Effectiveness of Repeated Implementation Intention-Interventions on

Organizations' Likelihood to File their Overdue Taxes

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Effectiveness of Repeated Implementation Intention-Interventions on Organizations' Likelihood to File their Overdue Taxes

This paper investigates the effect of facilitating implementation intentions on organizations' tax compliance behavior. We conducted a large-scale, multi-wave field experiment involving the tax-paying behavior of all organizations that failed to file timely annual returns for a payroll tax in the province of Ontario. Organizations were randomly assigned to receive one of two letters: Ontario's standard late notice (control) and a revised experimental late notice, which included step-by-step instructions of when, where and how to file a return. Our data indicate that instilling implementation intentions is effective at increasing organizations' timely tax payment. In addition to replicating these findings across two waves, we find no evidence of habituation to our intervention over time. Our results provide evidence that procrastination may be a substantial but solvable barrier to improving organizational tax compliance. Moreover, we demonstrate the effectiveness of an intervention that typically targets individual's behavior in the realm of organizational behavior.

1. Introduction

How can governments effectively increase tax compliance? This question is critically important because efficient funding of public services involves both levying tax and minimizing the cost of its collection (Andreoni, Erard, & Feinstein, 1998), yet tax compliance continues to be an ongoing problem (Matsaganis, Leventi, & Flevotomou, 2012). For example, in 2001 and 2006 the US Internal Revenue Service estimates that only 83 percent of total tax liabilities were collected voluntarily, and that additional enforcement measures were able to increase the collection of taxes owed to merely 86 percent (2012).

In order to increase tax compliance of individuals as well organizations, governments have typically made use of two approaches. The traditional approach, already in universal use by governments, is to apply financial disincentives for non-compliance (e.g. penalties, interest, and fines administered via enforcement efforts), thereby providing taxpayers with extrinsic motivation to pay their dues. A second, more recent approach has been to use insights from behavioral science, such as social norms or moral suasion, to bolster taxpayers' intrinsic motivation to contribute to the public good (Alm & McClellan, 2012; Alm & Torgler, 2006; Chetty, Mobarak, & Singhal, 2014; Cummings, Martinez-Vazquez, McKee, & Torgler, 2009; Hallsworth, List, Metcalfe, & Vlaev, 2014). In this paper we focus on organizational tax compliance and propose and examine a behavioral approach that has previously been applied to irregularly scheduled individual behaviors (such as getting a flu-vaccine or colonoscopy; Milkman, Beshears, Choi, Laibson, & Madrian, 2011, 2013): encouraging taxpaying organizations to act on existing motivations by instilling implementation intentions (Gollwitzer, 1999). We test the efficacy of this novel approach to tax collection using a large-scale, multi-wave (over two consecutive years) field experiment involving the tax-paying behavior of all organizations that failed to file timely annual returns for a payroll tax in the province of Ontario. Our results show that simplifying communications and providing concrete, instructions written in an active voice of when, where, and how to file taxes can be effective at promoting timely tax payment – even when repeated. The fact that these small, behaviorally informed changes

significantly impacted organizations' filing behavior, without changing incentives, suggests that procrastination may also be a substantial but solvable barrier to improved organizational (in contrast to individual) tax compliance.

In addition to applying a well-established behavioral insight to tax compliance for the first time (see Rogers, Milkman, John, & Norton, 2016, for a review of the efficacy of planning prompts in other domains), this research is significant in three ways. For one, it is among the first experimental studies of tax compliance involving organizations (however, see these unpublished working papers for field experiments of which the authors are aware: Brockmeyer, Hernandez, Kettle, & Smith, 2016; Chetty et al., 2014; Kettle, Hernandez, Ruda, & Sanders, 2016; Ortega & Sanguinetti, 2013; Pomeranz, 2013; Wenzel, 2002). Advancing our understanding of organizations' tax compliance is important because of the critical role they play as both remitters of sales and income taxes, and as subjects of corporate tax in their own right. Indeed, in Ontario where our field experiment was conducted, the taxes that organizations pay (not including the taxes they collect and remit on behalf of others) make up over 18 percent of the government's total tax revenue (Government of Ontario, 2016). Furthermore, although recent field experiments have begun to shed more light on individuals' tax compliance, it is unclear whether this knowledge can be applied across levels of aggregation (Ariel, 2012). Solutions that improve individuals' tax compliance need not necessarily be effective when it comes to organizational actors. For example, although Kettle and colleagues (2016) find that letters highlighting social norms and those framing non-declaration of income as a deliberate choice increase tax compliance for both individuals and organizations, Wenzel (2002) reports that letters highlighting a tax administration's adherence to procedural justice has a positive effect on individuals' compliance, but that the same letters have no or negative effects on organizations' tax compliance.

Second, rather than focus on tax evasion (deliberately misreporting information with the intention of underpaying taxes), which has been the focus of the majority of the tax compliance literature (see Alm,

2012, for a review), in this research, we turn our attention to increasing the payment of overdue taxes. This shift in attention to a relatively understudied component of tax compliance (but see Hallsworth et al., 2014) is valuable because the collection of outstanding tax debt is a large and expensive problem for many governments. For example, across OECD member states unpaid tax debt as a share of annual net tax revenue was 12.3 percent in 2011, and it was much higher in troubled economies such as Greece, where the proportion was 72.2 percent (International Monetary Fund, 2012). Typically, governments are unable to recover much of this debt as its collection is costly, involving phone calls, collection agencies, auditing, and assessment; furthermore write-offs are common. Moreover, our focus on tax payment behavior has the advantage of readily operationalizable behavioral metrics. Whereas tax evasion is a clandestine behavior which is usually measured by proxy variables of questionable validity (such as the size of tax declarations and deductions; Slemrod & Weber, 2012), timely tax payment can be directly measured, and thus it lends itself to experimental investigations in which causal conclusions about the effect of an intervention are sought.

Third, because our field experiment features two waves of a randomly assigned intervention and measurement, this research is able to examine how the effects of our intervention unfolded over time (i.e. two tax years). Gaining greater understanding of the temporal dimension of behavioral interventions is valuable. Nevertheless, relatively few field experiments can speak to longer-term effects because most administer an intervention and measure its effect only once for reasons of expediency. One important temporal aspect of behavioral interventions concerns the persistence of their effects once an intervention has stopped (Frey & Rogers, 2014). For example, Brockmeyer et al. (2016) examine the impact of an intervention in a single year on tax compliance not only immediately afterwards, but also a year later (without any additional communication). While this is an important first step in examining longer-term effects of behavioral interventions, consistency, or whether repeated exposure to the same intervention alters its effectiveness (Rogers & Frey, 2015), is another critically important temporal aspect of

behavioral interventions in the realm of tax compliance, as tax payers are liable to be exposed to the same collection process year over year. Here we are able to provide evidence addressing both the persistence and consistency of a behavioral intervention that we repeated province-wide for two consecutive years.

