LIQUIDITY CONSTRAINTS AND CONSUMER BANKRUPTCY: EVIDENCE FROM TAX REBATES

Tal Gross, Matthew J. Notowidigdo, and Jialan Wang*

Abstract—We estimate the extent to which legal and administrative fees prevent liquidity-constrained households from declaring bankruptcy. To do so, we study how the 2001 and 2008 tax rebates affected consumer bankruptcy filings. We exploit the randomized timing of the rebate checks and estimate that the rebates caused a significant short-run increase in consumer bankruptcies in both years, with larger effects in 2008 when the rebates were more generous and more widely distributed. Using hand-collected data from individual bankruptcy petitions, we document that households that filed shortly after receiving their rebate checks had higher average liabilities and liabilities-to-income ratios.

I. Introduction

OVER the past three decades, consumer bankruptcy rates have tripled. As of the late 1990s, nearly 10% of American households had declared bankruptcy (Stavins, 2000). By 2001, over 1.3% of American households were filing for bankruptcy every year (Zywicki, 2005). In an attempt to slow the increase in bankruptcies, the 2005 Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA) raised the barriers consumers must overcome in order to file. The BAPCPA required mandatory credit counseling for filers and raised court fees and paperwork requirements that resulted in a 50% increase in filing and legal fees from an average of $921 before the reform to $1,377 after the reform (U.S. GAO, 2008).

While there exists a divisive debate over these entrance fees (Zywicki, 2005; Mann & Porter, 2010), little empirical research has estimated their effects. Moreover, economic theory provides little guidance, as the welfare consequences of entrance fees are theoretically ambiguous. On the one hand, fees may act as an ordeal mechanism, screening out households that stand to gain little from filing for bankruptcy (Nichols & Zeckhauser, 1982). On the other hand, the fees may prevent liquidity-constrained households from filing for bankruptcy, and those households may benefit the most from filing.

In this paper, we analyze the interaction between household liquidity constraints and the entrance fees for bankruptcy. To do so, we exploit exogenous variation in liquidity induced by the 2001 and 2008 income tax rebates. The rebates were distributed over nine- to ten-week periods in both years, and households received between $300 and $1,200. The date households received their rebates was randomly assigned, which allows us to estimate the causal effect of a one-time, anticipated increase in liquidity on consumer bankruptcy filings.

We find that the tax rebates led to a significant short-run increase in consumer bankruptcies. Total bankruptcies increased by roughly 2% after the 2001 rebates and by 6% after the 2008 rebates. Consistent with the existence of liquidity constraints, we find that the increase in bankruptcies was driven entirely by Chapter 7 filings.[1] We possess little statistical power to estimate longer-run effects of the rebates, but our data suggest that affected households would have taken months to save up for filing fees if not for the tax rebates. Our findings are broadly consistent with recent survey evidence on the "financial fragility" of American households, suggesting that roughly one-quarter of Americans would not be able to raise $2,000 within thirty days (Lusardi, Schneider, & Tufano, 2011).

To interpret our results, we develop a simple model of consumer bankruptcy. The model predicts that tax rebates should affect the filing decisions of only liquidity-constrained households. Moreover, the model predicts that the impact of the tax rebates should increase with the size of bankruptcy entrance fees and the size of the tax rebates. Indeed, we observe a larger treatment effect in 2008 relative to 2001, and both the entrance fees and tax rebates were larger in 2008. We conclude that 4% of filers in 2001 and 8% of filers in 2008 would have been unable to file for several months in the absence of the tax rebates.

Our paper is related to a growing literature that studies the economic effects of liquidity constraints. Liquidity constraints have been shown to cause excessive consumption responses to transitory changes in income (Shapiro & Slemrod, 2003; Souleles, 1999; Hsieh, 2003; Stephens, 2003), limit investment in human capital (Dynarski, 2003), and amplify the behavioral response to unemployment insurance

[1] As described in more detail in section IV.A, households may elect to file for bankruptcy under either Chapter 7 or Chapter 13. Chapter 7 filers are more likely to be liquidity constrained since they have lower incomes and fewer assets. Moreover, Chapter 7 filers must generally pay fees in full at the time of filing, while Chapter 13 filers can pay off their fees gradually. As a result, upfront fees are 45% higher for Chapter 7 filers.

benefits (Chetty, 2008).[2] Liquidity constraints likely also play an important role in the optimal design of social insurance programs (Chetty, 2008; Hansen & İmrohoroğlu, 1992). Since consumer bankruptcy functions, at least in part, as a social insurance program, our paper is broadly related to the literature on the role of ordeal mechanisms and entrance fees in the optimal design of social insurance programs (Nichols & Zeckhauser, 1982). We discuss how our estimates shed light on the welfare consequences of changing the fee structure of the consumer bankruptcy system.

Our paper is also part of a growing literature on the economic effects of tax rebates. Most related papers focus on the effects of the tax rebates on consumption and expenditures (Johnson, Parker, & Souleles, 2006; Agarwal, Liu, & Souleles, 2007; Shapiro & Slemrod, 2003; Bertrand & Morse, 2009), while other studies have estimated the effect of the tax rebates on mortality and morbidity (Evans & Moore, 2011; Gross & Tobacman, 2011). To our knowledge, no previous studies have focused on the effect of the tax rebates on take-up of social insurance programs or on consumer bankruptcy.

The remainder of the paper proceeds as follows. The next section provides background on the tax rebates and describes the bankruptcy data that we have compiled. Section III outlines a theoretical model that explains how tax rebates can affect bankruptcy rates. Section IV demonstrates how the rebates affected the number of bankruptcies. Section V describes how the characteristics of bankruptcy filers changed after the rebates. Section VI discusses alternative explanations for our findings and the policy implications of our results. Section VII concludes.

II. Background on the Bankruptcy Data and the Tax Rebates

In order to estimate the impact of the rebates on bankruptcy rates, we compiled a unique data set based on the Public Access to Court Electronic Records system. Our sample consists of all consumer bankruptcy filings in the 81 courts (out of 94) that agreed to grant us full electronic access to their dockets. Figure 1 presents a map of our sample coverage. We verified that the data match aggregate counts of bankruptcies reported by the Administrative Office of the U.S. courts.

Table 1 compares the characteristics of districts in our sample to those not in our sample. The sample covers roughly 87% of bankruptcies in the United States and 88% of the population. Coverage remains consistent across our sample period, which extends from 1998 to 2008. The districts in the sample have populations with slightly lower income, less college education, and a higher unemployment rate.

The tax rebates were disbursed as part of the economic stimulus bills passed by Congress in 2001 and 2008 and were specifically designed to stimulate the economy during the

Figure 1.—Bankruptcy Districts in Sample

The 81 bankruptcy districts shaded in dark gray are included in the sample.

ongoing recessions.[3] The Internal Revenue Service (IRS) sent the rebate checks on a schedule determined by the head of household's Social Security number (SSN). Table 2 presents the dates on which checks were sent. We include in our sample all bankruptcies that were filed at most thirty weeks prior to the date that checks were sent and at most forty weeks after that date.[4] In 2001, Social Security numbers were divided into ten equal-sized groups. Checks were mailed from July 20 through the September 21. The payments ranged from $300 to $600.[5] In 2008, households could elect to receive their stimulus payments by check or direct deposit. As indicated in the right-most column of table 2, there were only three dates on which direct deposit transfers were made. Roughly 40% of households elected to receive their rebate checks by direct deposit (Parker et al., 2011). The rebate payments were higher in 2008 than in 2001, ranging from $300 to $600 for single filers to $600 to $1,200 for couples.[6] Figure 2 summarizes the bankruptcy rates by two-digit SSN group. As expected, the figure demonstrates that there was no systematic variation in bankruptcy rates across SSN groups in the months leading up to the rebates.[7]

In order to interpret our empirical results, we surveyed the relevant case law to understand how bankruptcy judges treated the tax rebates. Judges considered the tax rebates to be part of the bankruptcy estate, and the rebates were

---

[2] Liquidity constraints also affect subprime mortgage defaults in the months following lump-sum property tax payments (Anderson & Dokko, 2011). By contrast, Hurst and Lusardi (2004) do not find clear evidence that liquidity constraints restrict entry into entrepreneurship.

[3] The rebates were mandated by the Economic Growth and Tax Relief Reconciliation Act of 2001 and the Economic Stimulus Act of 2008.

[4] We restrict the sample by time relative to when the checks were sent, so that we have the same number of observations for each SSN group. We find similar results when we restrict by absolute calendar time and also when we extend the sample window.

[5] Individual tax filers with no dependents could receive up to $300 through the rebate, single parents a maximum of $500, and married couples jointly filing could receive $600. To receive the full amount, a single taxpayer had to have earned at least $6,000 in taxable income in 2000, and a married couple jointly filing had to have earned at least $12,000 in taxable income.

