TAX AMNESTY AND POLITICAL PARTICIPATION *

ABSTRACT

In many countries thinking about a (new) tax amnesty is currently in vogue. However, cross-national experience shows that the financial success of such a tax amnesty is not granted. Furthermore, it is debated whether in the long run tax amnesties undermine tax compliance. To measure the long run effects of an amnesty on compliance, experiments in different countries were conducted. In contrast to other experiments, we conduct an experiment in which the relationship between tax compliance and subjects' possibility to vote for or against an amnesty is analyzed. The results obtained from two experiments done in Costa Rica and Switzerland suggest that tax compliance only increases after voting, when people get the opportunity to discuss prior to ballots. Thus, voting with discussion induces a kind of civic duty, as taxpayers become aware of the importance to contribute to the provision of public goods.

I. INTRODUCTION

Tax amnesties are in vogue! In November 2001 Italian Finance Minister Giulio Tremonti declared a new tax amnesty "scudo fiscale" which expired in May 2002. According to the Italian government some 56 billion Euros of exiled money have returned to the fold during the amnesty. The returning money came in for a 2.5% tax so that the Italian government produced 1.4 billion Euro additional tax revenues (about 0.4 % of total tax revenues). Now, the Italian government discusses already a new tax amnesty "super-scudo" which will expand the remission to firms. Similarly, the Polish government enacted a tax amnesty from September 2002 till April 2003 where the declared money is taxed by 12%. In Summer 2002, the German chancellor, Gerhard Schröder, brought up a tax amnesty for discussion, with the intention to induce a major reflux of German flight capital laying in tax havens abroad. In the US, most states have made experiences with tax amnesties. From November 29, 1982 till the present day, a considerable number of more than 60 amnesty programs have been conducted in US states, indicating strong variation of the repatriated revenues among the states see, e.g., http://www.taxadmin.org/fta/rate/amnesty1.html and Hasseldine 1998, p. 307).

In general, during the last 30 years national amnesty programs have taken place virtually all over the world (see *Table 1*). Such a huge political interest in tax amnesty programs might suggest that tax amnesties are a major financial success for the government at least in the short run. However, as can be seen by the collection rate as a percentage of tax revenues, the financial success among the countries is very diverse. Similar, in a comprehensive overview of 43 tax amnesties in 35 US states between 1982 and 1997, Hasseldine (1998) shows that the highest amount of money collected through a tax amnesty did not exceed 2.6 % of total tax revenues whereas the lowest collection rate accounted for 0.008 %, only. Furthermore, it is debated whether in the long run tax amnesties undermine tax compliance. For example, honest taxpayers may feel upset by an am-

nesty. If most taxpayers voluntarily comply with tax laws, the option of an amnesty given to a small group of tax evaders can be understood by a majority of taxpayers as a violation of equity. The issue has a moral dimension since it touches sentiments of taxpayers. Thus, it is also possible that an amnesty ends up in a lower ex-post level of tax compliance.

When deciding whether or not to conduct an amnesty it is crucial to take taxpayers' attitude towards an amnesty into account. However, in hardly any country this was done by voters' approval[1]. The aim of this paper is to evaluate the impact of voter participation on tax amnesties conducting a laboratory experiment. We show that voters in general do not like tax amnesties. Even after discussing the issue prior to voting, such a "soft option" given to tax evaders is refused. Voters might interpret the remission given by the government as a signal that tax evasion must be high and that other taxpayers' tax morale[2] is very low. Thus, voters don't want to reward tax evaders with an amnesty. Nevertheless, the results of our experiment show that the mere possibility for taxpayers to decide on a tax amnesty increases future tax compliance. It seems that the voting procedure, namely public discussions prior to votes, is bringing about a sense of civic duty, as taxpayers become aware of the importance to contribute to public goods. Another reason why tax compliance raises after the votes lays in the possibility to reduce the likelihood of stricter enforcement efforts.

II. THEORETICAL CONSIDERATIONS AND HYPOTHESES

Tax amnesties are disputed in the tax compliance literature. On the one hand, a tax amnesty in the short run can generate an increase in the tax revenue and reduce administration costs (e.g., back-log of paperwork and arrears, see Alm 1998). Furthermore, it might get evaders "back to the route of honesty". This is particularly important when correct declaration is difficult due to a

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[1] An important exception is Switzerland where the latest tax amnesty in 1969 only passed a popular referendum after a major revision of the original law which had been refused in 1964.
[2] Tax morale is defined as the" intrinsic motivation for individuals to pay taxes" (Alm and Torgler 2004, p. 2).

complex tax system. Leonard and Zeckhauser (1986) point out that some people become tax delinquents only by mistake.[3] Such individuals might be willing to correct their behavior to become honest citizens when they are not confronted with punishment mechanisms as prosecution and penalties. Thus, future non-compliance might be reduced integrating former tax delinquents into the taxation procedures.

On the other hand, there are also disadvantages of tax amnesties. Honest taxpayers get informed about the presence of tax evasion, i.e., that other taxpayers are less compliant (see also Alm and Beck 1993). Thus, previously honest taxpayers often view an amnesty as unfair and feel less motivated to comply in the future. They interpret the amnesty as a signal that tax evasion is a forgivable and insignificant peccadillo (see, e.g., Leonard and Zeckhauser 1986). This might increase their belief to have paid too much in the past compared to other taxpayers. Therefore, the psychological costs of not complying are reduced when observing others' opportunistic behavior, which results in a crowding out of the intrinsic motivation to comply. Furthermore, an amnesty may induce anticipatory behavior of taxpayers. After an amnesty, previously honest taxpayers anticipate further amnesties by reducing their tax honesty (see Leonard and Zeckhauser 1986). All in all, the success of an amnesty does not only depend on its short-run revenue effects but also on the long-term effects on tax compliance.

Fisher, Goddeeris and Young (1989) point out that those individuals who were most involved in tax evasion getting the largest benefits do less likely participate in an amnesty. Compared to other taxpayers they face higher marginal participation costs. Furthermore, participants could fear that the government uses the new information for deterrence activities after the amnesty. The successful Italian tax amnesty in 1982 paid attention to this problem and integrated the

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[3] Joulfaian and Rider (1996) report form an empirical analysis with "Earned Income Tax Credits" that taxpayers' mistakes in their income declarations are quantitatively quite important. This is not only true for underreporting but also for overreporting income.

