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Incentive Effects of Funding Contracts: An Experiment *

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Abstract

We examine the incentive effects of funding contracts on entrepreneurial effort decisions and allocative efficiency. We experiment with funding contracts that differ in the structure of investor repayment and, therefore, in the incentives for entrepreneurial effort provision. Theoretically the replacement of a standard debt contract by a repayment-equivalent non-monotonic contract reduces effort distortions and increases efficiency. Likewise the replacement of outside equity by a repayment-equivalent standard-debt contract mitigates distortions. We test both hypotheses in the laboratory.

Our results reveal that the incentive effects of funding contracts need to be experienced before they reflect in observed behavior. With sufficient experience observed behavior is consistent with the theoretical predictions and supports both hypotheses. If we allow for entrepreneur-sided manipulations of the project outcome we find that non-monotonic contracts lose its appeal.

Keywords: hidden information, funding contracts, incentives, experiment, standard debt contract, non-monotonic contract, state manipulation

JEL codes: C91, D82, G21

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1 Introduction

There are many real-life instances of individuals facing the opportunity of conducting a "project" that yields risky returns. Cases in point range from starting entrepreneurship to students aiming for a University degree. Typically project execution requires fixed setup costs that exceed the available funds of the project's owner-manager (henceforth entrepreneur) and are financed by outside investors, e.g. the market for loans, the market for equity, or government subsidy programs.

The specified terms of repayment to the investor form an integral element of outside financing and can differ considerably; e.g., 1) government agencies subsidizing unemployed workers to start entrepreneurship may require no repayment at all; 2) an entrepreneur may take out a loan requiring the repayment of either a constant amount or all of the available assets in case of bankruptcy; 3) student loan programs may ask students to repay less if more successful in their studies than their fellows; 4) all potential returns to entrepreneurship may be divided at a specified share between entrepreneur and investor.

Since the entrepreneur can improve the prospects of higher returns by exerting more effort, the division of yet uncertain returns between entrepreneur and investor that is fixed in the funding contract potentially affects the entrepreneur's effort choice. This raises the question of how the incentives inherent in funding contracts shape the entrepreneurial outcome. This question matters as misallocations of external funding or suboptimal incentivization of entrepreneurs can lead to static and dynamic welfare losses; the latter can result since static inefficiencies potentially inhibit the economy's growth of per capita output as that relies on technical advance where entrepreneurial contributions, in the sense of innovations, are key.[1]

The seminal paper by Innes (1990) provides a thorough theoretical analysis of how the entrepreneurial outcome is shaped in the setting of external finance with hidden effort and limited liability. Recent research on behavioral corporate finance, however, demonstrates that theoretical predictions under the self-interest-hypothesis paired with full rationality can systematically deviate from empirical outcomes. For example, Malmendier and Tate (2005) have shown that CEO overconfidence can lead to corporate investment distortions for the case of Forbes 500 CEO's. More generally Baker and Wurgler (2012) and Baker et al. (2007) provide surveys of behavioral corporate finance that distinguishes between investor-sided and manager-sided behavioral effects. The purpose of our paper is to investigate experimentally how funding contracts influence entrepreneurial behavior and to inquire into their implications for allocative efficiency in an attempt to improve our understanding of scope and extent of entrepreneur-sided behavioral effects in our setting of external finance.

Innes (1990) shows that standard debt contracts induce inefficiently low effort, thus leading to substantial efficiency losses. In contrast, non-monotonic contracts can overcome this problem under a wide range of parameter choices (non-monotonic-contracts

[1] See, e.g., Romer (1990) and Aghion and Howitt (1992). For textbook treatments of growth economics, see, e.g., Aghion and Howitt (1998) and Barro and Sala-i-Martin (2003).

hypothesis). If designed accordingly, they can induce efficient effort choices. Furthermore, Innes (1990) demonstrates that standard-debt contracts, though inefficient, are more efficient than any other repayment-equivalent monotonic repayment contract in the class of monotonic contracts (monotonic-contracts-hypothesis). In our experiment we set out to test both hypotheses. For testing the non-monotonic-contracts hypothesis we compare behavior with a standard debt contract to that observed with a non-monotonic contract yielding the same expected repayment to the investor. For testing the monotonic-contracts hypothesis, we compare a standard debt contract to a repayment-equivalent outside equity contract. To obtain a more complete picture of the incentive effects of funding contracts we also study behavioral responses with no repayment as a benchmark.[2] As already noted by Innes (1990, p.46), practical disadvantages arise with non-monotonic contracts if contracting parties face opportunities of manipulating states, e.g., by investor-sided sabotage of the entrepreneurial project or by entrepreneur-sided outside borrowing. We experimentally test a non-monotonic contract also in a broader environment that allows for a reduced form of outside borrowing where entrepreneurs can misreport return states.

We find that the incentive effects of funding contracts are too subtle to be grasped by introspection alone. At the beginning of the experiment we find no differences in entrepreneurial behavior across contract conditions at all. This is of particular importance in the studied setting since many real-life entrepreneurs are similarly inexperienced when relying on external finance for the first, and possibly only, time. Nevertheless the differential theoretical predictions strongly attract behavior over the course of the experiment. With accumulating experience behavior moves closer to the theoretical point predictions and the comparative statics predictions across funding contracts apply. With sufficient experience, behavior is consistent with the theoretical point predictions.

This paper contributes to the growing experimental literature on credit markets since, to the best of our knowledge, this is the first study that inquires into the incentive effects of funding contracts and investigates the non-monotonic-contracts and the monotonic-contracts hypotheses. An experimental study related to our setting is Serra-Garcia (2010) that explores the effects of collateral. She observes a positive relationship between collateral and entrepreneurial effort which, in contrast to standard theory, emerges only if the repayment to the investor is sufficiently low. Other experimental studies of credit markets include Brown and Zehnder (2007, 2010) investigating the effect of information dissemination of loan defaults on repayment behavior and Fehr and Zehnder (2006) studying the role of reputation in credit markets.

Our findings also complement the literature on moral hazard.[3] In this literature it is

[2]For simplicity we refer to all cases where the entrepreneur can always keep the entire return to the project as cases with a no-repayment contract, even if no explicit repayment contract was written; for example, if the entrepreneurial project is fully subsidized or if entrepreneurs do not rely on external finance and self-finance their projects instead.

[3]E.g., DeJong, Forsythe and Lundholm (1985) demonstrate the relevance of moral hazard with flat wage employment contracts. Fehr, Kirchsteiger, and Riedl (1993) and Irlenbusch and Sliwka (2005) show that agents' effort increases in the generosity of flat wages. Fehr, Klein, and Schmidt (2007), on the other hand, report that bonus contracts outperform flat wage contracts while Brandts and Charness (2004)

natural to model the principal-agent relationship such that the residual claimant owns the project (principal) and–to execute the project–requires someone else (agent) to provide an unobserved input (effort). Our setting, in contrast, allows us to explore the diametral case in which the residual claimant owning the project is the same contract party as that providing the unobservable input (effort). This assumption is natural in our setting since the entrepreneur owns the project and executes it. A second party (investor) is needed here since the execution of the project requires the provision of an indispensable input (external funding) that the entrepreneurs lacks.

The paper is organized as follows. Section 2 provides the theoretical foundation of our experimental research, section 3 summarizes the experimental design, section 4 reports our experimental results, and section 5 concludes.

2 Theoretical considerations

This section first outlines a simple model of entrepreneurial external financing that serves as the foundation for our experimental investigation. Second, it introduces basic structures of repayment contracts and their incentives effects. We review contract structures that are either frequently observed in real-life or are optimal in our setting with costless state verification. We also introduce two fundamental theoretical results, the non-monotonic-contracts hypothesis and the monotonic-contracts hypothesis, when introducing repayment contracts.

2.1 A simple model of funding

The outlined model is a discrete variant of Innes (1990). Consider an entrepreneurial project with random return Z. The underlying probability function is such that greater entrepreneurial effort increases the likelihood of outcomes with high returns. There are n states. The project return in state i is denoted by $z_i \geq 0$. Return states are numbered in ascending order, i.e. $z_i < z_j$ if $i < j$. The probability of state i depends on entrepreneurial effort $x \in [0, \overline{x}]$ and is given by $p_i(x) \geq 0$ where $p_i(x)$ is twice-differentiable. For a proper probability distribution assume $\sum_i p_i(x) = 1$ and $\sum_i p'_i(x) = 0$. To model that greater effort increases the probability for higher return states to occur, suppose that the monotone likelihood ratio property is satisfied, ie. for all $z_i < z_j$ we have $p'_i(x)/p_i(x) < p'_j(x)/p_j(x)$ implying $\partial E[Z|x]/\partial x > 0$, cf. Milgrom (1981). To ensure an interior solution, suppose marginal benefit of effort does not grow to infinity, i.e. $\lim_{x \to \overline{x}} \partial E[Z|x]/\partial x$ is finite.

The project requires start-up cost $\Gamma > z_1$.[4] The entrepreneur is endowed with wealth W. The amount of external finance required to start the project is $D \equiv \Gamma - W$.

investigate the impact of competitive imbalances and minimum wages. Contract design has been show to also affect behavior in the field, e.g., Lazear (2000) finds that replacing flat rate hourly pay by piece rates for windshield installers increases productivity while Shearer (2004) reports a similar effect for workers in tree-planting.

[4]If the start-up cost is not larger than the lowest project return z_1, the financing problem is trivial.

Since we inquire into the effects of external financing schemes on entrepreneurial activity, $D > 0$. For simplicity, let $W = 0$. We assume the entrepreneur is subject to limited liability such that the realized project return constrains repayment in low return states. A feasible repayment contract $\vec{t}$ is characterized by $\vec{t} = (t_1, t_2, ..., t_n)$ such that $t_i \leq z_i$ due to limited liability, where t_i denotes the contracted amount of repayment in state i.

The preferences of the entrepreneur are additively separable in income y and effort cost $c(x)$

$$u(x, y) = y - c(x)$$

where $c(0) = 0$, $c'(x) > 0$, $c''(x) > 0$ and $\lim_{x \to \overline{x}} c'(x) = \infty$. Since the entrepreneur's income in state i is the difference of realized project return and contracted repayment, the entrepreneur's maximization problem for any given contract $\vec{t}$ is given by:

$$\max_{x} \quad EU(x, \vec{t}\,) = \sum_{i=1}^{n} p_i(x)\,(z_i - t_i) - c(x).$$

Expected utility is maximized by effort level $\widetilde{x}(\vec{t}\,)$. For ease of exposition, let $c(x)$ be sufficiently concave to always guarantee strict concavity of the objective function. Then, the first order condition of the maximization problem characterizes a unique global maximum of entrepreneurial expected utility:[5]

$$\sum_{i=1}^{n} p_i'(\widetilde{x})\, z_i = c'(\widetilde{x}) + \sum_{i=1}^{n} p_i'(\widetilde{x})\, t_i. \tag{1}$$

The LHS of (1) gives the marginal expected project return of additional effort. The first term of the RHS is marginal effort cost.

The key to understanding how entrepreneurial incentives are related to funding contracts lies in the second term on the RHS: the marginal expected repayment to the lender, $\text{MR}(x) \equiv \sum p_i'(x)\, t_i$. If the repayment contract implies that the marginal expected repayment vanishes from (1), the entrepreneur finds it optimal to supply first-best effort x^* that prevails in the absence of external financing ($t_i = 0\ \forall i$); hence, any flat contract ($t_i = \tau\ \forall i$) induces first-best effort.[6]

If the funding contract is designed such that the marginal expected repayment, however, does not sum up to zero, the funding contract distorts the entrepreneur's effort choice and leads to inefficient effort provision and a loss of economic surplus. Specifically, the entrepreneur's optimal effort $\widetilde{x}$ decreases in the marginal expected repayment as the

[5] Due to the generality of feasible contracts and revenue distributions, it is possible to find contracts that imply a strictly negative marginal entrepreneurial income net of repayment even with zero effort (e.g., a contract that always requires full repayment except for the lowest state where no repayment is required.) Then, it is impossible to satisfy the first-order condition (1) and a boundary solution emerges such that $\widetilde{x}(\vec{t}\,) = 0$.

[6] Recall that $\sum p_i'(x) = 0$ otherwise probability would sum up to more or less than unity with variations of effort.

application of the implicit function theorem to $\widetilde{x} = f(\mathrm{MR})$ implicitly defined by (1) shows:

$$\frac{d\widetilde{x}}{d\mathrm{MR}} = \frac{1}{\sum_{i=1}^{n} p_i''(\widetilde{x})\, z_i - c''(\widetilde{x}) - \mathrm{MR}'(\widetilde{x})} < 0.$$

The denominator is the maximization problem's second-order condition so that a strictly positive marginal expected repayment implies $\widetilde{x} < x^*$ resulting in lost economic surplus.

