010-15) in the UK, which imposed a comparatively high number of benefit sanctions and increased their severity through the Welfare Reform Act 2012. A longitudinal dataset is constructed using quarterly local authority-level data on Jobseeker's Allowance (JSA) sanctions and antidepressant prescriptions in England. Results from fixed effects analyses indicate that, in the post-reform period, every 10 additional sanctions are associated with 4.57 additional antidepressant prescribing items (95% CI: 2.14 to 6.99), which translates to approximately one additional person receiving treatment. Importantly, this finding indicates that sanctions are associated with both adverse mental health impacts and wider public expenditure implications, which motivates further investigation at the individual-level. In addition, punitive sanctions form a core part of the new Universal Credit (UC) and so the results suggest the need to reassess the use of sanctions within the contemporary social security system.

Key words: Conditionality, Activation, Unemployment, Benefit Sanctions, Mental Health

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1. Introduction

Across international social security systems, work-related behavioural conditions are increasingly attached to the receipt of out-of-work benefits and enforced through the threat and imposition of benefit sanctions (Knotz, 2018). The ethical justification of this process is highly contested by competing normative frameworks, though a central issue that all perspectives must attend to concerns the 'effectiveness in practice' (Watts and Fitzpatrick, 2018: 152) of conditional approaches. Research into the impacts of conditionality and sanctions has predominantly focused on labour market effects (Griggs and Evans, 2010), though a growing literature identifies a wider range of sanction-related outcomes such as financial hardship, homelessness and food bank usage (Watts *et al*., 2014). This article contributes to the empirical literature through a quantitative investigation into the mental health impacts of benefit sanctions, and thereby aims to inform the debate surrounding the role of conditionality within the contemporary social security system.

Whilst the development of 'activation requirements' has occurred internationally, this article considers the UK which currently maintains one of the most punitive approaches towards unemployment benefits across Europe and the OECD (Immervoll and Knotz, 2018). Whilst successive UK governments have expanded and intensified the work-related behavioural requirements demanded of claimants, the focus in this article is on sanctions policy during the Coalition government (2010-15). This period is characterised by what Webster (2016: 2) describes as a 'great sanctions drive', in which a comparatively high number of benefit sanctions were imposed and their severity increased through the Welfare Reform Act 2012. Following this period, a review of the benefit sanctions regime by the National Audit Office (NAO, 2016a) concluded that the reforms introduced by the Department for Work and Pensions (DWP) were not informed by sufficient evidence, which it argued needed to consider labour market effects alongside wider impacts on claimants and additional public expenditure costs. This informs the current aim to consider the impact of these changes now that they have been implemented.

Specifically, this article investigates the relationship between Jobseeker's Allowance (JSA) sanctions and antidepressant prescribing, an outcome that reflects impacts on both the mental health of claimants as well as on public expenditure more widely. To do so, a longitudinal dataset is constructed that uses quarterly local authority-level data on JSA sanctions and antidepressant prescriptions in England. Fixed effects models are then estimated that control for differences between local authorities, permitting investigation into the relationship between changes in JSA sanctions and corresponding changes in antidepressant prescribing within local authorities themselves. Importantly, the analysis considers whether this relationship changed following the Welfare Reform Act 2012, and the subsequent implementation of a comparatively harsher sanctions regime. These developments form the basis for current sanctions policy within Universal Credit (UC), and so the study offers insights to be considered as UC continues its protracted rollout. The remainder of this paper is divided into four parts: section 2 provides greater detail into sanctions and their possible links to mental health, followed by an explanation of the data and methods (section 3) and results of the analysis (section 4). Section 5 discusses the results and concludes.

2. Background

Conditionality, activation and benefit sanctions

Entitlement to unemployment benefits has never been fully unconditional; initial access, for example, has always been regulated by rules that require claimants to be available for work (Adler, 2016). In recent decades, however, a process of 'benefit activation' (Clasen and Clegg, 2011: 9) has seen the re-configuration of international social security systems so as to increasingly demand the fulfilment of work-related behavioural requirements as a condition of ongoing benefit receipt. Consequently, claimants are now expected to meet various availability and suitable work criteria, as well as job-search requirements and participation in training programmes or even unpaid work placements (Immervoll and Knotz, 2018). These conditions are enforced through monitoring and the threat and imposition of financial penalties, known as benefit sanctions. Importantly, the particular work-related conditions, as well as the magnitude and length of sanctions, vary across countries, including the claimant groups to which they apply. In the UK, for example, conditions and sanctions now affect benefits designed to support unemployed individuals, lone parents, long-term sick and disabled people as well as people in low-paid employment (Dwyer and Wright, 2014).

The shift towards work-related behavioural conditionality is an important constitutive element of a broader 'activation turn' (Bonoli, 2010: 435) in international social security and labour market policy. Distinctive from earlier forms of active labour market policies (ALMPs), initiatives since the 1990s have combined work incentivisation and job-search assistance through a variety of 'punitive and enabling mechanisms' (Raffass, 2017: 350). With regards work incentives, activation has involved the development of minimum wages and in-work benefits alongside cuts in the generosity of benefits and sanctions (Immervoll and Scarpetta, 2012). In terms of employment assistance, furthermore, activation differs according to 'human capital development' and 'work first' approaches (Lindsay *et al*., 2007). Human capital development emphasises investment in skills, education and training, whereas work first approaches focus on job search and more basic skills training in order to get unemployed individuals into work as quickly as possible. Prior to 2010, the UK's work first approach to activation combined both disciplinary and assistive measures, in the form of sanctions and a variety of employment-related support initiatives (Lindsay, 2007). These supportive aspects were then diminished, however, as a result of the Coalition government's (2010-15) subsequent 'punitive turn' (Fletcher and Wright, 2018: 324).

A variety of demanding social security reforms were implemented by the Coalition government; this article focuses specifically on sanctions imposed on unemployed individuals claiming Jobseeker's Allowance (JSA), which saw two important developments. First, the frequency of sanctions was consistently high compared with previous rates observed since the introduction of JSA in 1996 (NAO, 2016a). Between 2010 and 2015, nearly a quarter (24%) of JSA claimants received at least one sanction; monthly sanctions rates varied dramatically, more than doubling from approximately 3% in May 2010 to a peak of over 7% in October 2013, before gradually returning to their pre-Coalition level by late 2015. Second, the severity of sanctions was increased by the Welfare Reform Act 2012. Following its implementation in October 2012, the minimum sanction period increased from one to four weeks and the maximum from 26 to 156 weeks, depending on the type and number of sanctionable actions incurred (DWP, 2013). Such sanctions represent full benefit withdrawal for the period in question, from an already low benefit level. Sanctioned claimants can apply for hardship payments amounting to 60% of JSA or 80% for those deemed 'vulnerable', which begin in the third week after a sanction for the former group and immediately for the latter group. Before the October 2012 changes, less than 10% of sanctions resulted in a hardship award, a figure that rose to over 40% by the end of 2014 (Webster, 2015).

Sanctions policy generally, including these developments specifically, has generated significant debate regarding impacts on claimants, and it is to this literature that this article now turns.

The impacts of benefit sanctions

Work-related conditionality and benefit sanctions are explicitly intended to improve labour market outcomes for claimants, which has motivated an extensive literature on labour market effects (Griggs and Evans, 2010). Evidence from across different social security systems arguably provides some support to the claim that sanctions improve rates of employment re-entry in the short-term. In the longer-term, however, the same literature suggests that there are negative impacts on wages and job stability as well as no perceivable employment effect (Arni *et al*., 2013; van den Berg and Vikström, 2014; van den Berg *et al*., 2017; Taulbut *et al*., 2018). The most methodologically robust UK study is based on long-term unemployed Work Programme participants (NAO, 2016b). It finds that sanctions increase the likelihood of employment up to a year after being sanctioned, though the impact on earnings is negative. Indeed, sanctions are also found to increase exits from JSA without employment, a result that is supported by additional UK analysis conducted at the local authority-level (Loopstra *et al*., 2015). Importantly, the results of the NAO (2016b) study differ by the claimant group in question; employment re-entry effects are in fact negative for those claiming Employment and Support Allowance (ESA) due to sickness or disability.

