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Debtor income manipulation in consumer credit contracts[☆]

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ABSTRACT

We show that forcing insolvent consumer debtors to repay a larger fraction of debt causes them to strategically manipulate the data they report to creditors. Exploiting a policy change that required insolvent debtors to increase debt repayments at an arbitrary income cutoff, we document that some debtors reduce reported income to just below this cutoff to avoid the higher repayment. Those debtors who manipulate income have a lower probability of default on their repayment plans, consistent with having access to hidden income. We estimate this strategic manipulation costs creditors 12% to 36% of their total payout per filing.

1. Introduction

Credit decisions are made on the basis of information reported by debtors. Therefore, understanding debtors' incentives to strategically misreport information to creditors and the consequences of such data manipulation is important. Misreported financial information increases information asymmetry between debtors and lenders, which can lead to misallocation of credit by lenders and other credit market distortions. Clearly, lenders and governments would benefit from a better understanding of debtor information manipulation. Despite this, existing evidence on household debtor information manipulation is scarce, mostly limited to the mortgage market during the financial crisis (see Griffin, 2021, for a survey). Outside of that market, little is known about consumer debtors' strategic information manipulation. We shed light on this important and understudied topic by examining: (1) how changes in incentives to strategically manipulate data affect debtors' misreporting of financial information, (2) the effect of this manipulation on credit contract default, and (3) its effect on creditors and their response.

We study strategic information manipulation by debtors in the context of household debt modification using a policy change in Canada in 2009 that induced plausibly exogenous variation in their incentives to manipulate financial information reported to creditors. The credit contracts we examine are called *consumer proposals* (which are somewhat similar to Chapter 13 bankruptcies in the U.S.). These contracts are long-term, negotiated debt repayment plans, which result in the remaining debts being forgiven if the plan is successfully completed. Under these plans, the repayment amount depends on the borrower's income and expenses. The 2009 reform of the Bankruptcy and Insolvency Act (BIA) increased the amount that some proposal-filing debtors were required to pay creditors by implicitly introducing a sharp

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discontinuity at an arbitrary income-based cutoff in the repayment schedule. This new policy, therefore, increased debtors' incentives to manipulate the income and expenses that they report to creditors to situate themselves on the advantageous left side of the arbitrary new cutoff.

While we focus on insolvency in Canada, similar incentives for debtors to manipulate information exist in other consumer credit contexts. For example, in the U.S., the Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA) of 2005 changed bankruptcy rules such that, based on a "means test"-based discontinuity, it became advantageous for insolvent debtors to report income below the state median income (e.g., Gross et al., 2021; White, 2007). Similarly, as described in Yannelis (2020), wage garnishment laws in the U.S. only allow debtor's wages to be seized if their wages are above a specific threshold when the debtor defaults. In both of these contexts, debtors have an incentive to reduce their income to below some cutoff. We contribute to the literature by examining the design of these types of policies and how potential data manipulation affects the distribution of surplus between creditors and debtors. Thus, our study of strategic data manipulation by debtors in response to new regulation and its effects on debtors and creditors has value in various other regulatory contexts with similar incentives to manipulate information.

Under Canadian insolvency law, the amount payable to creditors in a consumer proposal implicitly depends on the debtor's Surplus Income (which is reported income minus allowable expenses). The 2009 reform introduced a sharp discontinuity in the total amount of repayment for debtors with a Surplus Income (SI) greater than or equal to $200. Since the reform, because of the structure of the payment schedule, debtors are required to pay an additional $1200 over the life of the plan when their reported SI increases from $199 to $200. For debtors whose SI is below that income cutoff, the reform has no effect on payments to creditors. This plausibly exogenous increase in the total payment amount creates an incentive for debtors with an SI over $200 to reduce their reported SI to below the $200 cutoff to avoid the higher debt repayment.

Using this natural experiment and a bunching methodology developed in the tax literature (see Kleven, 2016, for a survey), we examine how debtors reacted to the increased incentive to strategically manipulate income. Briefly, the bunching methodology asserts that, without data manipulation, the distribution of filings should be smooth around the $200 SI cutoff. A discontinuity in the distribution would indicate that filers manipulate SI to "bunch" on the more advantageous side of the cutoff. Using this methodology, we show that the higher debt repayment requirement leads to bunching responsible for 7.9% of post-reform filings just below the $200 SI cutoff. This result confirms that insolvent debtors respond strategically to the increased incentive to avoid higher income-contingent payments by manipulating their reported SI downward.

By strategically reporting lower SIs, bunching filers may retain additional "hidden" income, which, in turn, may provide additional liquidity and reduce their likelihood of default in bad periods. Using the Cox proportional hazards model, we document that debtors who manipulate data indeed are less likely to default on their repayment plans relative to their peers after the policy change, controlling for time-varying differences and other factors. This is consistent with SI-manipulating debtors having extra "hidden" income, which allows them to reduce their long-term default hazard on proposal repayment plans.

The strategic income manipulation we document implies that some debtors repay a lower proportion of their debts, which may induce losses to their creditors. Using our repayment data, we calculate two estimates of creditor loss. Our non-parametric estimate compares the repayment amount paid to creditors by bunchers with what the bunchers would hypothetically have paid if they had not manipulated their SI. Alternatively, our parametric estimate uses a linear regression to measure the difference in repayment between bunching region filings and filings in the SI region where bunchers would hypothetically belong if they had not manipulated SI. The two approaches have consistent findings that suggest that creditors lose between 12% and 36% of their total repayment amount per filing due to a manipulated SI.

Given the significant amount that creditors lose per filing, we next examine whether and how creditors respond to potential debtor data manipulation. After a consumer proposal is submitted by the debtor, creditors can either accept or reject it. Therefore, to test their response to bunching, we assess whether creditors are more likely to reject proposals in the bunching region after the reform. In the overall population, we find no statistically significant increase in rejections after the reform among bunching region filings. However, when we examine debtors with high asset values and debtors with high home equity, we find significantly higher rejection rates after the reform for bunching region filings. This suggests that, while creditors do not respond markedly to bunching for the typical filing, they do seem to respond where the reported SI is incongruous with other filing details (e.g., the reported SI is below $200, but the filer has high asset values).

Our paper makes several contributions to the financial economics literature. First, we expand the focus of debtor data manipulation studies beyond the mortgage market in financial crises (see Griffin, 2021; Ben-David, 2011; Elul et al., 2023; Garmaise, 2015; Griffin and Maturana, 2016; Jiang et al., 2014; Mian and Sufi, 2017; Pursiainen, 2020; Kruger and Maturana, 2021). In the mortgage market, debtors' incentives for data manipulation led them to inflate their reported personal financial situation (in particular, to report higher income levels), which the previous literature documents. Our study is unique in studying debtors' strategic manipulation in consumer debt renegotiations. Moreover, in our setting, debtors' incentives for data misreporting lead them to report lower income levels, in contrast to the prior studies focused on mortgages. Our results, combined with the existing literature, show that debtors may manipulate income in both directions, i.e., either overstating or understating their true financial situation based on different incentives in different settings.

Second, our paper offers new insights for the household finance literature studying the effects of increased debtor payments (see Campbell, 2013; Fuster and Willen, 2017; Di Maggio et al., 2017; Tracy and Wright, 2016; Keys and Wang, 2019). The existing studies document that raising debtor repayments (in mortgage and credit card markets) increased debtors' default rates, likely due to reduced debtor liquidity. In our study, we highlight an entirely different effect of increased debtor payments: strategic data manipulation to avoid increased repayments. This manipulation results in a reduction in debtors' repayments to creditors and a decrease in default rates.

A third literature we contribute to studies policies designed to protect creditor rights such as BAPCPA in the U.S. Most of the recent studies in this literature (Chakrabarti and Pattison, 2019; Gross et al., 2021) study BAPCPA and find that it strengthened creditor rights while improving credit access by making personal bankruptcy more costly and restrictive.[2] While we also examine a policy intended to strengthen creditor rights, our results show that the policy-induced bunching in our setting lowers creditors' surplus by reducing the total repayment they receive in proposals.

Finally, our paper adds to the literature studying policies that introduce arbitrary cutoffs and induce non-linear incentives for data manipulation.[3] Much of this literature studies tax evasion and avoidance (see Almunia and Lopez-Rodriguez, 2018; Camacho and Conover, 2011; Foremny et al., 2017; Kleven et al., 2011; Fack and Landais, 2016), showing that arbitrary income-based cutoffs in taxation schedules increase taxpayers' incentives to underreport income below such cutoffs. We are part of a small but growing literature documenting the

[2] Another related study in this literature, Li et al. (2011), found that mortgage defaults increased as an unintended consequence of BAPCPA.

[3] See Kleven (2016) for a survey.

incentive effects of such cutoffs in credit markets.[4] Our contribution to this literature is that we examine how discontinuities in debtors' income-based payment schedules motivate them to strategically manipulate income data reported to creditors, thereby reducing recovery by creditors.

2. Institutional setting

2.1. Insolvency in Canada

There are two kinds of insolvency available to consumers in Canada: consumer *proposal* and consumer *bankruptcy*, which are somewhat similar to Chapter 13 and Chapter 7 bankruptcy in the U.S., respectively. While consumer proposal involves a negotiated restructuring of debt wherein the debtor and their creditors reach an agreement in which the debtor repays a lower amount over a longer period, consumer bankruptcy involves a rule-based liquidation of assets. We discuss each in turn below.

Consumer proposals are legal agreements between insolvent debtors and their creditors to modify the debtors' unsecured debt obligations (e.g., credit card debt), while not altering the debtors' secured credit contracts (e.g., mortgages). Under this system, an insolvent debtor makes a "proposal" to their creditors to repay some portion of their unsecured debts over a period of time. If the creditors agree to the proposal, then the proposal becomes a legally binding contract, enforced by the Canadian bankruptcy regulator, the Office of the Superintendent of Bankruptcy (OSB). These proposal contracts typically entail the debtor making a series of regular payments for a period that can last for up to five years. If the debtor does not make the agreed-upon payments for three consecutive months, then the debtor has defaulted on the proposal contract and the contract is voided.[5]

Under consumer bankruptcy, some of the insolvent debtor's unsecured debt (e.g., credit card balances) is discharged in exchange for debtors relinquishing ownership of their non-exempt assets (e.g., real estate, automobiles, bank accounts), which are liquidated to repay creditors. In addition to the liquidation of any assets, debtors who file for bankruptcy are also required to pay a legally defined fraction of their income to their creditors. As we describe below, the plausibly exogenous variation we exploit in this paper is driven by a regulatory change in the fraction of their income that bankrupt debtors are required to pay their creditors.

Because insolvent debtors are free to select either kind of insolvency (a negotiation-based proposal contract or a rules-based bankruptcy contract), the relationship between these two kinds of insolvency is important. Crucially, any change in the amount that is required to be paid by bankruptcy filers in bankruptcy has an impact on negotiations between debtors and creditors entering into proposal contracts. Because of an asymmetry in the legal rights of creditors across the two forms on insolvency,[6] they are likely to reject a proposal filing if they believe that they will be better off if the debtor files for bankruptcy instead. As a result, the amount that a debtor needs to offer creditors in a proposal filing for their proposal to be accepted needs to be larger than or equal to the amount that would be repaid to creditors in a bankruptcy filing. As such, bankruptcy payments become an "informal floor" for proposal payments.[7] Therefore, when there is a regulatory increase in the payments required from debtors to creditors under the rules-based bankruptcy system, this results in creditors accepting new negotiation-based proposal filings only if there is a similar increase in the repayments proposed by the debtor.

2.2. September 2009 changes to the Bankruptcy and Insolvency Act

Canadian insolvency regulations changed on September 18, 2009, when the Bankruptcy and Insolvency Act (BIA) was amended by the Canadian Parliament. This followed the initial announcement of these amendments by the Office of the Superintendent of Bankruptcy (OSB) on August 14, 2009. Allen and Basiri (2018) provide a broad overview of these amendments to the BIA. In this paper, we focus on the changes to debt repayment rules in bankruptcy related to a measure of debtors' net income: Surplus Income (SI). There were other changes due to this reform,[8] but we exclude filings affected by these other changes from our analyses to make proposals before and after the reform comparable.[9]

The 2009 amendments to the BIA did not change any rules dictating how debtors and creditors negotiate consumer proposals. They did, however, increase the income-contingent payments that some bankruptcy filers were required to make to their creditors. These payments are contingent on the filers' monthly SI, which is essentially the income of the debtor minus authorized non-discretionary expenses, minus a family size-based deduction.[10] The SI reported by a bankrupt debtor determines the amount of income-contingent payments made to the creditor.

Our identification strategy exploits how the 2009 amendments affected debtors with different levels of SI in the post-reform period. Fig. 1 illustrates how the amount that a bankrupt debtor is required to pay to the creditor in income-contingent payments (vertical axis) changes with reported SI (horizontal axis). As displayed in that figure, the main rule (in both pre- and post-reform periods) is that bankruptcy filers who have an SI equal to or larger than $200 are required to pay their creditors 50% of their SI per month. The gray region in the figure for SIs between $0 and $200 represents different interpretations among trustees for SI-based repayments in this region. Most trustees (and Allen and Basiri, 2018) quote a monthly repayment of 50% of SI, whereas some other trustees quote no repayment for bankruptcies in this region. These different interpretations do not pose any issues for our analyses, as they persist across our sample period. Bankruptcy filers with a negative SI (i.e., income less than expenses) are not required to make any income-contingent payments to creditors.

The key part of the 2009 reform for our identification strategy is that the OSB increased the repayment period that bankrupt debtors

[4] E.g., DeFusco and Paciorek (2017) on discrete jumps in mortgage interest rates at conforming loan limits, Bachas et al. (2021) on notches in the guarantee rate schedule for SBA loans, and DeFusco et al. (2020) on how the Dodd-Frank Act introduced discontinuities in the cost of originating high-leverage mortgages.

[5] Both Canadian consumer proposals and Chapter 13 bankruptcy in the U.S. involve the restructuring of debt through a schedule of payments over a number of years. However, proposals are more flexible than U.S. Chapter 13 bankruptcy because debtors are able to propose any terms to their creditors, and the proposal only becomes legally binding when the creditors agree to those proposed terms.

[6] Creditors have no legal right to reject a bankruptcy filing by the debtor, but they are legally able to reject (or accept) any proposal filing by the debtor.

[7] The relationship between the two types of insolvency in Canada (bankruptcy and proposal) is somewhat similar to the relationship between Chapter 7 and Chapter 13 bankruptcy in the U.S., respectively, where the total amount that the debtor is obliged to repay under Chapter 13 (similar to Canadian proposals) cannot exceed the amount they would repay under Chapter 7 (similar to Canadian bankruptcy) (see Fay et al., 2002, p. 707).

