The Economic Consequences of Bankruptcy Reform*

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August 2019

Abstract

A more generous consumer bankruptcy system provides greater insurance against financial risks, but it may also raise the cost of credit to consumers. We study this trade-off using the 2005 Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA), which raised the costs of filing for bankruptcy. We identify the effects of BAPCPA on borrowing costs by exploiting variation in the effects of the reform on bankruptcy risk across credit-score segments. Using a combination of administrative records, credit reports, and proprietary market-research data, we find that the reform reduced bankruptcy filings, and reduced the likelihood that an uninsured hospitalization received bankruptcy relief by 70 percent. BAPCPA led to a decrease in credit card interest rates, with an implied pass-through rate of 60–75 percent. Overall, BAPCPA decreased the gap in offered interest rates between prime and subprime consumers by roughly 10 percent.

*The views expressed are those of the authors and do not necessarily reflect those of the Consumer Financial Protection Bureau or the United States. We thank Huan Zhao, staff at the California Office of Statewide Health Planning and Development, and Carlos Dobkin for their assistance accessing and compiling datasets used in this project. We are grateful to Amy Finkelstein, Paul Goldsmith-Pinkham, Erik Hurst, Neale Mahoney, Brian Melzer, Scott Nelson, Christopher Palmer, James Poterba, Amit Seru, Tavneet Suri, Heidi Williams, Jonathan Zinman, and seminar participants at MIT, NYU, AEA, Harvard Business School (EM), Arizona, Wharton (BEPP), the Federal Reserve Bank of New York, Microsoft Research, GWU, CFPB, Duke Fuqua, Harvard Law School, WAPFIN, and NBER Summer Institute Corporate Finance and Law and Economics sessions for thoughtful feedback and comments. Anran Li and Pinchuan Ong provided excellent research assistance.

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1 Introduction

In recent decades, the rate of consumer bankruptcy filings in the United States climbed from 0.3 percent of households annually in the early 1980s to 1.5 percent in the early 2000s (Board of Governors, 2006). This five-fold increase in the bankruptcy rate was cited by lawmakers as a reason to pass the 2005 Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA). The bill implemented a number of provisions that collectively made filing for bankruptcy more onerous, more expensive, and less financially beneficial.

By making bankruptcy less attractive, BAPCPA was widely expected to reduce bankruptcy filings. But there was considerable debate regarding how the reform would affect consumer credit markets. Proponents of the bill argued that creditors would pass through higher debt-recovery rates in the form of lower interest rates. Judge Richard Posner argued that "the new Act... should reduce interest rates and thus make borrowers better off" (Posner, 2005). By contrast, critics of the reform argued that a reduction in filings would not be passed-through to borrowers but would instead be captured by lenders.[1] Additionally, critics contended that claims of abuse were overstated, and that the bill would harm filers struggling with medical expenses and job loss (Warren and Tyagi, 2005).

This paper informs that debate by estimating how the decline in bankruptcy filings caused by BAPCPA affected the interest rates offered to consumers in unsecured credit markets. Our analysis consists of three main steps. First, we develop a stylized model of consumer bankruptcy to study the relationship between the bankruptcy filing rate and the cost of borrowing. We show that decreases in filing risk are associated with interest rate declines. The pass-through rate depends on the amount recovered from those deterred from filing at the margin. All else equal, the higher the average repayment rate for these marginal filers, the greater the change in interest rates. We calibrate the pass-through expression under perfect competition to produce a benchmark of expected interest rate pass-through of 87–112 basis points for a 1 percentage point change in the bankruptcy filing rate.

Second, we document that the reform indeed dramatically decreased the number of bankrupt-

[1] For instance, Warren (2004) wrote that the "industry never said that the $44 billion it planned to recover from bankrupt families would be passed on to customers. History suggests that it would not."

cies, using "excess mass" techniques borrowed from the recent literature studying bunching at tax kinks (Saez, 1999; Kleven and Waseem, 2013). We use this approach in order to estimate the net effect of BAPCPA after accounting for the large increase in bankruptcies just prior to the effective date of BAPCPA in October 2015. In the months immediately before the reform, we estimate a net increase in filings of more than 750,000, as consumers rushed to file for bankruptcy to discharge their debts before the new bankruptcy code was implemented. Over time, however, we find that the reform reduced the bankruptcy rate by roughly 50 percent. Net of the short-run increase during the "rush-to-file," we estimate that there were roughly one million fewer bankruptcy filings in the two years after BAPCPA than would have occurred without the reform.

Third, we estimate the pass-through of the change in bankruptcy filings to interest rates by exploiting variation in the effect of the reform on bankruptcy filings across the credit score spectrum. To do so, we first estimate the effect of BAPCPA on the bankruptcy filing rate within each 10-point credit score bin using detailed credit report data for a large sample of individuals. We combine the change in filing risk with proprietary data on interest rates of credit card offers for the same bins. We then estimate event study and difference-in-difference regression models that compare the change in prices to the change in filing risk for each credit score bin before and after the reform.[2] The key identifying assumption is that interest rates would have evolved similarly across credit-score segments absent the reform. We provide evidence supporting this "common trends" assumption with an event-study figure, which shows no differential trends across credit-score segments in the years leading up to the reform. We also show that the results are robust to subprime-specific trends, suggesting that the results are not driven by other factors differentially affecting the subprime credit market during this time period.

We find that a one percentage point decline in bankruptcy filing risk within a credit-score segment decreases average interest rates by 67 basis points. Interpreting these estimates through the lens of the stylized model, we conclude that between 60 and 75 percent of the cost savings to creditors from the reduced bankruptcy filings were passed on to consumers. According to the

[2] We focus primarily on the effect of BAPCPA on credit card interest rates. Lenders may also have responded to BAPCPA by changing credit limits (in addition to changing prices), but we do not study such adjustments in this paper. One benefit of focusing on price adjustments is that it is more straightforward to calculate pass-through estimates and compare our results to a perfect-competition benchmark (full pass-through).

model, these results imply that the marginal consumers deterred by BAPCPA likely repaid a large share of their unsecured debts instead of discharging their debts in bankruptcy.

One of the central provisions of BAPCPA was a "means test" which restricted the options available to high-income filers and was intended to "ensure that debtors repay creditors the maximum they can afford" (House Report, 2005). The means test was designed to shift filers with higher incomes from Chapter 7 to Chapter 13 by eliminating the option to file under Chapter 7.[3] While we find that BAPCPA lowered interest rates, we find suggestive evidence that this effect was *not* in fact driven by a disproportionate decline in filing by high-income filers, as policymakers had intended. Counter to the intent of the law, we find no change in the distribution of income of bankruptcy filers when we proxy for income using the median income in the filer's ZIP Code.[4] These findings are consistent with other research which has suggested that the cost of filing for bankruptcy deters filings (Gross et al., 2014).

We also find suggestive evidence that BAPCPA reduced the rate of bankruptcy filing following a hospital admission. Building on the event-study approach of Dobkin et al. (2018a), we find that an uninsured hospitalization increases the likelihood of filing for bankruptcy by 1.5 percentage points prior to the reform, but by just 0.4 percentage points after the reform. While a hospitalization is just one example of an adverse shock, to the degree that this finding generalizes to other types of negative wealth shocks, these results suggest that the bankruptcies deterred by BAPCPA were not limited to the most "abusive" filings and meaningfully reduced the insurance value of bankruptcy.[5]

Taken together, the results in this paper suggest that BAPCPA reduced bankruptcy filings, but not in the targeted way that proponents had intended. Nevertheless, the law still appears to have provided meaningful benefits to many consumers in the form of lower credit card interest rates. Our model reconciles these results by highlighting the importance of the average repayment

[3] Chapter 7 bankruptcy offers filers a "fresh start." All qualifying debts are discharged in exchange for their non-exempt assets. Chapter 13 bankruptcy offers filers a "reorganization." Chapter 13 filers lose no assets, but must commit to a repayment plan out of their future income.

[4] Additionally, we find only a small increase in the share of Chapter 13 filings and find that filings of both chapters declined after the reform. These results suggest that the BAPCPA caused large declines in bankruptcy rates across a broad range of consumers, swamping an effect of the means test on inducing consumers to switch between Chapter 7 and Chapter 13.

[5] While the share of bankruptcies caused by hospitalizations appears to be small according to Dobkin et al. (2018b), it is a frequently cited source of financial risk faced by households and one the bankruptcy system is frequently referenced as insuring.

rates of marginal filers in determining the change in interest rates. The model also reveals that high repayment rates for marginal filers is consistent with high costs of filing. This highlights the stark trade-off for policymakers: marginal filers may benefit substantially from a more generous bankruptcy system (getting a "fresh start" instead of repaying a larger share of their debts), but these benefits come at the cost of higher interest rates for other consumers.

This paper contributes to three main areas of research. First, it contributes new evidence on the effect of the bankruptcy code on consumer credit markets. Gropp et al. (1997), Berkowitz and White (2004), and Severino and Brown (2017) use cross-state variation in bankruptcy exemptions and find that more-generous exemptions are associated with less readily available credit, and Chakrabarti and Pattison (2016) find that the elimination of auto loan "cramdowns" under Chapter 13 reduced interest rates on auto loans.[6] This paper is also related to earlier evaluations of BAPCPA which focused on aggregate interest-rate spreads (Simkovic, 2009) and student loans (Alexandrov and Jiménez, 2017). We also implement a time-series analysis, which shows some apparent narrowing of interest rates between subprime and prime consumers, but our main results go beyond that simple time-series analysis by exploiting variation in effects of the reform across credit-score segments. To our knowledge, this paper provides the first event-study estimates of the effects of BAPCPA on credit card interest rates.[7]

Second, this paper provides new evidence on pass-through in consumer credit markets. Existing research on pass-through in credit markets emphasizes sticky interest rates (and thus limited pass-through), but this research typically estimates pass-through using shocks to the cost of funds rather than shocks to default risk (e.g., Ausubel, 1991; Calem and Mester, 1995; Stavins, 1996; Stango, 2000; Calem et al., 2006; Agarwal et al., 2017). We thus contribute new evidence to this literature by estimating how changes in bankruptcy filing risk are passed-through to credit card interest rates.

[6] Cramdowns allowed borrowers to reduce the principal of their loan to the current market value of the vehicle.

[7] Other related work on BAPCPA includes studies by Li et al. (2011), Morgan et al. (2012), and Mitman (2016). Those papers argue that by reducing the substitutability between bankruptcy and foreclosure, BAPCPA increased foreclosures and exacerbated the mortgage crisis. Albanesi and Nosal (2018) also evaluate BAPCPA and document similar declines in bankruptcy filing rates for Chapter 7, which they attribute to liquidity constraints from the increased cost of filing. They also document an accompanying rise in insolvency and, along with Han and Li (2011), show that bankruptcy filers have better access to credit than the insolvent. Nakajima (2017) argues that BAPCPA improved welfare by lowering default premia and improving consumption smoothing, but not for agents with temptation. Relative to this work, this paper contributes event-study estimates of the effect of BAPCPA on interest rates and estimates pass-through.