In light of the mixed evidence on labour market impacts, a growing area of research identifies a wider range of possible, consistently negative, sanction-related impacts. The risk that sanctions will lead to financial hardship for claimants is confirmed by several policy reviews carried out by separate UK government departments (Vincent, 1998; Saunders *et al*., 2001; Peters and Joyce, 2006; Dorsett *et al*., 2011). In the largest of these studies, Peters and Joyce (2006) found that over two-thirds of sanctioned claimants experienced financial hardship, whilst many were forced to borrow money from friends and family in order to survive. These individuals had difficulty paying utility bills, rent and managing debt; many had already been struggling to get by financially on JSA, a situation that was aggravated by the imposition of a sanction. These findings are supported by the academic literature, where sanctions have been linked to rises in the number of people being fed by food banks in both qualitative and quantitative research (Lambie-Mumford, 2014; Loopstra *et al*., 2018). Sanctions have also been associated with negative impacts on third parties, including the friends, family and children of claimants (Watts *et al*., 2014).

It is increasingly recognised, furthermore, that sanctions are likely to affect the mental health and wellbeing of claimants. Evidence with regards the impact of JSA sanctions specifically is, however, scarce. In particular, Stewart and Wright (2018) conduct longitudinal qualitative interviews with JSA claimants and find that sanctions are commonly associated with impacts such as stress, anxiety and depression, caused both by the fear of and actual imposition of benefit sanctions. Additional qualitative research identifies negative psychological impacts of sanctions on groups such as lone parents, disabled people and homeless people (Dwyer *et al*., 2018; Johnsen *et al*., 2018; Johnsen and Blenkinsopp, 2018). Quantitative research in this area has focused more broadly on the impact of work-related behavioural conditionality. A natural experiment study by Katikireddi and colleagues (2018), for example, finds that conditionality negatively impacts the mental health of lone parents in the UK, which is supported by evidence from the US (Davis, 2019). The research carried out here, in contrast, contributes to the emerging literature through consideration of the quantitative relationship between sanctions and mental health, focusing in particular on JSA sanctions and rates of antidepressant prescribing.

The determinants of antidepressant prescribing

Antidepressant medication is prescribed to treat individuals suffering from anxiety and depression, and therefore represents a salient – albeit imperfect – means of investigating these particular aspects of mental health. Not all individuals suffering from anxiety and depression will be prescribed antidepressant medication, since there are differences in the likelihood of recognising and reporting mental health problems, as well as in GP prescribing behaviour and the exploration of alternative treatments (Hyde *et al*., 2005). A high correlation, nevertheless, exists between the two (Barr *et al*., 2016). Existing quantitative research on the determinants of antidepressant prescribing in the UK considers the factors that explain variations in prescribing rates at the GP practice-level, and takes into account a combination of the characteristics of registered patients, the GP practice itself as well as area-level determinants. Spence and colleagues (2014), for example, find that antidepressant prescribing is higher in GP practices that have patients with higher proportions of older people, women and white people. Morrison and colleagues (2009), furthermore, find that greater proportions of GPs who are female, young or born in the UK are also associated with higher levels of antidepressant prescribing.

The quantitative analysis in this study is carried out solely at the local authority-level, which – due to data availability – is a feature of the UK quantitative literature on the impacts of sanctions more generally. Area-level factors associated with higher antidepressant prescribing at the GP-level include higher levels of deprivation as well as greater levels of urbanisation (Morrison *et al*., 2009; Sreeharan *et al*., 2013). Additional area-level research emphasises the influence of economic factors, with higher levels of antidepressant prescribing being associated with higher unemployment rates and lower rates of economic output (Barr *et al*., 2016). The study by Barr and colleagues (2016) also highlights the role of the social security system more broadly, finding that rates of antidepressant prescribing are higher in local authorities with a greater cumulative proportion of Work Capability Assessments (WCAs) for claimants of the main out-of-work disability benefit. Sanctions represent an important additional determinant to the factors already discussed, not least as a result of the serious material implications that they hold; in terms of JSA withdrawal absent of a hardship payment, a four-week sanction amounts to the loss of over £230 for somebody aged 18-24 and over £290 for somebody aged 25 and over (NAO, 2016a).

Ecological analyses are affected by well-known limitations, whereby correlations that hold at the area-level do not necessarily apply at the individual-level, and vice versa. Using aggregate-level data is nevertheless informative given current data constraints, as it permits investigation of the relationship between sanctions and antidepressants as indicated by variations in local authority-level rates through time. In addition, this level of analysis is able to capture potential mental health impacts of sanctions on claimants themselves as well as any possible third-party impacts on friends and family. As previously indicated, this article focuses on the period of Coalition government (2010-15) which imposed a comparatively high number of benefit sanctions and increased their severity through the Welfare Reform Act 2012. These developments in sanctions policy inform the following research questions:

1. Are benefit sanctions associated with higher rates of antidepressant prescribing at the local authority-level?
2. Does the observed relationship strengthen following the implementation of the Welfare Reform Act 2012?

3. Data and Methods

Analytic sample

NHS Digital publishes monthly antidepressant prescribing data for all practices in England, beginning in June 2010; this study carries out a quarterly analysis and thus begins at the third quarter of 2010, coinciding with the early months of the Coalition government and the initial rise in rates of JSA sanctions. February 2015 marked the start of the national expansion of Universal Credit (UC), the new benefit that replaces six existing means-tested benefits, including JSA (DWP, 2015). The rollout of UC systematically altered the composition of the remaining JSA claimant group by initially only being open to younger unemployed individuals without dependents (DWP, 2014). Due to data availability, however, UC sanctions could not be included in the analysis; in order to minimise the influence of compositional change, therefore, data are included up to and including the fourth quarter of 2014, prior to the national rollout. In the remaining pre-2015 sample, 31 local authorities were affected by the Pathfinder phase of UC that began in April 2013, totalling 78 local authority quarters. These local authority quarters are removed in the analysis presented here, though the results remain substantively unchanged with or without their inclusion; this similarity is unsurprising given the small number of individuals claiming UC by December 2014 (DWP, 2018). There are 326 local authority areas in England, though the City of London and the Isles of Scilly are excluded from the analysis due to their small population size.

Antidepressant prescribing data

Data on the number of antidepressant items prescribed by GP practices in England are accessed from NHS Digital (2018), measured using 'Selective Serotonin Re-Uptake Inhibitors' (SSRIs), the first-line medication for treating anxiety and depression (NICE, 2015). SSRIs are the most appropriate indicator to capture impacts on anxiety and depression since the broader total antidepressant measure includes items prescribed to treat non-psychiatric health conditions such as chronic pain (Spence *et al*., 2014). Prescription items are single supplies of a medicine that generally refer to month-long prescriptions, though the length of prescription items will vary depending on the length of treatment or quantity of medicine prescribed (HSCIC, 2015). Quarterly rates of SSRI prescribing per 100,000 population are constructed for each local authority by aggregating monthly GP practice-level data and using mid-year population estimates that were updated in 2018 and available through Nomis (ONS, 2018). A key limitation is that the prescribing data do not contain any patient-related information, meaning that it is not possible to construct prescribing rates per working age population, which is the group who are at risk of sanctioning if claiming JSA. Consequently, all variables included in the analysis are expressed as quarterly rates per 100,000 total population. This is unlikely to unduly influence the results obtained, however, which control for age and estimate the effect of sanctions by exploiting variations in local authority-level rates through time.

