FEDERAL RESERVE BANK
PHILADELPHIA

Working Papers
RESEARCH DEPARTMENT

Debtor Income Manipulation in Consumer Credit Contracts

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
National University of Singapore

WP 22-35

PUBLISHED
October 2022

REVISED
December 2023

FRB CFI PHILADELPHIA

ISSN: 1962-5361

DOI: https://doi.org/10.21799/frbp.wp.2022.35

Debtor Income Manipulation in Consumer Credit Contracts

Vyacheslav Mikhed, Sahil Raina, Barry Scholnick, and Man Zhang*

December 2023

Abstract

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

Keywords: consumer credit, 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: Institute of Real Estate and Urban Studies, National University of Singapore, email: man.zh@nus.edu.sg. We are grateful to the Office of the Superintendent of Bankruptcy (OSB), Canada, for the provision of consumer insolvency data. Financial support from the Social Sciences and Humanities Research Council of Canada (SSHRC) (Raina and Scholnick) is gratefully acknowledged. We would also like to thank Philip Armour, Julia Cheney, Robert M. Hunt, Sam Kruger, Igor Livshits, Philippe d'Astous, and participants at the Boulder Summer Conference on Consumer Financial Decision Making, the Consumer Finance Round Robin, the Bank of Canada, HEC Montreal, the Federal Reserve Bank of Philadelphia, Fordham University, the RAND Behavioral Finance Forum, University of Alberta, University of New South Wales, and University of Sydney for their comments. The views expressed in this paper are solely those of the authors and do not necessarily reflect the views of the Federal Reserve Bank of Philadelphia, the Federal Reserve System, the Government of Canada (Industry Canada), or the Office of the Superintendent of Bankruptcy (Canada).

1 Introduction

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

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

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

Under Canadian insolvency law, the amount payable to creditors in a consumer proposal implicitly depends on the debtor's Surplus Income (which is reported income minus allowable expenses). The 2009 reform introduced a sharp discontinuity in the total amount of repayment for debtors with a monthly Surplus Income (SI) greater than or equal to $200. Since the reform, because of the structure of the payment schedule, debtors are required to pay an additional $1,200 over the life of the plan when their reported SI increases from $199 to $200. For debtors whose SI is below that income cutoff, the reform has no effect on payments to creditors. This plausibly exogenous increase in the total payment amount creates an incentive for debtors with an SI over $200 to reduce their reported SI to below the $200 cutoff to avoid the higher debt repayment.

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

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

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

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

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

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

A third literature we contribute to studies policies and laws designed to protect creditor rights such as BAPCPA. Most of the recent studies in this literature (Chakrabarti and Pattison, 2019; Gross et al., 2021) study BAPCPA and find that it strengthened creditor rights while improving credit access by making personal bankruptcy more costly and restrictive.[1] While we also examine a policy intended to strengthen creditor rights, our results show that the policy-induced bunching in our setting lowers creditors' surplus by reducing the total repayment they receive in proposals.

Finally, our paper adds to the literature studying policies that introduce arbitrary cutoffs and induce non-linear incentives for data manipulation.[2] Much of this literature studies tax evasion and avoidance (see Almunia and Lopez-Rodriguez, 2018; Camacho and Conover, 2011; Foremny et al., 2017; Kleven et al., 2011; Fack and Landais, 2016), showing that arbitrary income-based cutoffs in taxation schedules increase taxpayers' incentives to underreport income below such cutoffs. We are part of a small but growing literature documenting the incentive effects of such cutoffs in credit markets.[3] Our contribution to this literature is that we examine how discontinuities in debtors' income-based payment schedules motivate them to strategically manipulate income data reported to creditors, thereby reducing recovery by creditors.

2 Institutional Setting

2.1 Insolvency in Canada

There are two kinds of insolvency available to consumers in Canada: consumer *proposal* and consumer *bankruptcy*, which are somewhat similar to Chapter 13 and Chapter 7 bankruptcy in the U.S., respectively. While consumer proposal involves a negotiated restructuring of debt wherein the debtor and their creditors reach an agreement in which the debtor repays a lower amount over a

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

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

[3] For example, see DeFusco and Paciorek (2017) on discrete jumps in mortgage interest rates at conforming loan limits, Bachas et al. (2021) on notches in the guarantee rate schedule for SBA loans, and DeFusco et al. (2020) on how the Dodd-Frank Act introduced discontinuities in the cost of originating mortgages with high debt-to-income (DTI) ratios.

longer period, consumer bankruptcy involves a rule-based liquidation of assets. We discuss each in turn below.

Consumer proposals are legal agreements between insolvent debtors and their creditors to modify the debtors' unsecured debt obligations (e.g., credit card debt), while not altering the debtors' secured credit contracts (e.g., mortgages). Under this system, an insolvent debtor makes a "proposal" to their creditors to repay some portion of their unsecured debts over a period of time. If the creditors agree to the proposal, then the proposal becomes a legally binding contract, enforced by the Canadian bankruptcy regulator, the Office of the Superintendent of Bankruptcy (OSB). These proposal contracts typically entail the debtor making a series of regular payments for a period that can last for up to five years. If the debtor does not make the agreed-upon payments for three consecutive months, then the debtor has defaulted on the proposal contract and the contract is voided.[4]

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

Because insolvent debtors are free to select either kind of insolvency (a negotiation-based proposal contract or a rules-based bankruptcy contract), the relationship between these two kinds of insolvency is important. Crucially, any change in the amount that is required to be paid by bankruptcy filers in bankruptcy has an impact on negotiations between debtors and creditors entering into proposal contracts. Because of an asymmetry in the legal rights of creditors across the two forms on insolvency,[5] they are likely to reject a proposal filing if they believe that they will be better off if the debtor files for bankruptcy instead. As a result, the amount that a debtor needs to offer creditors in a proposal filing for their proposal to be accepted needs to be larger than or equal to the amount that would be repaid to creditors in a bankruptcy filing. As such, bankruptcy payments

[4] 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.

[5] 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.

become an "informal floor" for proposal payments.[6] Therefore, when there is a regulatory increase in the payments required from debtors to creditors under the rules-based bankruptcy system, this results in creditors accepting new negotiation-based proposal filings only if there is a similar increase in the repayments proposed by the debtor.

2.2 September 2009 Changes to the Bankruptcy and Insolvency Act

Canadian insolvency regulations changed on September 18, 2009, when the Bankruptcy and Insolvency Act (BIA) was amended by the Canadian Parliament. This followed the initial announcement of these amendments by the Office of the Superintendent of Bankruptcy (OSB) on August 14, 2009. Allen and Basiri (2018) provide a broad overview of these amendments to the BIA. In this paper, we focus on the changes to debt repayment rules in bankruptcy related to a measure of debtors' net income: Surplus Income (SI). There were other changes due to this reform,[7] but we exclude filings affected by these other changes from our analyses to make proposals before and after the reform comparable.[8]

The 2009 amendments to the BIA did not change any rules dictating how debtors and creditors negotiate consumer proposals. They did, however, increase the income-contingent payments that some bankruptcy filers were required to make to their creditors. These payments are contingent on the filers' monthly SI, which is essentially the income of the debtor minus authorized non-discretionary expenses, minus a family size-based deduction.[9] The SI reported by a bankrupt debtor determines the amount of income-contingent payments made to the creditor.

Our identification strategy exploits how the 2009 amendments affected debtors with different levels of SI in the post-reform period. Figure 1 illustrates how the amount that a bankrupt debtor is required to pay to the creditor in income-contingent payments (vertical axis) changes with reported SI (horizontal axis). As displayed in that figure, the main rule (in both pre- and post-reform

[6] 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).

[7] For example, after the reform, debtors with net debt (debt minus principle residence's mortgage) between $75,000 and $250,000 were eligible to file consumer proposals.

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

[9] 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.

periods) is that bankruptcy filers who have an SI equal to or larger than \$200 are required to pay their creditors 50% of their SI per month. The gray region in the figure for SIs between \$0 and \$200 represents different interpretations among trustees for SI-based repayments in this region. Most trustees (and Allen and Basiri (2018)) quote a monthly repayment of 50% of SI, whereas some other trustees quote no repayment for bankruptcies in this region. These different interpretations do not pose any issues for our analyses, as they persist across our sample period. Bankruptcy filers with a negative SI (i.e., income less than expenses) are not required to make any income-contingent payments to creditors.

The key part of the 2009 reform for our identification strategy is that the OSB increased the repayment period that bankrupt debtors with an SI equal to and above \$200 are required to pay creditors from 9 months to 21 months. This rule change effectively created a new payment discontinuity, or notch, for proposal filers at $SI = \$200$ (where the term "notch" used here is taken from the bunching literature, described in detail in Section 4). Given the "informal floor" relationship between bankruptcy and proposal repayments, this regulatory change to bankruptcy meant that proposal filers could reduce their expected payment amount to creditors by approximately \$1,200[10] if they reduced their reported SI from \$200 to slightly below \$200.[11]

Figure 1 also illustrates various other elements of the regulatory environment. Because the rule change affects all debtors with an SI of more than \$200, in 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.[12] 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.

[10] In the post period, a bankruptcy filer with an SI slightly below the \$200 cutoff would make payments for 9 months times (50% of \$200) = \$900. If that debtor had an SI of \$200, she would make payments for 21 months times (50% of \$200) = \$2,100. Thus, moving the SI from just above to just below the \$200 notch would save \$1,200 in payments. As mentioned previously, due to the different interpretations of trustees, the savings from shifting the SI from \$200 to \$199 can be between \$1,200 and \$2,100. For the remainder of this paper, we use the more conservative \$1,200 savings as our estimate of the savings from SI manipulation.

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

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

3 Data

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

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

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

4 Evidence of Data Manipulation

In Section 2, we describe why the policy reform created a new payment discontinuity at SI = \$200. This discontinuity created a new incentive for debtors with true SIs equal to or greater than \$200 to manipulate their filings so their reported SIs fell below \$200 as this would significantly reduce their income-contingent payments. In this section, we use the bunching methodology[13] to show that

[13] 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

debtors strategically manipulate their reported SIs to fall below the $200 SI discontinuity.

4.1 Graphical Evidence of SI Manipulation

As a starting point for the empirical analysis, 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. On the other hand, in Figure 2(b), we see that, in the post-reform period, there is bunching below the $200 cutoff.[14]

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

To further confirm that our findings are not spurious, we conduct placebo tests to determine whether the post-reform bunching below $200 SI is unique. Specifically, we perform a McCrary discontinuity test using every hundred-dollar SI value from -$1,800 to +$1,800 (-$1,800, -$1,700, . . . , $1,800) as a threshold and calculate the statistical significance of a discontinuity at that threshold. For consistency, we include proposal filings up to $600 away from the pseudo-threshold for each discontinuity test.[15] We perform this placebo analysis for both the pre- and the post-reform periods and report our findings in Figure 4. Figure 4(a) shows that, in the pre-reform period, there is no threshold where there is a discontinuous decrease in proposal filing volume from below the threshold to above. Figure 4(b) shows that the $200 SI threshold is the only extremely highly significant (at the $p < 0.0001$ level) discontinuous drop in the distribution in the post-reform period. In both periods, we find some thresholds with a discontinuous increase in proposal filings, but those are not

many other contexts, as described in the survey of Kleven (2016).

[14] 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.

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

suggestive of bunching below a threshold like our focal \$200 SI threshold.[16]

4.2 The Bunching Estimation Methodology

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

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

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

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

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

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 excess mass region), and r_L is the lower bound of the exclusion region. The counterfactual distribution is estimated as shown on the right-hand side in Equation (1). We perform a high-order polynomial fit on bin counts for proposal filings wherein, by including fixed effects for bins in the exclusion region, we ignore the bins within the exclusion region. The counterfactual distribution is thus defined as: $\hat{C}_j = \sum_{i=0}^{q} \hat{\beta}_i (Z_j)^i$. $\hat{B_M}$ represents the excess number of filings within the exclusion region, which is the difference between the actual counts $\sum_{j=r_L}^{r_U} C_j$ and the estimated counterfactual distribution $\sum_{j=r_L}^{r_U} \hat{C}_j$. The expression in parenthesis on the left-hand side of Equation (1) represents the upward adjustment of the counterfactual estimates to the right of the exclusion region to satisfy the

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

integration constraint, which requires that the missing mass above the cutoff equal the excess mass in the exclusion region. The excess mass in the bunching region is thus defined by Equation (3): $\hat{b}_n$ is the excess mass in the exclusion region relative to the total mass under the counterfactual distribution.[17] This amount can be interpreted as the percentage increase of filings in the bunching region because of the discontinuity.

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

4.3 Estimation of Surplus Income Bunching

In this section, we describe how we implement the bunching methodology in our setting. We can precisely calculate reported SI based on data from the proposal filings with little measurement error. Our setting in this regard is similar to the tax literature using bunching techniques.

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

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

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

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 6. The vertical dotted lines demarcate the exclusion region (i.e., $SI \in (-100, 200)$). We use bins of size \$100 and a 7th degree polynomial to estimate the counterfactual distribution. The blue dashed line with solid circles plots the actual number of filings per bin. The estimated counterfactual distribution is indicated by the red smooth curve and can be seen to fit points outside the exclusion region well. The bunching within the exclusion region is easily observable on the left of the \$200 cutoff. The estimated excess mass is 0.079, which means that 7.9% of filings for SI in the range of -\$100 to \$200 arise due to the policy reform. This is an economically meaningful increase in filings below the cutoff and, given the standard error of the estimate, 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.[18] We find statistically significant bunching across all combinations, where the excess masses in percentage terms are comparable across different settings. In the illustrated estimate in Figure 6 and for the rest of the paper, we adopt the most conservative specification from column (7).

A critical assumption in the Chetty et al. (2011) bunching methodology is that there is no significant extensive margin switching near the policy cutoff. However, if the policy change induced more insolvent debtors to choose to file proposals below the \$200 SI threshold or fewer of them to choose to file proposals above the threshold, it could induce a filing distribution similar to what we observe but which does not arise because of SI manipulation. In Section 7, we consider these possibilities and present evidence inconsistent with extensive margin switching explaining our bunching magnitude estimates on its own.

Magnitudes of bunching observed in other contexts in the literature are quite heterogeneous (see Kleven, 2016). Our magnitude is smaller than some of the other studies, especially in the tax literature. This difference can be explained by the fact that, in our setting, every proposal filing

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

needs to be made to the regulator (OSB) via a third-party intermediary (i.e., a trustee, typically a for-profit chartered accountant). A trustee is an Officer of the Court with a legal duty to represent the interests of the debtor and of creditors. As such, the trustee is legally tasked with ensuring the accuracy of the filing by the debtor. Therefore, the presence of trustees in the proposal system should reduce the prevalence of data manipulation (as measured by bunching) compared with a system with no third-party intermediaries (as in some of the tax literature).

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

5 The Consequences of Data Manipulation on Future Default

Our results up to now imply that SI manipulators may have true income, which is higher than their reported income. In other words, they may have higher actual repayment capacity than that implied by their reported income. If SI manipulators lower their reported income to avoid higher debt repayment, these filers should default on their proposals less than a comparable group of filers not subject to the reform-induced incentive.[19] 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.

5.1 Measuring the Effects of Bunching on Proposal Default

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

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

like the above-cited papers, because our policy change alters the extent of bunching from the pre- to post-reform period.[20]

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

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

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

Instead, given that the lower bound of the Below \$0 Manipulation Zone is -\$100, we designate filings below -\$100 as the Comparison Zone, where debtors have no incentive to manipulate SI in either the pre- or post-reform periods. We designate -\$400 as the lower bound for this Comparison

[20] 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.

Zone to keep it close to the Manipulation Zone (in terms of SI) and make filings in the Comparison Zone more comparable to filings in both Manipulation Zones. Our results are robust to various alternative definitions of this lower bound.

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

5.2 Cox Proportional Hazards Model of Default

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

In our setting, the length of time from the start date to a default on the long-term proposal contract is modeled as the time to failure in the Cox proportional hazards model. An advantage of the Cox model is that it accounts for right censoring in our data, unlike other alternatives such as a logistical regression model. In addition, a large literature has documented that the default probability in a long-term credit contract is often related to the age of the debt contract, which we include in our Cox model.[21]

Our baseline specification is a standard Cox model estimated at the proposal level using proposal filings with SI reported from -\$2,000 to \$200:

$$\begin{aligned} h_i(t) = \gamma_0(t) \times \exp(\gamma_m \times \mathit{Below}\ \$200\ MZ_i \times \mathit{Post}_t + \gamma_b \times \mathit{Below}\ \$200\ MZ_i \\ + \gamma_k \times \mathit{Below}\ \$0\ MZ_i \times \mathit{Post}_t + \gamma_n \times \mathit{Below}\ \$0\ MZ_i \\ + \gamma_c \times \mathit{Controls}_i + \mu_k + \epsilon_i), \end{aligned} \tag{4}$$

where $h_i(t)$ is the monthly hazard of default (failure) for consumer proposal i at time t. $\gamma_0(t)$ is the baseline hazard function, which is the hazard rate when all covariates have values of zero. $\mathit{Below}\ \$200\ MZ_i$ takes the value of 1 if the proposal filer has SI between \$0 and \$200, and 0 otherwise, i.e., if the filing belongs in the Below \$200 Manipulation Zone. $\mathit{Below}\ \$0\ MZ_i$ is equal to 1 for proposals with SI between -\$100 and \$0, and 0 otherwise, i.e., proposals in the Below \$0 Manipulation Zone. We include three additional SI bins (the SI between -\$2,000 and -\$1,200, the SI between -\$1,200 and -\$800, and the SI between -\$800 and -\$400) and their interactions with Post_t in all of our specifications but omit them from our tables for brevity. $\mathit{Controls}_i$ are filing and filer characteristics as reported in Table 1. Continuous control variables are converted into sets of indicator variables to account for their potential nonlinear effects on default. We also control for repayment amount and payout ratio for default propensity prediction. The choice of control variables is guided by availability and the recent literature on personal bankruptcy. μ_k represents a series of fixed effects including liability type, joint filing, repayment schedule type, debtor province, occupation category, and filing year. ϵ_i is an error term. The variable of interest is γ_m, which captures the change in the default hazard rate for filings in the Below \$200 Manipulation Zone from the pre- to the post-reform period, in comparison to the change for filings in the Comparison Zone.

A key identification assumption of our DID analysis is that, in the absence of the policy change, the trends in loan performance for filings in the Below \$200 Manipulation Zone and the Comparison Zone should be similar. While we cannot test this hypothesis directly after the policy change, we provide two sets of evidence supporting this assumption. First, we show that loan performance for the reported SI in the different zones move together in the pre-reform period. We rerun the tests as specified in Equation (4) using each month from the start of 2007 to the end of 2008 as

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

the pseudo-policy change month. We report the estimated coefficient, γ_m, for each regression in Figure A4 in the Appendix. None of the odds ratios on the main DID term are statistically different from 1. These results support the parallel trends assumption and help to validate our empirical setting.

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

5.3 Default Results

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

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

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

5.4 Discussion of Default Results

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

6 How Data Manipulation Affects Creditors

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

6.1 Estimated Creditor Losses from Bunching

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

To estimate the loss to creditors from the bunching, we use unique data on the actual payments to creditors made by each proposal filer. These data come from OSB Form 14 (Statement of Receipts and Disbursements). They contain information on payments to all parties involved in proposals, including trustees, the OSB, the court, and creditors, for each completed proposal. Critically, this information exists for all proposals, including those that ended in default, were not paid in full, were amended, etc. In many other credit contexts, it is often difficult to observe data on actual payments on loans in default, partial payments on loans or loan modifications, as well as payments to various intermediaries. Thus, researchers typically need to make multiple assumptions to estimate payments and creditor losses for such loans. In our setting, on the other hand, we have actual data on all payments and fees paid in each proposal and can observe creditors' receipts directly. Because our payment data are only available for completed proposals, in this estimation, we restrict our sample to proposals with a planned completion date before the end of our sample (30 June 2019), which reduces our sample size.[22] On average, as shown in Panel A of Table 1, creditors receive 75.3% of their planned repayment amount.

