Tax Evasion and Income Source:
A Comparative Experimental Study

by

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and

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Abstract

We compare tax evasive behavior in a country in transition from communism to that in a developed economy by running an experiment across distinct social groups in Albania and the Netherlands. Aside from the tax compliance decision, subjects choose a source of income, where one type enables subsequent tax evasion. We show that they take the possibility of evasion into account when deciding on the income source. This yields potential allocative inefficiency in the labor market. In addition, we argue that the distinct levels of tax evasion outside of the laboratory in the two types of countries are not attributable to different norms or cultures.

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1. Introduction

The underground economy has become a topic for regular academic research. Over the years, numerous studies have emerged, analyzing it in developing and developed countries, as well as in countries in transition from socialist to market oriented economies.[1] The term 'underground economy' refers to a broad phenomenon, including tax evasion, activities against government regulation, illegal activities, and hidden employment. Research usually focuses on any one of these in isolation, even though they are often directly or indirectly related to each other. In this study, we focus on one of the phenomena related to the underground economy: tax evasion.[2]

There are two main reasons for focusing on tax evasion. First of all, it is directly linked to large budget deficits and hence to lower investments in public goods. Besides being of general interest from an economic point of view, this means that the effects may differ significantly depending on the level of development of a country. Second, studying tax evasion creates the opportunity to investigate the decision-making process related to the underground economy at the individual level. Though tax evasion has been studied extensively (Allingham and Sandmo, 1972; Kolm, 1973; Yitzhaki, 1974; Tanzi, 1982, Cowell and Gordon, 1988; Cowell, 1990; Frey and Weck, 1981; Feige, 1989; Pommerehne and Frey, 1994; Andreoni *et al.*, 1998; Johnson *et al.*, 1998; Schneider and Ernste, 2000, 2003 and many others), less research has been undertaken to get a better understanding of the individual decision whether or not to evade taxes (for exceptions, see Friedland *et al.*, 1978; Becker *et al.*, 1987; Alm, 1991; Alm *et al.*, 1992a; 1992b; Feld and Frey, 2002; Feld and Tyran, 2002; Güth *et al.*, 2003).

When studying tax evasion, it is important to distinguish between different types of income. In almost every country, one can make a distinction between income that is officially registered and unregistered income. Registered income is generally observed in jobs within the public sector and in private sector employment with contracts. Typically, taxes are withheld from the regular wage payments when income is registered. Unregistered income can occur in cases where there is no job contract and in case of self-employment. In this case, income must be self-reported to the tax authorities in order to determine the income tax owed.

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[1] For recent surveys, see Schneider and Enste, 2000, 2003 and Gërxhani, 2004a.
[2] Although the literature provides various definitions of tax evasion, there is a common idea underlying them. The term tax evasion simply refers to the underreporting of taxable money income with the intention to escape taxes.

Some (unknown) efficient distribution of labor across jobs with registered and unregistered income will exist. A well functioning labor market will help achieve this distribution. However, it is easier to evade taxes in case of unregistered income than when income is registered. This may provide individual incentives to choose a job where income is not registered. This incentive may distort the way the labor market works and cause allocative inefficiencies in the distribution across types of jobs.[3] The experimental environment we will discuss below distinguishes between the two types of income. This allows us to study the choice of income type (*i.e.*, labor supply) simultaneously with the decision whether or not to evade taxes. Hence, we can discuss the extent to which the evasion possibilities affect the choice of income type.[4]

The extent of tax evasion may be related to a country's economic and institutional development (Feige, 1997; Gërxhani, 2004a). To test this, we study tax evasive behavior in two countries, Albania (an underdeveloped country in transition) and the Netherlands (a developed country). The reason to focus on these two countries is as follows.

First of all, Albania was the most isolated country in Central and Eastern Europe and the latest to open itself up to democratic and economic changes. Therefore, when Albania entered the process of transition, it was struck by a crisis that was even deeper than in other transition countries. The crisis was economic -characterized by severe unemployment, poverty and instability- and institutional -where a collapse of existing institutions was not counterbalanced by the establishment of new ones. This created suitable conditions for the underground economy to be the prevalent economy in Albania. It appeared everywhere. Initially starting as an emergency exit from the numerous problems of the formal sector, it later became an inevitable part of Albanian society. The choice of the Netherlands as the developed country in our study is based on the observation that it is indeed a country with a high standard of living and (contrary to Albania) it is well developed and with stable economic and political institutions. Given our aim to compare the patterns of tax evasion in a country in transition to those in a developed country, the choice of Albania and the Netherlands creates a sharp contrast, which will allow for a fertile basis for testing the difference in tax evasive behavior between these two institutionally distinct types of countries.

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[3] Of course, there may be inefficiencies as a consequence of the income tax *per se* as well.
[4] Collins and Plumlee (1991) analyze a different, but related, issue. They present an experiment where subjects' income (to be reported to the tax authorities) depends on an effort made and observe more effort when tax evasion is possible.

In our study, we apply the experimental method. Although it has been used to study tax evasion before, this only holds for developed countries. An important motivation is that the laboratory provides an opportunity to obtain comparative results from the two types of countries, while controlling for the numerous institutional differences between them. In this way, we hope to get a grip on the individual decision involved and a better understanding of the factors that cause distinct levels of tax evasion.

More specifically, the experiment allows us to compare behavior across countries when *institutions* are controlled for. The choice to evade taxes may be affected by (formal) institutions, by culture (informal institutions), or by both.[5] By controlling for differences in formal institutions, we collect information about the effect of culture and norms of behavior.[6] The existing empirical evidence seems to show that Albanians evade taxes more often than the Dutch do (*cf*. section 2). The experiment will provide us with information as to whether Albanian culture is more open to tax evasion (in which case Albanians will evade more in the experiments) or whether the difference is more likely due to differences (in formal institutions) outside of the laboratory, that are held constant in the experiments.

When running an experiment in different countries, one needs to be careful when drawing conclusions from observed differences. If one observes differences in behavior of one subject pool in country A and another in country B, it might be true that behavior in both countries differs. It might also be the case that behavior differs across subject pools in general. One needs a comparison of differences in behavior across countries to differences within countries before one can truly attribute differences across subject pools to country differences. Our setup distinguishes various subject pools within each of the two countries.

In summary, the contribution of this paper is threefold:

(1)   it presents a new laboratory environment in which subjects can choose between two possible sources of income: 'registered' and 'unregistered';

(2)   it provides a comparative experimental study in a developed country and a country in transition, allowing for a study of the (relative) effect of cultural differences on tax evasion;

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[5] North's (1990) 'new institutional' terminology distinguishes between formal institutions (*e.g.*, tax laws, regulations, fines, etc.) and informal institutions (*e.g.*, tax culture, social norms regarding tax evasion, tax morale, etc). We acknowledge this dichotomy, though we will use the generic term 'culture' when referring to informal institutions.

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(3)   it compares tax evasion across subject pools (*i.e.*, socio-economic groups) within each country. This allows for a comparison of differences across countries to differences within a country.

The organization of this paper is as follows. In Section 2, we discuss the existing evidence of tax evasion in Albania and the Netherlands and review the relevant literature. Section 3 presents the experimental design and discusses the expected treatment effects. Section 4 presents the experimental results, which are discussed in section 5. Section 6 concludes.

2.   A brief review of the literature

Empirical evidence of tax evasion in the Netherlands and Albania

There are more studies about the Netherlands than about Albania. These typically do not refer to tax evasion per se. Instead, they use terms like the 'unobserved' or 'shadow' economy when referring to the underground economy. We assume that tax evasion is a major element in the phenomena they refer to. Boeschoten and Fase (1984) quantify the 'hidden' economy in the Netherlands in 1965–1982. They suggest a rising trend up to about 20% of GDP in this period. On the other hand, Frey and Weck (1981) give an estimate of 9.6% for 1977-1981. Schneider and Enste (2000) report on the average size of the 'shadow' economy for OECD countries, over 1990–93; 1994-95 and 1996-97. Their estimate for the Netherlands is about 13-16%, 13.7% and 13.8% respectively. Finally, Schneider (2002) gives estimates of 13.1% and 13.0% for 1999-2000 and 2001-02, respectively.

