Reducing Strategic Default in a Financial Crisis

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June 2021

Abstract

We document that increasing penalties for default reduces strategic default in financial crises by exploiting the 2009 changes to Canadian consumer insolvency regulations. Our novelty is that the incentives from increasing penalties for default operate in the opposite direction from incentives in more typical financial crisis policy interventions, which increase liquidity of debtors. We can identify strategic default because our policy intervention is independent of debtors’ liquidity, and initial selection into long-term debt contracts. Our results imply that financially distressed debtors can be incentivized to reduce default during financial crises, even without the typical interventions which increase debtors’ liquidity.

JEL Codes: G01, G21, G51

Keywords: Strategic Default, Financial Crisis

* We are grateful to the Office of the Superintendent of Bankruptcy (OSB), Canada, for the provision of consumer insolvency data. Financial support from the Social Sciences and Humanities Research Council of Canada (SSHRC) and the School of Business, University of Alberta is gratefully acknowledged by Scholnick. We thank Philippe d'Astous, Alberta Di Giuli, Robert M. Hunt, Lauren Lambie-Hanson, Igor Livshits, Sahil Raina, and seminar participants at the University of Alberta, University of Sydney, the Federal Reserve of Philadelphia and the Financial Intermediation Research Society (FIRS) Conference for their helpful comments. The views expressed in this paper are solely those of the authors and do not necessarily reflect the views of the Federal Reserve Bank of Philadelphia or the Federal Reserve System, the Office of the Superintendent of Bankruptcy Canada, Industry Canada, or the Government of Canada. Send correspondence to Barry Scholnick, School of Business, University of Alberta, 3-40P Business, Edmonton, AB, Canada T6G 2R6; telephone: 780-492-5669; e-mail: barry.scholnick@ualberta.ca.
1. Introduction

Strategic default occurs when debtors default in spite of having the current liquidity to pay their debt.\(^1\) Distinguishing between strategic default and non-strategic default is particularly important during financial crises because in a financial crisis creditors or governments often attempt to reduce costly default by increasing the liquidity of debtors (e.g., by reducing monthly payments for debtors or offering temporary forbearance).\(^2\) Many of these financial crisis interventions, however, are based on the implicit assumption that debtors are not strategic defaulters because they attempt to reduce default by increasing the current liquidity of debtors. By definition, strategic defaulters are not liquidity constrained, which implies that these typical liquidity increasing interventions should not be effective at reducing default of strategic defaulters.

If debtors are strategic defaulters, however, then an alternative type of intervention is required to reduce costly default, which does not operate by loosening liquidity constraints. One such non-liquidity-based intervention could be to increase the incentives against default (e.g., by raising the penalty for default). Because strategic defaulters are not liquidity constrained, it can be argued that these debtors would have the current liquidity to respond to an intervention that increases the incentive against default, which would thus lead to an observed reduction in the default hazard.\(^3\)

Based on this reasoning, the aim of this paper is to provide new evidence of strategic default in a financial crisis. Our empirical strategy is to exploit a very atypical financial crisis policy change, the September 2009 regulatory changes to Canadian consumer insolvency law, which increased the penalty for default on long-term debt repayment contracts, while not impacting the liquidity of debtors. During financial crises, most interventions aim to reduce default by loosening liquidity constraints (e.g., reducing monthly payments), while not increasing default penalties. On the other hand, very few financial crisis interventions act to disincentivize default (e.g., increasing default penalties), while not impacting the current liquidity of the debtor. Our study examines the latter.

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\(^1\) See e.g., Elul, Souleles, Chomsisengphet, Glennon, and Hunt (2010), Ghent and Kudlyak (2011), Guiso, Sapienza, and Zingales (2013), Melzer (2017), Meyer, Morrison, Pikorski, and Gupta (2014), Li, White, and Zhu (2011), Yannelis (2020), Gerardi, Herkenhoff, Ohanian, and Willen (2017), Bhutta, Dokko, and Shan (2017), Fuster and Willen (2017), Ganong and Noel (2020A), Ganong and Noel (2020B), Indarte (2020).

\(^2\) Well-known examples of financial crisis policies to reduce default by increasing liquidity of debtors, include the Home Affordable Modification Program (HAMP) and the Home Affordable Refinance Program (HARP) during the 2008-09 Financial Crisis. See e.g., Eberly and Krishnamurthy (2014); Agarwal, Amromin, Ben-David, Chomsisengphet, Piskorski, and Seru (2017); Scharlemann and Shore (2016); Ganong and Noel (2020A); Abel and Fuster (2021); Kaplan, Mitman and Violante (2020); Maturana (2017); Kruger (2018); Haughwout, Okah, and Tracy (2016). Similar policies have been implemented during the COVID-19 pandemic, e.g., Cherry, Jiang, Matvos, Piskorski, Seru, 2021.

\(^3\) This argument is similar to that of Yannelis (2020), who argues that “policy induced variation in non-repayment cost, that is unrelated to liquidity (can be used) to test for a strategic component to the non-repayment decision” (p. 1).
The focus of this study is on Canadian consumer proposals, which are part of the Canadian consumer insolvency system. Proposals are a long-term (up to five years) legal contract to restructure consumer unsecured debt outstanding through a long-term stream of lower payments. If the debtor does not make these agreed to payments on time, then the debtor has defaulted on the proposal, which causes the debtor to lose legal protection from creditors. Thus, after a debtor defaults on a proposal, the typical option for that debtor is to file for personal bankruptcy in order receive new legal protection from the creditors. For this reason, an increase in the cost of bankruptcy will act as an increase in the cost of defaulting on a proposal. This is the policy change we examine in this paper.

We define treatment and control groups based on Canadian bankruptcy rules, specifically a legally defined concept, known as Surplus Income, which is calculated as income minus authorized expenses. Debtors with a Surplus Income above $200 are required to pay 50% of Surplus Income to creditors for a number of months in bankruptcy. Our identification strategy exploits the 2009 regulatory changes to the Canadian Bankruptcy and Insolvency Act (BIA), which increased the number of monthly payments required from such debtors in bankruptcy, from 9 months to 21 months. This increase in the cost of bankruptcy thus acts as a plausibly exogenous increase in the cost of default on a proposal. Our treatment group therefore consists of pre-reform proposal filers with Surplus Income above $200, who would be required to pay higher costs in bankruptcy after the reform, while our control group consists of pre-reform proposal filers with Surplus Income below $200 who were not affected by the 2009 reform, because they would make the same monthly payments in bankruptcy, both before and after the reform.

Our main result is that treated debtors (who were subject to the increased default penalty) can be incentivized to reduce their default hazard by 14% (significant at 1%) relative to control debtors (who were not subject to the increased penalty), in the post vs. pre-reform periods. This finding, that a higher default penalty can incentivize debtors to reduce their default rate, is consistent with these debtors being strategic defaulters.

The setting of our study is particularly relevant for creditors and governments because we document the effectiveness of increasing the penalty for default at reducing default: (1) during a financial crisis (our intervention occurred in September 2009), and (2) for debtors who can be classified as being financially distressed (all debtors in our study are participants in the Canadian insolvency system). One possible assumption by governments or creditors is that the kind of debtors in our study (distressed debtors in a crisis) may be relatively less likely to be strategic defaulters compared with other debtors because both (1) debtor financial distress, and (2) financial crises are typically associated with increased liquidity constraints. Our main finding in this paper contradicts this assumption. Our results thus emphasize the importance of differentiating between strategic and non-strategic default,
when designing policy interventions aimed at reducing default, even for the types of debtors (e.g., distressed debtors in a crisis) who may be assumed not to be typical strategic defaulters.

Intuitively, our setting examines a context where an agent faces an increased incentive to remain in a pre-existing long-term contract (in our case, not to default on the pre-existing long-term consumer proposal), if there is an increased cost associated with the outside option of exiting from that long-term contract (in our case, the increased payments that are required under bankruptcy, from 9 months to 21 months). Importantly, the 2009 regulatory changes to the BIA only changed regulations for new insolvency filings, but had no effect on existing proposals. In other words, these 2009 regulatory changes did not affect any of the terms of the pre-existing long-term proposal contract (thus not impacting the liquidity or cash flow of the debtors), but only increased the cost of defaulting on that proposal contract.

We use the 2009 policy change to formulate a Difference-in-Differences (DID) empirical strategy to estimate the effect of higher default penalty on the probability of default on proposals. Our identification strategy closely follows the recent empirical literature on “ex-post moral hazard” as a motivation for strategic default, e.g., Meyer, Morrison, Piskorski, and Gupta (2014), Yannelis (2020), and Blouin and Macciavello (2019). The unique element of identification used in this literature, and in our paper, is the exploitation of an exogenous shift in the penalty for default (e.g., caused by regulators or courts) part way through a long-term contract. Because the terms of the long-term contract have been agreed to by the parties some period before the date of the shock to the penalty for default, the shock can thus be considered independent of the debtor’s current ability to pay (which allows us to identify strategic default), and also any factor which caused either party to initially select into the contract (which allows us to control for issues of selection).

Our paper contributes to the literature comparing the effectiveness of various policy interventions designed to reduce default. An unsettled debate in this literature concerns the relative effectiveness of interventions which increase short-term liquidity (e.g., reducing monthly debt

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4 We follow Blouin and Macciavello (2019) in labelling our specific setting as “ex-post moral hazard”. These authors distinguish between “ex-post moral hazard” (where an exogenous shift in the cost of default, in the middle of a long-term contract, causes a subsequent change in the default hazard) from “ex-ante moral hazard” (where an insured debtor has an ex-ante incentive to take more risks).

5 Meyer, Morrison, Piskorski, and Gupta (2014) examine the Countrywide case, and document that a court decision, which increased the incentive of mortgage holders to default, increased subsequent default. Blouin and Macciavello (2019) argue that coffee sellers strategically default on futures contracts if the exogenous spot price at maturity is higher than the agreed to futures price, set at the original contracting date. Yannelis (2020) documents that exogenous changes to US bankruptcy regulation, which decreased the incentive of debtors to default on student loans, reduced subsequent default.

6 Agarwal, Amromin, Ben-David, Chomsisengphet, and Evanoff (2011), Posner and Zingales (2009), Tracy and Wright (2016), Adelino, Gerardi, and Willen (2013), Piskorski, Seru, and Vig (2010), Rucker and Alston (1987).
payment), versus interventions that increase long-term equity (e.g., reductions in long-term debt balance through principal forgiveness). Some studies (e.g., Ganong and Noel, 2020A), conclude that short-term liquidity interventions are more effective at reducing default, while other studies (e.g., Dobbie and Song, 2020; and Kaplan, Mitman and Violante, 2020), conclude that short term liquidity interventions are less effective, compared with long term equity interventions. One possible explanation for the latter conclusion, that short-term liquidity interventions are relatively less effective at reducing default, could be that debtors are strategic defaulters, and thus not liquidity constrained. Thus, our main finding, showing the effectiveness of a policy to increase the penalty for default, could be consistent with the studies showing the relative ineffectiveness of short-term liquidity interventions.

Our study also contributes to the literature on empirically estimating the prevalence of strategic default. This literature is also unsettled, having produced a wide variety of different estimates. Studies that find that a large fraction of mortgage defaults are strategic include Guiso et al. (2013), who find that 35% of mortgage defaults are strategic, and Gerardi et al. (2018), who find that 38% of mortgage defaults are strategic. Other studies find that strategic default is less prevalent. Ganong and Noel (2020B) find that only 3% of mortgage defaults are strategic, and Bhutta et al. (2017) argue that mortgages have to be very deeply underwater before debtors strategically default.

There are a variety of empirical challenges that have resulted in this wide dispersion in the fraction of strategic defaults. The first is the difficulty in isolating clean exogenous variation in the incentives to default strategically. For example, a common argument in the literature on strategic default on mortgages is that debtors have a larger incentive to strategically default, the greater the extent to which the house is underwater (i.e., market value of house minus mortgage debt outstanding). However, many studies have highlighted the difficulties in isolating exogenous variation in the current value of a specific debtor’s house, relative to prevailing macroeconomic conditions in the surrounding geographic area. Second, in addition to current asset values, strategic default could also be triggered by expectations of future asset values, which are also challenging to capture empirically. A third empirical challenge (Ganong and Noel, 2020A) relates to the difficulty in measuring exogenous variation in adverse life events (e.g., health problems, income shocks) because if a default is caused by an adverse life event, then it should not be classified as being a strategic default. Thus, if adverse life events are measured with error, then this could also impact the measurement of strategic default.