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

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

In our non-parametric estimation exercise, we calculate the per-filing loss to creditors with as little structure as possible. For this exercise, we make two assumptions. First, we assume that, if SI-manipulating bunchers had reported their true SI, they would have repaid the same amount as non-manipulating filers in that SI range. For example, if a manipulating filer's true SI is $500, we assume that this filer would have repaid the same amount as non-manipulating filers with an SI of $500. This assumption allows us to use observable repayment amounts of non-manipulating filers in the missing mass region (above $200 SI) to proxy what SI-manipulators would have paid if they had reported their income truthfully. In other words, we assume that, conditional on true SI, we can proxy the hypothetical truthful repayment amount of SI manipulators by the repayment amount of non-manipulators in the missing mass region, which we can observe. Second, because we cannot distinguish SI manipulators and non-manipulators in the bunching region, we proxy for the repayment amount of SI-manipulating bunchers with the average repayment amount of bunching region filings.[23]

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

In the third column of Table 5, we report this non-parametric per-filing creditor loss estimate, varying the upper bound of the missing mass region from $500 to $2,000 by $250 increments.[24] We find that creditor losses increase monotonically with the upper bound of the missing mass region. At the relatively low upper bound of $500, which assumes that bunchers have true SIs between $200 and $500, we estimate that creditors lose $846 per manipulator filing. At a $2,000 upper bound, which, as in our bunching estimation, assumes that manipulators' true SIs are somewhere between $200 and $2,000, we estimate that creditors lose $3,511 per manipulator filing.[25] In the fourth

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

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

[25] As we explain in Section 4, the missing mass in our setting is diffuse, and the $2,000 upper bound of the missing mass region is implied by the bunching methodology.

column of the table, we report the loss to creditors as a percentage of how much they would have been repaid, on average, if the filers who moved to the bunching region had remained in the missing mass region. We estimate that creditors lose anywhere from 12% to 36% of their average repayment when filers misreport SI to place themselves in the bunching region.

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

We report the findings of this parametric estimation exercise in the fifth and sixth columns of Table 5. We show, in Column (5), that the loss per filing to creditors ranges from \$441 to \$1,901, increasing monotonically as we raise the upper bound of the missing mass region from \$500 to \$2,000. In Column (6), we report that these loss estimates also increase monotonically with the missing mass region upper bound as a percentage of average repayment amount in that region. Defining the missing mass region using the \$500 SI upper bound, creditors lose 6% per filing and, using the \$2,000 SI upper bound, they lose nearly 20%. Compared with the non-parametric dollar estimates of the losses, both the level and percentage estimates are smaller. This makes sense as, in this exercise, we control for the other characteristics of the filer and the filing, many of which are likely correlated with the filing's SI.

We note that our per-filing creditor loss estimates are similar in magnitude to the benefit of income manipulation for debtors. As we have discussed previously, after the 2009 policy reform, if a filer with true SI of $200 reported an SI of $199, they could reduce their repayment by $1,200. Our creditor loss estimates straddle this marginal debtor's benefit from income manipulation. That the marginal debtor's income-manipulation benefit lies within the upper and lower bounds of our estimates of creditor losses per filing gives us more confidence in the estimates.

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

6.2 Creditors' Response to Bunching

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

6.2.1 Empirical Methodology

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

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

Our baseline specification is a DID-type regression estimated at the proposal level using the sample of proposal filings with an SI reported from -\$2,000 to \$200. The regression equation is as follows:

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

where the dependent variable $LoanTerms_i$ is one of the proposal contract terms (proposal rejection by creditors, total repayment amount, natural log of total repayment amount, total repayment amount over total unsecured debt ratio, maturity, proposal maturity over 60 months, subsequent filing withdrawal by filer) for proposal filing i, and all remaining variables are defined as in Section 5. As in the regressions in that section, we include three additional SI bins (SI between -\$2,000 and -\$1,200, SI between -\$1,200 and -\$800, and SI between -\$800 and -\$400) and their interactions with $Post_t$ in all of our specifications but omit them from our tables for brevity. To account for serial correlation and region-specific random shocks, we cluster standard errors at the province level and include monthly fixed effects in all specifications. If the loan term/outcome is a binary variable (i.e., whether proposal maturity is more than 60 months, whether the proposal filing is subsequently rejected or withdrawn), we estimate Equation (5) using a logit regression. The coefficient of interest is β_{PB}, which measures the differential change in negotiation outcomes for the filings in the Below \$200 Manipulation Zone relative to filings in the Comparison Zone following the policy change, holding all filer and filing characteristics constant.

6.2.2 Proposal Rejection and Loan Terms

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

Table 6 reports results for creditor proposal rejection overall and for six different groups of filers, with different observable characteristics. To create these groups, we consider three debtor characteristics: asset holding, home equity, and the prevalence of round numbers in proposal filing.[26] We split the sample at the median for each of these variables and estimate the model in Equation (5) for each subsample. Column (1) of Table 6 reports our findings for the full sample and columns (2) through (7) report the subsample findings.

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

While outright proposal rejection is the most direct potential creditor response to data manipulation, we also consider contract term adjustments in response to debtor manipulation. This distinction between creditors rejecting a contract with a debtor outright versus attempting to adjust the terms of the contract with the debtor has been examined in a variety of credit market contexts.[27]

The results of our analyses of contract terms are reported in Table 7. We report results on five

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

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

contract terms. These results indicate that there are small economic and statistically insignificant changes for proposals in the Below $200 Manipulation Zone relative to the Comparison Zone (SI between -$400 and -$100) from the pre- to the post-reform period. As these outcomes reflect equilibrium outcomes of negotiations between creditors and debtors (rather than the direct actions of creditors), we find that data manipulation does not lead to higher debt repayment (in terms of dollars or repayment rate), shorter maturity, or fewer withdrawals. This result suggests that, in aggregate, creditors do not respond to data manipulation by changing proposal contract terms.[28] Taken together, our results show that creditors respond to manipulation with an outright rejection of proposals with incongruous details (e.g., high home equity and assets with low income), rather than changing the terms of proposals, consistent with the aforementioned evidence on creditor behavior in other credit market contexts.

7 Evidence on Identification Assumptions

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

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

7.1 Entries Below the Threshold

Although entry of new filers into proposals below the $200 SI threshold after the reform could, in theory, be a potential reason for bunching below that threshold, it is implausible, given the specifics of the reform. First, the 2009 reform did not substantially change the terms of proposal filings with

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

SIs under \$200 and net debts below \$75,000, which we study in this paper.[29] Therefore, debtors who would not have entered the consumer insolvency process prior to the reform would have no new incentives to enter afterward either. This lack of change in rules for the below-threshold region suggests that entries below the threshold are unlikely to explain the bunching we observe.

Moreover, recall that, because of the "informal floor" feature of the consumer default process in Canada discussed in Section 2, those who would otherwise have filed bankruptcy have no repayment-based reason to file proposals before or after the reform. This is because the amount that debtors need to offer creditors in proposals (in order for their proposals to be accepted) needs to be at least as large as the amount that the creditors would be repaid in bankruptcy. As this informal floor link between bankruptcy and proposal is unchanged by the reform, someone who would have otherwise filed a bankruptcy has no reason to file a proposal after the reform. Given these two key institutional details, it is unlikely that there is sufficiently increased entry into proposals from bankruptcy or solvency below the \$200 SI threshold after the reform to explain the observed bunching.[30]

7.2 Exits Above the Threshold

How might increased exits above the \$200 SI threshold after the reform explain the bunching we observe below that threshold? If the reform induces debtors with SIs above \$200, who would otherwise have filed proposals, to exit the proposal system, then there will be a dearth of filings above \$200 SI after the reform. This shortage above the threshold could make the filing count below the threshold appear to increase after the reform, in comparison.

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

[29] Allen and Basiri (2018) show that the total number of proposal filings increases significantly after the reform. The relevant figures in that paper incorporate the effects of another part of the reform that encouraged increased proposal filings: Debtors with net debt (debt minus principle residence's mortgage) between \$75,000 and \$250,000 were newly allowed to file consumer proposals. We exclude these newly eligible filers from our data. Without them, there are no substantial inflows to proposals around the \$200 SI cutoff after the reform, as we show in Figure 11.

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

dynamics of filer counts around the reform.[31]

7.2.1 Bunching Heterogeneity Across Filing Subgroups

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

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

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

In our McCrary discontinuity tests, presented in Figure 8, we observe that the discontinuity at $200 SI is five times larger among SE filers than the discontinuity among WE filers. Bunching magnitude is also much larger for SE filers than for WE filers. As we show in the first two subfigures of Figure 9, bunchers constitute approximately 6.3% of WE filers in the region below $200 SI, whereas they constitute 26% of SE filers in that region. The five-times-larger discontinuity at the threshold and four-times-larger bunching magnitude among self-employed proposal filers, who find

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

it relatively easier to manipulate SI, are highly inconsistent with the extensive margin explanation that the observed bunching after the reform is driven solely by exits from proposal filings above the threshold.

Homeowners. Insolvent homeowners who file for bankruptcy or go into foreclosure can lose their houses in the process, whereas, under proposal, they are allowed to keep their homes. This treatment of homes is unchanged by the reform and, therefore, homeowners tend to prefer proposal to the other forms of insolvency both before and after the reform. As a result, they are unlikely to switch out of proposal filing because of the reform. If the bunching we observe is driven by exiting out of proposals after the reform, then homeowners, who are unlikely to exit, should not exhibit any bunching.

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

High-asset filings. Debtors whose proposals have high asset values are unlikely to exit proposals after the reform because high-asset filers benefit from proposals as they can protect their assets from seizure by creditors.[32] Therefore, if the bunching we observe is driven primarily by exits from proposals above the $200 SI threshold, we should not find any bunching among high-asset filers, who are unlikely to exit proposals.

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

7.2.2 Filer Dynamics Near the $200 SI Threshold

If exits from proposals above the threshold drive our bunching findings, then we should observe some specific changes in the filer distribution near the threshold around the time of the reform. Below, we study two statistics that, if exits from proposal were driving bunching, should change in

[32] We study both high-asset filings and homeowner filings separately, despite both having high asset values because homeowners may have some idiosyncratic homeownership-related liquidity needs (e.g., home maintenance costs) that affect their decision of how to resolve their insolvency. On the other hand, high-asset filers do not necessarily have these homeownership-related liquidity needs.

a specific way. Our findings are again inconsistent with exits above the threshold being the main cause of the observed bunching.

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

To test this hypothesis, we study the dynamics of the distribution of unsecured debt as the primary benefit of proposal for filers just above the cutoff around the reform date.[33] In Figure 10, we examine unsecured debt in dollar terms (in panel (a)) and relative to total income (in panel (b)). The two panels plot the 10th, 25th, and 50th percentile of the two unsecured debt distributions for filings just above the $200 SI threshold (i.e., with SI between $200 and $400). Both panels show that the distribution of unsecured debt evolves smoothly from before to after the law was implemented in September 2009. The 10th percentile dynamics are especially important as this is where marginal-benefit filers are likely to be concentrated. Even for this percentile, we do not find any significant shift in proposal benefits after the reform's implementation. This result is inconsistent with the extensive margin argument that the observed bunching is driven primarily by exits from proposal of (marginal-benefit) filers with SI above $200 after the reform.

Filer Counts. If the bunching we observe arises from above-threshold insolvent debtors exiting proposals after the reform, we should observe a dip in filer counts above $200 SI following the reform. We examine this hypothesis and present results in Figure 11. This figure plots all proposal counts above and below the $200 threshold (panel (a)) and counts of proposals in a narrow SI range around the cutoff (panel (b)). Both panels show no evidence of a decline in the number of proposals with SI over $200. This result is again inconsistent with the alternative explanation that proposal

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

exits above the threshold after the reform are a major driver of our bunching finding. In addition, we do not find any evidence of substantial entry into proposals for the group of filings below the cutoff. This result supports our earlier institutional argument that new proposals below the cutoff are unlikely to enter the proposal system after the reform.

To summarize, in this section, we explored two extensive margin-based alternative explanations for the bunching we observed below the $200 SI threshold. First, we provided institutional details about the reform and consumer default in Canada that show there is no systematic reason for increased entry into proposal below the threshold and, therefore, this alternative explanation is unlikely to be a major reason for the post-reform bunching we observed. Second, we considered the plausibility of exits from proposal above the threshold as the main driver of the bunching. Given our findings for bunching among key subgroups of our data, and filer dynamics around the time of the reform, this alternative explanation is also unlikely to be a primary cause of the observed bunching. Overall, the evidence in this section strongly suggests that extensive margin responses to the reform are not exclusively responsible for the observed bunching and that it should, in large part, be caused by strategic SI manipulation by bunchers.

8 Suggestive Evidence of Fraudulent Manipulation

Is the bunching we observe caused by legal or fraudulent income manipulation? Distinguishing between legal and fraudulent manipulation is difficult because agents involved in fraudulent activities expend effort to ensure that their fraud is not observable.[34] In this section, we briefly summarize some suggestive evidence consistent with fraudulent behaviour by some debtors. We describe all of the tests and findings in much greater detail in Appendix C.

8.1 Bunching and Travel-Related Costs of Proposal Filing

As we describe in detail in Appendix Section C.1, under Canadian insolvency law, every consumer proposal filed by a debtor has to be submitted by an officer of the bankruptcy court called a Licensed Insolvency Trustee (LIT), who is typically a for-profit accountant. The debtor is free to select any trustee from the trustees licensed by the OSB at the time. While the debtor may select any trustee,

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

trustees cannot compete on price because the OSB regulates the prices that trustees can charge debtors. Geographic distance could also play an important role in the selection of trustees as, under the law, the debtor must conduct at least three face-to-face meetings at the office of the trustee.

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

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

---

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

8.2 Lenient Trustees and Rounding in Proposals

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

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

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

9 Conclusions and Policy Implications

In this paper, we study the strategic income manipulation caused by forcing some insolvent consumer debtors to repay more of their debt. First, we document that a significant proportion of the affected debtors misreport their income to avoid the increased repayment, evidenced by the bunching of filings we observe just below the threshold, above which debtors must repay a larger fraction of their debt. Next, we study whether the income-manipulating bunchers benefit from their actions in observable ways. Indeed, we find that these bunchers have a lower probability of default on their repayment plans than their peers, which is consistent with them having access to hidden income. We also examine how creditors are affected by this strategic income manipulation by their insolvent debtors. We show that, on average, each income-manipulated filing costs creditors 12% to 36% of their total repayment amount. Furthermore, we find some evidence that, among subgroups of filers where the reported income and other details are incongruous, creditors are more likely to reject filings submitted by bunchers. Finally, we offer evidence that the bunching we observe is unlikely to be solely caused by entry into proposals below the threshold or exit from proposals above the threshold and, therefore, it is driven in large part by strategic income manipulation.

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

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

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

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

References

Agarwal, Sumit, Vyacheslav Mikhed, Barry Scholnick, and Man Zhang, 2022, Reducing Strategic Default in a Financial Crisis, *FRB Philadelphia Working Paper* 21-36/R.

Allen, Jason, and Kiana Basiri, 2018, Impact of Bankruptcy Reform on Consumer Insolvency Choice, *Canadian Public Policy* 44, 100–111.

Almunia, Miguel, and David Lopez-Rodriguez, 2018, Under the radar: The effects of monitoring firms on tax compliance, *American Economic Journal: Economic Policy* 10, 1–38.

Bachas, Natalie, Olivia S. Kim, and Constantine Yannelis, 2021, Loan guarantees and credit supply, *Journal of Financial Economics* 139, 872–894.

Ben-David, Itzhak, 2011, Financial constraints and inflated home prices during the real estate boom, *American Economic Journal: Applied Economics* 3, 55–87.

Camacho, Adriana, and Emily Conover, 2011, Manipulation of social program eligibility, *American Economic Journal: Economic Policy* 3, 41–65.

Campbell, John Y., 2013, Mortgage market design, *Review of Finance* 17, 1–33.

Cattaneo, Matias D., Michael Jansson, and Xinwei Ma, 2020, Simple local polynomial density estimators, *Journal of the American Statistical Association* 115, 1449–1455.

Chakrabarti, Rajashri, and Nathaniel Pattison, 2019, Auto credit and the 2005 bankruptcy reform: The impact of eliminating cramdowns, *Review of Financial Studies* 32, 4734–4766.

Chetty, Raj, John N. Friedman, Tore Olsen, and Luigi Pistaferri, 2011, Adjustment costs, firm responses, and micro vs. macro labor supply elasticities: Evidence from Danish tax records, *Quarterly Journal of Economics* 126, 749–804.

Collier, Benjamin L., Cameron M. Ellis, and Benjamin J. Keys, 2021, The Cost of Consumer Collateral: Evidence from Bunching, *NBER WP* 29527.

Dee, Thomas S., Will Dobbie, Brian A. Jacob, and Jonah Rockoff, 2019, The causes and consequences of test score manipulation: Evidence from the New York regents examinations, *American Economic Journal: Applied Economics* 11, 382–423.

DeFusco, Anthony A., Stephanie Johnson, and John Mondragon, 2020, Regulating household leverage, *Review of Economic Studies* 87, 914–958.

DeFusco, Anthony A., and Andrew Paciorek, 2017, The interest rate elasticity of mortgage demand: Evidence from bunching at the conforming loan limit, *American Economic Journal: Economic Policy* 9, 210–40.

Demyanyk, Yuliya, and Otto Van Hemert, 2011, Understanding the subprime mortgage crisis, *Review of Financial Studies* 24, 1848–1880.

Di Maggio, Marco, Amir Kermani, Benjamin J. Keys, Tomasz Piskorski, Rodney Ramcharan, Amit Seru, and Vincent Yao, 2017, Interest rate pass-through: Mortgage rates, household consumption, and voluntary deleveraging, *American Economic Review* 107, 3550–88.

Elul, Ronel, Aaron Payne, and Sebatian Tilson, 2021, Owner-Occupancy Fraud and Mortgage Performance, *FRB Philadelphia Working Paper* 19-53/R.

Fack, Gabrielle, and Camille Landais, 2016, The effect of tax enforcement on tax elasticities: Evidence from charitable contributions in France, *Journal of Public Economics* 133, 23–40.

Fay, Scott, Erik Hurst, and Michelle J. White, 2002, The Household Bankruptcy Decision, *American Economic Review* 92, 706–718.

Foremny, Dirk, Jordi Jofre-Monseny, and Albert Solé-Ollé, 2017, "Ghost citizens": Using notches to identify manipulation of population-based grants, *Journal of Public Economics* 154, 49–66.

Fuster, Andreas, and Paul S. Willen, 2017, Payment size, negative equity, and mortgage default, *American Economic Journal: Economic Policy* 9, 167–91.

Garmaise, Mark J., 2015, Borrower misreporting and loan performance, *Journal of Finance* 70, 449–484.

Griffin, John M., 2021, Ten Years of Evidence: Was Fraud a Force in the Financial Crisis? *Journal of Economic Literature* 59, 1293–1321.

Griffin, John M., and Gonzalo Maturana, 2016, Who facilitated misreporting in securitized loans? *Review of Financial Studies* 29, 384–419.

Gross, Tal, Raymond Kluender, Feng Liu, Matthew J. Notowidigdo, and Jialan Wang, 2021, The Economic Consequences of Bankruptcy Reform, *American Economic Review* 111, 2309–2341.

Hertzberg, Andrew, Andres Liberman, and Daniel Paravisini, 2018, Screening on loan terms: Evidence from maturity choice in consumer credit, *Review of Financial Studies* 31, 3532–3567.

Homonoff, Tatiana, Thomas Luke Spreen, and Travis St. Clair, 2020, Balance sheet insolvency and contribution revenue in public charities, *Journal of Public Economics* 186, 104–177.

Jiang, Wei, Ashlyn Aiko Nelson, and Edward Vytlacil, 2014, Liar's loan? Effects of origination channel and information falsification on mortgage delinquency, *Review of Economics and Statistics* 96, 1–18.

