NBER WORKING PAPER SERIES

SHAMING TAX DELINQUENTS

Ricardo Perez-Truglia
Ugo Troiano

Working Paper 21264
http://www.nber.org/papers/w21264

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
June 2015, Revised March 2018

We thank the MITRE Anonymous Donor for financial support, Mary Mangum, Sam Russell and the Office of the General Counsel at the University of Michigan for invaluable support for the field experiment. Special thanks to Manisha Padi and Joel Slemrod for very useful feedback in the early stage of the project and for detailed comments. We also thank feedback from Nageeb Ali, David Autor, Charlie Brown, Raj Chetty, Magali Delmas, Mihir Desai, Leopoldo Ferguson, Mary L. Gray, Jonathan Gruber, Jim Hines, Caroline Hoxby, Wojciech Kopczuk, Greg Lewis, Day Manoli, Ben Marx, Nolan Miller, Markus Mobius, J.J. Prescott, Matthew Rabin, Justin Rao, David Rothschild, Jeff Smith, Alan Viard, Duncan Watts, Matthew Weinzierl, Glen Weyl and seminar participants at Harvard Business School, UCLA Anderson, Columbia University, University of California San Diego, Boston University, Barcelona IPEG, Microsoft Research New England, Microsoft Research New York, University of Michigan, Universidad Nacional de Los Andes, University of Illinois at Urbana-Champaign, Society for Economic Dynamics Annual Meeting, ASSA Annual Meeting, RIDGE Workshop on Political Economy and the NBER Summer Institute. This project was reviewed in advance and declared exempted from IRB oversight at the University of Michigan. We thank Will Boning and Kamala Kanneganti for outstanding research assistance. A previous version of the paper circulated under the title “Tax Debt Enforcement.” The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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NBER Working Paper No. 21264
June 2015, Revised March 2018
JEL No. H26,H63,K42

ABSTRACT

Many federal and local governments rely on shaming penalties to achieve policy goals, but little is known about how shaming works. Shaming penalties may be ineffective or may backfire by crowding-out intrinsic motivation. In this paper, we study shaming in the context of the collection of tax delinquencies. We sent letters to 34,334 tax delinquents who owed half a billion dollars in three U.S. states. We randomized some of the information contained in the letter to vary the salience of financial penalties, shaming penalties and peer comparisons. We then measure the effects of this information on subsequent payment rates. We find that increasing the visibility of delinquency status increases compliance by individuals who have debts below $2,500, but has no significant effect on individuals with larger debt amounts. Financial reminders have a positive effect on payment rates independently of the size of the debt, while information about the delinquency of neighbors has no effect on payment rates.

Ricardo Perez-Truglia
Anderson School of Management
University of California, Los Angeles
110 Westwood Plaza
Los Angeles, CA 90095
and NBER
ricardo.truglia@anderson.ucla.edu

Ugo Troiano
Department of Economics
University of Michigan
611 Tappan Street, Lorch Hall 219
Ann Arbor, MI 48109
and NBER
troiano@umich.edu

1 Introduction

Many federal and local governments rely on shaming as a penalty to achieve policy goals. Yet, little is known about whether shaming penalties work as intended. Indeed, there are reasons to believe that shaming could be ineffective, for example, anti-social individuals may not care about social sanctions. Shaming penalties could even backfire, for example, if they crowd-out the intrinsic motivation to do the right thing. In this paper, we focus on shaming penalties in one important context in which they have been widely implemented: the collection of tax delinquencies. To do so, we implemented a field experiment with 34,334 tax delinquents from three U.S. states who collectively owed half a billion dollars.

Tax delinquencies are the debts incurred by citizens with the tax administration. Even though they have been under-studied relative to other aspects of tax collection, such as tax evasion and tax avoidance, tax delinquencies plays an important role in the tax collection process. Even in the United States, a country where the debt collection tools are believed to be effective, delinquent taxes still comprised more than 25 percent of the total gross tax gap in 2006.[1] Moreover, tax delinquencies are the potential tax revenues that are most readily available to tax enforcement agencies. As a result, tax agencies invest substantial resources in policies aimed at reducing tax delinquency.

Some of the most traditional tools used to collect tax delinquencies are financial penalties and income garnishment. Additionally, tax delinquencies are collected through shaming penalties, which consists of publicly revealing the identities of tax debtors. For instance, as of January of 2015, twenty-three U.S. states maintained shaming lists on the Internet with the names, addresses, and other information of individuals and businesses with delinquent taxes (see Table 1 for a list of states and more details). Other local and national governments around the world use similar penalties.[2] Despite the popularity of shaming penalties, to the best of our knowledge, there is no evidence that they work as intended. In this paper, we address this question by studying the effect of the salience of shaming penalties.

Studying the effectiveness of financial and shaming penalties in the context of tax delinquency is a challenging task. The ideal experiment would consist of taking a sample of

[1]The U.S. Treasury reported $46 billion in underpayment of declared taxes and $65 billion in enforced and other late payments as of 2006. The tax gap amounts to $450 billion dollars, which in addition to the previous items includes nonfiling and underreporting. Source: U.S. Department of the Treasury, Internal Revenue Service (2012), "Updated Estimates of the TY 2006 Individual Income Tax Underreporting Gap. Overview," Washington, D.C.: Office of Research, Analysis, and Statistics.

[2]For example, online lists of tax delinquents are or have been published by local or national governments in Argentina, Bosnia and Herzegovina, Croatia, El Salvador, Greece, Macedonia, Mexico, Montenegro, Portugal, Serbia, Slovenia, Spain and the United Kingdom. Other countries, such as Canada, Ireland, Italy and New Zealand, published lists of tax evaders in newspapers or newsletters. A notable example is the city of Bangalore, India, which hires drummers as tax collectors to visit the homes of tax evaders and to bang the drum if they refuse to pay.

taxpayers and randomly assigning different penalties. Unlike the randomization of audit probabilities, however, randomizing the financial and shaming penalties would imply punishing the same crime differently, which may be infeasible for legal and other reasons.[3] Instead, our research design consists of using a sample of existing tax delinquents and sending them letters by mail. These letters were identical except for a few key pieces of information that were cross-randomized to vary the salience of financial incentives, shaming incentives and peer comparisons. We then estimated the effects of these random variations on the probability of paying the tax debt by using publicly available data to identify whether the subjects were still listed as delinquent after they received our letters.

We sent letters to 34,334 individuals from the online lists of tax delinquents published by the states of Kansas, Kentucky, and Wisconsin. These letters were sent independently by the research group, without mentioning the tax agency.[4] Individuals in this sample owed between $250 and $150,000, with a median of $5,500. All of these tax delinquents had already been informed by the tax agencies that their information, including full name, address and debt amount, had been listed online.[5] Indeed, these subjects had been delinquents for years, despite numerous solicitations from the tax agency, financial penalties and, possibly, failed collection attempts through income garnishing. For example, subjects in Kentucky had been delinquent for an average of 2.7 years and faced annual interest rates of up to 30 percent.[6]

We sent letters to all individuals from our subject pool, but we cross-randomized the information contained in the letter. The first treatment arm was designed to study the shaming incentives through the social interactions channel. We altered the visibility of recipients' delinquency status toward the neighbors of the delinquents. We randomized subjects into two treatment groups. In the first treatment, the delinquent was the only individual in the area who was randomly chosen to be informed about the online list of delinquents. The second treatment was identical in every respect, except that the delinquent's neighbors were also randomly chosen to be informed about the online lists of delinquents. Compared to the first treatment group, the second treatment should make delinquents feel more monitored by their neighbors and, if social pressure works, it should make them more likely to pay off their debts.

It is important to note that our experiment was not designed to measure the average

[3]For example, tax authorities have co-operated in the past with researchers for randomizing audit probabilities in the context of tax evasion (Slemrod et al. (2001); Kleven et al. (2011)).

[4]Note that our delivery method differs from most of the literature on tax compliance involving mailing experiments, in which the letters are sent from the tax agency. It is possible that some of our results would have been different if the letters had been sent directly by the tax agency. For example, the letters from the tax agency could be more effective if taxpayers trust the tax agency more than they trust researchers.

[5]Tax agencies are required to notify delinquents before disclosing the identity of tax delinquents.

[6]Kentucky is the only state that publishes the date when the delinquent debts were originated.

treatment effect of the introduction of the shaming policy. First, our experimental design measures the effect of shaming on a specific group of the population, delinquents who were already being listed online, but the shaming policy can also affect other individuals – for example, the website can deter individuals from being listed in the first place. Second, our experimental design studies the effects of shaming incentives through a specific channel, exposure to neighbors, while the introduction of the shaming policy may also affect behavior through other channels.

The second treatment arm was designed to create exogenous variation in the knowledge and salience of financial penalties. It is documented in a variety of settings that subjects systematically underestimate the financial penalties (Stango and Zinman (2011); Frank (2011); Ausubel (1991)) and are inattentive about financial penalties (Karlan et al. (2014)). In the first treatment, the letter contained a message summarizing the financial penalties incurred by the debt. The second treatment group was identical in every respect, except that it did not include the message about the financial penalties. If individuals care about financial penalties but were inattentive about them, adding the financial reminder to the letter should increase the likelihood of paying the debt off.

The third and final treatment arm was designed to create exogenous variation in peer comparisons. If delinquents use the online lists of tax delinquents to compare their own debt amount to the amounts owed by other delinquents, that comparison may affect their decision to pay. For instance, a delinquent who learns that the other delinquents in her area owe larger amounts may feel less guilty about not paying her own debt. This mechanism could change, for better or worse, the effects of shaming policies. To measure this mechanism, our experimental letters included some information about the delinquent behavior of others. Using a non-deceptive method, we created random variation in the amounts owed by the individuals listed in the letter. We can test whether, consistent with the social norm hypothesis, payment rates go down when delinquents observe that their neighbors owe larger amounts.

First, our evidence suggests that the salience of the shaming penalties can increase the probability of repayment. For delinquents in the first quartile of the debt distribution ($250–$2,273), the higher visibility increased the probability of repayment ten weeks after mail delivery by 2.1 percentage points. This effect is statistically significant at the 1% level, and also economically significant: the 2.1 percentage points effect amounts to 21 percent of the average payment probability. Given that our visibility treatment was so mild, this effect size is remarkable. Among individuals in the other three quartiles ($2,274–$149,738), the effect of the visibility treatment on the payment rate was close to zero and statistically insignificant. In other words, there is a limit to how much an individual would be willing to

pay in order to protect her reputation in the eyes of neighbors. Indeed, this interpretation is consistent with the prior beliefs of the tax agencies that implemented these shaming penalties. For example, in an interview for a newspaper, the Vermont deputy tax commissioner made the following declaration: "When you are talking about large debts, you do tend to get some people who just don't care. It's just not worth playing off their $450,000 or $1.2 million debt. Down on the lower levels, you get more of the Average Joe who is concerned."[7]

Second, our evidence suggests that individuals respond to financial reminders: over the same ten week period, the financial reminders increased the probability of payment by 0.7 percentage points (about 7 percent of the average probability). The effectiveness of financial reminders did not depend on the debt amount, which is consistent with the fact that financial penalties are proportional to the debt amounts. Also, the effect of the financial reminder was significantly higher in Kentucky, where the financial penalty was significantly higher. Third, we find suggestive evidence that peer information may not have a significant effect on the decision to pay tax debts: showing delinquents that their delinquent neighbors owe larger amounts does not have a significant effect on payment rates.

This paper builds on and relates to the tax enforcement, social incentives and behavioral public finance literatures. A growing body of evidence shows that tax compliance can be increased by a range of policies such as auditing (Slemrod et al. (2001)), third-party reporting (Kleven et al. (2011)), paper trails (Pomeranz (2015)), cross-checking (Bø et al. (2015); Carrillo, Pomeranz, and Singhal (2012); Drago, Mengel and Traxler (2011); Naritomi (2015)), and satellite detection of unregistered properties (Casaburi and Troiano (2015)). We contribute to this literature by being the first to study shaming as a policy to increase tax compliance.

This paper is also related to a literature showing that social incentives are significant in pro-social context such as voting, charitable and political contributions and energy conservation (Gerber, Green and Larimer (2008); DellaVigna et al. (2012); Delmas and Lessem (2014); Perez-Truglia and Cruces (2013); Allcott (2011)). However, it is unclear whether social incentives would also be significant in the context of anti-social behavior, which is the most relevant context for shaming penalties. We provide evidence suggesting that social pressure is effective in the context of anti-social behavior but peer comparisons are not effective. Last, this paper is related to a growing literature on behavioral public finance that proposes the use of less traditional incentives, such as social incentives, to attain public policy goals (Chetty, Saez and Sandor, 2014; Chetty, 2015).

The paper proceeds as follows. Section 2 introduces the institutional framework, the data

[7](Source: "To Collect Revenue, Some States Put Tax Scofflaws in Virtual 'Stocks'," Stateline, May 28, 2015).

sources and the experimental design. Section 3 presents the experimental results. The last section concludes.

2 Design of the Field Experiment

2.1 Data Sources and Subject Pool

At the time of our field experiment, twenty-three of the fifty U.S. states published online lists of tax delinquents (we present the full list in Table 1). From these twenty-three states, we selected our experimental states based on the following criteria.[8] Each state list delinquents with debt amounts above a certain threshold. First, we restricted our sample to thresholds of $5,000 or less, so that the list included a significant number of subjects. Of the five states that satisfied this criteria, we had to exclude two of them: we excluded Georgia because it does not publish the precise street address and thus it would be impossible for us to mail letters; and we excluded Indiana because we wanted to focus on individual delinquents and this state lists business delinquents only.

The resulting experimental subjects were the delinquents listed in the states of Kentucky, Kansas, and Wisconsin. Some differences exist in the way the program is implemented across the three states, as discussed in more detail in Appendix A.3. The main difference among the states is the debt threshold above which delinquents are listed: $250 in Kentucky, $2,500 in Kansas, and $5,000 in Wisconsin.[9] In our subject pool, 52.7 percent of subjects are from Kentucky, 25.4 percent are from Kansas, and the remaining 21.9 percent are from Wisconsin.

We downloaded the online lists of individuals for these three states on May 26, 2014. At that point, the online lists included 57,744 individual tax delinquents who owed $968,764,474 to departments of revenue in the three states. We ex-ante excluded the following individuals from the subject pool: (i) individuals with unreliable address information; (ii) records with full names corresponding to multiple addresses in the same state, because we could not

[8]The existence of these thresholds suggests that an alternative research design could exploit the exogenous variation generated by these discontinuities. A first limitation of such a design would be that delinquents right above the threshold receive an additional letter from the state, that not only informs them about the shaming policy, but also reminds them about their tax debt and other information unrelated to the shaming policy. Therefore, such a design would not be ideal to study the effect of the shaming policies. A second limitation of such a design would be that those results would not necessarily be externally valid to delinquents owing amounts that are farther away from the threshold. Indeed, our experimental results suggest that effects of shaming may vary sharply with the size of the debt.

[9]In Wisconsin, the public list at its inception in 2006 included delinquent taxpayers who owed more than more than $25,000 while, on January 2008, the threshold was lowered at $5,000. The Communications Officer of the Wisconsin Department of Revenue declared that the policy had been highly successful at increasing collected tax revenues, as one of the reasons to explain the lowering of the threshold (Communications Officer Press Release December 26, 2007, Wisconsin Department of Revenue).

confirm whether they corresponded to the same or different individuals; (iii) individuals living in Wisconsin whose debts were not for state income tax; (iv) individuals who moved out of the state; and (v) individuals with debts exceeding $150,000. We sent letters to a sample of 34,334 delinquents.[10]

The publication of lists of tax delinquents is regulated by state legislation.[11] In these three states, tax agencies are required by law to notify taxpayers and to allow time for payment before they are publicly listed. It is thus reasonable to assume that most subjects knew that they were included in these online lists of delinquents. The consent of the taxpayers, however, is not needed before the publication. The websites contain an e-mail address and a phone number that exposed tax delinquents can contact to pay off their debts and be removed from the lists. Once an individual is listed, his or her information is updated on a weekly basis, to reflect revisions to the original debt, new debts, interest, penalties, and fees. The tax agencies do not attempt to list individuals right after the tax obligation originated, but after a few months (for details see Appendix A.3). As a result, the individuals in our subject pool had already received notifications from the tax agencies about their tax obligations, and the tax agency may have already tried but failed to garnish their incomes.

Our subjects had been delinquents for years. For example, subjects in Kentucky (the only state publishing the exact date when the debt was originated) had been delinquent for an average of 2.7 years (median of 2 years). In addition to other fees, the amounts owed by our subjects were subject to annual interest rates of around 30 percent, 12 percent, and 18 percent in Kentucky, Kansas, and Wisconsin, respectively. The mean initial debt amount (i.e., as of May 26, 2014) ranged from $250 to $150,000, with a median of $5,500 and a mean of $13,000. The online lists do not include any demographic information about the delinquents. Using auxiliary data on the distribution of gender and ethnicity by first and last names, we estimate that our sample of delinquents is 35 percent female and 71 percent white. These figures imply that delinquents are somewhat more likely to be male and non-white relative to general population in these three states.[12] We do not have data on the subjects' incomes, although the fact that tax debts originated primarily from state income tax suggests that the income of these individuals is above the tax-exempted level.[13] For this same reason, if

[10] About 150 letters were returned to us as undeliverable. The results are virtually unchanged if we exclude ex-post these individuals.

[11] A related policy is the one of shaming sex offenders. There is a large literature focused on estimating the effects of shaming sex offenders, which we do not summarize here – e.g., Schram and Milloy (1995) and Adkins, Huff, and Stageberg (2000).

[12] In comparison, the frequency of females and whites in these three states are respectively 50 percent and 85 percent.

[13] In Kansas, only individuals who owe state income tax debts are listed. In Kentucky, the debts can be originated with non-income taxes, but it is not specified in the list. Even though there are no public statistics, private communications suggest that most delinquents from Kansas on the list had debts originating from

an individual owes a small tax debt, that does not necessarily imply that the debtor is poor.

2.2 Basic Descriptive Analysis of the Delinquency Rates

To the best of our knowledge, this is the first time that these administrative data on delinquents have been used for research. The determinants of tax delinquency are understudied relative to other aspects of tax compliance such as tax evasion and tax avoidance. In this section, we use this administrative data to provide some basic descriptive analysis.

We created a database with the number of delinquents living in each 5-digit ZIP code of the three experimental states as of May 26th 2014 (the date in which we formed our experimental sample). Given that the three states have different debt thresholds, and to keep the delinquency rates comparable we compute delinquency rates of debts over $5,000, which coincides with the highest of the three thresholds.[14] We estimate a negative binomial regression of the number of delinquents in a ZIP code on the logarithm of population and a few additional place characteristics that we believed to be important for understanding tax delinquency. All these variables, except population, have been normalized to have a zero mean and standard deviation of one. As a result, the coefficient on each of these variables can be interpreted as the effect of a one standard deviation increase in the covariate on the log of the expected number of delinquents.

A first characteristic of interest is *Mean Income*, which corresponds to the mean gross income in the ZIP code. *Share of Wage Income* is the share of income originating from wage income in the ZIP code, constructed with the Internal Revenue Service Statistics of Income database for 2012, and it is intended to proxy for income garnishability. *EITC Bunching* is a proxy for the propensity for tax evasion, computed as the share of self-employed individuals in the 3-digit ZIP code estimated to be mis-reporting income to take advantage of EITC benefits (see Chetty, Friedman, and Saez (2013)). *Share Republican* is the share of Republican votes in the 2012 U.S. Presidential Election, at the county-level. *Civic Life Index* is a county-level measure of social capital based on a number of indicators such as the density of civic and non-profit organizations (Rupasingha et al., 2006).

Table 2 shows the regression results. Column (1) corresponds to the pooled data for the three states. The coefficient on *Log(Population)* is close to one and precisely estimated, indicating that increasing the population in the ZIP code by 1% would increase the expected number of delinquents by 1%. The coefficient on *Mean Income* is close to zero and statisti-

state income tax. In Wisconsin, the list includes delinquents for both income and a variety of other taxes (e.g., estate tax). To improve the similarity across states, we ex ante excluded from the subject pool delinquents with debts not originated from state income tax.

[14]The mean number of delinquents with debts above $5,000 per 1,000 inhabitants is very similar across states: 2.27 in Kentucky, 2.31 in Kansas and 2.54 in Wisconsin.

cally insignificant, which is consistent with the view that tax delinquents are not limited to any particular income group. Given that wage income is easier to garnish by the tax agency, areas with a higher share of wage income should find it more difficult to avoid income garnishability and thus should have fewer delinquents. Consistent with this prediction, the coefficient on *Share of Wage Income* is negative and statistically significant (p-value<0.01). The magnitude of this correlation is also economically significant: a one standard deviation increase in *Share of Wage Income* decreases the expected number of delinquents in the area by about 8%. The coefficient on *EITC Bunching* is positive and statistically significant (p-value<0.01). The magnitude of this correlation is large: a one standard deviation increase in *EITC Bunching* reduces the expected number of delinquents in the area by about 17%. This finding suggests that the type of individuals who take advantage of opportunities to evade taxes may be the same type of individuals who take advantage of opportunities to avoid tax collection – for example, they may be more sophisticated agents, or agents with lower moral constraints. The coefficient on *Share Republican* is negative and both statistically and economically significant (p-value<0.01) suggesting that partisanship may play a role in shaping tax compliance (Cullen, Turner and Washington (2015)). The coefficient on *Civic Life Index* is negative and both statistically and economically significant (p-value<0.01). This negative correlation between tax delinquency and social capital suggests that intrinsic motivation, such as civic responsibility, may play a significant role in the decision to pay tax obligations (Putnam (2001); Casaburi and Troiano (2014)).[15]

In principle, the institutional context and regulation for tax collection may vary so much across states that there could be significant differences in the relationship between tax delinquency and the covariates across states. Columns (2) through (4) shows the results for each state on a separate basis. The results indicate that the majority of the correlations are qualitatively similar across states.

2.3 Experimental Design

Appendix 5 provides a snapshot of a sample letter and a sample envelope. The first paragraph explained that the letter was part of a research study about tax delinquency. Both the envelope and the letter included a logo of the Department of Economics at the University of Michigan, with the hope of making it clearer to the recipients that this was a legitimate communication from researchers. The letter also included a table listing ten tax delinquents from the recipient's geographic area, always including the recipient. All delinquents were identified by full name and in ascending order by debt amount, with the recipient's row

[15]On the relationship between cultural norms and tax compliance see also: Torgler (2003); Spicer and Becker (1980) and Alm, Jackson, and McKee (1992).

highlighted. The second paragraph identified the corresponding state's revenue department as the data source, with an explanation that "Names, addresses and other details about tax delinquents are freely available to see for anyone with access to the Internet. You can search for individual debtors by first and last name, or by ZIP code, by visiting the following webpage (...)." The second page of the letter contained a screenshot of this online search tool (for illustration purposes) and some additional information to reinforce the perceived legitimacy of the letter (e.g., the researchers' contact information, a link to the project's website, and a link to an online survey).[16]

Every subject is sent a letter that looks like the sample letter from Appendix 5, only that we cross-randomized three aspects of the information contained in the letter: the letter may or may not be sent to the neighbors of the recipient; the letter may or may not include information about financial penalties; and the amounts owed by the delinquents listed in table may be lower or higher. After the letters were sent, we measured how each piece of information affected the probability that the recipient was taken off the list.

