The Labor Market Consequences of an Increased Default Penalty

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Abstract

We examine whether debtors change their labor market choices to avoid paying higher default penalties. Using a 2009 Canadian reform that increased default penalties on debt repayment contracts, we find that higher default penalties reduce default rates by 14%. Debtors achieve this reduction by increasing employment, holding more jobs, and finding employment more quickly. However, they consequently also accept jobs with lower wages and contribute less to retirement savings. Our findings imply that debtors trade off job match quality for reduced unemployment risk when default becomes more costly. Upon exiting repayment, debtors' wages do not recover, suggesting a long-term scarring effect.

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1 Introduction

Default penalties are central to consumer credit contracts. They regulate how much creditors recover in case of default. They influence the moral hazard faced by borrowers in choosing how hard to work to avoid default. Yet, little is known about the effects of default penalties on outcomes beyond default rates. The existing literature shows that the default rate moves in the opposite direction of the default penalty (Ghent and Kudlyak, 2011; Mayer et al., 2014; O'Malley, 2021; Yannelis, 2020), but it does not study the means by which borrowers avoid default in the face of increased default penalties. Given that labor earnings are the main source of income for most consumer debtors, we examine the labor market channels that they can use to reduce default in response to a higher default penalty: job-seeking behavior, number of jobs worked, labor earnings, and unemployment duration. We further study long-term consequences on these borrowers of their labor market decisions such as retirement savings and long-term trends in employment and wage growth.

To identify the effect of higher default penalties on debtors' labor market activities, we use an unexpected 2009 policy change in Canada that increased the default penalty for some insolvent borrowers based on an arbitrary income-based cut-off. The policy intervention we examine in this study is unusual because it increased the penalty in case of default without affecting the current liquidity or net worth of debtors. This is the opposite of many policy interventions exploited in the literature, which typically increase the current liquidity of debtors (e.g., direct cash payments or debt deferrals) or offer debt forgiveness, while leaving the penalty in case of default unchanged.[1]

Canada, much like the United States, has two forms of consumer insolvency resolution: consumer proposals (a debt repayment plan) and consumer bankruptcies (asset liquidation). Importantly, insolvent borrowers who default on their consumer proposal repayments typically seek protection from creditors through the consumer bankruptcy process. The policy we exploit changes the mandated repayments for bankruptcy filers using an income-based measure. Bankruptcy filers above an arbitrary cutoff for that measure face higher repayments in bankruptcy after the policy. Therefore, the policy effectively increases the penalty in case of default for affected consumer proposal filers. We formulate a difference-in-differences (DID) empirical strategy comparing the changes in labor

[1] Well-studied 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 2008–2009 Financial Crisis see, e.g., Eberly and Krishnamurthy (2014); Agarwal et al. (2017); Scharlemann and Shore (2016); Ganong and Noel (2020); Abel and Fuster (2021); Kaplan et al. (2020); Maturana (2017); Kruger (2018) and Haughwout et al. (2016). Similar policies have been implemented during the COVID-19 pandemic, (e.g., Cherry et al., 2021).

market outcomes and debt performance for affected consumer proposal filers to changes in those outcomes for unaffected proposal filers.

Broadly, we exploit our setting to examine agents' behavior when faced with an increased incentive to remain in a preexisting long-term contract due to an increased cost associated with exiting early from that long-term contract. The 2009 policy increases consumer proposal filers' incentives to remain in their preexisting consumer proposals by increasing the payments required under bankruptcy if they were to default, i.e., exit their consumer proposals early. Importantly, the 2009 policy only changed regulations for new insolvency filings and had no effect on existing proposals or bankruptcies. Specifically, these regulatory changes did not affect any of the terms of the preexisting long-term proposal contract, leaving in-proposal debtors' liquidity and net wealth unchanged, and only increased the expected cost of defaulting on that proposal contract. In addition, the terms of the long-term contract were agreed upon before the policy change, thus, the increased default penalty is independent of any consideration which may have caused either party to initially enter into the proposal contract, which mitigates selection issues for our study.

To empirically study our research question, we use administrative data on all consumer proposals filed in Canada between January 2006 and September 2009 (before the reform was implemented). These data include complete income statements and balance sheets of proposal filers, their demographic information, repayment schedules, and proposal outcomes and their dates (e.g., paid in full, default, amended). These data are linked to comprehensive individual tax records and employment records provided by Statistics Canada. The tax records, crucially, include employer-employee links that we leverage to develop a detailed view of each debtor's employment history.

We first document that the increased default penalty reduces proposal default hazard by 14%. This finding is robust and present only among treated debtors at the time of treatment. The decrease in default is also evident in raw default data and among debtors in narrower income ranges. We also find no evidence of income manipulation to take advantage of the income-based cutoff or any anticipatory effect of the reform. Because the reform occurred during a crisis period, we estimate our effects for narrow time windows and for individuals subject to the same economic conditions, but not exposed to the increased default penalty, and find a robust effect only for those debtors subject to the penalty immediately after it was implemented.

Moving to our main research question, we examine how debtors alter their labor market activities to avoid default. Using employment history, we find that the higher default penalty increases the likelihood of debtors to be employed by 1.3 percent and it increases their number of jobs by up to

3.5 percent. In addition, we find that average earnings decline by $2,000 in response to the higher penalty. Furthermore, treated debtors spend less time in unemployment and find jobs faster. When they switch jobs, their earnings decline even more.[2]

Taken together, these results imply that debtors, in response to a higher default penalty, reassess their risk-return tradeoff decisions in the labor market, lowering their likelihood of unemployment at the cost of lower earnings. An increased penalty in case of default makes any future period of unemployment less tolerable because, if the debtor becomes unemployed, she will not have sufficient cash flow to avoid default and will incur the higher default penalty. Thus, debtors facing higher default penalties have a higher incentive to make labor market choices that avoid (or lower the risk of) unemployment. In our study, to reduce the risk of unemployment, debtors accept jobs faster and work multiple jobs, at the cost of taking on employment that earns them lower income.

We also estimate longer-term effects of facing a higher default penalty. First, we examine the effects on labor outcomes of finishing consumer proposals and no longer being subject to the penalty. We find that upon exiting proposal repayments, the positive effects on employment dissipate, thus, treated debtors revert to their pre-treatment employment levels. However, the negative effects on labor earnings do not revert upon exiting proposals and are persistent for many post-treatment years. Thus, treated debtors may have long-term job "scarring" from being in low-quality job matches. Second, we find negative effects of the increased default penalty on retirement savings, which could have longer-term effects on borrowers. In particular, treated debtors become less likely to make any Registered Retirement Savings Plan (RRSP) contributions.[3] Because saving for retirement can offer long-term benefits and asset accumulation, this negative effect on pension savings can lead to negative long-term consequences.

We contribute to three different strands of the literature. First, our study expands the literature on the effects of default penalties. In general, existing studies argue that the default rate moves in the opposite direction to default penalty. For example, Mayer et al. (2014) documents that reducing default penalty by allowing mortgage holders to receive loan modifications in case of default increases mortgage default (60 days delinquency). Similarly, O'Malley (2021) shows that mortgage default increases when borrowers are not exposed to the risk of repossession. In addition, Yannelis (2020)

[2] These results are consistent with findings in prior job search studies showing that time searching for a job is positively related to wages (Acemoglu and Shimer, 1999; Herkenhoff et al., 2024).

[3] RRSPs are similar to Individual Retirement Accounts in the U.S. as they allow individuals to save for retirement. Contributions to these accounts up to an individual-specific limit are tax-deductible and there is no tax on investment income. RRSP savings can be withdrawn and taxed upon retirement.

argues that an increase in default penalties reduces default on student loans. We contribute to this literature by showing how consumers are able to reduce default through their labor market activities, including labor income, which is the main source of income for them. We focus on labor outcomes because theoretical work suggests that default penalties may increase employment and decrease wages (Donaldson et al., 2019).

Our paper is also related to the literature on the link between consumer credit and debtors' outcomes, including labor outcomes. This literature documents that debt relief leads to improvements in consumer outcomes, such as higher income, greater employment and savings, and better health (Dobbie and Song, 2015, 2020); better credit scores, increased access to credit, and higher homeownership rates (Dobbie et al., 2017); and, persistent long-term effects on labor income, employment, and wealth (Bruze et al., 2024). In addition, credit supply affects labor outcomes through various channels. First, access to credit helps borrowers to spend more time searching for jobs and increases wages (He and le Maire, 2023; Herkenhoff et al., 2024). Second, households increase labor supply to pay mortgages (Bernstein and Koudijs, 2024; Zator, 2025), while foreclosure delays increase income and the probability of job switching (Herkenhoff and Ohanian, 2019; Pal, 2024). In addition, negative home equity reduces labor income and employment (Bernstein, 2021). Our contribution to this literature is that we examine how default penalties, which are important for credit markets and distinct from credit supply and debt relief, affect labor income, employment, job finding, and savings.

Finally, our study adds to the literature on market spillovers from bankruptcy policy changes. This literature shows that pro-creditor policy changes (e.g., the Bankruptcy Abuse Prevention and Consumer Protection Act of 2005 or BAPCPA) decrease the price of future unsecured credit (Gross et al., 2021) and increase the supply of credit (Chakrabarti and Pattison, 2019), but they may also increase mortgage default (Li et al., 2011). On the other hand, Dobbie and Song (2015) shows that debt relief through bankruptcy can benefit consumers. Our study extends this literature beyond the effects of pro-creditor policies on credit markets and documents important spillovers and costs for debtors in their labor market outcomes.

2 Institutional Background

This section provides a brief summary of the institutions we examine in this paper. For a more detailed description, please see Appendix A.1.

2.1 Consumer Insolvency in Canada

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 owed 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 five years.[4] 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. A proposal only becomes a binding legal contract, however, once its terms are accepted by the creditor(s).[5]

Under Canadian insolvency law, a default on a proposal is defined to occur after three monthly payments to creditors are missed.[6] 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) 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. For more detailed information on consumer insolvency in Canada, refer to the Appendix A.1.

2.2 The 2009 Amendments to the Bankruptcy and Insolvency Act

On August 14, 2009, the Office for the Superintendent of Bankruptcy (OSB) announced that it would implement a number of changes to the Bankruptcy and Insolvency Act (BIA). These changes

[4] In this study, we focus on consumer (also known as Division II) proposals, which, before the 2009 bankruptcy reform, were available for debtors with a total debt of less than $75,000, excluding debt on their primary residence. Most consumers file this type of proposal.

[5] 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 is rare. If creditors do not vote on a proposal within 45 days of filing, the proposal is deemed accepted.

[6] Other outcomes after nonpayment that 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).

became law on September 18, 2009, when the amendments to the BIA were passed by Parliament.[7]

The change to the BIA that we exploit involves the number of months that bankruptcy filers have to make payments to creditors. The repayment depends on bankruptcy filers' Surplus Income (SI), which is defined as the bankruptcy filer's income less authorized deductions and expenses. The basic rule requires that bankruptcy filers pay 50% of their SI each month to creditors for a fixed number of months. The 2009 BIA amendment increased the period of repayment from 9 months to 21 months for bankruptcy filers with SI greater than or equal to $200.

The increase in bankruptcy repayment period due to the 2009 policy affected the preexisting proposals we study only through the increased penalty in case of default on a proposal.[8] Unlike other existing studies, in our setting, the policy did not change the liquidity, net worth, or credit supply of the debtor. If proposal filers default on their consumer proposal, to protect themselves from creditors pursuing collection actions, they seek legal protection through some form of consumer insolvency. Most often, proposal defaulters file a consumer bankruptcy. If they do and their SI is $200 or greater, the 2009 BIA increase in bankruptcy repayment increased the penalty in case of default for proposal filers who subsequently file for bankruptcy. Alternatively, proposal defaulters may file another proposal. If they do, crucially, the proposal must be accepted by their creditors, whereas bankruptcies do not require creditors' approval. This key difference effectively means that creditors know that the "floor" for the repayment being offered by the proposal filer is the amount the filer would pay them in bankruptcy. Again, by increasing the repayment in bankruptcy, the 2009 BIA reform increased the penalty in case of default for proposal filers with an SI of $200 or greater who subsequently file another proposal. Therefore, whether proposal filers intend to protect themselves from creditors by filing a bankruptcy or a proposal, as long as their SI is $200 or higher, the 2009 amendment to the BIA increased their penalty in case of default.

In Figure 1, we visually demonstrate the relationship between default penalty and SI. The solid blue line demonstrates the repayment in case of default that debtors in proposals face prior to the 2009 policy change. If a debtor has an SI of $100 and she defaults prior to September 2009, her total repayment is $450 (9 months of paying half of their SI, $9 \times 0.5 \times 100$). If the debtor has an SI of $300, her total repayment is $1,350 ($9 \times 0.5 \times 300$). The dashed blue line demonstrates her default

[7] For additional details, see the following OSB directives `https://www.ic.gc.ca/eic/site/bsf-osb.nsf/eng/br02282.html` and `https://www.ic.gc.ca/eic/site/bsf-osb.nsf/eng/br02230.html`. A full description of the intentions of these amendments to the BIA are provided by Allen and Basiri (2018). We also provide a summary in Appendix A.2.

