WHAT DO FISCAL STIMULUS PACKAGES MEAN FOR HOUSEHOLD DEBT?

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Abstract

We study the links between fiscal stimulus packages during times of crisis and households' liabilities. We do so by using household-level data on income and liabilities from the Consumer Expenditure Survey, and estimating an empirical model along those in the literature on the consumption effects of these packages. We find that receiving a check from the government consistently translates into a reduction in outstanding liabilities for American households. This effect is present for each credit type as well as for the total, and it is robust to controlling for income levels and demographic characteristics correlated with consumers' access to credit.

Keywords: stimulus packages, household debt, borrowing and liquidity constraints.

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1 Introduction

Stimulus payments to consumers are often a central part of measures enacted by governments during times of crisis. Examples are the Economic Growth and Tax Relief Reconciliation Act of 2001, the Economic Stimulus Act of 2008, the American Recovery and Reinvestment Act of 2009 and the most recent Coronavirus Aid, Relief and Economic Security (CARES) Act of 2020.

Within the context of the CARES Act, much political debate has happened in the last few months around whether further stimulus should be directed mostly to firms or to households. When it comes to the latter, the current literature has a lot to say about this, in particular about the effects of stimulus payments on recipients' consumption patterns (see Bodkin, 1959; Parker, 1999; Souleles, 1999; Johnson et al, 2006; and Agarwal et al, 2007, among others). Unfortunately though, this work still lacks a good treatment of the effects of these payments on households' management of liabilities. Thus, the question of how the fiscal stimulus affects consumer credit markets remains open. Providing an answer to this question is particularly important during crisis times when emphasis is placed on fiscal policy, and even more so when the effectiveness of traditional monetary policy tools is limited.

We aim to start filling this gap in the literature by looking at past episodes of large stimulus packages and studying how American households' access to finance was impacted by these policies. We can think of three ways in which receiving a stimulus payment can affect the market for household debt. On the one hand, the government checks should directly lower the need for short-term credit to finance consumption (like credit card loans, for example) and/or they could be used by recipients to pay off previous stocks of debt. These would predict a negative relationship between payments and families' outstanding liabilities. On the other hand, if receiving this government support makes lenders see families as more creditworthy, then fiscal stimulus payments may work to alleviate borrowing constraints and ease the liquidity needs for these households. The latter channel would predict a positive relationship between stimulus and the outstanding amount of credit held by households or, at least, weaken the dependence of access to credit on borrowers' assets and income. Ultimately then, the question is an empirical one.

We address this question by looking at previous evidence from the Economic Stimulus Act of 2008. The Economic Stimulus Act was passed by the Senate in February of 2008 and signed into law by President George W. Bush. The law provided for tax rebates to low- and middle-income U.S. taxpayers[1], tax incentives to stimulate business investment, and an increase in the limits imposed on mortgages eligible for purchase by government-sponsored enterprises (e.g., Fannie Mae and Freddie Mac). The total cost of this bill was projected at $152 billion for 2008. For eligible households[2] this meant tax rebates received as either direct deposit or checks delivered through US mail of at least $300 per person ($600 for married couples filing jointly) and $300 per dependent child under the age of 17. Those with no net tax liability were still eligible to receive a rebate, provided they met the minimum qualifying income. Rebates were phased out for adjusted gross incomes greater than $75,000 per person ($150,000 for joint returns) at a rate of 5% of the income above this limit. Some taxpayers who exceeded the income limits, but had qualifying children, still received a rebate.[3]

So for example, in our data out of 6,417 households who got rebates, 1,096 of them qualified for $300; 2,276 for $600, 340 for $900, 1,372 for $1,200, 326 for $1,500, 292 for $1,800, 99 for $2,100 and 30 for $2,400, with the others receiving "non-round" numbers because of the way the rebate phased out by 5% for every dollar in excess of the income threshold.

We formulate an empirical model following Johnson, Parker, and Souleles (2006) and references therein. Taking advantage of the data at the household-level, we estimate the impact of stimulus on debt.

We use the Consumer Expenditure Surveys of 2008, which allows us to combine information on stimulus payments with credit, assets, income and expenditure data, as well as information on demographic characteristics. Being able to merge all these variables at the household-level is crucial to assess heterogeneities across consumers in the impact of those fiscal packages as well as to give an insight into the channels through which stimulus can affect credit markets including those for mortgages. We use the data on demographics to build proxies for potential credit constraints.

All results point to the stimulus having a statistically significant and economically important negative impact on households' liabilities. Along the lines of Johnson, Parker,

[1] A taxpayer is defined in the Act as someone with qualifying income of at least $3,000, someone with a positive net income tax liability, or someone with gross income greater than the sum of the basic standard deduction plus the exemption amount.

[2] The term "eligible individual" means any individual other than any non-resident alien, any individual with respect to whom a deduction is allowable to another taxpayer, or an estate or trust.