There is no quantitative evidence on the existence of tax evasion in Albania during communism (1945-1990). Nevertheless, as everywhere, there was an underground economy. Taxes could be evaded, for example, by not working in official employment or by selling privately grown agricultural products. For the period of transition, numbers on tax evasion were, until recently, mainly based on anecdotal evidence and small-scale sample surveys. For example, the 1996 EBRD Transition Report claims that 70% of the households in Albania do not pay their utility bills (tax bills included). The Albanian Center for Economic Research (ACER 1999) estimates the size of the shadow economy in Albania in 1994-96 to be 19-29%

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[6] In a survey on tax morale and tax compliance in experiments, Torgler (2002) reports that holding the probability of penalty, the fine rate and taxpayer's risk aversion constant, social and institutional factors systematically matter.

of GDP. According to UNDP (2000), the underground economy accounts for an estimated 50% of GDP. In a recent macro-economic study, Feige (2002) applies the 'electric consumption approach' and 'currency ratio model'[7] to Albania and estimates the average size of the Albanian unobserved economy at 65.4% and 48% of the total economy in 1989-2000, respectively. All in all, evidence appears to point towards a higher level of tax evasion in Albania than in the Netherlands.

Tools used to study the underground economy

Previous studies on the Netherlands and Albania have applied the traditional empirical tools in this field, focusing on two main approaches for analyzing the underground economy: the direct and the indirect approach. The direct approach uses sample surveys to gather information about individuals' behavior, whereas the indirect approach derives conclusions about the underground economy from an analysis of macro-economic statistics.

Both methods have serious shortcomings. The indirect approach does not give any information about the specifics of the underground economy (Thomas, 1992). In the direct method there is a potential respondent jeopardy[8] (Lee, 1985). In any survey, there is a problem of respondent's motivation to answer seriously, but this problem appears to be especially relevant in studies of the underground economy and tax evasion.

Recently, a third method is becoming more popular: laboratory experiments. Whereas other methods (direct and indirect) are used to analyze almost every aspect of the underground economy, the experimental method is mostly restricted to tax evasion. The first experimental study on tax evasion is a collaboration of social psychologists and economists (Friedland, Maital and Rutenberg, 1978). They study individuals' behavior when confronted with changes in tax rates, penalties and audit probabilities. This and the following experimental studies on tax evasion are closely related. The basic design of these experiments is described in a survey by Alm (1991). Basically, participants declare taxes based on an income determined in the experiment. They are provided with information regarding tax rates, audit probabilities and fines for cheating (underreporting income). Though there are varieties to this basic design, the results regarding tax compliance are quite robust. These results show that:

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[7] For details on these two methods, see Feige (1979, 1989, 1996); and Kaufman and Kaliberda (1996), Lacko (1996), Eilat and Zinnes (2002).
[8] This refers to the respondents' impression of being threatened by questions.

(i)   tax evasion increases with the tax rate (Friedland *et al.*, 1978);

(ii) tax evasion decreases with the level of fines (Friedland *et al.*, 1978);

(iii) tax evasion decreases with the audit probability (Friedland *et al.*, 1978);

(iv) tax evasion is lower when the proceeds are used to provide a public good (Alm *et al.*, 1992a);

(v)   the decision about tax evasion is made jointly with the decision on how much effort to put in income earning (Collins and Plumlee, 1991);

(vi) a large subset of people never cheat, because they appear to believe that cheating is wrong (Baldry, 1986);

(vii) tax evasion increases with income (Giese and Hoffman, 1999);

(viii) women evade taxes less than men do (Giese and Hoffman, 1999).

For further discussion of the role of experiments in studying tax evasion, see Webley *et al.* (1991). For a discussion of the external validity of laboratory experiments (the extent to which results can be generalized), see Schram (2003).

Cross-national experimental studies

By conducting and analyzing the same experiment in two countries, this paper is related to a variety of cross-country studies that have recently been undertaken (see Brandts *et al.*, 2003 for references). In many cases, differences in behavior are observed across countries (*e.g.*, Roth *et al.*, 1991; Saijo and Nakamura, 1995). Given that the experiments are conducted in exactly the same way in every country, this may cause some concern to economists because economic analysis is traditionally based on the premise that economic behavior is guided everywhere by common principles.

In Brandts *et al.* (2003), however, a public good experiment is conducted and no differences in behavior are found between subjects in Japan, the Netherlands, Spain and the Unites States. The question is then, why differences are observed in some studies and not in others. This might have to do with the subject pools used. As far as we know, no one has compared differences between subject pools within the same country to differences between subject pools across countries. Note that it is implicitly assumed that within country differences are relatively small if cross-country differences are attributed to cultural differences (as is often done). In this paper, we do include different subject pools in each country to get a more complete picture of behavior across countries.

3.   Experimental design and expected treatment effects

General features of the procedures and design[9]

The experiment is run manually in order to make it possible to organize them at various locations without having to arrange computer facilities. In each session there are 12 participants, divided into 3 groups of 4.[10] A session consists of 8 independent rounds. The experiment itself generally lasts less than one hour. Calculating payoffs takes some time, however, so the last subject usually leaves about 75 minutes after the start of the experiment. All sessions reported here took place in 1999-2000.

In the first part of each round, subjects' personal income for that round is determined. Income is independently determined for each round. This income is private information, unknown to the experimenter or other participants. For rounds 1-3 this is all that happens. Aside from allowing participants to get acquainted with the decision at hand, these three rounds allow us to measure the individuals' risk attitudes. We will use this to test the influence of risk attitude on the choice of income and tax evasion. In rounds 4-8 there is a second part of each round where subjects have to report their income to the experimenter. This reported income is taxed. In some cases there is an audit. In case of underreporting, the subject is fined. Tax proceeds are aggregated within a group of 4 and redistributed by dividing them equally among the group members. In this setup, taxes may be seen as contributions to a public good with mpcr = 1 (Isaac *et al*., 1984).

Treatments

Our design distinguishes two treatments, where we varied subject pools and audit probability. A summary of our sessions is presented in table 1, below.

The first important treatment in our experimental design is the distinction of subject pools. For reasons discussed above, these varied along two dimensions: country (the Netherlands, NL, and Albania, AL) and socio-economic category. For the latter we distinguished the following groups: (i) high school pupils (HS); (ii) university students (US); (iii) high school teachers (HT); (iv) university non-academic personnel (UP); and (v)

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[9] See Appendix 1 for the English version of the instructions.
[10] In one session (high school teachers in the Netherlands), there were only eight participants (two groups of four).

university teachers (UT).[11] Sessions in Albania were run at the 'Harry Fultz' high school in Tirana and at the Economics Faculty of the University of Tirana. The sessions in the Netherlands were run at the 'Vossius Gymnasium' high school in Amsterdam and at the University of Amsterdam.[12]

Second, we varied the audit probability for some subject pools. This allows us to test previous results on the effect of audit probability (Friedland *et al*., 1978 report decreasing tax evasion as the probability rises). In addition, it allows us to investigate whether the effect of the probability of being 'caught' is different in the Netherlands than in Albania. For all subject pools, we used a (high) probability of 1/2 of auditing the reported income (as described below). For the students in each country (NL-US and AL-US), we ran additional sessions where the audit probability was equal to 1/6. It will be shown below, that a risk-neutral subject will evade taxes when the audit probability is equal to 1/6 but not when it is 1/2.[13]

In all sessions, groups remain constant across rounds and subjects know how taxes and the public good are determined, but no information is provided between rounds about the tax proceeds themselves. Hence, even within groups, subjects are not provided with any kind of information about other subjects' choices.

Note from table 1 that the average earnings in experimental francs (the currency used in the experiment) across subject pools did not differ much. An exception is that earnings were higher in sessions with a low audit probability. This is a consequence of fewer fines being administered. Note that we varied the exchange rate from francs to local currency in order to account for differences in purchasing power, both across countries and across pools within a country.