"condono tombale" with the goal to prevent the tax authority from acquiring information about the evaded tax base (Cassone and Marchese 1995). On the other hand, for taxpayers who found themselves in such a position by accident, the marginal cost of participation is low and an amnesty offers a new start into an honest life. Personal guilt feelings can be reduced.

Empirical evidence on these theoretical effects of a tax amnesty is rare. Naturally, it is difficult to measure the real effects of tax amnesties as, e.g., to get an idea of how many evaders have participated in a tax amnesty since data from official investigations are often not available. Most empirical results are reported from the United States since their database is well developed and because their state amnesties are better comparable than amnesties between countries with very different backgrounds.

In their empirical work with field data, Alm and Beck (1993) analyze the long run effects of the Colorado tax amnesty for the period January 1980 through December 1989. Their time-series analysis indicates that the amnesty in Colorado had virtually no long-run effect on the level and the trend of tax collection, despite the fact that the Colorado Department of Revenue increased the post-amnesty enforcement efforts. In a cross-section analysis including 28 US states, Alm and Beck (1991) empirically analyze the effects of tax amnesties on the total amnesty revenues or the total revenues divided by state population. The results indicate that the participation of known delinquents and a reduction of interest payments on back taxes increase the amnesty revenues significantly[4]. Furthermore, strict post-amnesty penalties and enforcement mechanism also increase amnesty revenues. Alm and Beck (1991) additionally stress the fact that a government can implement all these strategies without costs except the enforcement mechanism.

As field data on tax amnesties are rare, the possibilities for investigations are rather limited. Alm, McKee and Beck (1990) point out that there is a lack of field data on the post-amnesty

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[4] Alm and Beck (1991) point out that the variable delinquent measures a reduction in the amnesty tax rate as criminal penalties are forgiven for delinquents who participate in an amnesty.

impact regarding taxpayers' expectations about future amnesties. Field data pose the problem that it is difficult to separate different effects as, e.g., enforcement efforts and changes based on the tax amnesty (see Alm and Beck 1993). Experiments offer the possibility to get own data and to check specific circumstances, which are difficult to control in field studies (for a survey see Torgler 2002). Tax amnesty experiments help to control this problem since they allow analyzing the effects of different tax amnesty structures in the long run.

To the authors' knowledge there are hardly any tax amnesty experiments. Alm, McKee and Beck (1990) found in an experiment that the average level of compliance falls after an amnesty. However, taxpayers who revealed a high compliance before an amnesty, continued to be compliant afterwards. On the other hand, subjects with a moderate tax compliance rate reduced their compliance in the post-amnesty phase. The authors found that a successful strategy to increase tax compliance after an amnesty is to intensify enforcement efforts. Enhancing the enforcement mechanism increases the cost of evasion and thus reduces the cost of participating in an amnesty. Short-term revenues support the transition to a new tax system (Graetz 1999). It might be seen as a fair warning, especially for those taxpayers who were honest before the tax amnesty. It aims at convincing tax delinquents that the probability of getting caught increases signaling that tax evasion is morally wrong (Fisher, Goddeeris and Young 1989). Alm, McKee and Beck (1990) also found that the anticipation of a further amnesty increases if individuals get the opportunity to participate in an amnesty although the government had stressed that no further amnesty will take place. The government loses credibility and makes evasion seemingly forgivable. Taxpayers get the incentive to wait for further grace periods to be reconsidered freely. In our experiment we will check for these effects. According to Alm (1999) experiments should be administered in a uniform and consistent manner to allow replicability. This allows testing the robustness of the design and prevents from erroneous conclusions.

However, our main focus in this paper is a different one. In contrast to other experiments, we analyze the relationship between tax compliance and subjects' possibility to vote for or against an amnesty. Previous approaches have been given little attention to analyze whether the ability of individuals to vote on an amnesty influences compliance. Our experimental evidence shows that voting on tax issues has a positive effect on tax compliance using a non-amnesty design (Alm, McClelland, and Schulze (1999) and Feld and Tyran (2002). Similar tendencies can be observed with field data. Pommerehne and Weck-Hannemann (1996) find in a cross-section/time series regression with Swiss data that tax evasion is lower in cantons with a higher degree of direct political control. Torgler (2004) provides evidence that the stronger direct democracy is established in a jurisdiction the stronger tax morale using Swiss survey data. Furthermore, Alm and Torgler (2004) analyzed tax morale in the United States and in Europe. The results based on a multivariate analysis indicate that compared to other countries in the United States and in Switzerland, two countries with a strong direct democratic tradition, higher tax morale has emerged. Feld and Frey (2002) conclude that differences in the treatment of taxpayers by the tax authority are decisive based on their empirical results using a data from Switzerland. From an institutional perspective, the relationship can be understood as a "psychological contract". The more taxpayers are able to participate in the political decision making process by popular rights, the more this contract is based on trust, which fosters tax morale. This is similar in the case of a tax amnesty.

We predict that voting possibilities have a positive effect on tax compliance. The voting procedure, especially public discussions prior to votes, creates a sense of civic duty, as taxpayers become aware of the importance to contribute to public goods. Voting possibilities provide utility in itself. Citizens value the right to participate, because it produces a kind of procedural utility as the opportunity set increases. It leads to an outcome (acceptance of the amnesty or not) more fa-

vorable compared to the situation where no such voting possibility exists. Thus, the following hypothesis can be developed:

Hypothesis 1: The possibility to vote on a tax amnesty increases tax morale fostering tax compliance.

According to our view, a key determinant in the voting procedure consists in the aspect of discussion. It allows for an exchange of arguments enhancing group identification. Others' preferences become visible while moral costs of free-riding increase, which has a positive effect on tax compliance. If discussion is possible prior to votes, citizens are confronted with arguments from both sides, those favoring and those opposing a certain outcome increasing the overall level of information. Thus, the outcome of the ballot is based on a comparatively higher level of information. Additionally, citizens become involved and feel responsible for the result. The voting and discussion procedure creates a sense of civic duty, as taxpayers become aware of the importance to contribute to public goods. Their interaction in a face-to-face situation gives citizens the opportunity to identify others' preferences, which may also enhance people's willingness to accept the final voting decision (see Bohnet and Frey 1994). Alm, McClelland and Schulze (1999) argue that there is a social norm of tax compliance affecting individual reporting decisions. Their findings indicate that communication combined with the vote influences tax compliance, so that paying taxes becomes the accepted mode of behavior. Discussion gives the opportunity to clarify benefits and costs of a topic and thus increases co-operation among group members. In general, Alm (1996, p. 123) points out surveying his experimental findings: "I believe that the cheap talk in combination with vote allows individuals to change the social norms, in this case to demonstrate that evasion will not be accepted".