[3] In terms of timing, the Act stipulated that the refund or credit had to be made as soon as possible and that no refund would be allowed after December 31, 2008. The Act also stated that no interest would be allowed on any overpayment attributable to this law.

and Souleles (2006), we also interpret our findings as a rejection of the benchmark rational-expectations permanent income hypothesis (PIH), which predicts that any wealth effects from the rebates should be reflected on direct changes to savings and thus, show up as a *proportional* reduction in households' liabilities.

We also look at the response of different types of credit separately, including mortgages, credit card debt, loans from banks, brokerages, savings and loans, credit unions or insurance companies and other types of debt like those due to medical expenses not covered by insurance, school or personal loans and loans from retirement plans. Furthermore, we study the impact of these payments for subsamples by state of employment and quartiles of income. Results are consistent across all these subsamples.

Last, to test the channel through which stimulus impacts access to credit we conduct additional tests in which we interact the rebates with four alternative measures of liquidity or borrowing constraints. Doing so we are able to show that the stimulus elasticity of debt is higher for older individuals and females, and lower for college educated recipients.

The rest of the paper is organized at follows. Section 2 contains a review of the literature to which we aim to contribute. Section 3 describes the data, and section 4 introduces the empirical methodology. Section 5 presents the main results as well as several robustness checks. The final section concludes and discusses policy implications for the current stimulus spending under the CARES Act.

2 Related Literature

There is no consensus in the literature on the effects of government spending on credit markets. Most theoretical macro-models predict government spending tightens credit markets due to the rise in nominal interest rates, leading to higher costs and lower credit access. The empirical evidence, however, does not often support this prediction. Some authors like Ramey (2011) and Fisher and Peters (2010) even find that fiscal spending causes interest rates to fall. More recently, Auerbach, Gorodnichenko, and Murphy (2020) and Gilje, Loutskina, and Strahan (2016) reinforce this evidence.

The literature on the effects of fiscal stimulus on consumer spending dates back at least to Bodkin (1959) who studies the effects of windfall income on consumer spending. He casts doubt on the permanent income hypothesis (PIH), according to which the anticipated changes in transitory income should not alter consumer spending. Since Bodkin's seminal paper, many researchers have studied the response of consumer spending to various fiscal stimulus programs. The evidence remains mixed. When the changes in income are well-anticipated, large, and regular, Hsieh (2003) and Browning and Collado (2001) find no response of consumption to these payments, which supports the PIH. Similarly, Shapiro and Slemrod (2009, 2003) analyze the survey data from the 2008 and 2001 tax rebates, respectively, and find that the rebates caused households to increase their spending, albeit only slightly. In contrast, there is a whole series of papers that find that anticipated changes in income do substantially alter the expenditure patterns. These are Shea (1995), Parker (1999), Souleles (1999), Johnson, Parker, and Souleles (2006), Parker et al. (2013), Broda and Parker (2014), and Parker (2017). Johnson, Parker, and Souleles (2006) and Parker et al. (2013) are particularly noteworthy since, unlike their predecessors, they exploit the random timing of the 2008 and 2001 stimulus payment receipts, respectively, to estimate the causal effects of these disbursements on household expenditures. Misra and Surico (2014) reinforces this evidence; however, they find that the consumption responses to the 2008 and 2001 fiscal stimuli are highly heterogeneous. Kaplan and Violante (2014) rationalize this microeconomic evidence in a dynamic structural model with households that are heterogeneous in asset holdings. They find this heterogeneity affects households when deciding how much to spend out of an additional dollar in their transitory income. Very recently Coibion, Gorodnichenko, and Weber (2020) have analyzed the effects of the latest stimulus transfers under the CARES Act on consumer spending. They find the recipients spend around 40% of the stimulus payments, and the responses are highly heterogeneous across households. Using a high-frequency survey data, Baker et al. (2020) reinforce these findings of large and heterogeneous consumption responses. Additionally, they find these disbursements increase mortgage, rent, and credit card payments.[4]

It would be tempting to argue that the lack of focus on consumers debt is not a significant limitation of previous literature since results from the work that focuses on spending as the main variable of interest could be used to directly infer how these payments affect debt. This would be an inaccurate conclusion, though. Studies on consumption responses can indeed be used to infer what the response of savings to stimulus payments looks like (due to the obvious relationship between consumption, savings and total income). However, since individuals can use savings to either pay down debt or accumulate assets, studies on consumption response to unexpected changes in income do not necessarily say much about how debt reacts to these changes.

[4] A few papers examine the effectiveness of fiscal programs specifically designed to stimulate housing markets: Best and Kleven (2018), Anenberg and Ringo (2019), and Berger, Turner, and Zwick (2020).