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[11] We did not run sessions with university teachers in Amsterdam because we feared that the personal contacts both authors have with most of the faculty could cause serious experimenter effects. One could argue that the subject pools used are quite similar to each other in many ways. Our results will show significant differences across some of these groups, however. With other groups these differences might even grow.

[12] We are grateful to the deans of both high schools and of the Faculty of Economics in Tirana for giving us the opportunity to run our experiment at their schools.

[13] After analyzing the results, we decided to run four additional sessions for students in Albania (AL-US). Two were run with no audits (probability equal to 0), because this group appeared to be insensitive to the audit probability. Sessions without audit allow us to test the robustness of this result. In the other two sessions for AL-US tax proceeds were not returned to the participants in any way. After observing a relatively high level of compliance in the Albanian sessions, we considered the possibility that Albanian participants might be complying to taxes because they wish to contribute to the public good. These new sessions were meant to test this conjecture. The current paper focuses only on the main treatments. In section 5, we will briefly discuss the results of the additional sessions. More information is available from the authors.

Table 1: Experimental sessions

country	group	acronym	# sessions/ #subjects	Audit Probability	Average earnings	Exchange rate
Albania	University Students	AL – US	2/24	1/2	3490	100 fr = 33 lek
	University Students	AL – US	2/24	1/6	4284	100 fr = 33 lek
	High School Pupils	AL – HS	2/24	1/2	3160	100 fr = 33 lek
	High School Teachers	AL – HT	2/24	1/2	3447	100 fr = 50 lek
	University Teachers	AL – UT	2/24	1/2	3334	100 fr = 50 lek
	University Personnel	AL – UP	2/24	1/2	3177	100 fr = 50 lek
Nether-lands	University Students	NL – US	2/24	1/2	3315	100 fr = fl.1.00
	University Students	NL – US	2/24	1/6	3414	100 fr = fl.1.00
	High School Pupils	NL – HS	2/24	1/2	3372	100 fr = fl.1.00
	High School Teachers	NL – HT	1/8	1/2	3501	100 fr = fl.2.00
	University Personnel	NL – UP	1/12	1/2	3076	100 fr = fl.2.00

Note: labels are defined in the main text; average earnings are in experimental francs (fr). The exchange rate is from francs to the local currency, Lek (Albania) and Guilder (NL, denoted by fl.). The official exchange rates were $1 = 141 Lek and $1 = 2,20 fl. at the time of the experiment.

Design issues related to cross-country experiments

Following Roth *et al*. (1991) and Brandts *et al*. (2003), we consider three aspects of the design which require special attention when conducting a multi-national experiment: experimenter effects, language effects, and currency effects.

The term *experimenter effect* refers to the possibility that different sessions of the same experimental treatment may yield different results, due to possible effects of uncontrolled procedural differences across locations. In our case, the two authors of this paper ran all sessions. Therefore, in principle, this kind of experimenter effects is not expected. On the other hand, a priori we were afraid that Albanian participants might be excessively impressed by the presence of a western professor 'handing out money' (one of the authors is Dutch). To avoid this type of experimenter effect, the other author, an Albanian national working in the Netherlands, addressed the participants in Albania. The Dutch experimenter stayed in the background. In addition, the experiment is 'double blind' as long as no audit takes place. When there is no audit, the experimenters have no way of knowing whether or not the subjects have truthfully declared their income. When there is an audit, the experimenters do, however, discover whether or not the participants have reported truthfully.

Second, to control for unwanted *language effects* the instructions for the experiment were initially written in English, and then translated into Albanian and Dutch. In addition, one of the authors speaks both Dutch and Albanian and is able to check for differences that might have occurred because of the translation.

With respect to *currency effects*, it should be noted that the differences in wealth and purchasing power between the two countries and across groups within a country are large. As

noted, we varied the exchange rate from experimental francs to the local currency as described in table 1 in order to maintain sufficient and comparable saliency across groups. The actual rates used were determined by an educated guess. It is hard to conceive more objective method to determine them.

Detailed description of the design

For each subject in every round, income is determined by a random draw from an independent distribution. The distribution is chosen separately by each subject in each round. This is done by distinguishing two sets of envelopes. These are called the X-envelopes and the Y-envelopes. At the beginning of each round, subjects indicate on a written form whether they want to choose an envelope from the X-set or the Y-set. They are then asked to pick one of the six envelopes in that set for that round. In this envelope, they find a note with their income for that round. They open the envelope privately, so the realized income remains unknown to the experimenter and the others. Note that draws are independent: we prepared 6 envelopes of each type for each subject in every round. Subjects know the distributions of incomes in the two sets. The distinct income values in the two sets are given in table 2.

Table 2: Income distributions

							Average	Standard deviation	Audit probability
X-set	100	300	400	500	600	700	433.33	216.03	1
Y-set	0	100	300	500	700	800	400	322.49	1/6 or 1/2

Note that the X-set has a higher average income and a lower standard deviation. The distribution of X-values stochastically dominates the distribution of Y values. Hence, X represents the risk neutral and risk averse choice, whereas a choice of Y is an indication of risk loving behavior. In each of the first three rounds, the choice between X and Y is the only decision subjects have to make. The number of X- and Y-choices in these rounds provides an indication of a subject's attitude towards risk.

In rounds 4-8, after they have chosen an envelop, subjects have to report an income and pay taxes. Here, we add a second difference between X and Y. Subjects are informed that X envelopes will always be audited. Instead, Y envelopes are audited with a probability of either 1/6 or 1/2, depending on the treatment (see table 1). Subjects always report their income, and they know they will certainly be audited in case of an X-choice. If they choose Y, a die is thrown (independently per subject and round) to determine whether or not an audit

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will take place.[14] This distinction between the two types of envelopes represents a difference between registered (X) and unregistered (Y) income.

Subjects have to pay 25% of the reported income as a tax. In case of an audit, the tax consists of 25% of their actual income. The aggregate tax proceeds from a group of 4 are divided equally across the group members. This is done after completion of all rounds.

If an audit reveals that a subject has underreported income, a fine of 25% of the actual income is imposed (on top of the tax payment). The proceeds of the fine are not added to the public good. Note that the fine is not dependent on the level of underreporting. Therefore, if a subject decides to evade taxes, expected earnings are maximized by reporting the minimum possible income (*i.c.*, 0). If an audit reveals that a subject has overreported income, no fine is imposed.

Summarizing, each of the rounds 4-8 proceeds as follows. First, subjects choose a source of income: registered (X) or unregistered (Y). Then, a random draw takes place to determine the realization of the income. Next, subjects report their income. It is then determined whether or not an audit will take place. If there is no audit, the tax is determined on the basis of the reported income. If an audit takes place and honest reporting is observed, actual income (which in this case equals the reported income) determines the tax and no fine is administered. If underreporting is observed, tax and fine are determined by the actual income. If overreporting is observed, actual income determines the tax and no fine is administered.

At the end of a session we first determine the total tax revenue and public good payoff per group. Then subjects are called privately, so we can determine their payoff. They give us the eight envelopes with the actual incomes in each of them. Note that for the cases without audit, this is the first time we observe the actual income. At this stage, however, we cannot determine how much they have declared. Hence, we can still not observe whether or not they have evaded taxes. We then determine the earnings as the sum of realized incomes plus the public good payoff and minus the taxes and fines paid.