While a full reconciliation of the different pass-through estimates is outside the scope of this paper, Grodzicki (2017) argues that credit card markets have become more competitive in recent years following the costly adoption of screening technology, which is consistent with a large amount of price responsiveness to changes in default risk due to bankruptcy reform.[8]

Lastly, this paper is related to studies of the consumer bankruptcy system that calibrate structural models (Zame, 1993; Dubey et al., 2005; Livshits et al., 2007; Chatterjee et al., 2007; Mitman, 2016; Nakajima, 2017). Those models typically emphasize the trade-off between using bankruptcy to smooth consumption across states of the world and the higher cost of smoothing consumption over time, often while assuming perfectly competitive credit markets (and thus full pass-through of the costs of lending to consumers). Our results suggest a large amount of pass-through from bankruptcy reform to interest rates in consumer credit markets, which provides empirical evidence in support of these modeling assumptions.

The remainder of this paper proceeds as follows. The next section provides background information on bankruptcy before and after BAPCPA. Section 3 develops a simple model to guide an assessment of the costs and benefits of bankruptcy reform. Section 4 describes our data sources and sample construction. Section 5 evaluates how BAPCPA affected the number of filings, Section 6 then estimates the pass-through of this decline in bankruptcy to borrowing costs. Section 7 evaluates how the reform changed *who* declares bankruptcy. Section 8 concludes.

2 Institutional Background

In contrast to other developed countries, American consumers have historically enjoyed an exceptionally debtor-friendly bankruptcy system.[9] In particular, American consumers filing for bankruptcy have had the option to freely choose between a "fresh start" (liquidating outstanding debts through Chapter 7) and a "reorganization" of debts (repaying debts on an installment plan over several years through Chapter 13). Chapter 7 filers must forfeit all non-exempt assets in exchange for discharge of their debt, while Chapter 13 filers are allowed to keep all of their assets

[8] More broadly, this paper is also related to other empirical studies of pass-through in imperfectly competitive markets, such as recent work on Medicare Advantage (Cabral et al., 2018) and the Spanish electricity market (Fabra and Reguant, 2014).

[9] Italy, for instance, had no form of consumer bankruptcy until 2015, and Germany only began allowing consumer bankruptcy in 1999. Before then, consumers in those countries had few options to discharge their debts (Tabb, 2005).

but must repay their debt out of future income.

Despite the potential financial benefits, consumer bankruptcy has historically been a relatively rare phenomenon in the United States. In the late 1970s, just 0.3 percent of households filed for bankruptcy in a given year. In 1978, the United States adopted a new bankruptcy code and legalized advertisements by bankruptcy attorneys. A 1978 Supreme Court decision allowed banks to export their home interest rates and so evade state usury laws.[10] These changes in policy catalyzed the growth of unsecured borrowing in the ensuing decades (White, 2007). By 1999, the bankruptcy rate had increased to 1.5 percent, prompting creditors to lobby for a more-stringent bankruptcy code.

To make their case, credit-industry lobbyists pointed to a handful of high-profile cases of "exemption shopping." For those bankruptcies, debtors moved across state lines to select the most-beneficial bankruptcy regime, and this was held up as emblematic of the abuse rampant in the bankruptcy system. A reform of the bankruptcy system was first drafted in 1998 and passed by Congress in 2000, but pocket-vetoed by President Clinton. The bill was reintroduced each Congress until it finally passed with broad bipartisan support in 2005. The Senate passed the bill on March 10, 2005, the House on April 14, 2005, and it was signed by President Bush on April 20, 2005. The new bankruptcy code went into effect for all bankruptcies filed on or after Monday, October 17, 2005.

BAPCPA made filing for bankruptcy less attractive in three primary ways. First, the law sought to prohibit higher-income households from filing Chapter 7. To do so, lawmakers introduced a means test which they referred to as "the heart of the bill" (House Report, 2005). The means test added a "presumption of abuse" for filers whose income is above certain thresholds. Debtors are subject to the means test if their income from the previous six full months before filing, adjusted for family size, is more than the state median income (Parra, 2018).[11] Debtors subject to the means test are

[10]In *Marquette National Bank of Minneapolis v. First of Omaha Service Corporation* (439 U.S. 299 (1978)), the U.S. Supreme Court ruled that state anti-usury laws regulating interest rates are not enforceable against nationally chartered banks based in other states.

[11]Virtually all income is included in this calculation with the notable exception of Social Security income. Those with debts that are not "primarily consumer debts" (e.g. business investments) are also exempt from the means test. Debtors can also "pass" the means test if they can demonstrate that their "disposable income" (income after allowed deductions) is less than \$182.50 or \$109.59, if that is enough to pay unsecured creditors more than 25 percent of the debt owed over five years.

functionally prohibited from filing Chapter 7, and can only file Chapter 13 (which, post BAPCPA, required higher repayment). This created an incentive for borrowers to suppress their labor supply and earnings below the state median in order to skirt the means test and file Chapter 7 or reduce their repayment obligation under Chapter 13.[12]

Second, BAPCPA limited the benefits of filing for bankruptcy along a number of dimensions. Prior to BAPCPA, Chapter 13 filers could propose their own repayment plan and faced no incentive to offer a repayment plan more generous than the relief they would receive under Chapter 7. After BAPCPA, Chapter 13 filers are required to forfeit 100 percent of their disposable income for five years to pay down their debts.[13] The reform also limited the ability of filers to discharge some purchases and "exemption shop" for the most-favorable state bankruptcy regime. Debtors who move must now wait two years before they are allowed to file under their new state's exemptions. Bankruptcy filers must wait a set number of years before they are allowed to file again. BAPCPA increased the waiting period from six years to eight years for Chapter 7 and from six months to two years for Chapter 13.

Finally, BAPCPA made the process of filing for bankruptcy much more burdensome and expensive. Bankruptcy court fees themselves increased. Bankruptcy filers are now required to take two educational courses: a credit-counseling course before filing and a financial-management course before their debt is discharged. Filing requirements increased and bankruptcy attorneys were made liable for any inaccuracies in the filing, which increased attorney costs by as much as $500 and subsequently increased the fees they charged their clients, which are not dischargeable in bankruptcy (House Report, 2005). Altogether, these changes increased the mean financial cost of filing from $868 to $1,309 for Chapter 7 and from $2,260 to $2,861 for Chapter 13 (Lupica, 2012).

[12]The incentive to suppress income prior to filing is relevant even if households cannot suppress it enough to get under the state median. Chapter 13 repayment plans, which are paid over the subsequent five years, are based on documented disposable income *over the prior six months*. As White (2007) points out, a reduction in monthly earnings of $1 for the six months prior to filing costs filers $6 in the short-run but reduces their repayment requirement by $60 ($1 each month over the next 60 months).

[13]Allowances for living expenses vary by metropolitan area and are largely based on the Internal Revenue Service policies for the treatment of delinquent taxpayers.

3 Economic Framework

This section develops a simple economic framework to determine the effects of an increase in the cost of filing on bankruptcy filing rates and borrowing costs.[14] In addition, we use the framework to calibrate a benchmark for the expected magnitude of any potential effects on interest rates.

3.1 Model Set-up

There are I different types of individuals, with individual types indexed by $i \in \{1, \ldots, I\}$. The types are intended to represent the credit score "segments" that we use in our empirical analysis. There is a unit mass of each type, and individuals within each type are ex-ante identical and live for two periods. In the first period, each individual borrows b_i at interest rate r_i, so that if the debt is repaid in full in the second period, then $(1 + r_i)b_i$ is repaid.[15] In the second period, each individual receives income y, drawn from distribution $f_i(y)$, with associated cumulative distribution function $F_i(y)$.

After observing their income, y, individuals can either repay their outstanding debt, or they can file for bankruptcy and retain all of their assets up to an exemption level, e. When individuals file, they must also pay cost c, which captures all relevant fixed costs of filing: filing fees, legal costs, hassle costs, and stigma.

This leads to a simple decision rule: an individual will file for bankruptcy if income (net of full repayment of debt) is less than the exemption amount minus the cost of filing $(e - c)$; that is, if $y - (1 + r_i)b_i < e - c$.[16] When individuals file for bankruptcy, creditors recover $\max\{0, y - e\}$. We work through the case where individuals with income below c are insolvent and unable to file for bankruptcy in Appendix Section A.3, but for simplicity here, we assume that $f_i(y)$ has no support below c for all types.

[14] The model we develop is most closely related to the model of Wang and White (2000), who simulate various bankruptcy reforms, but do not discuss the determinants of the magnitude of pass-through, which is one of the goals of our analysis.

[15] We treat b_i as exogenous throughout this simple framework. In reality, changes in the bankruptcy code should also affect the amount borrowed. We make this simplification in order to focus on the response of creditors to the change in bankruptcy filings induced by BAPCPA. Given the relatively short-run nature of our empirical analysis, we view this as the first-order impact of the changes to the bankruptcy code for our purposes.

[16] Consistent with the fact that the costs of filing are not dischargeable, in the model c must be paid out exempt assets.

3.2 Impact of Bankruptcy Reform on the Cost of Credit

Within the population of borrowers of each type, the probability of filing for bankruptcy can be defined as $p_i = F_i(e + (1 + r_i)b_i - c) = F_i(y_i^*)$, where y_i^* represents the second-period income at which a borrower of type i is indifferent between filing and not filing. Based on this decision rule, we can calculate how the exemption level e and filing costs c affect filing and repayment behavior. We are ultimately interested in the relationship between the bankruptcy code (i.e., e and c) and the cost of borrowing (r_i), but we begin by estimating how changes in the exemption level and cost of filing determine the share of individuals who choose to file, p_i.

Remark 1. *The direct effects of changes in the exemption level or the cost of filing on the share of the population filing for bankruptcy are given by the following expressions:*

$$\partial p_i/\partial e = f_i(y^*) > 0,$$
$$\partial p_i/\partial c = -f_i(y^*) < 0.$$

The expressions above are of equal and opposite magnitude, implying that marginal changes in the cost of filing and the exemption level have similar effects, with the magnitude determined by the mass of marginal individuals who were indifferent to filing prior to the reform.

Next we can examine the relationship between the bankruptcy code (i.e., e and c) and the cost of borrowing, r_i. As a benchmark, we assume that the credit market is perfectly competitive. We thus define the equilibrium interest rate, r_i, implicitly by setting the amount recovered by lenders to be equal to the amount of borrowing: $R_i(r_i) = b_i$.[17] To calculate the effect of changes in e and c on interest rates, we first define $R(r_i)$:

$$\begin{aligned} R_i(r_i) &\equiv \int_0^{e+(1+r_i)b_i-c} (\max\{0, y-e\}) f_i(y)dy + \int_{e+(1+r_i)b_i-c}^{\infty} ((1+r_i)b_i) f_i(y)dy, \\ &= \underbrace{\int_e^{e+(1+r_i)b_i-c} (y-e) f_i(y)dy}_{\text{Recovered from bankruptcy filers}} + \underbrace{\int_{e+(1+r_i)b_i-c}^{\infty} b_i(1+r_i) f_i(y)dy}_{\text{Recovered from non-filers}}. \end{aligned}$$

[17] An important implicit assumption in this setup is that each credit-score segment is priced separately and competitively, and individual types are fixed and do not respond endogenously to either market prices or the policy reform.