The advantage of our set-up, however, is that we are able to capture strategic default by directly examining a plausibly exogenous (mandated by a regulator) increase in the penalty for default, which is unrelated to other factors that may trigger strategic behaviour (e.g., current and expected future asset values; interest rates; current liquidity; and the presence or absence of adverse life events). Our setup is most similar to Yannelis (2020), who also exploits an exogenous, policy induced, increase in the
penalty for default (in the context of student loans), to isolate strategic default. Our study differs from Yannelis (2020), however, in that our policy shock occurred in the context of a financial crisis, when issues of strategic default are particularly salient to policy makers and creditors.

While our main specification only exploits the exogenous regulatory increase in the penalty for default, our data do also allow us to examine measures such as the equity in a house as well as the existence of adverse life events, as measured at the time of the start of the proposal. Given the discussion above regarding the difficulties in the identification of exogenous variation in these variables, we limit our use of these variables to examining possible mechanisms for our main results, by undertaking sample split type comparisons of our main specification.

In our Canadian proposal context, a default on a proposal can entail the debtor losing the house. Consistent with this argument, we find in specifications that split the sample based on home equity or home ownership, that treated proposal filers who are also homeowners, or who have positive home equity, will reduce their propensity to default on a proposal by an even larger amount (because they face an additional incentive to avoid losing the house) compared with treated non-homeowners, or treated debtors with zero or negative equity. When we split the sample based on adverse life events, our results show that treated debtors subject to adverse life events are less able to reduce default in response to the increase in the cost of default compared with treated debtors not subject to adverse life events. Adverse life events thus reduce the ability of treated debtors to respond strategically to the higher default penalty. These findings are consistent with the idea that both liquidity constraints (e.g., because of adverse life events) and a strategic motivation are important in explaining default, and liquidity-constrained individuals are less able to respond to the strategic motivation.

We also split the sample based on the current age of the proposal. We document that unconditional default rates of proposals close to completion are relatively low in both the pre and post periods, while unconditional default rates for recently started proposals are relatively high. Debtors with proposals close to completion may have a stronger incentive to avoid default because the legal advantages of insolvency discharge only accrue at the completion date. We find that the increase in the cost of default does not have a significant effect on treated proposals near proposal completion because of their already low default rates, but it reduces default for the cohorts of recently started proposals, where default rates are higher in the pre-period.

2. Institutional Background

2.1. Consumer Insolvency in Canada
In Canada a single federal government agency, the Office of the Superintendent of Bankruptcy (OSB) regulates all aspects of consumer and business insolvency. The OSB has provided us with the data used in this study. There are two main forms of consumer insolvency in Canada, consumer bankruptcy and consumer proposal. Under consumer proposal, financially distressed debtors make “proposals” to their creditors to modify their existing unsecured debt obligations (mostly credit card debt), while holding their secured debt contracts (e.g., mortgages) unchanged. These modifications to unsecured debt obligations may reduce the amount of debt and/or increase the time available to repay the unsecured debt outstanding. These debt repayment contracts typically entail a series of monthly payments and can last up to 5 years.\footnote{In this study, we focus on consumer (also known as Division II) proposals, which, before the 2009 bankruptcy reform, were available for debtors with total debt less than $75,000, excluding debt on their primary residence. Most consumers file this type of proposals.} Consumer proposals are somewhat similar to Chapter 13 bankruptcy in the U.S., in that both involve the restructuring of debt through a schedule of payments over a number of years. However, unlike Chapter 13, proposals are more flexible and allow debtors to propose any terms to their creditors. The proposal only becomes a binding legal contract, however, once the proposal by the debtor is accepted by the creditor(s).\footnote{Once a proposal is filed by the debtor, all creditors are able to vote on whether to accept the proposal, with voting rights of creditors proportional to the total unsecured debt outstanding as of the filing date. If the creditors reject a proposal, the debtor can amend it and propose a new proposal to creditors, file for bankruptcy or do nothing. Debtors are also able to withdraw proposals before they are considered by creditors, but this outcome is rare in the data. If the creditors do not vote on a proposal within 45 days of filing, the proposal is deemed accepted.}

While the OSB sets detailed rules regarding insolvencies, individual insolvency estates (i.e., cases) in Canada are administered by insolvency trustees (typically chartered accountants). Trustees are licensed by the OSB, and act as “officers of the court” in that they are designed to represent the interest of both debtors and creditors. The trustee is also responsible for verifying all the information provided by debtors in filing for consumer proposals, thus ensuring the accuracy of the data used in this study.

Under Canadian insolvency law, a default on the proposal is defined to occur after missing three monthly payments to the creditors.\footnote{Other outcomes after non-payment which do not result in default include an amendment to the terms of the proposal (which needs to be accepted by creditors), or a proposal revival (i.e., by making missed payments).} The proposal contract is automatically annulled if the debtor defaults. In addition, default causes the debtor to lose all money paid under the proposal, before the default, which we label as “sunk costs of default”. Once the proposal contract is annulled, the creditors are free to start any legal or other proceedings against the debtor (e.g., the use of collections agencies or wage garnishment) in order to recover the outstanding unsecured debt. The typical option for debtors...
after proposal default is to enter into personal bankruptcy, which protects the debtor against these legal proceedings from the creditors.

An important element of our study is the legal distinction between consumer proposals and consumer bankruptcy (the two types of consumer insolvency available in Canada). While consumer proposal can be considered as a restructuring of debt repayments, consumer bankruptcy can be considered as a liquidation of assets.\textsuperscript{10} Specifically, under consumer bankruptcy most of the debtor’s unsecured debts are discharged in exchange for the liquidation of non-exempt assets (e.g., house), and repayment of a fraction of filer’s income to the creditors for a fixed length of time. An important difference between proposal and bankruptcy is that creditors have no legal right to reject a bankruptcy filing by the debtor, but the creditors are legally able to reject (or accept) any proposal filing by the debtor. Because of this asymmetry in the legal rights of creditors, the creditors have an incentive to reject a proposal filing if they believe that they will be in a better position if the debtor filed for bankruptcy instead. The amount that would be repaid to creditors in a bankruptcy filing, thus, becomes an “informal floor” on the amount debtors need to offer under proposals, in order for their proposal to be accepted by creditors.\textsuperscript{11}

Proposal is also treated more leniently than bankruptcy by Canadian credit bureaus. A proposal is typically rated as R7, while a bankruptcy is typically rated as R9 (on a rating scale where R1 is the best, and R9 the worst credit rating). In addition, the proposal will remain on the credit record for a shorter period (6 years from the filing date or 3 years from the full payment date, whichever is soonest), compared with bankruptcy (7 years from the date of completion).

\section*{2.2. The 2009 Amendments to the Bankruptcy and Insolvency Act (BIA)}

On August 14, 2009, the OSB announced that it would implement a number of changes to the BIA, including increasing the number of months that bankruptcy filers have to make payments to their creditors from 9 months to 21 months. These changes took effect on September 18, 2009. Importantly, there were no regulatory changes to the proposals included in our study. These increases to costs of bankruptcy indirectly affected existing proposals because they increased the penalty for default on a proposal.

\textsuperscript{10} This distinction between restructuring and liquidation types of insolvency is also reflected in the choice between Chapter 13 and Chapter 7 bankruptcies in the U.S.

\textsuperscript{11} This “informal floor” (i.e., the fact that the amount creditors receive under proposal should be greater than or equal to the amount that the creditors would receive in bankruptcy) is commonly described by bankruptcy trustees when advising debtors choosing between bankruptcy and proposal.
The increase in the cost of bankruptcy raised the cost of default on proposals because these two types of consumer insolvency are interrelated. In particular, creditors have the legal right to reject any proposal filing, but cannot reject bankruptcy filings. In addition, many proposal defaulters subsequently file for personal bankruptcy. Thus, after a default on a proposal, a subsequent filing for bankruptcy can be considered as a valuable outside option for the debtor because of at least two reasons: (1) the creditors are legally obligated to accept any bankruptcy filing, and (2) the bankruptcy filing protects the debtor from proceedings such as collections lawsuits or wage garnishment (which creditors are allowed to pursue after proposal default). In our setting, we examine how an increase in the costs of this outside option (bankruptcy) affects default on a proposal.

While a significant majority of proposal defaulters subsequently file for bankruptcy, another legal option after proposal default is for the debtor to attempt to file a new proposal, although creditors have the right to reject any such proposal. As described in the previous section, the amount that would be repaid to creditors in bankruptcy is an “informal floor” on the amount debtors need to offer their creditors for the proposal to be accepted. However, after the 2009 increase in the amount of payments made by debtors under bankruptcy, this “informal floor” will increase because the policy change increased the amount that creditors would receive. This implies that after a proposal default, creditors would only accept a new proposal that offered even higher payments, compared with the original proposal negotiated in the pre-reform period. This provides yet another reason why the 2009 reform created an increased incentive for a debtor not to default on existing proposals, negotiated under the more debtor friendly pre-reform rules.

The specific policy change we examine concerns an important part of the costs that bankrupt debtors need to pay creditors, known as “Surplus Income” (SI). SI is calculated (based on detailed rules defined by the OSB) as the bankrupt’s total income after subtracting authorized deductions and expenses. The basic rule is that bankruptcy filers need to pay 50% of surplus income per month to creditors, for a number of months. Our identification strategy exploits the change in the SI rules in September 2009, which changed the number of months bankruptcy filers were required to pay their creditors, from 9 months to 21 months, if their surplus income was more than or equal to $200. In the pre-reform period, both groups of filers would only be required to pay 50% of their SI for 9 months. This more than doubling of the number of months that SI payments were due, increased the cost of those bankruptcy filers with SI equal to or above $200. Importantly, these 2009 regulatory changes had no effect for individuals with SI less than $200 per month. Debtors with expenses > income, i.e. negative SI (about half of our sample) pay zero dollars in surplus income (50% of a negative amount is considered to be zero), in both the pre and post-reform periods. In other words, the regulatory change from 9 months to 21 months of SI payments has no effect on debtors with negative SI.
Figure 1 graphically displays these various arguments, used in the definitions of our treatment and control groups. This figure displays the dollar amounts that bankrupt debtors have to pay creditors, as a function of the amount of surplus income. When SI is larger than 0, there is a linear upward sloping relationship. This linearity is because exactly 50% of SI is paid to creditors, no matter how high the amount of SI. The 2009 reform, which increased the number of months that payment (still of 50% of SI) is made by the debtor, from 9 months to 21 months are also displayed in Figure 1. This figure shows that the upward sloping linear relationship to the right (where SI \(\geq $200\)) is steeper post 2009, because the amount payable (50% of SI) is now paid over 21 months rather than 9 months. Also, there is a discontinuous increase in the amount paid in bankruptcy post-reform when SI = $200 because these payments are done for 21 months after the reform. Importantly, these rule changes had no effect for individuals with SI < $200 because they still pay the same amount in both the pre and post-reform periods. Debtors with negative SI (where expenses > income) pay zero in both the pre and post periods, because 50% of a negative amount is assumed to be zero.

This discussion leads to the definition of the treatment and control groups in our study. Because pre-reform proposal filers with surplus income equal to or larger than $200 would be required to pay an extra amount in bankruptcy after the reform, they have an increased incentive to avoid defaulting on their proposals in the post-reform period. On the other hand, pre-reform proposal filers who have less than $200 in surplus income do not face this additional incentive to avoid default on proposal after the reform. In other words, the treatment group (\(\geq $200\) in SI) would face a higher penalty for default on proposal, while the control group (less than $200 in SI) would not face this penalty. It is important to note that this penalty applies only to proposal defaulters when they subsequently file for bankruptcy and this rule does not change any other conditions of existing pre-reform proposals. It does not affect filer’s liquidity constraints or equity positions.