To the best of our knowledge, only Agarwal, Liu, and Souleles (2007) have studied the link between stimulus packages and consumers' indebtedness before. They study the response of debt to "lumpy" increases in income like tax rebates. We believe there is still room for further study since Agarwal, Liu, and Souleles (2007) focus on credit card debt only, which amounts to only 40% of total debt in our data.[5] Also, there are three important differences with our study. First, their analysis is conducted at the account level (for credit cards), not at the individual level. Results can be different since individuals can and in fact, tend to hold more than one credit card. Second, they study the federal income tax rebates of 2001, and we focus on those of 2008. Last, their measure of liquidity constraints is based on characteristics of the account (credit limits and usage rates) rather than of the individual like in our case.

Also, Demyanyk, Loutskina, and Murphy (2019) do take into account debt levels but only as an additional determinant of how consumption responds to these packages.[6]

Here we aim to keep filling this gap in previous literature that we have identified.

3 The Data

We use the data collected by the Census Bureau for the U.S. Bureau of Labor Statistics (BLS) under the Consumer Expenditure Surveys (CE) program. The CE program provides data on expenditures, income, and various demographics for a representative random sample of U.S. households. We use the interview survey, which is based on a sample double in size compared to the diary survey. The surveys are released yearly, and every release contains data for five quarters (four quarters of the previous year and the first quarter of the year of release).

The CE data is provided at the level of the consumer unit (CU). The Survey defines a consumer unit as either: (1) all members of a particular household who are related by blood, marriage, adoption, or other legal arrangements; (2) a person living alone or sharing a household with others or living as a roomer in a private home or lodging house or in permanent living quarters in a hotel or motel, but who is financially independent; or (3) two

[5] This share is also very volatile across the households in our sample, displaying a standard deviation of 45%, a minimum of 0.03% and a maximum of 100%.

[6] Using the Department of Defense (DOD) spending data, Demyanyk, Loutskina, and Murphy (2019) perform a core-based statistical area level analysis to estimate how the fiscal spending affects the economic growth in geographic areas with different pre-recession consumer debt-to-income ratios during the 2007-09 recession period. They find the fiscal multiplier is higher in geographic areas that started the recession with higher consumer indebtedness.

or more persons living together who use their income to make joint expenditure decisions.

The reference person of the survey participates in the interview. According to the CE, the reference person is the first person mentioned by the respondent when asked to "Start with the name of the person or one of the persons who owns or rents the home." The relationship of the other CU members is determined with respect to this reference person.

3.1 Income

Our income measure is the total amount of family income in the last 12 months of collected data as measured by the variable *fincbtax* in the Survey.

The Survey defines income as the aggregated value of all incomes of all CU members who are 14 years of age or older. The components of income are: (1) money income before taxes; (2) wages and salaries; (3) self-employment income; (4) Social Security, private and government retirement; (5) interest, dividends, rental income, and other property income; (6) unemployment and workers' compensation, and veterans' benefits; (7) public assistance, supplemental security income, and food stamps; (8) regular contributions for alimony and child support of own or foster children; and (9) other income like cash scholarships, fellowships and stipends[7].

During the Great Recession of 2007-08 and its aftermath, which is the period of our data, many households experienced steep declines in their incomes, with even steeper falls for those household members who were ineligible for unemployment insurance. The income measure we use also accounts for workers compensation, as well as for public assistance, supplemental income and food stamps, which even those without unemployment benefits would get. Thus, we argue that the steep fall in income during the recent financial crisis is at least partially accounted for by including data on non-labor income earned by survey respondents while unemployed.

3.2 Stimulus Payments

Stimulus payments were received by 25.1 million people in our sample in each of the quarters of 2008. Over the entire time frame of this study, the mean stimulus was $926.6 and it exhibited significant variability across the quarters. While the average household received a rebate of approximately $750 in the first and last quarter of 2008, the corresponding

[7] The definitions for the income components are provided at this link https://www.bls.gov/cex/csxgloss.htm

amounts were significantly higher in the second and third quarters, with people receiving \$960 and \$850, on average. When measured as a percentage of annual income, tax rebates were on average 0.33%. Furthermore, stimulus represented 0.57% of annual expenditure. The summary statistics stimulus data are presented in Table **??**.

3.3 Household Debt

We use data on the total amount owed to all credit sources as well as on each source of credit including mortgages, credit cards, bank loans and other loans. The credit cards category includes stores credit, gasoline credit cards (like those issued by Amoco or Exxon), store cards (like those issued by department, specialty, electronics or sporting goods stores), or major credit cards (VISA, Master Card, American Express, Discover or other revolving credit accounts). The "bank and other loans" category includes the total amount owed to financial institutions, such as banks, brokerages, savings and loans, credit unions, or insurance companies (does not include insurance premium payments), the total amount owed to doctors, dentists, hospitals, or other medical practitioners for expenses not covered by insurance, school loans, personal loans, and loans from retirement plans.

