Abstract

This paper investigates, from an experimental perspective, on the tax payers behaviour in a dynamic context and more precisely tries to cope with three main topics related to tax evasion. The first aspect analysed regards the effects produced by a repetitive dynamic choice process on the subjects' attitude towards risk and on the ability to learn to cope with risk. The second theme treated is about the effects produced on the tax payers by the inclusion in the experimental design of psychological moral constraints. The final topic is on the effects produced by the specific experimental context chosen (the simulation of a fiscal environment compared with other simulated environments). The main results emerged from the 8 experiments carried out confirmed the importance of the psychological factors in determining the tax payers actual behaviours and shown the complex dynamic that the agents activate to cope with risk.

DYNAMIC BEHAVIOURS IN TAX EVASION.
AN EXPERIMENTAL APPROACH*

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* The experiments described here are the result of team work involving many members of the Experimental and Computational Economics Laboratory of the University of Trento, and they were part of my PhD thesis. I am grateful to all of the team, my especial thanks going to my friend Paolo Patelli, who authored the software used in the dynamic experiments, and to Alessandra Gaburri who gave me a substantial help in the practical organisation of the experiments. As usual, all responsibility for errors or omissions is mine alone.

1. Preamble

The core of this paper consists of the results obtained from a programme of experiments conducted at the Computable and Experimental Economics Laboratory of the University of Trento (CEEL), the intention being to analyse the influences exerted by psychological factors on tax evasion. This is a issue well-known in the literature on the experimental approach to tax evasion, and the results of the experiments described in what follows cover a range of topics already examined by numerous authors (see the references). As regards those particular topics treated here, I cite only Alm, Jackson, McKee (1992) on the role of the provision of a public good in deterring tax evasion, and Webley and Halstead (1986) on the effects of an experimental design which models a real world context as opposed to one produced by a pure game context.

The experiments discussed in this paper constitute the second phase of the just- mentioned research programme but take the hint from the results obtained from the first stage of the project. The aim pursued at the beginning of the research programme was to verify the role of two distinct experimental devices: the publicising of the results from fiscal audits, and the redistribution of the tax yield. The assumption behind the first device was that people dislike being revealed as ‘guilty’ of evasion (social blame). The second device was based on the hypothesis that experimental subjects incur a (Kantian) moral cost from awareness that they are stealing their contribution to the tax yield from the other participants. To verify the effectiveness of these psychological constraints on tax evasion, two one-shot experiments were carried out. These experiments are not described here because they have been already discussed in previous articles (Bosco and Mittone, 1996; Mittone, 1997). I shall therefore restrict my discussion to the two main results that emerged:

a) the subjects’ perception of the risk of being detected and punished, and their attitude towards this, changed according to the way in which the probability of being audited was defined in the experiments, i.e. if the subjects received precise or vague information about the probability of being audited,[1] When the subjects received imprecise information on this probability, the number of evaders increased, and this happened despite the fact that the subjects declare an expected value from evasion which was lower than the sure choice (pay taxes).

b) The second experimental device (i.e. redistribution of the tax yield) had a significant influence on taxpayer behaviour, while the publicising of the results from the fiscal audits was ineffective.

These results in part confirm the findings reported by the literature. For example, the deterrent effect produced by the distribution of the tax yield was similar to that produced by the public good provision described by the just-cited article by Webley and Halstead (1986). On the other hand, the reactions of the subjects to the different ways in which the audit probability was defined are not confirmed (or rejected) in the literature.

2. Introduction: the hypotheses to test

I extracted from the insights yielded by the one-shot experiments a number of questions which I decided to explore by using a set of repeated choices experiments:

[1] In the first experiment the subjects were unaware of the audit probability, while in the second one the subjects knew that three members of each group of 16 participants would be audited.

Q_1) Does the possibility of playing more than once change the subjects' attitude towards risk and consequently towards fiscal evasion?
Q_2) Does the more effective of the two moral constraints introduced in the one-shot experiments (i.e. tax yield redistribution) play any role in a repeated choices framework?
Q_3) Can one identify some form of learning process which teaches the subjects how to cope with risk?
Q_4) Does the context (the simulation of a fiscal environment) have any effect on the subjects' behaviour?

It is clear from the questions just listed that the focus of the experiments discussed in what follows was the intersection between the psychological costs of tax evasion and the risk component embodied in the decision to evade. More precisely, one must bear in mind that the psychological constraint included in the experiments discussed here (the tax yield redistribution) may directly influence computation of expected monetary value of evasion. Supposing, therefore, that the taxpayer's utility depends only on money, we may write the usual tax evasion expected value EV^e formula:

$$EV^e = (1\text{-}p)\,[1\text{-}t\,(1-l)]Y + p\,[1\text{-}t\ \text{-}\ l\,P(l)t]Y \qquad [2.1]$$

where:

Y is income before taxation;

l is the percentage of tax evaded ($l = 0$ if the taxpayer is perfectly honest, $l = 1$ if the taxpayer is perfectly dishonest);

p is the probability that evasion will be discovered;

t is the tax rate;

$P(l)$ is the punishment scheme which links the surcharge to the amount of evasion.[2]

The taxpayer's problem, given [2.1], is simply a matter of comparing between the value of EV^e and the net income after taxation. When $EV^e = (1-t)Y$ the the taxpayer's choice is conventionally assumed by the expected utility theory to be between risk aversion and risk attraction.

How does [2.1] change if we include the tax yield redistribution? Recalling the assumption that the tax payer's utility depends only on her/his net income, and following the traditional approach to tax evasion theory, we may hypothesise that R – that is, the amount of money redistributed after taxation – is a function of the tax payers' attitude towards risk, of total income and of t. More precisely if we hypothesise:

[H1] - t is a fixed rate,
[H2] - the government redistributes the tax yield by simply dividing the total amount of money collected from taxes (without including the fines paid by evaders detected by the fiscal audits) into equal parts among tax payers;
[H3] - the punishment system is the same for every taxpayer,

we may state that R will depend only on the total income and on the average prevailing attitude towards risk. Equation [2.1] therefore becomes:

$$EV^{eR} = (1-p)[1-t(1-l)]Y + p[1-t\,l\,P(l)t]Y + R \qquad [2.2]$$

[2] I assume that the penalty rate is imposed on evaded tax, an institutional feature common to many developed countries.

where:

$$R = K\,\frac{\sum_{i=1}^{n} tY_i - l_i tY_i}{n}$$

and (i=1,..,n) is the total number of taxpayers.

Inspection of equations [2.1] and [2.2] shows that the nature of the taxpayer's decision problem is basically the same with or without redistribution. More precisely, and still assuming a risk-neutral taxpayer, one may expect to observe a different decision, moving from the without-redistribution context to that with redistribution, only when the ratio between the value of the sure choice (pay taxes) and the value of the uncertain choice (to evade) becomes greater than one as a consequence of the amount of money redistributed. The amount of "sure" income, in fact, rises as a consequence of redistribution; and therefore the original ratio $(1-t)Y/EV^e$ of the without-redistribution lottery changes, becoming $\left[(1-t)Y + R\right]/EV^{eR}$. It is to be noted that the value of R can only be foreseen by the individual taxpayer, given that it is highly unrealistic to assume that s/he could have "sure" information about the behaviour of the other taxpayers. Hence it follows that the only way to compute the value of the sure choice is to assume that none of the other tax payers will pay. The value of the sure income for taxpayer j therefore becomes $[(1-t)Y_j + tY_j / n]$ and in a similar way EV^{eR} changes as well.

This is the case from the microeconomic theory point of view, but as was easily predictable,[3] many of the behaviours observed in the experiments reported here are difficult to explain, either from this standpoint or from that of other theories, even when they are based on experimental data.[4]

To answer the questions listed at the beginning of this section, I ran eight dynamic experiments. It is these that are discussed in this paper. which starts with a description of the parts of the experimental design common to all the experiments.

3. The lottery structure of the dynamic experiments

The dynamic experiments were run using a computer-aided game designed for this specific purpose. Thirty subjects participated in each experiment, 15 men and 15 women, undergraduate students from the Faculty of Economics of the University of Trento. All the experiments were of the same length (60 rounds) and they were run by taking the variables that enter the lottery structure as constants. The values for the lottery were the following:

a) *income* - 1000 Italian Liras from round 1 until round 48, then 700 Italian Liras;
b) *tax rate* - 20% from round 1 until round 10, then 30% from round 11 until round 30, and finally 40% from round 31 until the end;
c) *tax audit probability* - 6% from round 1 until round 21, then 10% from round 22 until round 40, and finally 15% from round 41 until the end;

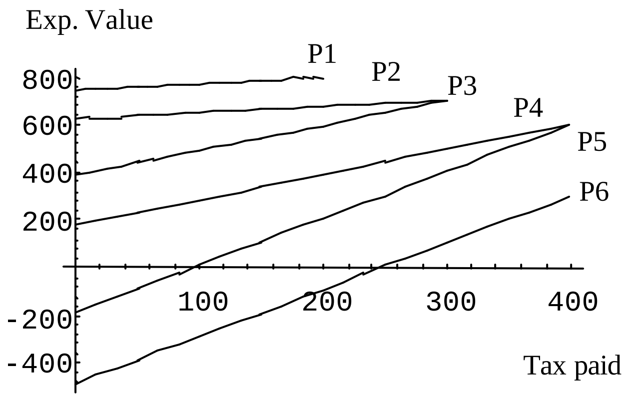
[3] There is a large body of experimental literature one the failures of the expected utility theory. Among the most interesting studies are in my opinion those by Kahneman and Tversky (Tversky and Kahneman, 1974, 1981, 1983, 1986; Kahneman, Slovic and Tversky, 1982; Tversky, Slovic and Kahneman, 1990). There is an almost equally large amount of literature on fiscal evasion from an experimental perspective: for a review and a sort of 'textbook' on the topic see Webley, Robben, Elffers and Hessing (1991).

[4] Among the most interesting of such theories I shall only cite the well-known *prospect theory* by Kahneman and Tversky (1979).

d) *fees* - the amount of the tax evaded plus a fee equal to the tax evaded multiplied by 4.5; the tax audit had effect over the current round and the previous three rounds.

To approximate a real life situation more closely, I decided, in the base experiment, to extend the tax audit over a period of four rounds. For this reason, and as the lottery structure changed during the experiment, computation of the expected value from evasion was rather complex. To calculate the expected values for the different lotteries I used a simple program produced using *Mathematica*©. The graphic result from the simulation is shown in fig. 3.1, in which the horizontal axis represents the tax paid and the vertical axis represents the expected value from evasion. Fig. 3.1 demonstrates that the lottery structure for the dynamic experiments was always unfair. As the lottery used in the one-shot experiments was a more than fair lottery, I expected that the percentage of evaders in the dynamic experiments would be smaller than it was in the one-shot ones. Obviously this consideration was valid only for the first round of the game.