While this policy change to increase the penalty for default occurred in the middle of a financial crisis, it was not designed to punish defaulters on consumer proposals. As described by Allen and Basiri (2018), the main motivation for the regulator to increase the cost of bankruptcy in 2009 was to incentivize new insolvents to select the proposal form of insolvency, rather than the bankruptcy form of insolvency. These authors show that this policy goal was indeed successful, in that after the 2009 reform, a larger fraction of new insolvents selected into proposals rather than bankruptcy. In this paper, however, we do not focus on this main motivation of regulators (initial selection by insolvents into either bankruptcy or proposal), but rather focus on a second order outcome from this regulatory change.

12 The 2005 changes to the U.S. bankruptcy system (under the Bankruptcy Abuse Prevention and Consumer Protection Act, or BAPCPA) had somewhat similar goals (see e.g., Gross, Kluender, Liu, Notowidigdo and Wang, 2019; Chakrabarti and Pattison, 2019; Li, White and Zhu, 2011).
This is the effect of these regulatory changes on those proposal filers who were already part way through their multi-year proposal repayment plan, as at the date of the 2009 reform. Because the policy shock occurs part way through a long-term contract, we argue that the shock does not have any confounding impacts on debtors who remained in the same long-term contract after before, during and after the event date, other than the increase in the cost of the option to default.

3. Data and Preliminary Evidence

3.1. Data

Our data are provided by the OSB, and include all records collected by the regulator, for the universe of insolvency filings in Canada. Our sample starts in 2006 when the OSB implemented an electronic filing system. As described in the previous section, the central element of our identification strategy is the focus on those consumer proposal filers whose proposals were already underway as at the date of the 2009 reform. For this reason, our main sample includes proposal filers with filing dates from 1 January 2006 to 18 September 2009, which results in 59,506 proposal filings.

Our data are made up of three components: (1) data provided by the debtor at proposal filing, (2) data on the various outcomes of proposals, and (3) duration-based data on the length of time from the origination date to the date of various outcomes.

Data provided by the debtor at the date of proposal filing are described in Table 1. These data include the full balance sheet and income statement of the debtor, as well as demographic data. The balance sheet data at proposal filing, include the consumers’ liability structure (i.e., secured debt, unsecured debt and preferred debt) and asset holding (e.g., cash, house value, car value). The income statement data include a variety of income and expense items (e.g., employment and self-employment income, non-discretionary and discretionary monthly spending). Demographic data include filer’s gender, age, marital status, occupation, household size and postal code. We are also able to observe the original proposal filing date and planned completion date. Using these dates, we calculate planned duration (as measured in days) of the proposal agreements, which are described in the histogram in Figure 2. As can be seen in this histogram, there are large percentages of proposals planned to be completed in 3, 4, and 5 years.

13 Unlike the U.S., where bankruptcy data are collected by 94 different bankruptcy court districts, all insolvency data in Canada are collected by the OSB, thus the OSB is able to provide us with the universe of all insolvency filers in Canada. Given that Canadian consumer proposals are somewhat similar to Chapter 13 bankruptcy filings in the U.S, our paper also relates to the literature on Chapter 13 bankruptcies in the U.S. (e.g., Li and Sarte, 2006; White and Zhu, 2010; Dobbie and Song, 2015; Eraslan, Kosar, Li and Sarte, 2017).
Importantly, all consumer insolvency filers in Canada (both bankruptcy and proposal) are required to complete the identical forms (OSB forms 65 and 79) when filing for either bankruptcy or proposal. Because of this requirement, we can compute the hypothetical costs of bankruptcy, including the amounts that would be paid in Surplus Income (SI), for every debtor who files a proposal. We are also able to accurately calculate the hypothetical increase in the cost of bankruptcy for every proposal filer, caused by the 2009 policy change, simply by multiplying the monthly SI payments the debtor would make in bankruptcy, by 21 months, instead of by 9 months. Our data (Table 2) also include data on the performance of each proposal over the complete length of the contract. These outcomes include: proposals paid in full, defaults, amendments (agreed to by both the debtor and creditors), proposals rejected by creditors and proposals withdrawn by debtors. Table 3 provides data on the length of time (measured in days) from the origination date to the dates of these various outcomes. Most proposal amendments, rejections and withdrawals occur within a few months of proposal filing. We exclude proposals that are rejected or withdrawn from further analysis because they cannot default.

3.2. Preliminary Evidence: The Hazard Function and Kaplan-Meier Curve

An important element of our data structure is that it consists of duration type data, measured in days, where we can exactly observe the exact start and end dates of all proposals, as well as the exact date of any default. Our study examines the duration (in days) from the start date of a proposal, until a default on the proposal or the end date of the contract. Figure 3 plots the unconditional hazard functions of proposal default over analysis time (the age of the proposals in days) for our full sample of proposals. The overall shape of the hazard functions has a peak in default rates for proposals at about 12 months, and then a steep and monotonic decline in default rates as the proposals get older (until the maximum age of approximately 5 years). This pattern of default over the life cycle of a long-term debt contract (i.e., peaking at approximately 12 months since origination) is similar to default rates that have been documented over the life cycle of other long-term consumer debt contracts, such as mortgages (Keys, Mukherjee, Seru and Vig, 2010; and Li, White and Zhu, 2011). As we describe in detail below, the very strong relationship between the age of the proposal (in days) and the default rate, as demonstrated in Figure 3, is why we use a Cox proportional hazard model in our main specification.

In Figure 4, we plot Kaplan-Meier survival functions for our treated and control groups, showing the proposal survival rate as analysis time (in days) increases. As expected, the survival rate for the treated group (who are incentivized to reduce default by the reform) is higher than the survival rate for the control group. In Section 7.3, we describe a variety of possible explanations for this observed shape of the Hazard Function (e.g., sunk costs, survivorship bias, and legal incentives to complete the proposal contract).
rate of the control group. However, while Figure 4 differentiates between treated and control groups, it does not account for when the 2009 policy change affects each proposal (they start at different points in calendar time, so the policy change would also occur at various proposal ages). With this caveat, it is difficult to define the pre- and post-reform periods for all the proposal (each will have its unique post-reform date) and compare survival rates in these two periods. That analysis is conducted in our full Cox proportional hazard model specified in Section 5.

4. Identification

4.1. Selection

As described in the introduction, our identification strategy closely follows the literature on “ex-post moral hazard” (e.g., Meyer, Morrison, Piskorski, and Gupta, 2014; Yannelis, 2020; and Blouin and Macciavello, 2019) by using a set-up where a change to the cost of default occurs part way through a long-term contract. The argument in this ex-post moral hazard literature, is that the exogenous shock to the cost of default (in our case, the change to the SI rule, from 9 months to 21 months of payment, in the September 2009 reform), are independent of the negotiated terms of the original contract (in our case the original proposal contract agreed to by the debtors and creditors) because the original contract was agreed to by debtors and creditors some period before the date of the 2009 policy reform. This set-up allows us to address issues of (possibly adverse) selection into the proposal contract. An important empirical challenge in identifying possible strategic default/moral hazard behavior is the need to disentangle strategic default/moral hazard from adverse selection, which occurs at the date of origination of the contract. For example, adverse selection may play a role in default if debtors who selected into a contract are of (unobservable) worse credit quality than debtors who selected into a different contract. We argue that our empirical setup allows us to control for adverse selection and isolate the effect of moral hazard/strategic default on default. Because by definition all debtors as well as creditors in our sample selected into their proposal contracts some period before the date of the 2009 reform, the increased penalty for default and all other changes introduced by the reform will not affect the initial selection into the proposal contracts or their terms. Thus, the effects from the increased penalty for default we find can be attributed to the strategic default / moral hazard behavior and not the initial selection into the proposal contracts.

4.2. Allocation into Treatment and Control Groups
As described in detail in Section 2.2, we define our treatment and control groups based on the SI cutoff of $200. The 2009 reform increased the cost of default for proposal filers with SI ≥ $200 (the treated group), but did not affect the cost of default for proposal filers with SI < $200 (the control group). The 2009 policy shift has no effect on the terms of the previously negotiated proposal, nor on the liquidity or cash flow of debtors in those proposals.

A key advantage of our setting is that the identical OSB Forms are used for bankruptcy and proposal filers, thus we are able to observe a “hypothetical” measure of SI, for all proposal filers, as measured at the date of their proposal filing, even though the SI = $200 cutoff is not directly relevant for proposal filers at that time. If, however, a debtor was to default on a proposal and subsequently file for bankruptcy, they would then submit new OSB forms, and thus new SI data, where the SI=$200 cutoff would become directly relevant in determining the costs of bankruptcy. In our set-up, we separate proposal filers into treatment and control groups based on their “hypothetical” SI calculation, as measured at the date of their original proposal filing (pre 2009 reform). We use this “hypothetical” SI measure, as a proxy for the SI amount that would be reported in a subsequent bankruptcy filing, in determining the expected costs of that future bankruptcy filing. Given that the allocation into our treatment and control groups is based on “hypothetical” bankruptcy cost calculation, that is measured some time before the date of the policy change, we argue that this SI=$200 cutoff should not be correlated with default on the proposal.

One possible concern with defining treatment and control groups based on the $200 cutoff is that debtors could be incentivized to manipulate the level of SI they report. Manipulation of self-reported data is well documented in contexts such as tax filings, etc. We argue, however, that there are strong institutional reasons why such data manipulation around the $200 cutoff is less likely in our context. These include the facts that $200 surplus income (SI) cutoff is not directly relevant in the proposal filing before the 2009 reform. This cutoff would only become relevant to the debtor, if they defaulted on the proposal and subsequently made a new bankruptcy filing. Even then, they are required to provide new SI data to their trustee. In addition, the central advantage of our ex-post moral hazard set-up is that all reporting of SI in our study is done at a time prior to the 2009 policy shift we are interested in. Based on these arguments, we argue that there is essentially no legal advantage to the proposal filers in our study to manipulate their SI (as reported at the time of their proposal filing, which occurred before the policy change) to either side of the $200 cutoff.

We provide evidence supporting this argument by running a standard assignment variable manipulation test developed by Cattaneo, Jansson, and Ma (2020), which examines whether proposal filers “bunch” significantly on either side of a cutoff – in our case the $200 cutoff in SI. We display these data visually in Figure 5. The data used in our test include the complete sample of filers from
January 2006 to 18 September 2009, from the 10\textsuperscript{th} to 90\textsuperscript{th} percentiles of SI (to remove outliers), which is SI within the range [-$700, $1500]. The test for significant bunching at the $200 cutoff produces a T test statistic of -1.1407 and a p-value of 0.254. In other words, there is no evidence of income manipulation at the $200 cutoff in this sample. Thus, this result is consistent with our institutional discussion above, arguing that debtors have no legal incentive to manipulate the level of SI around the $200 cutoff before the 2009 reform.

4.3. Macroeconomic Variation and Allocation into Treatment and Control Groups

Figure 6 plots the overall number of proposals filed in the treatment and control groups in the pre-reform period (from January 2006 to September 2009), which is the full data used in our study. This figure indicates that the raw number of filings is remarkably similar across the treated and control groups, which implies that our cutoff of SI = $200 occurs close to the midpoint of the SI distribution across all proposal filers. In addition, both the treated and control series trend similarly over time, (from 2006 to 2009) which indicates that broad macroeconomic trends are not impacting the proportion of proposal filers allocated into the treatment or control groups. The upward sloping trend in the number of proposal filings from 2006 to 2009 is because the period from 2006 to 2007 were boom years, with relatively fewer proposal filings, while 2008 and 2009 were financial crisis years with many more proposal filings. The key point for our identification strategy from Figure 6 therefore, is that this very large boom-bust variation between 2006 and 2009 did not impact the relative proportions of proposal filers in our treatment and control groups. Furthermore, evidence from this figure, indicates that there does not appear to be information leakage in the period before the August 2009 announcement, or any announcement effects in the brief period between the announcement and implementation (September 2009) of the policy. One additional implication of the relatively constant proportion of filers in the treated and control groups, over the business cycle, is that the mix of close to completion and recently started proposals are not different across the two groups, which is of importance in our analysis of seasoned and young proposals in the following sections.