2.2 Types of repayment contracts

Since the incentive effects of funding contracts are reflected in the marginal expected repayment to the investor, they are influenced by the structure of the funding contracts. We distinguish between four basic structures of repayment contracts that differ in the way how state-contingent repayments vary with higher project returns: flat contracts, standard debt contracts, non-monotonic contracts (hill-shaped), and outside equity. We single out these structures as they are either widely employed in real-life or because they constitute the optimal contract structure in our setting. In the following we introduce these contract structures in more detail, discuss their incentive effects, and review the non-monotonic-contracts hypothesis and the monotonic-contracts hypothesis in our framework.

2.2.1 Flat contracts and the no-repayment contract

A flat contract is fully specified by a constant payment $\tau \geq 0$ that the entrepreneur repays to the investor independently of the realized return state, hence $t_i = \tau$. In our setting feasible flat contracts satisfy $\tau \leq z_1$ due to limited liability. Since the repayment to the investor does not vary with effort, the marginal repayment under any flat contract is zero so that, trivially, any flat contract induces first-best effort. We experimentally study the no repayment contract $\vec{t}^{\mathrm{NoRepay}}$ with $\tau = 0$ that is a special case of flat contracts.

2.2.2 The standard debt contract

A widely applied funding contract is the standard debt contract that essentially reduces the repayment structure to a flat repayment claim τ independent of the realized return state. However, due to binding limited liability, the actual repayment to the lender is smaller than τ whenever the realized project return falls short off the flat repayment claim. Using our contract notation, a standard debt contract $\vec{t}^{\,\mathrm{SDC}}$ is given by

$$t_i^{\mathrm{SDC}} = \begin{cases} z_i & \text{if } z_i < \tau, \\ \tau & \text{otherwise.} \end{cases}$$

Under a standard debt contract, the entrepreneur shares with the lender the benefit of increased expected project return created by additional effort while bearing total marginal effort cost. A key characteristic of this type of contract is that the implied marginal expected repayment is strictly positive, so that the standard debt contract is inherently inefficient. To see this, note that the expected repayment to the lender under any standard debt contract is given by $\sum_{i=1}^{m-1} p_i(x)\, z_i + \sum_{i=m}^{n} p_i(x)\, \tau$ where m is the smallest payoff state that allows the entrepreneur to fully repay the fixed payment of the standard debt contract. Rewriting the expected repayment and differentiating it with respect to effort yields marginal expected repayment as

$$\mathrm{MR}^{\mathrm{SDC}}(x) = z_i \sum_{i=1}^{n} p_i' + (z_2 - z_1) \sum_{i=2}^{n} p_i' + \ldots + (z_{m-1} - z_{m-2}) \sum_{i=m-1}^{n} p_i' + (\tau - z_{m-1}) \sum_{i=m}^{n} p_i'.$$

By definition of a proper distribution function, the sum of marginal probabilities equals zero, $\sum_{i=1}^{n} p_i' = 0$, so that the first summation vanishes. All other summations differ from the first one in that marginal probabilities for low revenue states are not part of the summation. The fact that higher effort reduces the probability of low states and increases that of high states implies that the lowest payoff states are assigned negative marginal probabilities, so that, when omitting them, all remaining summations are strictly positive. It follows that the marginal expected repayment under any standard debt contract is always strictly positive and, henceforth, the induced entrepreneurial effort choice is suboptimal.

2.2.3 Optimal non-monotonic contracts and the non-monotonic-contracts hypothesis

Although standard debt contracts are inefficient, it is possible to design Pareto-improving contracts that can overcome the inefficiency inherent to standard debt contracts. These contracts are characterized by a non-monotonic repayment structure in the sense that repayment in some higher-return states is lower than repayment in some lower-return states. By decreasing repayment in high-return states, marginal repayment to the lender - being strictly positive under standard debt contracts - is reduced while the expected repayment to the lender can be preserved. It follows that the deviation from first-best effort and the implied efficiency loss with non-monotonic contracts are smaller than under standard debt contracts due to better incentives provided by the former. If designed accordingly, non-monotonic contracts can even lead the entrepreneur to exert first-best effort and eliminate any efficiency loss (Proposition 1). A numerical example that illustrates the potential magnitude of welfare gains through non-monotonic contracts which we experimentally investigate is provided in section 3.

Proposition 1. *(Non-monotonic-contracts hypothesis)*
There can exist non-monotonic contracts that are superior to standard debt contracts in terms of entrepreneurial profit and allocative efficiency due to a smaller deviation from first-best effort.

Proof omitted.

2.2.4 Outside equity contracts and the monotonic-contracts hypothesis

Outside equity contracts are a special case of monotonic contracts where the repayment to the investor is higher if the entrepreneur's revenue realization is higher, $t_i < t_j$ if $i < j$. For outside equity contracts, the share of investor repayment in revenue is the same in any state. We denote the share of investor repayment by $\sigma \in (0, 1]$ and also refer to it as the equity share. Then any outside equity contract $\vec{t}^{\,\text{Equi}}$ is defined by

$$t_i^{\text{Equi}} = \sigma\, z_i \qquad (i = 1, ..., n)$$

To pin down the contract incentives of outside equity contracts we derive the marginal expected repayment of outside equity contracts. By MLRP there is a state $q \in \{1, ..., n\}$ such that $p_i' < 0 < p_j'$ and $p_q' \geq 0$ for all $i < q < j$, i.e., all states with project returns larger than z_q become more likely with increased effort while all states with project returns smaller than z_q become less likely. Differentiating the expected repayment under outside equity, $R^{\text{Equi}} = \sigma \sum_{k=1}^{n} p_k(x)\, z_k$, and grouping terms by the sign of marginal probabilities leads to the marginal expected repayment under any outside equity contract as follows:

$$\text{MR}^{\text{Equi}}(x) = \sigma \cdot \left[\sum_{i=1}^{q-1} p_i'\, z_i + \sum_{j=q}^{n} p_j'\, z_j \right] > 0$$

where the first summation sums over strictly negative terms and the second summation sums over positive terms. Since all marginal probabilities sum up to zero and $z_k < z_m$ for any $k < m$, the second summation strictly exceeds the first one so that the sign of marginal repayment is strictly positive.

We have shown that the marginal repayment of either standard debt contract and outside equity contract is strictly positive so that both types of contracts provide incentives for suboptimal effort. It remains to address if one of the two contracts is the better one if both yield the same expected repayment. To this end Innes (1990) provides a general result that also holds in our discrete setting and that we record as the monotonic-contracts hypothesis as follows:

Proposition 2. *(Monotonic-contracts hypothesis)*
In the class of monotonic contracts, the standard-debt contract dominates any other repayment-equivalent monotonic contract, e.g. outside equity, in terms of entrepreneurial profit and allocative efficiency due to a smaller deviation from first-best effort.

Proof omitted.

3 Experimental design

3.1 Model parametrization, treatments, and theoretical predictions

In the experiment, we implement the model introduced in section 2 with three states and linear probability functions. Project revenues and probability functions for states 1, 2, and 3 to occur are as follows:

$$\begin{array}{ll} z_1 = 500 \text{ ECU} & \text{with } p_1(x) = 0.6 - 0.6\frac{x}{100}, \\ z_2 = 9,000 \text{ ECU} & \text{with } p_2(x) = 0.4, \\ z_3 = 10,000 \text{ ECU} & \text{with } p_3(x) = 0.6\frac{x}{100}, \end{array}$$

where effort $x \in [0, 100]$. By increasing effort, probability is shifted from the low project return of 500 ECU to the high project return of 10,000 ECU. This can be thought of as probability mass being shifted from the low to the intermediate return to the same extent as from the intermediate to the high return.

The entrepreneur faces effort cost $c(x) = 0.5\,x^2$. The start-up investment of the project is fixed at $\Gamma = 3,120$ ECU. The rate of return an outside lender requires to finance the project is $r = 0.25$.

We investigate eight treatments that we divide into four basic treatments and four extension treatments. The basic treatments provide the building block for our discussion of the incentive effects of contract structures such as the non-monotonic contract, the standard debt contract, the equity contract, and the no repayment contract. They differ in the repayment contract only. The extension treatments serve the purpose of extending the discussion and controlling for selected aspects with changes of the environment beyond the repayment contract.

To minimize confounding effects that could emerge from social preferences or strategic uncertainty, we use an individual-choice experiment where incentive structures are set exogenously by the experimenter and are not affected by the actual choice behavior of the subjects in the experiment. This aspect of our design captures the anonymous setting in much of the financial markets since funding contracts are frequently offered through financial institutions like banks where social preferences seem less relevant.[7] We refer to treatments by the name of the implemented contract structure as this is the main treatment variable and the only treatment variable that is changed in the basic treatments. The specifications of the exogenously chosen repayment contracts are as follows: As a benchmark we run a self-financing treatment (NoRepay), in which there is no repayment at all. Furthermore, we study three standard-debt-contract conditions (SDC, SDC2, SDC-OS), two non-monotonic-contract conditions (NMC, NMC-R), and two equity conditions (EQUI, EQUI2), in which subjects are exposed to the respective kind

[7]Reiss and Wolff (2011) endogenize the selection of repayment contracts and study the structures of subject-selected repayment contracts and their effects on entrepreneurial effort choice.

of repayment contract. Table 1 summarizes all repayment contracts used in the basic treatments and table 2 details the contracts used in the extension treatments.

Repayment/Treatment	SDC	NMC	NoRepay	EQUI (72.5%)
t_1 (z_1=500)	500.00 ECU	500.00 ECU	0.00 ECU	362.50 ECU
t_2 (z_2 = 9000)	7,383.30 ECU	9,000.00 ECU	0 ECU	6,525.00 ECU
t_3 (z_3 = 10000)	7,383.30 ECU	500.00 ECU	0 ECU	7,250.00 ECU
Effort prediction	15.7	57.0	57.0	15.7
Total surplus	1,551.66 ECU	2,404.50 ECU	2,404.50 ECU	1,551.66 ECU
Exp. Repayment	3,900.00 ECU	3,900.00 ECU	0.00 ECU	3,476.30 ECU
Entrepreneur's EU	769.93 ECU	1,624.50 ECU	2,404.50 ECU	1,195.35 ECU
Investor's EU	780.00 ECU	780.00 ECU	0.00 ECU	356.20 ECU

Table 1: Repayment contracts by basic treatment

First, consider the repayment contracts of the basic treatments given in Table 1. The required expected repayment to the lender, $(1 + r)\,\Gamma$, determines the state-contingent repayments under the standard debt contract SDC and the non-monotonic contract NMC. These two contracts each lead to the same expected repayment of 3,900 ECU. In contrast, the state-contingent repayments in treatment EQUI are chosen such that the effort prediction equals the effort prediction prevailing in treatment SDC of 15.7. This requires that the state-independent equity share is 72.5%. Evidently, the standard debt contract condition SDC and the equity contract condition EQUI lead to a loss in total surplus and the entrepreneur's payoff is substantially smaller. If the standard debt contract of treatment SDC is replaced by the repayment-equivalent non-monotonic contract of treatment NMC, total surplus increases by 55%, while the surplus accruing to the entrepreneur more than doubles.[8]

Second, consider the repayment contracts used in the extension treatments as given in Table 2. The non-monotonic contract used in treatment NMC-R is identical to the one used in treatment NMC, but both treatments differ in the way realized project return states are reported to the computerized investor. While there is automatic and accurate reporting of the realized return state in treatment NMC, entrepreneurs themselves report the realized return state with no verification in treatment NMC-R when it matters, i.e. in case of a medium return or a high return state. This means that entrepreneurs can falsely report a high return state whenever a medium return state was realized to decrease the state contingent repayment from 9,000 ECU to 500 ECU in NMC-R. Thus, an entrepreneur with self-regarding monetary preferences finds it optimal to falsely report a high return state whenever a medium return state was realized so that the repayment is always 500 ECU independently of the realized state. As a result, treatment NMC-R allows the entrepreneur to strongly increase the expected payoff at the expense of the investor by inaccurate state reporting.

[8]The exact numbers are 54.96% and 110.99%.

Treatments SDC2 and EQUI2 are designed such that the expected repayment to the investor is the same. Although there is a minute difference of 0.38 ECU between the expected repayments, we neglect it and regard the expected repayments in either treatment as sufficiently close to be essentially the same. Importantly, the difference in contract structures implies a large predicted loss of total surplus in EQUI2 as compared to SDC2 that also reflects in substantially different effort predictions. Finally, treatment SDC-OS is identical to basic treatment SDC except that SDC-OS implements subjects' effort choice as a one-shot decision with no repetition and high-powered incentives while there were 15 rounds with feedback in treatment SDC as well as in any other treatment.

