Journal of Public Economics 167 (2018) 120–137

Contents lists available at ScienceDirect

Journal of Public Economics

journal homepage: www.elsevier.com/locate/jpube

Shaming tax delinquents☆

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ARTICLE INFO

Article history:
Received 7 October 2016
Received in revised form 12 September 2018
Accepted 12 September 2018
Available online xxxx

JEL classification:
C93
H26
K34
K42
Z13

Keywords:
Tax debt
Enforcement
Financial
Shaming
Penalty

ABSTRACT

Many federal and local governments rely on shaming penalties to achieve policy goals, but little is known about how shaming works. Such penalties may be ineffective, or even 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 a total of 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 measured the effects of this information on subsequent payment rates. We found that increasing the visibility of delinquency status increased compliance by individuals who have debts below $2500, but had no significant effect on individuals with larger debt amounts. Financial reminders had a positive effect on payment rates independent of the size of the debt, while information about the delinquency of neighbors had no effect on payment rates.

1. Introduction

Many federal and local governments rely on shaming as a penalty to achieve policy goals (Kahan, 1996; Kahan and Posner, 1999). Yet little is known about whether shaming penalties work as intended. There are reasons to believe that shaming could be ineffective; for example, antisocial 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 (Bénabou and Tirole, 2003). In this paper, we study specific channels through which shaming penalties may affect behavior, using an 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 owed to tax agencies by citizens. Even though they have been understudied relative to other aspects of tax collection, such as tax evasion and tax avoidance, tax delinquencies play an important role in the tax collection process. For instance, in the United States, where debt collection tools are believed to be effective, delinquent taxes still comprised more than 25% of the total

☆ 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, Kamala Kanneganti and Felipe Montano Campos for outstanding research assistance. A previous version of the paper circulated under the title "Tax Debt Enforcement."

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https://doi.org/10.1016/j.jpubeco.2018.09.008
0047-2727/

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, by which the identities of tax debtors are publicly revealed. For instance, as of January 2015, 23 U.S. states maintained online shaming lists with the names, addresses, and other information on 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 on how they work or whether they have the intended effect.

Studying the effectiveness of financial and shaming penalties in the context of tax delinquency is challenging. For instance, the ideal experiment would randomly assign different penalties to a sample of taxpayers. Unlike the randomization of audit probabilities, however, randomizing financial and shaming penalties would imply punishing the same crime differently, which would likely be infeasible for legal and other reasons.[3] Instead, our research design consists of varying the salience of different incentives. We sent letters by mail to a sample of existing tax delinquents. These letters were identical except for a few pieces of information that were cross-randomized to vary the salience of financial incentives, the salience of shaming incentives, and the salience of 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 \$5500. 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] These subjects had been delinquent 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%.[6]

We sent letters to all individuals in our subject pool, but 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 offered to delinquents' neighbors. We randomized subjects into two treatment groups. In the first treatment, the delinquent was the only individual from the same area (defined as the 9-digit ZIP code) who was randomly chosen to be informed about the online list of delinquents. The second treatment was identical, except that other individuals from the same area were also randomly chosen to be informed about the online list of delinquents. The letter communicated the nature of the randomization explicitly and conspicuously—in other words, it was apparent to individuals in the first treatment group that their neighbors would not receive a letter, and apparent to individuals in the second treatment group that their neighbors would receive a letter. Compared to the first treatment group, the second treatment should make delinquents feel that they are being monitored more closely by their neighbors—and, if they are sensitive to social pressure, this should render them more likely to pay their debts.

It is important to note that in the letter, we refer generically to the other people who were being contacted as "neighbors." For practical, legal, and ethical considerations, we contacted neighbors who were themselves delinquent—and therefore on the shaming list—but did not contact any neighbors who were not. Although in reality we only contacted delinquent neighbors, the letter was worded to suggest that nondelinquent neighbors would be contacted. As a result, our estimates must be interpreted as such.

The second treatment arm was designed to create exogenous variation in the knowledge and salience of financial penalties. It has been documented in a variety of settings that subjects systematically underestimate financial penalties (Stango and Zinman, 2011; Frank, 2011; Ausubel, 1991) and are inattentive to financial penalties (Karlan et al., 2016). In the first treatment, the letter contained a message that summarized the financial penalties incurred by the debt. The second treatment group was identical, except that it did not include the message about financial penalties. If recipients cared about financial penalties but were inattentive to them, adding the financial reminder to the letter should increase the likelihood of paying the debt.

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 nondeceptive method, we created random variation in the amounts owed by the individuals listed in the letter. This allowed us to 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–\$2273), higher visibility increased the probability of repayment 10 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% of the average payment probability. Given that our visibility treatment was marginal, this effect size is remarkable. Among individuals in the other three quartiles (\$2274–\$149,738), the effect of the visibility treatment on the payment rate was close to zero and statistically insignificant.

[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.

[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 one of the three states that publishes the date when the delinquent debts were originated.

Table 1
States with online lists of tax delinquents (as of December 31, 2014).

State	Experiment	Start year	Current threshold	Type	Website	State Rank (1st to 50th) in...		
						Income	Social capital	Tax compliance
Kansas	Yes	2004	$2500	I, B	Link	24th	16th	10th
Kentucky	Yes	2007	$250	I, B	Link	47th	40th	37th
Wisconsin	Yes	2006	$5000	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 benefits (Chetty et al., 2013).

Our preferred interpretation for the heterogeneity by debt amount is that social incentives may be difficult to scale up. This interpretation is consistent with a simple social signaling model, according to which 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. Additionally, this interpretation is consistent with the prior beliefs of the tax agencies that implemented these shaming penalties. For example, the Vermont deputy tax commissioner stated during an interview: "When you are talking about large debts, you do tend to get some people who just don't care. It's just not worth paying 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] However, our preferred interpretation is by no means the only possible interpretation. Since the amount of tax debt was not randomized, it is possible that delinquents with larger debts are more sensitive to our shaming intervention because of unobservable characteristics correlated to debt size, such as liquidity constraints or concern for one's reputation.

Second, our evidence suggests that individuals respond to financial reminders: Over the same 10-week period, the financial reminders increased the probability of payment by 0.7 percentage points (about 7% 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 amount of the debt. Also, the effect of the financial reminder was significantly higher in Kentucky, where the financial penalty was significantly higher.

Third, we find that showing an individual that their delinquent neighbors owe larger amounts does not have a significant effect on payment rates. This constitutes suggestive evidence against the peer comparisons mechanism. Also, this evidence is consistent with findings from field experiments that messages of moral appeal are ineffective for reducing tax evasion (e.g., Blumenthal et al., 2001).

