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Self-regulating organizations under the shadow of governmental oversight: An experimental investigation

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Self-regulatory organizations (SROs) can be found in education, healthcare, and other not-for-profit sectors as well as the accounting, financial, and legal professions. DeMarzo et al. (2005) show theoretically that SROs can create monopoly market power for their affiliated agents, but that governmental oversight, even if less efficient than oversight by the SRO, can largely offset the market power. We provide an experimental test of this conjecture. For carefully rationalized parameterizations and implementation details, we find that the predictions of DeMarzo et al. (2005) are borne out.

Key words: Experimental Economics, Self-regulating organizations, Governmental oversight

JEL Codes: C90, L44, G18, G28

1. INTRODUCTION

Self-regulatory organizations (SROs) can be found in education, healthcare, and (other) not-for-profit sectors, as well as the accounting, financial, and legal professions; they can even be found in the nuclear power industry (DeMarzo et al., 2005; Studdert et al., 2004; Sidel, 2005; Hilary & Lennox, 2005; Maute, 2008; Ortmann & Myslivecek, 2010; Ortmann & Svitkova 2010). Examples are the Financial Industry Regulatory Authority in the securities industry, the so-called Donors Forums in not-for-profit sectors in Central and Eastern Europe (Ortmann & Svitkova, 2010), and the Institute of Nuclear Power Operations in the nuclear power industry (Rees, 1997; DeMarzo et al., 2005). The healthcare and education sectors are often regulated by systems of accreditation that are similar to self-regulatory entities.

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DeMarzo et al. (2005) show that SROs can create monopoly market power for their affiliated agents by investigating with a very low probability complaints from clients. These authors, however, also argue that the shadow of governmental oversight, even if less efficient than oversight by the SRO, can largely offset market power. In other words, the mere outside threat of government oversight might overcome the incentive-incompatibility problem that some have argued afflict self-regulatory bodies (e.g., Shaked & Sutton, 1981; Nunez 2001, 2007; Ortmann & Myslivеček, 2010).

We experimentally test the predictions of DeMarzo et al. (2005). We also test the model under the more realistic assumption that the government cannot immediately infer the investigation probability of the SRO. Under this arguably more realistic assumption, the players (named GOV and SRO) do not move sequentially, but rather simultaneously. Governmental oversight then crowds out private oversight and SRO stops investigating altogether. All investigations are done by GOV, even though GOV is more inefficient at investigating than SRO. Efficiency and welfare are thus lower when GOV and SRO move simultaneously than when they move sequentially.

Using carefully rationalized parameterizations and implementation details, we find that the predictions of the model of DeMarzo et al. (2005) are borne out, somewhat more so for the simultaneous variant than for the sequential variant.

In the following section we briefly sketch the model in DeMarzo et al. (2005). In section 3 we explain how we have translated the DeMarzo et al. (2005) theoretical model into an experimentally testable test-bed. In section 4 we rationalize our choice of particular parameterizations and other design choices. In section 5 we report our results. In section 6 we conclude with a discussion of our results.

2. THE MODEL

In the DeMarzo et al. (2005) model, an agent affiliated with an SRO provides a service for a client, such as making an investment. The outcome of the investment can be high or low, and is not directly observable for the client, but can be verified by the SRO at some cost. An opportunistic agent may have an incentive to deceive a client by reporting a low outcome when it is in fact high, and then keep the payoff difference himself. The agent thus needs to be incentivized to be honest. The agent can be incentivized with a “whip” or with a “carrot”. When an agent is found to have deceived a client, he could be given a financial punishment (the whip), and when the agent reports a high outcome, he could be given a bonus in the form of a success fee (the carrot). For a client, the cheapest method to incentivize the agent is a “whip”: to apply a punishment if the agent has been deceived. This strategy is, however, only applicable if the SRO detects the misreporting which requires monitoring and incurs a cost.

DeMarzo et al. (2005) show that the SRO—left to its own devices—will set the investigation probability ip^*_{SRO} inefficiently low, leaving punishment ineffective as an incentive for the agent to act honestly. As a result the client is forced to use "carrots": offering the agent a large proportion of the outcome as a success fee. When clients are homogeneous in their outside options, the SRO will set ip^*_{SRO} so low that the fee will extract all the surplus of the investment. Clients thus will have an expected profit equal to their outside option. When clients are heterogeneous in their outside options, a low investigation probability may dissuade clients with high outside options from entering. The SRO sets an investigation probability that maximizes the product of the expected fee and number of clients that will participate. From a welfare point of view, the resulting outcome is undesirable.

DeMarzo et al. (2005) show that governmental oversight may largely offset the market power the SRO has given to the agents. The government itself can start an investigation and assign penalties when fraud is detected. DeMarzo et al. (2005) assume that government and the SRO move sequentially, with the government moving last. GOV thus observes the investigation probability of SRO and subsequently chooses investigation probability $ip_{GOV}[ip_{SRO}]$, where $ip_{GOV}[0] > ip^*_{SRO}$ (GOV prefers a higher investigation probability than SRO) and $ip_{GOV}\left[ip_{GOV}[0]\right] = 0$.[1] The GOV investigation probability is thus positive for every level of SRO investigation probability that is lower than $ip_{GOV}[0] > ip^*_{SRO}$. The additional investigation by GOV lowers the agents' market power and also the success fee necessary to induce the agent to behave honestly. GOV has, however, higher investigation costs than SRO: $c_{GOV} > c_{SRO}$. As clients bear the investigation costs, the high costs of GOV dissuade clients with high outside options from entering, thus lowering the profits for both clients and agents. It is therefore in the interest of SRO to minimize the amount of investigation conducted by GOV. SRO can effectively pre-empt GOV by choosing $ip^{PRE}_{SRO} = ip_{GOV}[0]$, the investigation probability GOV would have chosen if GOV were the sole investigator. GOV then chooses zero as $ip_{GOV}\left[ip_{GOV}[0]\right] = 0$. The pre-emptive investigation probability assures that the investigation is solely conducted by SRO and the resulting reduction in investigation costs dissuades fewer clients with high outside options from participating. This increased participation of clients increases the profits of both GOV and SRO.

Under the arguably more realistic assumption that GOV cannot immediately infer the investigation probability of SRO,[2] the players (GOV and SRO) move simultaneously. It can be derived from the model of

[1] If GOV was the only one with investigative powers, the optimal level of investigation probability for GOV would be $ip_{GOV}[0] > ip^*_{SRO}$. Thus, if SRO chooses the level of investigation probability that GOV would choose had GOV been the only one with investigative powers, $ip_{GOV}[0]$, then GOV chooses an investigation probability of zero. Thus, $ip_{GOV}\left[ip_{GOV}[0]\right] = 0$.

