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Welfare Cuts, Local Spillovers and Financial Fragility

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Abstract

This paper shows that shocks to social benefits have local multiplier effects that are borne primarily by economically fragile households. Using a large welfare reform in the UK, we document that recipients not only lose benefits income, but also experience a lower relative likelihood of employment following the cuts. Employment effects are concentrated in areas more severely affected by the cuts and in small firms in the non-tradable sector, suggesting that local demand spillovers are responsible for amplifying the initial shock. Affected households respond by increasing debt usage, are less likely to become homeowners after the reform and are more likely to experience financial distress.

JEL Classification: G5, G51, D14

Keywords: austerity, employment, Spillovers, household finance

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Welfare Cuts, Local Spillovers and Financial Fragility*

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March 14, 2022

Abstract

This paper shows that shocks to social benefits have local multiplier effects that are borne primarily by economically fragile households. Using a large welfare reform in the UK, we document that recipients not only lose benefits income, but also experience a lower relative likelihood of employment following the cuts. Employment effects are concentrated in areas more severely affected by the cuts and in small firms in the non-tradable sector, suggesting that local demand spillovers are responsible for amplifying the initial shock. Affected households respond by increasing debt usage, are less likely to become homeowners after the reform and are more likely to experience financial distress.

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1 Introduction

Fiscal expansions play an important role in countries' response to contractionary shocks. As economies recover, political and financial market pressures commonly intensify to phase out stimulus spending and reduce government spending. With respect to the Covid-19 pandemic, the International Monetary Fund warned as early as April 2021 for the need to tackle "long-standing weaknesses in public finances once the recovery is firmly in place" ([International Monetary Fund \(2021\)](#)).

The academic literature has long debated the effects and merits of fiscal consolidation, most recently during the European sovereign debt crisis ([Stiglitz \(2011\)](#), [Krugman \(2015\)](#), [Barro \(2012\)](#), [Reis \(2013\)](#), [Blanchard et al. \(2013\)](#)). This strand of literature has primarily focused on aggregate (country-level) evidence to estimate the effects of spending cuts ([Blanchard and Leigh \(2013\)](#), [Alesina et al. \(2019\)](#) and [Alesina et al. \(2015\)](#)). Less is known about the micro-level mechanisms driving the magnitude of the fiscal multiplier, as well as the heterogeneous effects of fiscal consolidation across the income distribution. In particular, at the individual level we still have a limited understanding of the net effect of the labor supply response to government austerity relative to the possible negative impact on firms' labor demand due to a drop in economic activity.

This paper examines the direct and indirect effects of fiscal consolidation in the form of welfare cuts on individual households at the micro-level. We ask three questions: First, are households able to increase labor income (through increased labor supply) to offset lost benefits income at a time when the rest of the economy is performing well? Second, what happens to local employment following large-scale welfare cuts, and how are employment changes distributed across households differentially affected by the cuts? And, finally, how do household balance sheets adjust after losing welfare benefits? Understanding households' and local labor market adjustments to benefits cuts speaks to the current proposals for phasing out the pandemic-related fiscal stimulus (see, e.g., [International Monetary Fund \(2021\)](#)).

We use the introduction of the Welfare Reform Act of 2012 by the British government and the subsequent reduction in welfare spending as a laboratory to study these questions. These reforms were sizable (at roughly GBP 19bn per year) and were followed by a period when the overall UK economy experienced sustained growth. Existing evidence suggests that the cuts had very hetero-

geneous impacts across regions (Beatty and Fothergill (2013)) and were associated with increased support for Brexit and the UK Independence Party (UKIP) (Fetzer (2019)). The data come from a household-level panel of UK households and include information on income, welfare transfers, employment status, assets and liabilities.¹ We focus on the reduction in council tax benefits (a reduction in government support for paying local taxes) as the household-level shock, as this represents one of the most affected welfare components in the reform and is the best indicator that households lost this and other benefits under the reform.

We start by establishing that the welfare reform results in a significant reduction in benefits income for the affected households (the equivalent of a first-stage in our setting). Given that households in the treated group are observably (and potentially unobservably) different from the control group, we adopt a few different approaches to assess the effect of the policy. Specifically, the analysis compares outcomes for individuals receiving council tax benefits to several control groups, including one that encompasses all (unaffected) working age individuals, a subsample of unaffected low-income individuals, as well as a propensity score matched subsample. In addition, we control for a full set of interactions of pre-treatment characteristics with an indicator for the timing of the reform. This means that we allow the coefficient on each observable characteristic to be different before and after the reform. We find that, after the reform, affected households lose on average between GBP2,200 and GBP2,300 per year in welfare benefits relative to the control group. This implies a loss of about 13% of income relative to the mean for the treated group.

Households who suffer cuts in welfare benefits also suffer a reduction in employment income of approximately 1.5 times the magnitude of the lost benefits. When we divide the employment income effect into the extensive margin (the probability of being employed) and the intensive margin (change in income conditional on being employed), we find that this effect is entirely driven by the relative likelihood of having a job. In fact, treated individuals have an 8-9 percentage point lower probability of being employed after the austerity reform relative to control individuals, a substantial 35% decline relative to the mean probability of employment before the shock. We find no statistically (or economically) significant effects on employment income for individuals who are employed throughout the sample period.

¹Our primary dataset is the Household Wealth and Assets Survey (WAS) run by the Office of National Statistics, <https://www.ons.gov.uk/peoplepopulationandcommunity/personalandhouseholdfinances/debt/methodologies/wealthandassetssurveyqmi>.

The fact that individuals subject to welfare benefit cuts also have a lower probability of being employed after the shock suggests that the benefits income loss may have local spillover effects. Specifically, weaker local demand may lead to reductions in employment at firms in the non-tradable sector (e.g., retail, food). If there is significant overlap between firms that are most affected by this reduction in demand and those where benefits recipients work, we would observe this simultaneous loss in benefits income and employment. This produces a feedback loop whereby lower demand causes lower employment, which can, in turn, affect demand further.

We test this local spillover hypothesis directly using two different data sets and empirical strategies. First, we exploit the UK “Understanding Society” Household Longitudinal Study (UKHLS) to examine whether individuals in geographic regions that were more exposed to welfare cuts have a lower likelihood of being employed. If the employment effects are due to reduced local demand, they should be stronger in areas experiencing deeper welfare cuts. We find that a one-standard deviation higher welfare cut intensity leads to a 1.1 percentage points lower probability of employment after the shock on average across all income groups, which implies that moving from the first to the third tercile of cut intensity reduces the probability of employment of an average individual by 2.1 percentage points.² This effect is stronger for individuals receiving benefits before the reform – following the same increase in cut intensity at the district level, individuals who received council tax benefits before shock experience a 1.7 percentage points lower probability of employment than other (control) individuals in the same district, which represents an 11% fall relative to their mean probability of employment. This result is stronger in districts with a higher pre-reform unemployment rate, consistent with negative local demand spillovers and weak labor demand mattering more when there is more “slack”, so that affected individuals’ are less able to substitute the loss in benefits income with labor income.

As a second and complementary approach, we use firm-level observations from Bureau van Dijk’s FAME dataset to measure revenue and employment losses for firms of different sizes and in different sectors as a function of the intensity of local benefits cuts. If local drops in demand are responsible for employment loss, this should be especially true for firms in non-tradable sectors (as in [Mian and Sufi \(2014\)](#)), and for small firms that are present in fewer geographic markets (and,

²The “cut” measure at the district level is the expected loss in total benefit income in pounds per working age individual, as in [Beatty and Fothergill \(2013\)](#) and [Fetzer \(2019\)](#).

thus, more exposed if local demand drops). This is precisely what we find when we compare areas more and less exposed to affected households in the district. Firms with fewer than 100 employees in more affected districts lose revenues and employment relative to their counterparts in less affected districts, and the same is true for firms in sectors more dependent on local demand. We do not find these patterns for either revenue or employment for large firms (more spread out geographically) or for those in the manufacturing sector (less dependent on local demand). In sum, the results point to austerity cuts producing a feedback loop from lost benefits income to lower local demand and, consequently, lower employment for households most affected by the cuts.³

In the final part of the paper we ask how the loss in income (and employment) affects household balance sheets by measuring how affected households adjust their debt burden and borrowing across different sources. Even though only a small percentage of households are homeowners in the treated group (homeownership rate among the treated sample is just 17%), we first look at effects on housing debt because it is a very large component of all debt when it is present. The welfare reform and its subsequent employment effects reduce the likelihood of owning a home. The homeownership rate for the treated group drops by 3.7 to 4.0 percentage points relative to the control group, or about 23% of the mean. An even smaller percentage of the treated group own their home outright (i.e., without a mortgage), but in the subsample of outright owners there is a 7 to 9 percentage points higher (relative) likelihood of obtaining a mortgage after the reform. This means that austerity further increases the homeownership gap of welfare recipients relative to the rest of the population, and leads the small group of homeowners with spare debt capacity to use some of that debt capacity to smooth the income shock.

The reform also leads to an increase in the usage of unsecured debt, particularly the probability of having a credit card and of having outstanding debt balances on the cards. Households receiving council tax benefits before the reform are about 4 percentage points more likely to have a credit card after the reform relative to the control group, and 3.7 to 4.8 percentage points more likely to carry a credit card balance. This is a meaningful change (an approximately 28% increase) relative to treated individuals' mean probability of having credit card debt before the cuts (17%),

³We also find some evidence for the relevance of a labor supply channel: although, on net, affected individuals are less likely to be employed after the reform, they are weakly more likely to look for work. That is, affected individuals *try* to increase labor income to replace lost benefits, but on average do not manage to do so.

and again suggests that households turned to debt as a form of smoothing the impact of the shock. We do not find a similar increase in the usage of personal loans.

