Debtor Fraud in Consumer Debt Renegotiation

Vyacheslav Mikhed
Federal Reserve Bank of Philadelphia
Consumer Finance Institute

Sahil Raina
Alberta School of Business

Barry Scholnick
Alberta School of Business and
Federal Reserve Bank of Philadelphia Consumer Finance Institute Visiting Scholar

Man Zhang
University of Sydney Business School
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Vyacheslav Mikhed, Sahil Raina, Barry Scholnick, and Man Zhang∗

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Abstract

We study how forcing financially distressed consumer debtors to repay a larger fraction of debt can lead them to misreport data fraudulently. Using a plausibly exogenous policy change that required debtors to increase repayment to creditors, we document that debtors manipulated data to avoid higher repayment. Consistent with deliberate fraud, data manipulators traveled farther to find more lenient insolvency professionals who, historically, approved more potentially fraudulent filings. Finally, we find that those debtors who misreported income had a lower probability of default on their debt repayment plans, consistent with having access to hidden income.

Keywords: consumer credit, fraud, data misreporting, financial distress, default

JEL Codes: G21, G51, D82

∗Mikhed: Consumer Finance Institute, Federal Reserve Bank of Philadelphia, email: slava.mikhed@phil.frb.org
Raina: Alberta School of Business, University of Alberta, email: sraina@ualberta.ca
Scholnick: Alberta School of Business, University of Alberta, email: barry.scholnick@ualberta.ca
Zhang: University of Sydney Business School, email: man.zhang@sydney.edu.au

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1 Introduction

Understanding debtors’ incentives to fraudulently misreport information to creditors and the consequences of that misreporting is important. Misreported financial information can lead to a misallocation of credit by lenders, increased information asymmetry between debtors and lenders, and cause other credit market distortions. Clearly, lenders and governments would benefit from a better understanding debtor information misreporting. Despite this, existing evidence on household debtor fraud is scarce, mostly limited to the mortgage market during the financial crisis (see Griffin, 2021, for a survey). Little is known why and how consumer debtors misreport information to creditors overall, and the effect of this misreporting on credit outcomes. We address this issue by studying: (1) how changes in incentives to commit fraud in the context of household debt modification affect debtors’ misreporting of financial information, (2) the role of financial intermediaries in the misreporting, and (3) the effect on credit contract defaults and other equilibrium debtor-creditor negotiation outcomes.

We study debtor misreporting by exploiting plausibly exogenous variation in the incentives of debtors to misreport financial information to creditors stemming from a policy change in 2009. The specific credit contracts we examine are called consumer proposals in Canada (which have some similarities with 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) in Canada increased the amount that some proposal-filing debtors were required to pay creditors. This change in repayment, in turn, altered those debtors’ incentives to misreport their information to creditors.

Under the law, the amount payable to creditors in a consumer proposal depends on the debtor’s Surplus Income (which is reported income minus allowable expenses). The reform introduced a sharp discontinuity in the total amount of repayment for debtors with a Surplus Income (SI) greater than or equal to $200. Because of the structure of the payment schedule, debtors are required to pay an additional $1,200 over the life of the plan when their reported SI increases from $199 to $200. For other debtors, whose SI is below that income cutoff, the reform had no effect on payments to creditors. This plausibly exogenous increase in the total payment amount created an incentive for debtors with the SI over $200 to reduce the SI reported to their creditors 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 misreport. In simple terms, the bunching methodology proposes a hypothesis that, without data manipulation, the distribution of filings in an SI (our running variable) should be smooth around the cutoff. A discontinuity in the distribution (too many or too few filings on one side of the cutoff) indicates that filers manipulate surplus income to “bunch” on the more advantageous side of the cutoff. Using this methodology, we show that the higher debt repayment requirement created bunching responsible for 7.8% of post-reform filings just below the $200 cutoff. This result is consistent with insolvent debtors responding to the increased incentive to avoid higher income-contingent payments by manipulating their reported SI downward.

To support our argument that the misreporting of income by bunching debtors is fraudulent, we provide evidence involving the trustees chosen by debtors, which is consistent with fraudulent misreporting after the reform. All else equal, filers who do not intend to submit fraudulent proposals should prefer nearby trustees, as trustee services and fees are tightly regulated, and trustees who are farther away require filers to incur greater transactions costs. Specifically, we show that bunchers in the post-reform period are willing to incur greater travel-related transactions costs to work with more lenient trustees (who are more likely to allow a fraudulent filing). Measuring these transactions costs as the excess distance between a filer and their chosen trustee, we document significantly more bunching just below the $200 cutoff, on average, among debtors who choose to work with more distant trustees, despite nearby trustees being available to them. We confirm that, based on the prevalence of round numbers in their submitted filings, more distant trustees tend to be more lenient. Studying trustee leniency choice directly, we confirm that there is significantly more bunching below the $200 cutoff among filers employing historically lenient trustees. Jointly, these findings indicate that, on average, bunchers incur greater travel-related costs to engage lenient trustees in order to submit fraudulent filings.

To explain why some trustees are more lenient, we show that more lenient trustees gain market

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1 In the Canadian insolvency context, only Licensed Insolvency Trustees, who are typically for-profit chartered accountants, are allowed to submit insolvency filings. The insolvent debtor may select any licensed trustee to conduct the filing. An important role of a trustee is to verify all information submitted in a proposal filing.

2 This measure is used in previous literature (Garmaise, 2015) and by forensic accountants (see [https://us.aicpa.org/content/dam/aicpa/research/standards/auditattest/downloadabledocuments/au-c-00240.pdf] to detect fraud in financial documents.)
share after the policy change. This is consistent with lenient trustees attracting more fraudulent filers motivated to misreport financial data by the reform. A larger market share and more filings lead to higher profits for these trustees, who are otherwise unable to compete because of the strict regulation of their services and fees. This market share-based motivation may explain some trustees’ willingness to be more lenient and submit suspect filings. More generally, these findings demonstrate how a higher debt repayment requirement may lead to a greater acceptance of fraud among financial intermediaries.

We examine how this fraud affects proposal default using Cox proportional hazards models. We document that debtors who manipulate data become less likely to default on their repayment plans compared with filers in a similar SI range after the policy change, controlling for time-varying differences and other factors. This finding is consistent with debtors who manipulate reported income having extra “hidden” income, which allows them to reduce the long-term default hazard on their repayment plans. Thus, requiring higher income-contingent debt repayment may increase fraudulent data misreporting and exacerbate information asymmetry between debtors and creditors in this credit market.

Despite the presence of debtors’ data manipulation, we do not find evidence that equilibrium outcomes of debtor-creditor negotiations adjust to this manipulation. In particular, on average, the total repayment amount, repayment rate (repayment amount relative to the original debt), repayment plan maturity, and probabilities of proposal rejection by creditors, do not change. Creditors do not seem to react to debtors’ fraud. We propose two potential explanations for this lack of response. Creditors may not adjust contract terms because (1) they benefit from a lower default rate from manipulators who hide income, which may offset their losses from lower repayment, or (2) they are not able to distinguish between debtors who do not manipulate data and those who do.

Our paper makes several contributions to the literature. First, we contribute to the literature on debtor fraud in household finance contexts, which up to now has focused on the mortgage market in a financial crisis. In these studies, debtors typically face an incentive to fraudulently report that their financial situation is better than their actual situation to benefit from better mortgage loan terms. Therefore, in these studies, an increase in debtor fraud is associated with more default. In our setting, by contrast, we document that the debtors’ fraudulent response to an increase in income contingent repayments to creditors is to hide income, which causes a reduction in default.

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3 See Griffin (2021); Ben-David (2011); Elul et al. (2021); Garmaise (2015); Griffin and Maturana (2016); Jiang et al. (2014); Mian and Sufi (2017); Pursiainen (2020); Kruger and Maturana (2021).
because the debtor has more liquidity than reported. Our new results, combined with the existing literature, thus show that debtors may commit fraud in both directions, i.e., either overstating or understating their true financial situation, based on different incentives in different settings.

Second, we inform the household finance literature on the effects of raising payments required of debtors. These studies show that an increase in debtor payments typically causes an increase in default because increased payments reduce debtor liquidity. We examine a different mechanism that allows debtors to reduce default by first underreporting their income to creditors and then using this hidden income to reduce subsequent default. We also document that simply requiring higher income-contingent payments from debtors does not always lead to higher payments to creditors, but it may cause fraudulent income misreporting.

Third, our paper contributes to the literature on policies designed to protect creditor rights. The most relevant literature focuses on the effects of the U.S. Bankruptcy Abuse Prevention and Consumer Protection Act of 2005 (BAPCPA) on consumer and creditor outcomes (e.g., Li et al. 2011; Chakrabarti and Pattison 2019; Gross et al. 2021). Overall, these studies find that BAPCPA strengthened creditor rights by making personal bankruptcy more costly and restrictive, and thus allowed lenders to expand new credit and reduce the cost of credit (interest rates). However, BAPCPA also inadvertently increased mortgage default. Our study differs from these studies, however, because it documents that a policy meant to aid creditors by requiring higher income-contingent debt repayment from debtors in insolvency can lead to debtor fraud, advantageous selection, and the reduction of loan default.

Finally, our study is linked to literature in several fields employing the bunching methodology. We borrow techniques from tax literature, which has used bunching extensively to examine various methods of illegal manipulation of income and fraud. Our paper also forms part of the new and

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4 Mortgage-based studies that examine increased debtor payments because of interest rate dynamics include Campbell (2013), Fuster and Willen (2017), Di Maggio et al. (2017); Tracy and Wright (2016), while Keys and Wang (2019) examine creditors raising minimum payments required from credit card debtors.

growing literature using bunching-based techniques in the context of credit markets. In particular, we join the subset of this literature using bunching to study bankruptcy. Our study extends this literature by using bunching-based techniques to identify fraudulent misreporting of data by insolvent debtors.

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, insolvent debtors make 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, which is 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.

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.

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6 For example, Defusco and Paciorek (2017); Bachas et al. (2021); Defusco et al. (2020).
7 See, Gross et al. (2021); Homonoff et al. (2020); Indarte (2019).
8 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.
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 the asymmetry in the legal rights of creditors, 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 debtors need to offer creditors under proposals 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. Therefore, when there is a regulatory increase in the payments required from debtors to creditors under the rules-based bankruptcy system, it 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 The Change to the Bankruptcy and Insolvency Act - September 2009

Canadian insolvency regulations changed on September 18, 2009, when amendments to the Bankruptcy and Insolvency Act (BIA) passed through the Canadian Parliament. This followed the initial announcement of these amendments by the Office of the Superintendent of Bankruptcy (OSB) on August 14, 2009.Credits

9 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.

10 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).

11 Allen and Basiri (2018) provide a broad overview of these amendments to the BIA. There were other changes due to this reform, but we exclude filings affected by these other changes. Agarwal et al. (2021) also examine this 2009 change to the BIA as an exogenous policy change. However, that paper examines issues of moral hazard and strategic
These 2009 amendments to the BIA did not change any rules relating to how debtors and creditors negotiate consumer proposals. They did, however, increase the income-contingent payments required to be made by some bankruptcy filers to their creditors. These payments are based on the filers’ “Surplus Income” (SI). SI is essentially the income of the debtor minus authorized non-discretionary expenses, after accounting for family size and province of residence. The SI reported by the bankrupt debtor determines the amount of income-contingent payments made to the creditor.

