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Commitment to Pay Taxes:

Results from a Field and Laboratory Experiments

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Abstract

A tax authority's ability to successfully collect taxes depends on its relationship with the taxpayers as well as their commitment to contribute to the common good. In this paper, we examine the effect of promises on tax compliance aimed at fostering taxpayer commitment. First, in a field experiment, we investigate whether tax compliance changes when taxpayers make a formal promise to pay their taxes on time with compliance rewarded by entry into a lottery for either a financial or nonfinancial (in-kind) reward. We then complement this analysis with a laboratory experiment in which we measure the effect of promises in the different compliance domain of tax honesty and contrast the effect of a pure promise to pay with schemes that pair the promise with a reward offer. We find that taxpayers with a history of compliance or high scores on tax morale are more likely to make the promise, but solely offering the possibility to make a promise does not lead to a change in compliance behavior. Whether or not compliance improves depends on the type of reward to which the promise is linked. In our experimental analyses, for example, compliance only increases if the reward for promise fulfillment is nonfinancial.

Keywords: Tax compliance, field experiment, commitment, promise, supportive incentives, psychological tax contract

JEL classification: H26, C93, C91, A13.

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1. Introduction

Research on tax compliance convincingly argues that successful tax collection is not only the exercise of power (Alm et al. 2010, Kirchler 2007, Torgler 2007) but reliant on a mixture of "carrot" and "stick" approaches. Early models of tax compliance, such as that of Allingham and Sandmo (1972), follow Becker's (1968) theory of crime, which emphasizes deterrence-elicited fear (stick), including the probability of detection (control intensity) and punishment for noncompliance (fines). More recently, researchers and tax administrations have begun to place more emphasis on "carrots," moving toward a "service" rather than an "enforcement" paradigm and thus a "kinder and gentler" approach (Alm and Torgler 2011, p. 635). In particular, a citizen's consent to pay taxes may reflect identification with the tax authority's objectives (Boulding 1981), a relation that Braithwaite (2001) characterizes in terms of five motivational postures or belief and value sets: (a) commitment, (b) capitulation, (c) resistance, (d) disengagement, and (e) game playing. Our study takes a more in-depth look at the first motivational posture, commitment.

The literature offers many arguments as to why loyalty is sensitive to external influences (Feld 1997, Torgler 2007, 2006, 2005). For instance, it often explains noncompliance in terms of perceived disrespectful treatment by the tax administration (Kirchler 2007, Feld and Frey 2002). Taxpayers may then *react* to the tax administration's behavior in such way that brings exchange relationships and reciprocity to the fore. As regards commitment within these relationships, because it "reflects beliefs about the desirability of a tax system and feelings of moral obligation to act in the interest of the collective and pay one's tax with good will" (Braithwaite 2001, p. 6), it has several different dimensions. As a result, far too little is yet understood about how to enhance *pro-active* commitment to taxpaying.

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We begin our investigation with a field experiment exploring whether pre-commitment in the form of a specific promise can increase tax compliance. According to psychological commitment theory (Cialdini 1989, Kiesler 1971, Festinger 1957), a promise has a binding function because of an individual's need to behave consistently. In our setting, we conjecture that the promise strengthens the psychological tax contract between the taxpayer and the tax authority (Feld and Frey 2007, Feld et al. 2006) and emphasizes the moral obligation to comply with tax laws. Thanks to support from a Swiss local tax authority, we were able to conduct this experiment in a field setting that offers a different perspective from laboratory experiments. More specifically, in our treatment groups, taxpayers have the option of promising to pay their taxes on time. Those who make the promise and subsequently comply are entered into a lottery with the chance of winning either a financial or nonfinancial reward. In additional treatments, these rewards are offered only in response to compliance (i.e., without the possibility of the formal promise), allowing us to disentangle a pure reward effect from the commitment effect.

This field experiment assessed behavioral changes in pretax payments[5] during the 2013 financial year by over 2,000 taxpayers in Trimbach, a Swiss municipality in which pretax payments are compulsory. We manipulated the treatment using a letter to all taxpayers that included a reminder about the three pretax installment due dates. In the treatment groups, this letter stipulated that those who paid their pre-taxes on time would receive a reward. In the promise treatment, a postcard accompanied the letter on which the taxpayer could promise to pay

[5] The total of tax payments to the municipality include the local income and wealth tax (Gemeindesteuer) plus the church tax (Kirchensteuer) plus the fire brigade tax (Feuerwehrsteuer). The tax amount for the municipality is based on cantonal income and wealth taxes. The municipality levies 104% of the amount charged by the canton, but this rate can vary according to the municipality's outlays for the following year. The tax is progressive. For example, in 2013 a person with a taxable income of 50,000 CHF and wealth of 150,000 CHF had to pay a total of 8,036 CHF in taxes (without church taxes), 4,124 CHF of this amount paid as municipality tax to the local community. In mid-February (around the 15th), taxpayers receive an invoice declaring their tax liability for the current year, which is estimated based on previous years. This tax amount must be paid in three instalments throughout the year (at the end of March, June, and November 2013). For the Swiss tax system see also Feld (2000) or Feld and Kirchgässner (2003).

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all rates on time. This inclusion introduced a novel element that allowed us to observe the consequences of participants being held to a moral commitment; namely, a promise of compliance. Because this field experiment alone did not permit identification of the promise's pure effect, we complemented it with a laboratory experiment that offered the "extra insights" afforded by "applying the full spectrum of approaches in trying to answer a single question" (Levitt and List, 2009, p. 10).

The paper proceeds as follows: Section 2 offers a brief overview of psychological commitment theory and its applications. Section 3 describes the experimental setting and design, and outlines the treatment selection. Section 4 reports and critically reflects on our main results from the field experiment. Section 5 describes the laboratory experiment and its findings. Section 6 concludes the paper by summarizing the insights, discussing the differences between the laboratory and field experiment, and suggesting directions for future research.

2. Promises as a Commitment Device

Economists are increasingly interested in the relevance of promises, which are usually made with the intent of influencing the beliefs of an interaction partner and creating the trust to make an exchange reliable. Empirical studies confirm the efficacy of such messages, especially in settings characterized by anonymous one-shot interactions. By changing the interaction partners' expectations, promises improve coordination between actors. Because the promisors assume that the receivers will take their message for granted, they live up to their word even when doing so means foregoing material benefits (Hurkens and Kartik 2009, Bicchieri and Lev-On 2007, Charness and Dufwenberg 2006, Ellingsen and Johannesson 2004, Kerr and Kaufman-Gilliland 1994, Ostrom et al. 1992). Hence, the second effect of promises occurs via changed beliefs, a

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concept related to expectation-based guilt aversion (Charness and Dufwenberg 2006) and the fact that individuals feel guilty about letting others down. Put simply, a promise raises others' expectations, so promisors want to live up to their word to avoid inner conflict. Hence, cognitive dissonance theory (Festinger 1957) interprets promise-keeping in terms of an inner urge for consistency and avoidance of the discomfort that comes from behaving against stated intentions. Behavioral economists, in contrast, suggest that individuals keep promises because of a preference for keeping one's word (Ismayilov and Potters 2016, Ellingsen et al. 2010, Vanberg 2008, Ellingsen and Johannesson 2004) or a desire to conform to the social norm of truth-telling (Binmore 2006). Thus, once a promise is made, the probability of keeping it increases. Yet the fact that most of these findings are generated in laboratory settings raises questions about their external validity.[6] Our field experiment examines how promises work in a natural setting by observing real citizens in their actual taxpaying routine. The fact that these taxpayers are unaware of their participation reduces the risk of an experimental demand effect.[7]

In general, tax compliance can be characterized as a principal-agent problem (Andreoni et al. 1998), a setting analogous to the relationship between employers and employees and thus to the question of how to maintain employee motivation. In any principal-agent relationship, it is essential that rewards be perceived as acknowledgment for good work and not as compensation in order to avoid undermining self-determination and intrinsic motivation (Frey 1997, Deci 1971). In our setting, the willingness to make a promise is rewarded by the possibility of winning a prize for full compliance. We then compare this situation to a treatment that offers a reward

[6] Belot et al. (2010) is an exception in this regard. Using data from a television game show, the authors provide evidence for the external validity of promises as an effective coordination device. In their study, 50% of the players were more willing to cooperate when the interaction partner voluntarily made a promise to share. When the promise was elicited by the show's presenter, however, the promise had no effect.

[7] See Feld et al. (2006) for a discussion of field experiments in the area of tax compliance. There is an increasing trend towards using this method to better understand tax compliance (for an overview, see Hallsworth 2014).

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with no promise required. In the latter case, the reward's function is to signal that a good job has been done. We communicate the possibility of a reward *ex ante* to see whether rewards promote compliance.

The underlying aim of such an incentive is to be supportive and improve citizens' attitudes toward tax payment by acknowledging compliance. According to anecdotal evidence, some tax agencies are seriously considering the implementation of such supportive incentives. For example, in 2005, Uganda's Revenue Authority introduced a Taxpayers' Appreciation Day on which it presents the so-called Vantage Award to compliant taxpayers from different regions, dubbed Taxation's Rising Stars.[8] Asian countries have also implemented reward systems, with Japan offering the opportunity to take a picture with the Emperor, and the Philippines placing the names of compliant taxpayers into a lottery (Feld et al. 2006). Experimental studies have taken up these ideas and, in the controlled setting of the laboratory, have studied how rewards (announced *ex ante*) affect compliance (Carillo et al. 2017, Brockmann et al. 2016, Fochmann and Kroll 2016, Bazart and Pickhard 2011, Torgler 2003, Alm et al. 1992). The findings are, however, not conclusive. When a reward is at stake, extreme compliance behavior (evading all or nothing) becomes dominant (Kastlunger et al. 2011, Alm et al. 1992) and overall compliance is difficult to predict. In particular, it appears to be crucial how individuals perceive this reward. For example, in a recent field experiment, Dwenger et al. (2016) demonstrate that the reward's effect varies strongly between intrinsically and extrinsically motivated taxpayers. Extrinsically motivated taxpayers, who previously evaded the local church tax under study may interpret the reward's introduction as a sign that paying this tax is voluntary and/or only weakly enforced. Carillo et al. (2017) show that compliance rewards can have a positive and persistent effect.

