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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 tends to translate into a reduction in outstanding liabilities for American households. This effect is robust to controlling for income levels and household size. The effects are driven by households whose income is below the median and by those who remain employed during the crisis.

KEYWORDS
borrowing and liquidity constraints, household debt, stimulus packages

JEL CLASSIFICATION
D12, E21, E62, H24, H31

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 COVID-19 pandemic, much political debate has happened in the last few months around whether further stimulus should be directed mostly to firms or to households. Existent literature has a lot to say about the latter, in particular about the effects of stimulus payments on recipients' consumption patterns (see Agarwal et al., 2007; Bodkin, 1959; Johnson et al., 2006; Parker, 1999; Souleles, 1999 and, 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 the market for debt 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 seems to remain 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' liabilities were impacted by these policies.

We can think of three ways in which receiving a stimulus payment can affect household debt. On the one hand, the government checks should directly lower the need for short-term debt 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 or if stimulus packages induce macroeconomic recovery, 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 debt held by households or, at least, weaken the dependence of debt holdings 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, which we discuss in detail in Section 2. We formulate an empirical model building on Johnson et al. (2006) and references therein.[1] Taking advantage of the data at the household-level, we estimate the impact of stimulus on debt.

We use the Consumer Expenditure Surveys (CES) of 2008, which allow us to combine information on stimulus payments with liabilities, 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 et al. (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.

This effect is present for each type of debt as well as for the total, and it is robust to controlling for income levels and household size. The effects are driven by households whose income is below the median and by those who remain employed during the crisis.

We also look at the response of different types of debt separately, including mortgages, credit card debt, and 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.

Last, to test whether the response of debt to stimulus payments varies across demographic characteristics including age, gender, and education among others, 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, whites and females, who are more likely to use stimulus payments to pay down debt.

The rest of the paper is organized at follows. Section 2 situates the environment within which the Economic Stimulus Act was implemented. Section 3 contains a review of the literature to which we aim to contribute. Section 4 describes the data, and Section 5 introduces the empirical methodology. Section 6 presents the main results as well as several robustness checks. The final section concludes and discusses policy implications for stimulus spending under the CARES Act, the COVID-related Tax Relief Act of 2020, and the American Rescue Plan Act of 2021.

2 | THE POLICY ENVIRONMENT

The 2008–2009 financial crisis and the ensuing Great Recession is often blamed on subprime mortgage practices and lax lending standards that contributed to over borrowing for many American households. The banks and subprime lenders then took these mortgages and sold them on the secondary market in order to free up money to grant more mortgages. The financial firms that bought those mortgages repackaged them into bundles and resold them to investors as mortgage-backed securities. As a result, US homeownership had peaked at 69% by 2004. Early in 2006 home prices started falling, which caused significant hardship to families whose mortgage balances would now exceed the market value of their assets.[2] By mid-2007 the problem was extending to other countries through interbank markets that were essentially frozen. Banks and other lenders were filing for bankruptcy in the United States and abroad. As a result, the Federal Reserve and other central banks started coordinated policies to provide relief in credit markets (Neil-Baily et al. (2008), Park-Lazette (2017)).

In January of 2008, the Fed cut its benchmark rate by three-quarters of a percentage point—its biggest cut in 25 years. By the summer of 2008, the two largest mortgage lenders, Fannie Mae and Freddie Mac, had been nationalized by the US federal government. Financial markets were recording the worst losses in history. In October of 2008 Congress approved the very controversial Wall Street bailout package through which the government purchased "toxic"

loans from banks and bank shares. Before recovering in later years, stock markets reached their lowest point in March 2009.

Even in the presence of these measures, the economic damage was one of the worst in history. GDP growth fell from about 4% a year in 2008 to an average of only 1.5% a year in 2010. Unemployment reached 10% and about 3.8 million Americans lost their homes.

The Economic Stimulus Act was passed by the Senate in February of 2008 and signed into law by President George W. Bush to provide relief to consumers and businesses from this severe economic slowdown. As explained in detail in USCongress (2008), the law provided for tax rebates to low- and middle-income U.S. taxpayers,[3] 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 individuals[4] 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.[5]

Furthermore, in 2010, the Dodd-Frank Wall Street Reform and Consumer Protection Act[6] was passed with the objective of preventing the subprime-type of risky lending practices by banks. The Act restricts some of the riskier activities of large banks, increases government oversight of these activities, and requires banks to hold larger reserves.

In this paper we aim to focus on the rebates to low- and middle-income taxpayers under the 2008 Act and how they affected households' decisions in credit markets. After a review of previous literature, we present the empirical model and details on the data used to estimate it.

3 | RELATED LITERATURE

There is no consensus in the literature on the effects of government spending on credit markets. Most theoretical macro-models predict that increases in government spending tighten 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), Fisher and Peters (2010) even find that fiscal spending causes interest rates to fall. More recently, Auerbach et al. (2020), Gilje et al. (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 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), Browning and Collado (2001) find no response of consumption to these payments, which supports the PIH. Similarly, Shapiro and Slemrod (2003, 2009) analyze the survey data from the 2001 and 2008 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 et al. (2006), Parker et al. (2013), Broda and Parker (2014), and Parker (2017). Johnson et al. (2006) and Parker et al. (2013) are particularly noteworthy since, unlike their predecessors, they exploit the random timing of the stimulus payments 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 2001 and 2008 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 et al. (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 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.[7]

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 how consumption (and therefore, savings) respond to unexpected changes in income do not necessarily say much about how debt reacts to these changes.