Our identification strategy exploits how the law change affects debtors with different levels of SI in the pre- and post-reform periods. In Figure 1, we present the relationship between reported SI (horizontal axis) and the amount that a bankrupt debtor is required to pay to the creditor in income-contingent payments (vertical axis). As displayed in that figure, the main rule (in both pre- and post-reform periods) is that bankruptcy filers who have a positive SI (i.e., income more than approved expenses) are required to pay their creditors 50% of their SI per month for a specified number of months. On the other hand, 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 with SI above $200 are required to pay creditors from 9 months to 21 months. There were no regulatory changes affecting bankrupt debtors with SI below $200. 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 $1,200 if they reduced their reported SI from slightly above $200 to slightly below $200.

\[ \text{default of pre-reform proposal filers, whereas this paper examines misreporting of information by proposal filers.} \]

\[ ^{12} \text{These authorized non-discretionary expenses are very limited and consist of payments for child and spousal support, medical conditions, and fines and penalties imposed by the court, etc. Full details of the construction of SI are provided in Appendix A.} \]

\[ ^{13} \text{In the post period, a bankruptcy filer with an SI slightly below the $200 cutoff would make payments for 9 months times (50\% of $200) = $900. If that debtor had an SI slightly above $200, she would make payments for 21 months times (50\% of $200) = $2,100. Thus, moving the SI from just above to just below the $200 notch would save $1,200 in payments.} \]

\[ ^{14} \text{While this policy change increased the number of months in bankruptcy for filers with an SI above $200, it had no direct effect on the number of months negotiated between debtors and their creditors in proposals, as it only affects} \]
Figure 1 also reflects various other elements of the regulatory environment. Because the rule change affects all debtors with an SI of more than $200, in Figure 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. 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. Figure 1 also displays a “kink” at $0 for both the pre- and post-reform periods. This kink at $0 reflects the rule (in both periods) that bankruptcy filers pay 50% of a positive SI but do not make any payments if the SI is negative. Thus, income-contingent payments are set to zero for all filers with a negative SI for both the pre- and post-reform periods in Figure 1.

2.3 The Role of Trustees in Insolvency

Licensed Insolvency Trustees (LIT) play a key role in the administration of every insolvency filing in Canada. One of the main motivations for having such an intermediary in the process is to verify the data in the proposal and curtail any fraudulent reporting by the debtor. Trustees are typically for-profit chartered accountants, licensed and regulated by the OSB. Trustees are “officers of the court,” which means that they are legally obligated to represent the interests of both the debtor and the creditors in any insolvency filing. Under OSB rules, the price that a trustee charges a debtor to file for the proposal filing is highly regulated (the trustee receives 20% of the total payments received by creditors under the proposal). Because of this, the trustees are not able to compete on price, but rather they are forced to use other mechanisms to increase their profits, such as market share.

While the main parties to a proposal negotiation and agreement are the insolvent debtors and the creditors, the trustee as a third-party intermediary also plays an important role at various stages of the process. First, a debtor who is planning on filing a proposal is free to select any licensed trustee to undertake the proposal filing, using whatever criteria the debtor thinks is appropriate.

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15 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. The BAPCPA bankruptcy changes in the U.S. in 2005 also introduced a “means test”-based discontinuity, where it is advantageous to report income below state median income (e.g., Gross et al., 2019; White 2007).

16 In the bunching literature, a “kink” is a discontinuity in the relevant slope, while a “notch” is a discontinuity in the relevant level.
Thus, the debtor can select a trustee based on criteria such as geographic distance.

Geography plays an important role in debtors’ choice of trustees. Canadian personal insolvency law requires debtors to conduct at least three separate face-to-face meetings with the selected trustee (see Ramsay [2001] Industry Canada [2013]). Before the actual filing, according to an OSB directive, the debtor must meet the trustee at the trustee’s office to discuss the insolvency process. Two additional meetings, again at the trustee’s office, are also required later in the process to provide mandatory credit counseling. These three meetings impose non-trivial travel-related transactions costs for all insolvency filers.

Second, once the debtor has selected a trustee, the trustee advises the debtor on various issues, including the choice between bankruptcy and proposal, how much the reported SI affects payments to creditors, the amounts likely to be acceptable to creditors, etc. While the debtor signs the proposal filing indicating that all data provided are correct, the filing also needs to be reviewed, signed, and approved by the trustee. All trustee actions are highly regulated, and they have to approve filings based on uniform standards that apply to all trustees. Third, after the proposal filing is approved by the trustee (and agreed to by the debtor), it is then submitted to the creditors by the trustee for their acceptance or rejection. Fourth, during the multiyear repayment period of the proposal contract, it is the duty of the trustee to monitor that the agreed-upon payments are being made by the debtor at the agreed-upon dates. If the payments are not made for three consecutive months, then the trustee declares the debtor in default on the contract and the contract is voided.

3 Data

The main database used in this paper consists of the universe of electronic proposal filings in Canada, filed 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 of our data. As can be seen from this table, our data consist of almost half a million proposal filings.

There are three main components of the data, which are described in the three panels of Table 1. First, we observe 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 state-
ments. We use information about family size, year of filing, income, and expenses to construct the SI. Appendix A provides a detailed description of our SI construction method. Panel A of Table 1 also summarizes data on proposal negotiation outcomes such as total repayment amount, payout ratio, and proposal maturity.

Second, we observe information on each trustee used in proposal filings. Because we have the universe of proposal filings, we compute both filer-level statistics on trustees as well as trustee-level characteristics. Panel B of Table 1 provides summary statistics on the geographic distance (in kilometers) between individual debtors and individual trustees. We use these geographic distances as a measure of the transactions costs required for a debtor to engage a trustee. We describe in extensive detail how we use geographic data in Section 5.

Third, 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 C 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 a true SI greater than $200 to manipulate the reported SI to below $200 as this would significantly reduce their income-contingent payments. In this section, we show that debtors manipulate their reported SI to below the discontinuity using the bunching methodology. Subsequently, in Section 5 we examine whether the manipulation is fraudulent.

4.1 Graphical Evidence of SI Manipulation

As a starting point for the empirical analysis, Figure 2 plots the distributions of SI for proposal filings before and after the policy reform using histograms with SI bins of $40. Figure 2(a) shows that, in the pre-reform period, there is no perceptible discontinuity at $200. However, in Figure 2(b), we see

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).
that, in the post-reform period, there is bunching below the $200 cutoff\footnote{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.} As described in Section\footnote{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.} 2, the $0 SI cutoff is relevant to filers in both the pre- and post-reform periods. This institutional fact is evident in the histograms in Figure\footnote{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.} 2 where we observe a small amount of bunching below $0 in both periods.

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. (2018) discontinuity tests. These results are reported in Figures 3(a) and 3(b). In the pre-reform period, these tests fail to reject the null hypothesis of a continuous distribution at $200 (Figure A2 in Appendix C). However, in the post-reform period, both of these discontinuity tests reject the null hypothesis of continuity at the $200 cutoff with statistical significance at the 99% 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 repayment amount for filers with an SI above $200, led to debtors’ manipulation of SI downward to below the cutoff.

4.2 The Bunching Estimation Methodology

As discontinuity tests do not allow us to 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 to identify bunching 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 there is an incentive for individuals to manipulate the running variable around a certain threshold, then the distribution of this variable would 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. The excess mass and missing mass can be defined by comparing the actual distribution of the running variable with an estimated smooth counterfactual distribution. The actual distribution of the running variable is calculated by dividing the running variable into bins and then counting the number of observations per bin. The counterfactual (smooth) distribution of the running variable is estimated from the bins outside of the bunching region. The bunching magnitude is defined by comparing the difference between the actual counts of observations and the estimated counts from the smooth counterfactual distribution for the bins falling in the bunching region.

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

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

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

$$\hat{b}_n = \frac{\hat{B}_M}{\sum_{j=r_L}^{r_U} \hat{C}_j},$$ (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 bound of the “exclusion region” (which we also refer to as the bunching region), and $r_L$ is the lower bound of the exclusion region. The counterfactual distribution is estimated as in Equation (1) excluding the contribution of bins within the exclusion region: $\hat{C}_j = \sum_{i=0}^{q} \hat{\beta}_i (Z_j)^i$. $B_M$ represents the excess number of filings within the exclusion region, which is the difference between the actual counts $\sum_{j=r_U}^{r_L} C_j$ and the estimated counterfactual distribution $\sum_{j=r_L}^{r_U} \hat{C}_j$. The left-hand side of Equation (1) represents the upward adjustment of the counterfactual estimates above the exclusion region to satisfy the integration constraint, which requires that the missing mass above the cutoff be equal to the excess mass in the exclusion region. The excess mass in the bunching region is thus defined by Equation (3): $\hat{b}_n$ is the excess mass in the exclusion region relative to the total mass under the counterfactual distribution. This amount can be interpreted as the percentage increase of filings in the bunching region because of the discontinuity.

Figure 4 provides a hypothetical application of the bunching magnitude estimation method to our study. The red curve is the actual 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 dotted line in the figure. The dark gray area between the actual and the counterfactual bin counts in the exclusion region is an illustration of excess mass in our 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 SI based on data from the proposal filings with little measurement error. Our

19 Unlike Chetty et al. (2011), we measure $\hat{b}_n$ as the proportion of the exclusion region filings composed of excess mass.
setting in this regard is similar to the tax literature using bunching techniques.

An important element in our context is that the missing mass above the $200 cutoff can be quite diffuse, rather than being concentrated very close to the cutoff. 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 this cutoff. If the SI manipulation is achieved by fraudulent misreporting of data, the cost of such a fraud is primarily a fixed cost, which is not positively correlated with the true SI value. Therefore, fraudulent 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. This is why we employ the method used by Chetty et al. (2011), as its integration constraint is designed for a more diffuse 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 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 Figure 5. The vertical dotted lines demarcate the exclusion region (i.e., $SI \in (-100, 200)$). For this illustration, we use bins of size $100$ and a 7th degree polynomial to estimate the counterfactual distribution. The blue dashed line with filled-in 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 0.078, which means that 7.8% 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, 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. We find statistically significant bunching magnitude across all combinations, where the excess mass in percentage terms are comparable across different settings. For the rest of the paper, we adopt the most conservative specification from column (7).

A critical assumption in the bunching methodology is that there is no significant extensive margin switching near the policy cutoff. For example, if the policy change induced more filers to enter or exit proposal filings just near the $200 cutoff, this would affect our bunching magnitude estimation. In Section 8, we consider each possible direction of extensive margin switching and present evidence inconsistent with extensive margin switching driving our bunching magnitude estimation.

5 Detecting Fraud: Bunching and Proposal Filing Auditing

Our findings so far show that some proposal filers reduce their surplus income after the BIA regulation change, ostensibly to reduce their repayment amounts. Based only on the bunching results in the previous section, however, we cannot determine whether the debtor was fraudulent or not when manipulating income downward. In this section, we identify fraudulent behavior by examining the choice of insolvency trustee by debtors. An important contribution of our paper is that we document fraud in spite of the involvement of trustees, whose presence in the system is designed to prevent fraud.