Note that our experimental design does not compare individuals who did receive a letter to those who did not, because of the difficulties to determine the exact mechanism through which the letter can affect behavior. Additionally, as written above, the respective revenue departments had already notified all of our subjects that they were going to be included in the list of delinquents, so it is plausible to assume that informing them about the existence of the list would not affect their behavior. Last, our experimental design does not inform about the effect of introducing these penalties.

Table 3 summarizes the three treatment arms, including the number of subjects in each sub-treatment. The first treatment arm was designed to alter the visibility of the recipient's delinquency status with respect to their neighbors. The letter always include information intended to make the shaming penalty salient, such as the list of delinquents from the area and the snapshot of the online search tool. Conditional on this baseline salience, we randomize the visibility of the search tool in the eyes of neighbors. We followed the design in Perez-Truglia and Cruces (2013) by constructing a lower visibility message and a higher visibility message, which was prominently displayed in a box located right below the list of contributors:

Lower Visibility: "Your household was the only household randomly chosen from your

[16]The website provided basic information about the research project, and contact information to reach the research team. The main purpose of the website was to provide contextual information about our study to interested subjects, and to dissipate any doubts about its legitimacy, emphasizing its academic and non-partisan nature. Although the website provided some general information about the main research objective, to avoid the contamination of the experimental results, it did not provide any details about the precise hypotheses to be tested, nor about the existence of several different treatment types. We don't report the survey results because of its extremely low response rate (0.2 percent), but these results are available upon request.

area to receive a letter of this type."

Higher Visibility: "Your household and other households in your area were randomly chosen to receive a letter of this type."

Neither of these messages was deceptive; we divided the U.S. territory into small areas by 9-digit ZIP codes and then, consistent with the message, we randomized whether exactly one household from the area was selected to receive a letter, or whether all households in the area were selected to receive a letter.[17] We test the social interactions channel by comparing the behavior of individuals receiving the higher visibility statement to those receiving the lower visibility statement. The only difference between the lower visibility and higher visibility groups was that recipients in the latter group were informed that their neighbors also received information about how to access the online list of tax delinquents. The purpose of this notification was to make the recipients feel more monitored by their neighbors. The hypothesis is that, when feeling monitored by neighbors, delinquents will be more likely to pay up because of the fear of losing reputation in the eyes of neighbors. For a formalization of this argument, we offer a simple social signaling model in Appendix C.

Our visibility intervention was not meant to maximize the response to the letters, but to estimate a precise channel through which publishing lists of delinquents may affect payment: social interactions. This final design was the result of the collaboration with the Institutional Review Board and attorneys at the Office of the General Counsel at the University of Michigan, taking into account a number of ethical, legal, budget and practical constraints. In practice, the shaming penalties used by the government are expected to have a much larger effect on visibility and payment rates than our mild experimental intervention. For instance, our intervention increases visibility only among neighbors, while the online lists of delinquents published by the government increases visibility (arguably) among a much broader set of social contacts, including friends, relatives and coworkers.

It is plausible that, unlike the financial penalty, the cost of the shaming penalty is not proportional to the debt amount. Intuitively, assume that by getting off the list you can "savage" $500 worth of respect from your neighbors. As a result, to be taken out of the delinquent list it may be worth paying off a debt of $250, but it may not be worth paying off a debt of $150,000. Consistent with this observation, the social interaction model presented in Appendix E.3 predicts that the effectiveness of the shaming intervention should be inversely proportional to the debt amount. This observation is also consistent with anecdotal evidence

[17]Note that the probability of assignment to the message is conditional on the number of delinquents in the area, which we always include as a control variable in the regressions. Also, we chose the number of areas to be assigned to each group as to generate roughly the same number of letters in each of the two treatment groups.

from the tax agencies.[18]

In any case, how the effects of shaming scale up with the debt amount is ultimately an empirical question. This question is related to the broader literature on social incentives, because one of the most common criticisms to this literature is that social incentives may only be significant for low stakes (Levitt and List, 2007).[19] One advantage of our empirical setting is that the amounts owed by our subjects vary extensively, from $250 to about $150,000, allowing us to provide suggestive evidence about how the effect of higher visibility scales up with the stakes.

The second treatment arm is related to financial penalties. To measure the effect of financial penalties, the ideal experiment would consist of randomizing the individual's interest rate. Because randomizing the financial penalties was not feasible, we instead created exogenous variation in the knowledge and salience of this information by altering the letter as follows. The letter included or excluded a message with a brief summary of the interest rates applied to the recipient's debt amounts. To make this information more salient, the message was printed in boldface, below the snapshot of the search tool. We then compare the behavior of individuals who received a letter with this reminder to those who received a letter without this reminder.

For example, the financial reminder for Wisconsin recipients was the following: "This website also includes information about penalties. For instance, your tax debt is subject to, among other penalties, an annual interest rate of 18%." The corresponding interest rates were 12% in Kansas and 30% in Kentucky. In comparison, the U.S. average for the annual interest rate on a credit card was 14% (Source: CreditCards.com, accessed on January 5, 2015).[20] If, on average, delinquents underestimated or ignored the size of the financial penalties, then our reminder about financial penalties could make the recipients pay sooner. Indeed, there is evidence that people underestimate interest rates in many markets (Ausubel (1991); Stango and Zinman (2011); Frank (2011)) and that they are inattentive about interest rates (Karlan et al. (2014)).

Last, the third treatment arm explores the role of peer information. By publishing the list of tax delinquents, a delinquent's payment decision may be affected because the information

[18]See for example: "To Collect Revenue, Some States Put Tax Scofflaws in Virtual 'Stocks'," Stateline, May 28, 2015)

[19]For instance, DellaVigna et al. (2015) estimate that the social signaling value of voting is between $5 and $15, and DellaVigna et al. (2012) estimate that the social signaling value of charitable giving to a door-to-door solicitor is between $1.40 and $3.80. As a proportion of the average cost of voting and giving, these values of social signaling are significant. However, it is unclear how these social incentives would scale up with higher stakes.

[20]Individuals using less conventional sources of credit, which presumably would be the most liquidity-constrained individuals, can pay several times this rate; for example, the average annual interest rate for payday loans is estimated to be over 100% (Stegman (2007)).

contained in the list changes the delinquent's perception about the delinquent behavior of others. For instance, individuals have been documented to behave more pro-socially when they perceive that others behave pro-socially (e.g., Frey and Meier, 2004; Perez-Truglia and Cruces, 2013).[21] Alternatively, an individual may react to peer information because that information changes her perceptions about the costs and benefits of being a delinquent – for instance, observing that other people decided not to pay may serve as a negative signal about the effectiveness of the enforcement tools.

To test the effect of peer information, we created some exogenous variation in the recipient's perception of the delinquent amounts owed by others. To do this without being deceptive, we followed the methodology from Perez-Truglia and Cruces (2013). For each recipient, we identified her or his twenty closest delinquents. The nine neighbors to be shown in the table were selected by ordering the twenty closest delinquents according to a composite index, and then selecting the top nine delinquents from the ordered list. The index is comprised of a random term plus the debt amount of the individual weighted by a parameter α. Using a higher α results in a table with nine delinquents with higher debt amounts. Table 4 illustrates this, by showing the tables of delinquents that would be shown to a given recipient under three different values of α.

We randomly assign each recipient to $\alpha = -1$, $\alpha = 0$ or $\alpha = 1$, with equal probability. By generating random variation in α, we generate random variation in the the distribution of amounts owed by the delinquents shown in the table. According to the social norms hypothesis, being randomly assigned to a higher value of α should reduce the recipient's subsequent probability of paying off her debt.

2.4 Outcome of Interest and Econometric Specification

Once an individual is listed online as a tax delinquent, the main way to get off the list is to pay upfront the entire amount or enter a payment plan for the full amount and pay the first installment. That is, individuals cannot get off the list by paying the difference between the debt and the disclosure threshold. This feature of the law is clearly explained in the websites of the tax agencies.[22]

[21] In the public finance literature, the peer information interventions normally provide information to potential or discovered tax evaders on the population-wide extent of compliance or non-compliance. Such social comparisons are meant to highlight that the tax evader is part of a minority of non-compliers, and that his/her behavior goes against the social norm. Instead, we restrict our analysis, and our conclusion, to a population of tax delinquents.

[22] For the statutory evidence, see in Appendix A.3. For empirical evidence, see Appendix A.1. Also, note that even if someone had access to a loophole to pay just enough to take the debt amount slightly below the disclosure threshold, that would only result in being taken off the list for a short time period, because the financial penalties would accumulate over time and take the total amount back above the threshold.

For this reason, our dependent variable is a dummy variable for whether a delinquent is listed online as a delinquent at a given point in time.[23] We interpret changes in this variable as a indication of either paying back the debt in full or agreeing to a repayment plan for the full amount, although we do not have data on the relative composition of these two.[24] The baseline econometric specification is given by:

$$Y_i^t = \theta + \sum_{j=1}^{4} \beta_j Q_i^j V_i + \sum_{j=1}^{4} \gamma_j Q_i^j F_i + \sum_{j=1}^{4} \mu_j Q_i^j P_i + \sum_{j=1}^{4} \phi_j Q_i^j + \delta X_i + \epsilon_i \quad (1)$$

The outcome variable (Y_i^t) is a dummy variable that takes the value and 100 if the individual is listed online if the individual is included in the online list of tax delinquents t weeks after the letters were sent, and 0 otherwise. The dummy for higher visibility (V_i) takes the value 0 if the recipient was the only one in the area chosen to receive a letter and 1 if others in the area were chosen to receive a letter too. The dummy for financial reminder (F_i) takes the value 1 if the letter included the financial reminder and 0 if not. The peer information (P_i) variable takes the value 0, 0.5 or 1 depending on whether the individuals to be shown in the table of delinquent neighbors we selected with a parameter $\alpha = -1$, $\alpha = 0$ or $\alpha = 1$, respectively. Note that, we allow the treatment effects to differ by the quartile of the initial debt amount ($\left\{Q_i^j\right\}_{j=1}^{4}$). Finally, X_i is a vector of controls, including variables such as state dummies and the initial debt amount.

2.5 Random Assignment

Randomization into treatment groups was conducted so that all members of the same household were assigned to the same treatment group, and the randomization was stratified at the 3-digit ZIP code level. In Table 5, we present some descriptive statistics across treatment groups to check that the treatment assignment was balanced in observable pre-treatment characteristics. The only pre-treatment outcome that we observe directly is the initial debt

[23] We must note that it is not uncommon for delinquents to leave the list to then re-enter a few months later, after contracting new tax debt with the government. For example, 9.3 percent of our subjects leave the list temporarily during the 37 weeks after the sample began (May 26). Our empirical model does not treat exiting as an absorbing state. An individual may have exited the list at week 5 (in which case the dependent variable takes the value 1) and re-entered by week 10 (in which case the dependent variable goes back to 0). In the traditional survival models, the individual who exits is counted as exiting forever. As a result, estimating a survival model would make it look like the effects are larger than they actually are.

[24] There are some alternative ways to get off the list, such as due to death, bankruptcy or surpassing the 10-year limit since the debt was originated. Even though we do not have direct data on the share of individuals leaving the list due to these reasons, conversations with officials of the tax agency indicate that a very small minority leaves the list through these mechanisms. Appendix A.3 discusses in more detail the specific laws and requirements. Additionally, this appendix provides graphs with the week-by-week evolution of this outcome variable in the subject pool.

amount (and its logarithm). Additionally, we included other variables that we did not observe directly but could impute from secondary data sources, such as gender and ethnicity.[25] This table includes the p-value of a test of the null hypothesis that the average characteristics are the same across all seven treatment groups. As expected from random assignment, the individuals were balanced on pre-treatment characteristics, with the exception of a small difference in the percentage of African-Americans.[26] As an additional robustness check, we present falsification tests by estimating the "effects" of the treatments on the initial debt amount (i.e., the debt amount as of three weeks before the letters were delivered).

3 Results

3.1 Effect of Higher Visibility

Figure 1.a presents the effects of higher visibility (i.e., the difference between higher-visibility and lower-visibility treatment groups) on the probability of leaving the list ten weeks after the letters were sent. The effects are broken down by quartiles of the initial debt amount, as described in equation (1). For reference, Table 6 shows the corresponding average payment rates, as well as regressions results, in table form. Figure 1.a shows that, for the lowest quartile ($250–$2,273), the higher visibility increased by 2.1 percentage points the payment rate ten weeks after mail delivery. This effect is statistically significant at the 1% level, and compared to the baseline rate of 10 percentage points, suggests an economically significant effect of 21 percent of the baseline rate. The effect of higher visibility, however, was estimated to be very close to zero and statistically insignificant for the other three quartiles of the initial debt amount.

As a falsification test, Figure 2.a shows the same average treatment effects of Figure 1.a, but on the logarithm of the initial debt amount, three weeks before the experimental letters were delivered. As expected, the "fake" treatment effects are not statistically significant for any quartile of the initial debt amount.

Since the financial and shaming penalties differ across states, the effects of our reminders may vary by state.[27] To explore this, Figure 3 presents the results on state-level heterogeneity.

[25]Data for these characteristics is imputed using data on the joint distribution of first names and gender (several sources, including data from the U.S. Census Bureau), and the joint distribution of last names and ethnicities (data from U.S. Census Bureau).

[26]The null hypothesis of equality is rejected statistically for one of the seven individual characteristics, the percentage of African-Americans, albeit the size of the difference is small and one rejection may be due to chance given the the large number of combinations between treatment groups and individual characteristics. In any case, we always control for the percent of African-Americans in the analysis, but the results are similar if we do not control for this variable.

[27]For instance, the Kentucky website features a search tool to search individuals by name, lien balance

Given that the distribution of the debt amounts is so different in Kentucky compared with the other two states, we separated the heterogeneity by state from the heterogeneity by debt amount. We did this by splitting the Kentucky sample in two: initial amounts between $250 and $2,500 and initial amounts above $2,500. The resulting four groups each contain about one quarter of the sample. We use $2,500 to split the Kentucky sample, because it corresponds to the disclosure threshold for Kansas, but the results are very similar if we instead split the sample using the first quartile ($2,273), as before.

Figure 3.a presents the state-level heterogeneity for the effect of higher visibility. Although debtors in Kentucky with debts below $2,500 reacted to the higher visibility, debtors in Kentucky with debts above $2,500 did not react to the higher visibility, and neither did debtors with debts above $2,500 in Kansas and Wisconsin. These results suggest that, even within Kentucky, the effects of higher visibility decline significantly with the debt amount.[28] These results also suggest that, once we control for heterogeneity by debt amount, no significant differences remain in the effects of higher visibility across states.

To illustrate the timing of the effects, Figure 4.a shows the week-by-week estimates of the effects of higher visibility (for the first quartile). This figure shows that individuals reacting to the higher visibility leave the list quite promptly: the vast majority of the reaction occurs during the second to fifth week after mail delivery. After week ten, the effects of higher visibility decline slightly. Even twenty-nine weeks after the letters were delivered, the probability of leaving the list was still 1.6 percentage points higher, compared to the 2.1 percentage points effect at week ten. This suggests that nearly 75 percent of the individuals who reacted to the higher visibility by week 10 did not intend to pay during the subsequent nineteen weeks. Unfortunately, the effects are less precisely estimated for longer time horizons. As a result, we cannot reject that these longer horizon effects are statistically different from the effects in week 10, but we cannot reject that they are different from zero either. The finding that the salience of shaming seems more effective for smaller debts is consistent with the perception of tax practitioners, as mentioned in private conversations and in press releases.[29]

Our favorite interpretation for this finding is provided by the social signaling model shown in Appendix E.3. Under this interpretation, the implication would be that social incentives may not scale up well. However, one must take into account that our visibility intervation was a conservative one, and it is possible that more significant shaming interventions, like the

and/or location (e.g., street, city, state, ZIP code, county), while the Wisconsin website does not feature a search tool, but it provides the opportunity to sort the list of delinquents alphabetically by name or by city. The Kansas website allows for a name search, and it also provides the full list that can be sorted by name, county and amount due, among others.

[28] For a more finer break-down of the effects in Kentucky, see Appendix Figure B.2.

[29] See for example: "To Collect Revenue, Some States Put Tax Scofflaws in Virtual 'Stocks'," Stateline, May 28, 2015)

online lists published by the tax agencies, could have a significant effect on individuals owing larger amounts or even on individuals who are not delinquents yet. Additionally, we cannot rule out other explanations for the heterogeneity by debt amount: it is possible that people who owe larger amounts have unobservable characteristics, such as dishonesty, selfishness or financial stress, that are associated both with having larger debts and being less responsive to higher visibility.

Our experiment was designed to study a specific mechanism through which publishing the lists of tax delinquents may affect tax compliance, social interactions, but was not designed to measure the average effect of publishing online lists of tax delinquents.[30] We have at least three reasons to believe that our visibility intervention provides a conservative lower bound to the potential effect of the shaming policy. First, our treatment increased the visibility of the recipient's delinquency status among neighbors. For most individuals, however, the most valuable social interactions are with relatives, friends, coworkers, bosses, and clients, a majority of whom are not neighbors, and which would be affected by the publication of online lists of delinquents. Second, the tax agencies in all three experimental states must send letters to allow individuals to resolve their debt prior to the posting, and a significant fraction of the response to the shaming penalty happens when these warning letters are received.[31] Our subject pool comprised only those individuals who received such notification and did not respond, which by construction is a subset of individuals who are selected against caring about social interactions.[32] Last, a significant share of the individuals may not have read the letter. For instance, the U.S. Environmental Protection Agency estimates that only about half of unsolicited correspondence is opened. Thus, the average treatment effect on the treated (i.e., on those who actually read the letter) is probably a multiple of the intention-to-treat estimates reported in this paper.

On the other hand, it is also possible that the effects of our visibility intervention exaggerate the effects of the shaming policy. For example, it is possible that delinquents do not react to the shaming policy because they believe that none of their social contacts would ever visit the website listing the tax delinquents. On the contrary, by providing access to the website to neighbors, our mailing intervention would be more effective.

[30]Furthermore, we would not recommend that tax agencies shame tax delinquents with letters like ours, because publishing lists of tax delinquents is probably much more cost-effective.

[31]For instance, the chairman of the California Board of Equalization declared that when the list was introduced in California 41 percent of those who were about to appear on the list made payment arrangements before their names were published (Source: Stateline, May 28, 2015). Additionally the spokeswoman for the Illinois Department of Revenue declared that "The real success of the program is before the postings are made" (Source: CNN Money, December 23, 2005).

[32]Our experiment was not designed to provide an impact-evaluation of the shaming policy – for that, it would be key to measure the deterrence effect on all taxpayers, not only the ones who are already being shamed.

3.2 Effect of Financial Reminder

Figure 1.b shows the effects of the financial reminder (i.e., the difference between letters with and without information about the financial penalties). The effects of the financial reminder were roughly similar for individuals who owed different amounts. This is not surprising given that the financial penalties are proportional to the amount owed. For the first three income quartiles, the financial reminder increased the probability of leaving the list by about 1 percentage point (or 10 percent of the baseline rate). Although individually these three coefficients are statistically insignificant, they are jointly significant: an average effect of 0.98, with a p-value of 0.034. Unlike the effect for the first three quartiles, the effect of the financial reminder seems to be close to zero and statistically insignificant for the top quartile ($13,347–$150,000). As before, Figure 2.b shows that the financial reminders did not have significant effects on the pre-treatment outcome, the logarithm of the initial debt amount.

Figure 3.b explores state-level heterogeneity of the effects of the financial reminder. For debtors in Kentucky with both debts below and above $2,500, the financial reminder had a significant and similar effect. In other words, the effect of the financial reminder did not appear to change with the debt amount in Kentucky. On the other hand, the effect of the financial reminder was close to zero and statistically insignificant for Kansas and Wisconsin. This evidence suggests that the effect of the financial reminders differed between Kentucky and the other two states.[33] One simple explanation for this finding is that the financial penalty in Kentucky (30 percent) is significantly higher than the financial penalties in the other two states (14 percent in Kansas and 18 percent in Wisconsin), and also significantly higher than the typical interest rate on credit card debt (14%). In other words, if an average individual from Kentucky found out about the 30% annual interest rate on her tax debt, she should be able to save a significant amount of money by paying off the tax debt with her credit card. On the contrary, if the average individual from Kansas (Wisconsin) found out about the 14% (18%) interest rate on their tax debt, she should gain nothing (little) from paying off the her tax debt with her credit card.[34]

Figure 4.b shows the week-by-week estimates of the effects of the financial reminder (for the full sample). This figure suggests that the effects of the financial reminder build up during the first four months and then decline slowly.[35] Individuals reacting to the financial

[33] We can reject the hypothesis that the average effect in Kentucky (for all debt amounts) is equal to the average effect in Kansas and Kentucky (pooled together).

[34] Another possible explanation is that individuals in Kentucky were more likely to underestimate the true financial penalty. For instance, it is possible that Kentucky disseminated less information about the financial penalties.

[35] There is a jump around the tenth week, corresponding to one of the major updates to the databases made in Kentucky, that, as discussed above, is the state for which the financial reminder had the highest effect.

reminder react more slowly than those reacting to higher visibility. This may be because they owe higher amounts on average and thus may need more time to gather the funds needed to pay the full amount or the first installment. Just as in the case of the higher visibility, the slow decline implies that a majority of individuals who paid because of the financial reminder were individuals who, in absence of this reminder, would not have paid in the subsequent weeks.

3.3 Effect of Peer Information

Figure 1.c shows the effects of the *Peer Information* – a variable that takes the values from 0 to 1, depending on the weighting parameter used to form the list of delinquent neighbors. To interpret the coefficient on this variable, recall that higher values of *Peer Information* are associated with higher amounts in the table of delinquent neighbors shown to the recipient. On average, increasing *Peer Information* from 0 to 1 has the following effects on the distribution of delinquent amounts of the nine neighbors listed in the letter: the mean amount increases by $28,000, the median amount increases by $14,000, and the recipient's rank in the list goes down by 4 positions.[36]

The coefficients on *Peer Information* reported on Figure 1.b indicates that showing higher debt amounts in the letter does not have significant effects on the payment decisions ten weeks after the letter delivery. Each of the four coefficients are statistically insignificant. The average effect over the entire sample is close to zero (0.10pp), precisely estimated (s.e. 0.42) and statistically insignificant (p-value=0.811).

Figure 3.c and Figure 4.c show that this null result is robust across specifications. Figure 3.c shows the heterogeneity by state: the effects of peer information are close to zero and statistically insignificant in each of the states. Figure 4.c shows the week-by-week estimates of the effects of the peer information (for the full sample). This figure shows that the effects of peer information are close to zero and statistically insignificant for every time horizon.