The combination of reduced income and higher debt usage leads to increased financial distress. First, the reduction in support for council taxes increases the likelihood that households cannot meet their local tax obligations (consistent with the finding in [Ashton \(2014\)](#) in London). We find a 3 percentage points higher likelihood of missing local tax payments (relative to a mean of 5%). For individuals that are unemployed after the reform, the reduction in support further affects their ability to pay rent. They are 2 to 3 percentage points more likely to be behind on rent payments (relative to a mean of 6.5%). In addition, recipients of council tax benefits before the reform are ten percentage points more likely than the control group to report feeling that repaying debts represents “a financial burden” after the reform, a 24% increase relative to the mean before the reform. Taken together, these findings help validate the significance of the shock for affected households and their inability to fully undo the combined benefit and employment income shocks.

Our work is connected to several strands of literature. First, it relates to a long macro and micro literature on austerity. Closely related to our work and using the same reform, [Fetzer \(2019\)](#) shows that austerity in the UK was associated with extremist political views and support for Brexit (see also [Galofré-Vilà et al. \(2021\)](#) for evidence on 1930s Germany and the rise of the Nazi party, and [Ponticelli and Voth \(2020\)](#) for long-sample evidence on the connection between austerity and social unrest). At the macro-level, several studies assess the effect of fiscal contractions on growth, as well as how this may depend on the implementation of revenue vs. spending adjustments. [Lama and Medina \(2019\)](#) study the relevance of wage rigidities and total factor productivity for understanding the effect of fiscal consolidation on employment, while [Brinca et al. \(2021\)](#) show a correlation between the recessionary impact of fiscal consolidation and income inequality. In the finance literature, [Ağca and Igan \(2019\)](#) find that austerity is associated with a higher cost of credit, in particular for small and financially constrained firms. We contribute to this literature by documenting the effect of an austerity plan on household finances and depicting this effect on local demand and small firm employment at the micro-level. Our study that focuses on financially fragile households also attests of the heterogeneous effect that fiscal consolidation plans may have.

Recent studies have examined household debt and spending response after stimulus payments. The stimulus payments associated with the 2020 CARES Act in the US were used primarily

as additional savings or to pay down debt (Coibion et al. (2020)). Feldman and Heffetz (2021) find a similar effect using stimulus payments disbursed in Israel at around the same time. Unlike one-time stimulus payments, the cuts in welfare spending that we study represent a continued (possibly permanent) income reduction for affected households, rather than a transitory shock. In addition, we contribute to this literature by considering income drops, and whether there is an asymmetry in response relative to existing results on stimulus payments.

Our findings also speak to the literature on social assistance and labor outcomes. Across various datasets and empirical techniques, researchers have studied consumption and labor supply effects of social assistance expansions, in the form of disability insurance (Von Wachter et al. (2011); Maestas et al. (2013)), income tax rebates (Johnson et al. (2006); Sahm et al. (2010) and Sahm et al. (2012)) and housing assistance (Jacob and Ludwig (2012)). These studies generally report a reduction in labor supply after an increase in assistance, although the estimates are generally small in magnitude (Krueger and Meyer (2002) provides a survey of earlier literature). Cesarini et al. (2017) and Picchio et al. (2018) show small lifetime labor supply responses to wealth shocks due to lottery wins, while Zator (2021) shows substantial labor supply responses to changes (particularly increases) in mortgage debt repayments. By studying cuts to benefit income, our results provide novel evidence on the net effect of labor demand (originating in small firms dependent in non-tradable sectors) and labor supply responses due to large-scale welfare reforms on household finances and income.

These local demand effects relate to recent studies of the local fiscal multiplier, which typically assess aggregate employment or income responses of regions differentially exposed to a public spending shock (see Chodorow-Reich (2019) for a survey). For example, Nakamura and Steinsson (2014) document that US states more exposed to military spending experience an increase in employment and gross state product after nationwide military shocks, and Chodorow-Reich et al. (2012) and Wilson (2012) find that US states more exposed to the 2009 American Recovery and Reinvestment Act kept higher level of employment than states that received less funds.⁴ We contribute to this literature by analyzing the effect of a large negative shock to welfare spending (as opposed to government consumption or investment) and tracing its negative spillovers onto the

⁴See also Serrato and Wingender (2016), Adelino et al. (2017), or Goldman (2020) for other studies of local public spending at finer geographic-levels.

revenues and employment of local firms.

Finally, our results on the connection between local spillovers and household debt relate to a recent literature in finance looking at local spillovers of credit and house price shocks. [Guren et al. \(2021\)](#) show the long-term effect of housing wealth shock on local consumption while [Huber \(2018\)](#) shows how lending cuts propagate locally, including for firms that are not directly connected to affected banks. In the mortgage setting, several papers consider how foreclosures affect local housing markets ([Campbell et al. \(2011\)](#), [Gupta \(2019\)](#), [Gerardi et al. \(2015\)](#)). [Favara and Giannetti \(2017\)](#) show that local lending concentration changes the incentive to foreclose on properties. We find that localized government spending cuts not only affect household credit (secured and unsecured) but also employment.

2 The Welfare Reform of 2012 and the Reduction in Council Tax Benefits

Following the 2010 general elections, the newly-elected UK government embarked in a major reform of the British social security system. This reform (the Welfare Reform Act), passed in 2012 and implemented starting in 2013, profoundly changed the way social benefits are allocated in the UK. [Beatty and Fothergill \(2013\)](#) study in details 10 key elements of the reform that, together, were estimated to generate GBP 19bn in savings to the UK government.⁵ The majority of these 10 measures started to take effect in 2013. In aggregate, real social and welfare spending fell by about 9% in real terms between 2012 and 2015 ([Fetzer \(2019\)](#)).

Our analysis focuses on the reduction in council tax benefits as the source of identification. As we show, reception of council tax benefits before the reform is a good marker of households who relied on welfare benefits and were significantly affected by the overall cuts of the Welfare Reform Act of 2012. Council tax benefits were also the largest means-tested benefit in the UK at the time of the reform, providing close to GBP 5bn in support to just under 5 million recipients ([Ashton \(2014\)](#)).

Even though the amount of support increased with the value of a home, it represented a larger

⁵These measures include Housing benefit local housing allowance, Housing benefit under-occupation, Non-dependant deductions, Household benefit cap, Council tax benefit, Disability living allowance, Incapacity benefits, Child benefit, Tax credit, 1 per cent up-rating

share of income for low-income households. One of the important arguments for reducing council benefits was that they provided a disincentive for recipients to look for paid work (Adam and Browne (2012)). Additionally, a higher share of the burden for council taxes should fall on working age individuals, as the law required pensioners to maintain their level of support under new local programs. The reform of 2012 abolished the national council tax benefit and replaced it with support for local authorities to implement their own council tax benefit schemes. Existing evidence suggests that the new, local programs did not make up for the shortfall in support due to the disappearance of the national benefit (Bushe et al. (2013)). We directly observe from the survey whether a given household receives this benefit before its cut comes into effect in 2013 and our focus on working-age individuals ensures a higher incidence of the benefit cut on this group.

A few welfare-related cuts started in 2011 as part of the withdrawal of stimulus measures implemented by the Labour government during the 2008-09 financial crisis, but these are in magnitude substantially smaller than the cuts starting in 2013. Some of the changes that started in 2011 included early cuts to the budgets of Local Authorities (Fetzer (2019)); the beginning of a reduction in some support measures for lone parents⁶; a reform to the local housing allowance; the freeze of child benefit from 2011 (with the main reduction in child benefits starting in 2013); and the start of the implementation of some non-dependent deductions, incapacity benefits and tax credit changes. Not directly related to welfare programs and thus less relevant to this paper, Westminster department budgets and civil servant salaries were also frozen from 2012.

The timing of our empirical strategy is, in sum, consistent with the majority of the welfare cuts starting to bite from 2013 with the implementation of the Welfare Reform Act. This timing is also consistent with the effect of Fetzer (2019)'s overall austerity shock on political support for UKIP. Fetzer (2019) shows that support for UKIP among individuals most exposed to austerity substantially increased in 2013, consistent with the figures below on household-level impacts of the reform.

⁶<https://www.gov.uk/government/publications/2010-to-2015-government-policy-welfare-reform/2010-to-2015-government-policy-welfare-reform>

2.1 Data and Empirical Strategy

Our main data source is the Wealth and Assets Survey (WAS) administered by the UK Office of National Statistics. It is a biennial longitudinal survey that provides information on respondents' finances, including income, savings, investments, and debt. Our data span the years between 2010 and 2016, and so is not contaminated by the subsequent changes brought on by Brexit. Our analysis focuses on the 14,571 working age individuals in the panel in wave 3, which ran between 2010 and 2012 (that is, wave 3 is the last wave before the reform that we study).

Table 1 Panel A shows descriptive statistics for the individuals in the sample in the last wave before the reform. The average respondent is 43 years old, 77% of respondents are employed, 51% are male and 76% are homeowners. Their average household net annual income is about GBP40,000 (which corresponds to about GBP54,000 of annual gross income). 8% of individuals receive council tax benefits before the reform, and these households form the treatment group in throughout the paper.