[6] If a filer's 2007 tax return indicated over $3,000 in qualifying income, the filer was eligible for at least the minimum payment based on the following general guidelines: $300 to $600 for individuals, $600 to $1,200 for joint filers, and $300 for each qualifying child. The rebates phased out for higher-income households, being reduced by 5% of adjusted gross income above $75,000 for individuals and $150,000 for couples.

[7] An F-test fails to reject the hypothesis that the bankruptcy rates are equal across all groups with a p-value of 0.726 in 2001 and 0.864 in 2008.

TABLE 1.—SAMPLE COVERAGE

|	Districts in Sample	Districts Not in Sample	All Districts	Coverage in Our Sample
A. 2001				
Consumer bankrupcies	1,267,244	184,786	1,452,030	87%
Chapter 7	918,020	113,473	1,031,493	89%
Chapter 13	348,580	71,170	419,750	83%
Population	243,048,574	33,969,049	277,017,622	88%
Median family income	41,662	44,617	41,947	
Unemployment rate	4.57%	3.93%	4.51%	
Percent college	24.9%	26.7%	25.1%	
Median housing value	127,801	124,988	127,530	
B. 2008				
Consumer bankrupcies	946,601	127,624	1,074,225	88%
Chapter 7	639,804	74,585	714,389	90%
Chapter 13	306,045	52,902	358,947	85%
Population	265,426,846	38,632,882	304,059,728	87%
Median family income	51,689	55,970	52,102	
Unemployment rate	5.38%	4.61%	5.31%	
Percent college	27.2%	29.0%	27.4%	
Median housing value	211,448	220,553	212,326	

This table describes the characteristics of the 81 districts in our sample, compared with the 94 total districts in the United States (excluding territories). Nonbankruptcy statistics are obtained by postal code merge with the 2000 U.S. Census.

TABLE 2.—DATES WHEN REBATE CHECKS WERE SENT

Last Two Digits of SSN	2001 Rebate Check Sent	Last Two Digits of SSN	2008 Stimulus Check Sent	Last Two Digits of SSN	2008 Stimulus Deposit Made
00–09	July 20	00–09	May 16	00–20	May 2
10–19	July 27	10–18	May 23	21–75	May 9
20–29	August 3	19–25	May 30	76–99	May 16
30–39	August 10	26–38	June 6		
40–49	August 17	39–51	June 13		
50–59	August 24	52–63	June 20		
60–69	August 31	64–75	June 27		
70–79	September 7	76–87	July 4		
80–89	September 14	88–99	July 11		
90–99	September 21				

This table describes the dates on which the Internal Revenue Service sent tax rebate payments. The timing of when payments were sent was determined by the last two digits of the head-of-household's Social Security number.

FIGURE 2.—RANDOMIZATION TEST
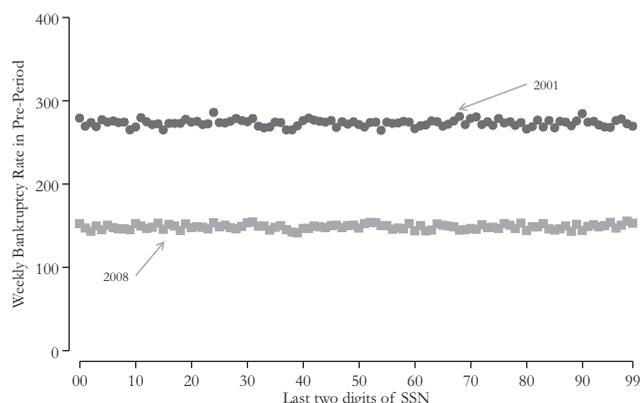
This graph plots bankruptcies in March, April, and May 2001 and in January, February, and March 2008. The distribution of the 2001 tax rebates began in July, and the distribution of the 2008 tax rebates began in May. An *F*-test fails to reject the hypothesis that weekly bankruptcy rates are equal across groups with *p*-value 0.726 in 2001 and 0.864 in 2008.

therefore subject to the normal rules governing cash assets.[8] Our theoretical model therefore assumes that the tax rebates

[8] The relevant legal cases are the following: *In re Lambert* (BK 601-61015-fra7, 2002), *In re Howell* (294 B.R. 613, 2003), *In re Rivera* (BK 01-42625, 2006), and *In re Alguires* (BK 08-10691, 2008).

are treated the same regardless of when households declare bankruptcy. In other words, households cannot strategically manipulate their filing dates in order to shield their rebates from the courts. The only way households would be able to shield their tax rebate would be to use the proceeds from the rebate for consumption before filing for bankruptcy. We address this issue below.

III. Conceptual Framework

This section describes a simple model of how increases in liquidity can affect bankruptcy rates. The key feature of the model is the existence of entrance fees that households must pay in order to file for bankruptcy. To conserve space, we summarize the main insights of the model here and provide details in section 2 of the online appendix.

Households owe a positive, predetermined amount of debt. At the start of the first period of the model, household wealth is realized from a known distribution. At the start of the second period, tax rebates are distributed. Households decide whether to file for bankruptcy in period 1, period 2, or not at all. They make that decision based on comparing their wealth after repaying their debts versus their wealth after filing for

TABLE 3.—EFFECT OF REBATE CHECKS ON BANKRUPTCIES

Dependent Variable: Level or Logarithm of Total Bankruptcy Filings per SSN Group per Week

	(1)	(2)	(3)	(4)	(5)	(6)
	Chapter 7		Chapter 13		All	
	Levels	Logs	Levels	Logs	Levels	Logs
			A. 2001 Tax Rebates			
After Check Sent	6.266	0.036	−0.778	−0.014	5.488	0.023
	(1.107)	(0.007)	(0.592)	(0.010)	(1.189)	(0.005)
	[0.000]	[0.000]	[0.192]	[0.157]	[0.000]	[0.000]
R^2	0.804	0.813	0.530	0.536	0.801	0.819
			B. 2008 Tax Rebates			
After Check Sent	5.916	0.049	−0.652	−0.015	5.264	0.030
	(1.014)	(0.008)	(0.531)	(0.011)	(1.174)	(0.007)
	[0.000]	[0.000]	[0.222]	[0.167]	[0.000]	[0.000]
After Direct Deposit	5.632	0.047	−1.289	−0.027	4.343	0.027
	(1.863)	(0.016)	(0.999)	(0.023)	(1.962)	(0.013)
	[0.003]	[0.005]	[0.200]	[0.253]	[0.029]	[0.030]
Total	11.548	0.096	−1.942	−0.042	9.606	0.058
Effect	(2.174)	(0.019)	(1.175)	(0.026)	(2.376)	(0.015)
	[0.000]	[0.000]	[0.102]	[0.120]	[0.000]	[0.000]
R^2	0.873	0.870	0.568	0.580	0.874	0.873

$N = 7{,}100$. The sample consists of counts of bankruptcies by two-digit SSN group and week, covering thirty weeks before and forty weeks after groups were sent their tax rebate checks. The standard errors in parentheses are robust to autocorrelation between observations from the same SSN group. The associated p-values are in brackets. SSN-group fixed effects and week fixed effects not shown.

bankruptcy. Filing for bankruptcy requires paying an upfront filing fee and then losing a fraction of remaining wealth to creditors. In order to file, households must have sufficient wealth to pay the legal and administrative costs associated with filing.

The tax rebates provided a one-time anticipated increase in liquidity. The model suggests that that increase in liquidity will affect the bankruptcy filings only of households that were previously liquidity constrained. That conclusion follows immediately from the assumption that households cannot strategically time their bankruptcy to hide their rebate income from the court. That assumption is partly justified based on the case law, discussed above. It also rules out the "consumption hypothesis," which we discuss in section VI.

Under these assumptions, the evolution of bankruptcy rates following the tax rebates reveals the share of filers who are liquidity constrained. Furthermore, the model predicts that increases in the average size of the rebates and increases in filing costs will lead to larger rebate effects. This suggests that the increase in bankruptcies should be larger in 2008 than in 2001 because the tax rebates were larger in 2008.[9]

IV.   The Effect of the Tax Rebates on Bankruptcies

This section presents our main empirical results. We first describe how the bankruptcy rate changed after the tax rebates were distributed and then how the rebate effect evolved.

[9] In the online appendix, when we relax some of the model's assumptions, the model suggests that the empirical estimates are a lower bound for the fraction of filers who are liquidity constrained. For instance, if some filers do strategically file before rebate receipt in order to try (unsuccessfully) to hide their rebates from the court, then our empirical estimates would be biased downward.