It is important to note that we conducted a mechanism experiment (Ludwig et al., 2011) instead of a policy evaluation experiment. That is, our experiment was not aimed at measuring the average effect of introducing online lists of tax delinquents on tax compliance, but rather at measuring some specific channels through which this method may influence tax compliance.[8] As in other mechanism experiments, this evidence can shed light on the nature of human behavior and provide insights for policymakers on how to design new tools to leverage established mechanisms more effectively. Also, we note that our experiment is focused on a convenience sample composed of tax delinquents who ignored previous shaming penalties imposed by the tax authority. Thus, one must be careful when extrapolating the results to other samples that may be relevant for policy analysis, such as individuals who are not delinquent or delinquents who responded promptly to the first communication from the tax authority.

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 et al., 2017; Drago et al., 2011; Naritomi, 2015); and satellite detection of unregistered properties (Casaburi and Troiano, 2016). We contribute to this literature by being the first to study shaming as a policy to increase tax compliance.

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

[8] While we focus on specific mechanisms (i.e., social interactions and peer comparisons), we are not implying that these are the only possible mechanisms or the most important. Also, 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 (e.g., the website can deter individuals from being listed in the first place).

This paper is also related to the literature in which social incentives are shown to be significant in prosocial contexts such as voting, charitable and political contributions, and energy conservation (Gerber et al., 2008; DellaVigna et al., 2012; Delmas and Lessem, 2014; Perez-Truglia and Cruces, 2017; Allcott, 2011). It is unclear, however, whether social incentives would also be significant in the context of antisocial behavior, which is the most relevant context for shaming penalties. We provide evidence suggesting that social pressure is effective in the context of antisocial behavior, but peer comparisons are not. 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 et al., 2014; Chetty, 2015).

The paper proceeds as follows. Section 2 introduces the institutional framework, the data sources and the experimental design. Section 3 presents the 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, 23 of the 50 U.S. states published online lists of tax delinquents (we present the full list in Table 1). From these 23 states, we selected our experimental states based on the following criteria.[9] Each state lists delinquents with debt amounts above a certain threshold. First, we restricted our sample to thresholds of $5000 or less, so that the list would include a significant number of subjects. Of the five states that satisfied this criteria, we excluded Georgia—because it does not publish the precise street address, and thus it would be impossible to mail letters—and Indiana because we wanted to focus on individual delinquents, and Indiana only lists business delinquents.

The resulting experimental subjects were delinquents listed in 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 B.3. The main difference is the debt threshold above which delinquents are listed: $250 in Kentucky, $2500 in Kansas, and $5000 in Wisconsin.[10] In our subject pool, 52.7% of subjects are from Kentucky, 25.4% from Kansas, and the remaining 21.9% from Wisconsin.

We downloaded the online lists of individuals for these three states on May 26, 2014. At that point, the online lists contained 57,744 individual tax delinquents who owed a total of $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 that corresponded to multiple addresses in the same state, because we could not 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 had moved out of the state; and (v) individuals with debts exceeding $150,000. We sent letters to a sample of 34,334 delinquents.[11]

Publication of lists of tax delinquents is regulated by state legislation.[12] 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 taxpayer's consent, however, is not needed before publication. The websites provide an e-mail address and phone number that tax delinquents can use to pay their debts and be removed from the list. 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. Tax agencies do not attempt to list individuals immediately after the tax obligation has been incurred, but rather wait for a period of time (for details, see Appendix B.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 agencies may have already tried and failed to garnish their income.

Our subjects had been delinquent for a significant period of time. For example, subjects in Kentucky (the only state that publishes the date the debt was incurred) had been delinquent for an average of 2.7 years (median of 2 years). In addition to other fees, the amounts owed were subject to annual interest rates of around 30%, 12%, and 18% 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 $5500 and a mean of $13,000. The online lists do not include any demographic information about 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% female and 71% White. These figures imply that delinquents are somewhat more likely to be male and non-White relative to the general population in these three states.[13] We do not have data on subjects' incomes, although the fact that tax debts arose primarily from state income tax suggests that the income of these individuals is above the tax-exempted level.[14] For the same reason, if 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 have been 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 26, 2014 (the date we formed our experimental sample). Given that the three states have different debt thresholds, and to keep delinquency rates comparable, we compute delinquency rates of debts over $5000, which coincides with the highest of the three

9 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.

10 In Wisconsin, the public list at its inception in 2006 included delinquent taxpayers who owed more than $25,000 while, on January 2008, the threshold was lowered at $5000. 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).

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

12 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 et al. (2000).

13 In comparison, the frequency of females and Whites in these three states are respectively 50% and 85%.

14 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 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.

thresholds.[15] 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 believe to be important for understanding tax delinquency. All of these variables, with the exception of 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 using the Internal Revenue Service Statistics of Income database for 2012, and 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 misreporting income to take advantage of EITC benefits (see Chetty et al., 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 nonprofit 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 statistically 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 garnishment 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 fewer 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 et al., 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, 2016).[16]

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 covariates across states. Columns (2) through (4) show the results for each state on a separate basis. The results indicate that the majority of correlations are qualitatively similar across states.

[15] The mean number of delinquents with debts above $5000 per 1000 inhabitants is similar across states: 2.27 in Kentucky, 2.31 in Kansas and 2.54 in Wisconsin.

[16] On the relationship between cultural norms and tax compliance see also Spicer and Becker (1980), Torgler (2003) and Alm et al. (1992).

Table 2
Place characteristics associated to the rate of tax delinquency.

	Number of delinquents in 5-digit ZIP code			
	(1) All	(2) Kentucky	(3) Kansas	(4) 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	1972	657	603	712

Notes: 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 $5000 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.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

2.3. Experimental design

Appendix A provides a snapshot of a sample letter and a sample envelope. The first paragraph explained that the letter was part of a research study on tax delinquency. Both the envelope and the letter bore the logo of the Department of Economics at the University of Michigan, in the hope that this would serve to identify the letter as a legitimate communication from researchers. The letter also contained a table that listed 10 tax delinquents from the recipient's geographic area, which always included the recipient. All delinquents were identified by full name and in ascending order by debt amount, with the recipient's listing highlighted. The second paragraph identified the corresponding state's revenue department as the data source, and stated: "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).[17]

[17] 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%), but these results are available upon request.

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.

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

Note that our experimental design does not compare individuals who received a letter to those who did not, because of the difficulty of determining the exact mechanism through which the letter can affect behavior. Additionally, as stated above, the respective revenue departments had already notified all of our subjects that they would be included on the list of delinquents. Last, our experimental design does not yield information on the effect of introducing these penalties.