Fig. 3.1 Expected values for the dynamic experiments



P1: income=1000; tax=20%; audit prob.=6%
P2: income=1000; tax=30%; audit prob.=10%
P3: income=1000; tax=40%; audit prob.=10%
P4: income=1000; tax=40%; audit prob.=15%
P5: income=1000/700; tax=40%; audit prob.=15%
P6: income=700; tax=40%; audit prob.=15%

4 The design of the experiments

The experimental subjects were recruited through announcements on the bulletin board of the Faculty of Economics. The subjects' personal data were collected by the staff of the Computable and Experimental Economics Laboratory. The total number of the experimental subjects who participated in the 8 experiments discussed here was 240.

During the experiment the players were not allowed to communicate, and they received information only from the computer screen, which showed the following items of information:

a) the total net income earned by the player since the beginning of the game,
b) gross income in the active round,

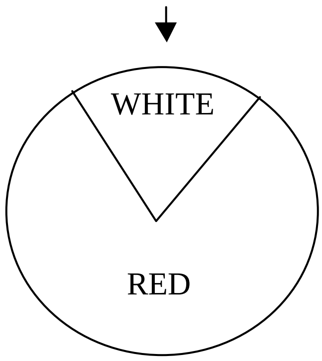
c) the amount of taxes to pay in the active round,
d) the number of the active round.

The subjects were divided into two groups, and they underwent a fiscal audit in correspondence to the same rounds (specifically rounds 13, 31, 34, 48, 54, 58 for the first group, and rounds 3, 24, 27, 40, 46, 50 for the second group).

A further information device took the form of a snap interruption: the computer screen changed and a message appeared which informed the subjects that the audit probability would change after three rounds (this item of information kept the subjects constantly informed on the relevant parameters of the lottery). When each subject had read the information on the screen and had taken her/his decision, s/he wrote, using the computer keyboard, the amount of money that s/he had decided to pay and then waited to see if s/he had been extracted for a fiscal investigation.

Finally, introduced into one version of the standard experiment was a visual device intended to help the experimental subjects in evaluating the probabilistic nature of their choices. This device was the red and white wheel shown in fig. 4.1.

Fig. 4.1 The probability wheel



The white sector of the wheel shows the probability of being audited: in each round the subjects could see the wheel rotate. If it stopped with the arrow in correspondence with the white sector they were inspected. Conversely, when the wheel stopped with the arrow in correspondence with the red sector they were not audited. Obviously, as the probability of being audited increased, so did the surface area of the white sector.

As said, the dynamic experiments were designed to test some specific hypotheses. For this purpose, the following changes were made to the original design:

DY1) is the standard (base) experiment;
DY1_0) is the standard experiment but with no retroactive tax audits (this obviously implies that the lottery structure for this experiment is simpler and fairer than that described in the previous section);
DY1_10) is the standard experiment but with a tax audit extended over the last 10 rounds (this time the situation is the reverse of DY1_0: the lottery is less fair that the lottery used in the standard DY1);
DY1_W) is identical to the standard experiment but with the adoption of a “visual” device, the red and white wheel showing the subjects their probability of being audited;
DY2) is the same as DY1 but with the introduction of the tax yield redistribution (which was one of the ‘moral’ factors investigated in the one-shot experiments);
DY3) is the same as DY2, except that the tax yield is used to finance the provision of a public good (the creation of a scholarship fund);

DY4) is exactly identical to the standard experiment, except that it is designed as a generic gamble and every reference to the fiscal environment is eliminated (I shall call this the "gamble experiment" for convenience);
DY5) is the only experiment with a different lottery structure and a different timing of the fiscal audits.

I shall give more details of the structure of the experiments in the following section, when I discuss the results.

5. The findings of the experiments

This section sets out the results of the experiments by means of graphs depicting the subjects' aggregate behaviour for the entire duration of each experiment. Before discussing the graphs, it may be of help to look at the number of evaders computed for the first round of each experiment and at the total tax yield produced:

Table 5.1 Evaders and tax yield (repeated choices experiments)

Experiment	Number of evaders (first round)	% of evaders (first round)	Number of evaders	total tax yield (Italian liras)
DY1	14	46.0%	951	393,519
DY1_0	16	53.3%	1250	257,061
DY1_10	17	56.6%	869	414,657
DY1_W	20	66.6%	1037	373,116
DY2	12	40.0%	499	495,345
DY3	16	53.0%	715	467,021
DY4	19	63.3%	1012	397,524
DY5	15	50.0%	-	-

The first consideration arising from Table 5.1 is that the percentages of evaders in the first round for all the repeated choices experiments were generally lower than the percentages recorded for the initial Bosco-Mittone (1996) one-shot experiment with known audit probability (when the tax evaders comprised 62% of the total sample) but much higher than in the second one-shot experiment (Mittone, 1997), i.e. the one with uncertain audit probability (in this experiment the evaders accounted for only 28.2% of the total sample). Since the starting lottery of all the dynamic experiments was unfair, while the lottery in both the one-shot experiments was more than fair, the result is coherent for the first one-shot experiment but rather difficult to explain as regards the second. With respect to the latter, it is worth noting that the average expected probability of being audited – as stated by the experimental subjects in a questionnaire distributed at the beginning of the experiment – was around three times higher than the actual probability used in the experiment. The only explanation for this apparently illogical finding, on the basis of expected utility theory, is that the subjects in the second one-shot experiments had strong risk aversion, while those in all the dynamic experiments were not risk averse.

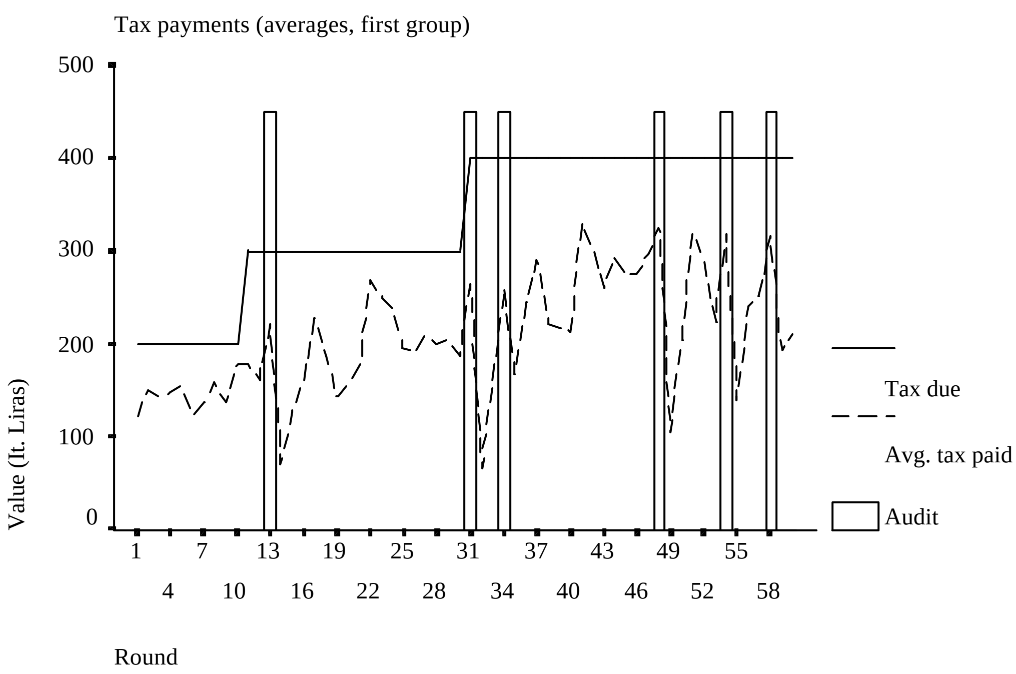
The second consideration concerns the total tax yield collected at the end of the experiments and the number of evaders (or more precisely the number of times that a subject decided to evade during the whole experiment). The differences among the experiments with the same lottery structure (i.e. experiments DY1; DY1_W; DY2; DY3 and DY4) confirm the importance of the tax yield redistribution as a moral constraint already tested in the one-shot experiments. Furthermore a similar result is obtained by the experiment with a public good financed from the tax yield. Both the experiments with these psychological, "moral", devices (DY2 and DY3) produced a higher tax yield than was collected in the two versions of the standard experiment with fiscal audit retroactivity extended over 3 rounds (DY1 and DY1_W), and in the gamble experiment (DY4). Similarly, also the number of evaders was noticeably lower in the experiments with redistribution (499) and with a public good (715) than it was in those with no moral constraint (respectively 951 evaders for DY1, 1037 for DY1_W and 1012 for the gamble experiment). These results therefore seem to confirm the anti-evasion effect exerted by a psychological factor implied by the redistribution of the tax yield either in the form of money or as a public good.

Comparison of the results within the sub-group of the DY1 experiment shows that the subjects react to the increase/decrease of the audit probability in a way which is coherent with the standard theory, i.e. tax evasion is lower when the risk of being audited is higher and vice-versa. On the other hand it is to be noted that the amount of the differences is smaller than expected. For example, if we consider that the risk of being audited in experiment DY1_0 was 3 times lower that the risk in the base experiment, we would expect evasion to increase approximately three times, whereas the data collected show a tax yield increase of only 34%. Even weaker is the effect produced by increasing the tax audit frequency. In fact, inspection of the tax yield collected in the experiment DY1_10 shows that the increase, once more with respect to DY1, is only 5% compared to a more that threefold increase in the probability of being audited.

Finally, the results obtained from the visual probability experiment (DY1_W) seemingly point to the conclusion that the inclusion of the probability wheel does not change the results substantially. The number of evasions is in fact slightly higher in the experiment with the wheel but the total tax yield collected is lower. I shall shortly return to this conclusion when discussing the graphs showing the aggregate behaviours of the experimental subjects.

The graphs from experiment DY1 are given in figs. 5.1 and 5.2, which show the dynamic of the subjects choices.

