# Consumer Bankruptcy Audits*

Fabian Nagel[1]

October 12, 2024

**Abstract**

Bankruptcy insures consumers against large and unexpected wealth shocks. However, debtors may abuse this insurance. Indeed, close to 20% of consumer bankruptcy filings contain at least one material misstatement. I exploit the conditionally random assignment of audits to estimate the effect of mandatory audits on debt forgiveness in consumer bankruptcy. I find that audits reduce debt forgiveness, but only when alternative oversight is low (Chapter 7). Audits come at the cost of increased case complexity for filers, deteriorating the long-run financial health of unsophisticated filers. Generally, audits drive a reallocation of debt relief from non-compliers and misreporters to truthful filers. Aggregate calculations show that the reduction in debt forgiveness due to misstatements and deterrence exceeds the direct cost of increasing the audit rate when oversight is low. Reductions in debt relief due to deterrence exceed reductions due to identified misstatements two-fold.

*This paper is based on my dissertation. I am grateful to my dissertation committee Christian Leuz (Chair), Anna Costello, João Granja, and Constantine Yannelis for thoughtful comments and suggestions. I thank Phil Berger, Matthias Breuer, Hans Christensen, Jonas Dalmazzo, Kalash Jain, Roope Keloharju, Maria Khrakovsky, Charles McClure, Andrew McKinley, Mike Minnis, Maxi Muhn, Valeri Nikolaev, Delphine Samuels, Gurpal Sran, Chris Stewart, Andrew Sutherland, Thomas Rauter, Hristiana Vidinova, Arndt Weinrich, Anastasia Zakolyukina, and seminar participants at Chicago Booth, Stigler Center Lunch Workshop, MIT Sloan, London School of Economics, NYU Stern, INSEAD, Stanford GSB, Wharton, Simon Business School – University of Rochester, Kellogg School of Management, London Business School, and Duke's Fuqua School of Business for helpful comments and suggestions. I am thankful for fee exemptions from various bankruptcy courts to access audit reports. Bronson Argyle, Sasha Indarte, Ben Iverson, and Christopher Palmer kindly shared data on bankruptcy judges and trustees (Argyle et al., 2022). Hugo Smith provided excellent research assistance collecting audit reports. This research was funded in part by the John and Serena Liew Fellowship Fund at the Fama-Miller Center for Research in Finance, University of Chicago Booth School of Business. This research was also funded in equal part by the Chookaszian Accounting Research Center, University of Chicago Booth School of Business. Credit data is provided by TransUnion, a global information solutions company. All errors are my own.

[1]Northwestern University, Kellogg School of Management, fabian.nagel@kellogg.northwestern.edu

# 1 Introduction

Annually, close to 1 million consumers file for Chapter 7 or Chapter 13 bankruptcy with $110 billion of debt being considered for forgiveness every year. However, large debt forgiveness also provides the opportunity for abuse (Becker, 1968). Indeed, congressional reports show about 20% of bankruptcy cases contain at least one material misstatement. Audits can solve this problem at a cost. Audits allow bankruptcy administrators to protect the program against fraudulent claims from consumers but could also impose large compliance costs that discourage the most disadvantaged groups from seeking relief. This paper examines a large audit program of consumer bankruptcy filings to understand how audits shape the efficacy and equity of bankruptcy relief in the US.

Consumer bankruptcy provides valuable insurance by protecting consumers from large and unexpected wealth shocks that affect their ability to service their debts. However, lenders may not be repaid in bankruptcy, increasing the cost of credit and reducing the ex-ante credit supply (Gross et al., 2021). When bankruptcy relief is not targeted to individuals who really need it, the social value of the insurance program is small, but its costs are shared by all consumers. Hence, it is paramount for the program's effectiveness that oversight mechanisms are in place to prevent socially inefficient debt forgiveness.[1]

In all consumer bankruptcies, a trustee administers the consumer's estate, gathers and liquidates non-exempt assets, and oversees the bankruptcy process. The consumer's bankruptcy filing is the basis for this process. This filing contains information about the filer's income (e.g., wages), expenses (e.g., rent, utilities, and insurance), a list of assets (e.g., bank and investment accounts), and a list of debts. The Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA) of 2005 introduced audits of those filings. The audits' role is to estimate the fraud prevalence and deter fraud in the bankruptcy system. In debtor audits, Certified Public Accountants (CPAs) analyze the consumers' bankruptcy filings. For example, CPAs compare the declared income to the income on tax returns, analyze receipts on bank statements, and search

[1] See, for example, the bankruptcy abuse by a former partner at various law firms here.

public records for assets such as cars and real estate.

Audits can curb opportunistic behavior by debtors. Opportunistic behavior imposes an agency cost when entering into a contract with creditors. Enforcing the contractual terms (i.e., debt repayment) and monitoring of the debtor's behavior can reduce the agency cost. As hypothesized by Jensen and Meckling (1976), audits can serve as a monitoring tool for preventing and detecting contract breaches. If the threat of misrepresentations being caught is credible, the expectation of oversight will induce bankruptcy filers to report truthfully when filing (Townsend, 1979). The easy verifiability of reported income and assets, e.g., via bank statements and public registries, amplifies the threat of oversight and increases the incentives to report truthfully.

But there could be a dark side to these audit programs as increasing the complexity and bureaucratic burden for debtors might affect the distribution and allocation of debt relief– especially if certain groups are unable to navigate the paperwork needed to comply with the audit request. An intended effect of the audit would be to separate cases with egregious misreporting from those reporting truthfully to improve resource and relief allocation. However, the regulatory burden imposed by audits can have unintended consequences. If audit compliance is complex, the audit requirement could deteriorate relief targeting. Disadvantaged groups frequently struggle to comply with complex regulations (Finkelstein and Notowidigdo, 2019). Consequently, less relief could be available to disadvantaged groups unable to fulfill the audit requirement.[2] Hence, audits present a welfare trade-off between a reduction in opportunistic behavior, ultimately reducing the cost of credit, and a potential deterioration in relief targeting due to increased complexity.

The consequences of audits in consumer bankruptcy are controversial. While industry organizations argue that oversight and audits, in particular, are important for affordable cost of credit, bankruptcy attorneys representing filers question the effectiveness of audits and even

[2]The president of the National Association of Consumer Bankruptcy Attorneys describes audits as "a real hardship" only adding cost to the bankruptcy process that is already difficult to afford for many (compare The Wall Street Journal here).

the consequences of misstatements.[3] Scrutiny and oversight during the bankruptcy process are contentious topics trading off fair bankruptcy proceedings with burdensome oversight. The US Trustee Program is the "watchdog" ensuring that only individuals unable to repay their debt obtain debt forgiveness. However, strict oversight may be counterproductive if it only increases the filing burden for individuals in need of debt forgiveness subsequent to an adverse shock.[4] Effective oversight mechanisms distinguishing valid bankruptcy filings from opportunistic filings are, therefore, crucial. I exploit a unique feature of the institutional setting to isolate plausibly exogenous variation in audit assignment. This variation allows for examining the consequences of audits for average debt forgiveness and its distribution.

Paragraph 603 of the Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA) of 2005 requires implementing an audit program to ensure the accuracy of financial information in consumer bankruptcy filings. The audits—known as "debtor audits"—are designed to estimate fraud prevalence and deter fraud in the bankruptcy system. Upon filing, cases are selected for audit either *randomly* or due to high income and expense variance of the filer.[5]

The audit reveals the accuracy of the filing ex-post. The auditor provides a report with three potential outcomes: (1) a report with no material misstatement, (2) a report with one or more material misstatements and the list of misstatements, or (3) a report stating that no audit could be completed. I refer to individuals for whom no audit could be completed (3) as non-compliant with the audit request, whereas (1) and (2) are compliant with the audit request. Individuals do not know that they will be audited at the time of filing. The audit is mandatory. The auditor is chosen, contracted for, and paid for by the US Trustee Program. Hence, the audit is a simple independent verification with few conflicts of interest.

I begin my empirical analysis by establishing that audit selection is consistent with the

[3] WSJ articles covering the suspension (here) and resumption (here) of debtor audits summarize some of these contentions.

[4] Particularly during the financial crisis, politicians were concerned that burdensome bankruptcy rules prevent individuals requiring debt forgiveness from filing for bankruptcy. The congressional hearing Serial No. 110-161 of the Subcommittee on Commercial and Administrative Law on October 2, 2007, is titled "United States Trustee Program: Watchdog or Attack Dog?" and examines the burdens of the bankruptcy proceedings.

[5] Random selection requires an audit of at least every 250th filing in a federal judicial district. Suspicion-based selection is due to high income or expense variance of the filer.

assignment mechanism prescribed by law. Audited cases are consistent with selection based on randomness or high income and expense deviation from district averages. I further establish that misrepresentations in bankruptcy filings are frequent, averaging around 20%. Aggregate statistics show that misstatements are more frequent in non-randomly selected cases.

Individuals frequently self-report financial information. For example, individuals self-report their financials in credit applications for mortgages and credit cards, on tax returns, applying for governmental assistance programs, and—as examined here—in bankruptcies.[6] In contrast to other settings, misstatements are directly observable to lenders, trustees, judges, and researchers on a case-by-case basis for audited consumer bankruptcy filings. Therefore, I show the type and severity of misstatements for a hand-collected sample of approximately 3,500 audit reports with misstatements.

The most frequently misstated line item is bankruptcy filer income. The bankruptcy reform of 2005 introduced means testing, preventing high-income individuals from filing liquidation cases. Misstating income may allow consumers to file for different types of bankruptcies or reduce repayment obligations. Misstatement or omission of savings and investments accounts is the second most common type of misrepresentation. Other categories of misrepresentations also relate to the concealment of assets. Vehicles are omitted or understated in value, real estate is not declared, or personal property is misstated in value. A common omission in bankruptcy filings is the transfer of assets to family members or the omission of asset sales yielding cash proceeds. These misstatements come at the cost of lenders by either reducing the repayment or enabling an unjustified discharge had they not been detected. Further, those costs are ultimately carried by society as a whole in the form of higher ex-ante interest rates to recoup higher losses in bankruptcies.

Audits can prevent debt forgiveness when misstatements are identified. If a bankruptcy case is dismissed, no debt is forgiven, while a discharge results in the forgiveness of eligible debts.

[6] In each of those instances, misreporting adversely affects society by amplifying booms and busts (Mian and Sufi, 2017, 2022), reducing tax collection (Kleven et al., 2011), and obtaining government assistance more helpful to others in need (e.g., GAO report on unemployment benefits fraud).

After a successful bankruptcy filing, creditors are not allowed to collect forgiven debts. Due to the long-lasting effects of debt forgiveness, the consequence of audits for debt forgiveness is the primary effect of interest. Whether debt is forgiven or not affects financial health and home ownership (Dobbie et al., 2017) as well as earnings, five-year mortality, and foreclosure rates (Dobbie and Song, 2015). However, theory suggests that the presence of those benefits depends on the existence of a debt overhang problem (Myers, 1977). Excessive debt can, for example, distort individuals' labor supply decision or induce individuals to leave formal employment to evade wage garnishment. When audits confirm that income and assets are low, effective relief targeting should lead to debt forgiveness. If audits indicate that filers' income is sufficient to repay large portions of the debt, audits should lead to reduced debt forgiveness and more case dismissals than in non-audited cases.

I show that the effect of audits on debt forgiveness are heterogeneous. Audits lead to more case dismissals when oversight and dismissal rates are low (Chapter 7) but do not increase dismissals when scrutiny and dismissal rates are already high (Chapter 13). This finding is robust to restricting the sample to cases with low income and expense deviation from the average filer in a district. Subsample tests show that regardless of their effect on case dismissals, audits increase the complexity of bankruptcy filings. Audited filings require more back-and-forth during the proceedings and modifications of the filings. The increased case complexity may deteriorate relief targeting if increased complexity prevents relief for disadvantaged groups. Consistent with a deterioration in relief for disadvantaged groups, audits increase case dismissal rates five times more for Chapter 7 filers without attorney representation.

The consequences of audit findings suggest that the audit improves relief targeting as long as individuals can comply with the audit. When individuals comply with the audit, the auditor submits an opinion of "No Material Misstatement" or "Material Misstatements." Among Chapter 7 cases, individuals with misstatements obtain reduced debt relief and have a higher likelihood of payments to creditors in liquidation. For individuals without misstatement, the likelihood of bankruptcy success is unaffected. Among Chapter 13 cases, individuals with mis-

statements need to modify their payment plans to obtain debt relief. For individuals without misstatement, the likelihood of bankruptcy success is increased at the cost of slightly modified repayment plans. For both Chapter 7 and Chapter 13 cases, these findings indicate improved relief targeting due to the presence of audits. However, the documented increase in case complexity due to audits could deteriorate relief targeting for unsophisticated individuals who cannot comply with the audit. To evaluate the aggregate importance of deterrence relative to the identification of misstatements I do a back-of-the-envelope calculation.

In aggregate, increasing the audit rate when oversight is low (Chapter 7) will likely result in gains for lenders while potentially harming relief access for unsophisticated bankruptcy filers. Back-of-the-envelope calculations suggest that a one-percentage-point increase in the audit rate of Chapter 7 cases would result in $14.2 million less debt forgiven due to identified misstatements, $13.7 million less debt forgiven due to the deterrence effect on selected individuals, and $13.7 million less debt forgiven due to reduced filings in expectation of the audit. This $41.6 million reduction in debt forgiveness benefitting lenders contrasts with direct audit cost of $6.5 million if the audit rate were to be increased.

Next, I gauge the indirect cost of audits. I merge bankruptcy records to consumer credit files from TransUnion to examine the long-term consequences of audits and deterrence. Comparing audited and matched non-audited individuals over time shows, on average, a small and short-lived deterioration in financial health among audited Chapter 7 cases. The deterioration is pronounced in Chapter 7 cases without attorney representation. Negative effects on financial health are concentrated in non-compliers without attorney representation, indicating that some deterrence prevents individuals in need of relief from obtaining it. A second source of indirect cost could be "false" dismissals as a result of minor misrepresentations. However, reductions in credit access due to dismissals of misreporters with low living expenses are short-lived and economically smaller than deterrence of individuals in need of forgiveness.

This paper offers several contributions. First, it adds to the literature on financial misrepresentations and fraud (for corporations (Dyck et al., 2010, 2023; Zakolyukina, 2018; Cook

et al., 2020) and individuals (Egan et al., 2019; Piskorski et al., 2015; Mian and Sufi, 2017)). Much of the research of individual misreporting in credit markets has focused on the mortgage market (e.g., Griffin and Maturana, 2016; Garmaise, 2015; Ambrose et al., 2016; Jiang et al., 2014)[7] and more recently, on pandemic fraud (e.g., Griffin et al., 2023). A notable exception is Mikhed et al. (2024) studying strategic income manipulation by debtors at an arbitrary income cutoff in Canadian bankruptcies. I provide baseline estimates for the prevalence and type of misstatements in consumer finances as well as the consequences of quasi-random audits uncovering and deterring misrepresentations.

I add to the literature studying consumer bankruptcy (Dávila, 2020; Wang et al., 2020; Indarte, 2020; Argyle et al., 2022; Lee, 2023), the consequences of bankruptcy (Dobbie et al., 2017, 2020), and the consequences of bankruptcy reforms (e.g., Gross et al., 2021; Chakrabarti and Pattison, 2019; Romeo and Sandler, 2023; Brown et al., 2024) by evaluating the effects of audits in bankruptcies. Classic models of optimal bankruptcy relief and exemptions have limited consideration for individuals' misreporting. This paper highlights the trade-off between monitoring to prevent abuse and its potential cost for relief targeting.

Bankruptcy is an institutionalized form of debt relief and also connects with the recent literature on discretionary debt relief programs (e.g., Kluender et al., 2024; Gyongyosi and Verner, 2024; Adelino et al., 2024) and its distributional consequences (Catherine and Yannelis, 2023).[8] I demonstrate that mandatory audits of personal finances can serve as a mechanism to reduce information asymmetry and mitigate abuse. However, to improve targeting, relief decisions have to take potentially lacking financial sophistication of individuals not complying with the audit into account.

Lastly, this paper adds to the audit literature by highlighting a setting in which mandated audits are valuable. Audits–even beyond financial statement audits–are commonplace (e.g., sustainability (Simnett et al., 2009), environmental (Duflo et al., 2013), and tax audits (Advani et al., 2021)) and their mandates widespread. Yet, the evidence on the value of mandatory

[7] Also see Griffin (2021) for a review.
[8] Also see Indarte and Kanz (2024) for a review of the literature on debt relief.

audits is mixed. While some programs indicate high returns (Boning et al., 2023; Shi, 2022), other audit studies are skeptical about economy-wide benefits of wide-ranging mandates (e.g., Breuer, 2021; Duflo et al., 2018; Bourveau et al., 2021; Breuer et al., 2023).[9]

## 2 Empirical Setting and Institutional Details

The United States Trustee Program—a division of the Department of Justice[10]—is the "watchdog over the bankruptcy process"[11] and is primarily funded through fees paid by bankruptcy filers. It has the mission "to promote the integrity and efficiency of the bankruptcy system for the benefit of all stakeholders–debtors, creditors, and the public."[12] The program selects Chapter 7 and Chapter 13 filings to be audited by certified public accountants or independent licensed public accountants. The authorization to select cases arises from the Bankruptcy Abuse Prevention and Consumer Protection Act (BAPCPA) of 2005. The USTP contracts with independent auditors to execute the audits. The audit's purpose is "to determine the accuracy, veracity, and completeness of petitions, schedules, and other information required to be provided by the debtor under sections 521 and 1322 of title 11."[13] The USTP shall randomly audit at least 1 in 250 bankruptcy filings in each federal judicial district. Furthermore, the USTP shall audit schedules with high income and expense deviations relative to the district they are filed in.[14]

However, budgetary constraints are the reason for lower observed audit numbers. The cost

[9] Research has documented private benefits of audits and higher-quality audits accruing to audited firms, e.g., from increased earnings responses (Teoh and Wong, 1993), lower cost of capital (Blackwell et al., 1998; Minnis, 2011), and fewer accounting errors (DeFond and Jiambalvo, 1991). However, to be beneficial, mandates must rely on a market failure, such as a positive externality from audits. Positive externalities from audits that could justify a mandate appear limited (Minnis and Shroff, 2017). Beyond a lack of positive externalities, important signaling information conveyed by firms' decision to get audited voluntarily is lost when audits are mandated (Lennox and Pittman, 2011; Kausar et al., 2016), implying a traditionally weaker case for audit mandates, contrasting with the findings in this paper. Also see DeFond and Zhang (2014) for a review of the audit literature.

[10] See About US Trustee Program here

[11] House Report No. 989, 95th Cong., 2d Sess. at 88 (reprinted in 1978 U.S. Code Congressional & Admin. News at 5787, 5963, 6049).

[12] See the USTP mission statement here

[13] Compare the public report on 2014 debtor audits here

[14] See Appendix Section C.

of audits is covered by the USTP and is, on average, approximately $1,000 (Flynn, 2015). Compensation for audits is a flat fee depending on the type of audit. An early procurement contract between the USTP and Tichenor & Associates from 2006 shows a cost of $300 for a random audit, $600 for a targeted audit, and $50 when no audit can be completed.[15]

To determine the accuracy of the filing, the auditors compare items on the bankruptcy filing and documents produced by the filer at the auditor's request. Documents to be produced by the bankruptcy filer are typically pay stubs for the six months before filing, two years of federal tax returns, and account statements for all depository and investment accounts. The documents must be provided to the auditor within 21 days of notice.[16] In addition, the auditor searches for unreported assets and their market value using publicly available and commercial records. If the auditor finds material misstatements, the filer will be given a chance to explain them. If those misstatements cannot be explained, debt discharge may be revoked, and creditors will be notified. In addition, the Trustee may take civil action against the filer and may make a criminal referral to the U.S. Attorney.

Appendix Section A shows an auditor report identifying material misstatements. Typically, the audit report will contain a standardized first page containing the filer's name, chapter, and case number. The report states according to which rule the auditor was contracted for an audit (28 USC §586 (f)(1)). It states that the audit was pursuant to the standards outlined in Appendix Section B. The example report also states that the report is only for the information of the US Trustee Program and any parties in interest in the bankruptcy proceedings. The report is not a legal determination. This statement is usually repeated when entering the report into the docket. Further, the audit report also contains the name of the audit firm conducting the audit. Notably, the audit report states whether none or one or more material misstatements are found during the audit.

The example report shows one or more material misstatements. The report then contains

[15] See Exhibit 2 on page 212 of the congressional hearing Serial No. 110-161 of the Subcommittee on Commercial and Administrative Law on October 2, 2007

[16] Compare the notice for debtor audit (here) as well as the sample document request for audit provided in Appendix Section D

an additional page with a list of material misstatements explaining the item misstated. Further, the list of misstatements contains the amount reported for each misstated item and the value as found by the auditor. These lists of misstatements are the source for Table 3 in Section 5. In the example audit, the filer underreported total combined monthly income on Schedule I, reporting an income of $6,795. However, the auditor found income to be $8,044.80. Hence, income was understated by $1,249.80. A second misstated item is the market value of a single item of personal property on Schedule B. The personal property was not reported. Hence, the reported value is $0. However, the auditor found the personal property item to have a fair market value of $12,655. Hence, the value was understated by $12,655. If no material misstatement was found during the audit, the box on the audit report for no material misstatement would be ticked, and no list of misstatements would be provided.