[8] We examine the effect of the policy change on new proposals in a separate paper (see Mikhed et al., 2024).

repayment after the 2009 policy change. The debtor with an SI of $100 faces the same default repayment, as the increased penalty does not affect her. The debtor with the $300 SI, however, sees her repayment increase to $3,150 (21 months of paying half of her SI, $21 \times 0.5 \times 300$), a relative increase of 133% in default penalty. This increase in default penalties induced by the 2009 policy change for debtors in proposals with SI of $200 or greater is what we exploit to examine the effect of increased penalty for default in our study.

3 Data and Sample

We use data from two sources to study the labor market effects of increased default penalty. First, for data on consumer proposal filers and their filings, we use data provided by the Office of the Superintendent of Bankruptcy (OSB). Second, for annual labor market data, we match the proposal filers to Statistics Canada's Canadian Employer-Employee Dynamics Database (CEEDD), which contains administrative tax data on the filers. In this section, we describe both data sets and explain how we combine them to construct our analysis sample.

3.1 Consumer Proposal Data

Our credit data are provided by the OSB and include all records collected by the regulator for the universe of insolvency filings in Canada.[9] 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 at the date of the 2009 reform. For this reason, our main sample includes proposal filers with filing dates from January 1, 2006, to September 18, 2009, which results in 59,506 proposal filings.

Our proposal data consist of three components: (1) data provided by the debtor at the 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 information. The balance sheet data at proposal filing include consumers' liability structure (i.e., secured

[9] 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 provides 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 et al., 2011; Dobbie and Song, 2015).

debt, unsecured debt, and preferred debt) and asset holdings (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 and non-discretionary and discretionary monthly spending). Demographic data include filers' gender, age, marital status, occupation, household size, and postal code. We also 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 Appendix Figure A1. As can be seen in this histogram, significant proportions of proposals are planned to be completed in three, four, and five years.

Importantly, all consumer insolvency filers in Canada (both bankruptcy and proposal) are required to complete identical forms (OSB forms 65 and 79) when filing for either bankruptcy or a proposal. Because of this requirement, we can compute the hypothetical costs of bankruptcy, including the amounts that would be paid based on SI, for every debtor who files a proposal. We can also accurately calculate the hypothetical increase in the cost of bankruptcy for every proposal filer due to the 2009 policy change simply by multiplying the monthly SI payments the debtor would make in bankruptcy by 21 months instead of by nine months. As we present in Table 2), we also have 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 the proposal filing.

3.2 Tax Data

For our labor market data, we use the Canadian Employer-Employee Dynamics Database (CEEDD) provided by Statistics Canada for the 2001 through 2018 tax years. Using individual-level tax records, Statistics Canada link consumer proposal filers in the OSB data to their annual tax data. Using this linkage, we extract labor market data we need for our study from two CEEDD datasets: the T1 Personal Master File (T1PMF) and the T4 Record of Employment (T4ROE).

The T1PMF provides annual individual-level tax information, as well as individuals' demographic details such as age. We use the annual tax data on individuals to create several of our key labor market measures. For instance, we directly use total labor earnings and Registered Retirement Savings Plan (RRSP) contributions as dependent variables. We also use an indicator for non-zero

labor earnings to measure filers' employment status.

The T4ROE data provides annual tax data for each individual from each employer for which they worked in that year. These data include employment information such as employment income and taxes deducted. Importantly for us, each record also indicates the dates of attachment to and separation from each employer for the individual. If the employee works for the employer the entire year, these dates are just the start and end of each year. However, if the employee is newly hired at the firm or leaves the firm in a given year, that is reflected in the attachment and separation dates reported in T4ROE. We use T4ROE data extensively for a number of variables in our paper, including calculating non-employment spell durations.

We also use T4ROE data to calculate the number of jobs held in a given quarter and identify job switching. To calculate the number of jobs held in a quarter, using the quarter of attachment and separation provided for every employer in a given year for an individual, we tally the number of quarters worked across all employers by an individual to arrive at the number of jobs held in that quarter. We identify job switching based on whether an individual's highest wage-paying employer changes from one month to the next and aggregate to the year as necessary for each analysis. In this way, we identify all years in which an individual changes their main employer.

3.3 Sample Construction

We construct our analysis sample by linking consumer proposal filers to their annual tax data. As the tax data are highly confidential, the linking is done by Statistics Canada. We provide the consumer proposal filers' data to Statistics Canada, which uses the full name and birth date of the filer and the six-character postal code of the filer's residence as of their proposal filing to match them to their administrative tax records. An important advantage in linking OSB bankruptcy data with CEEDD tax data is that both contain the exact legal name of each individual. In addition, Canadian six-character postal codes are exceedingly small, with the median postal code containing 13 households. As such, it is highly unlikely that there will be two people with the same name and date of birth in the same postal code at the same time. Our match rate between these two databases is 89.1%. After the matching, Statistics Canada removed all personally identifiable information and provided us with anonymized data for our analyses.

4 Empirical Methodology

We employ two empirical methodologies for our analyses in this paper. In this section, we explain each methodology in detail. First, we explain the Cox proportional hazard model that we use to study default rates and job searches. Second, we explain the dynamic DID model that we use to study various labor market outcomes.

4.1 Cox Proportional Hazard Model

Key institutional elements in our empirical setup are that proposals: (1) are originated at various dates over multiple years, (2) have different maturities, (3) have different end dates, and (4) have different default dates (if default is present). In addition, the September 2009 policy change would affect proposals at various stages of their lives. We present the distribution of the durations of proposals in Appendix Figure A1 and unconditional hazard function of default in Appendix Figure A2. The hazard of default peaks at about 12 months after the proposal origination and declines steadily over time, a pattern consistent with prior evidence on long-term debt contracts such as mortgages (Keys et al., 2010; Li et al., 2011). We also plot Kaplan–Meier survival functions for treated and control groups in Appendix Figure A3, showing higher survival rates (less default) for treated proposals that have higher surplus income.

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). We include a standard DID model within 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 (SI $\geq$ \$200 debtors) and control group (SI $<$ \$200 debtors) before and after the legislation change.

The Cox proportional hazard specification is given as follows:

$$h_i(t) = \gamma_0(t) \times \exp\begin{pmatrix} \gamma_t \times Post_t + \gamma_{SI} \times Treat_i \\ +\gamma_m \times Treat_i \times Post_t \\ +\gamma_k \times Controls_i + \mu_k + \varepsilon_i \end{pmatrix}, \tag{1}$$

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. $Post_t$

is a time-variant variable taking the value of 1 in the period after the reform, and zero otherwise. Our setup follows a 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 $Post_t = 0$), and for the time period after the date of the policy change (when $Post_t = 1$). 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, $Treat_i$, takes the value of 1 if the proposal filer has $SI \geq \$200$, and zero otherwise.[10]

$Controls_i$ are proposal and debtor characteristics including gender, age, marital status, household size, total asset value, total secured debt amount, total unsecured debt amount, homeownership, 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 modeled in bins to capture possible nonlinear functional forms. The choice of control variables is guided by availability and the recent literature on personal bankruptcy. μ_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. ε_i is an error term. We cluster standard errors at the individual proposal level.

The DID variable of interest is the standard interaction term $Treat_i \times Post_t$. This term captures the change in default hazard of treated individuals relative to control individuals in the post-reform period relative to the pre-reform period. As is standard in the literature, we report odds ratios for the coefficients of interest. They show the ratio of the probability of proposal default to the probability of the proposal not defaulting (finishing as planned) for a particular covariate. In other words, a reported 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) causes a significant reduction in the hazard of default for the group with this characteristic. We can define the percentage reduction in the hazard of default for the treated filings in the post-reform period as 1 less the estimated odds ratio.

We model the hazard of exit from non-employment in the same way as default, based on Equation (1), with two adjustments. First, we use monthly data on non-employment duration based

[10] This setup, where we include the treatment and post terms separately, as well as the interaction of $Treat_i \times Post_t$, is similar to other studies using duration models to examine the impact of policy shocks partway through a long-term consumer debt contract (e.g., Li et al., 2011).

on the matched employer-employee data. This is due to the level of granularity available from the CEEDD T4ROE data regarding dates of job attachments and separations. Second, we do not split non-employment spells into two time spans, but designate those spells which are ongoing in the post-reform period as "post" spells irrespective of whether they started in the pre- or post-reform period. This modification allows us to increase statistical power. Note that the non-employment duration data are different from proposal duration data as there is no pre-planned end date in non-employment and non-employment spells for our debtors, who filed their proposals pre-reform, can originate in the post-reform period (e.g., someone being fired after the reform). We include such non-employment spells for our debtors as they may be affected by the increased default penalty.

4.2 Dynamic DID Regression Model

Our dynamic DID regression specification for the probability of employment, job counts, earnings, and pension contribution indicators is:

$$Y_{it} = f\left(\sum_{\substack{\tau=2006 \\ \tau\neq 2008}}^{2018} \gamma_m^t \times \mathbb{1}(Year_t = \tau) \times Treat_i + \mu_{prov} + \mu_{type} + \mu_{taxyr} + \mu_{filemo} + \mu_i\right), \qquad (2)$$

where $f(x)$ varies based on the form of the dependent variable (e.g., amount, count, or binary indicator).[11] $Year_t$ is a variable indicating the calendar year for the observation. $Treat_i$ is the treatment indicator, taking a value of 1 if the debtor (i) has $SI \geq \$200$. μ_{prov}, μ_{type}, μ_{taxyr}, μ_{filemo}, and μ_i are fixed effects for the debtor's province, the filing type, calendar year, the proposal's year-month of filing, and the debtor, respectively. We cluster standard errors at the debtor level (i).

We can measure the number of jobs at the quarterly frequency, thus, in these regressions, we replace $Year_t$ and μ_{taxyr} with a year-quarter indicator and year-quarter fixed effects, respectively. Finally, for our regressions with triple interactions (where we interact the treated indicator with year dummies and another indicator such as a dummy for job switching), we use the same specification as in Equation 2, but include all double interactions and non-interacted terms which are not absorbed by individual and other fixed effects.

[11] For amount outcomes such as income, we use OLS regressions, so $f(x) = x$ for regressions with that type of dependent variable. For count and binary indicator outcomes such as number of jobs and employment status, we use Poisson regressions (Greenland, 2004; Zou, 2004), so $f(x) = \exp(x)/(1+\exp(x))$ for regressions with those types of dependent variables.

5 Results

In this section, we present the effects of higher default penalties on debtors' likelihood of default and their labor market outcomes.

5.1 Proposal Default

We present our default results in Table 4. Column (1) of this table presents our results for all proposal filers. The DID coefficient ($Treat_i \times Post_t$) is significant at 1% with an estimated odds ratio of 0.86. This estimate implies that raising the penalty for default reduced the likelihood of default on proposal by 14%.[12]

Appendix Figure A4, panel (a), presents additional results for narrower SI ranges. Since assignment into the treatment and control groups is based on SI at the time of proposal filing, there is a possibility of measurement error near the \$200 cutoff at the time of the reform. This measurement error would bias our estimates to zero because true treated individuals may be misclassified as controls and vice versa. To account for this measurement error, we exclude observations with $SI \in (\$100, \$300)$ from the sample and define new treatment and control groups as $SI \in (\$300, \$300 + I)$ and $SI \in (\$100 - I, \$100)$, respectively. We vary the bandwidth parameter I from \$100 to \$1,000 in increments of \$100. As the bandwidth decreases, the sample size becomes smaller and the standard errors increase. Nevertheless, even with the narrowest bandwidth of \$100, the estimated effect remains statistically significant, and the magnitudes across all bandwidths are similar.

In Figure 2, we plot the survival rates from the main Cox proportional hazard model for each of the four groups in the model (treated and control groups in the pre- and post-reform 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-reform period is very similar to the survival rate for the control group in the post-reform 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 debtors but not affecting lower SI debtors at the time of the

[12] Appendix Table A1 provides the coefficients on the large number of control variables in this specification. Most of these control variable coefficients are statistically significant, with signs in the expected direction.

reform. For example, it is possible to argue that higher SI individuals default less in a crisis compared with lower SI individuals because they have higher capacity to repay their debts. To test this hypothesis, we use variation in SI inside the control group (SI < \$200). By definition, this group is not affected by the policy change, but it still has individuals with higher and lower incomes. If our results are driven by an unobservable confounding factor (e.g., the financial crisis), we would expect higher SI individuals in the control group to default less than lower SI individuals in the control group.