Based on these considerations, we decompose the voting parameter into voting without discussion and voting with discussion. Thus, the following hypothesis can be developed:

Hypothesis 2: Discussion prior to the vote more strongly fosters tax compliance as compared to a voting procedure without discussion.

III. DESIGN OF THE EXPERIMENTS

1. General Structure of the Experiment

We have conducted experiments in Switzerland and Costa Rica. 122 subjects have participated in the experiment, 68 in Switzerland and 54 in Costa Rica. The experiment in Switzerland has been done at the University of Basel, in Costa Rica at the University INCAE in Alajuela and the University Fidélitas in San José. Almost all subject participated for the first time in an experiment[5]. The experiment lasted about an hour (25 rounds) and participants earned between 7 and 20$ in Switzerland and between 5 and 15$ in Costa Rica depending on the individually accumulated amount of money at the end of the experiment[6]. Each session consisted of 25 rounds. Subjects did not know when the experiment ended in advance. It was not allowed to communicate with each other, except in the situation where discussion was explicitly promoted by the experimenters (session 5). We did not use tokens as currency but fictive lab Dollars. The income distribution was exogenous as all subjects received the same income in every period (200 lab dollars). Thus, the obtained income per round did not change during the 25 rounds. Certainly, one can argue that distributing the same income throughout the 25 rounds might be boredom for the subjects. However, a change in income might produce biases, which we wanted to avoid since treatment

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[5] We controlled with a survey questionnaire after the experiment whether a subject had already experiences with experiments in general (participated as a subject in an experiment in his life). In Switzerland, 4 out 68 had already participated in an experiment in their life. In all cases, the experiment was not related to the one we conducted here. In Costa Rica nobody has previously attended an experiment.

[6] The difference in the payment amounts between Switzerland and Costa Rica are deduced from price comparisons of homogenous goods among different cultures (Coca Cola, Big Mac and a cinema ticket price).

changes occured within a session. The experiment implemented a public good structure. The taxes on the declared income were doubled and then redistributed in equal shares to the members of the group. After a round subjects' net income could thus be calculated as income after taxes plus share of the multiplied group tax fund. The tax rate was held constant (20 percent). *Figure A1* in the Appendix presents the declaration monitor screen. Subjects were told that all the accumulated earnings during the experiment would be redeemed for cash at the end of the experiment at a fixed conversion rate. The complete experiment, with the exception of a short instruction sheet at the beginning, was conducted on computers and was programmed with z-Tree (Zurich Toolbox for Readymade Economic Experiments, Fischbacher 1998). The experimental software is interactive. Subjects were informed in each round about the audit probability, penalty, the accumulated income (fortune) and the individual tax redistribution.

The use of a computer allows for minimal experimenter-subject interaction during experimental sessions, which reduces possible framing effects. Furthermore, a computer system facilitates the accounting process (income distribution, tax redistribution, and the accumulation of the income). Before playing 25 rounds in every session, 3 rounds took place to make sure everybody understood the program. Subjects were informed that the performance in the practice periods did not affect their payments. All in all, we believe people were well informed about the different tax parameters and they were confronted with a tax context language. We used tax terms such as income to declare, tax rate, audit probability, fine rate, to integrate contextual factors which are important in determining tax reporting behavior. This helps perceiving the experiment not as a mere gamble. However, it can be criticized that tax terms may bias subject choices (Alm and McKee 2004). For example, subjects' responses may be biases because of certain values they associate with words such as taxes, audits or fines. Most of the tax compliance experiments use neutral terminology though Alm, McClelland and Schulze (1992) concluded that there is no dif-

ference in behavior between experiments that use neutral terminology and those that use a tax specific language using students in their experiment. In addition to the experiment, subjects also completed a post-experimental questionnaire. The questionnaire helps to control for gender differences.

Contrary to many tax compliance experiments we assume that the tax agencies use information from the returns to determine audit. Such an experimental design is closer to reality since in many countries we can observe a selection of returns (Discriminant Index Function in the United States, DIF, see, e.g., Alm, Cronshaw and McKee 1993, Roth, Scholz and Witte 1989). Thus, our experimental design considers endogenous audit selection rules. If a subject is controlled and found to evade taxes, the previous four periods were controlled. All the unpaid taxes including a penalty on unpaid taxes of the same amount (fines rate = 2) must be paid. If the controlled subject has reported all income, the previous periods are not examined. Thus, tax agency goes back in time to previous period's declarations. Furthermore, the audit probability increases from 5% to 10% depending on the amount of non-declared income between this period and last period's declaration as a tax administration may strongly react to observed differences between two declarations[7]. In such an experimental design the probability of audit is endogenous, depending on the behavior of taxpayers throughout the experiment.

We paid attention to reduce problems, which arise in conducting a cross-culture experiment (see Roth, 1995, pp. 282- 284). The main experimenters were the same in Costa Rica and Switzerland, to eliminate possible variations arising from uncontrolled procedural differences or uncontrolled personal differences between the experimenters. All instructions were presented in the same language (English) in both countries. Otherwise systematic differences between coun-

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[7] Comparisons between the years may help a tax administration to make a pre-selection before choosing the tax forms to be analyzed more closely. Thus, the audit rate does not only increases for the subject audited. It is a linear increase from 5% to 10%. For example, a subject that does not evade taxes between two rounds will be controlled

tries might arise due to the way the instructions are translated. Furthermore, as already mentioned, payments given to the subjects are adapted to the situation in the country. Thus, differences in the degree of compliance are not caused by differences related to the experimental payments.

2. Experimental Sessions

Six sessions with different sets of individuals are conducted (see *Table 2*). The design of the session is in line with government policy strategies. In session 1[8], the control case, no amnesty is granted. In session 2[9] an amnesty is introduced after round 13. At this point, the subjects were given further instructions in the monitor (see screen in Figure A2 in the Appendix). Subjects do not have any information about the possibility of a tax amnesty. In session 3, the probability of audit and penalty has been doubled[10]. Such an experimental structure goes in line with many real amnesties. A tax amnesty indicates that the system fails to enforce the law. An increase in the enforcement regime indicates that the state is willing to find solutions to the tax evasion problem. In sessions 2 and 3 subjects were told that the amnesty would be a one-time opportunity to pay unpaid taxes back. In session 4 to 5 subjects had the possibility to decide whether they wanted an amnesty or not[11] after round 13. In line with previous session 3, subjects didn't have the information about the possibility of voting before round 14. In session 5, people have the possibility to discuss five minutes with each others before giving their vote[12]. The decision whether they wanted an amnesty or not in session 4 and 5 was based on simple majority vote (see the screen

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with a probability of 5%. On the other hand, a subject that first was honest and than evades all the income will be controlled with a probability of 10%.