Repayment/Treatment	NMC-R	SDC2		EQUI2 (80%)	SDC-OS
t_1 (z_1=500)	500.00 ECU	500.00 ECU		400.00 ECU	500.00 ECU
t_2 ($z_2 = 9000$)	9,000.00 ECU	6,449.40 ECU		7,200.00 ECU	7,383.30 ECU
t_3 ($z_3 = 10000$)	500.00 ECU	6,449.40 ECU		8,000.00 ECU	7,383.30 ECU
Effort prediction	57.0	21.3		11.4	15.7
Total surplus	2,404.50 ECU	1,767.38 ECU		1,364.82 ECU	1,551.66 ECU
Exp. Repayment	500.00 ECU	3,640.22 ECU	$\simeq$	3,639.84 ECU	3,900.00 ECU
Entrepreneur's EU	5,024.50 ECU	1,247.16 ECU		844.98 ECU	769.93 ECU
Investor's EU	−2,620.00 ECU	520.22 ECU	$\simeq$	519.84 ECU	780.00 ECU

Table 2: Repayment contracts by extension treatments

Behavior in the laboratory that deviates from our theory-based predictions which assume risk-neutrality may be attributable to the effects of individual risk preferences such as various degrees of risk-aversion. To address this concern, we reduce the risk in subjects' payoffs by paying them the average payoff over 50 different projects, with outcomes determined by independent draws from the probability distribution determined by effort choice instead of using the payoff realized for a single project. This method was successfully introduced by Kirchkamp, Reiss, and Sadrieh (2008) in an auction setting.

3.2 Procedures and other details

The experiment was programmed using z-tree (Fischbacher, 2007). Treatments NoRepay, SDC, NMC, and EQUI were run at the Erfurt Laboratory for Experimental Economics (eLab)and treatments SDC-OS, NMC-R, SDC2, and EQUI2 were run at the Behavioral and Experimental Economics Laboratory at Maastricht University (BEElab). Twelve subjects were recruited for each session using ORSEE (Greiner, 2004) at either location so that 8x12 = 96 subjects participated in the study. No subject participated in more than one session. We ran one session for each treatment, obtaining twelve independent observations per treatment.

On the day, subjects were welcomed and randomly assigned to private cabins. Written instructions were handed to them before being read aloud by the experimenter. Subsequently, subjects entered their cubicles and had some time to go over the instructions again and ask any questions they might have. Questions were answered individually.

Any round profit was added to a subject's capital balance and any loss was subtracted. At the end of the experiment the capital balance was converted into EUR and paid to the subject in cash. In principle it is possible that subjects go bankrupt by repeatedly choosing excessively high effort levels so that high effort cost that lead to losses accumulate. To avoid that effort choices are biased by limited liability considerations, every subject was given an initial endowment of 12 500 ECU.[9] The endowment allows subjects to survive a few rounds of the experiment at the maximum effort of 100 with an effort cost of 5 000 ECU. The instructions informed subjects that they would be removed from the experiment if their balance dropped below 2,500 ECU.[10] We did not expect to observe any case of bankruptcy, however, it happened once in treatment EQUI where a subject was bankrupt in round 6 after selecting the effort levels of $90, 85, 100, 100, 95$, and 100 in rounds 1-6. We removed this observation from the data set used for data analysis as the observation was incomplete.

Subjects played 15 repetitions of the game except for treatment SDC-OS with a single round of decision making and were paid according to their individual performance. The experimental sessions lasted for one hour or less, average earnings being € 9.65 (≈ US$ 12.50) for the experiments at the eLab and € 14.75 (≈ US$ 19.11) at the BEElab.[11] Payments were settled individually to ensure subjects' anonymity.

3.3 Testable hypotheses

We derive the hypotheses that we test in the experiment from the theoretical predictions summarized in Table 1 and Table 2. At the least demanding level we expect that effort choices are influenced by the contract condition in a systematic way. In particular, we hypothesize that observed behavior is qualitatively consistent with the comparative statics of changing the funding contract. This leads to our first and most basic hypothesis:

Hypothesis 1 *Observed effort choices are influenced by contract conditions and share the ordinal rank across contract conditions with the theoretical ranking prediction*:
$x^{\text{NoRepay}} \approx x^{\text{NMC}} > x^{\text{SDC}} \approx x^{\text{EQUI}}$.

Hypothesis 1 is weak in the sense that it is a qualitative one that disregards the quantitative nature of the theoretical point predictions. Since the precise optimal effort values allow us to predict behavior also quantitatively we strengthen the first hypothesis by hypothesizing that behavior is also consistent with the point predictions:

[9] Except of treatment NoRepay and one-shot treatment SDC-OS with different conversion rates, where the endowments were set to 100 000 ECU and 3 000 ECU, respectively.

[10] In treatment NoRepay the threshold was 20 000 ECU and in the one-shot treatment SDC-OS bankruptcy procedures were irrelevant and not mentioned in the instructions.

[11] Average earnings in Maastricht are higher than in Erfurt due to extension treatment NMC-R that allows for substantially higher earnings with false state reporting.

Hypothesis 2a *Observed effort choices on average match the theoretical point predictions of effort.*

A particular strength of the model is its parsimony. It provides a single equation, equation (1), that predicts the effort level for any repayment contract. Although hypothesis 2a relates to the optimal effort equation, it hypothesizes on the comparisons of observed effort to predicted effort for each contract condition separately. This allows for some flexibility as the point prediction of some contract condition may fit the data for some repayment contract better than for another repayment contract. To strengthen our hypothesis on the theory's predictive power, hypothesis 2b supposes that the optimal effort prediction holds for *all* repayment contracts in the basic treatments at the same time:

Hypothesis 2b *The optimal effort function (1) explains observed effort choices well in all basic contract conditions simultaneously.*

The non-monotonic-contracts hypothesis compares allocative efficiency and profits obtained under the non-monotonic contract NMC to that under the standard debt contract SDC yielding the same expected investor repayment theoretically.

Hypothesis 3 *(Non-monotonic-contracts hypothesis) Allocative efficiency and entrepreneurial profits are higher under the non-monotonic contract NMC than under the standard debt contract SDC.*

Innes (1990) has shown that the standard debt contract dominates any other repayment-neutral contract in the class of monotonic contracts. We summarize this result as the monotonic-contracts hypothesis:

Hypothesis 4 *(Monotonic-contracts hypothesis) Effort, allocative efficiency, and entrepreneurial profits are higher under the standard debt contract SDC2 than under the equity contract EQUI2.*

The subtle state-contingency of repayments under the non-monotonic contract is crucial for its success in implementing first-best effort. At the same time this key characteristic provides strong incentives for misreporting states to manipulate the repayment. This leads to practical disadvantages of the non-monotonic contract whenever there are opportunities for manipulation. We test for the misrepresentation of states with treatment NMC-R where subject entrepreneurs themselves report the project outcomes determining investor repayments without state verification. Here we put forward the hypothesis that there is the tendency of subjects to accurately report the realized payoff states when it matters though there is considerable evidence that subjects misrepresent states if it is to their monetary advantage:[12]

[12] See, e.g., Fischbacher and Heusi (2008).

Hypothesis 5 *(No-misrepresentation hypothesis) The number of reported medium return states is equal to the number of realized medium return states.*

In light of our experimental results we will argue that experiencing contract incentives matters. Experience with contract incentives accumulates over the course of the experiment. Since the cash payoffs of our subjects accumulate over each of the fifteen rounds of the experiment, subjects are exposed to relatively low monetary incentives in a given round. This raises the question if it is missing experience at the beginning of the experiment that explains suboptimal choices or if subjects employ an inexpensive experimentation strategy to find their way to the optimum instead of reasoning about contract incentives ex ante. To control for this possibility we compare the effort choices observed in a one-shot treatment where only ex ante reasoning about contract incentives matters to the first-round choices in an equivalent multiple-rounds treatment and expect to find no differences:

Hypothesis 6 *(Experience-matters hypothesis) Effort deviations from the predicted effort level in the first round out of fifteen rounds observed in treatment SDC are similar to these observed in the single-round treatment SDC-OS.*

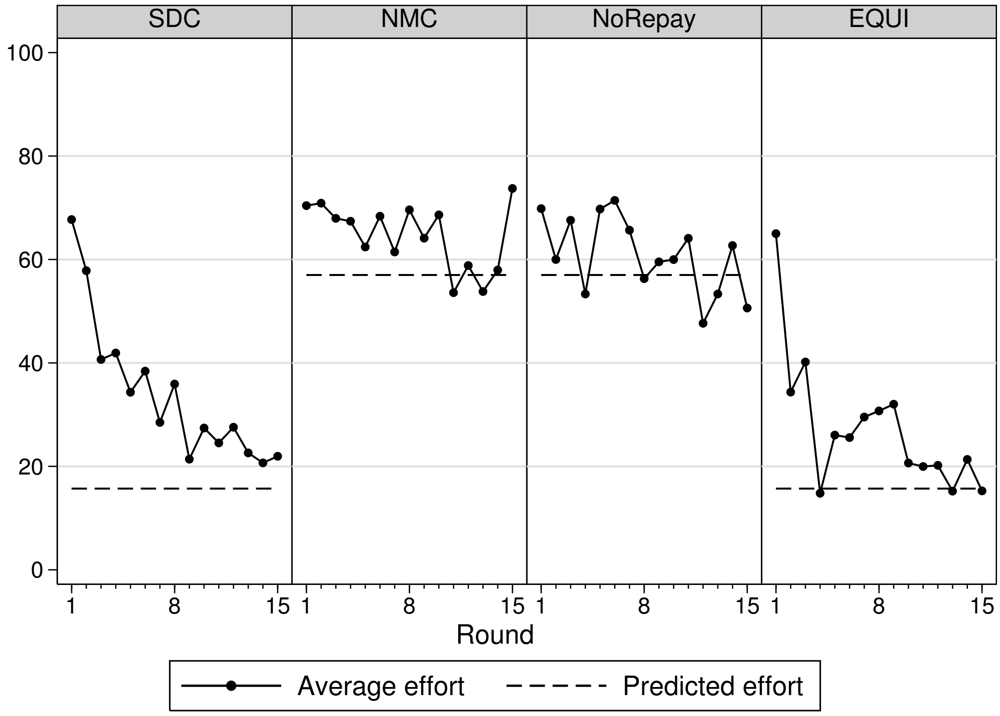
4 Experimental results

First we investigate if incentives matter and address the experience-matters hypothesis with the results of control treatment SDC-OS. Then we explore learning dynamics. Next we examine the non-monotonic-contracts hypothesis and the no-misrepresentation hypothesis. Finally we analyze the monotonic-contracts hypothesis.

4.1 Effects of funding contracts on effort choice

Let us begin by addressing the fundamental question if funding contracts affect behavior at all and, if so, see to what extent it is consistent with the theoretical predictions. In the experiment, any incentive effect of funding contracts should be reflected directly in the observed effort choices. Figure 1 depicts average effort by treatment and by round. The patterns of the average effort paths across the contract types show that funding contracts embody incentives strongly affecting behavior. Strikingly, hypothesis 1 on comparative statics seems to be confirmed entirely. The paths of average effort, $\{\overline{x}_t, t = 1, ..., 15\}$, seem similar when they are supposed to be similar, $\overline{x}_t^{\mathrm{SDC}} \approx \overline{x}_t^{\mathrm{EQUI}}$ and $\overline{x}_t^{\mathrm{NMC}} \approx \overline{x}_t^{\mathrm{NoRep}}$ and seem to differ in the hypothesized direction when they are supposed to differ, $\overline{x}_t^{\mathrm{NMC}}$, $\overline{x}_t^{\mathrm{NoRep}} > \overline{x}_t^{\mathrm{SDC}}$, $\overline{x}_t^{\mathrm{EQUI}}$. This impression is formally confirmed by testing for differences in average effort among any pair of contract conditions, separately for each round, using two-tailed Mann-Whitney-U-tests,[13] see Table 3 for a summary. The table shows the number of significant and insignificant differences of average effort that we find for each pair of

[13] We also checked for treatment differences using the t-test and essentially find the same results.



The figure depicts average effort (solid lines) over rounds under the standard debt contract (left), the non-monotonic contract (second-left), if there is no repayment (second-right), and under outside equity (right). The theoretical effort prediction is indicated by dashed lines.

