

The effects of coronavirus on household finances and financial distress

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Executive summary

The effect of the COVID-19 crisis on the economy has been huge. National income fell by 20% in April, to a level last seen in the early 2000s. But the impact of this vast aggregate shock on the finances of different households will vary widely.

In this report, we use a novel source of real-time data on households' finances from Money Dashboard, a budgeting app, to explore the impacts of the crisis so far on earnings, incomes and financial distress, and how they are evolving. We complement this with household survey data to explain and verify the key trends.

Key findings

The COVID-19 crisis led to abrupt falls in employment, earnings and incomes by April. There were no signs of a recovery in May.

By May 2020, when compared with what we would have predicted just before the crisis based on trends up to that point, the number of jobs was 4% lower, median after-tax household earnings were 9% lower and median household income (including benefits) was 8% lower (equivalent to an income loss of around £160 per month). These impacts had largely been felt in April, but with few signs of recovery in May.

The crisis has so far impacted the earnings of the poorest households the most.

Households in the poorest fifth – as measured by their pre-crisis income – have been hit hardest in terms of earnings, with a fall in their median household earnings of around 15% (or around £160 per month).

However, if we look at total income rather than just earnings, the poorest have not fallen further behind on average.

This highlights the important role of the benefits system in containing inequality and poverty. It partly reflects the government's temporary increases in benefits – set to last until April 2021 – and partly the simple fact that the benefits system replaces a relatively large share of lost earnings for the lowest earners.

Non-payment of household bills increased sharply after lockdown, and increased further between April and May.

By May, the number of households making mortgage, rental and council tax payments was, respectively, 14%, 11% and 9% below what we would have predicted based on pre-crisis trends. This represents a further deterioration since April, perhaps suggesting that some households are increasingly struggling to make ends meet as the crisis persists. Poorer households seem to be falling behind by more on council tax and utility bills. Non-payment of mortgages is spread more evenly across the income distribution.

In some cases, these unpaid bills will be important and sensible ways of weathering the storm (for example, mortgage holidays) – but they still mean additional debt that will be carried forward.

Looking at those who paid a given bill in January 2020 but did not pay that bill in May 2020, the average January bill amount was £1,660 for mortgages, £650 for rent, £170 for council tax and £139 for utilities. Increases in accumulated debts of these magnitudes are not sustainable, so this underlines the importance of a quick recovery in household incomes.

1. Introduction

The effect of the COVID-19 crisis on the economy has been huge. National income fell by 20% in April, to a level last seen in the early 2000s.¹ The impact of this vast aggregate shock on the finances of different households will vary widely. Some individuals have lost their job, others have been furloughed, some found re-employment elsewhere as sectors such as food retail increased recruitment, and others have found their livelihoods to be entirely robust to social distancing. Households also vary in their ability to cope with economic shocks: some can draw on other resources to smooth over a short-term financial hit, while others will have little such leeway and will quickly find themselves in financial distress. For example, about 30% of low-income households pre-crisis said that they could not manage a month if they were to lose their main source of household income.²

This report provides real-time evidence on how the myriad financial impacts of the crisis are actually playing out, using bank account data to assess changes in earnings, income and indicators of financial distress over the months of the crisis, and to document how these impacts vary across households. Further work with these data will focus on spending and financial balances and the role of policy in mitigating the economic shocks being experienced.

The data we use comes from the Money Dashboard (MDB) budgeting app, which provides daily information on (anonymised) user finances from bank accounts, detailing the credits and expenditures from all linked-in financial accounts (current, credit card and savings accounts). The data we exploit provide information on transactions directly, and cover all sources of income – as well as expenditures, which we will examine in further work. This is in contrast to survey data which typically rely on respondents to recall amounts in various categories. The data also record exactly when transactions happen, meaning they can be located precisely relative to the timing of key developments in the crisis and in the financial circumstances of the app’s users.

The sample that we use to form the basis of our estimates tracks the real-time finances of 5,684 users from May 2018 until the end of May 2020. As we describe in more detail in Section 2 and Appendix A, after using standard reweighting techniques to make the age and geographic profile of this sample comparable to the UK population as a whole, our (reweighted) sample matches up well to the pre-crisis national earnings and income distributions, number of workers in different age groups, levels of financial distress, and numbers of current accounts, credit cards and savings accounts per household. We see all of these users’ transactions throughout the period. This allows us to build a granular picture of precisely when and how their finances are being impacted by the crisis relative to a long pre-crisis period, and to document differences in how the crisis is changing total income and earnings and leading to financial distress.

These data complement evidence produced thus far on the effect of the crisis on individuals and households. Studies to date do not all accord precisely, but in combination

¹ Office for National Statistics, ‘GDP monthly estimate, UK: April 2020’, <https://www.ons.gov.uk/releases/gdpmonthlyestimateukapril2020>.

² D. Sturrock, ‘Despite short-term relief, households could face debt problems as a result of the coronavirus (COVID-19) pandemic’, IFS Observation, April 2020, <https://www.ifs.org.uk/publications/14820>.

the big picture they paint is clear: very large-scale disruption to the labour market and to people's finances. A survey carried out by Adams-Prassl et al. (2020)³ in late March (sample of 4,000 individuals) showed 8% of workers having lost their job and 30% being paid less than usual. Gardiner and Slaughter (2020)⁴ (sample of 6,000 individuals) reported 3% of employees having lost their job by early May. A COVID-19 survey module conducted by the long-running UK Household Longitudinal Study (UKHLS) in late April⁵ (a sample of more than 17,000) suggested that nearly a quarter of the workforce were working fewer hours, though the numbers losing jobs entirely were much more muted and similar to those reported by Gardiner and Slaughter. The government's Pay-As-You-Earn (PAYE) records showed a drop of 449,000 employees being paid between March and April, and a further drop of 163,000 in May.⁶ Kempson and Poppe (2020)⁷ used a survey (sample of 5,900 households) to analyse effects at the household level and found that by mid-April a quarter of households had lost at least a 'substantial part' of their earned income. We also know from job vacancy data that new job openings have, overall, been extremely scarce, despite some well-publicised labour shortages (for delivery drivers and fruit pickers, for example) early in the crisis.⁸

Some of these surveys also show high levels of financial distress, although it is difficult to assess how these levels have changed since the onset of the crisis. Kempson and Poppe (2020) report that in April, 17% of households were in arrears on housing costs, bills or debt repayments, while 15% say that they 'struggle to pay for food or expenses'. Using the same survey as Gardiner and Slaughter (2020), Judge (2020)⁹ finds that by early May, 9% of individuals had fallen behind with their rent or mortgage. The UKHLS data from late April show arrears having increased since the last available wave in 2017–18.

The financial accounts data that we use in much of this report complement the various survey-based pieces of evidence in several ways. First, whereas the surveys typically rely on 'snapshots' at particular moments in time, we can track the crisis continually as we have a consistent panel of users whose transactions are recorded on a daily basis and

³ A. Adams-Prassl, T. Boneva, M. Golin and C. Rauh, 'Inequality in the impact of the coronavirus shock: evidence from real time surveys', Cambridge Working Papers in Economics 2032, April 2020, <https://www.inet.econ.cam.ac.uk/working-paper-pdfs/wp2018.pdf>.

⁴ L. Gardiner and H. Slaughter, 'The effects of the coronavirus crisis on workers', Resolution Foundation, May 2020, <https://www.resolutionfoundation.org/app/uploads/2020/05/The-effect-of-the-coronavirus-crisis-on-workers.pdf>.

⁵ M. Benzeval, J. Burton, T. F. Crossley, P. Fisher, A. Jackle, H. Low and B. Read, 'Understanding Society COVID-19 Survey, April Briefing Note: the economic effects', University of Essex, Institute for Social and Economic Research (ISER), Working Paper 10/2020, April 2020, https://www.understandingsociety.ac.uk/sites/default/files/downloads/general/ukhls_briefingnote_covid_economics_final.pdf.

⁶ Office for National Statistics, 'Earnings and employment from Pay As You Earn Real Time Information, UK: June 2020', <https://www.ons.gov.uk/employmentandlabourmarket/peopleinwork/earningsandworkinghours/bulletins/earningsandemploymentfrompayasyouearnrealtimeinformationuk/june2020>.

⁷ E. Kempson and C. Poppe, 'Coronavirus financial impact tracker', Standard Life Foundation, April 2020, <https://www.standardlifefoundation.org.uk/our-work/coronavirus-finance-tracker2/coronavirus-finance-tracker/april>.

⁸ M. Costa Dias, A. Norris Keiller, F. Postel-Vinay and X. Xu, 'Job vacancies during the Covid-19 pandemic', Institute for Fiscal Studies, Briefing Note BN289, May 2020, <https://www.ifs.org.uk/publications/14854>.

⁹ L. Judge, 'Coping with housing costs during the coronavirus crisis', Resolution Foundation, May 2020, <https://www.resolutionfoundation.org/publications/coping-with-housing-costs-during-the-coronavirus-crisis/>.

over a long period starting from May 2018. Second, we can precisely measure both incomes and expenditures – rather than relying on survey reports, which can be subject to misreporting. In combination, this means that we have precise and high-frequency quantitative measures of both income and spending in the same data set for a constant panel of people as their situations evolve – a luxury that is very rare in research. Third, while the crisis itself may affect who responds to surveys – and therefore the results that they find – we can restrict our sample to those who were already MDB users before the crisis. These individuals are retained in the data automatically as the crisis hits (unless they decide to quit the app altogether), regardless of how the crisis affects them.

The MDB data have already been used to track this crisis in some early work by Surico et al. (2020) and Chronopoulos et al. (2020).¹⁰ We build on these papers, which largely focus on spending, by analysing trends in incomes and financial distress.

¹⁰ P. Surico, D. Känzig and S. Hacıoğlu, 'Consumption in the time of Covid-19: evidence from UK transaction data', Centre for Economic Policy Research (CEPR), Discussion Paper 14733, May 2020, <https://repec.cepr.org/repec/cpr/ceprdp/DP14733.pdf>; D. K. Chronopoulos, M. Lukas and J. O. S. Wilson, 'Consumer spending responses to the COVID-19 pandemic: an assessment of Great Britain', SSRN Working Paper 3586723, April 2020, https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3586723.

2. Data and methods

In this section, we outline the main features of our data and our methodology. Much more detail can be found in the appendices. Readers only interested in our results should skip straight to Section 3.

Data: Money Dashboard

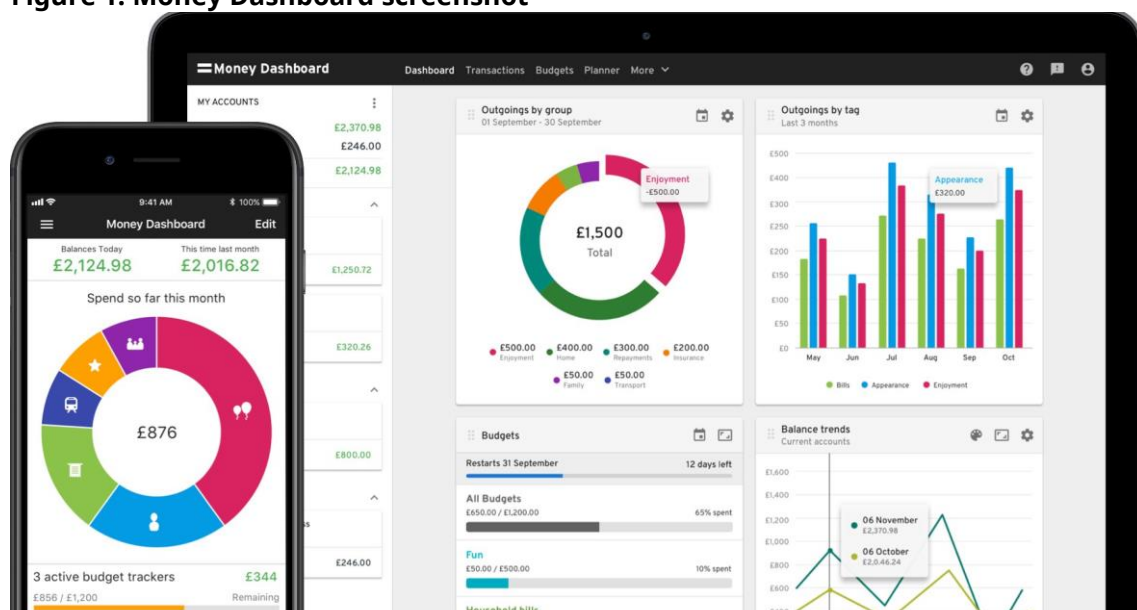
The data we use are from Money Dashboard (MDB), a free budgeting app running since 2010, accessible via computer and smartphone. When a user signs up to MDB, they provide their age, gender and postcode, and can then link in their financial accounts, including current accounts, credit cards and savings accounts. They can do this for their own accounts as well as those of a partner – and indeed they generally have a strong incentive to do so, since the point of the app is to help with budgeting and financial management.

On the eve of the COVID-19 crisis in January 2020, the app had over 100,000 registered users. An average of 300,000 transactions have been recorded daily since 1 January 2019 and there are now over 170 million transactions recorded since 1 January 2019. These data therefore provide an opportunity to study the economic impacts of the crisis on households in a very granular and quantitatively precise way, and in near real time as the situation evolves.

The app is designed to help users manage their finances and to budget accordingly. A screenshot of the app is shown in Figure 1.

When a user signs up and links in bank accounts to the app, MDB immediately downloads up to three years of past transactions from each linked-in account. Hence, for all of our sample, we have a substantial amount of baseline information, from the eve of the COVID-19 crisis all the way back to May 2018, about their financial circumstances. Going forward

Figure 1. Money Dashboard screenshot



from the date of the initial sign-up, the app receives updated transactions for each user (we describe the updating of transactions information in further detail below).

Based on the description of each transaction as it appears on bank statements (which typically describes the source/merchant of the transaction or its nature), MDB's machine learning algorithm categorises transactions into groups such as 'rent', 'salary', and 'electricity and gas'. Following MDB, we refer to this process of identifying the nature of transactions as 'tagging'. Transactions the MDB algorithm is unable to categorise are left 'untagged'. Users retain the ability to change tags and tell MDB to tag other similar transactions in the same way.

The data we exploit are at the user-transaction level. For each user, we observe their age, gender, MDB registration date and postcode sector. For each transaction, we see the date on which it occurred, the amount, whether it was a credit or debit, the merchant, when MDB received the data, the automatic tag applied by MDB and any manual tag the user might have added. Around 75% of all transactions are automatically tagged, and a further 5% are manually tagged by users.¹¹

The primary advantage of the MDB data is that they allow us to study real-time impacts of the coronavirus crisis on household finances. However, this means it is important to be clear on precisely when a transaction is loaded into MDB's system. As discussed above, when a user links in any financial account, MDB downloads up to three years of previous transactions. Subsequently, every 90 days, users must re-authorise MDB to continue to download new transactions.

A consequence of this updating procedure is that the sample size reliably covering transactions that happened several months ago is larger than the sample size that reliably covers transactions up until, say, yesterday (because many of yesterday's transactions will not yet have been updated into the app). Our analysis therefore focuses on users who fulfil two criteria: (i) they have a fully updated set of transactions by 31 May 2020; and (ii) they form part of a 'balanced panel': that is, we observe all of their transactions from May 2018 onwards. These criteria allow us to study household finances during the first two months of the COVID-19 crisis (the UK government imposed lockdown restrictions on the evening of 23 March), and to contrast and benchmark these changes to the financial circumstances for the same group of users going back almost two years before the crisis. This coverage of pre-crisis and crisis periods helps eliminate concerns that we might pick up natural changes over time, or seasonality, in household finances. In forthcoming work, we will also track outcomes in the post-lockdown recovery.

Individuals versus households

A key advantage of the MDB data is that users can link in *all* of their current accounts, credit cards and savings accounts, allowing us to see all of their transactions. Users have strong incentives to do this – the budgeting tools that MDB provide, and which are the main purpose of the app as indicated in Figure 1, are more effective if they cover all income and spending. Nevertheless, some users might link in only some accounts, or they

¹¹ Tagged income (expenditure) transactions correspond to around 50% (80%) of all incomes (expenditures).

might link in their personal accounts but not (all of) their partner's financial accounts, even if they share resources.

In Appendix A, we detail a scenario where there is an indication that we are missing an account, and we do not include these users in our analysis. For the remaining sample, we provide validating evidence that, for the most part, we are not missing accounts that we would want to pick up. For example, the distribution of users who have zero, one or two salaries matches fairly well to external data. This suggests that we are not missing a large number of unlinked partners' accounts and that the income and spending measures we describe are best thought of as capturing families' finances rather than just individuals'. Nonetheless we cannot rule out that we will miss some unlinked accounts. This is less significant for *changes* in income, earnings and financial distress (which we focus on) than it is for the *levels* of those outcomes.

We hence consider the data that we use to be best thought of as a measure of the finances of the 'nuclear family' – that is, people plus their partners. For ease, we refer to this unit as the 'household', but we note up front that this is slightly loose, as around 18% of households contain more than one family defined in that way.¹² For example, an adult who lives with their parents would, we presume, be highly unlikely to have their parents' accounts linked to their app, and so in this case it is not the finances of the entire household that we are tracking.

Sample selection

For our main analysis, we analyse the set of MDB users who:

- Have at least £200 of debits in all months, or all months but one, between May 2018 and May 2020.
- Have at least some transactions in April 2018 and June 2020 – to ensure that all transactions in the intervening months have been downloaded by MDB.
- Registered with MDB before March 2020 (to exclude those who might have signed up to MDB *because* of the COVID-19 shock).
- Are aged 22–64 for the whole May 2018 to May 2020 period.
- Do not have a business account – since debits and credits here are likely to reflect business costs and revenues and not, reliably, personal income and spending. These users will disproportionately be self-employed. UKHLS data suggest that around half of the self-employed have a business account.
- Do not appear to have a business account and do not appear to have failed to link a partner's account to the app. Appendix A discusses how we identify these users.

¹² Calculations using 2018–19 Family Resources Survey.

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- Have exactly the same set of current accounts throughout the period, to avoid including those who change banks but do not link their new account in to MDB, or appear to have changed their current account and linked in the new one.¹³

With these restrictions and covering transactions until 31 May 2020, our working sample size comprises 5,684 users in a balanced panel since May 2018. This is much smaller than, for example, the number of MDB users who have logged a transaction over the past year. The vast majority of this difference is due to our sample being made up of those for whom MDB has downloaded transactions for all of May, which is important for us here as we want an up-to-date account of the impacts of the crisis.

Measuring income, earnings and financial distress

For our main measure of income, we calculate total credits to current accounts, plus savings interest to savings accounts, excluding non-income credit transactions such as transfers from another account or refunded purchases. Appendix A details further how we identify transfers between accounts of the same user. We assume that any credit transaction tagged with an expenditure tag (for example, clothing) is a refunded purchase. We also exclude untagged transactions that are exact multiples of £100, as these are unlikely to be genuine income and may instead be a transfer from an unlinked account.¹⁴ However, our main results are not sensitive to this choice.

For our main measure of earnings, we use an algorithm we designed to pick up such credits based on regular credits users receive. The full details of the algorithm and a validation exercise are laid out in Appendix A. To be clear, this does not rely on MDB's salary tags because their engine to identify salaries has an error in March 2020 (which MDB will shortly be fixing). These salary tags cannot therefore be reliably used to understand the impacts of the crisis.