[8] 9 subjects in Costa Rica (Experiment 1), 11 subjects in Switzerland (Experiment 2).

[9] 7 subjects in Costa Rica, 8 subjects in Switzerland.

[10] 7 subjects in Costa Rica, 12 in Switzerland.

[11] 11 subjects in Costa Rica, 9 and 5 in Switzerland (the session has been done twice in Switzerland).

session 5 in *Table A3*). This allows to see whether cheap talk matters. Session 6 analyses the effects of taxpayers' expectations of future amnesties. The first amnesty has been declared without previous warning after round 10. Subjects have been informed that no further amnesties were going to take place. However, contrary to this announcement, subjects were again confronted with an amnesty after round 18[13]. The parameters of the experimental design are summarized in *Table 2*.

[TABLE 2 ABOUT HERE]

3. Individuals' Payoff

In this subsection we determine the optimal one period strategy for a subject in the tax compliance experiment. We assume that the individual's goal is to maximize the expected value and that an individual takes the actions of others as given. We can then define the expected value from the choice of how much income to report in line with Alm et al. (1999) as:

$$EV = Y - t\,Y^D + m\,s\,(G + t\,Y^D) - p\,f\,(\,t(Y - Y^D)) \tag{1}$$

where:

Y is income before taxation

Y^D is the declared income

t is the tax rate

m is the surplus multiplier

s is the individual's share of the group tax fund

G are taxes paid by all other group members, thus, $G + t\,Y^D$ are the total group taxes

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[12] 11 subjects in Costa Rica, 13 in Switzerland.

p is the probability of detection and, f the fine rate on unpaid taxes.

If we maximize equation (1) by the declared income Y^D, individuals will report the whole income if:

$$p f + m s \geq 1 \qquad (2)$$

Applying condition (2) to the reference group according to the values in *Table 2* we receive a value of 0.322 (Exp. 1) and 0.282 (Exp. 2) which are below 1 and would mean that the optimal strategy for the individuals in the reference groups would be to evade the whole income. However, two limitations should be put into account using the presented model. First, the endogenous audit selection rule is not integrated into the model. One would expect the values to be higher. Furthermore, the model presented does not integrate the aspect that the game covers more than one period. Effects of previous experiences or wealth changes are ignored. Subjects might learn during the experiment. Generally, literature on voluntary contribution mechanisms and social dilemmas shows time and again that public good contributions decline with each repetition (see, e.g., Isaac and Walker 1988, Andreoni 1988, Dawes and Thaler 1988).

The opportunity of voting and chance of an amnesty may affect individuals' social norm of tax compliance. This makes it relevant to introduce the role of social norms in Eq. (1). In line with Alm et al. (1999), the following extension can be done:

$$EV = Y - t\,Y^D + m\,s\,(G + t\,Y^D) - p\,f\,(\,t(Y - Y^D)) - \alpha t(Y - Y^D) \qquad (3)$$

The value α (fraction) can be seen as a tax morale coefficient. The higher the non-compliance *(Y - Y^D)* and thus the lower the paid taxes *t*, the higher the psychological costs and thus the psychological loss in the expected income. In other words, α measures how much an individual would pay to avoid the psychological costs or the loss associated with each dollar of unreported taxes. Condition (2) has now the following structure:

$$p f + m s + \alpha \geq 1 \qquad (4)$$

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[13] 10 subjects in Costa Rica, 7 in Switzerland.

The voting procedure increases the psychological costs of evading taxes. The value α thus increases and thus condition (4) gets more easily satisfied than condition (2). However, clear predictions whether a full declaration of the income is now an optimal decision cannot be derived, as we have no information about the magnitude of α. Amnesties may also change tax morale. For those taxpayers who can get back to the "route of honesty" and especially for those who became delinquents by mistake the possibility of an amnesty may increase tax morale (higher α).

On the other hand, tax morale of honest taxpayers may be undermined as they feel upset about an amnesty (decrease of α). However, an increase of tax enforcement after an amnesty may signalize that tax administration tries to improve compliance, which may reduce a crowding out of honest taxpayers' tax morale. Offering more than one tax amnesty although subjects were told that only one amnesty would take place reduces tax morale. Government's credibility to enforce taxation is harmed signaling that tax evasion is a "peccadillo" so that the anticipation of further tax amnesties emerges, i.e. tax evasion increases.

IV. EXPERIMENTAL RESULTS

1. Descriptive Findings

In our experiment, the dependent variable measures individuals' compliance rate *(CR)* in a given round, specified as the ratio of the reported income *(RI)* to the true income *(TI)* in a specific round, i.e., *CR = RI/TI*. First, we present in *Table 3* the average compliance rate across all sessions, differentiating between the pre-amnesty and the post-amnesty periods. Taking both experiments together we observe that for the reference group (session 1) the average compliance rate in the pre-amnesty period is in line with the post-amnesty period. On the other hand, compliance rate of the other sessions (treatment groups) increases in the post-amnesty period (except for

session 4). We observe differences between experiment 1 and 2. In experiment 1, compliance rate is in general higher and in session 4, contrary to experiment 2, we observe an increase in the compliance rate in the post-amnesty period.

TABLE 3 ABOUT HERE

In *Figure 1* to *3* we also present average compliance rates across rounds in a given session. In general we observe a high compliance rate across time. Individuals are clearly more compliant than our previous subsection model based on the expected utility theory would predict (see control group). In fact, the average compliance rate in any particular round never falls below 40 percent. Surprisingly, in most of the sessions, the compliance rate does not tend to decline over time. This result is not in line with many studies on voluntary contribution mechanisms and social dilemmas that show a public good contributions decline with each repetition (see, e.g., Isaac and Walker 1988, Andreoni 1988, Dawes and Thaler 1988).