Our empirical analysis uses a difference-in-differences approach to compare outcomes for individuals affected and unaffected by the council tax benefits cut ("CTB" below), from the years before the cuts (2010-2012) to those after the cuts (2013-2016). Our baseline specification is:

$$Y_{it} = \beta(CTB_i \times Post_t) + \alpha_i + \eta_{rt} + \zeta_{wt} + \epsilon_{it} \quad (1)$$

where i indexes individuals, t years, and w survey waves. Y_{it} represents the individual outcomes that we study (e.g., benefits income, employment, mortgage debt, credit card usage, among others). CTB is the treatment indicator and it is equal to 1 if the individual receives council tax benefits before the cuts are implemented. α_i are individual fixed effects that control for time-invariant characteristics. η_{rt} and ζ_{wt} are region-by-year and wave-by-year fixed effects that control for aggregate shocks, allowed to vary by region and survey wave. The coefficient of interest is β , the difference-in-differences estimator. Throughout the analysis, we cluster standard errors at the region-by-year level, which results in 77 clusters in total. This assumes independence of observations between region-years and represents a large enough number of clusters to avoid biased estimates of the standard errors in the regressions. Results are robust to clustering by individual respondent for all household-level regressions.

We perform our main analysis on three different control groups. The first control group consists of all individuals in the WAS who do not receive council tax benefits before the shock. Our second control group is restricted to individuals who are part of households earning less than GBP30,000 before the shock. When we use these two control groups, we augment our baseline specification to control for the possibility that household characteristics may be correlated with the reception of council tax benefits before the shock, and that differential shocks correlated with these characteristics might drive the response of the treated group to the reform. Specifically, we control for the interaction of pre-shock characteristics — like gender, employment status, age category, owner/renter status, household net income, educational achievement and debt burden — and the post-reform indicator.

We use the same set of pre-shock characteristics to form our third control group, which is a matched sample of the 3 nearest control individuals to each treated individual based on a propensity score.⁷

Table 2 compares treated and control individuals (all three groups described above), and we draw three main conclusions from this comparison. First, treated individuals are different from the average individual in the overall WAS panel – lower income, less likely to be a homeowner or to have a degree, more likely to be female. This justifies carefully controlling for differential trends interacted with pre-shock characteristics. Second, when we restrict the control group to households earning less than GBP30,000, this reduces the observable differences along all dimensions we consider, but we still see material differences on most of them. Finally, when we compare the means of treated and matched controls, we find that the matching procedure is effective in eliminating observable differences between the two groups.

Our preferred specifications are based on the second and third control groups (households earning less than GBP30,000 and propensity score matched subsample). We present the results based on the first control group (unrestricted sample) in the appendix.

⁷The propensity score is estimated by means of a logit regression of demographic characteristics before the shock, as well as year and region fixed effects. The coefficients from the logit estimation are in Table A.2.

3 Council Tax Cuts and Household Earnings

We start by examining the direct effect of the council tax benefit cuts on affected individuals' income from welfare benefits. To get an overall sense of the effect of the council tax benefits cuts and validate our empirical methodology, Figure 2 displays the evolution of the income derived from welfare benefits for treated and control individuals in households earning less than GBP30,000 per year. For each group (treated and control), we estimate a pooled regression of the outcome on year, wave and pre-reform employment status dummies, and plot the coefficients on the year fixed effects. We observe little difference in the coefficients for treated and control individuals in the years before the council tax benefit cuts, suggesting that the parallel trend assumption on which our difference-in-differences methodology relies is likely met. After the council tax benefit reform is implemented, starting in 2013, we observe a marked decline in treated individuals' annual income from welfare benefits.

Table 3 reports regression results from estimating equation 1 with household annual income from welfare benefits as an outcome. We perform the estimation on the sample of individuals in households earning less than GBP30,000 per year (columns 1 and 2), on two matched samples (columns 3 and 4), and on the full sample (column 5). Across all samples, the coefficient of interest is negative and significant. Its magnitude is also stable across columns. In column 1, where we estimate the effect on individuals in households earning less than GBP30,000, we find that treated individuals lose on average GBP2,312 in annual welfare benefits. Controlling for the interaction between households characteristics before the shock and the post-period indicator (column 2) has little effect on the coefficient (GBP2,265). Using the matched samples, we estimate the effect at between GBP2,171 (with 3 neighbors, in column 3) and GBP2,307 (with 5 neighbors, in column 4). In column 5, the estimation on the full sample with the pre-period controls depicts the same picture (a decline of GBP2,202). This decline of about GBP2,300 in annual welfare benefits is economically meaningful as it represents a 13% decline from the average annual welfare benefits received by treated individuals before the cuts were implemented.

3.1 Employment and Labor Market Outcomes

Figure 3 summarizes our main results regarding the employment effect of the welfare reform on affected individuals. We run regressions of each outcome on year and wave dummies and compare the evolution of the probability of being employed for treated individuals and those in the control group earning less than GBP30,000.⁸ We find that the probability of being employed diverges exactly in 2013. By 2015 the gap in probability of employment is substantial at around 10 percentage points.

Table 4 includes the regression results for employment and the associated income. The first two columns show that employment income drops by over GBP3,500 in the post-reform period, with a somewhat larger magnitude for the comparison with the matched sample. It is worth noting that the estimated effect on employment income is close to 1.5 times the direct effect from benefit income discussed in the previous subsection. This suggests that local spillover effects are large and important in evaluating the effects of welfare reforms (Bartik (2002) uses simulations to make a similar point).

The drop in employment income is entirely driven by the extensive margin, i.e. by a lower probability of being employed, and not by a reduction in income conditional on being employed. Columns 3 and 4 show that treated individuals are 8-9 percentage points less likely to be employed after the reform, but there is no difference in income for employed individuals (columns 5 and 6). The economic magnitude on the likelihood of employment is very sizeable given the average probability of employment of just 25% for treated individuals.

3.2 Local Spillovers

The fact that we observe such large effects on the probability of employment in the previous subsection raises the question of why these particular households become vulnerable after the austerity shock. We investigate the extent to which a local reduction in income may, in turn, cause a drop in demand that spills over onto employment. In order to investigate local spillovers we turn to two additional data sets that allow us to perform more granular individual-level and also

⁸These regressions are similar to those underlying Figure 2, with the difference that we omit the pre-reform employment dummies to be able to assess pre-trends in employment status. Including or omitting these dummies does not materially affect the figure.

firm-level analyses.

3.2.1 Individual-level evidence of local spillovers

All our previous results come from the Wealth and Assets Survey (WAS) which includes only 12 statistical regions, a number that makes it difficult to convincingly exploit heterogeneity in the extent of the cuts. We now first consider the “Understanding Society” UK Household Longitudinal Study (UKHLS) that contains more granular geographical units, namely local authority districts. These geographic units have populations of between 25,000 and 200,000 and coincide with the level at which most austerity cuts were administered (Fetzer (2019)).

We use the district-level per capita expected austerity cuts as in Fetzer (2019) as measure of the intensity of the shock at a local level and ask whether: 1) individuals are generally less likely to be employed after the austerity shock in areas that were most exposed to the shock, and 2) whether this effect is particularly pronounced for recipients of welfare benefits.

Table 5 starts by replicating the results in Table 4 that council tax benefits (CTB) recipients are less likely to be employed after the welfare cuts obtained using the WAS survey but this time in the UKHLS. Columns 1 and 2 show a 7.0 and 6.1 percentage points lower probability of employment for CTB recipients relative to other individuals, depending on whether we consider the full UKHLS sample or focus the analysis to individuals living in households earning less than GBP30,000. This result is consistent, though quantitatively somewhat smaller in magnitude, to the equivalent specification in the WAS (which is column 3 of Table 4), where we obtain a point estimate of 9 percentage points. Column 3 of Table 5 takes advantage of the local authority identifiers in the UKHLS to include district-year fixed effects to the regression which now compares the employment probability of CTB recipients within a local authority district before and after the reform. Focusing on individuals living in households earning less than GBP30,000, we find that including these additional fixed effects has little effect on the baseline results.

Column 4 of Table 5 shows that higher per capita cuts at the district level are associated with lower probability of employment after the welfare cuts. This is true on average, not just for CTB recipients, and a one-standard deviation higher cut intensity relative to the mean per capita cuts (which translates to moving from GBP447 in welfare cuts per capita per year to GBP568) leads to a lower employment probability of 1.1 percent among the sample of individuals in households

making less than GBP30,000. This effect becomes amplified for CTB recipients, which we measure using a triple-differences specification in column 5. Here, the point estimate of 0.064 means that, relative to the effect for control individuals, CTB recipients experience a further decrease in their probability of employment of 1.7% when we move from the average district in per capita cuts to one that is one standard deviation above the mean.

In Appendix Table [A.3](#), we further compare the latter result in districts with high and low pre-reform unemployment rates. We find that the lower likelihood of employment of CTB recipients in high cut areas is stronger in districts with a higher pre-reform unemployment rate. This finding underscores the role of labor market “slack” in amplifying the effects for affected individuals by reducing their ability to replace lost income.

The evidence from the UKHLS confirms and extends a surprising result obtained in the WAS: welfare cuts reduce employment in the locations most affected by the cuts, and the population where this is most visible is among welfare recipients themselves, which represents a further reduction in income for these households.

3.2.2 Firm-level evidence of local spillovers

In order to corroborate the result that local welfare cuts lead to weaker demand, and, in turn, lower employment we turn to Bureau van Dijk’s FAME dataset that contains employment and revenue information for private and public firms in the UK. Our hypothesis is that both revenue and employment loss should be concentrated among firms that are most dependent on local demand (the non-tradable sector), specifically in districts that are most affected by the cuts. This represents our most direct test of the hypothesis that local demand is responsible for the employment effects of Tables [4](#) and [5](#).