This finding can suggest that peer comparisons do not play a significant role in the payment decisions of tax delinquents. This interpretation would be consistent with related evidence from field experiments showing that messages of moral appeal are ineffective at reducing tax evasion. For instance, Blumenthal et al. (2001) and Fellner, Sausgruber and Traxler (2013) find that messages with moral appeals fail to reduce tax evasion. In the context of tax collection, the evidence on the effect of moral appeals is mixed: while Hallsworth et al. (2014) find significant effects, Castro and Scartascini (2013) do not.[37]

[36]For details about this calculations, see Table B.5.

[37]Dwenger et al. (2014) find that moral appeals fail to reduce tax evasion on average, although they do find evidence of heterogeneous effects in opposing directions. For a more general discussion about tax morale,

However, our evidence is about the effects of a specific form of social information, the delinquency of nine other debtors, so we cannot rule out that other pieces of social information may have a significant effect.[38] Furthermore, our evidence indicates that the social information does not affect the behavior of individuals who are already tax delinquents, but it is still possible that this same social information would affect the behavior of non-delinquents. Indeed, given that delinquents may have lower tax morale, it is somewhat unsurprising that social comparisons are ineffective in shaping their behavior. This interpretation would imply that the same moral interventions that have been proven to be effective with pro-social individuals (e.g., individuals who give to charity) may not be equally effective with anti-social individuals (tax delinquents).

3.4 Discussion: Welfare Implications

The evidence presented above suggests that the salience of shaming penalties has effects in the direction intended by the policymakers. This does not imply that shaming penalties have effects in the desired direction, although it is suggestive evidence. Even if they have, however, the evidence would not imply that, from a normative perspective, the shaming penalties ought to be used. Indeed, given that financial penalties are available, it is not obvious why the tax agencies are interested in using the shaming penalties in the first place. From our conversation with the tax agencies, one important reason why they seem to like the shaming penalties is because of their ability to target delinquents who are less vulnerable to the financial penalties – that is, individuals who expect to escape the collection tools. For instance, the Illinois Department of Revenue, in a press release from November 3, 2009 declared that: "The threat of disclosure and the negative publicity of being included in this list are particularly effective with self-employed professionals and cash businesses where some routine collection tools, such as the ability to garnish wages, may not work."

Appendix C provides a model that formalizes this argument.[39] Intuitively, if the tax agency cares about the private welfare of the individuals in addition to the tax revenues, the agency should try to be more lenient on debtors who do not pay because they are in financial distress rather than because they do not want to pay. Since individuals differ on their vulnerability to the collection tool, this creates a demand for additional penalties, such as shaming penalties, that can target debtors based on their income garnishability. This

see Luttmer and Singhal (2014).

[38] Other margins of peer effects may be important. For instance, individuals may care more about the number or share of taxpayers who are delinquent. Our experiment cannot identify the effect at this margin, because all letters included the same number of delinquent neighbors.

[39] Of course, there may be other reasons why tax agencies choose to use the shaming penalties, including but not limited to fairness concerns and frictions to modify the financial penalties.

appendix also provides suggestive empirical evidence in favor of this conjecture: we show that our visibility intervention was more effective in ZIP codes with a higher share of wage income, where the average individual is presumably less vulnerable to financial penalties.

Similarly, the fact our financial reminders increased payment rates does not imply that financial penalties are desirable from the perspective of social welfare. In an extension of the model shown in Appendix E.1, we show that correcting biases about financial penalties could be desirable for a tax agency that cares about the private welfare of the taxpayers in addition to tax revenues. Intuitively, the financial reminders can improve welfare by allowing the taxpayers to make more informed choices.

4 Conclusions

Increasing the efficiency of tax compliance is a key issue for fostering economic development. Even though there is little evidence on the effects of shaming penalties, these penalties are being increasingly used for various purposes in America and the rest of the world. We provided evidence from a field experiment in the context of tax debt enforcement. We showed that increasing the salience of shaming penalties has a positive effect on the payment rate, which is the direction intended by the policymakers when introducing shaming policies. Increasing the salience of shaming penalties, however, is only statistically significant for lower debt amounts. Reminders of financial penalties also increased payment rates, and their effectiveness did not depend on the size of the debt amount but did seem to depend on the size of the interest rate. Last, we provided evidence that the payment decisions are not significantly affected by specific information about the amounts owed by other delinquents.

Our results raise several questions for future research. First, our evidence comes from the tax delinquency setting, but shaming penalties may also be applied for other aspects of tax compliance, such as tax evasion and tax avoidance. Consistent with this observation, some tax agencies outside the United States have started to publish lists of tax evaders, although this policy is arguably less widespread compared to the one disclosing the identity of tax delinquents.[40] Future research could use our methods to test whether shaming is also effective for these other aspects of tax compliance. Second, we focused on a specific form of nonfinancial reminder consisting of the online publication of lists of debtors. In practice, tax agencies use other nonfinancial penalties, such as direct pressure through home visits and revocation of driving licenses and passports (Blank, 2014). Future research may

[40]For example, the U.K. publishes a list of top tax evaders (link). Also, even though it was not part of a regular policy, Chetty, Mobarak and Singhal (2014) present results from an intervention in Bangladesh that peer pressure may be effective in reducing tax evasion.

use a variation of our experimental design to test the effectiveness of these other types of nonfinancial policies. Last, even if shaming penalties had effects in the desired direction, that would not imply that they are desirable, especially if financial penalties are available. Thus, studying the normative aspects of shaming tax delinquents should be a priority for future research.

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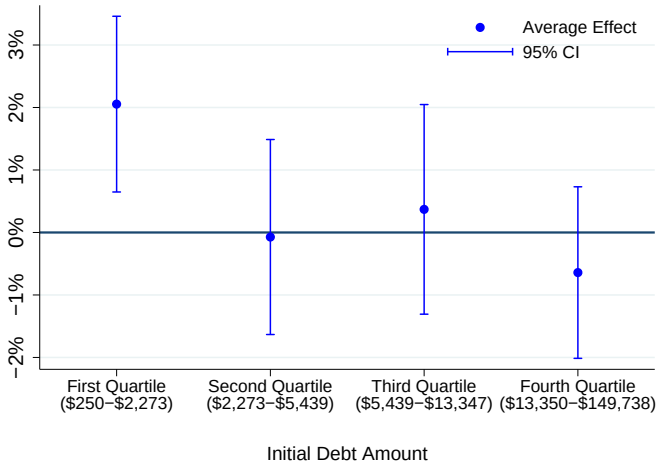
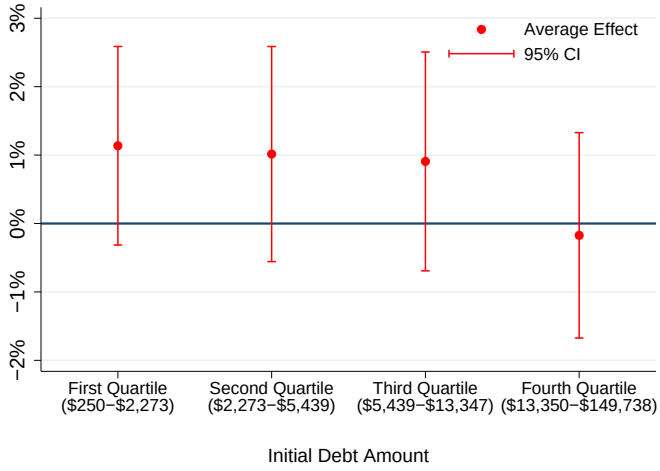
Figure 1: Effects of Information Treatments, 10 Weeks after Mail Delivery, by Quartile of Debt Amount

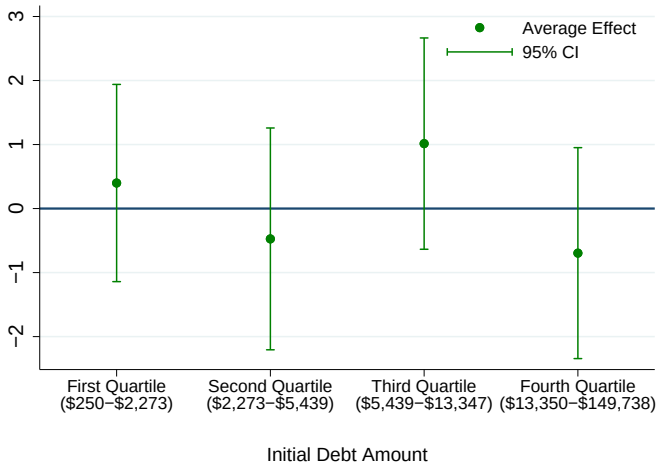
a. Effect of Higher Visibility



b. Effect of Financial Reminder



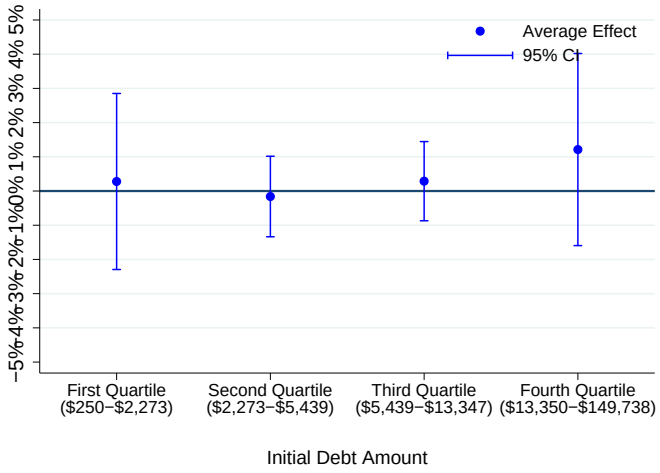
c. Effect of Peer Information



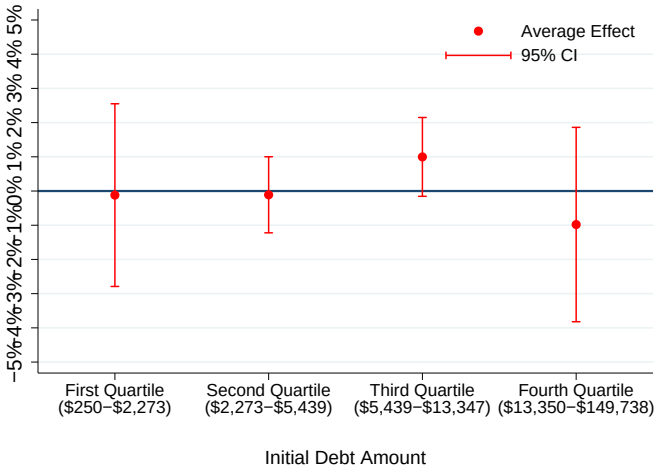
Notes: N=34,334. The debt amount in the x-axis corresponds to the amount owed when the subject pool was formed (May 26, 2014). The effects were estimated from OLS regressions (one for each group in the x-axis) where the dependent variable is a dummy for whether the subject is listed as a delinquent 10 weeks after the letters were delivered, and the right hand side variables are the treatment dummies plus a set of control variables: gender, ethnicity and state dummies, initial debt amount and its logarithm (with state-specific coefficients) and the number of delinquents in the ZIP code. *Higher Visibility* is a dummy that takes the value 0 if the recipient was the only one in the area chosen to receive a letter, and 1 if others in the area were chosen to receive a letter too. *Financial Reminder* is a dummy that takes the value 1 if the letter included information about the financial penalties and 0 if not. *Peer Information* indicates if the recipient was shown neighbors with higher delinquent amounts – it takes the value 0, 0.5 or 1 depending on whether the individuals to be shown in the table of delinquent neighbors we selected with a parameter $\alpha = -1$, $\alpha = 0$ or $\alpha = 1$, respectively. Confidence intervals computed with standard errors clustered at the 5-digit ZIP code level.

Figure 2: Falsification Test: "Effects" of Information Treatments on the Pre-Treatment (Log) Debt Amount

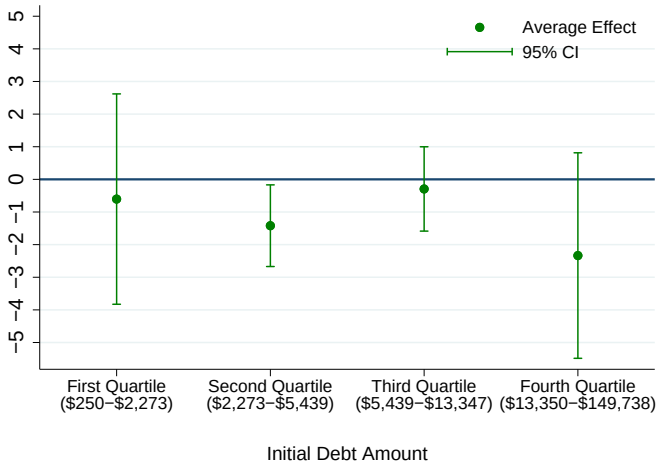
a. (Pre-Treatment) Effect of Higher Visibility



b. (Pre-Treatment) Effect of Financial Reminder

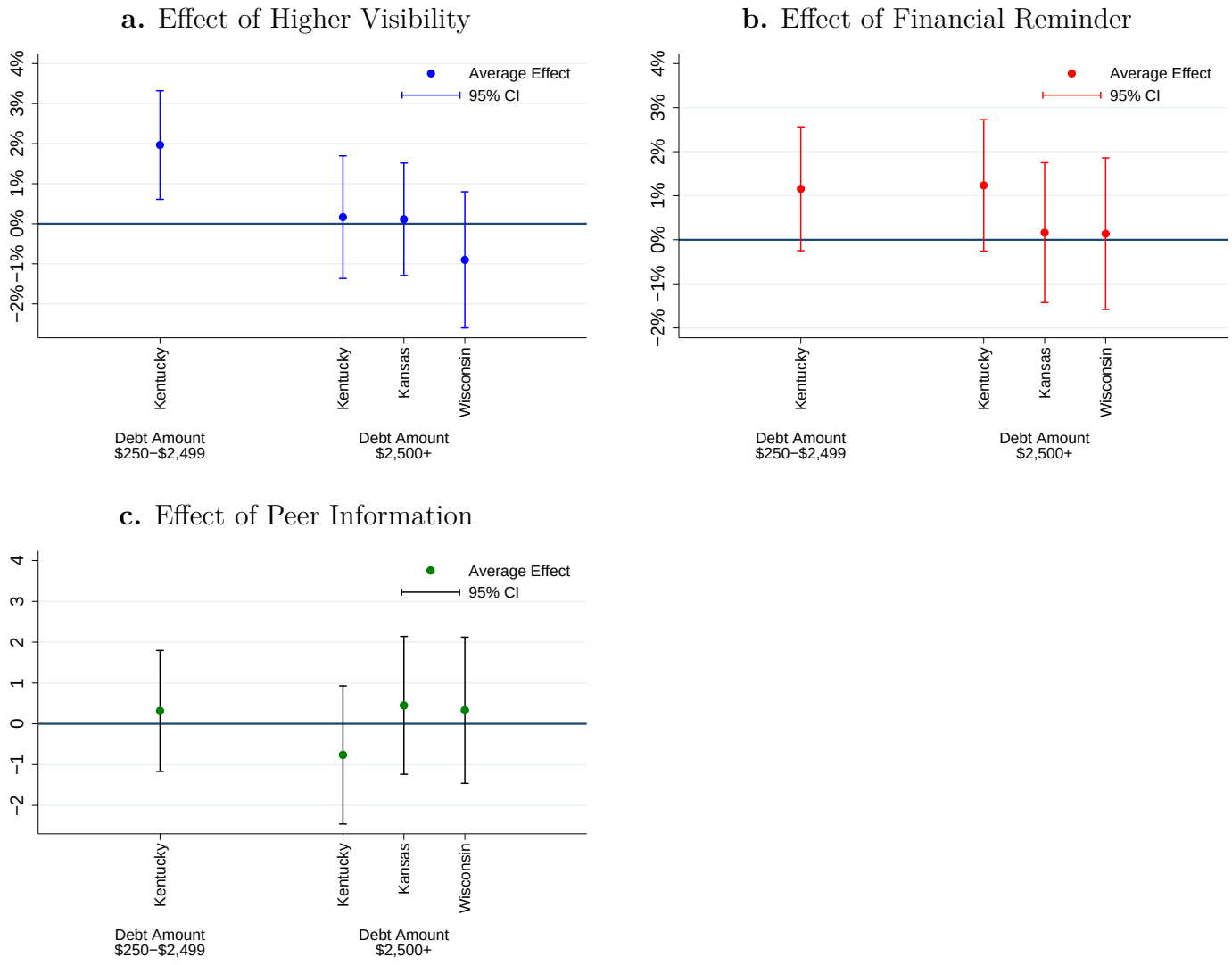


c. (Pre-Treatment) Effect of Peer Information



Notes: N=34,334. The debt amount in the x-axis corresponds to the amount owed when the subject pool was formed (May 26, 2014). The effects were estimated from OLS regressions (one for each group in the x-axis) where the dependent variable is the logarithm of the initial debt amount, and the right hand side variables are the treatment dummies. *Higher Visibility* is a dummy that takes the value 0 if the recipient was the only one in the area chosen to receive a letter, and 1 if others in the area were chosen to receive a letter too. *Financial Reminder* is a dummy that takes the value 1 if the letter included information about the financial penalties and 0 if not. *Peer Information* indicates if the recipient was shown neighbors with higher delinquent amounts – it takes the value 0, 0.5 or 1 depending on whether the individuals to be shown in the table of delinquent neighbors we selected with a parameter $\alpha = -1$, $\alpha = 0$ or $\alpha = 1$, respectively.. Confidence intervals computed with standard errors clustered at the 5-digit ZIP code level.

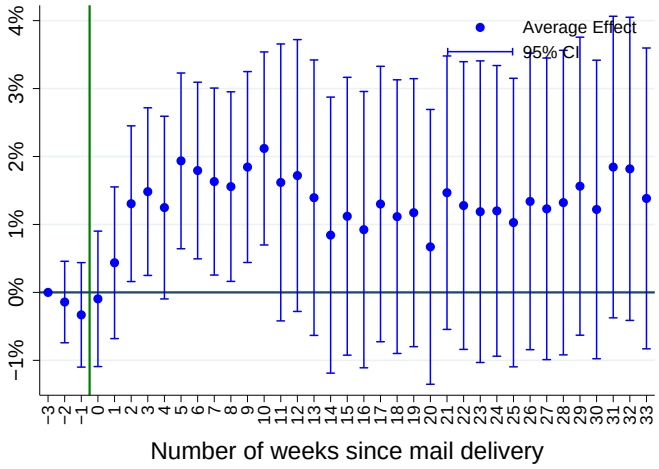
Figure 3: Effects of Information Treatments, 10 Weeks after Mail Delivery, by State and Debt Amount



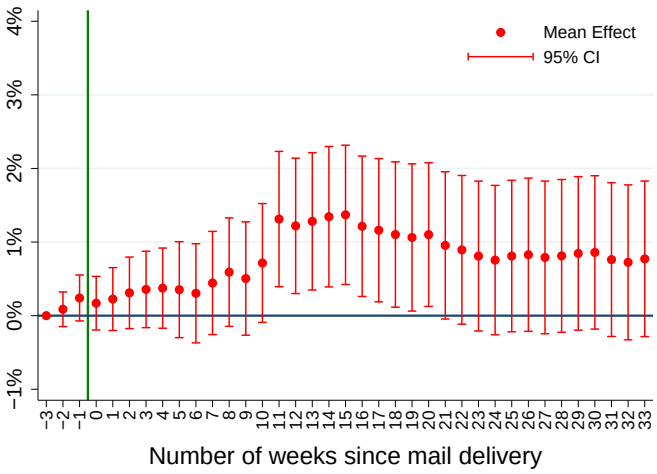
Notes: N=34,334 (9,029 from Kentucky $250-$2,499, 9,072 from Kentucky $2,500+, 8,710 from Kansas and 7,523 from Wisconsin). The debt amount in the x-axis corresponds to the amount owed when the subject pool was formed (May 26, 2014). The effects were estimated from OLS regressions (one for each group in the x-axis) where the dependent variable is a dummy for whether the subject is listed as a delinquent 10 weeks after the letters were delivered, and the right hand side variables are the treatment dummies plus a set of control variables: gender, ethnicity and state dummies, initial debt amount and its logarithm (with state-specific coefficients) and the number of delinquents in the ZIP code. *Higher Visibility* is a dummy that takes the value 0 if the recipient was the only one in the area chosen to receive a letter, and 1 if others in the area were chosen to receive a letter too. *Financial Reminder* is a dummy that takes the value 1 if the letter included information about the financial penalties and 0 if not. *Peer Information* indicates if the recipient was shown neighbors with higher delinquent amounts – it takes the value 0, 0.5 or 1 depending on whether the individuals to be shown in the table of delinquent neighbors we selected with a parameter $\alpha = -1$, $\alpha = 0$ or $\alpha = 1$, respectively. Confidence intervals computed with standard errors clustered at the 5-digit ZIP code level.

Figure 4: Week-by-week Evolution of Effects of Information Treatments

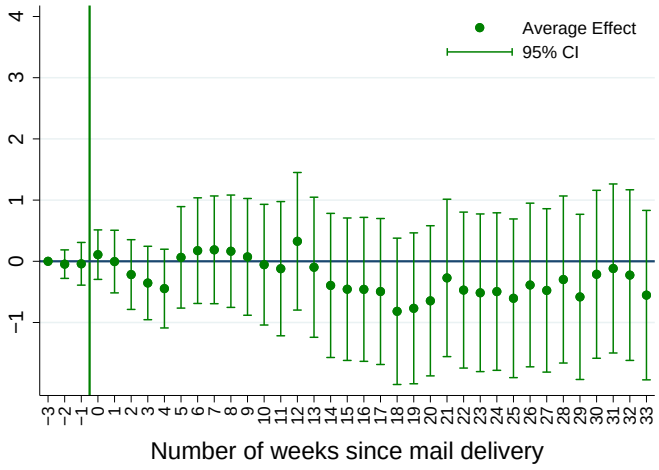
a. Effect of Higher Visibility (Lowest Quartile)



b. Effect of Financial Reminder (All Sample)



c. Effect of Peer Information (All Sample)



Notes: (a) N=8,584; (b) N=34,334 (c) N=34,334. In the x-axis, Week -3 corresponds to the date when the subject pool was formed (May 26, 2014). The green vertical line shows the approximate date when the letters were delivered. The effects were estimated from OLS regressions (one for each graph) where the dependent variable is a dummy for whether the subject is listed as a delinquent 10 weeks after the letters were delivered, and the right hand side variables are the treatment dummies plus a set of control variables: gender, ethnicity, and state dummies, initial debt amount and its logarithm (with state-specific coefficients) and the number of delinquents in the ZIP code. *Higher Visibility* is a dummy that takes the value 0 if the recipient was the only one in the area chosen to receive a letter, and 1 if others in the area were chosen to receive a letter too. *Financial Reminder* is a dummy that takes the value 1 if the letter included information about the financial penalties and 0 if not. *Peer Information* indicates if the recipient was shown neighbors with higher delinquent amounts – it takes the value 0, 0.5 or 1 depending on whether the individuals to be shown in the table of delinquent neighbors we selected with a parameter $\alpha = -1$, $\alpha = 0$ or $\alpha = 1$, respectively. Confidence intervals computed with standard errors clustered at the 5-digit ZIP code level.