The decay is observed when subjects know the length of the game for sure as well as when they do not. Two hypotheses are often proposed: strategies and learning (Andreoni 1988). The learning hypothesis holds that repeated games allow individuals to learn the incentives. Some learn more quickly than others but on average compliance decays towards zero. However, Andreoni (1988) states that there is a reverse effect as repetition allows subjects to signal future moves to each other. The strategies hypothesis holds that in an incomplete information situation, a subject may believe that other group members will possibly comply. If an individual free rides, she or he will educate the other participants. As a consequence, co-operation decreases.

FIGURE 1 TO 3 ABOUT HERE

2. Multivariate Analysis

1. Model

In general, *Figures 1* to *3* indicate the relevance to conduct a multivariate regression analysis to better investigate the causes and effects of the different treatments implemented in the experiments. We are going to use different models to better check the robustness of the obtained findings. First we present a Tobit maximum likelihood estimations[14] as the compliance rate *CR* varies between 0 and 1 and there are many observations with the values 0 and 1. To include the panel-structure of the data, we additionally include the random-effects function in order to control time-specific effects. The random-effects model is appropriate if we assume the individual specific constant terms as randomly distributed across cross-sectional units. Because of the presence of the public good, which means that one subject's payoff depends upon the behavior of all other subjects in a group, it seems to be reasonable to add pooled least squares estimations, clustering over groups[15]. Our estimation equation reads as follows:

$$TXP_{\iota\tau} = \beta_0 + \beta_1 \cdot XTP\Lambda_{\iota\tau} + \beta_2 \cdot AM1_{\iota\tau} + \beta_3 \cdot AM2_{\iota\tau} + \beta_4 \cdot \varsigma OTE_{\iota\tau} + \beta_5 \cdot \Gamma EN\Delta EP_{\iota} + \varepsilon_{\iota\tau}$$

where TCR_{it} denotes the tax compliance rate. $CTRL_{it}$ is a panel of control variables including a dummy variable equal to 1 if the individual was audited in the previous round and 0 otherwise,

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[14] The Tobit model assumes that the disturbance term has a normal distribution. However, the criteria of unbiasedness and efficiency do not depend on this assumption. Furthermore, if the sample is moderately large like in our estimations (3050 observations), normality of the disturbance term is not required in order to guarantee that the confidence intervals and p values are accurate. The "Central Limit Theorem" indicates that the confidence intervals and the p values are good approximations even when the disturbance term is not normally distributed if an estimation has anything more than 200 cases (see Allison 1999).

[15] Clustering allows helps to deal with the fact that the number of subjects varies in each session.

the nominal fine for tax evasion, and the transfer payment obtained in each period. $AM1_{it}$ is a dummy variable that compares the pre-amnesty period with the post-amnesty period (value=1) whereas $AM2_{it}$ considers the case for a second amnesty. $VOTE_{it}$ is the dummy variable of interest that differentiates between the pre-voting and the post-voting period. Furthermore, with the dummy variable $GENDER_i$ we differentiate between women and men. To analyze whether there is a difference in tax compliance when the voting procedure is accompanied by public discussions prior to the ballots in contrast when this option is not allowed, we decompose the dummy variable voting into the dummy variables VOTING WITH DISCUSSION and VOTING WITH-OUT DISCUSSION. We also differentiate whether enforcement increase in the post-amnesty period (TAX AMNESTY WITH ENFORCEMENT) or not (TAX AMNESTY WITHOUT EN-FORCEMENT). Alm, McKee and Beck (1990) found in their tax amnesty experimental study that revenues from an amnesty are greater if post-amnesty enforcement increases.

2. Results

Table 5 presents the results. We report the pooled estimations (experiment 1 and 2 together) and the findings in each experiment independently. Presenting experimental evidence of two different nations allows for a robustness check of our main hypotheses. As we can see most results remain robust through different estimation methods. In the TOTAL regressions we include a dummy variable without reporting it in *Table 5*, differentiating between EXPERIMENT 1 and 2. It can be argued that the audit variable is endogenous. However, a Hausman Chi-square test rejects the hypothesis that the variable is endogenous[16].

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[16] We used the number of times a subject has been controlled (adjusted after every audit) as an instrument for our AUDIT variable.

The coefficient of the variable VOTING WITH DISCUSSION is significant, whereas this does not hold for the variable VOTING WITHOUT DISCUSSION. The coefficient VOTING WITHOUT DISCUSSION is only statistically significant with a positive sign in EXPERIMENT 1. In the EXPERIMENT 2 and the pooled estimation in Eq. 1a and 1b we even observe a negative sign. Thus, the key message is: fostering public communication before casting votes for a tax amnesty favors tax compliance. Communication and identification seems to be a key element to enforce cooperation.[17] Our result is in line with experimental evidence demonstrating that communication supports cooperation (for an overview see Sally 1995). Discussion may clarify benefits and costs of an amnesty and increases the concern about other group members' welfare. Thus, "institutionalized communication opportunities enable individuals to privatize a decision" (Bohnet and Frey 1994, p. 1).

TABLE 5 ABOUT HERE

In general, in both experiments compliance behavior in the post-vote period is clearly different from the pre-vote behavior under the same fiscal regime. This result is very much in line with the experimental findings of Alm et al. (1999) arguing that only voting in combination with discussion has a positive impact on tax compliance. Most interestingly, all groups in experiment 2 rejected the choice of an amnesty. On the other hand, all groups in experiment 1 decided for an amnesty. This suggests that a rejection of an amnesty has a negative impact on tax compliance if voting is not accompanied with cheap talk.

Looking at Eq. 1a and Eq. 1b we observe that tax amnesty has a positive impact on tax compliance. But contrary to the findings of Alm et al. (1990) an amnesty with increased post en-

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[17] Frey and Bohnet (2001) also point out that the discourse between citizens is an important element of a lawful state and allows reaching a consensus.

forcement does not generate higher levels of compliance compared to an amnesty in which post-amnesty enforcement remains constant. In general, the positive impact is driven by the results in experiment 2. Furthermore, using a least squares estimation clustering over groups reduces the statistically significance of an amnesty. On the other hand, the second amnesty did not increase compliance significantly in the post-amnesty period. In most of the cases the coefficient is even negative, but without being statistically significant. These findings support the view that amnesties should not be conducted in short intervals, since individuals anticipate future tax amnesties eventually crowding out tax compliance. These results are in line with the findings of Alm et al. (1990) indicating that amnesty expectations and a reduction of state's credibility lower the positive effect of a tax amnesty.