2. Theoretical Approach

By far, the oldest and most well-studied approach to increasing tax compliance is the use of penalties and fines to decrease the expected utility of non-compliance (Allingham & Sandmo, 1972). However, if these disincentives were the only drivers of tax compliance, expected utility models would actually predict higher levels of non-compliance (e.g., Skinner & Slemrod, 1985; Slemrod & Yitzhaki, 2002), suggesting that extrinsic motivations are not the only determinants of tax compliance. Moreover, despite the widespread, if not universal, deployment of penalties and interest charged on late tax payments, non-compliance persists. While some overdue payments may be inevitable due to liquidity constraints among some taxpayers, experimental evidence discussed below suggests that this is not the case for all tardy payments (Chetty et al., 2014; Hallsworth et al., 2014). Indeed, research in a related domain has shown that the high interest rates of credit cards and money lenders do not dissuade many individuals from carrying debt for which they have the liquid assets to pay (Collins, Morduch, Rutherford, & Ruthven, 2009; Gross & Souleles, 2002). Extrapolating from this research, we therefore predicted that in spite of penalty fees and interest charges, a substantial portion of the late-paying taxpayers may be late, not because they lack the ability to pay, but because these extrinsic motivations fail to spur them to action.

If taxpayers are less sensitive to extrinsic motivations than standard expected utility models would predict, another approach to increasing compliance may be to bolster intrinsic motivation to pay taxes, otherwise known as tax morale (Luttmer & Singhal, 2014). Although the notion that people are motivated to pay taxes seems paradoxical, research suggests that they may be moved by a sense of obligation (Frey, 1997), personal responsibility (Mazar & Aggarwal, 2011), or the desire to preserve their

moral self-concept (Mazar, Amir, & Ariely, 2008). Indeed, cross-cultural differences in tax morale is a significant predictor of the size of countries' tax-evading shadow economies (Alm & Torgler, 2006), and self-reported tax morale predicts organizations' tax compliance behavior (Alm & McClellan, 2012). However, interventions designed to increase tax compliance by boosting tax morale have been met with mixed results. Field experiments in which *individual* taxpayers were reminded of prescriptive norms (i.e. that they ought to comply with tax law) have found no effect on either imputed tax evasion (Blumenthal, Christian, & Slemrod, 2001; Torgler, 2012) or timely tax payment (Torgler, 2004). A plausible explanation for these null results is that tax morale appears to be less the product of abstract notions of moral obligation, and more the result of a psychological contract with society and the state (Feld & Frey, 2007), in which one does their part because other citizens are also contributing and the state can be trusted to put tax dollars to good use (Cummings et al., 2009). Consistent with this, the two field experiments in which descriptive norms were deployed, detailing high levels of tax compliance within the population, showed positive effects on tax evasion (Wenzel, 2005) and timely tax payment (Hallsworth et al., 2014). Thus, there is some evidence that in addition to the extrinsic motivation created by disincentives, intrinsic motivation can be augmented to increase tax compliance—not by reminding taxpayers of their abstract duty, but by informing them that their fellow citizens accept and support the same social contract.

However, whether this knowledge generated from research investigating individuals' tax compliance behavior can be directly applied to organizational actors is an open question. Indeed, although the judgment and decision-making literature is rich with research on how seemingly small contextual factors, like defaults (Johnson & Goldstein, 2003), framing (Tversky & Kahneman, 1981), and descriptive norms (Goldstein, Cialdini, & Griskevicius, 2008), can be used to nudge individuals' behavior, we know relatively little about how such tools influence organizational behavior. While, on the one hand, one might expect the effects of choice architecture and nudges to be isomorphic across levels of aggregation, because organizational behavior is ultimately decomposable to the decisions and behaviors of individuals,

on the other hand, there are well-established ways in which the processes of organizing and making decisions in groups can alter the socio-psychological constructs on which such behavioral interventions operate.

For example, because people highly value their social and moral self-concepts (Dunning, 2007; Monin & Jordan, 2009), interventions that increase people's awareness of the social norms surrounding tax payment, (Hallsworth et al., 2014), or that increase the salience of the ethical dimension of insurance declarations (Shu, Mazar, Gino, Ariely, & Bazerman, 2012), have been shown to effectively curtail individuals' unethical behaviors. However, when individuals are acting as a group, or on behalf of others, these social norms and moral costs may exert less influence over their behavior. Research has found that jointly producing an outcome (Bandura, Underwood, & Fromson, 1975) or sharing a collective identity (Mazar & Aggarwal, 2011) facilitates a diffused sense of moral responsibility, and that acting for the benefit of others provides altruistic justification for misdeeds (Wiltermuth, 2011). Consequently, these group processes encourage individuals to act less ethically than they otherwise would because they can do so without negatively impacting their individual moral self-image (Bandura, 1999; Mazar et al., 2008). Thus, due to diffusion of responsibility, it is not clear that interventions designed to raise the moral cost of an activity (Levitt & List, 2007) will be effective when it comes to nudging organizational behavior. Indeed, in one of the only published field experiments involving organizational tax compliance, Ariel (2012) finds a moral suasion intervention to be counterproductive, having no impact on compliance, while resulting in an unintended increase in tax deductions and therefore a decrease in tax revenues.

There is also good reason to believe that many well-established behavioral interventions will not be as effective for organizations because individuals and organizations may not share the same cognitive limitations. In general, behavioral interventions are useful because individuals exhibit bounded rationality (Simon, 1955) and often rely on heuristics to make decisions (Kahneman, 2011). As a result, individuals can benefit from cognitive support, including reminders (Guy et al., 2012), feedback (Kluger & DeNisi,

1996), and decision engines, or nudges, which take advantage of heuristics to reshape behavior, such as messages which activate the social script of reciprocity in order to encourage a positive response (Sanders & Kirkman, 2014). However, in organizations individuals are embedded within bureaucratic systems designed to rationalize decision making for the efficient pursuit of organizational ends (Weber, 1922). Standard operating procedures, training, specialization, feedback, accountability, and technology are all common components of systems of organizing which are designed to rationalize decision making. These systems are continually refined by market pressures, which select against those organizations with ineffective decision-making capabilities (Hannan, Polos, & Carroll, 2007). As a result organizations are capable of more rationally processing information and making optimal decisions in the face of uncertainty than are individuals (Stinchcombe, 1990), although organizations by no means always manifest this capability. Moreover, even in the absence of formal systems of organization, experimental comparisons between individual and group decision making generally find that groups act more rationally and are less swayed by the cognitive limitations and biases which characterize the behavior of individuals (see Charness & Sutter, 2012; Kugler, Kausel, & Kocher, 2012, for reviews). Therefore, it could very well be that organizations stand to benefit less from established behavioral interventions because the processes of organizing and collaboration already effectively mitigate the cognitive limitations of individuals. Furthermore, to the extent that organizational behavior does fall short of maximizing organizational interests, this may be caused by organizational-level mechanisms, such as principal-agent problems (Jensen & Meckling, 1976) and barriers to information sharing (Connelly, Zweig, Webster, & Trougakos, 2012; Stasser & Titus, 2003), rather than the individual-level mechanisms on which behavioral interventions have mostly focused.