A.   The Change in the Bankruptcy Rate after the Rebates

The way in which both the 2001 and 2008 tax rebates were distributed lends itself to a simple difference-in-difference empirical framework. For the 2001 sample, we construct aggregate counts of bankruptcies by two-digit SSN group, $g \in \{00, 01, 02, \ldots, 99\}$, and week, w, and estimate the following regression:

$$y_{gw} = \beta \cdot I\{\text{After Check Sent}\}_{gw} + \alpha_g + \alpha_w + \varepsilon_{gw}.$$

The outcome y_{gw} is either the number of bankruptcies in group g and week w or its logarithm, and α_g and α_w are group and week fixed effects, respectively. The indicator function $I\{\text{After Check Sent}\}_{gw}$ is equal to unity starting one week after checks are sent for group g and 0 otherwise. For the 2008 sample, we include an additional indicator function to control for whether the SSN group has been given its direct deposit. Our standard errors are robust to autocorrelation between observations from the same two-digit SSN group; thus all regressions involve 100 clusters.

Panel A of table 3 presents estimates of this regression for the 2001 rebates, and panel B presents estimates for 2008. The first two columns present results when the level and the logarithm of Chapter 7 bankruptcies are the outcomes of interest, respectively. Both columns suggest a statistically significant increase in Chapter 7 filings after the rebates were distributed. In 2001, each two-digit SSN group experienced an average of 6.2 additional Chapter 7 bankruptcies per week. The estimates in column 2 indicate a 3.6% increase in bankruptcies after the rebates.

Panel B demonstrates that this effect was larger in 2008. The bankruptcy rate increased by 4.9% after the 2008 rebate checks were sent. But bankruptcies also increased by 4.7% after direct deposits were made. The total increase in bankruptcies after the 2008 tax rebates was thus 9.6%. For

both rebate years, the results presented in columns 1 and 2 are precisely estimated and statistically significant.

There are several possible explanations for the larger rebate effect in 2008. First, the rebate checks were larger in 2008, and the larger rebate checks may have enabled more liquidity-constrained households to file for bankruptcy. Second, the rebate checks were more widely distributed: roughly 85% of households received rebate checks in 2008 versus 57% in 2001 (Johnson et al., 2006; Parker et al., 2011). Third, the recession was more severe in 2008, which could have resulted in more liquidity-constrained households. All of these explanations would suggest a larger effect in 2008. Additionally, the BAPCPA dramatically changed the bankruptcy system in the intervening period (McIntyre, Sullivan, & Layton, 2010), raising attorney fees and encouraging households to choose Chapter 13 rather than Chapter 7. The expected effect of these legal changes on the 2008 results is less clear.
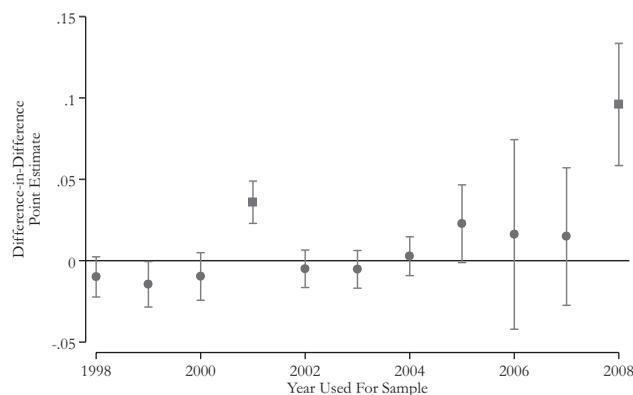
In contrast to Chapter 7 filings, table 3 suggests that the rebates had a smaller (and possibly negative) impact on Chapter 13 bankruptcies. Columns 3 and 4 present point estimates for Chapter 13 bankruptcies that are much smaller in magnitude than those for Chapter 7. The estimates suggest a 1% to 5% decrease in Chapter 13 filings, decreases that are not statistically significant at conventional levels. The small decrease in Chapter 13 filings suggests that some households may have substituted Chapter 7 for Chapter 13 after the tax rebates. The increase in the number of Chapter 7 filings, however, is much larger than the decrease in Chapter 13 filings. Therefore, the filers who switch chapters in response to the rebates likely represent a small share of the total rebate effect.

The contrast between chapters is consistent with the existence of liquidity constraints. Under Chapter 7, households receive immediate discharge of most debts in exchange for forfeiture of nonexempt assets and collateral. While Chapter 7 offers complete discharge of most debt obligations, Chapter 13 requires households to adhere to a three- to five-year repayment plan. Households typically choose to file under Chapter 13 in order to keep their homes, cars, or small businesses. As a result, Chapter 7 filers tend to have lower incomes and fewer assets than Chapter 13 filers. Another relevant difference between the chapters is that households that file under Chapter 13 are on average charged higher total legal fees but lower upfront fees, since legal fees can be written into the debtors' repayment plans. Chapter 7 filers must typically pay all of their attorneys in advance of filing.[10] Both of these differences suggest that Chapter 7 filers are more likely to be liquidity constrained.[11] And, indeed,

FIGURE 3.—CHAPTER 7 REBATE EFFECT BY YEAR

The figure presents point estimates from regression of log counts of Chapter 7 bankruptcies on indicators based on the SSN groups used to determine the timing of tax rebates. Indicators in 2001 and 2008 match the actual timing of rebates for each SSN group. For 1998 through 2004, placebo indicators match the 2001 rebate dates. For 2005 through 2008, placebo indicators match the 2008 rebate dates.

table 3 presents a much larger rebate effect for Chapter 7 bankruptcies.

Finally, columns 5 and 6 of table 3 present estimates for Chapter 7 and Chapter 13 filings combined. The point estimates are positive and statistically significant at conventional levels. They suggest that consumer bankruptcy filings overall increased by 2.3% in 2001 and by 5.8% in 2008 following the rebates. Since not all households received the tax rebates, we can scale our estimates by the share of households that received rebates. After rescaling, we find that the share of all households whose filing behavior responds to tax rebates was roughly 4% of all households in 2001 and 8% of all households in 2008.[12]

We next discuss a simple falsification test. Figure 3 presents the results of this test. Each point in this figure represents estimates from specifications identical to the one reported in column 2 of table 3 but are instead estimated for alternative years in our sample when rebate checks were not distributed. We focus on Chapter 7 filings since our main effect is most pronounced for Chapter 7, and we focus on the log specification in order to control for annual differences in filing rates. Although tax rebates were not distributed by SSN group in years other than 2001 and 2008, we construct indicator variables as if they were. Specifically, we construct placebo indicator variables consistent with the 2001 rebate distribution for 1998 through 2004. For 2005 through 2008, we construct placebo indicator variables consistent with the 2008 rebate distribution and plot the sum of the paper check and direct deposit placebo effects.[13]

---

[10] We investigated the cost of filing by constructing a random sample of 2001 and 2008 filings from the Central District of California. The average total cost of a Chapter 7 bankruptcy was $1,100, while the average total cost of a Chapter 13 bankruptcy was $1,749. The average attorney fees paid before filing were reversed in magnitude: $995 for Chapter 7 and $684 for Chapter 13.

[11] An additional reason for the contrast by chapter is that a large share of Chapter 13 filers turn to bankruptcy in order to halt a foreclosure (Mann & Porter, 2010). The timing of such bankruptcies is then determined by the foreclosure process rather than by tax rebates.

[12] The purpose of these calculations is to rescale our treatment effect to apply to the specific households eligible to receive rebate checks. We cannot extrapolate our results to the overall population, since households that did not receive rebate checks had very different characteristics. In particular, in both rebate years, households that did not receive rebates had very low taxable income in the previous year.

[13] The confidence intervals in figure 3 are wider for estimates after 2004 because we plot the sum of the paper check and direct deposit effects.

The figure presents no evidence of a strong rebate effect in years other than those in which rebates were actually distributed. In all placebo tests, the confidence intervals do not exclude 0. A joint test of the hypothesis that all estimates except those for 2001 and 2008 are equal to 0 fails to reject the null hypothesis with a p-value of 0.136. In contrast, a joint test that the 2001 and 2008 estimates are jointly equal to 0 leads to a p-value less than 0.001.

In the remainder of this section, we discuss the sensitivity of our results to alternative inference procedures. In table OA1, in the online appendix, we report alternative means of calculating the standard errors. We find that the precision of our results is very similar when we calculate standard errors that are robust to heteroskedasticity, autocorrelation by week, or autocorrelation based on the date on which checks were sent. This last method is most conservative, but it involves a small number of clusters (ten in 2001 and twelve in 2008). In any case, table OA1 demonstrates that the main results are very similar regardless of how the standard errors are computed.

Next, we conduct a simple randomization-inference exercise in which we randomly reassign check dates across two-digit SSN groups and compute the effect of the rebate check under each set of placebo assignments. We compute rebate effects for 10,000 random allocations of dates and graph the distribution of the estimated effects in figure 4. The empirical p-values from this simulation procedure are very similar to the p-values reported in panel A of table 3.