Keys, Benjamin J., Tanmoy Mukherjee, Amit Seru, and Vikrant Vig, 2010, Did securitization lead to lax screening? Evidence from subprime loans, *Quarterly Journal of Economics* 125, 307–362.

Keys, Benjamin J., and Jialan Wang, 2019, Minimum payments and debt paydown in consumer credit cards, *Journal of Financial Economics* 131, 528–548.

Kleven, Henrik Jacobsen, 2016, Bunching, *Annual Review of Economics* 8, 435–464.

Kleven, Henrik Jacobsen, Martin B. Knudsen, Claus Thustrup Kreiner, Søren Pedersen, and Emmanuel Saez, 2011, Unwilling or unable to cheat? Evidence from a tax audit experiment in Denmark, *Econometrica* 79, 651–692.

Kleven, Henrik Jacobsen, and Mazhar Waseem, 2013, Using notches to uncover optimization frictions and structural elasticities: Theory and evidence from Pakistan, *Quarterly Journal of Economics* 128, 669–723.

Kruger, Samuel, and Gonzalo Maturana, 2021, Collateral misreporting in the residential mortgage-backed security market, *Management Science* 67, 2729–2750.

Li, Wenli, Michelle J. White, and Ning Zhu, 2011, Did bankruptcy reform cause mortgage defaults to rise? *American Economic Journal: Economic Policy* 3, 123–47.

McCrary, Justin, 2008, Manipulation of the running variable in the regression discontinuity design: A density test, *Journal of Econometrics* 142, 698–714.

Mian, Atif, and Amir Sufi, 2017, Fraudulent income overstatement on mortgage applications during the credit expansion of 2002 to 2005, *Review of Financial Studies* 30, 1832–1864.

Pursiainen, Vesa, 2020, Inaccurate Information in Marketplace Loans, *Available at SSRN* 3326588.

Ramsay, Iain, 2001, Mandatory bankruptcy counseling: The Canadian experience, *Fordham J. Corp. & Fin. L.* 7, 525.

Saez, Emmanuel, 2010, Do taxpayers bunch at kink points? *American Economic Journal: Economic Policy* 2, 180–212.

Stiglitz, Joseph E., and Andrew Weiss, 1981, Credit rationing in markets with imperfect information, *The American Economic Review* 71, 393–410.

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.

White, Michelle J., 2007, Bankruptcy reform and credit cards, *Journal of Economic Perspectives* 21, 175–200.

Yannelis, Constantine, 2020, Strategic default on student loans, Technical report.

Tables and Figures

Table 1: Summary Statistics

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

(a) Proposal Details

	Obs	Mean	25th %ile	Median	75th %ile	Std Dev
Filer and filing characteristics						
Male (%)	478,053	56.2	0.0	100.0	100.0	49.6
Married (%)	478,053	48.0	0.0	0.0	100.0	50.0
Age (years)	478,053	44.0	34.0	43.0	52.0	12.4
Household members	478,053	2.3	1.0	2.0	3.0	1.4
Total assets ($ K)	478,053	89.0	4.5	17.0	151.4	343.6
Unsecured debt ($ K)	478,053	36.0	19.5	31.8	48.5	22.0
Secured debt ($ K)	478,053	68.5	0.0	6.8	117.8	111.7
Non-discretionary spending ($)	478,053	113.32	0.00	0.00	80.00	288.18
Discretionary spending ($)	478,053	3075.10	2118.00	2845.00	3859.00	1363.36
Homeowner (%)	478,053	31.6	0.0	0.0	100.0	46.5
Home equity ($)	478,053	13740.41	0.00	0.00	0.00	319831.36
Available family income ($)	478,053	3089.47	2123.00	2859.96	3894.00	1392.28
Self-employed (%)	478,053	7.1	0.0	0.0	0.0	25.8
Surplus income ($)	478,053	245.35	-339.00	231.00	814.00	1041.47
Negotiation outcomes						
Planned payment amount ($ K)	470,840	15.2	8.6	12.0	18.0	10.6
Planned payment to debt ratio (%)	470,840	54.5	32.9	44.6	66.6	51.7
Planned maturity (months)	451,758	55.6	58.0	60.0	62.0	11.3
Planned monthly payment ($)	446,167	297.64	150.00	225.00	325.00	777.10
Actual payment amount ($ K)	229,319	8.1	2.9	6.3	11.3	7.7
Actual to planned payment ratio (%)	229,319	75.3	42.0	59.7	65.6	4982.4

(b) Proposal Outcomes

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

Table 2: Bunching Magnitude Estimation

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

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

Table 3: Change in Filing and Filer Characteristics for Key SI Zones

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

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

Table 4: Effect of Bunching on Loan Performance

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

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

Table 5: Estimated Per-Filing Creditor Loss Due to Bunching

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

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

Table 6: Effect of Bunching on Proposal Rejection

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

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

Table 7: Effect of Bunching on Loan Terms

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

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

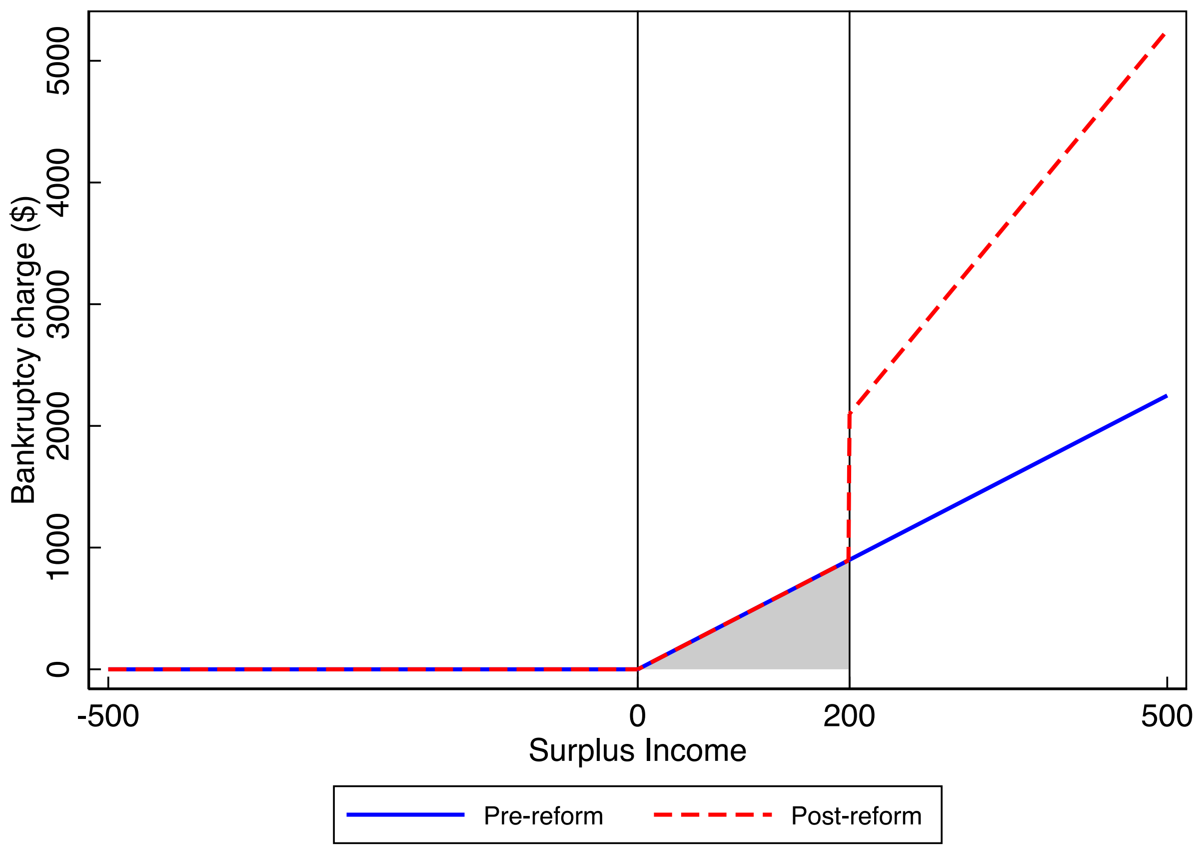
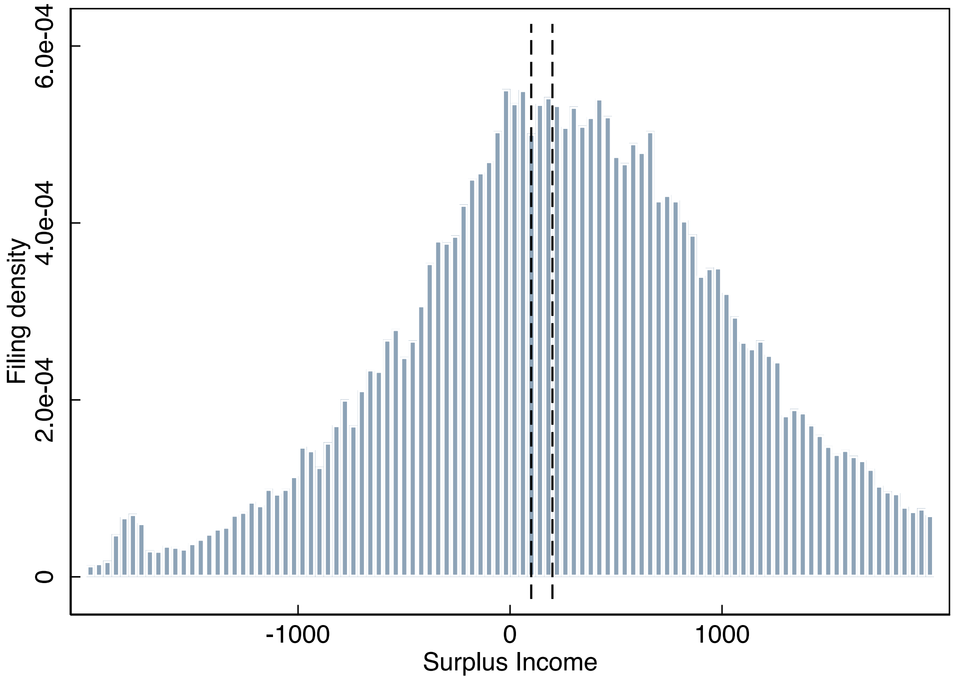
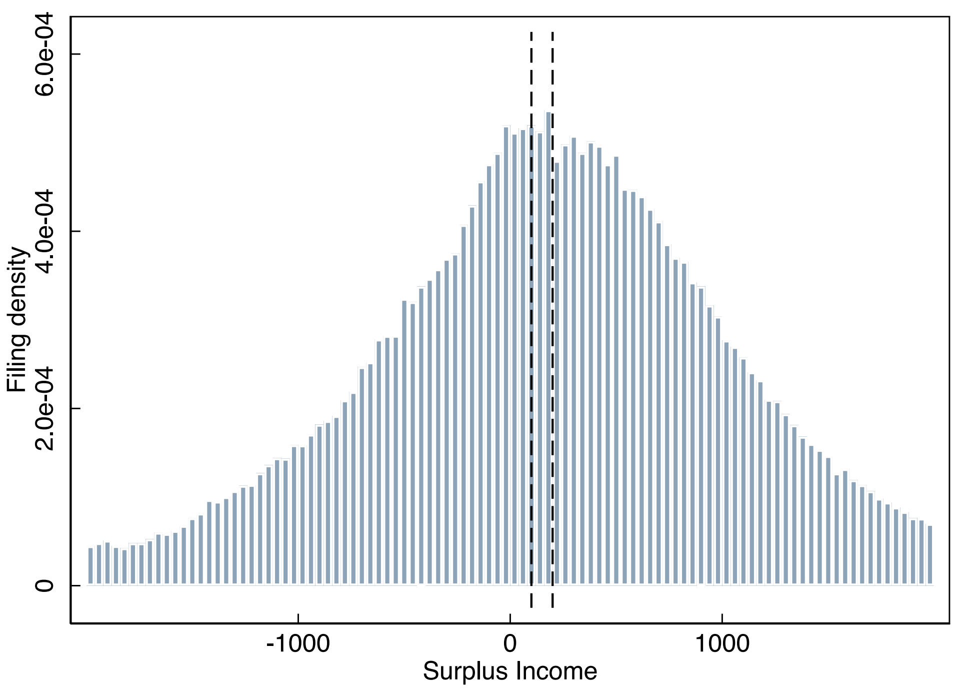


Figure 1: Illustration of the Effect of 2009 Reform on Surplus Income-Based Bankruptcy Repayment Amounts
This figure illustrates the effect of the 2009 regulatory reform on Surplus Income-based bankruptcy repayment amount. The vertical axis represents the total amount of repayment based on Surplus Income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

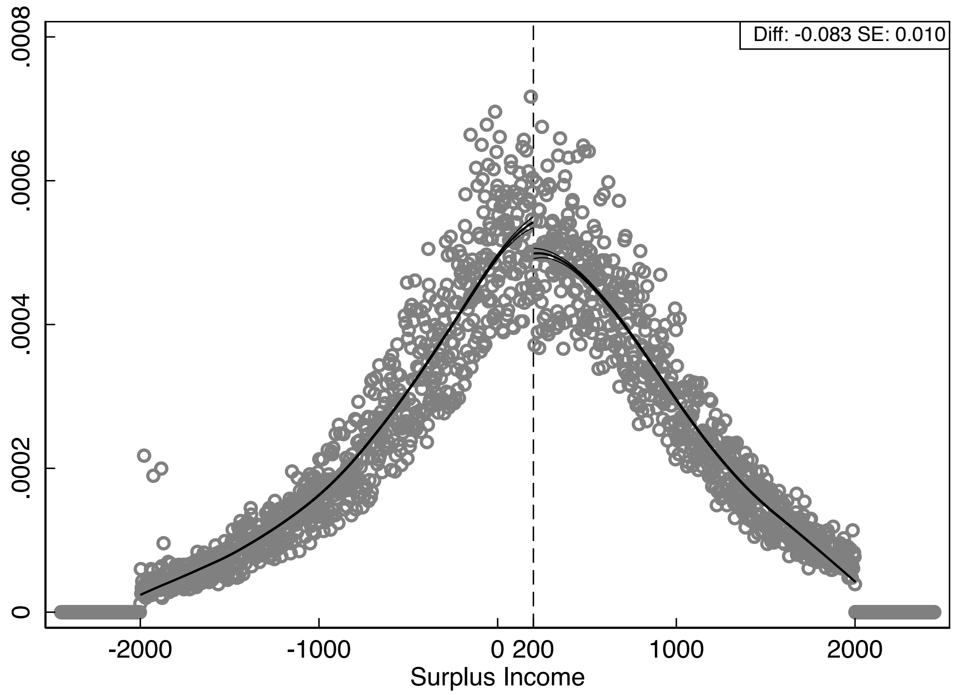


(a) Pre-reform distribution

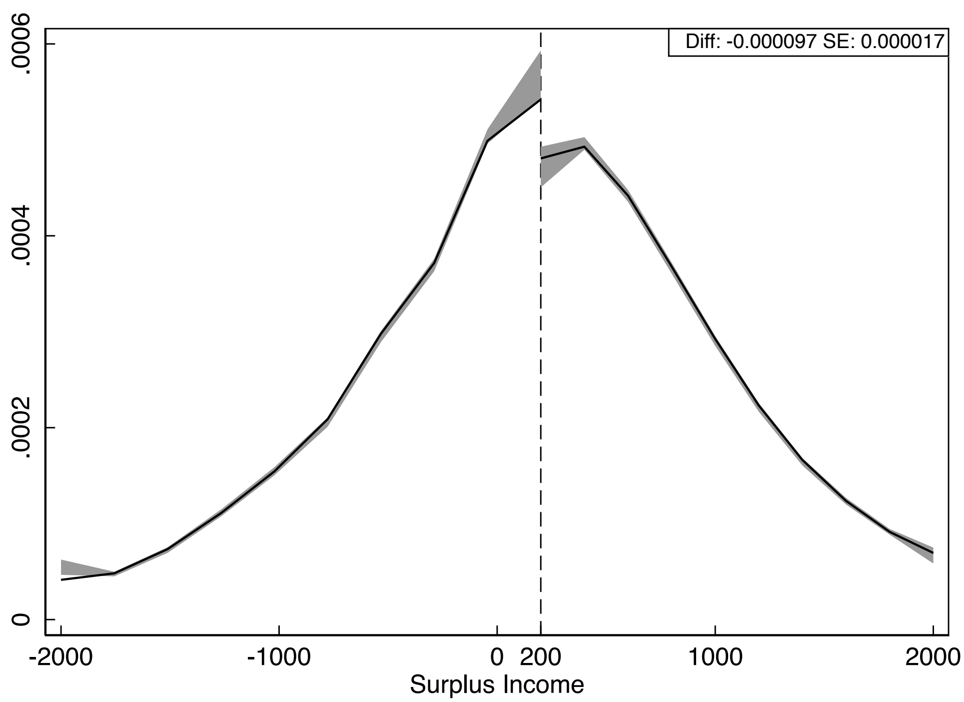


(b) Post-reform distribution

Figure 2: Distribution of Surplus Income
This figure plots the observed distribution of Surplus Income (SI) before and after the 2009 regulatory reform in panels (a) and (b), respectively. Both panels include all proposal filings with SI between -\$2,000 and \$2,000. The vertical dashed lines indicate SIs of \$0 and \$200.



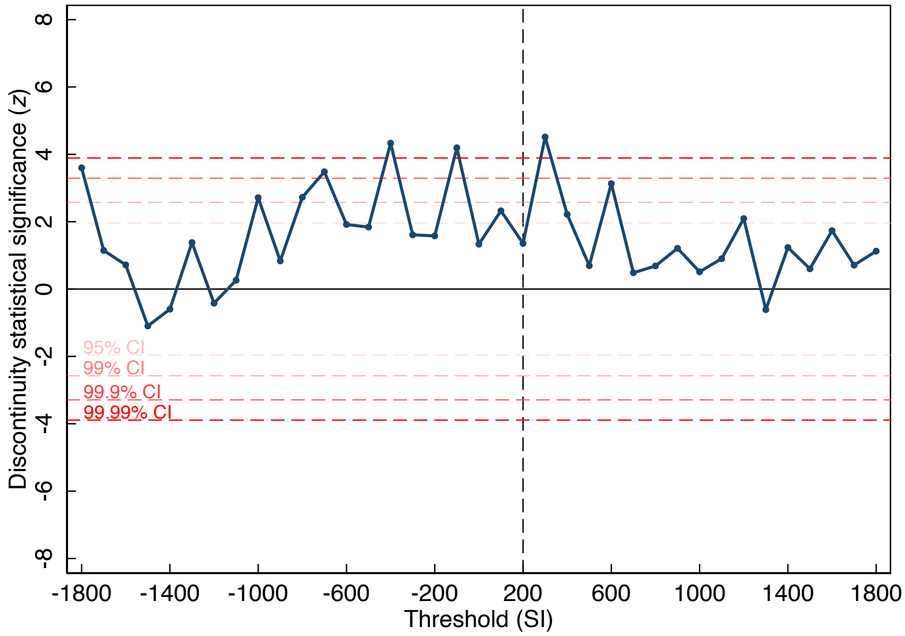
(a) McCrary (2008) Test



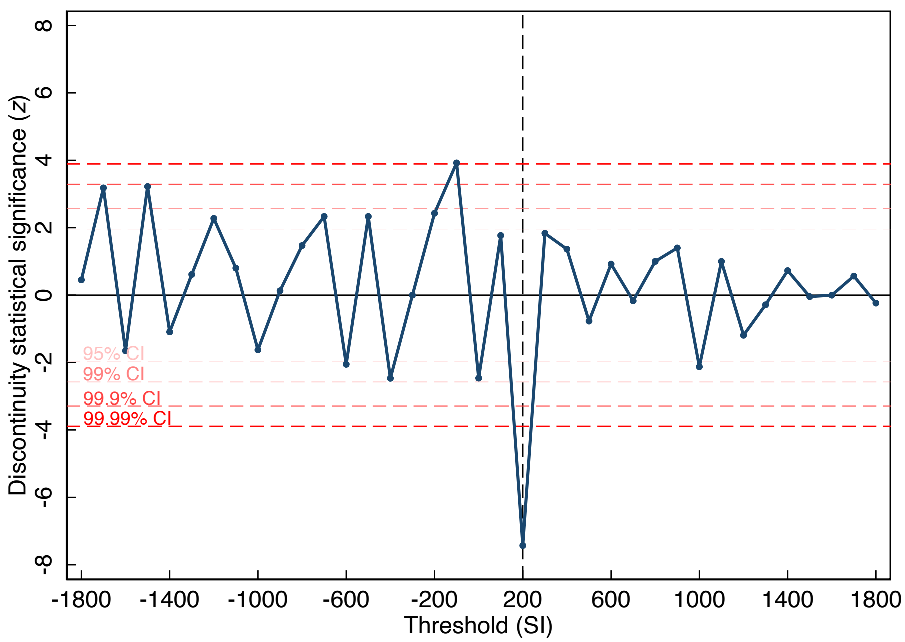
(b) Cattaneo et al. (2020) Test

Figure 3: Discontinuity Tests for Post-Reform Proposal Filings

This figure displays the results of discontinuity tests performed at $200 Surplus Income (SI) cutoff for proposal filings submitted after the 2009 policy change. Panel (a) displays the results of the discontinuity test from McCrary (2008). Panel (b) displays the results of the discontinuity test from Cattaneo et al. (2020). Both panels include all proposal filings with an SI between -$2,000 and $2,000. 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.