We identify sectors that are dependent on local demand using the definitions in [Mian and Sufi \(2014\)](#) (essentially firms in retail, food and accommodation), and those that are not dependent on local demand by focusing on manufacturing firms. We also construct a broader definition of local sectors that includes not only retail, food and accommodation, but also services other than motion pictures. In addition, we distinguish small and large firms (we use a 100-employee threshold) as these are more likely to operate in one or few locations. Large firms are more likely to have many locations where they operate, and thus are less likely to be affected by shocks in one or few

districts.

Table 6 shows the firm-level results of the local spillovers of welfare cuts. As we describe above, results are split by firm size (small firms, below 100 employees, and large firms, above 100 employees), as well as firm industry, particularly non-tradable and manufacturing sectors. Column 1 shows that revenues of small firms in the non-tradable sector are negatively affected by the welfare cuts in the districts in which they are located. The point estimates suggest that being one standard deviation above average per capita cuts leads to a 1.6% reduction in revenues for small firms in local sectors, and a somewhat higher 2.4% for small firms defined as non-tradable in [Mian and Sufi \(2014\)](#). We see no statistically significant effect of local per capita cuts on revenues of manufacturing firms, consistent with customers of these firms (even the small ones) being located, on average, far from where the firms themselves are located ([Adelino et al. \(2015\)](#)).

Column 2 of Table 6 shows that large firms in the non-tradable sector do not suffer similar revenue losses, nor do large manufacturing firms. This confirms that larger firms are less dependent on demand from the specific district in which they are headquartered, as they are much more likely to have operations spread over large geographic areas, even if they are in non-tradable sectors (for example, by having multiple retail locations).

Columns 3 and 4 present very similar results when using employment as an outcome. Consistent with the local spillovers hypothesis, small firms in sectors dependent on local demand reduce employment in districts most affected by austerity cuts. In [Figure A.1](#), we further show with UKHLS data that the individuals affected by the council tax benefit cuts are significantly more likely than other individuals to work in the food and accommodation sector (one of the sector most dependent on local demand), suggesting that the decline in employment in these firms particularly affects these individuals.

Finally, we find some evidence for the relevance of a labor supply channel: although, on net, affected individuals are less likely to be employed after the reform (see [Table 4](#)), they are weakly more likely to have looked for work in the 4 weeks before the survey interview (see [Table A.4](#)). Overall, this suggests that affected individuals *try* to increase labor supply to replace lost benefits, but on average do not manage to do so, as the firms in which they would typically work (e.g. those very dependent on local demand) are reducing employment following the reform.

4 Household Balance Sheet Effects

We now turn to the effect of the council tax benefit reform and its associated impact on total income and employment on household liabilities.

4.1 Mortgage Debt and Home Ownership

We start by considering the effects on mortgage debt and home ownership. Even though home ownership rates are low among benefits recipients in the UK, housing debt is the most important liability for most households who hold a mortgage, and home ownership is often hailed as an important policy goal in Western economies.

In columns 1 and 2 of Table 7, we find that the benefit cuts have adverse effect on home ownership. After controlling for pre-reform differences between treated and control individuals, either by interacting these characteristics with the post-period indicator or by matching individuals on these characteristics, we find that treated individuals are 3.7 to 4.0 percentage points less likely to own a house in the three years after the cut. This represents a relative decline of about 23% in the likelihood of home ownership for the treated.

The mechanism behind this drop can be either current owners selling and becoming renters at a higher rate, or renters converting more slowly into becoming homeowners (typically for younger households). In unreported regressions we find that the effect is primarily concentrated in households who are not previously homeowners, i.e. a lower conversion rate of renters into owners for the treated group.

One consequence of the results on home ownership is that treated individuals are less likely to have a mortgage after the reform. After controlling for pre-reform differences between treated and control individuals, the results in columns 3 and 4 of Table 7 indicate that the probability to have a mortgage declines by between 2.0 to 2.3 percentage points for treated individuals relative to the controls. The 2.3 percentage point coefficient in column 3 represents a decline of 20% from the pre-period average likelihood of having a mortgage for the treated, a magnitude consistent with the reduction in home ownership we observe in columns 1 and 2.

The pattern in Panel A of Figure 4 depicts the marked decline in mortgage use after the implementation of the cuts. The timing of the effects is consistent with a causal relation between the

benefit cuts and the reduction in mortgage use.

The reduction in mortgage use for treated individuals moves in lockstep with home ownership, so mortgage use does not particularly speak to mortgage debt usage holding housing tenure (i.e., rent vs. own status) fixed. When we consider owners who own their house outright – that is without a mortgage – before the cuts, we find a very different effect of the shock. Among this sub-sample, treated individuals are more likely to have a mortgage after the cuts, suggesting they used the available collateral to raise debt. The point estimate in column 5 (sample restricted to individuals in households earning less than GBP30,000) is somewhat imprecise and falls just short of significance at the 10% level ($p=0.125$) but that for the matched sample in column 6 indicates that among outright owners, the probability of having a mortgage significantly increases by 9.3 percentage points ($p=0.032$).

4.2 Unsecured Consumer Debt

After establishing that the reduction in council tax benefits affects home ownership, we assess how unsecured borrowing behavior changes after the reform. We examine consumer debt in the form of credit card and personal loans. Columns 1 to 4 of Table 8 present the results of estimating equation 1 for credit card outcomes. In columns 1 and 2, we find evidence that treated individuals are more likely to have a credit card after the austerity cuts. The estimate from the saturated regressions on households earning less than GBP30,000 is positive (2.1 percentage points) but is just short of statistical significance at the 10% level ($p=0.103$). The estimates from the matched sample are more precise. They suggest that, on average, treated individual experience a significant 3.7 percentage point increase in the probability of having a credit card ($p=0.009$), which represents a 13% increase from the mean for the treated.

Columns 3 and 4 presents the results of regressions assessing the actual usage of credit card debt. We estimate the change in the probability that a treated individual has a credit card balance outstanding after the reduction in council tax benefits, relative to control individuals. The coefficient in column 3 suggests that, among individuals belonging to households earning less than GBP30,000, treated individuals experience a 4.8 percentage point increase in the probability of having a credit card balance outstanding after the cuts. The coefficient is significant at the 1% level and represents a 28% increase relative to the mean probability of having a credit card

balance outstanding for the treated before the cuts. The results from the matched samples yield comparable estimates of 3.7 percentage points (a 22% relative effect from the mean). We do not find a similar increase in the usage of personal loans. The estimates in columns 5 and 6 do not suggest significant changes in the probability that treated individuals have personal loans (e.g., loans with a bank, building society, finance house, etc.) after the council tax benefit cuts. It is notable that households are more likely to obtain a credit card and that they increase the amount of unsecured debt given the nature of the shock – there was no indication that the reduction in benefits would be temporary, so the increase in unsecured debt is not easy to reconcile with a consumption smoothing motive.

Panel B of Figure 4 displays the trend in credit card outstanding balance for pooled treated and control individuals over time. As we observed for Figure 2 and most of the outcomes, the pre-period displays little difference between the two groups, and the difference materializes after the council tax benefit cut implementation.

4.3 Household Distress

In a final set of tests we measure the combined effect of benefit cuts, lower income and greater debt usage on possible financial distress. The results in columns 1 and 2 of Table 9 Panel A indicate that the council tax benefit cut materially affects the ability of individuals to meet their local tax obligations. We find that, after the reform, affected individuals are 3.1 to 3.7 percentage points more likely to miss local tax payments (relative to a mean of 5 percentage points). This finding suggests that affected individuals were very dependent on these welfare payments to meet their council tax bill, and they are not able to fully substitute this loss of benefit income with other sources of revenues or liquidity.⁹ In Panels B and C of Table 9, we split the sample according to individuals' employment status after the reform. We find that the difficulty in meeting council tax payments is predominantly concentrated among individuals who are unemployed after the reform.

In columns 3 and 4, we assess affected households' ability to meet rent payments. While the results in Panel A display a positive but imprecise average effect across all affected individuals,

⁹Affected households have virtually no savings, which also prevents them from – even temporarily – use savings to meet these obligations and possibly smooth the shock.

the results in Panel B indicate that for individuals unemployed after the reform the difficulty of meeting financial obligations extends to rent payments. These individuals who are severely affected by the reform become 2 to 3 percentage points more likely to miss rent payments than their control group (relative to a mean of 6.5%). In Panel C, we find that employed individuals manage to keep up with rent payments despite the cut in benefits.

In columns 5 and 6, we then leverage a question of the WAS that directly asks respondents about their perceived debt burden, i.e. whether keeping up with payments on their existing debt feels burdensome. Here, we also find evidence that the council tax benefit cut strains households' finances. Recipients of council tax benefits are 10.2 to 10.9 percentage points more likely than the control group to report feeling a debt burden after the reform. This represents approximately a 24% increase relative to the mean before the reform. Both employed and unemployed individuals report an increase in feeling the burden of debt.¹⁰

5 Conclusion

In this paper, we exploit a large welfare reform in the UK, to examine the direct and indirect effects of benefit cuts on economically fragile households' finances.

We show that welfare recipients not only lose benefits income, but are also less likely to be employed following the cuts. These employment effects happen in areas more severely affected by the cuts and in small firms in the non-tradable sector. This suggests that local demand reductions play a role in amplifying the initial shock and particularly affect households already directly affected by the welfare cuts. These individuals respond by trying to access liquidity by borrowing, either on credit card balances or from mortgage debt when they possess the necessary collateral. The vast majority of affected households do not own a house, however, and the reform makes them subsequently even less likely to be reach home ownership. We finally show that, overall, the reform makes them more likely to enter financial distress and feel pressure from accumulated debt burden.