B. Variation in the Rebate Effect over Time

This section describes how filing rates evolved over the weeks surrounding the rebates. To measure such patterns, we estimate an event-study specification. We modify the regression equation above to include indicator variables for two-week intervals before and after the rebates. The two weeks before each group received its rebate is the omitted category.

Figure 5 presents the estimates from that regression when the outcome is the logarithm of Chapter 7 filings in 2001 and 2008. The dotted lines plot 95% confidence intervals, and the solid line plots the point estimates. The figure demonstrates that the bankruptcy rate increased by roughly 4% in the month after the rebates were distributed, and the treatment effect decreases monotonically after week 4.[14]

Figure 6 present the same event-study estimates for Chapter 13 bankruptcies in 2001 and 2008. Nearly all of the point estimates are statistically indistinguishable from 0, though the figures suggest a slight decline in Chapter 13 bankruptcies following the rebates, consistent with the results in table 3.

As a whole, these figures suggest that the tax rebates led to an immediate short-run increase in Chapter 7 bankruptcies. The increase in bankruptcies lasted roughly four weeks after the rebates were distributed.

We cannot identify households that did not receive a rebate, as all SSN groups eventually received rebates; therefore, using this research design, we cannot test whether the rebates resulted in a transitory or permanent increase in the number of bankruptcies. In table OA2 in the online appendix, we report results from an alternative specification that attempts to estimate the permanent effect of the rebates by comparing bankruptcy rates across months in different years. The test assumes that the permanent effect of the rebates can be estimated by comparing the total number of bankruptcies in the months during and after the rebates with the same months in other years, controlling for within-year seasonality in bankruptcy filings and controlling for long-run, across-year trends in bankruptcy filings. We find no evidence of a permanent increase in bankruptcies resulting from the 2001 tax rebates. Our precision, however, is limited when using this alternative research design, and we are unable to rule out large, long-run effects.[15] Based on these tests, it is unclear whether the rebates allowed some households to file that would not have been able to file otherwise or whether the rebates simply allowed households to file earlier.

C. Variation in the Rebate Effect by Local Characteristics

This section tests how local characteristics are associated with the rebate effects. We record the ZIP code of residence for each bankruptcy filer in our database. We merge those ZIP codes to median household income and homeownership rate, as measured in the 2000 decennial census. This allows us to stratify our main specification by average income in the ZIP code. We also stratify filers by a proxy for their access to credit. Following Mian and Sufi (2009), we merge each ZIP code to the share of its residents in 1996 that were categorized as subprime borrowers.[16] Due to the rapid expansion of mortgage credit in subprime ZIP codes not matched by increases in household income, subprime ZIP codes are a plausible proxy for liquidity constraints (Mian & Sufi, 2009).

Our conceptual framework in section III predicts that areas in which liquidity constraints are more prevalent should

---

[14] The results in figure 5A suggest a modest, marginally significant increase in filing rates three and four weeks before the checks are sent in 2001. In contrast, figure 5B suggests no discernible pretrend in 2008. We cannot identify a cause for the pretrend in figure 5A; potentially, households may have filed early, hoping to receive their rebates after their bankruptcy case was discharged. We view this as unlikely, however, as bankruptcies generally last for months and judges were aware of the pending rebates. Nevertheless, it is possible that some households misperceived the laws regarding how the rebates were treated by the bankruptcy courts. The regression underlying figure 5B also includes an indicator variable for whether the SSN group had received its direct deposit, so that these event-study estimates report the dynamic effects of the rebates sent through the mail. A

similar event-study figure using the direct deposit dates is extremely imprecise because there are only three direct deposit dates, three weeks apart. This makes it difficult to estimate the dynamic effects of the rebates sent by direct deposit. By contrast, the paper check dates span roughly two months, and there were nine paper check dates.

[15] We estimate only the long-run effect of the 2001 tax rebate because we have too little data after the 2008 rebates.

[16] The variable captures the share of adults in the ZIP code whose FICO credit score was 660 or lower in 1996 (Mian & Sufi, 2009).

FIGURE 4.—RANDOMIZATION INFERENCE, 2001 REBATES
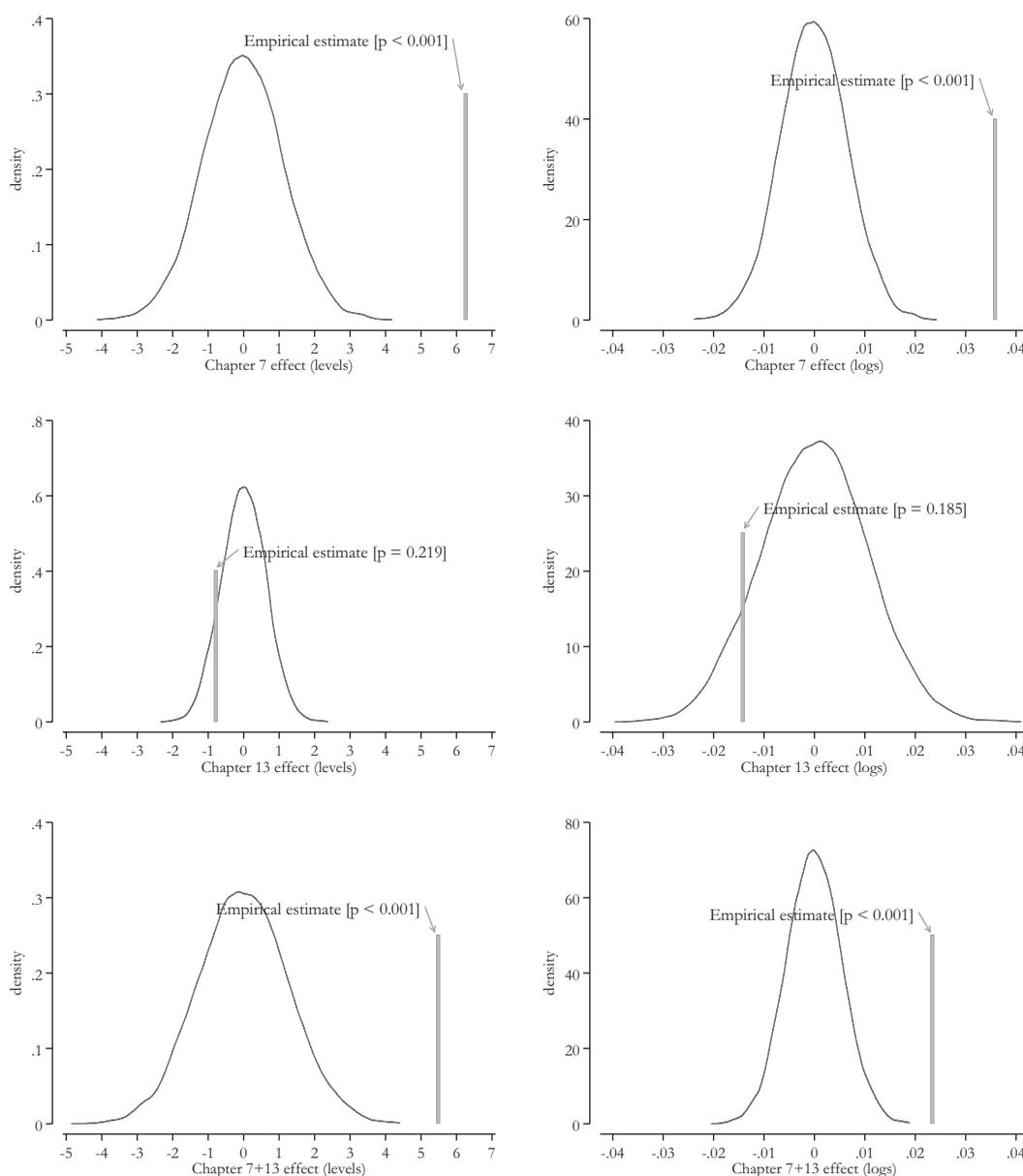
This figure presents results from a randomization-inference simulation. Each graph shows the distribution of estimated coefficients based on 10,000 placebo assignments of check dates to SSN groups. The empirical *p*-value is reported next to the empirical estimate. The six graphs correspond to columns 1 through 6 in panel A of table 3.

be associated with larger rebate effects. Thus, if income, homeownership, and subprime borrowing predict liquidity constraints, then these proxies should be associated with larger rebate effects. Liquidity, however, is determined by the difference between a household's income and expenditures, not just income, assets, or subprime status. Therefore, it is not clear a priori whether such proxies will have a discernible relationship with the rebate effect.