(a) Pre-reform



(b) Post-reform

Figure 4: Placebo Tests for Discontinuity at Pseudo thresholds
This figure shows the statistical significance levels (z-statistic) for McCrary (2008) discontinuity tests at hundred-dollar Surplus Income (SI) thresholds from -\$1,800 to \$1,800. To maintain consistency across all tests, proposal filings with an SI within \$600 of each pseudo threshold are included in each test (these results are similar for other ranges). In panel (a), the discontinuity tests are performed on pre-reform filings. In panel (b), the discontinuity tests are performed on post-reform filings. The red dashed horizontal lines indicate statistical significance levels: 95%, 99%, 99.9%, and 99.99%. Figure A2 reports these estimates using Cattaneo et al. (2020) discontinuity tests.

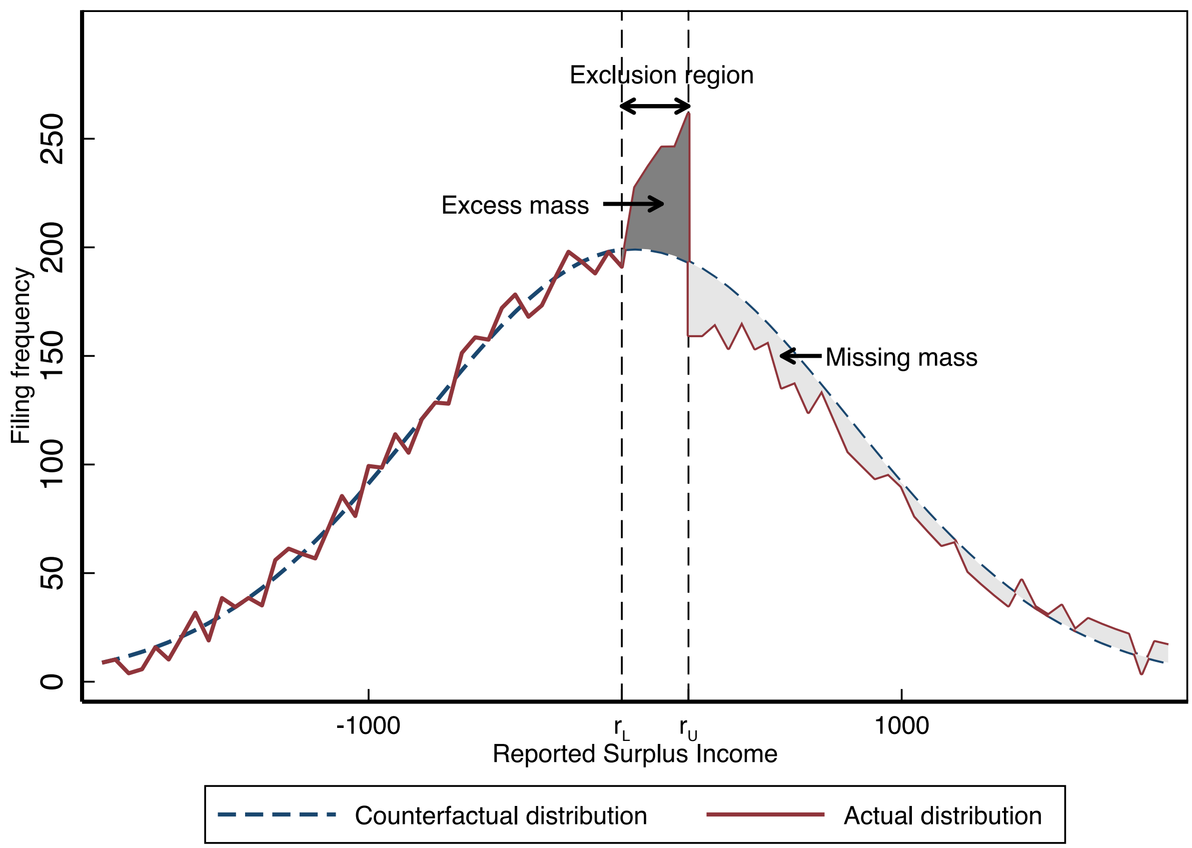


Figure 5: Illustration of Bunching Magnitude Estimation
This figure illustrates how bunching magnitude is estimated. The horizontal axis represents reported Surplus Income (SI) and the vertical axis represents frequency of filings. The red line depicts an illustrative distribution of SI. The exclusion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

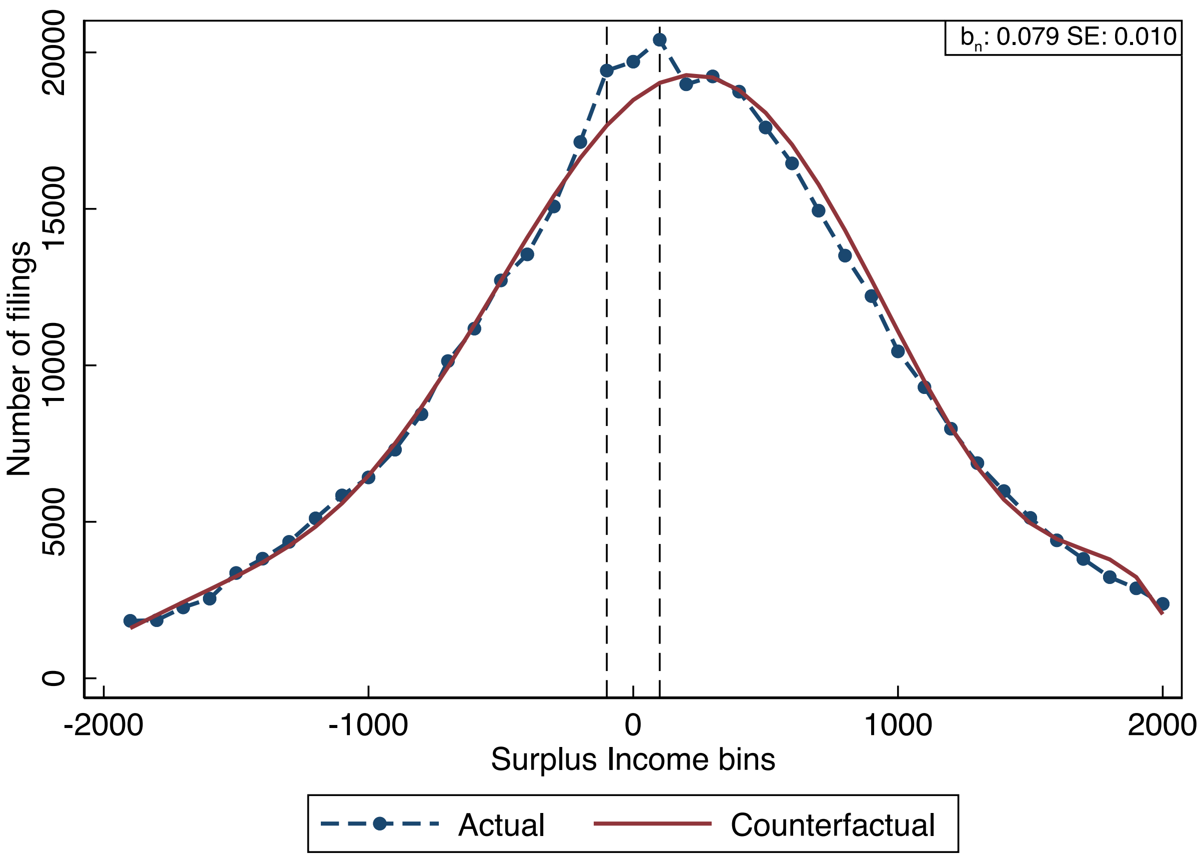


Figure 6: Estimation of Bunching Magnitude
This figure shows the result of estimating bunching magnitude using Surplus Income (SI) bins of size \$100 and a 7th degree polynomial to model the counterfactual distribution. The estimation is performed on all post-reform proposal filings with an SI between -\$2,000 and \$2,000. The horizontal axis represents SI bins (of size \$100 each). The vertical axis represents the number of filings in the post-reform period in each bin. The dashed line is the actual number of filings per bin. The red smoothed curve is the estimated counterfactual distribution of filings per bin. The black vertical dashed lines indicate the exclusion region, with SI between -\$100 and \$200. The estimated bunching magnitude, b_n, and its standard error are reported in the upper right box.

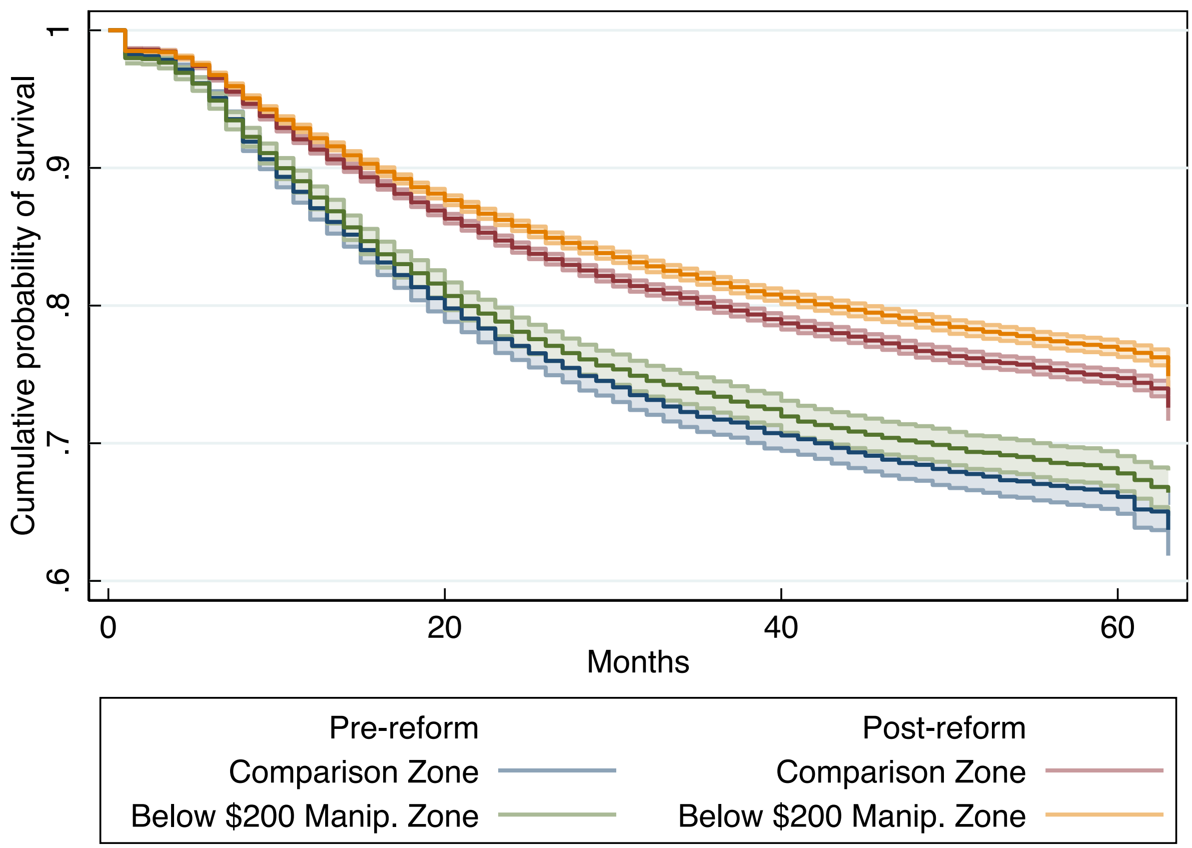
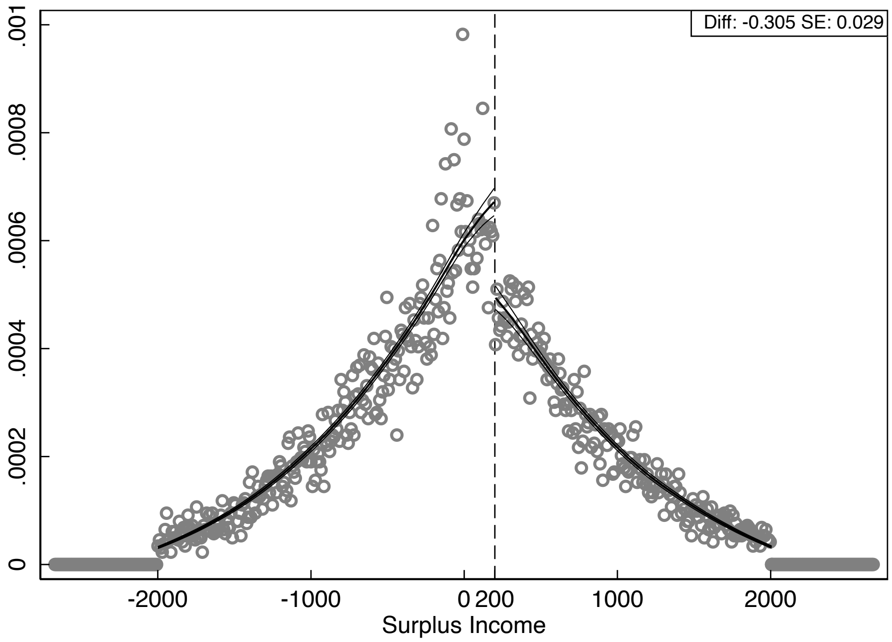
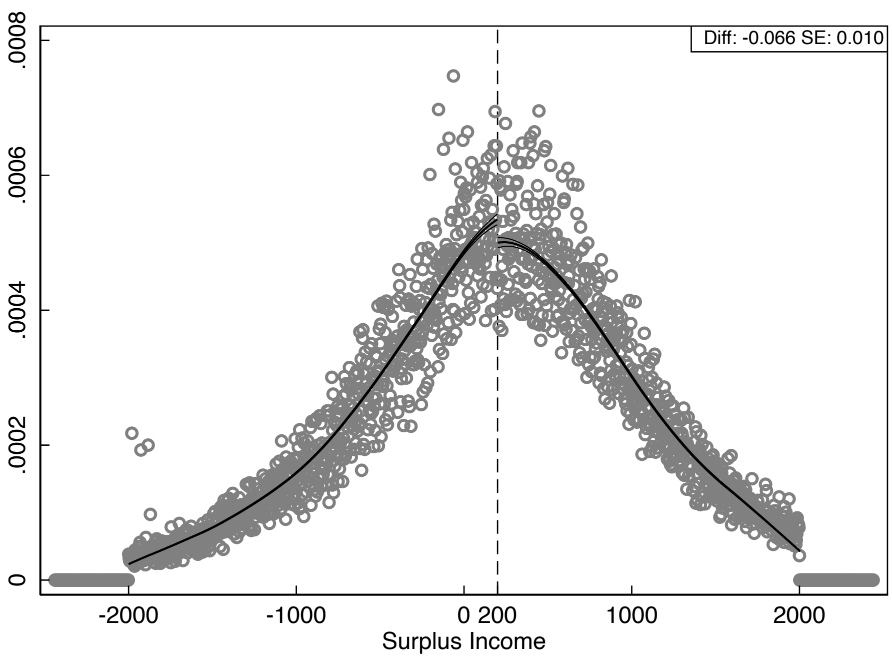


Figure 7: Kaplan-Meier Survival Plot

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

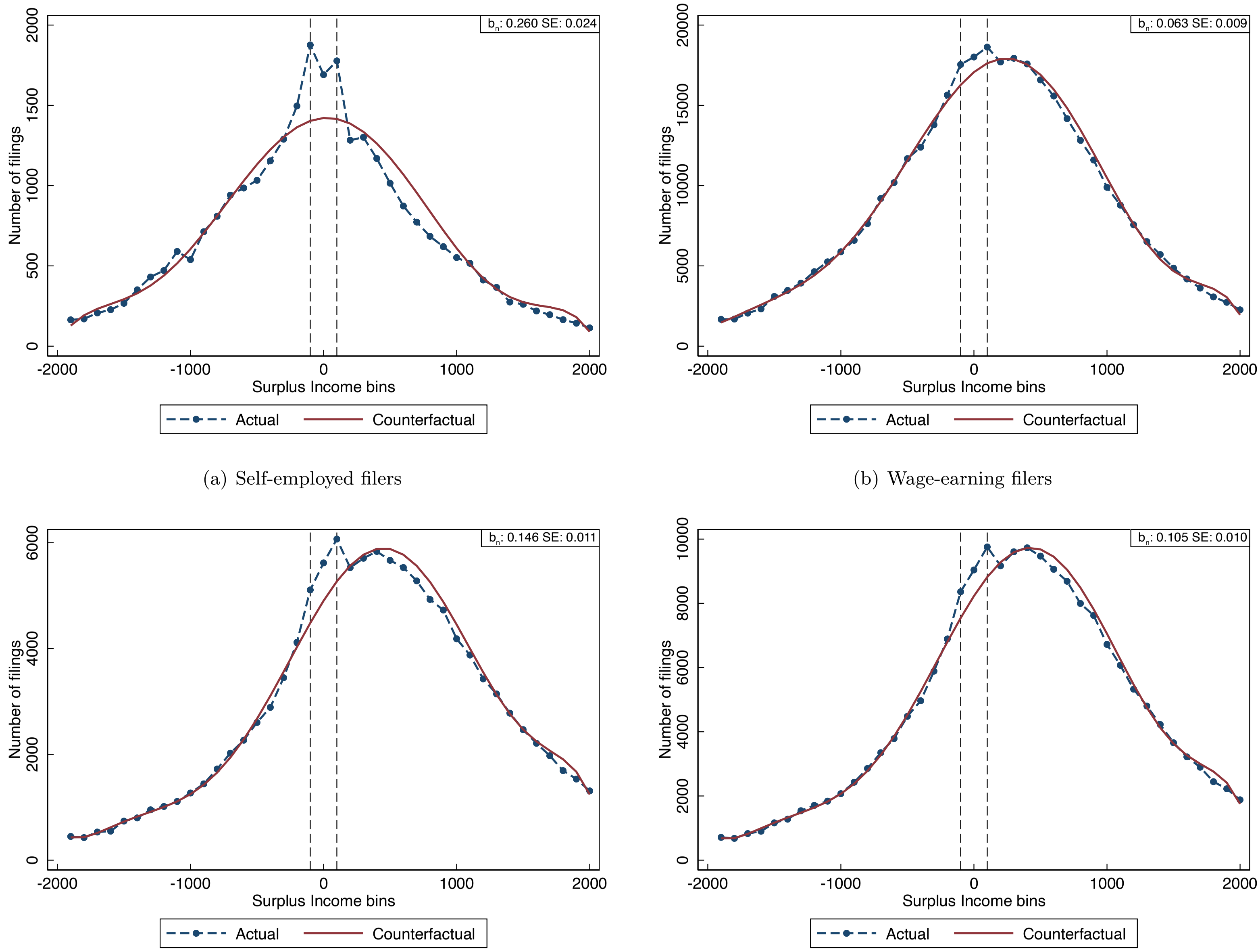


(a) Self-employed filers



(b) Wage-earning filers

Figure 8: McCrary (2008) Discontinuity Tests Among Self-Employed and Wage-Earning Filers
This figure displays the results of the McCrary (2008) discontinuity tests performed at \$200 Surplus Income (SI) cutoff for proposal filings submitted after the 2009 policy change for two kinds of proposal filers. Panel (a) and (b) display results for tests performed on post-reform proposal filings with an SI between -\$2,000 and \$2,000 submitted by self-employed and wage-earning filers, respectively. In each panel, the magnitude of the discontinuity and its standard error are reported in the upper-right corner.