Fig. 5.1 Standard experiment (DY1)



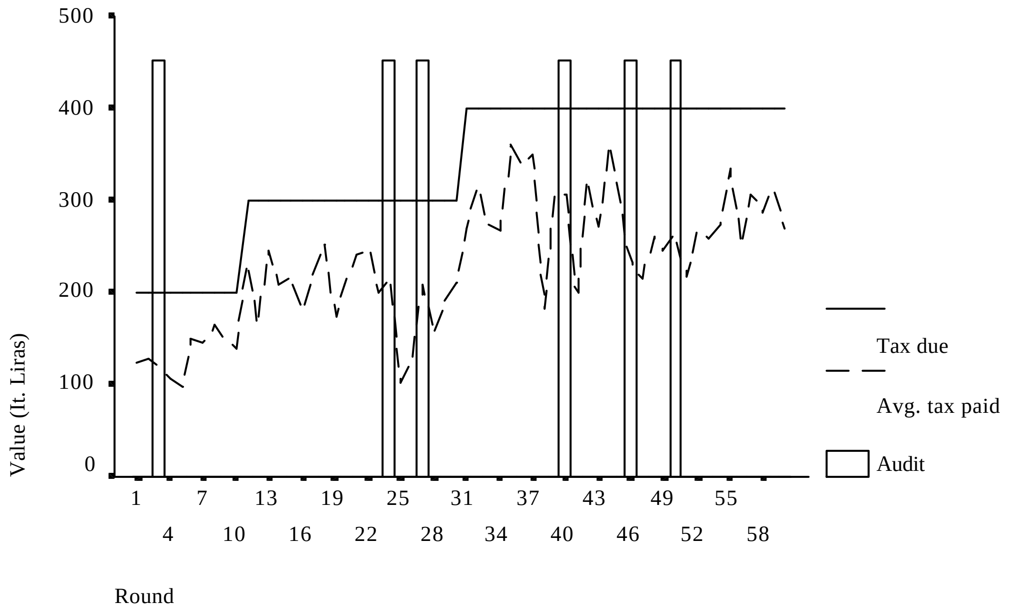
Two considerations arise: the first is that it is rather difficult to interpret these choices in the light of traditional expected utility theory; the second is that the evolution of the aggregate choices is apparently unaffected by modifications to the lottery structure. If the usual Von Neumann Morgenstern approach is used to interpret the dynamic observed, a very unrealistic hypothesis must be introduced, namely that there is a large proportion of subjects who are risk neutral (given the different lotteries) and who consequently choose the amount of money to pay in each round at random. This latter consideration can be evaluated better by looking at two periods of the DY1 experiment, i.e. from round 11 to round 30 and from round 31 to round 48. During these periods all the influential variables of the lottery were constant, except for the audit probability, which changed at rounds 21 and 40. Dividing these two periods into four sub-periods: rounds (11-21); (22-30); (31-40); (41-48), and computing for each sub-period the average tax-yield per round, we obtain the following values:

(sub-group A - rounds 11-21 - tax audit = 6%) - average tax yield per round 5,610.3
(sub-group B - rounds 22-30 - tax audit = 10%) - average tax yield per round 6,032.2
(sub-group C - rounds 31-40 - tax audit = 10%) - average tax yield per round 7,608.5
(sub-group D - rounds 41-48 - tax audit = 15%) - average tax yield per round 8,458.9

The increase in the average tax yield from A to B is 7.5%, compared to an increase of 66% in the audit probability. The tax yield increase from C to D is 11.1% while the increase in the audit probability is 50%. It follows that the effects on tax evasion exerted by the increase in the tax audit probability are very small.

Fig. 5.2 Standard experiment (DY1)

Tax payments (averages, second group)



A clearer picture of the subjects' behaviours, with respect to the probability of being audited and fined, is given by figs. 5.3 and 5.4, which show the average expected values computed by using the lotteries chosen by the two sub-groups of subjects that participated in the standard experiment.[5] It will be seen that the expected values for both the sub-groups are always lower

[5] The lottery chosen by the subjects can be easily computed by substituting for λ in [2.1] the amount of money that they actually decided to evade.

than the value of the corresponding sure choice (i.e. to pay taxes), and the amount of money evaded seemed unrelated to the trend of the expected values. By contrast, there is a negative correlation between the amount of money evaded and the value of the sure choice (the linear correlation coefficient between the value of the sure choice and the average tax paid by the first group of subjects is -0.48 and -0.36 for the second group, with a significance level of 0,000 for the first coefficient and 0.004 for the second). This relationship is coherent with expected utility theory because is rational to evade less when the value of the sure choice is high and more when it decreases, assuming a constant risk propensity.

Fig. 5.3 Standard Exp. (DY1)

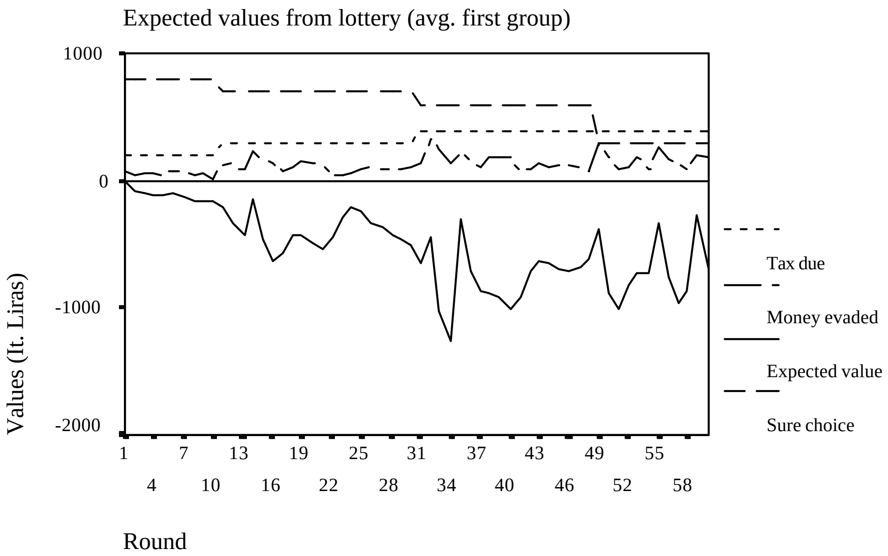
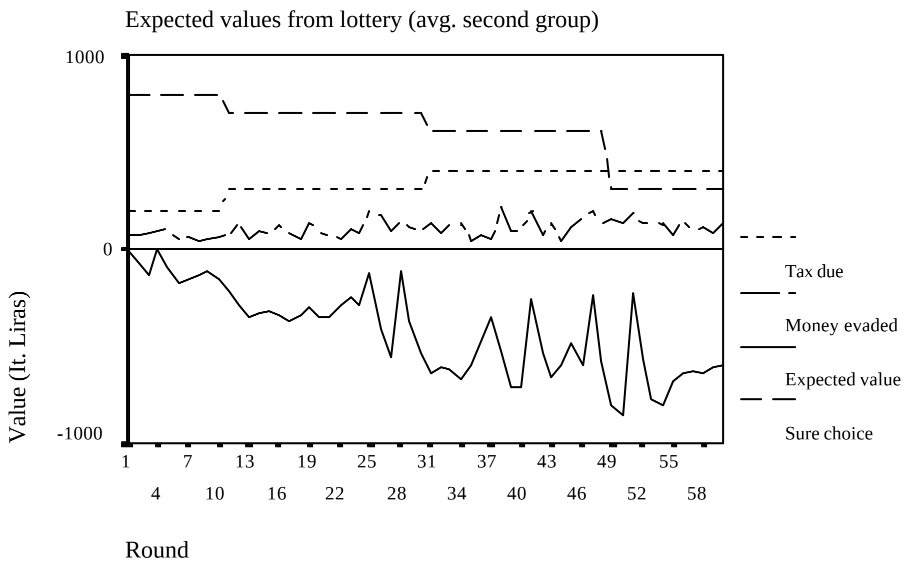


Fig. 5.4 Standard Exp. (DY1)



An even better picture of this phenomenon is provided by table 5.2, which reports the average tax evaded per round and the extent to which subjects tend to increase their evasion when the sure choice decreases. It is evident that as the value of the sure choice decreases, the average tax evaded increases.

On the other hand, and this time in contrast to expected utility theory, the expected value from evasion is negatively correlated with the amount of tax evaded. The linear correlation coefficients computed for these variables are - 0.2735 for the first group of subjects (with a significance level of 0.03) and - 0.2097 for the second group (with a significance level of 0.1). We have just seen that in order to justify the dynamics reported by figs. 5.1 and 5.2 from the expected utility theory perspective, we must assume that the subjects chose randomly. Now, explanation of the inverse correlation between tax evasion and expected value from evasion requires a different assumption: that is, we must hypothesise that the subjects' risk propensity changes with each round, and that it is negatively correlated with the expected value from evasion. I shall return to this topic after discussing the results of the other experiments.

Table 5.2 Sure choice and tax evaded (exp. DY1)

Round	Avg. tax evaded (It. Liras)		Sure choice (It. Liras)
	First sub-group	Second sub-group	
1-10	54.93	67.36	800
11-30	112.33	99.58	700
31-48	152.33	115.23	600
49-60	165.16	125.49	300

By combining the existence of an apparently rational behaviour, based on the inverse relationship between the sure choice value and the amount of tax evaded, with the continuously changing structure of the subjects choices, we may suppose that some form of adaptive dynamic behaviour is taking place. It seems that the subjects ignore the trend of the expected value from evasion (maybe because it is too difficult to compute) and that they 'explore' the space of their alternatives by changing their choices in each round. The situation is like that of a person doing a jigsaw puzzle who, when looking to the pieces of the puzzle on the table cannot combine them in her/his mind, and therefore decides to make numerous practical attempts to find the solution.

Returning to fig. 5.1 and 5.2 and introducing the graphs obtained from experiments DY1_0; DY1_10; DY1_W; DY2, DY3 and DY4 (fig. 5.5, 5.6, 5.7, 5.8, 5.9, 5.10, 5.11, 5.12, 5.13, 5.14, 5.15, 5.16), one notes another important aspect of the results obtained from the experiments which may in some way support this latter consideration. Even if the trends are highly unstable and apparently follow some sort of random walk, one discerns constancy in the rounds immediately after a fiscal audit, which is almost always followed by a systematic increase in tax evasion. This increase generally has its lowest peak in correspondence to the round immediately after the fiscal audit, and sometimes lasts for more than one round. I shall call this the "bomb crater effect":[6] the subjects decide to evade immediately after a fiscal audit because they believe that it cannot happen twice in the same place (time). Sometimes this effect may have some sort of echo, so that some subjects persist in evading for two or three rounds after the audit. The echo effect seems to be influenced in some way (i.e. it is more or less compressed in time) by the different systems of fiscal audits introduced into the experiment, that is to say, by the audit retroactivity extent (from 0 to ten rounds before the active round when the audit effectively took place).

The "bomb crater effect" is influenced neither by the tax yield redistribution, nor by the context, nor by the fiscal audit system adopted, nor by the method used to inform the experimental subjects of the probability of their being investigated (i.e. using numbers – percentages – or the visual device). It can thus be assumed to be some sort of mental representation of probability activated by the subjects. One can test whether experience modifies this mental representation of probability by examining the behaviour of the subjects

[6] The term derives from the First World War: during bombardments, soldiers would take shelter in bomb craters in the belief that it was impossible for a bomb to fall in the same place twice.

belonging to the separate sub-groups of each experiment. As just said, those involved in experiments DY1, DY1_W, DY2, DY3 and DY4 were all audited simultaneously during two sequences of rounds, i.e. sequence A: rounds 13, 31, 34, 48, 54, 58, and sequence B: rounds 3, 24, 27, 40, 46, 50. The main difference between the two sequences concerns the moment in the experiment when the first audit takes place. In sequence A, the first audit comes after a quite long period of game-playing (round 13), while in sequence B it occurs at the beginning of the experiment (round 3).