Sanctioning data and additional explanatory variables

Data on the number of JSA sanctions are accessed from Stat-Xplore (DWP, 2018). This database is limited in that it records only the latest decision for each sanction case, meaning that it is not possible to calculate the total number of sanctions imposed in any one quarter. Sanctions that have gone through the review, reconsideration or appeal process, for example, will be recorded at a later point in time from the original sanctioning decision, even though claimants will have had their benefits stopped throughout. Consistent with other studies in the literature, the analysis here uses original adverse sanctions as its main sanctions variable (Loopstra *et al*., 2018; Taulbut *et al*., 2018). This measure benefits from its specificity to the quarter in question, though it underestimates the true sanctions figure as it does not include sanctions that are challenged, reaching up to a fifth of sanctions during the period of analysis (Kennedy and Keen, 2016). One additional limitation is that it

is not possible to include ESA sanctions at the local authority-level. Similar to JSA sanctions, ESA sanctions saw a rise and fall in the frequency of their application around October 2013, whilst their severity increased from December 2012 (Webster, 2016). These variations, nevertheless, were smaller than those for JSA sanctions and occurred from a lower base-level. Indeed, Figure A1 in the online appendix contrasts the JSA and ESA sanctions rates throughout the period using original adverse sanctions in England; the contrasting trends suggests that the omission of ESA sanctions does not represent a serious source of omitted variable bias on the results of the analysis.

Additional explanatory variables are included in the analysis, informed by the previous discussion on the determinants of antidepressant prescribing and sourced from Nomis, Stat-Xplore and additional UK government departments (see online appendix Table A1). These include data on the monthly number of JSA claimants, averaged over the quarter to provide an estimate of the quarterly claimant count, as well as GVA per head – a local authority-level equivalent of GDP – to capture the influence of economic trends. Unemployment measured according to the ILO definition is also investigated, as well as economic inactivity, using Annual Population Survey (APS) data for 12-month periods beginning every quarter. In addition to GVA per head, a residence-based measure of local authority-level income – GDHI per head – is investigated, which did not alter the substantive results presented here. Demographic characteristics such as annual proportions of separate age groups, gender and ethnicity are included, as well as quarterly rates of WCAs, the Index of Multiple Deprivation and rural-urban classification. Finally, quarterly rates of antibiotic prescribing are included as a proxy for the propensity of GPs to prescribe in general, reflective of discretionary prescribing behaviour (Spence *et al*., 2014).

Statistical analysis

To investigate the relationship between sanctions and SSRI prescribing, the analysis estimates fixed effects models with a basic form described in Equation 1:

$$SSRI_{i,t} = \beta_0 + \beta_1 Sanctions_{i,t} + \beta_2 X_{i,t} + \mu_i + \lambda_t + \varepsilon_{i,t} \qquad (1)$$

In Equation 1, *i* denotes the local authority and *t* denotes the quarter. *SSRI* is the SSRI prescribing rate per 100,000 population, *Sanctions* is the JSA sanctions rate per 100,000 population and *X* represents a vector of additional control variables. μ denotes local authority fixed effects, λ denotes time fixed effects and ε represents the error term. The inclusion of local authority fixed effects controls for time-invariant unobserved differences between areas, meaning that the analysis estimates the average association between sanctions and antidepressants within local authorities. The inclusion of time fixed effects controls for the influence of factors that are constant across local authorities but that vary over time, without the need to impose a functional form on the relationship between SSRI prescribing and time. All estimated models use Driscoll-Kraay standard errors, which are robust to heteroscedasticity, correlation through time within local authorities and general forms of cross-sectional dependence (Hoechle, 2007).

As part of the initial regression modelling process, additional time-variant control variables were first included (see online appendix Table A2, Model A1). As a sensitivity check, this model was repeated with the inclusion of separate time trends by quintile of deprivation and rural-urban classification, which allows the trend in antidepressants to vary by level of baseline deprivation and rurality (Model A2). The coefficients for these interactions estimate how the effect of deprivation and rurality change over the period, with their main baseline effects absorbed into the local authority fixed effects as in Equation 1 (Allison, 2009). Given the significance of these interactions and the increase in within-R^2, this model was ultimately favoured and its results will be discussed in full in the next section. Diagnostic tests for this model are detailed in the online appendix. As a final check, the

analysis was repeated through estimation of a random effects model, which adjusts for time-invariant factors by making the relatively stricter assumption that any omitted variables are uncorrelated with the included explanatory variables (Model A3). A Hausman test of this assumption indicates that a random effects framework should not be favoured here ($p < 0.001$).

The next stage of the analysis considers the second research question outlined above, and investigates the impact of the Welfare Reform Act 2012. To capture the effect of this reform, which increased the average length of sanctions, Equation 2 modifies the initial fixed effects analysis through inclusion of an interaction term with *Sanctions* and *Reform*:

$$SSRI_{i,t} = \beta_0 + \beta_1 Sanctions_{i,t} + \beta_2 (Sanctions_{i,t} * Reform_t) + \beta_3 X_{i,t} + \mu_i + \lambda_t + \varepsilon_{i,t} \quad (2)$$

Reform is a dummy variable that marks the quarters before and after the implementation of the harsher sanctions regime brought about by the Welfare Reform Act 2012; it is coded 1 for quarters Q4 2012 onwards and 0 before that date.

4. Results

Full time period

Results for the full time period indicate that sanction rates are positively associated with rates of antidepressant prescribing. The correlation between original adverse sanctions and SSRI prescribing is displayed in Figure 1, which indicates that in local authority quarters where the rate of sanctioning is higher, so too are rates of SSRI prescribing ($r = 0.146$; $p < 0.001$). There were an average of 12,946 SSRI items and 223 sanctions per 100,000 population per quarter in local authorities (see online appendix Table A1); Blackpool stands out in particular with an average of 24,567 SSRI items and 520 sanctions. Beatty and Fothergill (2013) estimate that Blackpool, including a number of other seaside areas (Torbay, Hastings, Great Yarmouth and Thanet) were particularly badly hit in financial terms by welfare reform under the Coalition government. This is due to the high proportions of working-age adults claiming out-of-work benefits in these areas, attracted by the availability of cheap private rental sector accommodation. As a sensitivity test, these local authorities were removed from the sample in order to mitigate any potential undue influence; the substantive results remain unchanged, however, and so the results discussed here pertain to the full sample.

[Figure 1 about here]

Estimates from the fixed effects analysis are displayed in Table 1. Importantly, the results in Model 1 indicate that sanctions are associated with increases in antidepressant prescribing rates; for every 10 additional sanctions applied per 100,000 population, the rate of SSRI prescribing is 3.71 items higher per 100,000 population ($p < 0.001$). The additional control variables included in Model 1 generally conform to the expected relationship as discussed in Section 2; a negative association exists with regards GVA , whilst a positive association exists between SSRI prescribing and rates of economic inactivity, females and WCAs at the local authority-level, though the latter result is non-significant. A zero coefficient is estimated for rates of white UK born at the local authority-level; comparison with the estimated random effects coefficient in Model A3 (online appendix Table A2) indicates that this result is explained by the well-known difficulties in estimating slowly changing variables using fixed effects (Plümper and Troeger, 2007). The estimation of the separate age group coefficients was affected by high multicollinearity, furthermore, though their inclusion did not affect the main substantive results and so are included in Model 1 in any case.