Using this expression, we can calculate the effect on the interest rate of a reform that changes either the exemption level (de) or the cost of filing (dc) by implicitly differentiating the equation $R_i(r_i) = b_i$. In the Online Appendix, we derive the expressions for these two comparative statics (dr_i/dc and dr_i/de), and we discuss the technical conditions for the two expressions to have equal and opposite sign, just as in Remark 1 above.[18] Next, using these two expressions we can derive an expression for the pass-through of the reform-induced change in bankruptcy filings into borrowing costs. In other words, we calculate dr_i/dp_i in the case where the change in the probability of filing has been affected by a reform that changes either the exemption level or the cost of filing, which yields the following intuitive pass-through expression:

$$\frac{dr_i}{dp_i} = \frac{c/b_i}{1-p_i} = \frac{1+r_i}{1-p_i} \cdot \frac{c}{(1+r_i)b_i} > 0. \tag{1}$$

The full derivation of this expression is given in the Online Appendix. In words, this expression states that a bankruptcy reform that decreases bankruptcies (either by reducing exemptions or raising filing costs) will decrease the interest rate, and the magnitude of the decrease will be increasing in the change in the probability of filing. The ratio is scaled by c/b_i, where c is the amount repaid to creditors by the marginal filer. This suggests that for bankruptcy reform to have a meaningful effect on interest rates, it must be the case that c/b_i is not small. This requires that marginal filers repay a meaningful share of their debts. Intuitively, this expression shows that the pass-through of bankruptcy reform to interest rates requires that marginal filers must face substantial (financial, hassle, or stigma) costs of filing.[19]

In order to calibrate a realistic benchmark for the magnitude of pass-through, the last part of Equation 1 separates the pass-through expression into a product of two terms. The first term is the ratio $\frac{1+r_i}{1-p_i}$, and the second term, $\frac{c}{(1+r_i)b_i}$, represents what creditors recover from the marginal non-filer, c, over the expected repayment of non-filers, $(1+r_i)b_i$. In other words, this second term represents the counterfactual recovery rate for the borrowers who are deterred (at the margin)

[18]The Online Appendix shows that the comparative statics for both reforms are equal and opposite sign if we are willing to assume $F_i(e) \approx F_i(y_i^*)$. This is a reasonable assumption when the amount to be repaid $(1+r_i)b \approx c$, that is, the amount to be repaid is close to the full cost of filing.

[19]There is suggestive evidence that the collective cost of filing for bankruptcy is high. White (1998), for instance, estimated that 15 percent of households could benefit from filing for bankruptcy at a time when just over one percent did. Similarly, Indarte (2018) finds evidence in support of relatively large "all-in" costs of filing for bankruptcy.

from filing for bankruptcy. We calibrate the first term using measures of r_i and p_i for each credit-score segment using the Consumer Financial Protection Bureau Consumer Credit Panel (CCP) and Mintel Comperemedia (Mintel) data on credit card offers. To capture a more accurate measure of borrowing costs, we additionally scale up the credit card interest rates based on the Nelson (2018) estimates of the difference between interest charges and fee-inclusive borrowing costs for the most closely corresponding credit score bin.[20]

To calibrate the second term in Equation 1, we calculate default rates for each credit-score segment using the CCP. Specifically, we calculate the probability that a newly opened credit card loan goes into 60-day default or is charged-off within 12 months after opening. To use the average default rates in our calibration, we make two simplifying assumptions. First, we assume that the 60-day default rate can be used as a reasonable proxy for the recovery rate. This may be a reasonable assumption if the expected recovery rate is small conditional on a 60-day default, and the recovery rate is very high if individuals have not reached 60-day default. Second, we assume that the *average* default rate within a credit-score segment corresponds to the default rate for the *marginal* individual deterred from bankruptcy by the reform. In our setting, this may be a reasonable assumption since we use relatively fine credit-score segments in our main analysis: 10-point credit score bins, with credit scores running from 500 to 840.[21] Table 1 presents the weighted average results of this exercise, split by prime and subprime.

As shown in Appendix Table A1, most of the pass-through estimates are fairly tightly bunched around the weighted-average pass-through estimate (though the very low credit-score segments have somewhat lower pass-through estimates between 0.55 and 0.80). The limited amount of heterogeneity in the calibrated pass-through estimates is interesting, and reflects a kind of "balancing out" of declining default rates, bankruptcy probability, and interest rates across the credit score distribution. The limited heterogeneity is also reassuring for our empirical analysis, since our re-

[20]Specifically, we estimate the mark-up due to fees by calculating the ratio between the fee inclusive charges and interest charges from Table 6 (Pre-CARD Act Price Distribution on New Accounts) in Nelson (2018). We use the credit-score-bin mark-ups from accounts with 6-11 months of cumulative borrowing. The mark-ups are similar to those implied by the survey respondents in Stango and Zinman (2009).

[21]On the other hand, the marginal individuals deterred from bankruptcy may remain persistently insolvent and enter "informal bankruptcy" (Dawsey and Ausubel, 2002). In this case, the actual repayment rates of marginal individuals will be lower, and our calibration will overstate the expected change in interest rates under perfect competition. Ultimately, this would turn our pass-through estimates into a lower-bound of the true pass-through rate.

Table 1. Benchmarking Interest Rate Pass-through

(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
						Pass-through Calibration	
Credit Score Segment	Population Share	Bankruptcy Rate	APR	APR w/ Fees	60-Day Default Rate	APR	APR w/ Fees
Subprime	26%	3.1%	13.4%	23.1%	31.5%	0.80	0.87
Prime	74%	0.3%	10.2%	12.8%	1.1%	1.09	1.12
All	100%	1.0%	11.0%	15.5%	9.1%	1.02	1.05

Notes: This table reports pass-through estimates split by prime and subprime consumers. See main text for details and Appendix Table A1 for analysis by 10-point credit-score segment. Subprime includes all individuals with credit scores less than or equal to 620. The APR column comes from Mintel data on credit card offers, and the bankruptcy and 60-day default rates (in both cases measured over the next 12 months) comes from the Consumer Credit Panel (CCP). The pass-through estimate comes from combining the estimates in columns according to equation (1), using the default rate to proxy for one minus the recovery rate, which is the first term in equation (1).

search design is based on comparing across credit-score segments with different values of dp_i and relating these changes in bankruptcy filing to changes in interest rates dr_i. As a result, we interpret calibration estimates as providing a plausible magnitude that we can use to benchmark our empirical results against and also providing evidence against substantial treatment-effect heterogeneity across credit-score segments. This means that the average effect that we estimate in our difference-in-difference regressions may be informative for a range of credit scores, not just those credit-score segments most affected by the bankruptcy reform.[22]

We conclude by highlighting two simplifying assumptions of the model to keep in mind when interpreting the empirical results. First, like most models of consumer bankruptcy in the literature, we assume that individuals make rational bankruptcy filing decisions with full information. In addition to appearing through the cost of filing, c, a relaxation of this assumption could allow borrowers to under-estimate the financial benefit of filing.[23] Second, the model assumes that the market for loans is perfectly competitive. With imperfect pass-through, the same effect of reform on filings will lead to smaller change in interest rates, since some of the incidence of the reform will reduce firm profits. As a result, pass-through results that are smaller in magnitude than our calibrated estimates could be interpreted as consistent with imperfect competition.

[22] However, given that even this simple model highlights potential for treatment effect heterogeneity, we also report results of two-way fixed effects models allowing for heterogeneous treatment effects in Appendix Table A8, using the two-way fixed effects estimator developed by de Chaisemartin and D'Haultfoeuille (2019).

[23] Finkelstein and Notowidigdo (2018) provide a framework which incorporates this type of misperception, which may be more useful for welfare analysis if behavioral biases are important.

4 Data

Our analysis relies on three main data sets: legal dockets for all consumer bankruptcies in 78 (of 94) United States bankruptcy courts; Mintel Comperemedia data on credit-card offers made to more than 2,000 consumers each month; and hospital-discharge records for over half a million individuals merged with a ten-year panel of their credit reports.

Data on consumer bankruptcy filings come from the Public Access to Court Electronic Records (PACER) system. This dataset includes more than three million filings from 78 bankruptcy courts during our sample period of 2004 through 2007, roughly 86 percent of all filings in the United States during that period. Throughout our analysis, we limit the sample to the years before 2008 in order to mitigate the effects of the Great Recession on our results. We validate the data for each district by comparing the filings in the PACER records with the official totals published by the Administrative Office of the United States Courts (AOUSC).[24] Appendix Table A2 details the sample coverage by chapter and quarter-year, and Appendix Section B.1 provides more details on the sample.

To study pass-through to credit-market pricing, we use Mintel Comperemedia (Mintel) data on credit-card offers.[25] Mintel collects credit card offers from a representative sample of households in the United States, who are paid to send all direct-mail credit-card offers they receive to Mintel.[26] The data includes demographic information on the households (age of head of household, household composition), details on the credit-card offers (type of credit, interest rates, fees), and some limited credit measures (importantly, these include the same credit score observed in the CCP). Data is collected monthly and includes approximately 350,000 credit card offers (7,000 per month) and 100,000 individual-month observations (2,200 per month). Appendix Table A4 provides summary statistics on offers and Appendix Section B.2 provides more details on the sample.

To study the insurance value of the bankruptcy option, we analyze administrative hospital-

[24] The sample does not include the universe of bankruptcies because 13 districts did not grant fee waivers, and we drop 3 districts from the sample because the bankruptcies in the data do not match the AOUSC statistics.

[25] We do not observe bankruptcy filings in the Mintel data, so we additionally use the Consumer Financial Protection Bureau Consumer Credit Panel (CCP) to estimate the bankruptcy filing risk for each credit-score segment, combining public-record snapshots with credit score archives to estimate prospective filing probabilities.

[26] The sample is representative of United States credit card holders. According to Fulford and Schuh (2015), roughly 75 percent of American households hold at least one credit card.

discharge records from the California Office of Statewide Health Planning and Development for the universe of uninsured hospitalizations (and approximately 20 percent of individuals hospitalized with insurance) between 2003 and 2007. Our sample links hospitalized individuals to their panel of credit reports spanning the years 2002 to 2011. To isolate unexpected hospitalizations, we restrict the sample to individuals ages 25 to 64 who are hospitalized for non-pregnancy-related reasons and have not previously been to the hospital in the last three years. Appendix Table A9 provides pre-hospitalization summary statistics and Appendix Section B.3 provides more details on the sample.