The United States Trustee Program provides aggregate statistics on the debtor audit program. Table 1 shows statistics regarding the number of people audited for a subset of years since the launch of the program. In the period from 2007 to 2019, for 12,816 randomly assigned audits, the auditor submitted a report to the court. Over the same period, 9,807 reports for audits due to income and expense deviations were submitted to the court. The audit opinion submitted to the court is an assessment of whether the bankruptcy filing contains a material misstatement. Figure 2a shows the aggregate prevalence and development of material misstatements over time. The total share of misstatements varies between 20% and 30%. Approximately 30% of audits due to income and expense deviations contain a misstatement, and 15% to 20% of randomly selected cases contain a material misstatement.

The materiality of a misstatement is defined in the implementation documents.[17] They state that "In general, a material misstatement is an inaccuracy or omission that compromises the integrity and reliability of the bankruptcy documents filed, including an inaccuracy or omission that may impede a determination of whether there are estate assets to administer or whether enforcement action should be taken. The thresholds for determining whether discrepancies

[17]More information about the implementation can be found here.

identified by audit firms constitute a material misstatement are not published. To publish them would jeopardize the deterrent value of debtor audits and present enhanced opportunities for gaming the system." Section 5 further elaborates on the types and Appendix Section I on the determinants of misstatements.

Furthermore, Table 1 reveals that for approximately 5% of mandated audits, the auditor is unable to complete an audit. Primarily, auditors are not able to complete the audit when bankruptcy filers do not comply with the document requests of the auditor. Figure 2b shows the variation of non-compliance over time. Audits due to random or expense-based selection have approximately similar rates of non-compliance. After a drop in non-compliance just after introducing the audit requirement, the non-compliance rate stabilizes around 5%. The US Trustee Program argues that audit documents requested are needed to prepare bankruptcy filings, implying a non-substantial cost of compliance. However, attorneys representing bankruptcy filers argue that compliance costs are substantial and represent a real hardship for individuals in need of debt relief.

Debtor audits are not the only oversight mechanism for consumer bankruptcies. Each bankruptcy is assigned a private trustee from a panel of trustees. The panel is selected by the US Trustee, who is distinct from the private trustees. The private trustee administers the bankruptcy estate, makes the appropriate payments, and ensures that the rules and regulations of the bankruptcy process are followed. Private trustees are compensated with a flat fee and bonus for collections (Antill, 2021). Some task complementarity occurs between the private trustee and auditor as both ensure the accuracy of the bankruptcy filings. The final decision on whether to discharge the debt or dismiss a bankruptcy case is up to the judge presiding over the bankruptcy case. Several studies rely on the rotation schedule of private trustees and judges exploiting their varying strictness to examine the effects of debt relief or understand racial disparities in bankruptcy relief (Dobbie et al., 2017; Argyle et al., 2022). My paper does not rely on judge or trustee rotation schedules and instead focuses on the consequences of bankruptcy audits. Figure 1 summarizes the setting.

The USTP contracts professional audit firms to conduct debtor audits. Contracted audit firms are typically retained for multiple years with little variation in audit firms from 2007 to 2020. Recently only six audit firms conducted all debtor audits for the US Trustee Program. Audit firms' main incentive to do the audit well is to retain government contracts to perform those audits. As the government procures audit firms, their spending on procurement and contract awards is publicly accessible. Contracts can be accessed in the spending overview for the US Trustee Program on USA Spending here. For example, Award ID 15JUST20F00000387 refers to a contract between the USTP and Tronconi Segarra & Associates Llp to conduct Chapter 7 audits in geographical area 1 and 2. The contract start date is 9/30/2020, and the contract end date is 12/31/2023. The current award amount is $1,056,000 with a potential award amount of $1,973,191.

Consumers can file for Chapter 7 or Chapter 13 bankruptcy. In Chapter 7 bankruptcies, all nonexempt property is liquidated, and proceeds are distributed to creditors. Upon distribution, all dischargeable debt is forgiven. In Chapter 13 bankruptcies, debt is reorganized, and debtors fulfill a three to five-year repayment plan in exchange for greater asset protection. Over the three to five-year period, debtors repay their disposable income. Only upon the completion of the repayment plan all eligible debts are forgiven. Whether it is more beneficial for debtors to file for Chapter 7 or 13 bankruptcy is largely a function of individuals' income and assets. Further, debtors need to satisfy eligibility rules for each type of bankruptcy. To qualify for Chapter 7 bankruptcy, filers must either have below state median income or pass a "means test" testing eligibility after certain expense deductions. In Chapter 13 bankruptcies, creditors need to obtain at least the same repayment as they would in a liquidation bankruptcy.[18] Filing fees for Chapter 7 cases are $335 ($245 case filing fee, $75 miscellaneous administrative fee, and $15 trustee surcharge), and average attorney fees amount to $1,500 to $3,000. Filing fees for Chapter 13 cases are $335 ($235 case filing fee and $75 miscellaneous administrative fee),

[18]The US Courts summarize basic bankruptcy requirements and proceedings for Chapter 7/13 bankruptcies here and here. For further context on bankruptcy proceedings also reference Argyle et al. (2022) and Dávila (2020).

and average attorney fees amount to $3,000 to $4,000.[19]

[Figure 1, Figure 2, and Table 1]

## 3 Conceptual Underpinnings

This section describes the basic trade-offs in consumer bankruptcy and the economic role of audits. Consumer bankruptcy provides valuable insurance by protecting consumers against large, unexpected negative wealth shocks (Livshits et al., 2007; Dávila, 2020). Large, unexpected shocks can affect consumers' ability to service their debt. After a shock, consumers may face excessive debt distorting, for example, their decision to participate in formal labor markets. The discharge—forgiveness—of debt can solve this debt overhang problem and causes higher earnings, better financial health, fewer foreclosures, and lower 5-year mortality rates (Dobbie et al., 2017; Dobbie and Song, 2015). However, those benefits depend on the existence of a debt overhang problem distorting consumers' decisions. The legal incidence of losses from forgiven debts falls on the lenders, while some of the economic incidence falls on consumers through higher interest rates ex-ante to finance losses in bankruptcy (Gross et al., 2021; Brown et al., 2024). When individuals without negative shock and without excessive debt file for bankruptcy, the insurance value is limited, while the costs of forgiveness still fall on all other consumers. Yet, people may still seek relief by misstating their income, hiding assets, and misrepresenting their financial position to obtain relief for tens if not hundreds of thousands of dollars. Audits of the bankruptcy filing are one mechanism to prevent opportunistic behavior by debtors and enforce contractual obligations (i.e., debt repayment) when consumers are able to pay. As hypothesized by Jensen and Meckling (1976), audits can thereby reduce the agency cost between creditor and debtor when entering into a contract. However, the average effect of bankruptcy audits on debt relief is not obvious as multiple equilibrium effects are possible depending on the prevalence of other enforcement mechanisms and judges'

[19] Filing fee information is obtained from the US Courts websites referenced in the previous footnote. Attorney cost estimates are obtained from here with similar numbers here.

beliefs about the average misreporting of filers.

Audits could be wasteful if they do not increase the threat of detection while increasing the administrative burden of bankruptcy filings. Bankruptcy filers already expect oversight from judges, creditors, and trustees during the bankruptcy process. If the threat of misrepresentations being caught is credible, the expectation of oversight will induce bankruptcy filers to report truthfully regardless of the existence of audits (Townsend, 1979).[20] Reported income and assets are easily verifiable, amplifying the threat of detection even without audits. Hence, filers may report truthfully, regardless of the existence of audits. The threat of detection affecting audit consequences suggests that audits are more effective when alternative oversight via judges and bankruptcy administrators is low.[21]

Audits could lead to an increase in debt relief and a more lenient bankruptcy system if they increase the credibility of bankruptcy filings. The consequences of audits for debt relief depend on judges' beliefs about the average level of misreporting. Judges and bankruptcy administrators are likely aware of the insufficiencies of the bankruptcy proceedings and the potential for misrepresentations in bankruptcy filings. While they may not be able to determine the exact degree of misreporting of an individual filer, they are likely aware that bankruptcy filers are incentivized to underreport income and assets to increase the probability of debt forgiveness and avoid the liquidation of assets. Hence, audits can reduce debt forgiveness if the detected level of misreporting is lower than the average level of misreporting previously perceived by judges and bankruptcy administrators. The increased credibility of the reported financials could thereby result in more debt forgiveness (DeFond and Zhang, 2014; Gipper et al., 2020). Conversely, if detected opportunistic behavior by debtors is more severe than expected, the detected misrepresentations will result in an—on average—stricter bankruptcy

[20] A limited role for pure strategic defaults in mortgage defaults is consistent with limited exploitation of defaults for personal gain (Ganong and Noel, 2023). Similarly, five-time greater responsiveness of bankruptcy filings to cash-on-hand than relief-generosity found by Indarte (2020) is consistent with limited exploitation of the bankruptcy system. Beyond the threat of oversight, a general preference for truth-telling can also drive truthful reporting and audits remaining without effect (Abeler et al., 2019).

[21] A pure signaling role of audits similarly predicts no effect of mandatory audit oversight. The auditor is neither chosen nor paid by the debtor. This leaves no opportunity for debtors to exploit the audit as a device to signal their eligibility for debt relief by, e.g., picking an auditor with more stringent audit procedures (Spence, 1976).

system and less debt relief.

Audits could increase the bureaucratic burden and complexity of bankruptcy cases. Increased case complexity could be particularly problematic if certain groups in need of debt relief are unable to navigate the additional paperwork and back-and-forth with auditors and bankruptcy administrators. The audits are intended to separate cases with egregious misreporting from truthful filings to improve resource and relief allocation. However, the regulatory burden imposed by audits can have unintended consequences. If audit compliance is complex, the audit requirement could deteriorate relief targeting. Disadvantaged groups and small firms frequently struggle to comply with complex regulations (e.g., Finkelstein and Notowidigdo, 2019; Leuz et al., 2008). Consequently, less relief could be available to disadvantaged groups unable to fulfill the audit requirement.

## 4 Data and Summary Statistics

In this section, I describe the data and provide summary statistics. The data is compiled from four sources. First, I obtain bankruptcy snapshots for the universe of filers from the Federal Judicial Center. Second, I determine which cases were audited by exploiting case research databases such as Lexis and CourtListener. Third, I obtain supplemental case information from PACER (Public Access to Court Electronic Records). Fourth, I merge bankruptcy records with consumer credit records provided by TransUnion.

Bankruptcy snapshots from the Federal Judicial Center cover the universe of filers. The snapshots include filing dates, case numbers, reported income, assets, expenses, debt by type of debt, dischargeable and non-dischargeable debt, case dismissal or debt discharge, closing dates, and Chapter.

Exploiting common phrases on audited court dockets, I text search case research databases to determine which bankruptcy filings were audited. I identify 15,062 audited cases and merge the list of audited cases with bankruptcy snapshots based on case location and case number.[22]

[22] This paper covers approximately 60% ($\frac{15,062}{25,052}$) of the audited population over the sample period.

I then hand-check the audit reports of misstated bankruptcy filings using court records on PACER.[23] Similarly, I obtain the docket text of audited and control cases for subsample analyses from PACER. I am grateful to a number of Bankruptcy Courts for granting PACER fee exemptions to access a subset of cases free of charge.[24]

Table 2 summarizes statistics for bankruptcy filings from 2007 to 2020. It contains approximately 13 to 14 million Chapter 7 and Chapter 13 filings over a 14-year period. The average Chapter 7 (Chapter 13) case has $80,218 ($107,208) in assets and $140,911 ($159,049) in total debt. Average monthly income is $2,603 ($3,231), and average expenses are $2,811 ($2,785). A non-negligible portion of filers has previously filed for bankruptcy. 6.85% of Chapter 7 and 32.8% of Chapter 13 filers have previously filed for bankruptcy. Roughly 8% (8.8%) percent of filers represent themselves (pro se). The average case takes 185 (774) days. In the majority of Chapter 7 cases, the debt is discharged, with only 5.17% percent of cases getting dismissed. For Chapter 13 cases, substantially fewer cases have their debt forgiven.

[Table 2: Summary Statistics]

As outlined in Section 2, audits are selected via two mechanisms. Cases are (1) randomly selected for audits and (2) selected when their expense and income deviate from district averages. The empirical challenge is that case dockets do not indicate whether a case was selected at random or due to high income and high expense.[25] Relative to the distribution of non-audited cases, the distribution of audited cases should have two features. First, due to the random selection of cases for audit, the distribution of assets, income, and expenses should substantially follow the distributions in non-audited cases. This should particularly be true for cases with low deviations from district averages in income and expenses. Second, we should observe a higher concentration of high-income and high-expense deviations among audited

[23] I determine which audited cases contained a misstatement using the text of the docket.
[24] I focus on a sub-sample of cases as collecting additional case-level information for all cases would be too costly.
[25] I attempted multiple FOIA requests to obtain randomly selected case numbers from the Executive Office of the US Trustee Program. My requests, as well as the appeal to the decision, were denied by the DOJ. It appears to be the position of the DOJ that any case-level information is private and, therefore, exempted from FOIA requests. Highly sensitive case-level information accessed is made available by the US Courts and not the DOJ.

cases. Because income, expense, and assets are likely positively correlated, we can expect a similar pattern for the distribution of asset deviations from district averages among audited cases. In other words, we should expect a bimodal distribution in expense deviations. In particular, I expect cases to be roughly distributed as non-audited cases and high-deviation cases to be oversampled.

Figure 3 plots the distribution of deviations of average expense and income from their district averages in the respective year and chapter. In Figure 3 Panel 3a, the distribution of average income deviations from the district average is plotted for Chapter 7 cases. As expected, the figure shows a bi-modal distribution. The left half of the histogram for audited individuals traces the distribution of deviations for non-audited cases. Not considering cases with deviations greater than approximately $1,800, audited and non-audited case distributions are identical.[26] The excess density for audited cases in high expense deviations reflects the fact that the USTP selects cases with high income and expense deviations. Panel 3b plots the deviation of average expense from district averages for audited and non-audited Chapter 7 cases. Interestingly, the distributions and deviations across audited and non-audited cases look almost identical to Panel 3a. Again, we observe a bi-modal distribution for audited cases, reflecting the fact that cases with high expense deviations are more frequently selected. One distinction between Panels 3b and 3a is the small bump in large negative income deviation cases, likely reflecting low or zero income cases while expenses are unlikely to be zero.

Panels 3c and 3d repeat the analysis for Chapter 13 cases. The general patterns similarly hold for Chapter 13 cases but are somewhat less extreme. Chapter 13 cases with high average income deviation from the district average are more likely to be audited. Similarly, Chapter 13 cases with high expense deviation from the district average are more likely to be selected for audit. Those patterns overall confirm that the USTP audit selection broadly follows the specified rules. Hence, Figure 3 is important supporting evidence for the empirical strategy outlined in Section 6.1, arguing for identification through the institutional knowledge of the

[26] Appendix Figure F.1 illustrates that income and expense distributions are exactly identical for audited and non-audited cases not considering cases with income and expense deviations greater than approximately $1,800.

setting (Armstrong et al., 2022).

[Figure 3: Deviation Histograms]

## 5 Misstatements

While all misstatements identified by audit reports are, by definition, material, their type and severity likely vary. To evaluate the prevalence of misstatements and the cost incurred by the bankruptcy system, we need to assess the size and type of those misstatements.

Therefore, I hand-collect and hand-check the audit reports for a subset of the identified cases with material misstatements. I hand-check approximately 3,500 cases and merge cases with additional case information from the snapshots of the Federal Judicial Center. Cases can have more than one misstatement. I classify each misstatement into the following categories: Income misstatements, when filers misstate the current or average income earned; Account misstatements when filers do not disclose or misstate balances on checking, savings, or investment accounts; Vehicle misstatements when individuals do not declare cars/trucks/boats or misstate their value; Real Estate misstatements when real property is hidden or misstated in value; Transfer misstatements when a sale, gift or transfer of items is not disclosed and does not fit any of the other categories;[27] and Personal Property when the audit report identifies a misstatement but does not further specify the particular item. For example, Personal Property could be jewelry, cars, or any other valuable item not disclosed or misstated in value. Other collects all other miscellaneous misstatements not fitting other categories.

For each category, I show the frequency of the misstatement and the reported value. If the audit report specifies the value according to the audit finding, I report the average value according to the audit and the size of the misreporting. For Real Estate, the audit can occasionally not identify the value of the property not reported. Hence, these do not enter the calculation for the size of the misreporting.

[27] Transfers, where the type of transfer is reported in the audit report, are classified with the respective category. For example, a transfer of real estate is classified as a misstatement of real estate, while a general transfer or a transfer of non-classifiable items is classified as a general transfer.

Table 3 summarizes the types, frequency, and severity of misstatements by the original Chapter of the consumer bankruptcies. While in Chapter 7, non-exempted assets are liquidated, Chapter 13 protects some of these assets and forgives remaining debts after completing a payment plan, potentially leading to different misstatement distributions. The split in misstatements roughly follows the distribution of Chapter 7 vs. Chapter 13 cases summarized in Table 2. As more bankruptcy filings are Chapter 7 filings and as more of these are audited, the absolute number of misstatements is also larger among Chapter 7 filings. The distribution of misstatements, however, does not differ substantially across the two filing types.

[Table 3: Misstatements]

The most frequently misstated item is individual income. More than half of the misstatements found are misstated income items in individuals' bankruptcy filings. For Chapter 7 (Chapter 13) cases, the average reported income among misstatements is $5,024 ($8,000), while auditors find an average income of $7,977 ($11,171) among these. This implies a misstatement of almost $3,000 ($3,200) in monthly income. This finding is consistent with the incentives for bankruptcy filers. For Chapter 7 filings, means testing for bankruptcy filings requires income to be below state medians (depending on household size). Hence, some filers may need to misstate their income to become eligible for debt forgiveness. For Chapter 13 bankruptcies, the repayment plan depends on the income available to bankruptcy filers. The higher the income, the more debt can be repaid to creditors. In extreme cases, Chapter 13 bankruptcies may be dismissed because filers can repay essentially all of their outstanding debt given their income available.

The next most frequently misstated or hidden items are savings, checking, and investment accounts. On average, among those misstated, an account balance of $1,351 for Chapter 7 and $753 for Chapter 13 bankruptcies is reported (including non-declared accounts with a zero value). However, the average value of those accounts determined by auditors is $25,631 and $9,584, respectively. More than $24,000 ($8,000) is, on average, either misstated or hidden among the identified misstatements.

The third most frequently misstated item is personal vehicles. Among Chapter 7 (Chapter 13) bankruptcies, personal vehicles were found to be misstated or omitted 276 (146) times. The reported value of those vehicles is $3,163 ($1,590) on average. However, auditors found the value of those vehicles to be $16,734 ($13,890) on average, implying a misstatement of more than $12,000 on average.

In bankruptcy, assets are exempted only up to a specific dollar value. Hence, underreporting the true value of assets or not declaring them increases the likelihood of being able to keep these assets. Undeclared real estate closely follows personal vehicles in the number of misstatements found. However, the misreported dollar value is very large due to their higher value. When misstated, most real estate is not declared as an asset in the bankruptcy, implying that the average reported value for real estate is close to zero. For the real estate for which auditors are able to determine the approximate value, they found an average value of $170,575 ($125,980). However, for most real estate, the value cannot be determined during the audit.

Transfers before filing for bankruptcy are the sixth most common misstatement type. Since assets are likely liquidated during bankruptcy proceedings, some individuals filing for bankruptcy transfer assets to family members or friends to avoid liquidation of the assets to the benefit of the creditors. Auditors find 189 (113) transfers not declared in the filing. Auditors identified those transfers to have a value of $46,925 ($33,224) on average. Those transfers are mainly not declared and not understated. Other personal property like jewelry and other valuable items are also identified during audits. Among Chapter 7 (Chapter 13) bankruptcies, 130 (78) times these items were misstated in the hand-checked cases. Their average reported value was $2,746 ($10,044), while the audit determined value of those items was $14,998 ($41,651), amounting to a misreporting of $12,253 ($31,607) on average.

Table 3 shows mean and median reported and audited values. The median values clearly illustrate that most misstatement categories are skewed. The median values for reported numbers and audit findings are lower than the mean values. The median reported column also clearly illustrates that many misstatements are complete omissions and not underreporting of

asset values.

# 6 Effect of Audits

## 6.1 Effect of Audits - Empirical Strategy

The audit assignment mechanism prescribed by law is a unique feature of this setting. The empirical strategy exploits this mechanism to isolate quasi-exogenous variation in the audit assignment. The first mechanism is that at least one in 250 bankruptcy filings in a federal judicial district shall be randomly selected for audit. The second mechanism prescribes that cases with high income and expense deviation from the district average shall be selected for audits at a higher frequency. The key identification concern is that audit selection is based on unobservable characteristics correlated with examined outcomes biasing the estimated effect of audits.

For a more formal discussion, consider the binary audit treatment ($D \in \{0,1\}$) and potential outcomes $Y(0)$ and $Y(1)$ where $Y = DY(1)+(1-D)Y(0)$, as well as other observable variables $X$. For the random assignment mechanism, it is true by definition that audit treatment and potential outcomes are independent, that is, $(Y(0), Y(1)) \perp D$. Hence, there is no selection issue present.[28] If audit selection stated the mechanism through which a case was selected, the analysis would be straightforward to estimate the average effect of audit selection on outcomes. However, the analysis is complicated by the fact that the selection does not state whether the selection was at random or due to income and expense deviations. Hence, a simple mean comparison will not be informative about the effect of audits on outcomes (effect of D on Y). In particular, we need to argue that conditional on observables treatment assignment is unconfounded, that is, $(Y(0), Y(1)) \perp D|X$.