At this point, a reader may wonder why there should be a response to our experimental intervention when our subjects were unresponsive to earlier communications from the tax authority. At least two reasons might explain this. First, there could be some difference in credibility or other factors between receiving a researcher's letter versus a tax department letter. Second, and more important, while a letter from the tax authority is directed only to the recipient, our letters also informed recipients that their neighbors were receiving information on how to access the shaming website. Since the website is not actively promoted to the general public, it is plausible that before our intervention, the delinquents did not expect their neighbors to know about the website. As a result, our higher-visibility intervention can increase the perceived probability that neighbors will use the website in the future to look them up, and thus create an incentive to pay quickly to avoid remaining on the list.

Table 3 summarizes the three treatment arms, including the number of subjects in each subtreatment. The first treatment arm was designed to alter the visibility of the recipient's delinquency status with respect to their neighbors. The letter always included 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 randomized the visibility of the search tool in the eyes of neighbors. We followed the design of Perez-Truglia and Cruces (2017) by constructing a lower-visibility message and a higher-visibility message, which was prominently displayed in a box located right below the list of tax debtors:

> **Lower visibility:** "Your household was the only household randomly chosen from your 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 sample states into small areas based on 9-digit ZIP codes and then, consistent with the message, randomized whether only one household from the area was selected to receive a letter or all households in the area were selected to receive a letter.[18] 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 cause recipients to feel that they were being monitored more closely by their neighbors. The hypothesis is that this would render delinquents more likely to pay based on the fear of losing reputation in the eyes of their neighbors (Appendix D provides a formalization of this argument by means of a simple social signaling model).

Our visibility intervention was not meant to maximize the response to the letters, but rather to estimate a precise channel through which publishing lists of delinquents may affect payment: social interactions. This final design was the result of a collaboration with the Institutional Review Board and attorneys in the Office of the General Counsel at the University of Michigan, which took into account a number of ethical, legal, budgetary, and practical constraints. In practice, shaming penalties used by governmental agencies 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 online lists of delinquents published by governmental agencies arguably increase visibility among a much broader set of social contacts that includes friends, relatives, coworkers, and employers.

Since interest rate payments are proportional to the debt amount, we would expect the effectiveness of financial penalties not to depend on the debt amount. On the contrary, however, there is no obvious reason the effectiveness of the shaming penalty should be independent of the debt amount. This possibility is consistent with anecdotal evidence from the tax agencies. For example, assume that by getting off the list a tax debtor can "salvage" $500 worth of respect from her neighbors. As a result, being taken off the delinquent list may be worth paying a debt of $250, but may not be worth paying a debt of $150,000.

[18] 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.

Table 4
Peer information treatment: sample treatment lists generated with different parameter values.

Low amounts ($\alpha = -1$)		Medium amounts ($\alpha = 0$)		High 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	$2648	R. P.	$23,188	J. W. C.	$68,509
L. N. L.	$2638	G. J. D.	$12,051	R. P.	$23,188
T. C.	$2024	W. S.	$6247	G. J. D.	$12,051
R. T. C.	$1944	Recipient	$2648	E. T.	$8244
D. N.	$1505	L. N. L.	$2638	W. S.	$6247
S. M.	$1158	T. C.	$2024	G. G.	$4312
T. S.	$873	R. T. C.	$1944	Recipient	$2648
J. V.	$269	D. N.	$1505	L. N. L.	$2638

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.

In the social signaling model from Appendix D, we provide a formalization of this intuition. According to this model, taxpayers pay on time to signal their social type, as in Bénabou and Tirole (2006). Under this assumption, the effectiveness of the debt amount turns out to be inversely proportional to the debt amount. That being said, this is not the only way of modeling shaming incentives. Just as there is no obvious reason why the effectiveness of shaming should be constant with respect to the debt amount, we could think of reasons why this effectiveness may increase or decrease with the debt amount.

In sum, how the effects of shaming scale up with the debt amount is ultimately an empirical question. It is also related to the broader literature on social incentives, because one of the most common criticisms of 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, which allows us to provide suggestive evidence on 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 financial penalties was not feasible, we instead created exogenous variation in the salience of financial penalties 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 amount. 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 the reminder to those who received a letter without the reminder.

For example, the financial reminder for Wisconsin recipients was: "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 motivate recipients to 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 to interest rates (Karlan et al., 2016).

The third and last treatment arm explores the role of peer information. By publishing the list of tax delinquents, a delinquent's payment decision may be affected; this is because the information contained in the list changes the delinquent's perception of the delinquent behavior of others. For instance, individuals have been documented to behave more prosocially when they perceive that others behave prosocially (e.g., Frey and Meier, 2004; Perez-Truglia and Cruces, 2017).[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 of Perez-Truglia and Cruces (2017). For each recipient, we identified her or his 20 closest delinquents. The nine neighbors to be shown in the table were selected by ordering the 20 closest delinquents according to a composite index, then selecting the top nine delinquents from the ordered list. The index is composed 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 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 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 the entire amount upfront or enter

[19] For instance, DellaVigna et al. (2017) 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).

[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.

Table 5
Descriptive statistics and randomization balance test.

	Visibility		Financial reminder		Peer information			Difference
	(1) Lower	(2) Higher	(3) No	(4) Yes	(5) $\alpha = -1$	(6) $\alpha = 0$	(7) $\alpha = 1$	(8) p-Value
Initial Debt Amount ($1000s)	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.

into 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 on the websites of the tax agencies.[22] Our dependent variable indicates whether the delinquent is or is not listed online at a given point in time.[23] We interpret changes in this variable as an indication of either paying 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 $\left(Y_i^t\right)$ takes the value 100 if the individual is not included in the online list of tax delinquents t weeks after the letters were sent, and 0 otherwise — as a result, the coefficients can be interpreted directly as percentage point effects. 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 were selected with $\alpha = -1, \alpha = 0$ or $\alpha = 1$, respectively. Note that we allow the treatment effects to differ by the quartile of the initial debt amount $\left(\left\{Q_i^j\right\}_{j=1}^4\right)$. 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 pretreatment characteristics. The only pretreatment outcome that we observe directly is the initial debt 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, individuals were balanced on pretreatment characteristics, with the exception of a small difference in the percentage of African-Americans.[26] As an additional robustness

[22] For the statutory evidence, see in Appendix B.3. For empirical evidence, see Appendix B.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.

[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% 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 100) 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 B.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.

[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 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.