[8] See http://www.observer.ug/component/content/article?id=27845:kenyas-chris-kirubi-to-grace-ura-taxpayers-awards.

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Their study is based on an Argentinean municipality that organized a lottery for taxpayers who paid their property tax in which winners received a pavement construction or renovation. The results show that the compliance of lottery winners and their neighbors increased after receiving the sidewalk.

3. Field Experiment

Background

Of the three key aspects of tax compliance – accurate reporting, timely filing, and timely payment (Slemrod et al. 2001) – our field study focuses on the third, thereby avoiding such measurement errors as those stemming from difficulties controlling for auditing process quality. The payment data are taken from the tax administration database, which records the total tax amount owed and the amount and date of all payments. Although the information in the data set is anonymous, individual taxpayers can be matched over the years by their addresses and identification numbers. We therefore do not only know what payments were made in the treatment year but also what sums were paid in the five previous years (2008–2012), allowing us to measure the extent of prior taxpayer compliance.

Switzerland provides an interesting setting for field experiments on tax compliance because Swiss municipalities like Trimbach, the setting for our study, are fully responsible for regulating and collecting taxes. All taxes are collected as pretaxes in mid-February of each year, when taxpayers receive an invoice asking them to declare their tax liability for the current year based on taxes in the previous year. The assessed taxes must be paid at the end of March, June, and November. In the past, Trimbach has had to deal with pretax arrears of around 20% of taxes

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owed, making it difficult for the municipality to budget its expenses over the course of the year.[9] To better predict pretax funds, the local administrations announced at the end of 2012 that from 2013 onward, those who miss pretax payments will be dunned (see Appendix Figure A1 for the timeline). All taxpayers were informed about this institutional change with the invoice sent in mid-February.

Methodological Design

Our experiment was conducted using all Trimbach taxpayers, excluding firms and taxpayers who owed no taxes in the previous year. Our sample comprised 2,201 taxpayers who were randomly assigned to four treatment groups and one control group. By the end of the experiment, a further 244 taxpayers had been dropped because of either migration or change in civil status.[10] Shortly *before* receiving the tax invoice for the current year, all private taxpayers received a letter that did not only remind them of the payment due dates but also introduced the incentive for the treatment groups.[11] This simply worded letter was sent out by the tax authority a week before the tax invoices were dispatched.[12] In case of taxpayer questions about the experiment, all tax administration employees and local council members were also provided with a list of standardized answers.

[9] Around 2.5 million CHF were missing in 2012. When taxpayers missed their payment of pre-taxes during the current year, a default interest rate was charged when the tax debt was defrayed in the final accounting process. The default interest rate is based on what the canton charges for default. In 2013 this was 3%. Interest on an ordinary Swiss savings account was around 2% in 2013. Thus, it was not rational nor financially beneficial to delay the payment of the pre-taxes. This is particularly the case since additional dunning costs were introduced in 2013. Torgler (2013) explored under-reporting and over-declaration in the same municipality based on 2001 data. The share of noncompliance was small, namely only 2.3% of the mean net taxable income. Regarding wealth the compliance share was 78% and for deduction 94%. The frequency of corrections done by the tax administration was, however, higher (56% of cases for income, 65% for deductions, and 20% for wealth reporting).

[10] Additionally excluded are two taxpayers with exceptionally high tax debt.

[11] The variation between the CONTROL and treatment groups can be seen in the second paragraph of the reminder letter, which introduces the reward and promise option. Figure A2 and A3 in the Appendix show the respective letters for the CONTROL and WELL PRO group.

[12] Treatment letters and tax invoices had to be sent out separately since the dispatch of the tax invoices is standardized and it was not possible to realize the randomized allocation in treatment groups within this procedure.

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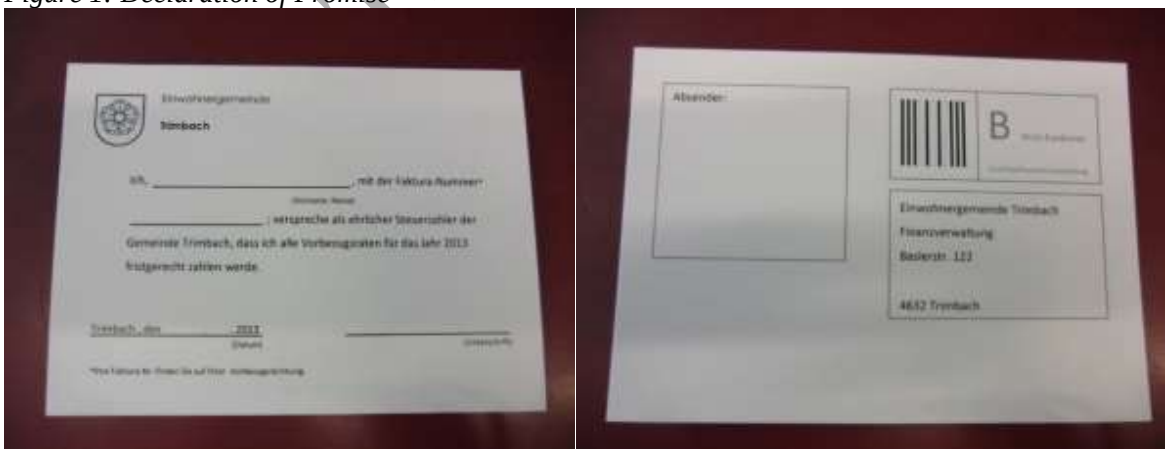
The promise treatments introduce a moral commitment by asking taxpayers to return a prepaid postcard to the tax administration voluntarily promising to pay all tax installments on time. The promise text, reproduced in Figure 1, is as follows;

> "I, (first name, last name), tax identification number XXX, promise as an honest taxpayer of the Trimbach municipality to pay all instalments of the pretax on time during 2013."

In all, 32 percent of the sample decided to make the commitment, returning the postcard and confirming their pledge with a signature. This promise commitment was a prerequisite for entry into a lottery to win either a cash prize of 1,000 CHF (cash + promise treatment, CASH PRO) or a wellness weekend for two valued at 1,000 CHF (wellness + promise treatment, WELL PRO).[13] Whereas cash payments allow for more flexible spending than a wellness weekend, the latter may be more perceived as a prize (Frey and Gallus 2017). In the two other reward treatments (CASH and WELLNESS), the same rewards were offered for payment compliance but without any prior requirement of a promise. Only the compliant taxpayers in any of the treatment groups were eligible for the lottery at the end of the year.

Figure 1: Declaration of Promise



[13] One thousand Swiss francs are roughly equal to 1,000 USD. This is equal to one-fourth of the average tax debt owed to the municipality and is seen as a reasonable amount for a wellness weekend for two in Switzerland.

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Note: These pictures show the cards, dispatched with the treatment letters, on which taxpayers declared their promise. Those interested in making the compliance promise were asked to fill in their name and tax identification number, sign the card and send it back to the tax authority within the given time period.

As regards intergroup differences, whereas the average tax debt in 2013 was 4,459 CHF, no significant differences are apparent between treatment groups in the distribution of tax amounts owed. Nor are any differences observable in average tax debt, past compliance, or demographic characteristics (see Appendix Tables A1–A4).

4. Results

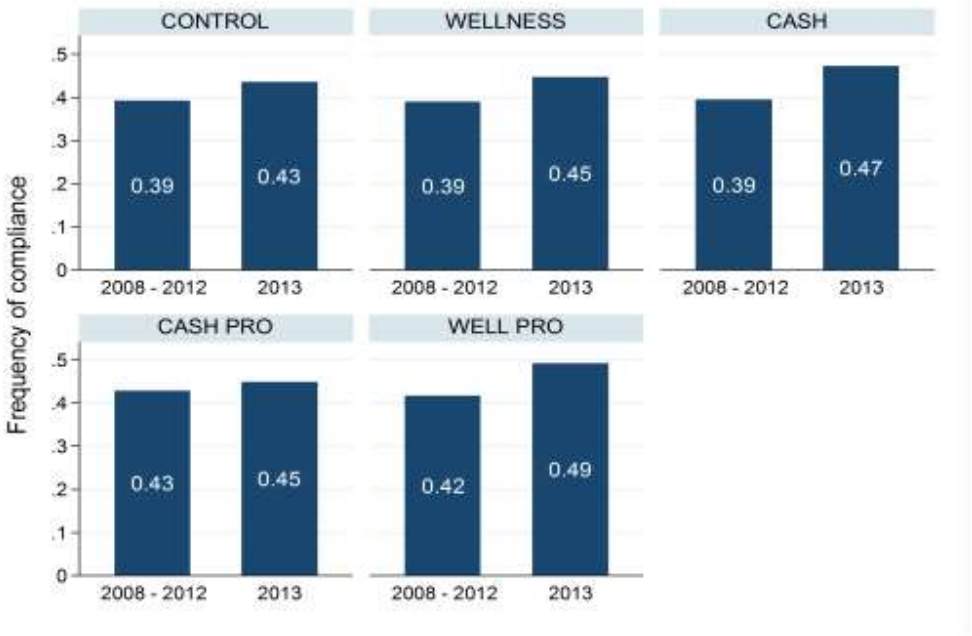
Intention-to-Treat Effect

We begin the analysis by comparing compliance rates based on original treatment assignment independent of whether an individual made the promise. In other words, we identify the impact of the intention to treat (ITT). Consistent with tax authority policies, we define compliance as paying all three installments on time, and then graph the average compliance frequencies for the random treatment groups both before and after the intervention (see Figure 2). Comparing payment behavior in the treatment year itself (2013) reveals that average compliance rates do not differ statistically significantly *between* the control group and the respective treatment groups, indicating that on average the interventions do not significantly improve payment behavior. For the promise treatments, effect dilution is to be expected given that only 31% of CASH PRO and 33% of WELL PRO taxpayers made the promise. Contrasting pre-experiment compliance (2008-2012) with payment behavior in the experimental year, however, does reveal a positive compliance trend. In the CONTROL group, we find a weakly significant improvement in payment behavior ($p=0.092$) that we attribute to the 2013 introduction of the new dunning system for unpaid pre-taxes. This new deterrence produces a slight increase in payment morale that, when combined with a reward, leads to additional improvements ($p<0.05$ for the within-

group comparison in CASH, WELL PRO, and WELLNESS). Only in the CASH PRO group is the change in payment behavior smaller than in the other groups and not statistically significant in the within-group comparison (p=0.452).