4.4. Parallel Trends in the Pre-Reform Period

Figure 7 plots the raw monthly default rates for the treated and control groups, for the 12 months pre and post the September 2009 regulatory shift. The data in this figure include all proposal filers who filed at any time before the reform date and have not defaulted at the start of the 12 month window.
Our key identification assumption is that the treatment and control groups would have parallel trends in the default rate in the absence of the policy change. While this assumption is not testable in principle, we can examine whether these two groups had parallel trends at least in the pre-reform period. Figure 7 suggests that default rates evolved similarly in the treatment and control groups in the 12 months before the 2009 reform, with no consistent pattern of either the treated or control series being larger than the other, across the various months. This pre-trend pattern thus provides some suggestive evidence to support our main identifying assumption.

In addition, Figure 7 shows that in the post-reform period the default rate of treated group is consistently lower than those of the control group, for every one of the 12 post-reform months. In addition, the difference in the monthly default rates between the treated and control groups appears larger in size in the post-reform period compared with the pre-reform period. These patterns may suggest that the reform reduced the monthly default rate in the treated group compared with the default rates in the control group. The magnitude of the effect of the treatment on the treated group relative to the control group is formally estimated using duration data models in the following sections.

An important point when examining Figure 7, for both the pre and post-reform periods, is that while the treatment and control default rate series track each other closely over time (with the exception of the wider gap between the series in the post period), the two series are both very volatile over time. This volatility in the default rates may reflect the volatile nature of the financial crisis in the 12 months before and after the September 2009 bankruptcy reform. Importantly for our identification strategy, the close tracking between the treated and control groups, in both the pre and post periods, is maintained despite this volatile macroeconomic environment in this period. This implies that both treated and control groups were similarly impacted by the volatile macroeconomic environment in 2009.

While our preferred specification uses a Cox proportional hazard model (described in Section 5), Cox models do not allow for standard tests of the parallel trends assumption. Thus, as an additional check of the parallel trends assumption, we estimate a simple Linear Probability model on our data, in which default is an outcome variable. Similar to the raw default rates (in Figure 7), we restrict our data to only the 12 months before and after the reform date. To account for issues of late entry into the sample (e.g., filing between September 2008 and September 2009) and early exits (e.g., because of default or maturity before September 2010), we set the dependent variable to missing before proposal entry and after exit. This model estimates monthly coefficients on the interaction of monthly dummies with a treatment dummy (equal to 1 for proposals with SI ≥ 200), with month -1 (August 2009) being the base category (Section 8.7 provides additional details on the model).
Figure 8 displays the coefficients on the interactions of monthly event dummies with the treatment indicator. The coefficients displayed in Figure 8 are consistent with the parallel trends assumption prior to the reform. The estimated monthly coefficients are statistically insignificant for each of the 11 months in the pre-period (relative to the omitted period, month -1). However, Figure 8 shows that the coefficients are significant and negative in most of the 12 months in the post-reform period. These negative coefficients indicate that treated proposal filers (who were incentivized to reduce default) did significantly reduced their default rate in the post-reform period. We thus argue that both the monthly raw default data (in Figure 7) as well as the monthly coefficients estimated by the Linear Probability model (in Figure 8) appear consistent with the parallel trends assumption.

4.5. Policy Endogeneity

An important identification assumption in our study is that the policy shift we examine (the 2009 change to the BIA) is plausibly exogenous. However, a recent strand of the DID literature (e.g., Freyaldenhoven, Hansen, and Shapiro, 2019), has emphasized the possibility that such an assumption can be violated if some element of the timing of the policy introduction is endogenously related to an unobserved confounding variable. An example provided by Freyaldenhoven et al. (2019), is if the timing of a minimum wage increase is related to the unobserved level of labor demand (e.g., that the minimum wage is increased by the policy maker, when the level of labor demand crosses a specific threshold), in which case the policy to increase the minimum wage could not be considered exogenous.

We argue, however, that such a concern is not relevant in our setting. Recall that the main motivation for the 2009 amendment to the BIA (as described by Allen and Basiri, 2018) was related to the number to of bankruptcy and proposal filers, whereas our study does not examine the number of filers, but rather uses this setting to examine how the policy shock impacted the rate of defaulters. Specifically, the main motivation behind the change to the BIA, was to increase the incentive to make proposal filings relative to bankruptcy filings, by increasing the costs of a bankruptcy filing (increasing surplus income payments from 9 months to 21 months), while leaving proposal regulations unchanged.

Our study, on the other hand, exploits this setting to examine a second order effect of the policy shift, the costs of default on pre-reform proposals. It is because our focus is on the change in the cost of default, rather than incentives for filing, that we limit our sample to only those proposal filers who are already part-way through their long-term proposal contract, as at the date of the 2009 policy change. The implications of this institutional discussion, is that the main incentives of the BIA policy makers were not related to the key outcome variable in our study, i.e. default on proposals. It is for this
reason that we argue that our setting is not subject to the critique of DID settings, raised by Freyaldenhoven et al. (2019).

4.6. Treatments are not Staggered

A large recent literature on the econometrics of DID methods concerns the problems that arise when policy shocks are staggered across different units (e.g., States), across different times. If a previous policy shock compounds over time, then this literature argues that it is inappropriate to use a unit with an earlier treatment, as a control for a unit with a later treatment. The advantage to our setting, in this regard, is that we do not have staggered interventions across multiple units and dates. The 2009 BIA change was implemented on a single date across Canada. Rather, our identification strategy is based on variation across SI levels from individual proposal filers.

5. Econometric Specification

Key institutional elements in our empirical setup are that proposals: (1) are originated at various dates over multiple years, (2) have different maturities, (3) different end dates and (4) different default dates (if default is present). In addition, the September 2009 policy reform would affect proposals at various stages of their life. Finally, as we document in Figure 3, there is an unconditional relationship between the age of the proposal in days and the default rate (we document a peak default rate at approximately 12 months, and then a steep decline in the default rate as the proposal ages until the maximum of approximately 5 years).

In order to account for all these issues of timing, we use a Cox proportional hazard model, where we can observe the span of time from the proposal origination date to the reform date, as well as the span of time from the reform date to the last observed event date (e.g., proposal default or completion). Another related advantage of the Cox proportional hazard model is that it is possible to account for both left censoring (e.g., where the proposal originated before the first observed date), as well as right censoring (e.g., where the proposal terminated after the last observed date). Given the long-term nature of proposal contracts, both left censoring as well as right censoring are important elements of our various specifications. We include a standard difference-in-differences model within

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15 See, Sun and Abraham (2020), De Chaisemartin, & d'Haultfoeuille (2020), Borusyak and Jaravel (2017), Callaway and Sant'Anna (2020), Goodman-Bacon (2018), Athey and Imbens (2018).

16 For these reasons, the use of Cox proportional hazard models is common in the literature examining consumer default on long term contracts such as mortgages (e.g., Li, White, and Zhu, 2011; Demyanyk and Van Hemert, 2011).
our Cox specification, to estimate the effect of the increased penalty for default on the probability of default. The treatment effect is defined as the difference in default in the treatment group (≥ $200 SI filers) and control group (< $200 SI filers) before and after the legislation change.

The Cox proportional hazard specification is given as follows:

\[
  h_i(t) = \gamma_0(t) \times \exp(\gamma_1 \times \text{Post}_i + \gamma_2 \times \text{Treat}_i \times \text{Post}_i + \gamma_3 \times \text{Treat}_i + \gamma_4 \times \text{Controls}_i + \mu_k + \epsilon_i),
\]

where \( h_i(t) \) is daily likelihood of default (failure) for the consumer proposal \( i \) at time \( t \), \( \gamma_0(t) \) is the baseline hazard function which is the hazard function when all the covariates are zero. \( \text{Post}_i \) is a time variant variable taking the value of 1 in the period after the reform, and zero otherwise. Our setup follows the standard hazard model approach of creating two separate time spans for each individual debtor, for the time period before the September 2009 policy change (when \( \text{Post}_i = 0 \)), and for the time period after the date of the policy change (when \( \text{Post}_i = 1 \)). Because of the creation of two time spans for each debtor, the total number of observations is approximately double the size of the number of debtors in the sample. Our treatment indicator, \( \text{Treat}_i \), takes the value of 1 if the proposal filer has surplus income equal to or more than $200, and zero otherwise.\(^{17}\)

\( \text{Controls}_{i,j} \) are filing characteristics and proposal filer characteristics including gender, age, marital status, household size, total asset value, total secured debt amount, total unsecured debt amount, home ownership, proposal debt amount over total unsecured debt ratio, and reasons for financial difficulties. Age, household size, total asset value, total secured debt amount, total unsecured debt amount, and the ratio of proposal amount over total unsecured debt are modelled in bins to capture possible non-linear functional forms. The choice of control variables are guided by the availability and the recent literature on personal bankruptcy. \( \mu_k \) represents a series of fixed effects including liability type fixed effect, single or joint filing fixed effects, repayment schedule type fixed effects, debtor province fixed effects and filing year-by-month fixed effects. \( \epsilon_i \) is an error term. We clustered standard errors at the individual proposal level.

The DID variable of interest is the standard interaction term \( \text{Treat}_i \times \text{Post}_i \). This term captures the change in default of treated individuals relative control individuals in the post-reform period relative to the pre-reform period. As is standard in the Cox literature, we report odds ratios for the coefficients of interest. They show the ratio of the probability of proposal default to the probability of proposal not defaulting (finishing as planned) for a particular covariate. In other words, a reported

\(^{17}\) This set-up, where we include the treatment and post terms separately, as well as the interaction of \( \text{Treat}_i \times \text{Post}_i \), is similar to other studies using duration models to examine the impact of policy shocks part way through a long-term consumer debt contract (e.g., Li, White, and Zhu, 2011).
coefficient that is not significantly different from 1 implies that a particular variable has no effect on proposal default. On the other hand, a finding that an odds ratio term is significantly less than 1 implies that a characteristic (e.g., being treated) does cause a significant reduction in the hazard of default for the group with this characteristic. As is standard, we can define the percentage reduction in the hazard of default for the treated*post observations as 1 – the estimated odds ratio.

Our main prediction in this paper is that the treatment (the exogenous increase in the penalty for default) should reduce default of treated debtors in the post period, relative to all other observations. This prediction implies an estimated odds ratio that is significantly less than 1 for the treated*post observations. In all tables in this paper, we report the odds ratios of the interaction term (treated*post) as well as the treated and post variables separately.

6. Full Sample Results

Our main full sample results are presented in Table 4, column 1. Our first result is based on all proposal filers, who started their proposals before the reform date. The DID coefficient (treated interacted with post) is significant at 1% with an estimated odds ratio of 0.86. This implies that raising the penalty for default reduced the hazard of default on proposal by 14%. This is the main finding of this paper, showing the effectiveness of increasing the penalty of default on reducing the hazard of default for treated debtors.18

In Figure 9, we plot the survival rates from the main Cox proportional hazard model, for each of the four groups in the model (treated and control interacted with pre and post periods). This figure shows that the treated group in the post-reform period has a significantly higher survival rate (i.e., lower default rate) compared with the three other group-periods. In addition, this figure shows that the survival rate for the control group in the pre-period is very similar to the survival rate for the control group in the post period, which is consistent with our identification assumption that the treatment does not have any impact on the control group.

An important assumption of our identification strategy is that there are no unobservable confounding factors affecting higher SI filers but not affecting lower SI filers at the time of the reform. In order to provide further evidence on this assumption, we replicate our main specification, with the exception that, instead of comparing between filers with SI≥$200 (i.e. treated) and filers with SI<$200 (i.e. control) groups, we compare between higher SI and lower SI filers within the control group, and

18 Table A1 in the Appendix provides the coefficients on the large number of control variables in this specification. Most of these control variable coefficients are statistically significant, with magnitudes in the expected direction.
also compare higher SI and lower SI filers within the treatment group. If there are unobservable factors that affect higher (or lower) income individuals in general, which coincide with the reform, then we would expect to find some effects in the post-reform period even within the control group, or within the treatment group. However, a finding of no effect in the post period within the groups, is consistent with no other unobservable factors affecting our sample at the time of the reform.