To test this hypothesis, we replicate our main specification but, instead of comparing between debtors in proposals with SI $\geq$ \$200 (i.e., treated) and those with SI < \$200 (i.e., control), we compare between higher and lower SI debtors within the control group. We define a new placebo treatment variable equal to 1 for debtors 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 2 of Table 4 shows that this new placebo treatment group has an estimated DID coefficient of 0.9442, which is not significantly different from 1. This finding is consistent with the idea that no individuals with an SI < \$200 were treated by the reform or affected by any other unobservable contemporaneous event.[13]

In addition, as described in Figure 1, debtors with an SI $\geq$ \$200 are required to pay their creditors 50% of their SI for nine months before the reform, and 21 months after the reform. Thus, conditional on having an SI $\geq$ \$200, debtors with a higher SI (i.e., significantly larger than \$200) would face a larger penalty for default compared with debtors with a 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 debtors in proposals with an SI $\geq$ \$200 and define a new treatment variable equal to 1 for individuals with an SI at or above the 75th percentile of the SI distribution, which is approximately \$900. All debtors in this specification are affected by the 2009 reform, but the debtors in the 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 3, 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 (SI $\geq$ \$900), who face a significantly higher cost of defaulting

[13] We run tests with alternative pseudo-cutoffs in the negative SI zone to further test whether some unobservable confounding factor affects higher SI debtors' default rates more. As reported in Appendix Figure A4, panel (b), all estimates for these pseudo-cutoff tests are statistically insignificant, which does not support the hypothesis that our main findings are driven by higher SI individuals defaulting less than low SI debtors due to some confounding factor.

on their proposal in the post-reform period, will have a significantly larger reduction in default compared with other treated debtors with a lower penalty for default.

Taken together, these tests (in Table 4, columns 2 and 3) indicate that both lower SI and higher SI debtors respond to the policy change in ways that are predicted by the relevant institutions. Thus, it does not appear as if there are other unobservable factors systematically affecting how higher SI debtors, relative to lower SI debtors, respond to the reform.

We provide additional results in Appendix B.1. They show that there is no evidence of income manipulation to take advantage of the $200 cutoff before the reform, as expected. These results also show that the numbers of proposals in the treated and control groups evolve similarly right up to the passing of the reform. We also estimate the effect of the default penalty on default using a linear probability model and find a decrease in default immediately after the reform (Appendix B.2). Finally, Appendix B.3 presents additional robustness checks, which show that our results are robust to excluding the announcement period and amended proposals, and focusing on narrower event windows.

5.2 Employment and Earnings

In the previous section, we showed that consumers who anticipate higher default costs are less likely to default. In this section, we examine labor outcomes of these consumers to understand the means by which they reduce default. Using tax filing data, we examine how individuals facing higher expected default penalties adjust their labor supply, wage earnings, and job search behavior during periods of non-employment.

5.2.1 Conceptual Framework for Labor Market Effects

In this section, we provide a simple framework to explain how the increased default penalty may affect debtors' labor market choices. The literature has previously argued that insufficient cash flow is the key driver of default (e.g., Ganong and Noel, 2023, show that negative life events that affect borrowers' cash flow are responsible for 94% of mortgage defaults). Since labor income is the main source of income for consumer borrowers, fluctuation in labor income, especially a severe and prolonged drop in labor income such as unemployment, is the main driver of cash flow-induced default. Because the increased default penalty makes default more costly, we argue that, to avoid default, debtors would be further incentivized to avoid loss of income.

There are two main ways for debtors to avoid losing income. First, they may become more

likely to stay in their existing jobs, even with lower earnings, compared with individuals without this incentive, who may be more likely to switch jobs when their wages decline. Indeed, Ganong et al. (2025) show that workers experience substantial fluctuations in earnings even without switching jobs. Second, in case of job separations, borrowers with the higher default penalty may have incentive to find new jobs faster and avoid cash flow default. A large literature has shown that a faster job search often results in lower wages (Acemoglu and Shimer, 1999; Herkenhoff et al., 2024).

Based on this conceptual framework, we consider three labor outcomes together: (1) the probability of being employed, (2) the duration of job search (for those workers who separate from their previous employers), and (3) labor earnings. Our prediction is that the higher default penalty would increase the probability of being employed and it would decrease the duration of job search and wages. We also explore additional related outcomes, such as wages of those debtors who switch jobs and the number of jobs held by debtors (to account for working additional jobs and hours).

5.2.2 Employment

Borrowers facing a higher penalty in case of default may become more likely to stay employed in order to continue repaying their debts. In this section, we test this hypothesis and find that, after the 2009 policy change, debtors facing increased default penalties indeed become more likely to stay employed.

We examine the effect of increased default penalties on employment using a Poisson dynamic difference-in-differences (DID) specification as outlined in Equation 2. We limit our sample to debtors in proposals for the years while they are in proposal. To measure employment, we use an indicator for whether an individual reports positive earnings.

In Figure 3, we visually present each of the annual treatment effects of the increased default penalty (γ_m^t). In the four years immediately following the reform (2009 through 2012), affected debtors are approximately 1.1 percent more likely to be employed, relative to unaffected debtors, with the effects being statistically significant for the years 2009 through 2011. In later years (2013 and 2014), the effects on employment weaken, both in magnitude and statistical significance. The non-omitted years prior to the reform (2006 and 2007) show no treatment effects, providing support for the parallel trends assumption for our DID analysis. Our findings confirm that, when faced with a higher penalty in case of default, borrowers become more likely to be employed.

5.2.3 Number of Jobs

If debtors who face higher default penalties become more likely to stay employed, we should also observe that the number of jobs held by affected debtors in our tests should increase. Considered in a moral hazard framework, higher debt penalties may also increase effort supplied (e.g., hours worked) by individuals. As a job count measure may (noisily) reflect effort supplied by debtors, an increase in jobs held among those facing higher default penalties would also be consistent with affected debtors working harder after the policy change. Consistent with our employment results, we find that debtors who face increased default penalties after the 2009 policy change increase the number of jobs they hold compared with their unaffected counterparts.

As with our employment analysis, we perform this analysis using a Poisson DID specification as outlined in Equation 2 on all debtors for the years they are in proposals. Using data in the CEEDD on the months of attachment and separation for each employer for an individual, we build a quarterly panel of number of jobs held by each debtor.[14]

As we report in Figure 4, we observe that affected borrowers hold approximately 2 to 5% more jobs in the first 3.5 years (14 quarters) after the policy change. This increase in job count is most statistically significant in the second year, though it shows statistical significance in other post-policy quarters as well. In the later post-policy quarters, this increase effectively disappears. Importantly, in the non-omitted quarters prior to 2009Q3, there are no pre-treatment effects, which supports the viability of our DID specification. Overall, these job count findings further support the conclusion that borrowers increase their employment (and work input) when they face higher default penalties.

5.2.4 Non-employment Duration

If higher default penalties cause borrowers to increase their employment, these borrowers may regain employment quicker when they lose a job. As such, time spent in non-employment should fall for debtors in proposals affected by the 2009 policy change. In this section, we test this hypothesis and find that, following the policy change, affected debtors' unemployment spells become relatively shorter than those of unaffected debtors.

To study debtors' non-employment durations, we employ the Cox proportional hazards model specified in Equation 1 to estimate the effect of the 2009 policy on the job match hazard rates for their non-employment spells. We consider all proposal debtors and include their non-employment

[14] We build a monthly panel based on these data and run our analysis on that panel, as well. While noisier, the results from the monthly panel are qualitatively the same as the quarterly panel findings reported here.

spells in our analyses.

In Table 5, we present the findings of our Cox proportional hazards model. The first column runs the estimation on all non-employment spells within five years of the policy change, while the second column runs it on all spells started during the proposal repayment period. In addition, column 3 presents results when we include all spells of proposal debtors. The coefficients on our variable of interest, Treat × Post show that, after the policy change, debtors affected by the policy become 5% to 10% more likely to end a non-employment spell by finding a job in a given month than unaffected debtors.

Figure 5 presents the survival curves for all four groups in our data (treatment and control group debtors in the pre- and post-reform periods) using the model specified in column 1 of Table 5. The figure shows that pre-policy non-employment spells for treatment and control group debtors are almost identical, which is consistent with our identifying assumption that debtors in the treatment and control group are similar in terms of job finding in the absence of the policy change. After the policy implementation, non-employment spells for treated debtors seem much more likely to end faster than for untreated debtors, as shown by the markedly lower path of the dashed green line than of the dotted orange line. These survival curves for debtors' non-employment spells give support to the hypothesis that unemployed debtors affected by the policy find jobs faster than their unaffected counterparts.

5.2.5 Wage Earnings

If borrowers who face higher default penalties hold more jobs and, generally, become more likely to be employed, holding everything else fixed, they should increase their income. However, everything else may not remain fixed, as these borrowers may stay employed in a job by accepting worse terms (e.g., lower wages). Furthermore, as these borrowers spend less time searching for jobs during non-employment, they may also find worse jobs and earn less income (Acemoglu and Shimer, 1999). In this section, we analyze the wage earnings of debtors in proposals and find that, following the policy change, affected debtors have lower total wage earnings than unaffected debtors.

We study the wage earnings of borrowers in proposals using the DID specification outlined in Equation 2. We run two analyses for wage earnings. First, we study the overall effect of the policy on affected borrowers using the baseline specification. Second, we study the policy's effect specifically on borrowers who switch jobs, using the triple difference modification to the baseline specification described in Section 4.2. The job switching indicator is equal to 1 in the year an individual switches

their primary (highest paying) employer, and it is equal to 0 otherwise. The data samples used for both analyses are in-proposal years for all borrowers.

We report our findings on the effect on total wage earnings of increased default penalties for affected debtors and debtors who switched jobs in Figures 6 and 7, respectively. Examining the overall effect reported in Figure 6, we find that the annual earnings of borrowers affected by the policy fall by $2,000.[15] Studying the dynamics of this effect, we find that there is an immediate drop of approximately $1,500 from 2009 to 2012. In later years, the effect deepens, with affected borrowers earning $3,000 less annually than unaffected borrowers in 2014. Finally, as in previous analyses, in the non-omitted years prior to 2009, we find no pre-treatment effects.

In Figure 7, we study the effect of the policy separately for affected debtors based on when they switch jobs. Panel (a) focuses on their income when they do not switch jobs. The estimates indicate that the higher default penalty reduces annual wage earnings by approximately $1,500 in this group. Consistent with Ganong et al. (2025), this finding suggests that even in the absence of job transitions, individuals can experience substantial variation in earnings. This could be driven by changes in work arrangements such as shift assignments or scheduled hours. Following the increase in default penalties, treated individuals face stronger incentives to avoid default and a higher demand for stable cash flows. This increased liquidity need may weaken workers' bargaining power with employers, leading to a decline in earnings even without a change in employer.

In panel (b) of Figure 7, which studies the additional effect of a job switch, we find that debtors affected by the policy change receive even lower earnings when they switch jobs. On average, the earnings of affected debtors fall by an additional $1,500 in years that they change jobs, with the reduction being statistically significant from 2010 to 2013. The effect tends to increase over time, with the 2009 drop for job switchers being the smallest in magnitude and the 2014 drop being the largest (with 2010 as a large negative dip in the overall pattern). Overall, these findings support the conclusion that borrowers who face increased default penalties accept employment in which they receive lower wage earnings.

[15] While we focus on total wage earnings here, we report the results for total income in Appendix Figure A5. Total income is defined in the CEEDD as wage earnings plus all other taxable income sources. Our results for total income also show that treated borrowers earn significantly less income after the policy.

5.3 Retirement Savings Contributions

In the previous section, we examined how an increase in default penalties may lead to lower wages. In this section, we study households' saving behavior through contributions to Registered Retirement Savings Plans (RRSPs). RRSPs allow individuals to contribute to savings for retirement with taxation deferred until funds are withdrawn upon retirement. Within an RRSP, individuals may invest in various financial instruments much like Individual Retirement Accounts (IRAs) in the U.S.[16] Importantly, RRSP contributions are reported in annual tax filings, which enables us to observe households' retirement saving decisions on a yearly basis. Reducing retirement savings may allow debtors to relax their short-term budget constraint and continue repaying debts to creditors on time under proposals. But, foregoing retirement savings to avoid default may be an especially costly way to reduce default, as reducing these savings would cause debtors to lose investment returns on these tax-advantaged savings that accumulate over time.

To estimate the effect of the default penalty on retirement contributions, we use the regression model in Equation 2 with a new dependent variable which is equal to one if the debtor made a positive RRSP contribution in that calendar year and zero otherwise. As we report in Figure 8, the higher default penalty reduces the likelihood of retirement contributions by 8 percent, with coefficients becoming statistically significant in 2012 through 2014. The delayed response may arise because treated debtors may initially rely on other methods to avoid default (such as adjusting employment and wages) before they eventually reduce retirement savings. This reduction in retirement savings represents a significant and long-term cost to debtors of the increased default penalty.

5.4 Employment and Wage Earnings after Proposal Completion

In this section, we examine how employment and wage earnings evolve following the completion of a consumer proposal. Proposal completion offers at least two benefits: (1) all remaining debts that were included in the proposal are forgiven upon its successful completion and (2) the debtor is no longer subject to the default penalty and may adjust their labor supply decisions. However, having a prolonged period of low-quality job match may have detrimental effects on debtor's long-term employment and wages.

To examine these outcomes, we define a new indicator variable, *Post-Proposal*, which equals to 1 in the years following proposal completion and 0 otherwise. Using data on all proposal debtors

[16] RRSP contributions are capped at an individual-specific limit. All Canadian residents for tax purposes are eligible to open an RRSP account.

and all their tax years (2006-2014), we interact this indicator with the treatment indicator and year indicators in a triple-difference specification, as described in Section 4.2. The resulting triple interaction term captures the effect of exiting a proposal on labor market outcomes for treated individuals, relative to the control group, in each year before and after the 2009 policy change. As proposals have different start and end dates, the composition of this sample will change over time with very few proposals ending in 2006 and all of them ending by 2014 (because even 5-year pre-September 2009 proposals would end in September 2014).