In Table 2 we present detailed summary statistics on household liabilities. There we show that the average US household had approximately \$195,000 in mortgage and another \$10,500 in consumer credit debt, which amount to 31 times and 25% their annual income, respectively. Within the category of consumer credit, most households have at most two types of liabilities, with the vast majority borrowing from only one source. In our sample, only 605 households had all types of loans. For them credit cards represented 36% of the total, and bank and other loans presented 32%.

In Table 3 we present correlation coefficients among all variables and their significance levels. The fact that the correlation between mortgage and non-mortgage types of loans, and between credit card and bank loans are only 0.15 at the maximum is evidence for the importance of studying the stimulus elasticity for the aggregate hosueholds' debt as well as for each of the individual types of credit. Also, the negative unconditional correlation between stimulus payments and the various types of debt allows us to formulate an initial hypothesis that stimulus payments may lead to deleveraging for American households.

4 Empirical Methodology

The data is an unbalanced panel of households (indexed by i) and quarters (indexed by t). The baseline model we run on this dataset is:

$$L_{i,t} = \sum_{s=1}^{S} \beta_{0s} * period_s + \beta_1 Y_{i,t} + \beta_2 R_{i,t} + \theta_i + u_{i,t} \tag{1}$$

where L is the log of credit outstanding (expressed in constant 2015 dollars) in each period, Y is gross-income (pre-tax) and R is the rebate amount received by the household. The *period* variable is a complete set of dummies for every period s in the sample, used to absorb the seasonal variation in consumer credit. θ_i are the fixed effects at the consumer unit level.

Our sample shows important differences in the level of credit among households located in different income brackets. For example, the cross-sectional average of the total amount of credit was $5,470.28 for households in the first quartile of the income distribution and $11,693 for those in the top quartile. Similarly, while the household in the bottom quartile with the highest amount of outstanding liabilities held $126K in debt, its counterpart in the top 25^{th} percentile held $218K. To account for these differences, we include the household's income variable Y among the regressors in equation (1). Income is of course an important determinant of the level of credit outstanding at the household-level. Also, since R is correlated with Y, omitting Y could result in a spurious correlation between stimuli and the level of credit. A positive sign is expected for the $\hat{\beta}_1$ coefficient.

To accurately estimate the causal effect of stimulus payments on the access to consumer credit and to provide an unbiased estimate of $\hat{\beta}_2$, we need to account for the fact that the stimulus variable could potentially be endogenous. The magnitude of stimulus payments was not random and depended on various household characteristics such as the taxpayer's marital status and the number of dependents in the consumption unit, which could potentially also be highly correlated with our dependent credit variable. Conversely, the month in which the stimulus was received was determined by the second-to-last digit of the taxpayer's social security number, which makes the timing of the stimulus payments random. We exploit this randomness to construct our instrument for the stimulus variable using a dummy indicating whether the stimulus was received in a given period. This timing dummy seems a proper instrument given its correlation with the stimulus payments but not with other unobserved variables that affect the access to consumer credit.

We have no strong priors on what sign to expect for $\hat{\beta}_2$. A positive coefficient would be explained by stimulus checks alleviating borrowing constraints for the recipients if lenders became more willing to lend to some households after they have qualified for government support during the crisis. A negative coefficient would signal a diminished need for credit among recipients and/or they using these payments to pay-off past debt.

To further understand the channels of transmission through which the receipt of a stimulus check would impact the amount of outstanding consumer credit and mortgage liabilities among households, we run an extended version of the model in equation (1) where we incorporate among the regressors information on households demographic characteristics as in equation (2).

$$L_{i,t} = \sum_{s=1}^{S} \beta_{0s} * period_s + \beta_1 Y_{i,t} + \beta_2 R_{i,t} + \sum_{m=1}^{M} \beta_{3m} X_{i,t,m} + \sum_{m=1}^{M} \beta_{4m} X_{i,t,m} * R_{i,t} + \theta_i + u_{i,t} \quad (2)$$

where X_m is one of M=6 demographic controls. The demographic information includes the age of the reference person (age), a dummy variable that equals one when the reference person has some post-secondary education ($college - dummy$), two dummies for the reference person in the consumer unit being female and white ($female - dummy$ and $white - dummy$), respectively, a dummy for the consumer unit being located in an urban area ($urban - dummy$), and the number of members in each consumer unit ($fam - size$).

We also include interaction variables between the demographic characteristics and the stimulus main variable of interest. Since we think of these characteristics as indicative of factors aside from income that might influence a household's level of debt, the estimated coefficients on the interactions allow us to test whether the stimulus receipt works to alleviate credit constraints.

5 Results

5.1 The Response of Consumer Credit to Fiscal Stimulus Packages

We study how households' liabilities responded to rebate payments during the 2008 Economic Stimulus Act policy intervention. We show these results in Table 4. In this benchmark set of results from standard OLS estimations, we find across specifications, that a 1% increase in the amount of stimulus spending yields a 0.0056% decrease in consumers' debt. Also, at the extensive margin, receiving a check from the government leads on average to a

0.0381% reduction in the amount of outstanding liabilities. These results are all consistent with a deleveraging effect of policies that target consumers during times of crises.