Figure 1: Average effort and predicted effort by round

contract conditions. The test results indicate that we observe significant differences in average effort in almost all of the rounds for all cases where theory predicts differences in average effort between contract conditions. Similarly, for all cases where theory predicts that average effort does not differ across contract conditions, in almost all of these rounds we do not observe significant differences. Hence, there is strong support for hypothesis 1.

Comparing the paths of average effort to the theoretical predictions, as indicated by the dashed lines in Figure 1, illustrates that observed behavior is, however, much richer than predicted by theory. For example, in contrast to the static prediction for the contract condition SDC, $x^{\text{SDC}} = 15.7$, observed average effort changes considerably over the course of the experiment as can be seen in the left panel of the figure. The graph for the contract condition SDC (left panel) shows that nevertheless, the static prediction turns out to be rather useful as it attracts observed average effort over time. Overall, the standard debt contract leads subjects to implement inefficiently low levels of effort consistent with theory most of the time but not in the beginning of the experiment: In the first two rounds of the experiment average effort does not differ from the first-best level of $x^* = 57$ ($p > 0.185$), inconsistent with the theoretical prediction for the SDC condition. For all of the remaining 13 rounds, the t-test indicates significant differences between average effort

Contract condition	NMC	NoRepay	EQUI
SDC	$x^{\text{SDC}} < x^{\text{NMC}}$	$x^{\text{SDC}} < x^{\text{NoRepay}}$	$x^{\text{SDC}} = x^{\text{EQUI}}$
No. of sign. diffs.	13 ($p \leq 0.026$)	12 ($p \leq 0.074$)	3 ($p \leq 0.090$)
No. of insign. diffs.	2 ($p \geq 0.229$)	3 ($p \geq 0.191$)	12 ($p \geq 0.185$)
Round average	$p = 0.000$	$p = 0.000$	$p = 0.268$
NMC		$x^{\text{NMC}} = x^{\text{NoRepay}}$	$x^{\text{NMC}} > x^{\text{EQUI}}$
No. of sign. diffs.		1 ($p = 0.088$)	14 ($p \leq 0.025$)
No. of insign. diffs.		14 ($p \geq 0.133$)	1 ($p = 0.829$)
Round average		$p = 0.326$	$p = 0.000$
NoRepay			$x^{\text{NoRepay}} > x^{\text{EQUI}}$
No. of sign. diffs.			14 ($p \leq 0.060$)
No. of insign. diffs.			1 ($p = 0.600$)
Round average			$p = 0.000$

The table reports, for any pair of treatments, the results of roundwise Mann-Whitney U-tests. The null hypothesis is that there is no difference in the central locations of effort observed in the paired treatments. The alternative hypothesis is that there is one. For any comparison of treatments, the theoretical comparative statics effort prediction and the number of significant and insignificant differences out of all of the 15 roundwise tests is reported together with the obtained highest and lowest p-values in parenthesis. Further, the p-value of the test on the round averages is reported.

Table 3: Summary of Mann-Whitney U-tests comparing effort across contract conditions.

and first-best effort (two-tailed, $p < 0.075$).[14] Comparing the data to the theoretical point prediction of inefficient effort, $x^{\text{SDC}} = 15.7$, shows that average effort mostly differs from this prediction except for the end of the experiment: According to the t-test, average effort is not significantly different from the predicted effort level in 6 of 15 rounds (two-tailed, $p > 0.110$) that happen to be at the end of the experiment (rounds 11-15 and round 9). It identifies significant differences for all other rounds ($p < 0.069$).[15] We explore the learning of funding contract incentives in more detail in subsection 4.3.

Next, let us consider the outside equity contract in more detail. It is designed to induce the same level of effort as the standard debt contract, $x^{\text{EQ}} = x^{\text{SDC}} = 15.7$. Though average effort observed in the outside equity condition (right panel) evolves very similar to that observed in the SDC condition (left panel), one subtle difference between both paths of average effort is, perhaps, that the convergence behavior towards the theoretically predicted effort level seems slightly faster under the outside equity contract. This is consistent with the results of roundwise comparisons of average effort to its prediction since deviations from the prediction fade away later in the SDC condition. In the EQUI condition, the t-test finds significant differences in the first three rounds only (two-tailed, $p < 0.012$ for rounds 1-3 and $p > 0.104$ for any other round), while it finds a significant difference in each of the first eight rounds in the SDC condition.[16]

[14]Similarly, the sign test reveals significant differences between observed median effort and first-best effort in 10 of 15 rounds (two-tailed, $p < 0.007$); for the remaining five rounds at the beginning of the experiment (rounds 1-4 and 8), observed differences are insignificant ($p > 0.145$).

[15]The sign-test finds no significant differences in rounds 7-15 and round 3 (two-tailed, $p > 0.146$) and reveals significant differences in all other rounds ($p < 0.039$).

[16]The sign test indicates significant differences in rounds 1-2 and round 7 (two-tailed, $p < 0.065$) in

In contrast to converging average behavior under the standard debt contract and under the outside equity contract, there is neither converging nor diverging behavior under the non-monotonic contract or under the no repayment contract. In the treatment conditions NMC and NoRepay, first-round average effort is close to the theoretical prediction of $x^{\text{NMC}} = x^{\text{NoRepay}} = 57$ and seems to fluctuate in its neighborhood over time as can be seen in Figure 1. In fact roundwise comparisons of average effort to the predicted level do not suggest a systematic trend over time. There are only a few significant differences that seem arbitrarily distributed over the course of the experiment in either treatment. Specifically the t-test reveals significant differences in six rounds (1-2, 6, 8, 10, and 15, $p < 0.081$) in condition NMC and significant differences in four rounds (1 and 5-7, $p < 0.099$) in condition NoRepay.[17]

Result 1. *Funding contracts strongly influence the choice of effort in a way that is consistent with the comparative statics predictions except for the beginning of the experiment (support for hypothesis 1). Behavior adjusts to the theoretical point predictions through repeated exposure to incentives over time (partial support for hypothesis 2a).*

To quantify the extent to which the incentives of funding contracts influence the effort choice once incentives have been absorbed, we estimate first-order condition (1) with data from the second half of the experiment, i.e. rounds 9-15. In our parametrization, the first-order condition can be explicitly solved for optimal effort and simplifies to

$$x^* = 57 - \frac{6\,(t_3 - t_1)}{1\,000}. \tag{2}$$

First, we estimate[18] the unrestricted model

$$x_{it} = \beta_0 + \beta_1 \widetilde{t}_1 + \beta_2 \widetilde{t}_2 + \beta_3 \widetilde{t}_3 + u_{it} \tag{3}$$

The dependent variable x_{it} is the effort choice of participant i in round t, $\widetilde{t}_s$ is the repayment in state s measured in thousands of ECU (i.e., $\widetilde{t}_s = t_s/1\,000$), and u_{it} is the residual. The unrestricted model does not impose any restriction derived from theory on the specification apart from the linearity assumption. This allows us to explore if the repayment in state 2, t_2, affects effort choice though theoretically irrelevant and to check if the repayments in states 1 and 3 influence behavior similarly strong.

If observed behavior is fully consistent with theory, then we expect to estimate coefficients such that the optimal effort function (2) is reproduced by specification (3), i.e. $\widehat{\beta}_0 = 57$, $\widehat{\beta}_1 = 6$, $\widehat{\beta}_2$ not significantly different from zero, and $\widehat{\beta}_3 = -6$. Table 4 presents regression results that are broadly consistent with theory. Although the estimates reproduce

the EQUI treatment and five of the first six rounds ($p < 0.039$ for rounds 1-2 and 4-6 and $p = 0.146$ for round 5.)

[17] The sign test finds significant differences in rounds 4 and 15 ($p \leq 0.039$) in the NMC condition and significant differences in rounds 5 and 7 ($p \leq 0.039$) in the NoRepay condition.

[18] We estimate this and the next model by OLS such that the computation of standard errors takes into account that observations of the same individual might be correlated across time (Rogers, 1993).

	coefficient	robust σ	t	p-value	95% conf	interval
I) Unrestricted model (3)						
(Intercept)	56.85	4.896	11.76	0.000	47.11	66.58
$\widetilde{t}_1$	34.74	52.585	0.66	0.512	-71.11	140.89
$\widetilde{t}_2$	-1.09	3.058	-0.36	0.723	-7.25	5.07
$\widetilde{t}_3$	-5.75	0.634	-9.06	0.000	-7.02	-4.47
No. of obs: 329, No. of clusters: 47, $R^2 = 0.4412$						
II) Restricted model (4)						
(Intercept)	59.19	3.038	19.48	0.000	53.07	65.30
Marg. Repayment	0.89	0.093	9.61	0.000	0.71	1.08
No. of obs: 329, No. of clusters: 47, $R^2 = 0.4361$						

Note: State-contingent repayments are normalized such that $\widetilde{t}_s = t_s/1000$. Estimation results are computed by OLS with robust standard errors using data from the second half of the experiment, rounds 9-15.

Table 4: Estimation results of equations (3) and (4).

essential features of the optimal effort function, the joint hypothesis that the estimated coefficients satisfy the theoretical point predictions precisely is rejected at 6.0%.[19]

In our parametrization, the additional repayment to the investor arising if the entrepreneur devotes one more unit of effort to the project, i.e. the marginal repayment, is constant for any repayment contract and given by

$$\mathrm{MR} = \frac{6\,(t_3 - t_1)}{1\,000}.$$

It depends on the funding contract through the repayments in states 1 and 3 only. Comparing marginal repayment MR to the optimal effort function (2) shows that optimal effort is simply given by first-best effort, $x^* = 57$, reduced by the amount of marginal repayment. To quantify the effect of a funding contract's marginal repayment on effort, we regress observed effort on marginal repayment using the following restricted specification:

$$x_{it} = \gamma_0 - \gamma_1\, 6\,(\widetilde{t}_3 - \widetilde{t}_1) + u_{it} \tag{4}$$

In this regression equation, coefficient γ_1 indicates the effect of marginal repayment on effort. Theoretically we expect to find on estimate of $\widehat{\gamma}_1 = 1$. Any positive estimate, $\widehat{\gamma}_1 > 0$, would indicate that reducing effort would be correlated with changes in the repayment contract that require greater repayment if the entrepreneur exerts additional effort. If we found an estimate of $\widehat{\gamma}_1 > 1$, then observed effort would respond excessively strong to contractual changes that lead to changes of marginal repayment. In this case a replacement of the standard debt contract (with strictly positive marginal repayment) by a non-monotonic contract (with zero marginal repayment) would increase effort by an

[19] An F-test of the joint hypothesis I) $\widehat{\beta}_0 = 57$, II) $\widehat{\beta}_1 = -\widehat{\beta}_3$, III) $\widehat{\beta}_1 = 6$, IV) $\widehat{\beta}_2 = 0$ with $F_{4,46} = 2.44$ yields $p = 0.0602$.

amount that is larger than predicted theoretically. Table 4 reports the estimation results. It turns out that the coefficient on marginal repayment is not significantly different from one (two-tailed t-test, $p = 0.259$). Therefore, on average, marginal repayment captures the incentives provided by funding contracts on effort choice as theoretically predicted. Before moving on to study the learning of contract incentives in more detail, we summarize our result on effort choice behavior as compared to optimal choice:

Result 2. *Observed average behavior is largely consistent with the theoretical point predictions given by the optimal effort choice function (1) once sufficient experience accumulates (partial support for hypothesis 2b).*

4.2 Experience-matters hypothesis: control treatment SDC-OS

In the preceding subsection we interpreted the convergence of average effort towards the optimum observed over the course of the experiment as showing that subjects need to acquire sufficient experience with contract incentives before incentives take effect. A similar convergence pattern would result, however, if subjects used a trial-and-error strategy for choosing effort. In the basic treatments discussed before, the cost of adopting this experimentation strategy at the beginning of the experiment is rather low since cash earnings accumulate over each of fifteen rounds. To test the validity of this explanation we compare the first-round choices of the 15-rounds-treatment SDC to the effort choices of the single-round treatment SDC-OS where the cash value of one ECU is raised by a factor of fifteen so that monetary incentives are high-powered in SDC-OS.

Treatment/Statistic	Average	Median	Std. Dev.
SDC (first round)	67.7	68.5	26.433
SDC-OS	54.8	54.5	27.303
SDC effort prediction	15.7		

Table 5: Effort choices in treatment SDC-OS and in the first round of treatment SDC.