While a systematic investigation of how the many behavioral interventions shown to be effective for individuals translate to organizational behavior is beyond the scope of this research, our goal is to encourage this line of inquiry with an experimental investigation of one of the most well-established

interventions: facilitating implementation intentions (Rogers et al., 2016). This approach has been shown to be effective for increasing for example, voter turnout (Nickerson & Rogers, 2010) as well as vaccination (Milkman et al., 2011) and cancer screening behavior of individuals (Milkman et al., 2013), but as yet has not been applied to organizational behavior nor tax compliance to the knowledge of the authors. A concrete, specific plan is useful because without one people often fail to act on their motivations (Orbell & Sheeran, 1998), instead preferring to put off unpleasant activities until later because of their present-biased preferences (O'Donoghue & Rabin, 1999). Concrete plans facilitate acting on motivations because they disambiguate the when, where, and how of action. Without a plan, some actors may wish to act on their motivations yet dither because they are unsure under what circumstances that action is necessary or appropriate. Furthermore, simple and self-relevant plans promote goal pursuit by facilitating the processing of goal-relevant information, inhibiting distraction, and encouraging evaluation of the goal as feasible and desirable (Gollwitzer, Kentaro, & Oettingen, 2004). Therefore, in an effort to promote timely tax payment among delinquent organizations, we provided them with simplified, concrete, instructions written in active voice of how, where, and when to pay, thereby testing whether procrastination can explain organizational non-compliance, and whether facilitating implementation intentions is a useful tool for ameliorating this behavior.

3. Field Experiment

3.1 Research Setting and Sample

To test the effectiveness of our implementation intentions intervention on tax compliance, we partnered with the Government of Ontario to run a large-scale field experiment involving organizations that failed to file their Employer Health Tax (EHT) return on time. In the province-wide experiment, we tested an experimental late notice against the standard late notice used by the government, which served as our control. In order to examine whether the intervention we developed had any persistent effects, or whether organizations might habituate to our intervention with repeated exposure, as well as to establish

the replicability of any findings, we conducted a two-wave field experiment in the 2013 and 2014 tax years. In both waves, the control and experimental notices were the same, but random assignment of organizations to each notice was carried out independently both years. The tax that served as the subject of this field experiment (EHT) is a payroll tax on remuneration paid to employees during 2013 and 2014. All organizations with permanent establishments in Ontario and payrolls greater than $400,000[1] were subject to EHT, constituting a tax base of some 85,031 organizations in 2013 and 79,912 organizations in 2014. The majority of these organizations (59.6% in 2013 and 61.7% in 2014) paid this tax by installments rather than by annual return, either by election or because they had payrolls greater than $600,000 and were required to do so. The remaining organizations (34,360 in 2013 and 31,773 in 2014) paid the entirety of this tax by annual return, which is due to be filed on or before March 15 or the following business day each year. Those organizations that paid by installments were also required to file returns to confirm that their installment payments were correct or to reconcile any differences between the installments and the final amount of tax owed. In both years, all organizations were reminded of their requirement to file their annual return via a letter that contained the necessary paperwork, which was mailed to their business address in mid-January. Options for filing the annual return included by mail, at in-person service centers, and via a website.

Those organizations that failed to file an annual return by the due date were subject to financial penalties, interest charges, and collection efforts in an effort to encourage timely tax remittance. On the business day following the tax's due date (March 15), unfiled accounts were charged a penalty of five percent of the amount owing, plus a one percent penalty for each full month delay of payment, as well as a six percent annualized interest charge that was compounded daily. Despite these costs, 6,406 organizations in 2013 and 6,291 in 2014, or about 7.5 and 7.9 percent of the subject population,

[1] The exemption limit was increased to $450,000 for the 2014 tax year. This increased exemption limit and other factors, such as organization attrition, resulted in 5,119 fewer organizations that were subject to the tax that year.

respectively, failed to file a return by the due date. Of these late filers, 6,316 organizations in 2013 and 6,189 in 2014 were included in our analyses because they had not filed by the estimated delivery date of the late notice[2], which served as the independent variable of our experiment. Data on organizations' filing behavior was extracted over 6 months after this late notice was sent to organizations.[3]

3.2 Methodology

Organizations were randomly assigned to receive one of two letters: a control notice, which was the standard late notice sent to delinquent organizations in years past, and a revised late notice, which included our experimental manipulation. The decision to include only two letters in the experiment was made collaboratively with the government partner on account of feasibility constraints and statistical power. It was furthermore decided that assignment to letters would be balanced such that in both waves approximately 40 percent of organizations would receive the control letter and 60 percent the experimental letter, in the hopes of maximizing the predicted benefit of conducting the intervention while preserving empirical rigor of the experiment. This over-assignment to the experimental condition also gave us greater statistical power to test for persistence and consistency of our effects, since organizations included in the experiment represented a minority of the population, and organizations that were included in Wave 1 would not necessarily be included in Wave 2 (i.e., they were only included in both waves if they were sufficiently late filing their returns both years). As displayed in Table 1, comparisons across multiple variables revealed that random assignment successfully created two groups of organizations that were approximately equivalent in terms of the proportion of foreign organizations, years of experience filing this particular tax, and amount of tax owed, as well as the number of organizations that received the

[2] This late notice was posted on 22 and 21 April, for the 2013 and 2014 tax years, respectively. We assume a minimum one-day delivery time, although all our analyses are substantively unchanged by assuming up to a three-day delivery time, which is the postal service's provincial service standard.

[3] Ten days after the late notice letter was posted other collections efforts commenced, including the posting of a second letter and phone calls by collections agents. These additional efforts were held constant across our entire experimental sample.

late notice in error because the government mailing list had not been updated to reflect the fact that they had recently filed or that the organization was not required to file a return. Therefore, although in both waves domestic organizations, Wave 1: b = -23.46, SE = 3.81, $t(6273) = -6.16$, $p < .001$; Wave 2: b = -16.86, SE = 2.58, $t(6100) = -6.53$, $p < .001$, and those with more years' experience filing this tax, Wave 1: b = -.46, SE = .10, $t(6274) = -4.61$, $p < .001$; Wave 2: b = -.12, SE = .07, $t(6100) = -1.91$, $p = .056$, took less time to file after receiving the late notice, due to random assignment these variables cannot account for the findings reported below, do not affect our results when controlled for, and thus are not discussed further.