[8] E.g., after the reform, debtors with net debt (debt minus principal residence's mortgage) between $75,000 and $250,000 were eligible to file consumer proposals.

[9] Agarwal et al. (2022) also examine this 2009 change to the BIA as an exogenous policy change. However, that paper examines whether the reform caused an increase in moral hazard and strategic default of pre-reform proposal filers, whereas this paper compares misreporting of information by proposal filers before and after the reform.

[10] These authorized non-discretionary expenses are very limited and consist of payments for child and spousal support, medical conditions, and fines and penalties imposed by the court, etc. Full details of the construction of SI are provided in Appendix Section A1.

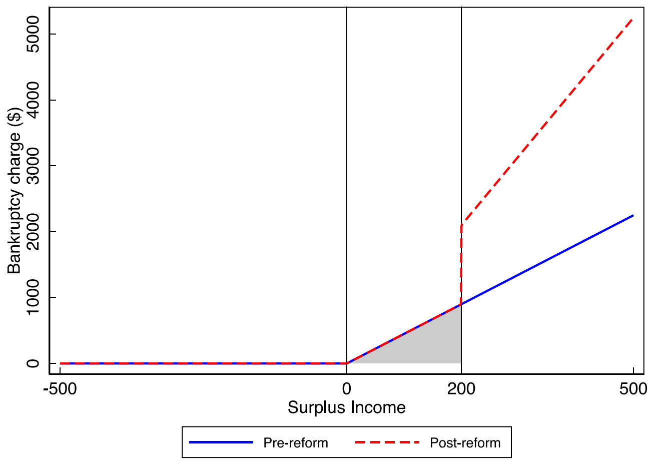


Fig. 1. Illustration of the Effect of 2009 Reform on Surplus Income-Based Bankruptcy Repayment Amounts
This figure illustrates the effect of the 2009 regulatory reform on Surplus Income-based bankruptcy repayment amount. The vertical axis represents the total amount of repayment based on Surplus Income (SI) charged under consumer bankruptcy. The horizontal axis is the filing's reported SI. The solid line represents SI-based repayment amounts in the pre-reform period and the dashed red line represents SI-based repayment amounts in the post-reform period. Gray shading represents possible SI-based repayment amounts where trustees offer different interpretations of the repayment amount. These differences in interpretations are present in both the pre- and post-reform periods.

with an SI equal to and above $200 are required to pay creditors from 9 months to 21 months. This rule change effectively created a new payment discontinuity, or notch, for proposal filers at SI = $200 (where the term "notch" used here is taken from the bunching literature, described in detail in Section 4). Given the "informal floor" relationship between bankruptcy and proposal repayments, this regulatory change to bankruptcy meant that proposal filers could reduce their expected payment amount to creditors by approximately $1200[11],[12] if they reduced their reported SI from $200 to slightly below $200.[13]

Fig. 1 also illustrates various other elements of the regulatory environment. Because the rule change affects all debtors with an SI of more than $200, in Fig. 1, the slope of the post-reform line is steeper than the slope of the pre-reform line for all filers with an SI of more than $200.[14] Thus, any proposal filer with an SI of more than $200 faces a greater incentive to manipulate reported SI downward in the post-reform period than in the pre-reform period, even if the reported SI remains above $200.

3. Data

The main database used in this paper consists of the universe of electronic proposal filings filed across Canada between 1 January 2006 and 30 June 2019, as provided to us by the OSB in August 2019. Proposal data prior to 2006 are not available for analysis because the OSB used a paper-based filing system prior to this date. The OSB switched to an electronic filing system in 2006, and nearly all proposal filings since 2007 have been handled electronically. Table 1 provides summary statistics for our data. As can be seen from this table, our data consist of almost half a million proposal filings.

There are two main components of the data, which are described in the two panels of Table 1. In the first panel, we summarize filer and proposal characteristics and negotiation outcomes at the time of filing. These include demographic characteristics of the filer and detailed balance sheet and income statements. We use information about family size, year of filing, income, and expenses to construct SI. Appendix Section A1 provides a detailed description of our SI construction method. Panel A of Table 1 also summarizes data on negotiation outcomes, such as planned repayment amount, planned payment as a percent of unsecured debt, proposal maturity, actual repayment amount, and actual repayment as a percent of planned repayment. Note that the actual repayment data are only available for proposals that were completed prior to the date that the data were generated by the OSB (30 June 2019).

Second, we have data from the OSB on proposal outcomes, such as creditor rejection, debtor withdrawal, and default. These data include both outcomes and their dates, which we use to measure time (duration) from proposal filing to the event. Panel B of Table 1 provides summary statistics on the actual long-term outcomes of each proposal agreement in the years following the proposal agreement coming into force (e.g., payment in full and default).

4. Evidence of data manipulation

In Section 2, we describe why the policy reform created a new payment discontinuity at SI = $200. This discontinuity created a new incentive for debtors with true SIs equal to or greater than $200 to manipulate their filings so their reported SIs fell below $200 as this would significantly reduce their income-contingent payments. In this section, we use the bunching methodology[15] to show that debtors strategically manipulate their reported SIs to fall below the $200 SI discontinuity.

4.1. Graphical evidence of SI manipulation

As a starting point for the empirical analysis, Fig. 2 plots the distributions of SI for proposal filings before and after the policy reform using histograms with SI bins of $40. Fig. 2(a) shows that, in the pre-reform period, there is no perceptible discontinuity at $200. On the other hand, in Fig. 2(b), we see that, in the post-reform period, there is bunching below the $200 cutoff.[16]

To formally test for a discontinuity in the distributions of filings in the pre- and post-reform periods at the $200 cutoff, we use the (McCrary, 2008) and Cattaneo et al. (2020) discontinuity tests. These results are reported in a box in the top right corners of Figs. 3(a) and 3(b), which also present the respective findings of the two tests visually. In the post-reform period, both of these discontinuity tests reject the null hypothesis of continuity at the $200 cutoff with very high statistical confidence (at a $p < 0.01$ level). Visually, the discontinuity seems to be driven by "excess" filings below the $200 cutoff. This is consistent with our conjecture that the 2009 reform, which sharply increased the

[11] In the post period, a bankruptcy filer with an SI slightly below the $200 cutoff would make payments for 9 months times (50% of $200) = $900. If that debtor had an SI of $200, she would make payments for 21 months times (50% of $200) = $2100. Thus, moving the SI from just above to just below the $200 notch would save $1200 in payments.

[12] As mentioned previously, due to the different interpretations of trustees, the savings from shifting the SI from $200 to $199 can be between $1200 and $2100. For the remainder of this paper, we use the more conservative $1200 savings as our estimate of the savings from SI manipulation.

[13] While this policy change increased the number of months in bankruptcy for filers with an SI above $200, it had no direct effect on the number of months negotiated between debtors and their creditors in proposals, as it only affects the total expected payment due to SI under proposal.

[14] Basically, the slope increases from 4.5 (9 months × 50% of SI) pre-reform to 10.5 (21 months × 50% of SI) post-reform for an SI above $200.

[15] The concept of bunching was initially developed by Saez (2010), Chetty et al. (2011), and Kleven and Waseem (2013) in the context of taxpayers manipulating taxable income. This methodology has subsequently been widely used in many other contexts, as described in the survey of Kleven (2016).

[16] In the Appendix, Figure A1 shows that SI is not generally a round number. As a result, SI does not naturally bunch at $200 as $200 is a round number.

Table 1
Summary statistics.

(a) Proposal details

	Obs	Mean	25th %ile	Median	75th %ile	Std Dev
Filer and filing characteristics						
Male (%)	478,053	56.2	0.0	100.0	100.0	49.6
Married (%)	478,053	48.0	0.0	0.0	100.0	50.0
Age (years)	478,053	44.0	34.0	43.0	52.0	12.4
Household members	478,053	2.3	1.0	2.0	3.0	1.4
Total assets ($ K)	478,053	89.0	4.5	17.0	151.4	343.6
Unsecured debt ($ K)	478,053	36.0	19.5	31.8	48.5	22.0
Secured debt ($ K)	478,053	68.5	0.0	6.8	117.8	111.7
Non-discretionary spending ($)	478,053	113.32	0.00	0.00	80.00	288.18
Discretionary spending ($)	478,053	3075.10	2118.00	2845.00	3859.00	1363.36
Homeowner (%)	478,053	31.6	0.0	0.0	100.0	46.5
Home equity ($)	478,053	13 740.41	0.00	0.00	0.00	319 831.36
Available family income ($)	478,053	3089.47	2123.00	2859.96	3894.00	1392.28
Self-employed (%)	478,053	7.1	0.0	0.0	0.0	25.8
Surplus income ($)	478,053	245.35	−339.00	231.00	814.00	1041.47
Negotiation outcomes						
Planned payment amount ($ K)	470,840	15.2	8.6	12.0	18.0	10.6
Planned payment to debt ratio (%)	470,840	54.5	32.9	44.6	66.6	51.7
Planned maturity (months)	451,758	55.6	58.0	60.0	62.0	11.3
Planned monthly payment ($)	446,167	297.64	150.00	225.00	325.00	777.10
Actual payment amount ($ K)	229,319	8.1	2.9	6.3	11.3	7.7
Actual to planned payment ratio (%)	229,319	75.3	42.0	59.7	65.6	4982.4

(b) Proposal outcomes

	Frequency	Percent
Full payment	325,103	68.01
Default	77,071	16.12
Amendment and full payment	49,347	10.32
Rejection	10,079	2.11
Withdraw	9,687	2.03
Amendment and default	6,766	1.42
Total	478,053	100.00

This table reports summary statistics for consumer proposals filed with the OSB in Canada between 1 January 2006 and 30 June 2019. In Panel A, we summarize all proposal details, including filer and filing characteristics and negotiation outcomes. In Panel B, we summarize loan outcomes for the proposal submissions. For Panel A, we present five summary statistics: number of observations, mean, standard deviation, 25th percentile, median, and 75th percentile. For Panel B, we present the number and proportion of proposal filings in each loan outcome category. For outcomes using actual repayment data, the sample is reduced to proposals with planned completion dates before the end of our sample period. Some variables have lower observation counts because of missing or irregular data (no monthly data for proposals with non-monthly frequency of payment). Detailed definitions of all variables are available in Table A1 in the Appendix.

repayment amount for filers with an SI above $200, led to debtors' manipulation of SI downward to below the cutoff.

To further confirm that our findings are not spurious, we conduct placebo tests to determine whether the post-reform bunching below $200 SI is unique. Specifically, we perform a McCrary discontinuity test using every hundred-dollar SI value from −$1800 to +$1800 (−$1800, −$1700, …, $1800) as a threshold and calculate the statistical significance of a discontinuity at that threshold. For consistency, we include proposal filings up to $600 away from the pseudo-threshold for each discontinuity test.[17] We perform this placebo analysis for both the pre- and the post-reform periods and report our findings in Fig. 4. Fig. 4(a) shows that, in the pre-reform period, there is no threshold where there is a discontinuous decrease in proposal filing volume from below the threshold to above. Fig. 4(b) shows that the $200 SI threshold is the only extremely highly significant (at the $p < 0.0001$ level) discontinuous drop in the distribution in the post-reform period. In both periods, we find some thresholds with a discontinuous increase in proposal filings, but those are not suggestive of bunching below a threshold like our focal $200 SI threshold.[18]

[17] The bandwidth is fixed at $600 to ensure equal SI ranges for all pseudo-thresholds. We find similar results if we vary the bandwidth to be narrower or wider.

[18] We report the corresponding figures based on Cattaneo et al. (2020) discontinuity tests in Appendix Figure A2. These figures suggest similar conclusions.

4.2. The bunching estimation methodology

As discontinuity tests do not estimate the extent of data manipulation, we use the well-established bunching methodology to estimate the magnitude of bunching below the threshold. The central assumption of this methodology is that, in the absence of data manipulation, the distribution of the running variable (in our case, the reported SI) should be smooth across the threshold. If individuals manipulate their data around a certain threshold, then the distribution of this variable should be discontinuous with an excess mass of individuals on the advantageous side of the threshold and missing mass on the other side of the cutoff.

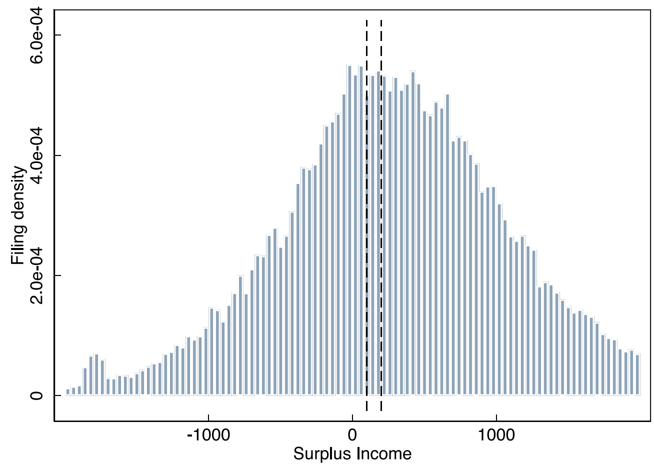
We primarily follow the methodology developed in Chetty et al. (2011) for our bunching estimation. In that paper, the magnitude of bunching is estimated as follows:

$$C_j \cdot \left(1 + \mathbb{1}[Z_j > r_U] \frac{\hat{B}_M}{\sum_{j=r_U+1}^{\infty} C_j}\right) = \sum_{i=0}^{q} \beta_i \times (Z_j)^i + \sum_{i=r_L}^{r_U} \gamma_i \times \mathbb{1}[Z_j = i] + \epsilon_j, \quad (1)$$

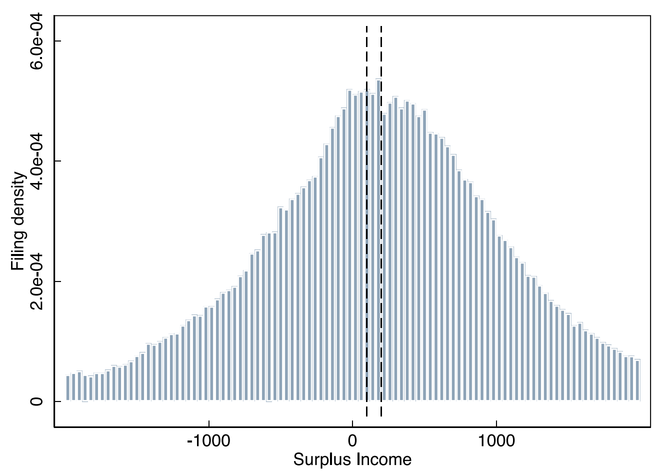
$$\hat{B}_M = \sum_{j=r_L}^{r_U} C_j - \hat{C}_j = \sum_{i=r_L}^{r_U} \hat{\gamma}_i, \quad (2)$$

$$\hat{b}_n = \frac{\hat{B}_M}{\sum_{j=r_L}^{r_U} \hat{C}_j}, \quad (3)$$

where C_j is the number of filings in SI bin j, Z_j is the maximum SI in each SI bin j, q is the order of the polynomial, r_U is the upper