(a) Self-employed filers

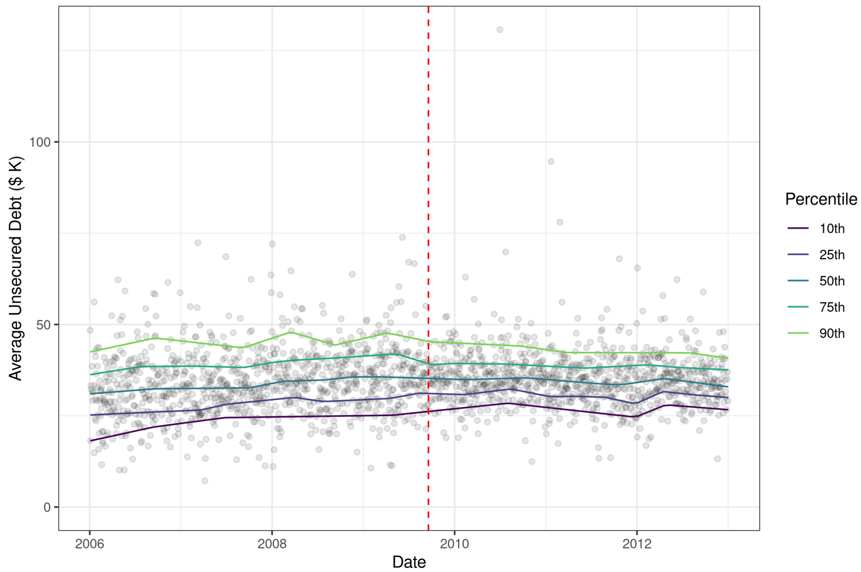
(b) Wage-earning filers

(c) Homeowners

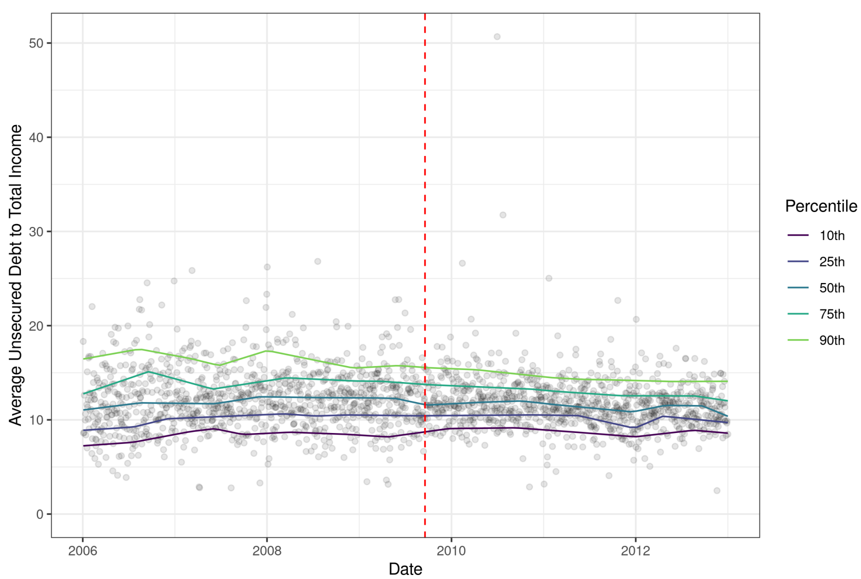
(d) High-asset Filings

Figure 9: Estimation of Bunching Magnitude Among Subgroups of Filings

These figures show the results of estimating bunching magnitude using Surplus Income (SI) bins of size \$100 and a 7th degree polynomial to model the counterfactual distribution for four subgroups of proposal filers. Panel (a) plots the bunching magnitude for self-employed filers, panel (b) plots it for wage-earning filers, panel (c) plots it for homeowner filings, and panel (d) plots it for high-asset filings. Each panel shows results of estimations performed on the relevant subgroup of post-reform proposal filings with an SI between -\$2,000 and \$2,000. In all four figures, the horizontal axis represents SI bins (of size \$100 each), the vertical axis represents the number of filings in the post-reform period in each bin, the dashed line is the actual number of filings per bin, the red smoothed curve is the estimated counterfactual distribution of filings per bin, and the black vertical dashed lines indicate the exclusion region, $SI \in (-100, 200)$. The estimated bunching magnitudes, b_n, and their standard errors are reported in the upper-right box in each subfigure.

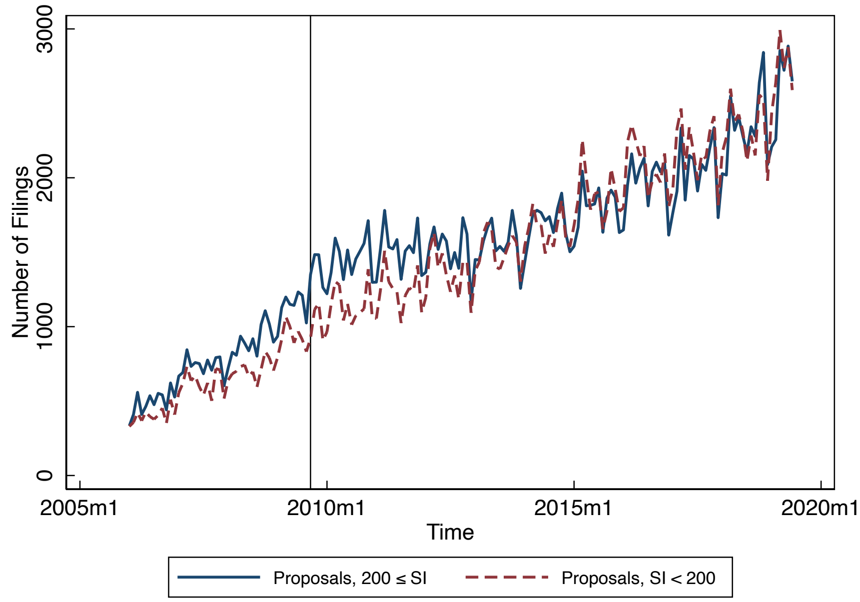


(a) Unsecured Debt

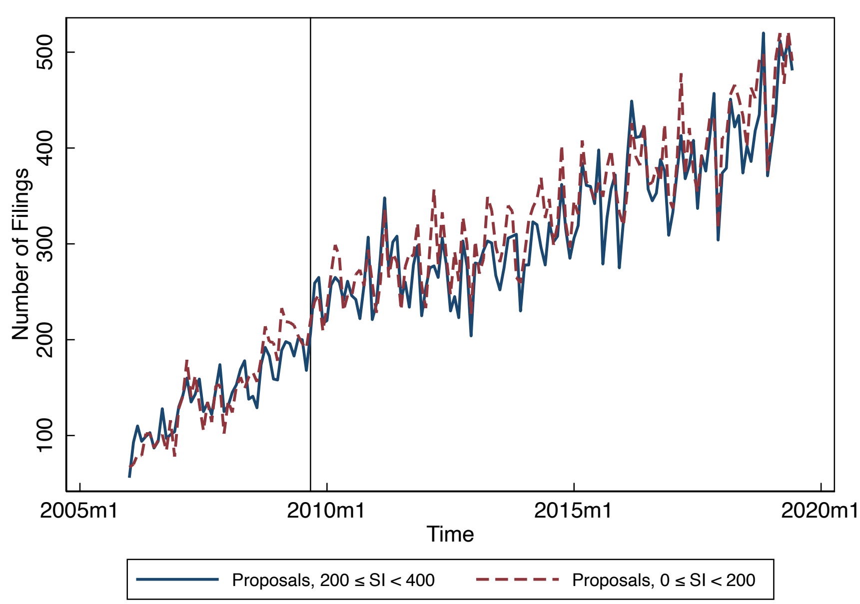


(b) Unsecured Debt to Total Income

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



(a) All Proposal Filings



(b) Proposal Filings Near $200 SI Threshold

Figure 11: 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). Panel (a) plots these proposal counts for all proposal filings, whereas panel (b) plots them for proposal filings with SIs between \$0 and \$400 (i.e., within \$200 of the \$200 SI threshold). In both panels, the solid blue line represents the monthly number of proposal filings with SI above \$200 (inclusive) and the dashed red line represents monthly filings with an SI under \$200. The vertical line in both panels represents September 2009, the month of the policy reform.

Appendices

Appendix A Construction of Surplus Income

We construct Surplus Income (SI) based on OSB rules using data reported on the balance sheet and income statement of the debtor. We use the following formula to determine SI:

$$SI_{i,t} = Pro\ rate_i \times (NetFamilyIncome_i - AuthExpenses_i - Deduction_t). \tag{6}$$

$Pro\ rate_i$ is an adjustment based on whether the filing is made as a single individual or as part of a family. $NetFamilyIncome_i$ is the total monthly income after tax from all family members. $AuthExpenses_i$ includes all non-discretionary spending as defined by the OSB (which includes child support expense, spousal support expense, child care, medical expense, fines, penalties, employment expense, etc.) $Deduction_t$ is an amount published every year by the OSB to adjust for inflation, which varies over family size.

Appendix B Using the Integration Constraint to Estimate Exits Above the Threshold

As explained in Section 7.2, an alternative explanation for the bunching we document in this paper may arise from increased exits above the SI threshold. In the paper, we present several analyses that strongly suggest that this alternative explanation cannot be exclusively responsible for the observed bunching. Below, we exploit a feature of the Chetty et al. (2011) bunching methodology to estimate the degree of these extensive margin exits.

As we detail in Section 4, Chetty et al. (2011) estimates bunching magnitude by modeling observation counts in bins using a high-degree polynomial regression to produce the counterfactual distribution. Satisfying the integration constraint is a crucial step in the Chetty et al. method. This integration constraint requires that the missing mass above \$200 SI equal the bunching mass in the region below \$200. Through an iterative procedure, the estimation method incrementally increases the area under the counterfactual line above \$200 SI, which simultaneously reduces the bunching mass estimate and increases the missing mass estimate until the two are equal. As a result, the integration constraint effectively imposes the assumption that there are no exits above \$200 SI due to the reform.[36]

[36] More precisely, the integration constraint imposes the assumption that there is no extensive margin response to the reform at all. That is, it assumes the reform does not cause any exits or entries above or below the threshold. In our

To estimate exits above the \$200 SI threshold, we calculate bunching magnitude without enforcing the integration constraint. By ignoring the integration constraint, we allow the bunching mass to be less than the missing mass, effectively allowing extensive margin exits.[37] The difference in the bunching mass with and without an integration constraint captures the filers missing above the threshold who do not reappear in the bunching region below the threshold (i.e., do not manipulate their SI to be below \$200 SI). As such, it is an estimate of the extent of exits, as measured within the Chetty et al. (2011) framework.[38]

We run this estimation exercise on each of the eight bunching models in Table 2 and present our results in Figure A5. As the figure shows, across all the models, proposal exits constitute approximately one-sixth of the number of missing filings in the region above the \$200 SI threshold. Five times as many (five-sixths) of the missing mass filings are present in the bunching region below the \$200 SI. This indicates that the vast majority of filers with true SIs over \$200 who choose not to file a proposal with an SI above the threshold after the reform choose instead to manipulate their SI and file a proposal with an SI below \$200.

Appendix C Suggestive Evidence of Fraudulent Manipulation

An important follow-up question to identifying bunching caused by SI manipulation is whether the manipulation is fraudulent or legal. For example, when the tax literature uses bunching to identify manipulation, a common follow-up question is whether that observed manipulation is either legal (i.e., tax avoidance) or fraudulent (i.e., tax evasion). Distinguishing between legal and fraudulent manipulation is difficult because, generally, agents involved in fraudulent activities expend effort to ensure that their fraud is not observable. The bunching literature thus often exploits idiosyncratic details of the setting in order to empirically isolate contexts where fraudulent behavior can be

setting, given the counterfactual distribution, the effective assumption imposed by the integration constraint is that there are no exits above the threshold due to the reform.

[37] We are, implicitly, assuming that the unmodified initial counterfactual distribution we estimate accurately captures the total effect of the reform on filers' decisions above the threshold: SI manipulation and proposal exit.

[38] Our approach is somewhat similar in spirit to the extensive margin estimation done in DeFusco et al. (2020), which calculates the extensive margin as the difference between the bunching mass and the missing mass for a treated group of jumbo mortgages after a reform, adjusting the pre-reform distribution of this group for time-varying factors using a group of conforming mortgages not affected by the reform. In both our paper and DeFusco et al. (2020), the extensive margin is ultimately estimated as the difference between the missing mass above the cutoff and the bunching mass below the cutoff, but we are not able to follow DeFusco et al. (2020) approach exactly because we do not have a part of the sample above and below the threshold, which is not treated by the reform.

assumed to be more or less likely.[39] Importantly, however, this kind of context-based methodological approach to identifying fraud can, at best, provide only suggestive or circumstantial evidence of fraudulent behavior. It is not able to provide definitive evidence of fraud. Similar to these prior studies, we develop and use a context-based approach to provide suggestive evidence that some debtor data manipulation may involve fraudulent behavior.

In this section, we first describe various institutional details in the Canadian proposal system, which we exploit in our tests to provide suggestive evidence of possible fraudulent behavior. In particular, we focus on the role of third-party intermediaries known as Licensed Insolvency Trustees (LITs), studying how these LITs may be involved in fraudulent behavior by debtors.

C.1 The Role of Trustees in Insolvency

Under OSB regulations, Licensed Insolvency Trustees (LITs) must submit and administer every insolvency filing in Canada. 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. 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 proposal process, which we describe chronologically below.

First, a debtor who is planning on filing a proposal is free to select any LIT to undertake the proposal filing. While the debtor can select any licensed trustee to undertake the filing, the debtor is required to pay the trustee for these services. Under OSB rules, the price that a trustee charges a debtor to file a proposal is highly regulated (the trustee receives 20% of the total payments received by creditors under the proposal). Because of this feature, trustees are not able to directly compete on price, but rather they are forced to use other mechanisms to increase their profits, such as market share.

Geography could also play an important role in debtors' choice of trustees because Canadian personal insolvency law requires debtors to conduct at least three separate face-to-face meetings

[39] See Kleven (2016) for a survey. Examples of bunching-based studies that identify fraudulent behavior by exploiting context specific institutional details include Almunia and Lopez-Rodriguez (2018), who examine firms bunching below a cutoff in tax enforcement, Camacho and Conover (2011), who document "corruption" based on bunching below a threshold for social programs, and Foremny et al. (2017), who document the "deliberate manipulation" of population based grants by local governments. Kleven et al. (2011) examine bunching in the context of random tax audits, and Fack and Landais (2016) examine bunching in the context of third-party reporting of tax data.

with the selected trustee at the trustee's office (see Ramsay, 2001).[40] These three meetings impose non-trivial, travel-related transactions costs for proposal 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 by the OSB, 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.

C.2 Bunching and Travel-Related Costs of Proposal Filing

In this section, we examine whether the distance that debtors travel to their selected trustee is correlated with the bunching we observe. For debtors who have no intention of manipulating their filing, all else equal, a closer trustee is preferable to a more distant one, because trustees file identical OSB forms and charge identical fees, but more distant trustees would require additional travel costs. Therefore, debtors with no intention of fraudulent SI manipulation 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 debtors who intend to fraudulently manipulate data. Potentially fraudulent debtors would like to minimize geographic transactions costs but would also like to locate a trustee who allows them to submit a fraudulently manipulated filing. A debtor with intent to fraudulently manipulate would thus face a trade-off between the increased costs of the fraud (in our case, increased travel costs to a more distant trustee who could aid in facilitating the fraud) and the increased benefits of the manipulation (in our case, the savings from reducing

[40] Before the actual filing, 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.

reported surplus income by fraud).

Importantly, this trade-off between the costs and benefits of manipulation may be different for fraudulent income manipulation by the debtor (e.g., misreporting income) versus legal but strategic income manipulation by the debtor (e.g., reducing hours worked or filing during periods with predictably low income). The costs to a trustee of agreeing to a fraudulent filing by the debtor could include having the trustee's license revoked by the OSB or other legal action by the OSB. In other words, the possible cost to the trustee, if the fraud is detected, is high. For this reason, a debtor with fraudulent intent may have to travel farther distances in order to locate a trustee who would be more likely to approve a fraudulent filing.

We argue that this trade-off is different for a debtor intent on strategic (but legal) income manipulation. This is because the cost to the trustee of approving a filing with legal income manipulation is relatively low (i.e., the trustee approving a filing with legal income manipulation cannot be legally sanctioned by the OSB). For this reason, a filer may not have to travel long distances to find and use a trustee willing to approve a legally manipulated filing. An empirically testable implication of our argument, therefore, is that debtors with fraudulent intent are more willing to incur larger travel-related transactions costs in order to locate trustees who are more likely to approve a fraudulent filing.

To test our hypothesis, we examine the relationship between travel costs of filers and bunching below the $200 SI threshold. Because we observe the exact geographic location of every insolvency filer and every insolvency trustee, we can calculate the "excess distance" that filers travel to their selected trustee as the additional kilometers traveled by the filers to their selected trustee compared with the average distance to their closest trustees. Using this measure of excess distance traveled to the selected trustee, we examine whether those debtors who travel excess distances to access more distant trustees (despite nearby trustees available to them) have a larger bunching magnitude below the cutoff after the reform compared with filers who do not travel these excess distances.

C.2.1 Measuring Distance 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 travel distance between a filer and a trustee as the road distance between the centroids of their postal codes. As Canadian postal codes are extremely small geographic units, containing approximately 13 households, on average, the distances we calculate are generally quite precise. Our measure of travel distance uses the Google Maps API, which gives us the travel distance by car for Google Maps' suggested road route between two points. These travel distances based on Google Maps-suggested routes are longer than direct, as-the-crow-flies distances between two points.[41]

We measure travel-related transactions costs as the difference between filers' distance to their selected trustee and filers' average distance to their three closest trustees. To remove outliers, in building this measure, we exclude any filings where the distance from filers to their 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 construct a relative distance measure to estimate travel-related transactions costs. We calculate this measure as the actual travel distance to the chosen trustee divided by the average distance to the three closest trustees, indicating how many times farther than necessary the filer chooses to travel to work with her selected trustee.

The top panel of Table A2 reports summary statistics for filer-trustee distances and the travel-related transactions costs measures (based on road routes). The median distance between a filer and the three closest trustees is 4.9 km, whereas the median distance between a filer and her selected trustee is 17.5 km. The median travel-related transactions cost in multiples of average distance to the three closest trustees is 2.3.

C.2.2 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 are more likely to bunch below the $200 SI threshold. In particular, we estimate bunching magnitudes in various subsamples based on the travel distance to the selected trustee in excess of the average distance to the three closest trustees. In Figures A6(a) and A6(b), we present our bunching magnitude estimates for filings after the policy change across two samples: filings where debtors travel less than 130% of the minimum distance (the Nearby Trustees sample) and filings where they travel more than 130% of the minimum distance (the Distant Trustees sample). As the figures depict, there is clearly more bunching below the $200 SI cutoff in the Distant Trustees sample (Figure A6(b)).[42]

This visual analysis is corroborated by comparing the estimated bunching magnitude for the

[41] As a robustness check, we also examine direct, as-the-crow-flies distances, but our results are quantitatively similar.