Theoretical analysis and expected treatment effects
We start with a theoretical analysis of the decision at hand. Assume that an individual is only interested in the own earnings (the consequences of attributing utility to the earnings of others

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[14] To avoid that the throw of a die reveals a subject's choice, we also throw a die in case an X-envelope is chosen. In this case, an audit takes place irrespective of the outcome.

are discussed in section 5). Recall that tax evasion can only be profitable in case a Y envelope is chosen. In this case, as argued above, our fining system makes reporting an income of 0 optimal once one has decided to evade taxes. If a Y has been chosen with realization y, in case of an audit (probability p) the payoff from reporting 0 equals y minus fine and tax plus 1/4 of the tax paid (from the public good). In case of no audit, the income is y. Taking account of an audit with probability p, the expected income from evasion is therefore p(y-0.25y-0.25y + 0.0625y) + (1-p)y = y-0.4375py. The income in case of honest reporting is equal to y-0.25y+0.0625y= 0.8125y. Hence, a risk neutral subject will evade taxes (by reporting y=0) when y-0.4375py > 0.8125y, or p < 0.43. Hence, given a choice of Y, a risk neutral subject will evade taxes in our p=1/6 treatment but will report honestly in case p=1/2.

A priori, the expected income, x*, from choosing X is 433.33 and from Y, y*, is 400 (*cf*. table 2). Taking into account the possibility of tax evasion implies that a risk neutral subject should choose X for the p=1/2 sessions. For p=1/6, the expected payoff from choosing Y and evading taxes is y*-0.4375y*/6 = 370.83. The expected payoff from choosing X is equal to x*-0.25x*+0.0625x* = 0.8125x* = 325.08, which is lower than the expected payoff from choosing a Y envelope.

Summarizing, in case there are no taxes, a risk neutral subject will choose an X envelope. In case of taxes, and an audit probability p=1/6, this subject will choose a Y-envelope and report an income of 0, irrespective of the income received. In case of taxes and p=1/2, this subject will choose X. Of course, a risk seeking (averse) subject will tend to choose Y more (less) often in all cases. Hence, observed choices of Y when p=1/2 indicate risk seeking behavior.

Now, we consider how tax evasion may be affected by the other experimental treatments than the audit probability. As argued by the 'new institutional economists': "institutions define and limit the set of choices of individuals" (North, 1990: 4). This refers to both, formal (laws, rules and regulations) and informal (culture, norms of behavior) institutions. It is the combination of the two that define an individuals' set of choices and (as a consequence) behavior.

Applying these notions, distinct individual tax evasive behavior in a developed and a transition country can occur as a result of differences in culture (such as norms about cheating), risk attitudes, or tax laws and their enforcement. In our laboratory experiment, we are able to control the formal tax institutions and their enforcement and keep them equal across the two countries. Hence, if we observe differences, we can attribute them to culture or risk attitudes. On the other hand, if we do not observe cross-country differences in the

laboratory, we can conclude that differences that exist outside of the laboratory cannot be attributed to distinct cultures or risk attitudes, because these would have given rise to distinct behavior in the laboratory. Finally, by including a treatment where we vary the audit probability (one type of enforcement mechanism), we can study its differential impact in the two countries while keeping other formal tax institutions constant.

4.   Results

The presentation of the results is split in three parts. After an overview of the choice of income type (X or Y), we will present the general results on tax compliance. This is followed by a multivariate regression analysis of both decisions. A more general discussion of our results and their implications will follow in section 5.

Choice of income

The choice of income is represented by the fraction of subjects choosing (unregistered) income type Y. Recall that there is a difference between the first three rounds of the experiment and the last five. In all eight rounds, risk seeking participants will choose Y envelops more often than others. In the last five rounds, this choice may also be influenced by the fact that X envelopes are always audited, whereas the Y envelopes are audited with a probability smaller than 1. In these rounds, subjects might choose income Y because it opens the possibility of tax evasion. In addition, a public good is provided in rounds 4-8. This too might be a reason to switch from one type of income to another.

Table 3 displays the Y choices for each subject pool. The choices are split up for the first three rounds (choice 1-3) and the last 5 rounds (choice 4-8).

The following conclusions (Results 1 and 2) about the choice between the two types of income can be derived from table 3.

RESULT 1: *Unregistered income (Y) is chosen more often when tax evasion is possible.*
Support: In all cases, the fraction of Y choices in rounds 4-8 > the fraction of Y choices in rounds 1-3. In 8 out of 13 subject pools, this difference is statistically significant at the 10%-level or better.

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Table 3: Fraction of Y-choices per subject pool

country	group	acronym	# sessions/ # subjects	choice 1-3	choice 4-8
		Choice of Y			
Albania	University Students	AL – US (prob.=1/6)	2/24	0.47	0.61*
	University Students	AL – US	2/24	0.44	0.62*
	High School Pupils	AL – HS	2/24	0.46	0.63*
	High School Teachers	AL – HT	2/24	0.43	0.56**
	University Teachers	AL – UT	2/24	0.56	0.63
	University Personnel	AL – UP	2/24	0.39	0.43
Nether-lands	University Students	NL – US (prob.=1/6)	2/24	0.17	0.79*
	University Students	NL – US	2/24	0.22	0.68*
	High School Pupils	NL – HS	2/24	0.46	0.57
	High School Teachers	NL – HT	1/8	0.25	0.58**
	University Personnel	NL – UP	1/12	0.58	0.62
ALBANIA (p=1/2)			5/120	0.46	0.57*
NETHERLANDS (p=1/2)			4/68	0.37	0.62*
PUPILS/STUDENTS (both countries & p=1/2)			4/96	0.40	0.62*
TEACHERS /PERSONNEL (both countries & p=1/2)			5/92	0.46	0.55*

(∗) indicates that the difference between choice 4-8 and choice 1-3 is statistically significant at the 5% level.
(∗∗) indicates that the difference between choice 4-8 and choice 1-3 is statistically significant at the 10% level.
Note: Numbers represent the fraction of Y-choices. Choice 1-3 refers to rounds 1-3 (rounds without taxes) and choice 4-8 refers to rounds 4-8 (with taxes). The shaded rows indicate the sessions where the audit probability p=1/6. All other rows represent sessions with p=1/2.

Aggregating per treatment gives the following results. With an audit probability of 1/2, Y was chosen 42.6% of the cases in the first three rounds and 58.7% of the time in rounds 4-8. This difference is significant at the 1%-level (paired sample t=6.10). With p=1/6, Y was chosen 31.9% of the time in the first three rounds and 70.0% of the time in rounds 4-8. This difference is statistically significant at the 1%-level (paired sample t=6.21).

When aggregating per country (p=1/2), the increase in the fraction of Y choices is significant (for Albania: an increase from 46% to 57%, paired sample t=4.21; for the Netherlands: an increase from 37% to 62%, paired sample t=4.52).

Finally, we can aggregate the data according to subjects' labor market position. We denote someone as being on the labor market if they have a job. The groups HS (high school pupils) and US (university students) are therefore not on the labor market (aside from small part time jobs).[15] As a consequence, they have little, if any, experience with paying direct taxes, while the other groups (teachers and personnel) do. The higher fraction of Y-choices in rounds 4-8 is statistically significant at the 1%-level for the aggregated observations in each group (paired sample t=5.65 for pupils/students; t=2.83 for teachers/personnel).

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[15] This is supported empirically by the answers to the questionnaire distributed at the end of the experiment.

<u>RESULT 2:</u> *There are no differences in the number of Y-choices across treatments.*

<u>Support:</u> When testing for differences between the two countries at the aggregate level (p=1/2), neither choice 1-3 (independent sample t=1.62) nor choice 4-8 (independent sample t=0.97) shows differences that are statistically significant at the 10%-level. The last two rows of table 3 show the results of the aggregation of our subject pools according to labor market position instead of country (p=1/2). The differences between pupils/students on one hand and teachers/personnel on the other are not statistically significant at the 10%-level for either choice 1-3 (independent sample t=1.31) or choice 4-8 (independent sample t=1.64). Finally, changing the audit probability does not affect the fraction of Y choices in rounds 4-8 significantly, either. Compare the fractions for university students US (p = 1/6) with those for US (p = 1/2). When testing for each country and variable separately (independent samples t-tests), none of the differences are statistically significant at the 10%-level, even though Y is chosen 11%-points more often by Dutch students in rounds 4-8 when p=1/6 than when p=1/2.