To the best of our knowledge, only Agarwal et al. (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 et al. (2007) focus on one type of debt, credit card debt, which amounts to only 40% of total debt in our data. 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%. 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, do 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 on demographic characteristics of the account holders like in our case.

Also, Demyanyk et al. (2019) do take into account debt levels but only as an additional determinant of how consumption responds to these packages.[8]

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

4 | THE DATA

We use the data collected by the U.S. Census Bureau for the Bureau of Labor Statistics under the CES program. The CES provide data on expenditures, income, and various demographics for a representative random sample of U.S. households. We use the interview surveys, which are based on a sample double in size compared to the diary surveys. 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 CES data are provided at the level of the consumer unit (CU). The surveys define a CU 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 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 CES, 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.

4.1 | 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* (income before tax) in the survey.

Income is defined as the aggregated value of all incomes for 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.[9]

During the Great Recession of 2007-08 and its aftermath, our period of analysis, 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.

4.2 | 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. Out of 6,417 households in the survey sample who got rebates, 1096 of them qualified for $300; 2276 for $600, 340 for $900, 1372 for $1,200, 326 for $1,500, 292 for $1,800, 99 for $2100 and 30 for $2,400, with the others receiving "non-round" numbers because of the way the rebate phased out for every dollar in excess of the income threshold.[10]

The stimulus amounts 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 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, payments were on average 0.33%. Furthermore, stimulus represented 0.57% of annual expenditure.

The summary statistics for stimulus data are presented in Tables 1 and 2.

4.3 | Household debt

We use data on the total amount owed to all credit sources as well as on each source of debt including mortgages, credit cards, bank 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

TABLE 1 Data summary statistics

	N	Mean	Std. Dev.	Min	Max
Stimulus and income (in 2021 dollars)					
Stimulus	16,222	81.02	351.49	0	3771.68
Stimulus (per capita)	16,222	31.63	134.23	0	2228.65
Stimulus (% of income)	16,222	0.12	0.59	0	8.75
Income (pre-tax)	16,222	92,898.57	83,219.85	0	1,403,019
Debt (in 2021 dollars)					
Total	16,222	161,657.5	192,311	1.24	2,084,124
Non-mortgage	10,487	11,204.12	20,029.65	1.24	276,240.4
Mortgage	11,423	219,286.6	195,229.1	662.40	2,084,12
Credit cards	9612	7311.21	11,082.16	1.24	171,066.3
Bank and other loans	2787	16,943.78	28,499.96	19.81	250,897.2
Demographics					
Family size	16,222	2.75	1.5	1	12
Female dummy	16,222	0.52	0.5	0	1
Age (in years)	16,222	47.82	14.59	16	87
College dummy	16,222	0.13	0.34	0	1
Non-minority dummy	16,222	0.84	0.37	0	1
Urban dummy	16,222	0.95	0.23	0	1

Note: The data referenced to here corresponds to all CUs in the sample including those who did not qualify for a stimulus payment with non-missing data on debt holdings. 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, 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. The variable college dummy equals 1 for consumer units in which the survey respondent indicates to have at least 16 years of education. The variable non-minority dummy equals 1 when the survey respondent is white. Urban dummy equals 1 when the consumer unit is located in an urban area.

TABLE 2 Data summary statistics: stimulus recipients only

	N	Mean	Std. Dev.	Min	Max
Stimulus and income (in 2021 dollars)					
Stimulus	1025	1282.23	644.475	14.86	3771.68
Stimulus per capita	1025	500.57	224.65	7.43	2228.65
Stimulus (% of income)	1025	1.94	1.4	0	8.75
Income (pre-tax)	1025	87,225.22	57,891.02	0	523,851
Debt (in 2021 dollars)					
Total	1025	146,004.7	162,024.8	37.72	1,303,625
Non-mortgage	699	11,961.42	20,656.66	37.14	186,215.9
Mortgage	730	193,553.1	158,192.4	748.05	1,278,480
Credit cards	655	7956.88	12,029.57	37.14	125,722.8
Bank and other loans	189	16,662.85	28,815.25	49.53	173,339.2
Demographics					
Family size	1025	2.87	1.503237	1	11
Female dummy	1025	0.53	0.5	0	1
Age (in years)	1025	47.41	14.17	17	87
College dummy	1025	0.10	0.30	0	1
Non-minority dummy	1025	0.85	0.36	0	1
Urban dummy	1025	0.93	0.25	0	1

Note: The data referenced to here corresponds to stimulus recipients only with non-missing data on debt holdings. 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, 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. The variable college dummy equals 1 for consumer units in which the survey respondent indicates to have at least 16 years of education. The variable non-minority dummy equals 1 when the survey respondent is white. Urban dummy equals 1 when the consumer unit is located in an urban area.

stores), or major credit cards (VISA, Master Card, American Express, Discover or other revolving 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,[11] 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 1 we present detailed summary statistics on household liabilities for all CUs, including those who did not qualify for government support during the 2008 crisis. There we show that the average US household had approximately $219,000 in mortgage and another $11,200 in consumer credit, which amount to 31 times and 25% their annual income, respectively.[12] 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 represented 32%. In Table 2 we present the same set of summary statistics for the subsample of stimulus recipients.