The economics-of-crime approach would predict that the extent of tax evasion depends negatively on the probability of being caught and the size of punishment in case of being caught. Some empirical findings indicate that a higher probability of being caught discourages evasion (see, e.g., Crane and Nourzad, 1987; Witte and Woodbury, 1985; Dubin and Wilde, 1988; Joulfaian and Rider, 1996). In experiments there is also the tendency that a higher audit rate leads to more compliance (see, e.g., Friedland et al., 1978; Beck et al., 1991; Alm, Jackson and McKee, 1992a, 1992b, Alm, Cronshaw, and McKee, 1993; for a survey see Alm 1999 and Torgler, 2002). However, the pooled cross section time series estimation for Swiss cantons over the years 1970, 1978, 1985, 1990, and 1995 done by Frey and Feld (2002) using tax evasion as dependent variable indicates that the probability of detection has a theoretically unexpected positive sign being not statistically significant, while the size of the fine is statistically significant at the 5 percent level. Beron, Tauchen and Witte (1992) found with tax return data from 1969 a weak deterrent effect from audits on tax compliance. Pommerehne and Weck-Hannemann (1996) found that the coefficients of the probability of detection and the penalty tax rate have a negative sign, but none

of them was statistically significant. Slemrod, Blumenthal and Christian (2001) used a controlled field experiment in Minnesota to analyze taxpayer response to an increased probability of audit. While low and middle-income taxpayers increased their reported tax between 1993 and 1994 relative to the control group, the reported income of high-income taxpayers fell sharply in relation to the control group. Torgler (2004) finds in an empirical study working with Swiss data of the International Social Survey Programme that the effects of deterrence parameters on tax morale are statistically insignificant. In our experiment, the audit probability has not a statistically significant impact and the penalty rate has even a negative impact on tax compliance.

Not surprisingly, a higher group transfer leads to significantly higher tax compliance[18]. Higher transfers give subjects a signal that the group on average behaves honestly. The moral costs of being opportunistic increase. Furthermore, women reveal significantly higher tax compliance than men.

V. CONCLUSIONS

Amnesty programs have lately obtained growing attention in the political process. In situations where the government has revenue shortfalls, alternative instruments as, e.g. tax amnesties, gain importance. Although many tax amnesties have been conducted all around the world, evidence about their (long-term) effects is largely lacking.

The main aim in this paper was to analyze the impact of voter participation on tax amnesties using experiments. There is a lack of field data on the post-amnesty impact. Experiments help to analyze longitudinal effects and check which factors enforce tax compliance. The novel framework in our analysis for the tax compliance literature is to combine a tax amnesty experiment with voting possibilities. Furthermore, conducting two experiments, each in a different country with different cultural and historical background (Costa Rica versus Switzerland) allows

checking whether similar tendencies are observable. Our results provide strong evidence that individuals are more compliant when they face the opportunity to vote coupled with communication among group members prior to the vote. On the other hand, voting without discussion produces mixed findings. Thus, discussion before voting is an essential feature to increase group cooperation. It enhances moral costs of free-riding and thus increases the social norm of compliances generating a higher tax compliance. This result is in line with a previous study done by Alm et al. (1999).

Furthermore, in line with Alm et al. (1990) amnesties tend to increase tax compliance. However, contrary to the findings of Alm, McKee and Beck (1990) an amnesty with an increase in the post-amnesty enforcement parameters does not outperform an amnesty without changes in the enforcement factors. The results also indicate that the effect of a second amnesty does not improve tax compliance. The coefficient is mostly negative, but without being statistically significant. Amnesty expectations reduce the positive effects of an amnesty. When the state does not keep its promise, tax compliance decreases. Such a result has a strong policy implication. If a state has the intention to increase the long-term effects of a tax amnesty, its commitment should be reliable, and only one amnesty should be conducted per generation.

Generally, our documented results indicate that there are limitations of the economics-of-crime approach. The results show the importance to incorporate the role of societal institutions and social norms into tax compliance models to better understand why so many individuals comply.

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[18] In each round, the group transfer sum of the previous round was shown on the screen. Subjects could see in the monitor in each round their group transfer sum from the previous round.

APPENDIX

Figure A1

Income Declaration

Figure reproduced as described:

INCOME TAX DECLARATION

Tax Policy Information:

Tax rate: **20 %**
Probability of audit: **5 %**
(increases with an increasing difference between this year's declared income and last year's declared income, max. 10 %)
If you were selected for an audit, the actual and the declared income for the previous 4 rounds are compared. If you did not fully comply, any back taxes are collected, and a fine equal to the unpaid taxes is also imposed.
Fine rate: **2.0** (200 % of the unpaid taxes)

Personal Information:

taxable income: **200 lab$**
Accumulated Income (fortune): **0 lab$**
therefrom: **0 lab$** state's transfer from last year
Taxes: **0 lab$** from last year.

Declaration:

Herewith I declare an assigned income of:

lab$: 0

Furthermore, you should know that the whole tax revenue from your group is multiplied with the factor 2 and redistributed in equal shares among the participants. Revenue from penalty tax and after taxes is not redistributed. If the whole amount of taxes (i.e., the sum of all single payments of all members of a group of 10 persons) is 100 $, every participant receives transfer payments of 20 $.

OK

Figure A2

Tax Amnesty (Group 2)

Year
1

In this round, you are going to see a special screen. It will display the total income that you did not declare over the previous four rounds, and give you the opportunity to pay any amount of the tax due for the non declared income.

Different from normal treatment, in this tax amnesty round, no fine (tax penalty) will be imposed on the back taxes you report. You make only the tax payment that you report.

In the following rounds, after this tax amnesty round, if audited you will not be subject to tax payments on any income amount that you choose to disclose. However, any remaining income amount will still be subject to tax payment and tax penalty.

This will be the unique tax amnesty round throughout the experiment.

OK

Notes: The screen for Session 3 additional points out "Furthermore, the audit probability will be increased from 5% to 10% and the fine rate from 2 to 4.

Figure A3

Voting (Group 5, with discussion)

Year

1

In this round, you are going to see a special screen. It will display the total income that you did not declare over the previous four rounds, and give you the opportunity to pay any amount of the tax due for the non declared income.

Different from normal treatment, you will first have the possibility to vote for or against a tax amnesty. If the simple majority of the individuals in your group decides for a tax amnesty, no fine (tax penalty) will be imposed on the back taxes you report. You make only the tax payment that you report.

In the following rounds, after this tax amnesty round, if audited you will not be subject to tax payments on any income amount that you choose to disclose. However, any remaining income amount will still be subject to tax payment and tax penalty.

This will be the unique voting possibility throughout the experiment.