Tax evasion

We only consider tax compliance after a Y-choice.[16] The distribution of decisions to declare an income less than (<), equal to (=) or more than (>) the actual income is shown in table 4, separately for each subject pool. In case less than the actual income was declared, the table also reports the number of times that this declared income was non-zero. In addition, it gives the average fraction of rounds that subjects evaded taxes. To calculate this, for each individual we first determined the fraction of times (s)he underreported income (out of 5 opportunities), treating overreporting as a missing value. Then, we calculated the mean of these fractions across individuals in a subject pool. This mean is reported in the table in the column 'evasion'.

Our conclusions on tax evasion after choosing unregistered income (Y-envelope) are presented as results 3-6. These are formulated in terms of the extent of tax evasion (in other words, the extent of underreporting per individual). We do not make a distinction with respect to how much is evaded: any reported income lower than the actual income is considered as tax evasion. This analysis is based on the numbers presented in the column 'evasion' in table 4. Results 3-5 are concerned with the subject pool treatment, result 6 deals with the variation in audit probability.

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[16] After an X-choice (where reported income is always audited), income was reported truthfully in 567 out of 588 cases.

Table 4: Distribution of income declaration

country	group	acronym	<	=	>	Evasion[+]
		P=1/2				
Albania	University Students	AL – US	12 (9)	50	12	0.10
	High School Pupils	AL – HS	22 (13)	50	3	0.19
	High School Teachers	AL – HT	5 (5)	54	8	0.05
	University Teachers	AL - UT	10 (2)	64	1	0.08
	University Personnel	AL – UP	2 (0)	45	4	0.02
Netherlands	University Students	NL-US	46 (3)	35	1	0.39
	High School Pupils	NL-HS	27 (6)	39	2	0.24
	High School Teachers	NL-HT	1 (1)	18	4	0.03
	University Personnel	NL-UP	5 (1)	30	2	0.09
Total			*130 (40)*	*385*	*37*	*0.14*
		P=1/6				
Albania	University Students	AL-US	11 (5)	45	17	0.11
Netherlands	University Students	NL-US	65 (10)	20	10	0.58
Total			*76 (15)*	*65*	*27*	*0.34*
TOTAL			**206 (55)**	**450**	**64**	**0.18**

Note: numbers represent the number of times the reported income was less than (<), equal to (=) or more than (>) the actual income, given that Y was chosen. The numbers in parentheses report the number of times that underreporting did *not* involve reporting income equal to 0.
[+]This column reports the average fraction of times income was underreported (including X-choices). The way in which this was calculated is described in the main text.

RESULT 3: *Tax evasion is higher in the Netherlands than in Albania.*

Support: Overall, the extent of tax evasion is larger in the Netherlands (0.58 for p=1/6 and 0.24 for p=1/2) than in Albania (0.11 and 0.09, respectively). This country difference is statistically significant at the 1%-level in both cases (independent sample t=5.93 for p=1/6 and t=3.58 for p=1/2).

RESULT 4: *Pupils and students evade taxes more than other groups do.*

Support: For p=1/2, we aggregate the data according to labor market position (pupils and students, HS/US versus teachers and personnel, HT/UT/UP). The pupils and students have a higher level of non-compliance (0.23 for both countries together; 0.14 in Albania and 0.31 in the Netherlands) than the pool of teachers/personnel (0.05 in aggregate, 0.05 in Albania and 0.06 in the Netherlands). This difference is statistically different at the 1%-level (independent sample t=5.11).

RESULT 5: *Differences across groups within a country are at least as important as differences between countries.*

<u>Support:</u> This follows from the analysis underlying results 3 and 4. The aggregated group of Dutch pupils and students evades taxes more often than this group in Albania does (0.31 versus 0.14). Both groups evade more often than teachers/personnel in either country (0.06 versus 0.05). Teachers/personnel do not differ across the two countries. The difference between pupils/students in the two countries dominates the comparison of the two nations in aggregate. The difference when aggregating per country (0.24-0.09) is smaller than when aggregating per labor market position (0.23-0.05).[17]

<u>R</u>ESULT 6: *Tax evasion in Albania is not affected by the audit probability; Dutch subjects evade more when the audit probability is lower.*

<u>Support:</u> An increase in the probability from 1/6 to 1/2 only marginally decreased evasion amongst Albanian (university) students, from 0.11 to 0.10 (cf. table 4). The difference is not statistically significant at the 10% level. Dutch students, however, had the tendency to comply less when the audit probability was 1/6 (0.58) than when it was 1/2 (0.39). This difference is statistically significant at the 10%-level (independent sample t=1.80).

Aside from the formal results related to our experimental treatments, two other things can be noted from the results reported in table 4. First, in aggregate, the number of times income is underreported (206) is much higher than the number of times subjects report too much (64). However, there are groups where the number of times income was underreported is similar to the number of times it was overreported. This only occurs when both numbers are relatively low, however (*i.e.*, when untruthful reporting is very low compared to truthful reporting). There appears to be some indication that Albanian students (AL-US) overreport more than other subject pools (12 out of 74 decisions when p=1/2 and 17 out of 73 decisions when p=1/6).[18] At first sight, reporting too much income does not make sense. If an audit takes place, nothing is gained or lost compared to truthful reporting. If no audit takes place, more taxes are paid (based on reported income) than necessary. Note that the payoff is determined by the actual income, not by the reported income.

Second, in 55 out of 206 cases (26.7%) subjects reported an income higher than zero but lower than their actual income. Again, there are differences between the two countries.

For p=1/2, Albanians evaded taxes 51 times. In 29 cases (56.9%) they declared more than 0, however. Only 11 out of 79 cases (13.9%) with tax evasion in the Netherlands showed a reported income higher than 0. Similar numbers hold for p=1/6. As argued in section 3, a selfish, rational individual evading taxes will report 0. We will return to the issues of overreporting and incomplete evasion in section 5.

Regression Analysis

The results presented above are based on univariate testing and do not correct for differences in individual characteristics that may affect income choice and tax compliance. In order to analyze subjects' decisions in a multivariate setting, we ran regressions. The first choice we want to explain is the decision whether or not to evade taxes. The dependent variable is $Evade_{it}$, which is 1 if individual i reports less than the actual income in round t (=4,..,8) and 0 if income is truthfully reported (overreporting is treated as a missing value). This is explained by a set of regressors, Z_i, using a random effects logit specification. The random effects correct for the multiple observations per i. Z_i includes variables that distinguish our treatments (subjects pools and audit probability), as well as individual characteristics. We use the following variables.

- COUNTRY is a dummy variable with value 0 for the Netherlands and 1 for Albania.
- NOTLABORMARKET is a dummy variable equal to 0 for teachers/personnel and 1 for pupils/students.
- AUDPROB6 is a dummy representing the audit probability with value 0 for sessions with p=1/2 and 1 when p=1/6.
- CHOICE 1-3 represents the fraction of choices in the first three rounds where the subjects chose Y. As explained above, this is a measure of risk attitude with higher values indicating more risk seeking behavior.
- EARN 4-8 is equal to actual earnings in rounds 4-8 (in francs, before taxes) divided by 10,000.[19]
- AGE is the subject's age divided by 100.
- JOB is a dummy variable with value 1 if the subject has a (part time) job and 0 otherwise. For all teachers and personnel, the value is 1. Because these groups are represented by NOTLABORMARKET = 0, JOB distinguishes pupils/students with a part time job from those without.
- GENDER is a dummy variable equal to 0 for men and 1 for women.

Table 5 (second column) presents the regression results explaining the decision to evade taxes, for the subjects who chose a Y-envelope.

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[18] This is tested formally using χ^2 tests per round. We compare the distribution under- and overreporting by AL-US and NL-US for p=1/2 and p=1/6. In 5 of the 10 tests (two audit probabilities, 5 rounds) this effect is statistically significant at the 5% -level.

[19] Obviously, there is a possible endogeneity problem related to the inclusion of earn 4-8 as an independent variable. It is the only direct way to check the effect of earnings on evasion in a regression context, however. The conclusions for the other variables do not change if earn 4-8 is dropped from the regressions.