The major differences between the control and experimental late notices were (1) simplified and concrete language written in active voice, (2) the addition of a deadline (*when*), and (3) step-by-step instructions of *how* and *where* to file a return (see Appendix for copies of the letters). The control letter informed organizations that their tax return had not been received by the March 15 due date, that the return must be filed immediately to prevent further charges and referral to the Collections Branch, and that continued failure to file is a serious offence. In addition to this information, the experimental letter provided organizations with a new deadline of May 2 for the 2013 tax year and May 1 for 2014, which was ten days after the late notice was posted. Deadlines are an effective tool for overcoming procrastination (Ariely & Wertenbroch, 2002), and this deadline was chosen because it marked the initiation of additional collection efforts and because it could be viewed as a more concrete goal than asking someone to pay "immediately" (concrete goals generally produce better performance than abstract goals; Locke & Latham, 2002). The experimental letter also contained numbered and simple step-by-step instructions of how and where to file a return, thus furnishing organizations with the three pieces of information normally used in experiments to instill an implementation mindset (*when*, *how*, and *where*), (Gollwitzer et al., 2004). Note that 97 percent of organizations in Wave 1, and 99.9 percent in Wave 2, had previous filing experience, and thus, should know when, how, and where to file a return. In addition,

this information is readily available on the ministry's website. Additional stylistic differences between the control and experimental letters (simplification through second-person pronouns, active voice, and clearer letter structure) supported the implementation intentions-intervention, and so we argue that this experiment provides an imperfect, but informative, test of the longer-term usefulness of implementation intentions in expediting organizations' payment of taxes.

Insert Table 1 about here

3.3 Results of Wave 1

The results obtained from Wave 1 of the experiment supported our hypothesis that procrastination explains some of organizations' late filing behavior: late notices instilling implementation intentions increased organizations' tax compliance rate. If liquidity constraints alone explained late filing, then our changes to the letter should not affect organizations' ability to pay. As can be seen in Table 1, compared to the control letter the experimental letter significantly accelerated the rate with which organizations filed their annual returns by an average of 4.4 days, b = -4.38, SE = 1.66, $t(6273)$ = -2.65, p = .008, d = -.07.[4] Consistent with our prediction that including a concrete deadline date (rather than the abstract injunction "immediately") would expedite filing behavior, a multinomial regression analysis, with a reference category of organizations that took more than 30 days after the late notice to file their annual returns, revealed that the experimental letter significantly increased the likelihood of filing before the 10-day deadline contained in the late notice, b = 0.44, SE = 0.07, Wald = 37.16, p < .001, OR = 1.54 [95% C.I. 1.34-1.78]. In contrast, during the two 10-day periods immediately afterwards the experimental letter did

[4] The 41 organizations that never filed their returns within the 559 observation days available for Wave 1 of the experiment are excluded from this analysis. Imputing a filing time of 560 days for these organizations does not substantively change the result.

not lead to any significant differences in the likelihood of filing (1-10 days after the deadline: $b = 0.12$, SE = .07, Wald = 2.90, $p = .089$, OR = 1.13 [95% C.I. 0.98-1.29]; 10-20 days after the deadline: $b = -0.12$, SE = .07, Wald = 0.03, $p = .865$, OR = 0.99 [95% C.I. 0.86-1.14]). The fact that those organizations that received the experimental letter did not file at a faster rate than the control group after May 2 can likely be explained by the start of additional collection efforts, which targeted all organizations remaining in default. Nevertheless, although these additional collection efforts gradually eroded the effect of the experimental letter, as can be seen from Figure 1, it took over 10 weeks before the initial impact of the experimental letter was no longer significant relative to the control letter.

To ascertain the financial value of our experimental letter in Wave 1, we determined the additional amount remitted to the government by the deadline set in the late notices, which was when additional collections efforts began. These additional collections efforts, including subsequent letters, staffing of call centers, and procurement of external collections agencies, are estimated by the government to cost approximately two cents for every dollar collected. We find that on average each control letter resulted in $149.88 of remitted taxes prior to the start of additional collections efforts, while each experimental letter garnered $241.32, a marginally significant difference ($b = 91.45$, SE = 40.97, $t(5197) = 2.32$, $p = .026$, $d = .07$).[5,6] This is an increase of 61 percent in tax remittance, resulting in an additional $288,335.54 being collected within ten days of mailing the experimental letter, thereby saving the government approximately $5,766.71 in collections cost during Wave 1 of the experiment. Had the experimental letter been sent to all 5,199 organizations that were not owed a tax refund and were late filing their annual return, the government should have expected to receive an additional $475,438.15 in remittance prior to initiating additional collections measures, thereby saving some $9,508.76.

[5] 1,117 organizations which had overpaid their taxes by installments during the year 2013, and thus were owed refunds with their annual return, are excluded from these analyses.

[6] To correct for significant positive skewness in the amount of tax collected, $z_{skewness} = 97.71$, a log transformation was performed, which did alter the significance of our results.

In order to further validate our results by replicating them in the same population, and to test for the longer-term effects of our intervention, we conducted a second wave of the same experiment for the subsequent (2014) tax year. Due to the fact that random assignment in both waves was carried out independently, this two-wave design enabled us to examine whether the effect of our letter had any year-over-year persistence, and whether the effect of the experimental letter remained consistent after repeated exposure.

Insert Figure 1 about here

3.4 Results of Wave 2

For the 2014 tax year, 79,912 organizations were due to file the payroll tax, which served as the setting for our study; 6,189 of these organizations were sufficiently late filing their taxes that year that they were included in the second wave of our experiment. When compared to their previously punctual peers, organizations that were late filers in 2013, and therefore were involved in Wave 1 of our field experiment (i.e. they received either the control or experimental late notice letter), were significantly more likely to also be late filing their 2014 tax return, $\chi^2(1, N = 79912) = 2012.31$, $p < .001$, OR = 3.43 [95% C.I. 3.24-3.63], and to be so late as to receive a 2014 late notice letter, thereby including them in Wave 2 of our field experiment, $\chi^2(1, N = 79912) = 3402.02$, $p < .001$, OR = 5.94 [95% C.I. 5.56-6.35]. Thus, of the 6,189 organizations involved in Wave 2 of our field experiment, approximately one quarter of them (n = 1,506) had also participated in Wave 1.