[2] GOV cannot infer the investigation probability when SRO, for example, does not make precise quantitative announcements in the form of an investigation probability, but instead makes a qualitative announcement such as "regular checks will be made in accordance with best practices". Not only should SRO make a precise quantitative announcement in the form of an investigation

DeMarzo et al. (2005) that, for SRO, any choice where $ip_{SRO} > 0$ and $ip_{SRO} + ip_{GOV} > ip^*_{SRO}$ (where ip^*_{SRO} is the value that is optimal for SRO were it the only regulating body) is dominated by the choice of a lower value of ip_{SRO}. Likewise, for GOV, any choice where $ip_{SRO} + ip_{GOV} < ip^*_{GOV}$ (where ip^*_{GOV} is the value that is optimal for GOV were it the only regulating body) is dominated by the choice of a higher value of ip_{GOV}. As $ip^*_{GOV} > ip^*_{SRO}$, this leaves only one non-dominated choice: a Nash equilibrium where SRO sets $ip_{SRO} = 0$ and GOV sets $ip_{GOV} = ip^*_{GOV}$. Thus, with simultaneous moves, GOV does all investigations and SRO does none, even though the cost of investigating is higher when done by GOV than when done by SRO. As a result, welfare is now lower than in the case of sequential moves.

3. IMPLEMENTATION OF THE MODEL

We implement the model of DeMarzo et al. (2005) for an experimental test. We assume that once SRO and GOV have announced their investigation probability, clients and agents make the Nash-equilibrium choices as derived in DeMarzo et al. (2005). Our test is thus focused on the behavior of SRO and GOV, which we consider the key protagonists of the story. As in DeMarzo et al. (2005), GOV would never duplicate an investigation already done by SRO. The total investigation probability is then equal to the GOV investigation probability plus the SRO investigation probability: $ip_{Total} = ip_{SRO} + ip_{GOV}$ (see footnote 17 of DeMarzo et al., 2005 for details). When players move sequentially, it would be irrational for GOV to choose ip_{GOV} such that $ip_{SRO} + ip_{GOV} > 1$, and we thus exclude such a move. When players move simultaneously, they could have target investigation probabilities that are together larger than 1. Following DeMarzo et al. (2005), we assume that neither GOV nor SRO would ever duplicate an investigation already done by the other party. An agent is then investigated, with a probability of 1, by either GOV or SRO. To determine the probability that the investigation is realized by GOV (SRO), we assume that the investigation probabilities of GOV (SRO) are then proportional to their efforts and thus given by $ip_{GOV} = \dfrac{ip^t_{GOV}}{ip^t_{GOV} + p^t_{SRO}}$ $\left(ip_{SRO} = \dfrac{ip^t_{SRO}}{ip^t_{GOV} + p^t_{SRO}}\right)$, where p^t_{GOV}, p^t_{SRO} are the target investigation probabilities for GOV and SRO.[3]

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probability, SRO should also be legally bound by the announcement. Moreover, the realized percentage of investigations by SRO should be checked and deviations from the announced percentage should be severely sanctioned. If these requirements are not fulfilled, GOV cannot trust the announcement of the SRO. In effect, one can thus think of GOV and SRO as choosing an investigation probability simultaneously.

[3] An alternative assumption is that SRO always realizes its investigation probability target, and that the remaining monitoring is done by GOV. For example, if SRO were to set an investigation probability target of 80% and GOV of 40%, then the realized investigation probability would be 1, with SRO having done 80% of the investigative work and GOV the remaining 20%. This changes only the numbers to the south-east of the diagonal and does not change the resulting game qualitatively.

Investigations are costly and thus, once the investigation probabilities are known, the expected costs of investigation can be calculated. These costs are paid up front by clients as a transaction fee $t_{SRO} + t_{GOV}$. The fine *x* levied on an agent found to have deceived a client is set maximally. Table 1 gives, for the case of heterogeneous agents, the timeline for the sequential game (identical to the one in DeMarzo et al., 2005) and for the simultaneous game.

TABLE 1

Timeline for the sequential game for heterogeneous agents

	Sequential game	Simultaneous game
Stage 1	A) SRO chooses an investigation probability ip_{SRO}.	SRO and GOV simultaneously each choose their investigation probability: SRO ip_{SRO} and GOV ip_{GOV}.
	B) After observing ip_{SRO}, GOV chooses an investigation policy ip_{GOV}.	
Stage 2	Given the investigation probabilities $ip_{SRO} + ip_{GOV}$ and the expected investigation costs $t_{SRO} + t_{GOV}$, clients calculate the lowest success fee that induces the agent to act honestly. The clients' expected profit with such a contract is equal to *a*. The proportion of clients with outside options larger than *a*, *1-F[a]*, will not deal with the clients and take their outside options. The proportion of clients with outside options smaller than *a*, *F[a]*, will offer the contract to the agents. They pay the transaction fees $t_{SRO} + t_{GOV}$.	
Stage 3	Agents that report a low outcome are investigated with probability $ip_{SRO} + ip_{GOV}$. If SRO investigates the agent, SRO pays the investigation cost c_{SRO} and the agent pays the penalty *x*. If GOV investigates the agent, GOV pays the investigation cost $c_{GOV} > c_{SRO}$ and the agent pays the penalty *x*.	

4. PARAMETERIZATION

In this section we develop our experimental test of the model of DeMarzo et al. (2005). We present our parameterizations (section 4.1.) and explain how we derived them from the model. We derive the Nash equilibrium predictions for interpretations of the SRO-GOV interaction as simultaneous (SIM) and sequential

(SEQ) game variants (sections 4.2 and 4.6). We also present two alternative experimental treatments that we used as robustness tests (sections 4.4 and 4.5)

4.1 *Baseline and Alternative Parameterization*

A basic problem of experimental tests of this kind is the appropriate choice of parameterization; one would ideally like to calibrate the experiment to reasonably likely real-life scenarios. This has turned out to be a very hard problem especially for micro-economics experiments (e.g., see List, 2006, for a rare successful calibration in this literature). If outright calibration is not possible, the researcher might want to systematically explore the universe of possible calibrations by casting an appropriate grid over it. Unfortunately, such a strategy is prohibitively expensive. This leaves as a third alternative the exploration of "plausible" sets of parameterizations where high and low values for key variables are explored. It is this strategy that we use here. One can think of it as a very coarse grid search.

Another important experimental translation problem is that an SRO, for example, is—as the name suggests—an organization that is unlikely to have, as experimental participants might, social preferences. The same might be true for other players. We will discuss this issue in our discussion and conclusion section below.

We explored three key variables: the success probability (LOW or HIGH), SRO investigation cost (LOW or HIGH) and GOV investigation cost (LOW, HIGH, or VERY HIGH). Our initial prior was that the cost differential—the difference between the SRO and GOV investigation costs—would be of particular importance. As a robustness test for the effect of the cost differential, we included Very High investigation cost of GOV (55).[4] Combined with the Low investigation cost of the SRO (10), this was meant to allow us to study the effect of a very large cost differential (GOV investigation costs are 5.5 times larger than those of SRO).

As in DeMarzo et al. (2005), we assumed clients to be risk-neutral and agents to be risk-averse. For the experimental design and implementation below, we assume that agents have a constant relative risk aversion utility function $u[x] = m \cdot x^{1-RA} / RA$, with a risk-aversion set equal to RA=0.5, which is a typical degree of risk-aversion (Holt & Laury, 2002). To lower the likelihood of social preferences affecting the experimental results, we choose, by setting the utility scaling factor m equal to 10, a constant relative risk aversion utility function that results in SRO payoffs being about the same magnitude as GOV payoffs in equilibrium. As the multiplication by m>0 is a monotonic transformation of the utility function, preferences are invariant for different positive values of m.