The argument that selection is only based on observable variables is particularly plausible

[28] Issues typically arising from non-compliance with the treatment are not present because even in the case of non-compliance, auditors submit an audit report. Hence, the audit effect is technically an effect of being selected for audit. I loosely refer to this as the audit effect in the text.

in the examined setting. The assignment mechanism for audit selection is pre-specified by the USTP and specified by the law as follows: "require audits of schedules of income and expenses that reflect greater than average variances from the statistical norm of the district in which the schedules were filed."[29] The remaining identification concern is that among those with high income and expense deviation, the USTP selects those bankruptcy filings that appear particularly suspicious. This concern, however, is alleviated by the following two observations. First, there is no interaction between bankruptcy filers and government staff selecting cases for audits. Hence, soft information gleaned in personal interactions cannot inform audit selection. Indeed, the staff selecting cases for audits is geographically removed from the bankruptcy filers. Audit selection takes place centrally in the Executive Office of the USTP. Oversight over the cases selected for audit is executed at the regional level in the regional offices of the USTP. Second, the staff selecting cases for audit only has the bankruptcy filings as the basis for their decision-making available to them. No additional inputs are available for audit selection. Hence, the information set for the audit selector and econometrician does not differ. Furthermore, I conduct placebo tests examining whether audit selection is correlated with pre-determined case characteristics.

To estimate the effect of audits on case outcomes, I first implement a strategy comparable to semi-saturated regressions by regressing outcome variables on fine buckets of covariates determining selection. This estimation yields a positively weighted average of treatment effects within each of the buckets (Angrist, 1995).

$$Y_i = \alpha + \beta Audit_i + \sum_j \gamma_{jt} ExpenseDeviationBucket_{jt} + \sum_k \delta_{kt} IncomeDeviationBucket_{kt} + \varepsilon_i \quad (1)$$

The coefficient of interest is $\beta$ and represents the effect of audits on the outcome variable. Technically, it is a weighted average of treatment effects for each of the buckets. Section 6.2 wil verify that the estimate of $\beta$ is close to the estimate of the Average Treatment Effect on the Treated (ATT) of audits.

[29] BAPCPA of 2005, Congressional Record Vol 151, No. 44 - Sec. 603 - Audit Procedures

### 6.2 Effect of Audits - Results

This section shows that audit assignment is consistent with the law-prescribed mechanism and that audits can increase the average strictness of bankruptcy relief. First, I validate the empirical strategy showing the effects of audits on pre-determined individual characteristics. By their nature of being pre-determined before audit selection, audits cannot have an effect on these characteristics. Controlling for the audit selecting mechanism, we should expect audited cases and non-audited cases to be similar along those observable characteristics. I present results estimating equation 1 with pre-determined characteristics as outcome variables in Table 4. Panel A shows placebo estimates for Chapter 7 cases. For none of the pre-determined characteristics, differences between audited and non-audited cases are meaningful, controlling for the selection mechanism—neither statistically nor economically. Panel B repeats the exercise for Chapter 13 cases with a substantially similar conclusion.

[Table 4: Placebo]

Next, I re-estimate equation 1 and show that audits can affect debt forgiveness when oversight is low. Table 5 reports the effects of audits on case dismissals across varying specifications. When a case is dismissed, the bankruptcy filer does not obtain debt relief and has to repay their debt while also having incurred the cost of bankruptcy proceedings.

Panel A of Table 5 reports the effects among Chapter 7 cases. When a bankruptcy filing is dismissed, the filers' debt is not forgiven. Hence, collection agencies are still allowed to contact the debtor, and losses for lenders are lower. The filer is not granted a fresh start and has to repay outstanding debts. Among Chapter 7 cases, the likelihood of case dismissal is increased substantially relative to the unconditional probability of 5 percent. The results indicate that case dismissals are more likely for audited cases suggesting that the consequences of misrepresentations and non-compliance outweigh the potential benefits of increased credibility. This result is robust across various specifications accounting for the audit selection mechanism as well as county-year, judge, and trustee-specific factors (columns (1) through (3)). Columns (4)

through (6) repeat the analysis only including cases with low income and expense deviations. Those specifications ensure that the effect is not driven by non-random selection within high expense and income deviation buckets. Hence, the effect is unlikely to be driven by the USTP picking more suspicious cases among high expense and income cases for audit. As the unconditional probability for debt forgiveness is high for Chapter 7 cases, judges seem to screen for negative signals with limited effects of positive signals. Section 8.2 will further corroborate this finding.

[Table 5: Audit Effects]

Panel B reports the effects among Chapter 13 cases. Audit effects for Chapter 13 cases differ from audit effects for Chapter 7 cases. In contrast to Chapter 7 cases, audits are not positively associated with case dismissals among Chapter 13 cases. This finding may be due to the higher level of scrutiny in Chapter 13 filings. On average, 55% of Chapter 13 filings are dismissed, while only 5% of Chapter 7 filings are dismissed, potentially indicating that the added value of debtor audits is limited when scrutiny is already high. Furthermore, high dismissal rates may allow for a greater role of credibility effects, off-setting effects from misstatements and non-compliance. In addition, Chapter 13 filers are more likely to have resources to mitigate the finding of the audit report and may increase the repayment in repayment plans, while Chapter 7 filers may become ineligible for Chapter 7 bankruptcies once more assets or income are discovered.

One concern about debtor audits is their potentially adverse effect on disadvantaged filers, such as filers without attorney representation. Therefore, I modify specification (1) by interacting the audit dummy with a dummy variable indicating self-representation (pro-se) by a bankruptcy filer. I also include the base effects and interact linear controls for (logged) assets, average income, average assets, and a prior bankruptcy dummy with the audit dummy.

Table 6 shows the coefficients for the interaction of self-representation with the audit dummy. Columns (1) through (3) show that audit effects on case dismissals are substantially stronger among self-represented filers unable to afford an attorney. The increase in case dismissals due

to audits is five times larger among self-represented Chapter 7 filers. This observation is particularly concerning as Chapter 7 filers have to pay attorneys in cash and upfront. Otherwise, attorney fees would become part of the forgiven bankruptcy mass. Hence, Chapter 7 filers with pro-se status are more likely to, indeed, be in need of debt forgiveness.

This finding contrasts with the negative estimates among Chapter 13 cases. The reduction in case dismissals for audited pro-se Chapter 13 filings only mitigates (and does not overturn) the positive base effect of self-representation on case dismissals. I attribute the difference in audit effects among pro-se filers across Chapter 7 and Chapter 13 filers to different sophistication levels of Chapter 7/13 pro-se filers as a result of their financial constraints. Chapter 13 bankruptcy filers can frequently finance bankruptcy attorneys through their repayment plan if they do not have the cash on hand to pay the attorney upfront. Hence, self-representation among Chapter 13 cases is less driven by severe financial constraints and more choice by the filer, likely resulting in higher sophistication among Chapter 13 pro-se filers.

[Table 6: Audit Effects Without Attorney Representation]

# 7 Effect of Audits - Subsample Analysis

## 7.1 Subsample Tests - Empirical Strategy

I analyze a subsample of the data to test for changes in case complexity and payments to creditors. Focusing on a subsample of the data allows for collecting additional, more granular case-level information from the court docket text that is not available on a large scale in bankruptcy snapshots. The additional information collected includes the length of the court docket, adjournment of creditor meetings, amendments to the bankruptcy filings, the existence of final reports of distribution to creditors, and modifications to repayment plans. Those outcomes can only be obtained from the case docket directly.

Collecting additional case information beyond publicly available bankruptcy snapshots is costly and often limited to a maximum number of cases when fee exemptions are granted.

Hence, I employ a matching strategy exploiting the audit assignment mechanism to reduce the sample size and make granular comparisons for audited and non-audited individuals along additional dimensions collected from PACER. The strategy targets to recover the Average Treatment Effect on the Treated (ATT) of audits on outcomes such as debt forgiveness, case complexity, and distribution to creditors. In particular, I match each audited case to a non-audited case exactly based on chapter, filing year, filer zip code, attorney representation status (pro-se or not), and the existence of a prior bankruptcy filing. Within those exactly matched characteristics, each audited observation is matched to the nearest non-audited observation without replacement as measured by their expense as long as the difference between the expense is less than $500. As argued in Section 6.1, a matching strategy is particularly suitable in this setting as the determinants of audit selection are prescribed by law, and the empirical patterns shown in Figure 3 are consistent with the mechanism prescribed by law. I choose to match on expense, not income, as income is frequently misstated, as shown in Table 3. This is likely also the reason why matching on expense is sufficient to achieve balance across observable dimensions for audited and non-audited individuals.

Assuming that the audit assignment is random conditional on the matched covariates, a simple regression of outcomes on an audit dummy and constant among matched individuals will recover the ATT:

$$\beta_{ATT} = E[Y(1) - Y(0)|D = 1] \tag{2}$$

### 7.2 Subsample Tests - Results

This section demonstrates that audits lead to increased case complexity regardless of the type of bankruptcy. Increased complexity raises concerns that potential benefits from testing program eligibility may be offset by regressive administrative burden. Audits lead to more docket entries increasing the back-and-forth during bankruptcy proceedings, an increased likelihood of adjourned creditor (341) meetings, and more amendments to bankruptcy schedules. Among Chapter 7 cases, those more complex proceedings result in more case dismissals (less debt for-

giveness) and increased distributions to creditors when the proceedings are successful. Among Chapter 13 cases, audits do not lead, on average, to more case dismissals, and modifications of repayment plans are also limited.

I measure case complexity in three different ways. First, I examine the length of court dockets. The number of docket entries indicates how much back-and-forth the bankruptcy case requires to reach a conclusion. Modifications and corrections of filings, objections to debt forgiveness, rebuttals to objections, judges' decisions, and most case proceedings require court docket entries. Hence, the length of the docket is a comprehensive measure of the complexity of the proceedings. Second, I exploit the adjournment of 341 meetings.[30] Adjournments of creditor meetings are consistent with discrepancies between bankruptcy filing and testimony that need to be resolved before the conclusion of the creditor meeting. More clarifications of discrepancies increase the complexity for filers. Third, I measure whether bankruptcy schedules were amended. Requiring amendments to modify the original filing imposes an additional burden on the bankruptcy filer and, therefore, increases the complexity for the bankruptcy filer.

Table 7 summarizes the results of this analysis for Chapter 7 and Chapter 13 cases. Panel A shows the results among Chapter 7 cases. Column (1) confirms the results of Table 5 indicating that audited cases are more likely to be dismissed and not have their debt forgiven. Relative to the mean rate of case dismissals, this effect is large. However, columns (2) to (4) indicate that potential savings from reduced debt forgiveness come at the expense of increased case complexity and administrative burden during case proceedings. Increased complexity can operate regressively, hindering debt forgiveness for individuals most in need of debt forgiveness. The number of docket entries increases by 20 percent; creditor meetings are more likely to be adjourned; and amendments to the bankruptcy petition increase substantially relative to the mean of amendments. Column (5) indicates that those more complex proceedings are also more likely to have a distribution of funds from liquidated assets to creditors if the bankruptcy proceeding is successful. Hence, even when debt is forgiven, the lilkelihood of distributions to

[30] Morrison et al. (2019) use 341 meeting adjournments in trustee randomization tests to measure trustee strictness.

creditors increases as a result of audits.

Panel B of Table 7 shows results among Chapter 13 cases. Similar to Chapter 7 cases, audited Chapter 13 cases are more complex than their unaudited counterparts. The number of docket entries increase by 10 percent, creditor meetings are more likely to be adjourned, and amendments to the bankruptcy petition increase relative to the mean of amendments. However, at least on average, cost savings from reduced debt forgiveness or increased repayments seem limited for Chapter 13 cases. Subsequent analysis in Section 8.2 will show that the zero effect on repayment plan modifications is due to a large reduction in modifications for debtors not complying with audit requests.[31] This reduction masks the increase in modifications for filers that comply with the audit request.

[Table 7: Subsample Analysis]

# 8 Effect of Audit Findings

## 8.1 Effect of Audit Findings - Empirical Strategy

This section examines the consequences of specific audit findings on bankruptcy outcomes. In contrast to previous sections where the mechanism for treatment assignment is prescribed by law, specific audit findings such as "no misstatement," "misstatement," and "non-compliance" are functions of individual reporting choices and not randomly assigned. This warrants more caution in interpreting the results of the subsequent section. In the ideal experiment, we would randomly—and hidden from the court—audit individuals. To examine the effect of certain audit findings, we would randomly reveal some misstatements to the court and hide other misstatements. To evaluate the effect of a misstatement on bankruptcy outcomes, we would then compare outcomes for the randomly revealed misstatements to cases with misstatements not randomly revealed to the court. Obviously, this design is practically not achievable.

[31] That non-compliant filers do not modify their repayment plans is not surprising as it likely would not result in successful bankruptcy proceedings regardless of the modifications.

I, therefore, resort to an approximation of this experiment by comparing individuals with a specific audit finding to ex-ante observably similar individuals that have not been audited. Cases are exactly matched on the chapter, filing year, filer county, attorney representation status (pro-se or not), and the existence of a prior bankruptcy filing. Within these strata, nearest-neighbor matching is applied to the closest neighbor in terms of living expenses and assets. Distances are weighted by the diagonal of the variance-covariance matrix (Mahalanobis distance). Observations are restricted to matches with a maximum expense deviation of $500. Placebo tests in Appendix Section H show that along observable characteristics, cases with a specific audit finding do not differ from matched non-audited cases in terms of pre-determined unmatched characteristics. If audit finding revelation is random conditional on the matched characteristics, this methodology will recover the ATT of an audit finding on bankruptcy outcomes.

### 8.2 Effect of Audit Findings - Results

This section shows the heterogeneity of audit effects by audit findings. Most strikingly, non-compliance with the audit request is associated with large increases in case dismissals. While misstatements are associated with more case dismissals in Chapter 7 cases, misstatements are not associated with dismissals in Chapter 13 cases. However, a passed audit decreases the likelihood of case dismissals relative to non-audited individuals for Chapter 13 cases, while this is not the case in Chapter 7 cases. Audited individuals complying with the audit take mitigating actions by amending their filings, increasing case complexity. Amendments are more common in cases with material misstatements. Similarly, payments to creditors increase in cases with material misstatements and, to some degree, also in cases without material misstatements (in Chapter 13 cases), potentially as a result of mitigating amendments in anticipation of the audit.

[Table 8: Audit Findings]

Table 8 summarizes the findings comparing cases with the respective audit outcome to non-audited cases. Panel A shows the consequences of audit findings for case dismissals and debt

forgiveness. Panel B shows the consequences of audit findings for mitigating actions by filers and case complexity as measured by amendments to bankruptcy filings. Panel C measures the effects of audit findings on repayments to creditors during bankruptcy, either via the liquidation of assets (Chapter 7) or the modification of repayment plans (in Chapter 13 bankruptcies). Generally, misstatements are associated with more complexity and mitigating actions by filers and greater repayments during bankruptcy. For Chapter 7 cases, misstatements are also associated with a lower success likelihood for the bankruptcy (and debt not being forgiven). For Chapter 13 cases, misstatements are not correlated with more dismissals, potentially due to the mitigating actions and amendments shown in column (4) of Panel B, while these amendments appear less successful in preventing dismissal for Chapter 7 cases.

Even for cases without misstatements, bankruptcy filers amend their petitions. For Chapter 13 cases, these bankruptcy schedule amendments without misstatements also seem to result in repayment plan modifications. However, those costs in the form of more amendments and repayment modifications for the filer come with the benefit of reduced case dismissal rates and increased bankruptcy success. For Chapter 7 cases, having no misstatements and modifications does not appear to affect bankruptcy success or repayments during the liquidation of assets.

## 9 Aggregate Implications

In this section, I investigate the aggregate effects of debtor audits. In particular, I consider counterfactually increasing the audit rate for Chapter 7 cases by one percentage point. Hence, the reported numbers can be thought of as per percentage point effects for various different policies. I concentrate on increasing the rate for Chapter 7 cases as Section 6.2 indicates that Chapter 7 audits are more effective than Chapter 13 audits. The aggregate effect will consist of several components I will compute separately: (1) during debtor audits, material misstatements are found, leading to case dismissals and resulting in lower debt discharged; (2) reduced debt discharge due to withdrawals in response to the audit (direct deterrence); (3) the deterrence effect of an increased audit rate resulting in fewer bankruptcy filings (indirect

deterrence).

**Dollars saved due to misstatement found:** During debtor audits, material misstatements are identified and raised to the court. While the material misstatement in itself is not yet a legal determination, it can influence the discharge decision of the court. In fact, Table 8 Panel A shows that misstatements in Chapter 7 cases are associated with a 4.7-percentage-point increase in the likelihood of case dismissal. Chapter 7 cases with misstatements have an average dischargeable debt balance of $201,782. Approximately 23% of Chapter 7 audits find a material misstatement. Hence, the detection of misstatements would result in debt not being forgiven in the sum $14.2 million per percentage point increase in the audit rate (650,000 * 0.01 (increase in audit rate)* 0.23 (rate of misstatements) * 0.047 (effect of misstatement) * 201,782 not discharged due to misstatements = 61.6 million * 0.23 = 14.2 million).

**Deterrence:** There are two types of deterrence.:(1) Direct deterrence arises from a spike in audit probability when an individual is selected for audit. The audit probability increases from the ex-ante audit prevalence to 1 (2) Indirect deterrence arising from a higher ex-ante probability of audit reducing the number of people filing.

**Direct Deterrence: Dollars not discharged due to unable to complete:** When individuals are selected for audit, they can elect not to submit their materials for the audit, which likely results in the case being dismissed. In those cases, the auditor files a report noting that he is "Unable to Complete" the audit. Of the audited Chapter 7 cases, 3.5% are "Unable to Complete." This finding leads to case dismissal in 36.9% of cases. Hence, in 1.3% of audit cases, the debt is not discharged due to direct deterrence of the audit selection. Per pentage point increase in the audit rate, this results in $13.7 million (650,000 * 0.01 (increase in audit rate)* 0.035 (rate of unable to complete) * 0.369 (effect of unable to complete) * 163,292 (discharge debt of unable to complete)) of debt not being discharged due to this direct deterrence when people are not expecting an audit (due to low ex-ante probability) are selected for an audit. Hence, lenders save about $13.7 million in non-discharged debt due to this direct deterrence.

**Indirect Deterrence: Higher ex-ante audit probability:** Indirect deterrence arises from

the higher ex-ante probability of audits and not filing for bankruptcy due to the audit probability in the first place. We can exploit the observed direct deterrence to compute the deterrence effects of a higher ex-ante audit probability.

The basic idea is to draw an equivalence between individuals' decision to (not) file for bankruptcy under a higher audit rate and individuals' decision to abandon their bankruptcy filing by not submitting materials for the audit. The decision to file under a low audit rate but abandon under a high audit rate (when selected) is observable in the data. Hence, drawing an equivalence to not submitting a bankruptcy filing in the first place will allow computing the share of bankruptcy filers that would not submit a filing if the audit rate were to be increased. Fewer filings then result in less debt forgiven and a lower financial burden on lenders unable to collect on forgiven debts. Appendix Section E shows the computations in a model building on Gross et al. (2021).

In the data, the share of withdrawals among Chapter 7 bankruptcies is 0.011 (basically unable to complete rate of audits times dismissal when unable to complete). Since we examine a counterfactual increase in audit probabilities of 0.01 instead of an increase of close to 1 (0.996), I approximate the deterrence effect by scaling the withdrawal by 0.01. Multiplying the deterrence effect by the number of filings per year and the average dischargeable debt of $163,292 among Chapter 7 cases unable to complete audits, the deterrence effect of a one-percentage-point increase in audit probability prevents the discharge of another $13.7 million.

**Cost of one-percentage-point increase in audit rate**: Approximately 650,000 individuals file for Chapter 7 bankruptcy every year. A one-percentage-point increase in the audit rate would mean auditing 6,500 more bankruptcy filings per year. At an estimated cost of $1,000 per audit, an increase in the number of individuals audited by 6,500 per year would result in a direct audit cost of $6.5 million per year. Additional cost components that could be considered are (1) an increase in proceeding duration and (2) the direct costs of providing the documents to be audited.

**Net benefits**: All in all, the cost of increasing the audit rate for Chapter 7 bankruptcies

will likely be offset by the increase in case dismissal due to material misstatements found. Hence, any potential benefit from direct and indirect deterrence of an increased audit rate will only add to the net benefit of increasing the audit rate. Furthermore, one could consider a more targeted approach for audit selection. The previous analysis suggests that audits are particularly effective for Chapter 7 cases with high expense deviation from the district average. Hence, one could focus the audit increase on high-expense filings to increase their effectiveness.

Two caveats apply to the analysis: First, while debtors are liable for debt not discharged, and debt not discharged can be collected on, the savings for lenders depend on the additional dollar amount lenders can collect outside of bankruptcy, as compared to in the bankruptcy. If debtors were to enter perpetual default (informal bankruptcy), lenders would not be able to collect on owed debts. However, lenders only need to be able to collect 15% (=6.5/41.6) more of dischargeable debt to break-even on an audit investment. Second, the above analysis does not consider changes in consumer surplus from case dismissals. Regarding consumer surplus, we should note that the marginal individual is indifferent between filing and not filing for bankruptcy. Hence, simply moving this individual out of bankruptcy will not affect the surplus for the marginal individual but will eliminate the monetary externality imposed on the bankruptcy system when filing for bankruptcy. Besides, dismissal decisions for audited cases are based on a greater information set, likely improving decision quality with respect to who should and who should not receive debt forgiveness.

## 10 Long-term Consequences

### 10.1 Data and Design

The previous section shows potentially large reductions in debt forgiveness if the Chapter 7 audit rate was increased. A majority of those reductions is due to deterrence. However, long-term deteriorations in financial health for audited individuals and, in particular, deterred unsophisticated filers may present an additional cost of increased audit rates. If individuals pushed

out of bankruptcy and not having their debt forgiven are substantially worse off in the years following their bankruptcy filing, it is questionable whether not forgiving debt was the correct decision. This section investigates audit consequences for financial well-being over time.