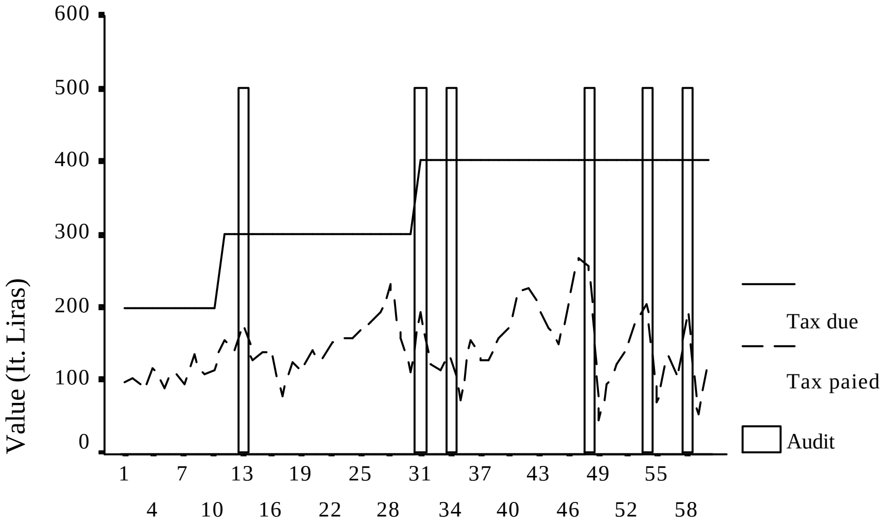
Table 5.3 shows the amount of money paid by the subjects belonging to the two subgroups of each experiment. One notes that being subjected to an audit at the beginning of the experiment produces a sort of risk aversion effect. The members of the first subgroups (sequence 1) always evade more, and consequently pay less, than the members of the second subgroups (sequence 2). The only and very interesting exception to this rule is experiment DY4 - that is, the experiment that was designed as a game and not as taking place in a real world context. I shall return to this important exception shortly, when I discuss the point on learning to be risk adverse.

Table 5.3 Evaders and tax yields of the sub-groups

Experiment	Number of evaders	total tax yield (It. liras)
DY1 first sub-group	502	187,201
DY1 second sub-group	449	206,318
DY1_W first sub-group	604	170,699
DY1_W second sub-group	433	202,417
DY2 first sub-group	275	236,782
DY2 second sub-group	224	258,563
DY3 first sub-group	436	217,763
DY3 second sub-group	279	249,258
DY4 first sub-group	502	197,561
DY4 second sub-group	510	199,963

Fig. 5.5 Stand. Exp. with No Retroact. (DY1_0)

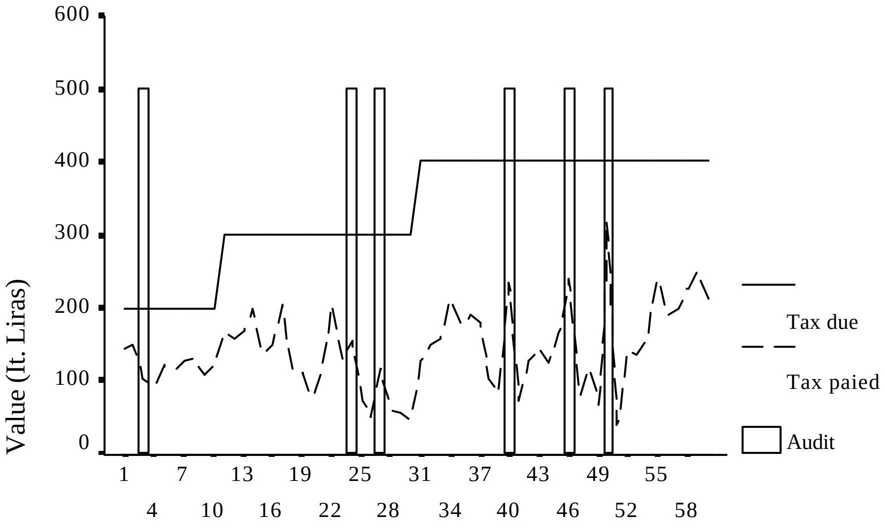
Tax payments (averages first group)



Round

Fig. 5.6 Stand. Exp. with No Retroact. (DY1_0)

Tax payments (averages second group)



Round

Fig. 5.7 Stand. Exp. 10 Rounds Retroact. (DY1_10)

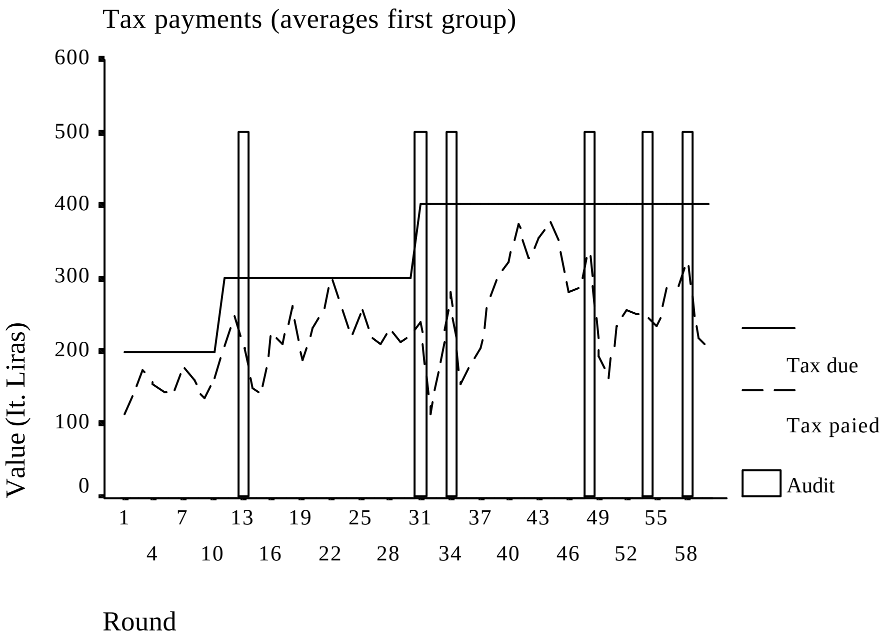


Fig. 5.8 Stand. Exp. 10 Rounds Retroact. (DY1_10)

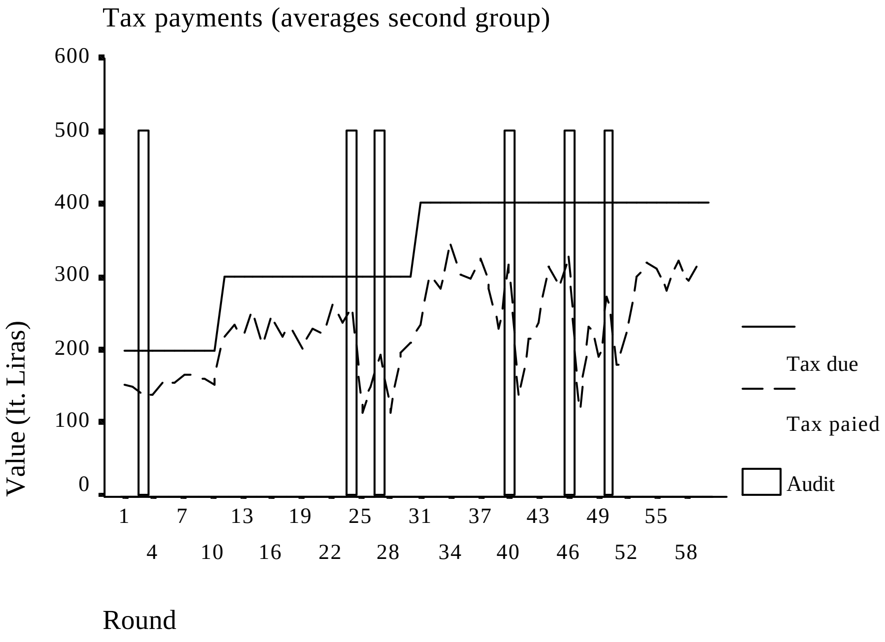


Fig. 5.9 Stand. Exp. with Prob. Wheel (DY1_W)

Tax payments (averages first group)

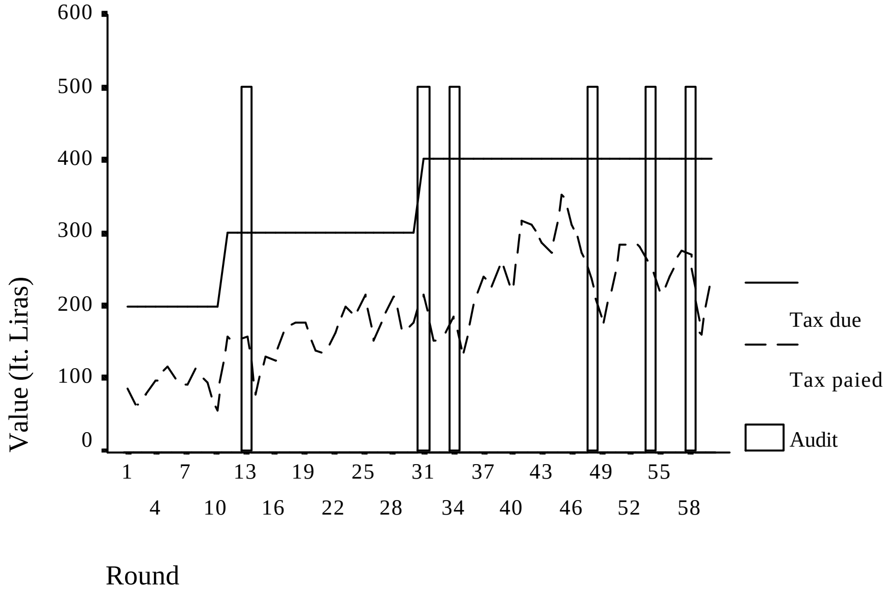


Fig. 5.10 Stand. Exp. with Prob. Wheel (DY1_W)

Tax payments (averages second group)

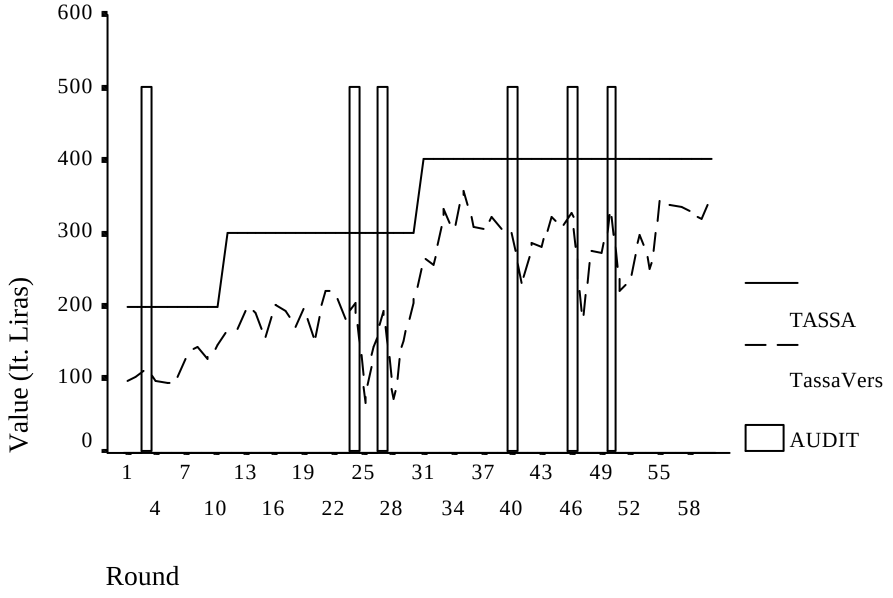


Fig. 5.11 Exp. with redistribution (DY2)