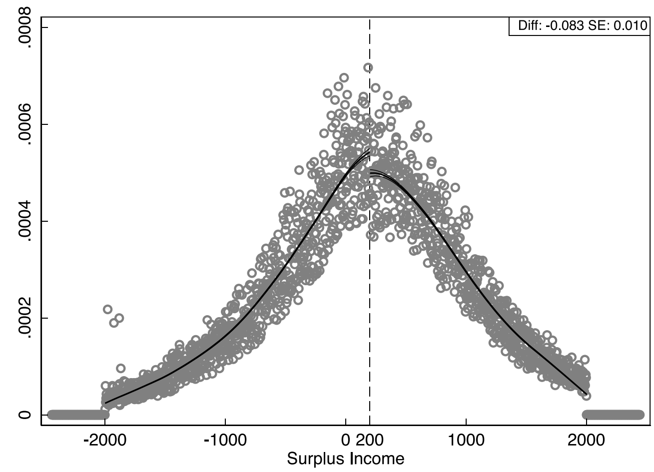


(a) Pre-reform distribution

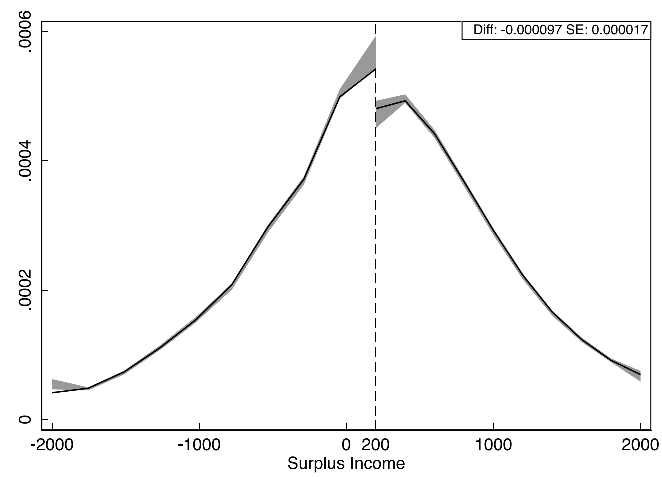


(b) Post-reform distribution

Fig. 2. Distribution of Surplus Income
This figure plots the observed distribution of Surplus Income (SI) before and after the 2009 regulatory reform in panels (a) and (b), respectively. Both panels include all proposal filings with SI between −$2000 and $2000. The vertical dashed lines indicate SIs of $0 and $200.



(a) McCrary (2008) Test



(b) Cattaneo et al. (2020) Test

Fig. 3. Discontinuity Tests for Post-Reform Proposal Filings
This figure displays the results of discontinuity tests performed at $200 Surplus Income (SI) cutoff for proposal filings submitted after the 2009 policy change. Panel (a) displays the results of the discontinuity test from McCrary (2008). Panel (b) displays the results of the discontinuity test from Cattaneo et al. (2020). Both panels include all proposal filings with an SI between −$2000 and $2000. In each figure, the magnitude of the discontinuity and its standard error are reported in the upper-right corner. Both tests suggest a discontinuity in the SI distribution at the $200 SI cutoff in the post-reform period.

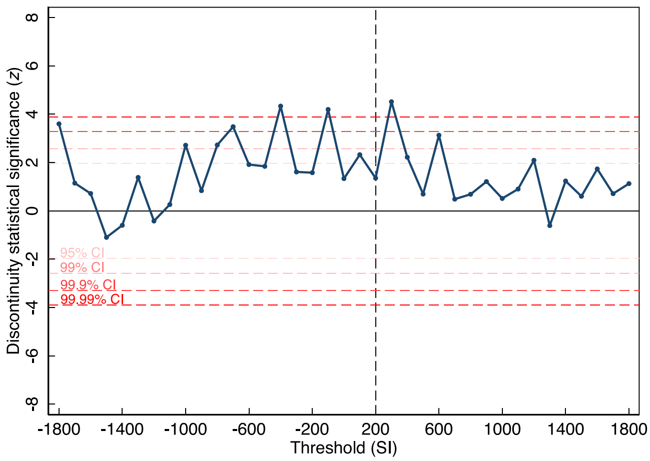
bound of the "exclusion region" (which we also refer to as the excess mass region), and r_L is the lower bound of the exclusion region. The counterfactual distribution is estimated as shown on the right-hand side in Eq. (1). We perform a high-order polynomial fit on bin counts for proposal filings wherein, by including fixed effects for bins in the exclusion region, we ignore the bins within the exclusion region. The counterfactual distribution is thus defined as: $\hat{C}_j = \sum_{i=0}^{q} \hat{\beta}_i (Z_j)^i$. $\hat{B_M}$ represents the excess number of filings within the exclusion region, which is the difference between the actual counts $\sum_{j=r_L}^{r_U} C_j$ and the estimated counterfactual distribution $\sum_{j=r_L}^{r_U} \hat{C}_j$. The expression in parenthesis on the left-hand side of Eq. (1) represents the upward adjustment of the counterfactual estimates to the right of the exclusion region to satisfy the integration constraint, which requires that the missing mass above the cutoff equal the excess mass in the exclusion region. The excess mass in the bunching region is thus defined by Eq. (3): $\hat{b}_n$ is the excess mass in the exclusion region relative to the total mass under the counterfactual distribution.[19] This amount can be interpreted as the percentage increase of filings in the bunching region because of the discontinuity.

Fig. 5 provides a hypothetical application of the bunching magnitude estimation method to our study. The red curve is the hypothetical observed distribution of SI. Each point represents the count of the number of filings in each SI bin. The exclusion region is the area between the two dashed vertical lines. Following the methodology of Chetty et al. (2011), we determine a counterfactual distribution satisfying the integration constraint, which is depicted as a blue dashed line in the figure. The dark gray area is the difference between the observed and the counterfactual bin counts in the exclusion region and illustrates excess mass in this hypothetical setting.

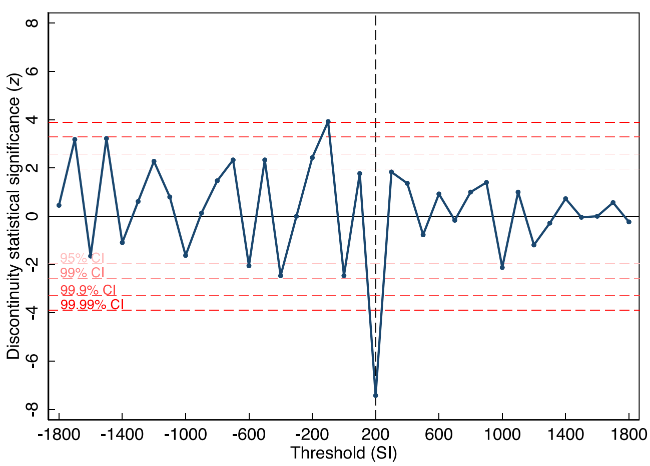
4.3. Estimation of surplus income bunching

In this section, we describe how we implement the bunching methodology in our setting. We can precisely calculate reported SI based on

[19] Unlike Chetty et al. (2011), we measure $\hat{b}_n$ as the proportion of the exclusion region filings composed of bunchers.



(a) Pre-reform



(b) Post-reform

Fig. 4. Placebo Tests for Discontinuity at Pseudo thresholds
This figure shows the statistical significance levels (z-statistic) for McCrary (2008) discontinuity tests at hundred-dollar Surplus Income (SI) thresholds from −\$1800 to \$1800. To maintain consistency across all tests, proposal filings with an SI within \$600 of each pseudo threshold are included in each test (these results are similar for other ranges). In panel (a), the discontinuity tests are performed on pre-reform filings. In panel (b), the discontinuity tests are performed on post-reform filings. The red dashed horizontal lines indicate statistical significance levels: 95%, 99%, 99.9%, and 99.99%. Figure A2 in the Appendix reports these estimates using (Cattaneo et al., 2020) discontinuity tests.

data from the proposal filings with little measurement error. Our setting in this regard is similar to the tax literature using bunching techniques.

Of the two prevailing methods of estimating bunching in the literature, we employ Chetty et al. because of the diffuse nature of our missing mass region. As described in Section 2, even debtors with a true SI far above the cutoff have an incentive to manipulate their reported SI to below the \$200 SI cutoff. If SI manipulation is achieved by misreporting data, the cost of such activity is primarily a fixed cost, which is not necessarily positively correlated with the true SI value. Therefore, manipulating debtors who report SI at just below the \$200 cutoff could have a true SI well above the cutoff. The bunching estimation method from Kleven and Waseem (2013) requires the missing mass region to be just above the cutoff, which does not fit this context. Rather, we employ the method used by Chetty et al. (2011), which, because of its integration constraint assumption, is designed for a more diffuse missing mass.

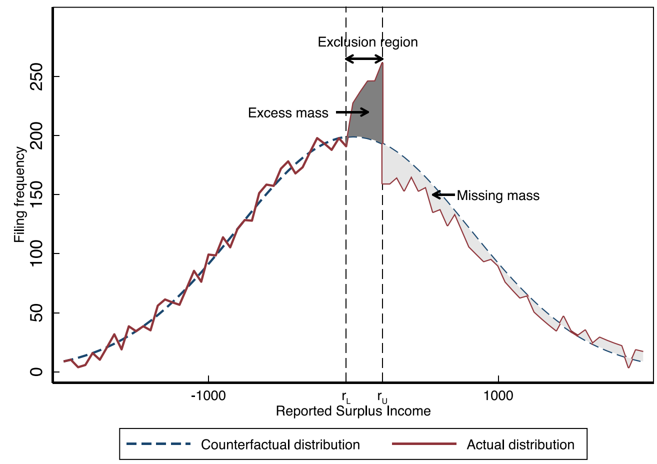


Fig. 5. Illustration of Bunching Magnitude Estimation
This figure illustrates how bunching magnitude is estimated. The horizontal axis represents reported Surplus Income (SI) and the vertical axis represents frequency of filings. The red line depicts an illustrative distribution of SI. The exclusion region is bounded by r_L and r_U on the horizontal axis, where r_U is the reform-induced notch and r_L is the lower bound of the bunching region. The dashed line represents the counterfactual distribution curve estimated based on the distribution of SI outside the exclusion region (as explained in Eqs. (1) through (3)). The difference between the actual density and the counterfactual density in the exclusion region represents the excess mass. The difference between the counterfactual density and the actual density on the right-hand side of r_U represents the missing mass.

Next, we determine the lower and upper bounds of the exclusion region. The upper bound is determined by the \$200 cutoff as reporting an SI equal to or slightly above \$200 will be subject to higher repayment with the new rules. As there is no theoretical or institutional guidance for the exact location of the lower bound, we follow the literature (e.g., Homonoff et al., 2020; Foremny et al., 2017) and determine the lower bound of the exclusion region based on visual inspection of the SI distribution. For robustness, we report estimation results based on different choices for the lower bound.

We report our main findings in Fig. 6. The vertical dotted lines demarcate the exclusion region (i.e., $SI \in (-100, 200)$). We use bins of size \$100 and a 7th degree polynomial to estimate the counterfactual distribution. The blue dashed line with solid circles plots the actual number of filings per bin. The estimated counterfactual distribution is indicated by the red smooth curve and can be seen to fit points outside the exclusion region well. The bunching within the exclusion region is easily observable on the left of the \$200 cutoff. The estimated excess mass is 0.079, which means that 7.9% of filings for SI in the range of −\$100 to \$200 arise due to the policy reform. This is an economically meaningful increase in filings below the cutoff and, given the standard error of the estimate, it is also highly statistically significant.

As the bunching methodology requires us to make a variety of empirical choices (i.e., bin sizes, polynomial order, and the lower bound of the exclusion region), we test the robustness of our findings by varying these choices. We report the bunching magnitude estimation based on different choices of bin size (\$40, \$50, \$60, and \$100), polynomial order (5 and 7), and lower bound of the exclusion region (−\$100, −\$80, −\$50, and −\$40) in Table 2.[20] We find statistically significant bunching across all combinations, where the excess masses in percentage terms are comparable across different settings. In the illustrated estimate in Fig. 6 and for the rest of the paper, we adopt the most conservative specification from column (7).

A critical assumption in the Chetty et al. bunching methodology is that there is no significant extensive margin switching near the

[20] We also report bunching estimation results for different bin sizes and lower bounds of the exclusion region in Figure A3 in the Appendix.

Table 2
Bunching magnitude estimation.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Bin size	50	50	40	40	60	60	100	100
Polynomial order	7	5	7	5	7	5	7	5
Exclusion region lower bound	−100	−50	−80	−40	−100	−40	−100	−100
Exclusion region upper bound	200	200	200	200	200	200	200	200
Bunching magnitude	4001	4695	3278	3888	4223	5051	4354	6483
Excess mass %	14.41	17.29	14.58	17.57	12.75	15.65	7.890	12.22
Standard error	1.340	3.020	1.560	2.520	1.450	3.030	1.010	2.620

This table presents details and results of eight bunching model estimations in this paper based on Eqs. (1) through (3). The eight models fit the counterfactual distribution for proposal filers' Surplus Income (SI). The data include all proposal filings with SI between -\$2,000 and \$2,000. Three input parameters, SI bin size, polynomial order of the model, and lower bound of the exclusion region, are varied across the models. The bottom three rows of the table provide the key results of each estimation: the bunching magnitude, the percentage of exclusion region filings made of bunchers, and the standard error of the bunching magnitude estimate.