I merge the samples described in section 7.1 and 8.1 to consumer credit records provided by TransUnion using individual-level information collected from court dockets. Specifically, I provide TransUnion with information on bankruptcy filers' names, addresses, and the last four digits of their social security number to identify respective consumer credit snapshots for each individual. The match rate using individual-level information is approximately 95% and substantially improves on matches utilizing only court docket numbers. I observe six snapshots of credit information for each individual. The snapshots are taken in June of every second year from 2013 to 2023. This choice of snapshot dates allows observing outcomes four to six years after the audit for virtually all audited cases (2007 to 2019) and also allows observing a pre-period for a large share of audited bankruptcy cases and individuals.

I run variations of the following two-way fixed effect specification to examine the effect of audits and their findings on individual-level outcomes.

$$Y_{it} = \beta Post_{it} \times Audit_i + \mu_t + \delta_i + \gamma_{t-j} + \varepsilon_{it} \tag{3}$$

where $\mu_t$ is a snapshot fixed effect, $\delta_i$ is an individual fixed effect, and $\gamma_{t-j}$ is a relative time fixed effect of snapshot time t relative to the year of bankruptcy j for individual i. The coefficient of interest is $\beta$, identifying the ATT of audits under homogeneous treatment and parallel trends assumptions.

Exploiting the time dimension of the snapshots for a subsample of the data also allows the inclusion of individual fixed effects that control for any time-invariant individual-level characteristics across audited and non-audited individuals. Hence, when replacing the audit dummy with a dummy for the audit finding, any concerns regarding fundamental differences across non-audited individuals and individuals with a specific audit finding are less severe. Instead of treatment assignment conditionally random with respect to potential outcomes, we only re-

quire treatment assignment to be conditionally random with respect to the trends of potential outcomes for the identification of the parameter of interest.

### 10.2 Results

This section shows that the average audit effects on credit access as measured by the credit score are small and short-lived (Chapter 7) or virtually zero (Chapter 13). This indicates only a limited cost of increased case dismissals and case modifications on average. However, I find that the negative consequences of audit findings for Chapter 7 cases are concentrated in more disadvantaged groups. These findings confirm concerns about the deterrence of individuals unable to afford an attorney and the identification of misstatements worsening relief allocation among individuals with low living expenses.

First, I run specification (3) with credit scores as the outcome variable. Table 9 Column (1) shows a reduction in credit scores of 3.5 points among Chapter 7 cases. This is a small effect, for example, in comparison to the removal of bankruptcy flags—an indicator on credit records showing past bankruptcies and removed after 7 to 10 years—with an effect of 17 points (Jansen et al., 2022). Column (3) even shows a point estimate close to zero for the effect of bankruptcy audits on credit scores among Chapter 13 cases. Figure 4 shows dynamic versions of Table 9 columns (1) and (3). The figures show no diverging pre-trends and confirm the findings of the table. Further, Panel A shows that the negative effect of audits on credit scores is very transient and concentrated in the year of and the year subsequent to the audit. The credit score drops by 6 points and already recovers two to three years after the bankruptcy.

Next, I examine whether the consequences for credit access are more pronounced among individuals without an attorney. Columns (2) and (4) modify specification (3) by interacting the audit and post dummies with a dummy for self-representation by the bankruptcy filer. The base effects are absorbed in the fixed effect structure. Column (2) indicates that, indeed, the negative consequences for credit access are more pronounced among Chapter 7 filers without attorney representation, confirming concerns that audits exacerbate outcomes for disadvan-

taged groups. Column (4) shows a larger point estimate among Chapter 13 cases. However, the coefficient is not statistically significant at conventional levels.

[Table 9: Audit on Credit Access by Pro-Se]

[Figure 4: Credit Access Audit]

A key risk of deterrence is that individuals most in need are deterred from seeking or obtaining bankruptcy relief. This risk is heightened for individuals unable to afford an attorney. Compliance with audit requests may be particularly challenging without attorney representation. I, therefore, investigate whether credit access deteriorates for individuals not complying with the audit request and not having attorney representation (pro-se). If these were egregious misreporters not in need of debt forgiveness, we should not expect a deterioration of credit access relative to their peers. I test for deteriorating credit access within the sample of unable-to-complete bankruptcy filers relative to the matched sample of Chapter 7 bankruptcy filers. I modify specification (3) interacting the treatment (unable to complete) with relative time dummies (excluding one two-year pre-treatment period) and further interact the relative time dummies and treatment with the pro-se status of a bankruptcy filer. Figure 5 Panel A plots the coefficients on the triple interaction terms. I find that, indeed, deterred individuals without attorney representation have substantially worse credit access. Credit scores persistently decline by 50 points more for deterred individuals relative to matched non-audited individuals among self-represented filers, relative to the same difference among filers with attorney representation.

A risk of audits is that identified misstatements lead to dismissals for individuals in need of forgiveness. For example, a small misstatement by a filer with low income and low living expenses may result in a dismissal even though the filer has an overwhelming debt burden. Hence, when identifying misrepresentations, audits could lead to "false" dismissals. If misrepresentations lead to false dismissals, we should see persistently worse credit access for filers with misrepresentations and low living expenses. I modify specification (3) interacting the

treatment (misstatement) with relative time dummies (excluding one two-year pre-treatment period) and further interact the relative time dummies and treatment with a dummy indicating below median living expense of a bankruptcy filer. Figure 5 Panel B plots the coefficients on the triple interaction terms. It shows that the identification of misstatements has more negative consequences for filers with low living expenses. This finding is consistent with "false" dismissals leading to worse outcomes for disadvantaged groups with errors in their bankruptcy filings. However, these negative consequences are short-lived and concentrated in the year of and year after the bankruptcy filing. Thereafter, the effect is still somewhat economically meaningful with 10 credit score points but not statistically distinguishable from zero.

[Figure 5: Credit Access Disadvantaged Groups]

## 11 Conclusion

While audits do not reduce debt forgiveness in otherwise highly scrutinized cases (Chapter 13), audits reduce debt forgiveness when alternative oversight is low (Chapter 7). However, reduced debt forgiveness comes at the cost of increased case complexity. Beyond changes in average debt forgiveness, audits also drive a reallocation of debt relief from non-compliers (with the audit request) and misreporters to truthful reporters. On average, negative consequences of the audit for financial health are small and short-lived. However, negative consequences for financial health are concentrated in disadvantaged filers without attorney representation. Non-compliant Chapter 7 filers without attorney representation experience decreased financial health four to five years after their bankruptcy filing. In aggregate, I find that increasing the audit rate for Chapter 7 bankruptcies likely results in a large reduction in debt forgiven relative to the direct cost of conducting those audits. Reductions in debt forgiveness due to deterrence are approximately two times larger than reductions from identifying misrepresentations.

## References


Abadie, A. and Spiess, J. (2022), 'Robust post-matching inference', *Journal of the American Statistical Association* **117**(538), 983–995.

Abeler, J., Nosenzo, D. and Raymond, C. (2019), 'Preferences for truth-telling', *Econometrica* **87**(4), 1115–1153.

Adelino, M., Ferreira, M. A. and Oliveira, M. (2024), 'The heterogeneous effects of household debt relief', *Available at SSRN* .

Advani, A., Elming, W. and Shaw, J. (2021), 'The dynamic effects of tax audits', *Review of Economics and Statistics* pp. 1–45.

Ambrose, B. W., Conklin, J. and Yoshida, J. (2016), 'Credit rationing, income exaggeration, and adverse selection in the mortgage market', *The Journal of Finance* **71**(6), 2637–2686.

Angrist, J. (1995), 'Estimating the labor market impact of voluntary military service using social security data on military applicants'.

Antill, S. (2021), 'Are bankruptcy professional fees excessively high?', *Available at SSRN 3554835* .

Argyle, B., Indarte, S., Iverson, B. and Palmer, C. (2022), 'Explaining racial disparities in personal bankruptcy outcomes'.

Armstrong, C., Kepler, J. D., Samuels, D. and Taylor, D. (2022), 'Causality redux: The evolution of empirical methods in accounting research and the growth of quasi-experiments', *Journal of Accounting and Economics* p. 101521.

Becker, G. S. (1968), Crime and punishment: An economic approach, *in* 'The economic dimensions of crime', Springer, pp. 13–68.

Blackwell, D. W., Noland, T. R. and Winters, D. B. (1998), 'The value of auditor assurance: Evidence from loan pricing', *Journal of accounting research* **36**(1), 57–70.

Boning, W. C., Hendren, N., Sprung-Keyser, B. and Stuart, E. (2023), A welfare analysis of tax audits across the income distribution, Technical report, National Bureau of Economic Research.

Bourveau, T., Breuer, M., Koenraadt, J. and Stoumbos, R. (2021), 'Public company auditing around the securities exchange act', *Columbia Business School Research Paper* .

Breuer, M. (2021), 'How does financial-reporting regulation affect industry-wide resource allocation?', *Journal of Accounting Research* **59**(1), 59–110.

Breuer, M., Le, A. and Vetter, F. (2023), 'Audit mandates, audit firms, and auditors', *Audit Firms, and Auditors (September 12, 2023)* .

Brown, M., Chakrabarti, R. and Severino, F. (2024), Personal bankruptcy protection and household debt, Technical report.

Catherine, S. and Yannelis, C. (2023), 'The distributional effects of student loan forgiveness', *Journal of Financial Economics* **147**(2), 297–316.

Chakrabarti, R. and Pattison, N. (2019), 'Auto credit and the 2005 bankruptcy reform: The impact of eliminating cramdowns', *The Review of Financial Studies* **32**(12), 4734–4766.

Cook, J., Kowaleski, Z. T., Minnis, M., Sutherland, A. and Zehms, K. M. (2020), 'Auditors are known by the companies they keep', *Journal of Accounting and Economics* **70**(1), 101314.

Dávila, E. (2020), 'Using elasticities to derive optimal bankruptcy exemptions', *The Review of*

*Economic Studies* **87**(2), 870–913.

DeFond, M. L. and Jiambalvo, J. (1991), 'Incidence and circumstances of accounting errors', *Accounting review* pp. 643–655.

DeFond, M. and Zhang, J. (2014), 'A review of archival auditing research', *Journal of accounting and economics* **58**(2-3), 275–326.

Dobbie, W., Goldsmith-Pinkham, P., Mahoney, N. and Song, J. (2020), 'Bad credit, no problem? credit and labor market consequences of bad credit reports', *The Journal of Finance* **75**(5), 2377–2419.

Dobbie, W., Goldsmith-Pinkham, P. and Yang, C. S. (2017), 'Consumer bankruptcy and financial health', *Review of Economics and Statistics* **99**(5), 853–869.

Dobbie, W. and Song, J. (2015), 'Debt relief and debtor outcomes: Measuring the effects of consumer bankruptcy protection', *American economic review* **105**(3), 1272–1311.

Duflo, E., Greenstone, M., Pande, R. and Ryan, N. (2013), 'Truth-telling by third-party auditors and the response of polluting firms: Experimental evidence from india', *The Quarterly Journal of Economics* **128**(4), 1499–1545.

Duflo, E., Greenstone, M., Pande, R. and Ryan, N. (2018), 'The value of regulatory discretion: Estimates from environmental inspections in india', *Econometrica* **86**(6), 2123–2160.

Dyck, A., Morse, A. and Zingales, L. (2010), 'Who blows the whistle on corporate fraud?', *The journal of finance* **65**(6), 2213–2253.

Dyck, A., Morse, A. and Zingales, L. (2023), 'How pervasive is corporate fraud?', *Review of Accounting Studies* pp. 1–34.

Egan, M., Matvos, G. and Seru, A. (2019), 'The market for financial adviser misconduct', *Journal of Political Economy* **127**(1), 233–295.

Finkelstein, A. and Notowidigdo, M. J. (2019), 'Take-up and targeting: Experimental evidence from snap', *The Quarterly Journal of Economics* **134**(3), 1505–1556.

Flynn, E. (2015), 'Consumer debtor audits', *American Bankruptcy Institute Journal* **34**(3), 46–47,57.

Ganong, P. and Noel, P. (2023), 'Why do borrowers default on mortgages?', *The Quarterly Journal of Economics* **138**(2), 1001–1065.

Garmaise, M. J. (2015), 'Borrower misreporting and loan performance', *The Journal of Finance* **70**(1), 449–484.

Gipper, B., Leuz, C. and Maffett, M. (2020), 'Public oversight and reporting credibility: Evidence from the pcaob audit inspection regime', *The Review of Financial Studies* **33**(10), 4532–4579.

Griffin, J. M. (2021), 'Ten years of evidence: Was fraud a force in the financial crisis?', *Journal of Economic Literature* **59**(4), 1293–1321.

Griffin, J. M., Kruger, S. and Mahajan, P. (2023), 'Did fintech lenders facilitate ppp fraud?', *The Journal of Finance* **78**(3), 1777–1827.

Griffin, J. M. and Maturana, G. (2016), 'Who facilitated misreporting in securitized loans?', *The Review of Financial Studies* **29**(2), 384–419.

Gross, T., Kluender, R., Liu, F., Notowidigdo, M. J. and Wang, J. (2021), 'The economic consequences of bankruptcy reform', *American Economic Review* **111**(7), 2309–41.

Gyongyosi, G. and Verner, E. (2024), 'Household debt relief and the debt laffer curve', *Available*

*at SSRN 4897897* .

Indarte, S. (2020), 'Moral hazard versus liquidity in household bankruptcy', *Available at SSRN 3378669* .

Indarte, S. and Kanz, M. (2024), 'Debt relief for households in developing economies', *Oxford Review of Economic Policy* **40**(1), 139–159.

Jansen, M., Nagel, F., Zhang, A. L. and Yannelis, C. (2022), 'Data and welfare in credit markets', *University of Chicago, Becker Friedman Institute for Economics Working Paper* (2022-88).

Jensen, M. and Meckling, W. H. (1976), 'Theory of the firm: Managerial behavior, agency costs and ownership structure', *Journal of Financial Economics* **3**(4), 305–360.

Jiang, W., Nelson, A. A. and Vytlacil, E. (2014), 'Liar's loan? effects of origination channel and information falsification on mortgage delinquency', *Review of Economics and Statistics* **96**(1), 1–18.

Kausar, A., Shroff, N. and White, H. (2016), 'Real effects of the audit choice', *Journal of Accounting and Economics* **62**(1), 157–181.

Kleven, H. J., Knudsen, M. B., Kreiner, C. T., Pedersen, S. and Saez, E. (2011), 'Unwilling or unable to cheat? evidence from a tax audit experiment in denmark', *Econometrica* **79**(3), 651–692.

Kluender, R., Mahoney, N., Wong, F. and Yin, W. (2024), The effects of medical debt relief: Evidence from two randomized experiments, Technical report, National Bureau of Economic Research.

Lee, B. J. (2023), 'Bankruptcy lawyers and credit recovery', *Available at SSRN 4649915* .

Lennox, C. S. and Pittman, J. A. (2011), 'Voluntary audits versus mandatory audits', *The accounting review* **86**(5), 1655–1678.

Leuz, C., Triantis, A. and Wang, T. Y. (2008), 'Why do firms go dark? causes and economic consequences of voluntary sec deregistrations', *Journal of Accounting and Economics* **45**(2-3), 181–208.

Livshits, I., MacGee, J. and Tertilt, M. (2007), 'Consumer bankruptcy: A fresh start', *American Economic Review* **97**(1), 402–418.

Mian, A. and Sufi, A. (2017), 'Fraudulent income overstatement on mortgage applications during the credit expansion of 2002 to 2005', *The Review of Financial Studies* **30**(6), 1832–1864.

Mian, A. and Sufi, A. (2022), 'Credit supply and housing speculation', *The Review of Financial Studies* **35**(2), 680–719.

Mikhed, V., Raina, S., Scholnick, B. and Zhang, M. (2024), 'Debtor income manipulation in consumer credit contracts', *Journal of Financial Economics* **157**, 103851.

Minnis, M. (2011), 'The value of financial statement verification in debt financing: Evidence from private us firms', *Journal of accounting research* **49**(2), 457–506.

Minnis, M. and Shroff, N. (2017), 'Why regulate private firm disclosure and auditing?', *Accounting and Business Research* **47**(5), 473–502.

Morrison, E. R., Pang, B. and Zytnick, J. (2019), 'Manipulating random assignment: Evidence from consumer bankruptcies in the nation's largest cities', *Columbia Law and Economics Working Paper* (614).

Myers, S. C. (1977), 'Determinants of corporate borrowing', *Journal of financial economics*

**5**(2), 147–175.

Piskorski, T., Seru, A. and Witkin, J. (2015), 'Asset quality misrepresentation by financial intermediaries: Evidence from the rmbs market', *The Journal of Finance* **70**(6), 2635–2678.

Romeo, C. and Sandler, R. (2023), 'The effect of bankruptcy exemptions on consumer credit', *The Journal of Law and Economics* **66**(4), 699–737.

Shi, M. (2022), 'Monitoring for waste: Evidence from medicare audits', *URL https://mshi311. github. io/website2/Shi_MedicareAudits_2022_09_15. pdf* .

Simnett, R., Vanstraelen, A. and Chua, W. F. (2009), 'Assurance on sustainability reports: An international comparison', *The accounting review* **84**(3), 937–967.

Slemrod, J. and Yitzhaki, S. (2002), Tax avoidance, evasion, and administration, *in* 'Handbook of public economics', Vol. 3, Elsevier, pp. 1423–1470.

Spence, M. (1976), 'Informational aspects of market structure: An introduction', *The Quarterly Journal of Economics* pp. 591–597.

Teoh, S. H. and Wong, T. J. (1993), 'Perceived auditor quality and the earnings response coefficient', *Accounting review* pp. 346–366.

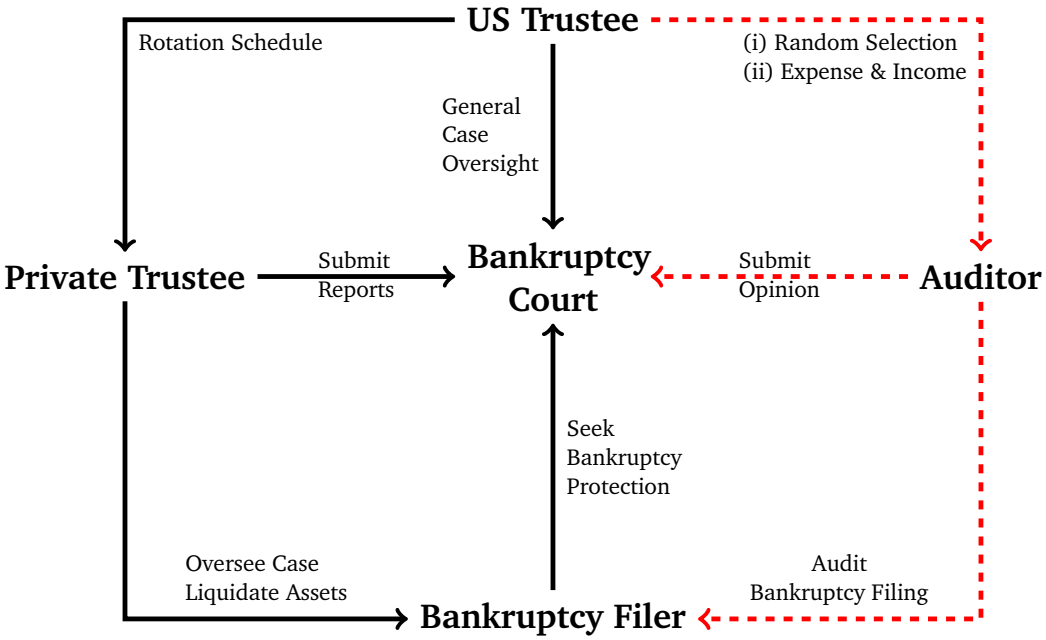
Townsend, R. M. (1979), 'Optimal contracts and competitive markets with costly state verification', *Journal of Economic theory* **21**(2), 265–293.

Wang, J., Yang, J., Iverson, B. C. and Kluender, R. (2020), 'Bankruptcy and the covid-19 crisis', *Available at SSRN 3690398* .

Zakolyukina, A. A. (2018), 'How common are intentional gaap violations? estimates from a dynamic model', *Journal of Accounting Research* **56**(1), 5–44.

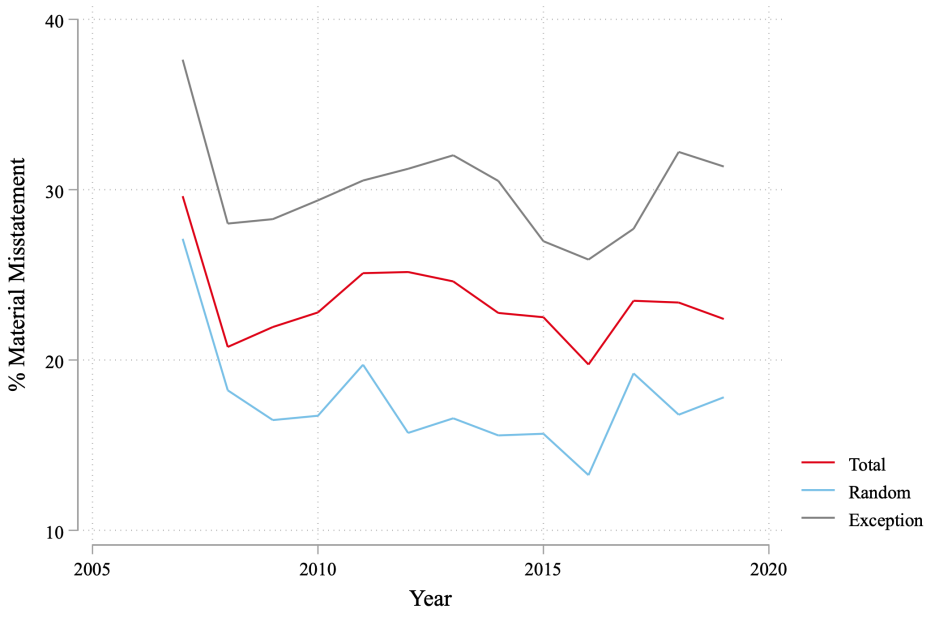
## Figure 1: Setting Summary

Figure 1 shows a stylized summary of the setting. The US Trustee contracts an auditor to complete an audit for selected bankruptcy cases. Case selection is based on randomness and an expense and income deviation from the district average. The auditor examines the bankruptcy filing and submits an audit opinion to the court. The red arrows are the focus of this paper. Regular oversight exercised for all filings is illustrated using black arrows. A private trustee is assigned to each bankruptcy filing from a panel of trustees based on a rotation schedule. The private trustee finds and liquidates the assets of the bankruptcy filer and ensures compliance with the bankruptcy law.