Note that, since we see earnings and incomes as they appear in people's bank accounts, they are largely net-of-tax measures. Specifically, they are net of tax taken at source through the PAYE system.

In identifying bill payments for our analysis of financial distress, we rely predominantly on the transaction tagging provided by MDB. For some classes of bill payment, this involves making use of a single tag – for instance, payments tagged as 'council tax' – while for others a number of tags are combined. In the case of mortgages, we make use of both the 'mortgage payment' tag and the 'mortgage or rent' tag, because a text search of the transaction descriptions associated with 'mortgage or rent'-tagged transactions reveals that 97% contained some mention of a mortgage, while none contained a mention of rent.

¹³ On this last point, we do not exclude users who have a current account that ceases to record transactions in the same month as when another current account begins to record them. We assume these are people who have switched banks.

¹⁴ Even though gross earnings are often rounded amounts, net earnings (which appear in bank accounts) rarely are.

Representativeness

MDB users are individuals who have chosen to use a budgeting app to manage their finances, not a random sample of the UK population. In Appendix B, we investigate the representativeness of the data along a battery of measures, including demographics, income, earnings, the number of salaries in the household, the number of bank accounts and credit cards, and pre-crisis indicators of financial distress, comparing the patterns we find in MDB with those from nationally representative data. MDB users are younger than the UK population, slightly more likely to reside in southern areas and more likely to be men. We reweight our sample to make it more representative. To do so, we assign each user a weight, based on the combination of their age (in five-year age bands) and region. The weighted sample matches the actual region and age composition of the age 22–64 population (specifically, the joint distribution of the two) as recorded in the Households Below Average Income (HBAI) 2018–19 data – the data source used for the UK’s official household income statistics. Reassuringly, after this relatively straightforward reweighting of the data, we find that they look fairly representative of the UK as a whole on many other key dimensions (see Appendix B).

When it comes to representativeness, another important advantage of these data in the context of examining the impacts of the COVID-19 crisis is that the circumstances of anyone using the app on the eve of the crisis will be automatically tracked (so long as they authorise MDB to download their data every 90 days) regardless of how the crisis affects them. This is not true of surveys of households conducted post-crisis, where it is always possible that response rates vary depending on how the crisis has affected them – for example, reflecting the stress they are under or the time they have to answer survey questions.

Seasonality, time trends and age effects

To effectively use the MDB data to understand changes in household finances due to the COVID-19 crisis, we need to distinguish them from seasonal factors and longer-term trends.

For some outcomes, there are seasonal patterns: some months have predictably high or low values. To help account for these seasonal patterns, we compare outcomes in 2019–20 with those in the same month one year earlier, and calculate the difference between the two. There are naturally going to be systematic time trends in some outcomes – for example, incomes tend to grow over time – so that outcomes in 2019–20 are persistently different from those in the same month one year before. In some cases, this can be further accentuated by the fact we are following a constant set of users over time, so these users are ageing. Some outcomes will exhibit natural ‘age effects’ – for example, incomes tend to grow with age over the working-age years of life, over and above any change that would be predicted simply from the economy-wide rate of income growth. To deal with pre-crisis time trends and age effects, we therefore also report the average annual growth rate over the nine-month pre-crisis period, May 2019 to January 2020. Absent COVID-19, we might reasonably have expected this year-on-year difference to be approximately sustained from February 2020 onwards. Much of our analysis focuses on the difference between this pre-crisis year-on-year change and the year-on-year change actually observed from February to May 2020.

To further verify that our results are not driven by time trends unrelated to COVID-19, we also conduct robustness checks known as ‘placebo’ exercises. In the placebo, we repeat the analysis as if the COVID-19 shock had happened exactly one year earlier. We use only data that had been downloaded by MDB one year ago, construct a panel of users observed between May 2017 and May 2019, and compare outcomes in 2018–19 with those one year earlier in 2017–18. In nearly all cases, outcomes in February, March, April and May in the placebo exercise do not look much different from outcomes earlier in the year, giving us confidence that the differences we see in our main analysis are driven by real crisis-related phenomena. The results from the placebo exercises are presented in Appendix C.

Finally, we reiterate that all our analysis is based on a balanced panel of MDB users who have been observed since May 2018. Hence the composition of our sample remains unchanged through our analysis of the crisis period. The sample reweighting ensures we build a picture of how the crisis has impacted the financial well-being of a more representative sample of UK households.

Data: UKHLS COVID module

We complement our analysis using MDB with an alternative survey-based data source – Understanding Society: the UK Household Longitudinal Study.¹⁵ UKHLS is a broadly representative household survey of the same individuals in the UK each year (starting in 2009) and contains detailed information on individual and household characteristics. This is a useful complement to MDB, both because it allows us to explain and explore some of the findings we document by utilising information about households that is not recorded in their bank statements (such as how many hours they are working) and because it acts as a cross-check on some key results.

The last available pre-COVID wave of the UKHLS covers 2017 to 2018. In April 2020, participants of the UKHLS were asked to complete a short online survey on the impact of the COVID-19 pandemic.¹⁶ This included questions on their pre-crisis (January/February 2020) and April 2020 labour market situation, including employment status, hours worked and earnings.¹⁷ There were 17,452 full responses to the survey, and the response rate was 46% among individuals who had been interviewed at the 2017 to 2018 wave. We use the COVID-19 survey cross-sectional weights, which are constructed by the University of Essex and adjust for unequal selection probabilities and differential non-response.¹⁸ To make our sample comparable to that of MDB, we restrict it to individuals aged 22–64 and drop any households where a self-employed member has a business account.

¹⁵ University of Essex, Institute for Social and Economic Research. (2019). *Understanding Society: Waves 1–9, 2009–2018 and Harmonised BHPS: Waves 1–18, 1991–2009*. [data collection]. 12th Edition. UK Data Service. SN: 6614, <http://doi.org/10.5255/UKDA-SN-6614-13>.

¹⁶ University of Essex, Institute for Social and Economic Research. (2020). *Understanding Society: COVID-19 Study, 2020*. [data collection]. UK Data Service. SN: 8644, <http://doi.org/10.5255/UKDA-SN-8644-1>.

¹⁷ For full details of sample design, response rates and response patterns, see Institute for Social and Economic Research, *Understanding Society COVID-19 User Guide. Version 1.0, May 2020*, http://doc.ukdataservice.ac.uk/doc/8644/mrdoc/pdf/8644_ukhls_covid19_user_guide_v1.0.pdf.

¹⁸ The COVID-module weights model response probabilities conditional on past response to wave 9 and assign zero weight to individuals who had not responded to wave 9. We are therefore implicitly providing estimates that are representative of the UK household population in 2017 to 2018.

Respondents are asked for their individual and household net (after-tax) earnings. Reported household earnings and the sum of reported individual earnings across members of a household are not always consistent. Examination of the data leads us to believe that the individual answers are more reliable, so we restrict our sample to individuals in households where all working-age members answered the pre-crisis and April 2020 earnings questions. This gives us a working sample of around 5,550 people aged 22–64. We then construct a consistent measure of household earnings by summing individual earnings within a household. We adjust the survey weights to account for non-randomness in which households provide earnings information for all individuals and all the other information (including from the 2017 to 2018 wave) required for our simulations of benefit income that we now describe.

The UKHLS COVID-19 survey asks people whether they are receiving benefits, but does not ask for the amounts received. We use TAXBEN, the IFS tax and benefit microsimulation model, to simulate benefit entitlements.¹⁹ As far as possible, we use information available in the COVID-19 survey about family structure, earnings and other relevant circumstances to simulate benefit entitlements, but for characteristics not recorded in that survey – for example, rent, which affects entitlement to support for housing costs – we use information from the 2017 to 2018 survey, updated with official indices (for example, average rent). We simply assume full take-up of benefit entitlements. We explored alternatives but it is difficult to account for incomplete take-up robustly. The obvious approach would be to only add on benefit entitlement if a household reports receiving that benefit in the UKHLS COVID-19 survey. However, unfortunately, the survey does not cover all types of benefits – for example, it does not ask whether households receive housing benefits. Furthermore, of households who report receiving a particular benefit in the survey, TAXBEN simulates about a third as not entitled to that benefit. This may be because of misreported benefit receipt or earnings in the COVID-19 survey, or because an important household characteristic has changed since 2017 to 2018 and is not asked about in the COVID-19 survey.

Our focus is on earnings and income changes by long-run net household income quintiles. We construct a long-run net household income measure by averaging reported household net income over waves 7–9 (the last three available waves prior to the onset of the crisis) of the UKHLS, following Benzeval et al. (2020).²⁰

Note that the UKHLS allows us to measure earnings and (by simulation) benefits. While these are the largest sources of households' incomes on average, and we use the shorthand of 'income' to refer to 'earnings plus benefits' in UKHLS, they are not always the only income sources. The MDB data will thus pick up sources of income not captured in the UKHLS data, such as interest income.

¹⁹ For a fuller description of TAXBEN, see C. Giles and J. McCrae, 'TAXBEN: the IFS microsimulation tax and benefit model', Institute for Fiscal Studies (IFS), Working Paper 95/19, 1995, <http://www.ifs.org.uk/publications/572> and T. Waters, 'TAXBEN: the IFS tax and benefit microsimulation model', Institute for Fiscal Studies User Guide, 2017, https://www.ifs.org.uk/uploads/publications/docs/taxben_guide.pdf.

²⁰ M. Benzeval, J. Burton, T. F. Crossley, P. Fisher, A. Jackle, H. Low and B. Read, 'Understanding Society COVID-19 Survey, April Briefing Note: the economic effects', University of Essex, Institute for Social and Economic Research (ISER), Working Paper 10/2020, April 2020, https://www.understandingsociety.ac.uk/sites/default/files/downloads/general/ukhls_briefingnote_covid_economics_final.pdf.

3. Jobs, earnings and incomes

Jobs

The MDB app data allow us to study changes in employment by identifying credit streams with repeated payments made in an identifiable regular cycle (the precise method we use is laid out in detail in Appendix A). Given that many of the most severe economic impacts of the crisis could stem from job losses, this is a natural outcome to begin with.²¹

Partly to try to mitigate the long-term effects of the labour market disruption seen over the past few months, the Coronavirus Job Retention Scheme (CJRS) was brought in. Workers who are ‘furloughed’ – who have no work to do – can be kept on by their employer and paid 80% of their February earnings (up to a cap) fully subsidised by the government, until this scheme is wound down between August and October of this year. The CJRS has not covered those who see a fall in hours or hourly wages as a result of COVID, nor those who lose their job entirely. As furloughed workers are paid in the same way as for their normal earnings, in the MDB data payments to furloughed workers look the same as salaries. Thus, in this subsection (and below when we analyse earnings), we count furloughed workers as having a job and having earnings. Our measure of ‘lost jobs’ relates to those who have not been furloughed and have seen their earnings fall to zero. The UKHLS data also count furloughed payments as earnings.

We start by analysing how the number of jobs per user has changed over time. Of course, a user might have two jobs associated with their MDB account if they are a member of a dual-earner couple, or, less frequently, if one individual works in two different jobs.

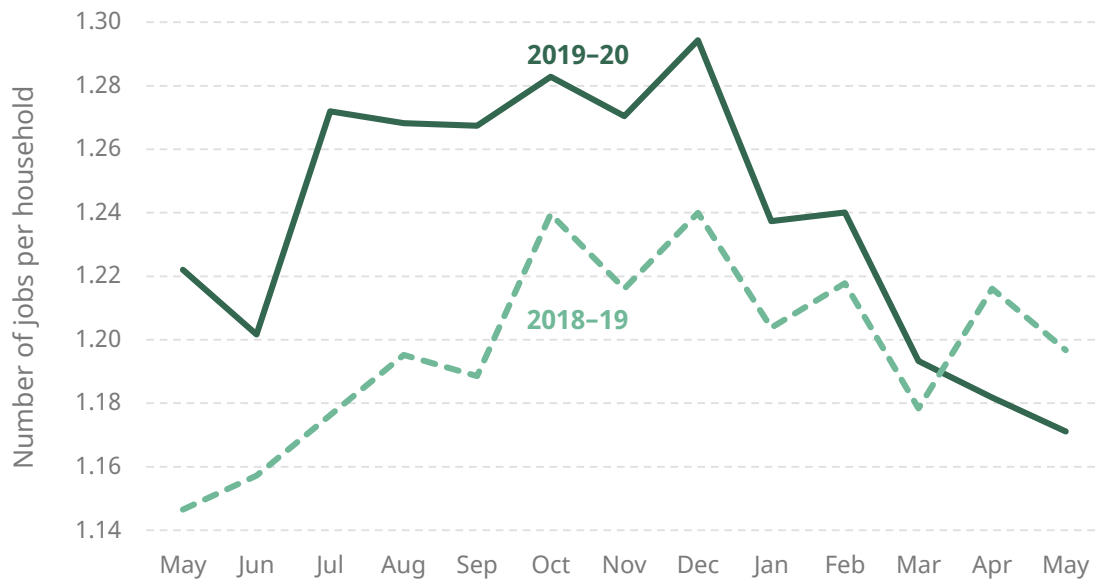
Figure 2 shows the trends in the average number of jobs per user from May 2018 onwards and then into the first few months of the crisis. As with other figures in the report, we show how the level of the outcome (in this case, jobs per user) evolved from May 2019 through to May 2020 (the solid dark green line) and, for comparison, over the same period one year earlier, from May 2018 to May 2019 (the dashed light green line).²² In 2018–19, the number of jobs per user was around 1.2. This increased steadily over 2018–19 and most of 2019–20, but reversed sharply as we entered the crisis in March 2020.

To help visualise better the impact of the crisis relative to pre-crisis trends, Figure 3 combines the data shown in Figure 2 with (on the right-hand axis) the year-on-year change in the number of jobs per user, by month, plotted by the yellow bars. These simply take the proportional difference between the two green lines in each month.

²¹ Given that the method we describe in Appendix A finds jobs by looking for payments that are regular in frequency and amount, if pay changes very substantially it could cause payments to not be picked up as salary transactions. It is therefore possible that some of the patterns we see are driven by very large cuts to earnings (but not to zero) in the crisis.

²² The end of the 2018–19 line and the start of the 2019–20 line cover the same month (May 2019). The number of jobs recorded is slightly different. This is because the algorithm used to identify salary transactions is run twice, once using data from May 2018 to May 2019 and once using data from May 2019 to May 2020. Depending when a job started, this may mean that a particular salary transaction is identified in one run of the algorithm but not the other.

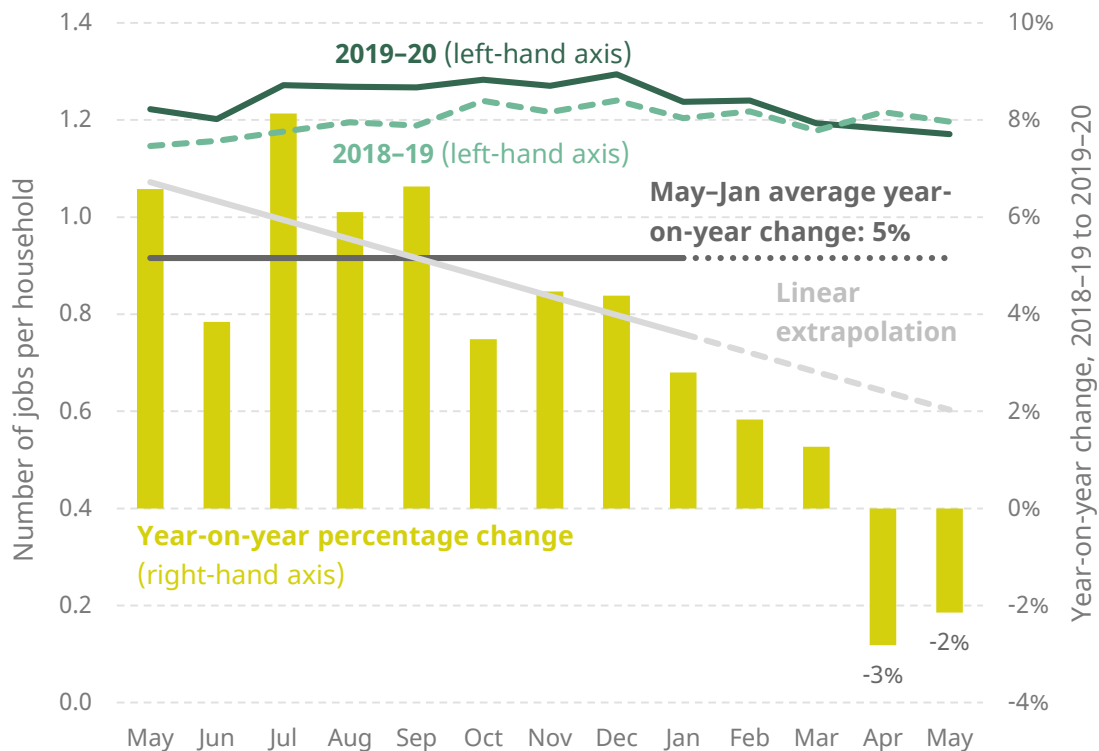
Figure 2. Number of jobs per household



Note: Transactions are classified as earnings based on the algorithm described in Appendix A. The method for determining the number of jobs for each user is also described in Appendix A.

Source: Authors' calculations using Money Dashboard data available 12 June 2020.

Figure 3. Number of jobs per household



Note: Transactions are classified as earnings based on the algorithm described in Appendix A. The method for determining the number of jobs for each user is also described in Appendix A.

Source: Authors' calculations using Money Dashboard data available 12 June 2020.

The horizontal line marks the average value of the year-on-year change over the pre-crisis period. This shows there was on average a 5% year-on-year increase in the number of jobs per user between May to January 2018–19 and May to January 2019–20. There is some evidence of a deceleration in jobs growth within the pre-crisis period. For this particular outcome, we therefore mark on a second point of comparison, which linearly extrapolates from the trend in annual jobs growth seen over the May 2019 to January 2020 period (grey line).

The year-on-year change bars show that in April and May, the number of jobs was 3% and 2% lower respectively than it was a year earlier. Compared with what would have occurred if the average pre-crisis rate of annual jobs growth of 5% had continued, this suggests the number of jobs was about 8% and 7% lower in April and May than would have been predicted in January. These numbers are comparable to the UKHLS which shows that 7% of individuals with earnings before the crisis had no earnings in April. If we assume that the slight deceleration in jobs growth pre-crisis would have continued at the same pace, the impact of the crisis on jobs looks a little smaller, but still substantial and sharp: the number of jobs in April and May was about 5% and 4% lower respectively than would have been predicted in January.