Table 2 gives the baseline parameterization and the three key variables.

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[4] We chose the very high value for the cost, 55, as the midpoint of the distribution of the outside options of the clients.

TABLE 2

Parameterizations (Success probability: (LOW/HIGH), cost difference: (LOW/HIGH/VERY HIGH))

Agent (A)	
Client (Cl)	
Utility function Clients	Linear ($u[x]=x$)
Utility function Agents	$u[x]=m\cdot x^{1-RA}/RA$
Utility scaling factor *m*	=10
Risk Aversion Agents (RA)	=0.5
Low investment outcome (*L*)	=20
High investment outcome (*H*)	=200
Success Probability (S*P*)	**= LOW (25%) or HIGH (50%)**
Outside Option (*OO*)	UD over [5,105]
Investigation Cost of SRO (I*Csro*)	**= LOW (10) or HIGH (20)**
Investigation Cost of GOV (I*Cg*)	**= LOW (20), HIGH (40), or VERY HIGH (55)**

Note: The three key variables are in bold.

TABLE 3

Overview of parameterizations

Success Probability

Cost of (SRO, GOV)	LOW 25%	HIGH 50%

Small cost differential

(LOW, LOW) (10, 20)

1.

		GOV None	GOV Low	GOV High
SRO	None	(10, 1)	(14, 4)	**(8, 6)**
SRO	Low	(17, 7)	(10, 9)	(0, 7)
SRO	High	**(11, 12)**	(0, 10)	(0, 9)

None=0%
Low=32%
High=67%

2.

		GOV None	GOV Low	GOV High
SRO	None	(20, 1)	(55, 22)	**(7, 45)**
SRO	Low	(57, 23)	(32, 44)	(0, 46)
SRO	High	**(7, 50)**	(0, 49)	(0, 48)

None=0%
Low=37%
High=94%

(HIGH, HIGH) (20, 40)

3.

		GOV None	GOV Low	GOV High
SRO	None	(10, 1)	(14, 4)	**(8, 6)***
SRO	Low	(16, 6)	**(11, 8)#**	(1, 6)
SRO	High	(10, 10)	(1, 8)	(0, 7)

4.

		GOV None	GOV Low	GOV High
SRO	None	(20, 1)	(52, 19)	**(13, 37)***
SRO	Low	(55, 21)	(31, 38)	(0, 39)
SRO	High	**(14, 45)#**	(0, 42)	(0, 40)

None=0%	**None=0%**
Low=30%	**Low=37%**
High=67%	**High=89%**

Large cost differential

(LOW, HIGH)

(10, 40)

5. Baseline Parameterization

		GOV		
		None	**Low**	**High**
SRO	**None**	(10, 1)	(14, 4)	**(8, 6)***
	Low	(17, 7)	(10, 9)	(0, 7)
	High	**(11, 13)#**	(0, 10)	(0, 9)

None=0%
Low=32%
High=67%

6. Alternative Parameterization

		GOV		
		None	**Low**	**High**
SRO	**None**	(20, 1)	(52, 19)	**(13, 37)***
	Low	(57, 23)	**(31, 40)#**	(0, 40)
	High	(15, 49)	(0, 46)	(0, 43)

None=0%
Low=37%
High=89%

Very Large cost differential

(LOW, VERY HIGH)

(10, 55)

7. L, 10-55

		GOV		
		None	**Low**	**High**
SRO	**None**	(10, 1)	(12, 3)	**(9, 4)***
	Low	(17, 7)	(9, 7)	(3, 6)
	High	**(14, 11)#**	(4, 9)	(0, 6)

None=0%
Low=32%
High=54%

8. H, 10-55

		GOV		
		None	**Low**	**High**
SRO	**None**	(20, 1)	(50, 18)	**(16, 32)***
	Low	(57, 23)	(30, 38)	(0, 36)
	High	**(20, 49)#**	(0, 43)	(0, 39)

None=0%
Low=37%
High=85%

Notes:
The NE of SIM—explained below—is indicated by "*".
The NE of SEQ—explained below——is indicated by "#".

Given the structure of the game and assuming the optimal responses of agents and clients, as in DeMarzo et al. (2005), we created from the set-up in Table 2 the 8 parameterizations shown in Table 3.[5] The payoff matrixes allow subjects in the roles of SRO and GOV to choose monitoring probabilities. We present the payoffs to the subjects to capture the common knowledge assumption of DeMarzo et al. (2005).[6]

As evidenced by the two sets of four cost differentials (one for LOW success probability in the left column, one for HIGH success probability in the right column), the investigation costs of GOV and SRO do not have very large effects. In contrast, the success probability induces considerable variability and increases

[5] Option "None" is equal to an investigation probability of zero. Option "Low" is equal to the investigation probability that the SRO will set when left to its own devices, ip^*_{SRO}. Option "High" is equal to the investigation probability that GOV will set if SRO sets an investigation probability of zero, $ip_{GOV}[0]$, where $ip_{GOV}[0] > ip^*_{SRO}$. The investigation probabilities "Low" and "High" are thus slightly different for the different cost differentials and success probabilities. "Low" varies between 30% and 37% and "High" between 54% and 94%.

[6] One could argue that by giving our subjects the payoff matrices, we have transformed the interaction of SRO and GOV into something somehow different, and that we simply test whether people can "solve" somewhat complicated matrix games. We beg to differ. As mentioned, we have rationalized our payoffs carefully and believe that the payoffs capture the players' conceptualization of the interaction in the same way as other interactions are captured in payoff matrices. That is not to say that it would not be interesting to give subjects a description of the available options only, i.e., without explicit payoff options.

the payoff contrast between the preferred outcomes. An increase in the success probability, for a given investigation probability, increases the gain of deceiving, and thus, to keep agents incentivized to be honest, the success fee has to increase. Also, the (expected) cost of investigation goes down. As a result, the profit of SRO increases. A second-order effect is that SRO will set a slightly higher investigation probability, which will allow clients to pay slightly lower success fees; this thus increases the number of clients, further increasing the profit of SRO. The effect of an increase in success probability on the profit of GOV is also positive: the profitability of the project increases and thus the profit of GOV as well. The effect of an increase is the largest for SRO (GOV) for low (high) investigation probabilities, as agents then extract a large (small) share of the surplus of the investment.

For example, the increase in the success probability between the 5th and 6th parameterizations in Table 3 (which are labeled for reasons to be explained below, Baseline Parameterization and Alternative Parameterization) increases the net profit for GOV the most for outcomes with a high investigation probability of the SRO. Specifically, the outcome (SRO, GOV)=(High, None) sees a payoff increase from 13 to 49 for GOV, while the outcome (SRO, GOV)=(None, None) sees no increase at all, as in the latter case all surplus is extracted by agents. Analogously, the payoffs for SRO increase most for outcomes with a low investigation probability. Specifically, the outcome (SRO, GOV)=(Low, None) sees an increase from 17 to 57 for SRO, while the outcome (SRO, GOV)=(High, High) sees no increase at all. The increase in the success probability thus increases the payoff contrast between the preferred outcomes.

For our experimental treatments we focus on the parameterization with the lesser payoff contrast: the 5th parameterization, referred to as "Baseline". We also test the 6th parameterization (referred to as "Alternative") as a robustness test.