We show results from this specification on employment in panel (a) of Figure 9. Across all post-2009 years, the point estimates are negative but statistically indistinguishable from zero.[17] Overall, we find no evidence that employment status among treated debtors changes following the completion of their consumer proposals. Recall that, in Figure 3, we showed a significant positive effect in early years (2009 through 2011) of the increased penalty for debtors still in proposal. That effect on debtors in proposal dissipated in later years. In combination, our findings for in-proposal and post-proposal debtors show that the positive effect of the increased default penalty on employment is transitory and disappears in the long run.

Next, we examine whether wage earnings exhibit any reversal in the post-proposal period. The empirical specification is the same as that used for employment, with annual wage earnings as the dependent variable. The results are presented in panel (b) of Figure 9. The effect of the increased default penalty on wages in the post-proposal period is close to zero and statistically insignificant across all post-proposal years. In Figure 6, focusing on the within-proposal period, we document a decline in average annual wage earnings among individuals in the treatment group. In contrast, we find no evidence of a recovery in wage earnings following proposal completion. That is, treated individuals do not experience an increase in earnings after exiting the proposal. These findings are suggestive of a scarring effect from a low-quality job match for treated debtors in proposals: even after their proposal debt repayment ends and they are no longer subject to the default penalty, these debtors are not able to increase their wages.

5.5 Back-of-the-Envelope Cost and Benefit Analysis

In this section, we develop a simple cost-benefit framework to quantify the overall effect of the default penalty on creditors' gains and borrowers' losses.

[17] The wide confidence interval in 2006 reflects the small number of consumer proposals that both began and ended in that year.

To quantify the benefits to creditors from the higher default penalties, we estimate how much money they receive as a result of the reduced default rate in the treated group. To estimate this amount, we need the proportion of scheduled payments completed before default occurs. While we observe this proportion for the affected individuals after the treatment (with the increased default penalties), this statistic is not observable for this group in the absence of the policy. Thus, we rely on the control group's repayment proportion to recover this benchmark. Based on the control group proposals, we compute the repayment share at default as the ratio of payment duration to planned proposal length. Among defaulted proposals, the average repayment share is 37.9%.

Next, we combine the repayment share with observed repayment schedules for filers in the treated group to estimate creditor gains from reduced default. Among the treated proposals, the average total scheduled repayment is $18,319. Given the estimated repayment share at default, a debtor who defaults repays approximately 37.9% of this amount, implying that a non-defaulting debtor pays an additional $11,374 ((1-37.9%)× $18,319=$11,374) relative to a defaulting one. Using our estimated 14 percent reduction in default odds over the loan life, we calculate that higher default penalties reduce the default probability by 4.07 percentage points.[18] This implies an expected increase in repayments to creditors of 4.07% × $11,374 = $463 per treated debtor.

Switching to debtor losses, our analysis in Section 5.2.5 shows that the increased default penalty reduces annual wages by $2,000, on average. Given that the average duration of a consumer proposal in the post-reform period is 2.8 years, this implies a cumulative income loss of approximately $5,600 per affected debtors, even without accounting for whether earnings recover to pre-proposal levels after proposal completion. This cost to the debtor is substantially larger than the expected creditors' gain. While a full general equilibrium welfare analysis is beyond the scope of this paper, the stark difference between debtor income losses and creditor gains suggests the presence of inefficiencies induced by higher default penalties.

6 Conclusion

Default penalties are a key element of consumer credit markets. However, while the literature has documented the inverse relationship between default penalties and the default rate (e.g., that an

[18] Assuming that the estimated 14% reduction in default odds applies uniformly over the loan life, the cumulative default probability for the treated group at horizon T is $F_{\text{treat}}(T) = 1 - \big(1 - F_{\text{cf}}(T)\big)^{0.86}$. Substituting $F_{\text{cf}}(T) = 0.3531$ yields $F_{\text{treat}}(T) = 1 - (1 - 0.3531)^{0.86} = 0.3124$. The implied change in the cumulative default probability is therefore $\Delta F = F_{\text{treat}}(T) - F_{\text{cf}}(T) = 0.3124 - 0.3531 = -0.0407$, corresponding to a 4.07 percentage point reduction in default from the increased default penalty.

increase in the default penalty will reduce the default rate by debtors), it has not examined the means by which debtors are able to reduce default. Given the importance of labor income to most consumer debtors, the contribution of this paper is to provide new evidence on how an increase in the penalty for default changes labor market choices of debtors.

We document that increasing the cost of default reduces default risk by 14%. This finding is consistent with the literature showing an inverse relationship between the cost of default and the default rate. We then examine the labor market mechanisms that allow debtors to reduce their default rate, which is the main contribution of this paper. Our main finding is that debtors respond to an increase in the default penalty by accepting lower compensation in exchange for a reduced risk of unemployment. The basic intuition is that unemployment, by reducing cash flow from wage earnings to zero, increases the probability of defaulting on the debt and of the debtor incurring the default penalty. When the default penalty increases, the debtor faces a stronger incentive to reduce unemployment risk. In this paper, we document that debtors reduce their unemployment risk by accepting jobs that have lower compensation.

Specifically, we find that debtors facing an increase in the cost of default: (1) become more likely to be employed, (2) hold more jobs, and (3) find new employment more quickly. All of these findings are consistent with debtors attempting to lower the risk of unemployment. However, these debtors accept a trade-off in their labor market choices. In exchange for lowering their risk of unemployment, they accept jobs that pay lower wages.

We also document long-term effects of being subject to the increased default penalty. First, affected debtors become less likely to contribute to retirement savings, which can have negative long-term consequences. Second, upon exiting proposal repayment, affected debtors do not experience a recovery in their labor earnings, consistent with a long-term scarring effect from a low-quality job match while in proposal.

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Figures and Tables

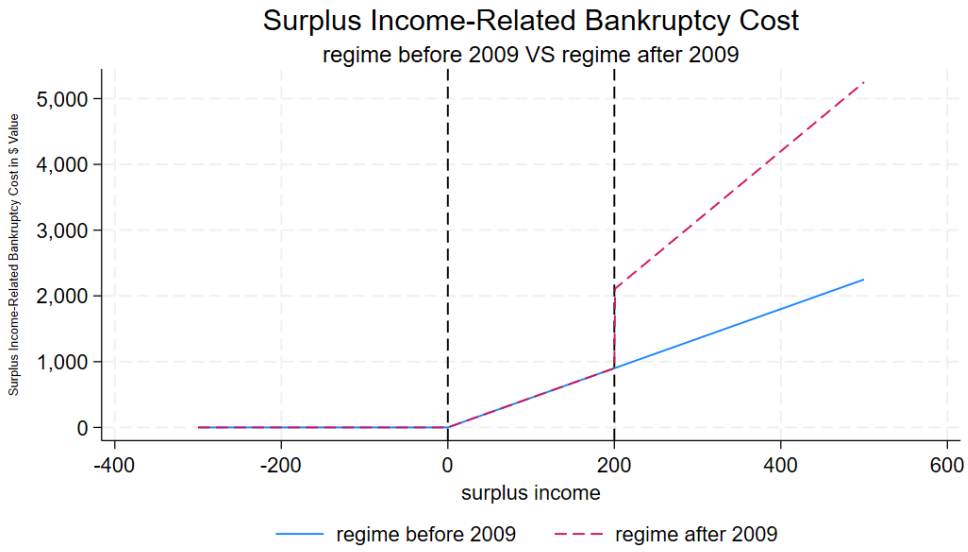


Figure 1: 2009 Consumer Insolvency Policy Change
This figure illustrates the effect of the 2009 insolvency reform on Surplus Income-based bankruptcy repayment amounts. The vertical axis graphs the total bankruptcy repayment based on Surplus Income (SI). The horizontal axis graphs SI. The solid blue line presents the SI-based repayment in the pre-reform period and the dotted red line presents the SI-based repayment in the post-reform period.

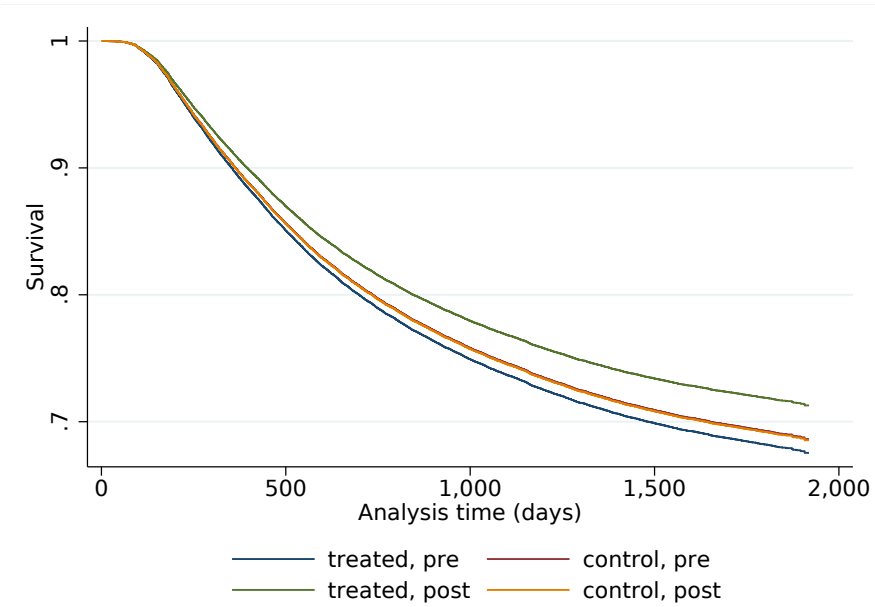


Figure 2: Survival Functions Based on Cox Model for Proposal Default
This figure shows the survival curves of default hazard based on our main Cox proportional hazards model in Equation 1, as reported in Column 1 of Table 4. These survival curves are estimated for treatment and control groups during the pre- and post-reform periods. The vertical axis graphs the cumulative survival rate for proposals. The horizontal axis graphs the number of days since a proposal was filed. The solid blue line represents the survival curve for treated debtors prior to the reform. The solid green line represents the survival curve for treated debtors after the reform. The solid maroon line represents the survival curve for untreated debtors prior to the reform. The solid orange line represents the survival curve for untreated debtors after the reform.

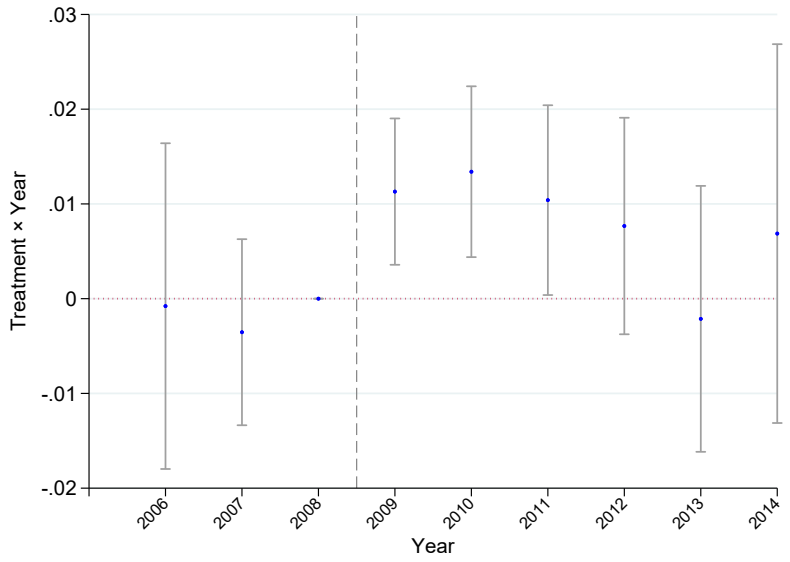


Figure 3: Employment

This figure plots the effect of a higher default penalty on the debtor's likelihood of being employed as estimated in the regression specified in Equation 2. We identify the effect of the higher default penalty using the 2009 insolvency reform. The vertical axis graphs the magnitude of the estimated effect of treatment. The horizontal axis graphs the years from 2006 to 2014. For each year except 2008, which is excluded as the base year for the regression, the figure plots the estimated coefficient and its 95% confidence interval for the interactions between the treatment group and the calendar-year dummies for each year. The figure plots results for a sample including all tax years within the proposal period.