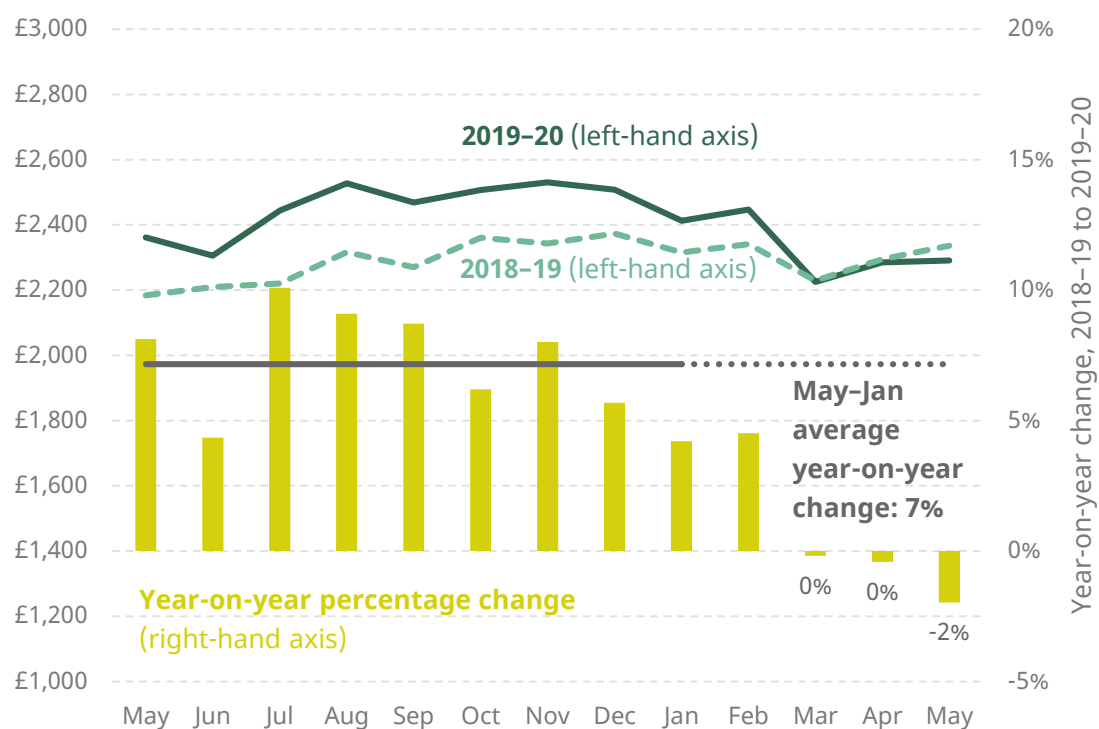
Earnings

Beyond individuals losing earnings entirely because of the crisis, others will have suffered a fall in earnings – for example, because of cuts in hours or because of being furloughed. Figure 4 shows the evolution of household earnings over time and into the crisis.²³ In this subsection, we include our entire working sample – including those with no earnings in any given period – so that we pick up the combined impacts on earned income of changes in employment and changes in earnings for those still working. We know that lower earners have been most likely to lose employment (as we document later), so showing earnings only among those with positive earnings in each period would distort the post-crisis figures and make them look misleadingly positive.

We visualise the impact using the same format as in Figure 3. We see first that median household earnings were trending up: the year-on-year increase in the pre-crisis period was 7%, as shown by the horizontal line. This is, as we would expect, higher than the kinds of numbers we are used to seeing for the rate of annual economy-wide wage growth (which, for example, the Labour Force Survey shows as running at around 1.7% in real terms over this period). The earnings growth figures here will also incorporate rises in employment (the LFS shows this to have increased by 0.6 percentage points over this period) and, importantly, the fact that we are following a constant sample of people – hence the sample ages as we follow it, with people benefitting from the earnings growth that tends to accompany additional age or experience, and in some cases from additional household earnings if they form new partnerships. What matters here is not the level of earnings growth pre-crisis, but how this deviated from the pre-crisis trend as the crisis hit.

²³ As in Figures 2 and 3, there is a slight discrepancy between the start of the 2019–20 line and the end of the 2018–19 line even though they cover the same month (May 2019). The explanation is the same as that given in the discussion of those figures.

Figure 4. Median real monthly household earnings



Note: Transactions are classified as earnings based on the algorithm described in Appendix A. The sample includes those with zero earnings.

Source: Authors' calculations using Money Dashboard data available 12 June 2020.

As the crisis took hold, we see those year-on-year increases wiped out immediately and then reversed: median earnings in March and April were back to their levels one year earlier, and by May were 2% lower than one year earlier. Relative to the median earnings level that would have been predicted just four months earlier based on pre-crisis trends, that May figure represents a huge 9% drop.

We next examine how these falls in household earnings are playing out across the earnings distribution.²⁴ To do so, we divide our sample into five quintiles based on pre-crisis household income (so measured from May 2019 to January 2020). We then show the change in median net earnings between January 2020 and May 2020 for each group.^{25 26}

²⁴ For most of our analysis, we conduct the 'placebo exercises' described in Section 2. We do not do this for the distributional analysis, because in general earnings and income distributions change from year to year and so what happened last year is not a particularly good guide to what we might have expected this year in the absence of COVID-19. Instead, we compare our results in MDB with those in an external data source (UKHLS) and show that the patterns are similar.

²⁵ As our working sample is restricted to users that we identify as having a partner and having linked in their partner's financial accounts, we interpret this as largely a measure of household earnings. Of course, in cases where a partner's account is missing, we will understate household earnings, although this will be less important for changes in earnings – which is what we focus on – than for the level of earnings.

²⁶ In Figure 5, we show the difference between earnings in January and May; the median change in the 'all' category here is therefore the median in January compared with the median in May. This is a slightly different statistic from the one that we highlight in Figure 4, which is the difference between median earnings in May and where we might have expected median earnings to be in May had the pre-crisis trend continued.

The MDB data show all income quintiles seeing a decline in household earnings between January and May 2020, with the severest falls in earnings being experienced by the lowest quintile: those households experienced a 15% fall in median household earnings (about £160 per month), with other quintiles all seeing a fall of 4–5% in their median earnings. Hence, household earnings inequality has increased during the crisis so far.

How does the distributional impact of the crisis on household earnings match up with other data sources? The only other source we are aware of that can currently provide something similar to Figure 5 is the UKHLS COVID module, conducted at the end of April. Figure 6 presents data from that source. As that is a household survey with detailed demographic information, we can properly adjust for household size and structure by ‘equivalising’ incomes (the standard practice to account for the fact that households with different numbers of adults and children need different amounts of income to achieve the same living standard), and we can rank people according to a longer-run measure of income by using the previous three waves of the UKHLS (waves 7–9). Hence, the figures are not entirely comparable and, as discussed in Section 2, both the MDB and UKHLS data sources have their own strengths and limitations (for example, UKHLS suffers from significant non-response to the COVID survey, as well as to the earnings questions, and its respondents are asked to recall their pre-crisis income retrospectively).

The magnitudes of the reported earnings losses in UKHLS are a little larger than those recorded in the MDB data. There could be various reasons for that given the differences and distinct pros and cons between the two sources (see Section 2). However, the two data sources accord with each other on the key distributional pattern, with the lowest income quintile seeing the biggest hit to its earnings during the crisis so far.

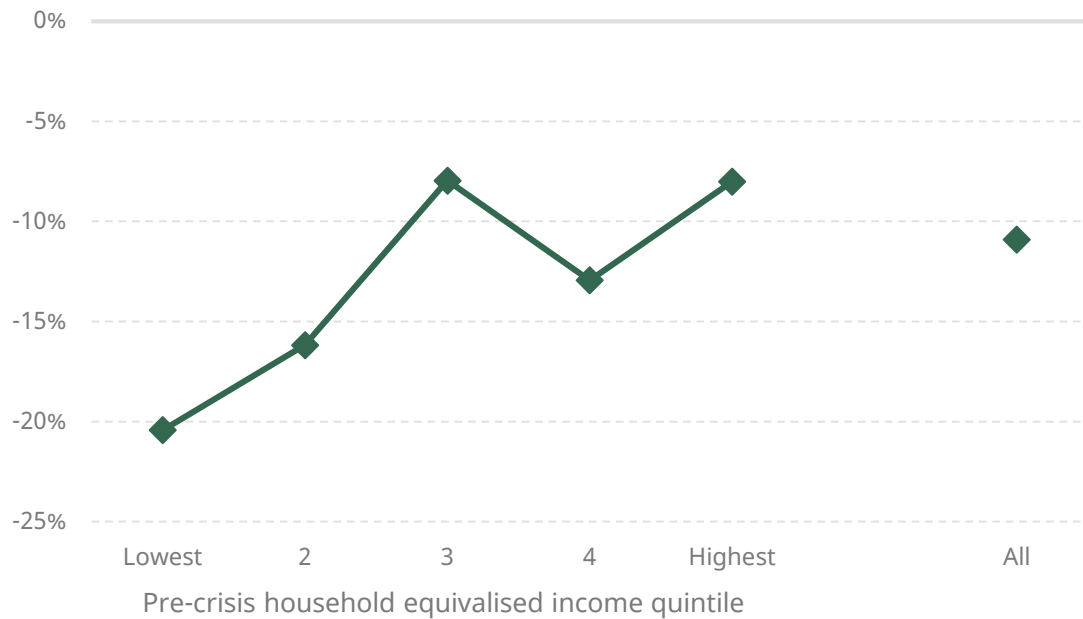
Figure 5. Change in median real household earnings between January and May 2020, by pre-crisis household income quintile



Note: Transactions are classified as earnings based on the algorithm described in Appendix A. Households are assigned to quintiles on the basis of their mean real income from May 2019 to January 2020. Household income quintile boundaries are defined using HBAI. Households falling outside of the top/bottom 5% of the HBAI household income distribution are excluded.

Source: Authors’ calculations using Money Dashboard data available 12 June 2020.

Figure 6. Comparison with UKHLS data: change in median net equivalised household earnings between January/February and April 2020, by pre-crisis household income quintile



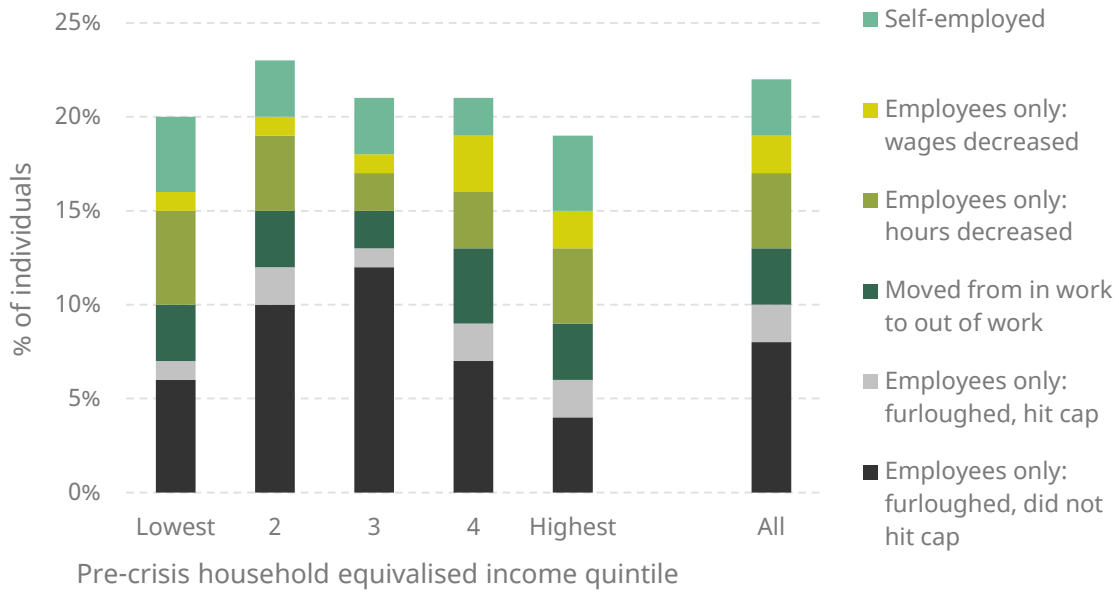
Note: Sample is individuals aged 22–64. We drop individuals in households with a self-employed member who has separate business and private accounts. Pre-crisis equivalised household income is measured averaging net equivalised household income from UKHLS waves 7–9.

Source: Understanding Society COVID-19 Study 2020 and Understanding Society (UKHLS) waves 7–9.

The UKHLS COVID module further complements the MDB data by allowing us to probe further into what is driving earnings falls. Figure 7 breaks down the set of individuals in UKHLS who saw at least a 10% fall in their net earnings between January/February 2020 and late April 2020, ranking them again by their long-run household income. (Note the difference from Figures 5 and 6: we consider individuals who saw a fall in their own earnings, rather than looking at the earnings changes across their whole household – because the reasons for earnings falls are much easier to interpret at the individual level than the household level.) All quintiles have benefitted substantially from the Coronavirus Job Retention Scheme for furloughed workers, with the lower-middle of the income distribution benefitting most – as is now well documented,²⁷ the lowest-paid workers are most likely to be furloughed, but the bottom quintile includes many people who were already not working before the crisis. People in the second quintile are most likely to have seen a fall of at least 10% in their net earnings (again, note that the first quintile contains many people who were already not in work). This is in contrast to the fact, shown in Figures 5 and 6, that the bottom quintile has on average lost the most when it comes to *household* earnings. A key reason for this is that when people in the bottom quintile lose earnings, they are much more likely to be the only source of earnings in their household. Figure 7 also shows that reductions in hours of work have been common right across the income distribution.

²⁷ For example, L. Gardiner and H. Slaughter, ‘The effects of the coronavirus crisis on workers’, Resolution Foundation, May 2020, <https://www.resolutionfoundation.org/app/uploads/2020/05/The-effect-of-the-coronavirus-crisis-on-workers.pdf>.

Figure 7. Reasons why individuals saw a fall in their net earnings of at least 10% in April 2020



Note: Sample is individuals aged 22–64. We drop individuals in households with a self-employed member who has separate business and private accounts. Pre-crisis equivalised household income is measured averaging net equivalised household income from UKHLS waves 7–9. The ‘cap’ on furlough payments is set at £2,500 per month, affecting individuals with pre-tax earnings of at least £37,500 per year.

Source: Understanding Society COVID-19 Study 2020 and Understanding Society (UKHLS) waves 7–9.

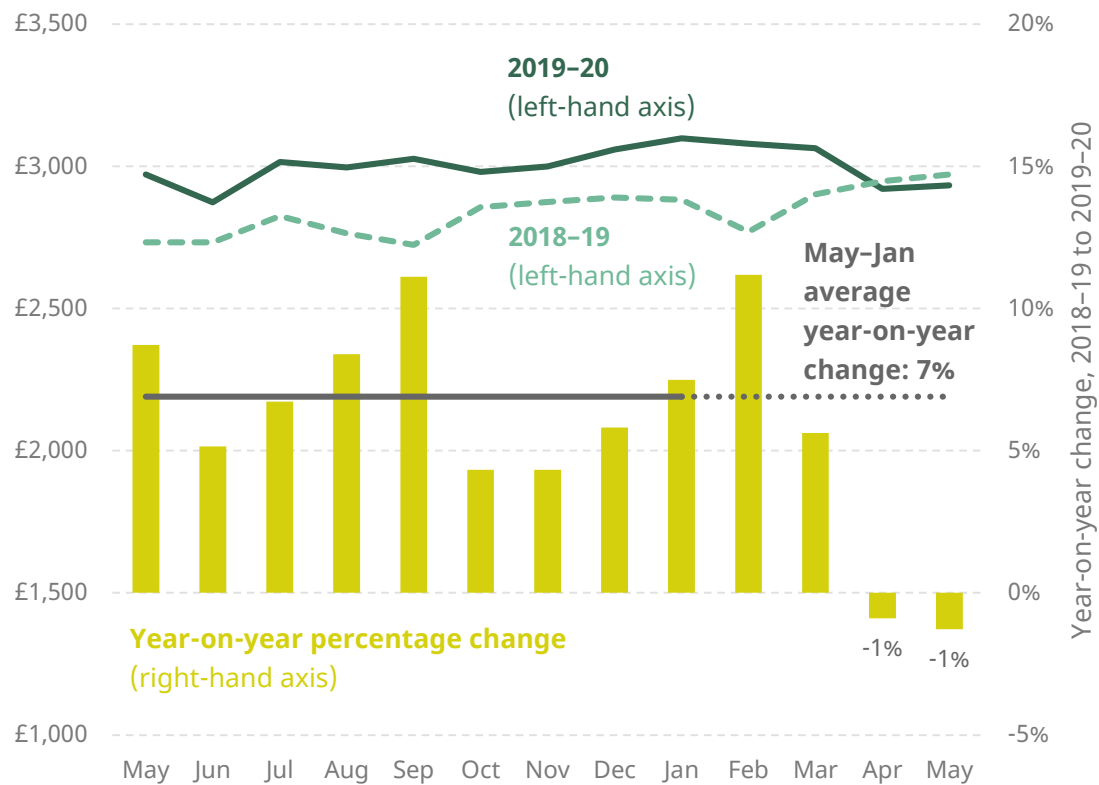
Incomes

We now turn to changes in total income. This differs from earnings primarily because of the receipt of benefits, but other sources of income (such as interest or rental income) may be important for some users. As with earnings, for the reasons discussed, we interpret this as largely a measure of household (rather than individual) income. By comparing changes in incomes with those for earnings, we start to get a sense of what impact policies are playing in reducing some of the earnings losses just documented, especially for those households that were poorest pre-crisis.

Figure 8 shows how the median real monthly income of the MDB sample has changed during the crisis. Using the same format as earlier, the left-hand axis shows how the level of median income evolved from May 2019 through to May 2020 (solid dark green) and, for comparison, over the same period one year earlier, from May 2018 to May 2019 (dashed light green). The bars show the year-on-year percentage change in median outcome; as for earnings, this averaged 7% for the pre-crisis period between May 2019 and January 2020.

Figure 8 then shows swift and large falls in median household income during the crisis. From a year-on-year increase of 7% in the pre-crisis period, we see substantial falls in income in April, largely sustained through May, which fully undo the previous year’s growth: median income in April and May was 1% lower than it had been a year earlier, which is 8% lower than we would have predicted just a few months earlier in January

Figure 8. Median real monthly household income



Note: Income is measured as total credits excluding non-income credit transactions, plus interest.