Result 1: *For the treatment year, average compliance rates show no statistically significant difference between the control group and the respective treatment groups.*

Figure 2: Compliance Rates ITT



Note: This graph shows the average compliance rates of the treatment groups in pre-experiment years (2008-2012) and in the year of the experiment (2013).

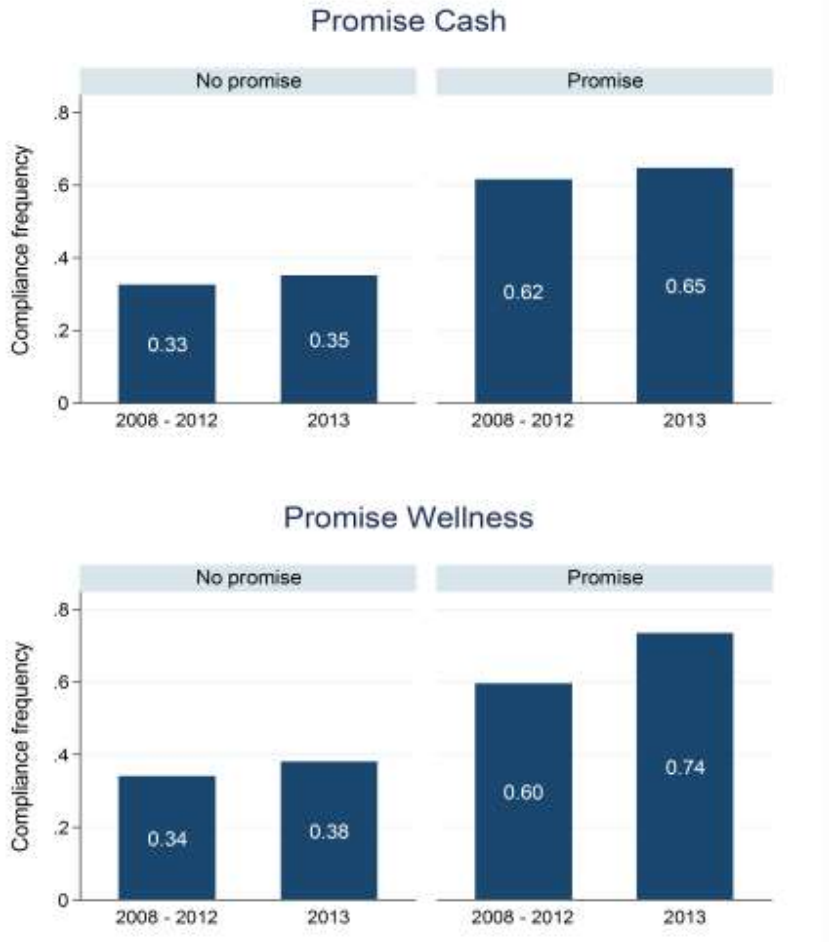
Treatment Effect on the Treated

In a next step, we measure the treatment effect on taxpayers who make the compliance promise (hereafter, promisors), who in the promise treatment groups are more likely than nonpromisors to comply and pay all three rates on time. As Figure 3 indicates, the compliance rates for promisors are 65 percent in the CASH PRO group and 74 percent in the WELL PRO group, while those for nonpromisors are 35 percent and 38 percent, respectively. Both differences are statistically significant at the 1% level in a two-sample test of proportions (prtest) and a chi-square test

(chi2). Nevertheless, recognizing the possibility of a selection effect among those willing to make the promise, we also employ a five-year average of tax compliance (2008–2012) to assess compliance behavior pre intervention. For 2013, we find statistically significant differences at the 1% level between promisors and nonpromisors in both treatment groups (CASH PRO and WELL PRO), which provides evidence for a strong selection effect.

Result 2: *Taxpayers who paid taxes punctually in the past are more likely to promise future compliance (selection effect).*

Figure 3: Promisor Compliance Rates



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Note: This graph shows the average compliance for promisors versus nonpromisors in 2013. The left bars, which depict average past compliance, confirm the strong selection effect in promise-making.

Having identified the selection effect, we then perform a nonparametric test comparing average past compliance rates (2008–2012) with those of 2013 to reveal a notable increase in compliance for the promisors in the WELL PRO treatment (prtest/chi2: p=0.003). In the CASH PRO treatment, however, compliance increases only slightly and fails to reach statistical significance (prtest/ chi2: p=0.505). Koessler et al. (2017) report similar evidence of a selection and commitment effect in their public goods laboratory experiment, although in the present study, the strength of the commitment effect differs with the incentive offered for making the commitment.

Probit Model Results

We further control for both individual differences and the 2013 policy by conducting an additional multivariate analysis (Table 1) while also testing for the pure potential reward effect. To do so, we first estimate the difference in 2013 compliance behavior between promisors and nonpromisors, pooled over the two available treatment groups (column (1)). Being a promisor as compared to a nonpromisor (reference group) increases the probability of compliance by 33.4 percentage points ($p<0.001$). We then use the control group as a new reference group to eliminate any general changes that may have occurred in 2013 (e.g., following introduction of the new dunning system) from the estimation of the promise effect (column (2)). Compared to control group individuals, promisors are 26.3 percentage points more likely to comply ($p<0.001$), while nonpromisors have a 7 percentage point lower compliance probability. We then distinguish between the promisors in each of the promise plus reward groups and measure their behavioral difference in comparison to the control group (column (3)). Relative to the latter, WELL PRO promisors have a higher compliance probability than CASH PRO promisors (31.5 percentage

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points vs. 21.4 percentage points). However, WELL PRO nonpromisors report lower (albeit not statistically significantly) compliance rates on average than the control group, while CASH PRO nonpromisors have an 8.7 percentage point lower compliance probability (p=0.025).

To explore and control for a selection effect as well as for the 2013 policy change, we include data from the three pre-experimental years in column (4) with standard errors clustered on the individual level to account for taxpayer heterogeneity. In this specification, the reference is the control group's past payment behavior, so the 2013 coefficient extracts the effect of the new dunning policy, namely, a significant 4.25 percentage point increase in compliance (p=0.042). Examining past behavior also reveals that promisors in 2013 are more likely to have paid their tax bills on time in the past than those in the control group. In fact, the past payment coefficients for promisors in both the CASH PRO and WELL PRO groups are positive and highly significant (p<0.001).

In 2013, once all previous factors are considered, compliance improves for promisors in the WELL PRO group by an additional 10.8 percentage points (p=0.02). This behavior is significantly different from that of nonpromisors within the same group (p=0.003) and also significantly different from that of CASH PRO promisors (p=0.045 for WELL PRO promisors vs. CASH PRO promisors in 2013). Finally, column (5) includes demographic characteristics as explanatory variables, but we observe no changes in our results.[14]

[14] Specifically, we control for level of tax debt, gender, marital status, children, age (65 + dummy) and for how many years the taxpayer has lived in the municipality, whether the registered taxpayer owns a property in the municipality, is registered as a church member of one of the three local churches or holds Swiss citizenship (dummy).

Table 1: Probit Models for Commitment and Selection

	(1)	(2)	(3)	(4)	(5)
		Compliance 2013		Compliance 2008-2013	
	Promise	**Promise vs. Control**	**Promise vs. Control**	**Promise vs. Control**	**Promise vs. Control**
	pooled	*pooled*	*individual*	*individual*	*individual*
Promisors	0.334***	0.263***			
	(0.04)	(0.04)			
Nonpromisor		-0.070**			
		(0.03)			
Promisors CASH PRO			0.214***	-0.010	-0.023
			(0.05)	(0.05)	(0.05)
Promisors WELL PRO			0.315***	0.108**	0.108**
			(0.05)	(0.05)	(0.05)
Nonpromisors CASH PRO			-0.087**	-0.016	0.023
			(0.04)	(0.04)	(0.04)
Nonpromisors WELL PRO			-0.055	0.001	-0.0001
			(0.04)	(0.04)	(0.04)
2013				0.043**	0.072***
				(0.02)	(0.02)
Promisors CASH PRO (past)				0.221***	0.190***
				(0.04)	(0.04)
Promisors WELL PRO (past)				0.201***	0.181***
				(0.04)	(0.04)
Nonpromisors CASH PRO (past)				-0.070**	-0.082**
				(0.03)	(0.04)
Nonpromisors WELL PRO (past)				-0.055*	-0.066**
				(0.03)	(0.03)
Demographics	no	no	no	no	yes
Observations	836	1,305	1,305	6,698	5,734
Robust SEs clustered on individual level	*no*	*no*	*no*	*yes*	*yes*

Note: The table reports marginal effects with standard errors in parentheses. The models' controls for demographics include gender; marital status; children; age (65 + dummy); for how many years the taxpayer has lived in the municipality and whether the registered taxpayer owns a property in the municipality, is registered as a church member of one of the three local churches, and holds Swiss citizenship (dummy) as explanatory variables. *** p<0.01, ** p<0.05, * p<0.1.