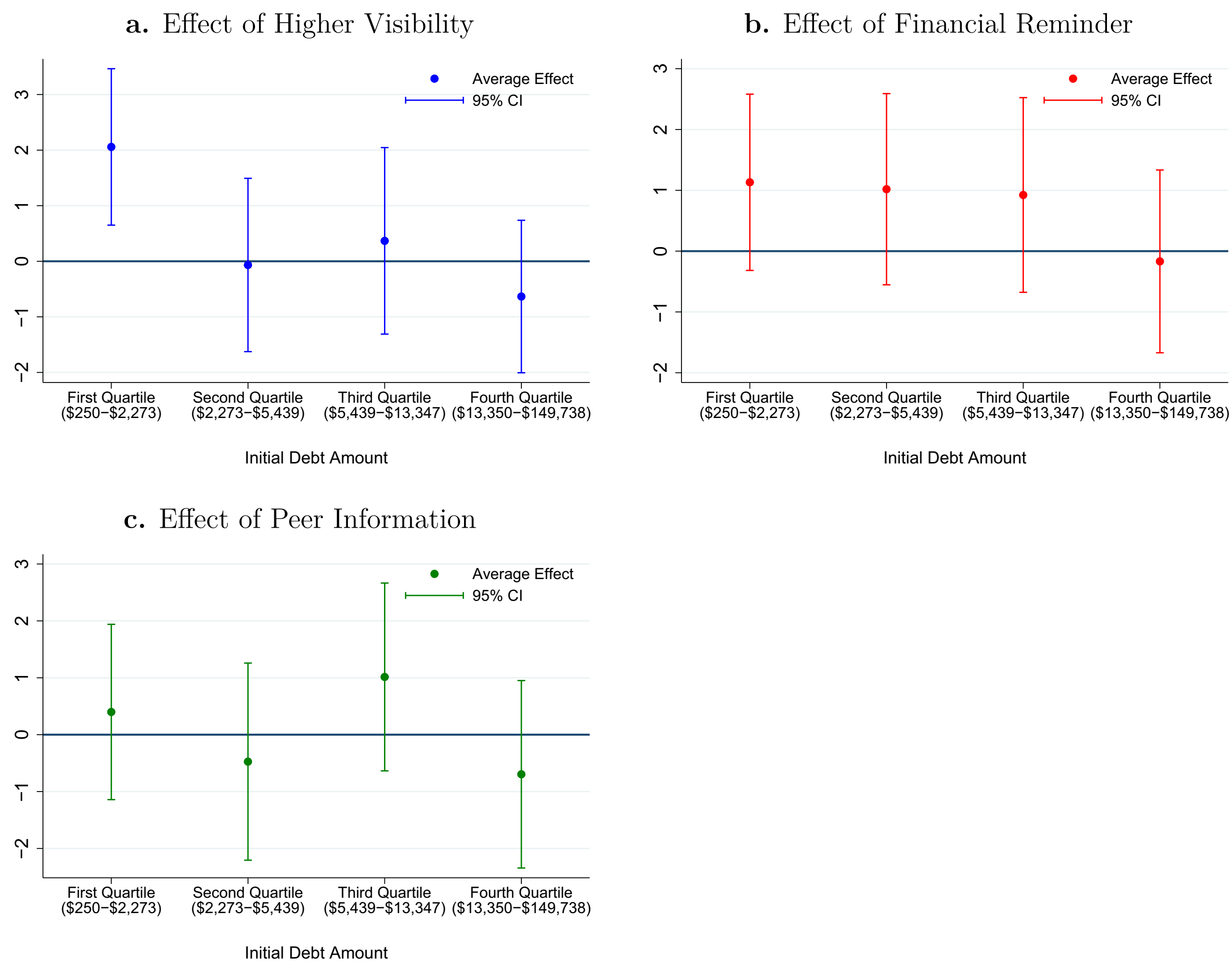


Fig. 1. Effects of information treatments, 10 weeks after mail delivery, by quartile of debt amount. Notes: N = 34,334. The debt amount in the x-axis corresponds to the amount owed when the subject pool was formed (May 26, 2014). The units of the y-axis correspond to percentage points. The effects were estimated from OLS regressions (one for each group in the x-axis) where the dependent variable takes the value 100 if the subject is not listed as a delinquent 10 weeks after the letters were delivered and 0 otherwise, 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.

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).[27]

3. Results

3.1. Effect of higher visibility

Fig. 1a 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 10 weeks after the letters were sent. The effects are broken down by quartiles of the initial debt amount, as described in Eq. (1). For reference, Table 6 shows the corresponding average payment rates, as well as regressions results, in table form. Fig. 1a shows that for the lowest quartile ($250–$2273), higher visibility increased the payment rate 10 weeks after mail delivery by 2.1 percentage points. 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% of the baseline rate. The effect of higher visibility, however, was estimated to be close to zero and statistically insignificant for the other three quartiles of the initial debt amount. The finding that the salience of shaming seems more effective for smaller debts is consistent with the perception of tax practitioners, as mentioned above based on private conversations and press releases.

As a falsification test, Fig. 2a shows the same average treatment effects of Fig. 1a, 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.

[27] One of the concerns with the results presented by debt quartiles is that our sample is not balanced within the quartiles. As a robustness check, Appendix C.2 shows that there is treatment balance within each of the four quartiles of the initial debt amount.

Since the financial and shaming penalties differ across states, the effects of our reminders may vary by state.[28] To explore this, Fig. 3 presents the results on state-level heterogeneity. Given that the distribution of debt amounts is notably different in Kentucky compared with the other two states, we separated heterogeneity by state from heterogeneity by debt amount. We did this by splitting the Kentucky sample in two: initial amounts between $250 and $2500 and initial amounts above $2500. The resulting four groups each contain about one quarter of the sample. We used $2500 to split the Kentucky sample because it corresponds to the disclosure threshold for Kansas, but the results are similar if we instead split the sample using the first quartile ($2273), as before.

Fig. 3a presents state-level heterogeneity for the effect of higher visibility. Although debtors in Kentucky with debts below $2500 reacted to the higher visibility, debtors in Kentucky with debts above $2500 did not react to the higher visibility, and neither did debtors with debts above $2500 in Kansas and Wisconsin. These results suggest that even within Kentucky, the effects of higher visibility decline significantly with the debt amount.[29] 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, Fig. 4a shows week-by-week estimates of the effects of higher visibility (for the first quartile). This figure shows that individuals who react to the higher visibility leave the list promptly: The vast majority of the reaction occurs during the second to fifth week after mail delivery. After week 10, the effects of higher visibility decline slightly. Even 29 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 10. This suggests that nearly 75% of the individuals who reacted to the higher visibility by week 10 did not intend to pay during the subsequent 19 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 also cannot reject that they are different from zero.