To conduct these tests, we extend our basic model by examining relationships between the penalty for default and default within different SI bands, based on the various discontinuities in the SI rules described in Figure 1. As shown in Figure 1, all debtors with SI lower than $200, paid the same amount to creditors in both the pre- and post-reform periods. The testable implication of this feature is that there should be no difference in response to the reform across any individuals with SI $< 200$. To test this implication, we define a new pseudo treatment variable equal to 1 for filers with income above the 25th percentile of the SI distribution, which is approximately -$211. In this test therefore, our new “treated” group has SI from -$211 to + $200, while our new “control” group has SI $< -$211. Column 3 of Table 4 shows that this new placebo treatment group has an estimated DID coefficient of 0.9442, which is insignificantly different from 1. This finding is thus consistent with the idea that no individuals with SI $< 200$ were treated by the reform, or impacted by any other unobservable contemporaneous event, thus supporting our identification assumptions.

Similarly, as described in Figure 1, debtors with SI $\geq 200$ are required to pay their creditors 50% of their SI for 9 months before the reform, and 21 months after the reform. Thus, conditional on having SI $\geq 200$, filers with higher SI (i.e., significantly larger than $200$) would face a larger penalty for default compared with filers with lower SI (i.e., above $200$, but not by a large amount) in the post-reform period. To test if this larger incentive to avoid default (intensity of treatment) leads to an additional decrease in default, we restrict the sample to filers with SI $\geq 200$ and define a new treatment variable equal to 1 for individuals with SI at or above the 75th percentile of the SI distribution, which is $900$. All debtors in this specification are impacted by the 2009 reform, but the debtors in our new “treatment” group (SI $\geq 900$) will face a significantly larger penalty for default compared with the debtors in the new “control” group ($200 \leq SI < 900$). Table 4, column 2 shows that the estimated DID coefficient for the new treatment group is 0.88 (significant at 1%). This result implies that debtors with a very high SI at proposal origination ($\geq 900$), who face a significantly higher cost of defaulting on their proposal in the post period, will have a significantly larger reduction in default compared with other treated debtors with a lower penalty for default (with $200 \leq SI < 900$).

Taken together, these tests (in Table 4, columns 2 and 3) indicate that both higher SI and lower SI debtors respond to the regulatory shock in ways that are predicted by the relevant institutions
In the following sections we examine whether observed heterogeneity can provide evidence on possible mechanisms for our main results in section 6. The three main mechanisms we examine include two traditional explanations for mortgage default, i.e., (1) negative home equity, and (2) adverse life events. We also examine a third possible mechanism: proposal seasoning and the age since origination. In all cases, our empirical approach is to either split our main sample into two subsamples (with or without various observable debtor characteristics, or observable contract characteristics), or alternatively, to use a triple difference specification, where we interact the treat*post term with a third term reflecting the observable characteristic. All of these observable characteristics are measured as at the date of the proposal origination, which occurred some period before the event date. This allows us to examine how each of these various observable characteristics interacts with the 2009 exogenous increase in the cost of default as captured by our main DID model.

7.1. House Ownership and House Equity as Mechanisms for Default

Many authors in the mortgage default literature have argued that if the negative equity position (i.e., where house value is lower than mortgage balance) of a mortgage owner contributes to mortgage default, then this can be considered as strategic default.\(^{19}\) This proposition is based on the idea that the extent of equity in the house should not affect the current liquidity position or current ability to pay of the debtor.\(^{20}\) We argue here that this logic is also applicable in our proposal default context, although the institutional setting is somewhat different. In the mortgage default context, the main cost of default

\(^{19}\) See, Foote, Gerardi and Willen (2008); Elul, Souleles, Chomisengphet, Glennon and Hunt (2010); Guiso, Sapienza, and Zingales (2013); Fuster and Willen, (2017); Gerardi, Herkenhoff, Ohanian, Willen, (2017); and Bhutta, Dokko and Shan (2017).

\(^{20}\) Foote and Willen (2018) describe the theoretical foundations for why negative equity should affect default, which is based on various transaction cost frictions in selling the house (e.g., costs of moving, costs of selling). The existence of transaction costs frictions in selling a house implies that the distressed debtor cannot simply and quickly liquidate the house in order to avoid defaulting on the mortgage, thus increasing the likelihood of default.
is that the debtor loses ownership in the house. As described in detail in the previous sections, an implicit cost of default on a proposal is that the debtor typically selects to file for bankruptcy. Importantly, house equity plays a similar role in bankruptcy to that of mortgage default because, in most cases, under bankruptcy the house is also liquidated to repay creditors, thus the default on a proposal often entails the debtor losing the house and non-exempt equity in it.\textsuperscript{21}

Our specific institutional setting and data allow us to make two separate contributions to the empirical literature linking house equity and default, compared to the exiting literature in the mortgage default context. First, while every mortgage holder (by definition) owns a house, in our proposal setting only a fraction of proposal filers (approximately one third in our data) own a house. Thus, under bankruptcy, unlike in the mortgage setting, we can examine variation in house-ownership status between house owners and non-house owners. We can thus provide evidence on the same mechanism as used in the mortgage default literature, but instead of only comparing across the intensive margin (i.e., level of equity conditional on being a house owner), we can, in addition, also compare across the extensive margin (i.e., the default choices of house owners vs. non-house owners).

Second, our tests examining how consumer heterogeneity (in either the extensive margin between owners and non-owners or in the intensive margin regarding negative equity of owners) affect default are effectively examining the interaction between two separate motivations for strategic default.\textsuperscript{22} The first strategic default motivation, as described in our main DID specifications is strategic default as related to the 2009 increase in the cost of default. The second strategic default motivation, as described in this section, is strategic default based on the house ownership or house equity position of proposal filers. Specifically, in this section we test the hypothesis that those treated filers who were affected by the 2009 increase in the cost of default, who in addition also owned a house (or had positive equity in their house), had an even larger motivation to reduce default. To the best of our knowledge no previous research has examined how these two separate motivations for strategic default can interact to affect the default hazard.

\textsuperscript{21} One difference between bankruptcy and mortgage default is the role of homestead exemptions in bankruptcy. In our setup, however, we argue that variation in homestead exemptions across Canadian provinces (which are based on provincial regulations) are independent of the 2009 increase to the cost of default, which was imposed across all provinces in Canada by the Federal Bankruptcy regulator, the OSB.

\textsuperscript{22} By sample design, every debtor in our sample had an origination date that occurs before the 2009 increase in the penalty for default. Thus, the 2009 change is plausibly exogenous with respect to these house related measures. Thus, these two separate incentives to behave strategically: (1) the 2009 increase in the cost of default, and (2) home ownership characteristics as measured at origination, can be examined separately from each other.
In order to provide evidence on whether house ownership or equity serves as an additional motivation for strategic default on a proposal, we exploit our origination dated data, which include very accurate measures of both the current mortgage outstanding, as well as the current market valuation of the house. While the empirical literature on negative equity in the mortgage context is often able to provide data on current mortgage balances outstanding, it often faces a challenge in providing data on the current market valuation of the house (for example this literature often uses local housing market price indices to impute the current market value of a specific debtor’s house). Uniquely, our proposal data on house valuation exploits the fact that Canadian insolvency regulations require the bankruptcy trustee (typically a chartered accountant) to accurately assess the current market valuation of the debtor’s specific house, as at the date of the proposal origination. Bankruptcy trustees in Canada are defined as “officers of the court”, which means that they are impartial between the debtors and creditors involved in the insolvency, and are required to provide an accurate assessment of the current market value of the house.

Our specifications in this section include both simple sample splits, as well as triple difference specifications, where we exploit our data on filers’ homeownership status or home equity position. We predict that treated proposal filers, who in addition, are home owners, will have a greater incentive against defaulting on their proposal, compared with treated proposal filers, but who are not homeowners. The sample split results in Table 5, columns 1 and 2, are consistent with this prediction. Specifically, we find that treated proposal filers who are homeowners have a 24 % (significant at 1%) reduction in default on their proposal in the post period relative to the pre-period, while treated proposal filers who are not homeowners had only a 10% (significant at 1%) reduction in default on their proposal in the post period relative to the pre-period. Recall that in our baseline model above (without any sample splits), we found that the 2009 increase in the penalty for default reduced the default hazard in the post period by 14 %. In other words, this new evidence is consistent with the interaction of a higher penalty for default and the possibility of losing the house leading to a larger reduction in default than the reduction from the higher penalty alone.

Our triple-difference results (in Table 8, column 1) are also consistent with this prediction. The coefficient on treat*post*house indicates that a house owner (who is also in the treatment and post groups) is 16% less likely to default (significant at 5%) compared with a non-house owner (who is also in the treatment and post groups). In other words, even within the treated*post group, house owners who face an additional motivation to reduce their default hazard are significantly more likely to do so, compared with non-house owners.

Our intensive margin sample split tests make essentially the same argument as the extensive margin test above, except that we compare proposal filers with positive house equity against all other
proposal filers (with zero or negative equity). This comparison is motivated by the argument that proposal filers with positive house equity will have an additional incentive to reduce their default hazard because proposal default will cause the loss of that positive equity. Approximately one quarter of proposal filers in our sample have positive equity at the origination date. Our results for these tests are presented in Table 5, columns 3 and 4. These results show that the default hazard for treated debtors, who in addition have positive equity in the house falls by 22% (significant at 1%), while the default hazard for treated debtors who do not have positive equity in the house only falls by only 13% (significant at 1%). These results are thus consistent with the argument that proposal filers with positive equity have an additional incentive to avoid default because they will lose that positive equity if they default on proposal and subsequently lose their equity in bankruptcy.

In the case of the triple-difference specifications however, (Table 8, column (2)), even though the coefficient is of the expected magnitude (less than 1), it is not statistically significant, possibly because of issues related to power and sample size. Our significant triple difference results for the extensive margin test above (house vs. no house), thus emphasize the value of being able to examine both extensive margin as well as intensive margin (extent of equity) measures of home ownership.

7.2. The Adverse Life Event Mechanism for Default

There is a large literature in the mortgage context documenting that idiosyncratic individual level adverse shocks ("adverse life events") are important in explaining mortgage default.\(^{23}\) The argument in this literature is that an adverse shock will negatively affect current liquidity and current ability to pay by the debtor, thus leading to default which is not strategic in nature.\(^{24}\) We argue that even though these arguments were developed in the mortgage default context, they are still applicable in our proposal default context because proposal filers are also subject to these same adverse life events.

To provide evidence on this adverse life event mechanism, we use a similar sample split methodology as in the previous section, i.e., split our main sample into two subsamples based on whether the debtor was or was not subject to an adverse life event (as defined below). Our prediction

\(^{23}\) See, Elul, et al. (2010), Gerardi, Herkenhoff, Ohanian, Willen, (2017), Foote and Willen (2018), Ganong and Noel, (2020 A and B), Scharlemann and Shore (2016), Gerardi, Herkenhoff, Ohanian and Willen (2017), and Indarte (2020).

\(^{24}\) Foote and Willen (2018) describe the theoretical foundation of the adverse life event mechanism, which is based on credit constraint frictions generating liquidity constraints. Because of credit constraint frictions, distressed debtors will not be able to compensate for adverse life events by easily accessing additional unsecured credit, thus causing them to select into default.
is that a debtor subject to an adverse life event will be more liquidity constrained, and will thus be less able to respond strategically to the increase in the cost of default in 2009, by reducing default in the post-reform period. In this setup we are thus examining the interaction between two motivations for default, one of which is strategic (the 2009 increase in the cost of default), and the other which is not strategic (adverse life events reducing liquidity).25

As emphasized by many authors, it is often empirically challenging to identify adverse life events as a motivation for default.26 In their literature survey, Foote and Gerardi (2018) emphasize the usefulness of self-reported data by defaulting debtors on their exposure to various adverse life events, as one way of empirically examining the effect of these shocks.27 They argue however, that the existing literature has often not had access to such self-reported adverse life event data. An important advantage of our data is information provided by debtors at the date they file the proposal, in response to the question “Give reasons for your financial distress”. Among the reasons provided in these data, are several common “adverse life events” used in the existing literature including: (1) loss of income, (2) business failure, (3) health shocks, and (4) relationship breakdown.28

These self-reported data provide us with two important advantages. First, our adverse life event data are linked to all other debtor characteristic data because it forms part of the proposal filing to the OSB. We are thus able to use these measures to examine how adverse life events interact with a higher penalty for default. Second, these data on adverse life events are provided by the debtor at the date of the origination of the proposal contract, which by sample design occurs before the date of the 2009 increase in the costs of default. This implies that these adverse life events measures can be considered independent of the 2009 change in default costs.