Table 4 presents estimates of rebate effects for Chapter 7 bankruptcies when the sample is stratified by terciles of these three variables. The first three columns present results for terciles of median income. The point estimates form different patterns in the two rebate years. In 2008, the point estimates suggest a U-shaped pattern; the second tercile of income is associated with the smallest rebate effect. In 2001, the third tercile of income is associated with the smallest total rebate effect. None of these differences across the terciles, however, are statistically significant at conventional levels.

The second set of columns of table 4 presents results when the sample is stratified by the likelihood of being a subprime borrower. The results also do not suggest a clear pattern. A Wald test of equality of the three coefficients in 2001 has a *p*-value of 0.110, and in 2008 the associated *p*-value is 0.820. We cannot reject the hypothesis that households from all terciles exhibited the same rebate effect.

FIGURE 5.—EVENT STUDY POINT ESTIMATES
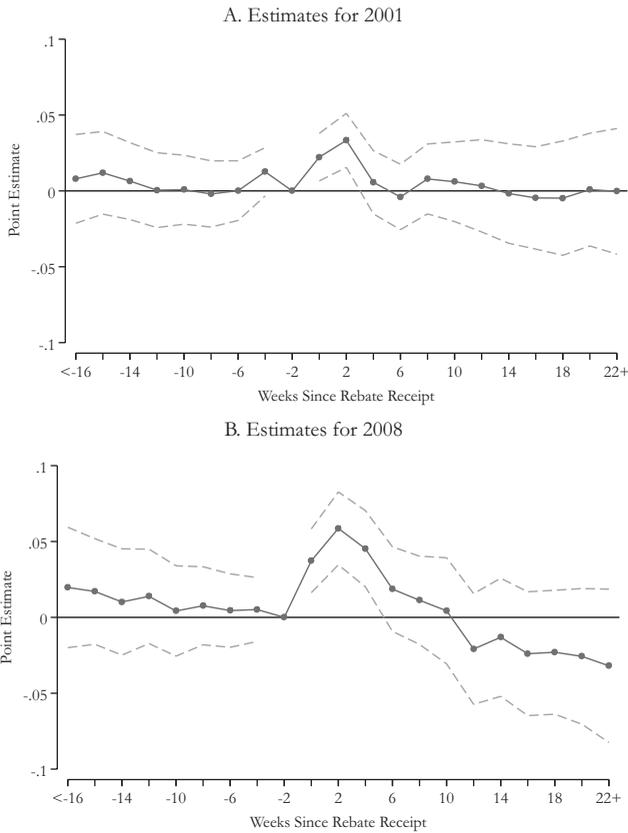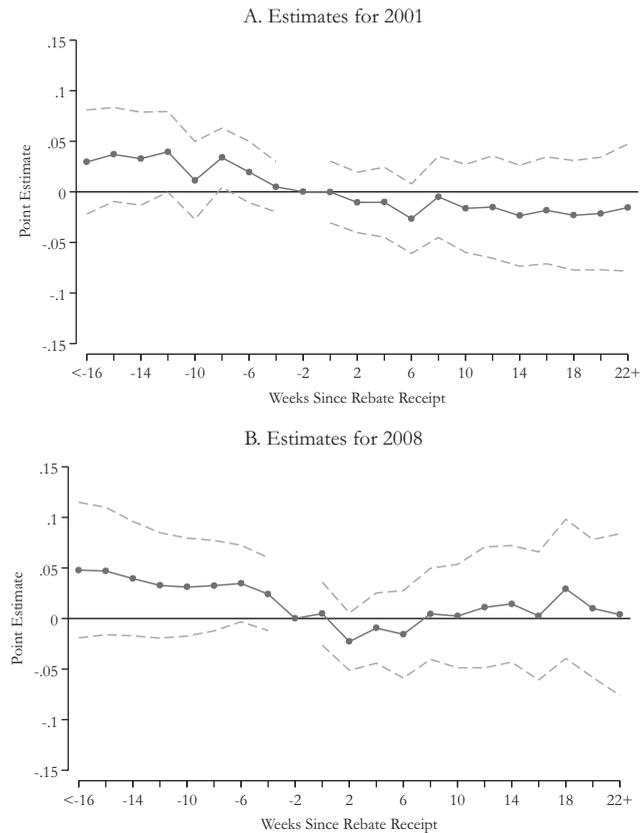Dependent Variable: Log of Chapter 7 Filings

FIGURE 6.—EVENT STUDY POINT ESTIMATES
Dependent Variable: Log of Chapter 13 Filings

A. Estimates for 2001

B. Estimates for 2008

The figure presents point estimates from a regression of log counts of bankruptcies on indicators for two-week intervals. The dashed lines represent 95% confidence intervals that are robust to autocorrelation between observations from the same SSN group. The sample consists of bankruptcies by SSN group and week, covering thirty weeks before and forty weeks after groups were sent their tax rebate checks. SSN-group fixed effects and week fixed effects not shown. The omitted time period is one and two weeks before rebate checks were sent.

A. Estimates for 2001

B. Estimates for 2008

The point estimates are from a regression of log counts of bankruptcies on indicators for two-week intervals. The dashed lines represent 95% confidence intervals that are robust to autocorrelation between observations from the same SSN group. The sample consists of bankruptcies by SSN group and week, covering thirty weeks before and forty weeks after groups were sent their tax rebate checks. SSN-group fixed effects and week fixed effects not shown. The omitted time period is one and two weeks before rebate checks were sent.

TABLE 4.—THE EFFECT OF REBATE CHECKS BY LOCAL CHARACTERISTICS
Dependent Variable: Logarithm of Chapter 7 Bankruptcy Filings per SSN Group per Week

	(1a)	(1b)	(1c)	(2a)	(2b)	(2c)	(3a)	(3b)	(3c)
	Bankruptcies Stratified by Median Family Income in Zip Code			Bankruptcies Stratified by Share of Zip Code Residents Who Are Subprime Borrowers			Bankruptcies Stratified by Homeownership Rate in Zip Code		
	First Tercile	Second Tercile	Third Tercile	First Tercile	Second Tercile	Third Tercile	First Tercile	Second Tercile	Third Tercile
	A. 2001 Tax Rebates								
After	0.040	0.049	0.019	0.049	0.027	0.034	0.028	0.031	0.047
Check	(0.011)	(0.010)	(0.011)	(0.010)	(0.011)	(0.012)	(0.011)	(0.012)	(0.011)
Sent	[0.001]	[0.000]	[0.078]	[0.000]	[0.013]	[0.005]	[0.013]	[0.014]	[0.000]
R^2	0.553	0.628	0.593	0.610	0.631	0.526	0.583	0.616	0.571
	B. 2008 Tax Rebates								
After	0.058	0.053	0.038	0.039	0.060	0.045	0.046	0.050	0.050
Check	(0.016)	(0.013)	(0.013)	(0.014)	(0.014)	(0.016)	(0.018)	(0.014)	(0.013)
Sent	[0.001]	[0.000]	[0.005]	[0.005]	[0.000]	[0.007]	[0.012]	[0.001]	[0.000]
After	0.001	0.068	0.057	0.057	0.044	0.036	0.024	0.045	0.061
Direct	(0.029)	(0.030)	(0.027)	(0.028)	(0.029)	(0.033)	(0.034)	(0.031)	(0.024)
Deposit	[0.970]	[0.029]	[0.039]	[0.043]	[0.130]	[0.274]	[0.477]	[0.151]	[0.014]
Total	0.059	0.121	0.095	0.096	0.105	0.081	0.070	0.095	0.111
Effect	(0.034)	(0.033)	(0.034)	(0.032)	(0.034)	(0.036)	(0.040)	(0.036)	(0.030)
	[0.091]	[0.000]	[0.005]	[0.004]	[0.002]	[0.026]	[0.080]	[0.010]	[0.000]
R^2	0.628	0.716	0.692	0.669	0.726	0.647	0.626	0.697	0.690

$N = 7,100$. The sample consists of counts of bankruptcies by two-digit SSN group and week, covering thirty weeks before and forty weeks after groups were sent their tax rebate checks. The standard errors in parentheses are robust to autocorrelation between observations from the same SSN group. The associated p-values are in brackets. SSN group fixed effects and week fixed effects not shown.