5 Effect of BAPCPA on Total Bankruptcy Filings

A first prediction of the model described in Section 3 is that higher costs of bankruptcy ought to lead to fewer bankruptcies. This section tests that prediction empirically, first by studying the effect of BAPCPA on total filings and then by studying the choice of chapter.

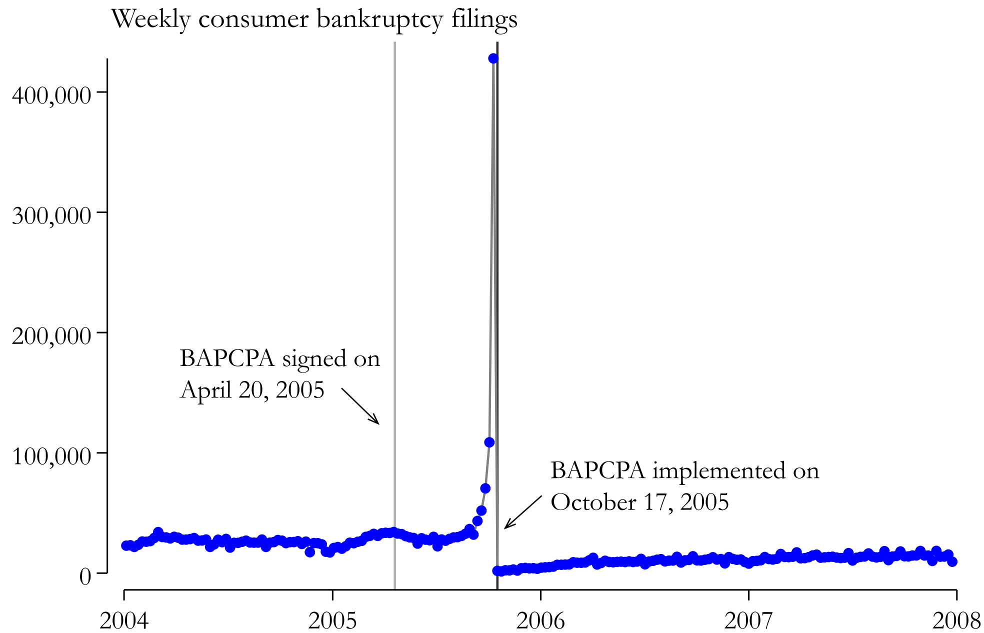
Figure 1 plots the total number of consumer bankruptcy filings in the PACER sample by week from January 2004 through December 2007. The most striking feature of Figure 1 is the dramatic rush-to-file after BAPCPA was signed but before the bankruptcy code was changed. In the five weeks before the law was implemented, from September 12$^{\text{th}}$ through October 16$^{\text{th}}$, the filing rate increased dramatically. In the final week before the implementation of the law, more than 400,000 households declared bankruptcy, roughly 13 times the typical weekly caseload.

To quantify the number of excess filings before implementation and to test whether, on net, the law led to a reduction in bankruptcies, we adapt "excess mass" methods from the tax-notch literature (e.g., Chetty et al., 2011; Kleven, 2016) to generate a counterfactual time-series in the absence of the changes to the bankruptcy code. We fit the following regression to the weekly filing count in the period before BAPCPA was passed by the Senate (March 10, 2005):

$$\text{Filings}_t = \gamma t + \tau_m + \varepsilon_t. \tag{2}$$

Here, Filings_t are nationwide filings in week t, t is a linear time trend, and τ_m are calendar-month fixed effects. Appendix Table A3 presents results when we additionally control for the national unemployment rate and to alternatively fit the counterfactual time-series through the passage of

Figure 1. Time-Series of Bankruptcy Filings



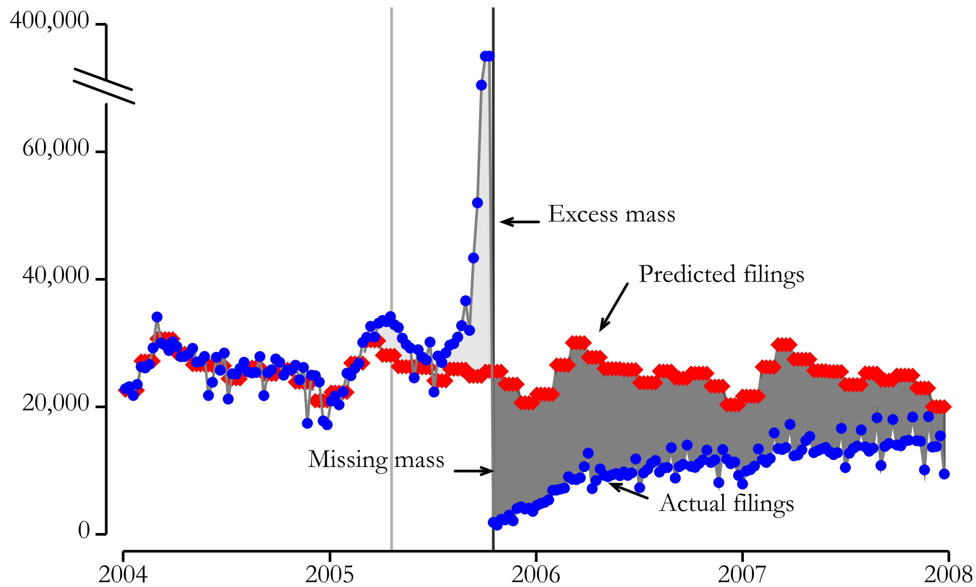
Notes: The sample consists of all consumer bankruptcy filings included in the PACER sample from January 2004 through December 2007. Each dot in the figure represents the total count of filings for that week.

the bill in the House (April 14, 2005) or its signing into law (April 20, 2005). The results are qualitatively similar across these alternative approaches.

We use Equation 2 to predict the counterfactual number of filings each week for the full sample period and calculate the sum of the difference between the predicted and actual filings for each week. Figure 2 presents this exercise by plotting the time-series of bankruptcy filings against the estimated counterfactual. As expected, the predicted filings closely match actual filings before the passage of the law. Actual filings diverge from the predicted time-series in September of 2005 in advance of the pre-BAPCPA filing deadline in mid-October. An excess of more than 750,000 households filed for bankruptcy between March 10, 2005 and October 17, 2005 relative to the counterfactual time-series. To calculate the net effect of BAPCPA on filings, we account for the filings which were intertemporally substituted before the implementation of the law.

Table 2 presents the difference between the predicted and the realized number of filings from the bill's passage in the Senate through the end of 2007. Column 1 presents the predicted number of filings for the 30-week period ending in the index date. Column 2 presents the difference between the realized and predicted filings for the same period. Column 3 presents the cumulative net difference

Figure 2. Excess and Missing Mass of Bankruptcy Filings



Notes: The sample consists of all consumer bankruptcy filings included in the PACER sample from January 2004 through December 2007. The total count of filings for each week is plotted against the predicted number of filings for the week. The predicted number of filings are the result of estimating equation 2 on the total count of filings from January 2004 through the day that BAPCPA was passed by the Senate (March 10, 2005). The two data points before implementation of BAPCPA are censored in this figure: there were 108,745 filings during the week that began on October 3, 2005 and 427,947 filings during the week that began on October 10, 2005.

in filings from the counterfactual time-series.

The more than 750,000 excess filings ahead of implementation suggest that debtors and their attorneys anticipated the changes to the bankruptcy code to be significant. Due to the mandated waiting period between filings, whose who file for bankruptcy must do so because the benefits of filing today must exceed the loss of the option to file at another point in the future.[27] For debtors who rushed to file before the new bankruptcy code went into effect, we can infer that the benefit from filing for bankruptcy under the previous system exceeded the option value of bankruptcy under the new system.

On net, the decline in filings after implementation exceeds the pre-implementation increase in filings by July of 2006. The temporary "rush-to-file" effect was quickly overwhelmed by the sustained reduction in filings under the new bankruptcy regime. As of the end of 2007, 114 weeks

[27]Before BAPCPA, filers needed to wait 6 years after a Chapter 7 bankruptcy before filing again. BAPCPA increased that waiting period to 8 years, a change that Appendix Figure A4 demonstrates was binding for the small share of filers who file more than once.

Table 2. Difference between Realized and Predicted Filings

		(1)	(2)	(3)
Weeks relative to implementation	Index Date	Predicted Filings	Realized Difference	Cumulative Net Difference
-30	March 21, 2005			
0	October 17, 2005	911,656	762,192	762,192
30	May 15, 2006	879,729	-656,283	105,909
60	December 11, 2006	857,796	-481,442	-375,533
90	July 9, 2007	889,823	-445,607	-821,140
114	December 24, 2007	659,619	-256,539	-1,077,679

Notes: This table presents a running sum of the net change in filings due to BAPCPA: the difference between actual bankruptcies observed each week and the number of bankruptcies that would have been predicted based on the counterfactual by estimating equation 2 from the beginning of the sample until BAPCPA was approved by the Senate in March of 2005. Index date for each row refers to the end of the 30 weeks period presented. The overall numbers are inflated to reflect the nation as a whole, based on our PACER sample coverage (see Appendix Table A2).

after the implementation of BAPCPA, 1,077,679 filings were deterred.[28] Appendix Figure A1 presents a similar exercise, but extending the pre-period back to 2002. This generates an estimate of -1,109,094 which is consistent with the estimates using the 2004 through 2007 period.

Bankruptcy reform clearly decreased the overall number of filings, but the introduction of the means test also sought to shift more filings from "fresh start" bankruptcies (Chapter 7) to repayment-plan bankruptcies (Chapter 13). Appendix Figure A2 plots the time-series for total filings separately for Chapter 7 and Chapter 13. The time-series patterns are not markedly different across the two chapters, as we might expect if the primary impact of the reform was a means-test-driven shift in the chapter of filings; however, the decline in filings is larger among Chapter 7 filings. The share of filings that were Chapter 13 remained persistently higher after the reform, as is clear from Appendix Figure A3. Appendix Table A3 estimates the net change in overall filings through 2007 and shows that both Chapter 7 and Chapter 13 filings declined substantially.

6 Effects of Bankruptcy Filing Risk on Interest Rates

This section tests the other main prediction from the model developed in Section 3: that BAPCPA reduced interest rates. We first estimate the change in filing risk for each credit-score segment. We

[28]To calculate a confidence interval for the estimated net change in filings, we implement the bootstrapping procedure described by Chetty et al. (2011). This leads to a 95-percent confidence interval with an upper bound of 1,125,242 and a lower bound of 1,034,709. Since we observe the full sample of bankruptcy filings, the standard errors reflect error due to misspecification of Equation (2) rather than sampling error.

then use those estimates to implement a difference-in-difference regression model that tests how offered interest rates change for each change in the risk of bankruptcy.