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Table 2: Probit Models for Promise Plus Reward vs. Reward Only

	(6)	(7)	(8)	(9)	(10)	(11)
	Compliance 2013				Compliance 2008-2013	
	Cash Pro vs. Cash		Well Pro vs. Wellness		Treatment vs. Control	
CASH PRO	-0.023					
	(0.03)					
Promisors CASH PRO		0.172***			-0.010	-0.023
		(0.05)			(0.05)	(0.05)
Nonpromisors CASH PRO		-0.121***			-0.016	0.024
		(0.04)			(0.04)	(0.04)
CASH					0.034	0.025
					(0.03)	(0.03)
WELL PRO			0.045			
			(0.03)			
Promisors WELL PRO				0.289***	0.108**	0.109**
				(0.05)	(0.05)	(0.05)
Nonpromisors WELL PRO				-0.064*	0.001	0.0001
				(0.04)	(0.04)	(0.04)
WELLNESS					0.016	0.025
					(0.03)	(0.03)
2013					0.042**	0.073***
					(0.02)	(0.02)
Promisors CASH PRO (past)					0.220***	0.182***
					(0.04)	(0.04)
Nonpromisors CASH PRO (past)					-0.070**	-0.081**
					(0.03)	(0.04)
CASH (past)					0.002	-0.005
					(0.03)	(0.03)
Promisors WELL PRO (past)					0.201***	0.175***
					(0.04)	(0.04)
Nonpromisors WELL PRO (past)					-0.054*	-0.066**
					(0.03)	(0.03)
WELLNESS (past)					-0.004	-0.015
					(0.03)	(0.03)
Demographics	no	no	no	no	no	yes
Observations	852	852	880	880	11,382	9,832
Robust SEs clustered on individual level	no	no	no	no	yes	yes

Note: The table reports marginal effects with standard errors in parentheses. The models' controls for demographics include gender; marital status; children; age (65 + dummy); for how many years the taxpayer has lived in the municipality; and whether the taxpayer owns a property, is registered as a church member of one of the three local churches, and holds Swiss citizenship (dummy) as explanatory variables. , *** p<0.01, ** p<0.05, * p<0.1.

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As a robustness check, we rerun the estimations using propensity score matching (Table A5 in the appendix). The matching enables us to compare the behavior of promisors only with the compliance of those individuals in the control group who are similar to the promisors in their past compliance (and demographic characteristics). The results validate our findings obtained in Table 1: After the pledge, promisors in WELL PRO additionally increase their compliance (commitment effect), whereas the higher compliance of promisors in CASH PRO is not robust and points to a pure selection effect.

Result 3: *Promises are associated with distinct selection and commitment effects.*

Result 4: *Promise effects differ depending on the reward offered for compliance.*

To investigate this reward-dependent difference in more detail and control for a pure reward incentive effect, we conduct an additional analysis using only the pure reward treatment groups (see Table 2), beginning with a comparison of behavioral differences in the 2013 CASH PRO group with the CASH (only) group as the reference (columns (6) and (7)). Although the combined pool of promisors and nonpromisors in the CASH PRO show no significant differences from the CASH group ($p=0.50$; column (6)), CASH PRO promisors have a 17.2 percentage points higher compliance probability than CASH only individuals ($p<0.001$) compared with a 12 percentage point probability among CASH PRO nonpromisors ($p=0.001$) (column (7)). Similarly, when we compare the pooled WELL PRO group with the WELLNESS individuals (columns (8) and (9)), no differences emerge between the two ($p=0.18$). However, WELL PRO promisors and nonpromisors demonstrate significantly different compliance behaviors, with the former being 28.9 percentage points more likely to pay on time ($p<0.001$) but the latter 6.4 percentage points less likely to do so ($p=0.072$) relative to WELLNESS only individuals.

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When we adjust for selection effects, policy change, and individual characteristics (columns (10) and (11)), however, controlling for policy-induced behavioral changes leads to only a slight improvement in payment behavior among CASH individuals and an actual negative change among CASH PRO promisors. Hence, although offering a cash reward for compliance has a positive incentive effect, combining this reward with a formal promise is less powerful. Although the observed difference is not statistically significant (column (10); p=0.356) and thus merely indicative, we interpret it as a sign that the additional promise requirement may crowd out the cash incentive effect.

In the WELLNESS groups, promisors improve their payment behavior by an additional 11 percentage points (p=0.016), a robust commitment effect that persists even when demographic factors are taken into account (column (11); p=0.033). Comparing the behavior of promisors in the WELL PRO group with that of taxpayers in the WELLNESS group, we find that the difference remains significant (p=0.040 in column (10) and p=0.102 in column (11)). These observations yield Result 5, which is in line with the finding from previous crowding effects research that offering financial rewards can backfire when the recipient perceives them as compensation rather than acknowledgment (Frey and Jegen 2001, Deci 1971).

Result 5: *Combining a compliance commitment with a nonfinancial reward leads to improved payment behavior. With a financial reward only a selection effect is present and no change occurs in payment behavior.*

IV Regression Results

To evaluate the promise effect while recognizing the associated endogeneity, we analyze the effect of our interventions using an instrumental variable analysis (see Table 3). Given the divergence between the assignment to and the receipt of treatment, we distinguish between participants offered the opportunity to formally make the promise (Promise) and those who

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voluntarily make the promise anyway (Promisors). Since exposure to the promise offer is randomized over all taxpayers, we use it as the instrument in our estimations (i.e., treatment groups CASH PRO and WELL PRO). We detail the results from the corresponding first-stage regressions and the statistical support for an IV approach provided by the Durbin-Wu Hausman test in the Appendix (Table A6). As previously noted, 31 percent of the taxpayers willingly made the promise in the CASH PRO treatment group and 33 percent in the WELL PRO treatment group. We contrast the payment behavior between these two promise treatments (CASH PRO and WELL PRO) pooling them together and the CONTROL group in column (1) and estimate the likelihood that a taxpayer pays all the 2013 installments on time. Such timely payments are 11 percentage points more likely for the pooled promisor subgroup than for the remaining taxpayers, but the difference is not statistically significant (p=0.198).

Next, to capture the incentive scheme when making the promise, we first estimate payment behavior for promisors offered the cash reward (see column (2)). Whereas their 2013 payment behavior is slightly but not statistically significantly better than that of the control group offered neither promise opportunity nor reward, promisors offered the in-kind reward (i.e., WELL PRO individuals) are significantly more compliant than CONTROL individuals (p=0.079), with an 18 percentage point higher likelihood of timely payments. Hence, the promise effect varies significantly with the reward offered for compliance.

To distinguish between the reward incentive effect and the promise's commitment effect, we contrast the behavioral changes in the CASH PRO and WELL PRO individuals with those of taxpayers in the CASH and WELLNESS groups; with particular attention to the payment behavior of promisors (from both promise groups) versus nonpromisors (see column (4)).

Although promisors made timely payments more frequently, the results are not statistically significant (p=0.631).

Table 3: Instrumental Variable Regressions

***Yi* =**	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
	Compliance 2013					
	Promise (pooled) vs. Control	**Cash Pro vs. Control**	**Well Pro vs. Control**	**Promise vs. Reward**	**Cash Pro vs. Cash**	**Well Pro vs. Wellness**
Promisors	0.113	0.042	0.182*	0.036	-0.071	0.143
	(0.088)	(0.101)	(0.104)	(0.074)	(0.104)	(0.106)
Constant	0.435	0.435	0.435	0.46	0.472	0.447
	(0.088)	(0.023)	(0.023)	(0.017)	(0.024)	(0.023)
Observations	1,305	870	904	1,732	852	880

Note: The instrument is the opportunity to make the formal promise; standard errors are in parentheses;*** p<0.01, ** p<0.05, * p<0.1.

To distinguish the types of incentives offered, in column (5) we contrast the payment behavior of promisors in the CASH PRO group with that of taxpayers in the CASH group, which again shows that the addition of the promise request generated no additional improvements in payment behavior (p=0.498). When we compare the timely payments of promisors in the WELL PRO group with the payments of the WELLNESS taxpayer (column (6)), however, in contrast to the negative coefficient in column (5), the promise does induce an improvement in payment behavior, albeit one that is not statistically significant (p=0.175).

Reflections on the Field Experimental Results

Our field experiment, which allows us to test for a compliance promise effect in a natural taxpaying environment, leads to two key observations: the presence of a strong selection effect by which compliant taxpayers are more likely to make a pledge, and an impact of reward type on how the promise affects consequent behavior, with an in-kind reward more likely to generate a

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positive commitment effect than a financial reward. Nonetheless, our approach to analyzing the potential effects of a tax compliance promise is subject to certain limitations.

First, even though talking about income or taxes is culturally discourteous in Switzerland and very few concerned citizens approached official institutions about our interventions, we have no control over taxpayer conversations and thus cannot rule out their discussing aspects of the intervention among themselves.[15]

Second, both the tax authority and the municipal council rejected the implementation of a promise only treatment in which taxpayers could promise timely payment with no reward offered, insisting that taxpayers gain some additional potential benefit from making the promise. Although this reluctance provides some insight into the feasibility and necessary design characteristics of a real world voluntary compliance promise, it prevents us from drawing conclusions about the promise's pure commitment effect. Hence, to address this shortcoming we conducted an additional laboratory experiment that does not only replicate the treatment schemes from the field experiment, but extends them to include a compulsory and voluntary promise treatment group who receive no offer of an additional reward for promise keeping.