TABLE 5.—SUMMARY STATISTICS FOR FILINGS FROM TEN DISTRICTS

	A. 2001			B. 2008		
	Mean	Median	SD	Mean	Median	SD
Household composition						
Female	24%			25%		
Single	35%			34%		
Separated or divorced	16%			20%		
Married	49%			46%		
Number of children	1.04	1	1.20	0.92	0	1.20
Fees						
Filing fee	$199	$200	$15	$299	$299	$0
Legal fee promised	$746	$700	$397	$1,265	$1,099	$654
Legal fee % paid	79%	100%	30%	86%	100%	30%
Self-representation	3.4%			1.8%		
Financial characteristics						
Annual income	$23,784	$20,403	$24,656	$31,581	$26,738	$26,369
Annual expenses	$28,212	$23,712	$54,312	$35,868	$30,480	$28,668
Total assets	$70,923	$31,883	$310,346	$112,259	$55,074	$440,894
Total liabilities	$136,541	$62,896	$1,021,721	$181,823	$101,943	$392,214
% of liabilities secured	42%	46%	30%	42%	44%	30%
Liabilities-to-income ratio	5.9	3.05	34.5	6.6	3.7	20.5

This table presents statistics for a sample of Chapter 7 bankruptcies from ten bankruptcy districts. The sample consists of 2,132 randomly chosen bankruptcies during our sample periods from these districts in 2001 and 4,355 bankruptcies in 2008.

The last set of columns presents results when we stratify the sample by homeownership rate, where, again, no clear pattern is present.

Overall, these results suggest a weak relationship between local characteristics and the rebate effect. The pattern of point estimates by tercile suggests that the rebate effect is not monotonically related to these proxies. Interestingly, Johnson et al. (2006) and Parker et al. (2011) also find a nonmonotonic effect for consumption expenditures. Both studies find that both low- and high-income households exhibit a higher sensitivity to tax rebates than middle-income households. The 2008 results in table 4 exhibit the same pattern. Such a pattern suggests a complex relationship between liquidity and income, although we do not have enough precision to reach strong conclusions on this point.

V. Analysis of Filers' Characteristics

While the results above demonstrate that Chapter 7 bankruptcy rates increased after the tax rebates, a remaining question is which types of filers were responsible for this increase. In this section, we describe how the average characteristics of bankruptcy filers changed in the weeks after the tax rebates. To do so, we collected legal documents for a random sample of consumer bankruptcies in ten districts.[17] We randomly selected 250 Chapter 7 filings from each district in

2001 and 500 filings per district in 2008.[18] For each filing, research assistants read the associated legal documents and recorded the financial characteristics of the household. Our final sample consists of 2,132 bankruptcies in 2001 and 4,355 bankruptcies in 2008.

A. Sample Statistics

Households declaring bankruptcy must reveal many financial and demographic details to the court. Summary statistics for these details are presented in table 5. The first set of rows describes the demographics of filers. These average characteristics changed relatively little between 2001 and 2008. For instance, the percentage of primary filers who were female increased from 24% to 25% between the two years. A t-test fails to reject that the fraction of female filers remained constant (the associated p-value is 0.53). Filers were single in 34% to 35% of cases, separated or divorced in 16% to 20% of cases, and married in 46% to 49% of cases.[19]

The next set of rows in table 5 describes the fees paid by filers. Fees generally increased from 2001 to 2008, largely driven by the BAPCPA. Filing fees are paid to the court at the time of filing. The BAPCPA standardized filing fees to $299 for all Chapter 7 cases starting in 2005, increasing the average filing fee 50% from 2001 to 2008.[20] Average legal fees increased 70% from $746 in 2001 to $1,265 in 2008;

---

[17] We selected the districts based on whether the court judge was willing to grant us a waiver to download the files and whether electronic records were available for both 2001 and 2008. The ten districts were the Central District of California, the Northern and Southern Districts of Iowa, the Western District of Louisiana, the Southern District of New York, the Eastern and Western Districts of Oklahoma, the District of South Carolina, the Eastern District of Texas, and the Northern District of West Virginia.

[18] Twice as many filings were used in 2008 because the significant fraction of households receiving direct deposits instead of checks decreases the precision of our estimates.

[19] All filers were categorized into one of three marital status categories according to the bankruptcy petition. If no marital information was provided, we categorized the filer as single. A χ^2-test fails to reject that the shares of filers in the marital status categories changed between 2001 and 2008, p-value 0.180.

[20] A small number of filers receive waivers for the filing fees or arrange to pay them on installment. We find that fewer than 1% fail to pay the full amount by the time of filing.

that difference across years is statistically significant at the 1% level.[21]

As shown in table 5, the majority of legal fees are paid by the time of filing. Despite the increase in fees, the percentage of fees paid increased from 79% in 2001 to 86% in 2008. Instead of paying for formal legal representation, filers can elect to represent themselves in court and pay a smaller amount for legal advice and document preparation. The share of filers representing themselves declined from 3.4% to 1.8%. This last comparison suggests that the increased paperwork required by the BAPCPA may have made it more difficult for filers to forgo formal legal representation.

The last set of numbers in table 5 presents statistics on the filers' finances. These statistics suggest three general patterns. First, filers were significantly wealthier in 2008 than in 2001. Average annual income increased from $23,784 to $31,581, total assets increased from $70,923 to $112,259, and total liabilities increased from $136,541 to $181,823.[22] These patterns are surprising since a main goal of the BAPCPA was to discourage high-income households from filing for Chapter 7 bankruptcy. At the same time, the average liabilities-to-income ratio rose from 5.9 in 2001 to 6.6 in 2008, suggesting greater indebtedness. Consequently, it is not clear from these simple comparisons whether filers were more or less liquidity constrained in 2008.

Another pattern in the data is that filers' liabilities dwarf their assets and income. In both years, the average filer reported liabilities roughly six times larger than their annual income and nearly twice as large as total assets. It is important to note that these financial variables are heavily skewed. For instance, mean liabilities in 2001 were $135,649, while the median was less than half as large ($61,989). As a result, we take the logarithm of these variables in the regression analysis reported in table OA3 in the online appendix.

### B. Changing Characteristics of Bankruptcy Filers after the Tax Rebates

This section describes how the characteristics of households filing for bankruptcy changed after the tax rebates. Both our conceptual framework and the estimates in section IV suggest that the number of liquidity-constrained filers increases in the weeks after the rebates. This suggests that we should observe a change in the average characteristics of the filers.

We evaluate whether the rebates changed the characteristics of filers by presenting the distribution of several financial characteristics: total liabilities, liabilities-to-income ratios, and annual income. The distributions allow us to compare those who filed before to those who filed after the rebates. We

FIGURE 7.—FILERS' LIABILITIES BEFORE AND AFTER THE REBATES
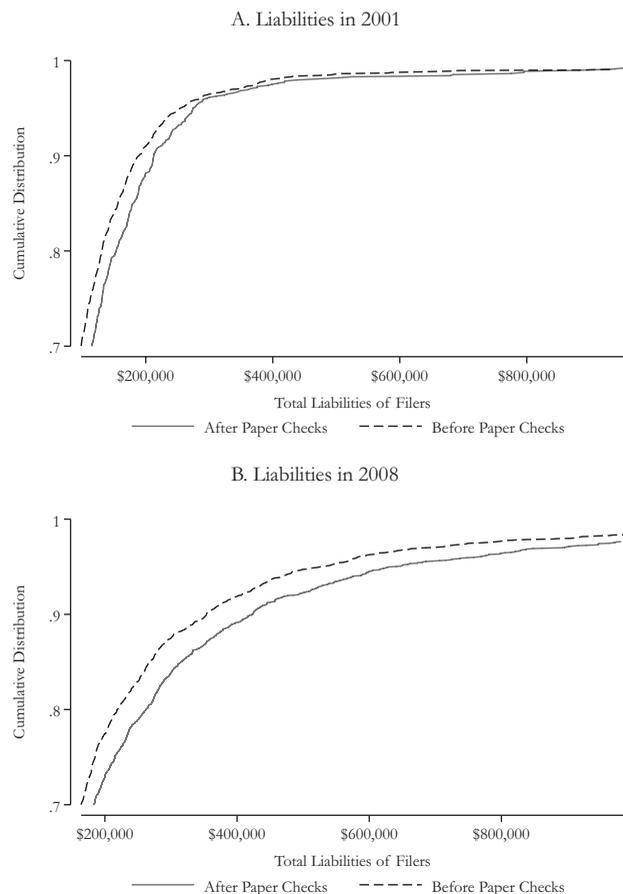
A. Liabilities in 2001

B. Liabilities in 2008

The figure presents the empirical CDFs based on a random sample of Chapter 7 bankruptcies. A Kolmogorov-Smirnov test of the null hypothesis that the two distributions are equal leads to a *p*-value of 0.001 in 2001 and 0.001 in 2008.

also report Kolmogorov-Smirnov (K-S) tests of the equality of these distributions. In addition, table OA3 reports regression tables analogous to the figures presented in this section.[23]

Figure 7 presents empirical cumulative distribution functions for the total liabilities of filers in 2001 and 2008. In each panel, the solid line plots the distribution of total liabilities for those who filed after the rebates, and the dashed line plots the distribution for the filers who filed before the rebates. Both panels suggest that households that filed after the rebates had higher total liabilities. In both figures, the associated K-S test rejects the null hypothesis that the distributions are identical.