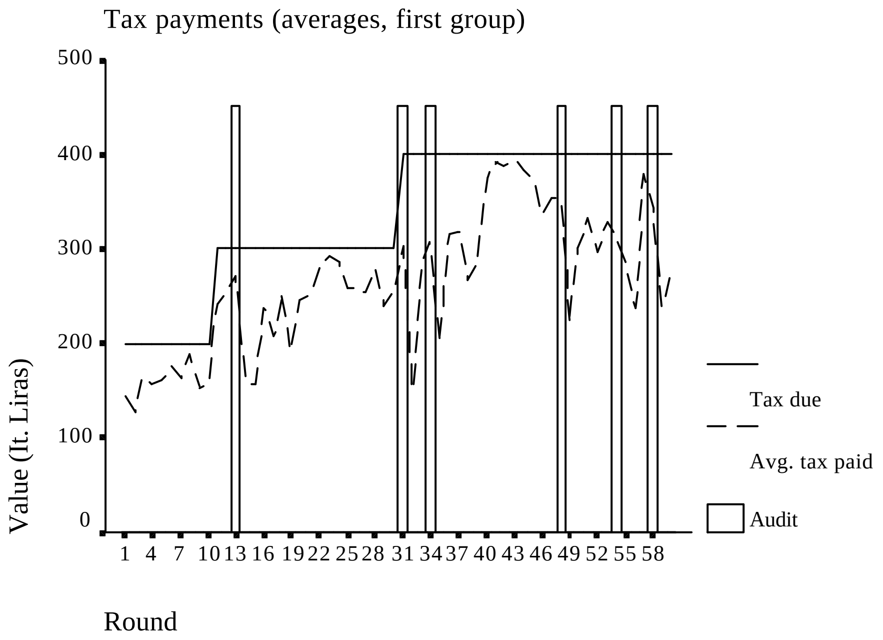


Fig. 5.12 Exp. with redistribution (DY2)

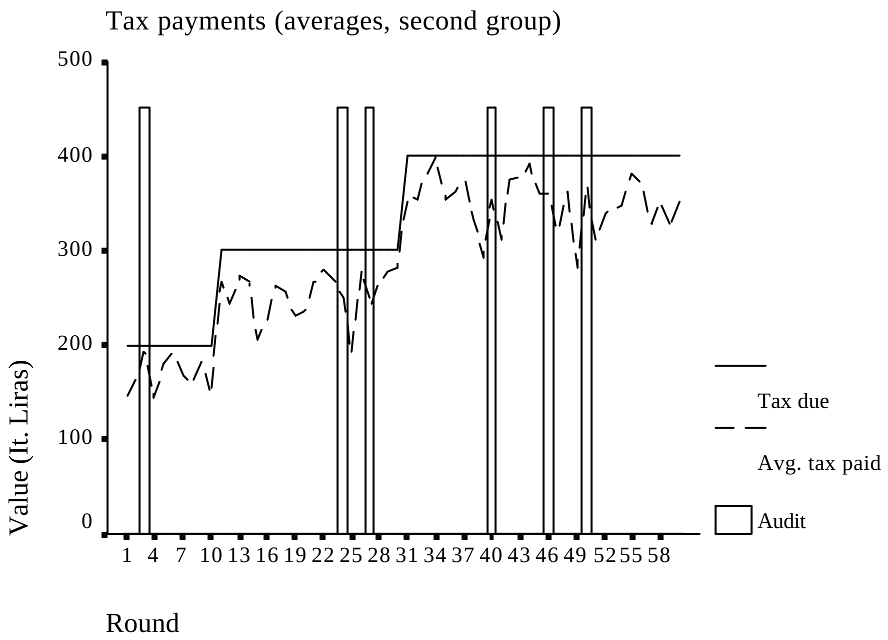


Fig. 5.13 Exp. with public good (DY3)

Tax payments (averages, first group)

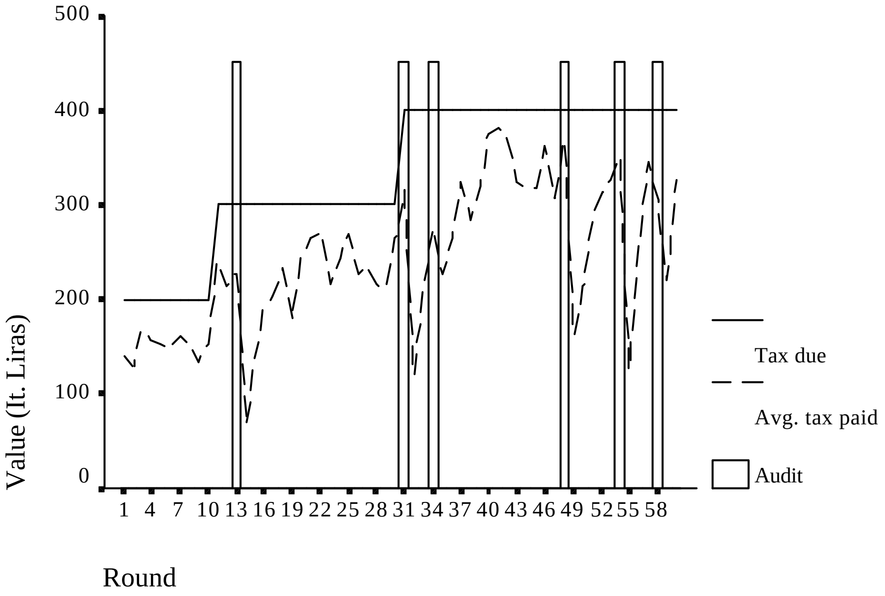


Fig. 5.14 Exp. with public good (DY3)

Tax payments (averages, second group)

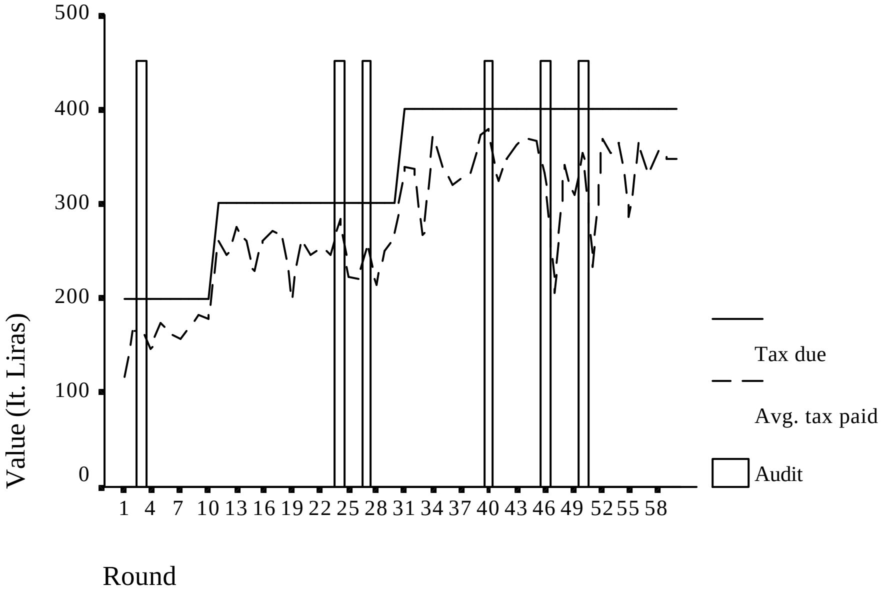
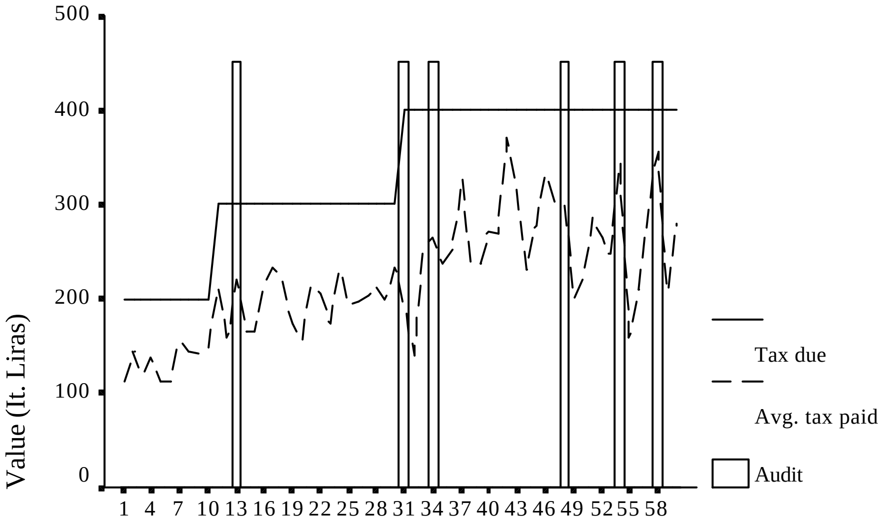


Fig. 5.15 Gamble experiment (DY4)

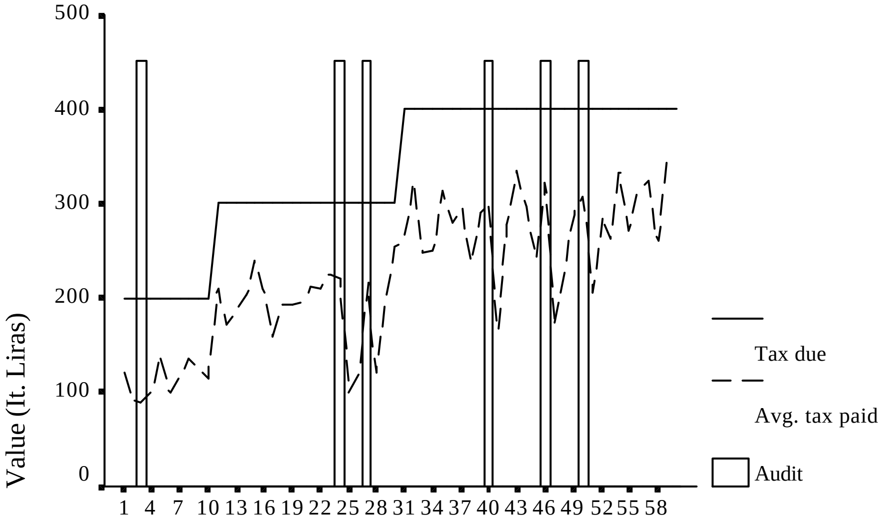
Tax payments (averages, first group)



Round

Fig. 5.16 Gamble experiment (DY4)

Tax paied (averages, second group)



Round

To investigate the apparent phenomenon of learning to be risk adverse requires the introduction of experiment DY5, the results of which are given in figs. 5.17 and 5.18.