6.1 Pass-through to Interest Rates

In determining interest rates, creditors must predict the expected repayment rates on the credit offered. A key input for determining repayment rates is the probability a borrower will discharge their debt through bankruptcy. Filing for bankruptcy either reduces repayment to zero under Chapter 7 or restructures the amount to be repaid under Chapter 13. As suggested by the model in Section 3, reducing the generosity of the bankruptcy system ought to increase prospective repayment rates, and thus decrease the cost of lending. We expect this decrease in the cost of lending to be passed-through to borrowers in the form of lower interest rates. In this section, we test this hypothesis by estimating the pass-through to interest rates on credit-card offers.

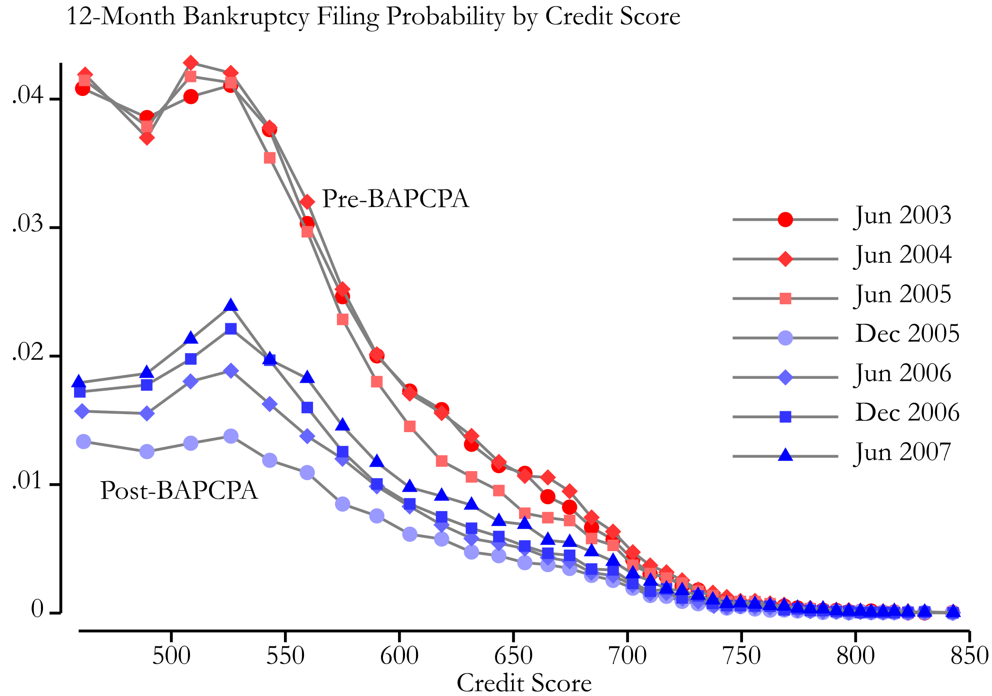
We focus on credit-card offers for three reasons. First, credit cards are the most common method of borrowing in the United States. Roughly 75 percent of Americans have at least one credit card (65 percent of whom carry a balance) and total revolving debt was over $800 billion for most of our sample period (Fulford and Schuh, 2015). Second, because credit-card debt is not collateralized, it is the most likely to be discharged in bankruptcy and thus the most responsive type of credit to changes in the bankruptcy code. Third, the Mintel dataset provides a clean measure of the cost of credit supplied to households in the credit card market, allowing us to overcome measurement challenges associated with other data sources.[29]

We are interested in identifying the change in borrowing costs (dr) for a given change in bankruptcy filing risk (dp). Our empirical approach to estimate pass-through is motivated by the observation that the bankruptcy risk of a potential borrower varies substantially by their credit score. This is evident in Figure 3, which plots the probability that borrowers across 40 equally sized credit-score segments file for bankruptcy over the next 12 months for each available CCP observation. Filing risk conditional on credit score bin decreased significantly after the new bankruptcy code was implemented.[30]

[29] For instance, credit bureau data do not include prices. Other datasets, such as the National Mortgage Database, include information on prices only conditional on loan take-up.

[30] This was not driven by changes in the credit scoring formula across the sample period. We use the same credit

Figure 3. Probability of Bankruptcy



Notes: The sample consists of individuals in the CFPB CCP. This figure presents a binned scatter plot of the share of individuals who file for bankruptcy within the next 12 months. Each point represents the 12-month prospective filing rate for one of 40 equally sized credit-score segments from the time of the credit report observation.

To parameterize the change in the probability of filing for bankruptcy, we define δ_b as the difference between the post-BAPCPA filing probability and the pre-BAPCPA filing probability for each 10-point credit-score segment. This provides us with a continuous treatment variable which we use to estimate pass-through as a function of changes in bankruptcy filing risk. In contrast to the exercise in Section 5, which accounts for individuals who rushed to file for bankruptcy before the law went into effect, here we focus on changes to *prospective* bankruptcy filing risk. We focus on prospective filing risk because that is the relevant input into lenders' pricing decisions. Those who rushed to file before BAPCPA went into effect likely increased the decline in prospective bankruptcy filing risk, because they are not allowed to file for bankruptcy again for 8 years after BAPCPA.[31]

We estimate the change in the *prospective* bankruptcy filing risk, δ_b, by comparing the 12-month

score throughout the period and there were no major changes to the standard, commercially available credit-scoring formulas over this period.

[31]BAPCPA implemented a longer waiting period between filings, increasing the minimum time between Chapter 7 filings from six to eight years. Appendix Figure A4 demonstrates that this was binding for a small share of repeat filers.

bankruptcy filing rates for each credit-score segment before and after the reform. In particular, we take the average 12-month filing rate for each available quarter of the CCP before and after the reform was implemented, and define δ_b as the change in filing risk.[32] Appendix Table A5 presents the estimates of δ_b. We use the change in bankruptcy filing probability, δ_b, as our parameterization of the change in bankruptcy filing probability we describe in Section 3 (i.e., $\frac{dp}{dc}$). This allows us to estimate the response of interest rates with respect to bankruptcy filing rates, which maps to the comparative static of interest (i.e., $\frac{dr}{dp}$) we derived.[33]

Our empirical strategy involves a comparison of different credit-score segments. We compare the changes in the interest rates offered to segments that experienced large declines in bankruptcy filing risk to the interest rates offered to segments that experienced little change in bankruptcy filing risk. That comparison isolates the object of interest ($\frac{dr}{dp}$) as long as the change in bankruptcy risk across credit-score segments is not correlated with other time-varying determinants of interest rates. That assumption would be violated if, for instance, the credit-scoring formula itself were changing during this time period. Reassuringly, however, the credit-scoring formula was unchanged during this time period and the distribution of credit scores was quite stable (Appendix Figure A5).

Another key identification assumption is the standard parallel-trends assumption. That is, in the absence of BAPCPA, the pre-period differences in offered interest rates across segments would have evolved along parallel trends. That assumption would be violated if, for example, credit-score segments that experienced larger declines in bankruptcy filing probability also experienced larger changes in interest rate offers for reasons beyond the factors we can include as controls. For instance, a concurrent expansion in the subprime mortgage market during this time period may have affected the creditworthiness of subprime borrowers, and that portion of the market may have been evolving along a different path. We address this concern by showing that the estimates below are robust to the inclusion of subprime- and prime-specific time trends.

[32] We use the prospective 12-month filing rate for each of the time periods plotted in Figure 3. To obtain the difference in the average between the pre-period observations (observed quarterly from September 2003 to September 2005, missing December 2003) and the post-period observations (observed quarterly from December 2005 to December 2007), we regress 12-month bankruptcy filing risk by credit-score segment and quarter on indicator variables for each 10-point credit-score segment both on their own and interacted with an indicator variable for whether the observation occurred in the post-period.

[33] One could also regress Equation 4 using only the pre-reform bankruptcy filing risk. Appendix Table A6 presents additional regressions that use only the pre-BAPCPA risk of filing for bankruptcy in lieu of the difference. The results are consistent with estimates using the change in filing probability.

The Mintel data consists of a repeated cross-section and the level of observation is a credit-card offer. In our main specifications, we include lender-specific fixed effects (ν_j) to absorb differences across lenders, credit-score-segment fixed effects to absorb time-invariant differences in prices across credit-score segments (ϕ_b), and fixed effects for card category and application type (e.g., pre-approved or invitation to apply) (χ_i). The estimating equation for the event study is:

$$y_{it} = \beta_0 \delta_b + \sum_{t=2004m1}^{2007m12} \lambda_t (\delta_b \times \tau_t) + \nu_j + \phi_b + \chi_i + \varepsilon_{it}. \tag{3}$$

The dependent variable y_{it} is the interest rate of offer i in month-year t. The variable δ_b is the "treatment" (the difference in the propensity to file before and after passage of BAPCPA for credit-score segment b) and τ_t, ν_j, ϕ_b, and χ_i are fixed effects for each month and year combination, lender, credit-score segment, and other offer features respectively.[34] We two-way cluster standard errors by credit-score segment and lender (Cameron et al., 2011).

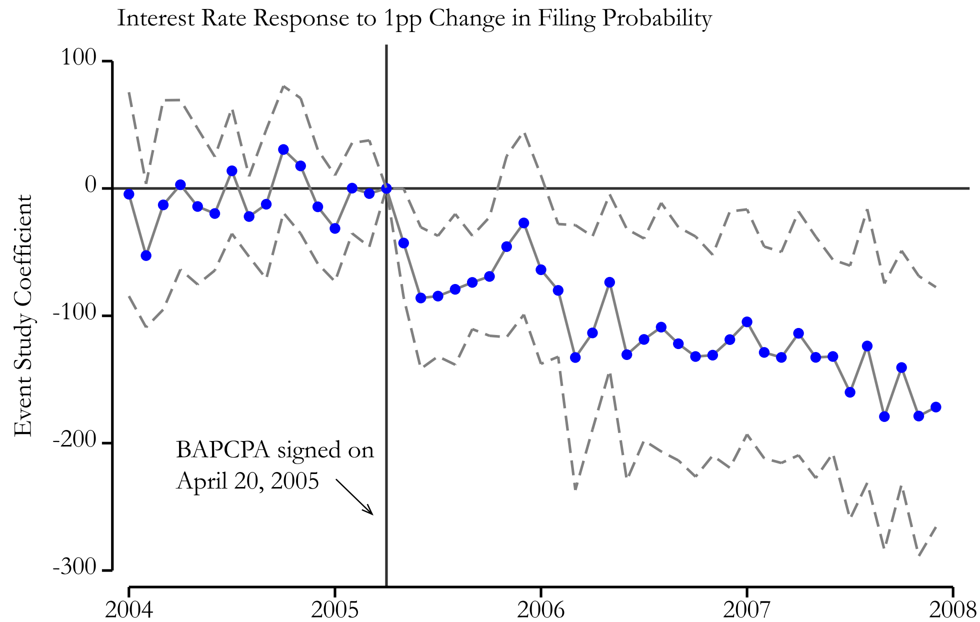
Our primary outcome is the regular annual percentage rate (APR). Appendix Figure A6 presents the time-series of the regular APR, split by prime and subprime and adjusted for the prime rate. We weight all credit-card offers using Mintel-provided weights designed to make the sample representative of the overall mail volume of each campaign. This allows us to estimate effects that are representative of the credit card market as a whole.[35] In the appendix, we also examine the adjusted APR, following Gross et al. (2016) in adjusting for whether the credit-card offer has an introductory "teaser" rate by taking a weighted average of the introductory interest rate and the regular interest rate over the first 12 months after origination.