Source: Authors' calculations using Money Dashboard data available 12 June 2020.

based on the pre-crisis trend. An 8% impact on median UK monthly net income is equivalent to about £160.²⁸

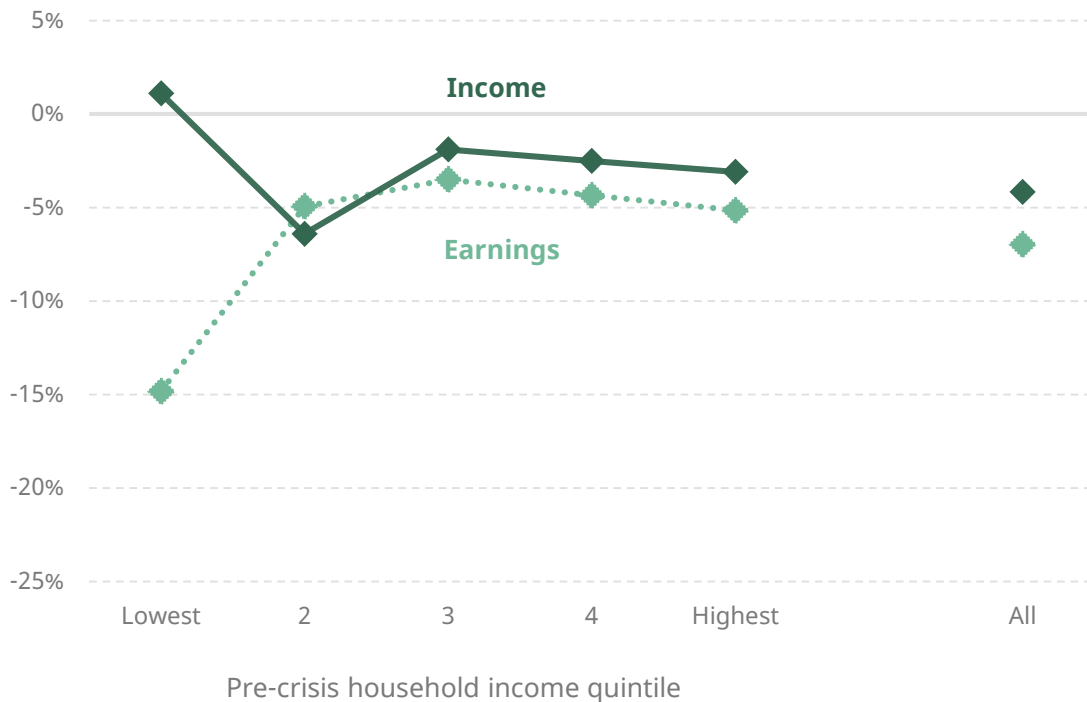
To see how declines in income have been spread across the income distribution, we again divide our sample into quintiles based on average monthly income pre-crisis, between May 2019 and January 2020. We then compare each quintile's median income in January with its median in May 2020.²⁹ To aid comparison, we also show the distribution of earnings changes already seen in Figure 5.

In stark contrast to the picture for earnings, Figure 9 shows that, on average, the lowest income quintile saw very little change in its income between January and May and did not fall any further behind the incomes of higher-income households. The MDB data show the largest falls in income in the second quintile (a pattern which, as we show below, is replicated in the UKHLS data). For the third quintile and above, median incomes fell by 2–3% – a little more modest than the falls in earnings they experienced, but the differences

²⁸ As documented in Appendix B, levels of income in the Money Dashboard data are somewhat above levels of income suggested by more fully nationally representative survey data. The £160 figure is derived by applying an 8% loss to the median level of (unequalised) net family income among 22- to 64-year-olds in the broadly nationally representative Households Below Average Income data set, which is £2,080 per month.

²⁹ In Figure 9, we show the difference between incomes in January and May; the median change in the 'all' category here is therefore the median in January compared with the median in May. This is a slightly different statistic from the one that we highlight in Figure 8, which is the difference between median income in May and where we might have expected median income to be in May had the pre-crisis trend continued.

Figure 9. Change in median real household income and earnings between January and May 2020, by pre-crisis household income quintile



Note: Households are assigned to quintiles on the basis of their mean real income from May 2019 to January 2020. Household income quintile boundaries are defined using HBAI. Households falling outside of the top/bottom 5% of the HBAI household income distribution are excluded.

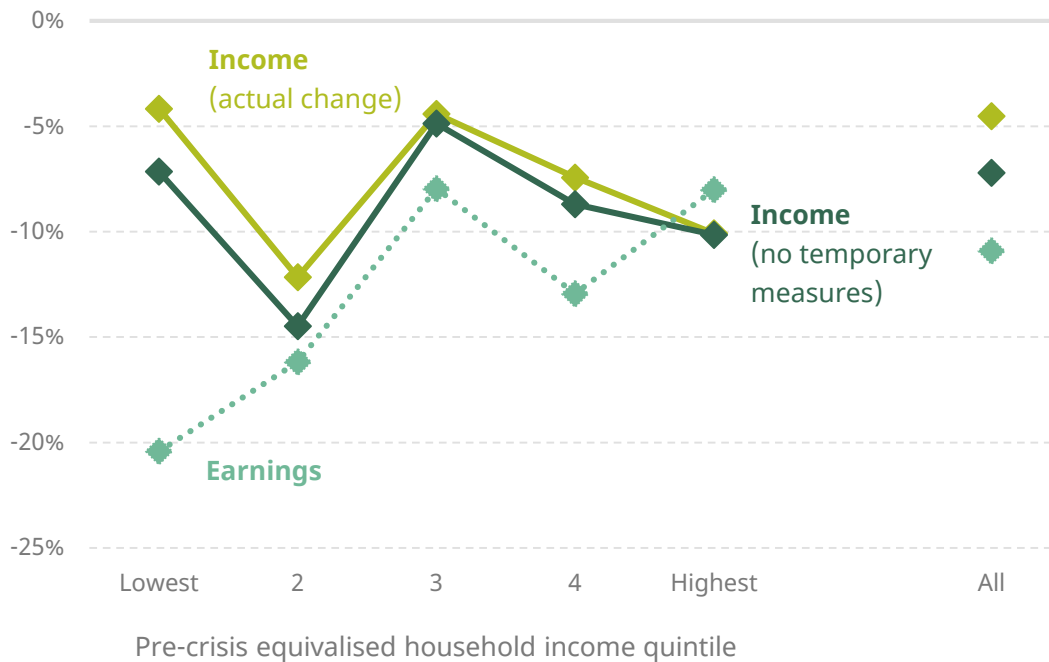
Source: Authors' calculations using Money Dashboard data available 12 June 2020.

between earnings and income changes for the middle and upper parts of the distribution are much smaller than those at the bottom.

This large contrast between changes in earnings and changes in incomes, particularly at the bottom of the income distribution, reflects the fact that income includes more than earnings. Specifically, and most importantly, it also includes benefit payments, which are there to cushion falls in earnings and which also increased significantly in generosity in April, in response to the crisis. It is worth re-emphasising that furlough payments are counted as earnings in the MDB and UKHLS data, and not as benefits – while the Coronavirus Job Retention Scheme no doubt stopped earnings falling further, it does not contribute to the difference between earnings and income shown in this analysis.

It is again instructive to compare these distributional impacts on household income with those derived from the UKHLS COVID module, using equivalised household income quintiles as we did for earnings in Figure 6. As well as the distribution of earnings changes from that figure, we add simulated benefit income (see Section 2) to household earnings in the module, and look at changes in the resulting household income. (Note that there are other income sources besides income and earnings, which are not captured in the UKHLS survey but would be captured in MDB. We continue with the shorthand 'household income' for brevity, but this is a source of non-comparability between the two data sources.) We also calculate what benefit entitlements (and therefore incomes) would have been without the temporary expansions to universal credit, housing benefit and working tax credit announced in March.

Figure 10. Comparison with UKHLS data: change in median net equivalised household income and earnings between January/February and April 2020, by pre-crisis household income quintile



Note: Sample is individuals aged 22–64. We drop individuals in households with a self-employed member who has separate business and private accounts. Pre-crisis equivalised household income is measured averaging net equivalised household income from UKHLS waves 7–9. We simulate benefit entitlements using TAXBEN, the IFS tax and benefit microsimulation model, and assume full take-up. ‘Income (no temporary measures)’ shows what income changes would have been in the absence of the temporary expansions to the benefit system announced in March, specifically to universal credit, housing benefit and working tax credit.

Source: Understanding Society COVID-19 Study 2020 and Understanding Society (UKHLS) waves 7–9.

As with earnings, the magnitudes of the income changes recorded in the UKHLS are larger than in MDB, but the key conclusions we drew above are robust across both data sets: the benefits system significantly flattens the distributional picture, preventing what would otherwise have been the bottom quintile falling significantly further behind than it was just before the crisis. One advantage of the UKHLS data is that we can use them to simulate what benefit entitlements would have been without the temporary benefit measures as well. Figure 10 shows that these measures served to increase the incomes of the bottom two quintiles by 2–3%, while having little effect on those further up the income distribution. Thus, these measures made a small (on average) but meaningful difference to containing inequality during the crisis.

In summary, the big takeaway from both the MDB and UKHLS analyses is that the immediate distributional impact of the crisis has been very different on an earnings and income basis. This makes sense, given the role of the benefits system. It also accords with more qualitative data: survey results summarised by Brewer and Gardiner (2020) found that reports of reduced incomes are much more evenly spread across income groups than reports of reduced earnings.³⁰ It shows how, for all its limitations and imperfections, the

³⁰ M. Brewer and L. Gardiner, ‘Return to spender’, Resolution Foundation Briefing, June 2020, <https://www.resolutionfoundation.org/app/uploads/2020/06/Return-to-spender.pdf>.

benefits system is absolutely crucial in containing inequality and financial hardship, and none more so than in times like these.

Nonetheless, this does not detract from the potentially severe long-term consequences of the earnings losses at the bottom, even if the benefits system is currently doing a lot to paper over the cracks – not least because the increases in benefits announced for the current year are only due to remain in place until next April.³¹ It is also worth noting that many low-income households will have seen much larger falls in income, in some cases because they are ineligible for benefits. Moreover, some of the earnings losses follow from job losses, as described in Figure 3, which will ultimately only be recovered if individuals are able to get back into work.

Taking everything into consideration, the financial situations of low- and high-income households may well be evolving quite differently over the crisis. On the other side of the balance sheet, low-income households are likely to struggle more to cut back spending if they do suffer falls in incomes – given that more of their spending goes on necessities – while those on higher incomes might find themselves spending less fairly automatically as a result of the prohibition of many recreational activities.³² Indeed, those on higher incomes have been more likely to report that they are now spending less.³³

The role of benefits, and the evolution of spending and financial balances, will be explored in more detail in further work.

Jobs, earnings and incomes: the timing of effects

Bringing together the analysis of outcomes so far, we utilise the richness of the MDB data to contrast more succinctly the dynamics of how jobs, earnings and incomes have been affected by the crisis.

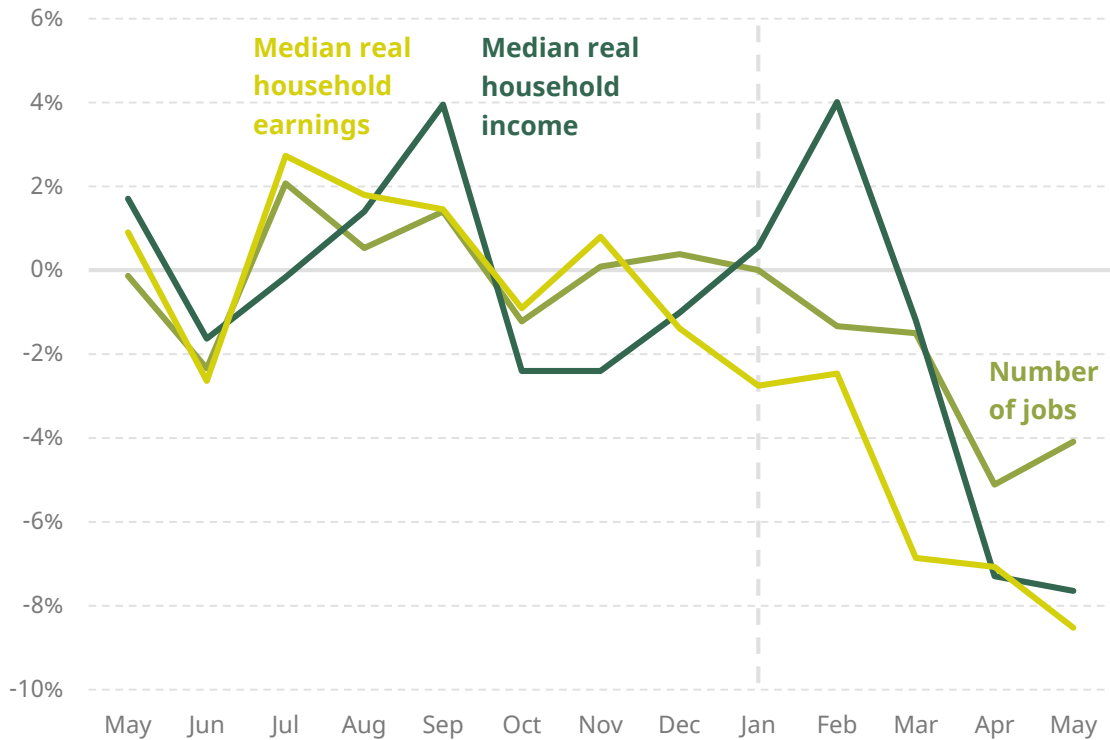
Figure 11 contains one series for each of the key outcomes we have explored so far: the number of jobs, median earnings and median incomes shown in Figures 3, 4 and 8 respectively. In each case, we take the information from those figures that gives us the best estimate of the impact of the crisis from February 2020 onwards: that is, the difference between what we observe and what we would have predicted in January 2020 based on the continuation of the trends up to that point. For jobs, this is the difference between the outcome observed in that month and the ‘linear extrapolation’ line marked on Figure 3 – accounting for the fact that jobs growth already appeared to be decelerating a little before the crisis struck (see the discussion around Figure 3). For earnings and incomes, the rate of change pre-crisis appeared to be stable, and so the plot on Figure 11 is based simply on the difference between the horizontal lines marked on Figures 4 and 8 and the actual year-on-year growth in the outcome observed each month. Recall these

³¹ In addition, many households that lose work will be exempt from the benefits cap for the first nine months, but some of these (in particular those with high rents and/or several children) will be affected by the cap after that if they have not found work again.

³² R. Crawford, A. Davenport, R. Joyce and P. Levell, ‘Household spending and coronavirus’, IFS Briefing Note 279, April 2020, <https://www.ifs.org.uk/publications/14795>.

³³ M. Brewer and L. Gardiner, ‘Return to spender’, Resolution Foundation Briefing, June 2020, <https://www.resolutionfoundation.org/app/uploads/2020/06/Return-to-spender.pdf>.

Figure 11. Deviations from pre-February-2020 trend in jobs per household, median household earnings and median household income



Note: For jobs per household, the pre-February-2020 trend is defined as the 'linear extrapolation' line marked on Figure 3. For median earnings and median income, the pre-February-2020 trend is defined as the horizontal line on Figures 4 and 8 respectively. The figures above are the monthly percentage deviation from the levels of the outcome that we would have seen had those trends continued.

Source: Authors' calculations using Money Dashboard data available 12 June 2020.

results are all derived from a constant sample of people observed every month, so they do not reflect changes in sample composition.

The figure shows two key things. First, there were precipitous declines in all three outcomes after the crisis hit. By May 2020, compared with what we would have predicted just four months earlier in January based on the trends observed up to that point, the average number of jobs per household was 4% lower, median household earnings were 9% lower and median household income was 8% lower. Second, there is little or no sign of any bounceback in these outcomes in May compared with April.

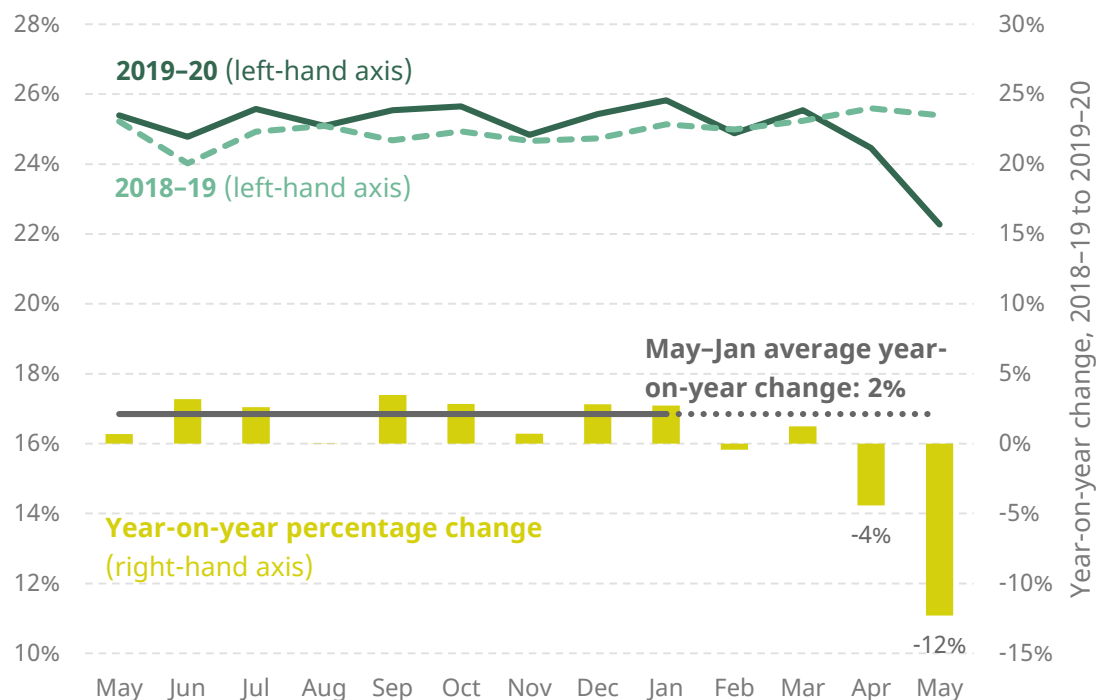
4. Non-payment of bills

Given these dramatic changes in incomes during the crisis period, it would not be surprising if it were creating substantial financial distress. In this section, we use non-payment of bills as an indicator of financial distress.³⁴ Specifically, we look at the payment of mortgages, rent, council tax and utility bills (electricity, gas, broadband).

For this analysis, we are dependent upon the tags that MDB creates. It is likely that some bills are not tagged by MDB. For example, a direct debit to a private landlord would, in many cases, be indistinguishable from a bank transfer to another person, and difficult for MDB to distinguish specifically as rent. MDB would be most likely to identify rental payments to social landlords or institutional landlords. It is possible that users with tagged bills have not been affected by COVID-19 in the same way as those with untagged bills: our analysis can only speak to the experience of the former.

Housing costs are the largest bills that people typically face, so we start with those. Figure 12 shows the share of users with a tagged mortgage payment over time. There is a slight increase between 2018–19 and 2019–20, perhaps a consequence of a small fraction of users in our balanced panel becoming owner-occupiers as they age. However, by April 2020, 4% fewer mortgage payments were being made than during the same month in the previous year, and this rose substantially further to 12% in May – a decline of 14% relative to what we would have predicted based on the continuation of the pre-crisis trend.