Table 5 shows that the average effort in treatment SDC-OS, 54.8, exceeds the predicted value of 15.7. Formal testing reveals that this difference is statistically significant (two-tailed t-test, $p = 0.000$; two-tailed sign test, $p = 0.006$). Further, the descriptive data shows that average effort in treatment SDC-OS is somewhat smaller than average effort observed in the first round of treatment SDC. This difference is, however, insignificant according to either the t-test or the MWU-test (two-tailed t-test, $p = 0.250$; two-tailed MWU-test, $p = 0.203$). As a result we rule out that subjects follow the trial-and-error-strategy and conclude that the actual experience of contract incentives matters which cannot be substituted by raising monetary incentives to promote ex ante reasoning about contract incentives.

Result 3. *Effort choices in the one-shot, high-powered incentives treatment SDC-OS are* *similar to the choices observed in the first round of treatment SDC (support for hypothesis 6).*

4.3 Learning incentives

The fundamental differences in the incentives provided by the experimentally studied funding contracts seem not to be reflected in the observed effort choices at the beginning of the experiment according to a comparison of first-round effort choices across contract conditions, see the left panel of Figure 1. It may be unsurprising that the differences in funding contract incentives do not induce behavior that is in line with the point predictions precisely– but it is striking that there seem to be no differences across contract conditions in the first round at all. To look at this aspect in more detail we compare the distributions of first-round effort choices. The left panel of Figure 2 depicts the empirical cumulative distributions and shows that they are rather similar and independent of the contract condition. In fact the Kolmogorov-Smirnov test applied to any pair of first-round distributions fails to reject the hypothesis of identical distributions of observed effort choices at any reasonable level of significance ($p \geq 0.777$).

The failure of finding significant differences in first-round effort behavior across these contract conditions where it should matter, e.g. SDC as compared to NoRepay, is important. It suggests that the incentives provided by funding contracts are too weak or too subtle to be grasped by *ex ante* introspection. The result that effort choices change over the course of the experiment towards the theoretical prediction reveals that repeated experience is required for contract incentives to take effect. Only after sufficiently-repeated exposure to contract incentives is average behavior consistent with the theoretical predictions, as suggested by Figure 1 and by the corresponding statistical tests.

To further our understanding of how the incentives of funding contracts are learned and to see if the shape of funding contracts affects the way of learning incentives, we estimate two learning models that have been applied in the previous literature, the experience-weighted attraction learning model (EWA; see Camerer and Ho, 1999), and a reinforcement learning model (RI; e.g., Roth and Erev, 1995).

4.3.1 Implementation of EWA and RI models

For details about the experienced-weighted attraction learning model, see Camerer and Ho (1999), Ho, Xin, and Camerer (2008), or Feri, Irlenbusch, and Sutter (2010). In brief, the EWA model describes a decision maker's choice by mapping state variables associated with actions, referred to as 'attractions', into a probability distribution of choice variables. EWA assumes that the attraction value A_t^x of choosing action x at the end of period t– after experiencing (or imagining) the payoff $\pi_t(x)$ from choosing (or potentially choosing) action x in period t–is a weighted average of its past attractions and its payoff, specifically,

$$A_t^x = \frac{\phi\, N_{t-1}\, A_{t-1}^x + \widetilde{\pi}_t(x)}{N_t}$$

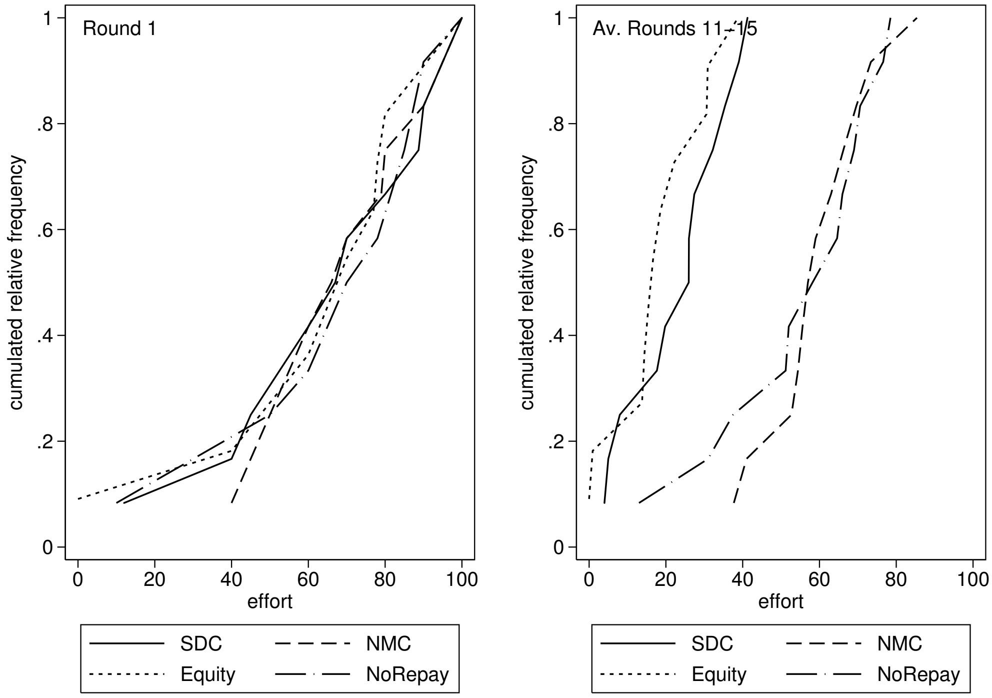


Figure 2: Cumulative distribution functions of effort observed in the first round (left panel) and averaged over the last five rounds (right panel) of the experiment.

where the experience process is governed by $N_t = \phi(1-\kappa)\,N_{t-1} + 1$ and the payoff to action x is

$$\widetilde{\pi}_t(x) = \begin{cases} \pi(x) & \text{if } x \text{ is chosen action in } t \\ \delta\,\pi(x) & \text{otherwise.} \end{cases}$$

The parameter ϕ discounts past attractions and the parameter κ indicates the importance of accumulated experience measured as the number of times the choice situation was experienced. An important difference between EWA and RI models is that EWA allows for attraction updating not only through experiencing payoffs via the actually chosen action but also through imagining payoffs to unchosen actions. It captures any potential difference between the actual payoff experience and its imagination by discounting imagined payoffs at δ.

For mapping attractions into choice probabilities we use the logistic form so that the probability of choosing action x in period $t+1$ is given by

$$\mathrm{Pr}^x_{t+1} = \frac{e^{\lambda\,A^x_t}}{\sum_{a=1}^m e^{\lambda\,A^a_t}}$$

where m is the number of actions. The parameter λ indicates the sensitivity of choice probabilities to attractions. E.g., there emerges random choice for $\lambda = 0$ and with increasing λ choice converges to the payoff-maximizing choice.

Before applying EWA to our setting we have to overcome two obstacles. First, the choice variable of our interest, effort, is continuous while EWA is designed for describing discrete choice. We address this issue by discretizing the effort space analogously to Capra, Goeree, Gomez, and Holt (1999). In particular we round observed effort to the nearest integer so that there are $m = 101$ effort choices, i.e. the discretized effort space is $\{0, 1, ..., 100\}$. Second, unlike with discrete choice under certainty, in our setting payoff information is only partially available due to unknown realizations of project outcomes that would have resulted from any unchosen effort level: our participants are informed about their actual payoffs implied by the actual set of project realizations for the chosen effort levels, but they do not know the payoffs that would have emerged for any unchosen effort level. Following Ho, Wang, and Camerer (2008) we replace the unknown payoff by the average over the set of possible forgone payoffs from the unchosen effort level which is the expected payoff in our case.[20] For consistency, we also replace the actual payoff by the expected payoff conditional on the actually chosen effort level. In our case this is a minor change as the entrepreneur's payoff in our experiment is the average payoff over 50 project realizations.[21]

Following Camerer (2003) and Ho, Camerer, and Chong (2007) we impose the restriction $N_0 = 1$. For specifying the levels of initial attraction, A_0^x, we use the approach of Ho, Wang, and Camerer (2008, fn 16), also followed by Feri, Irlenbusch, and Sutter (2010), to calibrate them such that the choice probabilities approximately[22] imply the distribution of relative frequencies as observed in the first round of the experiment. When obtaining the frequency distribution of first-round data we pool the data across contract conditions as first-round choices do not significantly differ. In particular, the initial levels of attraction satisfy the equation system $(j = 1, ..., m)$:

$$A_0^j - \frac{1}{m}\sum_j A_0^j = \frac{1}{\lambda}\ln(\widetilde{f}^j)$$

where $\widetilde{f}^j = f^j/\Pi_k f^k)^{1/m}$ and f^j is the frequency of observing action j in the first round.

We investigate the reinforcement learning model as a special case of the EWA model. For that we impose the restrictions $\delta = 0$, so that non-experienced payoffs do not influence attractions, and $\kappa = 1$, so that the count of experienced choices is irrelevant. With these restrictions attraction levels simplify to the reinforcement levels of the RI model with gradual forgetting as studied in Roth and Erev (1995). Unlike Roth and Erev (1995) we continue using the logistic form for mapping the reinforcement levels into the choice probabilities to facilitate parameter comparisons.[23]

[20] Ho, Wang, and Camerer (2008) provide an extension of EWA to partial payoff information and apply it to centipede game data.

[21] In each treatment, the difference between expected payoff and actual payoff is smaller than 0.5% of the expected payoff on average.

[22] It is only possible to approximately reproduce the frequency distribution of first-round choices since some effort levels were not chosen in the experiment and it is infeasible to calibrate the attraction level for the corresponding strategy such that the corresponding choice probability is zero.

[23] Roth and Erev (1995) employs the power form, i.e. $\Pr_{t+1}^x = A_t^x / \sum_a A_t^a$.

4.3.2 Estimation results

Contract types	λ	δ	ϕ	κ	BIC
1) EWA					
Standard debt contract	0.440***	0.132***	0.849***	0.432***	-709.25
	(0.097)	(0.023)	(0.038)	(0.139)	
Equity	0.538**	0.147***	0.928***	0.163**	-584.21
	(0.120)	(0.025)	(0.040)	(0.075)	
Non-monotonic contract	0.198***	0.235***	0.940***	0.277***	-744.51
	(0.036)	(0.057)	(0.032)	(0.072)	
No repayment	0.066***	0.411***	0.884***	0.503***	-686.88
	(0.012)	(0.087)	(0.026)	(0.106)	
2) Cum. Reinforcement					
Standard debt contract	0.330***	0	0.683***	1	-758.90
	(0.030)		(0.036)		
Equity	0.222***	0	0.707***	1	-626.98
	(0.017)		(0.034)		
Non-monotonic contract	0.091***	0	0.773***	1	-767.26
	(0.011)		(0.027)		
No repayment	0.029***	0	0.816***	1	-695.69
	(0.003)		(0.019)		

Table 6: Parameter estimates of the EWA and the RI learning models

Standard errors are reported in parentheses. There is no standard error reported if the parameter is not estimated but exogenously restricted to a value to obtain the RI model. BIC $= \text{LL} - 0.5k\log(NT)$ where k is the number of estimated parameters, N is the number of subjects, and T is the number of periods. Levels of significance: *** significant at 1%, ** significant at 5%, * significant at 10.%.

We use maximum-likelihood estimation to quantify the parameters of the EWA and RI learning models. Table 6 reports the estimation results. The significant estimates of λ in any contract condition and any learning model show that subjects do not randomly choose effort levels over the course of the experiment. Rather payoff differences substantially govern effort choice behavior. The fit of the learning models as summarized by the BIC shows that the EWA model explains the the data better than the RI model in any contract condition.[24]

An important reason why the EWA model fits the data better than the RI model lies in the fact that EWA also allows for the updating of attractions if the corresponding levels of effort were not chosen. The significantly positive estimates of the introspection discount factor δ show, consistently across contract conditions, that our participants not only responded to the actually experienced payoffs through choosing some effort level but also take into account non-experienced payoffs through introspection.

If participants took into account, through introspection, all non-chosen effort levels

[24] Note that the BIC corrects for increasing the number of parameters so that it is not simply the larger number of parameters under EWA explaining the improved fit.

in the same way as they are using chosen effort, then the introspection discount factor δ would be equal to one. There would be no discounting of payoffs and all attraction levels would be updated in the same way independent of the actual effort choice. In contrast, the estimates of δ show that introspection is limited as the estimates are much smaller in magnitude than one for any contract condition (Table 6). Therefore, experiencing the implications of the actual effort choice is essential in all contract conditions including NMC and NoRepay where average behavior starts out in the vicinity of the optimal value (Figure 1). We summarize our findings on learning incentives as follows.