Fig. 5.17 Special audits exp. (DY5)

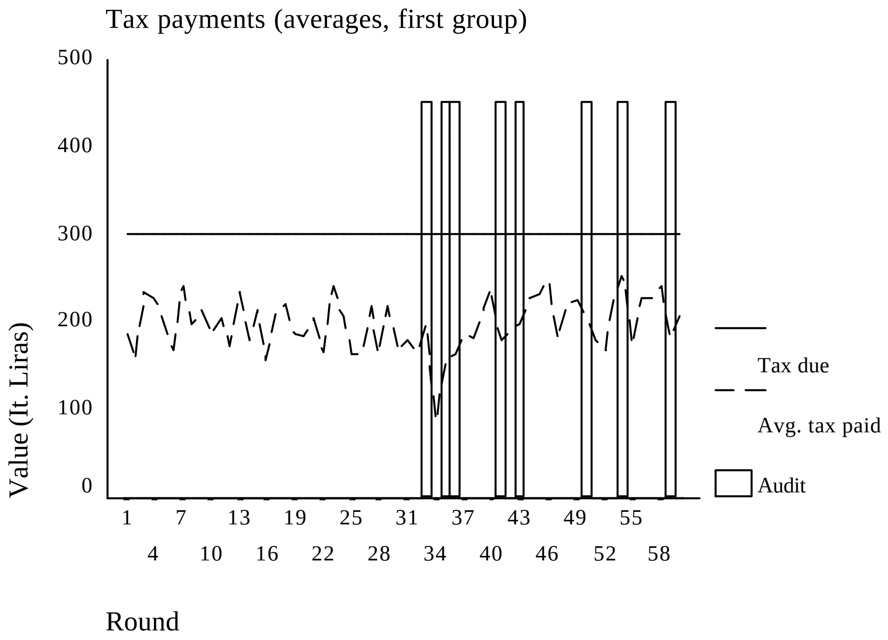
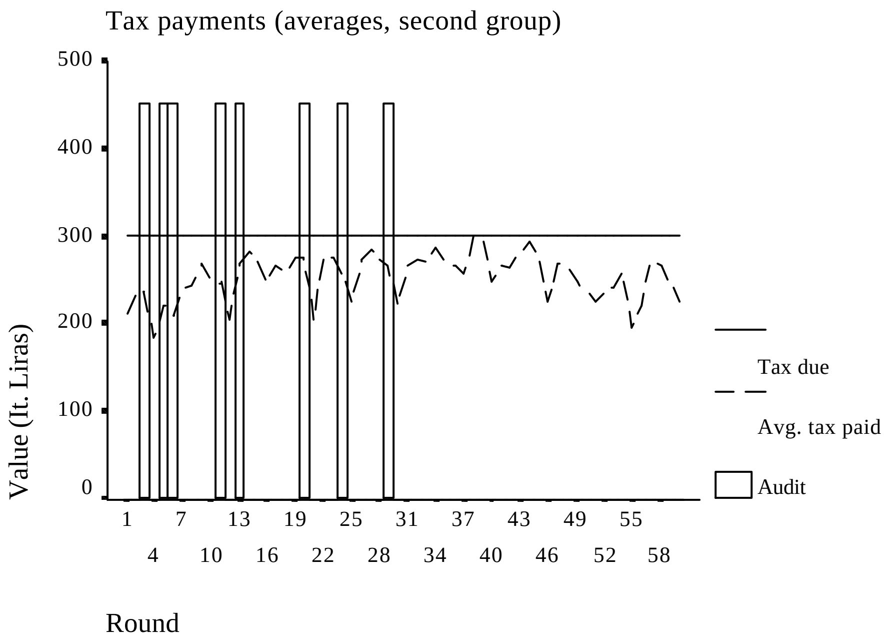


Fig. 5.18 Special audits exp. (DY5)



It is evident from figs. 5.17 and 5.18 that, in experiment DY5, the fiscal audits were concentrated in the second half of the experiment for the subjects belonging to the first group, and in the first half for the subjects belonging to the second group. The structure of the lottery in DY5 was kept constant for the entire experiment in order to isolate the effects produced only by the audit timing. The result is evident. Those subjects who "learnt" in the first half of their experimental lives that fiscal audits are very uncommon became risk takers (the total tax yield for them was only 177,429 lire, while the total tax yield for the subjects belonging to the second sub-group was 227,831 lire). They had a strong propensity to evade taxes, which persisted when they moved into the second half of their experimental lives, when the probability of being audited increased dramatically. The tax yield of the first 30 rounds (88,104 lire) was only slightly lower than the tax yield for the following 30 rounds (89,325 lire). By contrast, the subjects in the second group learned that fiscal audits were very frequent and consequently also learned to be risk adverse, maintaining this virtuous behaviour for the entire experiment. These subjects paid 111,645 lire in the first 30 rounds and paradoxically even more (116,186 lire) in the second part of the experiment, when they were never investigated.

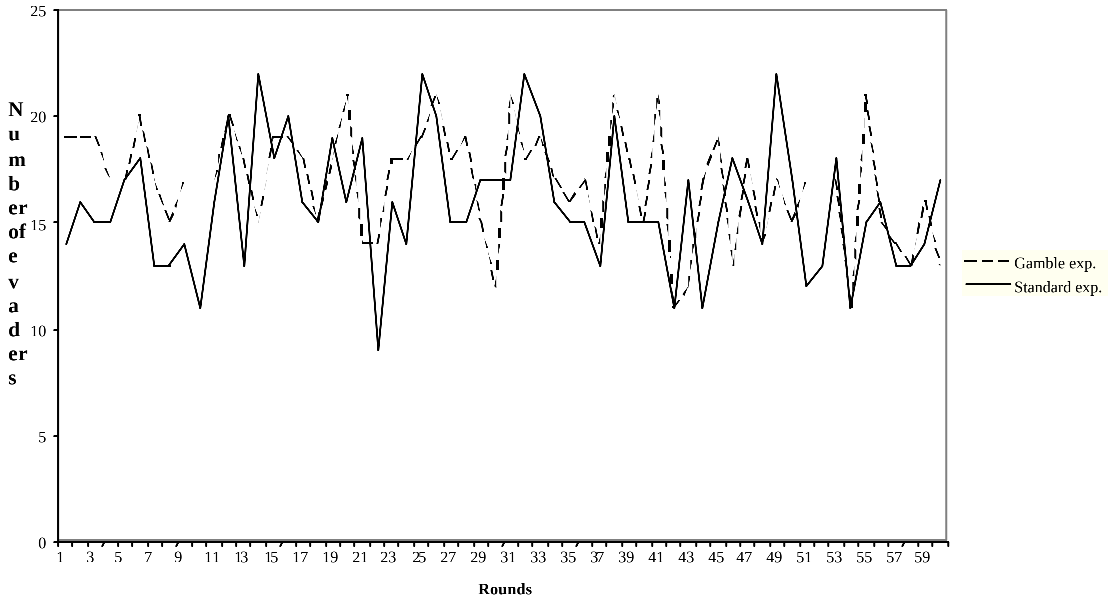
Table 5.4 Evaders and tax yield experiment DY5

Experiment		N° of evaders	Tax yield (It. Liras)	Avg. Evasion (It. Liras)
first sub-group	rounds 1-60	406	177,429	228
	rounds 1-30	221	88,104	212
	rounds 31-60	185	89,325	247
second sub-group	rounds 1-60	255	227,831	165
	rounds 1-30	146	111,645	160
	rounds 31-60	109	116,186	173

These results are reported in table 5.4, together with two other interesting items of information: the number of evaders (or, more precisely, the number of acts of tax evasion) and the average amount of money evaded in each tax evasion. One notes that the total number of evaders decreased for both the two sub-groups between the first 30 rounds and the second half of the experiment, while the average amount of taxes evaded increased. This finding suggests that two main forms of adaptive behaviour were developed by the subjects in both the experiments: those subjects who decided to adopt the first strategic response tried to save money by reducing evasion - that is, they progressively abandoned evasion and adopted predominantly honest behaviour. Those subjects who adopted the second strategy tried to push their luck by evading larger and larger amounts of money as the experiment progressed.

I have pointed out that there was an exception to the rule that the members of the first subgroups always evaded more than the members of the second subgroups. This exception was experiment DY4, where the subjects belonging to the two subgroups adopted almost identical behaviours, or more precisely, reversed the rule by evading more (510 evaders) in the first sub-group and less (502 evaders) in the second sub-group. This phenomenon introduces one of the topics addressed by the dynamic experiments, namely the role played by the experimental context. The point is this: did the subjects really perceive the context that we tried to reproduce (i.e. a tax payer problem) or did they behave as if they were playing a video game? Furthermore, it should be borne in mind that this suspicion is reinforced by the fact that the dynamic experiments were carried out using computers. One way to investigate this issue is to compare the results obtained from the standard experiment with the results reported from the gamble experiment, because they were perfectly identical in their lottery structure. The results from both the experiments are plotted in fig. 5.19.

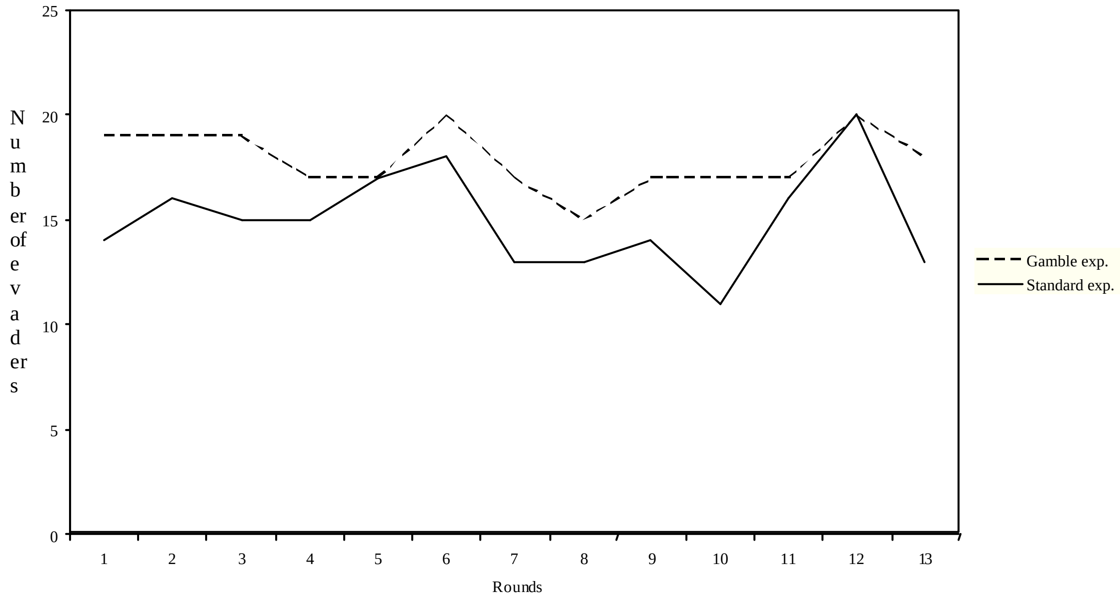
Fig. 5.19 Number of evaders standard exp. (DY1) and gamble (DY4)



At first glance, the plots shown in fig. 5.19 might suggest that the behaviours of the subjects of the two experiments were very similar, if not identical. Nonetheless, closer examination of the trends reveals that the behaviours were not always as similar as they first appear. In fact, if we look at the first 13 rounds we find that the number of evaders in the standard experiment was always lower (except for round 5 and round 12 where the number of evaders was the same for both experiments) than the number of evaders in the gamble experiment. This phenomenon can be evaluated better if we look at fig. 5.20 and fig. 5.21, which show the number of evaders respectively for the first 13 rounds and for rounds 14-31. The subjects that participated in the standard experiment paid about 10% less tax in the first 13 rounds than those who participated in the gamble experiment (59,705 It. Liras versus 54,140 It. Liras). By contrast, in the following 18 rounds the two groups of participants paid almost the same amount of taxes (respectively 106,395 liras were paid by the subjects of the standard experiment and 106,584 by the subjects of the gamble experiment). It is worth stressing that carrying out a statistical test to check if the differences between the two sub-groups of rounds are statistically significant is vital, because the number of observations is small. Moreover, the parametric tests usually adopted to verify whether two samples belong to the same statistical population, like the t-test, require satisfaction of the well-known postulates on the normality of the distribution of the population from which the samples are extracted. One way to overcome these limitations is to use a non-parametric test, which gives results that are less robust if compared with those obtainable by using a parametric test, but at the same time allow one to avoid the hypothesis on the distribution of the starting population. Furthermore, the non-parametric tests are able to handle small samples.