5. Laboratory Experiment

In the laboratory experiment, we choose honesty in reporting income as our measure of compliance, which does not only make our study comparable with most other laboratory experiments on tax compliance (e.g., Alm 1999, Torgler 2002, Alm et al. 2015), but serves as a relevant difference from our field study use of timely payments as the dependent variable.

[15] The tax administration received 7 written reactions and 12 phone inquiries. There was no media or social media coverage that could have contaminated the field experiment.

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Although the behavior measured is different, we expect the promise to work similarly in both compliance dimensions.

To simulate the tax payment setting, we design a real effort experiment in which subjects can earn income from counting the number of zeros in matrices (Abeler et al. 2011).[16] Next, over five rounds, subjects can accumulate earnings, although they must declare that income at the end of each round.[17] The tax rate is 20 percent, and the income reporting is framed as a tax declaration setting, using the terms *income, income declaration,* and *tax.* A weak deterrence mechanism is in place with an audit probability of 5 percent, meaning that at the end of the experiment, each subject faces a 5 percent probability that the income declarations from the previous rounds will be checked. When the audit shows the income declarations to be incorrect, the real total income after tax is used as payment and a fine in the amount of the evaded income is imposed.

In an extension of the field study, the laboratory experiment tests three separate compliance promotion schemes: a reward scheme, a promise scheme, and a combination of the two. In the reward scheme, subjects can obtain a reward when audited and found to be compliant. As before, dependent on the treatment group, this reward is either CASH (an additional €10 euro, which corresponds to the average experimental earnings) or an INKIND reward (a cinema voucher of the same amount added to the real effort task earnings). The promise scheme, on the other hand, differs from the field protocol in that it requests either a voluntary or a compulsory moral commitment in which subjects either can or must promise at the beginning of the experiment "….to give truthful information in this experiment about [their]

[16] This task is rather boring, such that subjects' productivity and resulting earnings are based on effort and not on a specific skill set.

[17] After the working phase in each round, subjects are informed how many matrices they counted correctly and the resulting 'round income'. Then they have to report their income so that the tax debt and corresponding 'round income after tax' can be calculated. By design and instructions, subjects can only underreport.

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income." The third scheme then combines the previous options: Subjects who voluntarily make a promise of compliance obtain either a cash (CASH PRO) or an in-kind reward (INKIND PRO) when found compliant.

The experiment, conducted in January 2018 at the LaER laboratory of Osnabrück University, Germany, involved 260 participants, the majority of them students (94%)[18] with an average age of 23.7 years (see Appendix Table A7). The treatments, administered with the help of the human experimental interaction platform, SoPHIE (see Hendriks 2012), were randomized within 13 sessions, with 40 observations for the control group and promise schemes and 30 observations for the pure reward schemes (see the protocols in the Online Appendix).

Laboratory Experimental Results

In the reference group with no promotional scheme in place (CONTROL), only 35% of the participants complied by reporting their income truthfully in all game rounds, which led to 37% of the total income not being taxed. To examine the impact of our treatment interventions on such compliance behavior, we first assess the promise only (PRO) effect that could not be addressed in the field experiment. We find that subjects who make the promise voluntarily (VOL PRO) are significantly more compliant than either those in the same group who decide against the promise in the VOL PRO treatment or individuals in the control group (Pearson chi2: $p<0.01$).

***Result* 6**: *Subjects who make a voluntary promise, even without being offered a reward for keeping it, behave more compliantly than subjects in the control group which can be attributed to a selection effect of historically compliant taxpayers.*

[18] This is evidence that the population from the laboratory study is very different from the sample observed in the field experiment. Choo et al. (2016) show in this context, that when the compliance behavior of student and non-student samples are compared in tax evasion experiments, the student sample complies significantly less, but at the same time is most receptive to treatment changes. The authors argue that the difference originates from the fact that non-student participants are also paying taxes in reality and thus enter the laboratory with a pre-existing compliance norm.

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Although the current experimental design does not permit us to disentangle whether the voluntary promise scheme's success comes from higher post-pledge commitment to compliance or selection, it does help us to elaborate on two aspects of promise making: how attractive the promise offer is (i.e., how many subjects are willing to make the pledge), and whether promisors have dominant characteristics that enable predictions about their behavior. For example, in the PRO group, as in the field experiment, only the 55% of individuals made the promise. On the average treatment level, however, no significant improvement in compliance can be observed (Pearson Chi2: p=0.175 in comparison to the control group). Also, the introduction of the compulsory promise does not lead to an improvement in compliance (p=0.491). When a reward was offered for fulfilling the promise, in contrast, a significant 82.5% and 72.5% of the CASH PRO and INKIND PRO groups, respectively, made the promise.

Assumedly, the reward provided an additional incentive to make the promise, although the motives for making the promise may also have differed between the VOL PRO and the two promise-plus-reward groups (CASH/INKIND PRO). Hence, as a proxy for these differences, we compare the participant's individual scores, elicited in a post-experimental questionnaire, on the "commitment" subscale of Braithwaite's (2003) tax compliance measure, which is designed to capture individual "belief(s) about the desirability of tax systems and feelings of moral obligation to act in the interest of the collective and pay one's tax with good will" (p. 18).[19] We find that although those who perceive taxpaying as a moral or civic obligation are weakly significantly more likely to make a compliance promise (p=0.09; column (2), Appendix Table A8), such is only the case among VOL PRO individuals. When a reward is offered for promise fulfillment, the motives to make the promise appear to be confounded and tax morale is no longer a good predictor for promise making.

[19] We used the German translation from Kirchler and Wahl (2010).

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Table 4: Compliance in the Laboratory Setting

	(1)	(2)	(3)		(4)	(5)	(6)
Y_i =	**Total Compliance**						
	Intention to Treat Effect				*Effect on Treated and Nontreated*		
VOL PRO	0.180	0.193	0.203	Nonpromisor	-0.224	-0.222	-0.219
	(0.124)	(0.126)	(0.130)		(0.172)	(0.176)	(0.184)
CASH PRO	0.216*	0.218*	0.281**		-0.378	-0.420*	-0.357
	(0.122)	(0.124)	(0.128)		(0.233)	(0.246)	(0.254)
INKIND PRO	0.207*	0.209*	0.272**		-0.256	-0.200	-0.116
	(0.124)	(0.126)	(0.131)		(0.269)	(0.266)	(0.260)
COM PRO	0.072	0.095	0.034		0.069	0.103	0.045
	(0.116)	(0.119)	(0.122)		(0.116)	(0.119)	(0.122)
Promisor × *VOL PRO*				Promisor	0.507***	0.521***	0.508***
					(0.155)	(0.157)	(0.159)
Promisor × *CASH PRO*					0.410***	0.436***	0.484***
					(0.136)	(0.140)	(0.143)
Promisor × *INKIND PRO*					0.302**	0.310**	0.374***
					(0.132)	(0.135)	(0.141)
CASH	0.087	0.111	0.120		0.083	0.121	0.127
	(0.136)	(0.139)	(0.142)		(0.136)	(0.141)	(0.144)
INKIND	0.468***	0.456***	0.484***		0.460***	0.457***	0.481***
	(0.139)	(0.142)	(0.148)		(0.139)	(0.142)	(0.147)
Female		0.111	0.056			0.108	0.046
		(0.071)	(0.076)			(0.075)	(0.080)
Age		0.009	0.001			0.016	0.008
		(0.009)	(0.010)			(0.010)	(0.010)
Student		0.375**	0.362**			0.398**	0.371**
		(0.164)	(0.170)			(0.172)	(0.178)
Economics major		-0.040	-0.009			-0.010	0.024
		(0.079)	(0.081)			(0.084)	(0.086)
Productivity		0.006	0.010			0.009	0.012
		(0.015)	(0.016)			(0.016)	(0.017)
Tax morale			0.145***				0.123**
			(0.051)				(0.056)
Willingness to take risk			-0.085***				-0.089***
			(0.027)				(0.028)
Observations	260	260	260		260	260	260

Notes: The table reports marginal effects, with standard errors in parentheses; all models include session fixed effects. Productivity refers to how many matrices the individual solved correctly; tax morale denotes an individual's motivational postures toward taxpaying (based on standardized mean scores on the Braithwaite tax compliance commitment subscale); willingness to take risk is a self-reported measure used in the German Socio-Economic Panel (R-1 scale). *** p<0.01, ** p<0.05, * p<0.1

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Interaction between Promise and Rewards

In a next step, therefore, we examine the interaction effect between the promise and the rewards. For this purpose, we compare the combined intervention separately with the pure promise and two pure reward groups (CASH and INKIND). Before doing so, however, we should point out that the highest compliance occurs in the INKIND group, one of our control groups for the pure reward effect, probably because of the type of in-kind reward chosen for the laboratory experiment. That is, students apparently perceive the cinema voucher as a very attractive reward and are thus strongly motivated to comply.[20] This result again underscores how the incentive effect can vary with different in-kind rewards and how important it is to select an attractive reward for the particular target group.