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 all far from 1 is evidence for the importance of studying the stimulus elasticity for the aggregate households' debt as well as for each of the individual types of debt separately. 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. We test this hypothesis through the empirical model that we introduce in Section 5.

TABLE 3 Data correlation matrix

	Total	Mortgage debt	Non-mortgage	Credit cards	Bank and other loans	Stimulus	Income (pre-tax)
Total debt	1						
Mortgage	0.9897*	1					
	(0.0000)						
Non-mortgage	0.5391*	0.1406*	1				
	(0.0000)	(0.0000)					
Credit cards	0.4954*	0.1499*	0.8623*	1			
	(0.0000)	(0.0000)	(0.0000)				
Bank and other	0.4326*	0.1122*	0.8302*	0.1368*	1		
	(0.0000)	(0.0000)	(0.0000)	(0.0000)			
Stimulus	−0.0402*	−0.0453*	−0.0194*	−0.0077	−0.0292	1	
	(0.0000)	(0.0000)	(0.0048)	(0.2867)	(0.0251)		
Pre-tax income	0.3417*	0.2900*	0.2419*	0.2551*	0.1593*	0.0609*	1
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	

Note: All variables are in logs. The cells represent the unconditional correlation coefficients between the variables on each row and each column. The significance level is in parenthesis. *Significant at the 10% level, $p < 0.1$.

5 | EMPIRICAL METHODOLOGY

To formulate our empirical model we build on Johnson et al. (2006). However, while they use first differences (as opposed to levels) as their dependent variable, due to the nature of the data on debt outstanding, we need to work with levels. While the consumption data they use is available for the same CU for several consecutive periods, this is not the case for the data on debt, and formulating the model in terms of changes in debt is not a possibility.[13]

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

$$L_{i,t} = \alpha + \beta_1 Y_{i,t} + \beta_2 S_{i,t} + \beta_3 R_{i,t} + \theta_i + \omega_t + u_{i,t} \quad (1)$$

where L (for loans) is debt outstanding in each period, Y is gross-income (pre-tax), S (for size) is the number of members in the CU, and R is the rebate amount received by the household (all in levels and in constant 2021 dollars).[14] θ_i are the fixed effects at the CU level, and ω_t are quarterly fixed effects.

Our sample shows important differences in the level of debt among households located in different income brackets. For example, the cross-sectional average of the total amount of debt was $5470.28 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 $126,000 in debt, its counterpart in the top quartile held $218,000. 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 debt 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 debt. A positive sign is expected for the $\widehat{\beta}_1$ coefficient.

For $\widehat{\beta}_2$ we expect a positive sign since larger households are expected to have larger amounts of debt even if the amount of liabilities per capita is the same. Also, larger families are expected to have more housing debt outstanding.

We have no strong priors on what sign to expect for $\widehat{\beta}_3$. A negative coefficient would signal a diminished need for credit among recipients and/or they using these payments to pay down past debt. A positive coefficient could be explained by recipients being now able to afford more high ticket items (like durables) and buying them on credit. Alternatively, a positive $\widehat{\beta}_3$ could indicate 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. It is important to clarify that our data does not really allow us to interpret a positive $\widehat{\beta}_3$ as indicating a story of credit constraints alleviation because we cannot identify the behavior of lenders on the supply-side.

To further understand if there are asymmetries across demographic characteristics in the way that households react to the receipt of these benefits, we run an extended version of the model where we incorporate among the regressors information on the households' age, education, gender, race and location as in Equation (2).

$$L_{i,t} = \alpha + \beta_1 Y_{i,t} + \beta_2 S_{i,t} + \beta_3 R_{i,t} + \sum_{m=1}^{M} \beta_{3m} X_{i,t,m} + \sum_{m=1}^{M} \beta_{4m} X_{i,t,m} * R_{i,t} + \theta_i + \omega_t + u_{i,t} \quad (2)$$

where X_m is one of $M = 5$ demographic controls. The demographic information includes the age of the reference person (*age*), a dummy variable that equals one when the reference person has a college degree (*college − dummy*), two dummies for the reference person in the CU being female or white (*female − dummy* and *white − dummy*), respectively, and a dummy for the CU being located in an urban area (*urban − dummy*).

We also include interaction variables between the demographic characteristics and the stimulus variable. Since we think of these characteristics as indicative of factors aside from income that might influence a household's level of debt and how they use stimulus payments, the estimated coefficients $\widehat{\beta}_4$ on the interactions allow us to test whether the effect of stimulus on debt is a function of these characteristics.

6 | RESULTS

6.1 | The response of household debt to fiscal stimulus packages

We study how households' liabilities responded to rebate payments during the 2008 Economic Stimulus policy. In Table 4 we first show these results from standard OLS estimations for the entire population including those who did not earn any stimulus. We present this benchmark set of results for both debt and stimulus measured in logs as the dependent variable in columns (1)–(4); and for both variables in levels in columns (5)–(6).