[Table 1 about here]

A key counterintuitive result in Model 1 is the estimated negative coefficient for the rate of unemployment, given the well-established link between unemployment and poor mental health, as well as the observed association with antidepressant prescribing at the individual-level (von Soest *et al*., 2012). The ILO measure of unemployment was favoured over the claimant count measure in the analysis for two key reasons; the claimant count captures a narrower set of unemployed individuals and had a high degree of collinearity with sanctions (r = 0.793), which was potentially driving an unexpected negative and statistically significant estimated coefficient. When the ILO measure of unemployment is used in Model 1, however, the coefficient remains negative but is non-significant. The correlation between unemployment and sanctions is lower (r = 0.586), suggesting that multicollinearity is not influencing the counterintuitive estimate. A plausible explanation of this result relates to the previously identified risk of ecological bias, whereby correlations that hold at the area-level do not necessarily apply at the individual-level. Indeed, this issue is present in existing area-level research into the relationship between unemployment and antidepressant prescribing, which finds contradictory results (Lundin and Hansson, 2014; Spence *et al*., 2014). This highlights an important limitation to the current study as well as emphasises the need for additional individual-level analysis to better understand the relationships that are investigated here.

Influence of the Welfare Reform Act 2012

Next, the analysis examines whether the observed associations between sanctions and SSRI prescribing are stronger in the period following the implementation of the Welfare Reform Act 2012. The full Model 2 results are displayed in Table 1, which indicate that before the implementation of the Act, for every 10 additional sanctions applied per 100,000 population the rate of SSRI prescribing is 1.74 items per 100,000 population higher, though this result is not significant at the 5% level. Consistent with the implementation of the harsher sanctions regime, however, following the reform the association increases by 2.82 prescribing items, so that every 10 additional sanctions applied per 100,000 population are associated with 4.57 additional SSRI prescribing items ($p < 0.001$). These results are summarised in Figure 2, which displays the estimated sanctions coefficient for the full period, as well as the pre- and post-Act periods.

[Figure 2 about here]

Robustness tests

In addition to the sensitivity checks already discussed, two further tests are conducted to investigate the robustness of the results obtained. First, a falsification test is carried out using the non-equivalent dependent variable approach (Shadish *et al*., 2002). This tests for omitted variables bias by identifying an additional dependent variable that should not be affected by sanctions but that could be influenced by the same potential unobserved confounding factors as for SSRI prescribing rates. Following Barr and colleagues (2016), the rate of cardiovascular drug prescribing is used, on the basis that it is unlikely that the health conditions treated by such items will be affected by sanctions, especially in the short term. Cardiovascular prescribing is not an arbitrary choice, however, as it will arguably be affected by potential unobserved confounders to SSRI prescribing, such as changes in access to primary healthcare across the study period, or changes in the propensity of individuals to report health problems to their GP. The results of the main analysis are supported by the fact that no statistically significant relationship is found between sanctions and cardiovascular prescribing, either across the time period or in the pre- and post-Welfare Reform Act periods (see online appendix Table A4).

Second, a Granger-test for reverse causality is carried out, to consider whether the main analysis specified the correct direction of causal inference (Dumitrescu and Hurlin, 2012). The focus of this article has been on the impacts of benefit sanctions at the local authority-level, though a conceivable

alternate explanation is that the direction of causality runs in the opposite direction: that there is an increased risk of sanctions in areas with higher levels of individuals already suffering from poor mental health and being prescribed antidepressants. The Granger-test is carried out in two steps. First, it tests whether lagged values of sanctions are jointly associated with SSRI prescribing, as is implied by the notion that cause precedes effect. It then tests whether lagged values of SSRI prescribing are jointly associated with sanctions, to provide an assessment of whether the model is affected by reverse causation. Here, sanctions are found to Granger-cause SSRI prescribing ($p < 0.01$) whilst SSRI prescribing is not found to Granger-cause sanctions ($p = 0.860$). This test is premised on a specific notion of causality based on the predictive content of variables, and therefore cannot be used to rule out the issue of reverse causation, though these results nonetheless support the local authority-level inferences made in the main analysis.

5. Discussion

The results of this study suggest that higher sanction rates are associated with increases in SSRI prescribing within local authorities, which is indicative of adverse mental health impacts relating to anxiety and depression. Following the implementation of the Welfare Reform Act 2012 and the introduction of a much harsher sanctions regime, the results indicate that every 10 additional sanctions applied per 100,000 population are associated with approximately 4.57 additional SSRI prescribing items, a result that is significant at the 1% level ($p < 0.001$). Given that the average length of a prescribing item is one month, meaning that one person might receive three prescribing items per quarter, this estimated quarterly relationship with SSRI prescribing items arguably translates to between one and two additional people receiving treatment. Since the analysis makes use of a sanctions indicator that underestimates the true quarterly rate, a best guess estimate would imply that every 10 additional sanctions applied per 100,000 population are associated with approximately one additional person receiving treatment.

As previously emphasised, this study is carried out at the local authority-level and the results obtained are subject to important limitations in this regard. Using such aggregate-level data, for example, it is not possible to ascertain whether the people being sanctioned are the same as those who are ultimately prescribed antidepressants. Indeed, the findings themselves pertain to SSRI prescribing items and not individuals, meaning that the estimated impact in terms of people outlined above should be treated as purely indicative. The risk of mistakenly applying area-level associations to individual-level relationships was made clear in the results section, furthermore, whilst it has been recognised that this level of analysis will also capture potential third-party mental health impacts on the friends and family of those being sanctioned. It is an important finding, nevertheless, that the scale and severity of sanctions following the implementation of the Welfare Reform Act 2012 were sufficient to have observable impacts even at the local authority-level.

The Welfare Reform Act 2012 was not limited to the JSA sanctions changes that have been the focus of this article, and the analysis is not able to control for all aspects of welfare reform that occurred throughout the period of Coalition government. In addition to providing for the introduction of Universal Credit, for example, the Act announced a benefit cap, changes to Housing Benefit (the 'bedroom tax'), the replacement of Disability Living Allowance with Personal Independence Payments and changes to eligibility for contributory ESA. The role of omitted variable bias is of concern in all quantitative analyses, and the same is true here, though it is important to emphasise that the findings are robust to a variety of different model specifications in fixed effects regressions that control for differences between local authorities. Indeed, for omitted variable bias to affect the key finding relating to the relationship between sanctions and antidepressant prescribing in the post-reform period, any omitted variable would have to be systematically correlated with sanction

rates from October 2012 onwards; it is not clear why that would pertain in the current context. These results therefore provide a valuable estimate that motivates further investigation of this issue using individual-level data, which would be better placed to identify causal impacts.

Despite the limitations outlined, the findings support existing empirical research regarding the negative mental health impacts of benefit sanctions. Indeed, there are good theoretical considerations that link sanctions to adverse mental health impacts, irrespective of a claimant's prior health status. Unemployment is itself associated with poor mental health, which has been explained through a combination of both material and psychosocial pathways (Sage, 2018); material factors are concerned with financial impacts, whilst psychosocial considerations attend to the experience of unemployment and how it is shaped by social structures. Material factors can be understood as central determinants with psychosocial factors providing an explanatory pathway that connects material circumstances to health outcomes (Smith and Anderson, 2018). Indeed, a growing body of research investigates how financial stress gets 'under the skin' (Sturgeon *et al*., 2016: 134) of individuals to impact their mental health, which is considered a psychosocial explanation despite its direct relationship to material concerns. In addition, psychosocial factors have been articulated to identify qualitatively specific aspects of the experience of unemployment that might explain impacts on health; Sage (2018), for example, categorises these in terms of loss of agency and loss of social status, the latter of which includes stigma.