Figure 12. Share of users making mortgage payments



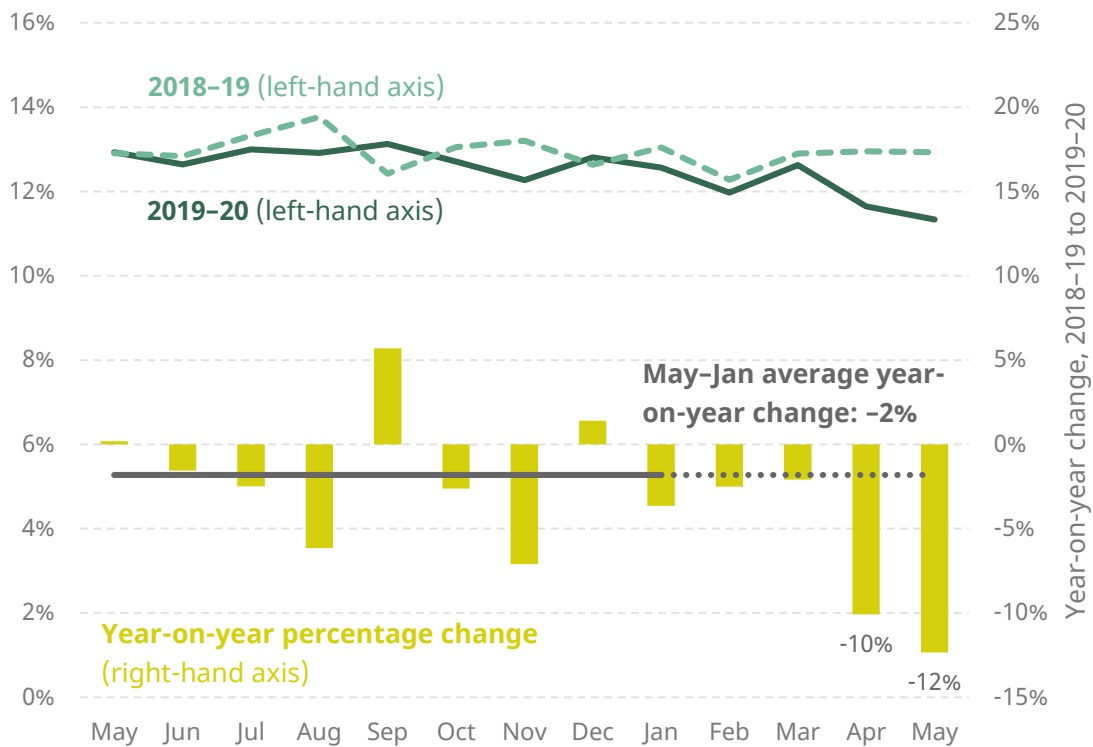
Source: Authors' calculations using Money Dashboard data available 12 June 2020.

³⁴ Another natural indicator for distress would be use of an overdraft. While we cannot see the balance of an account, we can usually identify overdraft usage because it triggers an overdraft charge transaction. However, many banks have relaxed their rules on overdraft charges since the COVID-19 crisis, so non-payment of such charges does not indicate non-use of an overdraft.

Non-payment of a mortgage does not necessarily indicate going into arrears: in many cases, it is likely the consequence of the increasingly easily available mortgage holidays. But even with a mortgage holiday, non-payment of mortgages represents an increase in the amount of outstanding debt that households have relative to what they would have had absent COVID-19 (the mortgage capital must still be repaid, and will accrue interest). Once mortgage holidays end, there is a clear risk that financial distress will accelerate, if incomes have not recovered in time.

Figure 13 investigates rental payments. In the months leading up to the crisis, the number of rental payments was very similar to a year earlier. From April though, as with mortgage payments, we see a clear decline. This was more rapid for rents than for mortgages, with 10% fewer payments in April 2020 than a year earlier. This rose slightly further to 12% by May – about 11% fewer rental payments that month than we would have expected based on pre-crisis trends. It is possible in some cases that this is a consequence of negotiated rent holidays. But holidays on rental payments are still likely to be indicators of financial distress, and a survey summarised by Judge (2020) found that only 1–2% of renters have received such a holiday.³⁵ Hence, the high prevalence of missed rental payments in the MDB data is suggestive of substantial levels of rental arrears building up, much of which has not been with the consent of landlords.

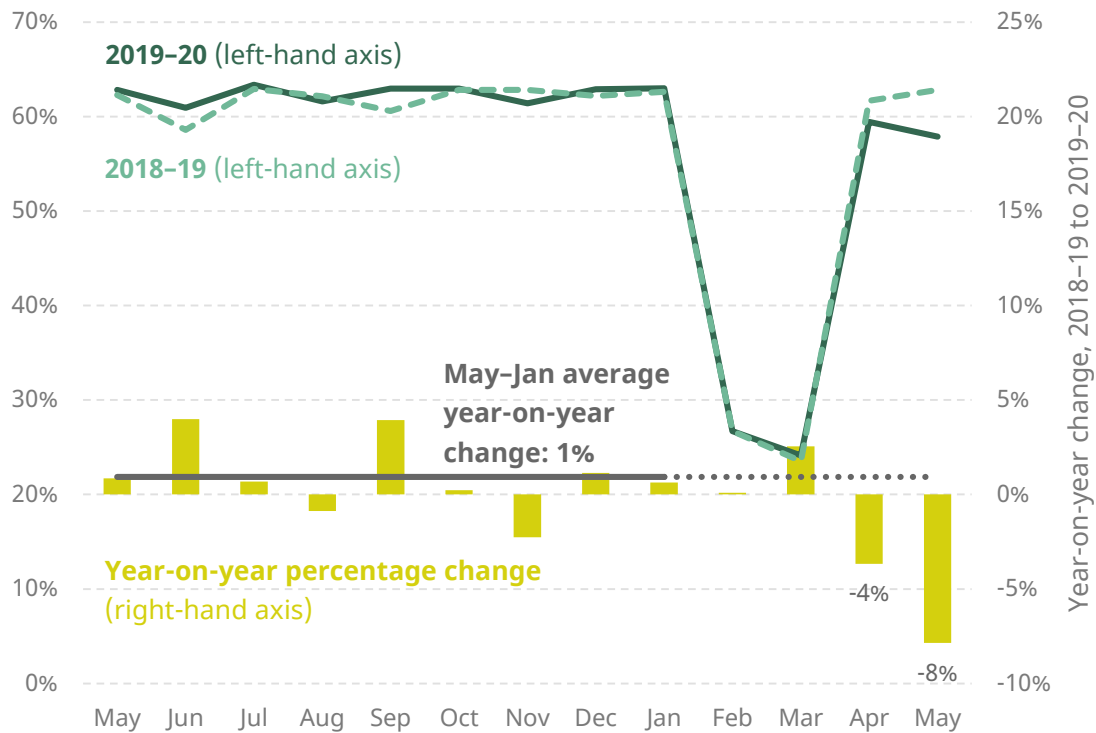
Figure 13. Share of users making rental payments



Source: Authors' calculations using Money Dashboard data available 12 June 2020.

³⁵ L. Judge, 'Coping with housing costs during the coronavirus crisis', Resolution Foundation, May 2020, <https://www.resolutionfoundation.org/publications/coping-with-housing-costs-during-the-coronavirus-crisis/>.

Figure 14. Share of users making council tax payments



Note: Council tax is generally paid each month from April to January. Those in arrears or with a special arrangement with the council may pay in February or March.

Source: Authors' calculations using Money Dashboard data available 12 June 2020.

Figure 14 shows the fraction of users making council tax payments.³⁶ Most households pay council tax for ten months of the year, between April and January, with no payments in February or March – something that is evident in the MDB data. Before February 2020, the number of users paying council tax was very steady and almost identical to the same month one year earlier. But in April 2020 the number of payees was 4% lower than a year earlier, and by May it was 8% lower – or 9% lower than we would have predicted based on the pre-crisis trend. Again, this will at least in part be the result of council tax holidays that have been permitted by councils.³⁷ But looking forward this still represents additional debt that will be hanging over those households coming out of the crisis. It will also, of course, have implications for the financing of the services and activities of local government, which are already under strain during the crisis because of losses in revenue streams from other sources as well.³⁸

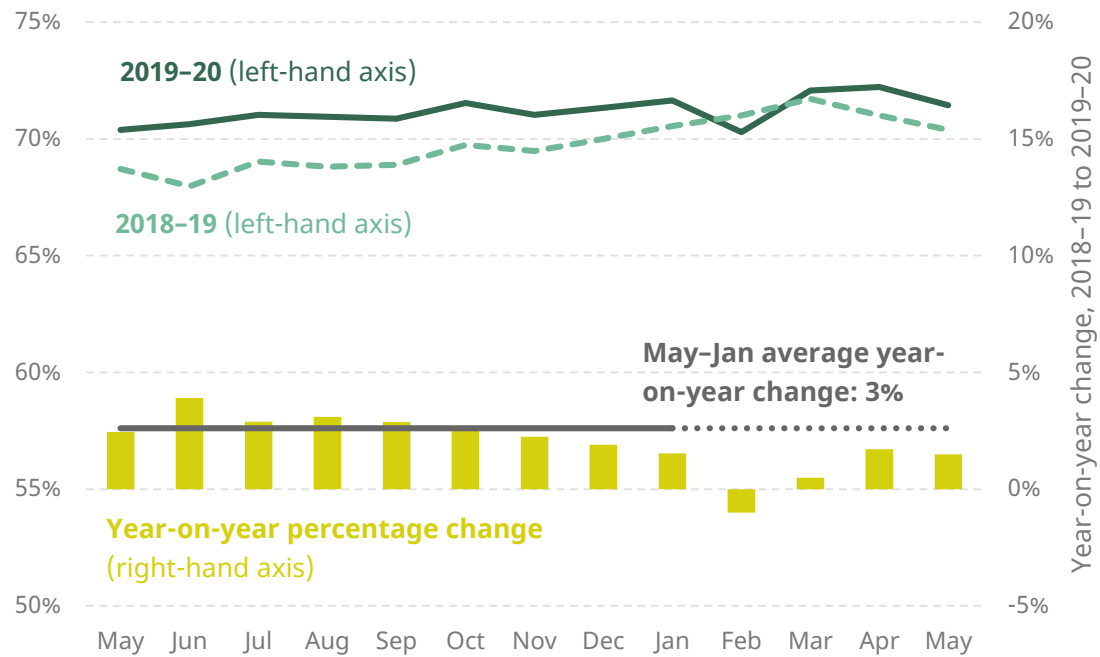
Finally, in Figure 15, we analyse financial distress in terms of the non-payment of utility bills. Specifically, we analyse the number of users paying *any* utility bill (electricity, gas or broadband) each month. While the number of users with utility bill payments in 2019–20 is very stable between May 2019 and January 2020, there was a small increase (roughly 3%)

³⁶ User accounts based in Northern Ireland are excluded from the balanced panel sample for the analysis of council tax payments because the requirement to make such payments has been delayed until June 2020 in that country.

³⁷ <https://www.bbc.co.uk/news/uk-england-52186175>.

³⁸ <https://www.bbc.co.uk/news/uk-53069772>.

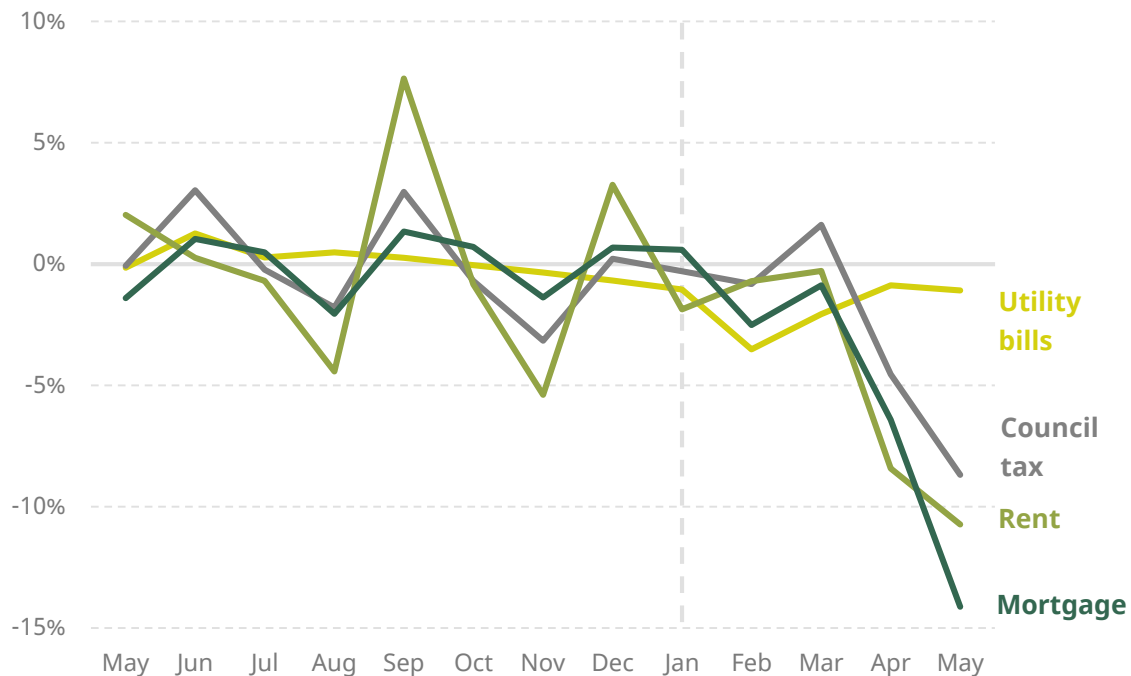
Figure 15. Share of users making utility bill payments



Note: A user is classified as making a utility bill payment if they have any payment for gas, electricity or broadband.

Source: Authors' calculations using Money Dashboard data available 12 June 2020.

Figure 16. Percentage deviation from pre-crisis trend in the share of households making bill payments



Note: For each type of bill payment, the pre-crisis trend is defined as the average monthly year-on-year percentage change in the share of bill payers between May 2019 and January 2020. The figures shown are the percentage deviation from this trend level.

Source: Authors' calculations using Money Dashboard data available 12 June 2020.

over 2018–19. This makes the year-on-year differences a little harder to interpret. Regardless of this, there does appear to be a small drop-off in payments in recent months of the crisis, but the magnitudes here are considerably smaller than for mortgages, rents and council tax payments.

Figure 16 brings together all four classes of bill payment discussed above and compares the degree to which the change in the number of bill payers between 2018–19 and 2019–20 differs over the course of the year. For each month, Figure 16 compares the number of bills paid with what we would have expected had the pre-crisis trend (between May 2019 and January 2020) continued.

Rent, mortgage and council tax payments can all be seen to have reduced sharply from April, with utility bill payments less affected on average. There is also a common theme, especially for mortgages and council tax, of a further drop in bill payments in May compared with April – suggesting a potentially concerning (though, given the lack of a recovery in income in May, unsurprising) deterioration in households’ ability to weather the storm as it persists.

By May, the number of people making mortgage, rental and council tax payments was, respectively, 14%, 11% and 9% below what we would have predicted in January based on pre-crisis trends. Overall, changes in utility bill payments have been much more muted, though we show below that this masks more change if we focus specifically on low-income households. To give a sense of scale for the amount of additional debt that this means households are carrying forward, if we take those who paid a particular bill in January but did not pay that bill in May, the average bill amount in January was £1,660 for mortgages, £650 for rent, £170 for council tax and £139 for utilities.

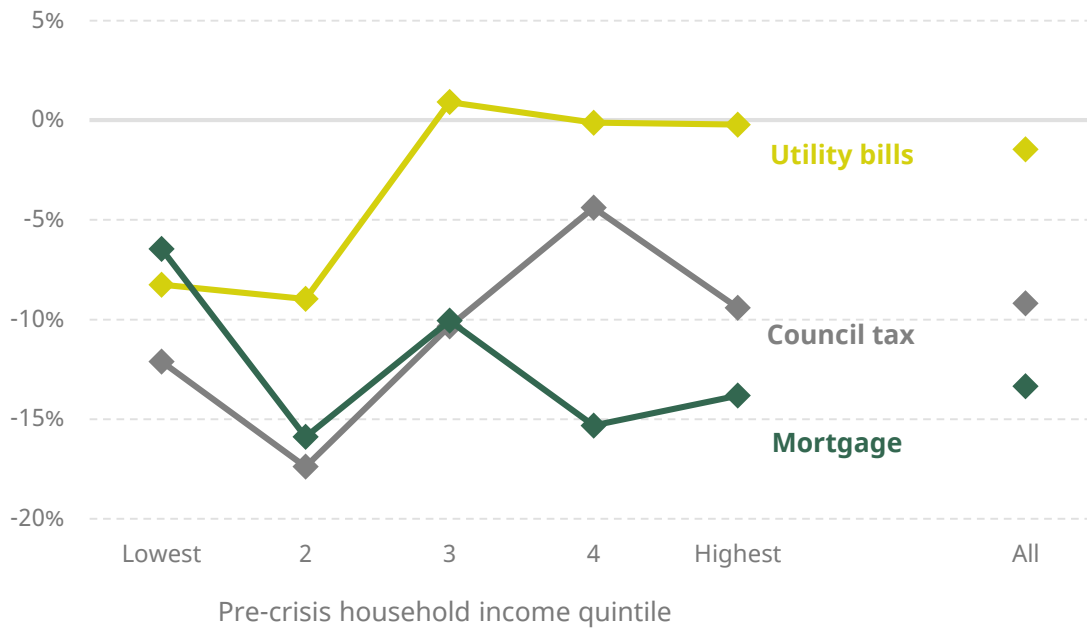
Figure 17 shows how the number of bill payments (specifically, deviations from the pre-crisis trend) had changed across the (pre-crisis) income distribution by May 2020.³⁹ While the number of utility payments being made shows few clear signs of decline across the population as a whole (see Figure 15), we do see much clearer effects amongst the 40% of households with the lowest pre-crisis incomes. For these households, the number of utility bill payments made in May 2020 was 8–9% below the level that would have been expected had the pre-crisis trend continued. Council tax shows a similar, if somewhat less stark pattern, with the decline more evident in lower-income households.

Non-payment of mortgage bills has been much more widespread across the income distribution of mortgages. This may be because better-off households want to take advantage of the extra liquidity that widely available mortgage holidays have provided.

Notably, lower-income households are seeing greater rates of non-payments of bills despite (as shown in Section 3) being, on average, relatively shielded from income falls. It is likely that part of what is going on is that the averages are masking the fact that some households – including poorer ones – are seeing very large declines in income, and poorer households are less able to cope with this.

³⁹ Due to the relatively small sample of users who have tagged rental payments (see Section 2), we do not include rent in this analysis.

Figure 17. May percentage deviation from pre-crisis trend in the share of households making bill payments, by pre-crisis income quintile



Note: For each type of bill payment, the pre-crisis trend is defined as the average monthly year-on-year percentage change in the share of bill payers between May 2019 and January 2020. The figures shown are the percentage deviation from this trend level in May 2020. Households are assigned to quintiles on the basis of their mean real income from May 2019 to January 2020. Household income quintile boundaries are defined using HBAI. Households falling outside of the top/bottom 5% of the HBAI household income distribution are excluded.

Source: Authors' calculations using Money Dashboard data available 12 June 2020.

5. Conclusion

In this report, we have used novel real-time data from a budgeting app that combines bank account data from users' linked accounts, to track in fine detail the early impacts of the COVID-19 crisis on jobs, earnings, incomes and financial distress. We have combined this with rich household survey data collected before and after the crisis struck to verify and help explain the key patterns.

We have documented not only that jobs, earnings and incomes were hit very hard just after the lockdown, but also that there were few signs of recovery in these outcomes in May. Of additional concern is the fact that signs of financial distress, in terms of non-payment of bills, increased sharply after lockdown and then increased further in May – suggesting that some households are increasingly struggling to make ends meet as the hit to incomes persists. In many cases, those missed bills of course represent crucial means for households of coping with the economic shock in the short term, and will be sanctioned by, for example, mortgage holidays. This serves to highlight the scale of financial vulnerability (in terms of additional debt) being carried forward – and hence the importance of as speedy a recovery in incomes as can be achieved compatibly with public health requirements.

The labour market disruption caused by the pandemic has affected the earnings of the lowest-income households severely and by more (proportionally) than for other households. If we look at total incomes rather than earnings, however, we see the crucial importance of the insurance provided by the benefits system: changes in income have so far been much more evenly spread than changes in earnings. That is, in the short term, a real achievement. It highlights how important benefits design will continue to be, not least while the current crisis persists.