4.2 *SIM and SEQ interpretations*

The parameterizations for possible treatments are shown above in a normal form that can be given a simultaneous or sequential interpretation. DeMarzo et al. (2005) think of it as a sequential game in which SRO is a Stackelberg leader: SRO takes action first while anticipating the best response of GOV. Technically, the authors thus conceptualize the interaction between SRO and GOV as a subgame-perfect game solvable by backward induction. The validity of this solution mechanism is not uncontested behaviorally (e.g., Beard & Beil, 1994; Cooper & Van Huyck, 2003). In section 2 we argued that the sequential interpretation may not be an adequate representation of the problem, as announcements by the SRO may in practice not be of a precise or numerical nature and may, moreover, not be credible. GOV thus will not immediately be able to verify the investigation probability that SRO has chosen. We hence study simultaneous (SIM) and sequential (SEQ) game variants.

In both game variants of the Baseline treatment, the Pareto-optimal outcome is where SRO chooses the investigation probability "High" and GOV "Low". The investigation is thus conducted by the party that has the lowest investigation costs. In SEQ, theory predicts that the first-mover, SRO, assumes that the second-mover, GOV, will choose the investigation probability that brings GOV the highest payoff. SRO thus predicts that GOV will choose "High" if SRO chooses "None", "Low" if SRO chooses "Low", and "None" if SRO chooses "High". This reduces the choices of SRO between (SRO, GOV) to (None, High), (Low, Low), and (High, None), where the last alternative has the highest payoff for SRO. The Nash equilibrium for SEQ thus coincides with the Pareto-optimal outcome. In SIM, the Pareto-optimal outcome of (High, None) is dominated for SRO by (Low, None), as SRO prefers a lower investigation probability. This outcome in turn is dominated for GOV by (Low, Low), as GOV prefers a higher investigation probability. This outcome is in turn dominated for SRO by (None, Low), which in turn is dominated for GOV by (None, High). The Nash equilibria for SIM and SEQ are thus in opposite quadrants. This is a result that is both intuitive and well known (e.g. Kreps, 1990).[7]

4.3 *The overall design*

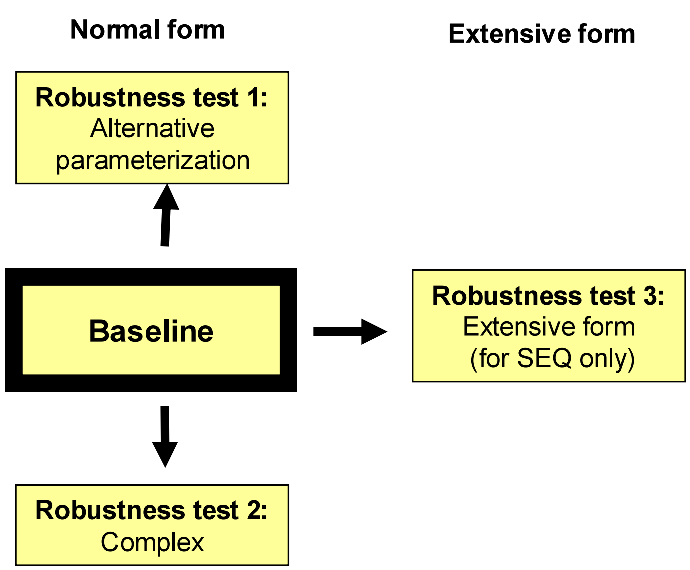


FIGURE 1

Summary of the treatments

[7] In our robustness test (the 6th parameterization in Table 3), the Nash equilibrium in SEQ is different in a discontinuous representation than in a continuous one: it is (SRO, GOV)=(Low, Low) instead of (High, Low). That discontinuous representation of continuous payoffs may result in additional or different equilibria is a common problem in designing experiments (Holt, 1985; Huck, Müller & Normann, 2001; Hinloopen, Müller & Normann, 2012). One strategy is to slightly manipulate the payoff table by adding or subtracting a small number from the payoffs so as to guarantee that the experimental representation has the same Nash equilibrium as the theoretical, continuous setup (Huck, Müller & Normann, 2001, p. 753; Hinloopen, Müller & Normann, 2012, p.9). In our case, even the smallest integer, "1", is large relative to the payoffs for GOV, and we thus decided not to do so.

Summarizing, we take parameterization 5 as our main (Baseline) treatment and test it both for the SIM and SEQ games, which can be thought of as different treatments. This parameterization can be found in the left middle row of Figure 1 which summarizes our design. We also added robustness tests. As a first robustness test, we added a treatment that uses parameterization 6 (“Alternative”) in Table 3. As a second robustness test, for the SIM and SEQ games, we added a treatment with a higher degree of complexity, and as a third robustness test, for the SEQ game only, we added a treatment with payoffs in the extensive game form. We explain robustness tests 2 and 3 in sections 4.4 and 4.5. We can think of many other robustness tests—see the Conclusion and Discussion sections below—but our budget was limited and hence we had to make choices. These were the ones that struck us as the most pressing.

4.4 *Robustness test 2: normal form representation in a complex format*

As second robustness test we add SIM and SEQ treatments with a higher degree of complexity by increasing the choice resolution from 3x3 to 6x6. Participants thus could choose from a set enlarged to 6 choices: {None, Very Low, Low, Medium, High, Very High}.[8] For an example, see Figure 3. The complex representation has the same order of play and parameterization as the baseline treatment.