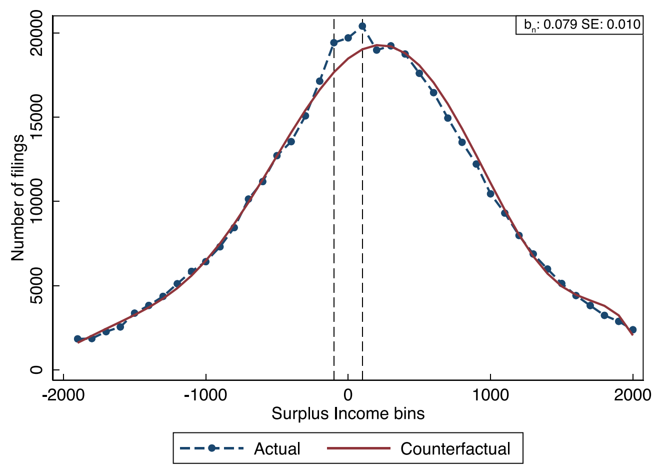


Fig. 6. Estimation of Bunching Magnitude
This figure shows the result of estimating bunching magnitude using Surplus Income (SI) bins of size \$100 and a 7th degree polynomial to model the counterfactual distribution. The estimation is performed on all post-reform proposal filings with an SI between −\$2000 and \$2000. The horizontal axis represents SI bins (of size \$100 each). The vertical axis represents the number of filings in the post-reform period in each bin. The dashed line is the actual number of filings per bin. The red smoothed curve is the estimated counterfactual distribution of filings per bin. The black vertical dashed lines indicate the exclusion region, with SI between −\$100 and \$200. The estimated bunching magnitude, b_n, and its standard error are reported in the upper right box.

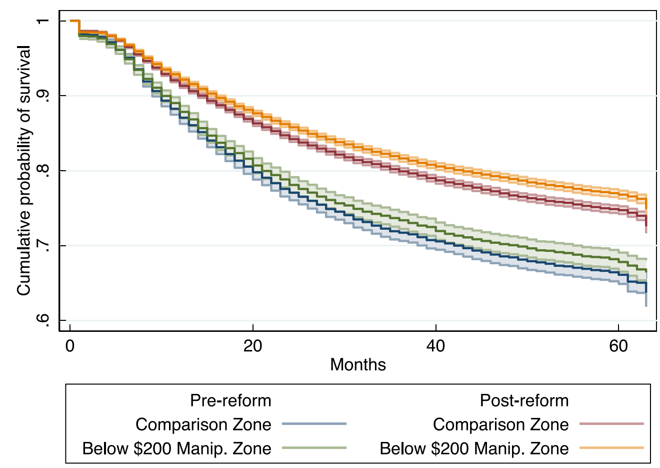


Fig. 7. Kaplan–Meier Survival Plot
This figure plots the Kaplan–Meier survival functions for proposals before and after the 2009 reform for two groups based on their Surplus Income (SI): Comparison Zone (SI between −\$400 and −\$100) and Below \$200 Manipulation Zone (SI between \$0 and \$200). The Kaplan–Meier survival function plots the cumulative probability of survival at each time period after the proposal starts. Shaded areas show 95% confidence intervals. The survival time is measured in months from the start of the proposal, ranging from 0 to 60 months as proposals have a maximum maturity of 5 years. Proposals with reported duration longer than 63 months are excluded as potential outliers and data errors.

policy cutoff. However, if the policy change induced more insolvent debtors to choose to file proposals below the \$200 SI threshold or fewer of them to choose to file proposals above the threshold, it could induce a filing distribution similar to what we observe but which does not arise because of SI manipulation. In Section 7, we consider these possibilities and present evidence inconsistent with extensive margin switching explaining our bunching magnitude estimates on its own.

Magnitudes of bunching observed in other contexts in the literature are quite heterogeneous (see Kleven, 2016). Our magnitude is smaller than some of the other studies, especially in the tax literature. This difference can be explained by the fact that, in our setting, every proposal filing needs to be made to the regulator (OSB) via a third-party intermediary (i.e., a trustee, typically a for-profit chartered accountant). A trustee is an Officer of the Court with a legal duty to represent the interests of the debtor and of creditors. As such, the trustee is legally tasked with ensuring the accuracy of the filing by the debtor. Therefore, the presence of trustees in the proposal system should reduce the prevalence of data manipulation (as measured by bunching) compared with a system with no third-party intermediaries (as in some of the tax literature).

The bunching we observe may also be smaller in magnitude from the tax setting because proposal filings involve multi-stage negotiations between debtors and creditors (and, of course, trustees). Tax filings, on the other hand, typically do not involve multi-stage negotiations. Thus, it is possible that creditors or trustees may reject more egregious attempts at data manipulation by debtors at early stages of the proposal process, which does not even result in a formal proposal submission and is not observable to us. This institutional setting could also reduce the bunching magnitude observed in our context relative to other settings.

5. The consequences of data manipulation on future default

Our results up to now imply that SI manipulators may have true income which is higher than their reported income. In other words, they may have higher actual repayment capacity than that implied by their reported income. If SI manipulators lower their reported income to avoid higher debt repayment, these filers should default on their proposals less than a comparable group of filers not subject to the reform-induced incentive.[21] In this section, we examine whether those debtors who bunched below the \$200 cutoff after the policy change are more or less likely to default in the subsequent years of the proposal contract.

[21] This hypothesis and methodology is similar to other studies of advantageous and adverse selection in credit markets (e.g., Hertzberg et al., 2018) that argue that hidden information about debtors' characteristics (e.g., credit risk) can be revealed by their loan performance.

5.1. Measuring the effects of bunching on proposal default

To test the effect of bunching on proposal default, we adopt a difference-in-differences (DID) type methodology used in other studies of the effect of bunching on individual outcomes (e.g., Dee et al., 2019; DeFusco et al., 2020; Collier et al., 2021). Using this methodology, we compare proposal outcomes (e.g., default) between filings in the SI manipulation zone to filings in a comparable SI nonmanipulation zone, before and after the policy change. We use a DID-like specification, much like the above-cited papers, because our policy change alters the extent of bunching from the pre- to post-reform period.[22]

Importantly, when using bunching as the basis of a DID specification, we must carefully define: (1) the area in the manipulation zone, and (2) a comparison zone just outside the manipulation zone. We define these zones based on institutional details. First, the benefits to manipulating a reported SI to below the cutoff only accrue if debtors manipulate their SI to below \$200. For this reason, we designate \$200 as the upper bound of a Below \$200 Manipulation Zone. Second, we designate \$0 as the lower bound of the Below \$200 Manipulation Zone because filers with an SI below \$0 do not face additional repayments due to their SI in both pre- and post-reform periods. The Below \$200 Manipulation Zone, therefore, runs from \$0 to \$200.

As described in Section 2, the 2009 policy change did not affect the area below the \$0 SI cutoff because the incentives to manipulate below this cutoff were the same in both the pre- and post-reform periods. Nevertheless, we still account for any possible SI manipulation below the \$0 cutoff by including a separate indicator variable for this zone in the DID specification. This variable (which we label the Below \$0 Manipulation Zone) is equal to 1 for filings with an SI from −\$100 to \$0. Our choices of upper and lower bounds for the Below \$0 and Below \$200 Manipulation Zones are guided by our findings on bunching in these zones (see Section 4).

We do not examine a zone above \$200 in this section because the composition of this zone changes in unobservable ways after the reform that are problematic for this analysis. The change in default for this zone is affected by at least three groups of filers: (1) those who decided to stay above \$200 (e.g., did not manipulate their data); (2) data manipulators from far above \$200, who lowered their SI, but not below \$200; and (3) data manipulators who left the zone to bunch below \$200. Because we cannot observe each filer's true SI, we cannot separate these three groups and can only measure the overall change in their default. As some of these changes may work in opposite directions, the overall change in this region's default rate is ambiguous (we discuss this in more detail in Section 7). For this reason, we do not use the above \$200 zone as a comparison zone or a manipulation zone in this section.

Instead, given that the lower bound of the Below \$0 Manipulation Zone is −\$100, we designate filings below −\$100 as the Comparison Zone, where debtors have no incentive to manipulate SI in either the pre- or post-reform periods. We designate −\$400 as the lower bound for this Comparison Zone to keep it close to the Manipulation Zone (in terms of SI) and make filings in the Comparison Zone more comparable to filings in both Manipulation Zones. Our results are robust to various alternative definitions of this lower bound.

We provide preliminary evidence on the effect of the reform on proposal default among bunchers using Kaplan–Meier survival functions. Fig. 7 plots these functions for the four groups of proposals we consider: the Comparison Zone before and after the reform and the Below \$200 Manipulation Zone before and after the reform. We also plot 95% confidence intervals for these estimates. This figure shows that proposals in the Manipulation Zone are more likely to survive (less likely to default) in both pre- and post-reform periods compared with proposals in the Comparison Zone, which may be because of the somewhat higher incomes of filers in the Manipulation Zone. In the pre-reform period, there is no statistical difference in the survival probability of proposals in the Manipulation Zone and the Comparison Zone, supporting our identification assumption that these two groups are similar in their default probability before the reform. This figure also shows that the difference between the survival functions of proposals in the Manipulation and Comparison Zones increases after the reform and becomes statistically significant. Thus, filers in the Manipulation Zone are less likely to default after the reform when compared with filers in the Comparison Zone. This is the first preliminary evidence that bunchers have lower default after the reform. We conduct more formal tests of this hypothesis in the next section.

5.2. Cox proportional hazards model of default

To model proposal default, we follow a large literature analyzing default in long-term debt contracts using a Cox proportional hazards model (Li et al., 2011; Demyanyk and Van Hemert, 2011; Agarwal et al., 2022). We can observe the exact start and end dates of the universe of long-term proposal contracts, as well as the exact date of any default on the proposal contract. Panel B of Table 1 reports the summary statistics of proposal loans' performance up to the end of the sample. The precise definition of these outcomes are provided in Table A1 in the Appendix. Approximately 78% of proposals are ultimately paid in full and 17% eventually default.

In our setting, the length of time from the start date to a default on the long-term proposal contract is modeled as the time to failure in the Cox proportional hazards model. An advantage of the Cox model is that it accounts for right censoring in our data, unlike other alternatives such as a logistic regression model. In addition, a large literature has documented that the default probability in a long-term credit contract is often related to the age of the debt contract, which we include in our Cox model.[23]

Our baseline specification is a standard Cox model estimated at the proposal level using proposal filings with SI reported from −\$2000 to \$200:

$$\begin{aligned} h_i(t) = \gamma_0(t) \times \exp(&\gamma_m \times Below\ \$200\ MZ_i \times Post_t + \gamma_b \times Below\ \$200\ MZ_i \\ &+ \gamma_k \times Below\ \$0\ MZ_i \times Post_t + \gamma_n \times Below\ \$0\ MZ_i \\ &+ \gamma_c \times Controls_i + \mu_k + \epsilon_i), \end{aligned} \tag{4}$$

where $h_i(t)$ is the monthly hazard of default (failure) for consumer proposal i at time t. $\gamma_0(t)$ is the baseline hazard function, which is the hazard rate when all covariates have values of zero. $Below\ \$200\ MZ_i$ takes the value of 1 if the proposal filer has SI between \$0 and \$200, and 0 otherwise, i.e., if the filing belongs in the Below \$200 Manipulation Zone. $Below\ \$0\ MZ_i$ is equal to 1 for proposals with SI between −\$100 and \$0, and 0 otherwise, i.e., proposals in the Below \$0 Manipulation Zone. We include three additional SI bins (SI between −\$2000 and −\$1200, SI between −\$1200 and −\$800, and SI between −\$800 and −\$400) and their interactions with $Post_t$ in all of our specifications but omit them from most of our tables for brevity. $Controls_i$ are filing and filer characteristics as reported in Table 1. Continuous control variables are converted into sets of indicator variables to account for their potential nonlinear effects on default. We also control for repayment amount and payout ratio for default propensity prediction. The choice of control variables is guided

[22] Note that this methodology is different from a classic DID, where a treatment group is compared to a control group in a panel setting, with both groups observed in both the pre- and post-reform period.

[23] Various studies (e.g., Keys et al., 2010; Li et al., 2011) have documented a hump-shaped curve of default over the life span of a long-term loan. Agarwal et al. (2022) document this hump-shaped default relationship for similar proposal data.

by availability and the recent literature on personal bankruptcy. μ_k represents a series of fixed effects including liability type, joint filing, repayment schedule type, debtor province, occupation category, and filing year. ϵ_i is an error term. The variable of interest is γ_m, which captures the change in the default hazard rate for filings in the Below $200 Manipulation Zone from the pre- to the post-reform period, in comparison to the change for filings in the Comparison Zone.

A key identification assumption of our DID analysis is that, in the absence of the policy change, the trends in loan performance for filings in the Below $200 Manipulation Zone and the Comparison Zone should be similar. While we cannot test this hypothesis directly after the policy change, we provide two sets of evidence supporting this assumption. First, we show that loan performance for the reported SI in the different zones move together in the pre-reform period. We rerun the tests as specified in Eq. (4) using each month from the start of 2007 to the end of 2008 as the pseudo-policy change month. We report the estimated coefficient, γ_m, for each regression in Figure A4 in the Appendix. None of the odds ratios on the main DID term are statistically different from 1. These results support the parallel trends assumption and help to validate our empirical setting.

Second, we compare the changes from before to after the policy change for observable characteristics of filings and filers in the Comparison Zone and the Below $200 Manipulation Zone. If the changes in observable characteristics across the two groups are similar, we can be more confident that the filings in those two zones would have evolved similarly over time in the absence of the policy change. In Table 3, we report our findings for this comparison across the two groups. For each observable characteristic, we report the pre-reform means in the Below $200 Manipulation Zone in the first column, the pre-reform means in the Comparison Zone in the second column, and the difference-in-differences coefficients, along with their standard errors and statistical significance, in the third column. The table shows that there is only one marginally statistically significant difference (at the $p < 0.1$ level) in the changes for filings across the two zones, unsecured debt, which changes by $600 less for Below $200 Manipulation Zone filings. Having just one economically small and marginally statistically significant difference across the two groups increases our confidence in all of our DID analyses.

Table 3
Change in filing and filer characteristics for key SI zones.