5.1 Travel-Related Transactions Costs of Fraudulent Proposal Filings

As we describe in Section 2.3 under Canadian insolvency law, every consumer proposal filed by a debtor has to be submitted by a Licensed Insolvency Trustee (LIT), an officer of the bankruptcy court, who is typically a for-profit accountant. As we describe in Section 2.3, the debtor is free to select any trustee from the list of trustees currently licensed by the OSB. In addition, geographic distance could play an important role in the selection of trustees because under the law the debtor is required to conduct at least three face-to-face meetings at the office of the trustee. In this section, we exploit these institutional settings to implement a test for fraudulent behavior, based on distance-related transactions costs.

\footnote{We also report bunching estimation results for different bin sizes and lower bounds of the exclusion region in Figure A3 in the Appendix.}
5.1.1 Trustee Selection and Travel-Related Costs

For debtors who have no intention of making a fraudulent filing, all else equal, a closer trustee is preferable to a more distant one because trustees use identical forms and charge identical fees, but more distant trustees would require additional travel costs. Therefore, such debtors should select the geographically closest trustee to minimize geographic transactions costs. While there may be other reasons for preferring a more distant trustee (e.g., cultural affinity or a shared language), these factors are unlikely to be correlated with the bunching that we observe.

However, this calculation may be different for potentially fraudulent debtors, who would like to minimize geographic transactions costs but would also like to locate a trustee that allows her to submit a fraudulent filing. The key point is that such “lenient” trustees (who may allow a fraudulent filing) comprise a small fraction of the trustee population and, therefore, are not necessarily located close to the fraudulent debtor. As a result, the fraudulent debtor will be required to travel a greater distance to find a more lenient trustee. Therefore, we hypothesize that, while the honest debtor generally prefers the geographically closest trustee, the fraudulent debtor must balance the fraud-enabling benefits of her lenient trustee against the additional transactions costs of traveling farther to the trustee.

We can use this insight to develop a testable hypothesis. Debtors who travel greater excess distances to their selected trustee (beyond their closest trustee) are more likely to be fraudulent (as indicated by bunching below the $200 cutoff), compared with debtors who travel shorter excess distances. This is because one reason for debtors being prepared to incur the costs of traveling greater distances to a more distant trustee is to choose a more lenient trustee who will be more likely to approve a fraudulent filing.\(^\text{21}\)

5.1.2 Measuring Distances Between Filers and Trustees

We build our measure of travel-related filer transactions costs based on geographic locations of filers and trustees. We observe the postal code of all filers and trustees in Canada. Extracting the postal code for filers, we measure the distance between a filer and a trustee as the geographic distance between the centroids of their postal codes. As Canadian postal codes are extremely small

\(^\text{21}\) Our discussion here regarding consumer debtors’ “trustee shopping” for more lenient trustees is to some extent similar to the concern with corporate debtors’ “forum shopping” when making bankruptcy filings, i.e., having the ability to choose more debtor-friendly jurisdictions.
geographic units, containing approximately 13 households on average, the distances we calculate are generally quite precise.

We measure travel-related proposal filer transactions costs as the difference between the filer’s distance to her selected trustee and the filer’s average distance to the three closest trustees. To remove outliers, in building this measure, we exclude any filings where the distance from the filer to her selected trustee is more than 200 kilometers. We also remove filings where the trustee has not approved at least 5 filings in the last three years to reduce noise in our distance measures.

We create two versions of our travel-related transactions costs measure. First, we measure the cost as a multiple of the closest-trustee distance, indicating how many times farther than necessary the filer chooses to go to work with her selected trustee. Second, we measure the cost in terms of excess distance, calculating the additional kilometers traveled by the filer to work with her selected trustee compared with the distance to the three closest trustees.

The top of Panel B of Table 1 reports summary statistics for filer-trustee distances and the travel-related transactions costs measure. The median distance between a filer and the three closest trustees is 3.6 km, whereas the median distance between a filer and her selected trustee is 12.2 km. The median travel-related transactions cost in multiples of average distance to the three closest trustees is 2.4 and, in kilometers above the average distance, is 5.7 km.

5.1.3 Bunching in Surplus Income and Travel-Related Transactions Costs

In this subsection, we test the hypothesis that proposal filers who incur greater travel-related transactions costs manipulate data (bunch in SI). In particular, we estimate bunching magnitudes in various subsamples based on differences in the average distance to the closest three trustees (herein referred to as “minimum distance”) and distance to the selected trustee. In Figures 6 (a) and 6(b), we present our bunching magnitude estimates for filings after the policy change across two samples: filings where debtors travel less than 120% of the minimum distance (the “Nearby Trustees” sample), which is the 25th percentile of the distribution, and filings where they travel more than 120% of the minimum distance (the “Distant Trustees” sample). As the figures depict, there is clearly more bunching in SI to the left of the $200 cutoff in the Distant Trustees sample (Figure 6(b)).

This visual analysis can be corroborated by comparing the estimated bunching magnitude for the Nearby Trustees and the Distant Trustees samples. This magnitude is equal to 6.6% for the former sample and 8.6% for the latter sample (see Figures 6(a) and (b)). There is approximately 30% more bunching to the left of the $200 cutoff in the Distant Trustees sample. Using bootstrapping
methods, we statistically compare the bunching estimates for the two subsamples. The distributions for the bunching estimate for the two subsamples are presented in Figure A6(a). It shows a clear difference in these distributions for the nearby and distant trustees samples, with the peak of the nearby trustees distribution located significantly to the left of the peak of the distant trustees distribution. Furthermore, we run a simple $t$-test on the bootstrapped distributions of the bunching magnitude for the two subsamples and find the difference between their means is highly statistically significant.\footnote{For details on our bootstrapping-based bunching estimate comparisons, please refer to Appendix B.}

To further test whether excess distances traveled to the chosen trustee are correlated with bunching magnitude, we split the sample of post-reform filings into four equal groups (quartiles) based on the excess distance traveled relative to the minimum trustee distance.\footnote{We use quartiles in this section instead of octiles (as in the next section) because the excess distance measure is noisy, and splitting the distribution into smaller groups introduces noise in our bunching magnitude estimation.} Figure 7 visually presents the bunching magnitudes and their 95% confidence intervals for each quartile. This figure shows that the bunching magnitude, $\hat{b}_n$, increases monotonically from the bottom quartile, where it is around 6%, to the top quartile, where it is 9%. Along with the previous findings, this monotonic relationship between excess trustee distance and bunching magnitude corroborates our hypothesis that debtors who bunch below an SI of $200 are more likely to choose a more distant trustee in order to file fraudulent proposals.

### 5.2 Lenient Trustees and Rounding in Proposals

In Section 5.1, we show that fraudulent proposal filers choose to work with more distant trustees even though closer trustees are available. Why do fraudulent debtors opt to incur greater travel-related costs? As not all trustees are the same, there may be more “lenient” trustees, who are more likely to submit fraudulent proposals. In this section, we highlight rounding of numerical data in proposals (e.g., reporting numbers in the multiples of $100), a characteristic consistent with trustee leniency, as a way for fraudulent debtors to find trustees allowing them to reduce their surplus income below the $200 cutoff (bunch) after the policy change. We show that potentially fraudulent filers employ lenient trustees who are more likely to approve their proposal filings.
5.2.1 Rounding as a Measure of Trustee Leniency

The American Institute of Certified Public Accountants (AICPA), in its clarified statement on auditing standards regarding *Consideration of Fraud in a Financial Statement Audit* (AU-C Section 240), explains that fraudulent financial statements “include entries ... containing round numbers.”\(^{24}\) Based on this guidance, we assert that trustees who allow rounding are generally more lenient, as they approve filings likely to be fraudulent.

As per the existing literature, lenient trustees may be more likely to approve proposal filings exhibiting more rounding for two main reasons: low effort or shirking (e.g., Herrmann and Thomas, 2005) and deliberate data manipulation (e.g., Garmaise, 2015).\(^{25}\) Note that fraudulent filings do not necessarily have a greater prevalence of round numbers. Sophisticated fraudulent filers may have other (better) ways to cheat that may be easier to get approved by a trustee known to be lenient. Approving filings with round numbers is an imperfect measure of trustee leniency, but it is the best measure available to us, as econometricians.

In our setting, approval of filings with round numbers by financially sophisticated trustees may result from either low effort (i.e., shirking) or deliberate data manipulation. We cannot identify which of these two motivations drives a trustee to approve such filings. Both motivations, however, lead trustees to be more lenient in their dealings with fraudulent proposal filers. If fraudulent filers are aware of a trustee’s leniency, they may seek out that trustee to improve their chances of successfully submitting a fraudulent proposal filing.

We employ the above argument to formulate our tests studying fraudulent debtors’ choice of lenient trustees. First, we study whether fraudulent debtors choose to work with historically lenient trustees by examining bunching magnitudes for filings approved by trustees with different historic rounding levels. Next, to briefly consider leniency from the trustee perspective, we inspect the dynamics of filing market share for trustees with different leniency levels.

5.2.2 Measuring Trustee Leniency with Rounding in Proposal Filings

We calculate our rounding measure directly from the values provided by filers about their financial condition in their filings. Recall that each filing includes data on the complete balance sheet and

\(^{24}\) AU-C Section 240 is available at [https://us.aicpa.org/content/dam/aicpa/research/standards/auditattest/downloadabledocuments/au-c-00240.pdf](https://us.aicpa.org/content/dam/aicpa/research/standards/auditattest/downloadabledocuments/au-c-00240.pdf).

\(^{25}\) A separate literature also characterizes rounding as caused by a lack of financial sophistication (e.g., D’Acunto et al., 2021; Kuo et al., 2015).
income statement of the insolvent debtor on the date of the filing. Our rounding measure is the proportion of a filing’s numerical entries over $100 that are rounded to the hundreds place, focusing on entries detailing the filer’s assets and liabilities.\textsuperscript{26} The precise formula we employ for calculating our rounding measure is:

\[
\%RN_i = \frac{\sum_{k \in \text{A&L vars}} \mathbb{1}\{val_{i,k} = 0 \mod 100\}}{\sum_{k} \mathbb{1}\{val_{i,k} > 100\}},
\]

where \(val_{i,k}\) is the value of financial variable \(k\) in filing \(i\).

In our rounding measure, we focus on filing entries detailing assets and liabilities reported. The other numerical entries in filings detail filer income and non-discretionary expenses. We omit these two categories of entries from our measure to exclude any possibilities of a mechanical relationship between our rounding measure and SI, as SI is calculated based on reported income and non-discretionary expenses. We construct an alternate measure using all financial filing entries and find qualitatively similar results.

As our interest here focuses on the past approvals of round number filings by trustees, we aggregate this filing-level \(\%RN\) variable to the trustee-year level as well. The aggregation assigns to a trustee in a given year the rolling average of the last three years of \(\%RN\) for all filings approved by that trustee. We drop a trustee-year observation if the number of filings approved by the trustee in the past three years is below 40, as the average of a small number of filings may not convey reliable information on the trustee’s leniency.\textsuperscript{27}

We present summary statistics on our rounding measure in the bottom half of Panel B of Table \(\text{I}\). On average, 45% of financial data reported in a filing is rounded. There is a slight increase to 47% in rounding levels if we omit income and non-discretionary data; 75% of filings report round numbers in two-thirds or less of their financial data. Aggregating to the trustee-year level as described above collapses the distribution substantially, with the 75th percentile dropping to 55% for the aggregated measure, but this does not significantly alter the mean.