Moreover, because the 2013 control and experimental letters did not have any differential effect on the likelihood that an organization would miss the subsequent, 2014 tax deadline, $\chi^2(1, N = 5293) = 0.00$, $p = .991$, OR = 1.00 [95% C.I. 0.90-1.12], nor its likelihood of being included in Wave 2, $\chi^2(1, N =$

5293) = 0.07, p = .793, OR = 1.02 [95% C.I. 0.90-1.15], the proportion of organizations in Wave 2 which had received the control (n = 595) and experimental letters (n = 911) in Wave 1 remained relatively unchanged from the original 40/60 split. While this lack of persistence in the effect of the 2013 experimental letter on timely filing behavior in 2014 is perhaps slightly disappointing, it is not surprising considering the year-long delay. Importantly, however, this lack of persistence also indicates that Wave 2 of our field experiment is free of selection bias that might have been produced by condition assignment in Wave 1. Moreover, independent random assignment in both waves, such that there is no association between which letter an organization received in 2013 and 2014, $\chi^2(1, N = 1506) = 0.38$, p = .538, OR = 0.94 [95% C.I. 0.76-1.16], provides a relatively unconfounded and well-powered test of whether the effect of our experimental letter is moderated by repeat exposure, which we discuss below. Finally, the lack of persistence of timely filing behavior, together with the fact that we observed our effect among firms with previous filing experience, suggests that timing is critical and implementation intentions must be provided at key points during the decision making process in order to be maximally effective.

As in Wave 1, the experimental letter in Wave 2 significantly accelerated the rate with which organizations filed their annual returns by an average of 5.3 days, b = -5.25, SE = 1.11, $t(6100) = -4.72$, $p < .001$, d = -0.12.[7] Using a multinomial regression we observed the same pattern of accelerated filing behavior prior to the 10-day deadline contained in the letter, b = .49, SE = 0.07, Wald = 54.12, $p < .001$, OR = 1.63 [95% C.I. 1.43-1.86]. Similarly, no differences were observed in filing behavior over the two 10-day periods immediately afterwards (1-10 days after the deadline: b = 0.17, SE = .08, Wald = 4.91, p = .027, OR = 1.18 [95% C.I. 1.02-1.37]; 10-20 days after the deadline: b = 0.06, SE = .08, Wald = 0.58, p = .447, OR = 1.06 [95% C.I. 0.91-1.23]). Again, although additional collection efforts commencing after the deadline contained in the letter gradually eroded the effect of the experimental letter, as can be seen in

[7] The 87 organizations that never filed their returns within the 195 observation days available for Wave 2 of the experiment are excluded from this analysis. Imputing a filing time of 196 days for these organizations does not substantively change the result.

Figure 2, more than 16 weeks passed before the initial impact of the experimental letter was no longer significant relative to the control.

Insert Figure 2 about here

In addition to replicating our original results, this two-wave design also enables us to test the hypothesis that the effect of the experimental letter diminishes with repeated exposure. It could be, for example, that organizational decision makers are responding to the novelty of the letter, and that its effect will be less pronounced when it is encountered more than once. To explore this possibility, we submitted our results to an ordinary least squares regression in which the number of days taken by organizations to file their 2014 annual tax return was regressed upon Wave 2 letter assignment (coded 0 = control; 1 = experimental letter), whether or not the organization had previously received the experimental letter in Wave 1 (coded 0 = had never received the experimental letter previously, including both those organizations that had been in the control condition in 2013 and those that had not received any late notice letter in 2013; 1 = had received the experimental letter in Wave 1), and their interaction term. This analysis revealed significant main effects for receiving the experimental letter in Wave 2, b = -4.65, SE = 1.21, $t(6098) = -3.85$, $p < .001$, $d = -.10$, and receiving the experimental letter the previous year, $b = 9.92$, SE = 2.39, $t(6098) = 4.15$, $p < .001$, $d = .11$, but the interaction term was not significant, $b = -4.14$, SE = 3.07, $t(6098) = -1.35$, $p = .178$, $d = -.03$. This suggests that repeated exposure to the experimental letter had no impact on its effectiveness, and if anything, there is a trend towards the experimental letter being more effective for those organizations previously exposed to it. The negative effect of exposure to the 2013 experimental letter on 2014 filing behavior can likely be explained by the fact that those organizations that received a collection letter in Wave 1, due to their tardiness that year, were much

slower filing their returns in response to either late notice in Wave 2 than were those that had avoided the collections process in the previous year, b = 7.98, SE = 1.27, t(6100) = 6.30, p < .001, d = .16. Indeed, as can be seen in regression Model 2, displayed in Table 2, and as is graphically depicted in Figure 3, when controlling for those organizations that were late filing in Wave 1, this negative effect of the Wave 1 experimental letter on 2014 filings disappears, although the positive effect of the 2014 letter remains unchanged. As well, as can be seen in Table 2 Model 3, when an organization's on-time collection status in Wave 1 is interacted with their Wave 2 experimental letter assignment in a similar regression model, there is no evidence that the main effect of the experimental letter, b = -5.92, SE = 1.27, t(6098) = -4.65, p < .001, d = .12, is moderated by an organization's previous tardiness, b = 2.48, SE = 2.58, t(6098) = 0.96, p = .337, d = .02, suggesting that the experimental letter is equally effective for both those organizations that were not late the previous year and those that were late two years in a row.

Insert Table 2 about here

Insert Figure 3 about here

As a result of increasing tax-filing compliance, the experimental letter also increased the amount of taxes remitted during Wave 2 prior to the commencement of additional collection efforts. We find that on average each control letter resulted in $189.75 of remitted taxes prior to the start of additional collections efforts, while each experimental letter garnered $291.70, a significant difference (b = 101.95,

SE = 38.00, $t(5066) = 2.68$, $p = .007$, $d = .08$).[8,9] Although the size of the experimental letter's effect is statistically small, in the context of this tax's scale, its impact is not insignificant. This is an increase of 54 percent in tax remittance, resulting in an additional $309,112.40 being collected within ten days of mailing the experimental letter, thereby saving the government approximately $6,182.25 in collections cost during Wave 2 of the experiment. Had the experimental letter been sent to all 5,066 organizations that were not owed a tax refund and were late filing their annual return, the government should have expected to receive an additional $516,478.70 in remittance prior to initiating additional collections measures, thereby saving some $10,329.57. Given that the changes in the experimental letter were cost-free to implement, and that we found no evidence that repeated exposure to the letter diminished its effect, we are encouraged to know that the government plans to implement the experimental letter as standard going forward.

4. Discussion

This study demonstrates that psychologically informed, costless changes to tax-collection letters can have large benefits for taxation. Specifically, we examined tax-paying behavior among a population of organizations that were late filing their annual returns and found that implementation intentions are a viable tool for overcoming payment procrastination. Our multi-wave experiment makes several valuable contributions. First, it applies a new theoretical perspective to a relatively understudied component to of tax compliance. Drawing on the insight that people often fail to act on their motivations, especially when the immediate consequence of that action is costly or undesirable, we argue that tax payment procrastination might not only result from the lack of sufficient motivation, but from the absence of a concrete plan of how to act on those motivations. Importantly, providing a plan with an explicit deadline

[8] 1,122 organizations which had overpaid their taxes by installments during the year 2014, and thus were owed refunds with their annual return, are excluded from these analyses.
[9] To correct for significant positive skewness in the amount of tax collected, $z_{skewness} = 229.74$, a log transformation was performed, which did not substantively alter our results.

date and specific, active instructions for its implementation appears to assist taxpayers in overcoming their procrastination.