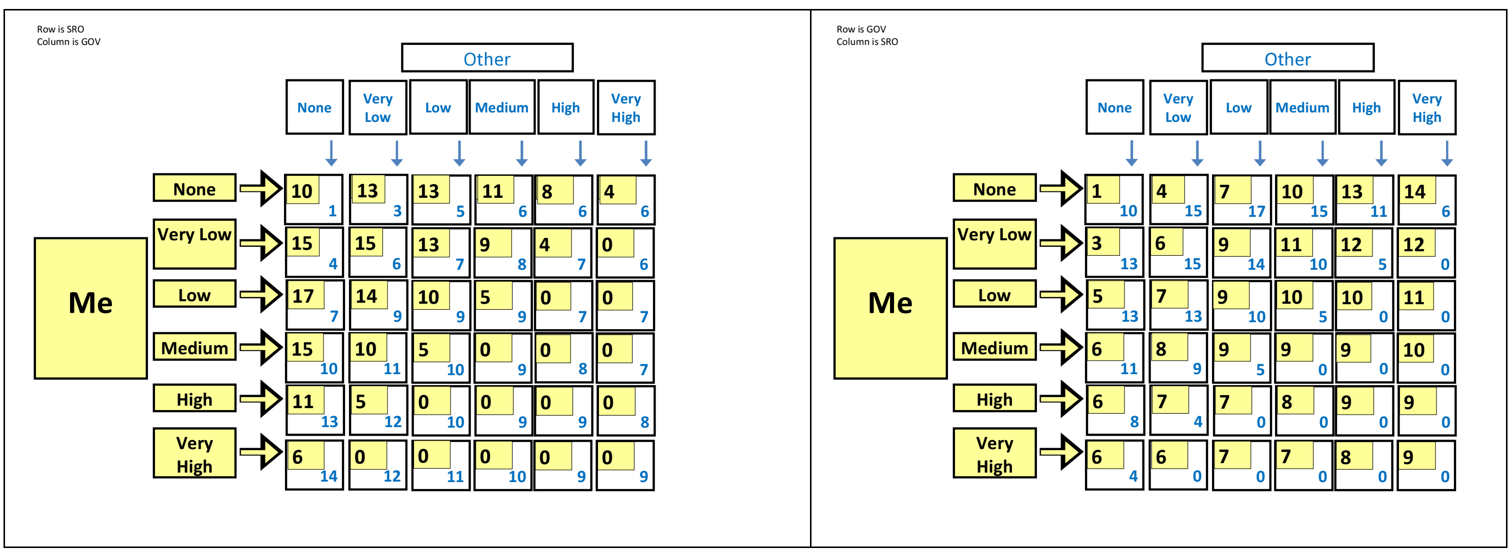


FIGURE 2

Presenting the normal-form game to participants in a complex format

[8] Option “None” is equal to an investigation probability of zero. The investigation probability is then increased by 16.67% for each of the successive options. Thus, the option “Very Low” is equal to an investigation probability of 16.67%, the option “Low” to one of 33.33%, the option “Medium” to one of 50%, and so on.

4.5 *Robustness test 3: extensive-form representation*

As a third robustness test we add a treatment with the payoffs in the extensive game form, as this representation may be more congruent with the decision process in SEQ than the normal form representation. In SEQ, the normal form representation is likely to make it harder for participants to understand the structure of the sequential decision process. We thus implemented an extensive-form representation of the Baseline parameterization to see if this would make a difference. For an example, see Figure 2. Our working hypothesis was that it would enhance understanding and hence lead to faster convergence to the equilibrium.

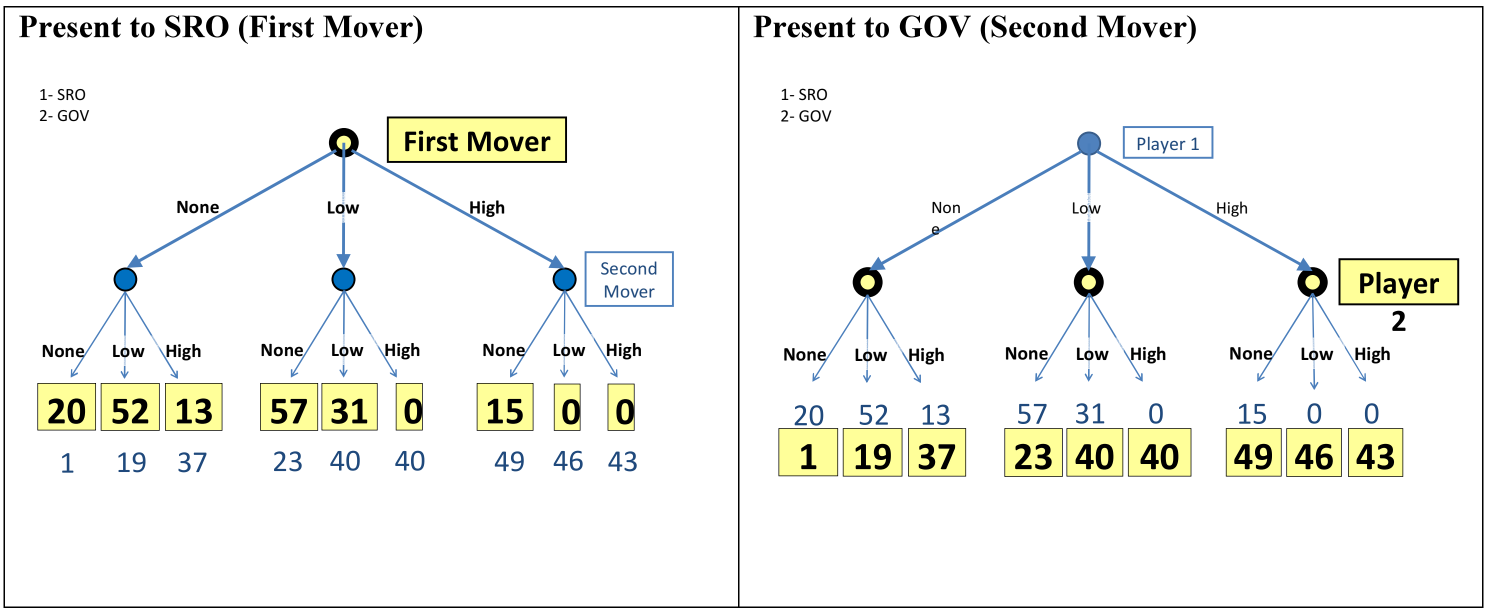


FIGURE 3

Presenting the extensive-form game to participants

4.6 *Nash equilibria for all treatments and experimental implementation details*

Table 4 shows the Nash equilibria for our Baseline treatment and the robustness tests. Due to the rounding of numbers, the Nash equilibria were not identical in some cases of the robustness test. In the first robustness test (Alternative Parameterization), the Nash equilibrium for SEQ is (Low, Low). In the second robustness test, using the complex representation, rounding created other Nash equilibria in addition to the theoretical one: (None, Medium) and (None, Very high) in SIM and (Low, Very Low) in SEQ. We count these responses as Nash equilibrium choices in our tests below.

TABLE 4

Nash equilibria

	Nash equilibrium SIM (SRO, GOV)	**Nash equilibrium SEQ (SRO, GOV)**

Baseline treatment	(None, High)	(High, None)
Robustness tests		
1. Alternative parameterization (normal form)	(None, High)	(Low, Low)
2. Baseline case complex (normal form)	**(None, High);** (None, Medium); (None, Very High)	**(High, None);** (Low, Very Low)
3. Baseline case (extensive form)	(None, High)	(High, None)

4.7 *Design summary*

The experiment was programmed and conducted with the software z-Tree (Fischbacher 2007). The data were processed and analyzed with the software programs ztree2stata[9] and STATA 12.

TABLE 5

Sessions, participants, and independent observations

	SIM	**SEQ**
Baseline treatment	3 sessions 72 participants 12 independent observations	2 sessions 48 participants 8 independent observations
Robustness tests		
1. Alternative parameterization (normal form)	1 session[10] 24 participants 4 independent observations	2 sessions 48 participants 8 independent observations
2. Baseline case complex (normal form)	2 sessions 48 participants 8 independent observations	2 sessions 48 participants 8 independent observations
3. Baseline case (extensive form)		1 session 24 participants 4 independent observations

Table 5 gives an overview of our sessions. We ran a total of 13 sessions in December 2011 in the "LEE" experimental lab of the University of Economics in Prague.[11] For each session we had 24 participants make decisions, as SRO or GOV, over 10 rounds. Following well-documented experimental practice, participants

[9] See http://www.econ.hit-u.ac.jp/~kan/research/ztree2stata/.

[10] We had planned on two sessions each for the Baseline treatment and the Alternative parameterization treatment but by mistake ended up with three and one, respectively. In light of our results, the mistake seems to have been inconsequential and doubling up on the observations in the Alternative parameterization cell does not promise new insight.