5.2.3 Fraudulent Debtors and Historical Trustee Leniency

In this section, we examine whether fraudulent debtors choose to work with more lenient trustees. In Figure \(\text{S}\), we present the results of our bunching magnitude estimations for filings approved by more and less historically lenient trustees after the policy change. Figure \(\text{S(a)}\) focuses on filings

\textsuperscript{26}We provide a full list of the data variables we use in Table \(\text{A1}\) in Appendix \(\text{C}\).

\textsuperscript{27}This cutoff is at the 5th percentile of the distribution.
approved by trustees whose aggregated historic %RN is below the 90th percentile, and Figure 8(b) focuses on filings approved by trustees with aggregated %RN above the 90th percentile. The two figures show that the bunching of filings below the $200 surplus income cutoff among more lenient trustees is much larger than this bunching among less lenient trustees. Moreover, our estimate of bunching magnitude, $\hat{b}_n$, is nearly twice as large for filings approved by more lenient trustees (11.2% versus 6.8%).

We statistically compare the bunching estimates for the two subsamples and find the distributions for the bunching magnitude for the two subsamples to be quite distinct, as we show in Figure A6(b). The peak of the less lenient trustees distribution is much further left than the peak of the more lenient trustees distribution. A simple t-test on the bootstrapped distributions of the bunching magnitude for the two subsamples confirms that the difference between their means is highly statistically significant. This difference in bunching magnitude among filings approved by the top decile versus the rest of trustees by historical leniency indicates that fraudulent debtors tend to work with more lenient trustees to maximize the likelihood of approval for their filings.

When we split post-regulatory change filings into equal octiles based on historical trustee leniency and estimate the bunching magnitude for each octile, we find similar results. Figure 9 visually presents the bunching magnitudes for each octile. As we can see in the figure, the level of bunching magnitude, $\hat{b}_n$, gradually increases from the bottom octile, where it is just above 5%, to the top octile, where it is approximately 12%. The more historically lenient a trustee is known to be, the more bunching we observe below the $200 surplus income cutoff. This result further confirms that fraudulent debtors are more likely to work with lenient trustees to improve the chances of having their filings approved.

Finally, we can confirm that our two methods of detecting fraudulent filers among bunchers are correlated. Correlation between these measures indicates that we are likely identifying significantly overlapping bunchers, which lends weight to our argument that our measures are identifying fraudulent filers. Figures 10(a) and (b) show that both the historical leniency of the trustee and the prevalence of round numbers in proposal figures are significantly higher for filers who choose trustees who operate farther from them. Figure 10(a) shows historical trustee leniency gradually increases when we vary the distance traveled by the filer to the chosen trustee (in excess of the distance to the three closest trustees). Figure 10(b) shows similarly greater prevalence of round numbers.

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28 Our results are robust to using 50th and 75th percentile as cutoffs.

29 For details on our bootstrapping-based bunching estimate comparisons, please refer to Appendix B.
numbers in proposal figures for filers who travel excess distances to their chosen trustees. These results are consistent with the argument that fraudulent filers travel excess distances to work with trustees who may also be more lenient.

5.2.4 Trustees’ Market Share

We next consider why some trustees may choose to be more lenient. Recall that the consumer insolvency process is heavily regulated and, in particular, trustees cannot adjust the fees they charge proposal filers to verify and approve their filings. As a result, these for-profit trustees cannot compete with each other on price. Leniency in filing approval may offer an alternative way for them to attract filers and increase their market share, though it comes at a potential cost of losing their license. In this section, therefore, we examine whether trustee leniency has any impact on their future market share.

Before presenting the results of our examination, we confirm that historic trustee leniency implies future leniency. Without this feature of trustees, fraudulent filers cannot choose lenient trustees based on their past behavior. To confirm this hypothesis, we examine the average current-year \%RN value for octiles of filings based on historical trustee leniency. In Figure 11, we present our findings. Filings are split into octiles based on the historical leniency of their chosen trustees. The figure shows that the bottom octile has the lowest levels of current-year rounding levels in filing data (around 30%), and this rounding level increases monotonically over the octiles, reaching 65% for the top octile. With this result, we establish the persistence of trustee leniency.

To test whether trustee leniency has any effect on trustees’ market share, we compare trustee market share across trustees with various levels of leniency. Then, we plot these shares for more and less lenient trustees over time. In Figure 12, we present the dynamics of market share for more and less lenient trustees. Figure 12(a) compares the number of filings for more and less lenient trustees, based on whether the trustee was above or below the 90th percentile cutoff for historical leniency in August 2009. It shows that more lenient trustees have lower market share prior to the policy change, catch up to the less lenient trustees at the time of the reform, and seem to retain the market share they gain for the rest of the period. Figure 12(b) shows estimates of the difference between the market share of the more and less lenient trustees in each quarter using an event study difference-in-difference specification, absorbing trustee fixed effects and using heteroskedasticity-robust standard errors. It confirms that there is a marked increase in the market share of more lenient trustees immediately following the policy change and no subsequent reversion later in the
These findings strongly suggest that the trustees who choose to be more lenient increase their market share, which may explain why they take on the potential cost of license revocation by being more lenient.

5.3 Identifying Fraud Using Bunching Techniques

The bunching we identified in Section 4 could arise from two types of filer manipulation: labor supply manipulation (e.g., reducing the number of hours worked or filing when unemployed) and data manipulation (e.g., fraudulent data misreporting). Labor supply manipulators file proposals with intentionally lowered recent incomes to arrange lower proposal repayments based on SIs below $200, likely with the intention of increasing labor supply (and income) afterward. Data manipulators file proposals with intentionally misreported recent income to arrange proposals based on SIs under $200. The aggregate bunching results in that section cannot distinguish between these two types of bunchers, the latter of whom are committing fraud by misreporting financial data in their proposal filings.

Our findings in this section confirm the existence of fraudulent data manipulation among the bunchers. We compare bunching magnitudes across different subsamples exploiting a key difference between data and labor supply manipulators: Filers who manipulate their labor supply prior to filing their proposals are asking their trustees to submit proposals with truthful figures, whereas filers who misreport income are asking their trustees to submit fraudulent proposals with falsified figures. Therefore, labor supply manipulators have no incentive to travel farther to find more lenient trustees, whereas data manipulators do.

First, we document the use of distant, more lenient, trustees by bunchers, which identifies the presence of fraudulent filers among the bunchers. All else equal, filers who do not intend to submit a fraudulent proposal prefer nearby trustees because all trustees charge the same fees and use the same forms but working with more distant trustees incurs additional travel costs. A key difference we find between nearby and distant trustees is the greater leniency of the more distant trustees. Bunchers using distant trustees are willing to incur greater travel-related transaction costs because

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30 This approach to detecting fraud can be found in other economics literature. The tax literature, in particular, uses this approach to identify tax evaders [Saez 2010, Kleven et al. 2011, Best et al. 2015]. Other settings also identify fraud or illegal reporting using this approach [Foremny et al. 2017, Palguta and Pertold 2017, Dee et al. 2019].

31 Note that the bunching methodology cannot identify any data manipulation or labor supply manipulation at the individual level. Our findings here only identify the aggregate presence of data manipulation among the bunchers.
they wish to use a trustee who is more likely to approve filings with inaccurate, falsified figures. Therefore, our finding of bunching among filers with faraway trustees indicates that some of the bunchers seek more distant trustees to submit fraudulent filings with misreported figures.

Second, we confirm the presence of fraudulent data manipulators among the bunchers via our comparison of bunching among historically less- and more-lenient trustees. Filers should, all else equal, be indifferent between trustees with less- and more-lenient histories if they are submitting truthful, accurate applications. We find significant bunching among historically lenient trustees, indicating that some of the bunchers seek these trustees to submit inaccurate figures in their filings. Jointly, these tests indicate that a sizable portion of the bunchers we identified in Section 4 are data manipulators, i.e., fraudulent filers misreporting figures in their proposals to reduce their SIs to under $200.

5.4 Integrity of the Insolvency System

As we discuss in Section 2.3, a major role of bankruptcy trustees in the insolvency process is to verify all financial information submitted by the debtor. However, our results on the debtor-trustee interactions show that some trustees are more willing than others to submit suspicious proposals and that data manipulating filers may be seeking such trustees. This can result in an increased number of filings submitted by such trustees. Overall, our findings may suggest that the additional incentive to manipulate data created by the 2009 BIA reform led to more data manipulation, which is not prevented by the trustees, and weakened the integrity of the insolvency system. This finding has important implications for the role of financial intermediaries in verifying financial data and how their incentives (to process more filings, in our case) may not help in maintaining the integrity of the system. These results are similar to findings in other nonfinancial contexts documenting inappropriate behaviors of agents designed to maintain system’s integrity.

6 The Consequences of Data Manipulation on Future Default

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.

32 For example, Camacho and Conover (2011) document “corruption” in social program eligibility by showing that bunching coincided with the release of the score algorithm to local officials. Dee et al. (2019) report that teachers manipulated students’ test score to meet proficiency cutoffs.
6.1 Advantageous Selection, Hidden Income, and Proposal 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 (some of them, fraudulently, as we show in Section 5) 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.\textsuperscript{33}

To test this hypothesis, we adopt a difference-in-differences (DID) type methodology used in other studies of the effect of bunching on individual outcomes (e.g., \cite{DeFusco2020,Dee2019,Collier2021}). 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.\textsuperscript{34}

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 of the $0 cutoff present 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 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 to 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).

\textsuperscript{33}This hypothesis and methodology is similar to other studies of advantageous and adverse selection in credit markets (e.g., \cite{Hertzberg2018}) that argue that hidden information about debtors’ characteristics (e.g., credit risk) can be revealed by their loan performance.

\textsuperscript{34}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.
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 our analysis. 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 were not able to reduce it to 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 identify their overall effect. As some of these effects work in opposite directions, the overall effect is ambiguous (we discuss this in more detail in Section 8). For this reason, we do not use the above $200 zone as a comparison zone or a manipulation zone in our analysis.

Given that the lower bound of the Below $0 Manipulation Zone is -$100, we designate filings below this point 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. We note that our results are robust to various alternative definitions of this lower bound.

6.2 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. 2021). Our data allow us to 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 C 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. Around 68% of proposals are paid in full. About 17% of the proposals 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 allows us to account for right censoring in our data. 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. Various studies (e.g., Keys et al. 2010; Li et al. 2011; Agarwal et al. 2021) have documented a hump-shaped curve of default over the lifespan of a long-term loan. Agarwal et al. (2021) document this hump-shaped default relationship for similar proposal data.
Our baseline specification is a standard Cox model estimated at the proposal level using the sample of proposal filings with SI reported from -$400 to $200:

\[
h_i(t) = \gamma_0(t) \times \exp(\gamma_m \times \text{Below } \$200 \text{ MZ}_i \times Post_t + \gamma_b \times \text{Below } \$200 \text{ MZ}_i \\
+ \gamma_k \times \text{Below } \$0 \text{ MZ}_i \times Post_t + \gamma_n \times \text{Below } \$0 \text{ MZ}_i \\
+ \gamma_c \times Controls_i + \mu_k + \epsilon_i),
\]

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 function when all the covariates are zero. \( \text{Below } \$200 \text{ MZ}_i \) takes the value of 1 if the proposal filer has surplus income between $0 and $200, and 0 otherwise, i.e., if the filing belongs in the Below $200 Manipulation Zone. \( \text{Below } \$0 \text{ MZ}_i \) is equal to 1 for proposals with SI between -$100 and $0, and 0 otherwise, i.e., proposals in the Below $0 Manipulation Zone. \( Controls_i \) are filing and filer characteristics as reported in Table 1. Continuous \( \mu_k \) 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 by availability and the recent literature on personal bankruptcy. \( \mu_k \) represents a series of fixed effects including liability type, joint filing, repayment schedule type, debtor province, occupation category, and filing year. \( \epsilon_i \) is an error term.