	Pre-Reform Manip. Zone	Comp. Zone	Post-Pre Diff
Male (%)	56.65	58.47	1.342
			(0.908)
Married (%)	49.58	42.07	−0.081
			(0.911)
Age (years)	41.28	40.83	−0.350
			(0.226)
Household members	2.29	2.21	−0.017
			(0.026)
Total assets ($ K)	66.59	48.38	0.566
			(1.836)
Unsecured debt ($ K)	32.19	29.42	−0.600*
			(0.331)
Secured debt ($ K)	52.18	36.03	0.307
			(1.609)
Total income ($)	2719.04	2210.00	4.767
			(14.875)
Non-discretionary spending ($)	86.50	70.61	6.069
			(4.120)
Discretionary spending ($)	2566.69	2103.95	10.898
			(14.498)
Planned payment amount ($ K)	12.47	11.46	−0.159
			(0.128)
Actual payment amount ($ K)	5.78	5.11	−0.061
			(0.106)

This table reports pre-reform means and the difference in changes from before the 2009 policy reform to after the reform in observable filing and filer characteristics across two groups of filings. The first column reports the pre-reform means for filings with Surplus Income (SI) in the Below $200 Manipulation Zone (SI between $0 and $200), and the second column reports the pre-reform means for filings in the Comparison Zone (SI between −$400 and −$100). The third column reports the coefficient and standard error for $Post_t \times Below\ \$200\ MZ_i$, as specified in Eq. (5), when these characteristics are used as the dependent variable. In the third column, ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

5.3. Default results

We estimate the Cox proportional hazards model as specified in Eq. (4) and report the results in Table 4 as odds ratios. The key result in our main specification in Column 1 of this table is that the odds ratio of the interaction of Below $200 MZ and Post is significantly less than 1 with an estimate of 0.93 (significant at the 5% level). This implies that defaults for filings in the Below $200 Manipulation Zone reduce by 7% more in the post-reform period, relative to filings in the Comparison Zone. In addition, the results in this table indicate that filings in the Below $0 Manipulation Zone do not have a significantly different default rate after the reform, compared with filings in the Comparison Zone. Since the policy change did not affect the filings in the Below $0 Manipulation Zone, this result confirms that there were no other changes affecting filings in this narrow SI region occurring at the time of the policy change.

In Columns 2 and 3 of this table, we report findings from alternative specifications in which we vary SI ranges for the Below $200 Manipulation Zone, the Below $0 Manipulation Zone, and the Comparison Zone. Our main result in Column 1, showing that the policy change and the resulting income manipulation reduced proposal default by 7% more for bunchers, is robust to these alternative definitions of the Manipulation and Comparison Zones.

A potential concern with this specification is that the estimate of γ_m may be a reflection of overall divergence in default rates between high and low SI filings that is unrelated to the policy change. We address this concern by comparing the Comparison Zone filings to filings with different negative SI levels. To do this, we use the additional negative SI indicator variables described above. Comparing them with the Comparison Zone filings (i.e., filings with SI between −$400 and −$100), we expect the odds ratios of the interaction term between these placebo SI zones and Post to be statistically indistinguishable from 1 because the policy change did not affect the incentive in reporting the SI in the placebo zones or our Comparison Zone. The remaining SI bin interactions with Post in the table confirm that, indeed, none of the placebo zones have statistically significant different changes in the default rate from the pre- to the post-reform period compared with the Comparison Zone. These findings thus address the concern that all higher SI proposals have a different default propensity in the post-reform period.

5.4. Discussion of default results

Our finding that filers in the Below $200 Manipulation Zone reduce their default rate in the post-reform period is consistent with SI manipulators having hidden payment ability (hidden income). This result supports our argument that some filers in this zone in the post-reform period manipulate their reported data to lower their SI to below the $200 cutoff. This increased level of hidden income creates additional liquidity, which can be used to reduce default. This finding contributes to the literature on how changes to debt payments can affect default (e.g., Fuster and Willen, 2017; Keys and Wang, 2019) by showing that higher income-contingent repayments may decrease default for debtors who hide their income and reduce their payment burden.

6. How data manipulation affects creditors

What is the effect on creditors of the bunching induced by insolvent debtors strategically reducing their SI? In this section, we explore two facets of the effect on creditors. First, we estimate the loss to creditors from a proposal filer bunching below the $200 SI cutoff using both non-parametric and parametric techniques. Second, we examine whether (and how) bunching affects the negotiated proposal contract between proposal filers and their creditors.

6.1. Estimated creditor losses from bunching

How much do creditors lose because of the bunching caused by proposal filers strategically manipulating their reported SI? When proposal filers strategically manipulate their reported SI, they do so to reduce total repayment amount, which means that their creditors receive less than they would if the filers reported their true SI (i.e., there was no bunching). We estimate, in this section, the magnitude of the loss creditors face when a filer whose true SI is above the $200 threshold reports an SI in the bunching region.

To estimate the loss to creditors from the bunching, we use unique data on the actual payments to creditors made by each proposal filer. These data come from OSB Form 14 (Statement of Receipts and Disbursements). They contain information on payments to all parties involved in proposals, including trustees, the OSB, the court, and creditors, for each completed proposal. Critically, this information exists for all proposals, including those that ended in default, were not paid in full, were amended, etc. In many other credit contexts, it is often difficult to observe data on actual payments on loans in default, partial payments on loans or loan modifications, as well as payments to various intermediaries. Thus, researchers typically need to make multiple assumptions to estimate payments and creditor losses for such loans. In our setting, on the other hand, we have actual data on all payments and fees paid in each proposal and can observe creditors' receipts directly. Because our payment data are only available for completed proposals, in this estimation, we restrict our sample to proposals with a planned completion date before the end of our sample (30 June 2019), which reduces our sample size.[24] On average, as shown in Panel A of Table 1, creditors receive 75.3% of their planned repayment amount.

Despite having actual payment data, we still face various obstacles in estimating the loss to creditors from the data manipulation (i.e., bunching). First, the true SI of the filers who manipulate their data is inherently unobservable. Thus, we do not know what their hypothetical repayment amount would have been if they had reported their true SI. Second, while the bunching methodology allows us to estimate the magnitude of bunching, we cannot distinguish the proposals of SI-manipulating filers in the bunching region from the filings of non-manipulating filers in this region. We tackle these difficulties in two ways and produce two sets of estimates of creditor loss due to strategic SI manipulation by filers. Both sets of estimates are provided in Table 5.

In our non-parametric estimation exercise, we calculate the per-filing loss to creditors with as little structure as possible. For this exercise, we make two assumptions. First, we assume that, if SI-manipulating bunchers had reported their true SI, they would have repaid the same amount as non-manipulating filers in that SI range. For example, if a manipulating filer's true SI is $500, we assume that this filer would have repaid the same amount as non-manipulating filers with an SI of $500. This assumption allows us to use observable repayment amounts of non-manipulating filers in the missing mass region (above $200 SI) to proxy what SI-manipulators would have paid if they had reported their income truthfully. In other words, we assume that, conditional on true SI, we can proxy the hypothetical truthful repayment amount of SI manipulators by the repayment amount of non-manipulators in the missing mass region, which we can observe. Second, because we cannot distinguish SI manipulators and non-manipulators in the bunching region, we proxy for the repayment amount of SI-manipulating bunchers with the average repayment amount of bunching region filings.[25]

With these assumptions in place, we calculate non-parametric creditor loss in three steps. First, we count the number of filings and calculate the average repayment amount in each hundred-dollar SI bin in the bunching and the missing mass regions. Next, we calculate the average of the repayment amount for the bunching and the missing mass regions, weighted by the number of filings in each bin within the respective regions. Finally, we calculate the per-filing loss from misreported SI as the difference in the average repayment amount between the missing mass region and the bunching region.

In the third column of Table 5, we report this non-parametric per-filing creditor loss estimate, varying the upper bound of the missing mass region from $500 to $2000 by $250 increments.[26] We find that creditor losses increase monotonically with the upper bound of the missing mass region. At the relatively low upper bound of $500, which assumes that bunchers have true SIs between $200 and $500, we estimate that creditors lose $846 per manipulator filing. At a $2000

Table 4
Effect of bunching on loan performance.

	(1) Default	(2) Default	(3) Default
Below $200 MZ × Post	0.931**	0.923**	0.916**
	(−1.97)	(−2.02)	(−2.31)
Below $200 MZ	1.017	1.029	1.034
	(0.52)	(0.81)	(0.98)
Below $0 MZ × Post	1.078*	1.035	1.021
	(1.66)	(0.87)	(0.46)
Below $0 MZ	0.933*	0.951	0.946
	(−1.70)	(−1.42)	(−1.40)
SI ∈ [−800, −400) × Post	1.032	1.032	1.024
	(0.88)	(0.87)	(0.68)
SI ∈ [−800, −400)	1.008	1.008	1.013
	(0.24)	(0.24)	(0.42)
SI ∈ [−1200, −800) × Post	0.976	0.975	0.968
	(−0.57)	(−0.57)	(−0.78)
SI ∈ [−1200, −800)	1.101**	1.101**	1.107***
	(2.44)	(2.42)	(2.63)
SI ∈ [−2000, −1200) × Post	0.969	0.969	0.961
	(−0.66)	(−0.66)	(−0.84)
SI ∈ [−2000, −1200)	1.106**	1.105**	1.111**
	(2.24)	(2.22)	(2.39)
Post	1.066	1.066	1.076
	(0.49)	(0.49)	(0.56)
Controls	Y	Y	Y
Fixed effects	Y	Y	Y
Model	CoxPH	CoxPH	CoxPH
Below $0 Manipulation Zone SI range	[−100, 0)	[−100, 50)	[−50, 50)
Below $200 Manipulation Zone SI range	[0, 200)	[50, 200)	[50, 200)
Comparison Zone SI range	[−400, −100)	[−400, −100)	[−400, −50)
Pseudo R^2	0.009	0.009	0.009
Observations	206,330	206,330	206,330

This table reports the results of estimating Eq. (4), comparing the default hazard of Manipulation Zone filings and Comparison Zone filings using Cox Proportional Hazards regressions. The regression is performed on proposals with Surplus Income (SI) between -$2,000 and $200. The control variables include all available filer and filing characteristics as reported in Table 1. The fixed effects include filing type, liability type, province of residence, occupation category, and filing year–month. Estimated coefficients are reported as odds ratios and t-statistics are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

[24] All our estimates are smaller, but qualitatively similar, if we use all available proposals with repayment data. This is to be expected as proposals with a completion date after the end of the sample are less likely to finish before 30 June 2019 and have payment data.

[25] We do not discount payments over the course of the proposal for two reasons. First, nominal interest rates during our sample period were effectively zero. Second, we do not observe the exact dates of the payments, so it is hard to precisely assign the right discounting value for each payment.

[26] Because we do not observe the true SI for each bunching filer, we estimate creditor loss for multiple ranges of potential true SIs.

Table 5
Estimated per-filing creditor loss due to bunching.

Missing Mass Region		Creditor Loss Per Filing			
Lower Bound	Upper Bound	Non-parametric		Parametric	
SI	SI	$	%	$	%
200	500	846	12.0	441	6.2
200	750	1242	16.6	649	8.7
200	1000	1880	23.2	1017	12.6
200	1250	2286	26.9	1276	15.0
200	1500	2821	31.2	1548	17.1
200	1750	3124	33.4	1680	18.0
200	2000	3511	36.1	1901	19.5

This table reports the results of estimating the effect of a proposal filer with a true Surplus Income (SI) in the missing mass region (SI over $200) reporting an SI in the bunching region (SI between $0 and $200). Each estimation is performed on all post-reform proposal filings. Each row provides two types of estimates of the per-filing creditor loss. The non-parametric estimates show the difference in the average amount distributed to creditors per filing between the missing region and the bunching region, with the average weighed by the number of proposals in each bin. The parametric estimates calculate the per-filing creditor loss as the weighted average of the coefficients for SI bins in a regression of the amount repaid to creditors by a filer on indicators of the filing's SI bin, a set of control variables including all available filer and filing characteristics reported in Table 1, except for SI, and fixed effects, including filing type, liability type, province of residence, occupation category, and filing year–months. Both types of estimates report creditor loss per filing in dollar terms ($) and as a percent of the average total amount distributed to creditors in the missing region (%). The columns show the estimates from varying the upper bound of the missing region from $500 to $2,000 in $250 increments.

upper bound, which, as in our bunching estimation, assumes that manipulators' true SIs are somewhere between $200 and $2000, we estimate that creditors lose $3,511 per manipulator filing.[27] In the fourth column of the table, we report the loss to creditors as a percentage of how much they would have been repaid, on average, if the filers who moved to the bunching region had remained in the missing mass region. We estimate that creditors lose anywhere from 12% to 36% of their average repayment when filers misreport SI to place themselves in the bunching region.

Our second approach to estimating creditor loss incorporates the effect of various factors that may affect a proposal's negotiation outcome or the proposal filer's repayment behavior. We isolate the effect of the reported SI by estimating how reported SI affects the average actual repayment amount while controlling for other observable characteristics of the filing. Effectively, we run a linear regression of the amount repaid to creditors by a filer on indicators of the filing's SI bin, a set of control variables including all available filer and filing characteristics reported in Table 1, except for SI, and fixed effects including filing type, liability type, province of residence, occupation category, and filing year–month. We run the regression on filings in the bunching and the missing mass regions only. The omitted SI bin in the regression is for SI between $0 and $200, which corresponds to the bunching region. The missing mass region SI bins are $100 wide, extending from $200 to the chosen upper bound of the missing mass region. Because of this arrangement of SI bin fixed effects, the estimated coefficients of the SI indicator variables can be interpreted as the average difference between the amount creditors receive when a filer reports SI in that SI bin versus the amount they receive if a filer reports SI in the bunching region, conditional on all other observable characteristics. To calculate the parametric average per-filing loss, we take the average of SI bin coefficients (with the sign reversed to estimate loss), weighted by the number of filings in each bin within the missing mass region.

We report the findings of this parametric estimation exercise in the fifth and sixth columns of Table 5. We show, in Column (5), that the loss per filing to creditors ranges from $441 to $1,901, increasing monotonically as we raise the upper bound of the missing mass region from $500 to $2000. In Column (6), we report that these loss estimates also increase monotonically with the missing mass region upper bound as a percentage of average repayment amount in that region. Defining the missing mass region using the $500 SI upper bound, creditors lose 6% per filing and, using the $2000 SI upper bound, they lose nearly 20%. Compared with the non-parametric dollar estimates of the losses, both the level and percentage estimates are smaller. This makes sense as, in this exercise, we control for the other characteristics of the filer and the filing, many of which are likely correlated with the filing's SI.

We note that our per-filing creditor loss estimates are similar in magnitude to the benefit of income manipulation for debtors. As we have discussed previously, after the 2009 policy reform, if a filer with true SI of $200 reported an SI of $199, they could reduce their repayment by $1200. Our creditor loss estimates straddle this marginal debtor's benefit from income manipulation. That the marginal debtor's income-manipulation benefit lies within the upper and lower bounds of our estimates of creditor losses per filing gives us more confidence in the estimates.

The findings of both our non-parametric and parametric creditor loss exercises offer an insight into creditors' motivations with respect to bunching. While the dollar amount lost per filing is not large in absolute dollar terms, the loss relative to the total repayment for a proposal filing is sizable. This result suggests that creditors may be somewhat motivated to identify misreporting filers in the bunching region because, while the absolute dollar amount of the loss per filing is not large, the relative losses amount to between one-fifth and one-third of their total repayment per filing with misreported SI. In the next section, we examine whether creditors respond to the data manipulation and how it affects proposal negotiation and terms.