The coefficient on the stimulus variable in (1)–(4) can be interpreted as a stimulus-elasticity of debt, that is, the percentage change in outstanding liabilities for the CU after a 1% increase in stimulus payments. We find that when accounting for both time and consumer units fixed effects, a 1% increase in the amount of stimulus spending yields a 0.00497% decrease in consumers' debt, and that this is robust to controlling for the households pre-tax income. The elasticity is significantly larger (−0.0202) when unobserved variation at the level of the CU is not accounted for. This last finding leads us to conjecture that there exist household demographic characteristics that are important in explaining how consumers respond to government aid packages. We explore this topic later in Section 6.6.

TABLE 4 The response of liabilities to stimulus payments

	Total debt (in logs)				Total debt (in levels)	
	(1)	(2)	(3)	(4)	(5)	(6)
Stimulus	−0.0146	−0.00496**	−0.0202**	−0.00497**	−6.088*	−1.566
	(0.132)	(0.019)	(0.034)	(0.019)	(0.088)	(0.315)
CU size	0.332***	−0.00821	0.297***	−0.00905	12,210.3***	1969.1
	(0.000)	(0.711)	(0.000)	(0.683)	(0.000)	(0.377)
Income (pre-tax)			0.220***	0.00673	0.923***	0.0163
			(0.000)	(0.162)	(0.000)	(0.730)
N	16,222	16,222	16,222	16,222	16,222	16,222
R^2	0.048	0.001	0.078	0.001	0.182	0.000
CU FE	NO	YES	NO	YES	NO	YES
Quarterly FE	YES	YES	YES	YES	YES	YES

Note: *p*-values in parentheses *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Standard errors are robust to heteroskedasticity. The stimulus and income variables are measured in logs for columns (1)–(4) and in levels for columns (5)–(6). CU size is the number of individuals in the consumer unit as defined by the Consumer Expenditure Survey. CU FE stands for consumer units fixed effects. Quarterly FE stands for quarter fixed effects. All specifications include quarterly fixed effects.

Interpreting these results as elasticities when both the dependent and the independent variable of interest are measured in logs can become misleading because the average value of the stimulus payments per CU (approximately $1300) is so drastically different from the average debt level (approximately $146,000). Expressing both debt and stimulus in levels (as we do in columns (5)–(6)) is more intuitive and the coefficient of interest shows the dollar change in debt associated with a $1 of stimulus. When we do so, we find that $1 in stimulus checks translates into debt falling by approximately $6. Importantly, when we introduce controls for time-invariant household characteristics through fixed effects at the level of the CU, the effect becomes weaker in size and statistically insignificant as well.

These initial benchmark results are consistent with a deleveraging effect of policies that target consumers during times of crises. However, we need to explore further potential endogeneity in the stimulus variable itself and a possible selection issue by looking at the subsample of consumers who did receive a payment from the government. We do so in the 6.2 and 6.3.

6.2 | Endogeneity

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 CU, which could potentially also be highly correlated with income and therefore, with consumption since consumption and income are so highly correlated with each other.

Therefore, previous work focusing on consumers' expenditure as the dependent variable has considered the potential for the independent stimulus variable being endogenous to income and therefore to the dependent variable, consumption. This is less of a concern for the case of outstanding debt since policies designing the allocation of benefits tend to look at income data (highly correlated with consumption), but not at households' balance sheets.

In any case, as a robustness check here we study the potential for $\widehat{\beta}_3$ to be biased by endogeneity. 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 et al. (2006) who exploit the random timing of the stimulus. The particular 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 level of debt.

The results from these 2SLS estimations show qualitatively similar responses of debt to stimulus as those of our OLS results.[15] The coefficients are larger in magnitude than for the case of the OLS regressions, which is related to the fact that in the first stage estimations the predicted stimulus payments are $1160, lower than the actual mean payments of $1282 (see Table 2).

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.

The non-significance of some of the coefficients presented in Table 4 for the entire dataset (when non-recipients are included) could be explained by the fact that 93% of our sample of consumers with positive outstanding liabilities corresponds to non-stimulus recipients for whom there is no variability in the independent variable (they all get zero). We explore this issue in the next subsection where we measure the effect among stimulus recipients only, so that identification comes from the variation in payment amounts conditional on qualifying for government support during the crisis.

6.3 | Selection—Only households receiving the stimulus

The lack of statistical significance for the stimulus coefficient when all consumers (even those who did not qualify for government aid) are included in the sample and households characteristics are accounted for through CU fixed effects underscores the need to study the variability within subsample of recipients to determine whether there is a significant effect of stimulus packages on debt. This is important since more than 90% of the survey respondents did not get a stimulus.

What we do here is to explore the effects on debt by first, including only households who got rebate payments, and second, households who got the rebates and replied to a qualitative question in the survey saying 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 variation in the amount of the stimulus conditional on receipt. The cost of this approach is that the sample size falls significantly.

These results are presented in Table 5, 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 debt by $67.11. The size of the effect is approximately the same ($66.58) for the second subgroup. When controlling for income these results retain their statistical significance; the value of the coefficients is slightly lower in size, though.[16]

These results confirm that there is some indication that the stimulus packages themselves or the recovery associated to them might have incentivized consumers to deleverage. Results are statistically stronger when identification comes only from variation in the amount of the checks and not from whether survey participants qualified for these payments.