The variable of interest is \( \gamma_m \), 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 the DID setting 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 evidence supporting this assumption by showing 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 Equation (5) using each month from the start of 2007 to the end of 2008 as the pseudo-policy change month. We report the estimated coefficients \( \gamma_m \), for each regression in Figure A3 in Appendix C. 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.
6.3 Default Results

We estimate the Cox proportional hazards model as specified in Equation (5) and report the results in Table 3 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 supports our argument 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 robustness tests 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, 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 $\gamma_m$ 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 to filings with different negative SI levels. To do this, we create additional indicator variables taking a value of 1 if the SI falls in a certain negative SI region (e.g., filings with SI between -$800 and -$400). Comparing 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 $Post_t$ to be statistically indistinguishable from 1 because the policy change did not affect the incentive in reporting the SI in the placebo zones. These tests can eliminate the concern that all higher SI proposals have a different default propensity in the post-reform period. Table A2 in Appendix C shows that none of the placebo zones has a statistically significant difference in change in the default rate in the post-reform period compared to the Comparison Zone.
6.4 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 fraudulently lower their reported 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.

We also contribute to the literature on fraud in consumer credit by showing that increasing debt repayment conditional on income leads to misreporting of income and lower loan default. This setting is different from the existing literature on debtor fraud in household finance markets, which has focused on mortgage markets. Prospective mortgage debtors have an incentive to fraudulently report their financial situation as better than their actual situation (e.g., to inflate assets or income) to benefit from either an increased supply or lower price of credit (Ben-David, 2011; Elul et al., 2021; Garmaise, 2015; Griffin and Maturana, 2016; Jiang et al., 2014; Mian and Sufi, 2017). Because the actual financial situation of the debtor is worse than the fraudulently reported financial situation, these studies generally find that fraudulent debtors have a higher subsequent default rate compared with nonfraudulent debtors. We show that when debtors are incentivized to lower their reported income, their loan default rates fall.

7 The Consequences of Manipulation by Debtors on Debt Renegotiation with Creditors

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 the possibly fraudulent data reporting by debtors (as indicated by bunching below the $200 cutoff after the policy change) affects negotiated proposal contract outcomes. The proposal setting allows us to conduct these tests because we observe both the initial (possibly fraudulent) data reported by the debtor to the creditors.

As discussed previously in Section 5.3, a substantial proportion of the post-reform bunchers are likely to be fraudulent data manipulators, while the rest may be labor supply manipulators.
creditors and the subsequent outcome of the debtor-creditor negotiations.

### 7.1 Empirical Methodology and Results

To test these hypotheses, we use the same DID research design as in Section 6. 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 6.

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 of possible bunchers 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 regression estimated at the proposal level using the sample of proposal filings with an SI reported from -$400 to $200. The regression equation is as follows:

\[
\text{LoanTerms}_i = \beta_0 + \beta_B \times \text{Below $200 MZ}_i + \beta_{PB} \times \text{Post}_t \times \text{Below $200 MZ}_i \\
+ \beta_K \times \text{Below $0 MZ}_i + \beta_{PK} \times \text{Post}_t \times \text{Below $0 MZ}_i \\
+ \text{Controls}_{i,t} + \epsilon_{i,t},
\]

(6)

where the dependent variable \( \text{LoanTerms}_i \) is one of the proposal contract terms (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, and subsequent filing rejection by creditor) for proposal filing \( i \) and all remaining variables are defined as in Section 6. 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 Equation (6) using a logit regression. The coefficient of interest is \( \beta_{PB} \), which measures the differential change in loan terms 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.
The results of our analyses of proposal negotiations are reported in Table 4. We report results on seven negotiation outcomes. These results indicate that there are no economically or statistically significant changes for proposals in the Below $200 Manipulation Zone relative to the Comparison Zone (-$400 to -$100 in the SI) from the pre- to the post-reform period. As these outcomes reflect equilibrium outcomes in the creditor-debtor negotiations, we find that data manipulation does not lead to higher debt repayment (in terms of dollars or repayment rate), shorter maturity, more proposal rejection, or fewer withdrawals. This implies that, in aggregate, creditors do not appear to take data manipulation by debtors into account when negotiating with them, thus allowing them to benefit from their manipulation.

7.2 Potential Explanations

We propose two possible, though not mutually exclusive, explanations for the lack of creditor response we document in Table 4. The first explanation argues that creditors receive both benefits and costs from this specific kind of debtor fraud. The cost to the creditor from this fraud is that the debtor will make smaller payments than would be charged based on truthfully reported income. However, as documented in Section 6, the creditor may also benefit from such fraud because a debtor who fraudulently hides income also has a lower probability of default in subsequent years. This is because the hidden income can be used to avoid default. For this reason, it is possible the creditor could decide not to impose any costs on the debtor in terms of the negotiated proposal terms.

The second reason for creditors not penalizing fraudulent debtors via worse proposal terms may stem from creditors’ inability to precisely identify which debtors in the Below $200 Manipulation Zone are fraudulent. This issue is raised by Garmaise (2015), who argues that if a creditor is not able to perfectly distinguish between fraudulent and nonfraudulent debtors and “punishes” all debtors who appear to be fraudulent, then the creditor would incur costs from punishing debtors who appeared fraudulent, but were actually nonfraudulent (in our setting, those with a true SI in the Below $200 Manipulation Zone).

8 Evidence on Identification Assumptions

Our test of whether debtors manipulate their SI in response to the 2009 policy reform measures the extent of bunching just below the $200 cutoff. The key identification assumption of our data
manipulation test is that, after the policy reform, there are no changes in motivation to file or not file proposals for filers around the cutoff, but only changes in how the SI is reported by similar pools of proposal filers. However, we may observe more debtors in the area below the cutoff (or fewer debtors above the cutoff) after the reform because of extensive margin switching by debtors, either into or out of proposal filings. To validate our bunching estimation, we verify that our results are not driven by such extensive margin switching.

There are two main possible types of extensive margin switching, both of which would be a threat to our bunching magnitude estimation: (1) switching by insolvent debtors near the cutoff from bankruptcy into proposal[37] and (2) switching by solvent debtors near the cutoff into proposal (or switching by proposal filers out of insolvency). Below, we present evidence that our bunching estimation findings are not driven by either of these types of extensive margin switching.

8.1 Switching Between Bankruptcy and Proposal

In this section, we provide both institutional and empirical evidence that extensive margin switching between bankruptcy and proposals is not driving our bunching estimation findings.

8.1.1 Institutions: The Informal Floor Mechanism

The “informal floor” mechanism, described in detail in Section 2, makes it unlikely that debtors switch from bankruptcy to proposals (or vice versa) because of the policy change. Recall that, because of creditors’ rights in proposals versus bankruptcies, the payment set by bankruptcy rules serves as the informal floor for payments that will be acceptable to creditors in proposal negotiations. Because creditors in a proposal negotiation will respond to any increase in the required payments in bankruptcy, a debtor who faces higher costs of bankruptcy (e.g., debtors with an SI over $200 who have to make larger bankruptcy payments because of the policy change) would face similarly higher costs if they switched from bankruptcy to proposal. Because of this link between payments under proposal and bankruptcy (due to the informal floor mechanism), it is unlikely that a debtor switches between bankruptcy and proposal simply because of the policy change.

37 There is no economic motivation for debtors to switch from proposals to bankruptcy due to this reform as it makes bankruptcy more costly to some debtors, while not changing proposal costs.
8.1.2 Empirical Evidence: Bunching by Homeowners

We also examine a subsample of proposal filers who have strong incentives not to make a switch between bankruptcy and proposal: homeowners. Homeowners filing for bankruptcy lose their houses, while they may be able to retain their houses under proposal. Thus, homeowners would prefer proposals to bankruptcy both before and after the policy change. As a result, it is unlikely that homeowners would switch between proposal and bankruptcy because of the policy change.

In Appendix Figure A7, we show that homeowners filing proposals display significant bunching below the cutoff after the policy change (with estimated bunching magnitude of 14.6%). This evidence of significant bunching by homeowners, who have no incentive to switch between bankruptcy and proposal, is inconsistent with the argument that our overall finding of bunching is entirely caused by policy-induced switching between bankruptcy and proposal.

8.2 Switching Between Solvency and Proposal

The second extensive margin switching threat to our bunching estimation arises if the policy change caused either: (1) debtors below the $200 SI cutoff to shift from solvency into proposal, inflating the debtor count below the cutoff, or (2) debtors above the cutoff to switch out of proposals into solvency, reducing the debtor count above the SI cutoff. Both of these potential policy-induced debtor behaviors would create an apparent discontinuity in the distribution of debtor counts at the $200 SI cutoff. In this section, we provide both institutional and empirical evidence that such switching is unlikely in our setting.

8.2.1 Institutions: Costs and Benefits of Proposal vs. Solvency

When a debtor is choosing whether to enter the insolvency system, the debtor needs to account for both the costs as well as the benefits of insolvency. Our data allow us to compare the costs and benefits of switching between solvency and proposal filings before and after the policy change.

For debtors just above the $200 cutoff, a threat to our bunching estimation would arise if the 2009 reform caused them to switch from proposal filing to solvency (i.e., lowering the number of proposal filers above the cutoff). However, the costs and benefits of proposals for such debtors are

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38 A very large literature, starting with Fay et al. (2002), has emphasized that the extensive margin choice (i.e., entry into insolvency) is based on the net financial benefits of insolvency, (i.e., financial benefits minus financial costs of insolvency).
not consistent with such a shift. The average cost of proposal for debtors just above the cutoff is $2,100 (50% of $200 in SI paid over 21 months). However, our data show that proposal filers just above the cutoff (SI from $200 to $500) reach an agreement with their creditors to repay an average of $13,128 out of an average total unsecured debt of $33,885. In other words, the average proposal filer just above the SI cutoff has a benefit of $20,752 from the debt that is written off by creditors. This debt write-off benefit is approximately 10 times the average costs imposed on the debtor after the policy change ($2,100). Even with the relatively small increase in filing cost induced by the reform, we argue that the benefits still greatly outweigh the costs and, thus, it is unlikely that potential filers will switch from proposal to solvency.

An even simpler comparison of the costs and benefits of proposal insolvency, relative to solvency can be made for debtors with an SI just below the cutoff. In this case, neither the costs nor benefits of proposal are affected by the policy change as it did not change how much proposal filers with SI below $200 have to pay their creditors. It is, therefore, unlikely that the policy caused the extensive margin entry into proposals of solvent debtors just below the cutoff.