Result 4. *Experiencing the implications of effort choice is essential for incentive effects of funding contracts to take effect. Incentive effects are learned through experience in all contract conditions and affect behavior increasingly with the accumulation of prior exposure.*

4.4 Non-monotonic-contracts hypothesis

In this section we take a closer look at the non-monotonic-contracts hypothesis. Would the replacement of a standard debt contract by a repayment-equivalent non-monotonic contract reduce efficiency losses and increase entrepreneurial income as predicted? Figure 3 shows the average incomes obtained under both contracts for each round. It is easy to see that entrepreneurial income in the NMC condition is much greater than in the SDC condition. Using data of the last part of the experiment, rounds 11-15, we find that NMC income exceeds SDC income by 170%.[25]

Result 5. *Observed entrepreneurial income (net of effort cost and repayment) under the non-monotonic contract is on average 170% greater than under the standard debt contract (support for hypothesis 3).*

Using the two-tailed t-test for roundwise comparisons of entrepreneurial income (net of agency costs and repayment) formally confirms that the income difference is significant in all fifteen rounds ($p < 0.033$) and highly significant in 13 out of 15 rounds ($p < 0.006$).[26]

Figure 4 illustrates how much additional total surplus would have been created in the SDC treatment if, instead of the standard debt contract, a non-monotonic contract had been used. The figure reveals that in the first four rounds of the experiment, there is essentially no welfare disadvantage of the standard debt contract.[27] Clearly, these initial effort levels are suboptimal and yield negative round incomes, as is obvious from figure 3. In the course of the experiment, subjects in the SDC treatment reduce their effort choices

[25] If earlier rounds are included, non-monotonic contracts perform even better, e.g. NMC income tops SDC income by 360% on average if data for rounds 3-15 is used.

[26] Similarly the two-tailed Mann-Whitney U test indicates highly significant income difference in 12 of 15 rounds ($p < 0.007$); for the remaining three rounds we find $p < 0.065$.

[27] In the first two rounds, SDC welfare is even slightly higher than NMC welfare. This is due to the fact that quite large effort levels are initially chosen in the SDC treatment, similar to those effort levels observed in the NMC treatment, but somewhat closer to first-best effort which is indicated by the horizontal line in the Figure.

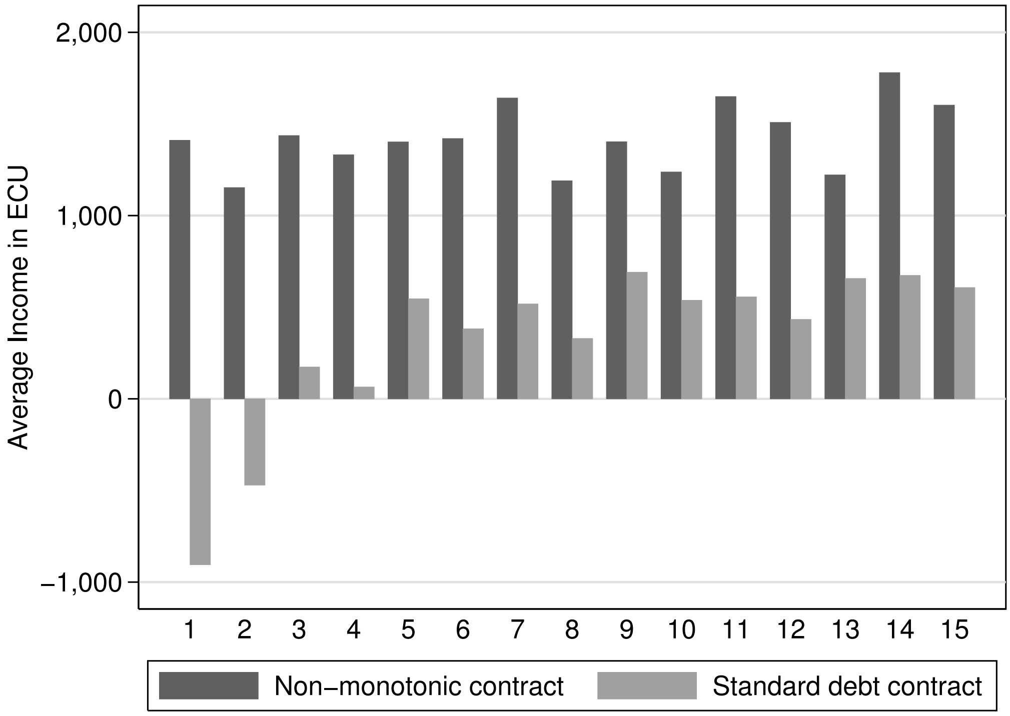


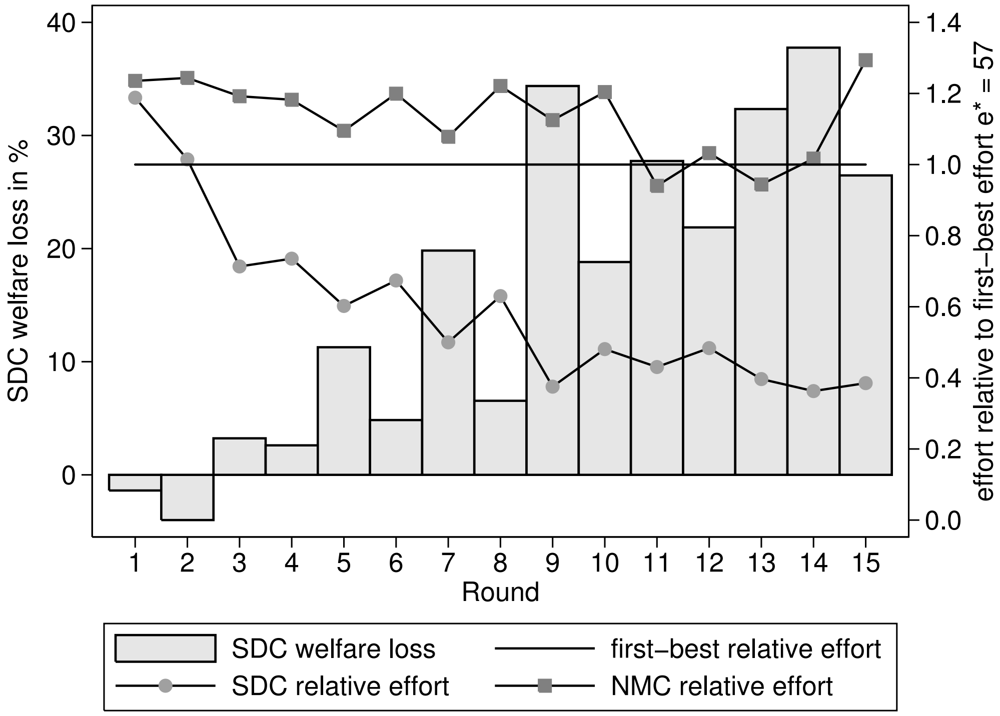
Figure 3: Average round income of entrepreneurs under NMC and SDC

towards the optimal level. As effort levels in the SDC treatment decrease, the inefficiency of the standard debt contracts grows sharply. Restricting attention to data from the last part of the experiment (rounds 11-15) where effort choices under SDC have stabilized (see figure 4), we find that the use of the non-monotonic contract would have increased total surplus in the SDC treatment by approximately 30%. Total surplus in the NMC treatment is significantly greater than in the SDC treatment in all of these five rounds (t-test, $p < 0.058$, two-tailed).[28]

Result 6. *Standard debt contracts lead to allocative inefficiencies that can be eliminated by using repayment-equivalent non-monotonic contracts (support for hypothesis 3).*

Our experiment provides strong evidence that non-monotonic contracts dominate standard-debt contracts. This result, however, stems from an environment where there is accurate and costless state verification. This rules out any disagreement between investor and entrepreneur about the project's realized return state. In the absence of costless state verification, it is, however, not clear if the reported benefits of the non-monotonic contract sustain. Whenever there are opportunities in real-life for either investor or entrepreneur to misrepresent states these might be exploited so that practical disadvantages arise. In the next subsection we report the results from treatment NMC-R that was designed to see if the availability of entrepreneur-sided state manipulation can erode the participation constraints of parties as theoretically predicted.

[28]Mann-Whitney U test, $p < 0.057$, two-tailed.



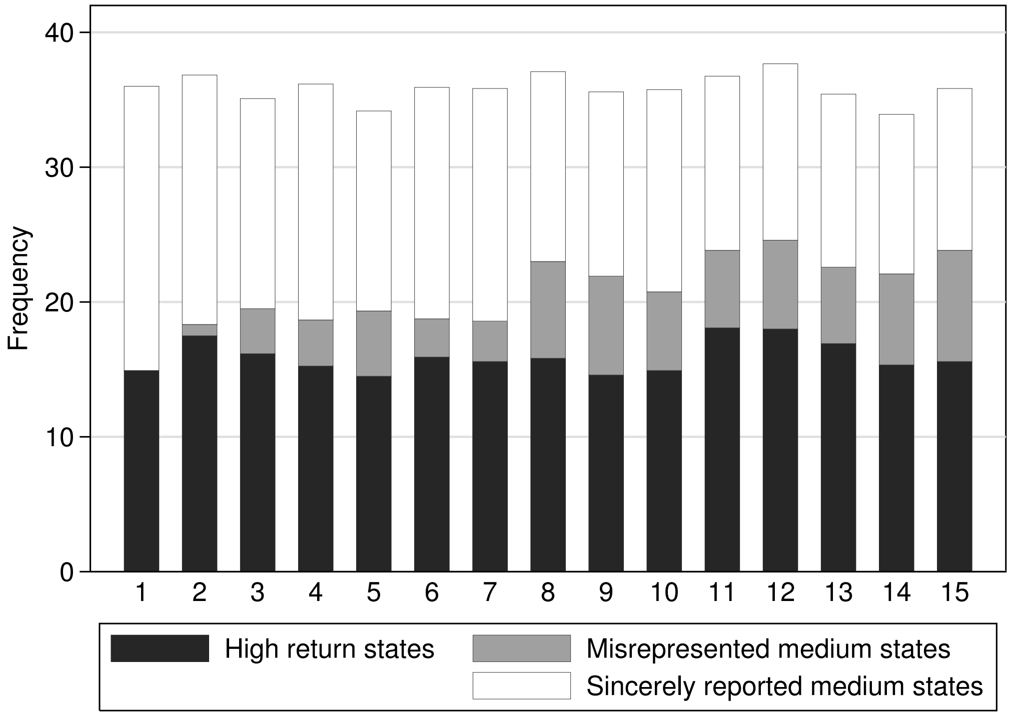
The figure depicts the additional total surplus (in %) that is created on average if the SDC is replaced by the corresponding NMC, given average effort levels observed under both contract conditions. This measure of welfare loss is shown as a series of bars (left scale). Furthermore, the figure illustrates average effort observed in both contract conditions relative to first-best effort (right scale). Eg., in round 5, NMC average effort exceeds first-best effort by roughly 10% while SDC average effort falls short off it by 40 %. Finally, the first-best effort benchmark (where total surplus is maximized) is represented by a horizontal line at unity (right scale).

Figure 4: Welfare loss with a standard debt contract, but eliminable by a non-monotonic contract

4.5 No-misrepresentation hypothesis

The non-monotonic contract relies on the accurate reporting of realized states. If there are opportunities that allow for the misrepresentation of realized states, then the mutual acceptance of a non-monotonic contract structure by lender and borrower erodes due to incentives for state misreporting and implied payoff redistribution between contracting parties. In treatment NMC-R we test a reduced form of outside borrowing that captures the opportunity for the entrepreneur-sided manipulation of states and associated repayments. Specifically, entrepreneurs can misrepresent a medium return state (with high repayment under NMC-R) as a high return state (with low repayment under NMC-R) and vice versa. Misrepresenting a medium return state by a high return state allows the entrepreneur to repay 500 ECU instead of 9000 ECU in our experiment.

Figure 5 shows the evolution of realized high return states by round on average. As can be seen, the average number of high return states does not vary much over the course of the experiment and stays between fourteen and eighteen. This results from average



The figure depicts the number of realized high return states averaged over subjects by round. It also indicates the number of misrepresented medium return states as grey-colored bars that are stacked on the bars representing high returns. Similarly, the outlined bars show the number of non-misrepresented medium return states. The difference between the total number of projects in a round, 50, and the sum of high return states, and misrepresented and non-misrepresented medium return states gives the number of low return states as the (non-indicated) residual.