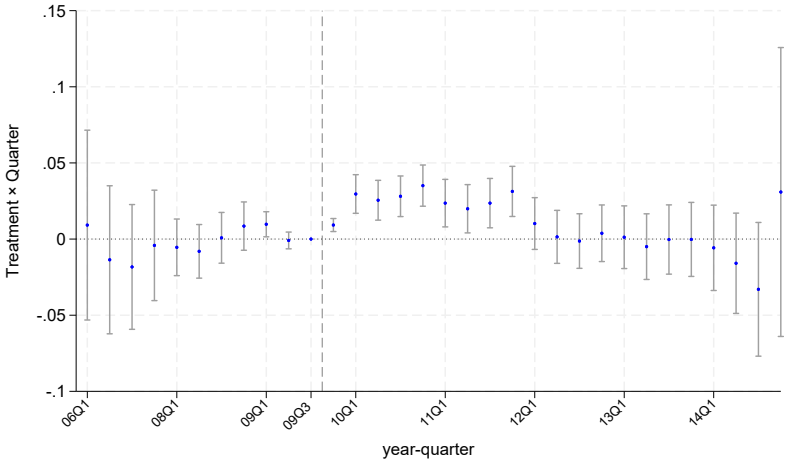


Figure 4: Number of Jobs Held

This figure plots the effect of a higher default penalty on the number of jobs held by the debtor in each quarter as estimated in the regression specified in Equation 2. We identify the effect of the higher default penalty using the 2009 insolvency reform. The vertical axis graphs the magnitude of the estimated effect of treatment. The horizontal axis graphs the quarters from 2006Q1 to 2014Q3. For each quarter except 2009Q3, which is excluded as the base quarter for the regression, the figure plots the estimated coefficient and its 95% confidence interval for the interactions between the treatment group and the calendar-quarter dummies for each quarter. The figure plots results for a sample including all tax quarters within the proposal period.

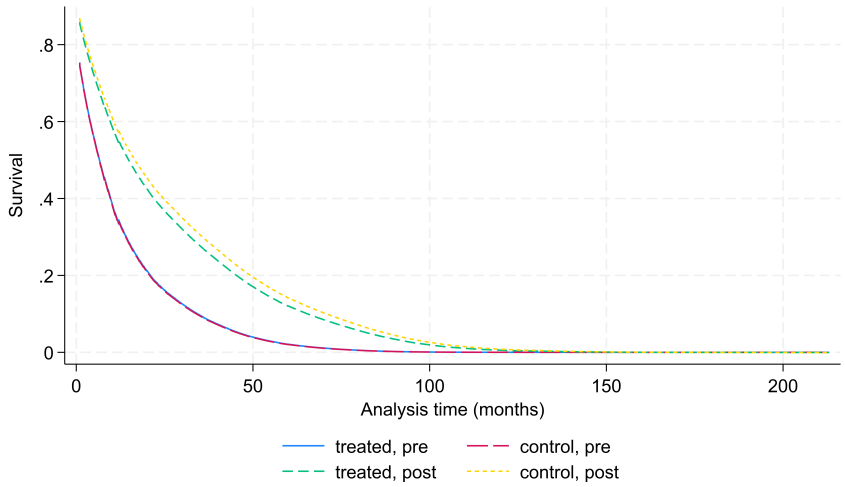


Figure 5: Survival Functions Based on Cox Model for Employment Hazard
This figure shows the survival curves of employment hazard for unemployment spells based on the Cox proportional hazards model in Equation 1, as reported in Column 1 of Table 5. These survival curves are estimated for treatment and control groups during the pre- and post-reform periods for all spells starting within five years of the policy reform. The vertical axis graphs the cumulative survival rate for unemployment spells. The horizontal axis graphs the number of months since an unemployment spell began. The solid blue line represents the survival curve for treated debtors prior to the reform. The dashed green line represents the survival curve for treated debtors after the reform. The long dashed maroon line represents the survival curve for untreated debtors prior to the reform. The short dashed orange line represents the survival curve for untreated debtors after the reform.

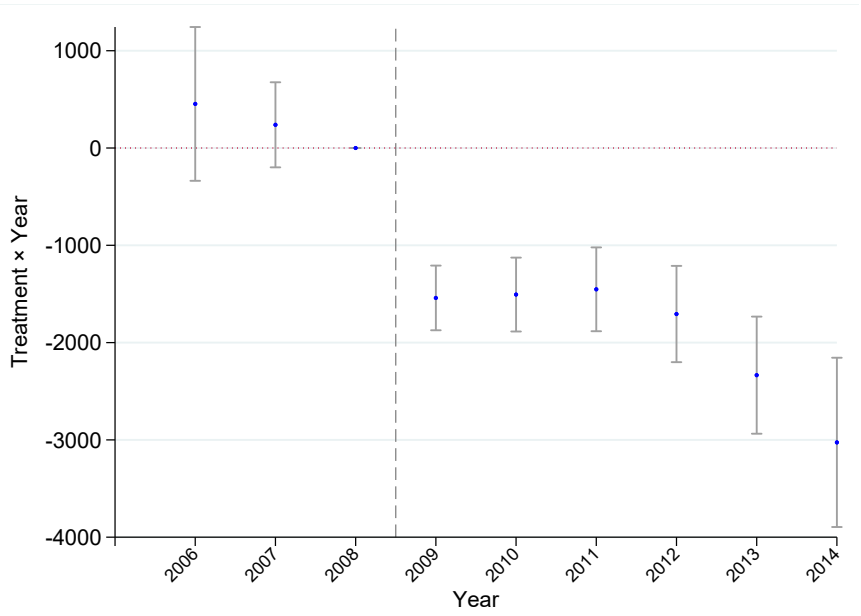
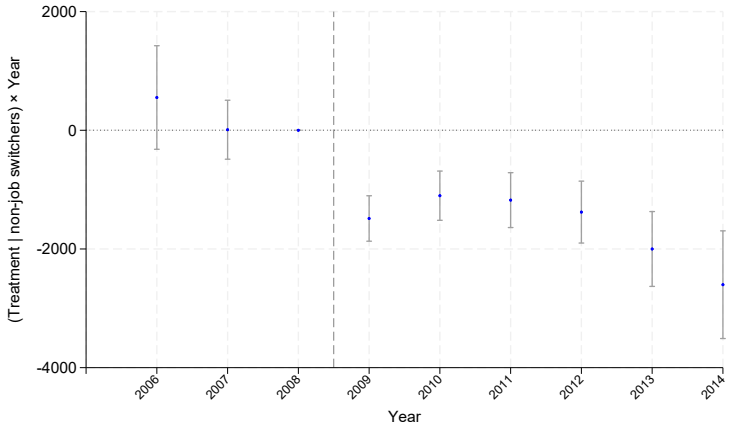
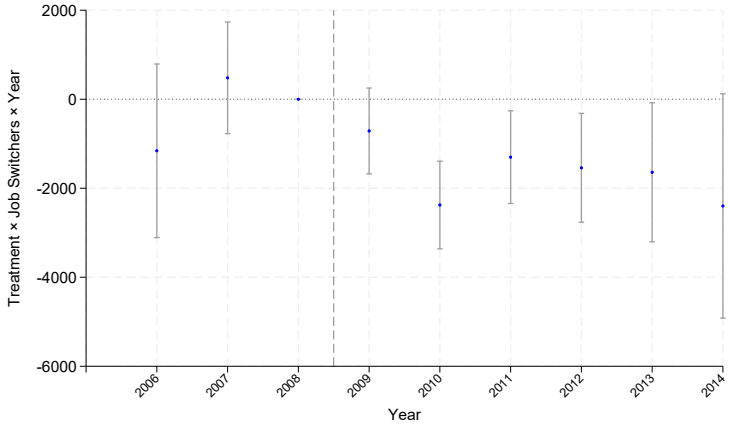


Figure 6: Wage Earnings

This figure plots the effect of a higher default penalty on annual total wage earnings as estimated in the regression specified in Equation 2. We identify the effect of the higher default penalty using the 2009 insolvency reform. The vertical axis graphs the magnitude of the estimated effect of treatment. The horizontal axis graphs the years from 2006 to 2014. For each year except 2008, which is excluded as the base year for the regression, the figure plots the estimated coefficient and its 95% confidence interval for the interactions between the treatment group and the calendar-year dummies for each year. The figure plots results for a sample including all tax years within the proposal period.



(a) Non-Job Switchers



(b) Job Switchers

Figure 7: Wage Earnings of Job Switchers

This figure plots the effect of a higher default penalty on annual total wage earnings for non-job switchers and job switchers as estimated in the regression specified in Equation 2. We identify the effect of the higher default penalty using the 2009 insolvency reform. The job switching indicator is equal to 1 in the year an individual switches their primary (highest paying) employer, and it is equal to 0 otherwise. Panel (a) reports the estimation for all the individuals who did not switch jobs during the sample period. In Panel (b), the vertical axis graphs the magnitude of the estimated effect of treatment for an individual who switched jobs in that year. The horizontal axis graphs the years from 2006 to 2014. For each year except 2008, which is excluded as the base year for the regression, the figure plots the estimated coefficient and its 95% confidence interval for the triple-interactions between the treatment group, the job switch indicator, and the calendar-year dummies for each year. The figure plots results for a sample including all tax years within the proposal period.

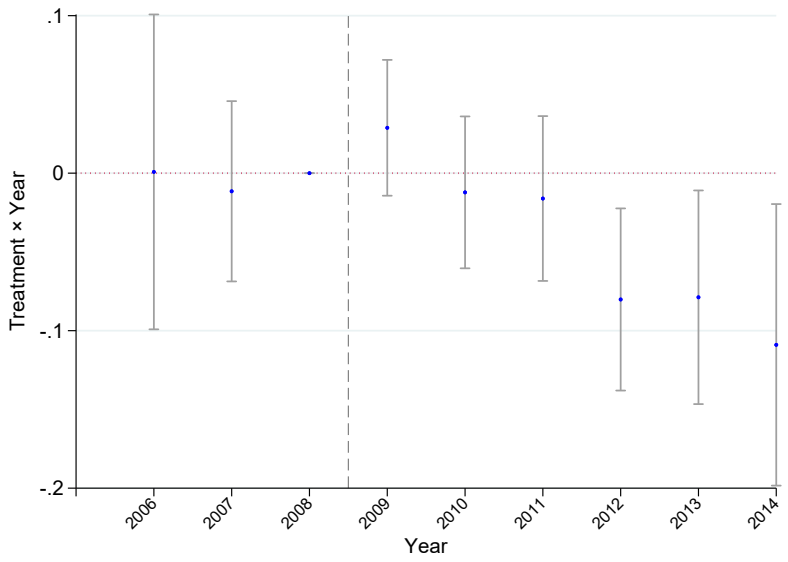
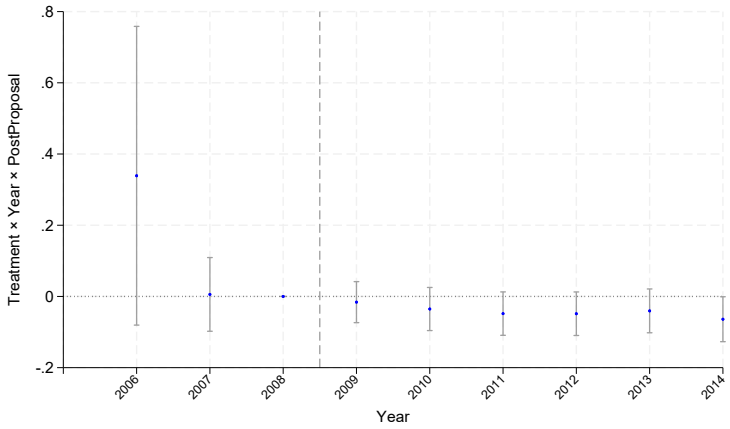
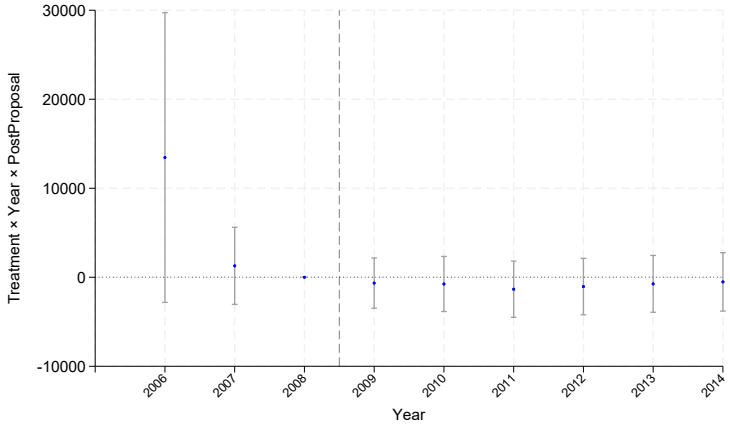


Figure 8: Registered Retirement Savings Plan Contributions

This figure plots the effect of a higher default penalty on the likelihood of making registered retirement savings contributions as estimated in the Poisson regression specified in Equation 2. We identify the effect of the higher default penalty using the 2009 insolvency reform. The vertical axis graphs the magnitude of the estimated effect of treatment. The horizontal axis graphs the years from 2006 to 2014. For each year except 2008, which is excluded as the base year for the regression, the figure plots the estimated coefficient and its 95% confidence interval for the interactions between the treatment group and the calendar-year dummies for each year. The figure plots results for a sample including all tax years within the proposal period.