### 8.2.2 Empirical Evidence: Neighborhood Lenient Trustee Accessibility

We exploit our data on neighborhood-level trustee leniency to provide additional evidence on switching from proposal to solvency near the cutoff because of the policy change. A debtor who faces increased costs of proposal filing because of the new policy (i.e., debtors with an SI \( \geq \$200 \)) will be less likely to switch to solvency if they can reduce some of these additional policy-induced costs. This may be easier if they can more easily access a lenient trustee. On the other hand, a debtor who is only able to easily access strict trustees may be forced to bear the full policy-induced increase in costs and will thus be more likely to exit the insolvency system (i.e., switch from proposal to solvency).

On the basis of this argument, we compare the distribution of filers with an SI \( \geq \$200 \) in the post-policy change period for: (1) filers with lenient trustees in the neighborhood and (2) filers with no lenient trustees in the neighborhood. If the increased costs induced by the policy caused debtors to switch from proposal to solvency and if debtors could reduce this cost by finding a lenient trustee, then this would imply that debtors in neighborhoods with lenient trustees would be less likely to

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39 It should be noted that these comparisons of benefits and costs are simple back-of-the-envelope comparisons, which cannot incorporate other potential motivations for a debtor to switch between solvency and proposal (e.g., stigma costs of insolvency and the costs of default out of insolvency).
exit to solvency, especially compared with debtors in neighborhoods with no lenient trustees.

Appendix Figure A8 displays the distributions of (1) proposal filings by SI bins in neighborhoods with less lenient trustees and (2) filers in neighborhoods with more lenient trustees. This figure shows that the two distributions are essentially identical near the $200 cutoff. This finding is inconsistent with the hypothesis that potential proposal filers with an SI above $200 and facing strict trustees in their neighborhoods (i.e., less likely to be able to manipulate their SI below the cutoff) would exit insolvency. We interpret this evidence as inconsistent with the hypothesis that our main bunching magnitude estimation is driven by proposal filers switching to solvency just above the $200 cutoff.

8.2.3 Empirical Evidence: Proposal Filing Dynamics

Finally, we provide further evidence on the question of whether the policy change causes switching between proposal and solvency by plotting proposal filing dynamics for: (1) before and after the 2009 policy change and (2) for filers with an SI $\geq$ $200 and filers with an SI $< 200$. We argue that if this policy change generated an extensive margin exit of filers with an SI $\geq$ $200$, then we should observe a sharp decline in the time series of the count of filers with an SI $\geq$ $200$, relative to filers with an SI $< 200$ after the reform date.

Figure 13 plots these time series. It clearly shows that both of these time series move similarly around the reform date and that the SI above and below $200$ series track each other very closely over time, both before and after the policy change. Taken together, we argue that this visual evidence is consistent with our argument that extensive margin effects (either entry into proposal or exit out of proposal) are unlikely to drive our bunching magnitude results.

Figure 13 also supports our arguments above concerning the extensive margin shifting between proposal and bankruptcy near the $200$ cutoff. If the policy had induced shifting between proposal and bankruptcy near the cutoff, this would be indicated by a sharp change in the number of proposals with an SI over $200$ relative to the number of proposals with an SI below $200$ after the reform date. The similar movements of these curves around the reform date are inconsistent with the policy causing either extensive margin switching between proposal and solvency or between proposal and bankruptcy near the cutoff.
9 Conclusions and Policy Implications

This paper documents that, when debtors face a sharp and discontinuous increase in their income-contingent payments to creditors, some of them respond by manipulating their reported income downward to reduce these payments. As the debtors who choose more distant trustees choose to incur additional costs to work with more lenient trustees, we argue that this data manipulation is consistent with deliberate fraud.

Our findings have important policy implications. First, we show that government intervention in a credit market (in our case, the introduction of the large payment discontinuity) 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 discontinuities and thresholds in credit markets. There are many important but arbitrary discontinuities in household credit markets (e.g., conforming loan limits for U.S. residential mortgages, debt-to-income ratio thresholds as in DeFusco et al. (2020)). Therefore, an important contribution of our study is documenting that such discontinuities can generate fraudulent behavior and other credit market distortions. Future household finance research may explore this topic in other settings.

Third, our paper documents that government attempts to increase payments from debtors to creditors can have unintended consequences. While the previous literature has documented that requiring higher payments from debtors often leads to financial distress and loan default, we focus on other important outcomes such as financial information misreporting. Our findings imply that requiring higher income-contingent payments from debtors may unintentionally generate increased fraudulent evasion of such payments. This may prevent creditors from receiving all of the expected increase in payments because of the increased incentive for debtors to commit fraud. On the other hand, we also show that some debtors in the manipulation zone may have hidden income, which they use to reduce default on their proposal repayment plans, which may benefit creditors. This strategic behavior of debtors and its consequences are important to consider in designing policies to increase payments from debtors to creditors.

Fourth, our findings document that incentives from discontinuities can generate fraudulent behavior, even in a context where for-profit, third-party intermediaries (in our case, bankruptcy
trustees) are present in the system specifically to safeguard against fraud. In other words, the presence of such for-profit intermediaries may not be enough to prevent fraudulent behavior because the intermediaries themselves may also have an incentive to facilitate fraud.
References


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Tracy, Joseph, and Joshua Wright, 2016, Payment changes and default risk: The impact of refinancing on expected credit losses, *Journal of Urban Economics* 93, 60–70.
### Tables and Figures

#### Table 1: Summary Statistics
This table reports summary statistics for consumer proposals filed with the OSB in Canada between 2006 and mid-2019. In Panel A, we summarize all proposal details, including filer and filing characteristics and negotiation outcomes. In Panel B, we summarize all trustee-related details of a proposal, including distances between proposal filers and trustees and the prevalence of round numbers in proposal filings. Finally, in Panel C, we summarize loan performance for the proposal submissions. For Panels A and B, we present five summary statistics: number of observations, mean, standard deviation, 25th percentile, median, and 75th percentile. For Panel C, we present the number and proportion of proposal filings in each loan outcome category. Detailed definitions of all variables are available in Table A1 in Appendix C.

**Filer and filing characteristics**

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<th>Median</th>
<th>75th ptile</th>
<th>Std dev</th>
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**Negotiation outcomes**

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<td>Maturity (months)</td>
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(b) Trustee Details

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**Trustee leniency**

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<td>0.62</td>
<td>0.25</td>
</tr>
<tr>
<td>Trustee % of numbers rounded (asset &amp; debt values)</td>
<td>5,114</td>
<td>0.48</td>
<td>0.39</td>
<td>0.47</td>
<td>0.55</td>
<td>0.13</td>
</tr>
<tr>
<td>Trustee % of numbers rounded (all)</td>
<td>5,114</td>
<td>0.45</td>
<td>0.37</td>
<td>0.45</td>
<td>0.53</td>
<td>0.12</td>
</tr>
</tbody>
</table>

(c) Proposal Outcomes

<table>
<thead>
<tr>
<th></th>
<th>Frequency</th>
<th>Percent</th>
</tr>
</thead>
<tbody>
<tr>
<td>Full payment</td>
<td>325,103</td>
<td>68.01</td>
</tr>
<tr>
<td>Default</td>
<td>77,071</td>
<td>16.12</td>
</tr>
<tr>
<td>Amendment and full payment</td>
<td>49,347</td>
<td>10.32</td>
</tr>
<tr>
<td>Rejection</td>
<td>10,079</td>
<td>2.11</td>
</tr>
<tr>
<td>Withdraw</td>
<td>9,687</td>
<td>2.03</td>
</tr>
<tr>
<td>Amendment and default</td>
<td>6,766</td>
<td>1.42</td>
</tr>
<tr>
<td>Total</td>
<td>478,053</td>
<td>100.00</td>
</tr>
</tbody>
</table>

**Table 2: Bunching Magnitude Estimation**

This table presents details and results of eight bunching model estimations run in this paper based on Equations (1) through (3). The eight models fit the counterfactual distribution for proposal filers’ Surplus Income (SI) using filings with SI between -$2,000 and $2,000. Of the four key inputs for fitting the counterfactual distribution, the exclusion region upper bound is fixed at $200 and the other 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.

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
<th>(8)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Bin size</td>
<td>50</td>
<td>50</td>
<td>40</td>
<td>40</td>
<td>60</td>
<td>60</td>
<td>100</td>
<td>100</td>
</tr>
<tr>
<td>Polynomial order</td>
<td>7</td>
<td>5</td>
<td>7</td>
<td>5</td>
<td>7</td>
<td>5</td>
<td>7</td>
<td>5</td>
</tr>
<tr>
<td>Exclusion region lower bound</td>
<td>-100</td>
<td>-50</td>
<td>-80</td>
<td>-40</td>
<td>-100</td>
<td>-40</td>
<td>-100</td>
<td>-100</td>
</tr>
<tr>
<td>Exclusion region upper bound</td>
<td>200</td>
<td>200</td>
<td>200</td>
<td>200</td>
<td>200</td>
<td>200</td>
<td>200</td>
<td>200</td>
</tr>
<tr>
<td>Bunching magnitude</td>
<td>3978</td>
<td>4682</td>
<td>3163</td>
<td>3835</td>
<td>4264</td>
<td>5071</td>
<td>4330</td>
<td>6466</td>
</tr>
<tr>
<td>Excess mass %</td>
<td>14.33</td>
<td>17.24</td>
<td>14.06</td>
<td>17.32</td>
<td>12.86</td>
<td>15.71</td>
<td>7.850</td>
<td>12.19</td>
</tr>
<tr>
<td>Standard error</td>
<td>1.350</td>
<td>2.940</td>
<td>1.690</td>
<td>2.480</td>
<td>1.530</td>
<td>3.040</td>
<td>0.970</td>
<td>2.700</td>
</tr>
</tbody>
</table>
### Table 3: Effect of Bunching on Loan Performance

This table reports the results of estimating Equation (5) comparing the default hazard of Manipulation Zone filings and Comparison Zone filings using Cox Proportional Hazards regressions. 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. Estimate coefficients are reported in exponentiated form and t-statistics are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

<table>
<thead>
<tr>
<th>Below $200 MZ × Post</th>
<th>(1) Default</th>
<th>(2) Default</th>
<th>(3) Default</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>0.930**</td>
<td>0.921**</td>
<td>0.924**</td>
</tr>
<tr>
<td></td>
<td>(-2.00)</td>
<td>(-2.10)</td>
<td>(-1.96)</td>
</tr>
<tr>
<td>Below $200 MZ</td>
<td>1.019</td>
<td>1.032</td>
<td>1.027</td>
</tr>
<tr>
<td></td>
<td>(0.57)</td>
<td>(0.90)</td>
<td>(0.76)</td>
</tr>
<tr>
<td>Below $0 MZ × Post</td>
<td>1.070</td>
<td>1.031</td>
<td>1.026</td>
</tr>
<tr>
<td></td>
<td>(1.48)</td>
<td>(0.78)</td>
<td>(0.56)</td>
</tr>
<tr>
<td>Below $0 MZ</td>
<td>0.940</td>
<td>0.954</td>
<td>0.943</td>
</tr>
<tr>
<td></td>
<td>(-1.53)</td>
<td>(-1.33)</td>
<td>(-1.44)</td>
</tr>
</tbody>
</table>