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Table 5: Regression results

Dependent variable Independent variable (Z_i)	Evade$_{it}$	Choice$_{it}$
Constant	-2.359 (0.01)*	0.137 (0.85)
COUNTRY	0.867 (0.00)*	-0.172 (0.63)
NOTLABORMARKET	1.295 (0.01)*	-0.453 (0.33)
AUDPROB6	0.912 (0.01)*	-0.320 (0.50)
CHOICE 1-3	-0.709 (0.12)	2.089 (0.00)*
EARN 4-8	2.615 (0.27)	-2.247 (0.26)
AGE	0.720 (0.65)	-1.024 (0.38)
JOB	0.134 (0.70)	-0.170 (0.59)
GENDER	-0.753 (0.01)*	-0.045 (0.89)
Rho (random effects estimator)**	0.312 (0.01)*	0.287 (0.00)
Inclusive value***	---	1.930 (0.02)*

(*) indicates statistical significance at the 5% level; (**) The significant values of Rho indicate individual differences in the tendency to evade taxes and report income; (***) The inclusive value is defined in the main text. The estimated value is not significantly different than 1 (at conventional significance levels).
Note: numbers represent the regression coefficient; p-values in parentheses.

The multivariate regression analysis supports Results 3-6. First of all, the treatment variables all show significant effects in line with these results: the Dutch are more likely to evade than the Albanians do, pupils and students evade more than other subject pools and a lower audit probability yields more evasion. The result that differences within a country are at least as large as differences between countries appears in a higher coefficient for NOTLABORMARKET than for COUNTRY. Furthermore, risk attitude (as measured by the number of Y choices in rounds 1-3) does not have a significant effect on tax evasion. Recall, however, that we are only considering tax compliance of subjects who have chosen a Y-envelope. We will see below, that risk attitude does have a significant impact on the choice between X and Y that precedes the evasion choice. Finally, the individual characteristics AGE and JOB do not affect tax evasion. Gender does have a significant influence, however: women evade taxes less than men do (a similar result is reported in Giese and Hoffman, 1999).

Next, we analyze the decisions of income choice (X or Y). In this case, the dependent variable, Choice$_{it}$, is a dummy variable equal to 0 (1) if i chooses X (Y) in t=4,..,8. We add as an independent variable:

- INCLVAL, which is the so-called 'inclusive value'; it is defined as $I_i \equiv \log[1+\exp(Z_i'\beta_e)]$, where Z_i gives the set of regressors defined above and β_e denotes the coefficient estimates given in the first column of table 5.

The coefficient for the inclusive value provides information about the way in which the decisions on income source and tax compliance are made. A coefficient close to 0 implies that the decision between X and Y is made independent of the subsequent tax evasion decision. A coefficient close to 1 is evidence that the decisions about income source and evasion are made simultaneously. In the latter case, subjects consider the possibility of tax evasion while deciding on the source of income. This 'nested logit' model is described in Maddala (1983). Schram (1992) provides an application. A brief overview of the method is given in Appendix 2.

The results of a random effects logit estimation, with the same variables used to explain tax evasion, are presented in the third column of table 5. We start with the inclusive value, which gives our last formal result.

RESULT 7: *The decisions about income source and tax evasion are made simultaneously.*

Support: The estimated coefficient for the inclusive value is 1.93. It is significantly different than 0 at the 5%-level but not different than 1 at any conventional levels of significance. Hence we reject the sequential model, but not the simultaneous model: subjects take the possibility of evasion into account when deciding on their source of income.

In addition, the regression for choice of income source confirms Result 2, that our treatment variables do not significantly affect the choice between X and Y envelopes. In addition, we find that this choice is not affected by age, having a job, gender, or earnings in rounds 4-8. The choice of income in rounds 4-8 is affected significantly by the subject's risk attitude, with higher aversion (a lower value of CHOICE 1-3) giving a lower probability of choosing a Y envelope. Because we measure risk aversion by the fraction of Y-choices in rounds 1-3, this may partly be attributed to consistency in behavior. Note, however, that our Result 1 shows a significantly higher fraction of Y-choices in rounds 4-8 than in 1-3. Hence, what we are observing is more than just a static continuation of earlier choices. The possibility of evading taxes gives rise to registered income (Y) being chosen more often. Moreover, subjects that are more risk loving are more likely to choose Y.

5. A discussion of the results

First, consider the three novel features of our experimental design: it introduces a new laboratory environment consisting of two types of income; it compares tax evasive behavior

in a developed country to that in a country in transition; and it compares tax evasion across subject pools within countries. All three have proven to be empirically important.

First, consider the endogenous choice of income type. When tax evasion was made possible, unregistered income was chosen more frequently in all subject pools. Moreover, Result 7 shows that subjects take the possibility of evading taxes with a Y-income into account when choosing a source of income. As discussed in section 1, this may distort the allocation of labor across different types of income. This has welfare effects that (to the best of our knowledge) have not been considered in traditional welfare economics. Though the direct effect on allocative efficiency will generally be negative, the net effect could be positive or negative (it could be positive, for example, if tax evasion decreases the welfare loss due to an income tax *per se*). Our experiment does not allow us to elaborate on this topic, but it does point to this aspect of the underground economy as an interesting topic for future research.

Second, though we found differences across countries, they were opposite to what we observe outside of the laboratory. When holding formal tax institutions and their enforcement constant in the experiments, the Dutch evade more than the Albanians. This cannot be attributed to the Dutch being more risk seeking. If anything, they are more risk averse than Albanians. This follows from the column "choice 1-3" in table 3, which shows that Albanians choose Y-envelopes more often than the Dutch do, although the difference is not statistically significant. Hence, the laboratory results imply that the Albanian culture is not more prone to 'cheating' than the Dutch. If anything, the reverse is true. How, then, can we explain the higher level of tax evasion outside of the laboratory in Albania? If Albanians do not have an innate tendency to evade, and do not evade as a consequence of risk seeking behavior, the differences outside of the laboratory must be explained by something we held constant in the experiments, the formal tax collecting institutions and their enforcement.[20] Outside of the laboratory, distinct formal institutions cause differences in tax evasion. This offsets the opposite effect of culture that dominates the laboratory results. This conclusion from our experiment nicely complements empirical findings based on attitudinal questions showing that many transition countries have not succeeded in designing tax systems and administrations that citizens trust (Torgler, 2003).

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[20] An alternative explanation is that we are observing erratic behavior, with Albanians making more mistakes than the Dutch. Overreporting and incomplete evasion (*cf*. section 4) may point in this direction. There are regularities in our data that reject simple error as an explanation, however. First, all phenomena are constant across rounds. Second, there are clear differences across groups. *E.g*., Albanian high school pupils evade much more than they overreport and much more than some Dutch groups. Also, differences between Albanian groups are large and significant. An alternative explanation for overreporting and incomplete evasion is given below.

Third, we have also shown that significant differences exist across groups within a country and that these are sometimes larger in magnitude than the differences between countries. One implication of this result is related to the experimental method. Our cross subject pool design in both countries has shown that it is dangerous to draw conclusions about 'cross-cultural' differences from experiments that use only one subject pool in each country. For example, if we had only compared high school teachers in both countries, we would have concluded that evasion is higher in Albania. This provides a *caveat* for the interpretation of distinct experimental results that have been observed across countries.

In our experiments, we also tested for the impact of variations in the audit probability on tax evasion. The fact that this only had an effect in the Netherlands is a direct consequence of the high levels of compliance that Albanians show, even when the audit probability is low. In other words, there is little room for a further decrease in tax evasion. To push the effect of the audit probability to the extreme, we ran two additional sessions amongst Albanian university students, where the probability of an audit of a reported Y-income was 0. In these sessions, one could always evade taxes without being caught. Even in this extreme case, only 15 out of 58 Y-income choices were followed by an evasion of taxes. If tax compliance is so high even without audit, it is not surprising that the effect of increasing the audit probability is small.