[42] We also assess bunching magnitude based on travel distance for other cutoffs and find similar results (see Figure A8).

Nearby Trustees and the Distant Trustees samples. This magnitude is equal to 6.5% for the Nearby Trustees sample and 8.8% for the Distant Trustees sample (see Figures A6(a) and (b)). These results imply that there is approximately 35% more bunching in the Distant Trustees sample relative to the Nearby Trustees sample.

To more rigorously compare the two bunching magnitude distributions, we use bootstrapping methods. First, for each group, we run the Chetty et al. (2011) bunching estimation procedure on 1,000 subsamples drawn (with replacement) from the group. This provides us with 1,000 estimates of the bunching magnitude for the group. Then, we compare the Nearby and Distant Trustee filing groups' bunching magnitudes in two ways: plotting the distributions of their estimated bunching magnitudes and performing a two-sample t-test to compare the means of the distributions.

Before detailing the findings of bootstrapping analysis, 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 present the bunching magnitude estimate distributions for the Nearby and Distant Trustee groups in Figure A7(a). Clearly, the filings using more distant trustees exhibit higher levels of bunching. Moreover, our t-test shows that the mean of the Distant Trustee group is 2.3 percentage points higher than the mean of the Nearby Trustee group and this difference is highly statistically significant (t-statistic of 29.8).

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 distance traveled in excess of the average distance to the three closest trustees.[43] Figure A8 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 4.3%, to the top quartile, where it is 9.7%. Along with the previous findings,

[43] 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.

this very substantial increase in the bunching magnitude between the first and fourth quartiles, as well as the monotonic increase across the four quartiles, corroborates our hypothesis that debtors who travel excess distances to their chosen trustee are more likely to bunch below the $200 SI threshold.[44]

C.3 Measuring Trustee Leniency Using Rounding in Proposals

In Section C.2, we examine the relationship between excess distance traveled to the selected trustee and bunching as a suggestive indicator of fraudulent data manipulation by the debtor. In this section, we examine the relationship between trustee "leniency" and bunching magnitude. There may exist more lenient trustees, who are more likely to submit fraudulently manipulated proposals on behalf of a debtor. We measure trustee leniency as the prevalence of round numbers in proposals (e.g., reporting numbers in the multiples of $100) for all filings submitted by that trustee in the past. We can calculate this trustee-level measure using an anonymized identifier of the trustee, which we observe for all proposal filings submitted to the OSB.

Our motivation for using round numbers as an indicator of trustee leniency is that more round numbers in a filing is consistent with less precision and diligence or willful ignorance on the part of the trustee. The American Institute of Certified Public Accountants (AICPA), for example, includes round numbers as a possible indicator of fraud.[45]

While rounding has the advantage of being observable to us, it is a relatively weak measure of fraudulent data manipulation. First, approval of filings with round numbers by financially sophisticated trustees may result from either low effort (i.e., shirking) or data manipulation. Second, sophisticated filers may have other (better) ways to fraudulently manipulate data than data rounding, that may be easier to get approved by a trustee. Third, it is possible that a trustee could round numbers to the benefit of creditors (i.e., increase the SI to above the cutoff), rather than to the benefit of debtors (i.e., reduce SI to below the cutoff). Analyzing filings with round numbers is thus, at best, an imperfect measure of trustee leniency in our setting, but it is the best measure available to us. It is worth noting that these measurement issues work against us finding evidence of fraud using round numbers. Therefore, any evidence of fraud we find with trustee leniency based

[44] We also compare the bunching magnitude in the sample of filers choosing the closest trustee versus filers working with not the closest trustee and find more bunching in the second sample.

[45] In its statement on auditing standards regarding *Consideration of Fraud in a Financial Statement Audit* (AU-C Section 240), the AICPA states that fraudulent financial statements "include entries ... containing round numbers. See `https://us.aicpa.org/content/dam/aicpa/research/standards/auditattest/downloadabledocuments/au-c-00240.pdf`.

on round numbers would arise despite these measurement issues.

C.3.1 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 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.[46] The precise formula we employ for calculating our rounding measure is:

$$\%RN_i = \frac{\sum_{k \in \text{A\&L vars}} \mathbb{1}\left\{val_{i,k} \bmod 100 = 0\right\}}{\sum_k \mathbb{1}\left\{val_{i,k} > 100\right\}}, \tag{7}$$

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. 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 of historical trustee rounding 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.[47]

We present summary statistics on our rounding measure in Table A2. 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 it does not significantly alter the mean.

[46] We provide a full list of the data variables we use in Table A1.

[47] The 40 filing cutoff is at the 5th percentile of the trustee filing count distribution.

C.3.2 Bunching Debtors Selecting Historically Lenient Trustees

In this section, we assess whether debtors who use historically lenient trustees exhibit a greater tendency to bunch below the $200 SI threshold. One possible interpretation of bunching debtors selecting historically lenient trustees is that the debtor has an intention to fraudulently manipulate income data.

In Figure A9, we present the results of our bunching magnitude estimations for filings approved by more and less historically lenient trustees after the policy change. Figure A9(a) focuses on filings approved by trustees whose aggregated historic $\%RN$ is below the 90th percentile, and Figure A9(b) focuses on filings approved by trustees with aggregated $\%RN$ above the 90th percentile.[48] The two figures show that the bunching of filings below the $200 SI threshold among more lenient trustees is much larger than this bunching among less lenient trustees. Our estimate of bunching magnitude, $\hat{b}_n$, is nearly twice as large for filings approved by more lenient trustees than less lenient trustees (11.0% versus 6.9%).

We statistically compare the bunching estimates for the two subsamples using the bootstrapping method explained in Appendix Section C.2. Figure A7(b) shows the bunching magnitude distributions of filings with historically lenient and strict trustees. Visual inspection confirms that these distributions are quite distinct. Furthermore, our t-tests comparing the bootstrapped distributions confirm a 4.1 percentage point greater bunching magnitude for filings with lenient trustees and that this difference is highly statistically significant (t-statistic of 41.7).

When we split post-reform filings into equal octiles based on historical trustee leniency and estimate the bunching magnitude for each octile, we find similar results. Figure A10 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 SI threshold. This result further confirms that debtors who fraudulently manipulate data are more likely to work with historically lenient trustees, likely to improve the chances of having their filings approved.

[48] Our results are robust to using 50th and 75th percentile as cutoffs. We also assess bunching magnitude based on trustee leniency octiles and find consistent results (see Figure A10).

C.4 Trustee Leniency and Market Share

We next consider why some trustees may choose to be more lenient. As described in Section C.1, 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 historical (pre-reform) trustee leniency has any effect on their future market share.

Before presenting the results of our examination, we confirm that historical 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 A11, 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. In Figure A12, we plot market share over time, for more and less lenient trustees, with a vertical line marking the September 2009 policy change. Figure A12(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. The figure indicates that more lenient trustees have relatively lower market share prior to the policy change, but they seem to gain market share afterward.

Given these patterns in the raw data, we then perform a more formal test of the hypothesis that the policy change caused an increase in the market share of more lenient trustees. Figure A12(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 shows an increase in the market share of more lenient trustees immediately following the policy change and no subsequent reversal later in the period. These findings 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.

C.5 Correlation Between Trustee Leniency and Excess Travel Distance

In Sections C.2 and C.3, we provide evidence that two methods of detecting fraudulent filers among bunchers (distance to the trustee and historical rounding by the trustee) are both correlated with bunching below the $200 SI threshold. In this section, we examine whether the two indicators are correlated, ignoring bunching. Correlation between these two measures would suggest that we are identifying significantly overlapping filers, which lends weight to our argument that our measures are identifying filers who are fraudulently manipulating their data. Figures A13(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 A13(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 A13(b) shows similarly greater prevalence of round numbers in proposal figures for filers who travel greater excess distances to their chosen trustees. These results are consistent with the argument that filers who fraudulently manipulate data choose to travel excess distances to work with trustees who are more lenient and, therefore, more likely to approve and submit fraudulently manipulated proposal filings.

Appendix Tables and Figures

Table A1: Description of variables used in this study

Variable name	Definition
total asset	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.
unsecured debt	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.
secured debt	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.
non-discretionary spending	total value of child support expense, spousal support expense, child care, medical expenses, fines and penalties, employment expense, debts and some other expense.
discretionary spending	total value of house utility expense, personal expense, medical expense, insurance expense and some other expense.
home equity	value of real property (house) minus principal mortgage amount.
available family income	total household income of net employment income, pension, child-support income, spousal support income, insurance benefit, social assistance, self-employment income and others.

Table A1 – continued from previous page

Variable name	Definition
reasons for financial difficulty	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.
planned payment amount	total planned required repayment amount including monthly installment and lump sum pay.
planned payout ratio	total planned repayment amount over unsecured debt.
maturity	number of months between the planned completion date and the consumer proposal filing date.
monthly payment	monthly required repayment amount if the payment schedule is monthly installment.
actual payment amount	total actual required repayment amount including monthly installment and lump sum pay.
actual to planned payout ratio	total actual repayment amount over total planned repayment amount.
% of rounding numbers (assets, debt)	percentage of variables whose value is a multiple of 100 from the categories of asset, secured debt, and unsecured debt.
% of rounding numbers (all)	percentage of variables whose value is a multiple of 100 from the categories of assets, secured debt, unsecured debt, income, and nondiscretionary expense.
trustee % of rounding numbers (assets, debt)	average % of rounding numbers (in assets, debt) of all the proposal filings submitted by the trustee in the past three years.

Table A1 – continued from previous page

Variable name	Definition
trustee % of rounding numbers (all)	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.
distance to the trustee	road-based travel distance (based on Google Maps) between the trustee postal code and the filer's residential address postal code.
distance to the nearest 3 trustees	average road travel distance to the nearest 3 available trustees from the filer's residential address.
searching cost (multiple)	distance to the chosen trustee divided by the distance to the nearest 3 trustees.
full payment	the proposal required repayment is paid in full according to the consumer proposal payment schedule.
default	the proposal filer fails to pay back consumer proposal debt according to the payment schedule in 3 consecutive months.
amendment and full payment	the consumer proposal is renegotiated and the proposal repayment is paid in full according to the new consumer proposal payment schedule.
rejection	the consumer proposal is rejected by the creditors.
withdraw	the consumer proposal is withdrawn by the debtor before approval.
amendment and default	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.

Table A2: Summary Statistics

This table reports summary statistics for proposal filings from 2006 to 30 June 2019. It summarizes trustee-related details of a proposal, including travel distances between proposal filers and trustees and trustee leniency based on round number prevalence. We present five summary statistics: number of observations, mean, standard deviation, 25th percentile, median, and 75th percentile. Detailed definitions of all variables are available in Table A1.

	Obs	Mean	25th %ile	Median	75th %ile	Std Dev
Travel distance and search costs						
Travel distance to chosen trustee (km)	441,826	33.1	6.9	17.5	41.2	40.2
Travel distance to nearest 3 trustees (km)	446,112	12.5	2.9	4.9	11.4	21.8
Search cost (multiple)	441,826	6.5	1.2	2.3	5.9	14.0
Trustee leniency						
% of numbers rounded (asset & debt values)	478,053	0.47	0.25	0.43	0.67	0.27
% of numbers rounded (all)	478,053	0.45	0.25	0.42	0.62	0.25
Trustee % of numbers rounded (asset & debt values)	407,261	0.48	0.39	0.46	0.55	0.12
Trustee % of numbers rounded (all)	407,261	0.45	0.37	0.44	0.52	0.11

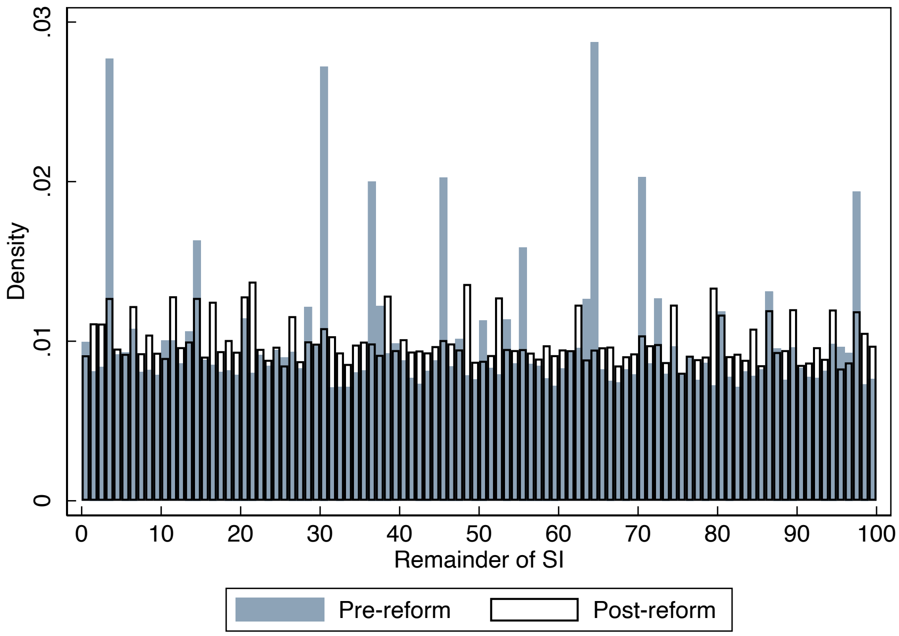
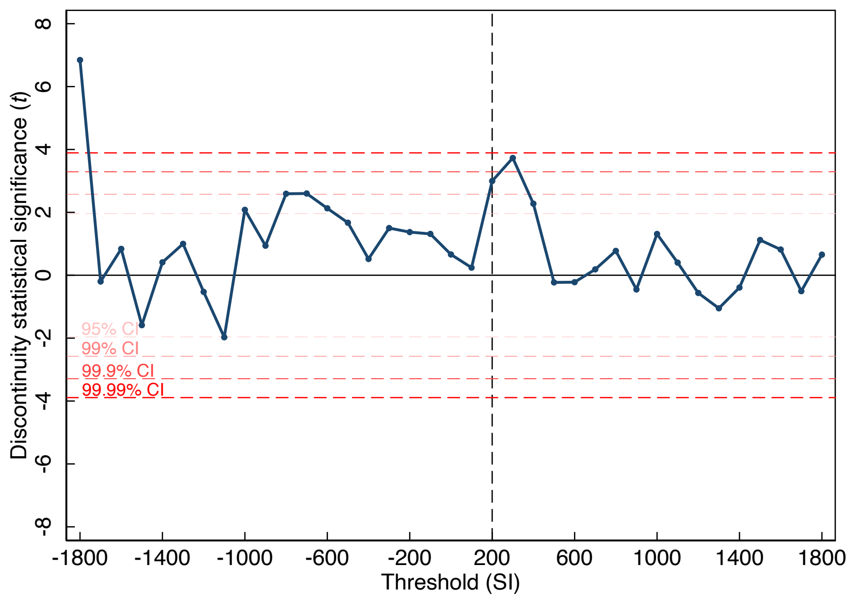
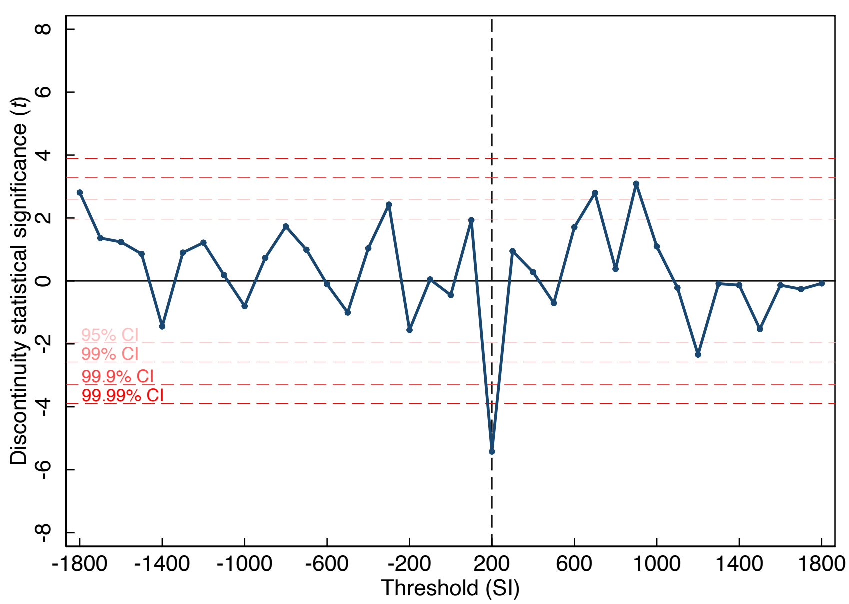


Figure A1: Distribution of SI mod 100

This figure plots the distribution of the remainder when SI is divided by 100 for the pre- and the post-reform periods. The remainder is calculated as: Remainder of SI $= SI \mod 100$. All proposal filings are used to calculate this remainder.



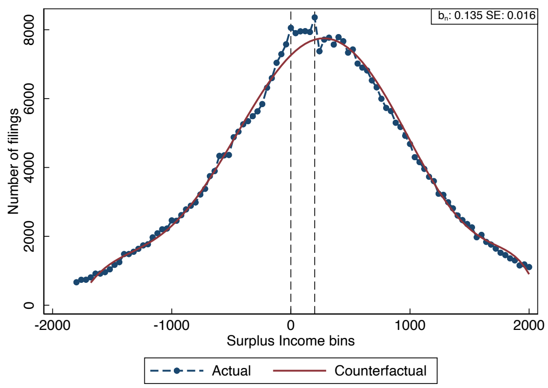
(a) Pre-Reform



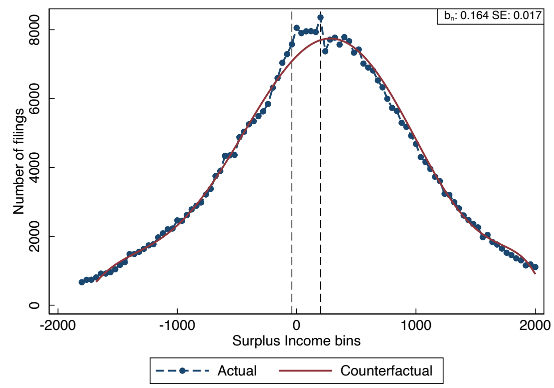
(b) Post-Reform

Figure A2: Placebo Tests for Discontinuity at Pseudo Thresholds

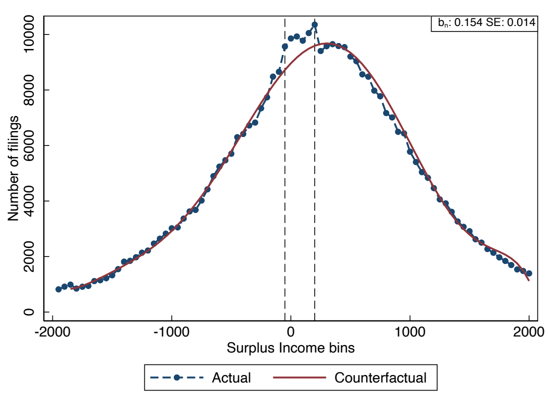
This figure shows the statistical significance levels (t-statistic) for Cattaneo et al. (2020) discontinuity tests at hundred-dollar Surplus Income (SI) thresholds from -$1,800 to $1,800. To maintain consistency across all tests, proposal filings with an SI within $600 of each pseudo threshold are included in each test (the results are similar for other ranges). In panel (a), the discontinuity tests are performed on pre-reform filings. In panel (b), the discontinuity tests are performed on post-reform filings. The red dashed horizontal lines indicate statistical significance levels: 95%, 99%, 99.9%, and 99.99%.