Perhaps a surprising result is that, although the income variable is used as a control, we are not able to uncover a statistically significant positive relationship between pre-tax income and debt levels for American households in 2008.

5.2 Endogeneity

It could be argued that the stimulus payments themselves are endogenous to the size of households' liability portfolio. Thus, in this section we run a robustness check in which we instrument for the stimulus variable using a dummy equal to 1 for consumer units that qualified for a stimulus payment. In doing so, we follow the identification strategy of Johnson, Parker and Souleles (2006) who explot the random timing of the stimulus.

In Table 5 we present these results. The size of the estimated coefficients falls slightly and they keep their statistical significance. Now, a 1% increase in the stimulus payments results in a 0.0047% reduction in the amount of credit.

The similarity between our benchmark results and those of the IV estimations indicate the lack of evidence for the presence of endogeneity. Thus, from now on we continue working with our initial benchmark specification assuming exogeneity of the stimulus variable.

5.3 Selection - Only Households Receiving the Stimulus

What we do next is to limit the sample more strictly by first, including only households who got rebate payments, and second, households who got rebates and replied to a qualitative question that they used them mostly for paying-off debt. Restricting the sample in this way is an interesting exercise because estimates identify the response of debt from only purely randomized variation in the timing of the rebate conditional on receipt.

These results are presented in Table 6, in columns (1) and (2) for the first subsample and in columns (3) and (4) for the second. For the first subgroup, our results indicate that a 1% increase in the stimulus lowers credit by 0.13%. The size of the effect increases to -0.14% for the second subgroup. When controlling for income these results are not statistically significant, though. The power of the estimations improves somehow when income is not controlled for. However, the sample size is very small for check recipients who also have recorded data on their liabilities in credit markets. Thus, we lose statistical power with the smaller number of observations.

Summarizing, while there is some indication that the stimulus itself or the recovery associated to the fiscal packages might have incentivized consumers to deleverage, results are not statistically strong when identification comes only from variation in the amount of the checks and not from the extensive margin of whether survey participants qualified for these payments.

5.4 Subsample Heterogeneities

In this section we explore the potential for the impact of stimulus payments being heterogeneous across subsamples of the population. In particular, we look at the effects separately for the unemployed and the employed at the time of receiving the fiscal stimulus, and for the bottom 25% of the income distribution and the top 75%. Results are presented in Table 7.

The deleveraging effects are qualitatively present for all subsets. Quantitatively though, they are stronger for the unemployed. While the stimulus elasticity of debt is -0.0080 for people who are not working when they get the stimulus, it is only -0.0052 for those holding jobs. Thus, people deleverage in response to the stimulus checks by 50% more when unemployed (see columns (1) and (2)).

When looking at the responses across income levels, the stimulus elasticity of -0.006 is statistically significant for the top three quartiles of the income distribution, but not for the bottom quartile (see columns (3) and (4)). The same result appears when we divide the population by income depending on whether they are below or above the mean (columns (5) and (6)).

In none of these subsamples does income (when measured as the average income over a year) seem to be a statistically significant factor in determining the amount of debt.

5.5 Heterogeneities by Credit Type

Important heterogeneities in the market for consumer credit and mortgages are evident in our dataset (see Table 2). Among people with consumer credit liabilities, total credit outstanding was on average \$141,475, with mortgage debt being on average \$191,920 and non-mortgage, \$9,832. Of the latter, the average credit card debt was \$6,394, and the sum of bank and other loans were \$14,917 on average. As a percentage of annual income, survey respondents held an average 35% and 15% of their annual income in bank loans and credit card debt, respectively. Total non-mortgage debt amounted to an average of 23% of annual income, and mortgage debt to 31.4 times annual income.

Given these differences, an interesting aspect to study is the potential for various types of credit to react to the stimulus receipt in heterogeneous ways. It could be that recipients decide to pay-off some types of debt and increase others, which would determine important reallocation within credit markets. To explore this idea, we rerun our empirical model separately for mortgage and non-mortgage debt, and within the latter category, for credit cards and bank loans. We present these results in Table 8.

These results indicate important heterogeneities in the qualitative responses. Mortgage debt responds negatively to stimulus payments, with an elasticity of -0.0041. Non-mortgage debt as a whole does not respond to fiscal stimulus payments. The composition of the effect is interesting since the stimulus elasticity of credit card debt is not statistically different from zero, but that of bank and other loans is negative. While the statistical precision of the estimate for the latter category is low, notice that only less than one third of the sample has non-missing data on these type of loans.

We see these results as adding value to those in Agarwal, Liu, and Souleles (2007) who focus on credit cards debt.