Our results raise the question of *how* the cultures of the two countries differ. On the one hand, one might simply conclude that Albanians are more inclined towards 'honesty' than the Dutch. There are elements in our data that point at an alternative explanation, however. The theoretical discussion presented in section 3 assumes that individuals attribute utility to personal earnings only. If they also attribute utility to the earnings of others (through altruism or considerations of fairness, for example) then individuals might want to give more (contribute to a public good by paying taxes) because it increases their utility.[21] In other words, this explanation points at norms like 'caring for' and 'sharing with' as key elements in the distinct cultures of the two countries.

In this perspective, overreporting and incomplete evasion (*cf*. section 4) can be interpreted as voluntary contributions to the public good. Both phenomena were observed

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[21] For a discussion of the literature in other regarding preferences, see Charness and Rabin (2002), Bolton and Ockenfels (2000), or Schram (2000), for example. The theory on other regarding preferences has developed tremendously over the past few years. Though it is beyond the scope of this paper, it would be interesting to run experiments and estimate the parameters of one of the models (the most general model, by Charness and Rabin, 2002 seems a prime candidate) in Albania and the Netherlands. Based on our results, we would predict that Albanians have a higher coefficient for social welfare and are more inequity averse than the Dutch.

more frequently in Albania than in the Netherlands (though interestingly, not for all subject pools in Albania). Subjects might also be more willing to switch to the (more risky) Y envelopes if their taxes contribute to a public good. If this argument holds, all of these effects would be smaller if there were no public good. To test this, we ran two additional sessions amongst university students in Albania (with audit probability p=1/2), where the tax proceeds were not redistributed, *i.e.*, no public good was provided. The results showed a lower fraction of Y-choices in rounds 4-8 (0.45) than for this subject pool with public good (0.62, see table 3). The difference is statistically significant at the 5%-level (independent samples t=2.02). Moreover, without public good, Albanian students that evaded taxes did so completely (reported an income of 0) on 6 out of 8 occasions (only 3 out of 12, with public good) and they underreported income more often than overreporting it (8 *vs*. 6; 12 *vs*. 12, with public good). All of these results are in line with the 'voluntary contributions to a public good' explanation of behavior in the Albanian sessions.[22]

If an explanation along the lines of other regarding preferences is maintained, the question is why these would be more prevalent amongst certain groups in Albania than in the Netherlands. This may be related to the communist past of Albania. A basic norm of the communist regime that ruled Albania for over 40 years was that of a common property. Concepts like 'caring for' and 'sharing with' were prominent in the communist philosophy. This could explain why the norm appears to play less of a role amongst Albanian high school pupils than for other groups. The high school pupils participating in our experiment were all younger than eight years old when the communist regime collapsed in 1991. Other subjects were typically (much) older.

6.   Conclusions

The literature shows fine examples of both non-experimental and experimental research on tax evasion. The latter offers the opportunity to study individual behavior in a controlled environment. This is what the study in this paper does. It allows us to compare individual tax compliance across two institutionally distinct countries as well as across various socio-

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[22] The additional sessions without audit of a Y-income provide further evidence in favor of the hypothesis. The 43 out of 58 cases, where taxes were not evaded, cannot be explained by fear of detection but may simply reflect a wish to contribute 'fairly' to the public good.

economic groups within these countries, when tax collecting institutions are controlled for. This makes it easier to understand differences in behavior across countries and cultures.

Our study increases the external validity of the laboratory environment compared to previous experiments on tax evasion by providing two sources of income: registered and unregistered. The results show that this is a relevant distinction: the decisions about income source (*i.e.*, labor supply) and evasion of taxes are made simultaneously. As a consequence, allocative inefficiency may occur in the labor market.

We are also able to draw conclusions from the comparative analysis. At first sight (supported by the scarce empirical evidence that is available), Albania is characterized by a larger extent of tax evasion than the Netherlands. However, in the laboratory, this country difference was the other way around. The results indicate that the difference outside of the laboratory is not a consequence of differences in risk attitudes or cultures. It seems that differences in formal tax institutions between the two countries matter. In ongoing research, one of the authors has analyzed data from a large household survey in Tirana, which collects more information about tax evasive behavior. In line with Feige (1997), she concludes that it is not the tax formal institutions per se, but a 'bad' match (clash) between these and the informal institutions (tax norms and culture) in Albania that is causing the high level of evasion (Gërxhani, 2004b). In this paper, we argue that the higher level of compliance by Albanian participants in our experiment might be attributed to other regarding preferences. Apparently, the Albanian government has not been able to construct tax-collecting institutions that can capitalize on these norms by providing a 'good' match between the tax institutions used and the culture of other regarding preferences. On the other hand, we observe Dutch participants evading laboratory taxes but showing a relatively low evasion outside of the experiments. Dutch participants in the experiment are sensitive to increases in the audit probability, however. This is an indication that the Dutch are sensitive to the tax collecting institutions and their enforcement. It would seem that, contrary to Albania, the existing tax institutions in the Netherlands are a relatively 'good' match with Dutch culture.

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Appendix 1: English version of the Albanian and Dutch instructions used in the experiments [23]

Instructions Part I

Introduction

Welcome to this experiment. Here you can earn money. The amount of money you earn will depend on your own decisions, on the decisions of other participants and on the outcome of a random event. The money will be paid to you personally and privately from the other participants. Your decisions are anonymous. They are not related to your name. In addition, there are no 'good' or 'bad' decisions in the experiment. This is not a test. We only want to study the decisions people make.

The currency we are going to operate with during the experiment is called experimental francs. When the experiment is finished, the amount in francs will be converted to guilders by dividing the total earnings by 50.[24] Therefore, you can always calculate your earnings in guilders by dividing the amount in francs by 50.

The structure of the experiment

The 12 participants in this experiment are divided into 3 groups of 4. We will call them group 1, group 2 and group 3. The experiment consists of 8 rounds. You will stay in the same group in all rounds, but you will not know who else is in your group. Soon, we will let you know what group you are in, but this is not so important with respect to the decisions.

The decision made in each round is independent of the decisions made in other rounds. After the third round, we will change something in the decision making process. The exact information about this change will be explained to you when that moment comes.

The decision

In every round, you have to choose an envelope. There are two types of envelopes. We call them X-envelopes and Y-envelopes. There are six envelopes of each type. We will soon come by every desk one by one. Then you will have to inform us whether you want an X-envelope or a Y-envelope. We will take the six envelopes of the type you chose and you may pick one of the six. You will find an amount of money written inside the envelope. For the first three rounds, this amount represents your earnings per round.

Of course, you do not know beforehand the amount you will get. However, we will now tell you the amounts written in the envelopes.

……

There are six different amounts in the X-envelopes. They are:
100 francs
300 francs
400 francs
500 francs
600 francs
700 francs

Hence, if you choose an X-envelope, you will get one of these amounts in that round.
…………………………………………………………………………………………………...
In the Y-envelopes, there are also six different amounts. They are:
0   francs
100 francs
300 francs

---

500 francs
700 francs
800 francs

Hence, if you choose a Y-envelope, you will get one of these amounts in that round.
…………………………………………………………………………………………………..

Presenting your decision
You are not allowed to talk during the experiment. For this reason you will inform us about your decision in writing. For this purpose, we have prepared a table, which will be distributed now.

participant: __
group _

	Choice X or Y
Round 1	X Y
Round 2	X Y
Round 3	X Y

The procedure is easy for rounds 1, 2 and 3. When round 1 begins, we will ask you to circle a X or a Y. If you want an X-envelope, you should circle 'X', and if you want a Y-envelope, you should circle 'Y'. When we come along, we will see what you have chosen. Then, we will let you choose from the six envelopes of that type. You can pick one of the envelopes. You may see for yourself how much you have earned. We do not need to know at that stage. You simply keep the envelope. When the experiment is finished, you take the envelope with you for the payment procedure. We will then give you the amount of money written inside the envelope in cash.

Now you will have a few of minutes to read these instructions again. If you have any questions, raise your hand, and one of us will come to you to answer that question. If you are finished, please remain quiet until we start the experiment.