Table 1: States with Online Lists of Tax Delinquents (as of December 31, 2014)

						State Rank (1st to 50th) in...		
State	Experiment	Start Year	Current Threshold	Type	Website	Income	Social Capital	Tax Compliance
Kansas	Yes	2004	$2,500	I, B	Link	24th	16th	10th
Kentucky	Yes	2007	$250	I, B	Link	47th	40th	37th
Wisconsin	Yes	2006	$5,000	I, B	Link	28th	11th	19th
California	No	2007	Top-500	I, B	Link	5th	27th	32nd
Colorado	No	2003	$20,000	I, B	Link	11th	15th	17th
Connecticut	No	1995	Top-50	I, B	Link	1st	17th	28th
Delaware	No	2007	Top-100	I, B	Link	19th	23rd	29th
Florida	No	2014	$100,000	I, B	Link	23rd	37th	46th
Georgia	No	2004	$0	I, B	Link	33rd	47th	47th
Indiana	No	2010	$0	B	Link	41st	25th	33rd
Maryland	No	2000	Top-25	I, B	Link	6th	32nd	30th
Massachusetts	No	2004	$25,000	I, B	Link	2nd	18th	20th
Montana	No	2010	$10,000	I, B	Link	40th	5th	6th
Nebraska	No	2010	$20,000	I, B	Link	21th	6th	5th
New Jersey	No	2010	Unknown	I, B	Link	3rd	36th	42nd
New York	No	2010	Top-250	I, B	Link	4th	35th	36th
North Carolina	No	2001	Unknown	I, B	Link	37th	41st	39th
Oklahoma	No	2009	$25,000	I, B	Link	26th	26th	25th
Pennsylvania	No	2010	Unknown	I, B	Link	17th	30th	21st
Rhode Island	No	2003	Top-100	I, B	Link	20th	24th	45th
South Dakota	No	2012	Top-200	B	Link	27th	2nd	1st
Vermont	No	2014	Top-100	I, B	Link	36th	3rd	8th
Washington	No	1997	$10,000	I, B	Link	9th	10th	18th

Notes: *Experiment* indicates whether the state was included in the field experiment. *Start year* is the year when the online list of delinquents was published for the first time. *Current threshold* indicates the minimum amount of debt such as the debtor is included in the public list (Top-N means that the list includes the N debtors with the highest debt amounts). *Type* indicates whether the lists includes Individuals (I) and/or Businesses (B). This list does not include states that maintain lists for minor taxes (e.g., Alabama for property tax and Minnesota for liquor tax). This table does not include other states which had lists of delinquents in the past but discontinued the policy (e.g., Hawaii, Illinois, Louisiana, South Carolina, Virginia). *Income, Social Capital* and *Tax Compliance* correspond to state ranks, from 1st (the state with the highest outcome) to 50th (the state with the lowest outcome). *Income* corresponds to gross household income from IRS-SOI data. *Social capital* corresponds to Putnam's social capital index. *Tax compliance* corresponds to the share of self-employed individuals in the 3-digit ZIP code estimated to be mis-reporting income to take advantage of EITC benefis (Chetty et al., 2013).

Table 2: Place Characteristics Associated to the Rate of Tax Delinquency

	Number of Delinquents in 5-digit ZIP code			
	(1)	(2)	(3)	(4)
	All	Kentucky	Kansas	Wisconsin
Mean Income (STD)	-0.001	-0.162***	0.064***	-0.056**
	(0.015)	(0.061)	(0.023)	(0.022)
Share of Wage Income (STD)	-0.079***	-0.123*	0.057	-0.115***
	(0.030)	(0.073)	(0.051)	(0.035)
EITC Bunching (STD)	0.177***	0.392***	0.178***	0.225***
	(0.023)	(0.054)	(0.067)	(0.025)
Share Republican (STD)	-0.086***	-0.104*	-0.063	0.106***
	(0.029)	(0.061)	(0.047)	(0.032)
Civic Life Index (STD)	-0.140***	0.089	-0.237***	0.017
	(0.031)	(0.074)	(0.041)	(0.052)
Log(Population)	1.030***	1.088***	1.000***	0.950***
	(0.018)	(0.036)	(0.026)	(0.021)
Observations	1,972	657	603	712

Notes: * significant at the 10% level, ** at the 5% level, *** at the 1% level. Heteroskedastic-robust standard errors in parenthesis. The coefficients correspond to a Negative Binomial Regression of the number of delinquents in the ZIP-5 on a number of place characteristics. All the independent variables (except Log(Population)) were normalized to have mean zero and standard deviation 1. *Number of Delinquents in ZIP-5* counts the number of unique individuals on the online lists of delinquents as of May 26th 2014 who owed $5,000 or more. The sample includes individuals with debts originating from Kentucky, Kansas and Wisconsin who are still living in the same state where the debt originated. *Log(population)* is constructed at the ZIP-5 level and comes from the 2012 U.S. Census data. *Mean Income* corresponds to the mean gross income at the ZIP-5 level, based on data from 2012 IRS SOI. *Share of Wage Income* is the share of gross income originating from wage income, also constructed at the ZIP-5 level and using data from the 2012 IRS SOI. *EITC Bunching* is the share of self-employed individuals in the 3-digit ZIP code estimated to be mis-reporting income to take advantage of EITC benefits (data source: Chetty et al., (2013)). *Share Republican* is the county-level share of votes for the Republican candidate in the 2012 U.S. Presidential Election. *Civic Life Index* is a county-level measure of social capital based on density of civic and non-profit organizations, voting turnout and census completion rates as of 2005 (Rupasingha et al., 2006). The regression in column (1) includes state dummies.

Table 3: Overview of Treatment Arms

Treatment Arm	Sub-treatment	Number of letters	Description
Visibility	Higher Visibility	17,179	*"Your household and other households in your area were randomly chosen to receive a letter of this type"*
	Lower Visibility	17,155	*"Your household was the only household randomly chosen from your area to receive a letter of this type"*
Financial Reminder	Reminder	18,209	**Wisconsin**: *"This website also includes information about penalties. For instance, your tax debt is subject to, among other penalties, an annual interest rate of 18%."* **Kansas**: *"This website also includes information about penalties. For instance, your tax debt is subject to, among other penalties, an annual interest rate of 12%."* **Kentucky**: *"This website also includes information about penalties. For instance, your tax debt is subject to, among other penalties, an annual interest rate of 30%."*
	No Reminder	16,125	
Peer Information	$\alpha = -1$	11,431	Higher α includes neighbors with higher delinquent amounts in the table of delinquents displayed in the letter.
	$\alpha = 0$	11,384	
	$\alpha = 1$	11,519	

Notes: We sent letters to all 34,334 subjects, and this table summarizes how we cross-randomized some of the information contained in the letter.

Table 4: Peer Information Treatment: Sample Treatment Lists Generated with Different Parameter Values

Lower Amounts ($\alpha = -1$)		**Medium Amounts** ($\alpha = 0$)		**Higher Amounts** ($\alpha = 1$)	
Name	**Amount**	**Name**	**Amount**	**Name**	**Amount**
J. W. C.	\$68,509	F. D.	\$82,142	B. J.	\$95,935
G. J. D.	\$12,051	J. W. C.	\$68,509	F. D.	\$82,142
Recipient	**\$2,648**	R. P.	\$23,188	J. W. C.	\$68,509
L. N. L.	\$2,638	G. J. D.	\$12,051	R. P.	\$23,188
T. C.	\$2,024	W. S.	\$6,247	G. J. D.	\$12,051
R. T. C.	\$1,944	**Recipient**	**\$2,648**	E. T.	\$8,244
D. N.	\$1,505	L. N. L.	\$2,638	W. S.	\$6,247
S. M.	\$1,158	T. C.	\$2,024	G. G.	\$4,312
T. S.	\$873	R. T. C.	\$1,944	**Recipient**	**\$2,648**
J. V.	\$269	D. N.	\$1,505	L. N. L.	\$2,638

Notes: This is an example of how the algorithm generates different lists of nine neighbors from a given sample of the recipient's twenty closest delinquent neighbors. The actual letters include the full names instead of the initials. See Section 2.3 for a description of the algorithm.

Table 5: Descriptive Statistics and Randomization Balance Test

	Visibility		Financial Reminder		Peer Information			Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Lower	Higher	No	Yes	$\alpha = -1$	$\alpha = 0$	$\alpha = 1$	P-value
Initial Debt Amount ($1,000s)	12.86	12.90	12.85	12.91	12.86	13.16	12.63	0.43
	(0.16)	(0.16)	(0.16)	(0.15)	(0.19)	(0.20)	(0.19)	
Log(Initial Debt Amount)	8.58	8.58	8.57	8.59	8.57	8.60	8.57	0.28
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	
Percent Male	64.32	64.56	64.81	64.11	64.26	64.10	64.95	0.40
	(0.37)	(0.36)	(0.38)	(0.36)	(0.45)	(0.45)	(0.44)	
Percent White	70.87	70.85	70.50	71.17	70.95	70.98	70.64	0.23
	(0.22)	(0.22)	(0.22)	(0.21)	(0.27)	(0.27)	(0.27)	
Percent Black	13.94	13.73	14.13	13.57	13.75	13.87	13.88	0.01
	(0.11)	(0.11)	(0.12)	(0.11)	(0.14)	(0.14)	(0.14)	
Percent Hispanic	5.86	5.83	6.02	5.69	5.75	5.82	5.97	0.48
	(0.15)	(0.15)	(0.15)	(0.14)	(0.18)	(0.18)	(0.18)	
Percent Other	3.33	3.37	3.36	3.35	3.34	3.37	3.35	0.98
	(0.05)	(0.06)	(0.06)	(0.05)	(0.07)	(0.07)	(0.07)	
Observations	17,155	17,179	16,125	18,209	11,431	11,384	11,519	

Notes: N=34,334. Pre-treatment mean individual characteristics by treatment group (standard errors in parenthesis). *Higher Visibility* is a dummy that takes the value 0 if the recipient was the only one in the area chosen to receive a letter, and 1 if others in the area were chosen to receive a letter too. *Financial Reminder* is a dummy that takes the value 1 if the letter included information about the financial penalties and 0 if not. *Peer Information* corresponds to the value of the weighting parameter used to select the delinquents to be listed in the table shown to the recipient. The p-value corresponds to the test of the null hypothesis that the average characteristics are the same across all treatment groups. The initial debt amount corresponds to the amount owed when the subject pool was formed (May 26, 2014). Gender and ethnicity are not observed directly, but imputed using data on the joint distribution of first names and gender (several sources, including data from the U.S. Census Bureau), and the joint distribution of last names and ethnicities (data from U.S. Census Bureau). The omitted category for gender is male, and the omitted category for ethnicity corresponds to unmatched last names.

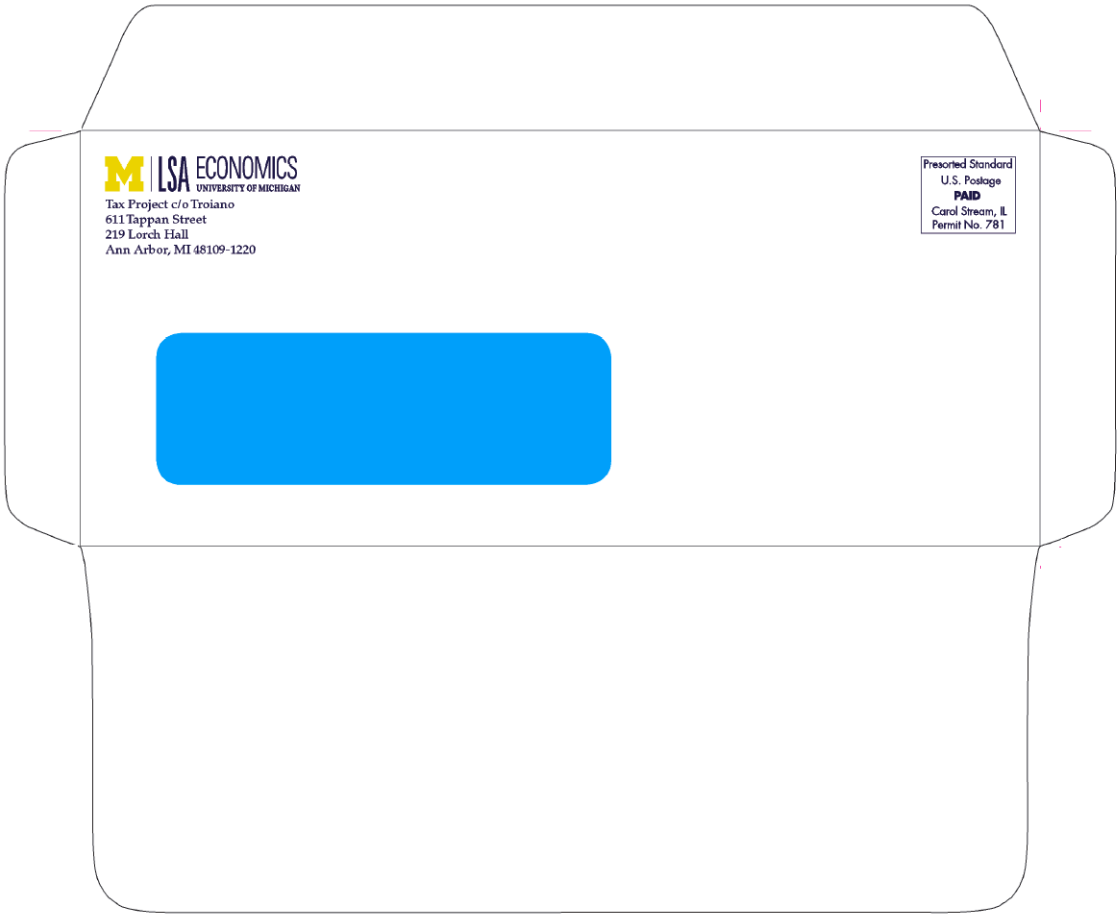
Table 6: Effects of Information Treatments on Probability of Payment

	Probability of Leaving the List		Log(Amount)
	(1)	(2)	(3)
	Week 5	Week 10	Week -3
Effect of Higher Visibility:			
First Quartile (250−2,273)	1.914***	2.083***	0.003
	(0.662)	(0.726)	(0.013)
Second Quartile (2,273−5,439)	-0.275	0.033	-0.001
	(0.631)	(0.795)	(0.006)
Third Quartile (5,439−13,347)	0.399	0.279	0.003
	(0.694)	(0.859)	(0.006)
Fourth Quartile (13,350−149,738)	-0.414	-0.662	0.012
	(0.636)	(0.705)	(0.014)
Effect of Financial Reminder:			
First Quartile (250−2,273)	0.939	1.061	-0.002
	(0.652)	(0.740)	(0.014)
Second Quartile (2,273−5,439)	0.578	1.023	-0.001
	(0.619)	(0.803)	(0.006)
Third Quartile (5,439−13,347)	0.010	0.874	0.010*
	(0.666)	(0.821)	(0.006)
Fourth Quartile (13,350−149,738)	-0.120	-0.123	-0.009
	(0.636)	(0.766)	(0.015)
Effect of Peer Information:			
First Quartile (250−2,273)	0.082	0.325	-0.006
	(0.703)	(0.780)	(0.016)
Second Quartile (2,273−5,439)	-0.796	-0.574	-0.014**
	(0.668)	(0.890)	(0.006)
Third Quartile (5,439−13,347)	1.515**	1.132	-0.003
	(0.693)	(0.837)	(0.007)
Fourth Quartile (13,350−149,738)	-0.543	-0.728	-0.024
	(0.736)	(0.840)	(0.016)
Mean Outcomes:			
First Quartile (250−2,273)	9.874	13.431	6.786***
	(7.880)	(9.000)	(0.015)
Second Quartile (2,273−5,439)	12.275	22.305**	8.190***
	(7.896)	(8.864)	(0.006)
Third Quartile (5,439−13,347)	10.321	21.597**	9.005***
	(8.387)	(9.481)	(0.006)
Fourth Quartile (13,350−149,738)	17.377**	27.315***	10.355***
	(8.696)	(9.885)	(0.016)

Notes: N=34,334. * significant at the 10% level, ** at the 5% level, *** at the 1% level. Standard errors in parentheses, clustered at the 5-digit ZIP code level. The coefficients were estimated from OLS regressions (one per column) where the right hand side variables are the treatment dummies, interacted with the quartile amount dummies, plus a set of control variables: gender dummy, ethnicity, state dummies, initial debt amount and its logarithm (with state-specific coefficients) and the number of delinquents in the ZIP code. See notes to Figure 1 for definitions for *Higher Visibility*, *Financial Reminder* and *Peer Information*.

5 Sample of the Envelope and the Letter

Sample Envelope



LSA ECONOMICS
UNIVERSITY OF MICHIGAN

Ann Arbor, May 26th 2014

Dear [redacted]

This letter is part of a research study about tax delinquency conducted by researchers at University of Michigan. We would like to share with you a sample of the public records from the Kentucky Department of Revenue. **The following is a sample of tax delinquents living close to your household as of today:**

First and Last name	Debt Amount
[redacted]	$68,509
[redacted]	$12,051
[redacted]	$2,648
[redacted]	$2,638
[redacted]	$2,024
[redacted]	$1,944
[redacted]	$1,505
[redacted]	$1,158
[redacted]	$873
[redacted]	$269

YOUR HOUSEHOLD AND OTHER HOUSEHOLDS IN YOUR AREA WERE RANDOMLY CHOSEN TO RECEIVE A LETTER OF THIS TYPE

Names, addresses and other details about tax delinquents are freely available to see for anyone with access to the Internet. You can search for individual debtors by first and last name, or by zipcode, by visiting the following web-page from the website of the Kentucky Department of Revenue:

http://ilp.ky.gov/ILPInterNet.aspx?dt=I

You can find a screenshot of this search tool on the reverse of the page.

219 Lorch Hall, 611 Tappan Street
Ann Arbor, MI 48109-1220

Program Page: http://www.umich.edu/~taxproj/tax.html
Email: taxproject@umich.edu

For illustration purposes, the following is a screenshot of the search tool:

This website also includes information about penalties. For instance, your tax debt is subject to, among other penalties, an annual interest rate of 4% and a monthly late payment fee of 2%.

We kindly ask you to visit our website and fill out an anonymous questionnaire:

http://www.umich.edu/~taxproj/survey.html

Additionally, on our website you will also be able to find more information about this project, including our contact information.

Ugo Troiano and Ricardo Perez-Truglia
Contact email: taxproject@umich.edu
Program website: http://www.umich.edu/~taxproj/tax.html

Online Appendix: For Online Publication Only

A More Details about the Experimental Sample

A.1 Descriptive Evidence about How Delinquents can Get Off the List

In this subsection we discuss the observational evidence supporting the statutory claim that, once included in the list, a delinquent can be taken off the list if and only if she commits to pay the full amount of the debt (rather than the minimum amount necessary to take the debt amount just below the threshold).

The evidence for Kentucky, Kansas and Wisconsin is shown in Figures A.1.a, A.1.b and A.1.c. The data corresponds to the subject pool. For each state, the figure shows the distribution of debt amounts. If individuals could pay a small amount of money to get below the threshold and get off the list, this would imply that there would be some "missing density" just above the threshold (i.e., those individuals could "aim" at having unpaid debts below the threshold). However, we do not find evidence of such missing density in any of the states. The graph also shows the mean probability of leaving the list in the next 6 months, for each of the bins of the initial debt. If individuals could pay epsilon below the threshold to get off the list, we should observe a spike in the probability of leaving the list just above the threshold (in the extreme case, the individual that is $1 to the right of the threshold could pay $1.01 and get off the list). Again, we find no evidence consistent with that conjecture. Last, it should also be noted that, even if there was a way of paying to be taken below the threshold, that would only result in being taken off the list for a short time period, because the financial penalties would accumulate and take the total amount back above the threshold.

In sum, all the evidence supports the statutory claim that, once listed, individuals must pay the entire debt amount or enter a payment plan for the entire debt amount.

A.2 Further Descriptive Statistics

In this subsection, we present some complementary figures and tables. First, Figure A.2 shows the evolution of this outcome variable for each week from the beginning of the sample (Monday, May 26, 2014). Figure A.2.a shows the evolution over the entire sample, while Figures A.2.b-A.2.d show the evolution in each of the three states separately. These figures show that the probability that a given delinquent is off the list increases quite smoothly over time, although in Kentucky and Wisconsin there are some specific points in time when a larger-than-usual fraction of individuals leave the list (e.g., fifth week in Wisconsin and eleventh week in Kentucky).

According to our conversations with the tax agencies, those discontinuities reflect time points when, for administrative reasons, the tax authority makes a higher number of updates to the list.[41]

A.3 Regulations of Lists of Tax Delinquents

A.3.1 Kentucky

A snapshot of the webpage with the list of tax delinquents from Kentucky is shown in Figure A.3. In Kentucky the publication of delinquents owning taxes or other fees is regulated by KRS 131.650. According to it, "a taxpayer may be included on a list if: (a) The taxes or fees owed remain unpaid at least forty-five (45) days after the dates they became due and payable; and (b) A tax lien or judgment lien has been filed of public record against the taxpayer before notice is given under KRS 131.654." The provision related to the privacy of taxpayers are regulated by KRS 131.190. The notification to tax debtors is regulated by KRS 131.654. The requirements to qualify as tax delinquent are regulated by KRS 131.652.

A.3.2 Kansas

A snapshot of the webpage with the list of tax delinquents from Kansas is shown in Figure A.4. In Kansas taxation matters are regulated by chapter 79 of the state Statute. Article 79-3235 regulates the collection of debts arising from state income tax. A warrant is issued if taxes are not paid within 60 days after they become due. The warrant comprises the delinquent taxes, with the added penalties, interest and the costs associated with the warrant itself. The process of state income taxation is regulated by article 32 in chapter 79 of the Kansas Statute. Article 79-3228 regulates the process of administering interests and penalties.