Benefit sanctions – and the work-related behavioural conditions that they enforce – are a key means through which the state shapes claimants' experience of unemployment, and the mental health impacts can therefore be expected to operate along the material and psychosocial dimensions outlined above. As has been previously highlighted, the imposition of a benefit sanction holds serious material implications for individuals both in terms of JSA withdrawal as well as additional knock-on effects on managing debt. The psychosocial route, in contrast, will operate in response to both the threat and imposition of benefit sanctions, through financial stress as well as through impacts on claimants' perceived agency and social status. Indeed, empirical research suggests that many sanctioned JSA claimants disagreed with the reasons behind their sanction, viewing the circumstances as unfair and reporting feelings of powerlessness and stigmatisation (Stewart and Wright, 2018). Whilst the financial impact of a sanction can be partially moderated through receipt of a hardship payment, fewer than half of claimants receive these. Since 2017, JSA claimants already suffering from mental health problems have been deemed to represent a 'vulnerable' group, and are therefore eligible for an immediate hardship payment (HM Government, 2017). Whilst this change is welcome, the findings presented here suggest that much greater consideration needs to be given to the mental health of all claimants subjected to sanctions.

In policy terms, the results of this study highlight an important dimension that needs to be considered when assessing sanctions policy. In its review of the benefit sanctions regime, the National Audit Office (NAO, 2016a: 10) argues that the DWP provided 'little evidence for its design choices' when implementing the changes specified in the Welfare Reform Act 2012; the findings presented here suggest that the reforms had serious implications for the mental health of claimants and those around them, leading to additional use of antidepressant medication. The evidence therefore motivates the need to reconsider the frequency and severity with which sanctions are imposed on unemployed claimants, a conclusion that arguably extends to other out-of-work claimant groups. The results also highlight that sanctions are likely to require additional public spending to support those affected by them, which needs to be taken into account when assessing the net cost of their application. These issues are of increasing relevance in the post-Coalition period, in which the Conservative government (2015-) continues the rollout of Universal Credit (UC).

The sanctions regime introduced by the Welfare Reform Act 2012 forms the basis of enforcement within UC, which – in addition to the introduction of repayable hardship payments – has operated with a higher rate of sanctions since its inception compared with JSA (NAO, 2016a).

More widely, this article informs ongoing debates regarding the ethical justification of conditionality within the contemporary social security system. The growth in work-related behavioural conditions for a variety of out-of-work benefits is subject to contestation by a range of normative perspectives, including – though not limited to – rights-based, social justice, contractualist and paternalist frameworks (Watts and Fitzpatrick, 2018). Whilst the differences between these perspectives are not resolvable through empirical investigation alone, an overall assessment of work-related behavioural conditionality must nevertheless take into account evidence on the impacts of conditional approaches. The findings presented here indicate that JSA sanctions are associated with adverse mental health impacts, which, whilst requiring further investigation, support the results from the literature on sanctions, conditionality and mental health for various claimant groups (Davis, 2019; Katikireddi *et al*., 2018; Stewart and Wright, 2018). As previously highlighted, the evidence in terms of labour market outcomes does not provide clear support for the use of sanctions, whilst the wider literature finds consistently negative impacts in terms of outcomes such as financial hardship and increased food bank usage. When such research is considered collectively, therefore, it would appear difficult for any normative viewpoint to support sanctions policy in its current form.

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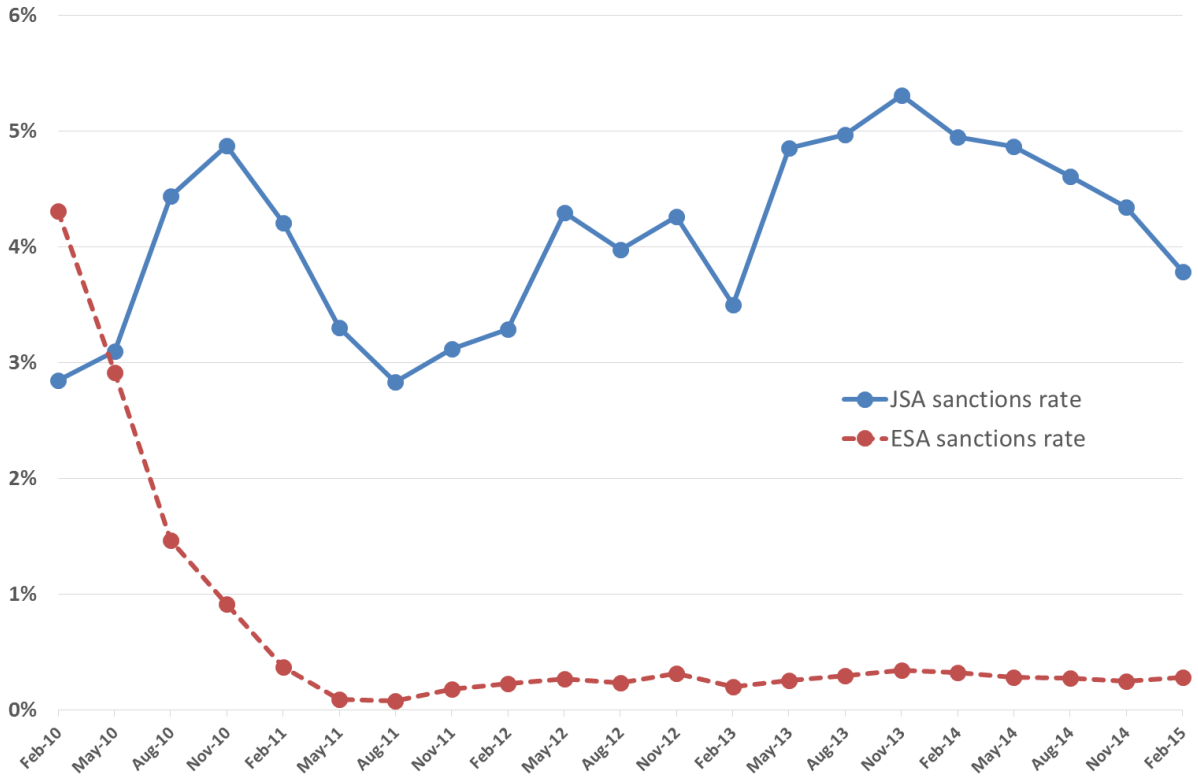
Appendix

Comparison of JSA and ESA sanction rates

Figure A1 compares the JSA and ESA sanctions rate during the period of analysis. Due to recent changes in how the DWP publish claimant statistics, the rates themselves are only presented for four particular months during each year (February, May, August and November). JSA and ESA sanction rates are calculated using original adverse sanctions relating to claimants in England only, and measure sanctions as a proportion of JSA claimants and ESA WRAG claimants respectively. The different variations in rates of JSA and ESA sanctions implies that the analysis is not seriously affected by its omission of ESA sanctions at the local authority-level in the fixed effects regression models.