As above, we examine how the increased penalty for default affects treated debtors who are, or are not subject to adverse life events, using either sample splits (Tables 6 and 7) or triple difference

25 Our specifications are somewhat similar to the recent “Double Trigger” literature on mortgage default, which also examines the interaction of one strategic and another non-strategic motivation for default (e.g. Elul, et al., 2010; Gerardi, Herkenhoff, Ohanian, Willen, 2017; Foote and Willen, 2018). The key difference is that the strategic motivation in the Double Trigger literature is negative home equity, while in our case the strategic motivation is the increase in the cost of default in 2009. In both our setting and the Double Trigger setting the non-strategic motivation is adverse life events.

26 Gerardi, Herkenhoff, Ohanian, Willen, (2017) argue that, “Measuring a borrower’s ability to pay fundamentally requires detailed, household-level data on borrowers’ economic attributes, including their income, their employment status, and their balance sheet, as well as their mortgage characteristics and payment status. However, previous studies have lacked data on many of these variables, and have either omitted variables from the analysis, or have used regional-level data to proxy for household-level data.” (p. 1099). Similarly, Ganong and Noel (2020B) argue that “it is unclear what qualifies as an adverse life event that is sufficiently important so as to cause a borrower to default” (p. 2).

27 Foote and Gerardi (2018) provide an example of surveys conducted by Freddie Mac (e.g., Cutts and Merrill, 2008) as examples of such self-reported adverse shock data, although such data is typically not linked to other data on the characteristics of individual debtors.

28 Our measures of adverse shocks are somewhat similar to that of Gerardi, Herkenhoff, Ohanian, Willen (2017), who use PSID data to capture previous unemployment shocks, as well as previous health disability shocks.
specifications (Table 8). Table 6 reports a sample split based on whether a debtor was subject to any one of the four adverse shocks we can observe: (1) loss of income, (2) business failure, (3) health shocks, and (4) relationship breakdown. By examining whether a debtor experienced at least one of these four shocks, we can generate a sample where approximately half of the debtors experienced at least one adverse shock, and the other half of debtors did not experience any adverse shocks. The results from this specification are strongly consistent with our prediction. We find that treated debtors, who were not subject to any adverse life event, reduced their default hazard by 19% in response to the increase in the penalty for default. On the other hand, treated debtors, who were subject to at least one of the adverse life events, only reduced their default hazard by 9% after the increased default penalty. Our triple difference results are reported in Table 8 (column 3). The treat*post*any adverse event coefficient indicates that debtors with any adverse event (in addition to being in the treatment and post groups) have a default hazard of 12% larger (significant at 10%) compared with debtors with no adverse event (in addition to being in the treatment and post groups). These results are thus consistent with our argument that adverse life events will reduce current liquidity, and thus reduce the ability of the debtor to avoid default in response to an exogenous increase in the cost of default.

Table 7 also reports similar tests for two frequent adverse life events (income loss, which is experienced by 31% of our sample, and medical shocks, which is experienced by 14% of our sample). In both cases we find that debtors who did not experience the adverse shock had a significant reduction in the default hazard, while for debtors who did experience these adverse shocks, we document no statistically significant reduction in default after the higher default penalty is introduced. The triple-difference versions of these tests are reported in Table 8 (columns 4 and 5). As predicted, the treat*post*income loss coefficient indicates that a debtor with an income related adverse event (in addition to being in the treated and control groups) has a 14% higher default hazard (significant at 5%) compared with a debtor with no income related adverse event (in addition to being in the treated and control groups). In the case of medical adverse events, however, while the magnitude of the treat*post*medical shock coefficient is as expected (larger than 1), the coefficient is not statistically significant, possibly because only 14% of the debtors in our sample face a medical shock.

7.3. How Age of the Proposal Affects Default

The third possible mechanism which we test is based on the age of the proposal – i.e. the length of time from the origination date of the proposal, and the length of time remaining until the successful completion of the proposal. Figure 3 plots the unconditional default rate of proposals relative to the current age of the proposal, and shows that default rates peak when proposals are about one year old,
and then decline monotonically as proposals age to approximately 5 years. Indeed, it is noteworthy from Figure 3 that in the later years of the five-year duration, the marginal default rate for each additional day of duration is close to zero.

This overall shape (default rates peaking at one year and then monotonically declining with age), could reflect three, not necessarily mutually exclusive, causes. Firstly, after default all the preceding payments made to creditors are not recoverable by the debtor, which implies that a debtor who has been making payments for many years on a “seasoned” proposal will have a greater incentive to avoid default (due to sunk cost). Secondly, if debtors are able to successfully reach the completion date of their proposal without defaulting, then they are “discharged” from insolvency, which provides them with many significant legal benefits (e.g., access to cheaper credit). Thus, debtors on “seasoned” proposals, who are closer to their completion date, face a significantly higher incentive to avoid default. Third, this pattern could reflect survivorship bias, such that low quality debtors who have a higher probability of default may already have defaulted in earlier stages of proposal life, which implies a lower default rate for seasoned proposals.

The data in Figure 3 are unconditional default rates, without any reference to the 2009 increase in the cost of default. Our setup, however, allows us to examine how the increase in the cost of default interacts with proposal age or seasoning. Using a similar sample split approach as in the previous sections, we replicate our main DID model, where we split our sample into four separate subsamples based on the filing year (2006, 2007, 2008, and 2009).29 This sample split is based on the idea that the 2009 reform would affect the four cohorts of proposals at different stages of their “lifecycle”. For example, the 2006 cohort would receive the increase in the cost of default toward the end of the lifecycle (in year three), while later cohorts (e.g., the 2009 cohort) receive the higher penalty for default earlier in the life (in the first year).

Table 9 summarizes the results from this specification. We find that the reductions in default from the higher penalty are largest and most statistically significant in the youngest cohort of filers (2009 filing year), and then decline in both magnitude and significance for more seasoned cohorts of filers. The percentage reductions in default from the increased penalty for default are 25% (significant at 1%) for 2009 filers, 17% (significant at 1%) for 2008 filers, but these reductions in default are insignificant for filers in the 2006 and 2007 cohorts.

Our interpretation of these results draws from the findings documented in Figure 3, which shows that the unconditional default rate declines with proposal age and is close to zero for the later stages of proposal life. The insignificant effects of the higher penalty for default for the 2006 and 2007

29 Recall that our data only start in January 1, 2006, when the OSB began using electronic rather than paper filings.
cohorts suggest that penalties for default are not effective for the cohorts in later stages of their life cycle because their default rates already are very low (close to zero). This pattern may occur because debtors remaining in more seasoned cohorts have a strong incentive to avoid default (because of the high sunk costs from many years of previous payments, or being close to successfully completing the proposal contract or having fewer low quality debtors who defaulted already).

On the other hand, our results showing a significant reduction in the default rate for “younger” cohorts of proposals (2008 and 2009 filing years) are consistent with the higher penalty for default affecting default decisions of debtors earlier in the proposal life cycle (when unconditional default rates are higher). Taken these findings together, we can argue that the higher penalty for default interacts with proposal age and is effective in early stages of proposal life. This finding may have implications for designing optimal debt repayment contracts for distressed debtors.

7.4. Credit Supply as a Mechanism

Another possible mechanism for our results is that the reduction in default could be caused by an increase in credit supply to treated debtors. We argue, however, that there are a number of institutional details which would not be consistent with such a mechanism. Most importantly, a proposal filing typically requires the removal of all existing credit cards from the debtor. In addition, any new sources of unsecured credit will need approval from the bankruptcy trustee, who as an “officer of the court” is obligated to act in the interests of both the debtor and the existing creditors. Given the very low credit rating of a proposal filer, any new unsecured credit is likely to be very costly. Such very expensive new credit could thus act against the interests of either the existing creditors, or the debtor herself, which could lead to objections from the bankruptcy trustee. Some evidence consistent with credit supply not being a significant mechanism in the specific context of the 2009 amendments to the BIA, is from Allen and Basiri (2016). They use credit bureau data on credit card limits, and conclude that “the 2009 BIA amendments had no impact on any of the credit limit variables” (p. 29).

8. Robustness Tests

In this section we report on a variety of robustness checks we perform on our main specification from section 6.

8.1. A Narrow Surplus Income Range within Treated and Control Groups
Our main test in section 6 includes all proposal filers with an SI ≥ $200 in the treatment group and all filers with an SI < $200 in the control group. One possible concern with this specification could be the very large range of SI in our treatment and control groups. We can address this concern by limiting our sample to individuals with surplus incomes in a narrower range around the $200 cutoff. To implement this idea, we define our new treatment group as proposal filers having an SI at the origination date in the range of $200 to $800, and defining our control group as proposal filers having an SI in the range of -$400 to $200. The assumption here is that individuals in these restricted groups (SI ranging from -$400 to $800) will be broadly similar in unobservable characteristics. Table 10, column 1, reports results for this new definition of the treatment and control groups. As expected, this restriction reduces the sample size compared to our full specification in section 6 by about half. However, the main DID coefficient equals 0.90 (significant at 1 %), which is only marginally different from our main results in Table 4. This finding suggests that our main results are not being driven by proposal filers in the tails of the surplus income distribution.

8.2. Announcement Effects

While the bankruptcy reform (i.e., changing the calculations for SI under bankruptcy) went into effect on 18 September 2009, this policy was announced by the OSB about a month before the implementation, on 14 August 2009. In order to control for possible announcement effects (i.e., where debtors strategically respond to the announcement of the policy by filing under the old rules in advance of the policy implementation), we re-estimate our main model, with the exception that we remove all filers for the period between the announcement date and the implementation date. The results for this specification are reported in Table 10, column 2. The main DID coefficient is 0.87 (significant at 1%) which is essentially identical to the coefficient in the full sample regression (0.86). This finding implies that the announcement did not lead to changes in the population of proposal filers or their behavior after filing.

8.3. Excluding Amended Proposals

An important element of proposal design is that a debtor is able to propose an amendment to terms of an existing proposal during the course of this contract. If the amendment is accepted by the creditors, the newly amended proposal comes into force, while if the amendment is rejected by creditors then the original proposal remains in force. One possible concern is that an amendment to a proposal contract after the policy shift could be an endogenous response to the policy shift. In our main
specifications above, we include amended proposals, however as a robustness check we exclude proposals that are amended at any stage of the process. We report the results for this sample in Table 10, column 3, and find that the estimated effect of the policy change is 0.85 (significant at 1%), which is essentially identical to the coefficient reported in our main results (0.86). In other words, our main results do not appear to be driven by possible contract amendments.

8.4. Excluding Proposals with SI between $0 and $200

The key element of Canadian bankruptcy law, used in this paper (as displayed in Figure 1), concerns the exogenous shift (i.e., notch) in the cost of bankruptcy at SI = $200 after the 2009 reform. However, Figure 1 also displays a kink in bankruptcy cost at SI = $0. We do not exploit this kink at SI = $0 for our identification strategy because the identical kink is relevant in both the pre- and post-reform periods. One possibility, however, is that control debtors with SI between $0 and $200 (who pay 50% of SI in bankruptcy) may be systematically different than control debtors with SI less than $0 (who pay nothing out of SI in bankruptcy). In order to address this, we re-estimate our main specification, but where we simply remove all debtors with SI between $0 and $200 from the control group, so our control group only consists of debtors to the left of the kink at $0. The results from this specification are reported in Table 10, column 4, and are essentially identical to our main specification. In other words, the inclusion of filers with SI between $0 and $200 into the control group is not driving our results.