A key result is that for delinquents in the bottom quartile of the distribution of debt amounts, the higher-visibility intervention increases tax compliance. Our preferred interpretation for this finding is based on social signaling models, as formalized in Appendix D. This interpretation is consistent with complementary evidence from Gerber et al. (2016), who conducted a survey experiment in which they showed participants information about a hypothetical individual, then asked how favorably they viewed the person. The authors found that respondents viewed the hypothetical individual less favorably when the individual was described as being late with tax payments relative to someone who pay taxes on time. Additionally, they found that other forms of antisocial behavior, such as failing to vote or recycle, has an effect on social image of similar magnitude as being late with tax payments.

Another notable result is that higher visibility is less effective for delinquents who owe higher amounts.[30] Our preferred interpretation for this finding is that social incentives do not scale up well, as in the social signaling model provided in Appendix D. There is, however, an important caveat with this interpretation: We did not randomize the initial debt amount, and therefore it is possible that delinquents who owe larger amounts have unobservable characteristics that cause less responsiveness to higher visibility.

Perhaps the most natural confounding factor could be that individuals who owe larger amounts are less reactive to shaming incentives because they face larger liquidity constraints. However, we have reason to believe that this is not the case in our context. First, household credit constraints may not play a large role in the decision to pay delinquent debt, because the tax agency is offering to finance debts of all sizes and for every household. In other words, if a delinquent who owes a large amount does not have the liquidity to pay the entire amount upfront, the delinquent can still be taken off

Table 6
Effects of information treatments on probability of payment.

	Probability of leaving the list		Log(Amount)
	(1) Week 5	(2) Week 10	(3) Week −3
Effect of higher visibility:			
First quartile ($250–$2273)	1.914***	2.083***	0.003
	(0.662)	(0.726)	(0.013)
Second quartile ($2273–$5439)	−0.275	0.033	−0.001
	(0.631)	(0.795)	(0.006)
Third quartile ($5439–$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–$2273)	0.939	1.061	−0.002
	(0.652)	(0.740)	(0.014)
Second quartile ($2273–$5439)	0.578	1.023	−0.001
	(0.619)	(0.803)	(0.006)
Third quartile ($5439–$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–$2273)	0.082	0.325	−0.006
	(0.703)	(0.780)	(0.016)
Second quartile ($2273–$5439)	−0.796	−0.574	−0.014**
	(0.668)	(0.890)	(0.006)
Third quartile ($5439–$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–$2273)	9.874	13.431	6.786***
	(7.880)	(9.000)	(0.015)
Second quartile ($2273–$5439)	12.275	22.305**	8.190***
	(7.896)	(8.864)	(0.006)
Third quartile ($5439–$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. Standard errors in parentheses, clustered at the 5-digit ZIP code level. The coefficients were estimated from OLS regressions (one per column). The dependent variable in column (1) (column (2)) takes the value 100 if the subject is not listed as a delinquent 5 (10) weeks after the letters were delivered and 0 otherwise. The dependent variable in column (3) is equal to the logarithm of the initial debt amount multiplied by 100. 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 Fig. 1 for definitions for *Higher visibility*, *Financial reminder* and *Peer information*.
* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.

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

[29] For a finer break-down of the effects in Kentucky, see Appendix Fig. C.2.

[30] This evidence does not rule out that shaming can be effective even with large debt amounts in other contexts such as in other populations (e.g., taxpayers who have not become delinquents yet) or with more substantial shaming interventions (e.g., making the delinquency status to friends, coworkers and relatives).