Figure 5: Average number of realized high return, misrepresented medium, and non-misrepresented medium return states by round.

effort behavior observed in treatment NMC-R that is close to the first-best level in each round[29], similar to effort behavior observed in treatment NMC. Figure 5 also shows the misrepresented medium return states and the non-misrepresented medium return states. In the first round no subject misrepresented any state at all. Over time, however, the number of faked high return states increases although it stays well below the maximum number that is possible. The slow behavior of misrepresenting states may be a result of our cautious way of introducing the misrepresentation opportunity in the instructions to avoid experimenter-demand effects. Also subjects may have needed some time to discover the advantages of misrepresentation coming with no disadvantages such as punishments for misreported states etc. Although the number of misrepresented medium return states is lower than predicted, its implications for payoff redistribution between contracting parties are large as shown in Figure 6. The figure depicts the evolution of the average payoffs of entrepreneurs and investors along with total surplus. Apparently the increasing number of faked high return states is accompanied by large increases of payoffs to entrepreneurs

[29] Average effort in treatment NMC-R does not significantly differ from the theoretical prediction in any of the fifteen rounds; two-tailed t-tests, $p > 0.326$, two-tailed sign-test, $p > 0.387$.

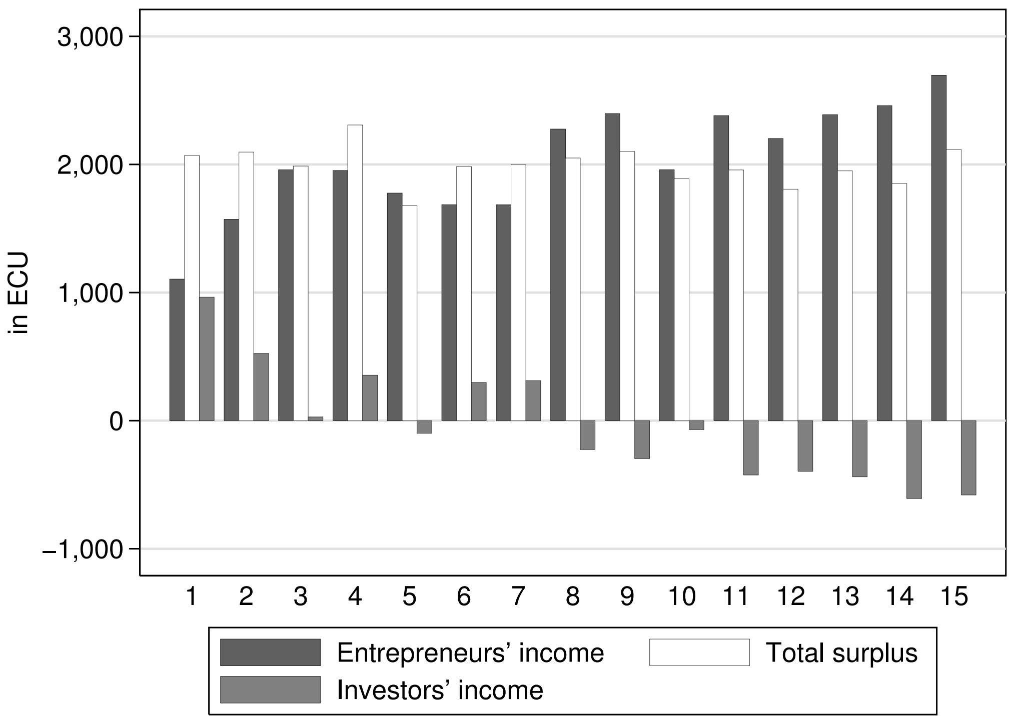


Figure 6: Average payoffs and total surplus by round in treatment NMC-R.

at the expense of investors. Towards the end of the experiment entrepreneurs manage to reap amounts from investors that exceed total surplus so that (computerized) investor payoffs become negative.[30] Most likely negative payoffs from the contract situation would lead investors to reject the contract so that our results confirm practical disadvantages with the non-monotonic contract structure if the environment offers opportunities for state misrepresentation. We summarize our findings as follows:

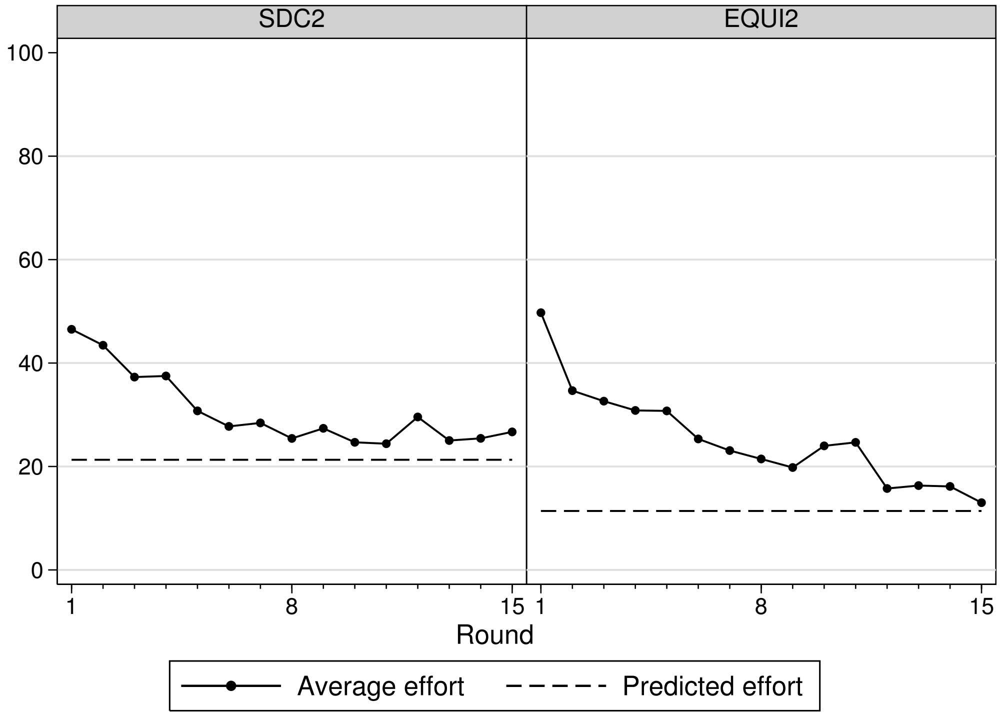
Result 7. *Opportunities for misrepresenting realized return states are partially exploited and erode the participation constraint of the investor by payoff distribution from investors to entrepreneurs (support for hypothesis 5).*

4.6 Monotonic-contracts hypothesis

Innes (1990) has shown that the standard debt contract maximizes total surplus in the class of monotonic contracts. In particular, a standard debt contract fares better than a repayment-equivalent equity contract theoretically. In the following we compare behavior observed in treatments SDC2 and EQUI2. Both contracts yield the same expected repayment to the investor but differ in the effort prediction by roughly ten units. While theory predicts 21.3 effort units with contract SDC2, it predicts 11.4 units with contract

[30] This is facilitated by average repayments falling short of the borrowed amount so that investors partially subsidize project execution.

EQUI2. The implied difference in total surplus is approximately 403 ECU (see Table 2 for the theoretical predictions under either contract.)

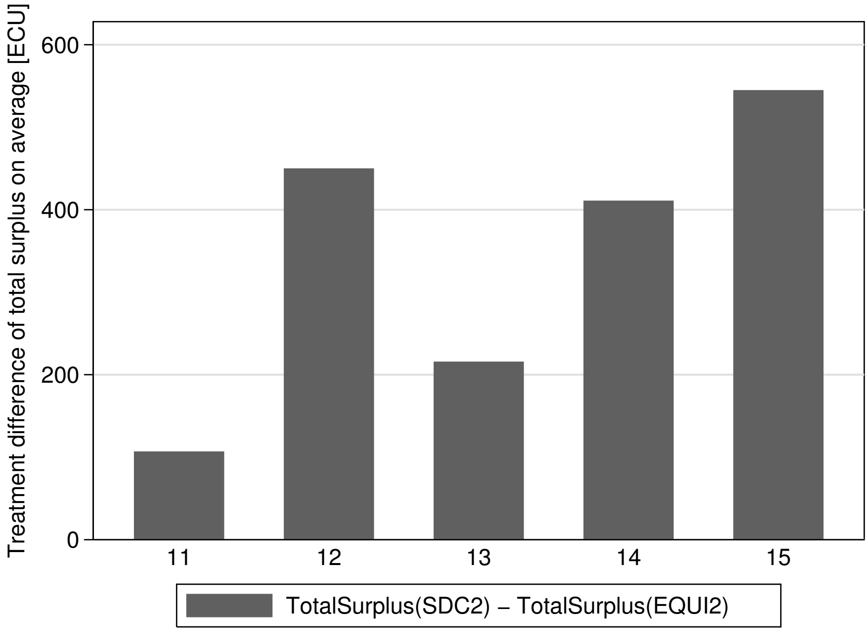


The figure depicts average effort (solid lines) over rounds under the standard debt contract SDC2 (left) and under outside equity EQUI2 (right). The theoretical effort prediction is indicated by dashed lines.

Figure 7: Average effort and predicted effort by round

Figure 7 depicts the time paths of average effort in either treatment. As can be seen from the figure, the behavior of average effort under SDC2 and EQUI2 is similar to that of our basic treatments SDC and EQUI: In both extension treatments average effort is at a similar level in the first round (two-tailed MWU,$p = 0.728$; two-tailed t-test, $p = 0.764$) and exceeds the effort prediction (two-tailed t-tests, for either treatment $p < 0.012$; two-tailed sign tests, $p^{\text{EQUI2}} = 0.001$, $p^{\text{SDC2}} = 0.146$). Over the course of the experiment and with increasing experience with contract incentives, average effort under either contract converges from above to the theoretical level. It has converged to the theoretical predictions under either treatment in the last four rounds: for either treatment two-tailed sign-tests reveal insignificant differences ($p \geq 0.146$); similarly two-tailed t-tests reveal insignificant difference in all final four rounds of treatment SDC2 ($p \geq 0.137$) and in three out of four rounds of treatment EQUI2 ($p \geq 0.121$; for round 13 $p = 0.088$).

For testing the monotonic-contracts hypothesis we look at the difference between average effort across both treatments. The graphs of average effort suggest that they are rather similar for rounds 1-11 and different for rounds 12-15. Roundwise comparisons show that the treatment differences of average effort are insignificant for all rounds 1-11 (two-tailed t-test, $p \geq 0.241$; two-tailed MWU-test, $p \geq 0.124$). For the final four rounds, both statistical tests reveal significant differences in two out of four rounds: for rounds 12 and 15 we find $p^{t\text{-test}} \leq 0.027$ and $p^{\text{MWU}} \leq 0.042$, and for rounds 13 and 14 we find $p^{t\text{-test}} \geq 0.159$ and $p^{\text{MWU}} \geq 0.223$. We interpret these mixed results at the end of our experiment carefully as favouring the monotonic-contracts hypothesis since we conjecture that the treatment difference in average effort would be more often significant than not at the end of a longer experiment with more rounds.



The figure depicts the difference of total surplus in treatments SDC2 and EQUI2 on average by round for rounds 11-15

Figure 8: Total surplus premium of SDC2 over EQUI2 by round

Figure 8 shows the total surplus premium of the SDC2 contract over the EQUI2 contract for these rounds where average effort has converged to the predicted effort value. In line with the monotonic-contracts hypothesis allocative efficiency is higher under the SDC2 contract than under the EQUI2 contract.

Result 8. *With sufficient experience, effort under the standard debt contract SDC2 tends to be larger than that under the repayment-neutral equity contract EQUI2 implying that allocative efficiency is higher under SDC2 than under EQUI2 (partial support for hypothesis 4).*

5 Concluding remarks

We examined the incentive effects inherent to funding contracts experimentally. Surprisingly, at the beginning of the experiment we found no incentive effects at all: effort is the same independent of the contract condition. This shows that there are limits to grasping incentive effects through mere introspection. As experience with the contract condition accumulates, incentive effects increasingly govern behavior. With sufficient experience behavior is largely consistent with the theoretical predictions so that the differential incentive effects of funding contracts apply in the long run. As a consequence we also find support for the non-monotonic-contracts and the monotonic-contracts hypotheses.