[11] See www.vse-lee.cz.

in each session were divided into 4 groups of 6 to increase the number of truly independent data points. In each group, 3 participants were randomly assigned the role of GOV and 3 the role of SRO. Roles were fixed throughout the session. In each round, participants were randomly matched with a participant of the other role within their group. Each session thus resulted in 4 (24÷6) independent observations. We ran a total of 13 sessions involving 312 participants, generating 52 independent data points. Participants were recruited with ORSEE software from among bachelor students of the University of Economics in Prague. We used neutral language in the instructions (reprinted at the end of this manuscript), and all treatments were implemented using the direct-response method. Participants earned on average CKZ 360 (= €14) in a session of 50 minutes (including the reading of the instructions).

5. RESULTS

In this section we show the proportion of choices by the participants in the experiments that are part of a Nash equilibrium. We interpret the outcome of a high proportion of choices (or at least a convergence to that outcome) as experimental corroboration of the theoretical model of DeMarzo et al. (2005). We show the results of both the simultaneous (SIM) and the sequential (SEQ) variant.

5.1 *Proportion of Nash equilibrium choices*

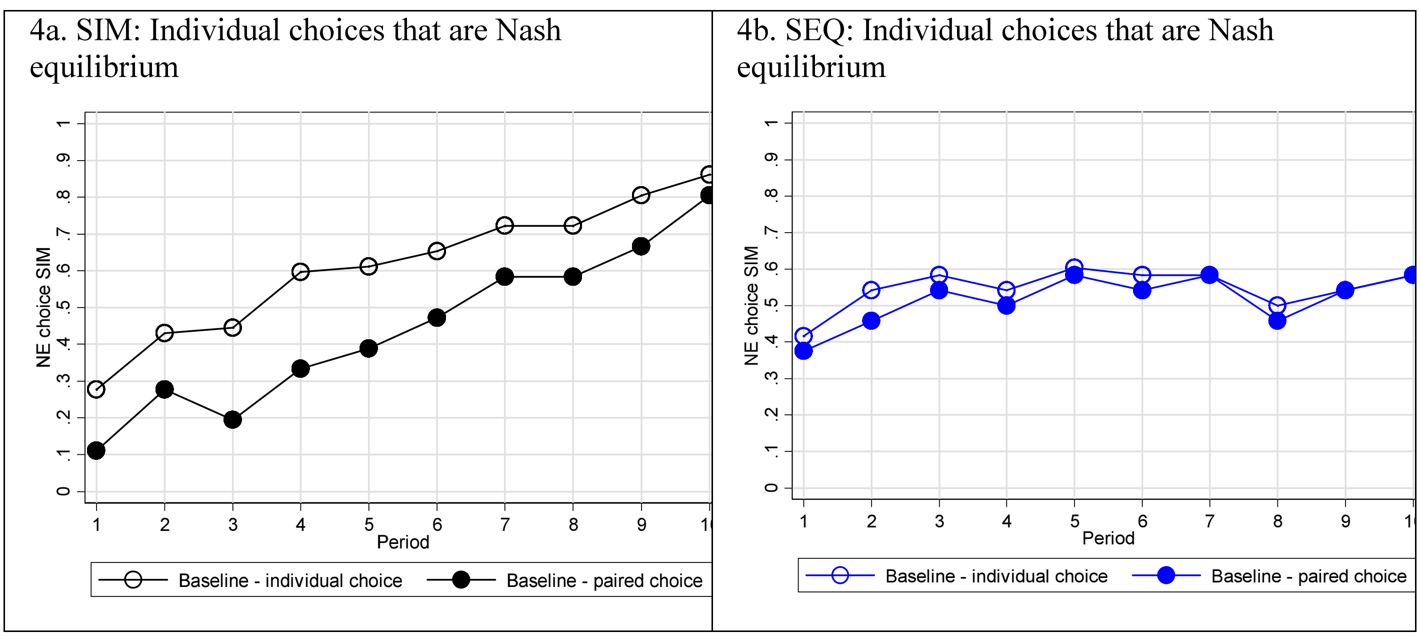


FIGURE 4

Proportion of Nash equilibrium choices for SIM and SEQ

FIGURE 4 shows, for the treatments in the SIM (4.a) and SEQ (4.b) variants, the proportion of choices that are congruent with a Nash equilibrium, both for the individual (the empty circles) and the paired choices (the solid circles).

Initially, individual choices are far from a Nash equilibrium in both SIM and SEQ variants: typically in less than half the cases an individual makes a Nash equilibrium choice. We see, however, a remarkable learning effect. In the last few rounds paired choices are in all treatments in the SIM variants in the range of 60%–80% and in all treatments in the SEQ variants in the range of 50%–60%. Contradicting our priors, learning is less pronounced in the SEQ variants than in the SIM variants, reflecting the fact that subjects find it more difficult to find SEQ Nash equilibria than SIM ones. For SEQ, the subjects in the role of the first-mover (SRO) often make a choice different from the one implied by the subgame-perfect Nash equilibrium.

While somewhat alike, the percentage of equilibrium choices in the Alternative Parameterization treatments ("Alternative") is higher initially and converges faster to full equilibrium play than the corresponding percentage in the Baseline, for both the SIM and SEQ variants. This is likely the result of the stronger contrast in payoffs for SRO and GOV in the Alternative parameterization. Recall that the Alternative parameterization has a stronger contrast because it has a higher success probability than the baseline (50% versus 25% success probability).

The figures for the paired choices (3c and 3d) have mainly the same shapes as those for the individual choices, while the percentages are lower. The lower percentages are expected as the paired choice is a part of a Nash equilibrium only if both of the individual choices in a pair are a part of a Nash equilibrium.

Figure 5 shows the experimental results of our robustness tests (recall that we use an alternative parameterization, a more complex format— 6 by 6 instead of 3 by 3—and an extensive form representation), which are in line with the Baseline SEQ treatment.

5.a SIM: Individual choices that are Nash equilibrium	5.b SEQ: Individual choices that are Nash equilibrium