Figure 4 plots the coefficients of interest, λ_t for each month t, with the regular APR as the dependent variable in Equation (3). By allowing the λ_t's to evolve flexibly over time, this regression allows us to assess the assumption that interest rate offers were evolving along parallel trends before the passage of BAPCPA. Further, by refraining from imposing any ex-ante restrictions on when interest rates should change, we can use the time pattern to gauge whether any changes in interest rates appear after the passage of BAPCPA but before the law's implementation.

[34] The other features of the credit-card offers are: card category, application type, and state of residence.

[35] Mail volume represents the effective weight on each mail piece. For instance, the sum of all mail volume weights in the full Mintel data for *Chase Freedom* cards in a given month should equal the total mailings for *Chase Freedom* in that month, nationwide. In practice, the weights do not meaningfully affect the coefficients.

Figure 4. Effect of Decline in Filing Probability on Offered Interest Rates



Notes: The sample consists of credit card offers made between January 2004 and December 2007 included in the Mintel data. The points represent estimates of the λ_t's in equation 3. The dashed lines provide 95-percent confidence intervals for each point. The dependent variable is the rate spread for regular offered interest rate.

In equilibrium, we would expect any changes to the bankruptcy code and interest rates to also affect borrowing behavior. We expect offered interest rates to respond immediately to anticipated changes in bankruptcy filing probability, while any secondary effect on borrowing would take longer. Therefore, we are particularly interested in whether there exists a break in the evolution of interest rate offers when BAPCPA was signed into law. We focus on the timing of passage (rather than implementation) here because the bankruptcy code considers debts incurred in the months just before filing as non-dischargeable; therefore, creditors could safely assume new lines of credit opened between passage and implementation would be unlikely to be discharged before the new bankruptcy code took effect.

Figure 4 suggests a decline in offered interest rates ahead of BAPCPA's implementation. While interest rates evolved similarly for the credit-score segments affected and unaffected by the reform throughout the pre-period, we observe a sharp drop in the λ_t's following the passage of BAPCPA in April 2005. The decline in interest rates among the credit-score segments who experienced a decline in the probability of filing for bankruptcy is stark and persistent—the interest rate spread drops immediately upon passage of BAPCPA and remains below the pre-period level through the post-period.

Motivated by the pattern in Figure 4, we estimate a difference-in-difference regression to quantify $\frac{dr}{dp}$. Specifically, we estimate the change in offered interest rates for a one-percentage-point decline in the 12-month probability of filing for bankruptcy:

$$y_{it} = \beta_0 \delta_b + \beta_1 \delta_b \times \text{post} + \tau_t + \nu_j + \phi_b + \chi_i + \varepsilon_{it}, \tag{4}$$

where, again, δ_b is the difference in the probability of filing before and after the passage of BAPCPA and ν_j, ϕ_b, and χ_i are indicators for lender, credit-score segment, and offer features. In lieu of the month-year indicators interacted with δ_b, we simply interact δ_b with a "post" indicator for the offer coming after the BAPCPA was signed into law. Offers are weighted by mail volume and standard errors are two-way clustered by credit-score segment and lender.

Table 3. Pass-through of Change in Bankruptcy Filing Probability to Interest Rates

	(1)	(2)	(3)	(4)	(5)
Dependent variable:		Regular Interest Rate			
Post-BAPCPA $\times \delta_b$	-101.3***	-60.4**	-59.1**	-66.7**	-66.6**
	(35.7)	(27.2)	(24.6)	(29.3)	(29.0)
R^2	0.432	0.433	0.524	0.551	0.552
Lender FEs	✓	✓	✓	✓	✓
Subprime $\times t$		✓	✓	✓	✓
Card Category			✓	✓	✓
Application Type				✓	✓
State FE					✓
N	391,153	391,153	390,975	390,975	390,975

Notes: The sample consists of credit card offers made to households from January 2004 through December 2007. All columns report effects based on OLS estimates of equation 4 and include month-year and credit-score-bin fixed effects in addition to the additional controls listed. The outcome variable is the interest rate on credit card offers. Standard errors (two-way clustered by credit-score segment and lender) are in parentheses. Offers are weighted by the mail volume of the campaign. Asterisks indicate significance at the 1 percent (***), 5 percent (**), and 10 percent (*) level, respectively.

Table 3 presents the coefficients of interest, β_1, from Equation 4. The baseline specification in column 3 includes month-by-year, credit-score segment, and lender fixed effects. In order to control for other factors which may have changed differentially by credit-score segment and correlated with δ_b, such as the expansion of the subprime market, we include prime- and subprime-specific time trends starting in column 4. The main coefficient declines by roughly forty percent with the inclusion of time trends. That change in the coefficient is potentially attributable to the expansion of the subprime credit market during our time period but is also partially mechanical given the trends soak up some of our identifying variation. After accounting for prime- and subprime-specific time trends, however, the estimates are very stable through the incremental inclusion of card category, application type, and state-specific fixed effects.

While there is limited evidence that the cost of funds is passed through to interest rates (e.g., Ausubel, 1991; Calem and Mester, 1995; Stavins, 1996; Stango, 2000; Calem et al., 2006; Agarwal et al., 2017), monetary policy changes have been shown to differentially affect different types of households (Coibion et al., 2017; Alexandrov et al., 2018). BAPCPA was passed in the middle of a protracted rise in the Federal Funds Rate (FFR). Even though the reform did not coincide with a sudden change in the FFR, one might be concerned that changes in the FFR affected some credit-score segments more than others. To address that possibility, we control for an interaction between the FFR and an indicator for subprime status. Appendix Table A7 presents estimates of that regression. Reassuringly, the results are consistent with those presented by Table 3.

Our preferred estimate of the relationship between interest rates and bankruptcy-filing risk comes from the most-demanding specification in Table 3: a 67-basis-point decline in the interest rate for each 1-percentage-point decline in the 12-month prospective bankruptcy filing risk. The subprime market-wide reduction in offered interest rates is approximately 84 basis points (given an average decline in the bankruptcy filing rate of 1.25 percentage points), which narrows the spread on offered interest rates between prime and subprime borrowers by about 10%.

We can use the pass-through expression derived in Section 3 to assess the magnitude of these estimates. The perfect-competition benchmark for $\frac{dr/dc}{dp/dc}$, above, is 105 basis points for each one-percentage-point change in filing risk. The estimates of Equation (4) are of similar magnitude to this benchmark. The estimate in column 4 of Table 3 represents pass-through of approximately 64 percent of the perfect-competition benchmark. While this exercise is not a conclusive statement on the competitiveness of credit card markets, it does suggest that our estimates of pass-through are of reasonable magnitude and consistent with some competitive pricing pressure as well as imperfect competition.

We document a decrease in the offered APR consumers face, but this could be offset by changes to other credit card features. Ru and Schoar (2016) argue that when unsophisticated borrowers are better credit risks, lenders rely on back-loaded features such as late fees and teaser rates. Appendix Table A6 presents the same regressions presented by Table 3 but with late fees and interest rates adjusted for the teaser rate as the outcome. We take a weighted average of the introductory (teaser) interest rate and the regular interest rate over the first 12 months after origination following Gross et al. (2016). We estimate a fairly precise zero change in late fees in response to a change in bankruptcy filing risk: the 95-percent confidence interval rules out an increase in the late fee of more than \$1.72.[36] The estimated effect for adjusted interest rates is smaller than the effect for the regular interest rate due to the mechanically lower weight on the interest rate when accounting for teaser rates, but otherwise consistent with the results of Table 3.

These decreases in offered interest rates benefit consumers to the degree that they take advantage of the lower rates. Consumers may not be especially strategic in doing so. Stango and Zinman (2015) and Woodward and Hall (2012) document substantial dispersion in borrowing costs that appear to be driven by consumers partaking in too little price-shopping. Similarly, Keys and Wang (2019) and Gathergood et al. (2017) document that consumers tend to repay their credit card debt based on anchoring and heuristics rather than minimizing borrowing costs. Nevertheless, as long as consumers borrow using unsecured credit, the decrease in interest rates as a result of BAPCPA are likely to have provided them with considerable savings.

6.2 Heterogeneity Analysis

A recent literature emphasizes potential sources of bias in panel event-study designs when the treatment is staggered or potentially endogenous (Abraham and Sun, 2018; Athey and Imbens, 2018; Borusyak and Jaravel, 2017; Callaway and Sant'Anna, 2018; Freyaldenhoven et al., Forthcoming; Goodman-Bacon, 2018), but in our setting we have a single, plausibly exogenous policy change,

[36] Appendix Table A4 shows that the mean late fee among prime consumers of \$36.05 is actually slightly higher than the late fee for subprime consumers, \$34.73.

so endogenous timing is less of a concern. A second strain of this literature emphasizes that heterogeneous treatment effects in a difference-in-difference design can generate negative weights for some groups (de Chaisemartin and D'Haultfoeuille, 2019). That possibility presents a more-direct potential threat to our identification strategy. de Chaisemartin and D'Haultfoeuille (2019) show that two-way-fixed-effects models produce average treatment effects from a weighted sum of the treatment effect for each group and time period. In the presence of heterogeneous treatment effects, such models can produce negative weights even when treatment effects are positive for every group and time period. The model in Section 3 suggests the possibility of treatment-effect heterogeneity, and so Appendix Table A8 addresses this by re-estimating our main specification using a recently developed two-way-fixed-effects estimator that allows for heterogeneous treatment effects (de Chaisemartin and D'Haultfoeuille, 2019). Reassuringly, we find similar results using this alternative estimator, and we also find no negative weights going into the average treatment effect.[37] This is consistent with the calibrations in Section 2, which show fairly tight bunching around the weighted-average pass-through estimate across credit-score segments.

We perform one additional heterogeneity analysis by examining how the response to the reform varies by lender. Figure 4 and Table 3 demonstrate that lenders lower the interest rates on new credit card offers in anticipation of a decline in the generosity of bankruptcy. Examining heterogeneity in the responses of lenders can serve to both clarify the mechanism behind those lower interest rates and reveal some information about the structure of the subprime credit card market during our sample period. The Mintel data include the name of the lender that issued each offer, which we partial out in regression Equation (4). To understand how the responses to the change in bankruptcy law varied by lender, we run Equation (4) separately for each lender.