## Figure 2: Aggregate Misstatement and Unable to Complete

Figure 2 shows aggregate rates of misstatements and share of audits that could not be completed. The figures are based on aggregate audit statistics collected from the public reports on Debtor Audits by the United States Trustee Program, also shown in Table 1. The red line (total) shows the rate of misstatements (Panel A) and the rate of non-completed audits (Panel B) across all cases selected for audit. The blue line (random) shows the rate of misstatements (Panel A) and rate of non-completed audits (Panel B) among cases randomly selected for audit. The grey line (exception) shows the rate of misstatements (Panel A) and rate of non-completed audits (Panel B) among cases selected for audit due to income and expense deviation from the district average in the bankruptcy filing.



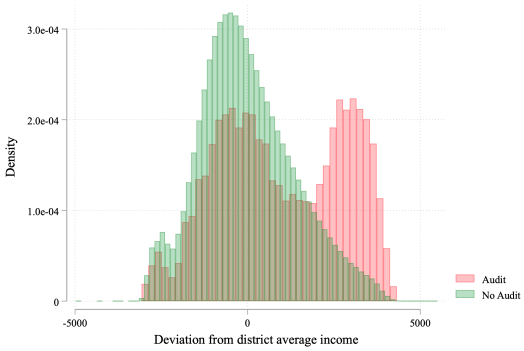

**(a) Material Misstatement**



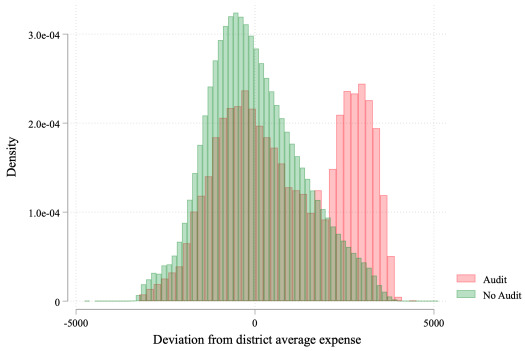

**(b) Unable to Complete**

## Figure 3: Deviations from District Average Income/Expense by Audit Status

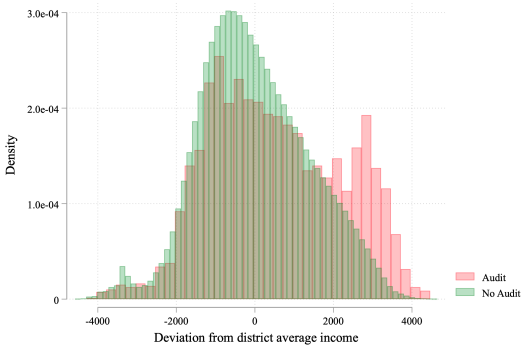
The primary determinant of exception audits is the deviation of a filer's income and expense from the average income and expense of filers in a given district. Figure 3 illustrates that audit selection adheres to this selection rule. Panel A shows histograms of the deviation of incomes from the average in the filing district among audited and non-audited individuals among Chapter 7 cases. Panel B shows the same deviation for expenses among Chapter 7 cases. Panels C and D repeat the exercise for Chapter 13 cases.



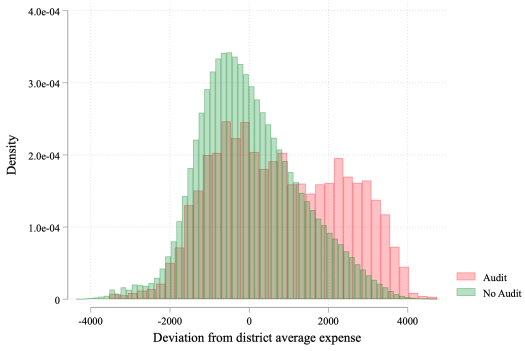

**(a) Chapter 7: Average income**




**(b) Chapter 7: Average expense**



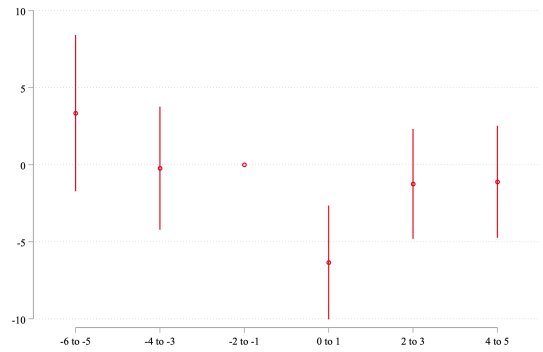

**(c) Chapter 13: Average income**




**(d) Chapter 13: Average expense**

## Figure 4: Credit Access Audit

Figure 4 shows estimation results for variations of specification (3) for matched samples of audited and non-audited cases merged to consumer credit records. The outcome variable is credit access as measured by credit scores. Specifically, I estimate $Y_{it} = \sum_{k\in\{-6,-4,0,2,4\}} \beta_k 1[t-j \in [k,k+1]] Audit_i + \mu_t + \delta_i + \gamma_{t-j} + \varepsilon_{it}$ leaving out relative years -2 and -1 as comparison period. I plot the $\beta_k$ coefficients and their associated 95% confidence bands. $\mu_t$ are snapshot fixed effects, $\delta_i$ are individual fixed effects, $\gamma_{t-j}$ are relative time fixed effects. Audited and non-audited observations are exactly matched on the chapter, filing year, filer zip code, attorney representation status (pro-se or not), and the existence of a prior bankruptcy filing. Within those categories, the matched observation is determined by the nearest expense within $500 as the legislation prescribes matching based on expense and income - and income is frequently misstated. The matching is without replacement. Following Abadie and Spiess (2022), standard errors are clustered at the matched pair level. t-statistics in parentheses. $^{*}$ $p<0.1$, $^{**}$ $p<0.05$, $^{***}$ $p<0.01$ Source: TransUnion



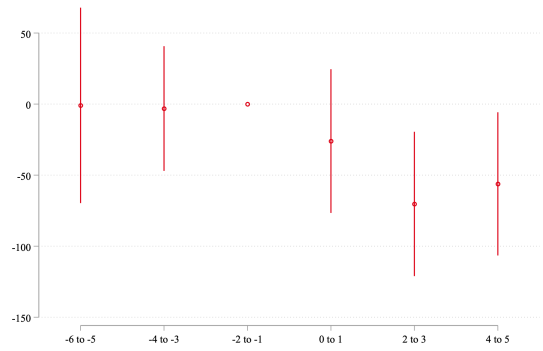

**(a) Audit: Chapter 7**



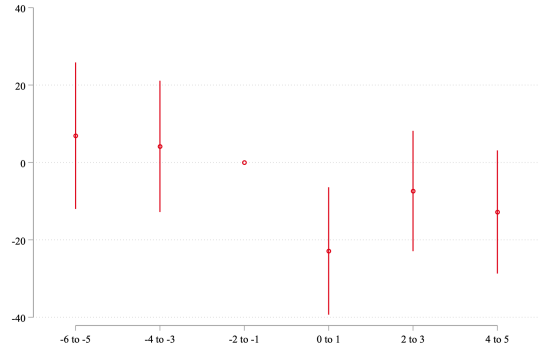

**(b) Audit: Chapter 13**

## Figure 5: Credit Access Disadvantaged Groups

Figure 5 Panel A plots the incremental effect of deterrence in pro-se filers relative to filers with attorney over time (in Chapter 7 cases). Specifically, I estimate $Y_{it} = \sum_{k\in\{-6,-4,0,2,4\}} \beta_k 1[t-j \in [k,k+1]] \times ProSe_i \times Unable_i + \mu_t + \delta_i + \gamma_{t-j,ProSe} + \varepsilon_{it}$ leaving out relative years -2 and -1 as comparison period. I plot the $\beta_k$ coefficients and their associated 95% confidence bands. $\mu_t$ are snapshot fixed effects, $\delta_i$ are individual fixed effects, $\gamma_{t-j,ProSe}$ are relative time by pro-se status fixed effects to account for additional base effects. The sample is a matched sample of unable to complete and non-audited cases merged to consumer credit records. The outcome variable is credit access as measured by credit scores. Cases are exactly matched on the chapter, filing year, filer county, attorney representation status (pro-se or not), and the existence of a prior bankruptcy filing. Within these strata, nearest-neighbor matching is applied to the closest neighbor in terms of living expenses and assets. Distances are weighted by the diagonal of the variance-covariance matrix (Mahalanobis distance). Observations are restricted to matches with a maximum expense deviation of $500. Figure 5 Panel B plots the incremental effect of misstatements in low-expense (below median) filers relative to filers with high expense over time (in Chapter 7 cases). Specifically, I estimate $Y_{it} = \sum_{k\in\{-6,-4,0,2,4\}} \beta_k 1[t-j \in [k,k+1]] \times LowExpense_i \times Misstatement_i + \mu_t + \delta_i + \gamma_{t-j,LowExpense} + \varepsilon_{it}$ leaving out relative years -2 and -1 as comparison period. I plot the $\beta_k$ coefficients and their associated 95% confidence bands. $\mu_t$ are snapshot fixed effects, $\delta_i$ are individual fixed effects, $\gamma_{t-j,LowExpense}$ are relative time by low-expense status fixed effects to account for additional base effects. The sample is a matched sample of misstatement and non-audited cases merged to consumer credit records. The outcome variable is credit access as measured by credit scores. Cases are exactly matched on the chapter, filing year, filer county, attorney representation status (pro-se or not), and the existence of a prior bankruptcy filing. Within these strata, nearest-neighbor matching is applied to the closest neighbor in terms of living expenses and assets. Distances are weighted by the diagonal of the variance-covariance matrix (Mahalanobis distance). Observations are restricted to matches with a maximum expense deviation of $500. Following Abadie and Spiess (2022) standard errors are clustered at the matched pair level. t-statistics in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$ Source: TransUnion




**(a) Unable Pro Se: Chapter 7**




**(b) Misstatement Low Expense: Chapter 7**

# Table 1: Aggregate Audit Numbers

Table 1 shows aggregate audit statistics collected from the public reports on Debtor Audits by the United States Trustee Program. Assignment shows the audit selection mechanism, either random audit selection or exception audits due to income and expense deviations. Cases Designated shows the number of cases selected. No Report shows the number of cases without an audit report. With Report shows the number of cases with audit reports. Report of No Audit shows the number of cases where auditors submitted a report that no audit could be completed. Report of Audit Filed shows the number of cases with audit reports. No Material Misstatement shows the number of cases without material misstatements. % No Material Misstatements shows the percentage of reports without material misstatements. Material Misstatements > 0 shows the number of reports with material misstatements. % Material Misstatements > 0 shows the percentage of reports with material misstatements.

| Year | Assignment | Cases Designated | No Report | With Report | Report of No Audit | Report of Audit Filed | No Material Misstatements | Material Misstatement > 0 | % Material Misstatement > 0 |
|---|---|---|---|---|---|---|---|---|---|
| 2007 | Total | 4095 | 146 | 3949 | 367 | 3582 | 2521 | 1061 | 30 |
| 2007 | Random | 3161 | 145 | 3016 | 287 | 2729 | 1989 | 740 | 27 |
| 2007 | Exception | 934 | 1 | 933 | 80 | 853 | 532 | 321 | 38 |
| 2008 | Total | 1691 | 306 | 1385 | 109 | 1276 | 1011 | 265 | 21 |
| 2008 | Random | 1177 | 152 | 1025 | 81 | 944 | 772 | 172 | 18 |
| 2008 | Exception | 514 | 154 | 360 | 28 | 332 | 239 | 93 | 28 |
| 2009 | Total | 2405 | 50 | 2355 | 94 | 2261 | 1765 | 496 | 22 |
| 2009 | Random | 1299 | 39 | 1260 | 46 | 1214 | 1014 | 200 | 16 |
| 2009 | Exception | 1106 | 11 | 1095 | 48 | 1047 | 751 | 296 | 28 |
| 2010 | Total | 2729 | 54 | 2675 | 113 | 2562 | 1978 | 584 | 23 |
| 2010 | Random | 1444 | 49 | 1395 | 62 | 1333 | 1110 | 223 | 17 |
| 2010 | Exception | 1285 | 5 | 1280 | 51 | 1229 | 868 | 361 | 29 |
| 2011 | Total | 1077 | 22 | 1055 | 47 | 1008 | 755 | 253 | 25 |
| 2011 | Random | 555 | 22 | 533 | 26 | 507 | 407 | 100 | 20 |
| 2011 | Exception | 522 | 0 | 522 | 21 | 501 | 348 | 153 | 31 |
| 2012 | Total | 1480 | 52 | 1428 | 77 | 1351 | 1011 | 340 | 25 |
| 2012 | Random | 600 | 35 | 565 | 37 | 528 | 445 | 83 | 16 |
| 2012 | Exception | 880 | 17 | 863 | 40 | 823 | 566 | 257 | 31 |
| 2013 | Total | 426 | 7 | 419 | 29 | 390 | 294 | 96 | 25 |
| 2013 | Random | 210 | 7 | 203 | 16 | 187 | 156 | 31 | 17 |
| 2013 | Exception | 216 | 0 | 216 | 13 | 203 | 138 | 65 | 32 |
| 2014 | Total | 1627 | 53 | 1574 | 76 | 1498 | 1157 | 341 | 23 |
| 2014 | Random | 857 | 39 | 818 | 41 | 777 | 656 | 121 | 16 |
| 2014 | Exception | 770 | 14 | 756 | 35 | 721 | 501 | 220 | 31 |
| 2015 | Total | 2897 | 105 | 2792 | 158 | 2634 | 2041 | 593 | 23 |
| 2015 | Random | 1174 | 60 | 1114 | 74 | 1040 | 877 | 163 | 16 |
| 2015 | Exception | 1723 | 45 | 1678 | 84 | 1594 | 1164 | 430 | 27 |
| 2016 | Total | 829 | 27 | 802 | 42 | 760 | 610 | 150 | 20 |
| 2016 | Random | 413 | 23 | 390 | 20 | 370 | 321 | 49 | 13 |
| 2016 | Exception | 416 | 4 | 412 | 22 | 412 | 390 | 101 | 26 |
| 2017 | Total | 1013 | 40 | 973 | 53 | 920 | 704 | 216 | 23 |
| 2017 | Random | 519 | 34 | 485 | 27 | 458 | 370 | 88 | 19 |
| 2017 | Exception | 494 | 6 | 488 | 26 | 462 | 334 | 128 | 28 |
| 2018 | Total | 2070 | 56 | 2014 | 123 | 1891 | 1449 | 442 | 23 |
| 2018 | Random | 1207 | 54 | 1153 | 69 | 1084 | 902 | 182 | 17 |
| 2018 | Exception | 863 | 2 | 861 | 54 | 807 | 547 | 260 | 32 |
| 2019 | Total | 2713 | 84 | 2629 | 139 | 2490 | 1932 | 558 | 22 |
| 2019 | Random | 1825 | 81 | 1744 | 99 | 1645 | 1352 | 293 | 18 |
| 2019 | Exception | 888 | 3 | 885 | 40 | 845 | 580 | 265 | 31 |

## Table 2: Summary Statistics

Table 2 shows summary statistics for consumer bankruptcy filings. Panel A shows summary statistics for Chapter 7 cases. In Chapter 7 cases, non-exempt assets are liquidated. Panel B shows summary statistics for Chapter 13 cases. In Chapter 13 cases, more massets are protected, and bankruptcy filers complete a debt repayment plan. Total assets are total assets as indicated on the bankruptcy filing. Current income is the current income declared on the bankruptcy filing and will primarily consist of wages. Average income provides the average over the past six months. Expenses are living expenses such as rent, utilities, and the cost of dependents. The average expense shows the average living expenses over the past six months, as declared on the bankruptcy filing. Total debt is the total debt declared by the filer. Dischargeable debt is debt potentially eligible for forgiveness in bankruptcy. Non-dischargeable debt is debt that cannot be forgiven. Prior filing is a summy indicating whether the filer has a prior bankruptcy filing. Pro se is a dummy indicating whether a filer represents themselves (1) or had a bankruptcy attorney (0). Audit is a dummy variable indicating whether a case is audited. Case duration is the number of days between the filing date and case closure. Debt discharge is a dummy indicating debt forgiveness. Case dismissed is a dummy indicating that a case is dismissed/no debt is forgiven.

*Panel A: Chapter 7*

| | count | mean | sd | p25 | p50 | p75 |
|---|---|---|---|---|---|---|
| Total assets | 8,694,488 | 80218 | 100562 | 7000 | 26131 | 131625 |
| Current income | 8,454,254 | 2819 | 1922 | 1362 | 2660 | 4079 |
| Average income | 8,550,288 | 2603 | 1398 | 1615 | 2432 | 3480 |
| Average expense | 8,387,242 | 2811 | 1353 | 1832 | 2643 | 3683 |
| Total debt | 8,190,755 | 140911 | 131200 | 41261 | 92571 | 203495 |
| Dischargeable debt | 8,165,264 | 132198 | 128245 | 35995 | 81247 | 193028 |
| Non-dischargeable debt | 8,242,232 | 3853 | 9742 | 0 | 0 | 1000 |
| Prior filing | 9,078,946 | 0.0685 | 0.2525 | 0.0000 | 0.0000 | 0.0000 |
| Pro se | 8,889,995 | 0.0795 | 0.2705 | 0.0000 | 0.0000 | 0.0000 |
| Audit | 9,078,946 | 0.0011 | 0.0329 | 0.0000 | 0.0000 | 0.0000 |
| Case duration | 9,003,031 | 185 | 210 | 103 | 115 | 149 |
| Debt discharge | 9,035,928 | 0.9483 | 0.2214 | 1.0000 | 1.0000 | 1.0000 |
| Case dismissed | 9,035,928 | 0.0517 | 0.2214 | 0.0000 | 0.0000 | 0.0000 |

*Panel B: Chapter 13*

| | count | mean | sd | p25 | p50 | p75 |
|---|---|---|---|---|---|---|
| Total assets | 4,175,925 | 107208 | 108119 | 13170 | 77052 | 172155 |
| Current income | 3,671,201 | 3384 | 2093 | 1791 | 3192 | 4908 |
| Average income | 3,720,698 | 3231 | 1474 | 2120 | 3078 | 4268 |
| Average expense | 3,882,583 | 2785 | 1396 | 1727 | 2590 | 3703 |
| Total debt | 3,828,861 | 159049 | 136204 | 49614 | 121421 | 229667 |
| Dischargeable debt | 3,818,401 | 147797 | 132600 | 40527 | 109716 | 215867 |
| Non-dischargeable debt | 3,777,389 | 4902 | 10539 | 0 | 0 | 3877 |
| Prior filing | 4,468,876 | 0.3279 | 0.4695 | 0.0000 | 0.0000 | 1.0000 |
| Pro se | 4,267,077 | 0.0883 | 0.2837 | 0.0000 | 0.0000 | 0.0000 |
| Audit | 4,468,876 | 0.0012 | 0.0342 | 0.0000 | 0.0000 | 0.0000 |
| Case duration | 3,501,956 | 774 | 589 | 239 | 617 | 1262 |
| Debt discharge | 4,103,447 | 0.4456 | 0.4970 | 0.0000 | 0.0000 | 1.0000 |
| Case dismissed | 4,103,447 | 0.5544 | 0.4970 | 0.0000 | 1.0000 | 1.0000 |

## Table 3: Misstatements

Table 3 shows misstatements hand-collected from approximately 3,500 audit reports. Each report can have multiple misstatements. Income are misstatements of income such as wages. Account are misstatements of checking, savings, and investment accounts. Vehicles are misstatements of cars and other vehicles. Real Estate are misstated real property. Transfer are general transfers or gifts of assets not falling into reported categories. Personal Property are misstatements of valuable personal items. Other are misstatements that could not be classified into the listed categories. Frequency shows the number of times a type of misstatement is listed in audit reports. Reported shows the mean/median reported value of the misstated item (zero if not declared). Audited shows the mean/median value of an item according to the auditor if the value could be determined. Misreporting is the difference between reported and audited value among misstatements. The % column shows the misreporting as a percentage of the audit finding. Panel A shows misstatements among Chapter 7 bankruptcy filings. Panel B shows misstatements among Chapter 13 bankruptcy filings.