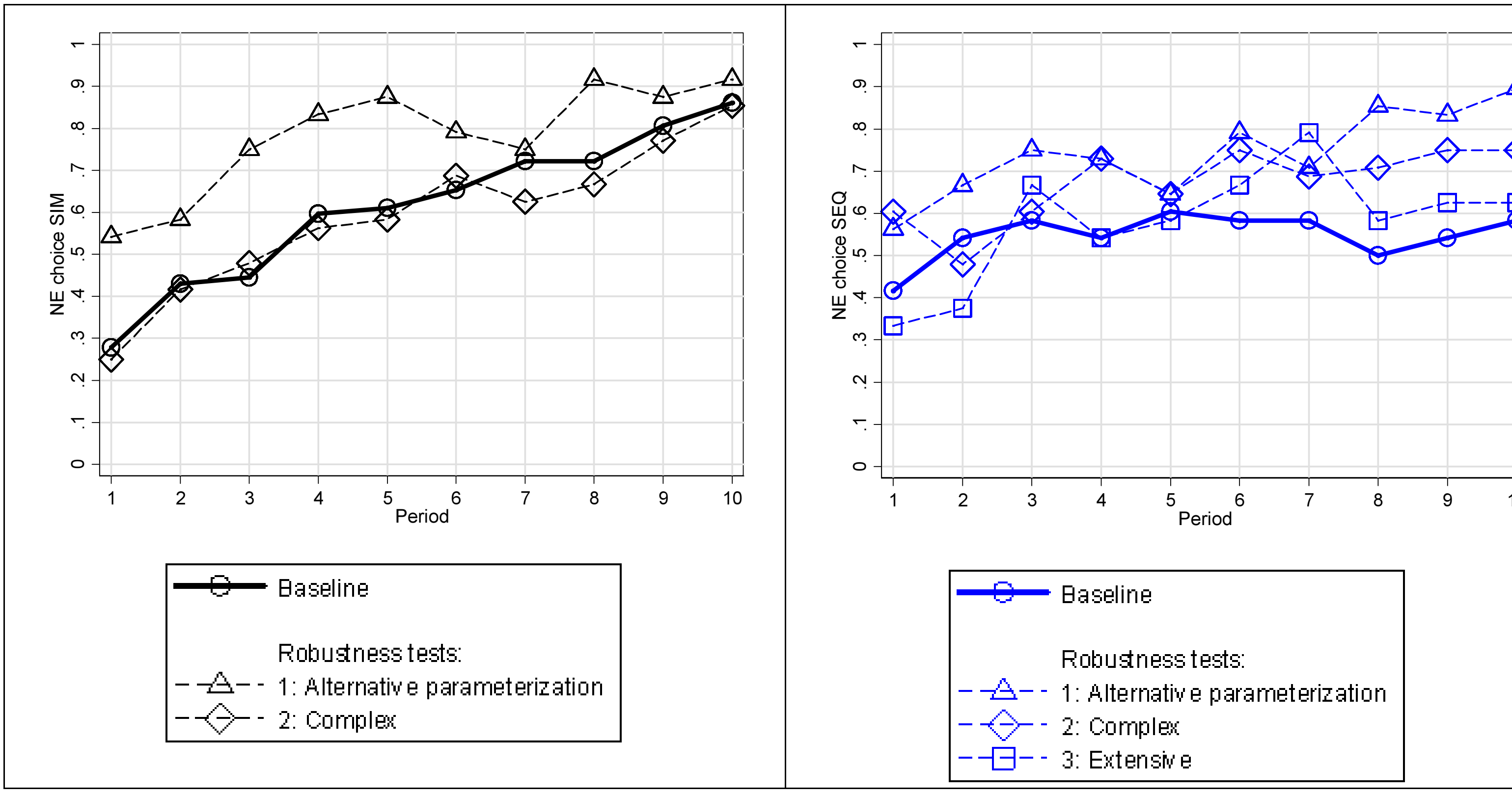


FIGURE 5

Comparing the complex treatment in SIM

Figure 5 compares the results of the robustness tests together with the baseline treatment. The choices for the baseline treatment are indicated by a thick line with large round markers. The choices for the treatments of the robustness tests, Alternative parameterization, Complex representation and Extensive representation are indicated by thin lines with triangles, diamonds, and squares, respectively. The choices for the Complex treatment are very much in line with the three treatments that were presented in the simple format and very close to the average of these three. This is true for both the SIM and SEQ games. We conclude that our conclusions are robust to a different parameterization, a considerable increase in complexity (from 3 to 6 choices for each participant), and the use of an extensive representation.

6. DISCUSSION AND CONCLUSION

As always in laboratory tests, a number of extensions and robustness tests suggest themselves. While we have carefully rationalized the appropriate parameterizations, there is room for further parametric exploration. We do not believe, however, that it will lead to substantially different results for rationalizable parameterizations. As mentioned, an important experimental translation problem is that an SRO is an organization that is unlikely to be, as experimental participants might, afflicted with social preferences. An obvious robustness test would therefore be (as a considerable and growing literature has documented) to let teams interact (e.g., Charness & Sutter, 2012; Cooper & Kagel, 2005). In light of the evidence that has emerged (suggesting that team interaction leads to more play in accordance with standard game theoretic predictions), we doubt that the essence of our results can be questioned on those grounds. Other obvious experimental treatments are those concerning standard experimental manipulations such as financial stakes or the question of the framing of instructions. Again, we do not believe that this would change our experimental results significantly (e.g., Rydval & Ortmann, 2004).

We have experimentally tested the interesting DeMarzo et al. (2005) model, in which the authors argue that the mere threat of punishment (government intervention) might lead to second-best self-regulation. This topic is of importance since many industries try to rely on self-regulation, which has been argued theoretically to be incentive-incompatible (Shaked & Sutton, 1981; Nunez 2001, 2007; Mysliveček & Ortmann, 2010; Ortmann & Svitkova, 2010) and has indeed empirically a questionable track record (e.g., Kleiner, 2006). In the light of a lack of empirical evidence of the efficacy of the threat of government intervention we conducted an experimental test of the propositions in DeMarzo et al. (2005) and find that in specific parameterizations and implementation details, the predictions of their model are borne out. The support we find for the predicted outcomes of low efficiency when players move simultaneously and high efficiency when players move sequentially implies interesting mechanism design suggestions to assure the realization of highly efficient sequential play. For example, the government might require the SRO to announce a precise investigation probability and this announcement should be legally binding. Moreover, the realized percentage of investigations should be checked and deviations from the announced percentage should be severely sanctioned.