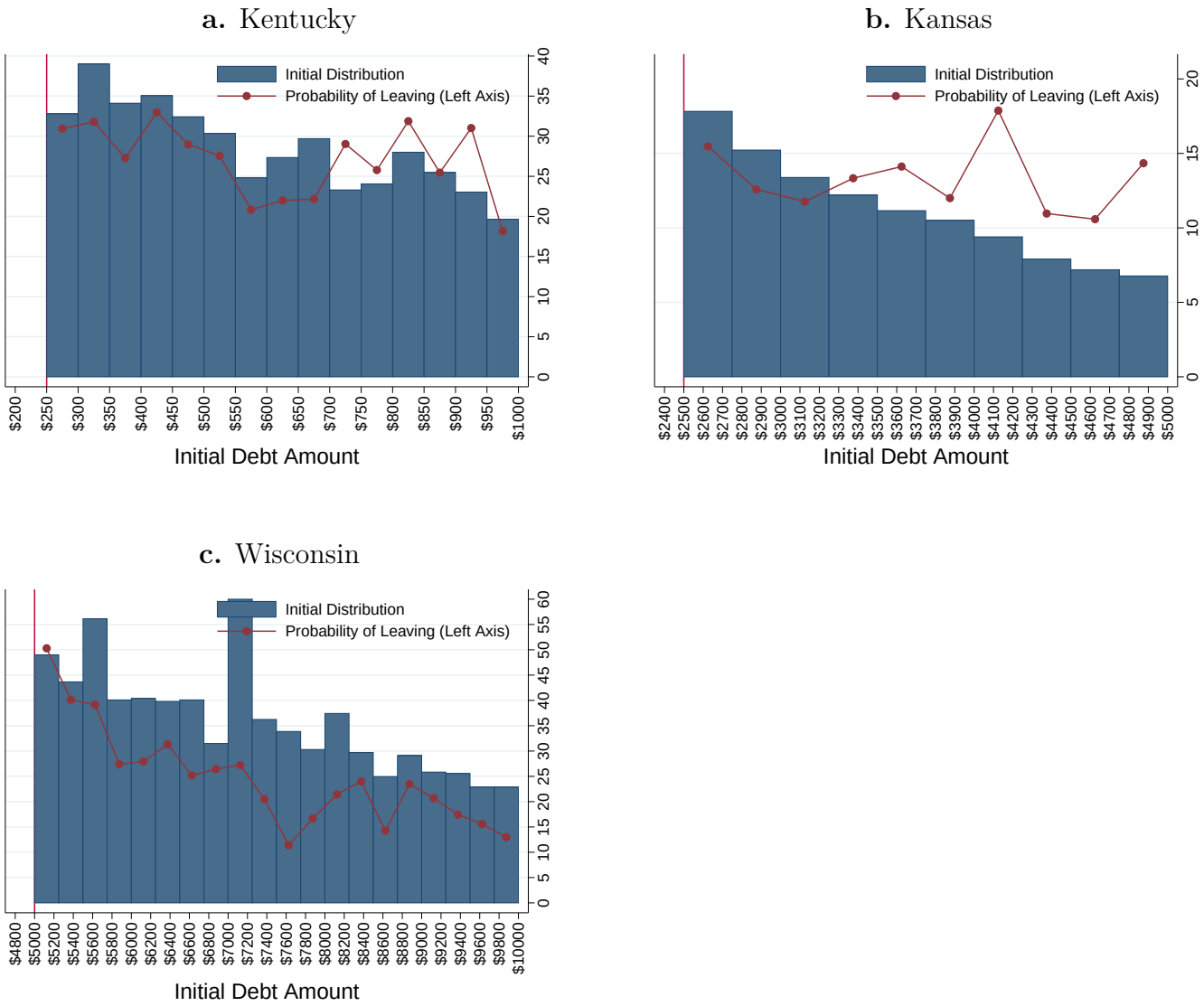
A.3.3 Wisconsin

A snapshot of the webpage with the list of tax delinquents from Wisconsin is shown in Figure A.5. In Wisconsin the publication of tax delinquents is regulated by section 73.03(62) of the Wisconsin statute. A requirement for publication is that the amount is unpaid more than 90 days after all appeal rights have expired. The Wisconsin department will not post the accounts of taxpayers who have: entered into a valid installment agreement, submitted a complete Petition for Compromise, or filed for bankruptcy. The process of reaching a repayment plan agreement with the Wisconsin Department of Revenue is regulated by section 71.92. The process of updating the online lists is regulated by s. 562.01 (3m). The process of taxing

[41] For example, given that many individuals submit their tax forms close to the same deadline, that tends to create spikes in the rate with which new individuals enter the list.

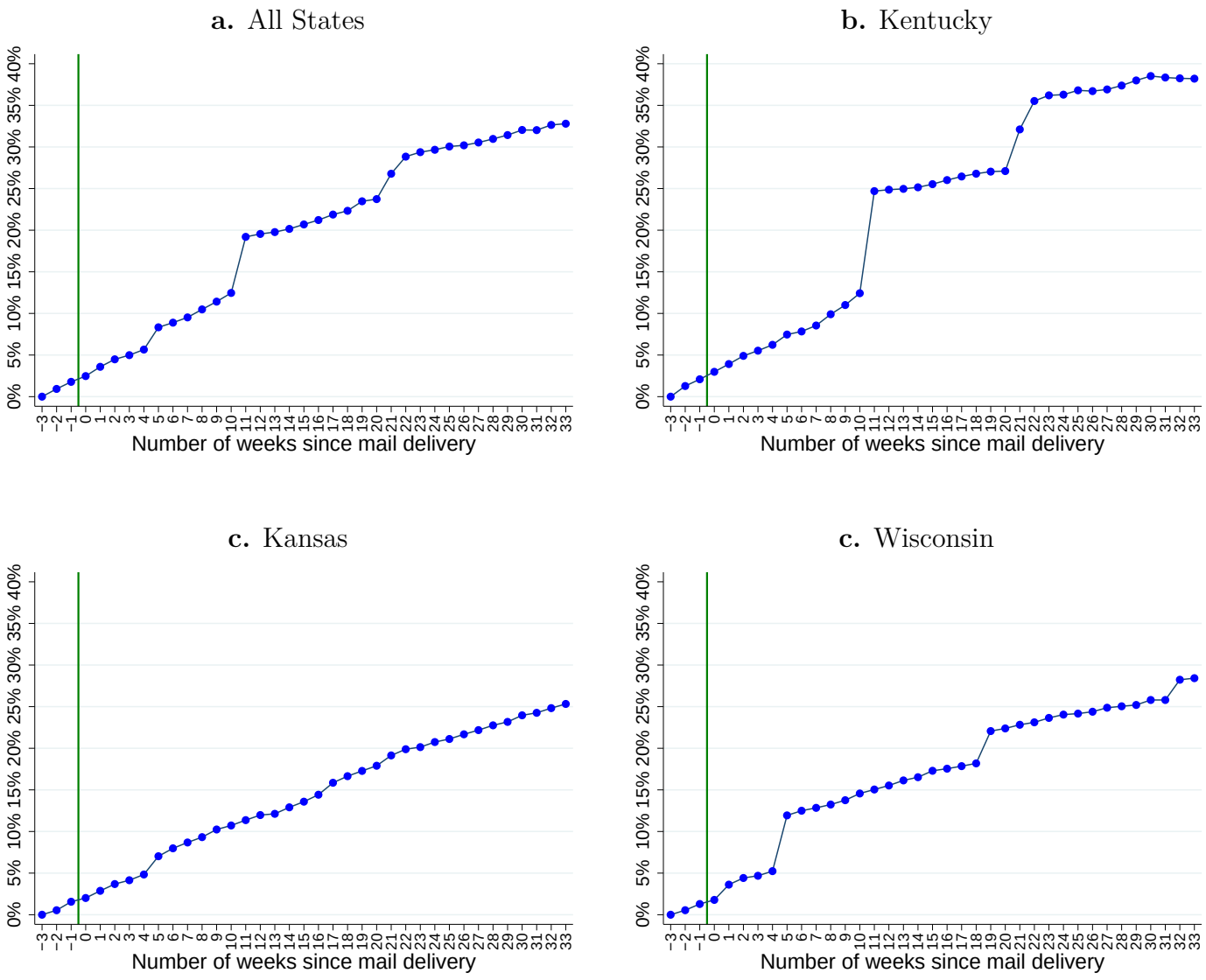
individuals is regulated by section 71.01. The interests and penalties are regulated by sections 71.82, 71.83, 71.84 and 71.85. The expression "liable for delinquent taxes" means that a person has exhausted all legal remedies to challenge the assertion that the person owes taxes, including penalties, interest, fees and costs, under ch. 71, 72, 76, 77, 78, 125 or 139 and sufficient time has elapsed so that the person is delinquent in the payment of those taxes.

Figure A.1: Descriptive Evidence about the Behavior of Tax Delinquents



Notes: N=18,101 in Kentucky (a.), 8,710 in Kansas (b.) and 7,523 in Wisconsin (c.). The blue bars show the histogram with the distribution of amounts owed by the subjects who appeared on the online lists of delinquents as of May 26th 2014. The red dots indicates, for the group of individuals in a given bin of amount owed as of May 26th 2014, the share of those individuals who are not listed as delinquents in exactly 6 months after May 26th 2014.

Figure A.2: The Evolution of the Probability of Leaving the List



Notes: N=34,334 (18,101 from Kentucky, 8,710 from Kansas and 7,523 from Wisconsin). In the x-axis, week -3 corresponds to the date when the subject pool was formed (May 26, 2014). The green vertical line shows the approximate date when the letters were delivered. The y-axis corresponds to the share of the subjects who were not longer listed online.

Figure A.3: Snapshot of Online Search Tool, Kentucky Department of Revenue

Figure A.4: Snapshot of Online Search Tool, Kansas Department of Revenue

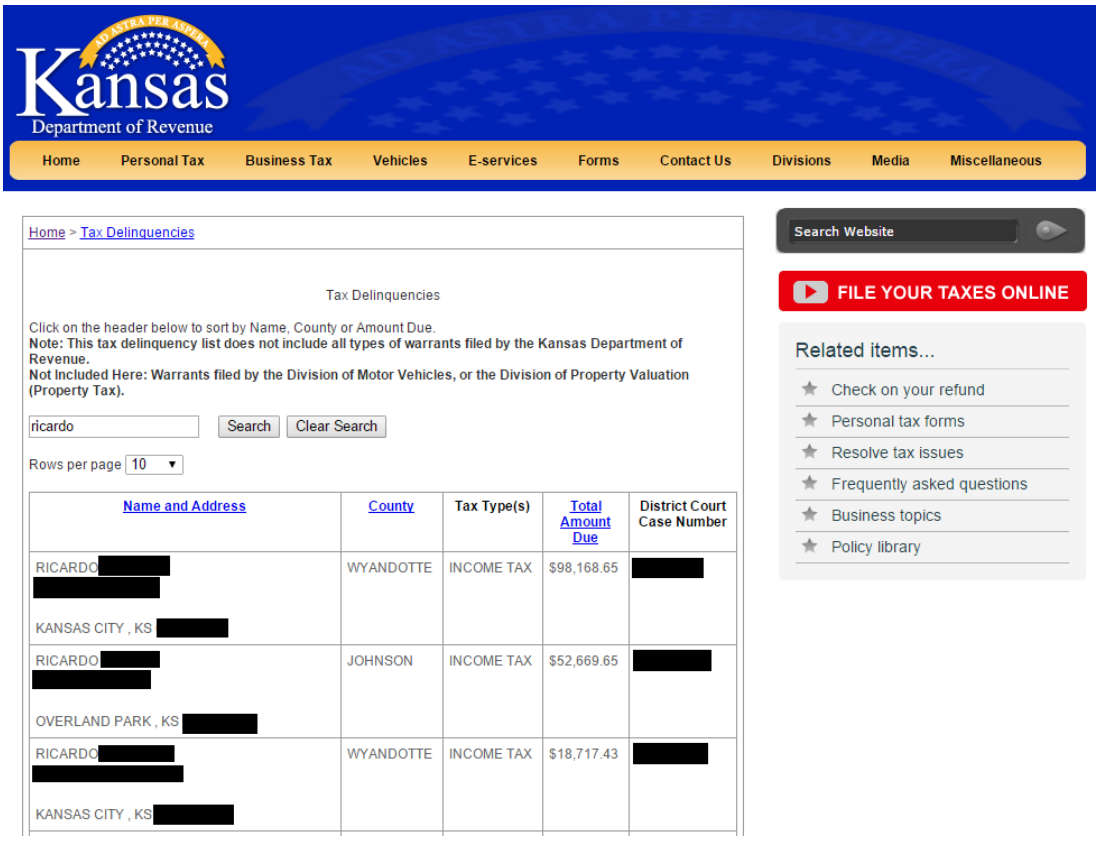
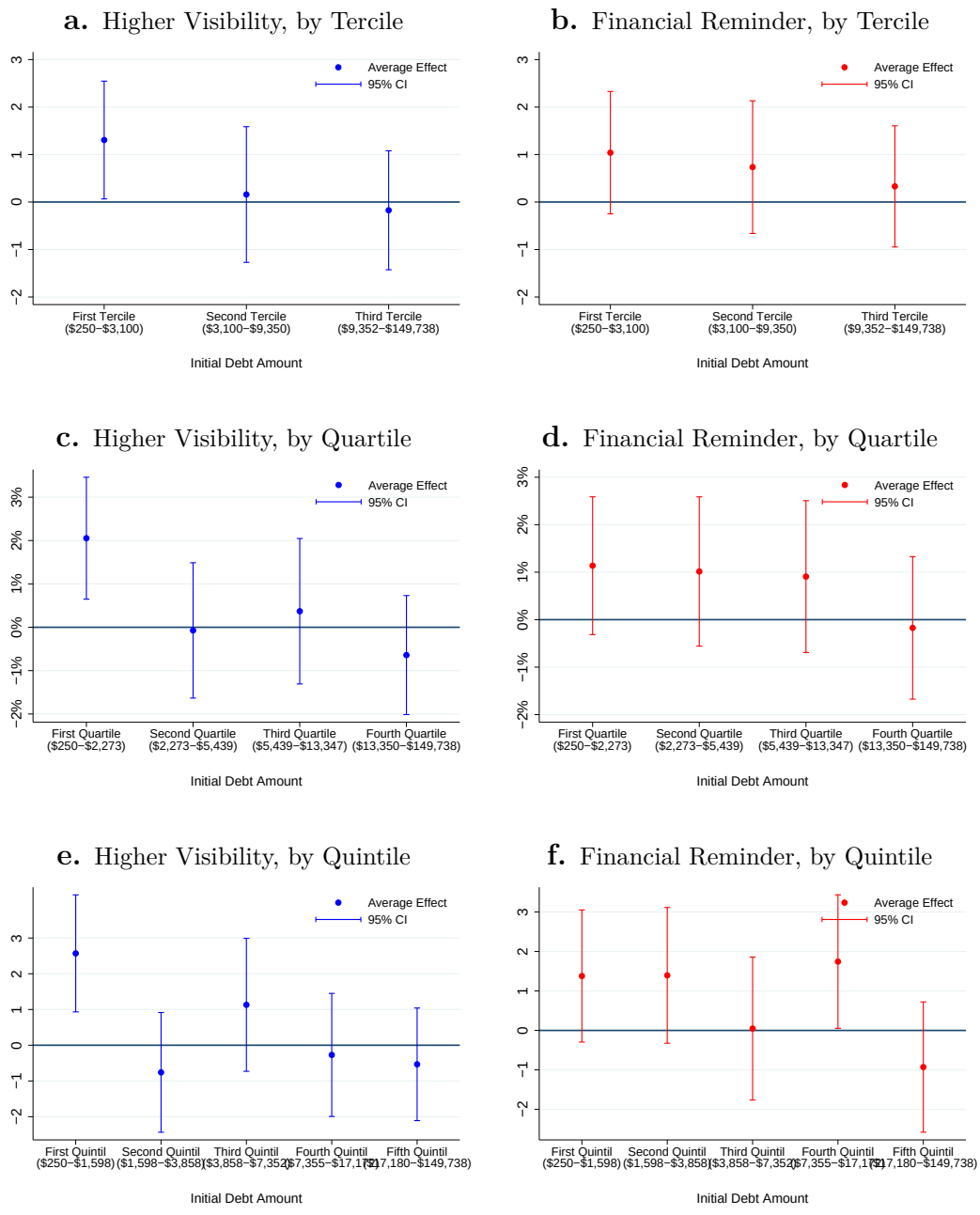


Figure A.5: Snapshot of Online Search Tool, Wisconsin Department of Revenue

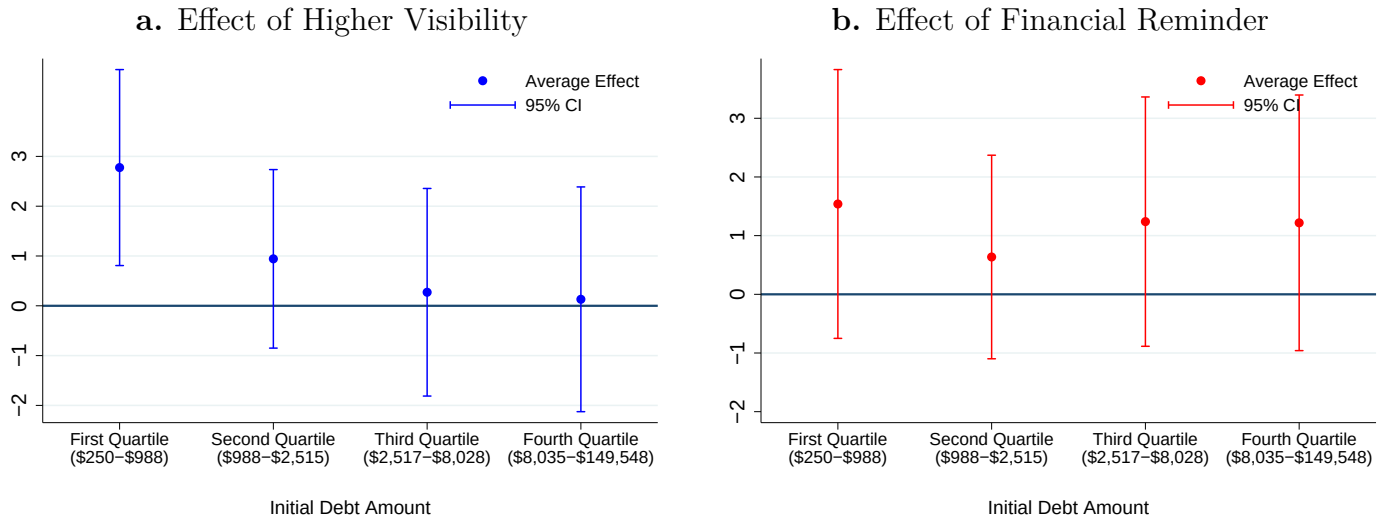
B Additional Robustness Checks

Figure B.1: Robustness Check: Effects of Information Treatments, 10 Weeks after Mail Delivery, by Terciles/Quartiles/Quintiles of Initial Debt Amount



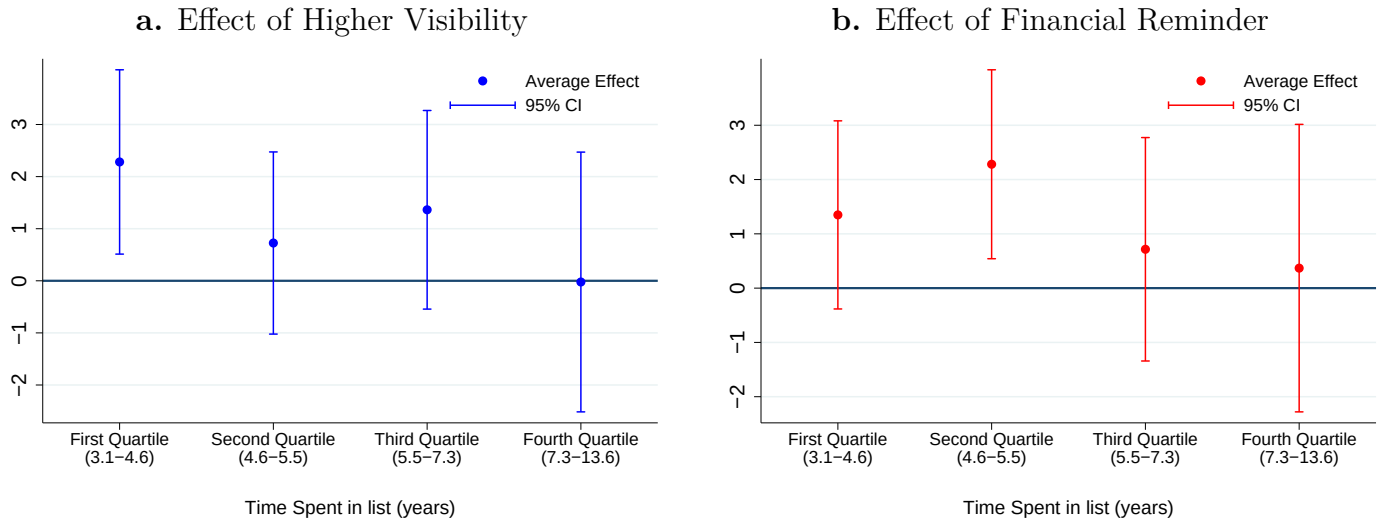
Notes: N=34,334. The debt amount in the x-axis corresponds to the amount owed when the subject pool was formed (May 26, 2014). The effects were estimated from OLS regressions (one for each group in the x-axis) where the dependent variable is a dummy for whether the subject is listed as a delinquent 10 weeks after the letters were delivered, and the right hand side variables are the treatment dummies plus a set of control variables: gender, ethnicity and state dummies, initial debt amount and its logarithm (with state-specific coefficients) and the number of delinquents in the ZIP code. *Higher Visibility* is a dummy that takes the value 0 if the recipient was the only one in the area chosen to receive a letter, and 1 if others in the area were chosen to receive a letter too. *Financial Reminder* is a dummy that takes the value 1 if the letter included information about the financial penalties and 0 if not. Confidence intervals computed with standard errors clustered at the 5-digit ZIP code level.

Figure B.2: Effects of Information Treatments, 10 Weeks after Mail Delivery, Kentucky Sample, by Quartile of Debt Amount



Notes: N=18,101. The debt amount in the x-axis corresponds to the amount owed when the subject pool was formed (May 26, 2014). The effects were estimated from OLS regressions (one for each group in the x-axis) where the dependent variable is a dummy for whether the subject is listed as a delinquent 10 weeks after the letters were delivered, and the right hand side variables are the treatment dummies plus a set of control variables: gender, ethnicity and state dummies, initial debt amount and its logarithm (with state-specific coefficients) and the number of delinquents in the ZIP code. *Higher Visibility* is a dummy that takes the value 0 if the recipient was the only one in the area chosen to receive a letter, and 1 if others in the area were chosen to receive a letter too. *Financial Reminder* is a dummy that takes the value 1 if the letter included information about the financial penalties and 0 if not. Confidence intervals computed with standard errors clustered at the 5-digit ZIP code level.