¹⁰In Table A.6 of the appendix, we examine whether affected households effectively keep up with debt payments (credit cards, loans, mortgage). We find that in the three years after the reform, affected households appear to maintain their ability to make debt payments, possibly because they prioritize these payments, or because the borrower affordability requirements with which lenders need to comply prevent or delay the materialization of debt payment difficulties.

Together, our micro-level evidence suggests that austerity policies and welfare cut programs have financial implications for affected households that go well beyond the direct effect of benefit income reduction, and are likely to increase disparities between low and middle income households. As such, the results also provide a plausible mechanism for some of the short-term radical political implications that austerity programs have had. How, in the longer term, the economic spillovers that we document may also affect the finances and the future prospects of individuals who grow up in the most affected areas and how these spillovers may affect inter-generational economic mobility are interesting avenues for further research.

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Tables and Figures

Figure 1: Expected Austerity Cuts across Regions and Districts

The figure shows the expected austerity cuts per capita by region (Panel A) and local authority district (Panel B). Data is from [Beatty and Fothergill \(2013\)](#) and [Fetzer \(2019\)](#).

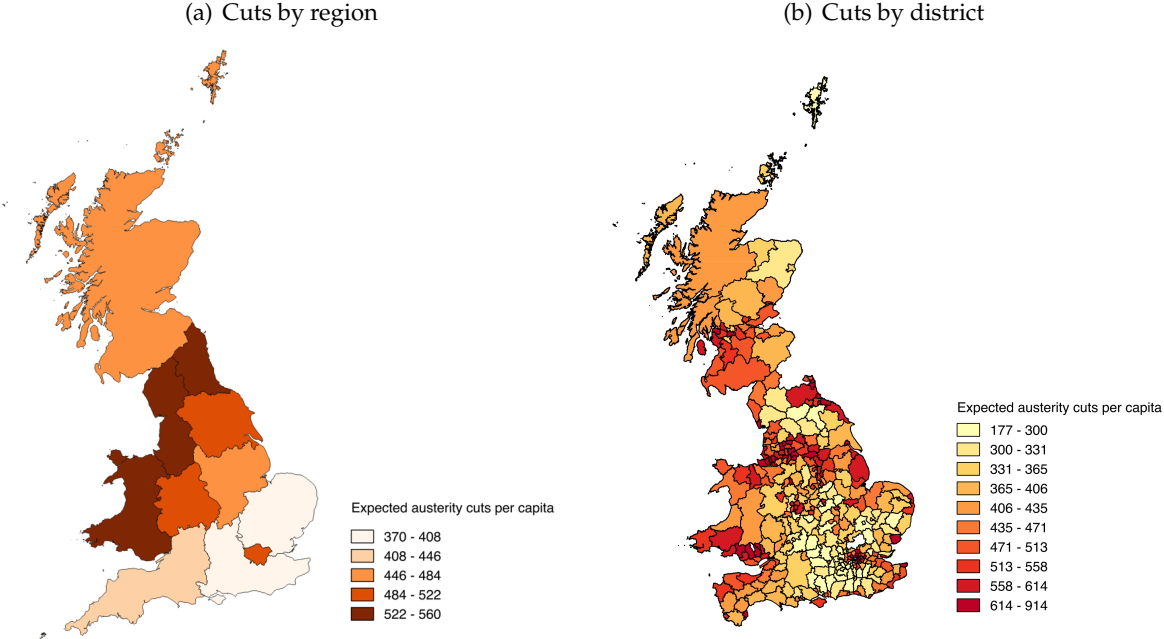


Figure 2: Reduction in Income from Welfare Benefits

The figure shows the effect of the council tax benefit cut on affected individuals' reception of welfare benefits. For each group of individuals (affected and unaffected in households earning less than GBP30,000), we estimate a regression of the outcome of interest on year fixed effects, wave fixed effects and a pre-austerity employment indicator. The figure presents the coefficients on the year fixed effects.

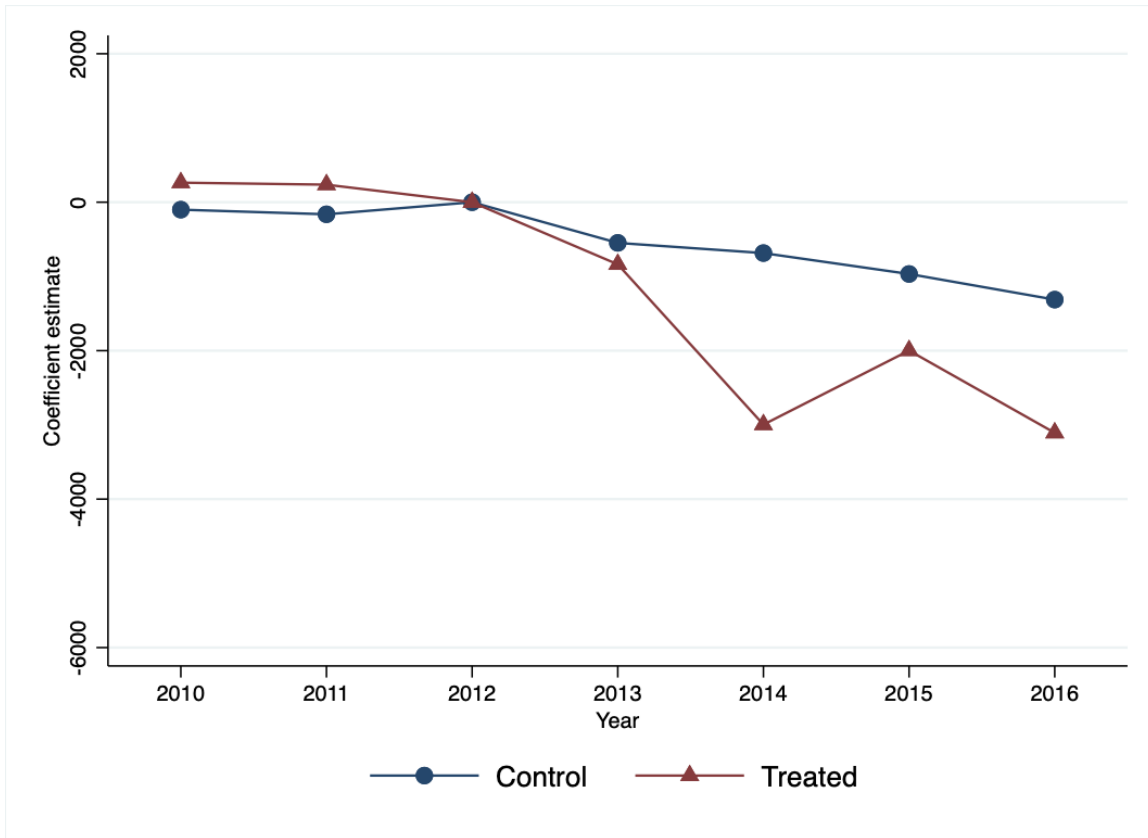


Figure 3: Probability of Employment

The figure shows the effect of the council tax benefit cut on affected individuals' probability of employment. For each group of individuals (affected and unaffected in households earning less than GBP30,000), we estimate a regression of the outcome of interest on year fixed effects and wave fixed effects. The figure presents the coefficients on the year fixed effects.

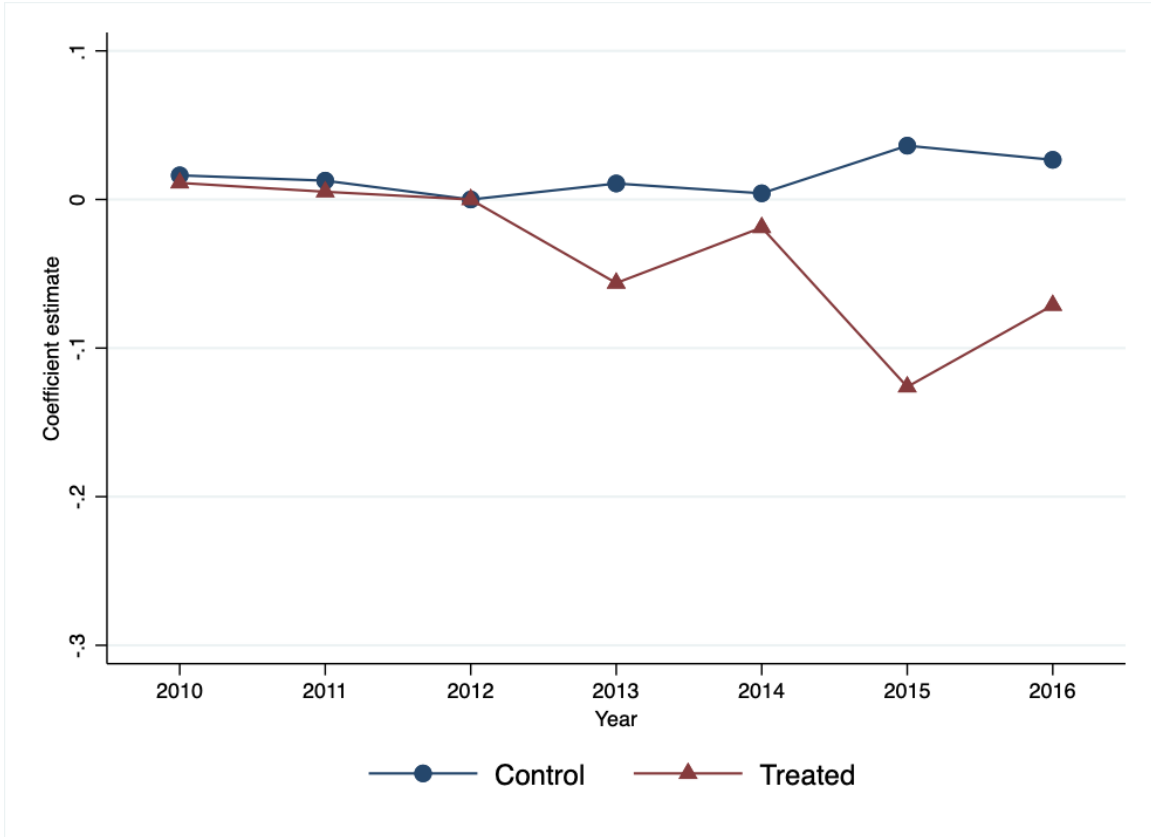
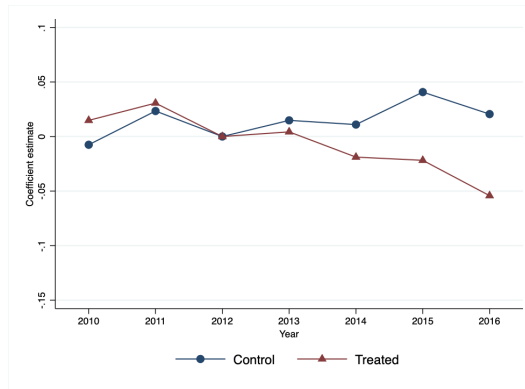