Figure A1: JSA and ESA sanctions rate (per cent of claimants), 2010-2014



Source: author's calculations using Stat-Xplore data

Summary statistics

Table A1: descriptive statistics for 324 local authorities, across 18 quarters (Q3 2010 – Q4 2014)

	N	Mean	St.d Dev.	Min.	Max.	Source
Dependent variable:						
SSRI prescribing	5,754	12,946	3,411	5,114	28,830	NHS Digital
Sanctions variable:						
Original adverse	5,754	223	139	9	969	Stat-Xplore
Control variables:						
Claimants	5,754	1,851	964	287	6,033	Nomis
Unemployment	5,459	3,514	1,393	603	10,044	Nomis
Economic Inactivity	5,754	13,809	3,133	5,618	25,575	Nomis
Employment	5,754	45,363	3,587	28,553	59,802	Nomis
Work Capability Assessments	5,754	248	129	26	1,173	Stat-Xplore
GVA	5,754	22,886	14,435	11,876	235,244	Nomis
GDHI	5,754	18,105	4,374	10,728	59,879	Nomis
Age						Nomis
0-15 year olds	5,754	18,586	1,837	13,712	26,967	
16-29 year olds	5,754	17,358	3,846	11,644	32,959	
30-49 year olds	5,754	27,132	2,817	18,670	37,897	
50-64 year olds	5,754	18,741	2,433	9,145	24,038	
65 and above	5,754	18,182	4,385	6,018	31,854	
Female	5,754	50,829	697	45,813	52,562	Nomis
White UK born	5,754	82,636	15,482	13,921	99,042	Nomis
Antibiotics prescribing	5,754	17,347	3,117	8,788	38,915	NHS Digital
Index of Multiple Deprivation						DCLG
Quintile 1	1,166					
Quintile 2	1,157					
Quintile 3	1,140					
Quintile 4	1,165					
Quintile 5	1,126					
Urban-Rural Classification						Defra
Predominantly rural	1,620					
Urban with significant rural	959					
Predominantly urban	3,175					
Falsification variable:						
Cardiovascular Prescribing	5,754	144,487	36,595	58,061	288,986	NHS Digital

Note: suppression of values for the APS unemployment estimates leads to the fall in the sample size.

Initial modelling process

Table A2: relationship between sanctions and SSRI prescribing

	Model A1: Fixed effects	Model A2: Fixed effects	Model A3: Random effects
Sanctions	0.465*	0.371***	0.478**
	(0.206)	(0.079)	(0.180)
Unemployment	-0.012	-0.013	-0.013
	(0.015)	(0.012)	(0.013)
Economic Inactivity	0.009***	0.005*	0.009
	(0.002)	(0.002)	(0.008)
WCAs	0.440	0.199	0.528***
	(0.595)	(0.412)	(0.162)
GVA	-0.054***	-0.021*	-0.038**
	(0.013)	(0.008)	(0.013)
Age			
16–29	-0.001	-0.168***	0.104
	(0.067)	(0.035)	(0.100)
30–49	-0.261*	-0.589***	-0.147
	(0.090)	(0.075)	(0.137)
50–64	-0.208**	-0.519***	-0.020
	(0.071)	(0.072)	(0.142)
65 and over	0.144**	0.011	0.229*
	(0.047)	(0.034)	(0.095)
Female	0.145	0.558***	0.083
	(0.108)	(0.069)	(0.161)
White UK born	0.002	-0.0001	0.015*
	(0.003)	(0.002)	(0.006)
Antibiotic Prescribing	0.111***	0.086***	0.133***
	(0.019)	(0.015)	(0.021)
Index of Multiple Deprivation			
Quintile 2			370.56
			(368.396)
Quintile 3			1,268.77***
			(371.098)
Quintile 4			2,215.73***
			(398.461)
Quintile 5			3,052.26***
			(469.230)
Urban-Rural Classification			
Urban with significant rural			-419.259
			(405.112)
Predominantly urban			-1224.087***
			(362.712)
Index of Multiple Deprivation			
Quintile 2 × Quarter		37.508***	
		(2.112)	

Quintile 3 × Quarter		60.046*** (3.042)	
Quintile 4 × Quarter		75.667*** (4.671)	
Quintile 5 × Quarter		114.015*** (7.715)	
Urban-Rural Classification			
Urban with significant rural × Quarter		-22.709*** (2.189)	
Predominantly urban × Quarter		-30.276*** (3.388)	
R^2 (within)	0.866	0.889	0.865
LA Quarters	5,459	5,459	5,459

Note: Robust standard errors in brackets. Model A1 and A2 include local authority and time fixed effects; Model A3 includes time fixed effects. Constant not shown. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

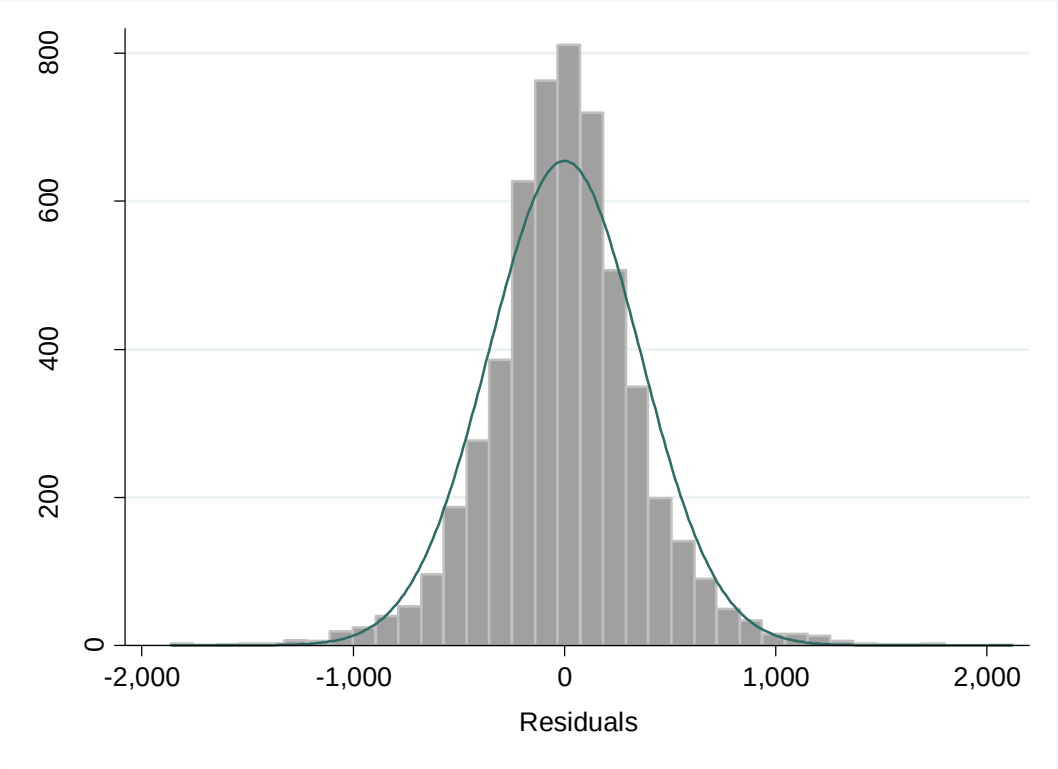
Diagnostic tests

Various diagnostic checks are carried out to test that the fixed effects model assumptions are satisfied (Greene, 2008). The diagnostic checks presented here are for regression Model 1 in Table 1.

Normality of the residuals

Figure A2 depicts a histogram of the regression residuals to check for serious deviations from the assumption of normality. Clearly, the residuals do not deviate sufficiently from the ideal of normality to be of concern to the results of the analysis. Three formal tests of normality, a Skewness/Kurtosis test ($p < 0.001$), a Shapiro-Wilk test ($p < 0.001$) and a Shapiro-Francia test ($p < 0.001$) reject the null of normality. However, as Ghasemi and Zahedias (2012) outline, such tests are sensitive to even very small deviations from normality at large sample sizes. The rejection of normality by such tests is therefore not of concern to the analysis, given the distribution that is actually observed.