To analyze the behavioral effects of PRO versus CASH/INKIND PRO, we employ a multivariate regression (Table 4) in which the treatment groups are randomized within one session and session fixed effects are included to control for session heterogeneity. Although the compliance rates in CASH PRO and INKIND PRO are weakly statistically significantly higher than those in the CONTROL group (p=0.077 and 0.095 in column (1)), the INKIND group, as previously mentioned, shows by far the highest compliance rate (p=0.001). For the VOL PRO group, compliance improves, but with only marginal significance (p=0.146 in column (1) and p=0.117 in column (3), Table 4). The COM PRO effect on compliance is also small and not statistically significant (p=0.536 in column (1) and 0.779 in column (3)), a result that remains robust even when the estimation accounts for individual demographic characteristics (column (2)) or willingness to take risk[21] and tax morale (column (3)).

[20] This impression is confirmed by the comments participants made in the post experimental questionnaire. Several subjects stated how much they like to go to the movies.

[21] We used the question from the German Socio-Economic Panel, asking subjects to state their willingness to take risks on a scale from 1 to 7; a higher value is associated with a higher willingness to take risks.

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Result 7: *A compliance promise alone does not significantly improve average compliance, whereas offering a reward for keeping the promise strengthens compliance.*

Because here, as in the field experiment, we assume that the promise predominantly affects the compliance decisions of those who agreed to make the promise, we list the results for promisors and nonpromisors separately in columns (4) to (6). For all promise schemes, the compliance rate is significantly higher for promisors than for nonpromisors or CONTROL group members (chi2 test: $p<0.001$ for VOL PRO and CASH PRO; $p=0.022$ for INKIND PRO, column (4)). Although this effect is stronger for promisors in the three voluntary promise treatments than in the compulsory promise schemes ($p<0.01$ for VOL PRO and CASH PRO, $p=0.022$ for INKIND PRO, see column (6)), no significant compliance differences emerge among promisors in any of the three voluntary promise schemes (PRO, CASH PRO, and INKIND PRO).

Result 8: *Although participants who make the promise are more compliant, no differences are observable between promisors in the three different voluntary promise schemes.*

Even when we additionally control for individual demographic characteristics (column (5)), willingness to take risks, and motivational posture toward taxpaying (column (6)), these core findings do not change. Consistent with the literature (Torgler, 2007; Torgler et al. 2008; Torgler and Schneider, 2009; Dulleck et al., 2016), tax morale is positively correlated with tax compliance but negatively correlated with individual willingness to take risks.

Because compliance in the laboratory experiment is measured in terms of honest income declaration (i.e., no cheating), we can test for a second compliance dimension; namely, the average proportion of income that remains undeclared (Table 5, left panel), for which the noncomplier outcomes are particularly interesting (right panel). That is, whereas the ratio of undeclared income is lowest in the INKIND group as expected, it is not only highest in the CASH group but also driven by noncompliers, for whom 81% of income remains undeclared.

Because this finding constitutes a statistically significant difference from the reporting behavior of noncompliers in the CONTROL group (Wilcoxon rank sum test: p=0.028), we interpret it as a crowding out effect. That is, although moral principles tend to take a backseat when cash is offered for compliance and emphasis is placed on the appropriateness of strategic reasoning, combining this cash reward with a formal promise reduces the financial incentive's boomerang effect. For the in-kind reward, in contrast, the previously observed findings are also replicated with this second compliance dimension. Given the strong positive incentive effect of the pure in-kind reward, it is apparent that linking such a reward with a formal promise significantly reduces the positive incentive effect (p=0.038). We assume that adding the promise as a second condition for earning the reward makes the scheme less attractive to the participants.

Table 5: Ratio of Undeclared Income

Treatment	**Average ratio**	***SD***	**N**	**Average ratio**	***SD***	***n***	***n/N***
	All participants			*Noncompliers*			
CONTROL	0.37	*0.40*	40	0.57	*0.37*	26	65%
COM PRO	0.32	*0.39*	40	0.56	*0.36*	23	58%
PRO	0.27	*0.38*	40	0.54	*0.38*	20	50%
CASH PRO	0.32	*0.44*	40	0.67	*0.42*	19	48%
INKIND PRO	0.29	*0.40*	40	0.61	*0.38*	19	48%
CASH	0.48	*0.45*	30	0.81	*0.27*	18	60%
INKIND	0.13	*0.30*	30	0.55	*0.42*	7	23%

Note: This table shows the average proportion of income that remained undeclared.

Overall, our experimental results suggest that individuals who are willing to make a formal promise, even when no reward is offered for its fulfillment exhibit higher compliance, but that, when such a reward is offered, even more individuals are willing to make the promise. However, whether the combined scheme of promise plus reward is effective in promoting compliance depends on the reward type. That is, in our experiment, although the pure financial

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reward (CASH) triggered the highest proportion of undeclared income while combining it with a formal promise seemingly counterbalanced this negative impact, the combination failed to raise compliance of either the overall population or the subgroup of promisors relative to promisors who were not offered the reward. However, linking the formal promise to an in-kind reward that was highly attractive to participants diminished the latter's positive incentive effect.

6. Conclusions

Because the empirical evidence on promise-making's relevance for subsequent behavior stems mostly from laboratory experiments, which struggle with problems of external validity, our understanding of the phenomenon remains preliminary. It is thus hardly surprising that politicians and tax administrators are still unsure how to promote pro-active commitment to pay taxes. To throw light on this problem, with the help of a Swiss tax administration, we conducted a novel field experiment that tests the importance of promises (i.e., commitment) in the tax compliance context. As it is impossible to implement a pure promise treatment in this real world scenario, we complemented the analysis with a laboratory experiment that allowed additional cross-comparisons.

A first key observation from our results is a strong selection effect: compliant taxpayers or taxpayers with a high tax morale are more likely to make a formal pledge. For example, in the field experiment, promisors had about 50 percent more past compliance than the average taxpayer in the control group, while taxpayers uninterested in committing to the promise were significantly less compliant than the reference (control) group. A second key observation is that after netting out any selection effects, promises alone do not lead to a behavioral change. This contradicts the results of other studies that argue that the promises can strengthen pro-social behavior. However, in these studies (e.g. Charness & Dufwenberg, 2006; Ellingsen &

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Johannesson, 2004; Koessler, Page, & Dulleck, 2018; Vanberg, 2008) individuals made promises to reduce uncertainty and facilitate the future collaboration of others. In our experiments, individuals were not able to use promises as strategic tool to influence the behavior of others.

A third key observation is that the type of reward affects the impact of a given promise: both in the laboratory and field experiments, the opportunity to earn a nonfinancial (in-kind) reward was more likely than the offer of a financial reward (cash) to generate a positive commitment effect. One interpretation is that, although the in-kind reward is understood as acknowledgment and may support the commitment made with the promise, financial incentives trigger the perception of an exchange relationship.

The laboratory experiment also suggests, however, that combining promises and rewards may not always be beneficial. For example, combining the promise condition with the highly attractive in-kind reward offered in our laboratory setting lowered the latter's positive incentive effect. Hence, the in-kind reward's incentive effect depends on its attractiveness to the target group, which in turn determines whether this type of reward alone, a pure promise, or the combination of both will most successfully promote compliance. Yet, the field experiment results, in addition to indicating the presence of a selection effect, suggest that offering an in-kind reward can indeed motivate taxpayers to increase their compliance. The relevant question is which type of rewards could realistically be offered in the real world given possible concerns about tax administrations compensating citizens for fulfilling a civic duty and statutory obligation. To address such apprehensions and strengthen the tax administration's credibility, future field experiments might offer local community benefits such as free access to public swimming pools or other public infrastructures, which carry no additional marginal costs and are directly related to pretax revenues.

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- Appendix

Table A1: Average Tax Debt in 2013

Treatment	Mean	Sd	Min	Max	N
CONTROL	4,553	4,591	91	57,011	469
WELLNESS	4,553	4,054	111	47,937	445
CASH	4,490	4,127	97	51,300	451
CASH PRO	4,346	4,513	95	52,203	401
WELL PRO	4,333	3,909	101	44,203	435
Total	4,459	4,244	91	57,011	2201

Table A2: Pairwise Tax Debt Comparison in 2013

Tax Debt	Tukey Test					
	Contrast	*Std. Err.*	*t*	*P>t*	*95% Conf. Interval*	
WELLNESS vs CONTROL	0.00	281.04	0.00	1.00	-767.24	767.25
CASH vs CONTROL	-62.70	280.08	-0.22	1.00	-827.33	701.92
CASH PRO vs CONTROL	-207.12	288.84	-0.72	0.95	-995.67	581.43
WELL PRO vs CONTROL	-219.70	282.69	-0.78	0.94	-991.46	552.06
CASH vs WELLNESS	-62.70	283.76	-0.22	1.00	-837.37	711.96
CASH PRO vs WELLNESS	-207.12	292.41	-0.71	0.96	-1005.41	591.17
WELL PRO vs WELLNESS	-219.70	286.34	-0.77	0.94	-1001.41	562.01
CASH PRO vs CASH	-144.42	291.49	-0.50	0.99	-940.19	651.35
WELL PRO vs CASH	-157.00	285.39	-0.55	0.98	-936.13	622.14
WELL PRO vs CASH PRO	-12.58	294.00	-0.04	1.00	-815.21	790.05
Observations	*CONTROL*	*WELLNESS*	*CASH*	*CASH PRO*	*WELL PRO*	
	469	*445*	*451*	*401*	*435*	

Notes: The Tukey test performs all pairwise comparisons between the means in tax debt across all treatment groups in one step. The results show that in the year 2013 no statistically significant difference existed in the average tax debt owed between the treatment groups.