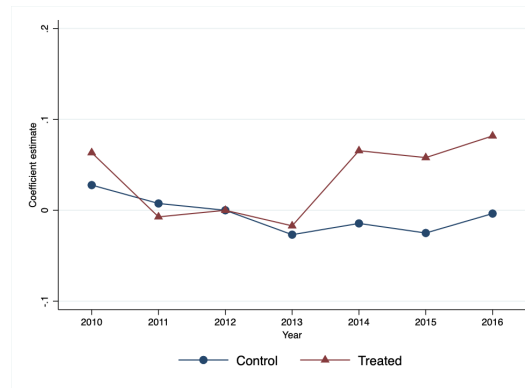
Figure 4: Individuals' Financial Situation

The figure shows the effect of the council tax benefit cut on affected individuals' probability of having a mortgage (Panel A), having a credit card balance outstanding (Panel B), and feeling a debt burden (Panel C). For each group of individuals (affected and unaffected in households earning less than GBP30,000), we estimate a regression of the outcome of interest on year fixed effects, wave fixed effects and a pre-austerity employment indicator. The figure presents the coefficients on the year fixed effects.

(a) Mortgage



(b) Credit Card Balance



(c) Debt Burden

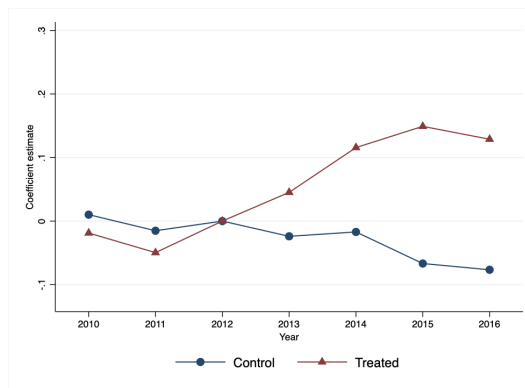


Table 1: Descriptive Statistics

This table presents the descriptive statistics for individuals in the Wealth and Asset Survey (Panel A) and in the Understanding Society / UK Households Longitudinal Study (Panel B), and for firms in the FAME dataset (Panel C). In Panel A and B, the statistics are calculated on the cross-section of working age individuals in the last wave before the reform. In Panel C, the statistics are calculated on the cross-section of firms in 2012. We focus on firms with positive revenues and more than 3 employees. We exclude firms with year-on-year growth rate in assets greater than 300%.

	Mean	S.D.	Min.	Median	Max.	Obs.
<i>Panel A, WAS</i>						
Age	43	12	18	42	62	14,571
Employed	0.77	0.42	0.00	1.00	1.00	14,571
Male	0.51	0.50	0.00	1.00	1.00	14,571
Owner	0.76	0.43	0.00	1.00	1.00	14,571
Degree	0.28	0.45	0.00	0.00	1.00	14,571
Debt burden	0.23	0.42	0.00	0.00	1.00	14,571
CTB	0.08	0.27	0.00	0.00	1.00	14,477
Welfare benefits	3,983	5,785	0	1,580	26,600	14,571
Net income	39,143	31,140	0	33,200	910,000	14,571
<i>Panel B, UKHLS</i>						
Expected district cut p.c.	479	117	177	478	914	37,781
CTB	0.05	0.23	0.00	0.00	1.00	38,792
<i>Panel C, FAME</i>						
Revenue						
Local sectors, <100 Emp.	6,628	54,923	1	1,409	4,170,838	20,996
Local sectors, >100 Emp.	114,032	1,128,317	8	18,356	67,228,128	8,991
Manufacturing, <100 Emp.	13,172	44,550	4	8,551	2,232,092	4,676
Manufacturing, >100 Emp.	162,453	941,194	74	31,588	27,532,292	4,022
Employment						
Local sectors, <100 Emp.	29	29	1	17	282	21,659
Local sectors, >100 Emp.	1,036	10,944	1	209	648,254	9,090
Manufacturing, <100 Emp.	52	30	1	52	253	4,774
Manufacturing, >100 Emp.	706	4,105	1	201	110,460	4,047

Table 2: Descriptive Statistics: Means for Treated and Control Groups

This table compares the mean of characteristics for treated and control individuals in the Wealth and Asset Survey. The statistics are calculated on the cross-section of working age individuals in the last wave before the reform. Column 1 presents the means for the individuals affected by the reform. Column 2 presents the means for all unaffected working age individuals, column 3 for those belonging to households earning less than GBP30,000 and column 4 for those matched to the treated group by propensity score.

	Treated	Control		
		Raw	<30k	Matched
Age	44	43	43	42
Employed	0.25	0.82	0.71	0.26
Male	0.39	0.52	0.51	0.37
Owner	0.17	0.81	0.65	0.19
Degree	0.07	0.30	0.17	0.08
Debt burden	0.42	0.22	0.25	0.41
Net income	18,061	40,903	20,334	21,372

Table 3: Effect of Welfare Cuts on Income from Welfare Benefits

This table presents the results of the difference-in-differences estimation of equation 1. *CTB* is a dummy variable that takes the value of one if the individual received council tax benefits before the reform. *Post* is a dummy variable that takes the value of one after the council tax benefit cut, from 2013 onward. In columns 1 and 2, we restrict the sample to individuals for which the household earns less than GBP30,000 per year before the reform. In columns 3 and 4, we use a matched sample where each individual affected by the reform is matched to three (column 3) and five (column 4) unaffected individuals. Individuals are matched by a propensity score. In column 5, we use our full sample of working age individuals. Standard errors presented in parentheses are clustered at the region-year level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	Income from welfare benefits				
	(1) <30k	(2) <30k Slopes	(3) Match n3	(4) Match n5	(5) Raw Slopes
CTB X Post	-2311.831*** (259.200)	-2265.334*** (280.856)	-2171.246*** (339.156)	-2307.338*** (322.720)	-2202.142*** (235.989)
Individual	Yes	Yes	Yes	Yes	Yes
Region X Year	Yes	Yes	Yes	Yes	Yes
Wave X Year	Yes	Yes	Yes	Yes	Yes
Employed _{pre} X After	-	Yes	-	-	Yes
Male _{pre} X After	-	Yes	-	-	Yes
Age category _{pre} X After	-	Yes	-	-	Yes
Owner category _{pre} X After	-	Yes	-	-	Yes
Education _{pre} X After	-	Yes	-	-	Yes
ln(Income) _{pre} X After	-	Yes	-	-	Yes
Debt burden _{pre} X After	-	Yes	-	-	Yes
Observations	16056	15955	7067	8482	38027

Table 4: Employment and Income Effects of Welfare Cuts

This table presents the results of the difference-in-differences estimation of equation 1. *CTB* is a dummy variable that takes the value of one if the individual received council tax benefits before the reform. *Post* is a dummy variable that takes the value of one after the council tax benefit cut, from 2013 onward. In columns 1, 3 and 5, we restrict the sample to individuals for which the household earns less than GBP30,000 per year before the reform. In columns 2, 4 and 6, we use a matched sample where each individual affected by the reform is matched to three unaffected individuals. Individuals are matched by a propensity score. Standard errors presented in parentheses are clustered at the region-year level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	Empl. income		Employed		Empl. income if employed	
	(1) <30k Slopes	(2) Matched	(3) <30k Slopes	(4) Matched	(5) <30k Slopes	(6) Matched
CTB X Post	-3397.407*** (470.596)	-3727.535*** (699.528)	-0.090*** (0.017)	-0.083*** (0.020)	174.977 (890.990)	441.393 (919.507)
Individual	Yes	Yes	Yes	Yes	Yes	Yes
Region X Year	Yes	Yes	Yes	Yes	Yes	Yes
Wave X Year	Yes	Yes	Yes	Yes	Yes	Yes
Pre-char. X Post	Yes	-	Yes	-	Yes	-
Observations	15942	7064	15955	7067	9779	2612