8.5. Tighter Event Windows Around the Reform Date

In our full sample results, we include all proposal filers who filed before the September 2009 reform and follow them until their proposals end or they default. This specification implies that our outcome of interest (default) can also occur at any date during this multi-year period. One possible concern with this specification is that because of the long multi-year durations of the pre and post periods, possible unobservable time varying factors could be affecting our comparisons between the pre and post periods.

In order to address this issue, we restrict our models to measure default in the pre and post periods of only a few months before and after the reform date. By examining these very much tighter windows around the policy change date, we argue that the concern over unobservable time-varying factors is less prevalent because the shorter pre and post time windows will have similar macroeconomic environments (e.g., the economic environment in June – August 2009 is similar to October – December 2009). A downside of this approach is the total time in both the pre and post
periods becomes very short. In addition, with these restricted event window specifications, we can provide evidence on how fast the 2009 increase in default penalty affected defaults.

Table 11 reports results for specifications measuring changes in default in 3, 6, 9, and 12 months before and after the reform date. In these specifications, we examine all proposals that started before each event window around the reform date, but left and right censor their duration data to be in the specific event window. By design, we exclude proposals that defaulted before the start of each event window. Our results for the DID coefficient indicate the largest reduction in default for the shortest (3 month) event window. The estimated coefficient is 0.77 which indicates a 23% reduction in default as a result of the increased penalty for default. All other specifications in Table 11 (6 months, 9 months and 12 months) have similar estimates for the DID coefficient of approximately 0.80, and all are statistically significant at 1%. In other words, these results indicate a very significant and swift response by debtors to the increased penalty for default.

8.6. Placebo Tests: September Dates in Different Years

Another possible concern with our baseline specification is whether September has any seasonal effect on proposal default, given that the reform date is September 2009. In order to address this concern, we run placebo type tests where instead of using the actual policy change date (September 2009), we re-estimate our main model using placebo reform dates, in both prior years (September 2007 and September 2008), as well as subsequent years (September 2010, September 2011, September 2012 and September 2013). In order to avoid issues of overlapping event windows, we restrict these specifications to compare default rates in 6 months on either side of these placebo reform dates. Table 12 summarizes results of these tests. The main conclusion from this table is that in no case the estimate DID coefficient for the pseudo reform dates (Septembers in various years) is statistically significant and below 1. In other words, these placebo tests suggest that our main finding is not because of a seasonal effect of September on proposal default, but coming from the September 2009 increase in default penalty.

8.7. Linear Probability Model

In Section 4.4 we described using a Linear Probability model as an additional robustness check of our main Cox proportional hazard results (following Borgschulte et al., 2021), and as a mechanism for providing evidence on the parallel trends assumption. In this section, we provide additional details on how this model is specified. The model takes the following form:
\[ Y_{it} = \beta_0 + \theta \text{Treat}_i \times \delta_t + \beta_1 \text{Treat}_i + \beta_2 \delta_t + \beta_3 \times \text{Controls}_{it} + \mu_i + \epsilon_{it}, \] (2)

where \( Y_{it} \) is an indicator variable for proposal default. We set this variable to 0 if a proposal does not default in a particular month, turn it to 1 in the first month of default and set it to missing in the following months. This feature is designed to capture the dynamic nature of default and the fact that proposals exit the sample after default. The vector \( \theta \) captures the coefficients of interests which are the interactions of monthly dummies \( \delta_t \) (from 12 months before the reform to 12 months after the reform) with the treatment indicator. We omit the dummy for month -1 before the reform (August 2009) to avoid multicollinearity; the monthly coefficient estimates are relative to this omitted period. We also include the treatment indicator and monthly dummy variables by themselves in this model. Finally, the model has a 5\(^{th}\) order polynomial in proposal age (measured in months) to capture the baseline hazard of default and individual fixed effects. We estimate this model on a panel of monthly data. Because individual fixed effects will be perfectly correlated with any time invariant proposal and characteristics (e.g., marital status or single-filer dummy), we exclude these variables from Equation (2). We cluster standard errors at the individual proposal level.

Figure 8 plots the estimates for the interactions of monthly dummy variables with the treatment indicator and their 95% confidence intervals. As we highlighted in Section 4.4, we do not observe any statistically significant differences in default rates between the treatment and control groups in the 11 months before the policy change. This results suggest that, at least prior to the reform, the treatment and control group had similar trends in default rate. After the reform (month 0), we observe statistically significant declines in the probability of proposal default in the treatment group compared with the control group. In particular, the probability of default declines by 0.003, on average, immediately after the policy change (month 1) and stays statistically significant at 5% level until month 12 after the reform. These results using a LPM model are thus consistent with our main results based on Cox proportional hazard models.

9. Conclusion

During financial crises, such as the Great Recession of 2008 and the COVID pandemic of 2020, governments and creditors often implement policies designed to reduce costly default by distressed debtors. These policies typically involve various forms of debt modification (e.g., reducing monthly payments for debtors, offering temporary forbearance) all of which aim to ease the liquidity constraints of debtors. An important implicit assumption inherent in these types of policies is that the debtors are non-strategic defaulters, in that they default because they currently are not able to pay the debt.
However, these debt modification policies may be less effective at reducing costly default if debtors are strategic defaulters, i.e., they default in spite of currently being able to repay their debt.

In this paper, we exploit a very atypical financial crisis policy change, where regulators increased the penalty for default (rather than attempting to reduce default by easing the financial constraints of debtors). We find that increasing the penalty for default reduced default on unsecured debt, which we argue is consistent with some distressed debtors being strategic defaulters. Our results highlight the importance of determining whether defaulting debtors are strategic or non-strategic, when designing policies to reduce default during financial crises.
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Figure 1. 2009 Consumer Insolvency Legislative Change

Notes: The Figure illustrates how the legislative regime shift in 2009 changed the relationship between bankruptcy cost and surplus income. The blue line represents the regime before 2009 and the dotted red line represents the regime after 2009. Our control group consist of debtors with SI < $200 (where the exogenous regulatory shock has no effect, i.e. the blue line is the same as the red line), while our treated group consist of debtors with SI > $200. Our treated*post coefficient in our DID is reflected by the gap between the blue and red lines to the right of the Figure, where SI > $200. These institutional details are described in detail in the text.
Figure 2. Originally Negotiated Proposal Duration (in days)

Notes: This Figure displays the original duration of the proposal agreement, negotiated between the debtor and the creditor(s). The large masses of proposals are around 3 years, 4 years and 5 years of duration.
Figure 3. Probability of Default by Age of Proposal (in days)

Notes: This Figure plots the unconditional default rate by the actual age of proposals as measured in days. This rate is estimated from raw data using our entire sample.
Figure 4. Kaplan-Meier Curves for Treated and Control Groups

Notes: This Figure reports the Kaplan-Meier survival functions for treated and control groups over analysis time (age in days). These functions are estimated from raw data using our entire sample. This figure does not account for the bankruptcy reform date (September 2009) nor the variation in default rates between pre and post periods. The results of that analysis are shown in Figure 9.
Figure 5. Test of Manipulation in Surplus Income at $200

Notes: Graphical Output from tests of manipulation in the running variable (Surplus Income) at $200. This evidence indicates no significant discontinuity at $200.
Figure 6. Actual Number of Proposal filings over time in Pre-Period

Notes: This Figure plots the actual number of proposal filers in the pre-period, which is the data we include in our sample. The red line shows the number of proposals in the treated group and the blue line represents the number of filings in the control group.
Figure 7. Actual Monthly Default Rates: Treated and Control Groups

Notes: This figure plots the raw monthly default rates for the treated group (blue line, SI ≥ $200) and the control group (red line, SI < $200), in the 12 months before and 12 months after the reform date (September 2009). These data measure new defaults recorded each month. These data include all proposals that have not defaulted before the event window. This figure shows that in the pre-reform period, treated and control groups had default rates that trended similarly, but in the post-reform period, the default rate of the treated group fell below that of the control group.
Figure 8. Monthly Default Rates: Linear Probability Model

Notes: This figure plots coefficients from a linear probability model (LPM) of the interactions of event month indicators with the treatment dummy. The omitted group is month -1 before the reform (August 2009). These data include all proposals that have not defaulted before September 2008 (12 months prior to the reform). This figure shows that in the pre-reform period, treated and control groups had default rates that trended similarly, but in the post-reform period, the default rate of the treated group fell below that of the control group.
Figure 9. Survival Functions based on Main Cox Model Results

Notes: This Figure shows the survival functions, as estimated by our main Cox PH model in Equation (1) as reported in Table 4. The main function of interest is the survival function for the treated group in the post-reform period, which is reflected by the green line. The other lines reflect the control group in the pre and post-reform periods (red and orange lines), and the pre-period for the treated group (blue). The green line is higher than the other lines which is indicative of our main conclusion that the treated group in the post-reform period has a significantly higher survival rate (i.e., lower default rate) compared with the control group and the other periods.
Table 1. Summary Statistics

| Variable                          | N       | mean   | median | Std. Dev. |
|-----------------------------------|---------|--------|--------|-----------|
| debtor age                        | 59,506  | 42.17  | 42     | 11.81     |
| household size                    | 59,506  | 2.27   | 2      | 1.37      |
| cash holding                      | 59,506  | 80.43  | 0      | 1,013.19  |
| total asset                       | 59,506  | 77,493.60 | 13,151 | 113,080.10 |
| home property value               | 59,506  | 64,036.35 | 0      | 107,544.70 |
| Share of homeowners               | 59,506  | 0.34   | 0      | 0.47      |
| home equity value if home owner   | 20,039  | 20,259.47 | 10,345 | 35,468.50 |
| total secured debt                | 59,506  | 61,341.85 | 2,200  | 100,246.90 |
| total unsecured debt              | 59,506  | 36,034.82 | 32,466 | 20,817.16 |
| total bankrupt income             | 59,506  | 2,362.49 | 2,289  | 1,029.11  |
| total family income               | 59,506  | 3,003.65 | 2,800  | 1,305.79  |
| total non-discretionary spending  | 59,506  | 97.21  | 0      | 267.36    |
| total available bankrupt income   | 59,506  | 2,375.07 | 2,262  | 1,074.99  |
| total available family income     | 59,506  | 2,906.74 | 2,721  | 1,246.45  |
| total discretionary spending      | 59,506  | 2,826.73 | 2,643  | 1,212.32  |
| payment per period ($)            | 54,124  | 334.49 | 250    | 764.13    |
| total proposal debt               | 56,693  | 14,794.01 | 12,180 | 9,071.62  |
| total proposal debt/unsecured debt| 56,693  | 0.47   | 0.41   | 0.24      |
| Proposal duration (days)          | 59,506  | 1,493.30 | 1,608  | 405.87    |

Table 2. Summary Statistics of Proposal Outcomes

| Proposal Debt Performance        | N   | Percentage |
|----------------------------------|-----|------------|
| pay in full                      | 36,505 | 58.00     |
| default                          | 15,406 | 24.48     |
| amendment and pay in full        | 5,472  | 8.69       |
| creditor rejected                | 2,302  | 3.66       |
| amendment and default            | 2,123  | 3.37       |
| withdraw                         | 1,133  | 1.80       |
Table 3. Length of Time (Days) from Proposal Origination to Status Changes

| Proposal Debt Performance | N   | mean   | median | Std. Dev. |
|---------------------------|-----|--------|--------|-----------|
| default                   |     |        |        |           |
| time-to-default           | 15,406 | 561.61 | 463    | 375.89    |
| amendment and pay in full |     |        |        |           |
| time-to-amendment         | 5,472  | 254.34 | 75     | 393.64    |
| creditor rejected         |     |        |        |           |
| time-to-rejection         | 2,295  | 105.58 | 74.00  | 168.46    |
| amendment and default     |     |        |        |           |
| time-to-amendment         | 2,123  | 164.79 | 76     | 222.50    |
| time-to-default           | 2,123  | 711.52 | 596    | 466.19    |
| withdraw                  |     |        |        |           |
| time-to-withdraw          | 1,133  | 79.20  | 52     | 219.60    |
Table 4. Main Cox Proportional Hazard DID Results

|        | (1) | (2) | (3) |
|--------|-----|-----|-----|
|        | All | Only SI>200 | Only SI<200 |
| Treat  | 1.0425* | 1.0537 | 0.9474* |
|        | (0.02429) | (0.03398) | (0.03063) |
| Post   | 1.0036 | 0.8903*** | 1.0465 |
|        | (0.03212) | (0.03819) | (0.04654) |
| Treat*Post | 0.8596*** | 0.8887*** | 0.9442 |
|        | (0.02694) | (0.03958) | (0.04300) |
| N      | 101,748 | 56,410 | 45,338 |