But this does not detract from the fact that, over the longer term, there are still good reasons to worry most about the economic legacy of this crisis on the worst off. The long-run effects of all the career disruption occurring at the bottom could be serious, even while benefits help to paper over the short-run impact of that on their incomes. The role of benefits has been boosted by some temporary increases, due to expire in April 2021.

The legacy of the crisis on the balance sheets of different households may be quite different from the short-term impacts on incomes: those on higher incomes who see falls in income are more likely to find it relatively straightforward to cut back spending too, and indeed this may be quite mechanical, given the high amounts those households often spend on recreational activities that are currently not possible. Having that spending restricted hurts them too, of course – but it represents forced saving that will increase their spending power later.

Our future work will examine precisely these issues, looking more closely at the role of income protection policies during the crisis and at the evolution of spending and financial balances. We will do so in terms of how these were impacted even before lockdown policies were introduced in March, how they have evolved alongside the dramatic changes in jobs, earnings, incomes and financial distress we have documented here, and how they are affected now, as the government takes tentative steps to start easing lockdown measures.

Appendix A. Data appendix

We set out the methods we use to infer additional information about users and about transactions in the MDB data. Where relevant, we point out the kind of measurement errors that could be induced and so that need to be borne in mind when interpreting the results.

Business accounts

The first key split we identify is between accounts used for businesses and those used for personal use by households. This distinction is motivated by a concern about artificially inflating the incomes of the self-employed by mistaking their revenues for profits and their costs for consumption. Moreover, identifying business accounts allows us to separately analyse the way that the self-employed as a group are impacted by the COVID shock.

We use several indicators to identify accounts used for business purposes.

- **Business expenses:** Accounts where more than 20% of debits are tagged as 'business expenses' over the course of a year.
- **Tax bunching:** The tax-minimising strategy for withdrawing profits from an owner-managed firm is to pay a salary equal to the primary threshold in the National Insurance system and then to make any further withdrawals as dividends. We identify accounts from which debits tagged as either salary, transfers, business expenses or 'no tag' are made that are within a pound of the (monthly) primary threshold.
- **Account name:** Bank customers can register an 'account name' with their bank. We use an MDB-supplied list of accounts with a name including the words 'company', 'business' or 'limited' (or derivations).
- **Business account type:** We use an MDB-supplied list of bank accounts that are explicitly registered as a business account with the bank.
- **HMRC VAT transaction:** We use an MDB-supplied list of accounts that include a transaction where the merchant is HMRC and the transaction description includes the word 'VAT'.

Any MDB user with at least one of these flags on any of their accounts is then designated a business account.

Children

We use two approaches to identify households with children. These can further help identify whether a partner's accounts have been linked into the MDB account.

Child benefit (number of children)

Child benefit payments can be identified using the 'benefits' tag and the statutory child benefit amount. These then give us the number of children for whom the user receives child benefit.

Around 90% of those listed as the registered claimant of child benefit are women.¹ We therefore take instances of male MDB users receiving child benefit as a strong indication that the accounts of a partner have been linked to the MDB account.

Child-associated spending

Another potential indicator of a user having children is spending on child-specific items. We identify a user as likely to have a child where *either* of the following criteria is satisfied:

- the user has three or more transactions in a given year that are tagged as one of the following: 'child clothes', 'child toys, clubs, or other', 'children other', 'children's club fees', 'toys' or 'child, every day or childcare'; or
- the user has at least *one* transaction in a given year tagged as either 'childcare fees', 'nursery fees' or 'school fees'.

If a user has child-specific spending but does *not* receive child benefit, that is suggestive of not linking in a partner's accounts (except for those who have gross earnings above £60,000, who have no incentive to claim child benefit since they have to pay it back with higher taxes). We exclude from our analysis users who earn under £60,000, have child-associated spending and do not receive child benefit. This should largely be users who have not linked in a partner's account, but will also include the separated partners of lone parents, who might still be spending on their children, not receive child benefit and yet have no 'missing' partner's account in the MDB app.

Number of jobs per user

Within a given month, a pair of a user's salaries are deemed to be part of the same salary 'stream' if they are:

- paid exactly 7, 14 or 28 days apart from one another, or the working day following if that date is a bank holiday; and
- not more than 20% different from one another in amount.

The intuition is that two salary transactions that are exactly one, two or four weeks away from one another and of a similar amount are likely to arise from the same job; and conversely that a pair of salaries that do not meet these conditions are likely to be from different jobs. We use this method to identify the number of jobs per user.

There will be some measurement error using this approach. For example, users who only have one job but have very irregular incomes may appear to have two (or more) salary streams; and couples who happen to get paid at the same time (perhaps because they

¹ <https://www.gov.uk/government/statistics/child-benefit-statistics-annual-release-2019>.

work for the same firm) and are in similarly paid roles may appear to only have one stream.

Untagged transfers

Many transfers from one account to another (for example, from a savings to a current account) are tagged as transfers. We identify additional transfers that are not tagged as such in the following way. Among transactions over £50, we search for transaction *pairs* satisfying *all* the following criteria:

- the two transactions are for the *exact* same amount;
- one transaction is a credit and the other a debit;
- the transactions are associated with the same user, but different accounts;
- the transactions occurred no more than two days apart; and
- each pair is unique: any transaction assigned to a pairing cannot be assigned to another.

We consider these pairs highly likely to be transfers. We do not include transactions of £50 or under as the tendency for transactions to bunch around round numbers increases the likelihood that we identify pairs of transactions that happen to meet the above conditions but in fact are not transfers. We restrict to transactions that are no more than two days apart based on an examination of tagged transfers.² This approach will miss transfers to or from an unlinked account (though note that these are still likely to be *tagged* as transfers).

Salaries

MDB has its own algorithm to tag credits as salaries. Unfortunately, this algorithm has an error from March 2020, causing too few salaries to be tagged since then (MDB is working on fixing this error in the near future). Thus, throughout this report, we use our own algorithm to identify salary payments. We have confirmed in correspondence with MDB that our algorithm is similar to MDB's; the key difference is that the descriptions of large credit transactions have been redacted in our data, so (unlike MDB) we cannot use that information. Our algorithm works as follows:

1. We start with untagged credits over 16 times the minimum wage that are paid into a current account, are not identified as a transfer by the transfer flag, and are not an exact multiple of £100. We gather together all of these transactions in our sample periods (for example, May 2019 to May 2020).

² One might have thought that we could tighten this criterion to the credit appearing no more than two days *later* than the debit, but not allowing for pairs where the credit appears first. However, based on an examination of tagged transfers, it appears that credits do sometimes appear before debits.

-
2. We place transactions in the same account into calendar 'cycles'. The simplest cycle is weekly: we identify all transactions that take place on Mondays, all that take place on Tuesdays, etc. We also examine cycles of four weeks, one month and 'last day-of-week of the month' (for example, last Friday of the month).
 - a. If a day in a cycle falls on a bank holiday or weekend, the cycle is adjusted to instead include transactions on the working day before and the working day after that bank holiday or weekend. We account for differences in bank holidays across UK nations.
 - b. Things are more complex in December, when many workers get paid early, though not all at the same time and not in a way obviously related to their standard cycle. Based on examining tagged salaries, we classify the following as 'Christmas pay days': all working days up to a week before Christmas, the last Friday that is at *least* a week before Christmas, and the first working day after Christmas. If a cycle includes a day after any of these 'Christmas pay days', we adjust the cycle to include transactions that take place on all earlier Christmas pay days. We do not do this for weekly cycles – as these seem to be less affected by Christmas – although we do adjust for Christmas bank holidays as described above.
 - c. We adjust each cycle other than weekly cycles to include the working day before and the working day after the expected day of the cycle (we refer to these as '+- 1 day' cycles. This reflects the fact that it appears (from examining tagged salaries) that sometimes workers get paid slightly early or late for reasons unrelated to bank holidays, weekends or December.
 3. Transactions can fall into multiple cycles – for example, a pair of transactions on two Tuesdays four weeks apart from one another will fall into the same *three* '4 weeks +- 1 day' cycles: 'every fourth Monday +- one day', 'every fourth Tuesday +- one day' and 'every fourth Wednesday +- one day'. They will also fall into the same weekly cycle: 'every Tuesday'. And they may fall into common 'month +- 1 day' cycles.
 - a. Within these cycles, we separate transactions falling in them into sets, where each transaction in a set is no more than 20% larger than the next-lowest transaction. This groups transactions that are of a similar size, which salary payments usually are. We delete sets that have fewer than four transactions in them. For weekly cycles, the threshold is 10%, reflecting the fact that weekly cycles cover many more days and so are more prone to pick up other transactions. We note that those with truly weekly cycles have many transactions for obvious reasons, which also means that, if their pay fluctuates, they can still have a set that spans a wide range of amounts.
 4. We now have a series of cycle-set combinations, and a given transaction may fall into multiple such combinations (because they can fall into multiple cycles, as described above).
 5. We go through these combinations and find, for each account, the one that *includes the most transactions*. These transactions are placed into 'group 1'. We then repeat the exercise with the remaining transactions, finding the cycle-set combination that includes the most of the remaining transactions. Those in that cycle-set combination

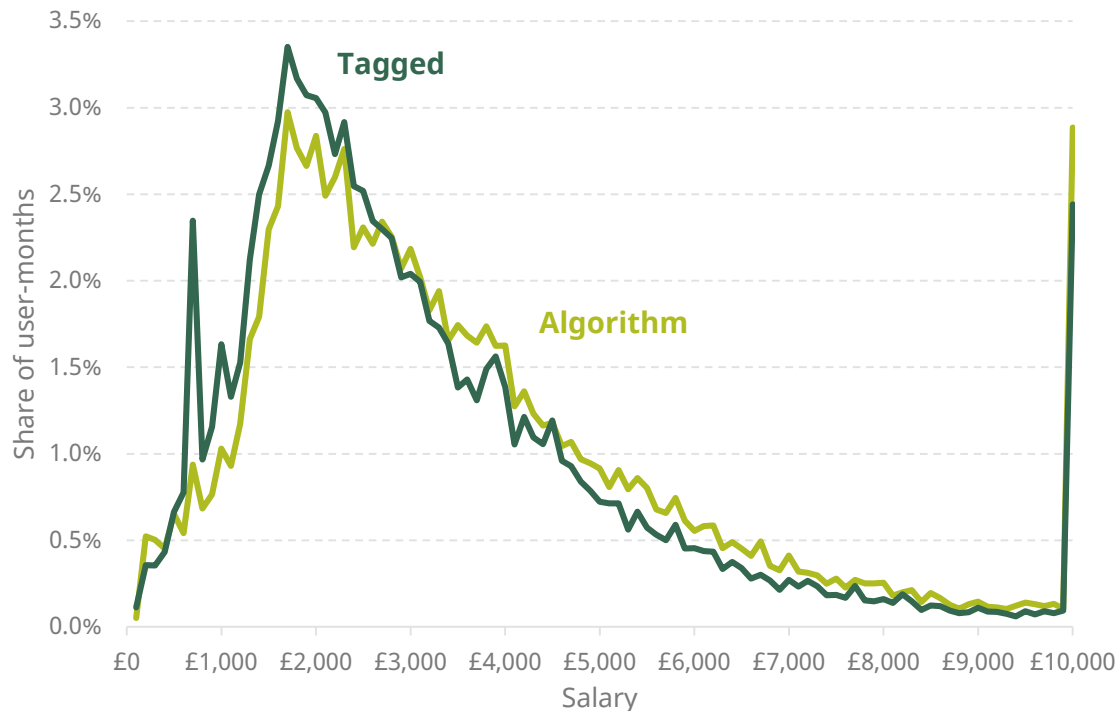
are placed into 'group 2', etc., until all transactions are assigned a group. Groups with fewer than four transactions are deleted.

- This gives us a series of transactions that are regular in the sense that they appear with at least three other transactions over the sample period at a regular frequency and are of a regular amount.

To be clear, what this method does *not* do is:

- Track well users who change jobs or see a substantial change in earnings during the sample period (unless they get four payments in the new job/payment level in the sample period).
- Take any account of the transactions having a regular *gap*. For example, suppose a user gets five transactions all on the same day. Those transactions will all be put in the same cycles, and may end up in the same group. There is no check that, for example, transactions in a weekly cycle are all at least one week apart.
- Identify jobs with zero earnings – say those on zero hour contracts who did not get any work that month.

Figure A1. The distribution of actual tagged salaries and identified salaries using our algorithm



Note: The data shown are at the user-month level. The lines show the distribution of user-months with at least £100 of the respective kind of salary. Salaries are binned to the nearest £100. Those with a salary above £10,000 per month are grouped together.

Source: Authors' calculations using Money Dashboard data available 12 June 2020.

We **test our algorithm** by using it to classify transactions that include tagged salaries (over the period April 2019 to February 2020, when MDB's tagging algorithm was working). Specifically, we give the algorithm the tagged salaries and untagged transactions of all users with at least one tagged salary in our balanced panel, and ask it to classify which of these are salaries. Our algorithm identifies 89% of user-months as having some salary, compared with 83% of user-months that have some tagged salary. The total salary identified by the algorithm is the same as the total tagged salary 50% of the time. This rises to 58% for user-months that have both an algorithm-identified and a tagged salary.

On the intensive margin of salary amounts, the distribution of algorithm and tagged salaries is very close, as Figure A1 shows.

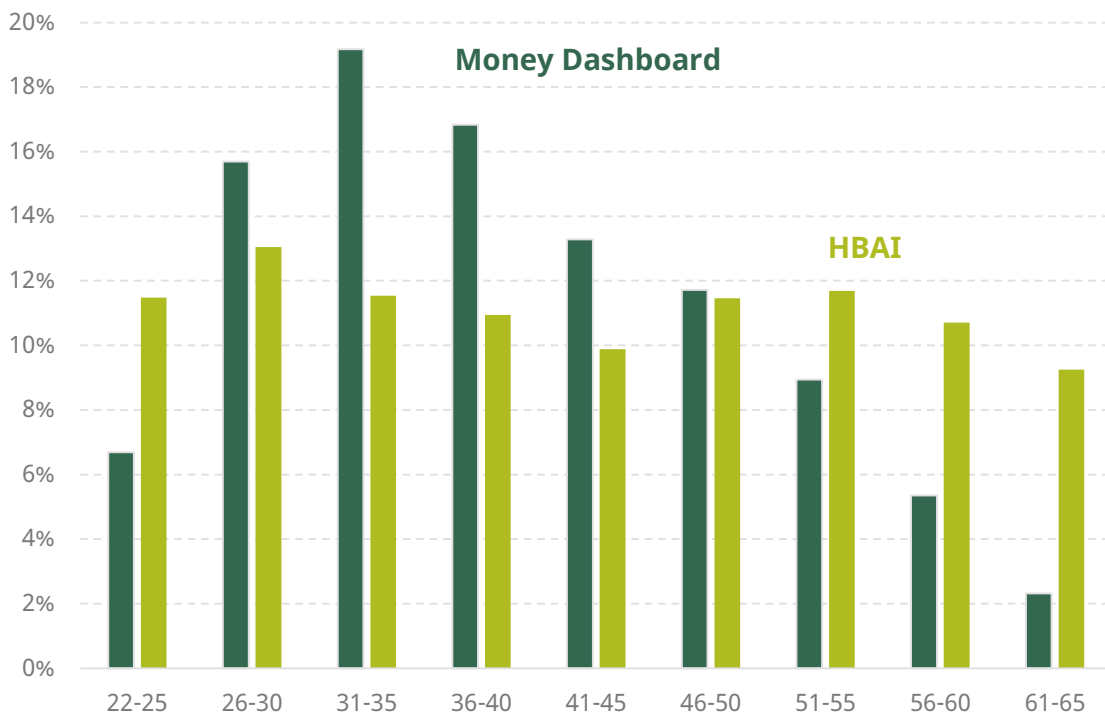
Appendix B. Representativeness

A chief concern with using data from the Money Dashboard (MDB) app is the extent to which this sample of users is representative of UK households overall. We now present descriptive statistics on our working sample from MDB: we assess its representativeness by comparing characteristics to samples drawn from the 2018–19 Households Below Average Income (HBAI) data set and the wave 6 (2016 to 2018) Wealth and Assets Survey (WAS), which are representative of the UK population.¹

Demographics

Figure B1 shows the age distribution of our balanced panel of MDB users against that of the HBAI. As might have been expected, the MDB sample is slightly younger with a greater share of users in the 26–45 age range, although the youngest and oldest individuals are less likely to be in the MDB data. However, there is nonetheless a wide spread of ages in the MDB sample: 7% of users are aged 22–25 and 8% are aged 56 or over.

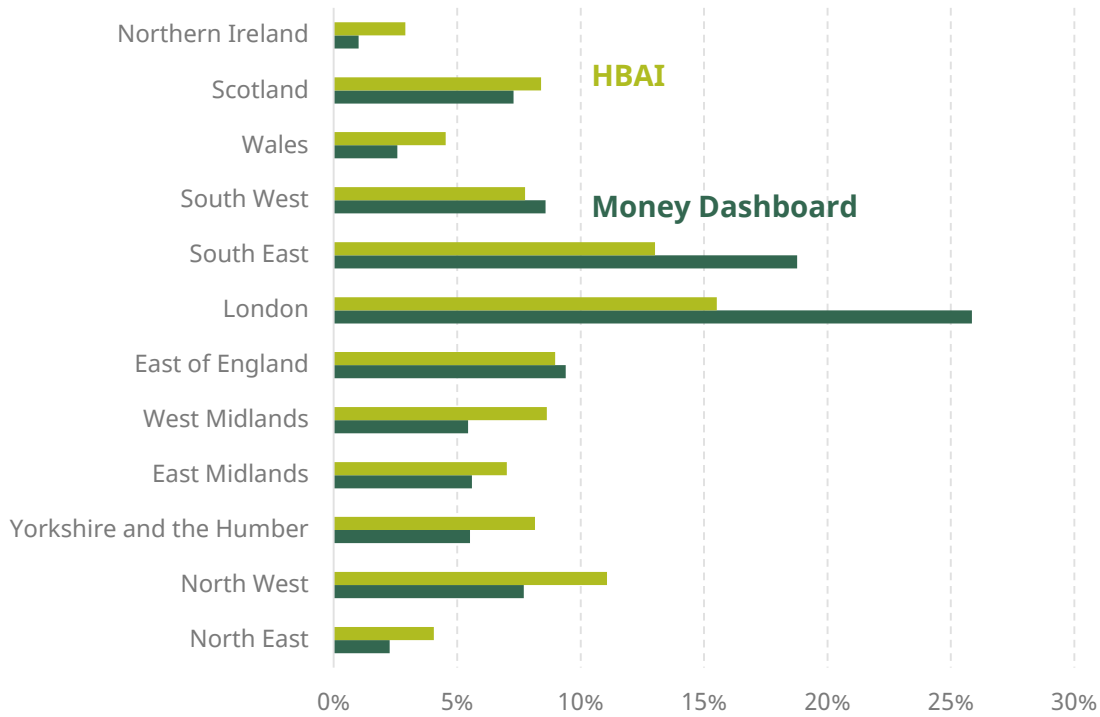
Figure B1. Share of HBAI and MDB balanced panel users, by age group



Source: Authors' calculations using Money Dashboard data available 12 June 2020 and HBAI 2018–19.