In what follows we keep working with the group of individuals who did qualify for stimulus and for whom we find significant effects even with consumer units fixed effects. We explore some further heterogeneities in their debt responses based on employment status, income, type of debt and demographics.

6.4 | Subsample heterogeneities

In this section we explore the potential for the impact of stimulus payments being heterogeneous across subsamples of the population.

During the crisis of 2008 the payments were not akin to a "lottery win" during a normal period, but a response to real increases in hardships. For example, some payments were received by consumers suffering from food insecurity which rose to record highs during the Great Recession and by households trying to avoid foreclosures and other types of default. Thus, it is important to consider potentially heterogeneous responses depending on the labor status of the recipients, and on their pre-crisis income level.

In this subsection, we look at the effects separately for the unemployed and the employed at the time of receiving the fiscal stimulus, and for each quartile of the income distribution. Results are presented in Tables 6 and 7.

The deleveraging effects show up only for the employed, who lower their outstanding liabilities by almost $57 for every $1 received in stimulus payments (see column (2) of Table 6). Conversely, the unemployed increase their debt

TABLE 5 The response of liabilities to stimulus payments: Stimulus recipients only

	Total debt (in levels)			
	Non-zero stimulus		**Stimulus used to pay-off debt**	
	(1)	**(2)**	**(3)**	**(4)**
Stimulus	−67.11**	−57.93***	−66.58**	−57.16***
	(0.042)	(0.003)	(0.036)	(0.001)
CU size	205,444.5***	232,383.3***	205,200.0***	232,109.9***
	(0.001)	(0.000)	(0.001)	(0.000)
Income (pre-tax)		3.307***		3.314***
		(0.001)		(0.000)
N	1025	1025	498	498
R^2	0.724	0.855	0.735	0.866
CU fixed effects	YES	YES	YES	YES
Quarterly dummies	YES	YES	YES	YES

Note: *p*-values in parentheses *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Standard errors are robust to heteroskedasticity. CU size is the number of individuals in the consumer unit as defined by the Consumer Expenditure Survey. Both the stimulus and income variables are measured in levels. The reported results are from OLS models. All specifications include CU fixed effects and quarterly dummies. In columns (1)–(4) we use only observations with non-missing data for the stimulus payments. In columns (3)–(4) we further restrict the sample to those consumer units indicating (in response to a qualitative question in the survey) that they used the stimulus mostly to pay off debt (as opposed to increase consumption or to increase savings).

TABLE 6 Unemployment status and households' debt

	Total debt (in levels)	
	Unemployed (1)	**Employed (2)**
Stimulus	36.89**	−56.78***
	(0.045)	(0.002)
CU size	12,098.20	231,986.9***
	(0.132)	(0.000)
Income (pre-tax)	0.929**	3.316***
	(0.011)	(0.001)
N	225	800
R^2	0.186	0.857
CU FE	NO	YES
Quarterly FE	YES	YES

Note: *p*-values in parentheses *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Standard errors are robust to heteroskedasticity. An observation is coded as coming from an unemployed individual if either the number of weeks worked by the reference person full or part-time in the last 12 months is zero or the individual has declared a positive amount received in the form of unemployment compensation. We cannot have CU fixed effects in the subsample of unemployed individuals due to small sample size of $N = 225$. The reported results are from OLS models. Only stimulus recipients are included in this regression.

TABLE 7 Income and households' debt

Total debt (in levels)						
	Income quartiles					
	Bottom (1)	**Second (2)**	**Third (3)**	**Top (4)**	**Below median (5)**	**Above median (6)**
Stimulus	96.25***	−10.66***	4.063***	−11.95	−7.070***	1.188
	(0.000)	(0.000)	(0.000)	(.)	(0.000)	(0.266)
CU size	-	345,908.0***	−4840.2***	-	348,274.2***	36,809.9***
	-	(0.000)	(0.000)	-	(0.000)	(0.000)
Income	-	-	-	-	1.137***	2.004***
	-	-	-	-	(0.000)	(0.000)
N	257	256	256	257	513	512
R^2	1.000	1.000	1.000	1.000	1	0.999
CU FE	YES	YES	YES	YES	YES	YES
Quarter FE	NO	YES	YES	NO	YES	YES

Note: *p*-values in parentheses *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Standard errors are robust to heteroskedasticity. The subsamples used for columns (1)–(4) correspond to CUs whose income is within each of the four quartiles of income, respectively. In these, the income variable is dropped due to collinearity. The subsamples used for columns (5) and (6) correspond to CUs whose income is below and above the median, respectively. The reported results are from OLS models. Only stimulus recipients are included in this regression.

levels by almost $37 when receiving $1 in government support (see column (1)). The result seems to be consistent with the fact that, if remaining employed during the crisis, people could afford to use the stimulus to payoff previously acquired debt, whereas, if unemployed, the average stimulus per capita of $500 was certainly not enough to make ends meet and households still had to increase their debt holdings despite receiving government support.