Appendix Figure A7 plots the lender-specific estimates on the vertical axis against the share of the firm's offers that are made to subprime consumers on the horizontal axis. The size of each circular marker corresponds to the number of credit card offers extended to subprime consumers over our sample period. The pass-through estimates are quite heterogeneous by lender and the effects we estimate in Table 3 are largely driven by the two prominent subprime lenders: Capital One and HSBC. These two lenders are well-known to be leaders in the expansion of credit to

[37] See the notes for Appendix Table A8 for additional discussion of this exercise.

subprime consumers. HSBC was additionally on the frontier of expanding credit to those with recent bankruptcy filings, which suggests a familiarity with the relationship between the bankruptcy system and repayment behavior (Jurgens and Wu, 2007). These results are also consistent with the high levels of heterogeneity in risk-based pricing documented by Stango and Zinman (2015), who show that variation in internal modeling across lenders results in substantial price dispersion based on differential treatment of identical customer characteristics.

7 Effects of BAPCPA on Targeting and the Insurance Value of Bankruptcy

The previous section documents how a decrease in bankruptcy filings passed-through to lower interest rates. This benefit to borrowers came at the cost of worsening the value of the bankruptcy option. How we weigh these impacts depends on *which* potential bankruptcy filers were deterred. Bankruptcy reform had the explicit goal of deterring "abusive" filings from higher-income filers who could otherwise repay their debts. In this section, we evaluate whether the means test was successful at shifting the distribution of filers away from lower-insurance-value bankruptcy filings, and estimate the effect of the reform on the likelihood that a negative financial shock is insured by bankruptcy.

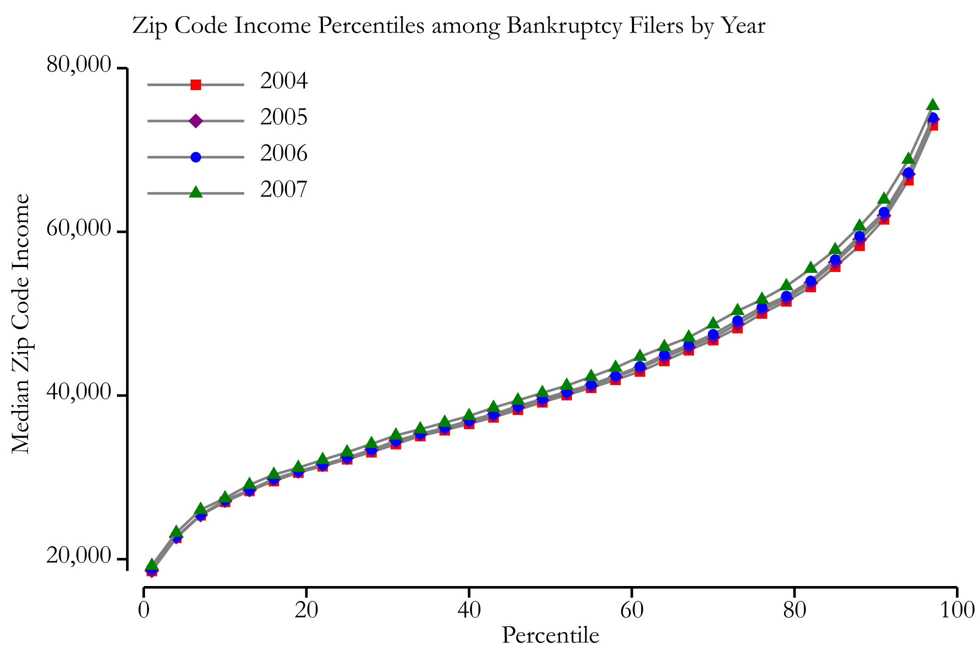
7.1 Characteristics of Filers

A key goal of bankruptcy reform was to deter high-income filers from accessing bankruptcy relief "opportunistically;" lawmakers referenced the income-based means test as the "heart of the bill" House Report (2005). By excluding households with income above the state median from the option to liquidate their debts, the law intended to target the bankruptcy code's most-generous provisions to lower-income filers. If the means test was an important force altering the composition of filers, we would expect to see the income of the average filer decrease.

While much attention has been given to the means test, the reform's collective impact on the composition of filers is ambiguous *a priori*. BAPCPA also made a number of additional changes to the bankruptcy filing process which collectively increased both the hassle costs of filing (through mandated credit counseling and financial management courses) and the liquidity requirements to file (through increased filing and attorney fees). The increased liquidity requirements to file, especially, may have deterred lower-income filers. The effect of hassle costs on the composition of filers is ambiguous, and depends on both filers' opportunity cost of time and their ability to navigate the requirements or to pay an attorney to help them do so.

The overall impact of the reform on the income of filers depends on the relative deterrent effects of these provisions across the income distribution. A cursory reading of the decline in filings, and the decline in Chapter 7 filings in particular, suggests the means test may have been effective in achieving its stated goals of deterring higher-income filers. This interpretation is belied by a closer examination of the composition of filers. We examine how the income of filers evolved through the reform by merging the filer's ZIP Code in the PACER sample with the median income for that ZIP Code from the 2000 Decennial Census.

Figure 5. ZIP Code Income Distribution of Bankruptcy Filers



Notes: The sample consists of all consumer bankruptcy filings included in the PACER sample from January 2004 through December 2007, matched with the ZIP Code median household income measured in the 2000 decennial census. The figure plots the percentiles of ZIP Code median household income among filers for each year of 2004 through 2007.

Figure 5 plots the full distribution of median ZIP Code income among filers for each year from 2004 through 2007. The distributions are strikingly similar—percentiles are virtually on top of each other through the 60^{th} percentile, at which point the post-BAPCPA distribution of filers drifts slightly upward. As is clear from the figure, there is no stark change in the composition of filers.[38] This suggests that the reform did not change the composition of filers by very much in terms of income (at least with respect to their ZIP Code), and, if anything, average income appears to creep upward after the reform. This surprising null effect ought to be explored by future research that examines how BAPCPA changed households' bankruptcy decision rules.[39]

Inspecting differences in the composition of filers before and after the reform can reveal how the self-targeting properties of the bankruptcy system changed. However, to measure changes in the insurance value of bankruptcy, we need to test how individuals facing the same negative financial shocks were able to access bankruptcy before and after BAPCPA. The next section takes such an approach.

7.2 Effect of BAPCPA on the Insurance Value of Bankruptcy

The results above demonstrate that BAPCPA deterred filings. To evaluate the cost of these deterred filings, we next ask how expense shocks (specifically, health shocks requiring hospitalization) were insured by bankruptcy before and after the reform. In the debate over bankruptcy reform, many expressed concern over how reform might affect the insurance value of bankruptcy. Warren (2005) argued against BAPCPA because the means test would "treat all families alike... A person who had a heart attack is treated the same as someone who had a spending spree at the mall."

This distinction between bankruptcies driven by medical costs and bankruptcies driven by discretionary consumption is present in life-cycle models of the bankruptcy decision.[40] Livshits et al. (2007) demonstrate that the existence of expense shocks, such as medical costs, can make "fresh start" (Chapter 7) bankruptcy regimes welfare-increasing despite increasing the cost of borrowing. Particularly when markets are incomplete, bankruptcy may be the only mechanism by which an individual can insure some negative events. We thus seek to estimate whether *specific expense shocks* were insured by bankruptcy, before and after the reform. We test the likelihood that

[38] Of course, income also varies within ZIP Codes and it is possible that there were large within-ZIP-Code changes in the incomes of filers. Nevertheless, we view it as unlikely that the ZIP Code income measure is masking large means-test-driven shifts in the income distribution of filers. This is consistent with anecdotal reports from bankruptcy attorneys (Littwin, 2016) and other evaluations of the reform (Ashcraft et al., 2007; Albanesi and Nosal, 2018).

[39] Several papers have used the means test as variation with which to study the effects of BAPCPA and bankruptcy more generally. See, for example, the work of Chatterjee et al. (2007), Li et al. (2011), Mahoney (2015), Mitman (2016), and Parra (2018)

[40] Divorce, job loss, and unplanned pregnancies are additional shocks that are discussed as relevant to the welfare implications of the bankruptcy code (Livshits et al., 2007; Fay et al., 2002; Keys, 2018), though medical expenses are often pointed to as the most "blameless."

individuals experiencing hospitalization shocks, before and after changes to the bankruptcy code, declare bankruptcy to obtain debt relief.

We study the universe of uninsured hospitalizations between 2003 and 2007 in California, where approximately 20 percent of residents lacked insurance during that time (California Healthcare Foundation, 2010). For comparison, we study insured hospitalizations in parallel. The insured include adults ages 25–64 hospitalized with either private or Medicaid coverage. Each hospitalization is linked to the patient's credit reports, which we observe at the start of each year from 2002 through 2011. We limit the sample to those who were not hospitalized in the three years prior to their hospitalization to isolate "health shocks."[41]

In the ideal experiment, we would randomly assign different bankruptcy regimes to otherwise-identical individuals experiencing a health shock. One might be concerned that the composition of uninsured hospitalizations might be different before and after the reform.[42,43] By isolating health shocks for individuals who have not been hospitalized in the previous three years, we come close to approximating this experiment. Hospitalizations are much less likely to be anticipated than other sources of health insurance demand like chronic conditions requiring outpatient care. To further address this concern, we re-weight the two sets of hospitalizations on their observable characteristics, though it has little effect on our estimates. We use propensity score matching to reweight those hospitalized in each period in order to match them on age, sex, race, zip code household income, whether the hospitalization was for a chronic condition, and on the major diagnostic category. Appendix Table A9 presents summary statistics by insurance status and hospitalization period.

Following Dobkin et al. (2018a), we estimate event-study regressions, additionally splitting the sample by whether the hospitalization occurred under the pre- or post-BAPCPA bankruptcy

[41] The precipitating event will be a hospitalization, but estimated effects of the hospitalization will include all sequelae (subsequent hospitalizations, poor health, lost earnings).

[42] As observed by Mahoney (2015), changes in the bankruptcy code also changes the incentives for individuals to purchase health insurance. This generates a concern that uninsured health shocks would be differentially selected before and after BAPCPA. Two patterns in the data ameliorate these concerns. First, the means test does not appear to be the primary driver of the change in bankruptcy filings. Second, at least in a coarse examination of the data, the share of Californians without health insurance was broadly unchanged over our sample period (California Healthcare Foundation, 2010). The estimated share of individuals lacking health insurance in California in each year from 2003 through 2007 was 19.1 percent, 19.5 percent, 19.9 percent, 19.6 percent, and 19.4 percent.