6.2. Creditors' response to bunching

A proposal is a negotiated contract, which becomes legally binding only after insolvent debtors and their creditor(s) agree upon terms. In this section, we examine how creditors respond to data manipulation by debtors (as indicated by bunching below the $200 cutoff after the policy change) and how it affects negotiated proposal contract outcomes. The proposal setting allows us to conduct these tests because we observe both the initial data reported by the debtor to the creditors and the subsequent outcome of the debtor–creditor negotiations.

6.2.1. Empirical methodology

To examine these questions, we use the same DID research design as in Section 5. We define the same Manipulation Zones (between −$100 and $0 and between $0 and $200) and Comparison Zone (SI between −$400 and −$100) as in the previous section. However, because most proposal negotiation outcomes are continuous variables (e.g., amount proposed to be repaid) or binary outcomes (e.g., proposal rejection), we use OLS and logit regressions to model these outcomes instead of the Cox proportional hazards regressions used in Section 5.

If negotiations between creditors and debtors in the Below $200 Manipulation Zone change after the reform, we predict statistically significant coefficients on the interaction of the Below $200 Manipulation Zone indicator with the post-reform indicator. Because the policy change did not affect incentives in the −$100 to $0 manipulation zone, the coefficient on this interaction should be insignificant, as long as there are no other contemporaneous changes affecting filers in this zone and the $0 to $200 zone.

Our baseline specification is a DID-type regression estimated at the proposal level using the sample of proposal filings with an SI reported from −$2000 to $200. The regression equation is as follows:

$$\begin{aligned} LoanTerms_i = \; & \beta_0 + \beta_B \times Below\ \$200\ MZ_i + \beta_{PB} \times Post_t \times Below\ \$200\ MZ_i \\ & + \beta_K \times Below\ \$0\ MZ_i + \beta_{PK} \times Post_t \times Below\ \$0\ MZ_i \\ & + Controls_{i,t} + \epsilon_{i,t}, \end{aligned} \tag{5}$$

[27] As we explain in Section 4, the missing mass in our setting is diffuse, and the $2000 upper bound of the missing mass region is implied by the bunching methodology.

where the dependent variable $LoanTerms_i$ is one of the proposal contract terms (proposal rejection by creditors, total repayment amount, natural log of total repayment amount, total repayment amount over total unsecured debt ratio, maturity, proposal maturity over 60 months, subsequent filing withdrawal by filer) for proposal filing i, and all remaining variables are defined as in Section 5. As in the regressions in that section, we include three additional SI bins (SI between −$2000 and −$1200, SI between −$1200 and −$800, and SI between −$800 and −$400) and their interactions with $Post_t$ in all of our specifications but omit them from our tables for brevity. To account for serial correlation and region-specific random shocks, we cluster standard errors at the province level and include monthly fixed effects in all specifications. If the loan term/outcome is a binary variable (i.e., whether proposal maturity is more than 60 months, whether the proposal filing is subsequently rejected or withdrawn), we estimate Eq. (5) using a logit regression. The coefficient of interest is β_{PB}, which measures the differential change in negotiation outcomes for the filings in the Below $200 Manipulation Zone relative to filings in the Comparison Zone following the policy change, holding all filer and filing characteristics constant.

6.2.2. Proposal rejection and loan terms

The most direct response by creditors to the data manipulation is outright proposal rejection. By doing this, creditors may reject proposals of potential data manipulators and not allow them to enter the insolvency system. However, it may be difficult for creditors to precisely identify which debtors in the Below $200 Manipulation Zone are data manipulators. In fact, if it were possible to identify manipulators perfectly, then creditors would be able to reject all of these proposals, and there would be no bunching in the remaining (accepted) proposals, contrary to what we document in Section 4. Thus, in addition to examining the effect of bunching on outright proposal rejections, we also examine the extent of proposal rejection for filers with various observable characteristics that may allow creditors to distinguish filers with different amounts of hidden income or wealth.

Table 6 reports results for creditor proposal rejection overall and for six different groups of filers, with different observable characteristics. To create these groups, we consider three debtor characteristics: asset holding, home equity, and the prevalence of round numbers in proposal filing.[28] We split the sample at the median for each of these variables and estimate the model in Eq. (5) for each subsample. Column (1) of Table 6 reports our findings for the full sample and columns (2) through (7) report the subsample findings.

Focusing on the interaction term of the Below $200 Manipulation Zone and Post, we find that bunching on average leads to an increase in the probability of proposal rejection across all proposals, even though this coefficient is not significant. Column (2) of Table 6 shows that proposals in the Manipulation Zone submitted by filers with above median assets are more likely to be rejected by creditors. However, there is no statistically significant effect for below-median asset filers (column (3)). When we split the sample based on home equity, we find that proposals from above-median home equity filers in the Manipulation Zone are more likely to be rejected by creditors (column (4)). On the other hand, there is no statistically significant effect on rejections for below-median equity filers (column (5)). Finally, creditors do not seem to reject proposals with more round numbers compared with less round numbers (columns (6) and (7)). Thus, we find that high asset holding and high home equity may prompt creditors to reject proposals in the Manipulation Zone, which suggests that they respond to data manipulation among filers where the reported SI is incongruous with other filing details (e.g., a low reported SI, but high asset values).

[28] As we discuss in detail in Appendix C.3, financial statements with many round numbers can be indicative of data misreporting.

While outright proposal rejection is the most direct potential creditor response to data manipulation, we also consider contract term adjustments in response to debtor manipulation. This distinction between creditors rejecting a contract with a debtor outright versus attempting to adjust the terms of the contract with the debtor has been examined in a variety of credit market contexts.[29]

The results of our analyses of contract terms are reported in Table 7. We report results on five contract terms. These results indicate that there are small economic and statistically insignificant changes for proposals in the Below $200 Manipulation Zone relative to the Comparison Zone (SI between −$400 and −$100) from the pre- to the post-reform period. As these outcomes reflect equilibrium outcomes of negotiations between creditors and debtors (rather than the direct actions of creditors), we find that data manipulation does not lead to higher debt repayment (in terms of dollars or repayment rate), shorter maturity, or fewer withdrawals. This result suggests that, in aggregate, creditors do not respond to data manipulation by changing proposal contract terms.[30] Taken together, our results show that creditors respond to manipulation with an outright rejection of proposals with incongruous details (e.g., high home equity and assets with low income), rather than changing the terms of proposals, consistent with the aforementioned evidence on creditor behavior in other credit market contexts.

7. Evidence on identification assumptions

While we argue that the 2009 bankruptcy reform induced some proposal filers with high SIs to strategically manipulate SIs below the $200 SI threshold, potential proposal filers could also respond to the bankruptcy reform by “switching” out of or into proposals. These insolvent debtors could, instead, file for bankruptcy within the consumer insolvency system or exit the system altogether (and risk foreclosure, default, wage garnishment, or other creditor collection actions).

While there are several forms of extensive margin “switching” that may exist, two forms, in particular, offer alternative explanations for the bunching we document below the $200 SI threshold after the policy change. First, entry into proposals below the $200 SI threshold of new debtors after the reform could be responsible for the bunching result. Second, exit from proposals above the $200 SI threshold could make the region below the threshold seem to have bunching (excess filings). We consider these alternative explanations in the following sections.

7.1. Entries below the threshold

Although entry of new filers into proposals below the $200 SI threshold after the reform could, in theory, be a potential reason for bunching below that threshold, it is implausible, given the specifics of the reform. First, the 2009 reform did not substantially change the terms of proposal filings with SIs under $200 and net debts below $75,000, which we study in this paper.[31] Therefore, debtors who would

[29] The seminal credit rationing paper of Stiglitz and Weiss (1981) provides a theoretical model of credit rationing with creditors rejecting high risk borrowers rather than charging them higher interest rates. DeFusco et al. (2020) considers a quantity and price response to mortgage market regulation. Chakrabarti and Pattison (2019) documents the effect of a policy change on auto loan originations and interest rates.

[30] We do not have sufficient statistical power to estimate small economic effects. Thus, our estimates should be interpreted as imprecise zero effects. When we examine subsamples similar to Table 6 for contract terms, we do not find robust statistically significant results for these subsamples, possibly due to small sample sizes.

[31] Allen and Basiri (2018) show that the total number of proposal filings increases significantly after the reform. The relevant figures in that paper incorporate the effects of another part of the reform that encouraged increased proposal filings: Debtors with net debt (debt minus principal residence's mortgage) between $75,000 and $250,000 were newly allowed to file consumer

Table 6
Effect of bunching on proposal rejection.

	(1) Overall	(2) High Assets	(3) Low Assets	(4) High Equity	(5) Low Equity	(6) High Round	(7) Low Round
Below \$200 MZ × Post	1.074	1.453**	0.893	3.100***	0.945	1.011	1.164
	(0.66)	(2.07)	(−0.82)	(2.79)	(−0.16)	(0.08)	(0.93)
Below \$200 MZ	0.981	0.769	1.105	0.364***	1.060	0.947	1.018
	(−0.20)	(−1.60)	(0.84)	(−2.66)	(0.19)	(−0.43)	(0.12)
Below \$0 MZ × Post	0.918	0.993	0.910	1.298	0.612	0.907	0.928
	(−0.64)	(−0.03)	(−0.56)	(0.48)	(−1.15)	(−0.54)	(−0.38)
Below \$0 MZ	1.121	1.042	1.130	0.698	1.335	1.104	1.145
	(0.98)	(0.21)	(0.84)	(−0.71)	(0.83)	(0.63)	(0.77)
Controls	Y	Y	Y	Y	Y	Y	Y
Fixed effects	Y	Y	Y	Y	Y	Y	Y
Model	Logit	Logit	Logit	Logit	Logit	Logit	Logit
R^2	0.062	0.065	0.069	0.091	0.100	0.070	0.067
Observations	219,282	84,713	134,492	19,674	17,713	109,368	109,861

This table reports the results of estimating Eq. (5) comparing the probability of proposal rejection for Manipulation Zone filings and Comparison Zone filings. The regression reported in column (1) is performed on all proposals with SI between -\$2,000 and \$200. The regressions reported in the remaining columns are performed on subsets of that sample, as labeled in the column header. The median values of assets, home equity, and share of round numbers in filing are used to define subsamples in columns (2) to (7). Three additional Surplus Income (SI) bins are included in each regression but omitted from this table for brevity: SI between -\$2,000 and -\$1,200; SI between -\$1,200 and -\$800; and, SI between -\$800 and -\$400. The control variables include all available filer and filing characteristics as reported in Table 1. The fixed effects include filing type, liability type, province of residence, occupation category, and filing year–month. The SI ranges for the Below \$0 Manipulation Zone, Below \$200 Manipulation Zone, and the Comparison Zone are -\$100 to \$0, \$0 to \$200, and -\$400 to -\$100, respectively. The reported coefficients are odds ratios. t-statistics are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 7
Effect of bunching on loan terms.

	(1) Repay amt	(2) ln(Repay amt)	(3) Repay ratio	(4) Maturity	(5) Mat > 60mths	(6) Withdrawn
Below \$200 MZ × Post	−83.86	−0.0105	−0.00211	0.0119	1.069	0.992
	(−0.96)	(−1.50)	(−0.58)	(0.05)	(1.35)	(−0.08)
Below \$200 MZ	13.63	0.0167**	−0.00142	0.376	0.913*	1.014
	(0.17)	(2.53)	(−0.41)	(1.62)	(−1.92)	(0.14)
Below \$0 MZ × Post	−64.96	−0.00777	−0.00115	−0.111	1.003	1.035
	(−0.59)	(−0.88)	(−0.25)	(−0.37)	(0.05)	(0.24)
Below \$0 MZ	−41.91	0.00230	−0.00381	0.301	0.980	0.903
	(−0.41)	(0.28)	(−0.88)	(1.06)	(−0.35)	(−0.79)
Controls	Y	Y	Y	Y	Y	Y
Fixed effects	Y	Y	Y	Y	Y	Y
Model	OLS	OLS	OLS	OLS	Logit	Logit
R^2	0.491	0.462	0.348	0.091	0.076	0.041
Observations	217,917	217,917	217,917	220,956	221,757	214,051

This table reports the results of estimating Eq. (5) comparing loan terms for Manipulation Zone filings and Comparison Zone filings. Each regression is performed on proposals with SI between -\$2,000 and \$200. Three additional Surplus Income (SI) bins are included in each regression but omitted from this table for brevity: SI between -\$2,000 and -\$1,200; SI between -\$1,200 and -\$800; and, SI between -\$800 and -\$400. The control variables include all available filer and filing characteristics as reported in Table 1. The fixed effects include filing type, liability type, province of residence, occupation category, and filing year–month. The SI ranges for the Below \$0 Manipulation Zone, Below \$200 Manipulation Zone, and the Comparison Zone are -\$100 to \$0, \$0 to \$200, and -\$400 to -\$100, respectively. Coefficients in columns 5 and 6 are reported as odds ratios. t-statistics are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

not have entered the consumer insolvency process prior to the reform would have no new incentives to enter afterwards either. This lack of change in rules for the below-threshold region suggests that entries below the threshold are unlikely to explain the bunching we observe.

Moreover, recall that, because of the "informal floor" feature of the consumer default process in Canada discussed in Section 2, those who would otherwise have filed bankruptcy have no repayment-based reason to file proposals before or after the reform. This is because the amount that debtors need to offer creditors in proposals (in order for their proposals to be accepted) needs to be at least as large as the amount that the creditors would be repaid in bankruptcy. As this informal floor link between bankruptcy and proposal is unchanged by the reform, someone who would have otherwise filed a bankruptcy has no reason to file a proposal after the reform. Given these two key institutional details, it is unlikely that there is sufficiently increased entry into proposals from bankruptcy or solvency below the \$200 SI threshold after the reform to explain the observed bunching.[32]

7.2. Exits above the threshold

How might increased exits above the \$200 SI threshold after the reform explain the bunching we observe below that threshold? If the reform induces debtors with SIs above \$200, who would otherwise

proposals. We exclude these newly eligible filers from our data. Without them, there are no substantial inflows to proposals around the \$200 SI cutoff after the reform, as we show in Fig. 11.

[32] Entries below the threshold may also systematically alter the composition of the filings in that region. For our debtor default analysis in Section 5, we compared the change from before the policy change to afterward in the observable characteristics of filings in this bunching region ($SI \in (0, 200]$) to filings in a comparable region ($SI \in (-400, -100]$). We reported these comparisons in Table 3. Across all observable characteristics, we found no substantial evidence of an abnormal change in the filings in the bunching region, which is further evidence that is inconsistent with this extensive margin explanation for bunching.

have filed proposals, to exit the proposal system, then there will be a dearth of filings above $200 SI after the reform. This shortage above the threshold could make the filing count below the threshold appear to increase after the reform, in comparison.