(a) Employment Status



(b) Wage Earnings

Figure 9: Employment and Earnings during Post-Proposal Period

This figure plots the effect of a higher default penalty on employment and wages in the post-proposal period as estimated in the regression specified in Equation 2. We identify the effect of the higher default penalty using the 2009 insolvency reform. The post-proposal indicator is equal to 1 in each year after the end of proposal repayment, and it is equal to 0 otherwise. Panel (a) shows results for the likelihood of being employed and panel (b) shows results for total wages. For each year except 2008, which is excluded as the base year for the regression, the figure plots the estimated coefficient and its 95% confidence interval for the triple-interactions between the treatment group indicator, the post-proposal indicator, and the calendar-year dummies for each year.

Table 1: Summary Statistics

This table reports summary statistics for consumer proposals filed across Canada from 1 January 2006 to 18 September 2009, just before the 2009 insolvency reform was implemented. Proposal details are provided for debtor and filing characteristics. Four summary statistics are presented in the table: number of observations, mean, median, and standard deviation. All income and expense data are provided at the monthly level.

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 homeowner	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 nondiscretionary 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 repayment amount	56,693	14,794.01	12,180	9,071.62
Repayment amount / 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 Statuses

This table reports frequencies of statuses for consumer proposals filed across Canada from 1 January 2006 to 18 September 2009, just before the 2009 insolvency reform was implemented. The number and percentage of proposal filings are provided for each status.

Proposal Status	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 from Proposal Origination to Status Changes

This table reports summary statistics on the length of time (in days) from the date of proposal origination to the date of the status change. Four summary statistics are provided: number of observations, mean, median, and standard deviation. For the "Amendment and Default" status, time from origination to each status change is provided separately.

Category	N	Mean	Median	Std. Dev.
Default				
Time to default	15406	561.61	463	375.89
Amendment and Pay in Full				
Time to amendment	5472	254.34	75	393.64
Creditor Rejected				
Time to rejection	2295	105.58	74	168.46
Amendment and Default				
Time to amendment	2123	164.79	76	222.50
Time to default	2123	711.52	596	466.19
Withdraw				
Time to withdraw	1133	79.20	52	219.60

Table 4: Effect of Default Penalties on Proposal Default

This table reports the effect of a higher default penalty on default hazard estimated using the Cox Proportional Hazards model specified in Equation 1. We identify the effect of the higher default penalty using the 2009 insolvency reform. The three columns differ in the sample of debtors studied and the classification of treatment and control groups. In column 1, all debtors in proposal are included and a debtor is treated if their SI $\geq$ 200, as per the insolvency reform. In columns 2 and 3, the sample is restricted to debtors with SI $<$ 200 and debtors with SI $\geq$ 200, respectively, and debtors are treated if, for column 2, their SI $\geq$ -\$211 and, for column 3, their SI $\geq$ 900. In each column, the same set of control variables are included, all of which are listed in Appendix Table A1. Estimated coefficients are reported as odds ratios and standard errors are reported in parentheses. The percentage change in the hazard of default can be calculated as (estimated odds ratio) $-$ 1. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

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

Table 5: Effect of Default Penalties on Job Search

This table reports the effect of a higher default penalty on unemployment exit hazard (i.e., the hazard of finding a job) estimated using the Cox Proportional Hazards model specified in Equation 1. We identify the effect of the higher default penalty using the 2009 insolvency reform. The three columns differ in the unemployment spells used in the analysis. In column 1, spells that start within five years of the policy reform (September 2009) are included in the sample. In column 2, spells starting during proposal repayment periods are included in the sample. In column 3, all unemployment spells are included in the sample. In all three columns, the same set of control variables are included, all of which are listed in Appendix Table A1. Estimated coefficients are reported as odds ratios and standard errors are reported in parentheses. The percentage change in the hazard of default can be calculated as (estimated odds ratio) − 1. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) W/in 5 yrs	(2) W/in proposal	(3) All
Treat	0.992	0.969	1.0322***
	(0.0118)	(0.0258)	(0.00877)
Post	0.497***	0.543***	0.452***
	(0.0144)	(0.0225)	(0.0128)
Treat × Post	1.0935***	1.103***	1.0458***
	(0.0169)	(0.0294)	(0.0137)
N (nearest 1000)	57000	24000	98000

Appendix A Additional Institutional Background

A.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. While the federal regulator sets detailed rules regarding insolvencies, individual insolvency estates 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 intended 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.

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 a restructuring of debt repayments, consumer bankruptcy is a liquidation of assets.[19] Specifically, under consumer bankruptcy, most of the debtor's unsecured debts are discharged in exchange for the liquidation of nonexempt assets (e.g., house) and repayment of a fraction of a filer's income to 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 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.[20]

Consumer proposals are 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 (six years from the filing date or three years from the full

[19] 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.

[20] 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.

payment date, whichever happens first), compared with bankruptcy (seven years from the date of completion).

A.2 The Policy Change

On August 14, 2009, the OSB announced that it would implement a number of changes to the Bankruptcy and Insolvency Act (BIA), including increasing the number of months that some bankruptcy filers have to make payments to their creditors from nine months to 21 months. These changes became law on September 18, 2009, with the amendments to the BIA passing through Parliament. According to an official OSB announcement, these "changes [were] designed to modernize the insolvency system, increase fairness and reduce abuse of the system, and encourage [debt] restructuring [via consumer proposals] as an alternative to bankruptcy. The legislative amendments ensure that bankrupts with the financial means to contribute to their estates do so in order to maximize the return to their creditors."[21] A full description of the intentions of these amendments to the BIA are provided by Allen and Basiri (2018). In brief, the main motivation for the BIA 2009 amendment (increasing the number of months some bankruptcy filers need to pay creditors) was to incentivize new insolvents to select the proposal form of insolvency and disincentivize the bankruptcy form of insolvency.

In this paper, however, we do not focus on this main motivation of these amendments (initial selection by insolvents into either bankruptcy or proposal), but rather, we focus on a second-order outcome from this regulatory change. This is the effect of these regulatory changes on only those proposal filers who were already partway through their multiyear proposal repayment plan as of the date of the 2009 reform. Because the policy change occurs partway through a long-term contract for existing consumer proposal debtors, the reform does not induce any selection issues among the existing consumer proposal insolvent population, whom we study in this paper.

A.3 Treatment and Control Groups from the 2009 Policy Change

We derive our treatment and control groups from this policy change (as also described in Figure 1). An important element of the 2009 amendments to the BIA was that, while the rules regarding bankruptcy were changed (paying creditors for 21 months instead of nine months before the reform), there were no changes to the preexisting proposals studied in this paper.

[21] For additional details, see the following OSB directives `https://www.ic.gc.ca/eic/site/bsf-osb.nsf/eng/br02282.html` and `https://www.ic.gc.ca/eic/site/bsf-osb.nsf/eng/br02230.html`.

The policy change's increases to bankruptcy costs indirectly affected preexisting proposals, however, because they increased the penalty for default on a 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 individual's total income after subtracting authorized deductions and expenses. The basic rule is that bankruptcy filers need to pay 50% of an SI 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 nine months to 21 months, if their SI 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 nine months. This more than doubling of the number of months that SI payments were due increased the cost of those bankruptcy filers with an SI equal to or above $200. Importantly, these 2009 regulatory

changes had no effect for individuals with an SI of less than $200 per month. Debtors with expenses surpassing 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 nine months to 21 months of SI payments has no effect on debtors with a negative SI.

Figure 1 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 an SI. When an 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 an SI. The 2009 reform, which increased the number of months that payment (still 50% of SI) is made by the debtor — from nine months to 21 months — also displayed in Figure 1. This figure shows that the upward-sloping linear relationship to the right (where an SI $\geq$ $200) is steeper post-2009 because the amount payable (50% of SI) is now paid over 21 months rather than nine months. Also, there is a discontinuous increase in the amount paid in bankruptcy post-reform when an SI = $200 because these payments are done for 21 months after the reform. Importantly, these rule changes had no effect for individuals with an SI $<$ $200 because they still pay the same amount in both the pre- and post-reform periods. Debtors with a negative SI (where expenses surpass income) pay zero in both the pre- and post-reform 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 an SI 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 SI do not face this additional incentive to avoid default on a proposal after the reform. In other words, the treatment group (SI $\geq$ $200) would face a higher penalty for default on the proposal, while the control group (SI $<$ $200) would not face this incremental penalty. It is important to note that this penalty applies to only 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 a filer's liquidity constraints or equity positions.

Appendix B Additional Results

B.1 Validating Identification Assumptions

As described in detail in Section A.3, we define our treatment and control groups based on the SI cutoff of \$200. The 2009 reform increased the cost of default for proposal debtors with an SI $\geq$ \$200 (the treated group) but did not affect the cost of default for proposal debtors with an SI $<$ \$200 (the control group). A key advantage of our setting is that the identical OSB forms are used for bankruptcy and proposal debtors; thus, we are able to calculate SI for all proposal debtors, as measured at the date of their proposal filing. If debtors were to default on a proposal and subsequently file for bankruptcy, they would then submit new OSB forms, and thus, new SI data. In our setup, we separate proposal debtors into treatment and control groups based on their SI, as measured at the date of their original proposal filing (pre-2009 reform). We use this 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. While the SI measure at the time of proposal filing may be different from the SI at the time of bankruptcy filing, thus introducing a measurement error, it offers a benefit of being pre-determined before the policy was announced and implemented, thus not being subject to potential income manipulation or selection in response to the reform.

Our identification strategy builds on the literature on ex-post moral hazard (Mayer et al., 2014; Yannelis, 2020; Blouin and Macchiavello, 2019) which studies situations where the cost of default changes during the life of a long-term contract. The central argument in this literature is that such an exogenous shift in default costs is unrelated to the terms negotiated in the original agreement. In our setting, the September 2009 reform that extended the SI rule from nine to 21 months of payment altered the cost of default, but the proposal contracts between debtors and creditors had already been agreed upon prior to the reform's announcement. Accordingly, we can rule out concerns of self-selection into different contract terms or strategic manipulation of surplus income.

We provide evidence supporting this argument by running a standard assignment variable manipulation test developed by Cattaneo et al. (2020), which examines whether proposal filers "bunch" significantly on either side of a cutoff — in our case, the \$200 SI cutoff. We display these data visually in Figure A6. The data used in our test include the complete sample of filers from January 2006 to September 18, 2009, from the 10th to 90th percentiles of SI, which is an SI within the range [-\$700, \$1500]. The test for significant bunching at the \$200 cutoff produces a t-statistic of -1.14 and a p value of 0.254 which do not reject the null of continuity. In other words, there is no evidence

of income manipulation at the $200 cutoff in this sample.

Next, we examine whether there was any information leakage before the policy announcement, and how the proportions of the treatment and control groups change over time. Figure A7 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 SI cutoff of $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, which indicates that broad macroeconomic trends are not affecting the proportion of proposal filers allocated into the treatment or 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 date 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.

B.2 Linear Probability Model and Parallel Trends in the Pre-Reform Period

Figure A8 plots the raw monthly default rates for the treated and control groups, for the 12 months before and after the September 2009 policy change. 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 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 in the pre-reform period. Figure A8 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. These pre-reform trends thus provide suggestive evidence in support of our main identifying assumption.

In addition, Figure A8 shows that, in the post-reform period, the default rate of the treated group is consistently lower than that 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 models in Section 5.1.

While our preferred specification uses a Cox proportional hazard model (described in Section 4), 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.

$$Y_{it} = \beta_0 + \boldsymbol{\theta} Treat_i \times \delta_t + \beta_1 Treat_i + \boldsymbol{\beta_2} \delta_t + \beta_3 Controls_{it} + \mu_i + \varepsilon_{it}, \tag{3}$$

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, to 1 in the first month of default, and 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. Similar to the raw default rates (in Figure A8), 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. The vector $\boldsymbol{\theta}$ captures the coefficients of interests, which are the interactions of monthly dummies δ_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 5th 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 3. We cluster standard errors at the individual proposal level.

Figure A9 plots the estimates for the interactions of monthly dummy variables with the treatment indicator and their 95% confidence intervals. 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. These 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 sig-

nificant 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 Linear Probability Model are thus consistent with our main results based on Cox proportional hazard models.

B.3 Robustness Checks and Placebo Tests

In this section, we report on a variety of robustness checks we perform on our main default results presented in Section 5.1.

B.3.1 Narrower SI Ranges for Treated and Control Groups

Our main test in Section 5.1 includes all proposal filers with an SI $\geq$ \$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 SIs in our treatment and control groups. For example, one may argue that high SI individuals are less likely to default than low SI individuals in a financial crisis. We can address this concern by limiting our sample to individuals with SIs in a narrower range around the \$200 cutoff. With a narrower SI range, we can argue that individuals in both treatment and control groups would have similar propensity to avoid default in the absence of the reform.

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. Appendix Table A2, column 1, reports results for this new definition of the treatment and control groups. As expected, this restriction reduces the sample size compared with our full specification in Section 5.1 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 SI distribution or high SI individuals defaulting less than low SI individuals in general.

B.3.2 Announcement Effects

While the bankruptcy reform went into effect on September 18, 2009, this policy was announced by the OSB about a month before the implementation on August 14, 2009. 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 default 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 Appendix Table A2, column 2. The main DID coefficient is 0.87, 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.