Recall that the vast majority of firms in our study (over 97%) had previously filed their EHT return, and therefore had experience with when, where and how to file their return. Moreover, this information is readily available on the government's webpage. Therefore, it seems unlikely that our results were driven simply by the inclusion of this information in the letter, but rather, by the concrete date and actionable plan provided. Unfortunately, our experiment was limited to only testing two versions of the collection letter because of the feasibility constraints of our government partner and statistical power considerations. This means that we cannot disentangle the individual contributions of all stylistic differences between the letters. This limitation is further compounded by the fact that we do not have any mediating variables that would enable us to measure the psychological variables we argue underlie our effects. Nevertheless, given the substantive similarity between the content of the control and experimental letters, we contend that the most impactful difference between them is the provision of concrete active instructions of when, where, and how to act on extant motivations to file an organization's taxes.

Second, by employing a multi-wave design, this experiment examines the persistence and consistency of our effects in the medium term. Our study found no evidence that receiving the experimental letter impacted firms' likelihood of filing in a timely fashion the following year. These results demonstrate the importance of timing for this behavioral intervention, suggesting that implementation intentions may be most effective when they are instilled at the point in time when one is making a decision. Importantly, although our intervention did not appear to have effects that persisted for as long as 12 months, its effects were consistent across repeated exposures. This suggests that firms did not habituate to our manipulation. The fact that our experimental letter was equally effective after two exposures is critically important when assessing the policy implications and longer-term impact of such a behavioral intervention.

Third, this experiment is among the first to test the application of behavioral interventions designed for individuals in the realm of organizational behavior. In this instance, we find that despite the documented differences in organizational decision making, interventions designed to instill implementation intentions can be just as effective for organizations as they have been shown to be for individuals. We find that firms presented with our experimental letter were significantly more likely to file their taxes before a set deadline, and the effect of the letter remained significant for up to 16 weeks afterwards despite collections efforts beginning merely 10 days after. Critically, any early remittance significantly saves on these additional collection costs (as well as costs to the organizations).

Together, not only did this experiment produce behavioral data, which advances our understanding of organizational tax-compliance behavior, it also advanced the goals of the government and the public it serves: it saved money in terms of collection costs and increased collected taxes at virtually no cost. Moreover, our experiment provides strong evidence for a readily applicable tool that promotes efficient tax collection without added enforcement or penalties.

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Appendix

ORIGINAL LETTER
Letter was issued on 22-Apr-2014 and 21-Apr-2015 in Waves 1 and 2, respectively

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Employer Health Tax
Requirement to File

Employer Health Tax: XXXX

If your Employer Health Tax Return has been filed, please disregard this notice.

We have not received your Employer Health Tax Annual Return(s) for the following calendar year(s):

31-Dec-2013

Your return was due on or before 15-Mar-2014. Please complete and file your return with full payment **immediately**. Interest and penalty charges will apply.

Your return must be filed **immediately** with full payment in order to prevent further interest charges to you or referral to the Collections Branch. ***Continued failure to file the required return is a serious offence. If arrangements are not made to correct this situation, the ministry may file an estimated return including applicable penalties and interest for each period that is in default.***

If you have any questions or require additional information, please visit our website or call the Ministry of Finance at the number listed below.

Enquiries 1 866 ONT-TAXS 1 866 668-8297 Fax 1 866 888-3850 Teletypewriter (TTY) 1 800 263-7776 Internet **ontario.ca/finance**

EXPERIMENTAL LETTER
Letter was issued on 22-Apr-2014 and 21-Apr-2015 in Waves 1 and 2, respectively

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Your Employer Health Tax Annual Return Is Overdue!
You must file immediately.

Employer Health Tax: XXXX

We have not received your Employer Health Tax Annual Return(s) for the following calendar year(s):

31-Dec-2013

Your return was due on or before 15-Mar-2014.

You must file your return(s) immediately with full payment to avoid further interest and penalty charges.

If you fail to deliver your return(s) by 2-May-2014, the Ministry of Finance will refer you to collections. This could have **escalating legal consequences as failure to file a return is a serious offence.**

Here is what you need to do now and definitely before 2-May-2014:

1) File and pay your return(s) in full immediately:
    a. By mail to Ministry of Finance, [Street Name, PO Box, City, State, Zip Code]
    b. In person at selected [Government] centers, see [website address], and click "Visit a Centre"
    c. If you already registered with [website name], log on with your [proprietary key name] at [website address]
2) Keep a copy for your records.

If you have filed your Employer Health Tax return, please disregard this notice.

If you have any questions or require additional information, please visit our website at [website address] or call the Ministry of Finance at [phone number].

Enquiries 1 866 ONT-TAXS 1 866 668-8297 Fax 1 866 888-3850 Teletypewriter (TTY) 1 800 263-7776 Internet **ontario.ca/finance**

Variable	2013 (n = 6,316) Control Letter	Experimental Letter	Statistic	*p*	2014 (n = 6,189) Control Letter	Experimental Letter	Statistic	*p*
Organizations included in analyses	2515	3801			2,471	3,718		
Exclusion Criteria and Covariates								
Organizations filing before late notice posted (excluded from analyses)[a]	87 (3.34%)	128 (3.26%)	$X^2(1) = 0.04$	.849	80 (3.13%)	90 (2.36%)	$X^2(1) = 3.50$	.061
Organizations not required to file an annual return (excluded from analyses)[a]	232 (8.19%)	319 (7.51%)	$X^2(1) = 1.09$	.297	225 (8.11%)	350 (8.42%)	$X^2(1) = 0.21$	.644
Foreign organizations	117 (4.65%)	181 (4.76)	$X^2(1) = 0.41$	.840	119 (4.82%)	173 (4.65%)	$X^2(1) = 0.09$	.767
Mean years filing EHT	10.59 (8.13)	10.61 (8.17)	$t(6314) = 0.10$	.921	10.94 (8.34)	11.06 (8.46)	$t(6187) = 0.56$	.576
Mean declared annual tax	$15,462.27 ($49,369)	$45,647.66 ($1,669,399)	$t(6314) = 0.91$	.365[c]	$17,358.28 ($70,710)	$22,585.40 ($163,421)	$t(5455)^b = 1.50$	.085[c]
Organizations claiming overpayment refunds	469 (18.6%)	648 (17.0%)	$X^2(1) = 2.66$	.103	435 (17.6%)	686 (18.5%)	$X^2(1) = 0.717$	.397
Dependent Variables								
Mean days taken to file after first late notice delivered	51.30 (66.19)	46.92 (62.87)	$t(5150)^b = -2.62$	.008[c]	44.51 (43.54)	39.26 (41.89)	$t(5075)^b = -4.72$	<.001[c]
Returns filed within 10-day deadline of first late notice	406 (16.1%)	855 (22.5%)	$X^2(1) = 38.21$	<.001	511 (20.7%)	1,072 (28.8%)	$X^2(1) = 51.83$	<.001
Mean tax paid before deadline[e]	$149.88 ($857)	$241.32 ($1,719)	$t(4918)^b = 2.54$	.011	$189.75 ($1,151)	$291.70 ($1,432)	$t(4905)^b = 2.80$	.005

Table 1. Key statistics from Waves 1 and 2.