In order to address this alternative explanation, in this section, we provide a large battery of different tests exploiting various elements of our data that are observable to us. First, we consider subgroups of filers with different propensities to exit proposals and manipulate data. Second, we consider the benefit from proposal for marginal filers around the reform. Finally, we examine the dynamics of filer counts around the reform.[33]

7.2.1. Bunching heterogeneity across filing subgroups

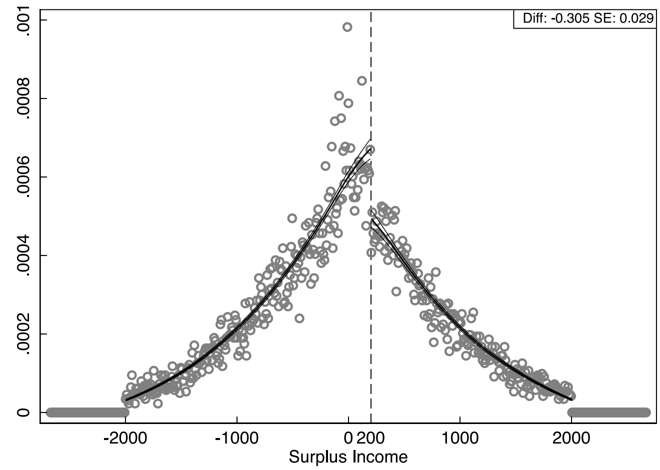
In this subsection, we use subsamples of filers that differ in the costs of proposal exit or ability to manipulate income to assess the impact of extensive margin exits from proposals on the observed bunching. We exploit these differences and study key subgroups of filers to determine whether exits from proposal could be the main cause of the bunching we observe below $200 SI. Using the standard Chetty et al. method, we estimate bunching magnitudes for three sets of subgroups: (a) self-employed versus wage-earning filers, (b) homeowners, and (c) high-asset filers. Our findings across all three analyses provide evidence inconsistent with exits from proposal filing above the threshold causing the observed bunching below $200 SI.

Self-employed versus wage-earning filers. As documented in the previous literature (e.g., Kleven and Waseem, 2013; Kleven et al., 2011; Garmaise, 2015), self-employed (SE) individuals can manipulate their reported income (and, therefore, their SI) more easily than wage-earning (WE) individuals. However, in our setting, OSB regulations do not differentiate between SE and WE filers, which implies that SE and WE individuals should be similar in terms of their proposal filing choices (other than choices regarding income manipulation). Thus, the reform should have the same effect on the probability of SE and WE individuals deciding to exit the proposal system.

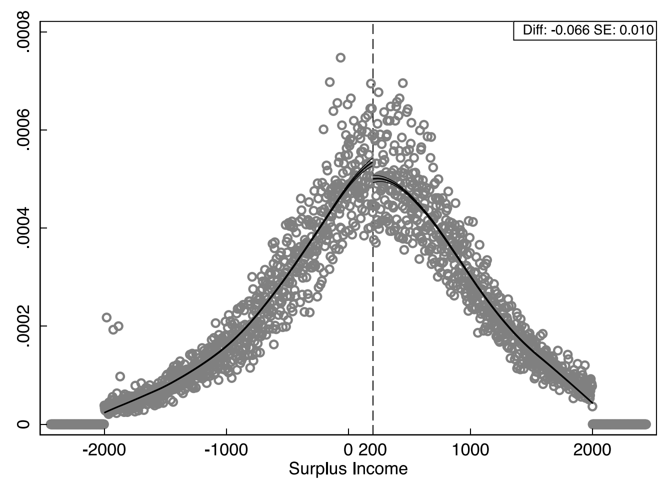
We use this difference in manipulation ability to formulate and test two alternative hypotheses. If the bunching we observe is entirely driven by exits, then there should be no difference in bunching between SE and WE groups. Alternatively, if the bunching is driven by filers manipulating income, then we should observe more bunching in the SE group compared with the WE group because the SE group can manipulate income more easily than the WE group. We test these two alternative hypotheses and present results in Figs. 8 and 9.

In our McCrary discontinuity tests, presented in Fig. 8, we observe that the discontinuity at $200 SI is five times larger among SE filers than the discontinuity among WE filers. Bunching magnitude is also much larger for SE filers than for WE filers. As we show in the first two subfigures of Fig. 9, bunchers constitute approximately 6.3% of WE filers in the region below $200 SI whereas they constitute 26% of SE filers in that region. The five-times-larger discontinuity at the threshold and four-times-larger bunching magnitude among self-employed proposal filers, who find it relatively easier to manipulate SI, are highly inconsistent with the extensive margin explanation that the observed bunching after the reform is driven solely by exits from proposal filings above the threshold.

Homeowners. Insolvent homeowners who file for bankruptcy or go into foreclosure can lose their houses in the process, whereas, under proposal, they are allowed to keep their homes. This treatment of homes is unchanged by the reform and, therefore, homeowners tend to prefer proposal to the other forms of insolvency both before and after the reform. As a result, they are unlikely to switch out of proposal filing because of the reform. If the bunching we observe is driven by exiting



(a) Self-employed filers



(b) Wage-earning filers

Fig. 8. McCrary (2008) Discontinuity Tests Among Self-Employed and Wage-Earning Filers
This figure displays the results of McCrary (2008) discontinuity tests performed at $200 Surplus Income (SI) cutoff for proposal filings submitted after the 2009 policy change for two kinds of proposal filers. Panel (a) and (b) display results for tests performed on post-reform proposal filings with an SI between −$2000 and $2000 submitted by self-employed and wage-earning filers, respectively. In each panel, the magnitude of the discontinuity and its standard error are reported in the upper-right corner.

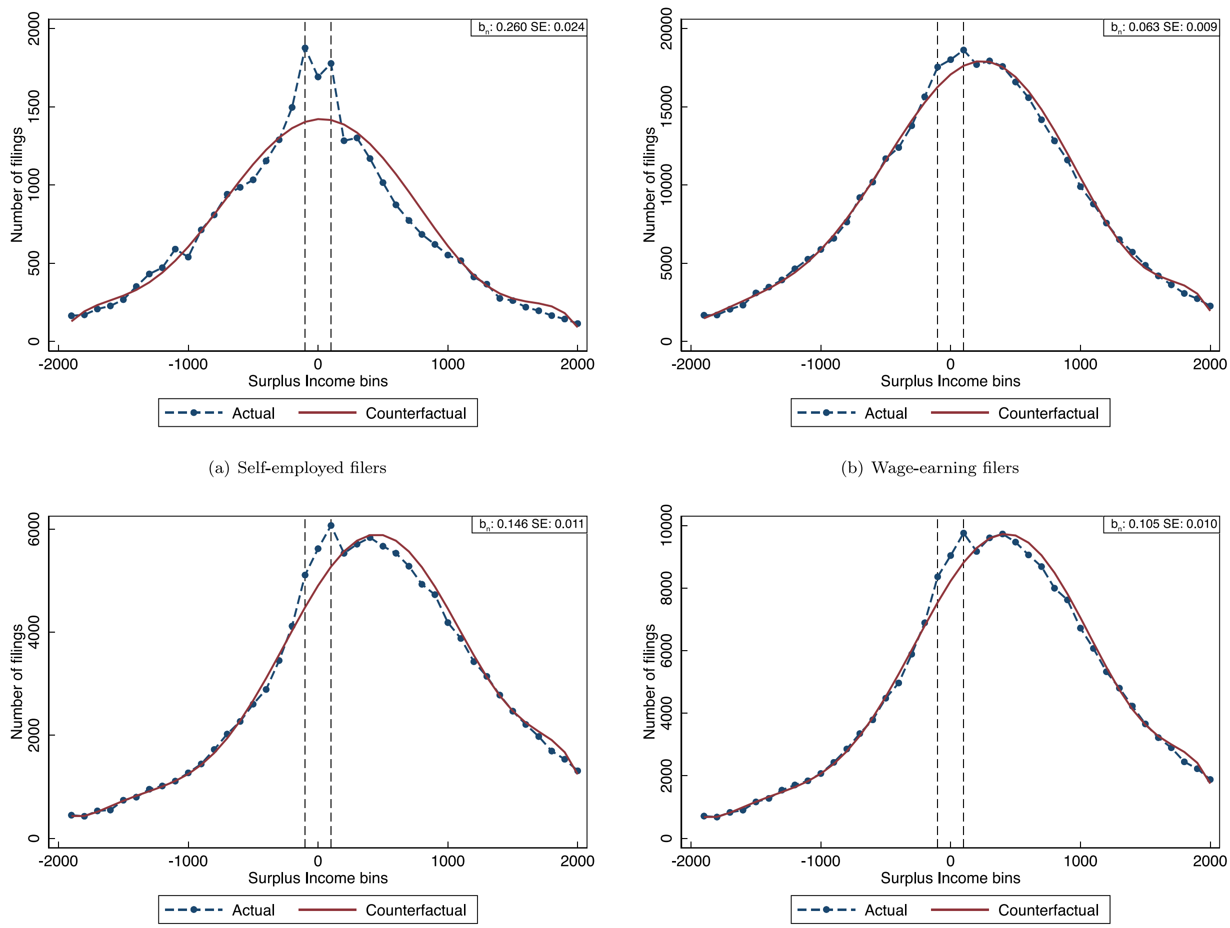
out of proposals after the reform, then homeowners, who are unlikely to exit, should not exhibit any bunching.

Proposal filers who own homes bunch quite significantly, as we report in subfigure (c) of Fig. 9. Approximately 14.6% of homeowning filers in the region below the $200 SI threshold are bunchers (compared with 7.9% in the overall sample). The considerable level of bunching among homeowners is inconsistent with the argument that the observed bunching is entirely caused by exits from proposals above the threshold after the reform.

High-asset filings. Debtors whose proposals have high asset values are unlikely to exit proposals after the reform because high-asset filers benefit from proposals as they can protect their assets from seizure by creditors.[34] Therefore, if the bunching we observe is driven primarily

[33] In Appendix B, we estimate the extent of exits above the threshold by applying and relaxing the integration constraint in turn and comparing the level of bunching below the threshold using the two analyses.

[34] We study both high-asset filings and homeowner filings separately, despite both having high asset values because homeowners may have some idiosyncratic homeownership-related liquidity needs (e.g., home maintenance costs)



(a) Self-employed filers

(b) Wage-earning filers

(c) Homeowners

(d) High-asset Filings

Fig. 9. Estimation of Bunching Magnitude Among Subgroups of Filings
These figures show the results of estimating bunching magnitude using Surplus Income (SI) bins of size \$100 and a 7th degree polynomial to model the counterfactual distribution for four subgroups of proposal filers. Panel (a) plots the bunching magnitude for self-employed filers, panel (b) plots it for wage-earning filers, panel (c) plots it for homeowner filings, and panel (d) plots it for high-asset filings. Each panel shows results of estimations performed on the relevant subgroup of post-reform proposal filings with an SI between −\$2000 and \$2000. In all four figures, the horizontal axis represents SI bins (of size \$100 each), the vertical axis represents the number of filings in the post-reform period in each bin, the dashed line is the actual number of filings per bin, the red smoothed curve is the estimated counterfactual distribution of filings per bin, and the black vertical dashed lines indicate the exclusion region, $SI \in (-100, 200)$. The estimated bunching magnitudes, b_n, and their standard errors are reported in the upper-right box in each subfigure.

by exits from proposals above the \$200 SI threshold, we should not find any bunching among high-asset filings, who are unlikely to exit proposals.

We estimate bunching magnitude for filers whose total assets are above the median (approximately \$18,000). As we report in subfigure (d) of Fig. 9, we find considerable bunching in the high-asset value subgroup, with approximately 10.5% of filings in the below-threshold region being bunchers. Again, high-asset filings exhibiting significant bunching is inconsistent with the notion that exits from proposal drive the bunching we observe below the threshold.

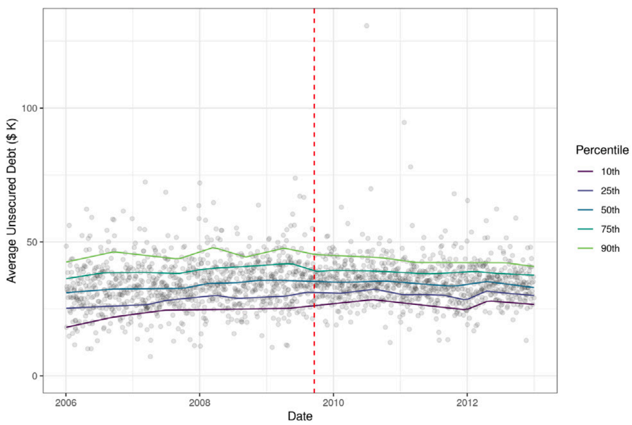
7.2.2. Filer dynamics near the \$200 SI threshold

If exits from proposals above the threshold drive our bunching findings, then we should observe some specific changes in the filer distribution near the threshold around the time of the reform. Below, we study two statistics that, if exits from proposal were driving bunching, should change in a specific way. Our findings are again inconsistent with exits above the threshold being the main cause of the observed bunching.

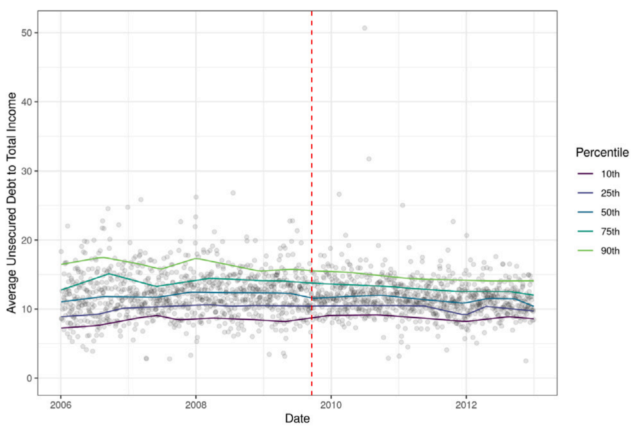
Marginal Filer Distribution. One possible reason for exit from proposals is that the 2009 reform imposed new costs on filers, which could impact their cost–benefit calculations. For filers with a very high benefit of insolvency, it is unlikely that any new costs from the 2009 reform would persuade them to exit the insolvency process, and thus lose those benefits. If there is exit from proposals above the threshold after the 2009 reform, potential filers with *low* benefits from filing would be the most likely to exit. In other words, these “marginal” filers should constitute a significant proportion of the (unobserved) above−\$200-SI post-reform proposal exits. If the reform caused marginal filers with low benefits of filing to exit, we should observe an upward shift above \$200 SI in the distribution of the financial benefits of proposals among filers who remain in the sample after the reform. On the other hand, if the reform had little or no effect on the exit of marginal filers, then we would expect the observed financial benefits of proposal to remain similar in the pre- and post-reform periods.