Notes: For individuals with active proposals after the September 2009 reform, we define two time spans: for the time period before the reform (when $Post_t = 0$), and for the time period after the policy change (when $Post_t = 1$). Our treatment group indicator, $Treat_t$, takes the value of 1 if the proposal filer has surplus income equal to or more than $200, and zero otherwise. The $Treat_t \times Post_t$ term captures the change in default of treated individuals relative control individuals in the post-reform period relative to the pre-reform period. The percentage reduction in the hazard of default is $1 - $ the estimated odds ratio reported here. The results in column 1 are from our main Cox hazard model in Equation (1). Columns 2 and 3 report on specifications that restrict the sample to individuals all of whom are either above or below the $200 surplus income cutoff, as defined in the text. For column 2, the treated group is defined as filers with SI higher than $900, which is the 75th percentile of the SI distribution. For column 3, the treatment group is as filers with SI higher than -$211, which is the 25th percentile of the SI distribution. Control variables and fixed effects are defined in the text. Coefficients for all control variables in Column (1) are reported in Table A1 in the Appendix.
### Table 5. House Ownership and House Equity as Mechanisms

|         | (1) House | (2) No House | (3) Positive Equity | (4) Zero/Negative Equity |
|---------|-----------|--------------|----------------------|--------------------------|
| Treat   | 1.1442*** | 1.0116       | 1.1859***            | 1.0107                   |
|         | (0.05435) | (0.02761)    | (0.06681)            | (0.02616)                |
| Post    | 1.0451    | 0.9963       | 0.9937               | 1.0102                   |
|         | (0.06770) | (0.03716)    | (0.07784)            | (0.03561)                |
| Treat*Post | 0.7582*** | 0.8987***    | 0.7877***            | 0.8750***               |
|         | (0.04620) | (0.03371)    | (0.05783)            | (0.03067)                |
| N       | 36,587    | 65,161       | 26,049               | 75,699                   |

Notes: These specifications are identical to our main specifications from Equation (1) (as reported in column 1 of Table 4), with the exception that we split the sample based on whether the debtor is a house owner or not (columns 1 and 2), or whether the debtor has positive house equity (column 3) or zero or negative equity (column 4).
Table 6. Any Adverse Life Event as Mechanisms

|                | (1)                      | (2)                      |
|----------------|--------------------------|--------------------------|
|                | Any Adverse Event        | No Adverse Events        |
| Treat          | 1.0015                   | 1.0880**                 |
|                | (0.03282)                | (0.03619)                |
| Post           | 0.9690                   | 1.0450                   |
|                | (0.04409)                | (0.04709)                |
| Treat*Post     | 0.9072**                 | 0.8097***                |
|                | (0.04041)                | (0.03582)                |
| N              | 49,168                   | 52,580                   |

Notes: Column (1) presents results for debtors subject to any one of the four types of adverse life event in our data: (1) loss of income, (2) business failure, (3) health shocks, and (4) relationship breakdown. Column 2 shows results for debtors not subject to any adverse event.

Table 7. Specific Adverse Life Events as Mechanisms

|                | (1)                      | (2)                      | (3)                      | (4)                      |
|----------------|--------------------------|--------------------------|--------------------------|--------------------------|
|                | Income Loss              | No Income loss           | Medical Shock            | No Medical Shock         |
| Treat          | 0.9913                   | 1.0684**                 | 1.0692                   | 1.0386                   |
|                | (0.04174)                | (0.03001)                | (0.06500)                | (0.02625)                |
| Post           | 0.9671                   | 1.0254                   | 0.9588                   | 1.0112                   |
|                | (0.05538)                | (0.03958)                | (0.08053)                | (0.03502)                |
| Treat*Post     | 0.9404                   | 0.8211***                | 0.9207                   | 0.8491***                |
|                | (0.05305)                | (0.03105)                | (0.07713)                | (0.02875)                |
| N              | 31,125                   | 70,623                   | 14,031                   | 87,717                   |

Notes: This table reports results for debtors who are or are not subject to specific adverse life events (Income Loss and Medical Shocks). Other adverse events examined in Table 6A are less frequent and generate sample sizes that are too small to be examined separately.
Table 8. Differences in Effects of the Penalty for Default in Subsamples

|                | (1)      | (2)      | (3)      | (4)      | (5)      |
|----------------|----------|----------|----------|----------|----------|
|                | House    | Positive equity | Any Adverse | Income loss | Medical shock |
| Treat*Post*Cut | 0.8382** | 0.8945   | 1.1220*  | 1.1427** | 1.0894   |
|                | (0.05983)| (0.07242)| (0.07022)| (0.07752)| (0.09778)|
| N              | 101,728  | 101,728  | 101,728  | 101,728  | 101,728  |

Notes: This table reports results for the full sample of debtors. The models include the interaction of the DID term (Treat*Post) with a dummy variable for each of the cuts described in Tables 5, 6, 7. These specifications also include all double interactions and individual terms (Treat, Post, Cut). The first row indicates which particular cut is examined in each column. Each column represents one equation with only the triple-interaction coefficient (odds ratio) and its standard error reported. The rest of the specification is the same as in Equation (1). The results in the table suggest that the coefficients for the house split, income loss split and adverse event split are statistically different from each other (at 10 % or 5 % levels).
Table 9. Proposal Seasoning (as Captured by Filing Year) as a Mechanism

|       | (1) 2006 | (2) 2007 | (3) 2008 | (4) 2009 |
|-------|----------|----------|----------|----------|
| Treat | 0.9968   | 1.0393   | 1.0744*  | 1.1741   |
|       | (0.04572)| (0.04069)| (0.04603)| (0.13345)|
| Post  | 1.0471   | 1.0273   | 0.9726   | 0.9553   |
|       | (0.16642)| (0.09256)| (0.06247)| (0.09964)|
| Treat*Post | 0.9769 | 0.9925 | 0.8288*** | 0.7333*** |
|       | (0.12345)| (0.06891)| (0.04584)| (0.08555)|
| N     | 14,023   | 24,257   | 31,424   | 32,044   |

Notes: This table shows results for sample splits based on the year of proposal filing (origination), thus capturing how the age of the proposal as of the September 2009 reform date interacts with the higher penalty for default.
Table 10. Robustness Tests - Various

|                | (1)  | (2)     | (3)     | (4)     |
|----------------|------|---------|---------|---------|
| SI (-400 to 800) |      | Announcement Effect | Not amended | No SI (0 to 200) |
| Treat          | 1.0661** | 1.0424* | 1.0351  | 1.0392  |
|                | (0.03156) | (0.02435) | (0.02555) | (0.02622) |
| Post           | 0.9535  | 0.9987  | 1.0095  | 1.0018  |
|                | (0.04087) | (0.03256) | (0.03414) | (0.03507) |
| Treat*Post     | 0.8964*** | 0.8715*** | 0.8518*** | 0.8563*** |
|                | (0.03782) | (0.02783) | (0.02837) | (0.02868) |
| N              | 55,615 | 97,052 | 89,129 | 91,323 |

Notes: Results in Column 1 are for the sample of debtors with SI in the range of -$400 to $800. Column 2 reports results excluding filers between the announcement date of 14 August 2009 and the policy implementation date of 18 September 2009, thus controlling for a potential announcement effect. Column 3 shows results for a sample of filers excluding proposals that were amended at any stage during the life of the proposal. Results in Column 4 are for the sample of debtors with SI below $0 or above $200.
|                  | (1)       | (2)       | (3)       | (4)       |
|------------------|-----------|-----------|-----------|-----------|
|                  | 3 months  | 6 months  | 9 months  | 12 months |
| Treat            | 1.0661    | 1.0801*   | 1.0668*   | 1.0813**  |
|                  | (0.06829) | (0.04898) | (0.04166) | (0.03964) |
| Post             | 0.9660    | 0.9959    | 1.0588    | 1.0811    |
|                  | (0.08857) | (0.06779) | (0.06565) | (0.06747) |
| Treat*Post       | 0.7681*** | 0.7958*** | 0.7952*** | 0.8067*** |
|                  | (0.06435) | (0.04951) | (0.04541) | (0.04585) |
| N                | 80,997    | 70,893    | 62,725    | 54,202    |

Notes: These tests restrict the observation period when we measure changes in proposal default to only be a few months before and after the reform date. Column titles show the exact number of months included in the pre and post periods in each case.
Table 12. Pseudo Reform Dates – September in Alternative Years (6-month window)

|       | (1)   | (2)   | (3)   | (4)   | (5)   | (6)   |
|-------|-------|-------|-------|-------|-------|-------|
|       | 2007  | 2008  | 2010  | 2011  | 2012  | 2013  |
| Treat | 1.0351| 0.9809| 0.9059*| 0.9913| 0.9488| 0.8590|
|       | (0.08232)| (0.05700)| (0.04841)| (0.07769)| (0.11031)| (0.17376)|
| Post  | 1.1270| 0.9345| 1.0237| 1.0162| 0.9903| 1.6988*|
|       | (0.13320)| (0.07898)| (0.08022)| (0.11780)| (0.17427)| (0.47675)|
| Treat*Post | 0.9135| 1.1052| 0.9871| 0.9380| 0.8787| 0.8183|
|       | (0.09642)| (0.08371)| (0.07149)| (0.09820)| (0.13962)| (0.22592)|
| N     | 21,656| 44,830| 78,327| 61,246| 41,603| 22,964|

Notes: We run placebo tests where instead of using the actual policy change date (September 2009), we re-estimate our 6-month window model using placebo reform dates, in Septembers of the years before the reform (2007 and 2008), as well as subsequent years (2010, 2011, 2012 and 2013). In order to avoid issues of overlapping event windows, we restrict these samples to measure default in 6 months on either side of these placebo dates.
Online Appendix

Table A1. Control Variables from the Main Cox PH Model reported in Table 4, Column (1)

|                          | coef    | se       |
|-------------------------|---------|----------|
| If male                 | 1.2386*** | (0.02143)|
| If married              | 0.7712*** | (0.02018)|
| If age between 30 and 40| 0.9533**  | (0.02224)|
| If age between 40 and 50| 0.9422**  | (0.02218)|
| If age more than 50     | 0.8027*** | (0.02055)|
| If household size 2 or 3| 1.1160*** | (0.02737)|
| If household size more than 3 | 1.2355*** | (0.03985)|
| If asset value between 4K and 16K | 0.8804*** | (0.01855)|
| If asset value between 16K and 160 K | 0.7981*** | (0.02157)|
| If asset value more than 160 K | 0.7543*** | (0.03938)|
| If secured debt between 0 and 120K | 1.0645*** | (0.02176)|
| If secured debt more than 120K | 1.2814*** | (0.06179)|
| If unsecured debt between 20 K and 33 K | 0.9039*** | (0.02028)|
| If unsecured debt between 33K and 50K | 0.8355*** | (0.02169)|
| If unsecured debt more than 50 K | 0.7602*** | (0.02322)|
| If own home              | 0.7796*** | (0.02083)|
| If repayment / debt between 0.32 and 0.41 | 1.0079   | (0.02522)|
| If repayment / debt between 0.41 and 0.55 | 1.1489*** | (0.02962)|
| If repayment / debt above 0.55 | 1.6458*** | (0.04410)|

liability type dummy       Y
single filing dummy         Y
filing reason dummy         Y
province dummy              Y
payment type dummy          Y
filing year*month dummy     Y
Observations                101,748