Figure B.3: Effects of Information Treatments, 10 Weeks after Mail Delivery, Kentucky Sample, by Quartile of Pre-Treatment Debt Age



Notes: N=18,101. The x-axis corresponds to the quartiles of the distribution of the time elapsed from the origination of the debt until the date in which the subject pool was formed (May 26, 2014) – this information is only available for subjects from Kentucky. The effects were estimated from OLS regressions (one for each group in the x-axis) where the dependent variable is a dummy for whether the subject is listed as a delinquent 10 weeks after the letters were delivered, and the right hand side variables are the treatment dummies plus a set of control variables: gender, ethnicity and state dummies, initial debt amount and its logarithm (with state-specific coefficients) and the number of delinquents in the ZIP code. *Higher Visibility* is a dummy that takes the value 0 if the recipient was the only one in the area chosen to receive a letter, and 1 if others in the area were chosen to receive a letter too. *Financial Reminder* is a dummy that takes the value 1 if the letter included information about the financial penalties and 0 if not. Confidence intervals computed with standard errors clustered at the 5-digit ZIP code level.

Table B.1: Randomization Balance Test: 1st Quartile of Initial Debt Amount

	Visibility		Financial Reminder		Peer Information			Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Lower	Higher	No	Yes	$\alpha = -1$	$\alpha = 0$	$\alpha = 1$	P-value
Initial Debt Amount ($1,000s)	1.04	1.04	1.04	1.04	1.05	1.03	1.04	0.83
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	
Log(Initial Debt Amount)	6.78	6.78	6.78	6.78	6.79	6.78	6.78	0.97
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	
Percent Male	59.38	60.38	59.88	59.88	60.39	59.40	59.82	0.83
	(0.75)	(0.74)	(0.77)	(0.73)	(0.90)	(0.93)	(0.92)	
Percent White	73.38	73.33	73.20	73.49	73.57	73.67	72.83	0.65
	(0.36)	(0.36)	(0.37)	(0.35)	(0.44)	(0.44)	(0.45)	
Percent Black	15.83	15.82	16.03	15.63	15.55	15.84	16.08	0.44
	(0.22)	(0.22)	(0.22)	(0.21)	(0.27)	(0.26)	(0.27)	
Percent Hispanic	4.07	4.22	4.35	3.96	4.05	3.90	4.48	0.41
	(0.23)	(0.23)	(0.24)	(0.22)	(0.27)	(0.27)	(0.30)	
Percent Other	3.02	3.04	3.12	2.95	3.00	3.03	3.06	0.47
	(0.07)	(0.07)	(0.08)	(0.05)	(0.07)	(0.08)	(0.09)	
Observations	4,271	4,313	4,088	4,496	2,951	2,786	2,847	

Notes: Sample of individuals in the first quartile of the initial debt amount ($250–$2,273). Average individual (pre-treatment) characteristics by treatment group, with standard errors in parenthesis. *Higher Visibility* is a dummy that takes the value 0 if the recipient was the only one in the area chosen to receive a letter, and 1 if others in the area were chosen to receive a letter too. *Financial Reminder* is a dummy that takes the value 1 if the letter included information about the financial penalties and 0 if not. *Peer Information* corresponds to the value of the weighting parameter used to select the delinquents to be listed in the table shown to the recipient. The p-value corresponds to the test of the null hypothesis that the average characteristics are the same across all treatment groups. The initial debt amount corresponds to the amount owed when the subject pool was formed (May 26, 2014). Gender and ethnicity are not observed directly. Data for these characteristics is imputed using data on the joint distribution of first names and gender (several sources, including data from the U.S. Census Bureau), and the joint distribution of last names and ethnicities (data from U.S. Census Bureau). The omitted category for gender is male, and the omitted category for ethnicity corresponds to unmatched last names.

Table B.2: Randomization Balance Test: 2nd Quartile of Initial Debt Amount

	Visibility		Financial Reminder		Peer Information			Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Lower	Higher	No	Yes	$\alpha = -1$	$\alpha = 0$	$\alpha = 1$	P-value
Initial Debt Amount ($1,000s)	3.68	3.68	3.68	3.68	3.70	3.69	3.65	0.25
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	
Log(Initial Debt Amount)	8.18	8.18	8.18	8.18	8.19	8.18	8.17	0.23
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	
Percent Male	62.33	62.03	62.87	61.56	61.19	61.27	64.01	0.09
	(0.74)	(0.74)	(0.76)	(0.72)	(0.93)	(0.91)	(0.89)	
Percent White	69.80	69.11	69.15	69.72	69.68	69.42	69.27	0.67
	(0.43)	(0.45)	(0.45)	(0.43)	(0.55)	(0.53)	(0.54)	
Percent Black	14.86	14.21	14.82	14.28	14.52	14.84	14.25	0.07
	(0.23)	(0.23)	(0.24)	(0.22)	(0.30)	(0.29)	(0.27)	
Percent Hispanic	6.76	7.36	7.38	6.77	7.26	6.95	6.98	0.43
	(0.32)	(0.33)	(0.34)	(0.31)	(0.41)	(0.39)	(0.39)	
Percent Other	3.45	3.55	3.42	3.58	3.50	3.57	3.44	0.82
	(0.12)	(0.12)	(0.11)	(0.13)	(0.15)	(0.15)	(0.14)	
Observations	4,290	4,293	4,051	4,532	2,775	2,874	2,934	

Notes: Sample of individuals in the second quartile of the initial debt amount ($2,273–5,439). Average individual (pre-treatment) characteristics by treatment group, with standard errors in parenthesis. *Higher Visibility* is a dummy that takes the value 0 if the recipient was the only one in the area chosen to receive a letter, and 1 if others in the area were chosen to receive a letter too. *Financial Reminder* is a dummy that takes the value 1 if the letter included information about the financial penalties and 0 if not. *Peer Information* corresponds to the value of the weighting parameter used to select the delinquents to be listed in the table shown to the recipient. The p-value corresponds to the test of the null hypothesis that the average characteristics are the same across all treatment groups. The initial debt amount corresponds to the amount owed when the subject pool was formed (May 26, 2014). Gender and ethnicity are not observed directly. Data for these characteristics is imputed using data on the joint distribution of first names and gender (several sources, including data from the U.S. Census Bureau), and the joint distribution of last names and ethnicities (data from U.S. Census Bureau). The omitted category for gender is male, and the omitted category for ethnicity corresponds to unmatched last names.

Table B.3: Randomization Balance Test: 3rd Quartile of Initial Debt Amount

	Visibility		Financial Reminder		Peer Information			Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Lower	Higher	No	Yes	$\alpha = -1$	$\alpha = 0$	$\alpha = 1$	P-value
Initial Debt Amount ($1,000s)	8.45	8.48	8.42	8.51	8.48	8.45	8.47	0.42
	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	
Log(Initial Debt Amount)	9.01	9.01	9.00	9.01	9.01	9.01	9.01	0.40
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	
Percent Male	64.74	64.55	65.39	63.99	64.78	64.87	64.28	0.71
	(0.73)	(0.73)	(0.75)	(0.71)	(0.89)	(0.90)	(0.89)	
Percent White	69.14	69.30	68.78	69.61	69.39	69.74	68.54	0.41
	(0.47)	(0.46)	(0.48)	(0.45)	(0.57)	(0.56)	(0.57)	
Percent Black	13.47	13.42	13.81	13.12	13.30	13.15	13.88	0.09
	(0.24)	(0.23)	(0.24)	(0.23)	(0.29)	(0.28)	(0.29)	
Percent Hispanic	6.60	6.29	6.75	6.17	6.14	6.57	6.62	0.53
	(0.31)	(0.30)	(0.33)	(0.29)	(0.37)	(0.38)	(0.38)	
Percent Other	3.60	3.55	3.70	3.46	3.66	3.58	3.49	0.67
	(0.13)	(0.13)	(0.14)	(0.12)	(0.16)	(0.16)	(0.15)	
Observations	4,271	4,313	4,002	4,582	2,862	2,844	2,878	

Notes: Sample of individuals in the third quartile of the initial debt amount ($5,439–$13,347). Average individual (pre-treatment) characteristics by treatment group, with standard errors in parenthesis. *Higher Visibility* is a dummy that takes the value 0 if the recipient was the only one in the area chosen to receive a letter, and 1 if others in the area were chosen to receive a letter too. *Financial Reminder* is a dummy that takes the value 1 if the letter included information about the financial penalties and 0 if not. *Peer Information* corresponds to the value of the weighting parameter used to select the delinquents to be listed in the table shown to the recipient. The p-value corresponds to the test of the null hypothesis that the average characteristics are the same across all treatment groups. The initial debt amount corresponds to the amount owed when the subject pool was formed (May 26, 2014). Gender and ethnicity are not observed directly. Data for these characteristics is imputed using data on the joint distribution of first names and gender (several sources, including data from the U.S. Census Bureau), and the joint distribution of last names and ethnicities (data from U.S. Census Bureau). The omitted category for gender is male, and the omitted category for ethnicity corresponds to unmatched last names.

Table B.4: Randomization Balance Test: 4th Quartile of Initial Debt Amount

	Visibility		Financial Reminder		Peer Information			Difference
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Lower	Higher	No	Yes	$\alpha = -1$	$\alpha = 0$	$\alpha = 1$	P-value
Initial Debt Amount ($1,000s)	38.00	38.68	38.74	37.98	38.46	38.97	37.57	0.17
	(0.43)	(0.44)	(0.46)	(0.41)	(0.53)	(0.53)	(0.52)	
Log(Initial Debt Amount)	10.34	10.35	10.35	10.34	10.35	10.36	10.32	0.16
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	
Percent Male	70.76	71.36	71.26	70.88	70.77	70.69	71.71	0.84
	(0.69)	(0.69)	(0.72)	(0.67)	(0.85)	(0.85)	(0.84)	
Percent White	71.15	71.65	70.84	71.88	71.04	71.15	72.00	0.32
	(0.47)	(0.47)	(0.49)	(0.45)	(0.58)	(0.57)	(0.56)	
Percent Black	11.61	11.45	11.80	11.30	11.58	11.69	11.32	0.41
	(0.22)	(0.21)	(0.23)	(0.21)	(0.27)	(0.27)	(0.26)	
Percent Hispanic	6.00	5.44	5.62	5.81	5.62	5.79	5.75	0.70
	(0.29)	(0.28)	(0.30)	(0.28)	(0.35)	(0.35)	(0.35)	
Percent Other	3.26	3.36	3.21	3.40	3.24	3.31	3.38	0.72
	(0.11)	(0.12)	(0.12)	(0.12)	(0.14)	(0.14)	(0.15)	
Observations	4,323	4,260	3,984	4,599	2,843	2,880	2,860	

Notes: Sample of individuals in the fourth quartile of the initial debt amount ($13,350–$149,738). Average individual (pre-treatment) characteristics by treatment group, with standard errors in parenthesis. *Higher Visibility* is a dummy that takes the value 0 if the recipient was the only one in the area chosen to receive a letter, and 1 if others in the area were chosen to receive a letter too. *Financial Reminder* is a dummy that takes the value 1 if the letter included information about the financial penalties and 0 if not. *Peer Information* corresponds to the value of the weighting parameter used to select the delinquents to be listed in the table shown to the recipient. The p-value corresponds to the test of the null hypothesis that the average characteristics are the same across all treatment groups. The initial debt amount corresponds to the amount owed when the subject pool was formed (May 26, 2014). Gender and ethnicity are not observed directly. Data for these characteristics is imputed using data on the joint distribution of first names and gender (several sources, including data from the U.S. Census Bureau), and the joint distribution of last names and ethnicities (data from U.S. Census Bureau). The omitted category for gender is male, and the omitted category for ethnicity corresponds to unmatched last names.

Table B.5: Peer Information: Effects of the Parameter α on the Distribution of Amounts by Delinquent Neighbors

	Peer Mean	Peer Median	Own Rank	Own Share
	(1)	(2)	(3)	(4)
All sample (N=34,334):				
Peer Information	28.040***	14.282***	-4.018***	-0.353***
	(1.078)	(0.567)	(0.053)	(0.004)
Constant	4.132***	2.422***	7.412***	0.604***
	(0.168)	(0.131)	(0.028)	(0.002)
Kentucky (N=18,101):				
Peer Information	18.852***	8.617***	-3.756***	-0.378***
	(0.716)	(0.287)	(0.072)	(0.007)
Constant	1.742***	0.543***	7.307***	0.596***
	(0.124)	(0.079)	(0.038)	(0.004)
Kansas (N=8,710):				
Peer Information	17.222***	7.117***	-3.939***	-0.293***
	(1.196)	(0.306)	(0.081)	(0.006)
Constant	4.025***	3.172***	7.398***	0.605***
	(0.112)	(0.055)	(0.048)	(0.003)
Wisconsin (N=7,523):				
Peer Information	63.295***	34.754***	-4.743***	-0.365***
	(3.052)	(1.357)	(0.075)	(0.006)
Constant	9.841***	6.656***	7.678***	0.623***
	(0.314)	(0.210)	(0.045)	(0.003)

Notes: * significant at the 10% level, ** at the 5% level, *** at the 1% level. Standard errors in parentheses clustered at the 5-digit ZIP code level. The coefficients were estimated from OLS regressions (one per column/state) *Peer Mean* is the mean of delinquent amount by neighbors listed in the letter (in \$1,000s), excluding the recipient *Peer Median* is the median of delinquent amount by neighbors listed in the letter (in \$1,000s), excluding th recipient. *Own Rank* is the recipient own position in the table of delinquent amounts in the area, from bottom o the table (1) to top of the table (10). *Own Share* is the recipient's own amount divided by the sum of over th amounts of the 10 delinquents listed in the table. *Peer Information* indicates if the recipient was shown neighbor with higher delinquent amounts – it takes the value 0, 0.5 or 1 depending on whether the individuals to be shown in the table of delinquent neighbors we selected with a parameter $\alpha = -1$, $\alpha = 0$ or $\alpha = 1$, respectively.

C A Model of Tax Delinquency with Financial and Shaming Penalties

C.1 Financial Penalty

There is a continuum of taxpayers indexed by subscript i, who each have a tax responsibility normalized to 1. There are two periods. In the first period, the individual can either pay the tax due ($x_i = 1$) or not pay it ($x_i = 0$) and as a result becoming a tax delinquent. Some individuals are liquidity constrained, so that paying taxes in the first period will not allow them to conduct their lives normally (e.g., eat, keep their businesses alive, pay medical bills). To represent this preference for paying later, we assume that paying in the first period has a cost of $R_i > 1$, which is uniformly distributed between $\underline{R}$ and $\overline{R}$.[42] The government does not observe each R_i, although it knows how this variable is distributed in the population.

The government also prefers revenues in the first period to revenues in the second period. The government's value for receiving payment in the first period is $R_g > 1$. For individuals who did not pay in their first period, the effective debt at the second period will be F, where $F > 1$ is the size of the financial penalty.[43]

C.2 Collection Tool

In this second period, if the individual refuses to pay then the government will try to force the payment using some collection tool. For example, the creditor can force payments of wage earners through wage garnishment.[44] However, the collection tool is imperfect: an individual expects to be immune with probability $1 - q_i \in [0, 1]$. As a result, debtor i expects to pay $F \cdot q_i$ in the second period. We allow for heterogeneity in q_i: a proportion $\theta \in (0, 1)$ of the population expects its income to be "garnishable" with probability $q_i = \underline{q}$, and the remaining $1 - \theta$ expects to be garnishable with probability $q_i = \overline{q} \geq \underline{q}$. One easy interpretation is that $1 - \theta$ is the share of future wage earners, from whom it is easier to garnish income. The government knows the

[42] This heterogeneity, of course, depends on a number of factors, such as number and type of credit lines that are still open, ability to borrow money from family and friends, credit history and possibly many other considerations.

[43] We implicitly assume that there is not an ex-ante limit to the size of financial penalties, which turns out to be consistent with a number of facts. For example, states typically exempt the Department of Revenues from complying with usury laws when setting up penalties for tax delinquencies (see for example Revised Code of Washington 19.52.140). Also, the financial penalties typically vary from year to year, with both increases and decreases, which is suggestive of the absence of restrictions.

[44] In practice, there are a number of different technologies for enforcing collection so that, for a given individual, the government may want to use the one that is most cost-effective for that particular individual. We abstract from this aspect because it is not relevant for the results that follow.

distribution of q_i but does not observe the q_i of each individual.[45] For the sake of simplicity, we assume that q_i is always observable to one's peers (this assumption does not change the main results, but does make the model considerably more tractable).

C.3 Shaming Penalty

Whether the individual is a tax delinquent is observable to peers with probability p, which is a parameter under the control of the tax agency. The creation of an online list of tax delinquents can be seen as an increase from $p = 0$ to some $p > 0$. Further steps, such as including a search tool or advertising the list of delinquents, can increase p even further. We assume that increasing p has no significant costs. This simplifying assumption is for the purposes of clarity: even though adding a cost would lead to a lower use of the policy, it would not change the main qualitative results that follows.

To model how the shaming penalty affects the decision to pay taxes, we take a signaling approach to social interactions (Cole, Mailath and Postlewaite (1992); Bernheim (1994); Bénabou and Tirole (2006, 2011)). According to this framework, individuals may avoid anti-social behavior because that may signal untrustworthiness.[46] In our model, taxpayers interact with peers after deciding whether to pay the tax or not. The individual's utility from these interactions depends on her peers' perception about her financial trustworthiness. This value of reputation may be instrumental (e.g., through a higher likelihood of obtaining an invitation, a job/business opportunity, a romantic/friendly proposal) or purely affective (e.g., social esteem as an hedonic good). To represent this, we assume that the utility of the individual is a linear function of her expected financial trustworthiness, from the perspective of her peers: $-\eta \cdot E\left[R_i|I_i\right]$, where I_i is the observable information about i and η is a parameter that scales the value of social interactions. With probability $p \in [0,1]$, I_i includes whether the individual paid her taxes in the first period.

The expected utility from social interactions can be re-expressed as:

$$-\eta\left[p \cdot (1-x_i) \cdot \left(E\left[R_i|x_i=0\right] - E\left[R_i|x_i=1\right]\right) + (1-p) \cdot \frac{\underline{R}+\overline{R}}{2}\right]$$

The term $p \cdot \eta \cdot \left(E\left[R_i|x_i=0\right] - E\left[R_i|x_i=1\right]\right)$ is the signaling cost from failing to pay taxes

[45] In practice, even if q_i was perfectly observable to the collector, the results would stay the same as long as the financial penalty cannot be made dependent on q_i: e.g., if the tax agency cannot charge a higher interest rate to wage earners than to the self-employed. In practice, fairness concerns may prevent government from discriminating even if they could (as in Mankiw and Weinzierl (2010)). Additionally, benevolent states may not want to give bureaucrats the power to discriminate because that power may be exploited for personal gain (i.e., corruption) or to harass individuals based on their race or political beliefs.

[46] There is a body of evidence consistent with this social signaling approach. For example, Tadelis (2011) provides evidence from a lab experiment that individuals not only exhibit concerns for how they are perceived by others, but they seem strategically rational by anticipating the change in behavior of their opponents.

in the first period. That is, increasing the visibility of the decision to pay taxes makes paying on time more attractive, because failing to pay on time can serve as a bad signal of trustworthiness and thus result in worse outcomes in social interactions.[47]

Integrating over the population of individuals, we can obtain the average utility from social interactions: $-\eta\frac{\underline{R}+\overline{R}}{2}$. This value does not depend on p, meaning that disseminating information about delinquents redistributes utility across taxpayers (i.e., from individuals who did not pay in the first period to individuals who did pay), but does not affect the aggregate utility from social interactions. This convenient property is a direct product of the linearity assumption on the value of social interactions: $-\eta \cdot E\left[R_i|I_i\right]$.[48]

This model of social interactions assumes that peers care about financial trustworthiness. The results, however, are similar when peers care instead about moral trustworthiness. Appendix E.2 provides an extension of the model where individuals have an additional source of utility from paying on time, related to the pride from doing what is right. Instead of signaling financial trustworthiness, being a delinquent can serve as a signal of moral trustworthiness. The Appendix shows that the main qualitative results that follow are the same under this alternative specification.[49]

C.4 The Taxpayer's Problem

Combining the financial and social incentives, the individual solves the following maximization problem:

$$\max_{x_i\in\{0,1\}} U\left(x_i;R_i\right), \text{ with}$$

$$U\left(x_i;R_i\right) = -R_i\cdot x_i - (1-x_i)\cdot\left[q_i\cdot F + p\cdot\eta\cdot\left(E\left[R_i|x_i=0\right] - E\left[R_i|x_i=1\right]\right)\right] + \eta\cdot(1-p)\cdot\frac{\underline{R}+\overline{R}}{2}$$

[47] Gerber et al. (2015) provides some suggestive linking tax compliance to social image. They conducted a survey experiment where respondents were shown information about a hypothetical individual and then were asked how favorably they view this person. They show that respondents view the hypothetical individual less favorably when the individual was described as someone who never votes in presidential elections relative to someone who always votes. Additionally, they show that never voting (vs. always voting) has an effect on social image of similar magnitude than paying taxes late (vs. paying taxes on time). They also show similar effects of other informational treatments, such as recycling (vs. not recycling) and returning books on time (vs. late).

[48] A non-linear specification could make the aggregate utility from social interactions depend on p either negatively or positively. As a result, relaxing this assumption could make the shaming penalty either more or less desirable. Furthermore, we are ignoring the utility that the peers obtain from the social interactions with taxpayers. Including this utility into the social welfare function would make the shaming penalty more desirable. A higher p should increase the utility of the peers, because they have more information and thus should get better outcomes from their social interactions. Indeed, the peers could even increase the number of interactions that they choose to have.

[49] In the real world, it is likely that peers care about a combination of both financial and moral trustworthiness.

Let the debtor's optimal response be denoted $x^*(q,R) = \arg\max_{x\in\{0,1\}} U(x;q,R)$. This optimal response can be characterized as a threshold decision:

$$x^*(q,R) = 1\left[R \leq \hat{R}(q)\right]$$

Assuming all taxpayers are responding like this, we can obtain the rational inference from the perspective of peers:

$$E[R_i|x_i=1] = \frac{\underline{R}+\hat{R}}{2} \text{ and } E[R_i|x_i=0] = \frac{\hat{R}+\overline{R}}{2}$$

Replacing that back into the objective function:

$$U(x_i;R_i) = -R_i\cdot x_i - (1-x_i)\cdot\left[q_i\cdot F + p\cdot\eta\cdot\frac{\overline{R}-\underline{R}}{2}\right] + (1-p)\cdot\eta\cdot\frac{\underline{R}+\overline{R}}{2}$$

Thus, individual i chooses $x_i = 1$ iff:

$$R_i \leq q_i\cdot F + p\cdot\eta\cdot\frac{\overline{R}-\underline{R}}{2}$$

Which confirms our guess that the optimal response consists of the cutoff decision $\hat{R}(q) = \max\left\{\underline{R},\ \min\left\{q\cdot F + p\cdot\eta\cdot\frac{\overline{R}-\underline{R}}{2}, \overline{R}\right\}\right\}$. As expected, the proportion of individuals paying in the first period is decreasing in the financial penalty, F, and in the shaming penalty, p.