Table 5: Labor Market Spillovers of Austerity: Individual-Level Evidence

This table presents the results of regressions assessing the effect of austerity on individuals' employment status. Data is from the UKHLS survey, which includes information on individuals' location at the local authority level. *Cut* is the expected total amount of austerity cuts per capita in a district (Fetzer (2019)). *CTB* is a dummy variable that takes the value of one if the individual received council tax benefits before the reform. *Post* is a dummy variable that takes the value of one after the council tax benefit cut, from 2013 onward. Columns 1 to 3 assess the specific effect of the council tax benefit cut on individuals receiving such benefits before the reform (that is, it reproduces the results of columns 1 and 2 of Table 3 with UKHLS data, without and with District X Year fixed effects). Column 1 presents the estimate without the District X Year fixed effects for the full sample of working age individuals in UKHLS. Columns 2 and 3 (and all other columns) present results for the sample of individuals for which the household earns less than GBP30,000 per year before the reform. Column 4 assesses the effect of austerity cuts at the district level. Column 5 assesses the differential effect of the council tax benefit cut in districts affected by overall austerity cuts of different intensity. Standard errors presented in parentheses are clustered at the region-year level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	Employed				
	(1) Full	(2) <30k	(3) <30k	(4) <30k	(5) <30k
CTB X Post	-0.070*** (0.010)	-0.061*** (0.010)	-0.057*** (0.010)		
ln(Cut) X Post				-0.041*** (0.010)	
ln(Cut) X CTB X Post					-0.064** (0.032)
ln(Cut)				-0.018 (0.030)	
ln(Cut) X CTB					0.092 (0.142)
Individual	Yes	Yes	Yes	Yes	Yes
Wave X Year	Yes	Yes	Yes	Yes	Yes
Region X Year	Yes	Yes	-	Yes	-
Pre-char. X Post	Yes	Yes	Yes	Yes	Yes
District X Year	-	-	Yes	-	Yes
CTB X Year	-	-	-	-	Yes
Observations	166721	75270	75220	75270	75220

Table 6: Labor Market Spillovers of Austerity: Firm-Level Evidence

This table presents the results of difference-in-differences regressions assessing the effect of austerity on firms of different sizes and active in different sectors. The regressions compare revenues (rev.) and employment (empl.) of firms before and after the austerity cuts, as a function of the total amount of austerity cuts per capita in the district in which the firm is located. Each cell of the table represents the coefficient on $Post \times \ln(Cuts)$ of a separate regression. $Cuts$ is the expected total amount of austerity cuts per capita in a district (Fetzer (2019)). $Post$ is a dummy variable that takes the value of one after the council tax benefit cut, from 2013 onward. Columns 1 and 3 (2 and 4) estimate the regression on firms with fewer (more) than 100 employees (average over the sample period). Row 1 presents the results for firms in local sectors (retail and services), row 2 presents the results for firms in non-tradable industries (defined as in Mian and Sufi (2014)) and row 3 presents the results for firms in the manufacturing sector. Standard errors presented in parentheses are clustered at the district level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	ln(Rev.)		ln(Empl.)	
	<100 empl. (1)	>100 empl. (2)	<100 empl. (3)	>100 empl. (4)
Local sectors (retail and services)	-0.060** (0.024) 153778	0.005 (0.022) 71878	-0.030*** (0.009) 191538	0.037 (0.033) 79580
Non-tradable (Mian and Sufi 2014)	-0.088** (0.037) 15866	0.025 (0.053) 14243	-0.108*** (0.032) 17995	0.020 (0.054) 15208
Manufacturing	-0.018 (0.033) 39443	0.004 (0.036) 36022	-0.010 (0.029) 44716	-0.014 (0.056) 37924
Firm	Yes	Yes	Yes	Yes
Industry \times Year	Yes	Yes	Yes	Yes

Table 7: Housing Ownership and Mortgage Debt

This table presents the results of the difference-in-differences estimation of equation 1. *CTB* is a dummy variable that takes the value of one if the individual received council tax benefits before the reform. *Post* is a dummy variable that takes the value of one after the council tax benefit cut, from 2013 onward. In columns 1, 3 and 5 we restrict the sample to individuals for which the household earns less than GBP30,000 per year before the reform. In columns 2, 4 and 6 we use a matched sample where each individual affected by the reform is matched to three unaffected individuals. Individuals are matched by a propensity score. Standard errors presented in parentheses are clustered at the region-year level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	Owner		Mortgage		Mortgage if outright owner before	
	(1) <30k Slopes	(2) Matched	(3) <30k Slopes	(4) Matched	(5) <30k Slopes	(6) Matched
CTB X Post	-0.037*** (0.006)	-0.040*** (0.008)	-0.023*** (0.006)	-0.020*** (0.006)	0.070 (0.045)	0.093** (0.043)
Individual	Yes	Yes	Yes	Yes	Yes	Yes
Region X Year	Yes	Yes	Yes	Yes	Yes	Yes
Wave X Year	Yes	Yes	Yes	Yes	Yes	Yes
Pre-char. X Post	Yes	-	Yes	-	Yes	-
Observations	15954	7067	15955	7067	3056	555

Table 8: Consumer Debt

This table presents the results of the difference-in-differences estimation of equation 1. *CTB* is a dummy variable that takes the value of one if the individual received council tax benefits before the reform. *Post* is a dummy variable that takes the value of one after the council tax benefit cut, from 2013 onward. In columns 1, 3 and 5 we restrict the sample to individuals for which the household earns less than GBP30,000 per year before the reform. In columns 2, 4 and 6 we use a matched sample where each individual affected by the reform is matched to three unaffected individuals. Individuals are matched by a propensity score. Standard errors presented in parentheses are clustered at the region-year level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	Credit card		C.C. balance outstanding		Personal loan	
	(1) <30k Slopes	(2) Matched	(3) <30k Slopes	(4) Matched	(5) <30k Slopes	(6) Matched
CTB X Post	0.021 (0.012)	0.037*** (0.014)	0.048*** (0.017)	0.037* (0.019)	-0.010 (0.013)	-0.022 (0.017)
Individual	Yes	Yes	Yes	Yes	Yes	Yes
Region X Year	Yes	Yes	Yes	Yes	Yes	Yes
Wave X Year	Yes	Yes	Yes	Yes	Yes	Yes
Pre-char. X Post	Yes	-	Yes	-	Yes	-
Observations	15749	6959	15718	6940	15733	6842

Table 9: Financial and Debt Burden

This table presents the results of the difference-in-differences estimation of equation 1. *CTB* is a dummy variable that takes the value of one if the individual received council tax benefits before the reform. *Post* is a dummy variable that takes the value of one after the council tax benefit cut, from 2013 onward. In columns 1, 3 and 5 we restrict the sample to individuals for which the household earns less than GBP30,000 per year before the reform. In columns 2, 4 and 6 we use a matched sample where each individual affected by the reform is matched to three unaffected individuals. Individuals are matched by a propensity score. Panel A presents the results from regressions estimated all individuals in the sample. Panel B and C presents the results for individuals without and with employment in the post-reform period, respectively. Standard errors presented in parentheses are clustered at the region-year level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	Behind council tax		Behind rent		Any debt burden	
	(1) <30k Slopes	(2) Matched	(3) <30k Slopes	(4) Matched	(5) <30k Slopes	(6) Matched
<i>A. All individuals</i>						
CTB X Post	0.037*** (0.010)	0.031*** (0.011)	0.010 (0.012)	0.011 (0.011)	0.109*** (0.021)	0.102*** (0.025)
Observations	15955	7067	6596	5114	15955	7067
<i>B. Individuals unemployed post reform</i>						
CTB X Post	0.049*** (0.013)	0.053*** (0.016)	0.030** (0.013)	0.022* (0.013)	0.115*** (0.023)	0.087*** (0.033)
Observations	4748	3510	2778	2742	4748	3510
<i>C. Individuals employed post reform</i>						
CTB X Post	0.025* (0.013)	0.002 (0.012)	0.002 (0.021)	-0.004 (0.020)	0.096*** (0.035)	0.113** (0.045)
Observations	11207	3557	3818	2372	11207	3557
Individual	Yes	Yes	Yes	Yes	Yes	Yes
Region X Year	Yes	Yes	Yes	Yes	Yes	Yes
Wave X Year	Yes	Yes	Yes	Yes	Yes	Yes
Pre-char. X Post	Yes	-	Yes	-	Yes	-

ONLINE APPENDIX
for
“Welfare Cuts, Local Spillovers and Financial Fragility”

Figure A.1: In Which Sectors Do Employed Council Tax Benefit Recipients Work?

The figure shows the coefficients of OLS regressions comparing the probability of employment in a particular sector for employed council tax benefit recipients relative to other employed individuals. Each regression regresses an indicator for employment in the particular sector on an indicator for the reception of council tax benefit, as well as local district, year, gender, and age category (3 categories) fixed effects. Each dot represents the coefficient on the reception of council tax benefit indicator. The models are estimated on the last pre-austerity year. Sector categories correspond to UK SIC 2007 classification.