In Table 7 we study the response of debt across income levels and show significantly different elasticities for households at different quartiles of the income distribution. While the debt response to stimulus is negative and statistically significant for the second quartile of the income distribution (see column (2)), families in the bottom and the

third quartiles increase their debt holdings during the period of the stimulus (see columns (1) and (3)). When we divide the population by income depending on whether they are below or above the median (columns (5) and (6)), we show that the deleveraging effect is present for households with income below median but no significant effect for those with income above the 50th percentile. The fact that consumers at the very low end of the income distribution do not exhibit the deleveraging effect, but actually increase their debt holdings in periods of crises is not surprising since the average stimulus per capita was still low. This is consistent with what we found before for unemployed consumers. The deleveraging effect is present when looking at all consumers with below median income. The fact that debt for consumers in the second and third quartiles is affected differently and that those in the third behave like the low(est) income individuals is interesting. However, the size of our dataset does not allow us to look further into that third quartile to try to understand the reasons, like whether this is driven by a particular type of loans. What we do know is that if we introduce an extra regressor consisting of the interaction term between stimulus and income, for consumers in the third quartile, we also uncover a deleveraging effect and a $1 stimulus payment lowers debt outstanding by $3.85.

6.5 | Heterogeneities by debt type

There are a few reasons why looking at different types of debt separately is important.

On the one hand, since mortgage rates are lower than rates on credit card debt, households might choose to use stimulus checks to pay credit card debt first, and mortgages only later. This would imply a weaker deleveraging effect, that is, a lower (in absolute value) stimulus elasticity of debt when mortgage debt is included in total liabilities. Also, there might be penalties for paying mortgages early which would discourage families from using their checks to do so. On the other hand, since the Great Recession was accompanied by a financial crisis and a bursting of the housing bubble, and with many subprime mortgages being adjustable-rate ones, families might have had stronger incentives to pay down this debt instead of other loans. If this is the case, the stimulus elasticity of total debt (including mortgage debt) is expected to be larger than that of just consumer credit.

Ultimately then, there is no clear direction of the bias that might be generated by including mortgages in total household debt. This is an important empirical question, and we need to look at mortgage debt separately to answer it. To contribute to this analysis, we now estimate the stimulus elasticity of debt separately for each category of loans.

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 debt outstanding (in 2021 dollars) was on average $146,005, with mortgage debt being on average $193,553 and non-mortgage, $11,961. Of the latter, the average credit card debt was $7,957, and the sum of bank and other loans were $16,663 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 debt 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. Also, we see these results as adding value to those in Agarwal et al. (2007) who focus on credit cards debt exclusively.

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 plus other loans.[17] We present these results in Table 8.

We uncover important heterogeneities in the response of debt to stimulus payments and we show that the deleveraging effect that we documented in the initial results is driven by particular types of debt. Mortgage debt responds negatively to stimulus payments while non-mortgage debt as a whole increases in response to fiscal stimulus payments. The composition of the effect is interesting since the stimulus elasticity of credit card debt is positive, but that of bank and other loans is negative. When stimulus recipients decide to use benefits to payoff debt, they choose to payoff mortgages and bank loans. At the same time, it seems that receiving a stimulus induces American consumers to engage in more credit card debt. This last finding could be explained by what Coibion et al. (2020) report that American consumers spent almost 90% of their stimulus payments on large durable goods. If the average price of these durables was higher than the average stimulus payment, potentially consumers would have had to use their credit cards to afford these purchases.

Our result of deleveraging of mortgage debt is also in line with those in Andersson et al. (2013) who study the pecking order in which consumers default on their debt. They report that consumers are eight times more likely to prioritize mortgage payments over credit card payments.

TABLE 8 Response by type of debt

	Mortgage debt (1)	Non-mortgage debt (2)	Credit cards (3)	Other (4)
Stimulus	−7.913***	0.287*	0.279*	−0.214***
	(0.005)	(0.085)	(0.051)	(0.000)
CU size	348,687.0***	−1223.3	−1163.9*	.
	(0.000)	(0.122)	(0.079)	.
Income (pre-tax)	1.923***	−0.0551	−0.0493*	0.434***
	(0.000)	(0.103)	(0.072)	(0.000)
N	730	699	655	189
R^2	0.999	0.712	0.708	1
CU FE	YES	YES	YES	YES
Quarterly FE	YES	YES	YES	YES

Note: *p*-values in parentheses $^*p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Standard errors are robust to heteroskedasticity. Non-mortgage debt (column (2)) is composed by credit card debt (column (3)) and bank and other types of loans (column (4)). The reported results are from OLS models. Only stimulus recipients are included in this regression. All specifications include CU fixed effects and quarterly dummies. Only households with non-zero stimulus data are used for this estimation.

6.6 | Demographic patterns in debt management

Next, we check whether the way in which receiving the fiscal stimulus affects debt depends on the demographic characteristics of survey respondents. We do so based on the literature that shows that individuals who belong to under-represented minority groups consistently apply to credit less often, and borrow less when they do (see Asiedu et al. (2012), Blanchflower et al. (2003), Robb et al. (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.[18] With regards to the role of education in determining debt levels, 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 school dropouts.

Based on this evidence, we rerun the estimations introducing demographic controls for age, education, gender, race and urban versus rural location. We show these results in Table 9.

In columns (1) and (2) we show that the main results about deleveraging are not robust to introducing the demographic controls. In particular, a $1 increase in the dollar value of the stimulus induces an *increase* in total liabilities of between $17.47 and $20.[18] depending on how education and age are measured.[19] It is evident from these results that all else equal, younger and more educated individuals borrow more.[20] Also, urban households borrow an average of approximately $34.5 K more than those located in rural areas. Finally, gender and race seem to not have statistically significant impacts on debt levels.