5.6 Demographic Patterns in Access to Consumer Credit

Next, we check whether and if so, how receiving the fiscal stimulus affects the patterns of debt across survey respondents. We choose to use demographic characteristics to measure credit constraints. We do so based on the literature that shows that individuals who belong to underrepresented minority groups consistently apply to credit less often, and borrow less when they do (see Blanchflower, Levine, and Zimmerman (2003), Asiedu, Freeman, and Nti-Addae (2012) and Robb, de Zeeuw, and Barkley (2018), among others). Also, Hai and Heckman (2017) present evidence that borrowing limits are a function of age and the amount of human capital accumulation. Borrowing limits generally increase with age for individuals from 17 to 30 and start falling after that. However, the growth rate of the natural borrowing limit is much higher for individuals with higher initial ability endowments.[8] With regards to the role of education in determining access to credit, Abbott et al. (2019) show that among working-age married households, the borrowing limit of $75,000 if the most educated spouse is a college graduate drops drastically to $25,000 if the most educated spouse is a high school graduate, and to $15,000 if both spouses are high

[8]For individuals at age 30, the borrowing limit increases monotonically with the number of years in school.

school dropouts.

Based on this evidence, we introduce a control for credit constraints, and we test how instituting a stimulus program affects these constraints. We show these results in Table 9.

In column (1) we show that the main results are robust to introducing the demographic controls. In particular, a 1% increase in the dollar value of the stimulus (an average of approximately \$1.75) induces a 0.0040% drop in mortgage liabilities by \$6.83 (from an average mortgage of \$170,736 for stimulus recipients).

In column (2) we show the relationship between age and American households' mortgage debt. While being 65 or older does not seem to significantly affect the amount of household liabilities, the stimulus elasticity increases (in absolute value) to 0.0136 for older individuals. This indicates that senior citizens are more prone to using stimulus checks to pay off their mortgages. When we look at the interaction variable between the stimulus and a dummy indicating whether the reference person in the consumption unit has any post-secondary education, we see that college educated individuals seem less likely to devote fiscal stimuli to paying-off debt (see column (3)). The coefficient on the interaction term is not statistically significant, but the positive sign is consistent with a story in which individuals with more human capital and thus, better labor market post-recession prospects, tend to not alter their debt portfolios as a result of fiscal packages. Gender also seems to be associated to how individuals decide to use stimulus checks. While only marginally significant, the negative coefficient on the interaction variable between the stimulus and a female dummy indicates that the stimulus elasticity is larger for females (see column (4)). In column (5) we study whether race plays any role in how recipients use the stimulus proceeds. The elasticity of interest is not significantly associated to race, as measured by a white/non-white dummy. In column (6) we show that urban households are less likely to pay-off debt with their stimulus. We take this result cautiously though since most survey respondents are indeed in urban areas, which diminishes the statistical power of the test. Finally, in column (7) we show that even though, intuitively, larger consumer units have higher outstanding balances, we cannot find significant evidence for larger households responding to the stimulus heterogeneously relative to smaller ones.

6 Conclusions and Policy Implications

The link between fiscal stimulus packages during times of crisis and household debt has not been studied in depth so far. Thus, there is no clear answer yet to the question of how receiving a stimulus check from the government affects households' decisions with respect to credit markets. Do beneficiaries devote the payment only to paying off debt (instead of increasing consumption)? Do they save it all as the permanent-income hypothesis literature would predict? Do they take on more short-term credit to help with increased consumption of durables that the stimulus might incentivize? Are lenders willing to lend more to stimulus recipients since they see the government support as some sort of "intangible collateral" that makes stimulus recipients more creditworthy in the eyes of lenders? These are questions that our paper tries to shed light on.

We uncover a statistically significant and economically meaningful negative relationship between stimulus receipts and the level of credit. We interpret this finding as evidence that consumer units tend to use a non-negligible share of their stimulus to pay-off debt.

With our results showing that stimulus recipients considerably reduce debt, it is possible that this type of policy could cause substantial deleveraging. Our results show that this is a potential outcome of future policies both at the aggregate level and in individual markets like mortgages and credit card debt.

Our results yield some worthy of note policy implications for stimulus payments in the wake of the coronavirus pandemic. The Coronavirus Aid, Relief and Economic Security (CARES) Act signed into law by President Trump on March 27^{th}, 2020 provides a stimulus package focused on two main goals: preserving jobs in American industries and providing direct assistance for small businesses and families.[9] If what happened back during the crisis of 2008/09 can be used as a predictor for the consumer credit markets response to the CARES Act, then it can be expected that American households will use part or all of these payments for deleveraging.

From a policy perspective, this result involves interesting trade-offs between the immediate and long-run consequences of fiscal stimulus packages. Less debt has obvious positive implications in the long-run but can mean weaker recovery patterns from the pandemic-induced recession in the short-run than if most rebates were allocated to increased consumption. Thus, understanding which of these two effects - the "spending effect" or the "deleveraging effect" - dominates is important for governments.