B.3.3 Excluding Amended Proposals

An important element of proposal design is that a debtor is able to propose an amendment to the 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; 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 previous specifications, 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 Appendix Table A2, column 3, and find that the estimated effect of the policy change on default is 0.85, 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.

B.3.4 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 an SI of $200 after the 2009 reform. However, Figure 1 also displays a kink in bankruptcy cost at an SI of $0. We do not exploit this kink at $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 an SI in bankruptcy) may be systematically different than control debtors with an SI less than $0 (who pay nothing out of SI in bankruptcy). To address this, we re-estimate our main specification but simply remove all debtors with an 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 Appendix Table A2 column 4, and are essentially identical to our main specification. In other words, the inclusion of filers with an SI between $0 and $200 into the

control group is not driving our results.

B.3.5 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 multiyear period. One possible concern with this specification is that, because of the long multiyear durations of the pre- and post-reform periods, possible unobservable time varying factors could be affecting our comparisons between the pre- and post-reform periods. For example, the macroeconomic environment in 2009 may be significantly different from the environment in later years.

To address this issue, we restrict our models to measure default in the pre- and post-reform 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-reform 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-reform 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.

Appendix Table A3 reports results for specifications measuring changes in default in three, six, nine, 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 (three-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 A3 (six months, nine 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. They are not consistent with the hypothesis that our results are driven by the changing macroeconomic environment after 2009.

B.3.6 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. To address this concern, we run placebo-type tests in which 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 in subsequent years (September 2010, September 2011, September 2012, and September 2013). To avoid issues of overlapping event windows, we restrict these specifications to compare default rates in the six months on either side of these placebo reform dates. Appendix Table A4 summarizes results of these tests. The main conclusion from this table is that in no case is the estimate DID coefficient for the pseudo reform dates (Septembers in various years) statistically significant and below 1. In other words, these placebo tests suggest that our main findings are not because of a seasonal effect of September on proposal default, but is coming from the September 2009 increase in default penalty.

These tests also allow us to refute the idea that our results are affected by high SI individuals defaulting less than low SI individuals in a financial crisis. The pseudo policy change date of September 2008 is also in a financial crisis, yet we do not find any significant reduction in default for the treatment group (SI $\geq$ \$200) relative to the control group (SI $<$ \$200) in the absence of the 2009 reform.

Appendix Figures and Tables

Table A1: Covariate Coefficients and Fixed Effects in Primary Cox Proportional Hazards Analysis
This table reports the covariates and the fixed effects included in all Cox Proportional Hazards analyses performed in this paper, as specified in Equation 1. In the upper panel, the table briefly describes each covariate and provides the coefficient estimate and standard error for the covariate for the Table 4, column 1, analysis. In the bottom panel, the table presents the fixed effects included in these Cox Proportional Hazards analyses, along with the observations for the Table 4, column 1, analysis.

Variable	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 $160K	0.7981***	(0.02157)
If asset value more than $160K	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 $20K and $33K	0.9039***	(0.02028)
If unsecured debt between $33K and $50K	0.8355***	(0.02169)
If unsecured debt more than $50K	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 dummies		Y
Single filing dummy		Y
Filing reason dummies		Y
Province dummies		Y
Payment type dummies		Y
Filing year × month dummies		Y
Observations		101,748

Table A2: Robustness Tests for Effect of Default Penalties on Proposal Default

This table reports the effect of a higher default penalty on default hazard estimated using the Cox Proportional Hazards model specified in Equation 1 for four additional samples to assess the robustness of the findings reported in Table 4. We identify the effect of the higher default penalty using the 2009 insolvency reform. Column 1 reports results for debtors in proposal with Surplus Income (SI) between -$400 and $800. Column 2 reports results excluding proposals filed between the announcement date of August 14, 2009, and the policy implementation date of September 18, 2009. Column 3 reports results excluding proposals that were amended at any stage during the life of the proposal. Column 4 reports results excluding proposals filed by debtors with Surplus Income between $0 and $200. All other filers are included in the data. In each column, the same set of control variables are included, all of which are listed in Appendix Table A1. Estimated coefficients are reported as odds ratios and standard errors are reported in parentheses. The percentage change in the hazard of default can be calculated as (estimated odds ratio) − 1. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) SI (−400 to 800)	(2) Announcement Effect	(3) Not Amended	(4) 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

Table A3: Effect of Default Penalties on Proposal Default in Narrower Time Windows

This table reports the effect of a higher default penalty on default hazard estimated using the Cox Proportional Hazards model specified in Equation 1 for four narrower time windows to study the immediacy of the default effects reported in Table 4. We identify the effect of the higher default penalty using the 2009 insolvency reform. Column 1 reports results for all proposals filed within three months of September 18, 2009 (policy implementation date), column 2 reports results for all proposals filed within 6 months of this date, column 3 reports results for all proposals filed within 9 months of the date, and column 4 reports results for all proposals filed within 12 months of the date. In each column, the same set of control variables are included, all of which are listed in Appendix Table A1. Estimated coefficients are reported as odds ratios and standard errors are reported in parentheses. The percentage change in the hazard of default can be calculated as (estimated odds ratio) − 1. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) 3 Months	(2) 6 Months	(3) 9 Months	(4) 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

Table A4: Effect of Default Penalties on Proposal Default for Policy Pseudo-Enactment Dates

This table reports the effect of hypothetical insolvency reforms on default hazard estimated using the Cox Proportional Hazards model specified in Equation 1 for six policy pseudo-implementation dates to confirm the findings reported in Table 4 are driven by the policy. To avoid using overlapping samples, each analysis's sample only includes proposals filed within 6 months of the pseudo-implementation dates occurring annually on September 18 (i.e., spaced one year apart). The six columns in the table report results for September 18 pseudo-implementation dates increased by one year in each column, starting from 2007 for column 1 to 2013 for column 6, skipping the true implementation date of September 18, 2009. In each column, the same set of control variables are included, all of which are listed in Appendix Table A1. Estimated coefficients are reported as odds ratios and standard errors are reported in parentheses. The percentage change in the hazard of default can be calculated as (estimated odds ratio) -1. ***, **, * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1) 2007	(2) 2008	(3) 2010	(4) 2011	(5) 2012	(6) 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

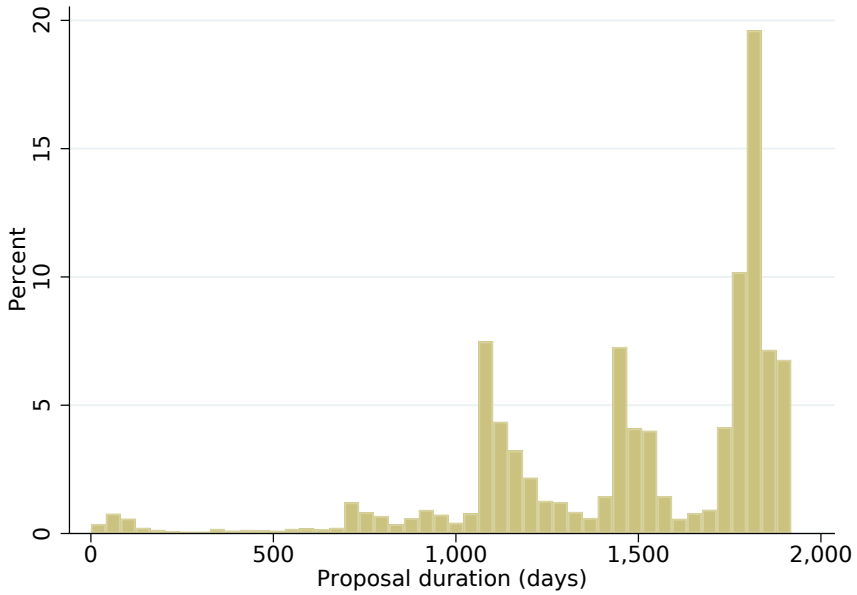


Figure A1: Planned Proposal Durations

This figure plots the planned durations of proposal agreements, negotiated between debtors and their creditors. The vertical axis graphs the percentage of proposals with durations in a given bin. The horizontal axis graphs proposal durations in days, using bins of equal duration.

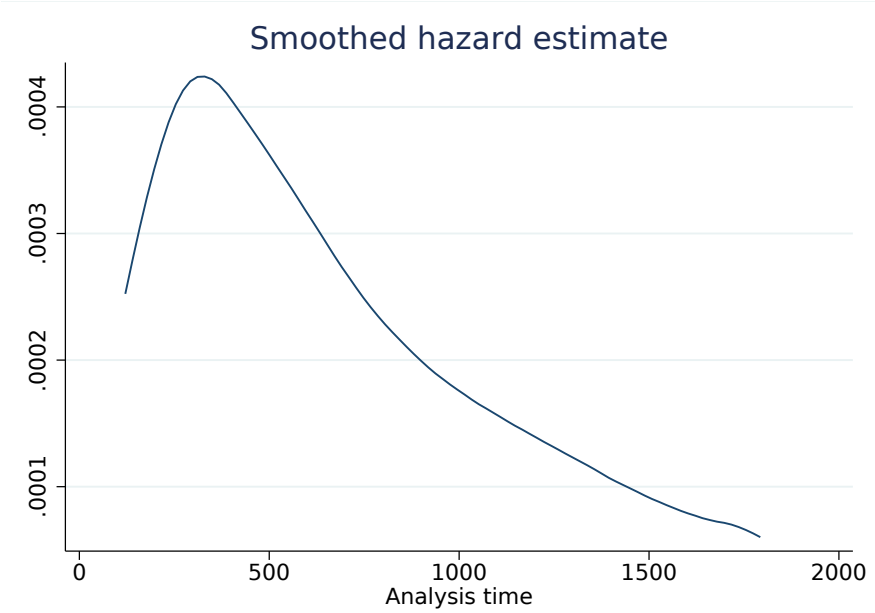


Figure A2: Probability of Default by Age of Proposal
This figure plots the unconditional default rate of proposals by the actual age of proposals as measured in days. This rate is estimated from raw data using our entire sample. The vertical axis graphs the proportion of defaults among proposals of a specific age. The horizontal axis graphs proposal age in days.

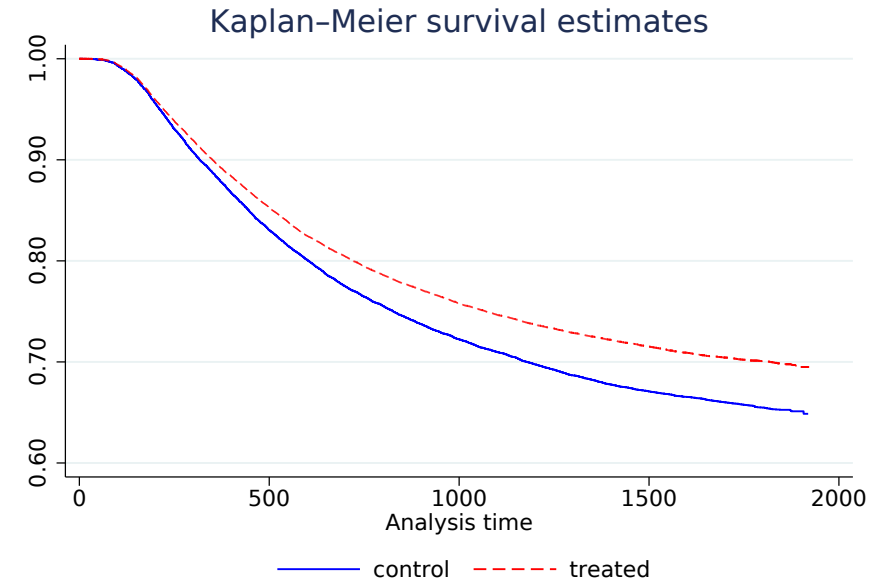
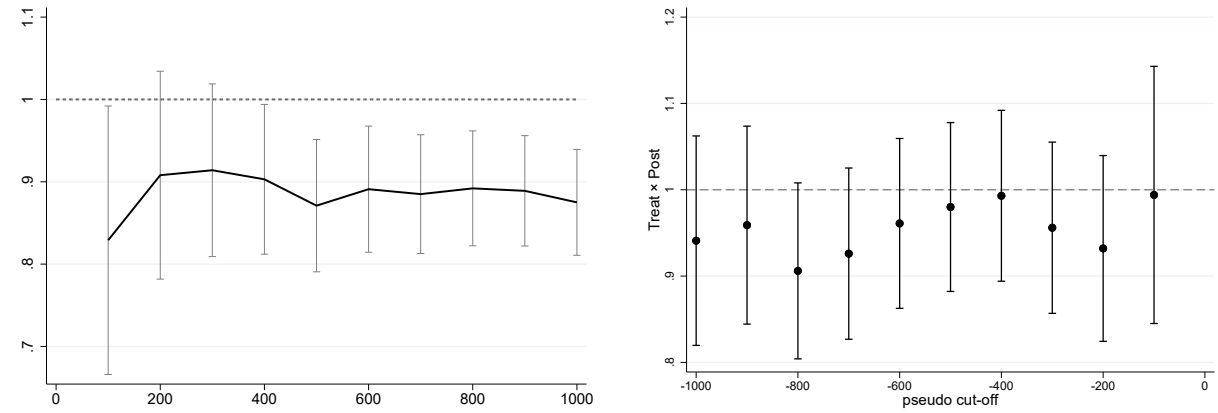


Figure A3: Kaplan-Meier Survival Curves, by Treatment Group
This figure plots Kaplan-Meier survival curves of default hazard for treatment and control groups (irrespective of policy effect). The sample used in this figure includes all debtors in proposals. The vertical axis graphs the cumulative survival rate for proposals. The horizontal axis graphs the number of days since a proposal was filed. The dashed red line represents the survival curve for treated debtors (SI $\geq$ \$200). The solid blue line represents the survival curve for untreated debtors (SI $<$ \$200).