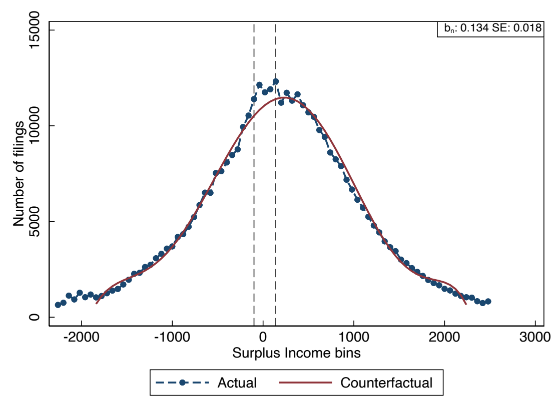
(a) Bin size: \$40, exclusion region: (-40,200)



(b) Bin size: \$40, exclusion region: (-80,200)



(c) Bin size: \$50, exclusion region: (-100,200)



(d) Bin size: \$60, exclusion region: (-100,200)

Figure A3: Bunching Magnitude Estimation Based on Alternative Bin Sizes
This figure replicates Figure 6 in the main text with different bin sizes and exclusion regions. The panel captions indicate the change in estimation methods relative to Figure 6.

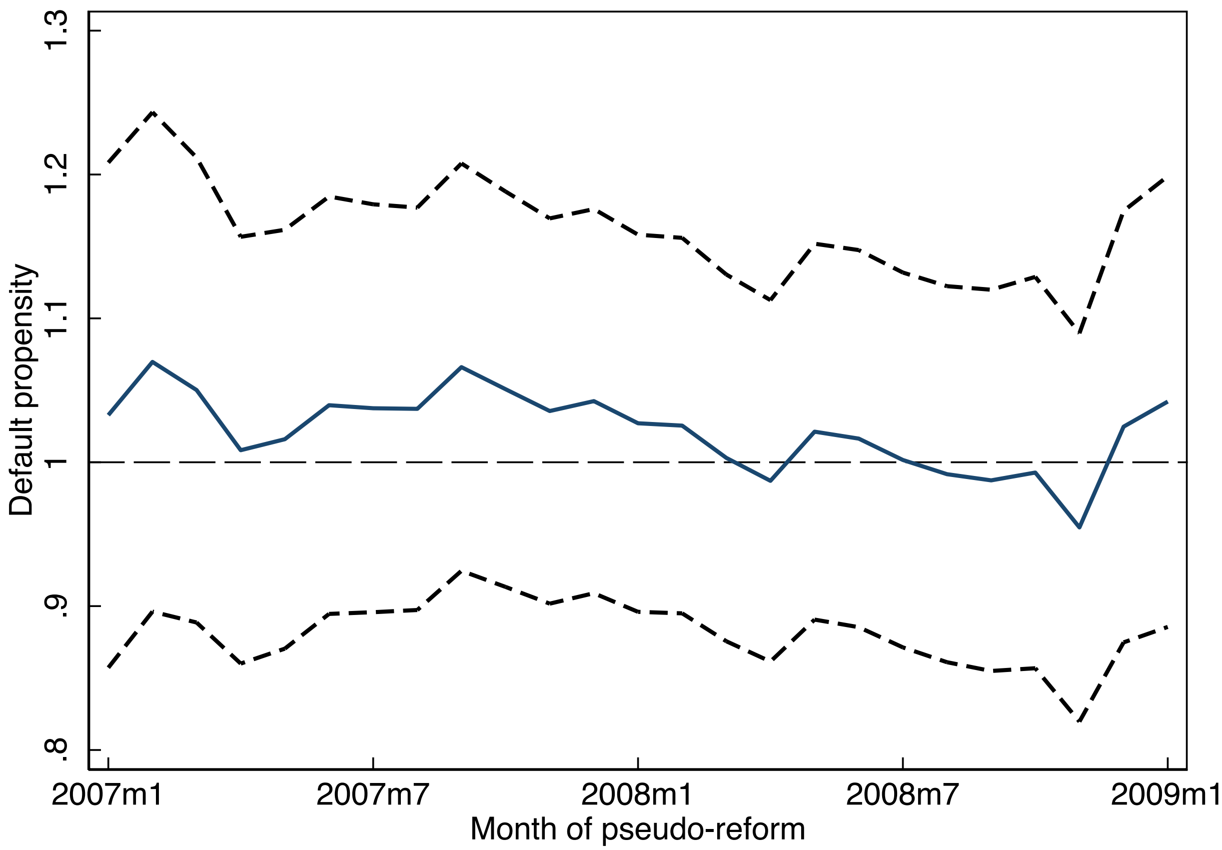


Figure A4: Pseudo Regulation Change Dates and Proposal Default
This figure reports the odds ratio and 95% confidence intervals for the effect of the policy change on proposal default (Below \$200 MZ × Post) using the same regression employed for Table 4, column (1), but for placebo policy change dates. In place of using the actual policy change date, we use each month from January 2007 to December 2008 as a placebo reform date. The x-axis shows the placebo policy change dates. On the y-axis, the estimates of the effect of the placebo policy changes on default and the 95% confidence interval for the estimates are reported as odds ratios.

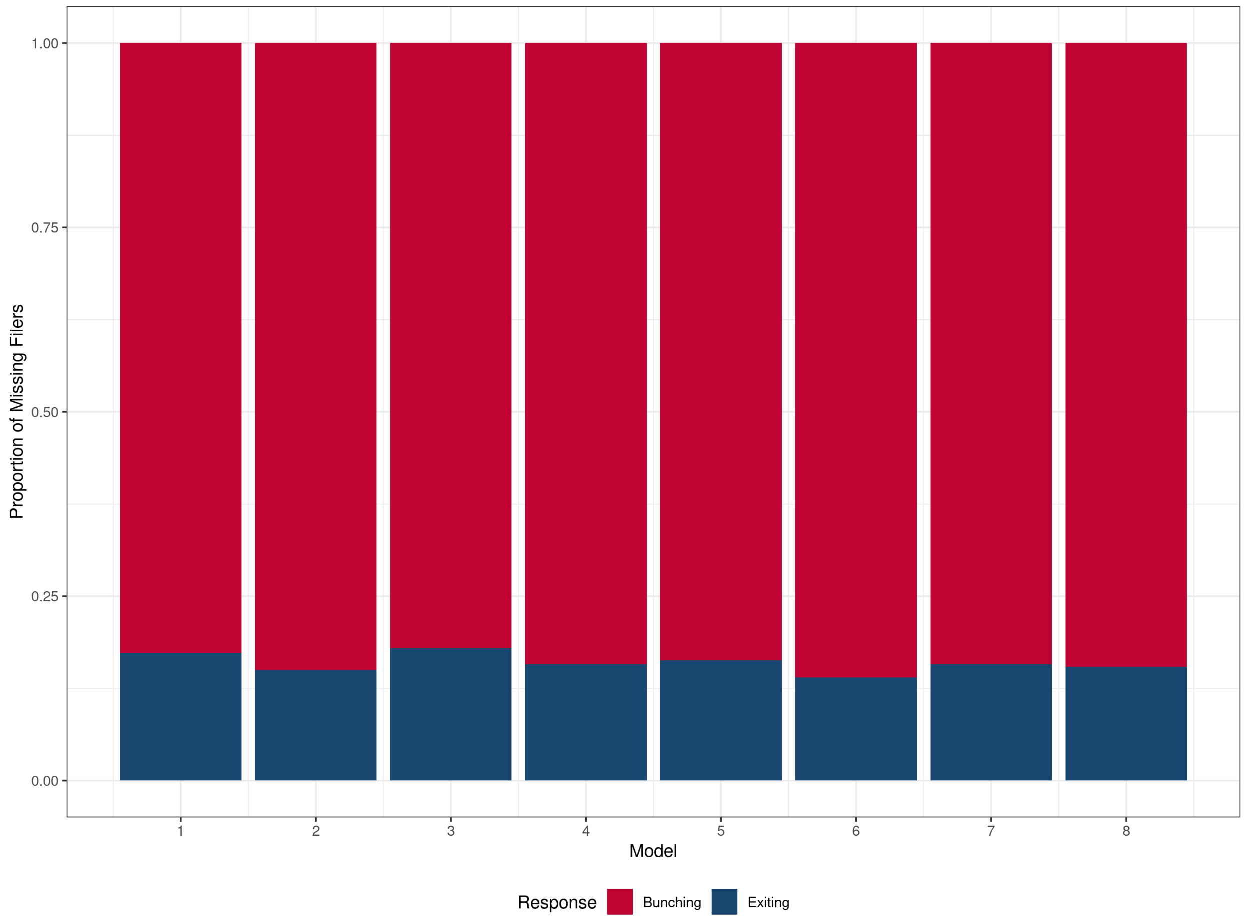
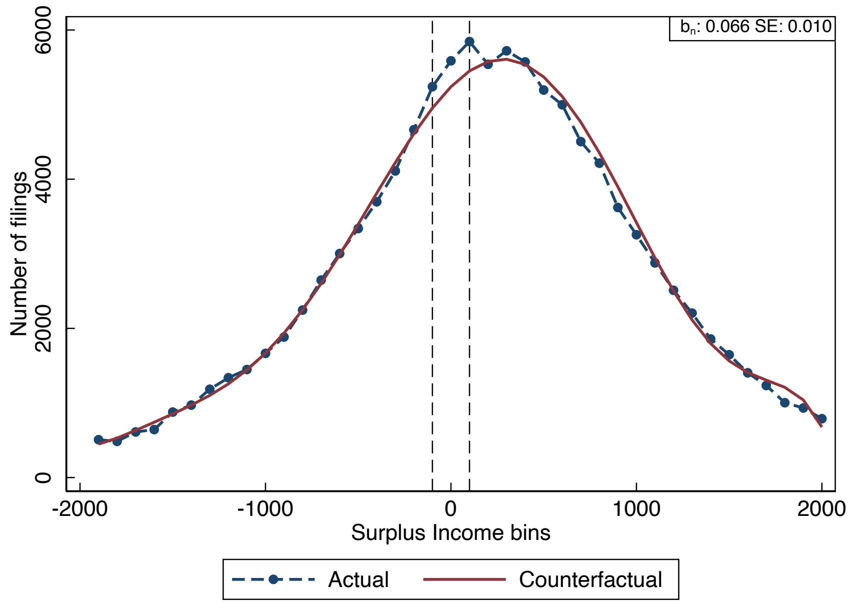
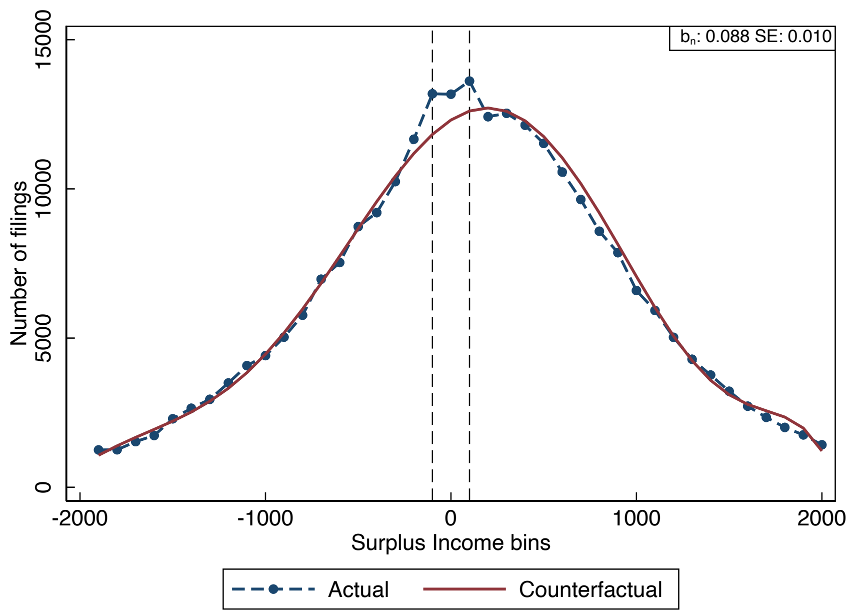


Figure A5: Estimated Proposal Exits Above \$200 SI Threshold Because of Reform

This figure plots estimates of the proportion of potential proposal filers with true Surplus Incomes (SIs) above the \$200 threshold (in particular, between \$200 and \$2,000), who respond to the 2009 introduction of the threshold at \$200 by strategically manipulating their income downward (Bunching) and those who respond by not filing a proposal (Exiting). The estimates are based on the empirical exercise detailed in Section B, in which the integration constraint of the Chetty et al. (2011) bunching methodology is exploited to calculate the proportion of exiters and bunchers. The estimates are calculated using all post-reform proposal filings. The eight estimates provided in the figure correspond to the eight bunching models estimated in Table 2.

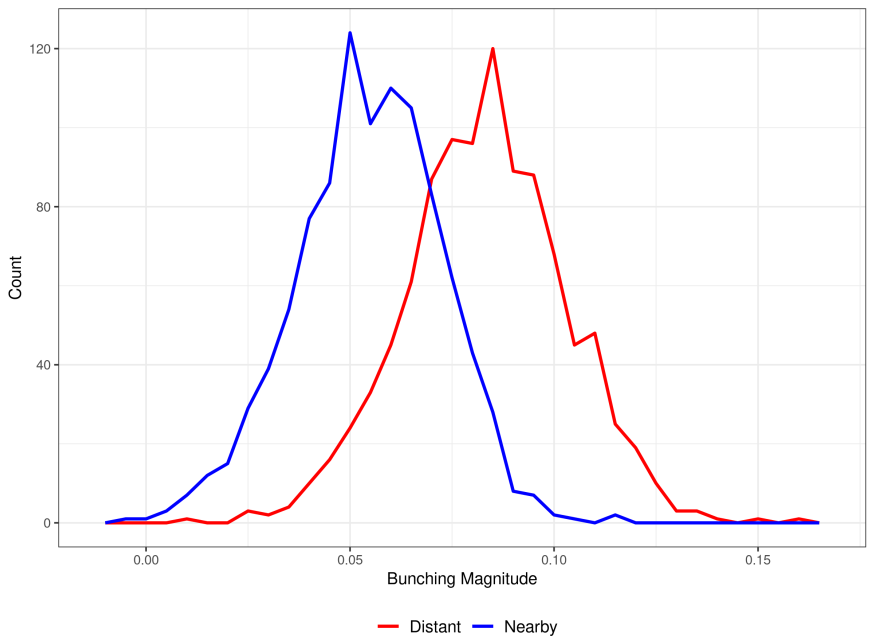


(a) Nearby Trustees

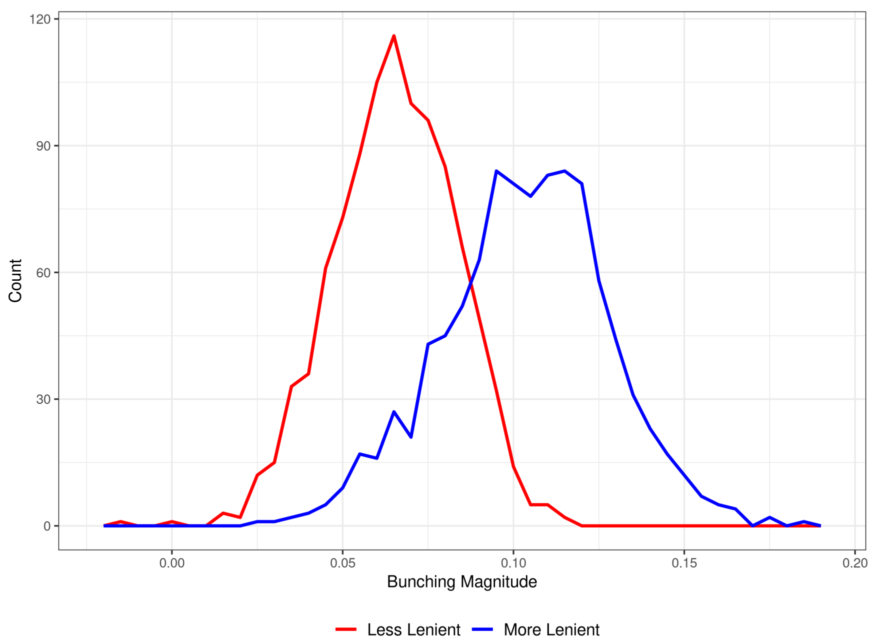


(b) Distant Trustees

Figure A6: Bunching Magnitude by Travel-Related Transactions Costs
This figure shows the estimates of bunching magnitude for subsamples based on filers' travel-related transaction costs, as measured by the excess travel distance between filers and their chosen trustee as a multiple of the average travel distance to the three nearest trustees. Panel (a) shows the bunching magnitude for filers who travel excess distances of less than 1.3 times the average travel distance to the three closest trustees, i.e., employ nearby trustees and panel (b) shows bunching magnitude for filers who travel excess distances of more than 1.3 times that threshold, i.e., employ distant trustees. As in all our primary bunching analysis, the bunching magnitude is calculated using \$100 Surplus Income (SI) bins 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.



(a) Distant vs. Nearby Trustees



(b) Less vs. More Lenient Trustees

Figure A7: Bunching Magnitude Comparison

These figures present the distributions of bootstrapped estimates of the bunching magnitude, following the method detailed in Appendix C.2. Panel (a) shows the distribution of bootstrapped bunching estimates for populations of proposal filings with distant and nearby trustees in red and blue, respectively. Distant trustees are defined as trustees located 1.3 times farther from the filer than the average travel distance to the three trustees closest to the filer and nearby trustees are trustees located closer than the same threshold. Panel (b) plots the distribution of bootstrapped bunching estimates for populations of proposal filings with less and more lenient trustees in red and blue, respectively. Less lenient trustees are defined 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. All post-reform proposal filings are used in the bunching magnitude estimation procedure.