Figure A2: distribution of regression residuals compared against normal distribution curve



Cross-sectional independence, homoscedasticity, no serial correlation and stationarity

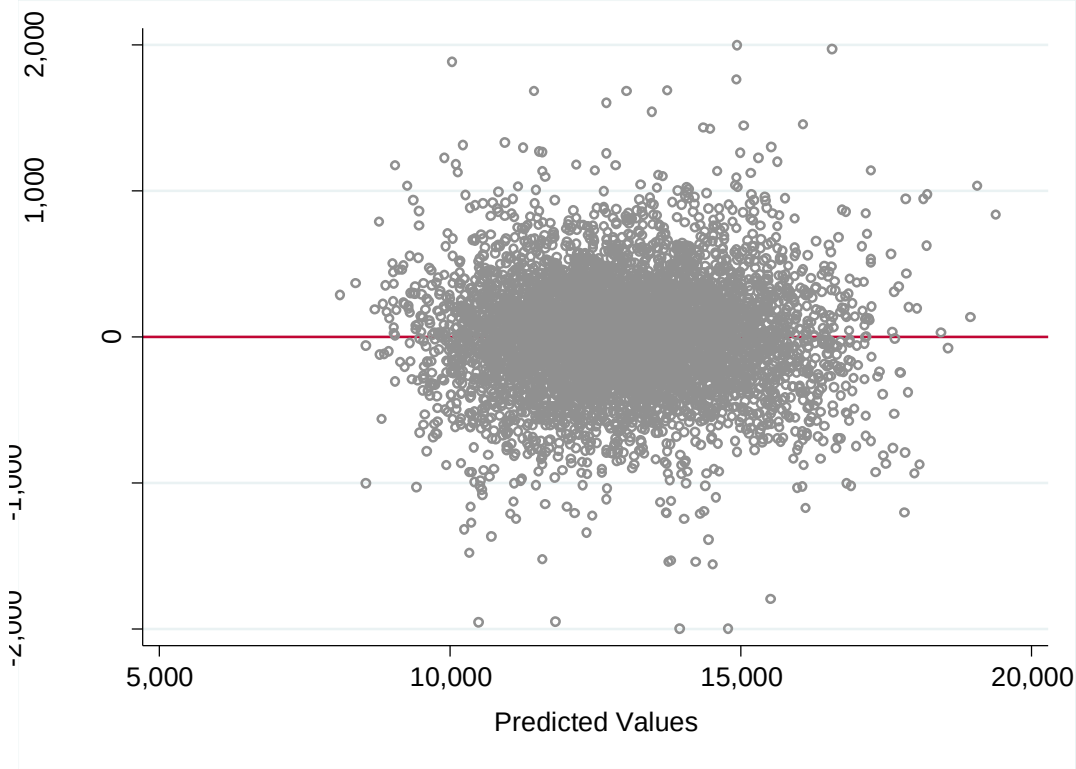
The tests carried out in this sub-section indicate that the fixed effects models suffer from cross-sectional dependence, heteroscedasticity and autocorrelation, though there are important caveats on the tests themselves that will be explained in more detail in the following discussion. Consequently, the fixed effects regression models estimated throughout the analysis use Driscoll-Kraay standard errors (Driscoll and Kraay, 1998), which are robust to cross-sectional dependence, heteroscedasticity and correlation through time within local authorities. These are implemented using the Stata command 'xtscc', developed by Hoechle (2007).

First, a check for cross-sectional dependence is carried out. The standard test of this issue is the Breusch-Pagan Lagrange multiplier (LM) test, as developed by Breusch and Pagan (1980). This test isn't valid in panels with a large number of observations (*N*) but a small number of observations per cross-sectional unit (*T*), which is the case here ($N = 324$, $T = 18$). Instead, Pesaran's (2004) cross-sectional dependence (CD) test is carried out, using the 'xtcsd' Stata command developed by De

Hoyos and Sarafidis (2006), which is compatible with unbalanced datasets. The Pesaran (2004) CD test rejects the null hypothesis of no cross-sectional dependence ($p < 0.05$).

Next, in order to check for heteroscedasticity, a modified Wald test (Greene, 2008) is carried out that tests for group-wise heteroscedasticity in the residuals of fixed effect regression models, using the Stata command 'xttest3' developed by Baum (2001). The modified Wald test rejects the null of homoscedasticity ($p < 0.001$), which indicates that the residuals display heteroscedasticity. This test, however, has a very low power in the context of fixed effects with 'large *N*, small *T*' (Baum, 2001: 102) panels, as is the case here. The result of the modified Wald test should, therefore, be treated with caution. Indeed, a scatter plot of the regression residuals against predicted values, furthermore, suggests that the error term has an approximately constant variance, since there is no sign of a fanning out effect over different predicted values. This is depicted in Figure A3.

Figure A3: scatter plot of the regression residuals against predicted values



Next, in order to check for serial correlation, a Wooldridge (2002) test is carried out using the Stata command 'xtserial' developed by Drukker (2003). The Wooldridge (2002) test rejects the null of no autocorrelation ($p < 0.001$), though – like the modified Wald test – is very sensitive in the context of fixed effects with a large *N* and small *T* panel (Drukker, 2003).

Finally, in order to test for non-stationarity, Pesaran's (2007) panel unit root test is carried out which – unlike many unit root tests – does not require the assumption of cross-sectional independence to be met. This is carried out using the Stata command 'pescadf' developed by Lewandowski (2007), which rejects the null of non-stationarity with or without a time trend included ($p < 0.001$).

Unusual and Influential Data

Next, checks for the influence of outliers and extreme observations are carried out. Firstly, observations with residuals that are two standard deviations from the mean in Model 1 are removed and the regression models re-estimated (Cousineau and Chartier, 2010). The results are shown in Table A3, Model A4. To check for the role of extreme observations, furthermore, the results from

Model 1 were re-run with the top and bottom one percentiles removed for sanctions (Model A5). Finally, the results were re-run with the seaside areas discussed in the results section removed (Model A6). The results across the separate models in Table A3 remain similar to the estimated sanctions coefficient in Model 1.

Table A3: relationship between sanctions and SSRI prescribing

	Model A4	Model A5	Model A6
Sanctions	0.344 *** (0.086)	0.327** (0.109)	0.409*** (0.083)
R^2 (within)	0.889	0.889	0.889
LA Quarters	5,265	5,362	5,369

Note: Robust standard errors in brackets. Models include local authority and time fixed effects. Constant and additional control variables not shown. * p < 0.05, ** p < 0.01, *** p < 0.001

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Falsification test

Table A4: relationship between sanctions and cardiovascular prescribing

	Model A7	Model A8
Sanctions	1.503	1.478
	(1.288)	(1.685)
Sanctions x Reform		0.035
		(1.575)
Unemployment	-0.037	-0.037
	(0.086)	(0.086)
Economic Inactivity	-0.054	-0.054
	(0.057)	(0.057)
WCAs	-2.463**	-2.465**
	(0.939)	(0.937)
GVA	0.099	0.099
	(0.083)	(0.082)
Age		
16–29	0.282	0.282
	(1.154)	(1.152)
30–49	0.843	0.842
	(1.710)	(1.708)
50–64	1.639	1.639
	(1.587)	(1.581)
65 and over	2.555*	2.555*
	(1.039)	(1.041)
Female	2.811*	2.810*
	(1.134)	(1.130)
White UK born	-0.008	-0.008
	(0.041)	(0.041)
Antibiotic Prescribing	0.677***	0.676***
	(0.163)	(0.164)
Index of Multiple Deprivation		
Quintile 2 × Quarter	206.595*	206.465*
	(84.586)	(84.596)
Quintile 3 × Quarter	100.641	100.372
	(68.404)	(70.299)
Quintile 4 × Quarter	263.887***	263.433***
	(73.243)	(75.009)
Quintile 5 × Quarter	283.358**	282.593**
	(93.980)	(105.256)
Urban-Rural Classification		
Urban with significant rural × Quarter	-175.721*	175.766*
	(77.698)	(77.863)
Predominantly urban × Quarter	-192.588*	192.665*
	(77.830)	(78.036)
R^2 (within)	0.631	0.631
LA Quarters	5,459	5,459