Instructions Part II

The fund
We will add something for the following rounds. Again, you have to choose an X- or a Y-envelope, which determines your earnings. Now, everybody must contribute part of her/his earnings to a fund. To be more precise, everybody should contribute 25% or a quarter of her/his earnings.

The money collected from a group (there are three groups in total) will then be distributed to all participants of the group. Hence, if one group contributes a total of 500 francs to the fund in a round, everybody of that group will receive 125 francs from the fund. This is independent of the amount contributed by each member of the group individually.

Reporting your earnings
Because we cannot see how much you earn per round (you pick an envelope and open it yourself), you must report your earning to us in each round. The amount reported will determine your contribution. It is up to you to report the actual amount of your earning or some other amount.

The audit
There is a probability that we will audit your reported income. If you have chosen an X-envelope, we will always audit whether you reported the correct amount. We will do this by looking inside your envelope. If you have picked a Y-envelope, we will throw a die to decide whether or not we will audit. If the die shows a 1 (one), 2 (two) or 3 (three), we will audit the amount reported. Otherwise, we will not. Hence, in the case of a Y-envelope, there is a probability of 1/2 that the reported amount will be audited. In the case of an X-envelope, the report will always be audited.

There are two possibilities if we audit your reported amount.

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- If the amount written inside the envelope is the same as the amount reported, we will write it down and you will contribute 25% of that amount to the fund.
- If the amount is not the same, we will write down your actual earnings. You will have to pay 25% of these earnings to the fund. In addition, you have to pay 25% of your actual earnings as a fine for declaring less. If you have reported more than your actual earnings, you do not have to pay a fine. Collected fines are not deposited in the fund.

Again: if we do not audit your reported earnings, your contribution will be 25% of these reported earnings. The actual earnings, that will be paid to you at the end, will be determined by the actual amount in the envelope. You have to take this envelope with you when you go to be paid at the end of the experiment. If we do not audit you, we do not know during the experiment whether or not you have reported the correct amount.

Your final earnings
Your final earnings from round 4 to round 8 will be calculated when you come one by one at the end of the experiment and receive the money you have earned today.

(1) We determine the actual earnings (in francs) by adding up the amounts in your envelopes.
(2) Then, we count the amount that your group deposited in the fund for the five rounds altogether. This total amount will be divided by 4 (the number of group members) and the result (in francs) will be added to your earnings.
(3) Further, we determine the total amount that you contributed to the fund. This amount will be subtracted from your earnings.
(4) At the end, we determine whether you have to pay fines or not, and if yes, how much. This amount will also be subtracted from your earnings.
(5) In this way, we know your total earnings in francs. These will be divided by 50 in order to determine your earnings in guilders. This is the amount you will receive from us.

Registration
We want your decisions to remain anonymous. This is the reason why the procedure is made such that the other participants cannot see from our behavior what you have chosen. Therefore, the procedure during rounds 4-8 is as follows.

First, we come along to give you a chance to choose an envelope. This is done in the same way as in the first three rounds. Then, you will have a chance to declare your earnings in the space provided in the table that will now be distributed.

participant: __
group __

	Choice X or Y	**Earnings** (reported by you)	Actual earnings (to be filled out by us)	**Contribution to the fund** (to be filled out by us)	**Fine** (to be filled out by us)
Round 4	X Y				
Round 5	X Y				
Round 6	X Y				
Round 7	X Y				
Round 8	X Y				

The reported earnings can be the same as the amount in your envelope, but you can also report a different amount.

Then, we will come by again, and the following will happen.
(i)   First, we throw a die.
(ii)   Then, we see whether you have chosen an X- or a Y-envelope in that round.
(iii)   If you have an X-envelope, we will audit the amount, independent of the number on the die. If you have a Y-envelope, we will audit the amount only if the die shows number 1 (one), 2 (two), or 3 (three).

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(iv)   If we do not audit the envelope, we will write in your table (in the space 'actual earnings') 'no audit'. If we do audit the envelope, we will write the actual earnings in your table. This is your reported amount, if you reported the actual amount.

(v)   Then, we will write down your contribution to the fund of your group in the table. In case of an audit, this is 25% of the actual amount. In case no audit took place, this is 25% of your reported amount.

(vi)   Finally, we determine whether you should pay a fine. If you are not audited or if your reported amount appears to be equal to (or larger than) the actual amount after been audited, we will write down a zero (0). If after being audited, your reported amount is smaller than the actual amount, we will write down 25% of the actual amount.

After we have been by everybody, we will start the next round. Note that during the experiment, we do not give any information about the total amount in the funds.

It may take a while to finish a round. It is important that you remain seated quietly and do not communicate with the other participants.

Now you can read these instructions again for a few minutes before we start the fourth round.

The questionnaire distributed at the end of the experiment

Your gender (please check):   … male   … female
Your age (please enter):   ……
Your education (please check or enter):   …..Economics
                                                   …..Other, to wit …………………
How many hours per week do you work (please enter)? ………

Appendix 2: The nested logit model

The model is a two step logit for the choice of income source and tax evasion. The method is explained in Maddala (1983) and applied in Schram (1992). For the description below, we avoid notational complexities and exclude the random effects that we used in the estimated model. It is a straightforward extension to include these.

First (step 1), a logit regression is estimated for the binary choice whether or not to evade taxes in round 4. This is done for the set of subjects that chose Y. The parameters estimated (β_e) are then used to determine the so-called 'inclusive value' I_i:

(1) $I_i = \log[1+\exp(Z_i'\beta_e)]$,
        where Z_i denotes the vector of independent variables for individual i.

In step 2, a logit regression is estimated for the binary choice between income source X or Y. For this regression, the inclusive value of step 1 is added as a regressor. For the probability of choosing Y, this yields:

(2) $Pr_{iy} = 1/\{1+\exp(Z_i'\beta_x - \sigma I_i)\}$,
        where β_x and σ are parameters to be estimated.

To understand (2), consider two special cases. For $\sigma = 0$, (2) reduces to:

(3) $Pr_{iy} = 1/\{1+\exp(Z_i'\beta_x)\}$,

and for $\sigma = 1$ we have:

(4) $Pr_{iy} = \{1+\exp(Z_i'\beta_e)\}/\{1+\exp(Z_i'\beta_e) + \exp(Z_i'\beta_x)\}$.

Equations (3) and (4) both describe the probability of choosing Y for special cases. (3) is a binomial logit model for the choice between X and Y. When deciding on X or Y, the individual does not consider the possibilities of evading after choosing Y (Pr_{iy} is independent of β_e). Hence, for $\sigma = 0$ the income and evasion decisions are made *sequentially*.

In (4), we have a multinomial logit probability for a model with three choices: choose X (denoted by x), choose Y and evade (ye), or choose Y and comply (yc) (the coefficients of which are normalized to 0). The corresponding probabilities are, respectively:

(5a) $Pr_{ix} = \exp(Z_i'\beta_x)\}/\{1+\exp(Z_i'\beta_e) + \exp(Z_i'\beta_x)\}$
(5b) $Pr_{iye} = \exp(Z_i'\beta_e)\}/\{1+\exp(Z_i'\beta_e) + \exp(Z_i'\beta_y)\}$
(5c) $Pr_{iyc} = 1/\{1+\exp(Z_i'\beta_e) + \exp(Z_i'\beta_y)\}$.

In this case, i is considering the choice problem as if it consists of three distinct and equally weighted options: X, Y-evade and Y-comply. Hence, when deciding which envelope to choose, this individual takes the possibility of evading into account. The probability of choosing Y (4) is the sum of the probabilities of choosing Y and evading (5b) and choosing Y and not evading (5c). Hence, for $\sigma = 1$ the income and evasion decisions are made *simultaneously*.

As a consequence, an estimate of σ provides information about the way in which the decisions to choose X or Y and whether or not to evade taxes are related. A coefficient close to 0 implies that the decision between X and Y is made independent of the subsequent tax evasion decision. A coefficient close to 1 is evidence that the decisions about income source and evasion are made simultaneously.