Numbers in brackets are either percentages, standard deviations, or degrees of freedom.

[a] These organizations received their late notice in error. Although they are not included in the analysis, their assignment to control and intervention notices are shown here as evidence of successful randomization.

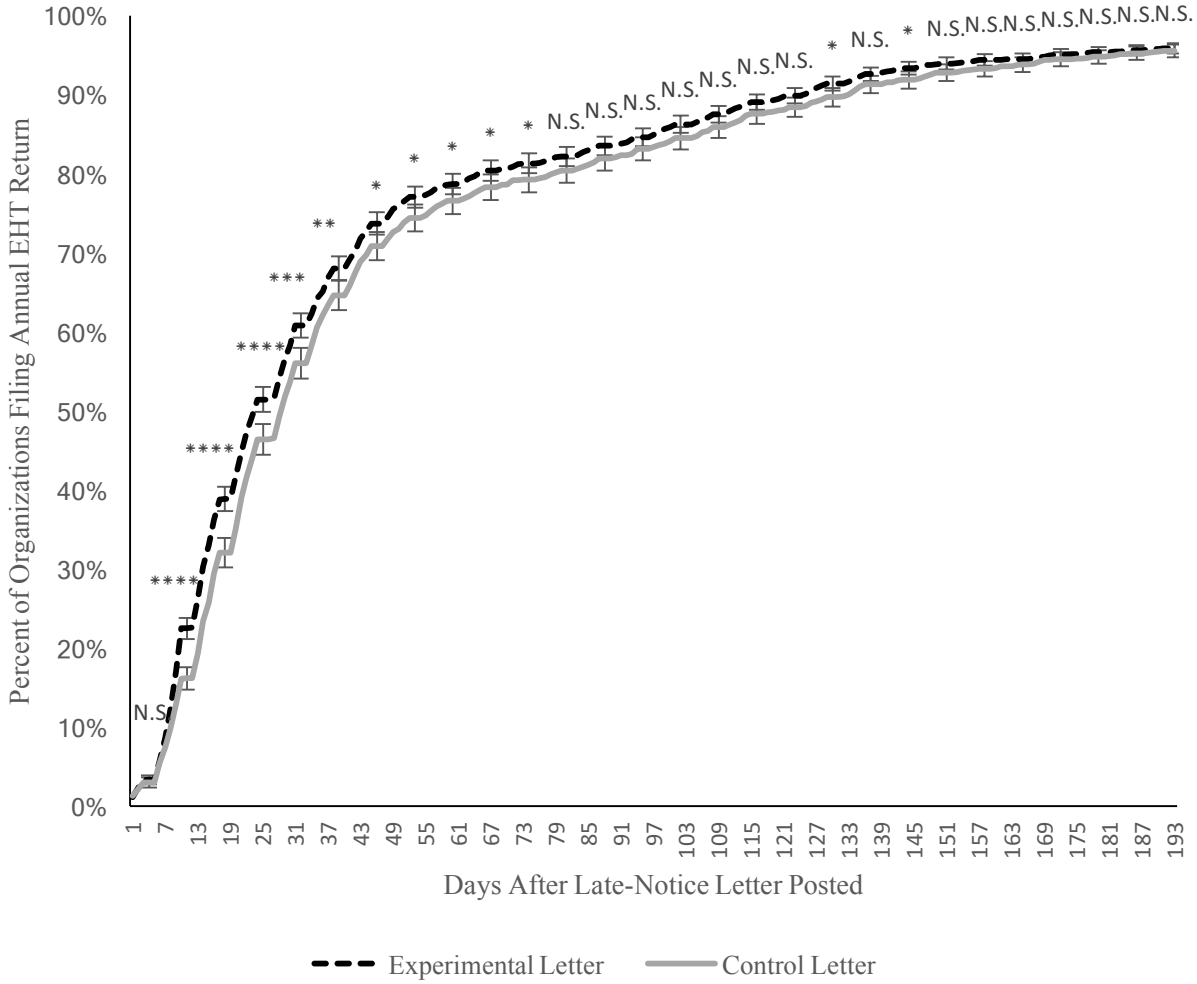
[b] Where homogeneity of variances was violated, Welch-Satterthwaite adjustments to the degrees of freedom were made, although in no cases did this alter the significance of the results.

[c] Logarithmic transformations intended to address significant positive skew did not alter the significance of these results.

[d] Negative values indicate payments owed to the employer by the government as the result of overpayment of taxes through previous installments that tax year.

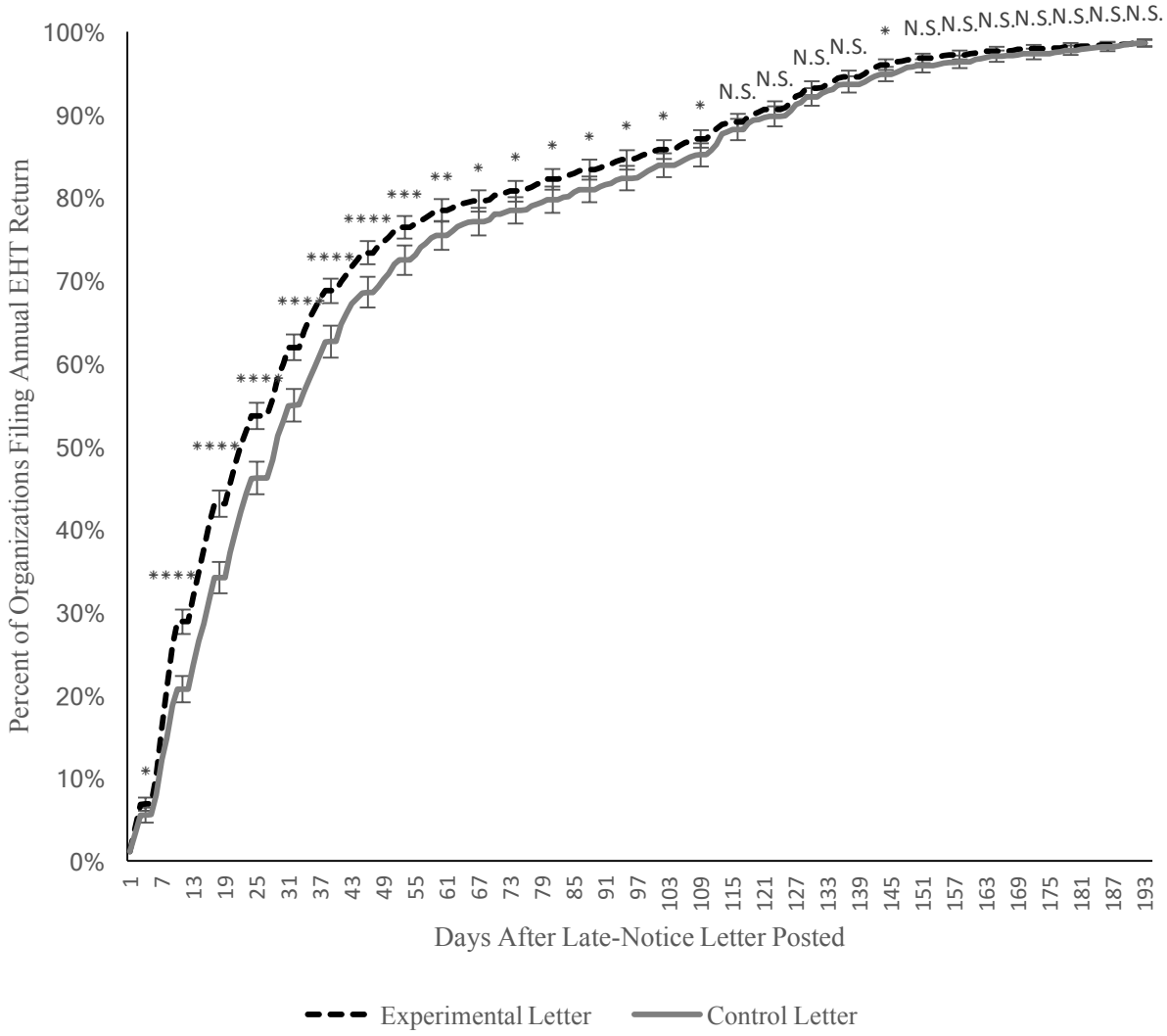
[e] "Mean tax paid before deadline" was calculated for firms that did not claim overpayment refunds, as those firms owed no taxes.