Controls: Y, Y, Y
Fixed effects: Y, Y, Y
Model: CoxPH, CoxPH, CoxPH

### Table 4: Effect of Bunching on Loan Terms

This table reports the results of estimating Equation (6) comparing loan terms for Manipulation Zone filings and Comparison Zone filings. 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 Surplus Income ranges for the Below $0 Manipulation Zone, Below $200 Manipulation Zone, and the Comparison Zone are (-100,0], (0, 200], and (-400,-100], respectively. t-statistics are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

<table>
<thead>
<tr>
<th>Below $200 MZ × Post</th>
<th>(1) Repay amt</th>
<th>ln(Repay amt)</th>
<th>(2) Repay ratio</th>
<th>(3) Maturity</th>
<th>(4) Mat &gt; 60mths</th>
<th>(5) Rejected</th>
<th>(6) Withdrawn</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>-83.88</td>
<td>-0.0108</td>
<td>-0.00196</td>
<td>0.0373</td>
<td>0.0568</td>
<td>0.103</td>
<td>-0.0162</td>
</tr>
<tr>
<td></td>
<td>(-0.97)</td>
<td>(-1.55)</td>
<td>(-0.54)</td>
<td>(0.16)</td>
<td>(1.44)</td>
<td>(0.94)</td>
<td>(-0.14)</td>
</tr>
<tr>
<td>Below $200 MZ</td>
<td>121.4</td>
<td>0.0278***</td>
<td>0.00211</td>
<td>0.108</td>
<td>-0.105***</td>
<td>-0.0319</td>
<td>0.0449</td>
</tr>
<tr>
<td></td>
<td>(1.40)</td>
<td>(4.13)</td>
<td>(0.59)</td>
<td>(0.46)</td>
<td>(-2.87)</td>
<td>(-0.33)</td>
<td>(0.44)</td>
</tr>
<tr>
<td>Below $0 MZ × Post</td>
<td>-84.60</td>
<td>-0.0104</td>
<td>-0.00105</td>
<td>-0.0885</td>
<td>0.0587</td>
<td>-0.104</td>
<td>0.0386</td>
</tr>
<tr>
<td></td>
<td>(-0.78)</td>
<td>(-1.19)</td>
<td>(-0.23)</td>
<td>(-0.30)</td>
<td>(1.20)</td>
<td>(-0.79)</td>
<td>(0.27)</td>
</tr>
<tr>
<td>Below $0 MZ</td>
<td>34.06</td>
<td>0.0107</td>
<td>-0.00198</td>
<td>0.155</td>
<td>-0.0808*</td>
<td>0.135</td>
<td>-0.0988</td>
</tr>
<tr>
<td></td>
<td>(0.34)</td>
<td>(1.32)</td>
<td>(-0.46)</td>
<td>(0.55)</td>
<td>(-1.80)</td>
<td>(1.17)</td>
<td>(-0.76)</td>
</tr>
</tbody>
</table>

Controls: Y, Y, Y, Y, Y, Y, Y
Fixed effects: Y, Y, Y, Y, Y, Y, Y
Model: OLS, OLS, OLS, OLS, Logit, Logit, Logit

$R^2$: 0.474, 0.452, 0.346, 0.105, 0.116, 0.067, 0.042
Observations: 120,486, 120,486, 120,486, 116,282, 122,190, 120,911, 116,930
Figure 1: 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 dotted red line represents SI-based repayment amounts in the post-reform period.
Figure 2: Distribution of Surplus Income

This figure plots the observed distribution of Surplus Income (SI) before and after the 2009 regulatory reform in figures (a) and (b), respectively. The vertical lines indicate SIs of $0 and $200.
Figure 3: Discontinuity Tests for Post-Reform Proposal Filings
This figure displays the results of discontinuity tests performed at $200 Surplus Income cutoff for proposal filings submitted after the 2009 policy change. Subfigure (a) displays the results of the discontinuity test from McCrary (2008). Subfigure (b) displays the results of the discontinuity test from Cattaneo et al. (2018). 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.
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 Equations (1) through (3)). The difference between the actual density and the counterfactual density in the exclusion region is labeled bunching area and represents the bunching magnitude. The difference between the counterfactual density and the actual density on the right-hand side of \( r_U \) is the missing mass.
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 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, $SI \in (-100, 200)$. The estimated bunching magnitude, $b_n$, and its standard error are reported in the upper right box.
Figure 6: Bunching Magnitude by Travel-Related Transactions Costs
This figure shows the results of estimating bunching magnitude for subsamples based on filers’ travel-related transaction costs, as measured by the excess distance between filers and their chosen trustee in terms of multiples of the average distance to the three nearest trustees. Subfigure (a) shows the bunching magnitude for filers who travel excess distances of less than 1.2 times the minimum distance, i.e., employ nearby trustees and subfigure (b) shows bunching magnitude for filers who travel excess distances of more than 1.2 times the minimum distance, i.e., employ distant trustees. As in all our primary bunching analysis, the bunching magnitude is calculated using Surplus Income (SI) bins of size $100 and a 7th degree polynomial to model the counterfactual distribution. The estimated bunching magnitude, $b_n$, and its standard error are reported in the upper right box.
This figure plots the bunching magnitude estimate across the travel-related transactions costs distribution, where transactions costs are based on the excess distance between a proposal filer and their chosen trustee (see Section 5.1). All filings in the post-reform period are divided into 4 quartiles, from the lowest transactions costs (least excess distance) trustees (bottom quartile) to the highest transactions costs (greatest excess distance) trustees (top quartile). The solid line is the bunching magnitude estimate and the dashed lines represent the 95% confidence interval. As in all our primary bunching analysis, the bunching magnitude is calculated using Surplus Income (SI) bins of size $100 and a 7th degree polynomial to model the counterfactual distribution.
Figure 8: Bunching Magnitude by Trustee Leniency

This figure shows the results of estimating bunching magnitude for subsamples based on trustee leniency levels, as measured by round number prevalence in trustees’ approved proposals in the last three years (see Section 5.2). Subfigure (a) shows the bunching magnitude for trustees in the bottom 90% based on trustee leniency and subfigure (b) shows the bunching magnitude for trustees in the top 10% based on trustee leniency. As in all our primary bunching analysis, the bunching magnitude is calculated using Surplus Income (SI) bins of size $100 and a 7th degree polynomial to model the counterfactual distribution. The estimated bunching magnitude, $b_n$, and its standard error are reported in the upper right box.
Figure 9: Bunching Magnitude over Trustee Leniency Distribution

This figure plots the bunching magnitude estimate across the trustee leniency distribution, where trustee leniency is measured by round number prevalence in trustees’ approved proposals in the last three years (see Section 5.2). All filings in the post-reform period are divided into 8 octiles, from the least lenient trustees (bottom octile) to the most lenient trustees (top octile). The solid line is the bunching magnitude estimate and the dashed lines represent the 95% confidence interval. As in all our primary bunching analysis, the bunching magnitude is calculated using Surplus Income (SI) bins of size $100 and a 7th degree polynomial to model the counterfactual distribution.
This figure plots trustee leniency and prevalence of round numbers across filings with different levels of travel-related transactions costs. We measure these transactions costs based on the excess distance between proposal filers and their chosen trustee (see Section 5.1). All post-reform filings are categorized based on excess distance to chosen trustee: under 1 times, 1 to 1.2 times, 1.2 to 2 times, and over 2 times the average distance to the three nearest trustees. Trustees’ leniency is the round number prevalence for each trustee’s approved proposals in the last three years (see Section 5.2) and averaged across all post-reform filings within an excess distance group. Filing % of round numbers is measured for each post-reform proposal filing and averaged within excess distance groups. We report differences in trustee leniency and filing round numbers for each excess distance group relative to the comparison group of filers who travel under 1 times the average distance to the three nearest trustees. The point estimate for the difference for each group is represented by a filled-in square and the vertical capped bars represent 95% confidence intervals.
Figure 11: Use of Round Numbers Across Trustee Leniency Distribution
This figure plots the distribution of filing percentage of round numbers over trustee leniency groups. Filing % of round numbers is measured for each post-reform proposal filing and averaged within trustee leniency octiles, where trustee leniency is the round number prevalence for trustees’ approved proposals in the last three years (see Section 5.2). Each filing is categorized into one of eight octiles based on the trustee used, from the least lenient trustees (bottom octile) to the most lenient trustees (top octile). The plots are standard box-and-whisker plots, with the box reflecting the interquartile range for the % of round numbers, the line in the middle of each box reflecting the median value, and the caps reflecting the value 1.5 times farther from the median than the nearest quartile.
Figure 12: Trustee Market Share Dynamics by Trustee Leniency
This figure plots the dynamics of market shares of less and more lenient trustees. The groups are split based on whether the trustee used is in the bottom 90% (less lenient) or top 10% (more lenient) of trustees based on round number prevalence in each trustee’s approved proposals in the last three years as measured in 2009 (see Section 5.2). Subfigure (a) plots the quarterly number of filings for the two groups. The vertical line represents Q3 2009, the quarter of the reform. Subfigure (b) plots the quarterly estimated change in the difference of market share between the two groups. The estimate is calculated using an event study difference-in-differences specification regressing the number of filings against filing quarter dummy and a dummy for trustee leniency groups, absorbing trustee fixed effects and using heteroskedasticity-robust standard errors. The point estimate for the difference between the two groups in each quarter, relative to Q2 2009, is represented by a filled-in square and the vertical capped bars represent 90% confidence intervals. The vertical dashed line represents Q2 2009, which is the base category and omitted in the regression.
Figure 13: Proposal Count Dynamics

This figure plots the dynamics of proposal filings at a monthly frequency for 2006 through 2019 separated based on reported levels of Surplus Income (SI). 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 SI under $200. The vertical line represents September 2009, the month of the policy reform.
Appendices

Appendix A Construction of Surplus Income

Surplus Income (SI) is constructed based on exact OSB rules. This is based on data reported on the balance sheet and income statement of the debtor, using the following formula.

\[ SI_{i,t} = \text{Pro rate}_i \times (\text{NetFamilyIncome}_i - \text{Deductions}_i - \text{Threshold}_t). \]  

(7)

Pro rate is an adjustment based on whether the filing is made as a single individual or as part of a family. Net family income is the total monthly income after tax from all family members. Deductions includes all nondiscretionary spending as defined by the OSB (which includes child support expense, spousal support expense, child care, medical expense, fines, penalties, employment expense, etc.) The “Threshold” is an amount published every year by the OSB to adjust for inflation, which varies over family size.

Appendix B Statistical Comparison of Bunching Magnitudes Using Bootstrapping Methods

We statistically compare bunching magnitude estimates for different proposal filing groups using bootstrapping methods. Below we describe the procedure in further detail.

First, for each group, we run the bunching estimation procedure developed in Chetty et al. (2011) on 1,000 subsamples drawn (with replacement) from the overall group population. This provides us with 1,000 estimates of the bunching magnitude for the group.

Then, we compare filing groups' bunching magnitude in two ways: plotting the distributions of their 1,000 estimated bunching magnitudes and running a two-sample t-test to compare the means of the two distributions.