Note: Robust standard errors in brackets. Models include local authority and time fixed effects. Constant not shown. * p < 0.05, ** p < 0.01, *** p < 0.001

Tables

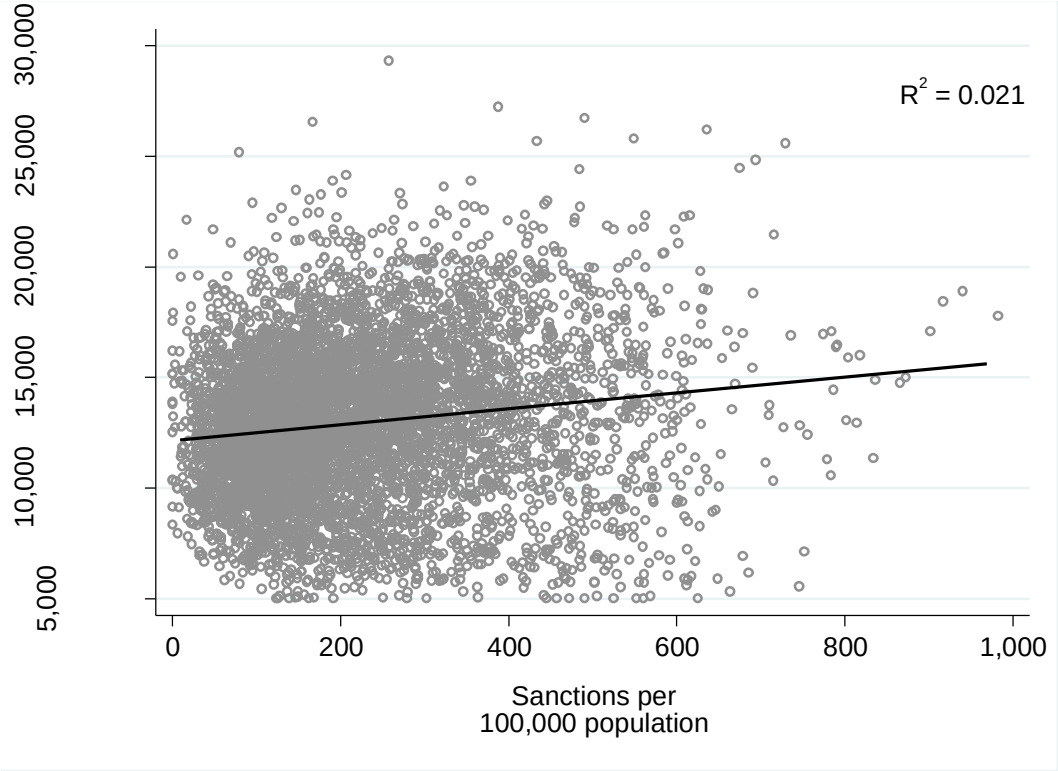
Table 1: relationship between sanctions and SSRI prescribing

	Model 1	Model 2
Sanctions	0.371*** (0.079)	0.174 (0.179)
Sanctions x Reform		0.282 (0.252)
Unemployment	-0.013 (0.012)	-0.013 (0.012)
Economic Inactivity	0.005* (0.002)	0.005* (0.002)
WCAs	0.199 (0.412)	0.186 (0.413)
GVA	-0.021* (0.008)	-0.021* (0.008)
Age		
16–29	-0.168*** (0.035)	-0.170*** (0.035)
30–49	-0.589*** (0.075)	-0.590*** (0.076)
50–64	-0.519*** (0.072)	-0.525*** (0.073)
65 and over	0.011 (0.034)	0.014 (0.035)
Female	0.558*** (0.069)	0.548*** (0.063)
White UK born	-0.0001 (0.002)	-0.0001 (0.002)
Antibiotic Prescribing	0.086*** (0.015)	0.084*** (0.014)
Index of Multiple Deprivation		
Quintile 2 × Quarter	37.508*** (2.112)	36.465*** (2.271)
Quintile 3 × Quarter	60.046*** (3.042)	57.878*** (3.400)
Quintile 4 × Quarter	75.669*** (4.671)	72.009*** (5.015)
Quintile 5 × Quarter	114.015*** (7.715)	107.853*** (8.275)
Urban-Rural Classification		
Urban with significant rural × Quarter	-22.709*** (2.189)	-23.077*** (2.128)
Predominantly urban × Quarter	-30.276*** (3.388)	-30.891*** (3.212)
R^2 (within)	0.889	0.889
LA Quarters	5,459	5,459

Note: Robust standard errors in brackets. Models include local authority and time fixed effects. Constant not shown. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

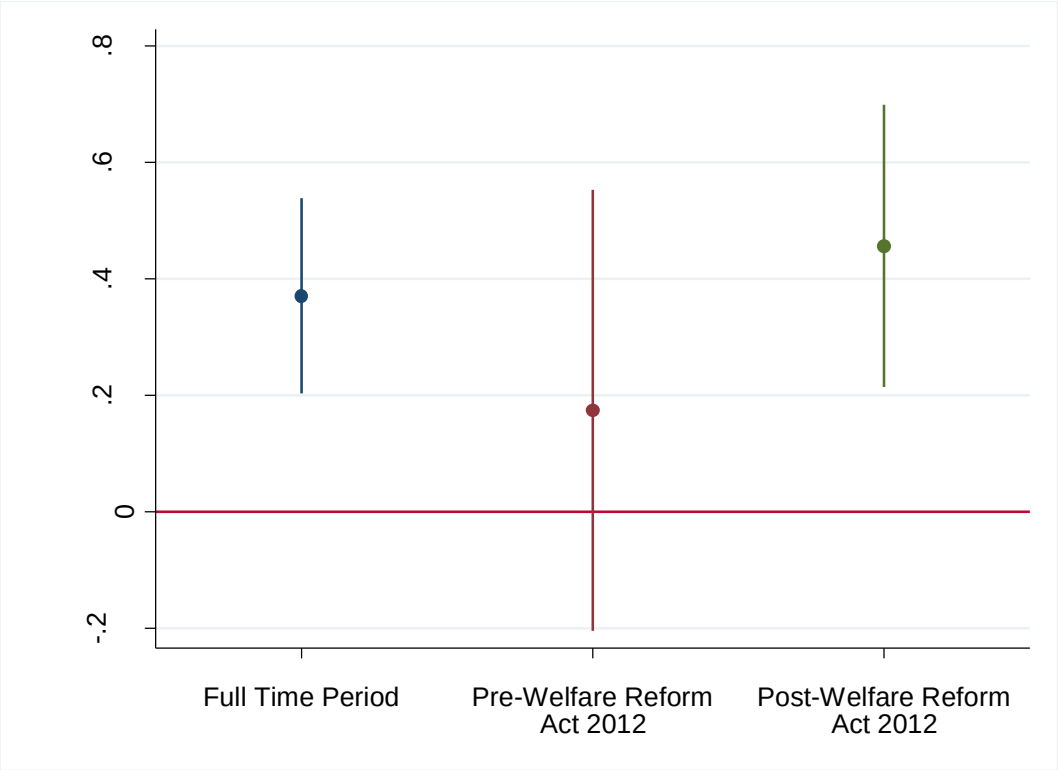
Figures

Figure 1: relationship between sanctions and SSRI prescribing



Note: quarterly rates for 324 local authority districts, Q3 2010 – Q4 2014

Figure 2: relationship between sanctions and SSRI prescribing



Note: Point estimates for sanctions are derived from Table 1.
Vertical bars represent 95% confidence intervals.