C.5 The Government's Problem

The government chooses two policies: the financial penalty, F, and the shaming penalty, p. Let T denote the present value of government revenues:

$$T(F,p) = \int\int\left[x^*(q,R)\cdot R_g + (1-x^*(q,R))\cdot q\cdot F\right]dF(R)\,dG(q)$$

Recall that R_g denotes the government's own discount rate. Let $PW(F,p)$ denote the present value of private welfare of the taxpayers:

$$PW(F,p) = -\int\int\left[x^*(q,R)\cdot R + (1-x^*(q,R))\cdot q\cdot F\right]dF(R)\,dG(q)$$

Note that we omit the aggregate utility from social interactions, without loss of generality, because this aggregate utility does not depend on F or p. The government maximizes social welfare, which is a weighted average of the tax revenues and private welfare:

$$\max_{F\geq 1, p\in[0,1]} SW(F,p) = \alpha\cdot T(F,p) + (1-\alpha)\cdot PW(F,p)$$

Where $\alpha \in \left[\frac{1}{2}, 1\right]$ measures how much the government values an additional dollar in its own pocket (in the second period) versus in the pockets of the taxpayers (in the second period). In one extreme case, $\alpha = \frac{1}{2}$, the government is indifferent between the two. This can be consistent with a government that maximizes social welfare and faces a marginal return from government expenditures that is equal to the marginal return faced by the individual taxpayers. An $\alpha > \frac{1}{2}$ is a reduced-form representation of a benevolent government that values tax revenues more than private welfare because it uses those revenues to provide the efficient level of a public good, as in Samuelson (1954). The higher the returns to government spending, the higher α should be. In the other extreme case, $\alpha = 1$, the government only cares about maximizing revenues, no matter the cost to the taxpayers. In reality, we expect tax agencies to have preferences somewhere in the middle of the two extreme cases.

C.6 Ranking Policies

The followings definitions are useful to rank sets of policies:

Definition 1. Given two sets of policies A and B, they are interchangeable if for every policy in B there is a policy in A such that the government attains the same social welfare and for every policy in A there is a policy in B such that the government attains the same social welfare.

Definition 2. Given two sets of policies A and B, A dominates B if for each policy in B there is a policy in A such that the government attains higher social welfare.

For ranking policies, the possibility of corner solutions can introduce an extra layer of complexity. For example, consider an extreme case where R_g is arbitrarily larger than $\overline{R}$: i.e., the government is infinitely impatient. In that case, the shaming penalty could not help the government do better, because the government can attain the first best by simply setting a financial penalty that is arbitrarily large so that everyone pays in the first period. However, this negative results stems entirely from the (simplifying) assumption that R is bounded. To separate these extreme cases, we introduce the following assumption:

Assumption 1. Let $F^*(\alpha)$ be the set of optimal financial penalties that maximize $SW(F, p = 0)$ given a set of parameter values $\left\{\underline{R}, \overline{R}, \underline{q}, \overline{q}, \theta, \eta, R_g\right\}$. We assume that these parameter values are such as: $\cup_{\alpha \in \left[\frac{1}{2}, 1\right]} F^*(\alpha) \in \left(\frac{\underline{R}}{\underline{q}}, \frac{\overline{R}}{\overline{q}}\right)$.

This assumption implies that, if the government only had access to the financial penalty and regardless of the value of α, the optimal financial penalty would always be an interior solution (i.e., some individuals pay in the first period and some other individuals pay in the second period). This assumption excludes the possibility of extreme cases like the infinitely-impatient government discussed above.

The next two subsections presents the main results. We want to show that for the shaming penalty to be optimal we need both heterogeneity in q_i and a low enough α. For ease of exposition, we present the results in two parts. First, we show that with a homogeneous q_i, there is no α such as the shaming penalty is optimal. Second, we show that under a heterogeneous q_i, the shaming penalty is optimal as long as α is low enough.

C.7 Optimal Penalties under Homogeneity in q_i

The following proposition ranks the policies under homogeneity in q_i:

Proposition 1. *If $\overline{q} = \underline{q}$:*

- If $\alpha = \frac{1}{2}$, then the sets of policies $\{(F,p):\ F \geq 0,\ p = 0\}$ is interchangeable with the set $\{(F,p):\ F \geq 0,\ p \in (0,1]\}$.

- If $\alpha > \frac{1}{2}$, then the set of policies $\{(F,p):\ F \geq 0,\ p = 0\}$ dominates $\{(F,p):\ F \geq 0,\ p \in (0,1]\}$.

Proof. See section C.9.1.

To see the intuition behind this result, it is easier to start with the case $\alpha = 1$, when the government wants to maximize revenues. Both the financial and the shaming penalties have the capability of increasing the proportion of individuals who pay in the first period. However, the financial penalty is superior to the shaming penalty because it generates additional revenues from the individuals who do not pay in the first period and therefore must pay above-market interest in the second period. As a result, a revenue-maximizing collector would not rely on shaming penalties if the use of financial penalties is unrestricted. In the other extreme case, when $\alpha = \frac{1}{2}$, the government simply wants the group with $R < R_g$ to pay right away and the group with $R > R_g$ to pay in the second period. For that, the government can simply choose $F \cdot q = R_g$ and let the individuals maximize the utility of the government, attaining the first best. Even though combining F with $p > 0$ would not harm the government, it could not make it better either. That is, the government is indifferent about whether using $p > 0$ or $p = 0$. As a result, even if the government has an infinitesimal bias in favor of revenues, $\alpha \in (\frac{1}{2}, 1]$, the government is strictly better off by not using the shaming penalty.[50]

C.8 Optimal Penalties under Heterogeneity in q_i

The following proposition ranks the policies when q_i is heterogeneous:

Proposition 2. *If $\overline{q} > \underline{q}$, there is a unique threshold $\alpha^* \in \left(\frac{1}{2}, 1\right)$ such as:*

- if $\alpha \leq \alpha^*$, then the set of policies $\{(F,p):\ F \geq 0,\ p \in (0,1]\}$ dominates $\{(F,p):\ F \geq 0,\ p = 0\}$.

- if $\alpha > \alpha^*$, then the set of policies $\{(F,p):\ F \geq 0,\ p = 0\}$ dominates $\{(F,p):\ F \geq 0,\ p \in (0,1]\}$.

[50] It must be noted, however, that if the financial policy was restricted (e.g., because of political constrains or because of laws), then a $p > 0$ could be optimal even if $\alpha > \frac{1}{2}$.

Proof. See section C.9.2.

To see the intuition behind this result, it is easier to start with the case $\alpha = \frac{1}{2}$. As before, the government simply wants the group with $R < R_g$ to pay right away and the group with $R > R_g$ to pay in the second period. However, this is not attainable any more by using just the financial penalty. Intuitively, if the government is only using a financial penalty, there will be two different thresholds, $\hat{R}_{\underline{q}}$ and $\hat{R}_{\overline{q}}$, for individuals with high and low income garnishability. This is because individuals with different garnishability expect to escape the financial penalty with different probabilities and thus face different probabilities of actually having to pay thefinancial penalties. As a result, the government cannot attain the first best.

Consider for example setting the financial penalty such as $\hat{R}_{\underline{q}} > R_g > \hat{R}_{\overline{q}}$. The government would prefer that the individuals with $R_i \in \left(R_g, \hat{R}_{\overline{q}}\right)$ pay in the second period, but those individuals are paying in the first period instead. The government would also prefer that the individuals with $R_i \in \left(\hat{R}_{\underline{q}}, R_g\right)$ pay in the first period, but those individuals are paying in the second period instead. By decreasing or increasing F, the government cannot solve one of the problems without aggravating the other one. Instead, were the government using the shaming penalty alone, there would be a single threshold and the government could attain the first best solution $\hat{R} = R_g$ (in practice, the value of social interactions is bounded so the optimal policy would still involve a combination of financial and shaming penalties).

The shaming penalty is still inferior in terms of producing tax revenues, as in the case of homogeneous q_i. Thus, when $\alpha > \frac{1}{2}$ the government faces a trade-off between the advantages and disadvantages of the shaming penalty. As a result, the optimality of the shaming penalty depends on the value of α: the shaming penalty will be desirable if and only if α is low enough, that is, if the government cares enough about private welfare relative to tax revenues.

The comparative advantage of the shaming penalty is that its effectiveness does not depend on income garnishability (q_i). This property would be violated if the value of social interactions (η_i) were heterogeneous and correlated with income garnishability (q_i). We believe that it is plausible that income garnishability is more related to the ability to escape the tax burden than to the value social interaction. Even assuming that garnishability were related to social interactions, this would not necessarily weaken our result. On the one hand, if the effectiveness of the shaming penalty were increasing in income garnishability (i.e., η_i positively correlated to q_i), that would diminish the comparative advantage of the shaming penalty relative to the financial penalty, possibly even to the point of making the shaming penalty no longer optimal. On the other hand, if the effectiveness of the shaming penalty were decreasing in income garnishability (i.e., η_i negatively correlated to q_i), the comparative advantage of the shaming penalty would be even greater. The latter case is arguably more likely than the former. Consider the case of self-employed professions such as lawyers or doctors, with arguably lower

income garnishability. Relative to wage earners, the self-employed may value their social image more because this social image is important for building relationships with clients and suppliers.

The above discussion implies that our main result would change only if higher garnishability were to reduce the effectiveness of the shaming penalties at a higher rate than reducing the effectiveness of the financial penalties. Consistent with our model, our conversations with professionals in this sector reveal that the ability to comparatively better target individuals with lower garnishability is perceived as a key advantage of the shaming penalty over the financial penalty. For example, in a press release from November 3, 2009, the Illinois Department of Revenue declared that: "The threat of disclosure and the negative publicity of being included in this list are particularly effective with self-employed professionals and cash businesses where some routine collection tools, such as the ability to garnish wages, may not work." The field experiment presented below tries to provide some evidence about the relationship between the effectiveness of penalties and income garnishability.

C.9 Proofs of Propositions

C.9.1 Proof of Proposition 1

Let's start with the case $\alpha = \frac{1}{2}$. The objective function of the government can be written as a function of the threshold $\hat{R}$:

$$\frac{1}{2}\frac{\left(\hat{R}-\underline{R}\right)}{\overline{R}-\underline{R}}\cdot\left(R_g-\frac{\underline{R}+\hat{R}}{2}\right)$$

Given $\{F,p\}$ if we find a $\{F',p'\}$ such as the same threshold arises in equilibrium, then the value of the objective function of the government will be the same. When $\{F,\ p>0\}$, we can use the alternative policy $\left\{F'=F+\frac{p\cdot\eta}{q}\frac{\overline{R}-\underline{R}}{2},\ p'=0\right\}$. And when $\{F,\ p=0\}$, we can use the alternative policy $\left\{F'=F-\frac{p'}{q}\cdot\eta\cdot\frac{\overline{R}-\underline{R}}{2},\ p'\right\}$. Thus, the set of policies with and without shaming penalties are interchangeable.

When $\alpha > \frac{1}{2}$, the government's objective function is:

$$\frac{\hat{R}-\underline{R}}{\overline{R}-\underline{R}}\cdot\left(\alpha R_g-(1-\alpha)\frac{\underline{R}+\hat{R}}{2}\right)+\frac{\overline{R}-\hat{R}}{\overline{R}-\underline{R}}\cdot q\cdot F\cdot(2\alpha-1)$$

Given $\{F,p>0\}$, consider the alternative policy $\left\{F'=F+\frac{p\cdot\eta}{q}\frac{\overline{R}-\underline{R}}{2},\ p'=0\right\}$ that attains the same $\hat{R}$ but reduces p to zero. The first term of the objective function will be the same. The second term, provided $\hat{R}<\overline{R}$, will be even higher because the F increases. Thus, the utility of the government under $\{F',p'=0\}$ is higher than under $\{F,p>0\}$. The other possible case is if $\{F,p>0\}$ was such as we are in the corner solution $\hat{R}=\overline{R}$. In that case, the second term would

always be zero and thus the utility of the government would be the same under $\{F, p > 0\}$ and $\{F', p' = 0\}$. However, given Assumption 1, it follows that since the candidate $\{F', p' = 0\}$ is a corner solution it cannot be optimal, and thus there must be at least another $\{F'', p'' = 0\}$ that attains strictly more utility than $\{F', p' = 0\}$. By transitivity, this $\{F'', p'' = 0\}$ must attain strictly more utility than the original $\{F, p > 0\}$. That is, we proved that even when $\{F, p > 0\}$ is a corner solution there is an alternative $\{F'', p'' = 0\}$ that attains strictly higher utility. This completes the proof that the set of policies with $p = 0$ dominates the set of policies with $p > 0$.

C.9.2 Proof of Proposition 2

The proof is organized as follows. First, we prove that using the shaming penalty makes the government strictly better off if $\alpha = \frac{1}{2}$. Second, we prove that using the shaming penalty makes the government strictly worse off if $\alpha = 1$. Third, we will prove that these two results imply that there must be is a unique $\alpha^* \in \left(\frac{1}{2}, 1\right)$ such as the set of policies with $p > 0$ dominates $p = 0$ if $\alpha < \alpha^*$, the two are interchangable if $\alpha = \alpha^*$, and the set of policies with $p = 0$ dominates $p > 0$ if $\alpha > \alpha^*$.

First, consider the case $\alpha = \frac{1}{2}$. The government's objective function can be written as:

$$\frac{1}{2}\frac{1}{\overline{R} - \underline{R}} \cdot \left(-\frac{1}{2}\left(\theta \hat{R}_{\overline{q}}^2 + (1-\theta)\,\hat{R}_{\underline{q}}^2\right) + R_g\left(\theta \hat{R}_{\overline{q}} + (1-\theta)\,\hat{R}_{\underline{q}}\right) + \frac{\underline{R}^2}{2} - \underline{R} \cdot R_g \right)$$

Given a policy $\{F,\ p = 0\}$, consider the alternative policy $\left\{F' = F - \frac{\epsilon}{\theta\overline{q} + (1-\theta)\underline{q}} \cdot \eta \cdot \frac{\overline{R} - \underline{R}}{2},\ p' = \epsilon\right\}$ with ϵ positive but arbitrarily close to zero. If $\hat{R}_{\overline{q}}$ and $\hat{R}_{\underline{q}}$ were originally not a corner solution, this transformation leaves $\left(\theta \hat{R}_{\overline{q}} + (1-\theta)\,\hat{R}_{\underline{q}}\right)$ unchanged while reducing the gap between $\hat{R}_{\overline{q}}$ and $\hat{R}_{\underline{q}}$ (and, additionally, reduces F). As a result, the only term of the objective function that changes is $-\frac{1}{2}\left(\theta \hat{R}_{\overline{q}}^2 + (1-\theta)\,\hat{R}_{\underline{q}}^2\right)$. Given that the gap between $\hat{R}_{\overline{q}}$ and $\hat{R}_{\underline{q}}$ is reduced, by Jensen's inequality we know that the average $\theta \hat{R}_{\overline{q}}^2 + (1-\theta)\,\hat{R}_{\underline{q}}^2$ must decrease, so that the entire term $-\frac{1}{2}\left(\theta \hat{R}_{\overline{q}}^2 + (1-\theta)\,\hat{R}_{\underline{q}}^2\right)$ increases. That is, the new policy makes the government strictly better off. If, on the other hand, $\hat{R}_{\overline{q}}$ and $\hat{R}_{\underline{q}}$ were originally not a corner solution, due to Assumption 1 that implies that there must be another policy $\{F'',\ p'' = 0\}$ that is not a corner solution and it is strictly better than $\{F,\ p = 0\}$. Using the above method, it follows that we can find a $\{F',\ p' = 0\}$ that is strictly better than $\{F,\ p = 0\}$, completing the proof that the government is better off by using the shaming penalty than by not using it.

Second, consider the case $\alpha = 1$. The government's objective function can be written as:

$$\frac{1}{\overline{R} - \underline{R}}\left[\left(\theta \hat{R}_{\overline{q}} + (1-\theta)\,\hat{R}_{\underline{q}}\right) R_g - R_g \cdot \underline{R} + \left(\overline{R} - \left(\theta \hat{R}_{\overline{q}} + (1-\theta)\,\hat{R}_{\underline{q}}\right)\right) \cdot \left(\theta\overline{q} + (1-\theta)\,\underline{q}\right) \cdot F\right]$$

We can show that the optimal cannot involve $p > 0$. Take any candidate $\{F, p > 0\}$. Consider the alternative $\left\{F' = F + \frac{p \cdot \eta}{\theta\overline{q} + (1-\theta)\underline{q}} \cdot \frac{\overline{R} - \underline{R}}{2},\ p' = 0\right\}$. There are number of possible cases. The first case is that $\hat{R}_{\overline{q}}$ and $\hat{R}_{\underline{q}}$ were originally not a corner solution and still are not a corner solution under the alternative policy. In this case, the transformation leaves $\theta\hat{R}_{\overline{q}} + (1-\theta)\,\hat{R}_{\underline{q}}$ unchanged while increasing F (and, additionally, increases the gap between $\hat{R}_{\overline{q}}$ and $\hat{R}_{\underline{q}}$). Note that, since we are not in a corner solution: $\overline{R} - \left(\theta\hat{R}_{\overline{q}} + (1-\theta)\,\hat{R}_{\underline{q}}\right) > 0$. Thus, since $F' > F$ then the last term of the objective function is higher under $\{F',\ p' = 0\}$, meaning that the government is better off by not using the shaming penalty. A second case is that $\hat{R}_{\overline{q}}$ and $\hat{R}_{\underline{q}}$ were both a corner solution. In that case, the alternative $\left\{F' = F + \frac{p \cdot \eta}{\theta\overline{q} + (1-\theta)\underline{q}} \cdot \frac{\overline{R} - \underline{R}}{2},\ p' = 0\right\}$ must involve $\hat{R}_{\overline{q}}$ and $\hat{R}_{\underline{q}}$ both as corner solutions as well. This transformation still leaves $\theta\hat{R}_{\overline{q}} + (1-\theta)\,\hat{R}_{\underline{q}}$ unchanged while increasing F, so that it is still true that the government is better off by not using the shaming penalty. Using the same logic, it is straightforward to prove that the government is better off by not using the shaming penalty in the remaining cases.[51]

Third, we will prove that these two results imply that there must be is a unique $\alpha^* \in \left(\frac{1}{2}, 1\right)$ such as the set of policies with $p > 0$ dominates $p = 0$ if $\alpha < \alpha^*$, the two are interchangeable if $\alpha = \alpha^*$, and the set of policies with $p = 0$ dominates $p > 0$ if $\alpha > \alpha^*$. The trick for this step is that the objective function of any intermediate case, $\alpha \in \left(\frac{1}{2}, 1\right)$, can be written as a weighted average between the objective functions evaluated at $\alpha = \frac{1}{2}$ and $\alpha = 1$. Let β $(1-\beta$) be weight on the objective function with $\alpha = 1$ ($\alpha = \frac{1}{2}$), with $\beta \in [0, 1]$ and with $\beta = 0$ and $\beta = 1$ corresponding to the extreme cases $\alpha = \frac{1}{2}$ and $\alpha = 1$. Given a policy $\{F,\ p > 0\}$, the alternative policy $\left\{F' = F + \frac{p \cdot \eta}{\theta\overline{q} + (1-\theta)\underline{q}} \cdot \frac{\overline{R} - \underline{R}}{2},\ p' = 0\right\}$ leaves $\left(\theta\hat{R}_{\overline{q}} + (1-\theta)\,\hat{R}_{\underline{q}}\right)$ unchanged while increasing F and the gap between $\hat{R}_{\overline{q}}$ and $\hat{R}_{\underline{q}}$. In the previous step we showed that this policy increases the objective function when $\alpha = 1$ but decreases the objective function when $\alpha = \frac{1}{2}$. By the mean value theorem, there must be a critical and unique $\alpha^* \in \left(\frac{1}{2}, 1\right)$ such as the objective function increases under the alternative policy (i.e., the shaming penalty makes the government worse off) if $\alpha > \alpha^*$ and leaves it the same if $\alpha = \alpha^*$. Finally, we can use the same logic to show that the shaming penalty makes the government better off if $\alpha < \alpha^*$.

[51] For instance, when the original $\hat{R}_{\overline{q}}$ and $\hat{R}_{\underline{q}}$ were not corner solutions but at least one would be a corner solution under the proposed alternative, what we can do is to propose a different alternative with $p = 0$ but where F increases such as $\theta\hat{R}_{\overline{q}} + (1-\theta)\,\hat{R}_{\underline{q}}$ remains the same even though now $\hat{R}_{\overline{q}}$ and/or $\hat{R}_{\underline{q}}$ is a corner solution.

D Testing Predictions of the Model

The previous Appendix provided a positive theory of why tax agencies are using shaming penalties. This model suggests that shaming penalties may be desirable if they are relatively more effective on individuals who are less vulnerable to the financial penalties. In this subsection we test this hypothesis by measuring how the effects of shaming and financial penalties vary with the debtor's degree of income garnishability.

As discussed in subsection 2.2, the variable *Share of Wage Income* is the fraction of gross income from wages in the 5-digit ZIP code. We use this variable as a proxy for higher income garnishability, because wages are one of the sources of income that are easiest to garnish (as opposed, for example, to business income). This variable was normalized to have a mean of zero and a standard deviation of one within each of the three states.[52]

Results are shown in Table D.1. Column (1) presents the baseline results. The dummy on shaming penalty is normalized as in subsection 3.3. The financial penalty increases the probability of leaving the list four months after the treatment by 1.15 percentage points, whereas the shaming penalty increases this probability by 1.1 percentage points (again, for an individual with a $1,000 initial debt). Column (2) adds the variable *Share of Wage Income* along with its interaction with the shaming and financial penalties. The effect of financial penalties is stronger in places characterized by a higher fraction of wage income, which is consistent with the view that individuals in those places are more vulnerable to financial penalties. This heterogeneity is significant in magnitude: the coefficients imply that a one standard deviation increase in *Share of Wage Income* doubles the effectiveness of the financial penalty. On the contrary, the coefficient on the interaction between the shaming penalty and *Share of Wage Income* is close to zero and statistically insignificant. This result suggests that, relative to financial penalties, shaming penalties are more effective on individuals with lower wage garnishability. This evidence is consistent with the model, and also consistent with the beliefs reported by tax agencies: e.g., the Illinois Department of Revenue, in a press release from November 3, 2009 declared that "The threat of disclosure and the negative publicity of being included in this list are particularly effective with self-employed professionals and cash businesses where some routine collection tools, such as the ability to garnish wages, may not work."

Of course, it should be noted that the heterogeneity with respect to *Share of Wage Income* could be due to heterogeneity with respect to unobservable place characteristics correlated with the this variable (e.g., income, tax sophistication, tax morale). We alleviate those concerns, at least partially, by assessing how sensitive the results are when we control for the interactions with other place characteristics. Columns (3), (4) and (5) show results including the interactions

[52]The goal of normalizing within each state is that the variable does not pick up cross-state heterogeneity in the effectiveness of the penalty. In practice, the results are similar if applying the same normalization over the entire population instead of within each state.

with some of the variables used in the descriptive analysis of subsection 2.2: *Share of Wage Income, EITC Bunching* and *Share Republican*, respectively.[53] In turn, column (6) controls for the interactions with all three variables simultaneously. The interactions of the shaming and financial penalties with the *Share of Wage Income* are very robust under all of these specifications, both in terms of magnitude and statistical significance.