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Table A3: Summary statistics of observables

Treatment group		age	female	married	family	house-owner	Swiss citizen	church member
CONTROL	mean	55.39	0.29	0.50	0.19	0.28	0.77	0.59
	N	*395*	*445*	*395*	*445*	*405*	*411*	*395*
WELLNESS	mean	56.78	0.32	0.47	0.19	0.33	0.77	0.58
	N	*386*	*432*	*386*	*432*	*392*	*400*	*386*
CASH	mean	57.70	0.31	0.48	0.16	0.36	0.76	0.61
	N	*387*	*437*	*387*	*437*	*396*	*398*	*387*
CASH PRO	mean	56.84	0.29	0.50	0.18	0.32	0.78	0.63
	N	*331*	*382*	*330*	*382*	*337*	*347*	*330*
WELL PRO	mean	56.93	0.28	0.50	0.15	0.34	0.78	0.64
	N	*364*	*398*	*363*	*398*	*368*	*376*	*363*
Total	mean	**56.71**	**0.30**	**0.49**	**0.17**	**0.33**	**0.77**	**0.61**
	N	*1863*	*2094*	*1861*	*2094*	*1898*	*1932*	*1861*

Notes: The demographic information is based on the year 2012, the year before the field experiment took place and the information above was retrieved from a separate registry after the experiment was conducted. Observation numbers are lower since not all taxpayers could be matched with help of their identification number across the two registries, because they for example moved or changed their civil status.

Table A4: Pairwise Comparison of Mean Compliance in Past

Mean Compliance in Past	**Tukey Test**					
	Contrast	*Std. Err.*	*t*	*P>t*	*95% Conf. Interval*	
WELLNESS vs CONTROL	-0.004	0.016	-0.23	1	-0.047	0.04
CASH vs CONTROL	0.002	0.016	0.15	1	-0.041	0.045
CASH PRO vs CONTROL	0.036	0.016	2.17	0.19	-0.009	0.08
WELL PRO vs CONTROL	0.024	0.016	1.49	0.57	-0.02	0.068
CASH vs WELLNESS	0.006	0.016	0.37	1	-0.038	0.049
CASH PRO vs WELLNESS	0.039	0.017	2.36	0.13	-0.006	0.084
WELL PRO vs WELLNESS	0.027	0.016	1.69	0.44	-0.017	0.072
CASH PRO vs CASH	0.033	0.017	2.01	0.26	-0.012	0.078
WELL PRO vs CASH	0.021	0.016	1.33	0.67	-0.023	0.066
WELL PRO vs CASH PRO	-0.012	0.016	-0.7	0.96	-0.058	0.034

Notes: The Tukey test performs all pairwise comparisons between the means in past compliance across all treatment groups. No statistically significant difference exists between the treatment groups, and only the difference in mean past compliance between CASH PRO and WELLNESS is close to statistical significance at the 10% level. This cross comparison is, however, not relevant for our study since we never compare treatment groups with a pure incentive from one kind (cash) with the promise combination of another kind (wellness).

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Table A5: Propensity score estimations

	(1) *Pooled*	(2) *Pooled*	(3) *CASH PRO*	(4) *CASH PRO*	(5) *WELL PRO*	(6) *WELL PRO*
ATE						
Promise	0.129***	0.0903**	0.0918*	0.0490	0.167***	0.133**
	(0.0378)	(0.0405)	(0.0532)	(0.0531)	(0.0511)	(0.0533)
Demographics	No	Yes	No	Yes	No	Yes
Observations	681	587	557	482	552	476
# Control	428	371	428	371	428	371
# Treatment	253	216	129	111	124	105
Requested # matches	5	5	5	5	5	5
Min. # matches	5	5	5	5	9	5
Max. # matches	171	5	171	5	171	5

*Notes: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1*

Table A6: First Stage of IV Regressions corresponding to Table 3 in the main text

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Y_i =			**Compliance 2013**			
	Promise vs. Control	*Cash Pro vs. Control*	*Well Pro vs. Control*	*Promise vs. Reward*	*Cash Pro vs. Cash*	*Well Pro vs. Well*
Promise offer (Instrument)	0.322***	0.332***	0.313***	0.322***	0.332***	0.313***
	(0.022)	(0.022)	(0.022)	(0.156)	(0.022)	(0.022)
Durbin-Wu Hausman						
p-value	*0.024*	*0.025*	*0.135*	*<0.001*	*0.001*	*0.07*
F-stat	*5.096*	*5.074*	*2.345*	*12.609*	*10.343*	*3.292*
Observations	1,305	870	904	1,732	852	880

*Note: `Promise offer' (being in a treatment group in which a promise can be made) serves as an instrument. The Durbin-Wu Hausman tests are performed to test for endogeneity. The null hypothesis that OLS and TSLS estimates are identical can be rejected for all estimations. Standard errors are in parentheses, *** p<0.01, ** p<0.05, * p<0.1*

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Table A7: LAB Summary Statistics - Demographics

Treatment group	age	female	student	econ major	productivity
CONTROL	23.85	0.50	1.00	0.35	9.69 (2.29)
COMPRO	23.53	0.64	0.88	0.25	10.04 (2.18)
PROMISE	24.58	0.58	0.90	0.23	10.02 (2.46)
CASH PRO	23.83	0.53	0.98	0.20	9.55 (1.57)
INKIND PRO	23.15	0.48	1.00	0.25	10.16 (2.57)
CASH	22.63	0.60	0.90	0.33	9.74 (2.34)
INKIND	24.20	0.73	0.93	0.17	9.76 (2.70)
Total	23.70	0.57	0.94	0.25	9.86 (2.29)

Table A8: LAB - Probit models on the Likelihood of making a promise

	(1)	(2)
Y_i =	**Likelihood of making a promise**	
CASH PRO	0.186**	0.177*
	(0.087)	(0.091)
INKIND PRO	0.272***	0.244***
	(0.092)	(0.094)
Tax morale × PROMISE	0.176	0.192*
	(0.118)	(0.116)
Tax morale × *CASH PRO*	0.093	0.091
	(0.078)	(0.077)
Tax morale × *INKIND PRO*	0.138	0.126
	(0.092)	(0.097)
Willingness to take risk		0.001
		(0.032)
Female		0.045
		(0.087)
Age		-0.015
		(0.012)
Econ major		-0.070
		(0.096)
Student		0.168
		(0.204)
Observations	120	120

Marginal effects, Standard errors are in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Note: 'Tax morale' measures an individual's motivational postures towards paying taxed, based on the subscale 'Commitment' from Braithwaite (2003)'s tax compliance measure. Standardized mean scores are taken as a basis for the estimations. 'Willingness to take risks' is a self-reported measure on the individual's willingness to take risks, as used in the German Socio-Economic Panel (R-1 scale). Both models include session fixed effects.

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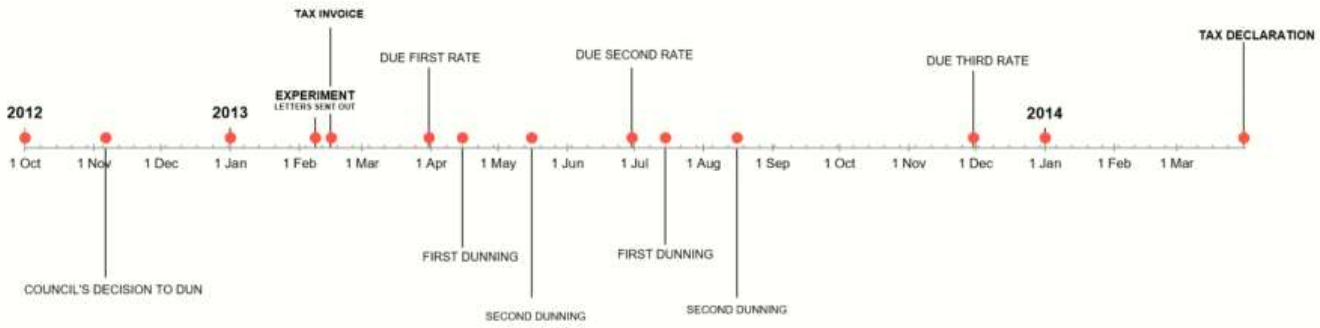
Figure A1: FIELD - Timeline



Notes: Although the payment of pretaxes is a legal obligation in Switzerland, in our study 30-40% of the taxpayers failed to make the pretax payments on time, and 18% did not make any payment during the corresponding year. Prior to 2013, no enforcement took place: the missing amount was simply charged a default interest rate when the final tax calculation was made in the following year. In November 2012, however, in a public council meeting, the tax administration proposed its plan to implement a dunning system to highlight the statutory tax obligation. All taxpayers were informed about the change in practice by the following announcement: "Non-paid pretaxes will be dunned after expiration of the payment deadline. This new practice was adopted by the Council because of diminishing payments". According to this new policy, noncompliant taxpayers receive a first dunning letter two weeks after the payment due date. If the tax administration receives no pretax payment after four weeks, it sends out a second letter notifying the taxpayer that a penalty of 50 CHF has been added to the current tax debt. As in previous years, owed amounts are also charged default interest of 3% once the final tax calculation has been made. The moral cost of noncompliance were raised by justifying the penalty as follows: "Reason: The municipality is paying current expenditures with tax revenues. If the necessary money has not been received, it must borrow money, incurring interest and fees" (see Figure A4 and its translation).