¹ Department for Work and Pensions. (2020). *Households Below Average Income, 1994/95-2018/19*. [data collection]. 14th Edition. UK Data Service. SN: 5828, <http://doi.org/10.5255/UKDA-SN-5828-12>; Department for Work and Pensions, Office for National Statistics, NatCen Social Research. (2020). *Family Resources Survey, 2018-2019*. [data collection]. UK Data Service. SN: 8633, <http://doi.org/10.5255/UKDA-SN-8633-1>; Office for National Statistics, Social Survey Division. (2020). *Wealth and Assets Survey, Waves 1-5 and Rounds 5-6, 2006-2018*. [data collection]. 11th Edition. UK Data Service. SN: 7215, <http://doi.org/10.5255/UKDA-SN-7215-11>.

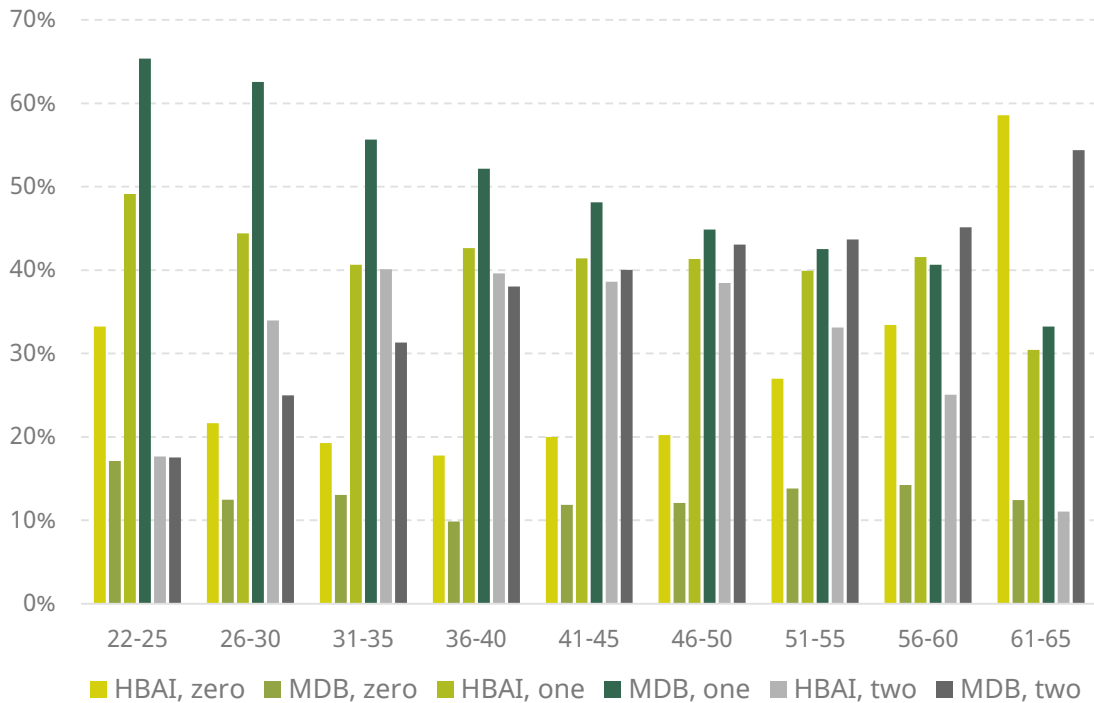
Figure B2. Share of HBAI and MDB balanced panel users, by region



Note: MDB users for whom we do not have a postcode are excluded from the figure.

Source: Authors' calculations using Money Dashboard data available 12 June 2020 and HBAI 2018-19.

Figure B3. Number of earners per household, by age



Note: MDB data have been reweighted to match the national age and region distribution, as described in the text. Number of workers per household in MDB calculated using the method described in Appendix A. Users who appear to have more than two jobs are coded as two.

Source: Authors' calculations using Money Dashboard data available 12 June 2020 and HBAI 2018-19.

We next examine the regional spread of users in our working sample against that of HBAI (Figure B2). Our sample is weighed more heavily towards London and the South East, though overall the patterns are fairly similar.

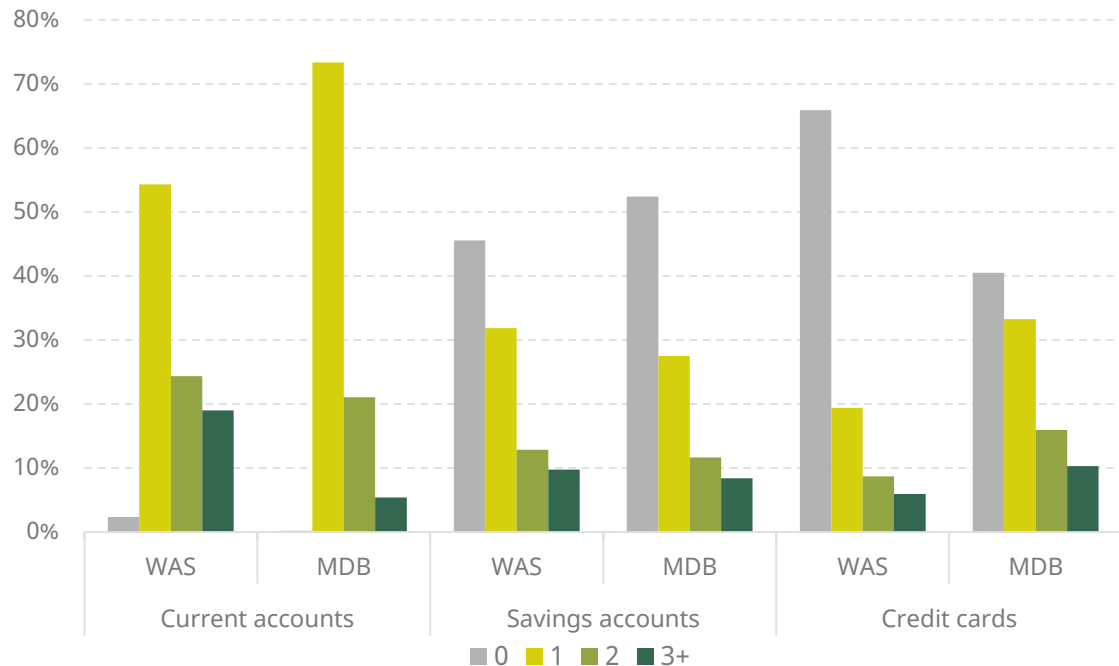
As noted in the main text, we reweight our sample to make it more representative of the population as a whole. To do this, we assign each user a weight, based on the combination of their age (in five-year age bands) and region. The weighted sample matches the actual region and age composition of the age 22–64 population (specifically, the joint distribution of the two) as reported in the HBAI 2018–19 data. All figures other than Figures B1 and B2 are based on the weighted MDB data.

Figure B3 examines the number of earners per household across different age categories, again against HBAI data. For most of the age groups, MDB matches the distribution of households with zero, one or two workers fairly closely, though it is much less close for users at the top of the age distribution. This adds confidence both that the bulk of users have linked in their partners’ financial accounts and that we are able to identify earnings transactions pertaining to separate jobs.

Financial accounts

Much of our analysis relies on us being able to observe all the financial accounts of a user (current, savings, credit card). Figure B4 shows the share of households with zero, one, two, or three or more of each type of account, in the WAS and in our working sample of MDB users. The two samples are fairly well matched, especially on the distribution of savings accounts.

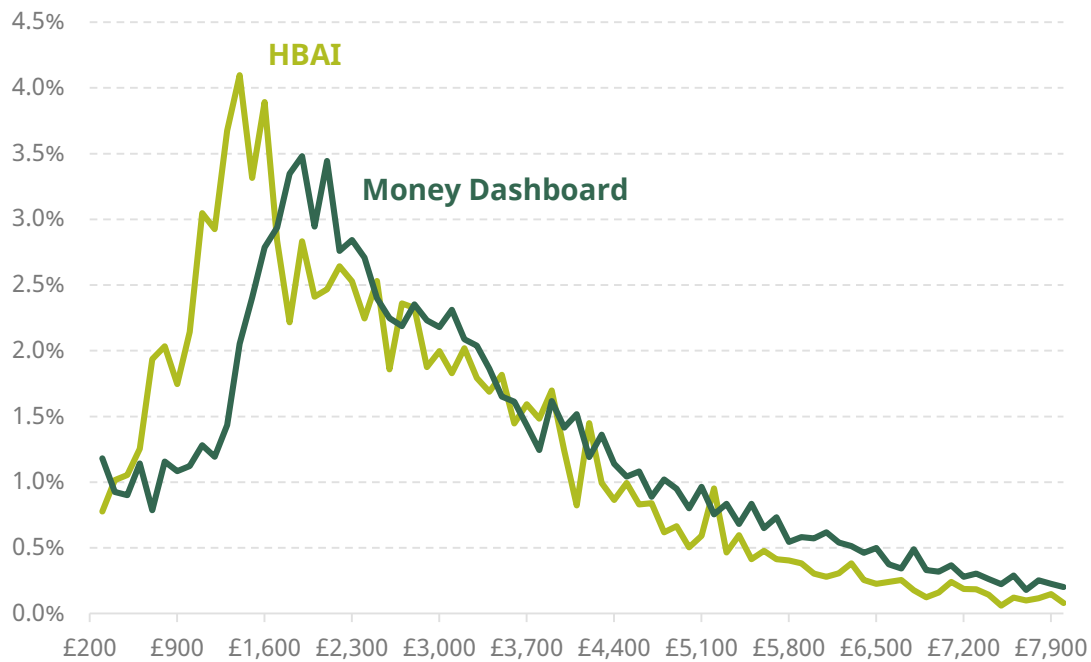
Figure B4. Share of WAS and MDB balanced panel users, by financial account type and number



Note: MDB data have been reweighted to match the national age and region distribution, as described in the text. The WAS measure of credit cards includes store cards.

Source: Authors’ calculations using Money Dashboard data available 12 June 2020 and WAS wave 6.

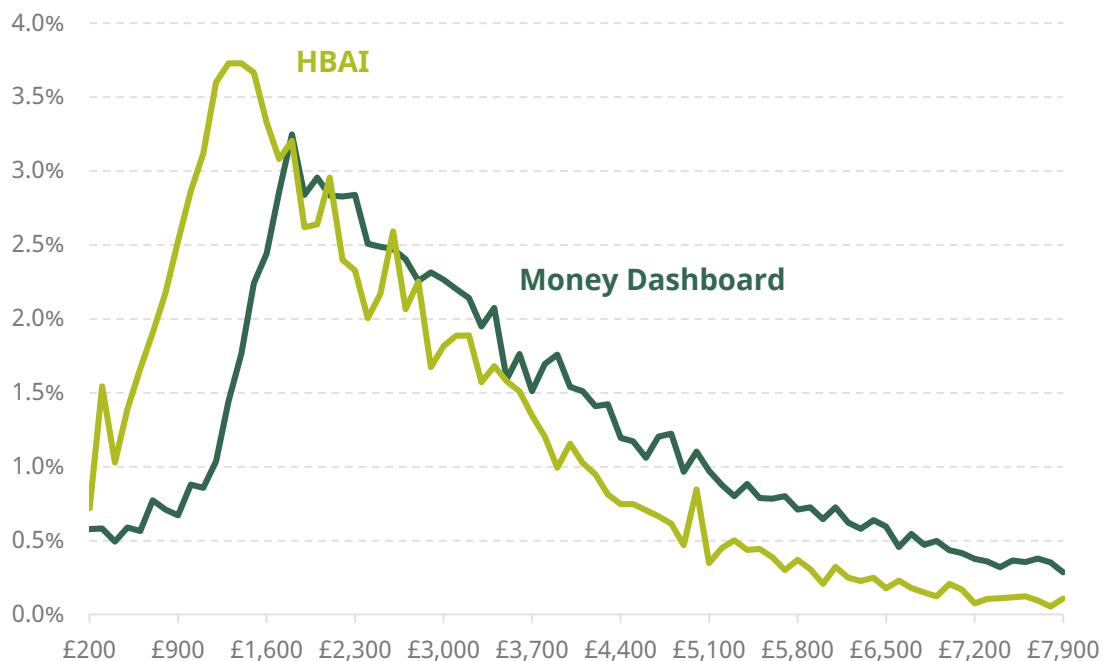
Figure B5. Distribution of monthly household earnings, HBAI and MDB samples



Note: MDB data have been reweighted to match the national age and region distribution, as described in the text. Earnings transactions are identified in the Money Dashboard data using the algorithm described in Appendix A. The sample is trimmed to exclude households with earnings of less than £150 or more than £8,000.

Source: Authors' calculations using Money Dashboard data available 12 June 2020 and HBAI 2018-19.

Figure B6. Distribution of monthly household income, HBAI and MDB samples



Note: MDB data have been reweighted to match the national age and region distribution, as described in the text. The measure of income used in the Money Dashboard data is as described in Section 2. The sample is trimmed to exclude households with monthly incomes of less than £150 or more than £8,000.

Source: Authors' calculations using Money Dashboard data available 12 June 2020 and HBAI 2018-19.

Pre-crisis distribution of household income and household earnings

Figure B5 shows the distribution of monthly household earnings in the MDB data (before the crisis) where, for expositional ease, we trim the distribution to exclude households with earnings of less than £150 or more than £8,000. The distribution is somewhat right shifted relative to the HBAI sample, though the match is quite close.

Figure B6 repeats the analysis but for the distribution of monthly household incomes (again trimming the sample to remove those households with monthly incomes of less than £150 or more than £8,000). Again, the MDB sample provides a somewhat right-shifted distribution, more so than seen for earnings. Part of the difference may be that our measure of income occasionally includes transfers from unlinked accounts, whereas this is unlikely to affect the measure of earnings substantially. Not shown in the figure is that the upper tail of the income distribution is over-represented in MDB relative to HBAI. Whereas 2.7% of households in HBAI have a monthly income in excess of £8,000, that figure is 10.5% for MDB. Both data sets have a small number of users with incomes under £150 (2.0% and 0.7% for MDB and HBAI respectively).

Pre-crisis financial distress

We might think that those using a budgeting app such as MDB may systematically differ from the rest of the population with regards to budgeting ability. This could have consequences for our measures of financial distress, such as making bill payments on time. To check for representativeness on this margin, ideally we would compare missed bills in MDB with those in another data source. However, while the MDB data can be used to obtain the *change* in missed bills, it is not suitable for obtaining the *level* of missed bills. This is because if someone does not pay a particular bill, we do not know whether they have 'missed' it, they are not liable for it, it is not tagged or it is paid out of an unlinked account.

Instead, we compare overdraft usage in our MDB sample with that reported by families in WAS. In the MDB data, we can observe whether a user is subject to an overdraft *charge*, but not all overdraft usage generates such a charge. This causes us to undercount the number of MDB users using their overdraft. Moreover, even if a user is subject to an overdraft charge, it is not clear when they stop using their overdraft. Nonetheless, frequency of overdraft charges can give an indication of the budgeting ability of the sample. We find that:

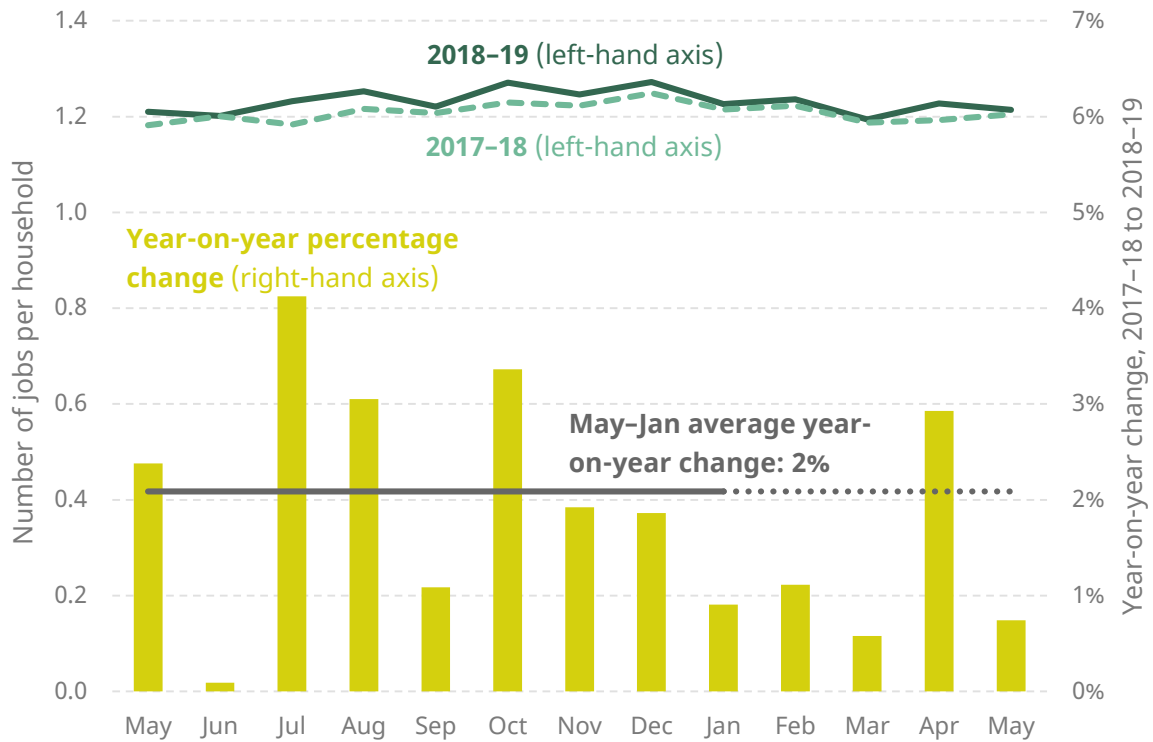
- 13% of families in WAS report being overdrawn on a current account;
- 2.5% of our MDB sample are subject to an overdraft charge each weekday on average;²
- 5% of our MDB sample are subject to at least one such charge each week;
- 12.5% of our MDB sample are subject to at least one such charge each month.

For the reasons given above, we cannot directly compare the MDB and WAS statistics. What we can see, however, is that MDB users do frequently use overdrafts at a rate in at least the same neighbourhood as the population at large.

² The vast majority of overdraft charges are on weekdays.