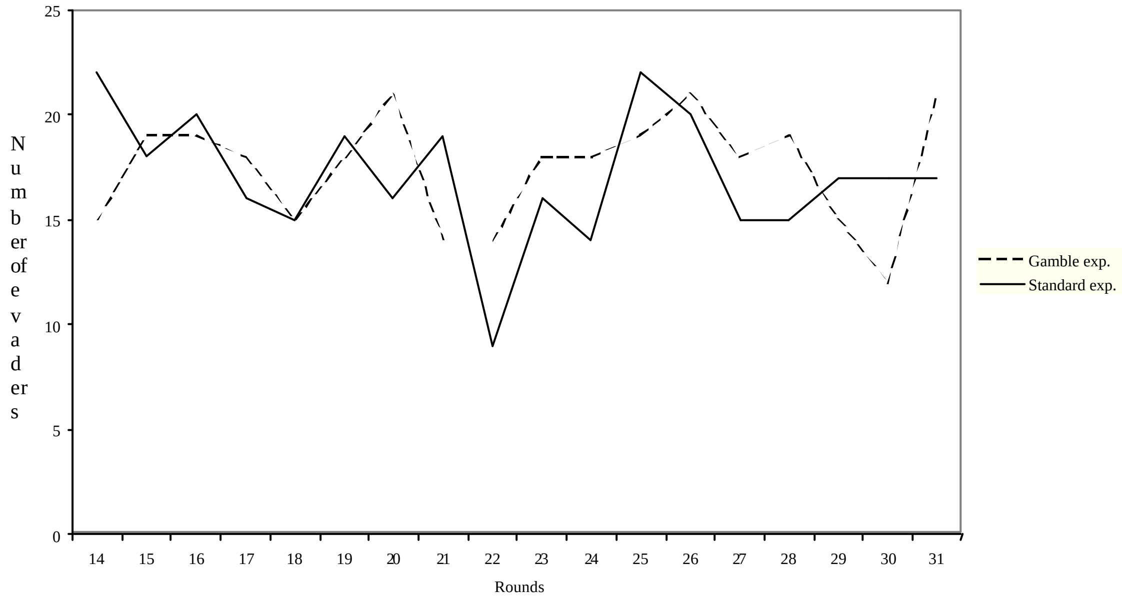
On running the Mann-Whitney (1947) test to verify whether the difference in the number of evaders in the first 13 rounds is statistically significant between the two groups of subjects, we may reject with a 0.002 level of significance the null hypothesis that the two samples derive from populations with the same distributions.

Fig. 5.20 Number of evaders exp. DY1 and DY4



The data collected from the experiments do not provide a certain explanation for this phenomenon, but a reasonable one is forthcoming from consideration of the role played by the fiscal audits (or by the corresponding "drawing" of the gamble experiment). After the first 13 rounds, all the subjects in both the sub-groups of each experiment experienced one inspection (or drawing). The first two sub-groups at round 13 and the second sub-groups at round 3. The interesting point is that the effect of this experience apparently modified the risk attitude only of the participants in the gamble experiment. In other words, it seems that at the beginning of the experiment the subjects involved in the gamble experiment were more risk-takers than were the subjects of the standard experiment, but this attitude changed after they experienced the first fiscal inspection, and became almost identically distributed among the participants in both experiments. This change in the risk attitude of the participants in the gamble experiment suggests that there is some form of relationship between the different experimental contexts and the attitude towards risk. At the same time, the risk attitude seemed to be influenced in accordance with the experimental context, by the experience of being extracted for audit.

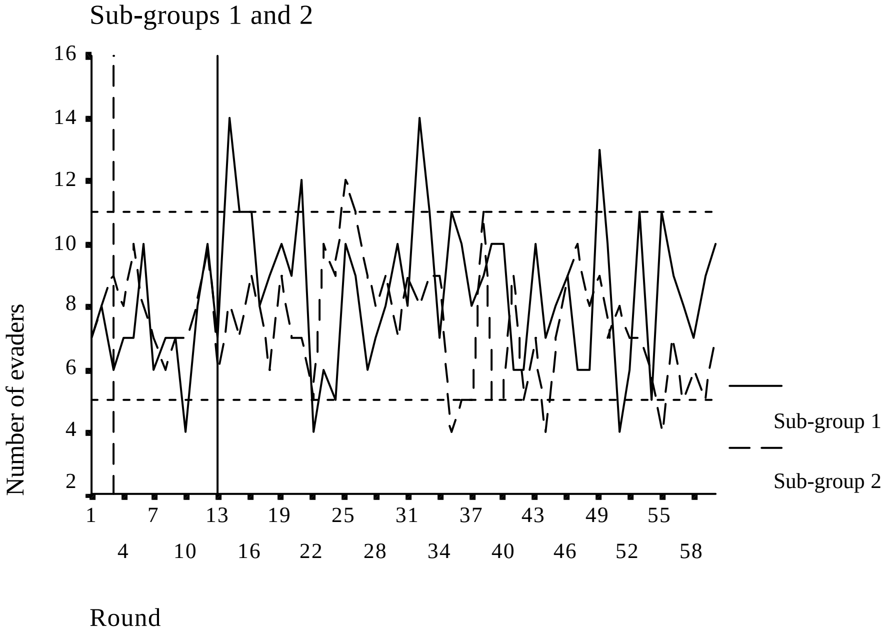
Fig. 5.21 Number of evaders exp. DY1 and DY4 rounds 14-31



This phenomenon is confirmed by figs. 5.22 and 5.23, which show the trends in the number of evaders for the two sub-groups in the standard experiment and the gamble experiment. In the standard experiment, as well as in all the other tax evasion experiments, the different timing of the audit experience produced a different behavioural pattern within the sub-groups of each experiment. In the tax evasion experiments, as will be seen shortly, those who experienced an audit within the first rounds tended to evade less frequently (and to a lesser amount) than those who were inspected later. By contrast, in the gamble experiment the subjects who evaded more frequently were those who belonged to the sub-group drawn in the first part of the experiment.

In figs. 5.22 and 5.23, I have fixed an arbitrary “high” number of evaders corresponding to 11 evaders per each round and a similarly arbitrary “low” number of evaders equal to 5 evaders. On counting the number of rounds with a number of evaders equal to or higher than 11 and, symmetrically, the number of rounds with a number of evaders equal to or lower than 5, one finds that the behaviours of the two sub-groups in the two experiments are mirror-reversed.

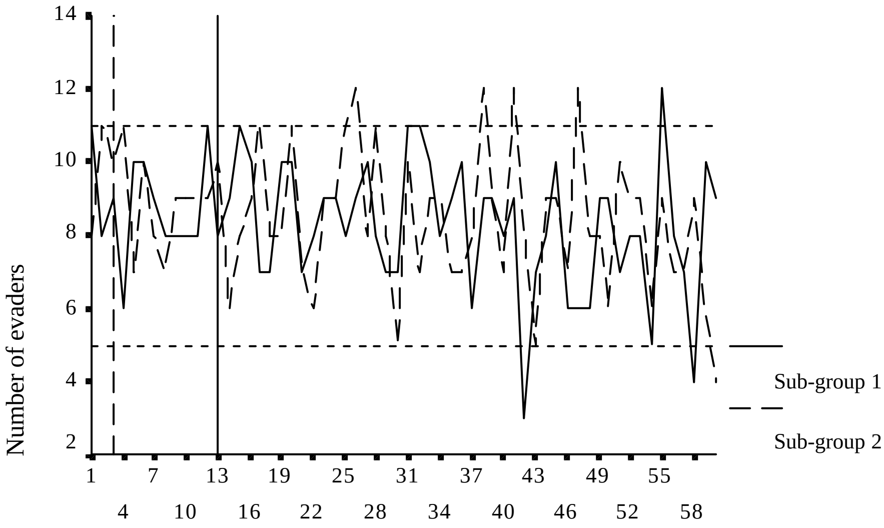
Fig. 5.22 Number of evaders exp. DY1



In the gamble experiment (fig. 5.23) those who experienced the first draw at round 13 (sub-group 1) reached or exceeded the "high" arbitrary line only 4 times, while those who were drawn for the first time at round 3 (sub-group 2) reached or exceeded the high line 10 times. By contrast, in the standard experiment the subjects belonging to the first sub-group reached or exceeded the "high" line in 9 rounds, versus only 2 rounds for those in the second sub-group. Symmetrically, and always with regard to the results from the standard experiment, one also sees that the "low" number of evaders line is more frequently reached or went down by the sub-group 2 (11 times) than by the sub-group 1 (4 times). Conversely, in the gamble experiment the two sub-groups reached or went down the "low" line the same number of times (3 times). The learning process which developed the individual attitude towards risk therefore appears to have been quite markedly influenced by the experimental context, and at the same time yields further evidence on the different roles played respectively by the tax evasion frame and by the pure gamble design.

Fig. 5.23 Number of evaders exp. DY4

Sub-groups 1 and 2



Round

These considerations highlight another interesting point concerning the nature of the relationship that ties the risk attitude to the subjective representation of probability. In all the dynamic experiments the probability of being audited was a known datum, and it should therefore be taken as “objective” information (this is also the reason why it is correct to talk of taking decisions under conditions of “risk”, instead of uncertainty). However, it is not possible to exclude the hypothesis that the experimental subjects do not treat this data within the “correct” computational frame. The well known probabilistic paradoxes discovered by relatively simple experiments (most notably the Allais paradox and the Ellsberg paradox), demonstrate that even when all the probabilistic properties of a decisional problem are perfectly known the subjects may commit “logical errors”.