| | | Mean | | | Median | | |
|---|---|---|---|---|---|---|---|
| Type of Misstatement | Frequency | Reported | Audited | Misreporting | Reported | Audited | Misreporting |
| *Panel A: Chapter 7* | | | | | | | |
| Income | 1,838 | 5,024 | 7,977 | 2,953 | 4,905 | 7,064 | 2,159 |
| Account | 565 | 1,351 | 25,631 | 24,279 | 0 | 2,525 | 2,525 |
| Vehicle | 276 | 3,163 | 16,734 | 13,572 | 0 | 10,038 | 10,038 |
| Real Estate | 222 | 4,912 | 170,575 | 165,663 | 0 | 124,000 | 124,000 |
| Transfer | 189 | 27 | 46,925 | 46,897 | 0 | 8,718 | 8,718 |
| Personal Property | 130 | 2,746 | 14,998 | 12,253 | 0 | 10,403 | 10,403 |
| Other | 28 | 1,102 | 43,842 | 42,740 | 0 | 7,958 | 7,958 |
| *Panel B: Chapter 13* | | | | | | | |
| Income | 927 | 8,000 | 11,171 | 3,171 | 7,381 | 9,970 | 2,589 |
| Account | 354 | 753 | 9,584 | 8,832 | 0 | 2,267 | 2,267 |
| Vehicle | 146 | 1,590 | 13,890 | 12,299 | 0 | 10,150 | 10,150 |
| Real Estate | 116 | 2,217 | 125,980 | 123,763 | 0 | 57,000 | 57,000 |
| Transfer | 113 | 357 | 33,224 | 32,867 | 0 | 8,778 | 8,778 |
| Personal Property | 78 | 10,044 | 41,651 | 31,607 | 0 | 11,839 | 11,839 |
| Other | 12 | 0 | 12,554 | 12,554 | 0 | 13,850 | 13,850 |

## Table 4: Placebo

Table 4 shows placebo estimates of audits on pre-determined case characteristics. All outcome variables shown are determined at the time of the filing before cases are selected for audit. If the identification strategy is successful, no association between pre-determined characteristics and audits is expected. The regression includes granular $10 wide buckets for income and expense deviations from the average in the filing district for each year. Ln(1+Assets) is the natural logarithm of one plus reported assets. Ln(1+Debt) is the natural logarithm of one plus reported debt. Ln(1+Dischargeable) is the natural logarithm of one plus reported dischargeable debt. Ln(1+Non-Dischargeable) is the natural logarithm of one plus reported non-dischargeable debt. Prior Filing is a dummy variable equal to one if the filer has a prior bankruptcy filing. Pro-Se is a dummy variable equal to one if the filer does not have an attorney at the time of filing. Standard errors are heteroskedasticity robust but not clustered. Panel A shows estimates among Chapter 7 bankruptcy filings. Panel B shows estimates among Chapter 13 bankruptcy filings. t-statistics in parentheses. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

*Panel A: Placebo Chapter 7*

| | (1) Ln(1+Assets) | (2) Ln(1+Debt) | (3) Ln(1+Dischargeable) | (4) Ln(1+Non-Dischargeable) | (5) Prior Filing | (6) Pro-Se |
|---|---|---|---|---|---|---|
| Audit | -0.000 | -0.016 | -0.019 | 0.027 | -0.002 | -0.003 |
| | (-0.02) | (-1.19) | (-1.21) | (0.51) | (-0.75) | (-1.14) |
| Observations | 8139341 | 7919332 | 7896859 | 7853445 | 8363262 | 8182897 |
| Adjusted $R^2$ | 0.282 | 0.255 | 0.208 | 0.020 | 0.004 | 0.042 |
| Avg Income Deviation Bucket x Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Avg Expense Deviation Bucket x Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Cluster | No | No | No | No | No | No |

*Panel B: Placebo Chapter 13*

| | (1) Ln(1+Assets) | (2) Ln(1+Debt) | (3) Ln(1+Dischargeable) | (4) Ln(1+Non-Dischargeable) | (5) Prior Filing | (6) Pro-Se |
|---|---|---|---|---|---|---|
| Audit | -0.040 | -0.017 | -0.016 | 0.106 | -0.006 | -0.004 |
| | (-1.43) | (-0.80) | (-0.63) | (1.28) | (-0.76) | (-1.49) |
| Observations | 3553523 | 3527700 | 3517691 | 3423161 | 3699847 | 3512691 |
| Adjusted $R^2$ | 0.406 | 0.464 | 0.357 | 0.027 | 0.015 | 0.199 |
| Avg Income Deviation Bucket x Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Avg Expense Deviation Bucket x Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Cluster | No | No | No | No | No | No |

## Table 5: Audit Effects

Table 5 shows estimates for variations of specification 1. The regression includes granular $10 wide buckets for income and expense deviations from the average in the filing district for each year. County × Year fixed effects to control for geography and year-specific effects. Judge fixed effects control for judge-specific factors. Trustee fixed effects control for private trustee-specific effects. Dismiss is the primary outcome of interest and is a dummy variable equal to one if the bankruptcy filing is dismissed and equal to zero if the bankruptcy filing is successful and debt is forgiven. Columns (1) through (3) show effects for the full sample. Columns (4) through (6) show effects among cases with low income and expense deviations. I define low income and expense deviation as not exceeding average filer income and expense in a district by more than $1,800. Standard errors are clustered at the judge and zip code levels. Panel A shows estimates among Chapter 7 bankruptcy filings. Panel B shows estimates among Chapter 13 bankruptcy filings. t-statistics in parentheses. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

*Panel A: Chapter 7 Case Dismissal (= No Debt Forgiveness)*

| | Dismiss | | | Low Deviation Cases: Dismiss | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Audit | 0.012*** | 0.013*** | 0.011*** | 0.008*** | 0.008*** | 0.007** |
| | (5.02) | (5.20) | (4.60) | (2.69) | (2.66) | (2.33) |
| Observations | 8067841 | 8066418 | 8066307 | 7044602 | 7043059 | 7042947 |
| Adjusted $R^2$ | 0.112 | 0.117 | 0.119 | 0.122 | 0.127 | 0.128 |
| Avg Income Deviation Bucket x Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Avg Expense Deviation Bucket x Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| County x Year FE | No | Yes | Yes | No | Yes | Yes |
| Trustee FE | No | No | Yes | No | No | Yes |
| Judge FE | No | No | Yes | No | No | Yes |
| Judge and Zip Cluster | Yes | Yes | Yes | Yes | Yes | Yes |

*Panel B: Chapter 13 Case Dismissal (= No Debt Forgiveness)*

| | Dismiss | | | Low Deviation Cases: Dismiss | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Audit | -0.010 | -0.013 | -0.016* | -0.002 | -0.004 | -0.007 |
| | (-0.99) | (-1.35) | (-1.71) | (-0.14) | (-0.40) | (-0.66) |
| Observations | 2964951 | 2961187 | 2960830 | 2587390 | 2583459 | 2583122 |
| Adjusted $R^2$ | 0.079 | 0.151 | 0.223 | 0.075 | 0.149 | 0.222 |
| Avg Income Deviation Bucket x Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Avg Expense Deviation Bucket x Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| County x Year FE | No | Yes | Yes | No | Yes | Yes |
| Trustee FE | No | No | Yes | No | No | Yes |
| Judge FE | No | No | Yes | No | No | Yes |
| Judge and Zip Cluster | Yes | Yes | Yes | Yes | Yes | Yes |

## Table 6: Audit Effects Without Attorney Representation (Pro-se)

Table 6 shows estimates for variations of specification 1. I modify specification 1 by interacting the audit dummy with a dummy variable indicating whether a filer is self-represented (pro-se) or has attorney representation. I estimate $Y_i = \alpha + \beta_1 Audit_i + \beta_2 ProSe_i + \beta_3 ProSe_i \times Audit_i + \beta_4 Controls_i + \beta_5 Controls_i \times Audit_i + \sum_j \gamma_{jt} ExpenseDeviationBucket_{jt} + \sum_k \delta_{kt} IncomeDeviationBucket_{kt} + \varepsilon_i$. Table 6 shows estimates for $\beta_3$. The regression includes granular \$10 wide buckets for income and expense deviations from the average in the filing district for each year. County × Year fixed effects to control for geography and year-specific effects. Judge fixed effects control for judge-specific factors. Trustee fixed effects control for private trustee-specific effects. Controls are logged assets, logged avg. monthly expenses, logged avg. monthly income, and a dummy indicating prior bankruptcy filing. All those controls are interacted with the audit dummy (Interacted controls). Dismiss is the primary outcome of interest and is a dummy variable equal to one if the bankruptcy filing is dismissed and equal to zero if the bankruptcy filing is successful and debt is forgiven. Columns (1) through (3) show effects for Chapter 7 cases. Columns (4) through (6) show effects for Chapter 13 cases. Standard errors are clustered at the judge and zip code levels. t-statistics in parentheses. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

| | Chapter 7: Dismissed | | | Chapter 13: Dismissed | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Audit=1 × Pro se=1 | 0.060*** | 0.060*** | 0.059*** | -0.142** | -0.144** | -0.111* |
| | (2.64) | (2.64) | (2.61) | (-2.22) | (-2.33) | (-1.93) |
| Observations | 7690436 | 7688878 | 7688761 | 2691346 | 2687461 | 2687124 |
| Adjusted $R^2$ | 0.145 | 0.148 | 0.149 | 0.130 | 0.184 | 0.253 |
| Avg Income Deviation Bucket x Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Avg Expense Deviation Bucket x Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| County x Year FE | No | Yes | Yes | No | Yes | Yes |
| Trustee FE | No | No | Yes | No | No | Yes |
| Judge FE | No | No | Yes | No | No | Yes |
| Base Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Interacted Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Judge and Zip Cluster | Yes | Yes | Yes | Yes | Yes | Yes |

## Table 7: Subsample Analysis: Audit Effects

Table 7 shows the regression of outcome variables on an audit dummy and a constant for a matched sample. Panel A shows results among Chapter 7 cases. Panel B shows results among Chapter 13 cases. Audited and non-audited observations are exactly matched on chapter, filing year, filer zip code, attorney representation status (pro-se or not), and the existence of a prior bankrtupcy filing. Within those categories, the matched observation is determined by the nearest expense within $500 as the legislation prescibes matching based on expense and income - and income is frequently misstated. The matching is without replacement. Dismissed is a dummy variable equal to one if the bankruptcy filing is dismissed and equal to zero if the bankruptcy filing is successful/debt is forgiven. Ln(Entries) is the natural logarithm of the number of court docket entries in a bankruptcy case and measures the number of procedural steps and back-and-forth during a bankruptcy case. Adjourn 341 is a dummy variable equal to one if the meeting between creditors, debtors and the trustee (341 meeting) is adjourned and needs to be continued at another date. It is set to one if at least one docket entry contains '341' and 'adjourn'. During the 341 meeting debtors answer questions about their bankruptcy schedules, conduct and financial position under the penalty of perjury. An adjournment of the meeting may happen in the case of discrepancies between bankruptcy petition and testimony. Amended Schedules is dummy variable equal to one if the debtor needed to amend the bankruptcy schedule to add or correct filed information. The variable is set to one if at least one docket entry contains 'amended schedules' after the audit selection docket entry for treated cases and after the matched audit selection entry for control cases. Final Report is a dummy variable equal to 1 if the docket contains a final report of distribution and equal to zero when a case is dismissed or contains a report of no distribution to creditors. It proxies for an increase in distributions to creditors and is measured by setting the dummy to one if at least one docket entry contains 'final report'. Modify Repayment Plan is a dummy variable equal to one if the Chapter 13 repayment plan is modified. It is set to one if the docket either contains at least one entry with 'amended' and 'plan' or at least one entry with 'modify' and 'plan'. The mean row shows the mean of the outcome variable when a dummy and the mean number of entries when the outcome is Ln(Entries). Following Abadie and Spiess (2022) standard errors are clustered at the matched pair level. t-statistics in parentheses. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

*Panel A: Chapter 7*

| | Dismissed | Ln(Entries) | Adjourn 341 | Amended Schedules | Final Report |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Audit | 0.019*** | 0.202*** | 0.016*** | 0.072*** | 0.018*** |
| | (5.70) | (28.92) | (3.58) | (12.64) | (3.32) |
| Observations | 10368 | 10488 | 10488 | 10488 | 10488 |
| Mean | .03 | 26.95 | .07 | .21 | .09 |
| Cluster | Matched Pair | Matched Pair | Matched Pair | Matched Pair | Matched Pair |

*Panel B: Chapter 13*

| | Dismissed | Ln(Entries) | Adjourn 341 | Amended Schedules | Modify Repayment Plan |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Audit | -0.008 | 0.102*** | 0.015** | 0.070*** | 0.012 |
| | (-0.47) | (8.85) | (2.13) | (6.41) | (0.92) |
| Observations | 3074 | 4372 | 4372 | 4372 | 4372 |
| Mean | .53 | 60.78 | .09 | .48 | .59 |
| Cluster | Matched Pair | Matched Pair | Matched Pair | Matched Pair | Matched Pair |

## Table 8: Audit Findings

Table 8 shows the regression of outcome variables on an audit finding dummy and a constant for a matched sample of cases that were not audited. Cases are exactly matched on the chapter, filing year, filer county, attorney representation status (pro-se or not), and the existence of a prior bankruptcy filing. Within these strata, nearest-neighbor matching is applied to the closest neighbor in terms of living expenses and assets. Distances are weighted by the diagonal of the variance-covariance matrix (Mahalanobis distance). Observations are restricted to matches with a maximum expense deviation of $500. Panel A shows the effects of audit findings on case dismissals and, thereby, implicitly debt forgiveness. Columns (1) to (3) show the effects among Chapter 7 cases. Columns (4) to (6) show the effects among Chapter 13 cases. Panel B shows the effects of audit findings on case complexity and filers' mitigating actions as measured by amendments to the filers' bankruptcy schedules. Panel C measures the consequences of audit findings for payments and the distribution to creditors. Outcome variable definitions are equivalent to Table 7. The mean row shows the mean of the outcome variable in the matched sample. Following Abadie and Spiess (2022) standard errors are clustered at the matched pair level. t-statistics in parentheses. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

*Panel A: Case Dismissal (= No Debt Forgiveness)*

| | Chapter 7: Dismissed | | | Chapter 13: Dismissed | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Misstatement | 0.047*** | | | -0.057 | | |
| | (5.21) | | | (-1.51) | | |
| No Misstatement | | -0.001 | | | -0.042** | |
| | | (-0.30) | | | (-2.40) | |
| Unable to Complete | | | 0.369*** | | | 0.199*** |
| | | | (9.57) | | | (5.13) |
| Observations | 2214 | 9494 | 374 | 562 | 2758 | 372 |
| Mean | .05 | .02 | .24 | .48 | .51 | .78 |
| Cluster | Matched Pair | Matched Pair | Matched Pair | Matched Pair | Matched Pair | Matched Pair |

*Panel B: Complexity and Mitigation*

| | Chapter 7: Amended Bankruptcy Schedules | | | Chapter 13: Amended Bankruptcy Schedules | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Misstatement | 0.212*** | | | 0.171*** | | |
| | (13.96) | | | (6.18) | | |
| No Misstatement | | 0.042*** | | | 0.042*** | |
| | | (6.82) | | | (3.47) | |
| Unable to Complete | | | -0.016 | | | -0.046 |
| | | | (-0.58) | | | (-1.51) |
| Observations | 2264 | 9578 | 380 | 818 | 3988 | 438 |
| Mean | .22 | .11 | .08 | .36 | .3 | .16 |
| Cluster | Matched Pair | Matched Pair | Matched Pair | Matched Pair | Matched Pair | Matched Pair |

*Panel C: Payments*

| | Chapter 7: Final Report with Distribution | | | Chapter 13: Modification of Payment Plan | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Misstatement | 0.072*** | | | 0.090*** | | |
| | (5.49) | | | (3.08) | | |
| No Misstatement | | 0.004 | | | 0.044*** | |
| | | (0.77) | | | (3.22) | |
| Unable to Complete | | | -0.000 | | | -0.219*** |
| | | | (-0.00) | | | (-5.30) |
| Observations | 2264 | 9578 | 380 | 818 | 3988 | 438 |
| Mean | .12 | .08 | .07 | .66 | .59 | .42 |
| Cluster | Matched Pair | Matched Pair | Matched Pair | Matched Pair | Matched Pair | Matched Pair |

## Table 9: Audit on Credit Access by Pro-Se

Table 9 shows estimation results for variations of specification (3) for matched samples of audited and non-audited cases merged to consumer credit records. The outcome variable is credit access as measured by credit scores. Columns (1) and (3) show estimates for specification (3) among Chapter 7 and Chapter 13 cases, respectively. Columns (2) and (4) modify specification (3), introducing a triple interaction with Pro-Se filer status. To absorb base effects into the fixed effects and control for varying trends across asset, expense, and income relative to bankruptcy time, I include relative year by pro-se status fixed effects, as well as asset bucket (20 buckets), expense bucket (20 buckets), and income buckets (20 buckets) by relative time fixed effects. Audited and non-audited observations are exactly matched on the chapter, filing year, filer zip code, attorney representation status (pro-se or not), and the existence of a prior bankruptcy filing. Within those categories, the matched observation is determined by the nearest expense within $500 as the legislation prescribes matching based on expense and income - and income is frequently misstated. The matching is without replacement. Following Abadie and Spiess (2022) standard errors are clustered at the matched pair level. t-statistics in parentheses. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$ Source: TransUnion

| | Chapter 7 | | Chapter 13 | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Audit=1 × Post=1 | -3.790** | -3.347** | 0.210 | 0.222 |
| | (-2.52) | (-2.20) | (0.10) | (0.10) |
| Audit=1 × Post=1 × Pro Se=1 | | -15.760* | | -38.509 |
| | | (-1.73) | | (-1.47) |
| Observations | 53158 | 53158 | 21196 | 21196 |
| Adjusted $R^2$ | 0.454 | 0.458 | 0.404 | 0.409 |
| Relative Year FE | Yes | No | Yes | No |
| Individual FE | Yes | Yes | Yes | Yes |
| Snapshot FE | Yes | Yes | Yes | Yes |
| Relative Year x Pro Se FE | No | Yes | No | Yes |
| Relative Year x Asset Bucket FE | No | Yes | No | Yes |
| Relative Year x Expense Bucket FE | No | Yes | No | Yes |
| Relative Year x Income Bucket FE | No | Yes | No | Yes |
| Judge FE | Yes | Yes | Yes | Yes |
| Trustee FE | Yes | Yes | Yes | Yes |
| Cluster | Matched Pair | Matched Pair | Matched Pair | Matched Pair |

# Appendix

## A Report of Debtor Audit with Material Misstatements

The following three pages show an audit report with material misstatement. The audit report is obtained from the text "Debtor Audits and Practice Tips from Chapter 7 Trustees" by Assistant U.S. Trustee Margaret K. Garber, Chapter 7 Trustee William E. Callahan, Jr., and Chapter 7 Trustee W. Stephen Scott in the Western District of Virginia. The text can be accessed here. For the hand-checked cases, I access the audit opinion in unredacted form via the court docket on PACER.

UNITED STATES BANKRUPTCY COURT
WESTERN DISTRICT OF VIRGINIA

| | | |
|---|---|---|
| In re: | ) | |
| | ) | |
| [redacted] | ) | Case No. [redacted] |
| | ) | Chapter 7 |
| | ) | |
| | ) | |
| Debtors. | ) | |

REPORT OF DEBTOR AUDIT

Pursuant to 28 U.S.C. § 586 (f)(1), the United States Trustee contracted for an audit to be performed of the above-captioned debtors' petition, schedules and other information filed by the debtors in this case. In accordance with the Debtor Audit Standards established pursuant to Section 603 (a) of the Bankruptcy Abuse Prevention and Consumer Protection Act of 2005, McBride, Lock & Associates performed the procedures enumerated in the contract between ourselves and the United States Trustee Program to determine whether certain items in the bankruptcy petition, schedules, and statements as originally filed by [redacted] in Bankruptcy Case No [redacted] contain material misstatements concerning the debtors' income, expenditures, or assets.

| The auditor finds: | |
|---|---|
| No material misstatement | |
| One or more material misstatements<br>The material misstatements are listed on<br>the attached List of Material Misstatements. | √ |

The debtors were responsible for the preparation of the bankruptcy petition, schedules, and statements in this case. The United States Trustee Program is responsible for the sufficiency of the procedures developed to determine the accuracy, veracity, and completeness of the petitions, schedules and other information that the debtors are required to provide under 11 U.S.C.§§ 521 and 1322. McBride, Lock & Associates make no representation regarding the sufficiency of the procedures either for the purpose for which this report has been requested or for any other purpose.

The analysis and findings contained in this report are intended solely for the information and of the United States Trustee Program and parties-in-interest in the subject civil bankruptcy proceeding and are not intended to be and should not be used by anyone other than these parties. However, this report is a matter of public record and its distribution is not limited. The Report is not a legal determination, and the legal effect of the auditor's findings of material misstatement is a question for the Court. Further, the findings contained in the report neither require the United States Trustee Program or other related

parties in interest to take, nor preclude these parties from taking, legal action in or relating to this case, including with respect to matters not discussed in this report.

Respectfully submitted,



McBride, Lock & Associates, LLC
Certified Public Accountants
1111 Main Street, Suite 900
Kansas City, Missouri 64105
(816) 221-4559

Dated this 16th day of February, 2016.

List of Material Misstatements

| Material Misstatement | Amount Reported in Schedules and Statements | As Found in Audit |
|---|---|---|
| Underreported Total Combined Monthly Income on Schedule I. | $6,795.00 | $8,044.80 |
| Understatement on Schedule B of the Fair Market Value of a single item of personal property. | $0.00 | $12,665.00 |

## B Debtor Audit Standards

The Department of Justice provides Debtor Audit Standards according to which auditors shall determine the accuracy, completeness, and truthfulness of the bankruptcy filing. The standards can be found in Federal Register Vol. 71, No. 190 (here). For context I directly quote them below:

> Debtor Audit Standard No. 1
>
> The debtor audit engagement shall be performed by individuals having adequate technical training and proficiency for performing attest engagements.
>
> Debtor Audit Standard No. 2
>
> The debtor audit engagement shall be performed by individuals having adequate knowledge of bankruptcy petitions, schedules, and statements; the Bankruptcy Code; and the Federal Rules of Bankruptcy Procedure.
>
> Debtor Audit Standard No. 3
>
> In all matters relating to the debtor audit, an independence in mental attitude shall be maintained by the individuals performing the engagement.
>
> Debtor Audit Standard No. 4
>
> Due professional care shall be exercised in the planning and performance of the engagement.
>
> Debtor Audit Standard No. 5
>
> The work shall be adequately planned and assistants, if any, are to be properly supervised.
>
> Debtor Audit Standard No. 6
>
> Sufficient evidence must be obtained to provide a reasonable basis for the conclusion expressed in the report filed with the court.
>
> Debtor Audit Standard No. 7
>
> The report shall identify that the subject matter of the debtor audit is the petition,

schedules, and other information as originally filed by the debtor in the bankruptcy case and state that the debtor audit was conducted in accordance with the Debtor Audit Standards and the procedures established by the United States Trustee Program.