[53]On the link between politics and tax enforcement see for example Casaburi and Troiano (2014).

Table D.1: Evidence about the Interaction between Income Garnishability and Treatments Effects

	Probability of Leaving the List, 16 weeks After Treatment					
	(1)	(2)	(3)	(4)	(5)	(6)
Higher Visibility	1.095**	1.127**	1.150**	1.150**	1.115**	1.154**
	(0.527)	(0.530)	(0.527)	(0.532)	(0.528)	(0.527)
Financial Reminder	1.146**	1.111**	1.130**	1.052**	1.091**	1.069**
	(0.486)	(0.474)	(0.475)	(0.474)	(0.475)	(0.475)
Share of Wage Income		-1.575***	-1.080*	-1.028**	-1.482***	-0.683
		(0.480)	(0.607)	(0.490)	(0.474)	(0.610)
Interaction with Higher Visibility		0.035	0.193	-0.146	0.004	0.042
		(0.539)	(0.657)	(0.526)	(0.533)	(0.635)
Interaction with Financial Reminder		1.205***	1.055*	1.207**	1.188***	1.061*
		(0.456)	(0.567)	(0.469)	(0.458)	(0.570)
Extra Controls (with interactions)	None	None	Mean Income	EITC Bunching	Share Republican	All

Notes: N=34,334. * significant at the 10% level, ** at the 5% level, *** at the 1% level. Standard errors in parentheses, clustered at the 5-digit ZIP code level. The coefficients were estimated from OLS regressions (one per column) where the dependent variable is a dummy for whether the subject is listed as a *delinquent* 16 weeks after the letters were delivered and the right hand side variables are the treatment dummies plus a set of control variables (e.g., gender, state, inverse of the initial debt amount). *Higher Visibility* is a dummy that takes the value 0 if the recipient was the only one in the area chosen to receive a letter and 1 if others in the area were chosen to receive a letter too, and then it is divided by the initial debt amount. *Financial Reminder* is a dummy that takes the value 1 if the letter included information about the financial penalties and 0 if not. *Share of Wage Income* is the share of gross income from wages in the 5-digit ZIP code, as reported by the Internal Revenue Service Statistics of Income (IRS-SOI) database for 2012. This variable was normalized to have mean zero and standard deviation 1 within each of the three states. The extra controls correspond to other ZIP code level variables, including the interaction with the two treatment variables. Mean Income corresponds to the average gross income in 2012 at the 5-digit ZIP code, also from IRS-SOI. EITC Bunching corresponds to the share of self-employed individuals in the 3-digit ZIP code estimated to be mis-reporting income to take advantage of EITC benefits (data source: Chetty et al., (2013)). Share republican is the county-level share of votes for the Republican candidate in the 2012 U.S. Presidential Election. The last columns includes these three control variables (plus the interactions). All these control variables were normalized to have mean zero and standard deviation 1 within each of the three states.

E Extensions to the Model

E.1 Informing Delinquents about Financial Penalties

There is a rapidly growing body of evidence about a systematic under-estimation of financial penalties in a variety of settings, such as consumer loan market (Stango and Zinman (2011)) and credit card debt (Ausubel (1991); Frank (2011)). Indeed, our experimental design exploits this tendency to identify the effects from financial penalties, by introducing an intervention that increases the salience of financial penalties and (possibly) corrects systematic biases. In this extension of our model we study whether a general policy consisting of correcting misperceptions about the interest rate would be desirable from the perspective of the tax agency.

E.1.1 The Debtor's Problem

We focus on the case of homogeneous garnishability: $q_i = q \; \forall i$. In reality, some individuals may under-estimate the financial penalties while some others may over-estimate it, but the evidence suggests that, on average, individuals under-estimate. For the sake of simplicity, we assume that a fraction s of individuals incorrectly perceive that the financial penalty are lower than it actually is, $F = \underline{F} < F$, while the remaining $(1-s)$ correctly perceive the financial penalty F. Let membership in s be independent of R_i. The optimal response to the perceived penalty is characterized by one threshold for individuals who correctly perceive the financial penalty and a different threshold for individuals who incorrectly perceive the financial penalty. Peers are correctly informed about the fraction s, and both groups of debtors correctly perceive peers expectations about R_i for those who do and do not repay their debts.

Debtors who correctly perceive the financial penalty maximize:

$$\begin{aligned} U(x_i; R_i) = &- R_i \cdot x_i - (1 - x_i) \cdot [q \cdot F + p \cdot \eta \cdot (E[R_i | x_i = 0] - E[R_i | x_i = 1])] \\ &+ \eta \cdot (1 - p) \cdot \frac{\underline{R} + \overline{R}}{2} \end{aligned}$$

Debtors who misperceive the financial penalty maximize:

$$\begin{aligned} U(x_i; R_i) = &- R_i \cdot x_i - (1 - x_i) \cdot [q \cdot \underline{F} + p \cdot \eta \cdot (E[R_i | x_i = 0] - E[R_i | x_i = 1])] \\ &+ \eta \cdot (1 - p) \cdot \frac{\underline{R} + \overline{R}}{2} \end{aligned}$$

Define the cutoff $\tilde{R}$ for debtors who misperceive the financial penalty and the cutoff $\hat{R}$ for debtors who correctly perceive the financial penalty. The cutoff rules are then:

$$x^*(R) = 1[R \leq \hat{R}]; \; \tilde{x}(R) = 1[R \leq \tilde{R}]$$

It is easy to show that, as in the model without misperception, $\hat{R} = q \cdot F + p \cdot \eta \cdot \frac{\overline{R}-\underline{R}}{2}$ defines the cutoff below which the debtors who correctly infer the penalties repay their debt, and $\tilde{R} = q \cdot \underline{F} + p \cdot \eta \cdot \frac{\overline{R}-\underline{R}}{2}$ defines the cutoff for the biased debtors. Note that since $\underline{F} \leq F$, $\tilde{R} \leq \hat{R}$, and types in between the two thresholds would have higher utility if they repaid their debt, but do not repay their debt due to their misperception of the financial consequences.

E.1.2 The Government's Problem

In addition to the financial and shaming penalties, the government can decrease the fraction s of debtors who misperceive the financial penalty of failing to repay at no resource cost: i.e., it chooses $s^* \in [0, s]$. The government collects greater revenues in the second period by not correcting the misperception, as the misperception leads to more failure to repay in the first period. However, the debtors who misperceive the penalty lose actual (as opposed to perceived) utility by behaving incorrectly.

The government revenues and private welfare are now:

$$T(F, p, s^*) = \int [s^* \cdot \tilde{x}(R) \cdot R_g + (1 - s^*) \cdot x^*(R) \cdot R_g + s^* \cdot (1 - \tilde{x}(R)) \cdot q \cdot F + (1 - s^*) \cdot (1 - x^*(R)) \cdot q \cdot F] dF(R)$$

$$PW(F, p, s^*) = -\int [s^* \cdot \tilde{x}(R) \cdot R + (1 - s^*) \cdot x^*(R) \cdot R + s^* \cdot (1 - \tilde{x}(R)) \cdot q \cdot F + (1 - s^*) \cdot (1 - x^*(R)) \cdot q \cdot F] dF(R)$$

The government solves:

$$\max_{F \geq 1,\ p \in [0,1],\ s^* \in [0,s]} \alpha \cdot T(F, p, s^*) + (1 - \alpha) \cdot PWD(F, p, s^*)$$

The following proposition ranks the policies when the government can also decide whether to correct debors' misperceptions about the financial penalties:

Proposition 3. *There is a threshold $\alpha^* \in \left(\frac{1}{2}, 1\right)$ such as:*

- if $\alpha \leq \alpha^*$, then the set of policies $\{(F, p, s) : F \geq 0,\ p \in [0, 1], s^* = 0\}$ dominates $\{(F, p, s) : F \geq 0,\ p \in [0, 1], s \in (0, 1]\}$.

- if $\alpha > \alpha^*$, then the set of policies $\{(F, p, s) : F \geq 0,\ p \in [0, 1], s \in (0, 1]\}$ dominates $\{(F, p, s) : F \geq 0,\ p \in [0, 1], s = 0\}$.

Proof. In the case where $\alpha = 1/2$, the government's objective function is now

$$\frac{1}{2}\int x^*(R)(R_g - R)dF(R) - s^* \cdot \frac{1}{2}\int [x^*(R) - \tilde{x}(R)](R_g - R)dF(R)$$

The government wants to collect if and only if $R_g \geq R_i$, which it can accomplish by setting $x^*(R)$ to the appropriate threshold. The second term indicates that the fraction s^* of debtors behave in a way that costs the government utility conditional on it setting the threshold correctly, since $x^*(R) - \tilde{x}(R)$ is either zero or one, and is only one in a range when $R_g - R_i$ is positive (as the debtors who do not pay are people who would have paid had they correctly perceived the financial penalty). Thus the government's first-best behavior sets $s^* = 0$ - it completely corrects the misperception.

The second term is:

$$-\frac{s^*}{2}\frac{\hat{R} - \tilde{R}}{\overline{R} - \underline{R}}\left[R_g - \frac{\hat{R} + \tilde{R}}{2}\right]$$

Note that $\hat{R} \geq \tilde{R}$ from above. Then setting $s^* > 0$ is not optimal so long as the average of $\hat{R}$ and $\tilde{R}$ is not greater than R_g, which is true so long as the government has not set F and p too much higher than optimal.

Since setting $s^* > 0$ strictly leads debtors to behave in a way that does not maximize their utility, if the government does not choose $s^* > 0$ when $\alpha = 1/2$, the government will not choose $s^* > 0$ for any $\alpha < 1/2$, as lowering α only increases the weight the government puts on debtor's welfare.

$$\begin{aligned} U(F, p, s^*) = &\int x^*(R)[\alpha R_g - (1-\alpha)R] + (1 - x^*(R)) \cdot (2\alpha - 1) \cdot q \cdot F dF(R) \\ &+ s^* \int [x^*(R) - \tilde{x}(R)] \cdot [(2\alpha - 1) \cdot q \cdot F - \alpha \cdot R_g + (1-\alpha) \cdot R] dF(R) \end{aligned}$$

Consider now the case where $\alpha = 1$, and the government maximizes total revenues, which are

$$\begin{aligned} &\int x^*(R) \cdot R_g + (1 - x^*(R)) \cdot q \cdot F dF(R) + s^* \cdot (q \cdot F - R_g) \int [x^*(R) - \tilde{x}(R)] dF(R) \\ &= \frac{1}{\overline{R} - \underline{R}}\Big[R_g \cdot (\hat{R} - \underline{R}) + q \cdot F \cdot (\overline{R} - \hat{R}) + s^* \cdot (q \cdot F - R_g) \cdot (\hat{R} - \tilde{R})\Big] \end{aligned}$$

As one could see from the previous equation, the elasticity of the government's objective function with respect to the tax debtors misinformation depends on the sign of $(qF - R_g)$, which is endogenous. However, we can make the problem simpler by considering what happens from a small deviation from the optimal policy without tax debtors misinformation. Because

there is no heterogeneity in q, any optimal policy will have $p^* = 0$, following the results of the previous propositions. For simplicity, let's start from the policy $\{F, p, s) : F \geq 0, p = 0, s = 0\}$ and show that this policy is dominated by a policy with $\{F, p, s) : F \geq 0, p = 0, s = \varepsilon\}$. Given this assumptions, the objective function of the government simplifies to:

$$\frac{1}{\overline{R} - \underline{R}}\Big[R_g \cdot (\hat{R} - \underline{R}) + q \cdot F \cdot (\overline{R} - \hat{R})\Big]$$

which is maximized when $F^* = \frac{R_g + \overline{R}}{2q}$. This trivially implies that moving to a situation where setting $\{F, p, s) : F \geq 0, p = 0, s = \epsilon\}$ increases the objective function of the government, because $\hat{R} > \tilde{R}$, and $q \cdot F^* > R_g$.

Last, as in the proofs to the previous propositions, we can use the same argument with the mean value theorem to prove that there must be a critical and unique $\alpha^* \in \left(\frac{1}{2}, 1\right)$ such as the objective function increases with $s > 0$ if $\alpha < \alpha^*$ and decreases with $s > 0$ if $\alpha > \alpha^*$.

The main intuition is the following. When the government cares about both welfare of tax debtors and tax revenues, the first-best can be achieved by correcting the misperceptions of everyone and setting the first-best policy. This would be true also when the government cares more about the welfare of tax debtors than raising tax revenues. However, if the government cares comparatively more about raising tax revenues, it is optimal not to correct the debtors who underestimate the financial penalties, letting them act as if the financial penalty is low and surprising them with high penalties in the second period.

E.2 Signaling Moral Type

This baseline model of social interactions assumes that peers care about financial trustworthiness. In this section, we provide an extension of the model that shows that the main results are robust if, instead, peers care about moral trustworthiness.

E.2.1 The Debtor's Problem

Suppose that all debtors have $q_i = q$, and debtors have types $\{R_i, m_i\}$, where $m_i | R_i \sim U[\underline{m}, \overline{m}]$. The new type m_i is the moral cost the debtor bears if she has unpaid debt. Peers do not care directly about the credit-constraint measure R_i, and observe R_i, while the government does not observe R_i. Neither peers nor the government observe m_i. The type m_i is correlated with likelihood that a debtor will repay social favors, so peers wish to extend more social favors to higher-m types. Debtors then receive expected social utility equal to:

$$\eta\left[pE[m_i | R_i, x_i] + (1 - p) \cdot \frac{\underline{m} + \overline{m}}{2}\right]$$

Where $\eta > 0$ is the relative value of social favors. Note that low-m types are now punished and high-m types are now rewarded; previously low-R types were rewarded and high-R types were punished (hence the absence of the minus sign in front of η).

Debtors' utility functions are:

$$U(x_i; R_i, m_i) = -R_i \cdot x_i - (1-x_i) \cdot [q \cdot F + m_i - p \cdot \eta \cdot (E[m_i|R_i, x_i = 0] - E[m_i|R_i, x_i = 1])] + \eta \cdot (1-p) \cdot \frac{\underline{m} + \overline{m}}{2}$$

The debtor's optimal response $x^*(R_i, m_i) = \arg\max_{x \in \{0,1\}} U(x_i; R_i, m_i)$ is characterized by a threshold for each R_i, $\hat{m}(R_i)$:

$$x^*(R_i, m_i) = 1[m_i \geq \hat{m}(R_i)]$$

Peers rationally infer that:

$$E[m_i|R_i, x_i = 1] = \frac{\hat{m}(R_i) + \overline{m}}{2} \text{ and } E[m_i|R_i, x_i = 0] = \frac{\underline{m} + \hat{m}(R_i)}{2}$$

Substituting into the objective function:

$$U(x_i; R_i, m_i) = -R_i \cdot x_i - (1 - x_i) \cdot \left[q \cdot F + m_i + p \cdot \eta \cdot \frac{\overline{m} - \underline{m}}{2} \right] + \eta \cdot (1-p) \cdot \frac{\underline{m} + \overline{m}}{2}$$

Each debtor then chooses $x_i = 1$ when

$$m_i \geq -q \cdot F + R_i - p \cdot \eta \cdot \frac{\overline{m} - \underline{m}}{2}$$

This confirms our guess that the optimal response is characterized by the thresholds:

$$\hat{m}(R_i) = \min \left\{ \overline{m}, \max \left\{ -q \cdot F + R_i - p \cdot \eta \cdot \frac{\overline{m} - \underline{m}}{2}, \underline{m} \right\} \right\}$$

As expected, the proportion of debtors paying in the first period is increasing in the financial penalty F and the shaming penalty p, and for a given moral cost debtors pay in the first period provided R_i is low enough.

E.2.2 The Government's Problem

Government revenues and private welfare of the taxpayers are:

$$T(F, p) = \int \int [x^*(R, m) \cdot R_g + (1 - x^*(R, m)) \cdot q \cdot F] dF(m|R) dF(R)$$

$$PWD(F,p) = -\int\int [x^*(R,m)\cdot R + (1-x^*(R,m))\cdot q\cdot F]dF(m|R)dF(R)$$

Which uses the fact that the aggregate utility from social interactions is fixed. The government again maximizes a weighted sum of tax revenue and the private welfare of debtors:

$$\max_{F\geq 1, p\in[0,1]} \alpha T(F,p) + (1-\alpha)PWD(F,p)$$

E.2.3 Optimal Penalties under Homogeneous q_i

The following is parallel to Proposition 1:

Proposition 4. *In the case that $\alpha = 1/2$ ($\alpha > 1/2$), for any policy $\{F,p\}$ there exists an alternative policy $\{F',p'\}$ with $p'=0$ that attains the same (or higher) utility for the government.*

Proof. In the case where $\alpha = 1/2$, the government's objective function becomes

$$\frac{1}{2}\int [R_g - R]\frac{\overline{m}-\hat{m}(R)}{\overline{m}-\underline{m}}dF(R)$$

Then for any $\{F,p\}$, the alternative $\left\{F' = F + \frac{p\cdot\eta}{q}\frac{\overline{m}-\underline{m}}{2}, p'=0\right\}$ produces the same thresholds $\hat{m}(R_i)$ for all R_i and thus produces the same utility for the government.

For $\alpha > 1/2$ the government's objective function is:

$$\int \frac{\hat{m}(R)-\underline{m}}{\overline{m}-\underline{m}}[\alpha R_g - (1-\alpha)R] + \frac{\overline{m}-\hat{m}(R)}{\overline{m}-\underline{m}}(2\alpha-1)\cdot q\cdot F dF(R)$$

Again, for any $\{F,p\}$, the alternative $\left\{F' = F + \frac{p\cdot\eta}{q}\frac{\overline{m}-\underline{m}}{2}, p'=0\right\}$ produces the same thresholds $\hat{m}(R_i)$ for all R_i. The first term is the same under both policies, but the second term is larger under $\{F',p'\}$ since $F'\geq F$ and $2\alpha-1>0$. Thus the alternative policy produces at least as much utility for the government.

E.2.4 Optimal Penalties under Heterogeneous q_i

The following is parallel to Proposition 2:

Proposition 5. *If $\underline{q} < \overline{q}$, for some values of α, and some policies $\{F,p\}$, the government can obtain strictly greater utility by choosing $\{F' < F, p' > p\}$.*

Proof. Let $\alpha = 1/2$. The government's utility function is:

$$\frac{1}{2}\int [R_g - R]\frac{\overline{m} - \hat{m}(R,\overline{q}) - \theta \cdot [\hat{m}(R,\underline{q}) - \hat{m}(R,\overline{q})]}{\overline{m} - \underline{m}} dF(R)$$

The wedge introduced by the types' difference on ability to collect is:

$$\frac{-\theta}{2(\overline{m} - \underline{m})}\int [R_g - R]\Big[\hat{m}(R,\underline{q}) - \hat{m}(R,\overline{q})\Big] dF(R)$$

Note that:

$$\begin{aligned}\hat{m}(R,\underline{q}) - \hat{m}(R,\overline{q}) = &\min\left\{\overline{m}, \max\left\{ -\underline{q}\cdot F + R_i - p\cdot\eta\cdot\frac{\overline{m}-\underline{m}}{2}, \underline{m}\right\}\right\} \\ &- \min\left\{\overline{m}, \max\left\{ -\overline{q}\cdot F + R_i - p\cdot\eta\cdot\frac{\overline{m}-\underline{m}}{2}, \underline{m}\right\}\right\} \\ &\geq 0\end{aligned}$$

If, for example, the range of m_i is sufficiently spread out that the boundaries of the $\hat{m}(\cdot)$ function do not bind, then this term is simply $F \cdot (\overline{q} - \underline{q})$. In general, the wedge will be proportional to F and $\overline{q} - \underline{q}$. Assuming that $\int [R_g - R] dF(R) \geq 0$ (which is a sufficient condition for the government wishing to raise positive revenue in the first period), then the wedge will be weakly positive. The wedge is strictly positive when both $\int [R_g - R] dF(R) > 0$ and the range of m_i is large enough relative to the policy choices $\{F, p\}$ that some types choose to pay while others do not. Using F as a policy instrument incurs this wedge, while using p as a policy instrument does not, so the government will prefer p as its first-choice policy instrument and only use F when it has set p as large as possible. Suppose, for example, that $\{F > 0, p < 1\}$. Then setting $F = 0$, $p' = p + \overline{q} \cdot F\frac{2}{\eta\cdot(\overline{m}-\underline{m})}$ maintains the same $\hat{m}(R,\overline{q})$ for all R (provided that $p' \leq 1$). This policy change also removes the wedge, and thus generates strictly greater utility for the government whenever the wedge is greater than zero.

E.3 Heterogeneous Debt Amounts

This baseline model assumed that individuals were homogeneous with respect to the debt amount. This assumption was made to simplify the notation, and does not change the main intuitions and results. In this subsection, we relax this assumption to examine whether the effectiveness of the shaming penalty should depend on the debt amount.

Let d_i denote the debt amount. The simplest case is when debt amounts are common knowledge, and each taxpayer can choose whether to pay the full amount in the first period ($x_i = 1$) or pay nothing in the first period ($x_i = 0$). The utility of the taxpayer is given by:

$$-R_i \cdot d_i \cdot x_i - (1 - x_i) \cdot [q_i \cdot F \cdot d_i + p \cdot \eta \cdot (E[R_i | x_i = 0] - E[R_i | x_i = 1])] + \eta \cdot (1 - p) \cdot \frac{\underline{R} + \overline{R}}{2}$$

Note that the key assumption is that peers only care about the individual's financial health (R_i), in which case the value of the non-market good, η, does not vary with the debt amount. Solving for the debtor's problem as before, debtor i pays in the first period if and only if R_i is below the following threshold:

$$\hat{R}(d_i, q_i) = q_i \cdot F + p \cdot \frac{\eta}{d_i} \cdot \frac{\overline{R} - \underline{R}}{2}$$

Note that the effect of the financial penalty on the threshold does not change with the debt amount: i.e., $\frac{\partial^2 \hat{R}(d)}{\partial F \partial d} = 0$. This property arises directly from the fact that the financial penalty is proportional to the debt amount. The social penalty, on the other hand, is decreasing in the debt amount: i.e., $\frac{\partial^2 \hat{R}(d)}{\partial p \partial d} = -\frac{\eta}{d^2} \cdot \frac{\overline{R} - \underline{R}}{2} < 0$. Intuitively, the cost of obtaining the non-market good (i.e., paying off the debt) is increasing in the debt amount but the benefit of obtaining the non-market good (η) is constant. As mentioned previously, this property arises directly from the assumption that peers only care about the individual's financial health (R_i). In a more general model, peers may care about some combination of R_i and d_i, in which case this property may or may not be maintained.