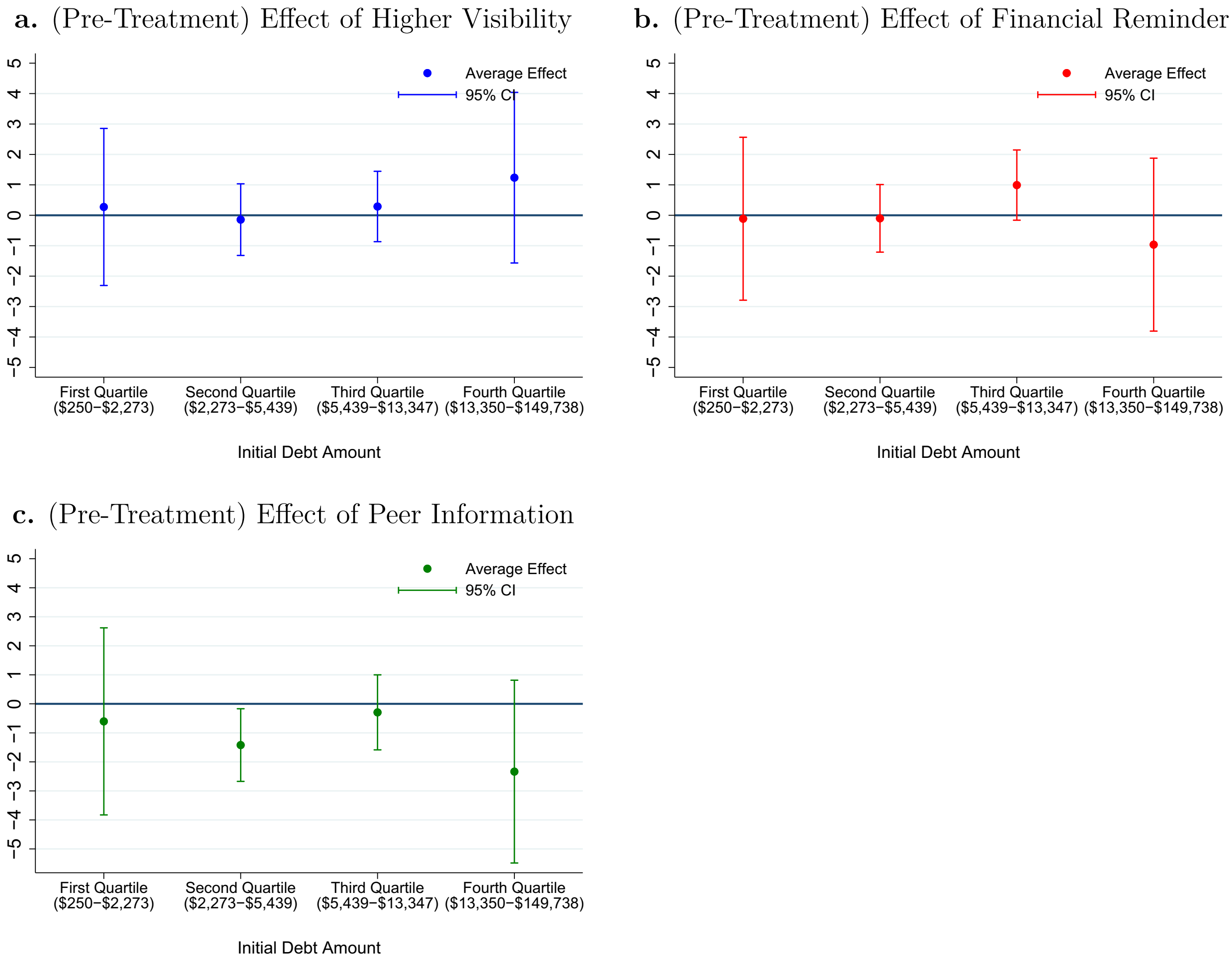


Fig. 2. Falsification test: "effects" of information treatments on the pre-treatment (Log) debt amount. Notes: N = 34,334. The debt amount in the x-axis corresponds to the amount owed when the subject pool was formed (May 26, 2014). The units of the y-axis correspond to log differences. The effects were estimated from OLS regressions (one for each group in the x-axis) where the dependent variable is the logarithm of the initial debt amount multiplied by 100, 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.

the list by entering into a payment plan for the full amount. Second, if delinquents with higher debt amounts react less to higher visibility because they are more liquidity constrained, we should expect them to also react less to financial reminders. However, as discussed below, the effectiveness of financial reminders does not decrease with debt amount.

Even though we cannot rule out alternative interpretations, Appendix C.3 reports some suggestive evidence consistent with our preferred interpretation of the heterogeneity by debt amount. This appendix reports regressions in which we allow for heterogeneity by debt amount as well as for heterogeneity by other observable characteristics (e.g., ZIP code income, ZIP code credit score, and individual debt age). We show that these other forms of heterogeneity are not nearly as important for the effects of higher visibility as is the heterogeneity by debt amount. Most important, we show that controlling for those alternative sources of heterogeneity does not change the magnitude or statistical significance of the heterogeneity by debt amount. Last, the appendix also discusses obvious limitations with this suggestive evidence—for example, we cannot rule out the possibility that the heterogeneity by debt amount is spuriously driven by unobservable sources of heterogeneity.

We have at least three reasons to believe that our visibility intervention provides a conservative lower bound for 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 these interactions would more likely be affected by publication of online lists of delinquents. Second, the tax agencies in all three experimental states are required to send letters to allow individuals to resolve their debts prior to being included in online lists, 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

[31] For instance, the chairman of the California Board of Equalization declared that when the list was introduced in California 41% 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).

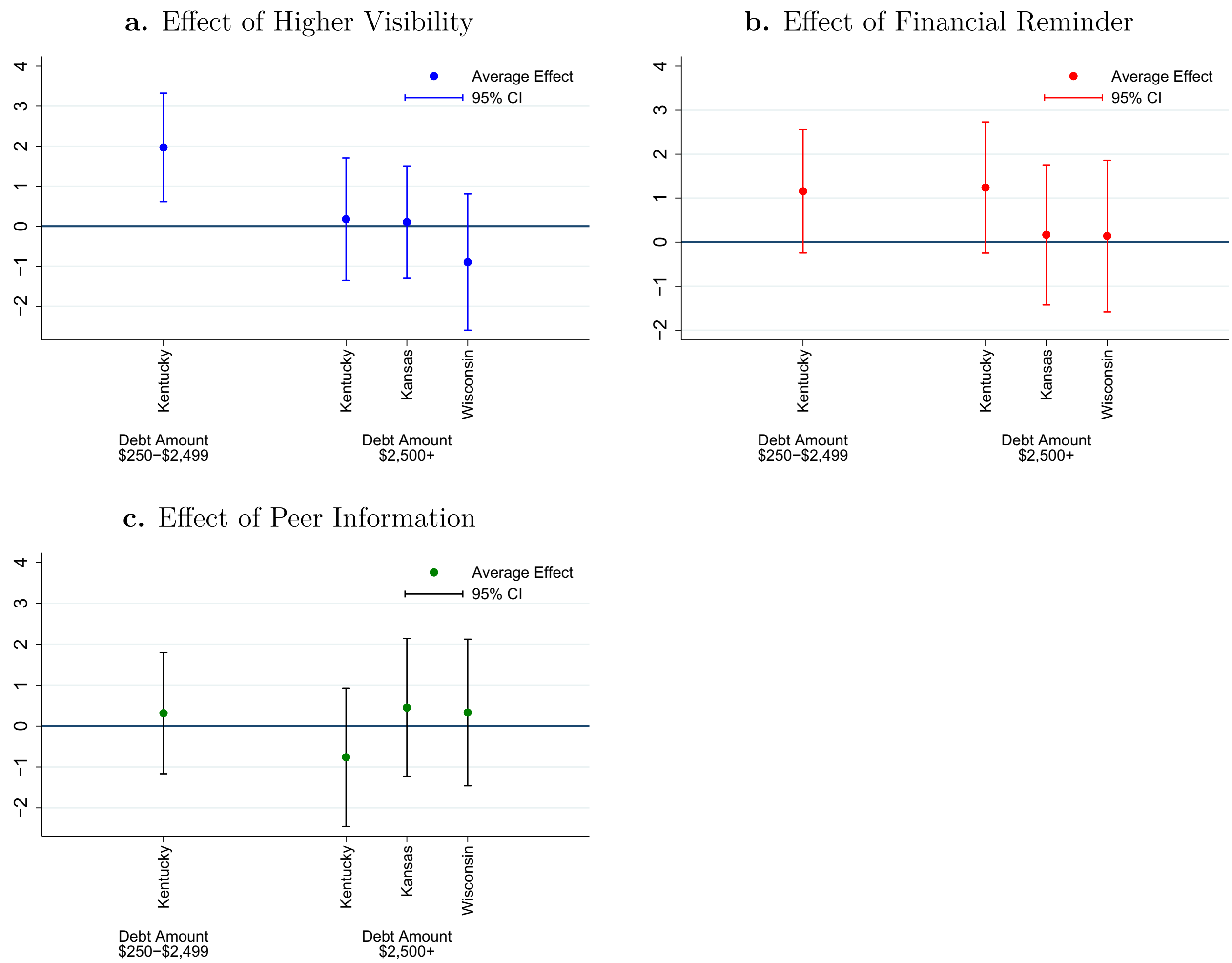


Fig. 3. Effects of information treatments, 10 weeks after mail delivery, by state and debt amount. Notes: N = 34,334 (9029 from Kentucky $250–$2499, 9072 from Kentucky $2500+, 8710 from Kansas and 7523 from Wisconsin). The debt amount in the x-axis corresponds to the amount owed when the subject pool was formed (May 26, 2014). The units of the y-axis correspond to percentage points. The effects were estimated from OLS regressions (one for each group in the x-axis) where the dependent variable takes the value 100 if the subject is not listed as a delinquent 10 weeks after the letters were delivered and 0 otherwise, 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.