The literature finds evidence for both by the way. Baker et al. (2020) find that households, particularly low-income ones, substantially increase spending in the first weeks after

[9]The CARES Act provides up to $1,200 per adult and $500 per child under 17 years old and up to $1,200 to Social Security recipients who did not file tax returns in 2018 and 2019. This is for individuals whose income was less than $99,000 or $198,000 for joint filers.

receiving a rebate. They also find that stimulus receipts cause households to pay rents, mortgages, and credit cards, which is in line with our own findings. In contrast, Coibion, Gorodnichenko, and Weber (2020) find a very low impact on consumer spending. They find that households who receive a one-time stimulus primarily save it.

We see our work as a first step to fill a big gap in the literature and to address the questions with which we started this concluding remarks. Nevertheless, a couple caveats are worth mentioning. First, the coverage of the data on households' liabilities in the publicly available Consumer Expenditure Survey is inferior to that in proprietary datasets like the one in Agarwal, Liu, and Souleles (2007). Second, we have no way to link data on standards of lending from the supply-side of credit markets with our household-level credit data from the demand-side. This data constraint means there is still no real way to tell whether the supply of loans responds in any meaningful way when a credit applicant receives government stimulus money. Addressing these points is left for future research.

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Table 1: Data Summary Statistics

	N	**Mean**	**Std.Dev.**	**Min**	**Max**
Stimulus (current $)	5,372	926.5944	506.2863	1	3660
Stimulus (constant 2005 $)	31,032	175.7561	448.2486	0	4024.736
Stimulus (% of annual income)	31,032	0.3284	0.8884	0.0000	6.4302
Stimulus (% of annual expenditure)	31,032	0.5672	2.0667	0.0000	58.8882

Table 2: Credit Summary Statistics

	N	**Mean**	**Std.Dev.**	**Min**	**Max**
Total credit	16,214	141,475.7	168,251.2	1.0830	1,822,912
Non-mortgage	10,489	9,831.7	17,580.7	1.0830	241,618.1
Mortgage	11,415	191,919.7	170,797.8	579.4	1,822,912
Credit Cards	9,612	6,394.2	9,690.6	1.0830	149,625.9
Bank and other loans	2,793	14,917	25,027.1	17.3273	219,451.3

The types of loans included in each category are described in section 3.3 of the paper.
All variables are measured in constant US dollars of 2015.

Table 3: Data Correlation Matrix

	Total	**Mortga**
Total debt	1	
Mortgage	0.9897*	1
	(0.0000)	
Non-mortgage	0.5391*	0.1406
	(0.0000)	(0.0000
Credit cards	0.4954*	0.1499
	(0.0000)	(0.0000
Bank and other	0.4326*	0.1122
	(0.0000)	(0.0000
Stimulus	-0.0402*	-0.0453
	(0.0000)	(0.0000
Pre-tax income	0.3417*	0.2900
	(0.0000)	(0.0000

* significant at the 1% level, p<0.01.

Table 4: The Response of Liabilities to Stimulus Payments

PANEL A: OLS RESULTS						
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Stimulus$_{it}$ (logs)	-0.00553**	-0.00553**	-0.00560**			
	(0.012)	(0.012)	(0.011)			
Stimulus dummy				-0.0381**	-0.0381**	-0.0381**
				(0.013)	(0.013)	(0.012)
Pre-tax income$_{it}$ (logs)			0.0109			0.0108
			(0.285)			(0.285)
Constant	10.64***	10.64***	10.52***	10.64***	10.64***	10.52***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
N	16,214	16,214	15,926	16,214	16,214	15,926
R-sq	0.001	0.001	0.001	0.001	0.001	0.001
Time trend	NO	YES	YES	NO	YES	YES

p-values in parentheses
* p<0.10 ** p<0.05 *** p<0.01
All estimations include a time trend and consumer units (CU) fixed effects.

Table 5: The Response of Liabilities to Stimulus Payments: 2SLS Results

	(1)	(2)	(3)
Stimulus$_{it}$ (logs)	-0.00456*	-0.00457*	-0.00466*
	(0.063)	(0.062)	(0.056)
Pre-tax income$_{it}$ (logs)			0.00958
			(0.346)
Constant	10.66***	10.65***	10.55***
	(0.000)	(0.000)	(0.000)
N	16,214	16,214	15,926
Time trend	NO	YES	YES

p-values in parentheses
* p<0.10 ** p<0.05 *** p<0.01
All estimations include a time trend and consu

Table 6: Only Households Receiving Stimulus Payments

	(1)	(2)	(3)	(4)
	Stim>0		**Stim>0 & howused=3**	
Stimulus$_{it}$ (logs)	-0.159	-0.127	-0.174	-0.144
	(0.239)	(0.433)	(0.228)	(0.382)
Pre-tax income$_{it}$ (logs)		1.761		1.771
		(0.140)		(0.128)
Constant	11.72***	-8.055	11.89***	-7.867
	(0.000)	(0.540)	(0.000)	(0.535)
N	1,006	997	490	485
R-sq	0.107	0.322	0.116	0.332

p-values in parentheses
* p<0.10 ** p<0.05 *** p<0.01
All estimations include a time trend and consumer units (CU) fixed effects.