Debtor Audit Standard No. 8

The report shall clearly and conspicuously state the conclusion as to the presence or absence of material misstatements in income, expenses, or assets, in the petition, schedules, and statements originally filed by the debtor in the bankruptcy case.

Debtor Audit Standard No. 9

The report shall state that it is intended solely for the information and use of the United States Trustee and other parties in interest to the bankruptcy case and that it is not intended to be and should not be used by anyone other than these specified parties; noting however, that since the report is a matter of public record, its distribution is not limited.

# C Bankruptcy Abuse Prevention and Consumer Protection Act Of 2005; Congressional Record Vol. 151, No. 44 - SEC. 603. Audit Procedures.

Congressional Record Vol. 151, No. 44 - Section 603 outlines the audit procedures and, notably, the selection mechanism to determine which cases are audited. I provide the entire section below. The procedures state that cases shall be randomly audited, and at least 1 in 250 cases shall be audited. It further outlines that individuals with high income and expense shall be audited:

(a) In General.–

(1) Establishment of procedures.–The Attorney General (in judicial districts served by United States trustees) and the Judicial Conference of the United States (in judicial districts served by bankruptcy administrators) shall establish procedures

to determine the accuracy, veracity, and completeness of petitions, schedules, and other information that the debtor is required to provide under sections 521 and 1322 of title 11, United States Code, and, if applicable, section 111 of such title, in cases filed under chapter 7 or 13 of such title in which the debtor is an individual. Such audits shall be in accordance with generally accepted auditing standards and performed by independent certified public accountants or independent licensed public accountants, provided that the Attorney General and the Judicial Conference, as appropriate, may develop alternative auditing standards not later than 2 years after the date of enactment of this Act.

(2) Procedures.–Those procedures required by paragraph (1) shall–

(A) establish a method of selecting appropriate qualified persons to contract to perform those audits;

**(B) establish a method of randomly selecting cases to be audited, except that not less than 1 out of every 250 cases in each Federal judicial district shall be selected for audit**;

**(C) require audits of schedules of income and expenses that reflect greater than average variances from the statistical norm of the district in which the schedules were filed if those variances occur by reason of higher income or higher expenses than the statistical norm of the district in which the schedules were filed; and**[32]

(D) establish procedures for providing, not less frequently than annually, public information concerning the aggregate results of such audits including the percentage of cases, by district, in which a material misstatement of income or expenditures is reported.

(b) Amendments.–Section 586 of title 28, United States Code, is amended–

(1) in subsection (a), by striking paragraph (6) and inserting the following:

[32]emphasis added

"(6) make such reports as the Attorney General directs, including the results of audits performed under section 603(a) of the Bankruptcy Abuse Prevention and Consumer Protection Act of 2005;"; and

(2) by adding at the end the following:

"(f)(1) The United States trustee for each district is authorized to contract with auditors to perform audits in cases designated by the United States trustee, in accordance with the procedures established under section 603(a) of the Bankruptcy Abuse Prevention and Consumer Protection Act of 2005.

"(2)(A) The report of each audit referred to in paragraph (1) shall be filed with the court and transmitted to the United States trustee. Each report shall clearly and conspicuously specify any material misstatement of income or expenditures or of assets identified by the person performing the audit. In any case in which a material misstatement of income or expenditures or of assets has been reported, the clerk of the district court (or the clerk of the bankruptcy court if one is certified under section 156(b) of this title) shall give notice of the misstatement to the creditors in the case.

"(B) If a material misstatement of income or expenditures or of assets is reported, the United States trustee shall– "(i) report the material misstatement, if appropriate, to the United States Attorney pursuant to section 3057 of title 18; and

"(ii) if advisable, take appropriate action, including but not limited to commencing an adversary proceeding to revoke the debtor's discharge pursuant to section 727(d) of title 11.".

(c) Amendments to Section 521 of Title 11, U.S.C.–Section 521(a) of title 11, United States Code, as so designated by section 106, is amended in each of paragraphs (3) and (4) by inserting "or an auditor serving under section 586(f) of title 28" after "serving in the case".

(d) Amendments to Section 727 of Title 11, U.S.C.–Section 727(d) of title 11,

United States Code, is amended– (1) in paragraph (2), by striking "or" at the end; (2) in paragraph (3), by striking the period at the end and inserting "; or"; and (3) by adding at the end the following: "(4) the debtor has failed to explain satisfactorily– "(A) a material misstatement in an audit referred to in section 586(f) of title 28; or "(B) a failure to make available for inspection all necessary accounts, papers, documents, financial records, files, and all other papers, things, or property belonging to the debtor that are requested for an audit referred to in section 586(f) of title 28.". (e) Effective Date.–The amendments made by this section shall take effect 18 months after the date of enactment of this Act.

## D Sample Document Request for Audit

The following two pages show a sample document request for an audit. In cases selected for audit, the auditor will send this or a similar document request to the bankruptcy filer or their attorney. The sample document request is obtained from the text "Debtor Audits and Practice Tips from Chapter 7 Trustees" by Assistant U.S. Trustee Margaret K. Garber, Chapter 7 Trustee William E. Callahan, Jr., and Chapter 7 Trustee W. Stephen Scott in the Western District of Virginia. The text can be accessed here.

**Debtor(s): _____** **Case No.: _____**

**Attorney:_____**

**DOCUMENT REQUEST**

**Please provide copies of the documents listed below.** ***Do not provide originals.*** **Return this form and the attached documents by [DATE-3 weeks from date of Audit Notification Letter] to:** Debtor Audit Firm; Street Address; City, State, Zip**.**

**In the space provided next to each document category listed below, indicate whether all the documents requested are provided by marking Yes, No, or N/A. Explain all No or N/A answers at the end of this form. You do not need to explain a N/A answer to Question No. 4.**

| | | |
|---|---|---|
| 1 | Payment advices or other evidence of payment from an employer for the six full calendar months preceding the date of the bankruptcy petition plus those received in the calendar month in which the bankruptcy was filed, from the debtor(s), or from an individual debtor and the individual debtor's non-filing spouse unless the debtor has checked the Boxes on line 1, Form B122A-1 indicating that the debtor is "Married and your spouse is NOT filing with you," and the debtor and spouse are "living separately or are legally separated." (Chapter 7 cases only). | _____ |
| 2 | Federal income tax returns, including all schedules and all W-2, 1099, and K-1 forms, for the two most recent taxable periods prior to the date of the bankruptcy petition. If either of the returns has not been filed, provide copies of the two most recently filed federal income tax returns. (If joint case and debtors filed separate returns, provide both returns.) | _____ |
| 3 | Account statements for the six months preceding the date of the bankruptcy petition for all depository and investment accounts in which the debtor(s) had an interest in any of the six months, including statements (even if received post petition) that reflect activity in the month in which the petition was filed; along with sufficient documentation to explain the source of every deposit or credit over $500. (Include information for checking, savings, money market, mutual fund, and brokerage accounts. Examples of documentation for deposit transactions include check registers and annotations on or attached to the account statements.) **Audit firms may request that you provide additional documentation to sufficiently explain the source or purpose of an account statement entry or entries**. | _____ |
| 4 | If the debtor(s) is divorced, (a) the divorce decree, (b) any orders regarding property settlements entered within the last three years, and (c) any alimony or child support orders currently in effect and amendments thereto. | _____ |

| | | |
|---|---|---|
| 5 | If the debtor(s) is self-employed, then for each business owned by debtor or from which debtor derives self-employment income, (a) business tax returns for the two most recent taxable periods prior to the date of the bankruptcy petition, (b) most recent accounts receivable ledger and aging schedule/report, (c) most recent balance sheet prior to the date of the bankruptcy petition, (d) income statement for the most recent period ended prior to the date of the bankruptcy petition, (e) quarterly sales tax return for the most recent period ended prior to the date of the bankruptcy petition, if any, (f) account statements for business depository account(s) for the six months preceding the date of the bankruptcy petition, and the month in which the petition was filed, along with sufficient documentation to explain the source of every deposit or credit, and the purpose of every check, withdrawal, or debit, and (g) most recent business asset listing and depreciation schedule, if any. | _____ |

Explanation for any "No" or "N/A" responses (attach pages as necessary):_____

_____
_____
_____
_____
_____
_____
_____

I declare under penalty of perjury that the responses to this Document Request are true and correct.

Date:_____ Signature:_____
Debtor

Date:_____ Signature:_____
Joint Debtor, if any

# E Ex-ante Deterrence

## E.1 Basic Set-up and Derivations

Suppose individuals live for two periods. In the first period, they borrow an amount d at interest rate r, and in the second period, they receive income drawn from a distribution F(y). In the second period, individuals can decide to either default and receive e-c, where e is the exemption level and c is the cost of bankruptcy, or not file for bankruptcy, repay their debt, and consume their income minus debt and interest payment. This set-up builds on Gross et al. (2021). If individuals file for bankruptcy, their debt is discharged with probability Pr(discharge), and the case is dismissed without debt forgiveness with probability (1-Pr(discharge)). When the bankruptcy filing is dismissed, consumers still have to repay their debt and also may incur the cost of bankruptcy c. Hence, in period two, an individual will receive

$$V_{NB} = u(y - (1+r)d) \tag{4}$$

when not filing for bankruptcy and in expectation

$$V_B = Pr(discharge)u(e-c) + (1 - Pr(discharge))u(y - (1+r)d - c) \tag{5}$$

when filing for bankruptcy. Hence, consumers maximize:

$$\max_{b \in \{0,1\}} (1-b)V_{NB} + bV_B \tag{6}$$

However, thus far, the model is independent of audit rates. Suppose that the discharge probability depends on whether a case was audited. We can then re-write the utility when filing for

bankruptcy as the surplus from bankruptcy filing without audit plus an audit deterrence term:

$$V_B = Pr(audit) * \underbrace{\Delta}_{Pr(discharge|audit) - Pr(discharge|noaudit)} (u(e-c) - u(y-(1+r)d-c)) + \underbrace{Pr(discharge|noaudit) * u(e-c) + (1 - Pr(discharge|noaudit)) * u(y-(1+r)d-c)}_{V_B(y,noaudit)} \tag{7}$$

We can define the income $y^*$ at which an individual is indifferent between filing and not filing for bankruptcy as:

$$V_{NB}(y^*) = V_B(y^*) \tag{8}$$

Hence, the share of consumers filing for bankruptcy is $p = P(y < y^*) = F(y^*)$. I define the deterrence effect of an increase in ex-ante audit rates as the share of filers who file when the ex-ante audit probability is low ($Pr(audit) = a_{low}$) but don't file when the ex-ante audit probability is high ($Pr(audit) = a_{high}$). Expressing the indifference income between filing and not filing for bankruptcy as a function of the ex-ante audit rate. This means that

$$\begin{aligned} Deterrence &= P(y < y^*(a_{low}) | y < y^*(a_{low})) - P(y < y^*(a_{high}) | y < y^*(a_{low})) \\ &= 1 - F(y^*(a_{high}) | y < y^*(a_{low})) \end{aligned} \tag{9}$$

When setting the low audit rate to the targeted 1 in 250 cases, we can then write the deterrence effect as:

$$\begin{aligned} Deterrence &= F(y^*(a_{low}) | y < y^*(a_{low})) - F(y^*(a_{high}) | y < y^*(a_{low})) \\ &= \int 1\Big[ [\underbrace{a_{low}}_{=1/250} - a_{high}] * [Pr(discharge|audit) - Pr(discharge|noaudit)] \\ &\quad * [u(e-c) - u(y-(1+r)d-c)] > x(y) \Big] dF(y | y < y^*(\underbrace{a_{low}}_{=1/250})) \end{aligned} \tag{10}$$

where $x(y)$ is the surplus from filing for bankruptcy in the first place when the audit rate is low.[33] In words, audit deterrence is the share of bankruptcy filers who would not file because

[33] x is defined as: $x(y) = V_B(y, Pr(audit = \frac{1}{250})) - V_{NB}$.

the negative impact from the increased audit probability exceeds the surplus from filing when the audit rate is low. Obviously, this share is not directly observable in the data. However, we can exploit the increase in audit probability and non-compliance with the audit requirement to estimate this deterrence.

Individuals facing an increase in the audit probability may withdraw or throw their case. For individuals who file under low audit rate and throw their case facing an increase in the rate, we know that:

$$V_B = \underbrace{a_{low}}_{=1/250} * \Delta(u(e-c) - u(y-(1+r)d-c)) + V_B(y, noaudit) > u(y-(1+r)d) = V_{NB}$$

and

$$V_B = \underbrace{a_{high}}_{=1} * \Delta(u(e-c) - u(y-(1+r)d-c)) + V_B(y, noaudit) < u(y-(1+r)d) = V_{NB} \tag{11}$$

defining x similarly to the above, we can write the share of filers withdrawing their filing as:

$$\begin{aligned} &Withdraw = \\ &= \int 1\Big[[\underbrace{a_{low}}_{=1/250} - \underbrace{a_{high}}_{=1}] * [Pr(discharge|audit) - Pr(discharge|noaudit)] \\ &* [u(e-c) - u(y-(1+r)d-c)] > x(y)\Big] dF(y|y < y^*(\underbrace{a_{low}}_{=1/250})) \end{aligned} \tag{12}$$

Note that under the above assumptions, Withdrawal and Deterrence decisions are equivalent if the ex-ante audit rate is set to 1. Also, note that we know the share of individuals withdrawing from the data. The share of withdrawals among Chapter 7 bankruptcies is 0.011 in the data (basically unable to complete rate of audits times dismissal when unable to complete). Since we examine a counterfactual increase in audit probabilities of 0.01 instead of an increase of close to 1 (0.996), I approximate the deterrence effect by scaling the withdrawal by 0.01. Multiplying the deterrence effect by the number of filings per year and the average dischargeable debt of $163,292 among Chapter 7 cases unable to complete audits, the deter-

rence effect of a ten percentage point increase in audit probability prevents the discharge of another $137 million.

The above derivation assumes no bankruptcy costs are incurred when a case is withdrawn. Appendix E.2 extends the framework to include bankruptcy cost for case withdrawal and argues that the estimates in the main text are a lower bound for indirect deterrence when incurred bankruptcy costs reduce case withdrawals.

### E.2 Ex-ante Deterrence with Bankrupcy Cost when Withdrawing

To this point, I assumed that consumers filing for bankruptcy and then withdrawing their filing do not incur any cost. However, as consumers already filed, they may incur some of the bankruptcy cost like stigma or reduced credit access in the future. Hence, to withdraw their case once selected for audit, consumers must fulfill the following two conditions. They withdraw after filing iff:

$$V_B = \underbrace{a_{low}}_{=1/250} * \Delta(u(e-c) - u(y-(1+r)d-c)) + V_B(y, noaudit) > u(y-(1+r)d) = V_{NB}$$

and

$$V_B = \underbrace{a_{high}}_{=1} * \Delta(u(e-c) - u(y-(1+r)d-c)) + V_B(y, noaudit) < u(y-(1+r)d-\gamma c) = V_{Withdraw} \tag{13}$$

where $\gamma$ indicates the share of bankruptcy cost already incurred.[34] Defining k as the difference between filing utility and withdrawing utility:

$$\begin{aligned} k = & \underbrace{a_{low}}_{=1/250} * [Pr(discharge|audit) - Pr(discharge|noaudit)] * [u(e-c) - u(y-(1+r)d-c)] \\ & + Pr(discharge|noaudit) * u(e-c) + (1 - Pr(discharge|noaudit)) * u(y-(1+r)d-c) \\ & - u(y-(1+r)d-\underbrace{\gamma c}_{Cost}) \\ = & V_B(y, Pr(audit = \frac{1}{250})) - u(y-(1+r)d-\underbrace{\gamma c}_{Cost}) \end{aligned} \tag{14}$$

we can write the share of filers withdrawing their filing as:

$$\begin{aligned} & Withdraw = \\ & = \int 1\Big[[\underbrace{a_{low}}_{=1/250} - \underbrace{a_{high}}_{=1}] * [Pr(discharge|audit) - Pr(discharge|noaudit)] \\ & * [u(e-c) - u(y-(1+r)d-c)] > k\Big] dF(y|y < y^*(\underbrace{a_{low}}_{=1/250})) \end{aligned} \tag{15}$$

Since $u(y-(1+r)d) > u(y-(1+r)d-\underbrace{\gamma c}_{Cost})$ and as those terms are subtrace in x and k respectively, we know that $x < k$. As a consequence, we know that measured deterrence is smaller than actual deterrence, implying that my deterrence estimate is a lower bound for ex-ante deterrence in the absence of bankruptcy cost incurred by filers not submitting their audit

[34] $V_B(y, noaudit) = Pr(discharge|noaudit) * u(e-c) + (1 - Pr(discharge|noaudit)) * u(y-(1+r)d-c)$

materials:

$$
\begin{aligned}
&Deterrence = \\
&= \int 1\Big[[\underbrace{a_{low}}_{=1/250} - \underbrace{a_{high}}_{=1}] * [Pr(discharge|audit) - Pr(discharge|noaudit)] \\
&* [u(e-c) - u(y-(1-r)d-c)] > x\Big] dF(y|y < y^*(\underbrace{a_{low}}_{=1/250})) \\
&\geq \int 1\Big[[\underbrace{a_{low}}_{=1/250} - \underbrace{a_{high}}_{=1}] * [Pr(discharge|audit) - Pr(discharge|noaudit)] \\
&* [u(e-c) - u(y-(1-r)d-c)] > k\Big] dF(y|y < y^*(\underbrace{a_{low}}_{=1/250}))
\end{aligned}
\tag{16}
$$

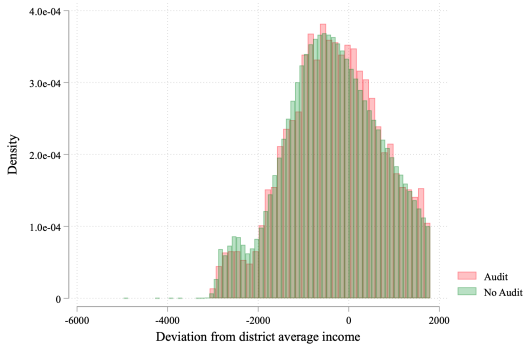
# F Audit Effect for Low Income and Expense Deviation Cases

In this section, I repeat the repeat the placebo analysis for bankruptcy cases with low income and expense deviation. These cases are likely to purely randomly selected. As Table 5 shows, the conclusions are similar. Non-random case selection is due to high deviation from a district's average income and expense. The primary identification concern is that for those high deviation cases, the USTP audits cases that appear more suspicious. To ensure that cases included in the analysis were likely selected via the random selection mechanism, I exclude cases with high income and expense deviation from the district average. I restrict cases to only those not exceeding the district average income and expense by more than $1800 in a given year. Hence, audited cases remaining in the sample were likely randomly selected. Figure F.1 illustrates that audited and not-audited cases cannot be distinguished in their income and expense distributions consistent with random assignment.

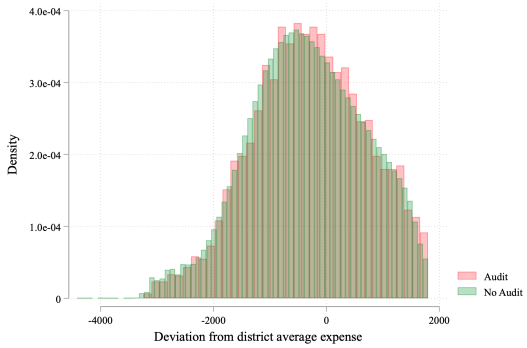
Table F.1 further verifies that audited and non-audited cases do not differ along pre-determined case characteristics. Cases do not differ statistically or economically in their assets, debts, dischargeable debts, non-dischargeable debts, prior filings, and self-representation. Overall those findings further corroborate the conclusions of Section 6.2 Table 5 columns (4) to (6).

## Figure F.1: Income/Expense Deviations by Audit Status - Conditional on Low Deviation

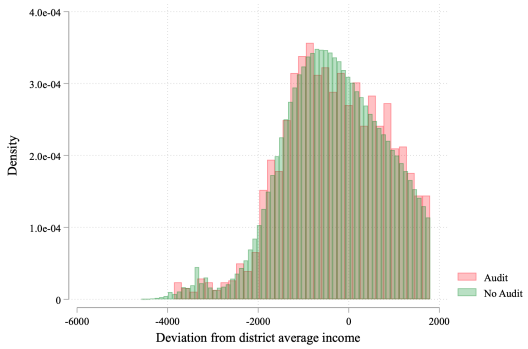
The primary determinant of exception audits is the deviation of a filer's income and expense from the average income and expense of filers in a given district. Figure F.1 illustrates that audit selection is consistent with random assignemnt for low deviation cases. Panel A shows histograms of the deviation of incomes from the average in the filing district among audited and non-audited individuals among Chapter 7 cases conditional on income and expense deviations not exceeding $1800. Panel B shows the same deviation for expenses among Chapter 7 cases conditional on income and expense deviations not exceeding $1800. Panels C and D repeat the exercise for Chapter 13 cases.