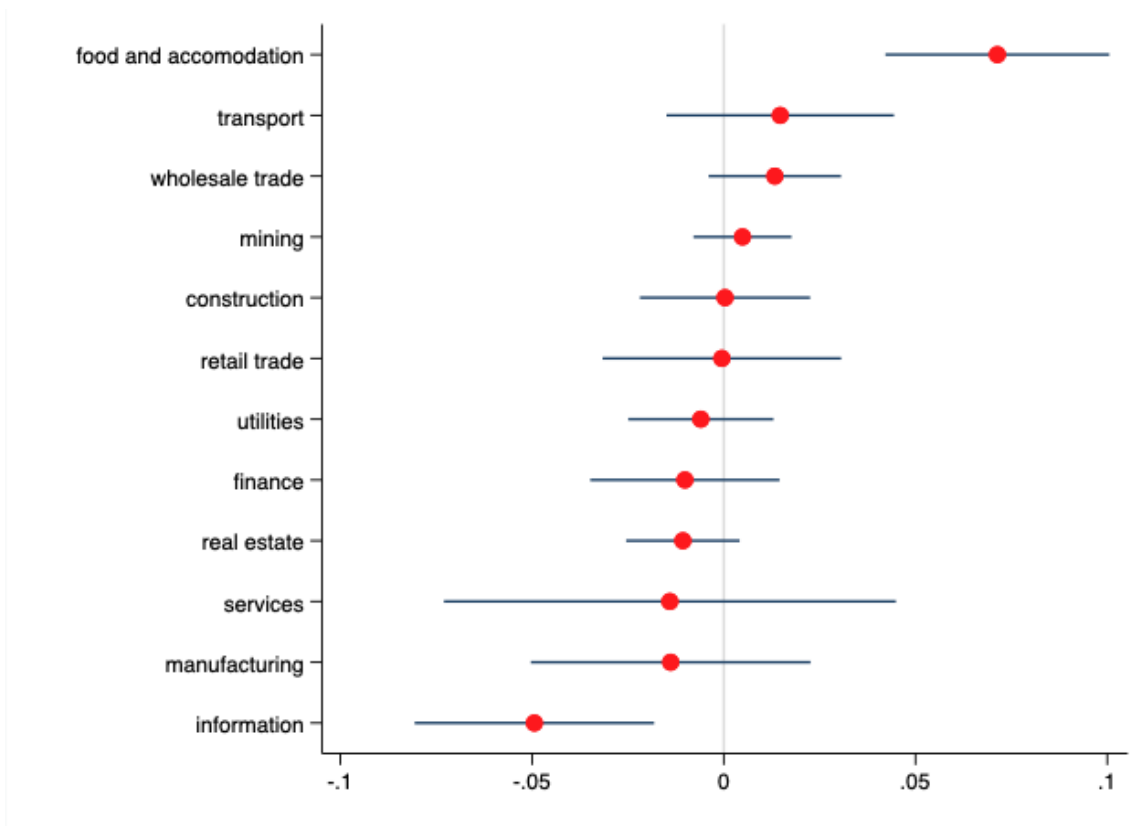


Table A.1: Propensity Score Estimation

This table presents the results of the propensity score estimation. Data is from working age individuals in the Wealth and Asset Survey (WAS). The regressions are estimated with a logit model, in the last wave before the austerity reform. The dependent variable is *CTB*, an indicator that takes the value of one if the individual received council tax benefits before the reform. Age categories are younger than 40 (=1), 40 to 49 (=2), and 50 and over (=3). Owner categories are outright owner (=1), owner with a mortgage (=2) and renter and other (=3). Robust standard errors are presented in parentheses. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	(1) CTB
Male	-0.353*** (0.079)
Employed	-1.946*** (0.089)
Age category=2	0.676*** (0.098)
Age category=3	0.651*** (0.095)
Owner status=2	0.166 (0.172)
Owner status=3	2.537*** (0.152)
Has degree	-0.787*** (0.144)
ln(Net income)	-0.502*** (0.073)
Debt burden	0.942*** (0.083)
Year	Yes
Yegion	Yes
Observations	14440

Table A.2: Labor Market Spillovers of Austerity: Individual-Level Evidence by Tercile of Austerity Cuts

This table presents the results of regressions assessing the effect of austerity on individuals employment status. Data is from the UKHLS survey, which includes information on individual's location of the local authority level. $Cuts_{terc2}$ and $Cuts_{terc3}$ denote dummy variables that take the value one when the expected total amount of austerity cuts per capita in the district are above the second or third tercile, respectively. CTB is a dummy variable that takes the value of one if the individual received council tax benefits before the reform. $Post$ is a dummy variable that takes the value of one after the council tax benefit cut, from 2013 onward. Standard errors presented in parentheses are clustered at the region-year level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	Employed	
	(1)	(2)
$Cut_{terc2} \times Post$	-0.005 (0.007)	
$Cut_{terc3} \times Post$	-0.021*** (0.007)	
$Cut_{terc2} \times CTB \times Post$		-0.032 (0.020)
$Cut_{terc3} \times CTB \times Post$		-0.047** (0.021)
Cut_{terc2}	-0.002 (0.018)	
Cut_{terc3}	-0.005 (0.019)	
$Cut_{terc2} \times Post$		-0.150** (0.068)
$Cut_{terc1} \times Post$		0.022 (0.084)
Individual	Yes	Yes
Wave X Year	Yes	Yes
Pre-char. X Post	Yes	Yes
District X Year	-	Yes
CTB X Year	-	Yes
Observations	75270	75220

Table A.3: Labor Market Spillovers of Austerity: Effect in Districts with High and Low Unemployment Rate

This table presents the results of regressions assessing the effect of austerity on individuals' employment status, in districts with low and high unemployment rate before the reform. Data on individuals is from the UKHLS survey, which includes information on individuals' location at the local authority level. *Cut* is the expected total amount of austerity cuts per capita in a district (Fetzer (2019)). Data on local authority district unemployment rates come from the Office of National Statistics, and is measured as the average unemployment rate in the district between 2010 and 2012. The low and high unemployment subsamples are defined according to the sample median. *CTB* is a dummy variable that takes the value of one if the individual received council tax benefits before the reform. *Post* is a dummy variable that takes the value of one after the council tax benefit cut, from 2013 onward. The regressions are based on the sample of individuals for which the household earns less than GBP30,000 per year before the reform. Standard errors presented in parentheses are clustered at the region-year level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	Employed	
	(1) Low unempl.	(2) High unempl.
In(Cut) X Post	-0.044 (0.060)	-0.049 (0.136)
In(Cut) X CTB X Post	-0.042 (0.047)	-0.207*** (0.078)
Individual	Yes	Yes
Wave X Year	Yes	Yes
Pre-char. X Post	Yes	Yes
District X Year	Yes	Yes
CTB X Year	Yes	Yes
Observations	37322	36651

Table A.4: Job Search Effects of Welfare Cuts

This table presents the results of the difference-in-differences estimation of equation 1. The dependent variable is defined for unemployed individuals. It is a dummy variable that takes the value of one if the individual has looked for work in the past 4 weeks, and zero otherwise. *CTB* is a dummy variable that takes the value of one if the individual received council tax benefits before the reform. *Post* is a dummy variable that takes the value of one after the council tax benefit cut, from 2013 onward. In columns 1 and 4, we restrict the sample to individuals for which the household earns less than GBP30,000 per year before the reform. In columns 2, 3, 5 and 6, individuals are matched to 3 or 5 neighbors by a propensity score. Since the dependent variable is only defined for unemployed individuals, columns 1 to 3 present the results of regressions without individual fixed effects to avoid restricting the estimation only to individuals that are necessarily unemployed before *and* after the reform. For completeness, columns 4 to 6 nonetheless present the results of the regressions with individual fixed effects. Standard errors presented in parentheses are clustered at the region-year level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	Looked for work					
	(1) <30k Slopes	(2) Matched n3	(3) Matched n5	(4) <30k Slopes	(5) Matched n3	(6) Matched n5
CTB	-0.051*** (0.016)	-0.017 (0.021)	-0.022 (0.022)			
Post × CTB	0.033 (0.028)	0.059* (0.031)	0.052* (0.031)	0.019 (0.025)	0.031 (0.028)	0.031 (0.025)
Individual				X	X	X
Region × Year	X	X	X	X	X	X
Wave × Year	X	X	X	X	X	X
Pre-charact. × Post	X			X		
Observations	5546	4083	4590	4798	3694	4121

Table A.5: Robustness: Full Sample

This table presents the results of the difference-in-differences estimation of equation 1, on the raw sample (all individuals of working age before the reforms). *CTB* is a dummy variable that takes the value of one if the individual received council tax benefits before the reform. *Post* is a dummy variable that takes the value of one after the council tax benefit cut, from 2013 onward. Standard errors presented in parentheses are clustered at the region-year level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Income	Employed	Owner	Mortgage	C.C. Balance	Behind CT	Debt Burden
CTB X Post	-4683.979*** (612.143)	-0.107*** (0.017)	-0.051*** (0.006)	-0.037*** (0.006)	0.037** (0.016)	0.032*** (0.009)	0.103*** (0.020)
Individual	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region X Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave X Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-char. X Post	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	38021	38027	38026	38027	37460	38027	38027

Table A.6: Behind Debt Payments

This table presents the results of the difference-in-differences estimation of equation 1. *CTB* is a dummy variable that takes the value of one if the individual received council tax benefits before the reform. *Post* is a dummy variable that takes the value of one after the council tax benefit cut, from 2013 onward. In columns 1, 3 and 5 we restrict the sample to individuals for which the household earns less than GBP30,000 per year before the reform. In columns 2, 4 and 6 we use a matched sample where each individual affected by the reform is matched to three unaffected individuals. Individuals are matched by a propensity score. Standard errors presented in parentheses are clustered at the region-year level. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

	Behind credit card		Behind loan		Behind mortgage	
	(1) <30k Slopes	(2) Matched	(3) <30k Slopes	(4) Matched	(5) <30k Slopes	(6) Matched
CTB X Post	-0.004 (0.006)	-0.005 (0.009)	-0.001 (0.007)	-0.001 (0.010)	-0.001 (0.006)	-0.001 (0.004)
Individual	Yes	Yes	Yes	Yes	Yes	Yes
Region X Year	Yes	Yes	Yes	Yes	Yes	Yes
Wave X Year	Yes	Yes	Yes	Yes	Yes	Yes
Pre-char. X Post	Yes	Yes	Yes	Yes	Yes	Yes
Observations	15955	7067	15955	7067	15955	7067