(a) Bandwidth Sensitivity of Treatment Effect Estimates

(b) Placebo Tests Using Negative SI Cutoffs

Figure A4: Bandwidth Sensitivity and Placebo Tests

This figure plots the treatment effect of an increase in default penalty on default hazard, as estimated by the Treat×Post coefficient in the Cox Proportional Hazards model specified in Equation 1. Panel (a) presents the treatment effect for samples that vary the range of Surplus Income (SI) values included in the treatment and control groups. To reduce the measurement error of filings near the cutoff, in all specifications in that panel, filings with SI within \$100 of the cutoff (\$200) are excluded, removing proposals with SI between \$100 and \$300 from the sample. For each reported estimation result, the treatment group includes filings with SI between \$300 and \$300+I and the control group includes filings with SI between \$100 and \$100−I, where I is a bandwidth parameter that is varied from \$100 to \$1,000 in increments of \$100. The vertical axis graphs the treatment effect (i.e., the Treat×Post coefficient) for each specification. The solid line shows the estimated effect, while the bands represent 95% confidence intervals. The horizontal axis graphs the value of I for each specification. Panel (b) presents the treatment effect for treatment and control groups centered around pseudo-cutoffs. For each reported estimation result, we assign a pseudo-cutoff C ranging from -\$1,000 to -\$100 in increments of 100. For a given pseudo-cutoff C, the control group consists of proposals filed by debtors with SI between the minimum SI value and C, while the treatment group consists of proposals with SI between C and 0. The vertical axis graphs the treatment effect (i.e., the Treat×Post coefficient) for each specification. The solid line shows the estimated effect, while the bands represent 95% confidence intervals. The horizontal axis graphs the pseudo-cutoff (C) for each specification.

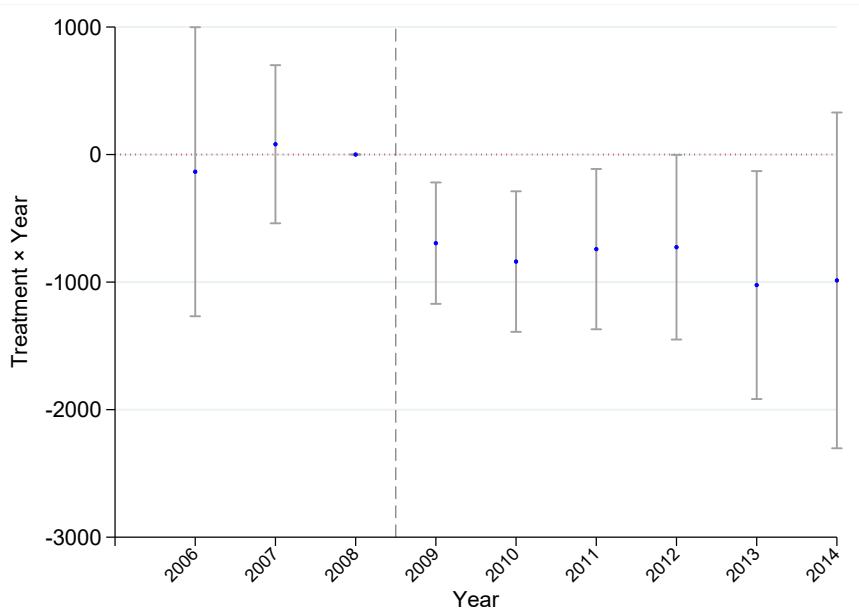


Figure A5: Total Income

This figure reports the effect of a higher default penalty on income (similar to Figure 6) using total annual income. We identify the effect of the higher default penalty using the 2009 insolvency reform. The vertical axes graph the magnitude of the estimated effect of treatment. The horizontal axes graph the years from 2006 to 2014. For each year except 2008, which is excluded as the base year for the regression, the panels plot the estimated coefficient and its 95% confidence interval for the interactions between the treatment group and the calendar-year dummies for each year. The panels plot results for a sample including all tax years within the proposal period.

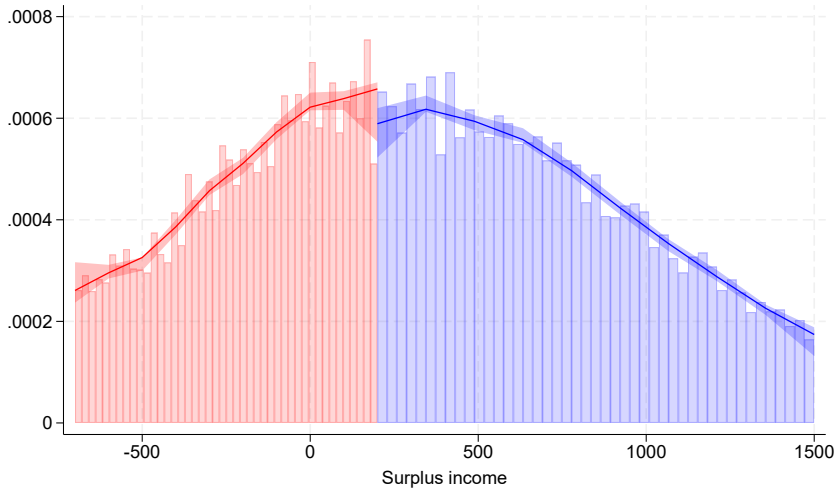


Figure A6: Test of Manipulation in Surplus Income around $200

This figure plots the findings of tests of manipulation (Cattaneo et al., 2020) in Surplus Income (SI), the running variable, at $200 as measured at the date of the original proposal filing. The test includes all proposal filings with SI between -$700 and $1,500. The red and blue line and shaded areas show the estimates and 95% confidence intervals for proposal filing shares based on the discontinuity test for proposals on the control and treatment sides of the SI cutoff, respectively. The test indicates no significant discontinuity in the SI distribution at the $200 SI cutoff.

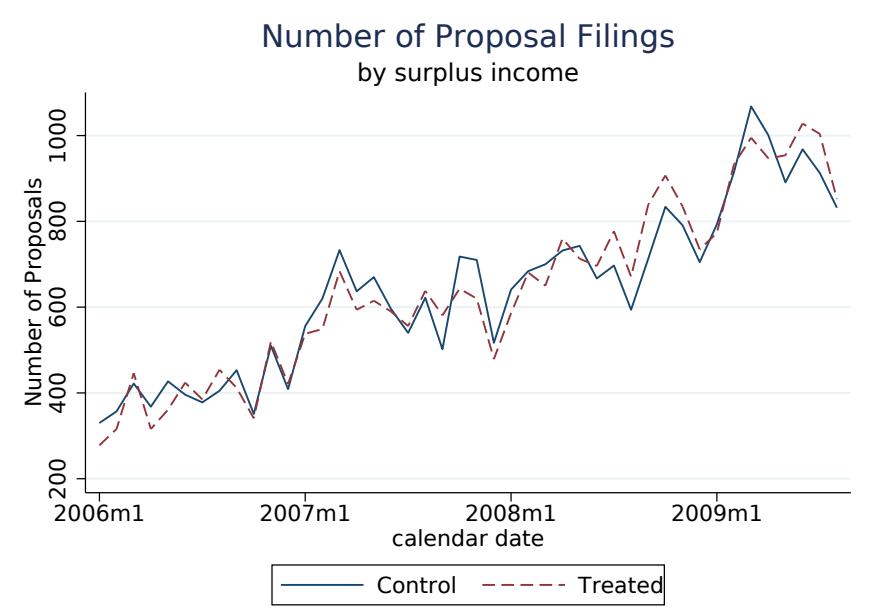


Figure A7: Number of Proposal Filings over Time in Pre-Policy Period
This figure plots the total number of proposal filings filed between January 1, 2006 and the policy implementation date, September 18, 2009, the analysis sample for this paper. The dashed red line represents the number of proposals in the treatment group (proposal filings by debtors with Surplus Income (SI) $\geq$ \$200), and the solid blue line represents the number of filings in the control group (filings by debtors with SI $<$ \$200).

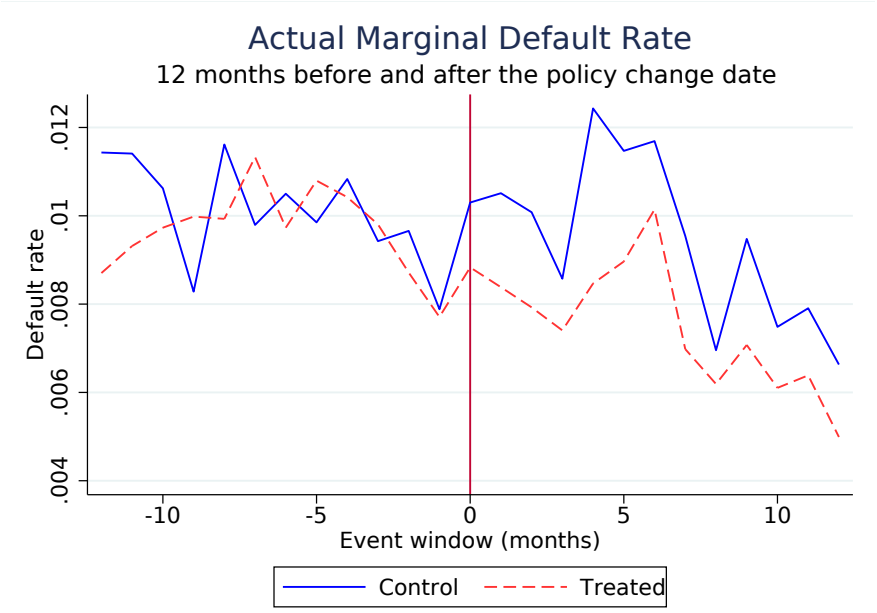


Figure A8: Monthly Default Rates: Treated and Control Groups

This figure plots the raw monthly default rates for the treatment group (dotted red line, SI $\geq$ \$200) and the control group (solid blue line, SI $<$ \$200) in the 12 months before and after the insolvency reform was implemented (September 2009). These data measure new defaults recorded each month, including all proposals that have not defaulted before the event window. This figure shows that in the pre-reform period, treatment and control groups had default rates that trended similarly, but in the post-reform period, the default rate of the treatment group fell below that of the control group.

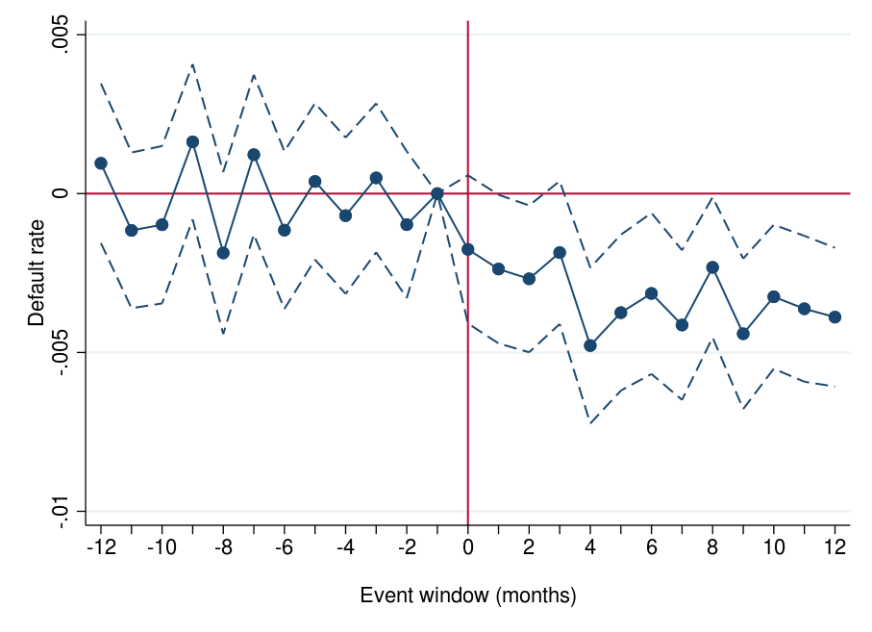


Figure A9: Monthly Default Rates: Linear Probability Model

This figure plots the effect of a higher default penalty on defaults using a Linear Probability Model (LPM), graphing the coefficient on the interactions of event-month indicators with the treatment dummy. We identify the effect of the higher default penalty using the 2009 insolvency reform. 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, treatment and control groups had default rates that trended similarly, but in the post-reform period, the default rate of the treatment group fell below that of the control group.