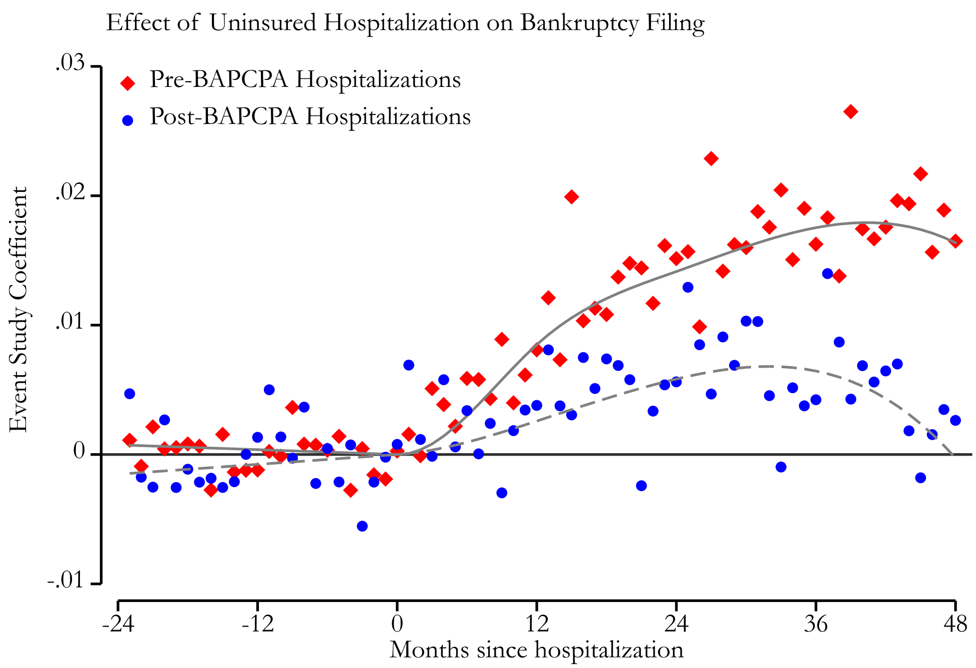
[43] A related concern is that the financial consequences of a hospitalization may vary over time. California implemented the "Hospital Fair Pricing Act" in 2007 which required hospitals to offer discounts or charity care to individuals making less than 350 percent of the Federal Poverty Line. Appendix Figure A11 plots the implied effects by year of hospitalization, and shows the results within the post-period do not look dramatically different between 2006 and 2007 hospitalizations.

regime. We define the pre-BAPCPA period to be January 2003 through December 2004 and the post-BAPCPA period to be from October 2005 through December 2007.[44]

We define event time m as the number of months relative to the hospitalization, which occurs at $m = 0$. Omitting the month prior to the hospitalization ($m = -1$) and including calendar-year-specific fixed effects, we specify a non-parametric event-study regression to estimate the evolution of the outcome variable preceding and following the hospitalization:

$$y_{it} = \gamma_t + \mathbb{1}\{\text{Pre-BAPCPA}\}\left(\sum_{m=-24}^{-2} \mu_m + \sum_{m=0}^{48} \mu_m\right) + \varepsilon_{it}. \tag{5}$$

Figure 6. Effect of Hospitalization on Bankruptcy Filing



Notes: The sample consists of individuals aged 25–64 who are hospitalized without insurance in California, additionally split by the timing of the hospitalization (January 2003 through December 2004 for the pre-BAPCPA sample, October 2005 through December 2007 for the post-BAPCPA sample). The points represent the estimated effects of event time (i.e., the μ_r's from the non-parametric event study in equation 5) and the lines represent the parametric event study in equation 6 with the pre-trends normalized between the two periods for ease of visual comparison.

In order to estimate how frequently a hospitalization leads to bankruptcy, we also estimate a parametric event-study specification. This allows us to calculate the "implied effect" at each

[44] We exclude hospitalizations occurring between January 2005 and September 2005 to avoid those most likely to coincide with the rush-to-file in October 2005.

month relative to hospitalization. We allow for a linear pretrend in event time m (months relative to admission) and a flexible cubic spline with breaks at 0, 12, and 24 months in the post-period. These allow us to estimate the effect of the hospitalization at any point, separately by the hospitalization period:

$$y_{it} = \gamma_t + \mathbb{1}\{\text{Pre-BAPCPA}\}\left(\beta_{0q}m + \beta_{1q}m^2\{m > 0\} + \sum_{s=0}^{2}\beta_{(s+2)q}(m-12s)^3\{m > 12s\}\right) + \varepsilon_{it}. \tag{6}$$

Figure 6 suggests that the parametric spline fits the non-parametric event-study coefficients well. The identifying assumption requires that, separately for pre-BAPCPA and post-BAPCPA hospitalizations, conditional on having a hospital admission and jointly estimated calendar-year fixed effects, the timing of the admission is uncorrelated with deviations of the outcome from a linear trend in event time.

Table 4. Implied Effects of Hospitalization on Bankruptcy

	(1)	(2)	(3)	(4)
Insurance Coverage:	Uninsured		Insured	
Hospitalization Period:	Pre	Post	Pre	Post
Implied Effect at 12 Months[a]	0.89	0.18	0.19	0.15
	(.12)	(.08)	(.08)	(.05)
Implied Effect at 24 Months[b]	1.49	0.43	0.37	0.25
	(0.23)	(0.15)	(0.15)	(0.10)
Pre-Hospitalization Mean	2.11	4.94	2.07	4.38
p-value for Null of 12 Month Pre/Post Equality	[<0.001]		[0.84]	
N	53,611	62,912	164,207	145,502

Notes: The sample consists of individuals aged 25–64 who are hospitalized in California, additionally split by the timing of the hospitalization (January 2003 through December 2004 for the pre-BAPCPA sample, October 2005 through December 2007 for the post-BAPCPA sample) and insurance coverage (uninsured or insured which includes those with private insurance or Medicaid coverage). All columns report effects based on OLS estimates of equation 6. The outcome variable is whether an individual has filed for bankruptcy since the beginning of the sample (January 2002). Standard errors (clustered on the individual) are in parentheses. The universe of qualifying uninsured hospitalizations are included in the sample; estimates for the insured are weighted to adjust for individuals' sampling probabilities. All implied effects are significant with p-values less than or equal to .015.

[a] The implied effect at 12 months is calculated from equation 6 as $144 \times \beta_2 + 1,728 \times \beta_4$

[b] The implied effect at 24 months is calculated from equation 6 as $576 \times \beta_2 + 13,824 \times \beta_4$

Figure 6 presents the results of both event studies for the probability an uninsured hospitalization resulted in a bankruptcy filing. The red-diamond markers trace the path of individuals hospitalized in the pre-BAPCPA environment, while the blue-circle markers trace the path of those hospitalized in the post-BAPCPA environment. The pre-BAPCPA hospitalizations result in a pronounced spike in bankruptcy filings following hospitalization, increasing starkly around the time debt is typically sent to collections (around 180 days after the hospitalization). The rate of filings remains persistently higher for the subsequent four years. By comparison, those hospitalized after changes to the bankruptcy code were implemented display a muted filing response to the hospitalization.

Table 4 provides estimates of the "implied effect" of the hospitalization at 12 and 24 months, separately by bankruptcy regime and insurance coverage. The implied effect is the deviation of the parametric coefficients from the linear pretrend, which we interpret as the impact of the hospitalization on whether or not the individual has filed for bankruptcy.

After the reform, uninsured hospitalizations were much less likely to be discharged through bankruptcy. At 24 months post-hospitalization, the pre-BAPCPA uninsured are 1.49 percentage points more likely to file for bankruptcy due to the hospitalization. After implementation, the implied effect of the hospitalization on filing for bankruptcy is just 0.43. While the share of individuals eligible to file for bankruptcy is smaller post-BAPCPA, this is a mechanical result of the construction of a stock variable for ever filing for bankruptcy over the sample period. As further reassurance on this point, the insured demonstrate a similar increase in the share of individuals who have filed for bankruptcy in advance of their hospitalization (4.38 percent from 2.07 percent versus 4.94 percent from 2.11 percent for the uninsured), but a substantially smaller decline after the reform. The marked decline in the implied effect of a hospitalization on filing for bankruptcy indicates that bankruptcy reform significantly reduced the share of uninsured individuals who access bankruptcy as implicit health insurance.

This effect does not appear to be driven by differences in medical debt. Uninsured hospitalizations result in a similar amount of debt sent to collections under both bankruptcy regimes, but 70 percent fewer bankruptcy filings after the reform. Appendix Table A10 shows the implied effect on debt sent to collections 24 months after the hospitalization increased from $6,700 to $6,900 after the reform. While hospitalizations in and of themselves may make up a small share of overall bankruptcy filings (Dobkin et al., 2018b), to the degree that uninsured health shocks can be generalized to other types of uninsured shocks, these results suggest that the reform meaningfully reduced the insurance value of bankruptcy.

8 Conclusion

The option to file for bankruptcy provides a form of insurance for American households by providing a process for them to discharge their debts. However, by limiting the ability of borrowers to commit to repayment, the option of bankruptcy increases the cost of borrowing, and hence the cost consumers face to smooth their consumption over time. This paper uses changes to the bankruptcy code to evaluate this trade-off empirically.

We find that the 2005 bankruptcy reform induced a net reduction of more than one million bankruptcy filings in the two years after implementation. We demonstrate that this reduction in the risk of bankruptcy filing was passed-through to consumers in the form of lower borrowing costs. The results suggest that a 1-percentage-point reduction in bankruptcy filing risk leads to a 66 basis-point decline in offered credit-card interest rates. Using our model to calibrate a perfectly competitive benchmark, these results imply a pass-through rate of roughly 60 percent. The incomplete pass-through that we find lines up with recent results using other shocks to the consumer credit market (such as Agarwal et al. 2014), and is consistent with some degree of imperfect competition.[45]

Policymakers intended the law's means test to deter higher-income filers, but we find that the income distribution of filers remained essentially unchanged in the wake of the reform. In addition, we find that those hospitalized without health insurance were less likely to declare bankruptcy after the reform. This suggests that BAPCPA decreased the insurance value of bankruptcy.

Collectively, the findings emphasize the trade-offs in determining the optimal generosity of

[45]The pass-through results are somewhat larger in magnitude than the estimates reported by Agarwal et al. (2014), but our focus is on interest rates instead of credit limits. A full accounting for the difference in pass-through estimates is outside the scope of this paper, but we speculate that the salience and persistence of the shock that we are studying may account for some of the difference between the estimates. In particular, the BAPCPA reform was widely publicized and well-understood; the huge "rush-to-file" by consumers that we document strongly suggests that other market participants (such as creditors) were likely well-aware of the new law, as well. Additionally, the reform was likely expected to be permanent, which could help creditors overcome their reluctance to modify interest rates in the face of adjustment costs.

the bankruptcy code. More-generous insurance comes at the cost of higher interest rates. This paper's estimates can inform debates over future changes to the bankruptcy system, changes to other social-insurance programs, and to the regulation of credit markets. To infer the overall welfare impact of the reform would require, at the very least, quantifying the money-metric loss of insurance value for the marginally deterred filers, as well as the gain from lower interest rates for the affected credit-score segments. Strategic borrowing responses to the reform are also difficult to weigh: less generous bankruptcy may decrease moral hazard attributable to the option to discharge debts (Indarte, 2018); but, reducing the generosity of bankruptcy also reduces its salutary effects on entrepreneurship (Fan and White, 2003). We do not make the assumptions or impose the structural framework required to perform this welfare exercise. Nevertheless, the results above identify and quantify a number of the critical inputs for this exercise, which we leave for future research.

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