Before getting into the details of our methodology, we should note that these statistical tests compare the statistical differences between distributions of estimated figures. The bunching magnitudes calculated 1,000 times for each group are based on the excess mass in the exclusion region compared to a fitted polynomial, which is almost surely different for each group of filings (and likely slightly different for each draw within a group, as well). As we do not assert that the underlying population distribution of proposal filings should be identical across the groups, comparing bunching estimates using different counterfactual densities is not a problem for us. We use these bootstrapped distributions merely to compare the estimated level of bunching between groups.
We run two statistical comparisons using the method described above. The first comparison is between filings using nearby trustees and filings using distant trustees, as described in Section 5.1. Our distribution plot for these two groups are provided in Figure A6(a), where we clearly observe that filings using more distant trustees exhibit higher levels of bunching. In our two-sample t-test, we find that the mean of the distant trustee distribution is 2.9 percentage points higher than the mean of the nearby trustee distribution, and this difference is highly statistically significant (t-statistic of 35.7 with 999 degrees of freedom). This gives us considerable confidence in our rejection of the hypothesis that the magnitude of bunching is the same for proposals using nearby and distant trustees.

Second, we compare filings using historically less and more lenient trustees, as described in Section 5.2. Again, we plot the distributions for the two groups in Figure A6(b), which shows that there is more bunching among filers using more lenient trustees. Our t-test confirms that the mean bunching magnitude of the more lenient trustee distribution is 3.6 percentage points higher than the mean of the less lenient trustee distribution, and this difference is highly statistically significant (t-statistic of 37.3 with 999 degrees of freedom). On the basis of these findings, we argue that we can reject the hypothesis that the bunching magnitude for proposals using less and more historically lenient trustees is the same.

Appendix C Appendix Tables and Figures

Table A1: Description of variables used in this study

<table>
<thead>
<tr>
<th>Variable name</th>
<th>Definition</th>
</tr>
</thead>
<tbody>
<tr>
<td>total asset</td>
<td>total value of cash, furniture, personal effects, cash-surrender value of life insurance, securities, real property or immovable, motor vehicle, recreation equipment, tax refund, other assets.</td>
</tr>
<tr>
<td>unsecured debt</td>
<td>total value of unsecured debt of real property or immovable mortgage, bank loans, finance company loans, credit cards, taxes, student loans, loans from individuals, and others.</td>
</tr>
<tr>
<td>Variable name</td>
<td>Definition</td>
</tr>
<tr>
<td>-------------------------------</td>
<td>-------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------</td>
</tr>
<tr>
<td>secured debt</td>
<td>total value of secured debt of real property or immovable mortgage, bank loans, finance company loans, credit cards, taxes, student loans, loans from individuals, and others.</td>
</tr>
<tr>
<td>non-discretionary spending</td>
<td>total value of child support expense, spousal support expense, child care, medical expenses, fines and penalties, employment expense, debts and some other expense.</td>
</tr>
<tr>
<td>discretionary spending</td>
<td>total value of house utility expense, personal expense, medical expense, insurance expense and some other expense.</td>
</tr>
<tr>
<td>home equity</td>
<td>value of real property (house) minus principal mortgage amount.</td>
</tr>
<tr>
<td>available family income</td>
<td>total household income of net employment income, pension, child-support income, spousal support income, insurance benefit, social assistance, self-employment income and others.</td>
</tr>
<tr>
<td>reasons for financial difficulty</td>
<td>marital breakdown, unemployment, insufficient income, business failure, health concerns, accidents, overuse of credit, student loans, gambling, tax liabilities, loans cosigning, poor investments, garnishee, legal actions, moving relocation, substance abuse, supporting relatives.</td>
</tr>
<tr>
<td>repayment amount</td>
<td>total required repayment amount including monthly installment and lump sum pay.</td>
</tr>
<tr>
<td>payout ratio</td>
<td>total repayment amount over unsecured debt.</td>
</tr>
<tr>
<td>maturity</td>
<td>number of months between the planned completion date and the consumer proposal filing date.</td>
</tr>
<tr>
<td>Variable name</td>
<td>Definition</td>
</tr>
<tr>
<td>---------------------------------------------------</td>
<td>-------------------------------------------------------------------------------------------------------------------------------------------</td>
</tr>
<tr>
<td>monthly payment</td>
<td>monthly required repayment amount if the payment schedule is monthly installment.</td>
</tr>
<tr>
<td>% of rounding numbers (assets, debt)</td>
<td>percentage of variables whose value is a multiple of 100 from the categories of asset, secured debt and unsecured debt.</td>
</tr>
<tr>
<td>% of rounding numbers (all)</td>
<td>percentage of variables whose value is a multiple of 100 from the categories of assets, secured debt, unsecured debt, income and nondiscretionary expense.</td>
</tr>
<tr>
<td>trustee % of rounding numbers (assets, debt)</td>
<td>average % of rounding numbers (in assets, debt) of all the proposal filings submitted by the trustee in the past three years.</td>
</tr>
<tr>
<td>trustee % of rounding numbers (all)</td>
<td>average % of rounding numbers (in assets, secured debt, unsecured debt, income and nondiscretionary expense) of all the proposal filings submitted by the trustee in the past three years.</td>
</tr>
<tr>
<td>distance to the trustee</td>
<td>pairwise distance (based on latitude and longitude) between the trustee postal code and the filer’s residential address postal code.</td>
</tr>
<tr>
<td>distance to the nearest 3 trustees</td>
<td>average distance to the nearest 3 available trustees from the filer’s residential address.</td>
</tr>
<tr>
<td>searching cost (multiple)</td>
<td>distance to the chosen trustee divided by the distance to the nearest 3 trustees.</td>
</tr>
<tr>
<td>searching cost (km)</td>
<td>distance to the chosen trustee minus distance to the nearest 3 trustees.</td>
</tr>
<tr>
<td>full payment</td>
<td>the proposal required repayment is paid in full according to the consumer proposal payment schedule.</td>
</tr>
<tr>
<td>default</td>
<td>the proposal filer fails to pay back consumer proposal debt according to the payment schedule in 3 consecutive months.</td>
</tr>
<tr>
<td>Variable name</td>
<td>Definition</td>
</tr>
<tr>
<td>-------------------------------</td>
<td>-----------------------------------------------------------------------------</td>
</tr>
<tr>
<td>amendment and full payment</td>
<td>the consumer proposal is renegotiated and the proposal repayment is paid in full according to the new consumer proposal payment schedule.</td>
</tr>
<tr>
<td>rejection</td>
<td>the consumer proposal is rejected by the creditors.</td>
</tr>
<tr>
<td>withdraw</td>
<td>the consumer proposal is withdrawn by the debtor before approval.</td>
</tr>
<tr>
<td>amendment and default</td>
<td>the consumer proposal is renegotiated and the proposal filer fails to pay back consumer proposal debt according to the new payment schedule in 3 consecutive months.</td>
</tr>
</tbody>
</table>
Table A2: Effect of Bunching on Loan Performance (including other SI ranges)

This table reports the results of estimating Equation (6) comparing the default hazard of Manipulation Zone filings and Comparison Zone filings using Cox Proportional Hazards regressions. 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. These results are identical to those reported in Table 3 but include coefficients for other SI ranges left unreported for brevity in Table 3. Estimate coefficients are reported in exponentiated form and t-statistics are in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

<table>
<thead>
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<th>(1) Default</th>
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<td>Below $200$ MZ × Post</td>
<td>0.930**</td>
<td>0.921**</td>
<td>0.924**</td>
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<td>(-2.00)</td>
<td>(-2.10)</td>
<td>(-1.96)</td>
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<td>Below $200$ MZ</td>
<td>1.019</td>
<td>1.032</td>
<td>1.027</td>
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<td></td>
<td>(0.57)</td>
<td>(0.90)</td>
<td>(0.76)</td>
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<td>Below $0$ MZ × Post</td>
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<td>(1.48)</td>
<td>(0.78)</td>
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<td>Below $0$ MZ</td>
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<td>(-1.33)</td>
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<td>1.029</td>
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<td>(0.80)</td>
<td>(0.79)</td>
<td>(1.09)</td>
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<tr>
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<td>1.011</td>
<td>1.011</td>
<td>0.994</td>
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<td>(0.33)</td>
<td>(0.33)</td>
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<td>SI ∈ (-1200,-800] × Post</td>
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<td>0.971</td>
<td>0.974</td>
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<td>(-0.68)</td>
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<td>1.107**</td>
<td>1.101**</td>
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<td>(2.58)</td>
<td>(2.57)</td>
<td>(2.41)</td>
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<td>0.969</td>
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<td>(2.30)</td>
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<td>Post</td>
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<td>0.998</td>
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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] (-300,-50]
Pseudo $R^2$: 0.009 0.009 0.009
Observations: 206,330 206,330 206,330
Figure A1: Distribution of SI mod 100
This figure plots the distribution of the remainder of SI when divided by 100 for the pre- and the post-reform periods. We calculate the remainder as: Remainder of SI = SI mod 100.
Figure A2: Discontinuity Tests at Different Cutoffs in Pre-reform Period
This figure replicates Figure 3 in the main text with the discontinuity tests conducted at the cutoff of $0 and $200, respectively, in the pre-reform filing sample.
Figure A3: Bunching Magnitude Estimation Based on Alternative Bin Sizes
This figure replicates Figure 5 in the main text with different bin sizes and exclusion regions.
Figure A4: Bunching Magnitude over Trustee Leniency Distribution
This figure is constructed similarly to Figure 9 except that leniency level is measured by the % of round numbers from asset-, debt-, income-, and expense-related variables.

Figure A5: Pseudo Regulation Change Dates and Proposal Default
This figure plots estimates of the main DID coefficient and the 95% confidence intervals of the effect of the policy change on proposal default as in Table 3, column (1). However, instead of using the actual policy change date, we use every month from January 2007 to December 2008 as a placebo reform date. The placebo policy change dates are shown on the x axis. The estimates of the effect of the placebo policy changes on default are shown as odds ratios. This table shows that none of these estimates is statistically significant.
These figures present the distributions of bootstrapped estimates of the bunching magnitude, following the method detailed in Appendix B. Figure (a) shows the distribution of bootstrapped bunching estimates for populations of proposal filings with distant and nearby trustees in red and blue, respectively. We define distant trustees as trustees located 1.2 times farther from the filer than the average distance to the filer’s three closest trustees and nearby trustees are trustees located closer than the same threshold. Figure (b) plots the distribution of bootstrapped bunching estimates for populations of proposal filings with less and more lenient trustees in red and blue, respectively. We define less lenient trustees as trustees in the bottom 90 percent of trustees by three-year historic leniency and more lenient trustees are trustees in the top 10 percent.

Figure A6: Bunching Magnitude Comparison
Figure A7: Estimation of Bunching Magnitude for Homeowners

This figure shows the result of estimating bunching magnitude for proposal filers who own a home using Surplus Income (SI) bins of size $100 and a 7th degree polynomial to model the counterfactual distribution. 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. The estimated bunching magnitude, $b_n$, and its standard error are reported in the upper right box.
Figure A8: Surplus Income Distribution by Available Trustee Leniency

This figure shows the distribution of proposal filings across Surplus Income (SI) bins for two groups of proposal filings: those filed by debtors residing in neighborhoods with less lenient trustees and those filed by debtors residing in neighborhoods with more lenient trustees. The horizontal axis represents SI bins (of size $50 each). The vertical axis represents the number of filings in the post-reform period in each bin. The dashed blue line with circular points is the number of filings per bin for filings from neighborhoods with less lenient trustees. The red solid line with triangular points is the number of filings per bin for filings from neighborhoods with more lenient trustees.