In column (3) we show the relationship between age and American households' debt response to stimulus. The negative and significant coefficient for the interaction term between stimulus and age indicates that the deleveraging effect documented before is driven in part by older consumers being more likely than their younger counterparts to use the stimulus payments to pay down debt. With the mean age in the sample being 47, a $1 increase in stimulus payments would raise debt by approximately $15 for a consumer of average age.

Furthermore, when we look at the interaction variable between the stimulus and a dummy indicating whether the reference person in the consumption unit has a college degree, we see that college educated individuals do not seem significantly more or less likely to devote fiscal stimuli to paying down debt (see column (4)).

Gender also seems to be associated to how individuals decide to use stimulus checks. 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 (5)).

TABLE 9 Demographics and households' mortgage debt

	Total debt (in levels)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Stimulus	17.47*	20.18**	57.73**	17.52*	38.64***	61.36***	−0.737
	(0.060)	(0.034)	(0.027)	(0.077)	(0.002)	(0.002)	(0.951)
CU size	7062*	6474	6772	4962	5234	4729	5093
	(0.100)	(0.132)	(0.113)	(0.257)	(0.237)	(0.278)	(0.246)
Income	0.771***	0.858***	0.786***	0.870***	0.861***	0.872***	0.872***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Age	−880.5***		109.1	−1046.8***	−1111.8***	−1116.7***	−1049.2***
	(0.003)		(0.852)	(0.001)	(0.000)	(0.000)	(0.001)
Education	15,639***		15,774***				
	(0.000)		(0.000)				
Female	−7934	−5926	−9365	−6189	39,385**	−6662	−6228
	(0.397)	(0.536)	(0.319)	(0.516)	(0.034)	(0.484)	(0.514)
White	2156	2349	336.3	4012	3342	64,305***	3717
	(0.866)	(0.856)	(0.979)	(0.755)	(0.797)	(0.008)	(0.774)
Urban	34,656***	45,593***	33,852***	42,711***	42,588***	41,538***	13,928
	(0.001)	(0.000)	(0.002)	(0.000)	(0.000)	(0.000)	(0.444)
Senior		−30,874***					
		(0.001)					
College		34,869*		1,278,831	35,087**	32,731*	35,134**
		(0.051)		(0.698)	(0.048)	(0.062)	(0.049)
Age*Stim			−0.920*				
			(0.077)				
College*Stim				16.61			
				(0.526)			
Female*Stim					−35.82**		
					(0.012)		
White*Stim						−49.57**	
						(0.017)	
Urban*Stim							21.83*
							(0.086)
N	1025	1025	1025	1025	1025	1025	1025
R^2	0.201	0.178	0.203	0.183	0.188	0.188	0.184
CU FE	NO	NO	NO	NO	NO	NO	NO
Quarterly FE	YES	YES	YES	YES	YES	YES	YES

Note: *p*-values in parentheses *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Standard errors are robust to heteroskedasticity. The dependent variable is the level of total debt measured in constant 2021 dollars. The reported results are from OLS models. Only stimulus recipients are included in this regression. No consumer unit fixed effects are included in these specifications. The fixed effects are collinear with the demographic characteristics of the home. All specifications include quarterly dummies. "Female" indicates that the survey respondent (typically the head of household) is female. "White" means that the survey respondent is white. "Urban" means that the consumer unit is located in an urban area. "Education" is measured in years of schooling. "Senior" indicates that the survey respondent is at least 65 years old.

In column (6) we study whether race plays any role in how recipients use the stimulus proceeds. The elasticity of interest is significantly associated to race, as measured by a non-minority dummy. In particular, while whites hold significantly more debt than minorities, they are also more likely to use stimulus checks to help with deleveraging. This result seems consistent with the evidence for small businesses presented in the online appendix to this paper according to which minority-owned businesses are more credit-constrained than their counterparts owned by whites.[21]

The result also speaks to what a survey conducted by CNBC shows that 31% of Asian Americans, 40% of Latinos and 50% of blacks were counting on government financial assistance just to "get by" versus only 22% of white respondents who felt that same level of financial anxiety. Also, 20% of white respondents said they didn't need stimulus checks, and that the government should give the money to someone else who needs it. That's roughly twice the percentage of Blacks and Latinos who answered similarly.[22]

Finally, in column (7) we show that urban households are less likely to pay-off debt with their stimulus and in fact more likely to increase debt levels in response to receiving government aid. We take this result cautiously though since 94% of survey respondents in our sample are indeed in urban areas, which diminishes the statistical power of the test.

7 | 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 yet no clear answer 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 down 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 debt to help with increased consumption of durables that the stimulus (and the associated recovery) 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 outstanding debt. We interpret this finding as evidence that consumer units tend to use their stimulus to pay down debt. This could be explained by the fact that consumers try to save the interest cost of servicing the debt and other potential costs like late payment fees on mortgages and credit cards. Also, while in normal times it would not make sense for consumers to reduce mortgage debt, which typically entails an interest rate lower than the average return earned on retirement and other long-term savings in capital markets, this could have been an interesting alternative during the Global Financial Crisis of 2008 when large losses were recorded in both bond and stock markets.