Particularly interesting as regards the experiments discussed here is the lesson from the Allais paradox, which can be condensed into the following statement: human beings tend to over-estimate the weight of small probabilities, making mistakes in the computation of the expected value of a given lottery and behaving in an apparently irrational way or, put more precisely, violating the substitution axiom. A simple description of the substitution axiom is suggested by Kreps (1990, p. 75). “Suppose that p and q are two probability distributions such that p is preferred to q ($p \succ q$). Suppose that a is a number from the open interval (0.1) and r is some other probability distribution. Then $ap + (1 - a)r \succ aq + (1 - a)r$.” In the experiments analysed here, and more generally in all experiments obtaining results similar to those of the Allais paradox, it seems that p and/or q are not taken by the subjects to be given distributions, but are “re-modelled” as subjective probability distributions. Furthermore, in the experiments discussed here it seems that the decisional frame in some way influences the modelling of these subjective probability distributions, i.e. they are functions of context variables. It follows that if the values of the context variables change, then p or q changes, or both them.

Note that this latter consideration goes into a different direction from that taken by the above hypothesis that the learning process leads to becoming risk averse or risk taker, because it states that the lesson learnt by the subjects during the experiments has nothing to do with their disposition towards risk, which can be assumed to be a sort of “built-in” attitude; rather, it concerns their subjective representation of the probability of being audited. This representation can be imagined as a model (of functional form which cannot be easily deduced from the data) whose independent variables include the experience of being audited and the experimental context.

6. Conclusions

The main conclusions reached by analysing the data are summarised in table 6.1.

Table 6.1 Summary of the aggregate results from the repeated choices experiments

Experiment	Risk attitude	Psychological effects
DY1 objective probability; unfair lottery (3 rounds of retroactivity for the fiscal audits)	1) higher number of evaders than the one-shot experiments 2) complex dynamic of choices 3) "bomb crater effect"	1) Correlation between the sure choice value and evasion 2) non correlation between the gain expected from evasion and the observed behaviours
DY1_0 objective probability; unfair lottery (no retroactivity for the fiscal audits)	1) higher number of evaders than in DY1 2) complex dynamic of choices 3) "bomb crater effect"	
DY1_10 objective probability; unfair lottery (10 rounds of retroactivity for the fiscal audits)	1) almost identical number of evaders as in DY1 2) complex dynamic of choices 3) "bomb crater effect"	
DY1_W objective probability; unfair lottery; probability wheel	1) almost identical number of evaders as in DY1 2) complex dynamic of choices 3) "bomb crater effect"	
DY2 objective probability; unfair lottery; tax yield redistribution	1) complex dynamic of choices 2) "bomb crater effect"	tax yield redistribution reduces evasion
DY3 objective probability; unfair lottery; public good	1) complex dynamic of choices 2) "bomb crater effect"	the production of a public good reduces evasion but to a lesser extent than tax yield redistribution
DY4 "gamble" experiment	1) complex dynamic of choices 2) "bomb crater effect"	1) the gamble context increases the risk attitude 2) the learning process is different if compared with the learning process accomplished in the tax evasion context
DY5 objective probability; unfair lottery; artificial audits	1) complex dynamic of choices 2) "bomb crater effect"	3) learning to be risk adverse

Table 6.1 shows that the most robust results concern the effect of tax yield redistribution and of audits (the "bomb crater effect"), while all the other devices introduced in the experiments exerted less impact on the behaviours of the experimental subjects. Both the psychological deterrent produced by the "moral" constraint embodied in the tax yield redistribution (and in the public good provision) and the particular reaction produced by the tax audit experience can be viewed, from a normative perspective, as devices to reduce evasion. Obviously, this conclusion requires further analysis.

More in general, the results from these experiments are highly instructive for study of behaviours under conditions of risk/uncertainty. The complex mix of "substantial rationality" (in the neoclassical meaning of the term) and of bounded rationality (in Herbert Simon's sense) used by the experimental subjects to cope with the uncertain choice to evade warrants further investigation.

Finally, one may conclude that the dynamic perspective adopted by these experiments has borne out the considerations that emerged from the one-shot experiments with which I began, and which showed that it is more realistic to study a behaviour that is typically dynamic in nature.

Appendix A: instructions for the subjects (translated from the Italian)

These are the general instructions given to the participants of the experiments DY1, DY1_W, DY2, DY3, DY5:

"This game is about the behaviour of tax payers. The game is computer aided, the software that you will use is pre-built, and there will be no direct intervention by the researchers during the experiment. The results of your choices will be collected only when the experiment has finished, and they will remain anonymous.

The game simulates a real world environment. It comprises several rounds which represent different time periods (for example years). In each period you will receive a round income (which at the end will be your reward for the work you have done). In each period you will also be required to pay a tax, but you can decide to evade part of this tax, or even all of it. Regardless of your choice, you may be investigated at any moment during the game, and if you have evaded in one or more of the last four rounds you must pay the taxes evaded plus a fine. The inspection may never take place, and it is decided and performed only by the computer, without any direct intervention by the researchers.

All relevant information will be provided directly on the computer screen, and you must not communicate with anyone during the whole experiment.

At the beginning of the experiment your probability to be audited during the active round is 6% and is independent from the probability that other people would be investigated. This means that in a given round can happen that none subject is audited, or, on the opposite, that all subjects are investigated. The probability can change during the experiment but you will be informed thorough the computer screen before that this change should take place.

This is an example of the fine that you should pay if you decided to evade and would be audited:

round	Round gross income	audit	Tax to pay	Tax evaded	Round net income	fine	Total net income
9	1000	NO	300	0	700	0	5900
10	1000	NO	300	200	800	0	6600
11	1000	NO	300	200	900	0	7400
12	1000	SI	300	0	***	2400	8300
13	1000	SI	300	300	§§§	250	6500

******* => you have been audited, the audit analyses the last 4 rounds. As you evaded 200 Liras in round 11 and 200 liras in round 10 you must pay 400 liras (200 + 200) plus the fine, which is 6 times the amount evaded, i.e. 400*6=2400. Your reward in this round is therefore – 1800 Liras and the fine will be subtracted from your final reward. The final reward cannot be negative so your worst possible result is to finish the experiment without any money reward.

§§§ => The tax evasions that have been detected and fined in a previous round cannot be fined twice. At round 13 the tax evasions of round 11 and 10 are not considered in computing the fine that will be computed only for round 13 i.e. 300*6=1800 Liras. In the case that the experiment should finish at round 13 your final reward should be 6500+1000–300–1800=5000 Liras.

This is the sequence that you must follow in each round of the game:

1) write your secret code, that you will see on the computer screen, on the small sheet piece of paper that you will receive at the beginning of the experiment, you will use the secret code to be paid at the end of the experiment;

2) get the information about your round income and the tax to pay;

3) decide the amount of tax to pay (between zero and the total amount required) and write it in the appropriate window of the computer screen;

4) press the enter key.

If you do not perform the entire routine, the computer will not allow you to pass to a new round, and you will have to repeat everything.”

End of instructions.

The instructions for the experiments DY1_0 and DY1_10 are identical to those just reported with the only obvious modification of the different audit retroactivity. The instructions for experiment DY1_W contained the explanation of the meaning and functioning of the probability wheel.

The experimental subjects of DY4 experiment received this modified instructions:

“This game is about gambling. The game is computer aided, the software that you will use is pre-built, and there will be no direct intervention by the researchers during the experiment. The results of your choices will be collected only when the experiment has finished, and they will remain anonymous.

The game simulates a pure game environment: there are several rounds representing different game shots. In each round you will receive an amount of money to play with (which at the end will be your reward). In each period you must decide either to bet some of this money or to save it. Regardless of your choice, you may be pulled out at any moment of the game, and if you have decided to bet in one or more of the last four rounds you will lose the bet and have to pay a penalty. The extraction may never take place, and it is decided and performed only by the computer, without any direct intervention by the researchers.

All relevant information will be provided directly on the computer screen, and you must not communicate with anyone during the whole experiment.

At the beginning of the experiment your probability to be pulled out during the active round is 6% and is independent from the probability that other people would be extracted. This means that in a given round can happen that none subject is pulled out, or, on the opposite, that all subjects are extracted. The probability can change during the experiment but you will be informed thorough the computer screen before that this change should take place.

This is an example of the fine that you should pay if you decided to bet and would be extracted:

round	Round money	Extraction	Money to bet	Money actually Bet	Round net win	penalty	Total win
9	1000	NO	300	0	700	0	5900
10	1000	NO	300	200	800	0	6600
11	1000	NO	300	200	900	0	7400
12	1000	SI	300	0	***	2400	8300
13	1000	SI	300	300	§§§	250	6500

******* => you have been extracted, the extraction regards the last 4 rounds. As you betted 200 Liras in round 11 and 200 liras in round 10 you must pay 400 liras (200 + 200) plus the penalty, which is 6 times the amount betted, i.e. 400*6=2400. Your win in this round is therefore – 1800 Liras and the penalty will be subtracted from your final reward. The final reward cannot be negative so your worst possible result is to finish the experiment without any money reward.

§§§ => The bets that have been detected and penalised in a previous round cannot be penalised twice. At round 13 the bets of round 11 and 10 are not considered in computing the penalty that will be computed only for round 13 i.e. 300*6=1800 Liras. In the case that the experiment should finish at round 13 your final reward should be 6500+1000–300–1800=5000 Liras.

This is the sequence that you must follow in each round of the game:

1) write your secret code, that you will see on the computer screen, on the small sheet piece of paper that you will receive at the beginning of the experiment, you will use the secret code to be paid at the end of the experiment;

2) get the information about your round income and the money that you can bet;

3) decide the amount of the bet (between zero and the total amount) and write it in the appropriate window of the computer screen;

4) press the enter key.

If you do not perform the entire routine, the computer will not allow you to pass to a new round, and you will have to repeat everything.”

Appendix B: Computation of the dynamic experiments expected values with Mathematica.

```
p1=p
p2= (1 - p1) p
p3= (1 - p2) p
p4= (1 - p3) p
prob = p1 + p2 + p3 + p4
uno= prob /. p -> .06 (* Probability of being audited on 4 round with p=6%  *)
due= prob /. p -> .1  (* Probability of being audited on 4 round with p=10% *)
tre= prob /. p -> .15 (* Probability of being audited on 4 round with p=15% *)
(* The following plots show the expected values for the lotteries *)
(* Audit = 6% Income = 1000 tax = 200 *)
p1=Plot[(uno (1000-tax-5.5*(200-tax))+ (1-uno) (1000-tax)), {tax,0,200}]
(* Audit = 6% Income = 1000 tax = 300 *)
p2=Plot[(uno (1000-tax-5.5*(300-tax))+ (1-uno) (1000-tax)), {tax,0,300}]
(* Audit = 10% Income = 1000 tax = 300 *)
```

```
p3=Plot[(due (1000-tax-5.5*(300-tax))+ (1-due) (1000-tax)), {tax,0,300}]

(* Audit = 10% Income = 1000 tax = 400 *)

p4=Plot[(due (1000-tax-5.5*(400-tax))+ (1-due) (1000-tax)), {tax,0,400}]

(* Audit = 15% Income = 1000 tax = 400 *)

p5=Plot[(tre (1000-tax-5.5*(400-tax))+ (1-tre) (1000-tax)), {tax,0,400}]

(* Audit = 15% Income = 800 tax = 400 *)

p6=Plot[(tre (800-tax-5.5*(400-tax))+ (1-tre) (800-tax)), {tax,0,400}]

Show[p1,p2,p3,p4,p5,p6]
```

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