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FIELD - Announcement letters

Figure A2: Control Group

Einwohnergemeinde
Trimbach

Finanzverwaltung
Baslerstrasse 122, 4632 Trimbach
Telefon 062 289 23 10 / Fax 062 289 23 30
E-Mail finanzverwaltung@trimbach.ch

4632 Trimbach im Februar 2013

Provisorische Steuern 2013, Vorbezugsraten

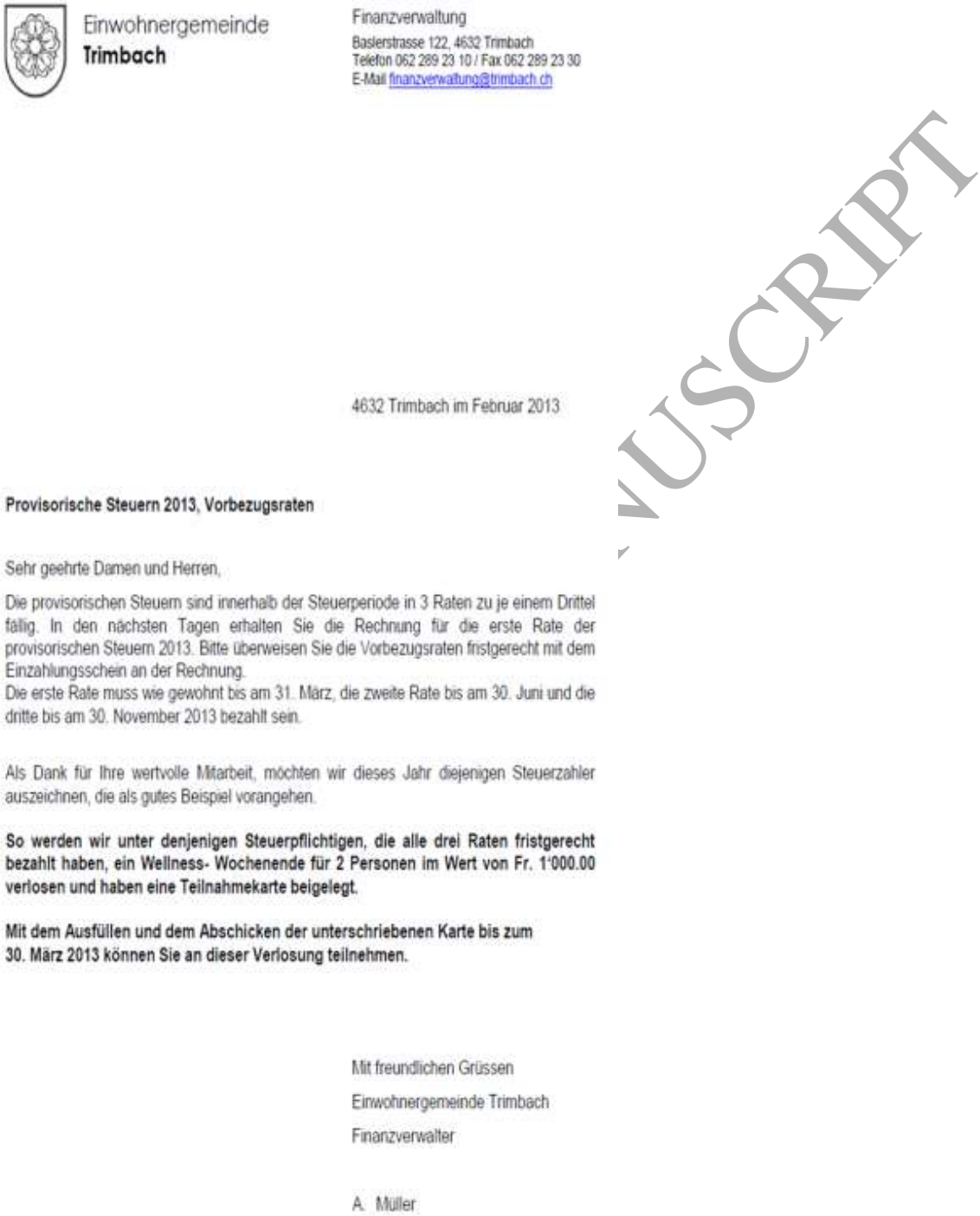
Sehr geehrte Damen und Herren,

Die provisorischen Steuern sind innerhalb der Steuerperiode in 3 Raten zu je einem Drittel fällig. In den nächsten Tagen erhalten Sie die Rechnung für die erste Rate der provisorischen Steuern 2013. Bitte überweisen Sie die Vorbezugsraten fristgerecht mit dem Einzahlungsschein an der Rechnung.
Die erste Rate muss wie gewohnt bis am 31. März , die zweite Rate bis am 30. Juni und die dritte bis am 30. November 2013 bezahlt sein.

Mit freundlichen Grüssen
Einwohnergemeinde Trimbach
Finanzverwalter

A. Müller

Figure A3: Treatment



Einwohnergemeinde
Trimbach

Finanzverwaltung
Baslerstrasse 122, 4632 Trimbach
Telefon 062 289 23 10 / Fax 062 289 23 30
E-Mail finanzverwaltung@trimbach.ch

4632 Trimbach im Februar 2013

Provisorische Steuern 2013, Vorbezugsraten

Sehr geehrte Damen und Herren,

Die provisorischen Steuern sind innerhalb der Steuerperiode in 3 Raten zu je einem Drittel fällig. In den nächsten Tagen erhalten Sie die Rechnung für die erste Rate der provisorischen Steuern 2013. Bitte überweisen Sie die Vorbezugsraten fristgerecht mit dem Einzahlungsschein an der Rechnung.
Die erste Rate muss wie gewohnt bis am 31. März, die zweite Rate bis am 30. Juni und die dritte bis am 30. November 2013 bezahlt sein.

Als Dank für Ihre wertvolle Mitarbeit, möchten wir dieses Jahr diejenigen Steuerzahler auszeichnen, die als gutes Beispiel vorangehen.

So werden wir unter denjenigen Steuerpflichtigen, die alle drei Raten fristgerecht bezahlt haben, ein Wellness- Wochenende für 2 Personen im Wert von Fr. 1'000.00 verlosen und haben eine Teilnahmekarte beigelegt.

Mit dem Ausfüllen und dem Abschicken der unterschriebenen Karte bis zum 30. März 2013 können Sie an dieser Verlosung teilnehmen.

Mit freundlichen Grüssen

Einwohnergemeinde Trimbach

Finanzverwalter

A. Müller

Note: In the WELLNESS treatment the last sentence was deleted.
Translation

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Provisional Taxes 2013, pre tax rates

Dear Sir or Madam,

The provisory taxes are due during the tax period in three instalments, with a third of the tax liability each. In the next few days, you will receive an invoice for the first pretax instalment. Please transfer the pretax amount on time using the form attached to the invoice.

As usual, the first instalment must be paid by March 31, the second by June 30, and the last by November 30.

To thank you for your valuable help, this year we will honour those tax payers who lead by good example.

As a reward for their valuable collaboration, all taxpayers that pay all three pretax instalments on time will

[be entered into a lottery to win a cash prize of 1,000 CHF.]

[be entered into a lottery to win a wellness weekend for two valued at 1,000 CHF.]

Addendum for the promise treatments:

To be eligible for the lottery, please also sign the attached card and return it to the tax administration by March 31.

Yours sincerely

Tax administrator

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FIELD - Information on pretax bill

Important Amendment:

Non-paid pretaxes will get dunned after expiration of the payment deadline.

This new procedure was determined by the local council due to the diminishing payments as of November 6, 2012.

Reason:

The municipality is paying current expenditures with the tax revenues. If the needed money is missing, the municipality has to borrow money and needs to pay interest and fees. Hence, in the municipal assembly, the citizens set the following regulations and gave the tax administration the following instructions:

Extract from the tax regulation, 2010, 01.01.2008:

§ 11, Passage 2:

As a general rule, taxes are paid in 3 *instalments at a third of the pretax liability.*

The due dates are

- *First instalment: March 1, payable up until March 31*
- *Second rate: May 31. payable up until June 30*
- *Third rate: October 31, payable up until November 30*

§ 12, Passage 1:

Tax payments must be made within 30 days of the due date.

Missed payments will be dunned. *For each dunning, a fee will be charged based on the fee regulations.*

Payment problems:

At the explicit request of the taxpayer, the tax administration can split the annual tax liability into monthly instalments. Nevertheless, interest may be owed as a default penalty according to tax regulation § 12, Passage 1.

Figure A4: Original Text

Wichtige Änderung:

Nicht bezahlte Vorbezugsrechnungen werden nach Ablauf der Zahlungsfrist gemahnt ! Dies hat der Gemeinderat aufgrund einer schlechter werdenden Zahlungsdisziplin am 6.11.2012 beschlossen.

Warum:

Mit den Steuereinnahmen werden die laufenden Ausgaben der Gemeinde bezahlt. Fehlt das benötigte Geld, müssen Kredite aufgenommen und dafür Zinsen und Gebühren bezahlt werden. Die Einwohner haben deshalb an der Gemeindeversammlung das Steuerreglement beschlossen und damit der Verwaltung den Auftrag wie folgt erteilt:

Auszug aus dem Steuerreglement 2001, Stand 01.01.2008:

§ 11, Abs. 2
Die Steuern sind in der Regel in **der Steuerperiode** in 3 Raten zu je einem Drittel fällig (Vorbezug).

Die Fälligkeiten sind:

1. Rate: 1. März,	zahlbar bis 31. März
2. Rate: 31.. Mai,	zahlbar bis 30. Juni
3. Rate: 31. Oktober,	zahlbar bis 30. November

§ 12, Abs 1
Die Steuer muss innert 30 Tage seit der Fälligkeit entrichtet werden. **Säumige Steuerpflichtige sind zu mahnen**. Für jede Mahnung wird eine Gebühr gemäss Gebührenreglement erhoben.

Bei Zahlungsproblemen:

Auf ausdrücklichen Wunsch einer steuerpflichtigen Person kann die Finanzverwaltung die Jahressteuer auf Monatsraten aufteilen. Ein allfällig daraus entstehender Verzugszins bleibt gemäss Steuerreglement § 12 Abs. 2, geschuldet.

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