Our results yield some policy implications important for the economic impact payments in the wake of the coronavirus pandemic intended to provide economic relief to eligible individuals and families. These direct payments were sent out by the Treasury Department, the Bureau of the Fiscal Service, and the Internal Revenue Service in three rounds (Treasury Department (2021) and Murphy (2021)). The first round started in March of 2020 under the CARES Act, and provided up to $1200 direct payments per adult and $500 per qualifying child under the age of 17. The individuals that had adjusted gross income greater than $75,000 received reduced payments. Furthermore, families with four members received direct payments up to $3400. The second round payments were sent out under the COVID-related Tax Relief Act, enacted in December 2020. It provided up to $600 additional direct benefits per adult for eligible individuals and up to $600 for each qualified child. The last round took place under the American Rescue Plan Act, enacted in March of 2021 and provided up to $1400 for eligible individuals (or $2800 for married couples filing jointly). Furthermore, eligible families received an additional $1400 per qualifying dependent.[23]

Acknowledging that the crises in 2008/2009 and 2020/2021 are of very different nature, with the first originating in financial frictions and the second being a global health crisis, we argue that our results in this paper can still be used as a predictor for the consumer credit markets response to the stimulus payments of 2020 and 2021. Just as during the Global Financial crisis, financial markets exhibited weak performance during the COVID pandemic, so that households with income outside of the lowest brackets and those that remained employed faced no incentives to save and did face incentives to avoid the interest and fees on debt. Moreover, these consumers were not able to engage in consumption of travel or housing improvements due to the lockdown. Purchases of durables, a use of stimulus payments alternative to paying down debt, did increase (see Coibion et al., 2020) but supply chain disruptions and shortages created by the pandemic in the market for these goods could arguably have weakened this effect.

This leads us to believe that once the data for COVID times becomes available, it will be clear that American households have used part or all of this stimulus to pay down debt and that substantial deleveraging will be observed. We argue that this is a potential outcome of policies both at the aggregate level and in individual markets including mortgages, bank loans and other debt like that toward healthcare providers and schools.

From a policy perspective, these results involve interesting trade-offs between the immediate and long-run consequences of fiscal stimulus packages. Less debt as a result of the stimulus, the "deleveraging effect", 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, the "spending effect". Thus, understanding which of these two effects - the "deleveraging effect" or the "spending effect"- dominates is important for governments.

The literature finds mixed evidence for the "spending effect" during COVID times. Baker et al. (2020) find that households, particularly low-income ones, substantially increased spending in the first weeks after 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 et al. (2020) find a very low impact on consumer spending. They find that households who received a one-time stimulus in 2020 primarily saved 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 these concluding remarks. Nevertheless, a couple caveats are worth mentioning. First, the coverage of the data on households' liabilities in the publicly available CES is inferior to that in proprietary datasets like the one used by Agarwal et al. (2007). Second, we have no way to link data on standards of lending from the *supply-side* of credit markets with our household-level data on debt 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.

ENDNOTES

[1] See the initial paragraph in Section 5 and footnote 13 to explain how our methodology differs from Johnson et al. (2006).

[2] The issue was compounded for those families with adjustable-rate mortgages for whom debt was increasing.

[3] A "taxpayer" is defined in the Act as someone with qualifying income of at least $3000, 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.

[4] 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.

[5] 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.

[6] The full text of the Act is available at https://www.congress.gov/bill/111th-congress/house-bill/4173/text.

[7] 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 et al. (2020).

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

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

[10] These numbers do not necessarily coincide with those in Tables 1 and 2 because of missing data on debt outstanding for many CUs. Tables 1 and 2 refer only to the data we use in the estimations, that is, data with non-missing information for the dependent variable.

[11] It does not include insurance premium payments.

[12] These numbers are all in 2021 dollars.

[13] For example, 18.95% of the households have two observations only. Also, while 46.18% of the consumer units have four quarterly observations, these are not necessarily consecutive quarters, which does not allow us to use first-differences as the dependent variable.

[14] Only in a couple of specifications in Table 4, debt, stimulus and income are expressed in logs (rather than levels) as a robustness check.

[15] The 2SLS results are available from the authors upon request.

[16] Notice that in all these specifications we control for the size of the consumer unit. This is important since debt, stimulus and income are all measured in levels, and larger families receive larger subsidies when both the reference person and their spouse as well as eligible dependents may qualify.

[17] This category of debt includes loans from financial institutions (such as banks, brokerages, savings and loans, credit unions, or insurance companies), healthcare debt (toward hospitals, doctors, dentists and other medical practitioners) and any other sources of debt.

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

[19] In column (1) both age and education are measured in years. In column (2) age is measured by introducing a senior dummy which equals 1 if the survey respondent is 65 years of age or older; and education is measured through a college-educated dummy which equals 1 if the number of years of education is at least 16.

[20] In the overall sample the correlation coefficients between the level of debt and age and education are −0.1408 and 0.2534, respectively both significant at the 1% level.

[21] The appendix is available at www.mariapia-olivero.com.

[22] According to a study by Holtzblatt and Karpman (2020), 74% of eligible white adults received stimulus checks, in contrast to 69% of Black Americans and 64% of Latinos.

[23] https://home.treasury.gov/policy-issues/coronavirus/assistance-for-american-families-and-workers/economic-impact-payments.

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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