You will now have five minutes to discuss this topic with the other members of your group in a pre-voting process. After the five minutes you have to cast your vote.

OK

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Tables and Figures

Table 1

Tax Amnesties Around the World

country	Amnesty Year	form/main taxes covered	Collection ($ Mio.)	% of the tax rev.
Argentina	1987	previously unreported income for investment purpose	virtually no revenue	
Argentina	1995	General tax amnesty	3,900	
Australia	Twice during 80s	Participants in specific avoidance scheme, persons not lodging returns		
Austria	1982	All tax claims prior to 1979	poor results	
Austria	1993	special program to encourage repatriation of untaxed assets	increase of the tax base (around 58 percent)	
Belgium	1984/1985	Income exempted from tax if invested (e.g., government bonds)	poor results	
Colombia	1987	report previously unreported assets or over-reported liabilities	100	0.3 % of gross domestic product
Finland	1982/1984	Surplus Interest Affairs		
France	1982	general tax amnesty	19 (only 2786 participants)	0.007
		special program to encourage repatriation of untaxed assets	22 (only 276 participants)	0.008
France	1986	second special amnesty for assets held abroad		
India	1981	Government bonds designed for untaxed income		
India	1997	general tax amnesty	2,500	8.5
Ireland	1988	general tax amnesty	700-750	4.5
Ireland	1993	general tax amnesty	significantly lower than 1988	
Italy	1982	general tax amnesty	100	15
Italy	1984	Entrepreneurs and self employed	5,000	
Italy	2001/2002	special program to encourage repatriation of untaxed assets	1,400 (in Euro)	0.4
Netherlands	1934, 1940, 1945, 1955	1955, exemption from penalties and interest	very good	
New Zealand	1988	general tax amnesty	18 (good response)	
Portugal	1981, 1982, 1986, 1988	Limited to income taxation	40 % of the forecasted amount	
Russia	1993	enterprises, organisations, private entre-preneurs not liable for any sanctions on unpaid liabilities		
Russia	1996, 1997	enterprises and organisations were allowed to defer payments on the arrears	1996 (1997) negative (positive) but insignificant effect on revenues	
Spain	1977	Exemption from penalty for tax liabilities settled prior to 1976		

Sources: Alm (1998, pp. 5-6), Alm, Martinez-Vazquez, Wallace (2001), Cassone and Marchese (1995, p. 62), Marchese and Privilegg (1997, p. 403). Feld (2002, p. 7), Hasseldine (1998, p. 307), OECD (1990, p. 90), US Joint Committee on Taxation 1998 (JCS-2-98, F 31ff), and Uchitelle (1989, p. 50-52).

Table 2

Parameters of the experimental design

Sessions	Amnesty	Voting	Discussion	Audit Probability	Fine Rate	Tax Rate
S1	no	no	no	5%	2	0.2
S2	yes	no	no	5%	2	0.2
S3	yes	no	no	10%	4	0.2
S4	no	yes	no	5%	2	0.2
S5	no	yes	yes	5%	2	0.2
S6	yes	no	no	5%	2	0.2

Table 3

Average Compliance Rate Pre- and Post-Amnesty Period

SESSIONS	Round 1-13 (pre-amnesty)	Round 14-25 (post-amnesty)	
Total			
SESSION (S) 1 EXP 1 & 2	0.714	0.710	
SESSION (S) 2 EXP 1 & 2	0.804	0.828	
SESSION (S) 3 EXP 1 & 2	0.713	0.750	
SESSION (S) 4 EXP 1 & 2	0.700	0.691	
SESSION (S) 5 EXP 1 & 2	0.805	0.840	
	Round 1-10 (pre-amnesty)	**Round 11-18 (post-amnesty 1)**	**Round 19-25 (post-amnesty 2)**
SESSION (S) 6 EXP 1 & 2	0.673	0.728	0.750
Costa Rica			
SESSION (S) 1 EXP 1	0.709	0.810	
SESSION (S) 2 EXP 1	0.855	0.890	
SESSION (S) 3 EXP 1	0.719	0.742	
SESSION (S) 4 EXP 1	0.802	0.829	
SESSION (S) 5 EXP 1	0.858	0.906	
	Round 1-10 (pre-amnesty)	**Round 11-18 (post-amnesty 1)**	**Round 19-25 (post-amnesty 2)**
SESSION (S) 6 EXP 1	0.667	0.751	0.736
Switzerland			
SESSION (S) 1 EXP 2	0.718	0.628	
SESSION (S) 2 EXP 2	0.760	0.774	
SESSION (S) 3 EXP 2	0.653	0.725	
SESSION (S) 4 EXP 2	0.619	0.583	
SESSION (S) 5 EXP 2	0.760	0.785	
	Round 1-10 (pre-amnesty)	**Round 11-18 (post-amnesty 1)**	**Round 19-25 (post-amnesty 2)**
SESSION (S) 6 EXP 2	0.677	0.712	0.761

Table 4

Description of Variables

Variables	Description
compliance rate	ratio of the reported income to the true income
audit	equal to 1 if the individual was audited in the previous round and 0 otherwise
penalty	total penalty amount after detection
transfers	amount an individual obtains from the group fund at the end of the previous round
voting without discussion	dummy variable (0=pre-voting period, 1=post-voting period)
voting with discussion	dummy variable (0=pre-voting period, 1=post-voting period; discussing offered before subjects had to vote)
amnesty without enforcement	dummy variable (0=pre-amnesty period, 1=post-amnesty period, enforcement variables remain constant)
amnesty with enforcement	dummy variable (0=pre-amnesty period, 1=post-amnesty period, enforcement variables multiplied by a factor 2)
second amnesty	dummy variable (0=pre-second amnesty period, 1=post second amnesty period after the enforcement parameters have been doubled)
woman	gender dummy variable woman=1, man in the reference group