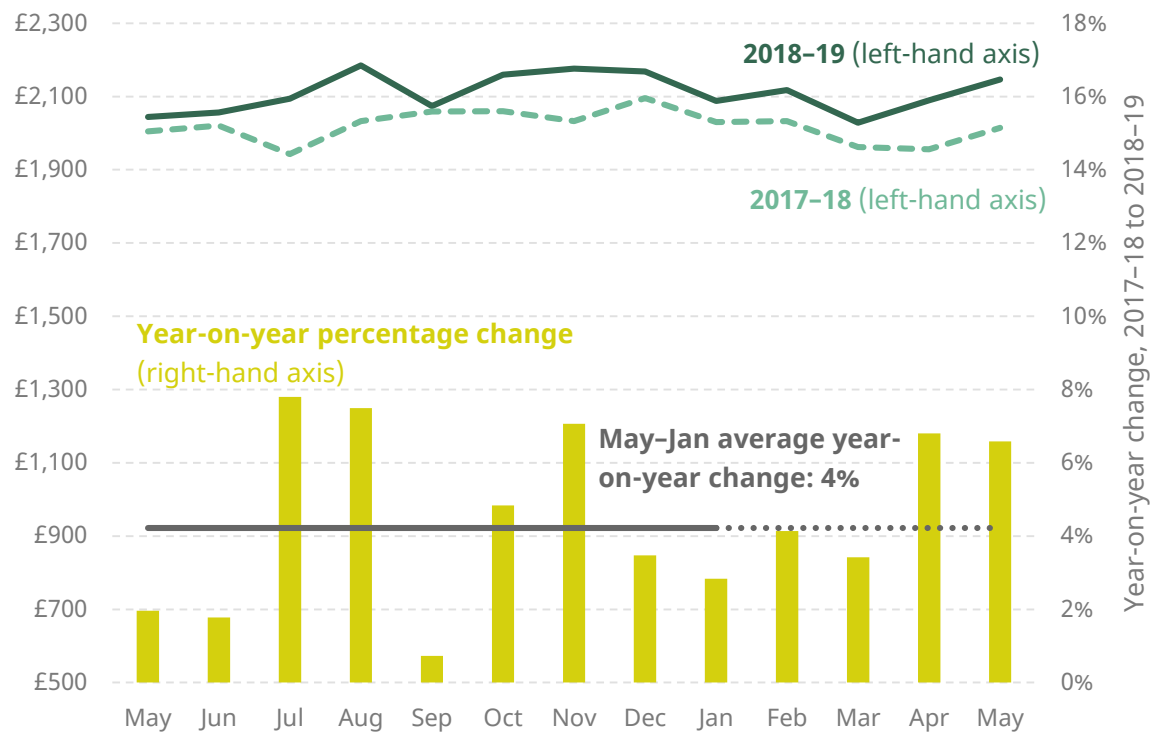
Appendix C. Placebo exercises

Figure C1. Number of jobs per household (placebo for Figures 2 and 3)



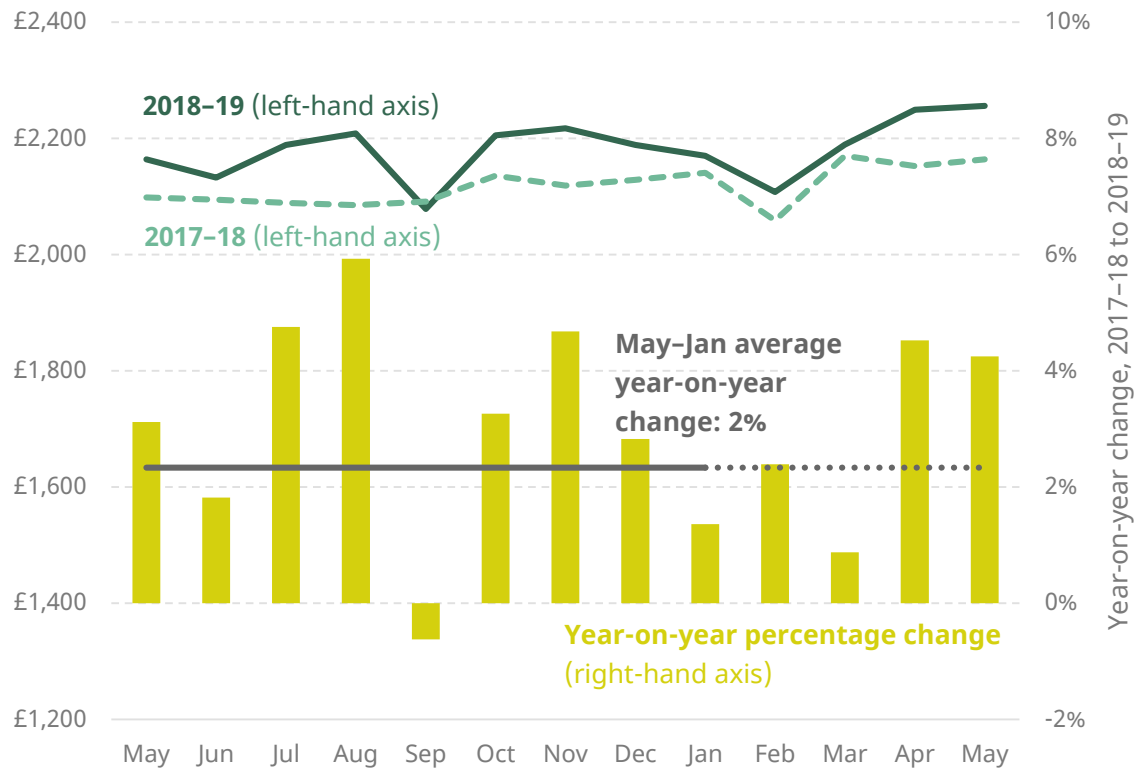
Note and source: See Figures 2 and 3.

Figure C2. Median real monthly household earnings (placebo for Figure 4)



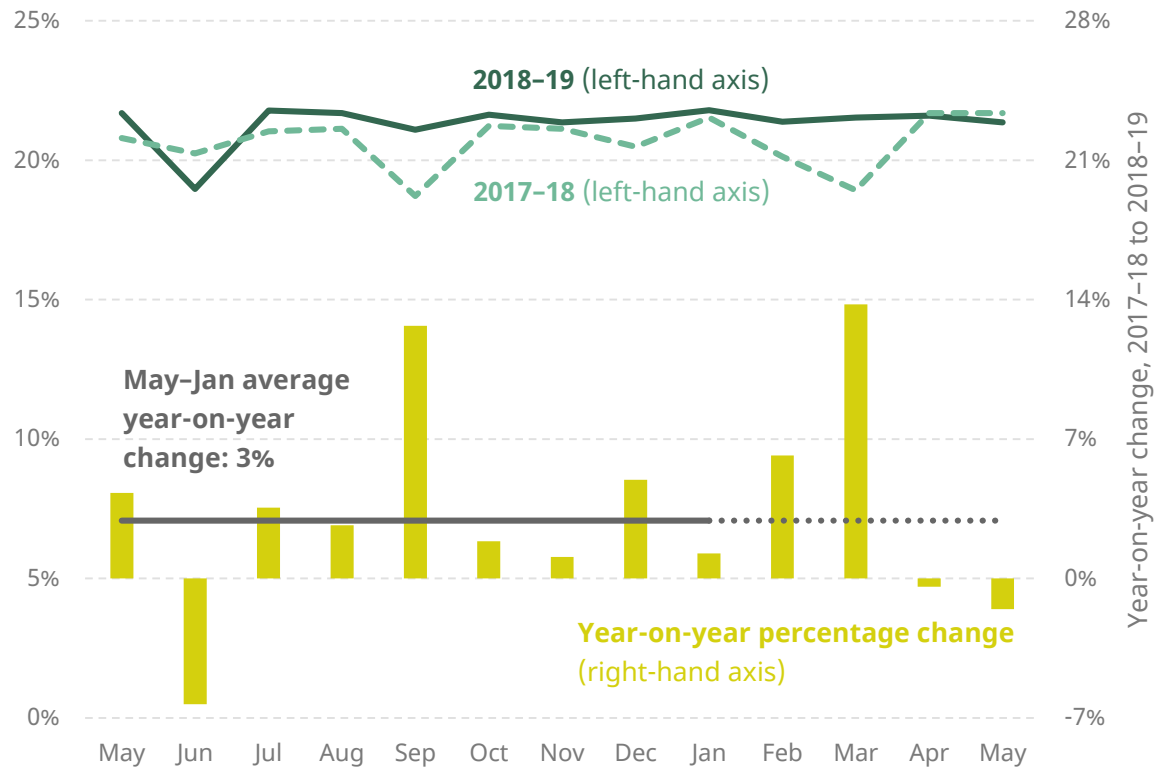
Note and source: See Figure 4.

Figure C3. Median real monthly household income (placebo for Figure 8)



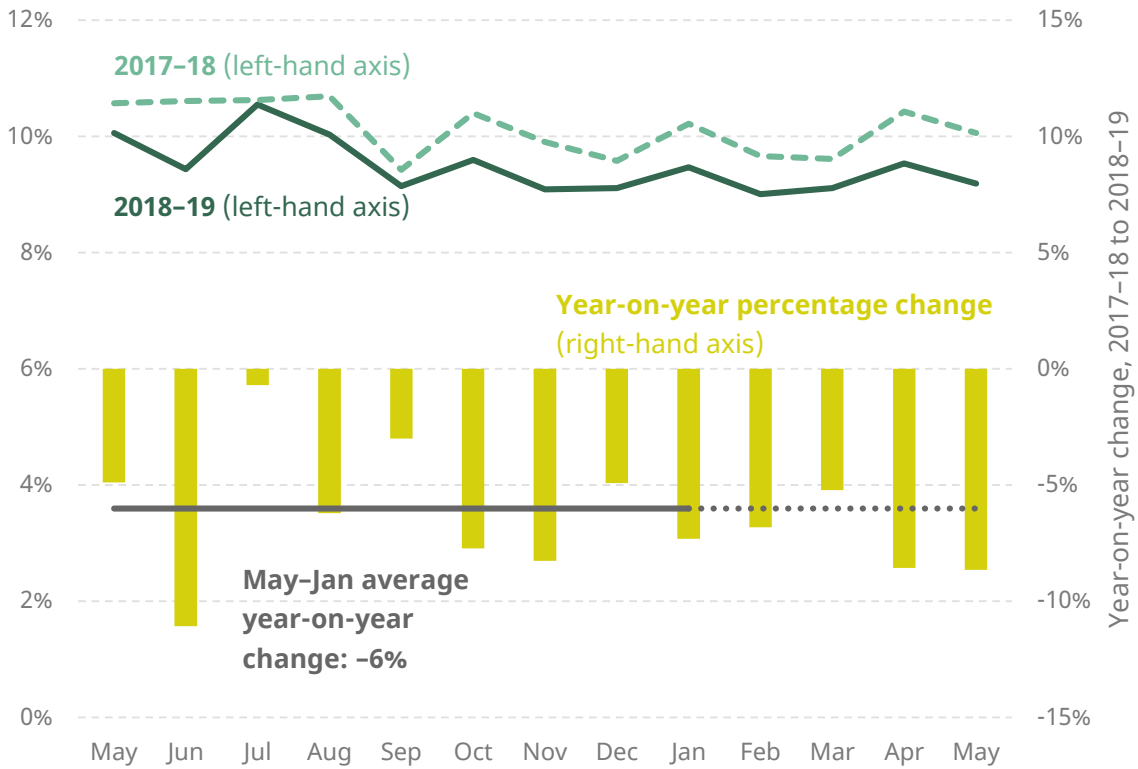
Note and source: See Figure 8.

Figure C4. Share of users making mortgage payments (placebo for Figure 12)



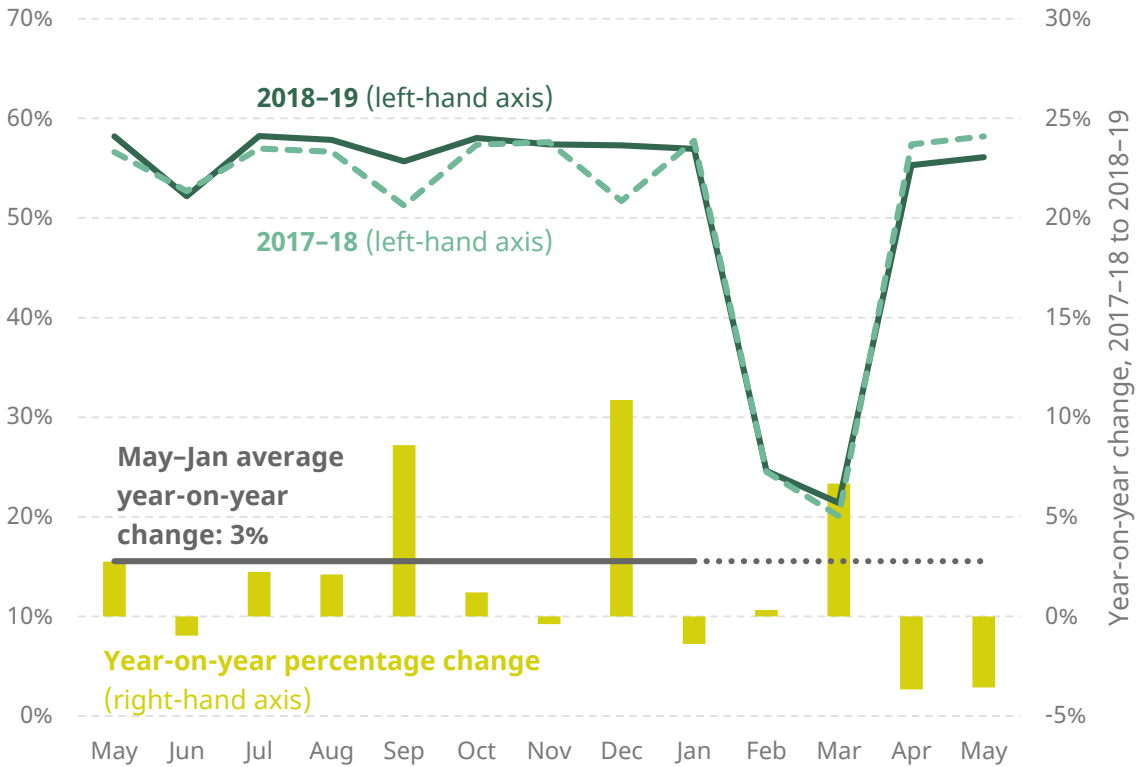
Source: See Figure 12.

Figure C5. Share of users making rental payments (placebo for Figure 13)



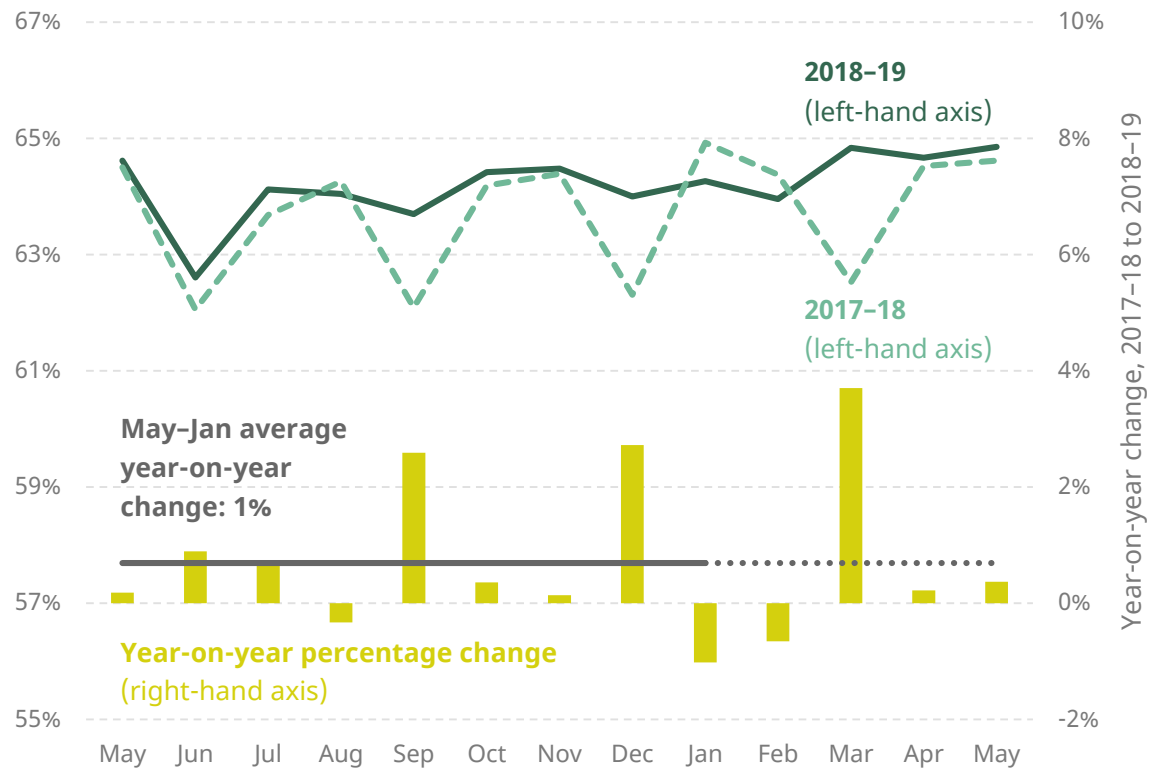
Source: See Figure 13.

Figure C6. Share of users making council tax payments (placebo for Figure 14)



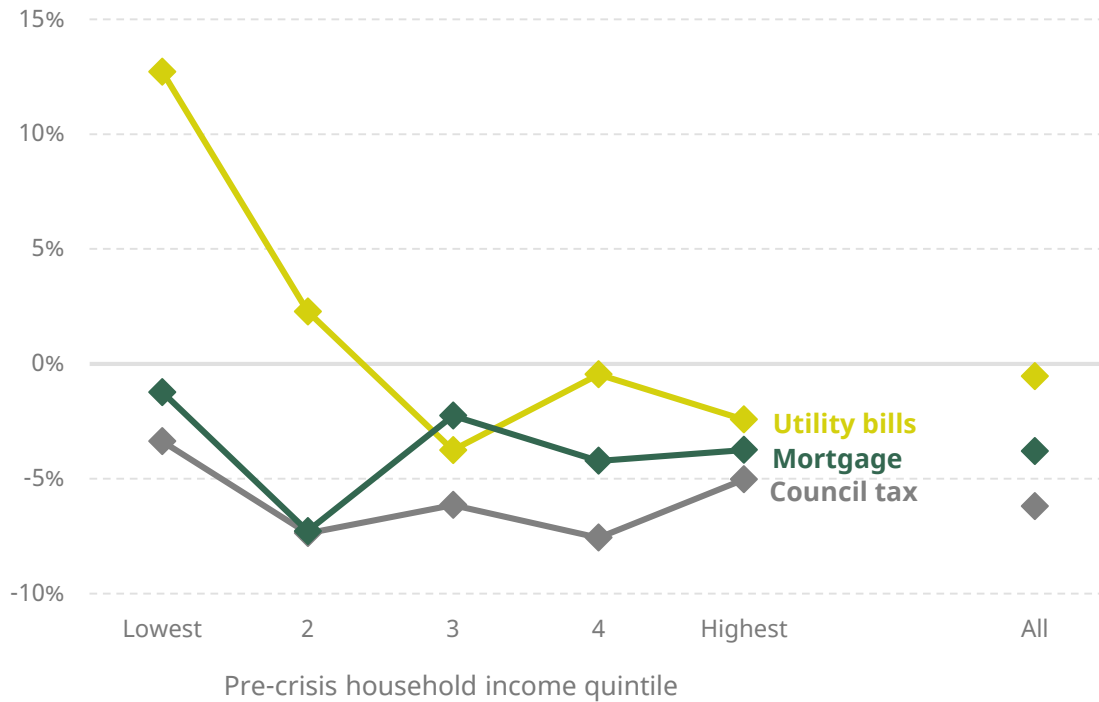
Note and source: See Figure 14.

Figure C7. Share of users making utility bill payments (placebo for Figure 15)



Note and source: See Figure 15.

Figure C8. May percentage deviation from pre-crisis trend in the share of households making bill payments, by pre-crisis income quintile (placebo for Figure 17)



Note and source: See Figure 17.