Figure 8 presents a similar pattern for the ratio of total liabilities to income of each filer (debt-to-income ratio). The post-rebate filers have higher debt-to-income ratios. By contrast, we do not find consistent evidence that the distribution of income differs across the two groups of filers (figure 9).
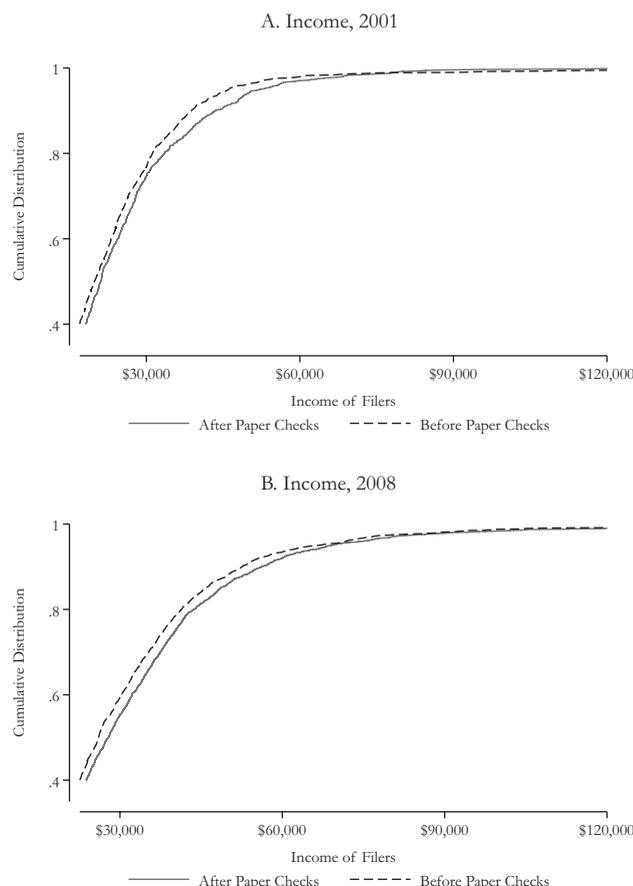
A. Liabilities-to-Income Ratio, 2001

A. Income, 2001

B. Liabilities-to-Income Ratio, 2008

B. Income, 2008
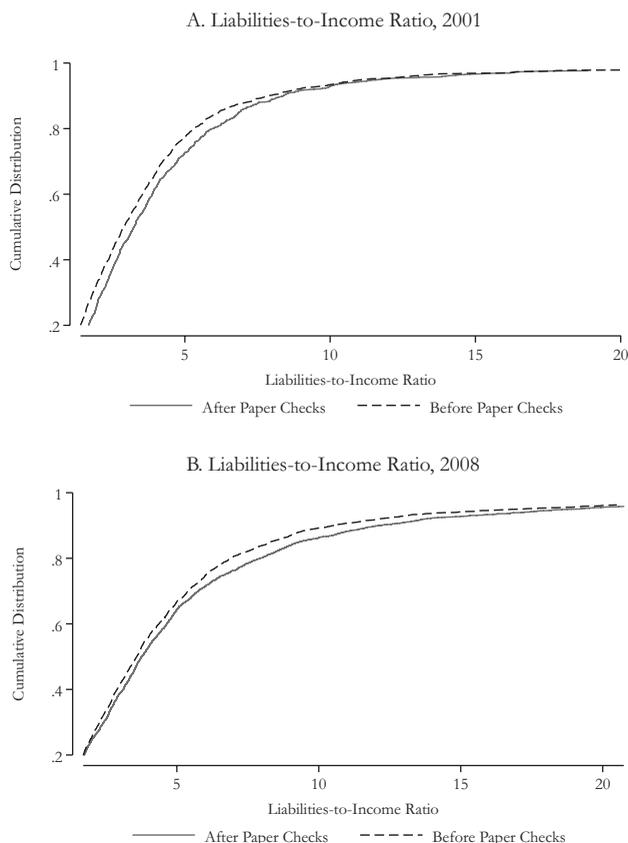
The figure presents the empirical CDFs based on a random sample of Chapter 7 bankruptcies. A Kolmogorov-Smirnov test of the null hypothesis that the two distributions are equal leads to a p-value of 0.004 in 2001 and 0.015 in 2008.

The figure presents the empirical CDFs based on a random sample of Chapter 7 bankruptcies. A Kolmogorov-Smirnov test of the null hypothesis that the two distributions are equal leads to a p-value of 0.097 in 2001 and 0.002 in 2008.

Overall, the results suggest that households filing for bankruptcy after the rebates are more likely to be liquidity constrained. Households filing after the rebates have larger liabilities and a higher debt-to-income ratio than households filing before the rebates. By contrast, they have roughly similar incomes.

VI. Discussion

This section considers alternative explanations for our empirical findings and discusses their implications for policy.

A. Alternative Explanations

Our preferred explanation for the pattern of results we find is that liquidity-constrained households are unable to afford bankruptcy. Three alternative explanations merit discussion. The first alternative explanation is that households timed their bankruptcy in order to keep their rebates from creditors or the court. We find this explanation unlikely since it should lead households to file before receiving the rebates, not after. Since prefiling income is subject to creditor action, filers would want to file before receiving the rebates in order to

shield them from creditors, but we observe the opposite timing. As described in section II, the relevant case law suggests that bankruptcy judges were aware of the rebates and treated rebate income identically to other income. Still, were such an effect to exist, it would likely bias our estimates towards 0, implying that our estimates of the importance of liquidity constraints are conservative.

A second alternative explanation, which we call the consumption hypothesis, suggests that households waited to receive their rebates, consumed their rebates, and then filed for bankruptcy. The law, however, limits this type of behavior. Upon filing, bankruptcy trustees would become aware that households received rebate checks. Activities taken solely for the purpose of avoiding creditors are considered in bad faith and can result in case dismissal. Moreover, the rebates were exempt from creditor action for nearly all households, obviating the need for strategic behavior. Note that the average wild card exemption under Chapter 7 is $7,073 (Mahoney, 2012), and 94% of filings in our sample are no-asset bankruptcies in which all of the debtor's assets were exempt. The rebates could not have shifted a large share of households beyond that exemption threshold.

Moreover, if households were to file for bankruptcy only after consuming their rebates, then we would expect a decrease in bankruptcies before the rebates were distributed. The event-study results do not suggest such a decrease, although we concede that our power is limited to detect such an effect. Finally, this alternative explanation cannot readily account for the pattern across chapters or for the change in average liabilities before and after the rebates, as demonstrated in section V. Our preferred interpretation more readily accounts for the differences across Chapters 7 and 13.

Finally, a third alternative explanation for our results is that creditors or debt collectors initiated actions based on the timing of the tax rebates, thereby driving some households to file for bankruptcy. This "supply-side hypothesis" does not readily account for the difference in treatment effects across chapters, since debt collectors had similar incentives to initiate actions toward households considering Chapter 13. Our discussions with industry experts suggest that creditors and debt collectors are often aware of anticipated changes in liquidity such as annual tax refunds and social security payments. It is difficult, however, for collectors to finely tune their actions in response to individual debtors' rebate dates. The number of collections inquiries a consumer may receive is limited by law.[24] Since bankruptcy greatly curtails the prospects of debt recovery, creditors face an incentive to limit their own activities so as not to push households into bankruptcy.

Recall that the rebate effects were larger in 2008 than in 2001 and that paper checks and direct deposits accounted for similar shares of the total rebate effect in 2008. These patterns are inconsistent with the supply-side explanation. Households could choose to receive rebate checks by direct deposit in 2008 (but not in 2001), and creditors had no way of knowing which households chose direct deposit. The direct deposit dates were up to two months earlier than the paper check dates in 2008, making it unlikely that creditors could have precisely timed their actions in a way that would have induced some households to file immediately after receiving rebate checks.

Overall, we cannot completely rule out any of these alternative explanations. Our view is that these hypotheses are unlikely to be the primary explanations for the rebate effects. Only the liquidity constraints hypothesis can account for the pattern of effects we document: the contrast in rebate effects across chapters and years, the immediate and short-run response to the rebates, and the concentration of the effect among households with high liabilities.

B. Policy Implications

Our empirical evidence suggests that legal and administrative fees force liquidity-constrained households to delay filing for bankruptcy. It is not clear, however, whether lower fees would raise welfare. The effect of fees on social welfare depends on whether liquidity-constrained filers are those with the largest or the smallest utility gain from bankruptcy. If liquidity-constrained filers have the most to gain from bankruptcy, then entrance fees are likely to be socially inefficient. In this case, the bankruptcy system could rely on exemptions or the seizure of assets instead in order to deter bankruptcies. Conversely, if liquidity-constrained filers gain less from bankruptcy than other filers, then entrance fees may serve as an efficient mechanism to deter such bankruptcies. In this way, liquidity constraints transform entrance fees into ordeal mechanisms (Nichols & Zeckhauser, 1982).