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 individuals may not have read the letter. For instance, the U.S. Environmental Protection Agency estimates that only about one-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 publication of online lists of tax delinquents. 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 that lists tax delinquents. Our mailing intervention would be more effective, therefore, because it provides neighbors with access to the website.

3.2. *Effect of financial reminder*

Fig. 1b shows the effects of the financial reminder (i.e., the difference between letters with and without information about financial penalties). 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% of the baseline rate). Although individually these three coefficients are statistically insignificant, they are jointly significant; the average effect is 0.98, with a p-value of 0.034. Unlike the effect for the first

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

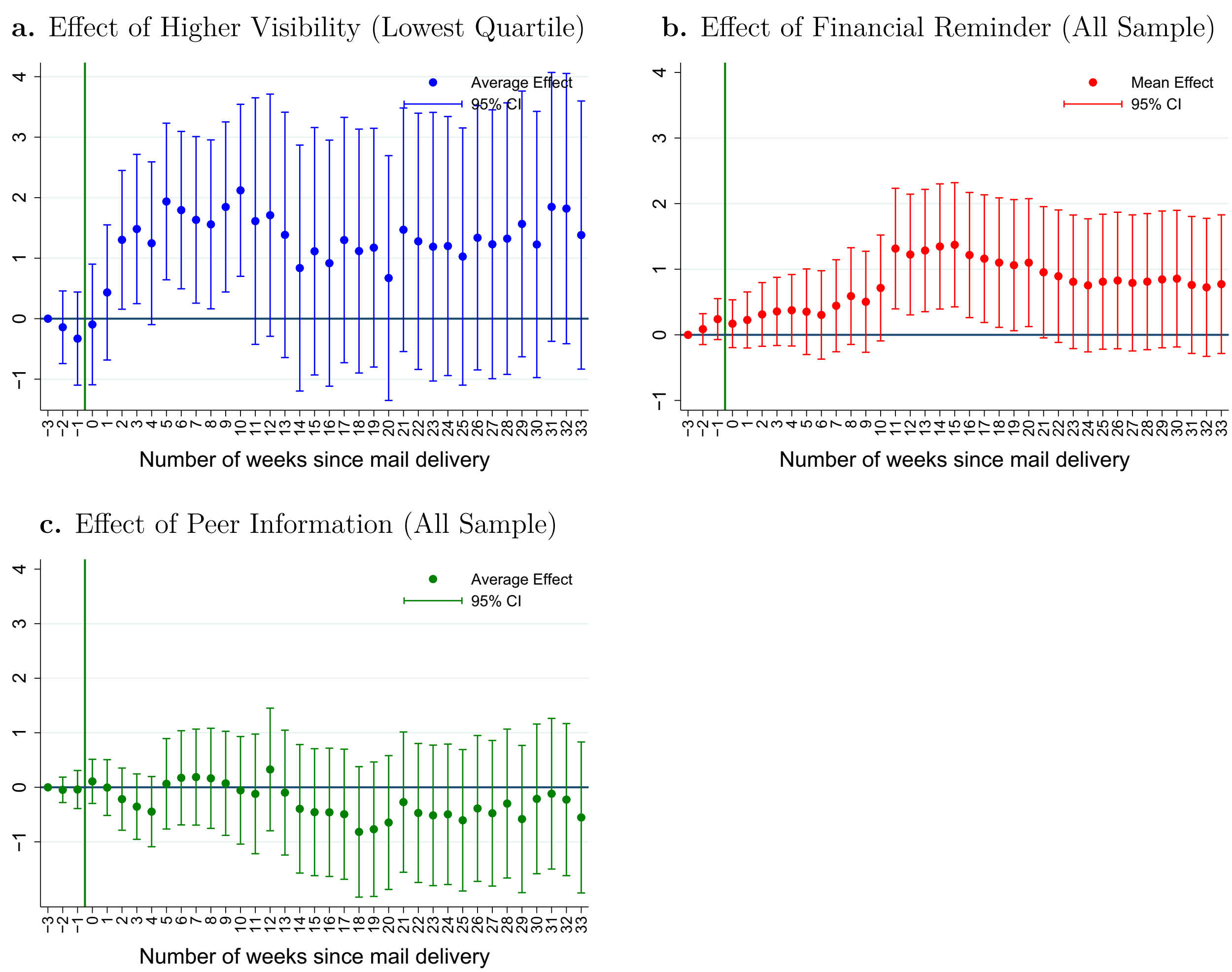


Fig. 4. Week-by-week evolution of effects of information treatments. Notes: (a) N = 8584; (b) N = 34,334; (c) N = 34,334. In the x-axis, week −3 corresponds to the date when the subject pool was formed (May 26, 2014). The green vertical line shows the approximate date when the letters were delivered. The units of the y-axis correspond to percentage points. The effects were estimated from OLS regressions (one for each graph) where the dependent variable takes the value 100 if the subject is not listed as a delinquent 10 weeks after the letters were delivered and 0 otherwise, 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. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

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, Fig. 2b shows that the financial reminder did not have a significant effect on the pretreatment outcome, which is the logarithm of the initial debt amount.[33]

Fig. 3b explores state-level heterogeneity of the effects of the financial reminder. For debtors in Kentucky with debts both below and above $2500, 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 reminder differed between Kentucky and the other two states.[34] One simple explanation for this finding is that the financial penalty in Kentucky (30%) is significantly higher than the financial penalties in the other two states (12% in Kansas and 18% 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. In contrast, if the average individual from Kansas (Wisconsin) found out about the 12% (18%) interest rate on her tax debt, she should

[33] Appendix C.2 provides some robustness checks for this heterogeneity — for instance, we show that the results are similar if, instead of splitting the sample by quartiles of initial debt amount, we split the sample using terciles or quintiles of initial debt amount.

[34] 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).

gain nothing (little) from paying off the her tax debt with her credit card.[35]

Fig. 4b 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.[36] Individuals who react to the financial reminder react more slowly than those who react 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 higher visibility, the slow decline implies that a majority of individuals who paid because of the financial reminder were individuals who, in the absence of this reminder, would not have paid in subsequent weeks.