To test this hypothesis, we study the dynamics of the distribution of unsecured debt as the primary benefit of proposal for filers just above

that affect their decision of how to resolve their insolvency. On the other hand, high-asset filers do not necessarily have these homeownership-related liquidity needs.

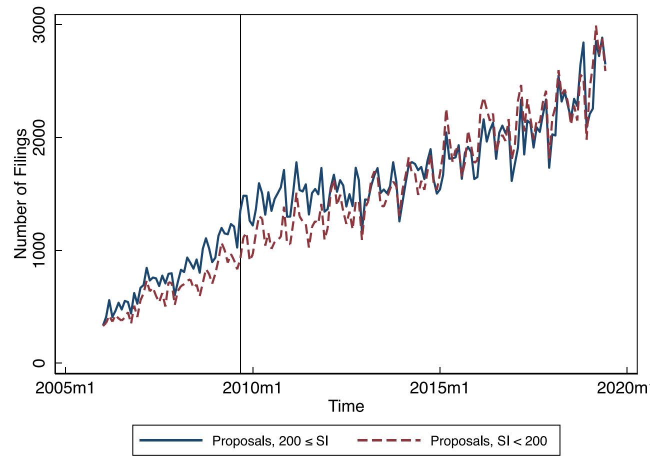


(a) Unsecured Debt

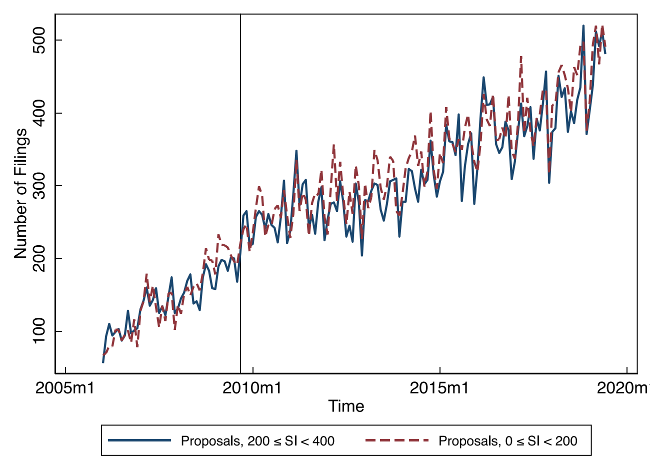


(b) Unsecured Debt to Total Income

Fig. 10. Percentile Plots of Unsecured Debt Dynamics for Filings with an SI Between $200 and $400
This figure plots the values and relevant percentiles of unsecured debt for proposal filings with Surplus Income (SI) between $200 and $400 from the start of 2006 to the end of 2012. Panel (a) plots the absolute levels of unsecured debt of proposal filings, and panel (b) plots unsecured debt of filings relative to the total income reported in the filings. In both panels, unsecured debt values are plotted as gray circles with dark gray outlines, and the 10th, 25th, 50th, 75th, and 90th percentiles of unsecured debt distributions are plotted using solid lines.



(a) All Proposal Filings



(b) Proposal Filings Near $200 SI Threshold

Fig. 11. Proposal Count Dynamics
This figure plots the dynamics of proposal filings at a monthly frequency for January 2006 through June 2019 separated based on reported levels of Surplus Income (SI). Panel (a) plots these proposal counts for all proposal filings, whereas panel (b) plots them for proposal filings with SIs between $0 and $400 (i.e., within $200 of the $200 SI threshold). In both panels, the solid blue line represents the monthly number of proposal filings with SI above $200 (inclusive) and the dashed red line represents monthly filings with an SI under $200. The vertical line in both panels represents September 2009, the month of the policy reform.

the cutoff around the reform date.[35] In Fig. 10, we examine unsecured debt in dollar terms (in panel (a)) and relative to total income (in panel (b)). The two panels plot the 10th, 25th, and 50th percentile of the two unsecured debt distributions for filings just above the $200 SI threshold (i.e., with SI between $200 and $400). Both panels show that the distribution of unsecured debt evolves smoothly from before to after the policy was implemented in September 2009. The 10th percentile dynamics are especially important as this is where marginal-benefit filers are likely to be concentrated. Even for this percentile, we do not find any significant shift in proposal benefits after the reform's implementation. This result is inconsistent with the extensive margin argument that the observed bunching is driven primarily by exits from proposal of (marginal-benefit) filers with SI above $200 after the reform.

Filer Counts. If the bunching we observe arises from above-threshold insolvent debtors exiting proposals after the reform, we should observe a dip in filer counts above $200 SI following the reform. We examine this hypothesis and present results in Fig. 11. This figure plots all proposal counts above and below the $200 threshold (panel (a)) and counts of proposals in a narrow SI range around the cutoff (panel (b)). Both panels show no evidence of a decline in the number of proposals with SI over $200. This result is again inconsistent with the alternative explanation that proposal exits above the threshold after the reform are a major driver of our bunching finding. In addition, we do not find any evidence of substantial entry into proposals for the group of filings below the cutoff. This result supports our earlier institutional argument that new proposals below the cutoff are unlikely to enter the proposal system after the reform.

To summarize, in this section, we explored two extensive margin-based alternative explanations for the bunching we observed below the $200 SI threshold. First, we provided institutional details about

[35] Recall that unsecured debts are discharged after a successful proposal completion, thus they capture the benefit of proposal. We do not include SI payments (which is the primary cost of proposal) into our benefit calculation because the SI payment requirement changes discontinuously at the cutoff after the reform.

the reform and consumer default in Canada that show there is no systematic reason for increased entry into proposal below the threshold and, therefore, this alternative explanation is unlikely to be a major reason for the post-reform bunching we observed. Second, we considered the plausibility of exits from proposal above the threshold as the main driver of the bunching. Given our findings for bunching among key subgroups of our data, and filer dynamics around the time of the reform, this alternative explanation is also unlikely to be a primary cause of the observed bunching. Overall, the evidence in this section strongly suggests that extensive margin responses to the reform are not exclusively responsible for the observed bunching and that it should, in large part, be caused by strategic SI manipulation by bunchers.

8. Suggestive evidence of fraudulent manipulation

Is the bunching we observe caused by legal or fraudulent income manipulation? Distinguishing between legal and fraudulent manipulation is difficult because agents involved in fraudulent activities expend effort to ensure that their fraud is not observable.[36] In this section, we briefly summarize some suggestive evidence consistent with fraudulent behavior by some debtors. We describe all of the tests and findings in much greater detail in Appendix C.

8.1. Bunching and travel-related costs of proposal filing

As we describe in detail in Appendix Section C.1, under Canadian insolvency law, every consumer proposal filed by a debtor has to be submitted by an officer of the bankruptcy court called a Licensed Insolvency Trustee (LIT), who is typically a for-profit accountant. The debtor is free to select any trustee from the trustees licensed by the OSB at the time. While the debtor may select any trustee, trustees cannot compete on price because the OSB regulates the prices that trustees can charge debtors. Geographic distance could also play an important role in the selection of trustees as, under the law, the debtor must conduct at least three face-to-face meetings at the office of the trustee.

For debtors who do not intend to manipulate income in their filings, a closer trustee is, all else equal, preferable to a more distant one because trustees use identical OSB forms and charge identical fees, but more distant trustees require additional travel costs. Therefore, such debtors should select the geographically closest trustee to minimize geographic transactions costs. There may be other reasons for preferring a more distant trustee (e.g., cultural affinity or a shared language), but such factors are unlikely to be correlated with the bunching that we observe. However, this calculation may be different for debtors who intend to fraudulently manipulate SI. Such debtors would like to minimize geographic transactions costs but would also like to locate a trustee that allows them to submit a fraudulent filing. A debtor with intent to fraudulently manipulate income must, therefore, balance the benefit of a more lenient trustee with the cost involved in finding and employing this more lenient but potentially more geographically distant trustee.

Because we observe the exact location of every insolvency filer and every insolvency trustee, we are able to calculate the “excess distance” the filer travels to the selected trustee as the additional kilometers traveled by the filer to their selected trustee compared with the average distance to the three trustees located closest to the filer.[37] Using this measure of excess distance traveled to the selected trustee, we then examine whether those debtors who travel larger excess distances to access more distant trustees (despite having closer trustees available to them) have a larger bunching magnitude below the cutoff after the reform compared with filers who do not travel such large excess distances. We report our findings in Appendix Figures A6, A7, and A8. Our main result is that there is approximately 35% more bunching below the $200 SI threshold in the “Distant Trustees” sample than in the “Nearby Trustees” sample. Moreover, based on our bootstrapped t-tests (see Appendix C.2 for more details), this difference is highly statistically significant. This finding provides suggestive evidence that at least some debtors who bunch below the threshold are willing to incur larger transaction costs to find and employ a more distant trustee who may be more amenable to fraudulent income manipulation.

8.2. Lenient trustees and rounding in proposals

In Section 8.1, we show that proposal filers who bunch are more likely to choose to work with more distant trustees even though closer trustees are available. Why do potentially fraudulent debtors opt to incur greater travel-related costs? We argue that there may be more “lenient” trustees who are more likely to submit fraudulent proposals on behalf of a debtor. To test our argument, we examine whether filings employing more historically lenient trustees have a greater bunching mass below the $200 SI threshold. We measure trustee leniency based on the prevalence of round numbers in proposals (e.g., reporting numbers in the multiples of $100) for all filings submitted by the trustee in the past (see Appendix C.3 for details). A finding of more bunching among more lenient trustees would be suggestive evidence that debtors who intend to manipulate income are more likely to search for and use more lenient trustees.

While rounding has the advantage of being observable to us, it has some issues as a measure of fraud. First, approval of filings with round numbers by financially sophisticated trustees may result from either low effort (i.e., shirking) or fraud. Second, sophisticated filers may have other (better) ways to manipulate data than data rounding, that may be easier to get approved by a trustee. Third, it is possible that a trustee could round numbers to the benefit of creditors (i.e., increase SI to above the cutoff), rather than to the benefit of debtors (i.e., reduce SI to below the cutoff). All three of these issues work against us finding evidence of fraud using round numbers. Therefore, any evidence we do find linking bunching to round numbers would arise despite these measurement issues.

Given these caveats, we examine whether bunching magnitude is different for filers working with more historically lenient trustees compared with filers with less lenient trustees (see Appendix C.3). In summary, our results, presented in Figures A9, A10, and A11 in the Appendix, show that the bunching of filings below the $200 surplus income cutoff among more lenient trustees is nearly twice as large as bunching among less lenient trustees (11.2% versus 6.8%) and, based on our bootstrapped t-tests (Figure A7), this difference is highly statistically significant. This finding is consistent with filers intent on fraud selecting more lenient trustees in order to have their manipulated filings approved.

9. Conclusions and policy implications

In this paper, we study the strategic income manipulation caused by forcing some insolvent consumer debtors to repay more of their debt. First, we document that a significant proportion of the affected debtors misreport their income to avoid the increased repayment, evidenced by the bunching of filings we observe just below the threshold, above which debtors must repay a larger fraction of their debt. Next, we study whether the income-manipulating bunchers benefit from their actions in observable ways. Indeed, we find that these bunchers have a lower probability of default on their repayment plans than their

[36] The difficulty in identifying fraud can be observed in the existing bunching literature. For instance, tax research that attempts to disentangle legal and fraudulent tax avoidance often does so by exploiting idiosyncratic details of its empirical setting. We follow a similar strategy to search for any evidence of fraudulent income manipulation in this section.

[37] In Appendix C.2, we describe various alternative measures of excess distance, including whether the debtor selects the closest trustee or not, and obtain similar results.

peers, which is consistent with them having access to hidden income. We also examine how creditors are affected by this strategic income manipulation by their insolvent debtors. We show that, on average, each income-manipulated filing costs creditors 12% to 36% of their total repayment amount. Furthermore, we find some evidence that, among subgroups of filers where the reported income and other details are incongruous, creditors are more likely to reject filings submitted by bunchers. Finally, we offer evidence that the bunching we observe is unlikely to be solely caused by entry into proposals below the threshold or exit from proposals above the threshold and, therefore, it is driven in large part by strategic income manipulation.

Based on our findings, we offer a few implications. The most fundamental implication of our study is that government intervention in a credit market (in our case, the introduction of the large discontinuity in a debt repayment schedule) can cause an increase in the information asymmetry between debtors and creditors (in our case, increased data manipulation by debtors). This is the exact opposite of a commonly proclaimed goal of government interventions to improve the functioning of credit markets by reducing information asymmetry and opacity.

Second, our findings highlight the potentially problematic incentives created by regulatory discontinuities and thresholds. Such discontinuities can induce strategic information manipulation, which may lead to credit market distortions. We also show that debtors can manipulate their income and other financial information downward, in addition to findings in the previous literature documenting debtors inflating their financial position. Our findings, therefore, would urge regulators to consider carefully how (rather than if) market participants may react to regulatory thresholds by strategically manipulating the information they report. Similar incentives exist in other credit markets (e.g., means-tested programs, wage garnishment schedules, mortgage markets thresholds), and this conclusion may apply to them as well.

Third, our paper highlights the potentially unintended consequences of regulatory attempts to increase debtor repayments to creditors. Prior literature has documented that requiring debtors to repay more to their creditors leads to financial distress and default. We focus on debtor strategic information manipulation as a previously undocumented response to such regulation. Our findings imply that requiring higher income-contingent payments from debtors may unintentionally generate increased avoidance of such payments and, in doing so, be costly to creditors. Therefore, our findings would suggest that regulators should design such policies carefully to avoid negating their intended effect.

Fourth, our results also suggest that creditors do not fully counteract debtor data manipulation. Even though, in our context, creditors reject some proposal filings where income and asset data seem incongruous, debtors are still able to manipulate their income data strategically. In addition, we document that the involvement of financial intermediaries (insolvency trustees), who are required to verify financial data in proposal filings, does not eliminate debtor strategic data manipulation. These findings may be important for designing other similar policies in the context of bankruptcy, debt recovery, and debt renegotiation.

CRediT authorship contribution statement

Vyacheslav Mikhed: Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Sahil Raina:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Barry Scholnick:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Man Zhang:** Writing – review & editing, Writing – original draft, Visualization, Validation, Supervision, Software, Resources, Project administration, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

https://data.mendeley.com/datasets/99ngcz6fwj/2.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jfineco.2024.103851.

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