The finding that experience crucially determines how the incentives of funding contracts affect behavior is of particular importance in our setting as real life entrepreneurs, who are endowed with all sorts of "projects", differ in their experience. For example, any entrepreneur requiring external finance to start a project is inexperienced with the implications of funding contracts at the beginning of the entrepreneurial career. Our results suggest that no efficiency loss arises with standard debt or equity in these cases due to limited introspection. The inexperienced entrepreneurs, however, suffer from their inexperience as they receive lower incomes than predicted due to filing for bankruptcy less often and repaying to the investor more often than is expected. Depending on the individual entrepreneur and the particular project(s), there are entrepreneurs who accumulate experience with the incentive effects of funding contracts over the course of their careers. In contrast to the inexperienced ones, we provide evidence that inefficiencies arise with the experienced entrepreneurs under the standard debt contract and the equity contract. Replacing these contracts by non-monotonic contracts would mitigate the losses in allocative efficiency. One possibility of setting up non-monotonic contracts is to combine a standard debt contract with bonus payments of the investor to the entrepreneur conditional on reaching relatively high return states.

Interestingly our data allows us to see if the learning of incentives applies not only globally but also locally. The estimation of the EWA model revealed that exposure to experience matters if behavior starts out far away from the optimal effort choice as in the SDC and Equity conditions. This type of global learning should be expected as the rewards from learning, that is the payoffs when moving into the direction of optimal choice, increase in the distance of actual choice to optimal choice. If actual choice begins in the neighborhood of optimal choice, it might be less obvious if exposure would matter. The EWA estimates show, however, that increasing exposure to incentives also matters if choice behavior starts out in the neighborhood of the optimal effort level as in the NMC and NoRepay conditions.

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Appendix

A Instructions for the basic treatments (Translation from German)

[Part I:] **General explanations for participants**
You are now participating in an economic experiment that is financed by METEOR. If you carefully read the following explanations, you can earn a substantial amount of money, contingent on your decisions. Therefore, it is very important that you read these explanations carefully.

The instructions handed out to you are for your private information only.During the experiment there is a strict prohibition of any kind of communication. If you do not abide by this rule, you will be excluded from the experiment as well as any payments. If you have any question, please, raise your arm. We will then answer your question COMING TO YOUR CUBICLE.

During the experiment we will not count in Euros but in ECU (Experimental Currency Units). Therefore, your total earnings will first be calculated in ECU. The total amount of ECU you attain during the experiment will be converted to Euros at the end of the experiment and paid in cash.

[Part II:] **Information regarding the experiment**
The experiment today is divided into separate rounds. In total, there will be 15 rounds. The following elaborations explain the course of action of the experiment for each round.

Each round, you undertake a new project and decide on how much effort you want to invest into the project. By choosing your effort level, you determine the probabilities of the project to attain a low, intermediate, or high revenue. At the same time, a higher effort choice leads to higher costs. Undertaking the project requires start-up costs of ECU 3 120. [*The next two sentences were only included in the instructions for the SDC condition, the NMC condition, and the EQUITY condition:* As you do not dispose of the start-up capital needed for the start-up investment, you have to raise the capital on the capital market. You are acting under limited liability: in case your project revenue does not cover the fixed repayment to the capital provider, only your project revenue will be used for repayment, the remainder will be waived.] [*The next two sentences were only included in the instructions for the NoRepay condition:* Undertaking the project requires start-up costs of ECU 3 120. You dispose of the necessary start-up capital and you do not have to raise the capital on the capital market.]

Course of action At the beginning of each round, you are asked to set the effort level X for this round's project. The effort level cannot be negative or exceed 100, and may only exhibit one decimal place.

The costs arising from your effort are $\frac{1}{2}X^2$. The project revenue is random and may take on a low, intermediate, or high level. By choosing your effort level X, you can influence the probabilities for a low or a high project revenue. The higher your effort level, the lower the probability of a low revenue and the higher the probability of a high revenue. In particular:

- probability of the low revenue of 500 ECU: $60\% - \frac{X}{100} \cdot 60\%$
- probability of the intermediate revenue of 9,000 ECU: 40%
- probability of the high revenue of 10,000 ECU: $\frac{X}{100} \cdot 60\%$

To ease calculating probabilities and effort costs, on the input screen (see figure 1 [*Please see the first screenshot in the next section*]) you may enter any number of values into the effort entry field and have the according probabilities and effort costs displayed by clicking on the button "calculate probabilities". By clicking on the button "confirm choice", you make your decision in this round irrevocable.

[*The following paragraph was only included in the instructions for the SDC condition:*
Fixed repayment to the capital provider Out of the project revenue, the capital provider receives ECU 7 383.30 as fixed repayment for financing the project. In case the project revenue does not cover the fixed repayment to the capital provider, you will only repay the project revenue, the remainder will be waived.]

[*The following paragraph was only included in the instructions for the NMC condition:*
Fixed repayment to the capital provider Out of the project revenue, the capital provider receives ECU 500 in case of a low revenue, ECU 9 000 in case of an intermediate revenue, and ECU 500 in case of a high revenue as a fixed repayment for financing the project.]

[*The following paragraph was only included in the instructions for the EQUITY condition:*
Fixed repayment to the capital provider Out of the project revenue, the capital provider receives ECU 362.50 in case of a low revenue, ECU 6 525 in case of an intermediate revenue, and ECU 7 250 in case of a high revenue as a fixed repayment for financing the project.]

[*The following paragraph was only included in the instructions for the SDC condition:*
Your project income Your project income equals the project revenue minus repayment costs and minus effort costs, i.e.:

- in case of a low revenue: $500 - 500 - \frac{1}{2}X^2 = -\frac{1}{2}X^2$ ECU.
- in case of an intermediate revenue: $9,000 - 7,383.30 - \frac{1}{2}X^2 = 1,616.70 - \frac{1}{2}X^2$ ECU.
- in case of a high revenue: $10,000 - 7,383.30 - \frac{1}{2}X^2 = 2,616.70 - \frac{1}{2}X^2$ ECU.]

[*The following paragraph was only included in the instructions for the NMC condition:*
Your project income Your project income equals the project revenue minus repayment costs and minus effort costs, i.e.:

- in case of a low revenue: $500 - 500 - \frac{1}{2}X^2 = -\frac{1}{2}X^2$ ECU.
- in case of an intermediate revenue: $9,000 - 9,000 - \frac{1}{2}X^2 = -\frac{1}{2}X^2$ ECU.
- in case of a high revenue: $10,000 - 500 - \frac{1}{2}X^2 = 9,500 - \frac{1}{2}X^2$ ECU.]

[*The following paragraph was only included in the instructions for the EQUITY condition:*
Your project income Your project income equals the project revenue minus repayment costs and minus effort costs, i.e.:

- in case of a low revenue: $500 - 362.50 - \frac{1}{2}X^2 = 137.50 - \frac{1}{2}X^2$ ECU.
- in case of an intermediate revenue: $9.000 - 6,525.00 - \frac{1}{2}X^2 = 2,475.00 - \frac{1}{2}X^2$ ECU.
- in case of a high revenue: $10,000 - 7,250.00 - \frac{1}{2}X^2 = 2,750.00 - \frac{1}{2}X^2$ ECU.]

[*The following paragraph was only included in the instructions for the NoRepay condition:*
Your project income Your project income equals the project revenue minus effort costs, i.e.:

- in case of a low revenue: $500 - \frac{1}{2}X^2$ ECU.
- in case of an intermediate revenue: $9,000 - \frac{1}{2}X^2$ ECU.
- in case of a high revenue: $10,000 - \frac{1}{2}X^2$ ECU.]

Number of projects and round income In each round, you will undertake 50 identical projects. That is to say, by choosing your effort level you do not determine the revenue probabilities and effort costs of a single project only, but those of 50 independent projects. To this effect, each project's revenue will be determined by a random draw under the probabilities determined by your effort choice. All random draws are independent of each other. You will be shown the number of projects with low, intermediate, and high revenue as well as the project incomes on an informational screen (see figure 2 [*Please see the second screenshot in the following section*]). Your round income will be determined as follows:

Your round income = average revenue of the 50 projects.

Payment At the end of the experiment you will be paid [*in SDC, NMC, and EQUITY:* EUR 0.0004] [*in NoRepay:* EUR 0.00005] for each ECU on your ECU account. At the beginning of the experiment, an initial endowment of [*in SDC, NMC, and EQUITY:* ECU 12 500] [*in NoRepay:* ECU 100 000] will be credited to your account to cover potential losses. Each round, your round income will be added to your account,

so that your account balance either increases (in case of a positive round income) or decreases (in case of a negative round income). You can avoid losses with certainty by making decisions accordingly. In case your account balance falls below [*in SDC, NMC, and EQUITY:* ECU 2 500] [*in NoRepay:* ECU 20 000], you may not continue the experiment.

If you have any questions, please, let us know by raising your hand.

B Screenshots

Figure 9 shows an input screen with hypothetical data for the SDC treatment. In the screenshot used in the set of instructions given to subjects, there was no data available.

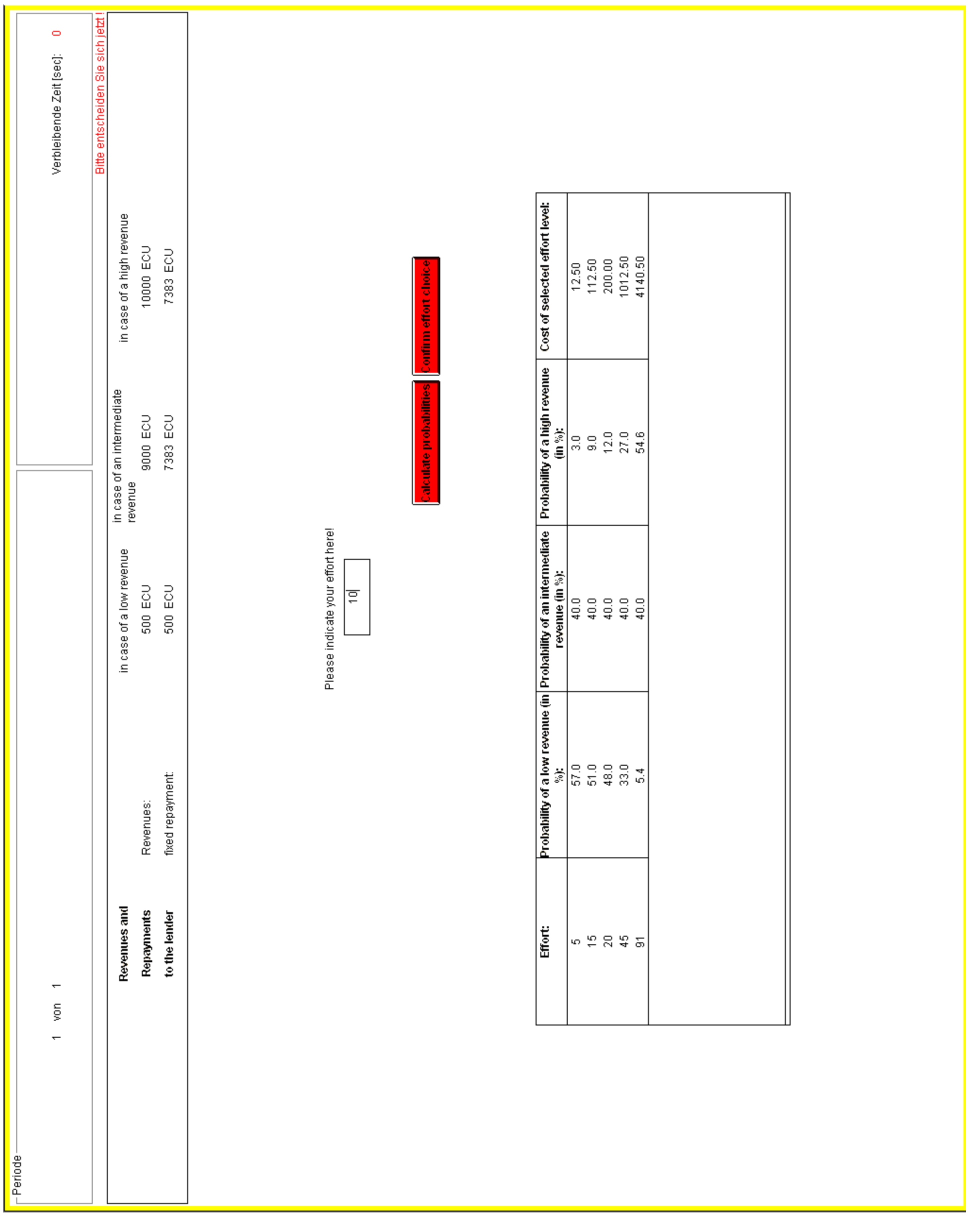

Figure 9: Input screen in the SDC treatment with hypothetical data (translation)

Figure 10 shows the feedback screen with information about project outcomes and round income. A similar screenshot, also with data substituted by letters "XXX", was used in the set of instructions give to subjects.

Figure 10: Feedback screen from instructions in SDC treatment with erased data (transl.)

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