3.3. *Effect of peer information*

Fig. 1c shows the effects of *Peer information*—a variable that takes a value 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 four positions.[37]

The coefficients on *Peer information* reported on Fig. 1c indicate that showing higher debt amounts in the letter does not have significant effects on payment decisions 10 weeks after delivery of the letter. Each of the four coefficients is statistically insignificant. The average effect over the entire sample is close to zero (0.10 percentage points), precisely estimated (s.e. 0.42), and statistically insignificant (p-value = 0.811). Figs. 3c and 4c show that this null result is robust across specifications. Fig. 3c shows the heterogeneity by state: The effects of peer information are close to zero and statistically insignificant in each of the states. Fig. 4c shows the week-by-week estimates of the effects of 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 may 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), Fellner et al. (2013), and Dwenger et al. (2016) find that messages with moral appeals fail to reduce tax evasion. In the context of tax delinquency, the evidence on the effect of moral appeals is mixed: Although Hallsworth et al. (2017) find significant effects, Castro and Scartascini (2015) do not.[38]

However, our evidence concerns 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.[39] Furthermore, our evidence indicates that the social information does not affect the behavior of individuals who are already tax delinquents. It is still possible, however, that this same social information would affect the behavior of nondelinquents. 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 prosocial individuals (e.g., individuals who give to charity) may not be equally effective with antisocial individuals (e.g., tax delinquents).

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 the United States and the rest of the world. We provide evidence from a field experiment in the context of tax debt enforcement, and show 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; it did, however, seem to depend on the size of the interest rate. Last, we provide evidence that payment decisions are not significantly affected by specific information about the amounts owed by other delinquents.

Our results provide some avenues for future research. First, our evidence comes from a 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 policy of 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 penalties 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 drivers' licenses and passports (Blank, 2014). Future research may use a variation of our experimental design to test the effectiveness of these other types of nonfinancial penalties.

Last, our evidence 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, shaming penalties ought to be used. Given that financial penalties are available, it is not obvious why tax agencies are interested in using shaming penalties in the first place.[41] Thus, theoretical and empirical study of the normative aspects of shaming should be a priority for future research.

[35] 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.

[36] 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.

[37] For details about this calculations, see the discussion of Appendix Table C.1.

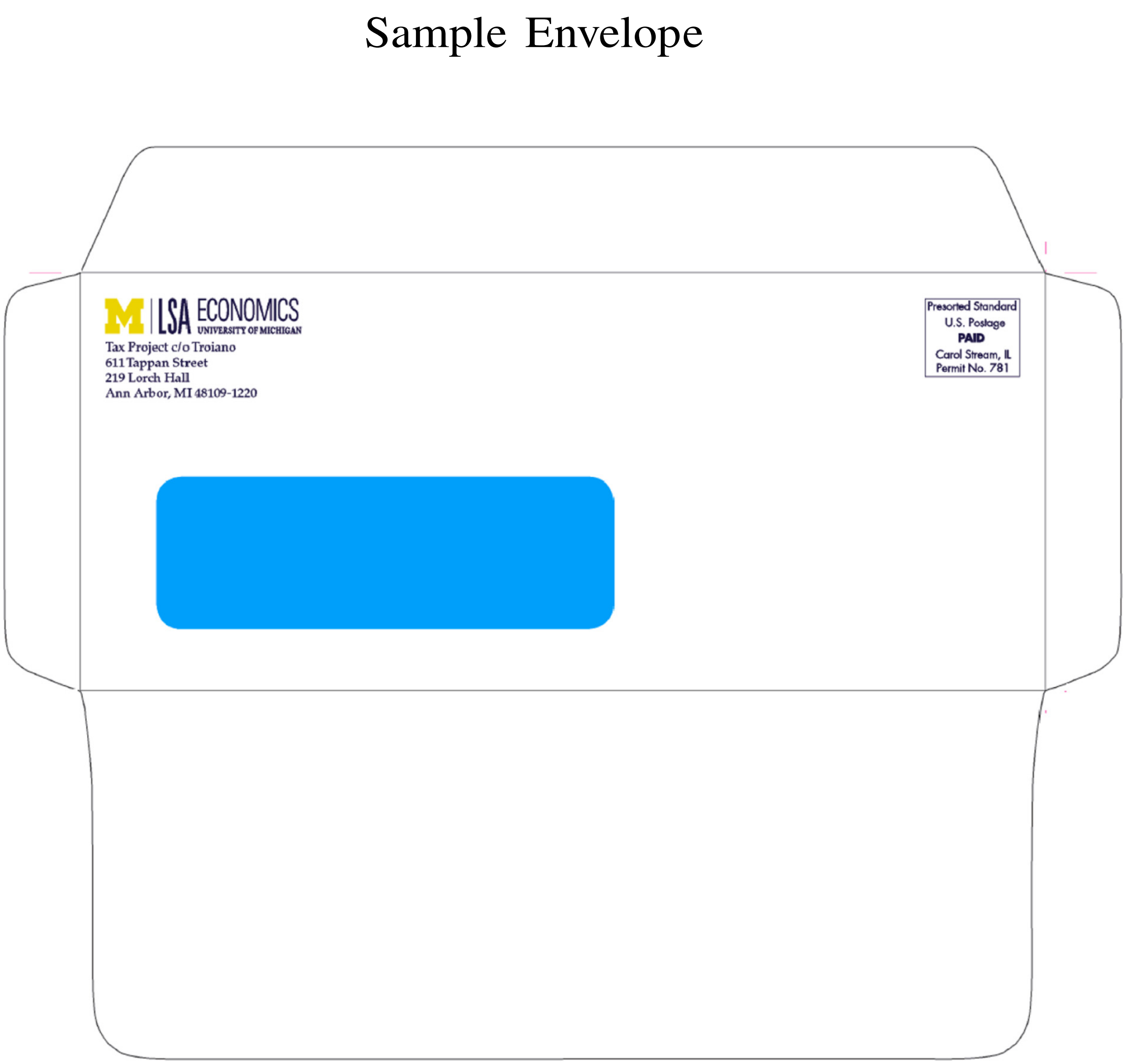
[38] For a more general discussion about moral suasion see Luttmer and Singhal (2014).

[39] 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.

[40] For example, the U.K. publishes a list of top tax evaders (http://www.hmrc.gov.uk/defaulters/). Also, even though it was not part of a regular policy, Chetty et al. (2014) present results from an intervention in Bangladesh that peer pressure may be effective in reducing tax evasion.

[41] Similarly, the fact our financial reminders increased payment rates does not imply that financial penalties are desirable from the perspective of social welfare.

Appendix A. Sample of the envelope and the letter



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

Please recycle

10001

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

Appendix B. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.jpubeco.2018.09.008.

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