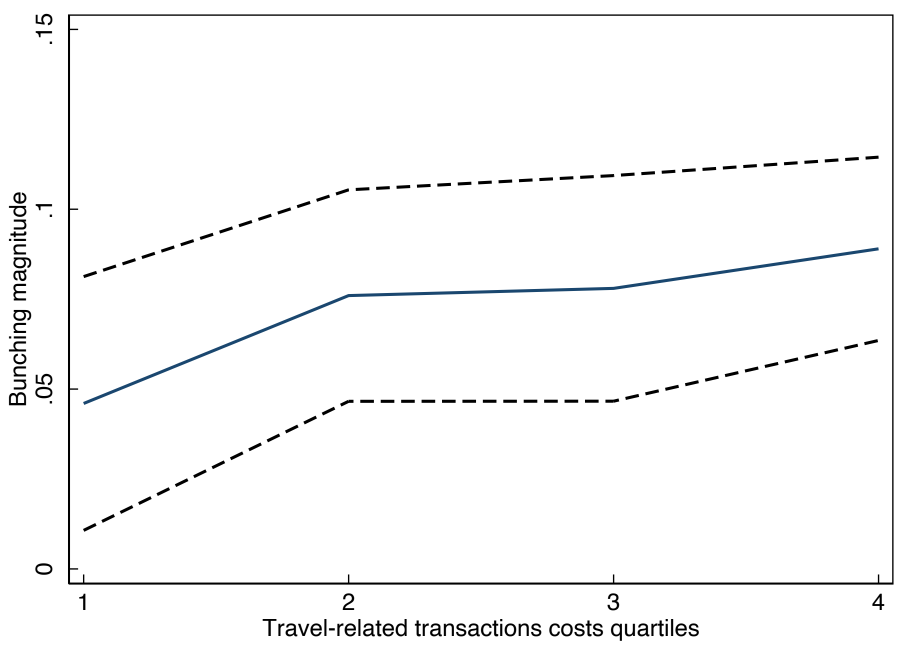
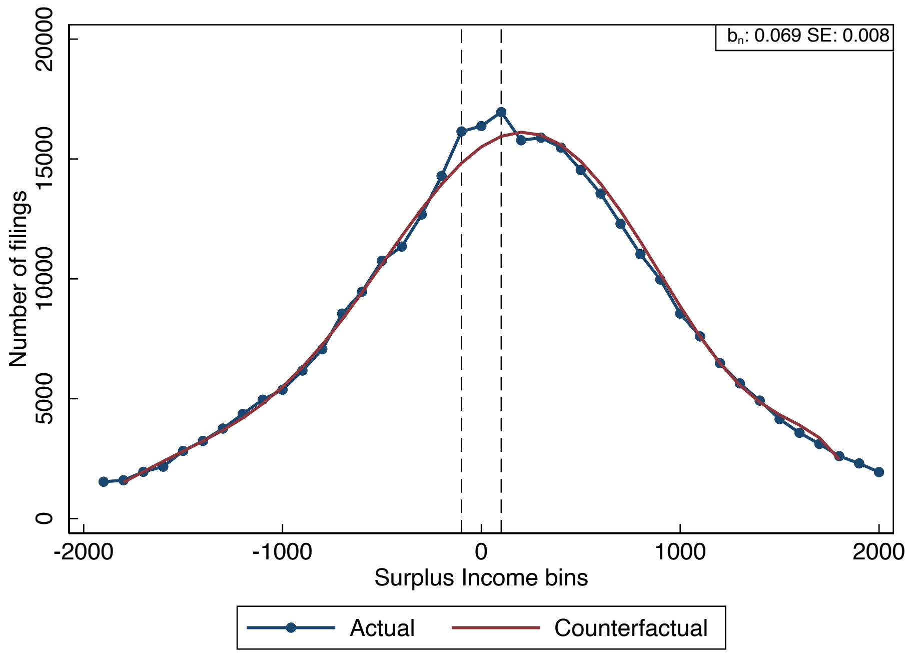
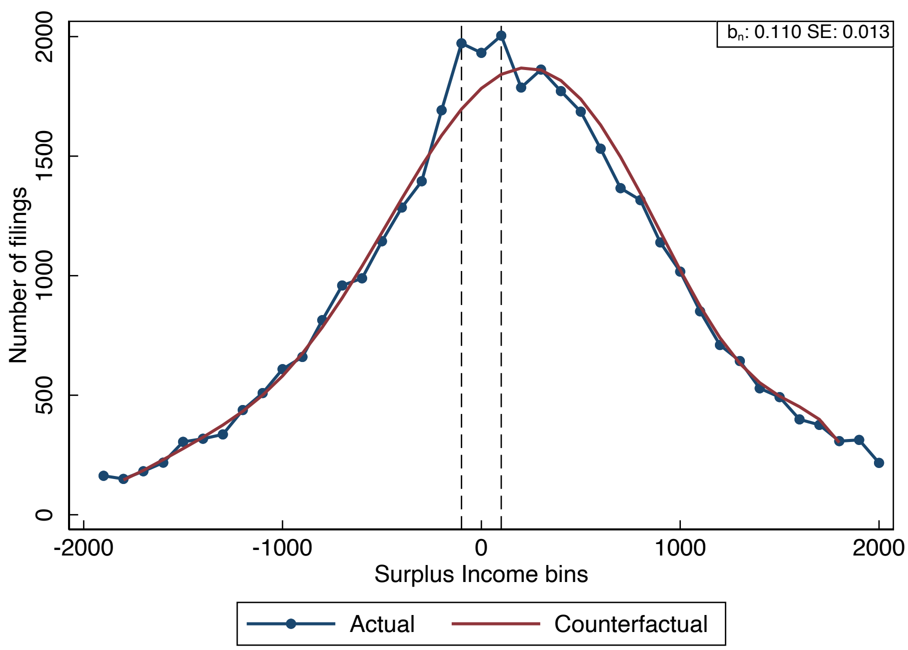


Figure A8: Bunching Magnitude over Search Cost Distribution
This figure plots the bunching magnitude estimate across the travel-related transactions costs distribution, where transactions costs are based on the excess travel distance between a proposal filer and their chosen trustee (see Section C.2 for details). All proposal filings in the post-reform period are divided into quartiles, from the lowest transactions costs trustees (least excess travel distance) to the highest transactions costs trustees (greatest excess travel distance). The solid line shows the bunching magnitude estimates for each quartile, and the dashed lines represent the 95% confidence interval of the estimates. As in all our primary bunching analysis, the bunching magnitude is calculated using $100 Surplus Income (SI) bins and a 7th degree polynomial to model the counterfactual distribution.



(a) Less Lenient Trustees (below 90th percentile)



(b) More Lenient Trustees (above 90th percentile)

Figure A9: Bunching Magnitude by Trustee Leniency
This figure shows the bunching magnitude estimates for subsamples based on trustee leniency levels, as measured by round number prevalence in trustees' approved proposals in the last three years (see Section C.3). Panel (a) shows the bunching magnitude for trustees in the bottom 90% based on trustee leniency, and panel (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 \$100 Surplus Income (SI) bins 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.

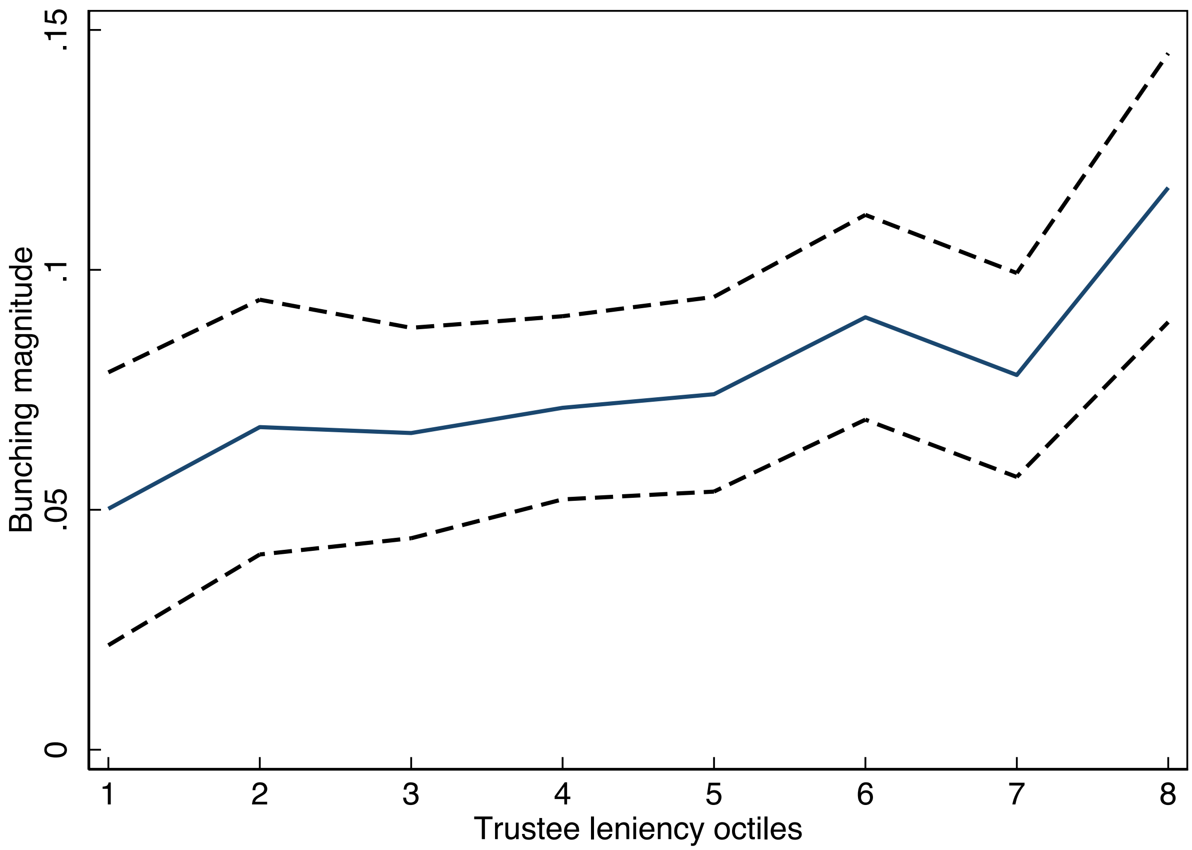


Figure A10: Bunching Magnitude over Trustee Leniency Distribution
This figure plots the bunching magnitude estimates across the trustee leniency distribution, where trustee leniency is measured by round number prevalence in trustees' approved proposals in the last three years (see Section C.3 for details). All proposal filings in the post-reform period are divided into octiles, from the least lenient trustees (bottom octile) to the most lenient trustees (top octile). The solid line shows the bunching magnitude estimates and the dashed lines represent the 95% confidence intervals for the estimates. As in all our primary bunching analysis, the bunching magnitude is calculated using \$100 Surplus Income (SI) bins and a 7th degree polynomial to model the counterfactual distribution.

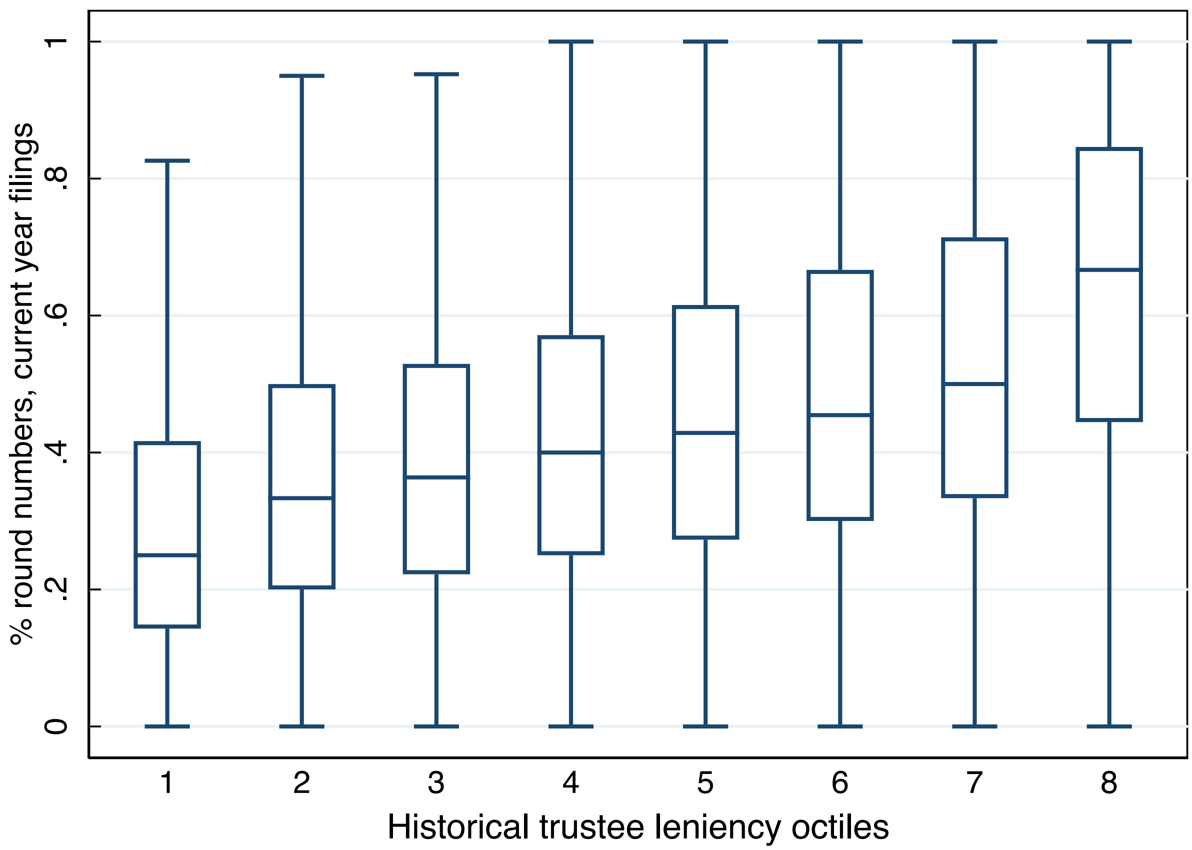
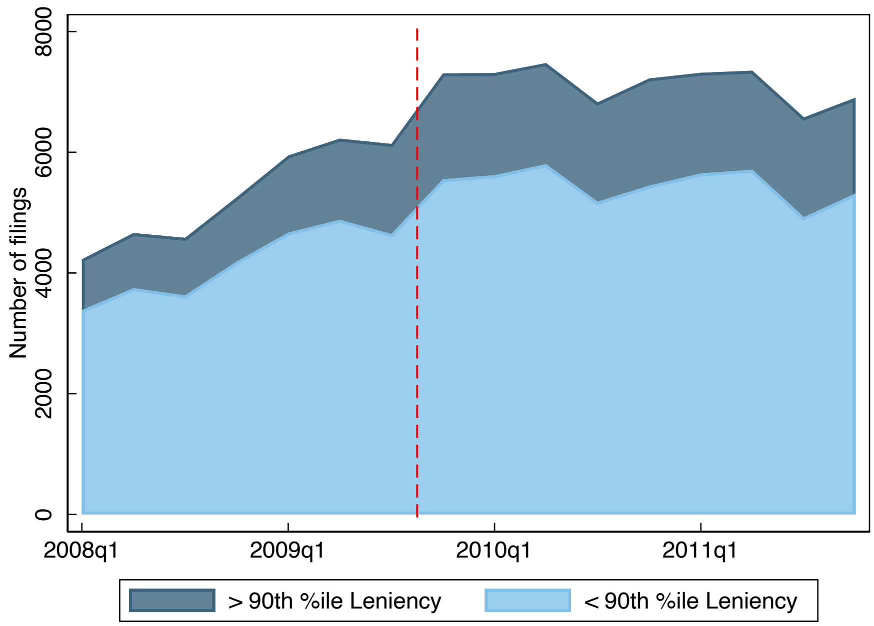
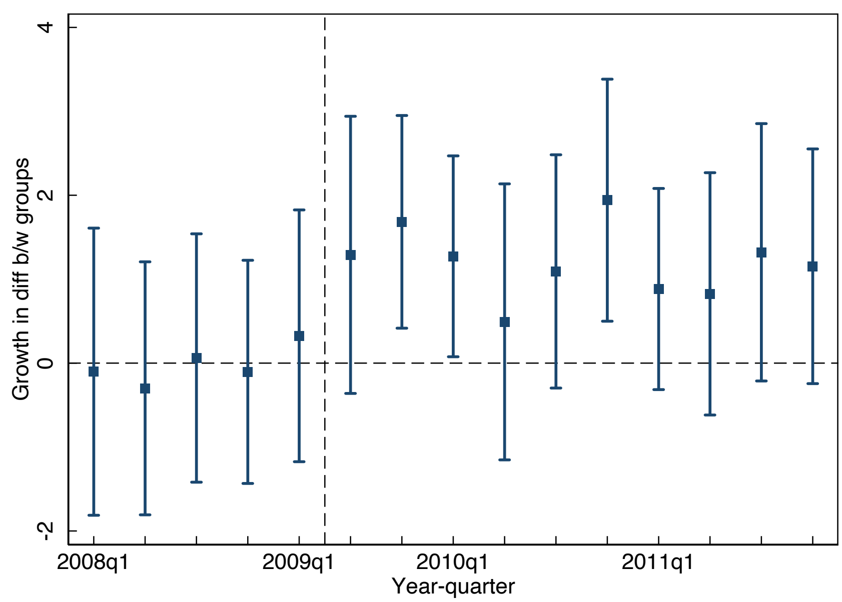


Figure A11: Use of Round Numbers Across Trustee Leniency Distribution
This figure plots the distribution of average percentage of round numbers in filings over trustee leniency groups. Percentage of round numbers in filings is measured for each post-reform proposal filing and averaged within trustee leniency octiles, where trustee leniency is the round number prevalence for trustees' approved proposals in the last three years (see Section C.3 for details). Filings are categorized into 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 percentage of round numbers, the line in the middle of each box reflecting the median value, and the caps reflecting the value 1.3 times farther from the median than the nearest quartile.

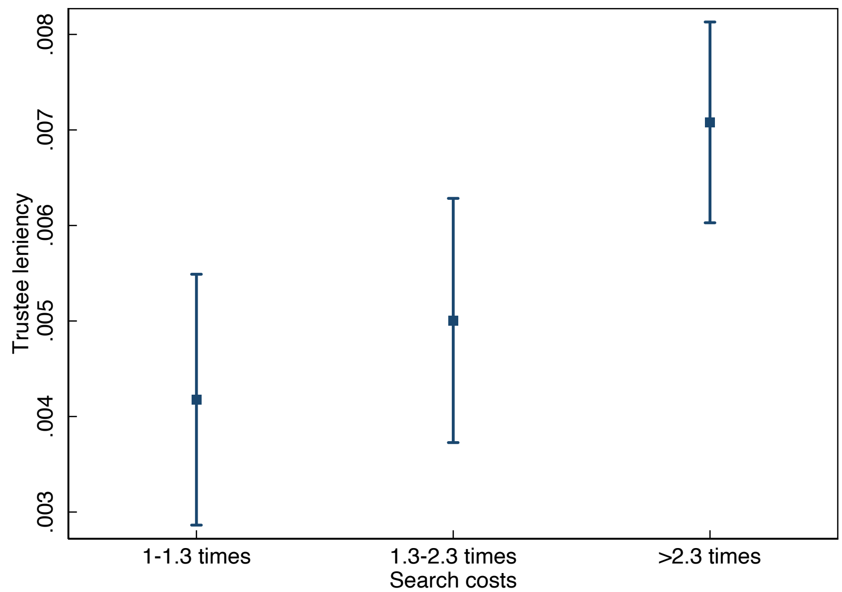


(a) Market share dynamics

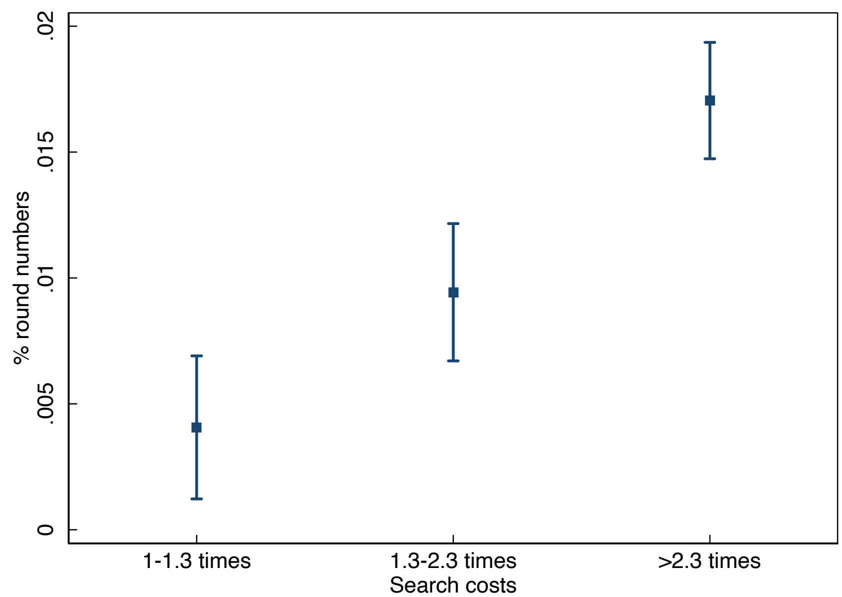


(b) Estimated change of market share

Figure A12: 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 three years before the reform (see Section C.3). Panel (a) plots the quarterly number of filings for the two groups. The vertical line represents Q3 2009, the quarter of the reform. Panel (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 (omitted) category in the regression.



(a) Trustees leniency



(b) Filing % of rounding numbers

Figure A13: Trustee Leniency, % of Round Numbers and Search Cost

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 travel distance between proposal filers and their chosen trustee (see Section C.2). All post-reform filings are categorized based on excess travel distance to chosen trustee: under 1 times, 1 to 1.3 times, 1.3 to 2.3 times, and over 2.3 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 C.3) and averaged across all post-reform filings within an excess distance group. Percentage of round numbers per filing is measured for each post-reform proposal filing and averaged within excess distance groups. Differences in trustee leniency and filing round numbers are reported 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 for the estimates.