Table 7: Unemployment, Income and Households' Debt

	(1) Unemployed	**(2) Employed**	**(3) Bottom 25th quartile**	**(4) Top quartiles**	**(5) Below mean**	**(6) Above mean**
Stimulus$_{it}$(logs)	-0.0080***	-0.0052*	-0.0058	-0.0060***	-0.0027	-0.0067**
	(0.008)	(0.057)	(0.626)	(0.007)	(0.524)	(0.011)
Pre-tax income$_{it}$(logs)	0.0083	0.0089	0.0289	0.0350	0.0206	-0.0060
	(0.542)	(0.472)	(0.539)	(0.267)	(0.538)	(0.886)
Constant	9.814***	10.75***	8.992***	10.50***	9.225***	11.25***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
N	3,627	12,299	2,394	13,532	4,751	11,175
R-sq	0.006	0.001	0.001	0.001	0.001	0.001

p-values in parentheses
*p<0.10 **p<0.05 ***p<0.01
All estimations include a time trend and consumer units (CU) fixed effects.

Table 8: Response by Type of Credit

	(1) Mortgage	**(2) Non-mortgage**	**(3) Credit Cards**	**(4) Bank and other**
Stimulus$_{it}$ (logs)	-0.00412**	0.000428	0.00121	-0.00302
	(0.033)	(0.839)	(0.575)	(0.285)
Pre-tax income$_{it}$ (logs)	0.00966	0.0414**	0.0295*	0.121
	(0.265)	(0.041)	(0.098)	(0.179)
Constant	11.72***	7.641***	7.498***	7.083***
	(0.000)	(0.000)	(0.000)	(0.000)
N	11,197	10,369	9,505	2,679
R-sq	0.001	0.003	0.002	0.011

p-values in parentheses
* p<0.10 ** p<0.05 *** p<0.01
All estimations include a time trend and consumer units (CU) fixed effects.

Table 9: Demographics and Households' Mortgage Debt

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Stimulus$_{it}$ (logs)	-0.00401**	-0.00335*	-0.0194	-0.00107	-0.00225	-0.0183	-0.00696
	(0.038)	(0.099)	(0.188)	(0.633)	(0.687)	(0.125)	(0.128)
Pre-tax income$_{it}$ (logs)	0.00867	0.00883	0.00885	0.00852	0.00865	0.00844	0.00861
	(0.318)	(0.309)	(0.309)	(0.326)	(0.319)	(0.330)	(0.321)
Age dummy	0.0702	0.0706	0.0691	0.0709	0.0701	0.0713	0.0693
	(0.369)	(0.366)	(0.382)	(0.363)	(0.369)	(0.361)	(0.375)
Age*stimulus$_{it}$		-0.0103*					
		(0.080)					
Education dummy	-0.0347	-0.0349	0.0374	-0.0339	-0.0346	-0.0349	-0.0344
	(0.847)	(0.846)	(0.430)	(0.850)	(0.847)	(0.846)	(0.848)
Education*stimulus$_{it}$			0.00112				
			(0.277)				
Female dummy	-0.156	-0.156	-0.123	-0.153	-0.155	-0.157	-0.156
	(0.327)	(0.326)	(0.427)	(0.338)	(0.328)	(0.324)	(0.327)
Female*stimulus$_{it}$				-0.00579			
				(0.131)			
White dummy	-0.242	-0.242	-0.238	-0.238	-0.240	-0.241	-0.241
	(0.209)	(0.209)	(0.217)	(0.217)	(0.213)	(0.210)	(0.211)
White*stimulus$_{it}$					-0.00201		
					(0.736)		
Urban dummy	0	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)	(.)
Urban*stimulus$_{it}$						0.0157	
						(0.192)	
Family size	0.0212**	0.0212**	0.0208**	0.0216**	0.0212**	0.0209**	0.0211**
	(0.032)	(0.031)	(0.035)	(0.029)	(0.031)	(0.037)	(0.032)
Family size*stimulus$_{it}$							0.000969
							(0.440)
Constant		11.97***	11.42***	11.97***	11.98***	11.98***	11.98***
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
N	11,197	11,197	11,197	11,197	11,197	11,197	11,197
R-sq	0.002	0.002	0.003	0.002	0.002	0.003	0.002

The dependent variable is mortgage debt measured in constant 2015 dollars and in logs.
p-values in parentheses
* p<0.10 ** p<0.05 *** p<0.01
All estimations include a time trend and consumer units (CU) fixed effects.