Table 5

Determinants of tax compliance

Tobit	Random-Effects Tobit Regressions						Least Squares, Clustering Over Groups					
	TOTAL		EXP. 1		EXP. 2		TOTAL		EXP. 1		EXP. 2	
Variables	Coeff.	z-Stat.	Coeff.	z-Stat.	Coeff.	z-Stat.	Coeff.	z-Stat.	Coeff.	z-Stat.	Coeff.	t-Stat.
	Eq. 1a		Eq. 2a		Eq. 3a		Eq. 1b		Eq. 2b		Eq. 3b	
a) Deterrence												
audit	-0.009	-0.20	-0.132	-1.25	0.018	0.35	0.016	0.55	0.014	0.24	0.017	0.73
penalty	-0.003***	-7.92	-0.002***	-4.52	-0.003***	-5.45	-0.002***	-4.56	-0.002***	-5.25	-0.001***	-4.19
b) Group Transfer												
transfers	0.001***	2.64	0.002**	2.50	0.001*	1.81	0.001***	2.79	0.001**	2.19	0.001**	2.58
c) Political Participation												
voting with discussion	0.206***	4.60	0.532***	5.64	0.130**	2.31	0.082***	5.34	0.114***	4.98	0.092***	3.31
voting without discussion	-0.117***	-2.89	0.210**	2.51	-0.209***	-3.98	-0.059***	-4.27	0.063**	2.06	-0.127***	-3.59
d) Tax Amnesty												
amnesty without enforc. incr.	0.135***	3.99	-0.082	-1.34	0.122**	2.30	0.058**	2.02	0.001	0.03	0.052	1.69
amnesty with enforc. incr.	0.094*	1.92	-0.046	-0.61	0.147**	2.25	0.014	0.52	-0.025	-0.67	0.032	1.06
second amnesty	-0.081	-1.20	-0.003	-0.03	0.025	0.27	-0.026	-1.14	-0.031	-1.02	0.016	0.71
e) Gender												
woman	0.220***	8.86	0.256***	6.05	0.239***	7.42	0.128***	2.95	0.120***	3.01	0.152**	2.49
Log-likelihood	-2465.023		-944.199		-1503.794							
Prob > chi2	0.000		0.000		0.000							
Number of Observations	3050		1350		1700		3050		1350		1700	
R-squared							0.11		0.12		0.09	

Notes: Dependent variable: tax compliance rate as the ratio of reported income on true income. In the reference group is MAN. In Eq. 1 and 4, an experimental dummy (EXP1=1) has been included. Significance levels: * $0.05 < p < 0.10$, ** $0.01 < p < 0.05$, *** $p < 0.01$.

Figure 1: Compliance Rate in Experiment 1 and 2
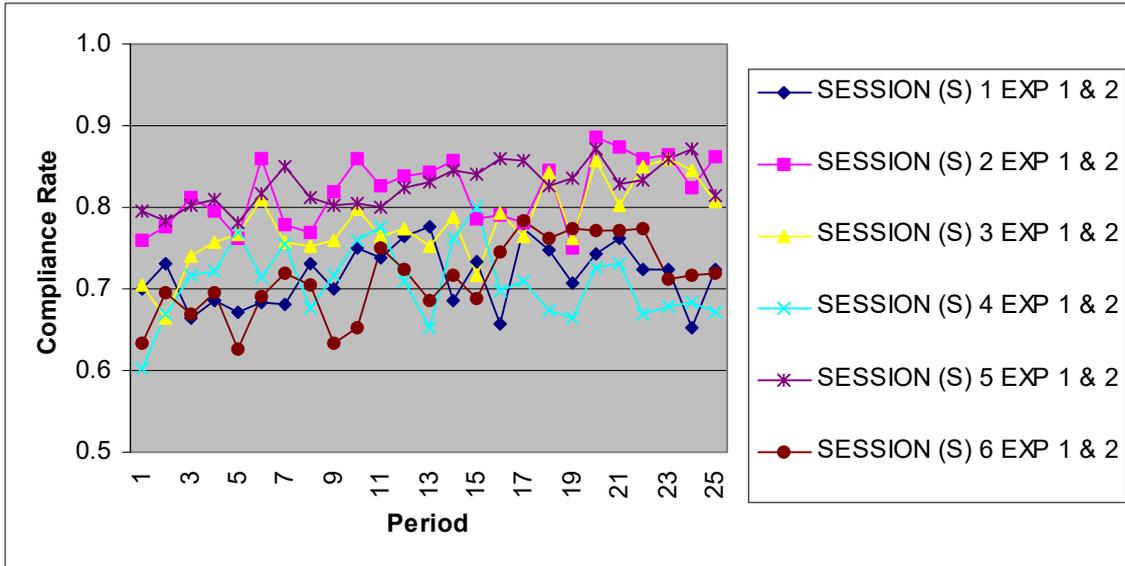
Figure 2: Compliance Rate in Experiment 1
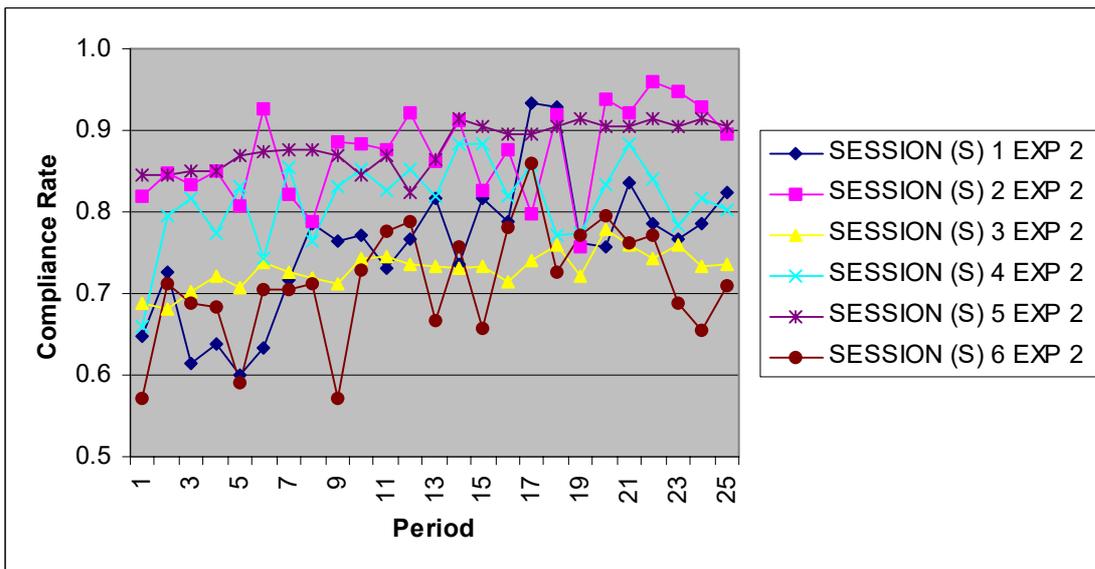
Figure 3: Compliance Rate in Experiment 2 TINR B 11.64