Figure 1. Time series of organizations in Wave 1 filing their 2013 annual returns by letter assignment.



Error bars represent 95 percent confidence intervals calculated each weekend when there was little to no processing of returns.
N.S. $p \geq .05$
* $p < .05$
** $p < .01$
*** $p < .001$

Figure 2. Time series of organizations in Wave 2 filing their 2014 annual return by letter assignment.



Error bars represent 95 percent confidence intervals calculated each weekend when there was little to no processing of returns.
N.S. $p \geq .05$
* $p < .05$
** $p < .01$
*** $p < .001$

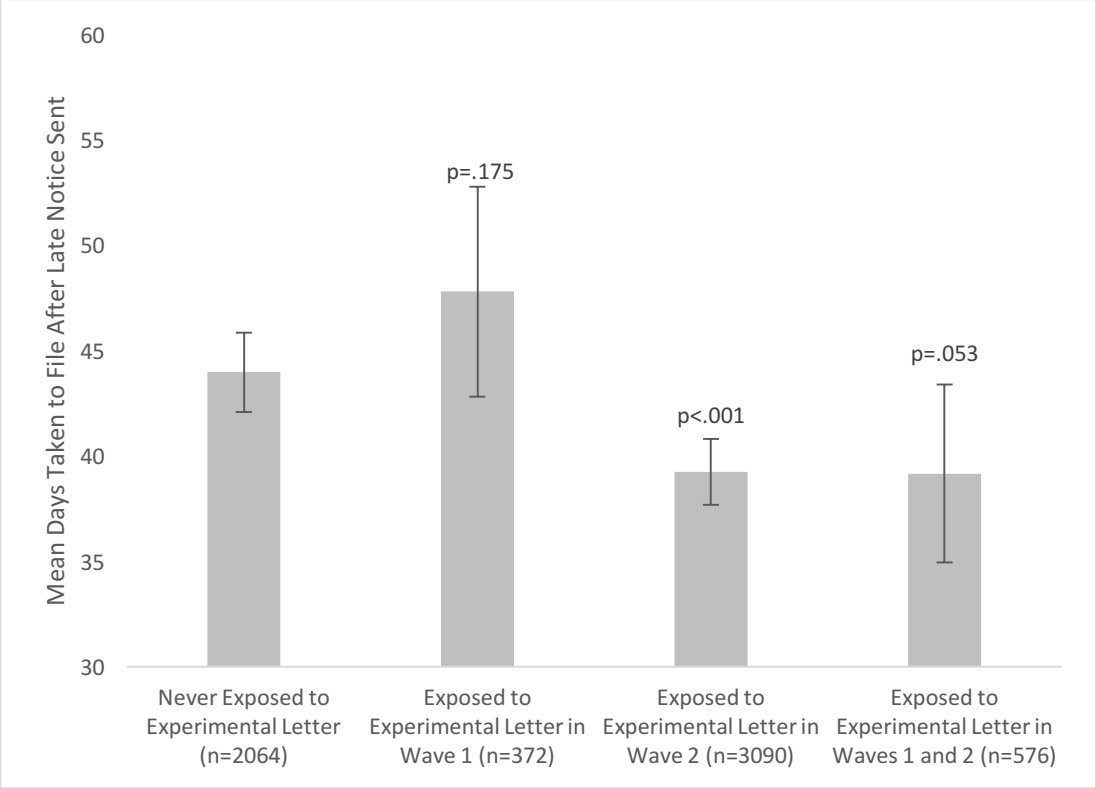
Table 2. Results of Ordinary Least Squares regression analyses testing consistency across repeated exposures

Variables	Number of days taken to file 2014 annual tax return after receipt of Wave 2 late notice		
	Model 1	Model 2	Model 3
Intercept	**43.00*** **	**42.21*** **	**42.95*** **
Wave 2 Experimental Letter	**-4.65*** **		
		-4.72* **	**-5.92*** **
	(1.21)		
		(1.21)	(1.27)
Wave 1 Experimental Letter	**9.92*** **	3.84	
	(2.39)		
		(2.83)	
Wave 2 X Wave 1 Experimental Letter	-4.14	-3.92	
	(3.07)	(3.07)	
Included in Wave 1 (Late Filing 2013 Taxes)		**7.16*** **	**6.53*** **
		(1.78)	(2.02)
Included in Wave 1 X Wave 2 Experimental Letter			2.48
			(2.59)

Unstandardized coefficients are presented with standard errors in parentheses. N = 6,102 due to 87 organizations which did not file being excluded on account of the days they took to file their return being unknown or potentially infinite.

† $p < .10$
* $p < .05$
** $p < .01$
*** $p < .001$

Figure 3. Effect of letter assignment on time taken for organizations to file their annual return in Wave 2.



Results controlling for having received a late notice the previous tax year.
Error bars represent 95% confidence intervals.
P-values are relative to those organization never exposed to the experimental letter in Waves 1 or 2.