We speculate that reducing legal and administrative fees is likely to improve social welfare. Our model suggests that liquidity-constrained households suffer the greatest utility loss from fees and enjoy the greatest utility gain from being able to file for bankruptcy. Therefore, the results support the argument made by legal scholars that a reduction in legal fees would be welfare enhancing (Mann & Porter, 2010).[25]

However, we temper this conclusion with several caveats. Our model assumes that households are ex ante identical and borrow identical amounts of debt, leading to the result that liquidity-constrained households are those with the least realized wealth. In practice, bankrupt households vary considerably by income, assets, and indebtedness. While we do not find evidence that the households that respond to rebates have lower income or assets (as predicted by the model), we do show that the households responding to rebates have higher liabilities. The model predicts that this would lead to greater utility gains from filing for bankruptcy.

More importantly, this empirical setting cannot shed any light on the moral hazard costs of lowering entrance fees. High fees may prevent two forms of moral hazard that our model does not address. First, fees may inhibit households from borrowing excessively. Second, fees may deter bankruptcy, holding borrowing constant. Both of these forms of moral hazard must be balanced against the benefits of reducing fees. To the extent that liquidity-constrained filers impose larger moral hazard costs than the average filer, filing fees may be effective in reducing moral hazard costs overall. An important task in future work will be quantifying the moral hazard costs associated with reducing entrance fees to bankruptcy.

VII. Conclusion

We find that tax rebates cause a significant short-run increase in consumer bankruptcies. This evidence is consistent with the hypothesis that legal and administrative fees force liquidity-constrained households to delay filing.

---

[24] See note 26 of Mann and Porter (2010) for a list of such state laws.

[25] Mann and Porter (2010) argue that Congress can lower the amount of paperwork required for bankruptcy, which would lower legal fees. They propose an expedited form of bankruptcy for low-asset filers.

These results highlight the importance of liquidity constraints in the optimal design of the consumer bankruptcy system.

An important area of future work is the consumption-smoothing benefits of bankruptcy. This is an important parameter in any comprehensive welfare analysis of the bankruptcy system. Such research may also shed light on the extent to which rebate-induced bankruptcies provide effective economic stimulus. Our evidence suggests that tax rebates allow some households to avoid a delay in filing for bankruptcy. If these households substantially increase consumption following the discharge of their debts, then perhaps the timely discharge of household debt is an important component of economic stimulus policies (Mian, Rao, & Sufi, 2012).

Another area of future work involves the determinants of bankruptcy. A long-running debate centers over whether bankruptcies are primarily caused by unexpected negative shocks (Himmelstein et al., 2009; Fay, Hurst, & White, 2002). More recent work has emphasized the importance of myopic behavior (Hankins, Hoekstra, & Skiba, 2011; Zhu, 2011). By contrast, our results suggest that an important (and overlooked) determinant of bankruptcy may simply be the ability of households to afford the fees.

Finally, the concept that liquidity constraints affect the utilization (or take-up) of social insurance likely extends beyond consumer bankruptcy. Previous work has found that liquidity constraints are an important determinant of the behavioral response to unemployment insurance (Chetty, 2008), and we suspect that the decision to claim unemployment insurance benefits at all is also affected by liquidity constraints. Similarly, we suspect that the waiting periods for disability insurance interact with liquidity constraints in affecting the timing of individuals' applications. We thus believe a promising area for future research involves estimating the effect of liquidity constraints on the take-up of a broad range of social insurance programs.

REFERENCES

Agarwal, S., C. Liu, and N. Souleles, "The Reaction of Consumer Spending and Debt to Tax Rebates: Evidence from Consumer Credit Data," *Journal of Political Economy* 115 (2007), 986–1019.

Anderson, N. B., and J. K. Dokko, "Liquidity Problems and Early Payment Default among Subprime Mortgages," Board of Governors of the Federal Reserve System working paper (2011).

Bertrand, M., and A. Morse, "What Do High-Interest Borrowers Do with Their Tax Rebate?" *American Economic Review Papers and Proceedings* 99 (2009), 418–429.

Chetty, R., "Moral Hazard versus Liquidity and Optimal Unemployment Insurance," *Journal of Political Economy* 116 (2008), 173–234.

Dynarski, S., "Does Aid Matter? Measuring the Effect of Student Aid on College Attendance and Completion," *American Economic Review* 93 (2003), 278–288.

Evans, W. N., and T. J. Moore, "The Short-Term Mortality Consequences of Income Receipt," *Journal of Public Economics* 95 (2011), 1410–1424.

Fay, S., E. Hurst, and M. White, "The Household Bankruptcy Decision," *American Economic Review* 92 (2002), 706–718.

Gross, T., and J. Tobacman, "Income Shocks and the Demand for Health Care: Evidence from the 2008 Stimulus Payments," unpublished manuscript (2011).

Hankins, S., M. Hoekstra, and P. M. Skiba, "The Ticket to Easy Street? The Financial Consequences of Winning the Lottery," this REVIEW 93 (2011), 961–969.

Hansen, G. D., and A. İmrohoroğlu, "The Role of Unemployment Insurance in an Economy with Liquidity Constraints and Moral Hazard," *Journal of Political Economy* 100 (1992), 118–142.

Himmelstein, D., D. Thorne, E. Warren, and S. Woolhandler, "Medical Bankrutpcy in the United States, 2007: Results of a National Study," *American Journal of Medicine* 122 (2009), 741–746.

Hsieh, C., "Do Consumers React to Anticipated Income Changes? Evidence from the Alaska Permanent Fund," *American Economic Review* 93 (2003), 397–405.

Hurst, E., and A. Lusardi, "Liquidity Constraints, Wealth Accumulation, and Entrepreneurship," *Journal of Political Economy* 112 (2004), 319–347.

Johnson, D., J. Parker, and N. Souleles, "Household Expenditure and the Income Tax Rebates of 2001," *American Economic Review* 96 (2006), 1589–1610.

Lusardi, A., D. J. Schneider, and P. Tufano, "Financially Fragile Households: Evidence and Implications," NBER working paper 17072 (2011).

Mahoney, N., "Bankruptcy as Implicit Health Insurance," mimeograph, Harvard University, (2012).

Mann, R., and K. Porter, "Saving Up for Bankruptcy," *Georgetown Law Journal* 98 (2010), 289–290.

McIntyre, F., D. Sullivan, and T. Layton, "Did BAPCPA Deter the Wealthy? The 2005 Bankruptcy Reform's Effect on Filings across the Income and Asset Distribution," unpublished manuscript (2010).

Mian, A., K. Rao, and A. Sufi; "Household Balance Sheets, Consumption, and the Economic Slump," University of Chicago working paper (2012).

——— "The Consequences of Mortgage Credit Expansion: Evidence from the US Mortgage Default Crisis," *Quarterly Journal of Economics* 124 (2009), 1449–1496.

Nichols, A. L., and R. J. Zeckhauser, "Targeting Transfers through Restrictions on Recipients," *American Economic Review* 72 (1982), 372–377.

Parker, J. A., N. S. Souleles, D. S. Johnson, and R. McClelland, "Consumer Spending and the Economic Stimulus Payments of 2008," NBER working paper 16684 (2011).

Shapiro, M., and J. Slemrod, "Did the 2001 Tax Rebate Stimulate Spending? Evidence from Taxpayer Surveys," *Tax Policy and the Economy* 17 (2003), 83–109.

Souleles, N., "The Response of Household Consumption to Income Tax Refunds," *American Economic Review* 89 (1999), 947–958.

Stavins, J., "Credit Card Borrowing, Delinquency, and Personal Bankruptcy," *New England Economic Review* (July–August 2000), 15–30.

Stephens, M., "'3rd of the Month': Do Social Security Recipients Smooth Consumption between Checks?" *American Economic Review* 93 (2003), 406–422.

U.S. GAO, "Bankruptcy Reform: Dollar Costs Associated with the Bankruptcy Abuse Prevention and Consumer Protection Act of 2005," Report to Congressional Requesters GAO-08-697, (Washington, DC: U.S. Government Accountability Office, 2008).

Zhu, N., "Household Consumption and Personal Bankruptcy," *Journal of Legal Studies* 40 (2011), 1–37.

Zywicki, T. J., "An Economic Analysis of the Consumer Bankruptcy Crisis," *Northwestern University Law Review* 99 (2005), 1463–1542.