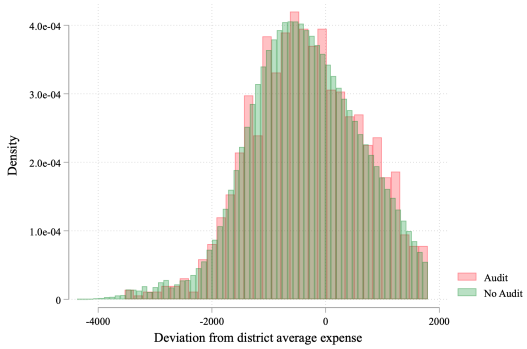

(a) Chapter 7: Average income




(b) Chapter 7: Average expense




(c) Chapter 13: Average income




(d) Chapter 13: Average expense

## Table F.1: Low Deviation Cases: Placebo

Table F.1 shows placebo estimates of audits on pre-determined case characteristics when conditioning on observations with income and expense deviations from the district average in a given year that is below $1800. All outcome variables shown are determined at time of the filing before cases are selected for audit. If the identification strategy is successufl,no association between pre-determined characteristics and audits is expected. The regression includes granular $10 wide buckets for income and expense deviations from the average in the filing district for each year. Ln(1+Assets) is the natural logarithm of one plus reported assets. Ln(1+Debt) is the natural logarithm of one plus reported debt. Ln(1+Dischargeable) is the natural logarithm of one plus reported dischargeable debt. Ln(1+Non-Dischargeable) is the natural logarithm of one plus reported non-dischargeable debt. Prior Filing is a dummy variable equal to one if the filer has a prior bankruptcy filing. Pro-Se is a dummy variable equal to one if the filer does not have an attorney at the time of filing. Standard errors are heteroskedasticity robust but not clustered. Panel A shows estimates among Chapter 7 bankruptcy filings. Panel B shows estimates among Chapter 13 bankruptcy filings. t-statistics in parentheses. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

*Panel A: Placebo Chapter 7*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Ln(1+Assets) | Ln(1+Debt) | Ln(1+Dischargeable) | Ln(1+Non-Dischargeable) | Prior Filing | Pro-Se |
| Audit | -0.005 | -0.008 | -0.001 | -0.034 | -0.005 | -0.004 |
| | (-0.21) | (-0.46) | (-0.07) | (-0.52) | (-1.28) | (-1.12) |
| Observations | 7164191 | 6972132 | 6952161 | 6874634 | 7287866 | 7130024 |
| Adjusted $R^2$ | 0.250 | 0.229 | 0.188 | 0.018 | 0.004 | 0.041 |
| Avg Income Deviation Bucket x Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Avg Expense Deviation Bucket x Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Cluster | No | No | No | No | No | No |

*Panel B: Placebo Chapter 13*

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Ln(1+Assets) | Ln(1+Debt) | Ln(1+Dischargeable) | Ln(1+Non-Dischargeable) | Prior Filing | Pro-Se |
| Audit | -0.023 | 0.001 | 0.005 | 0.065 | -0.008 | -0.002 |
| | (-0.66) | (0.05) | (0.16) | (0.68) | (-0.77) | (-0.70) |
| Observations | 3122899 | 3087799 | 3078695 | 2992878 | 3217960 | 3054709 |
| Adjusted $R^2$ | 0.407 | 0.469 | 0.364 | 0.025 | 0.012 | 0.213 |
| Avg Income Deviation Bucket x Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Avg Expense Deviation Bucket x Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Cluster | No | No | No | No | No | No |

## G Effect of Audits - Subsample Placebo

This section shows placebo estimates for the matched subsample of audited and non-audited cases analyzed in Section 7. Audited and non-audited observations are exactly matched on the chapter, filing year, filer zip code, attorney representation status (pro-se or not), and the existence of a prior bankruptcy filing. Within those categories, the matched observation is determined by the nearest expense within $500 as the legislation prescribes matching based on expense and income - and income is frequently misstated. The matching is without replacement. Table G.1 shows placebo estimates for Chapter 7 cases in Panel A and for Chapter 13 in Panel B. Across all placebo tests, audited and matched unaudited cases are not statistically distinguishable. Particularly among Chapter 7 cases, the economic significance of the point estimates is also small. The maximum difference between audited and unaudited cases is a 2.3 percent difference in the dischargeable debt balance. While the economic difference among Chapter 13 cases is somewhat larger, the cases are neither statistically distinguishable.

## Table G.1: Subsample Analysis: Placebo

Table G.1 shows the regression of outcome variables on an audit dummy and a constant for a matched sample. Panel A shows results among Chapter 7 cases. Panel B shows results among Chapter 13 cases. Audited and non-audited observations are exactly matched on chapter, filing year, filer zip code, attorney representation status (pro-se or not), and the existence of a prior bankrtupcy filing. Within those categories, the matched observation is determined by the nearest expense within $500 as the legislation prescibes matching based on expense and income - and income is frequently misstated. The matching is without replacement. Ln(1+Assets) is the natural logarithm of one plus reported assets. Ln(1+Debt) is the natural logarithm of one plus reported debt. Ln(1+Dischargeable) is the natural logarithm of one plus reported dischargeable debt. Ln(1+Non-Dischargeable) is the natural logarithm of one plus reported non-dischargeable debt. The mean row shows the mean of the unlogged outcome variable. Following Abadie and Spiess (2022) standard errors are clustered at the matched pair level. t-statistics in parentheses. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

*Panel A: Chapter 7*

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Ln(1+Assets) | Ln(1+Debt) | Ln(1+Dischargeable) | Ln(1+Non-Dischargeable) |
| Audit | 0.008 | 0.012 | 0.023 | 0.014 |
| | (0.28) | (0.60) | (1.02) | (0.16) |
| Observations | 9940 | 9584 | 9544 | 8786 |
| Mean | 84376 | 138957 | 126446 | 5224 |
| Cluster | Matched Pair | Matched Pair | Matched Pair | Matched Pair |

*Panel B: Chapter 13*

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Ln(1+Assets) | Ln(1+Debt) | Ln(1+Dischargeable) | Ln(1+Non-Dischargeable) |
| Audit | 0.056 | 0.047 | 0.036 | 0.130 |
| | (1.35) | (1.60) | (0.97) | (0.94) |
| Observations | 4122 | 4152 | 4150 | 3624 |
| Mean | 102203 | 142047 | 128589 | 5795 |
| Cluster | Matched Pair | Matched Pair | Matched Pair | Matched Pair |

## H Effect of Audit Findings - Placebo

This section shows placebo estimates for the matched samples for cases with each audit finding to observably similar unaudited cases. The placebo estimates correspond to the effects shown in Section 8. For each audit finding, cases are matched on the chapter, filing year, filer county, attorney representation status (pro-se or not), and the existence of a prior bankruptcy filing. Within these strata, nearest-neighbor matching is applied to the closest neighbor in terms of living expenses and assets. Distances are weighted by the diagonal of the variance-covariance matrix (Mahalanobis distance). Observations are restricted to matches with a maximum expense deviation of $500.

Table H.1 shows the results of regressing pre-determined case characteristics on a dummy for the audit finding within each of the samples. Expense and asset columns should be expected to be indistinguishable as observations are matched on assets and expenses. The income and debt columns show placebo estimates. Panel A shows estimates among Chapter 7 cases. Panel B shows estimates among Chapter 13 cases. Across all tests, cases with a specific audit finding are statistically indistinguishable from their matched unaudited counterpart. Economic differences also appear limited across a wide range of estimates.

# Table H.1: Audit Findings: Placebo

Table H.1 shows the regression of outcome variables on an audit finding dummy and a constant for a matched sample of cases that were not audited. Cases are exactly matched on the chapter, filing year, filer county, attorney representation status (pro-se or not), and the existence of a prior bankruptcy filing. Within these strata, nearest-neighbor matching is applied to the closest neighbor in terms of living expenses and assets. Distances are weighted by the diagonal of the variance-covariance matrix (Mahalanobis distance). Observations are restricted to matches with a maximum expense deviation of $500. Panel A shows placebo estimates for Chapter 7 cases. Panel B shows placebo estimates for Chapter 13 cases. Ln(1+Expense) is the natural logarithm of one plus reported living expenses. Ln(1+Income) is the natural logarithm of one plus reported income. Ln(1+Assets) is the natural logarithm of one plus reported assets. Ln(1+Debt) is the natural logarithm of one plus reported debt. The mean row shows the mean of the unlogged outcome variable. Following Abadie and Spiess (2022) standard errors are clustered at the matched pair level. t-statistics in parentheses. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

*Panel A: Chapter 7*

| | Ln(1+Expense) | | | Ln(1+Income) | | | Ln(1+Assets) | | | Ln(1+Debt) | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| Misstatement | 0.002 | | | -0.055 | | | 0.008 | | | 0.027 | | |
| | (1.03) | | | (-1.48) | | | (0.45) | | | (0.74) | | |
| No Misstatement | | 0.000 | | | 0.020 | | | -0.000 | | | -0.008 | |
| | | (0.45) | | | (1.15) | | | (-0.02) | | | (-0.47) | |
| Unable to Complete | | | 0.004 | | | 0.005 | | | -0.056 | | | -0.052 |
| | | | (1.54) | | | (0.05) | | | (-0.80) | | | (-0.34) |
| Observations | 2264 | 9578 | 380 | 2264 | 9578 | 380 | 2264 | 9578 | 380 | 2060 | 9012 | 338 |
| Mean | 4015 | 3400 | 3297 | 3751 | 3206 | 3018 | 111049 | 78959 | 68365 | 167311 | 130008 | 121530 |
| Cluster | Matched Pair | Matched Pair | Matched Pair | Matched Pair | Matched Pair | Matched Pair | Matched Pair | Matched Pair | Matched Pair | Matched Pair | Matched Pair | Matched Pair |

*Panel B: Chapter 13*

| | Ln(1+Expense) | | | Ln(1+Income) | | | Ln(1+Assets) | | | Ln(1+Debt) | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| Misstatement | 0.004 | | | -0.017 | | | -0.016 | | | -0.008 | | |
| | (1.32) | | | (-0.54) | | | (-0.35) | | | (-0.19) | | |
| No Misstatement | | 0.000 | | | -0.000 | | | -0.008 | | | -0.002 | |
| | | (0.28) | | | (-0.03) | | | (-0.54) | | | (-0.10) | |
| Unable to Complete | | | -0.007 | | | 0.014 | | | -0.102 | | | -0.021 |
| | | | (-1.37) | | | (0.73) | | | (-1.45) | | | (-0.16) |
| Observations | 818 | 3988 | 438 | 818 | 3988 | 438 | 818 | 3988 | 438 | 772 | 3832 | 402 |
| Mean | 3434 | 2882 | 2563 | 4195 | 3496 | 3143 | 141101 | 102948 | 100844 | 177473 | 134993 | 129855 |
| Cluster | Matched Pair | Matched Pair | Matched Pair | Matched Pair | Matched Pair | Matched Pair | Matched Pair | Matched Pair | Matched Pair | Matched Pair | Matched Pair | Matched Pair |

# I Additional Statistics on Material Misstatements

## I.1 Determinants of Misstatements

This section investigates partial correlations of filing characteristics with different types of misstatements. Determining optimal audit rates and audit selection is difficult (Slemrod and Yitzhaki, 2002). To improve audit selection and the targeting of oversight, understanding the association between filer characteristics and misstatements is essential. I find that assets and expenses are associated with misstatements. Besides, self-representation and liquidation cases are more associated with misstatements. However, despite including a host of fixed effects capturing year and location characteristics, I find explanatory power for misstatements to be low.

Table I.1 reports the results of regressing dummies for identified misstatements on case characterics among audited bankruptcy filings. In columns (1) and (2) the outcome variable is 1 if any misstatement is found and zero if a case got audited and no misstatement was found. A 1 percent increase in filing assets is correlated with a 1.5 percentage point increase in misstatement probability. Reported average income is negatively correlated with misstatements. That is, a 1 percent decline in average reported income is associated with a 1.1 percentage point increase in misstatement probability. Higher reported average expense is correlated with more misstatements. This is likely because filers misreport their income but to a lesser degree reduce reported expenses. A 1 percent increase in reported average expense is associated with a 3.5 percentage point increase in misstatement likelihood. Pro-se and chapter 7 cases are more likely to contain a misstatement. Pro-se cases are associated with a 10.6 percentage points higher likelihood of a misstatement. Chapter 7 cases are associated with a 4.9 percentage points higher likelihood of misstatements.

Columns (3) and (4) report regression results of a dummy for income misstatements. The dummy is 1 if the case contains a material misstament of income and zero for all other audited cases. The findings for income misstatements are similar to the findings for misstatements

## Table I.1: Misstatement Determinants

Table I.1 shows partial correlations of misstatements with case characteristics among audited cases. In columns (1) and (2) the outcome variable is equal to one if any misstatement is found during the audit and zero otherwise. In columns (3) and (4) the outcome variable is one if an income misstatement is found during the audit and zero otherwise. In columns (5) and (6) the outcome variable is one if any other misstatement other than income misstatements are found during the audit and zero otherwise t-statistics in parentheses. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

| | Misstatement | | Income Misstatement | | All Other Misstatements | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| ln(1 + total assets) | 0.014*** | 0.015*** | 0.005* | 0.007* | 0.008*** | 0.009** |
| | (3.93) | (4.02) | (1.85) | (1.95) | (3.27) | (2.61) |
| ln(1 + current income) | -0.001 | -0.000 | 0.001 | 0.001 | -0.002 | -0.002 |
| | (-0.30) | (-0.09) | (0.41) | (0.49) | (-1.26) | (-1.03) |
| ln(1 + average income) | -0.009 | -0.011* | -0.009* | -0.011* | 0.000 | -0.001 |
| | (-1.47) | (-1.90) | (-1.90) | (-1.92) | (0.02) | (-0.20) |
| ln(1 + average expense) | 0.038*** | 0.035*** | 0.033*** | 0.031*** | 0.005 | 0.004 |
| | (5.31) | (4.61) | (5.67) | (4.89) | (1.06) | (0.79) |
| ln(1 + total debt) | 0.011** | 0.008 | 0.011** | 0.011* | -0.000 | -0.003 |
| | (2.15) | (1.39) | (2.33) | (1.89) | (-0.15) | (-0.72) |
| Prior filing | 0.008 | 0.002 | 0.016 | 0.010 | -0.008 | -0.008 |
| | (0.57) | (0.15) | (1.39) | (0.79) | (-1.17) | (-0.95) |
| Pro se | 0.111*** | 0.106*** | 0.085*** | 0.080*** | 0.026* | 0.026* |
| | (4.18) | (3.79) | (4.10) | (3.20) | (1.92) | (1.86) |
| Chapter 7 | 0.033*** | 0.049*** | 0.031*** | 0.041*** | 0.001 | 0.008 |
| | (2.82) | (3.37) | (3.81) | (4.21) | (0.19) | (0.93) |
| Constant | -0.344*** | | -0.291*** | | -0.053* | |
| | (-5.53) | | (-5.42) | | (-1.85) | |
| Observations | 8541 | 7930 | 8541 | 7930 | 8541 | 7930 |
| Adjusted $R^2$ | 0.020 | 0.043 | 0.017 | 0.027 | 0.004 | 0.024 |
| County FE | No | Yes | No | Yes | No | Yes |
| Filing Year FE | No | Yes | No | Yes | No | Yes |
| Cluster | District | District | District | District | District | District |

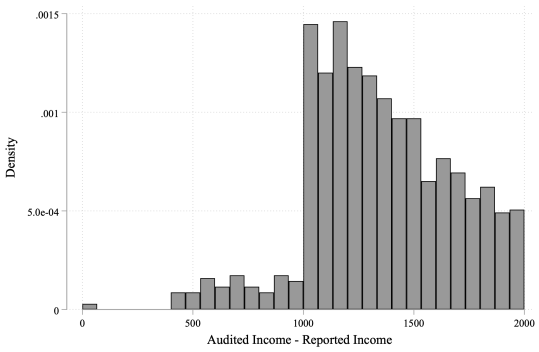
overall. Higher assets, lower income, higher expense, pro-se, and chapter 7 cases are more likely to contain a material income misstatement. One difference to misstatements overall is that higher total debt levels remain marginally significantly associated with income misstatements when including county and filing year fixed effects. Columns (5) and (6) show correlates with all other types of misstatements. The findings for all other misstatements contrast with the findings for income misstatements. While assets remain significantly associated with misstatements, income and expenses are not correlated with other misstatements. This finding reinforces that expenses are positively associated with income misstatements in columns (3) and (4) as it proxies for income. Individuals with higher income also have higher expenses but are less likely to misstate their expenses in the bankruptcy filing. Pro-se casesremain associated with other misstatements while chapter 7 cases are not significantly more likely to contain other misstatements than chapter 13 cases. Jointly, these findings indicate that targeting filings with more assets may improve the rate of misstatements found in general while expense based selection is useful to identify income misstatments.

### I.2 Materiality of Misstatements

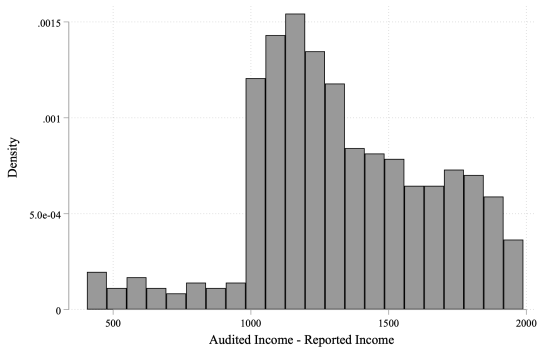
This section infers the materiality threshold for the most common type of misreporting - income misstatements. The USTP privately communicates materiality thresholds for misstatements to be reported to the auditors and does not publicly disclose them. To infer the lower bound, I plot the distribution of income misstatements in Figure I.1. The Figure shows a jump in reported misstatements around a $1,000 discrepancy between audit income and reported income. However, it is important to keep in mind that not all material misstatements will be actionable. The US Trustee will review each of the reported misstatements and determine if follow up actions to sanction the individual need to be taken.

## Figure I.1: Income Misreporting Frequency

This figure plots the income misreporting between $0 and $2,000. Income misreporting is computed as the income reported by the auditor minus the filer reported income. Positive numbers mean that the bankruptcy filer underreported their income. Panel A shows income misreporting for Chapter 7 cases with material income misstatement. Panel B shows income misreporting for Chapter 13 cases with material income misstatement.




**(a) Chapter 7: Income Misreporting**




**(b) Chapter 13: Income Misreporting**

### I.3 Misstatements by Pro-se Status

This section repeats the misstatement analysis from the main text splitting by bankruptcy pro-se status instead of their bankruptcy filing chapter. A concern regarding misstatements is that they are simple errors arising primarily from unsophisticated individuals not knowing how to file for bankruptcy. This section shows that misstatements are not solely concentrated in bankruptcy filers without an attorney to prepare the filing. As Table I.2 shows, most of the material misstatements arise from bankruptcy filings with attorney. The share of misstatements among pro-se filers approximately follows the share of pro-se filers among bankruptcy filers. Table I.1 shows that pro-se filings are more likely to contain a misstatements. However, Table I.2 shows that most misstatemetns are among filers with misstatements. Most misstatements tend to be larger on average among filers with attorney than filers without attorney. It is also noteworthy that virtually all material misstatements in bankruptcies amount to the underreporting of income or the value of assets - only one of the 3,500 inspected reprots indicates a material overstatement of income or asssets.

## Table I.2: Misstatements by Pro-se Status

Table I.2 shows misstatements hand-collected from approximately 3,500 audit reports. Each report can have multiple misstatements. Income are misstatements of income. Account are misstatements of checking, savings, and investment accounts. Vehicles are misstatements of cars and other vehicles. Real Estate are misstated real property. Transfer are general transfers or gifts of assets not falling into reported categories. Personal Property are misstatements of valuable personal items. Other are misstatements that could not be classified into the listed categories. Frequency shows the number of times a type of misstatement is listed in audit reports. Reported shows the average reported valued of the misstated item (zero if not declared). Audited shows the average value of an item according to the auditor if the value could be determined. Misreporting is the average difference between reported and audited value among misstatements. The % column shows the misreporting as a percentage of the audit finding. Panel A shows misstatements among bankruptcy filings with attorney (not pro-se). Panel B shows misstatements among bankruptcy filings without attorney (pro-se).

| Type of Misstatement | Frequency | Reported | Audited | Misreporting | % |
|---|---|---|---|---|---|
| *Panel A: Not Pro Se* | | | | | |
| Income | 2,587 | 6,200 | 9,285 | 3,085 | 33 |
| Account | 879 | 1,172 | 20,206 | 19,034 | 94 |
| Vehicle | 407 | 2,674 | 15,922 | 13,248 | 83 |
| Real Estate | 314 | 4,361 | 159,254 | 154,892 | 97 |
| Transfer | 295 | 154 | 39,799 | 39,645 | 100 |
| Personal Property | 192 | 5,934 | 25,832 | 19,898 | 77 |
| Other | 35 | 873 | 11,585 | 10,712 | 92 |
| *Panel B: Pro Se* | | | | | |
| Income | 127 | 3,451 | 5,951 | 2,500 | 42 |
| Account | 33 | 2 | 3,240 | 3,239 | 100 |
| Real Estate | 21 | 0 | 117,979 | 117,979 | 100 |
| Vehicle | 15 | 1,521 | 11,902 | 10,381 | 87 |
| Personal Property | 14 | 0 | 13,538 | 13,538 | 100 |
| Transfer | 6 | 0 | 137,983 | 137,983 | 100 |
| Other | 1 | . | . | . | . |