APPENDIX

Rational choices in the sequential game

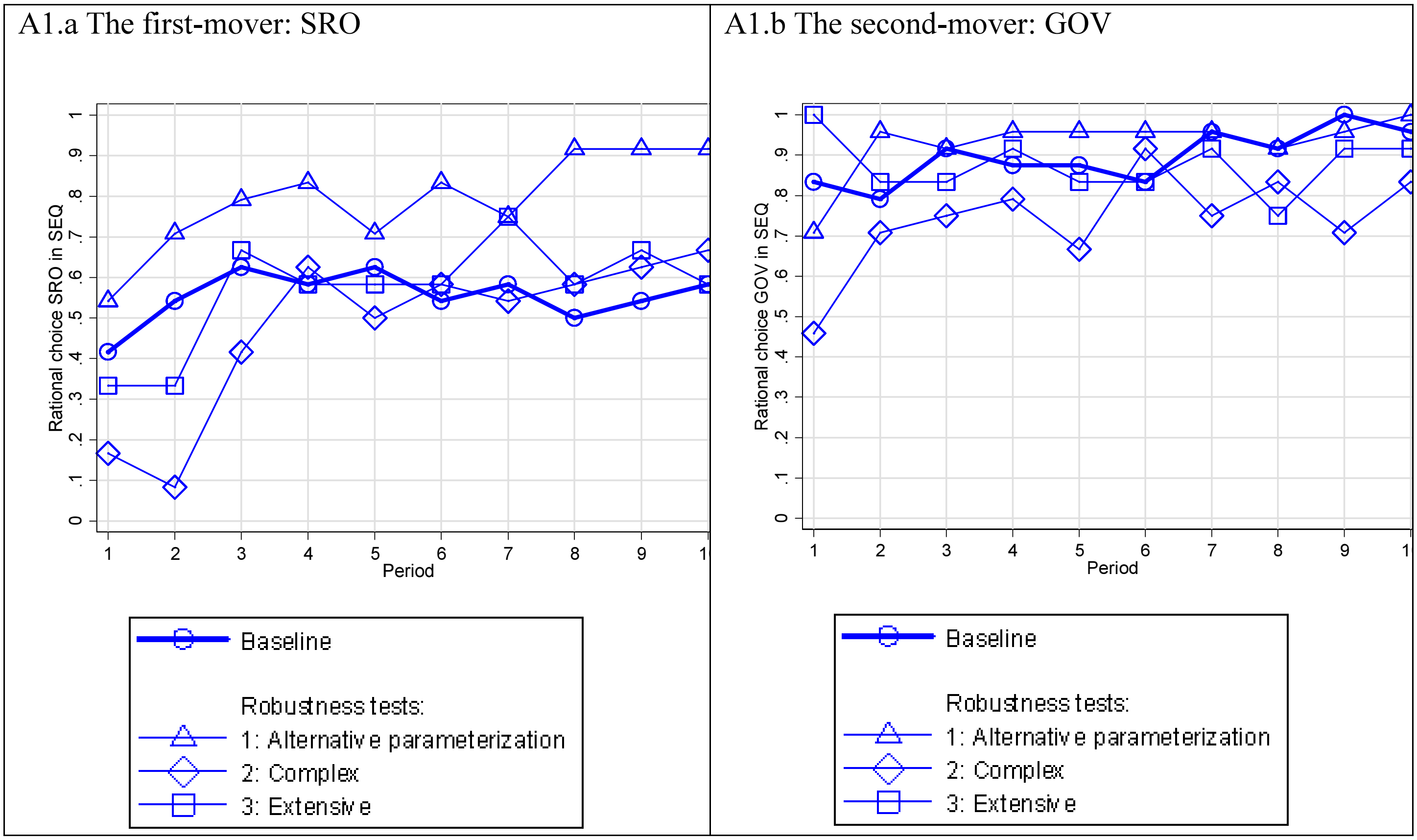


FIGURE A1

Proportion of individual rational choices in SEQ

In the sequential game SRO is the first-mover and GOV the second-mover. GOV thus makes its choice knowing the choice of SRO. This allows us to look if GOV makes profit-maximizing choices given the choice of the first-mover SRO. Figure 5 shows the proportion of individual choices that are rational in the SEQ game. As before, the treatments for Baseline and Alternative parameterizations and Extensive representation are indicated by the round, square, and x-shaped markers, respectively. The choice of the first-mover, SRO, is regarded rational if it is the Nash equilibrium choice. The choice of the second-mover, GOV, is regarded rational if it is the best reply to the choice of the participant in the role of SRO he is paired with. The rational reply for GOV is thus only the Nash equilibrium choice if the paired SRO also made the Nash equilibrium choice..

Figure A1.b shows that GOV makes highly rational choices. Already in the first few rounds the proportion of rational choices is mostly above 80%. In the last two rounds, the proportion of rational choices is above 90% in all treatments. The participants in the role of SRO seem to face more of a struggle to make rational choices. Figure 5.a shows that in the first rounds, the percentage of rational choices ranges between 30%–60% and in the last rounds between 50%–90%, indicating learning, most notably in the Alternative Parameterization (again, due to the larger pay-off contrast between SRO and GOV in this parameterization). Participants in the role of SRO have a more difficult task to make a rational choice, as they must deduce their best choices through backwards induction, assuming a rational response by GOV. It is thus to be expected that the process of convergence for participants in the role of the first-mover SRO is slower. It is reassuring that we see convergence for the choices of SRO and that the rational choices of GOV give the right incentive to SRO to learn the rational responses.

Consolidated instructions

Codes used to indicate the treatment:

Base	– Baseline parameterization treatment of 3x3 (either SIM or SEQ)
Alt	– Alternative parameterization treatment of 3x3 (either SIM or SEQ)
SIM	– Simultaneous play
SEQ	– Sequential play
6x6	– A 6x6 payoff matrix
EXT	– Extensive game representation.

A code indicating the start of a text referring to a specific treatment or set of treatments always starts with “[“ and follows with the codes indicating the specific treatment(s). A code indicating the end of a text referring to a specific treatment or set of treatments always ends with the codes indicating the specific treatment(s) and finishes with “]“.

INSTRUCTIONS

Welcome to the experiment!

General rules
Please turn off your mobile phones now.

If you have a question, raise your hand and the experimenter will come to your desk to answer it.

You are not allowed to communicate with other participants during the experiment. If you violate this rule, you will be asked to leave the experiment and will not be paid (not even your show-up fee).

Introductory remarks
You are about to participate in an economics experiment. The instructions are simple. If you follow them carefully, you can earn a substantial amount of money. Your earnings will be paid to you in cash at the end of the experiment.

The currency in this experiment is called "Experimental Currency Units", or "ECU"s. At the end of the experiment, we will exchange ECUs for Czech Crowns as indicated below. Your specific earnings will depend on your choices and the choices of the participants you will be paired with.

Your exchange rate will be:

[Base
2 Czech Crowns for an ECU.
Base]

[Alt, 3x3, SIM/SEQ, SRO
1.5 Czech Crown for an ECU.
Alt, 3x3, SIM/SEQ, SRO]

[Alt, 3x3, SIM/SEQ, GOV
0.5 Czech Crown for an ECU.
Alt, 3x3, SIM/SEQ, GOV]

This experiment should take at most 60 minutes. There are 10 paid rounds in this experiment.

You are encouraged to write on these instructions and to highlight what you deem particularly relevant information.

[Please go to the next page now.]

Group assignment
You will always be a member of a group consisting of you and ONE other person in this room. Group membership is anonymous; you will not know who is in a group with you and the other person in your group will not know that you are in his or her group. **Group membership is assigned anew in each round, in a random way.**

You will be asked to make a series of **interactive** decisions in this experiment, i.e. **your earnings in each round will depend both on your decision and that of the person that you are paired with for that round.**

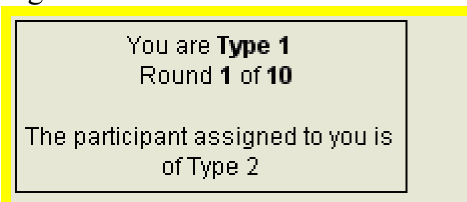
In each group one participant will be of Type 1 and the other one will be of Type 2.

[SEQ
The Type 1 participant will make a decision first ("move first"). The choice of the Type 1 participant is then communicated to the Type 2 participant . The Type 2 participant will make a decision subsequently ("move second").
SEQ]

[SIM
You will not know beforehand what the other participant chooses and the other participant will not know beforehand what you choose.
SIM]

The roles of Type 1 and Type 2 are randomly assigned at the beginning of the experiment and remain the same throughout the experiment. Once the experiment starts, you will see whether you are Type 1 or Type 2 on your screen in the upper left corner. Below it you can also see the round. For an example, see Figure 5.

Figure 5



[Please turn over]

Decision Screen
In each round you will be presented with a Decision Screen where you will make a choice by clicking on one of the

[3x3
three buttons labeled NONE, LOW, or HIGH.
3x3]
[6x6
six buttons labeled using NONE, VERY LOW, LOW, MEDIUM, HIGH, and VERY HIGH.
6x6]

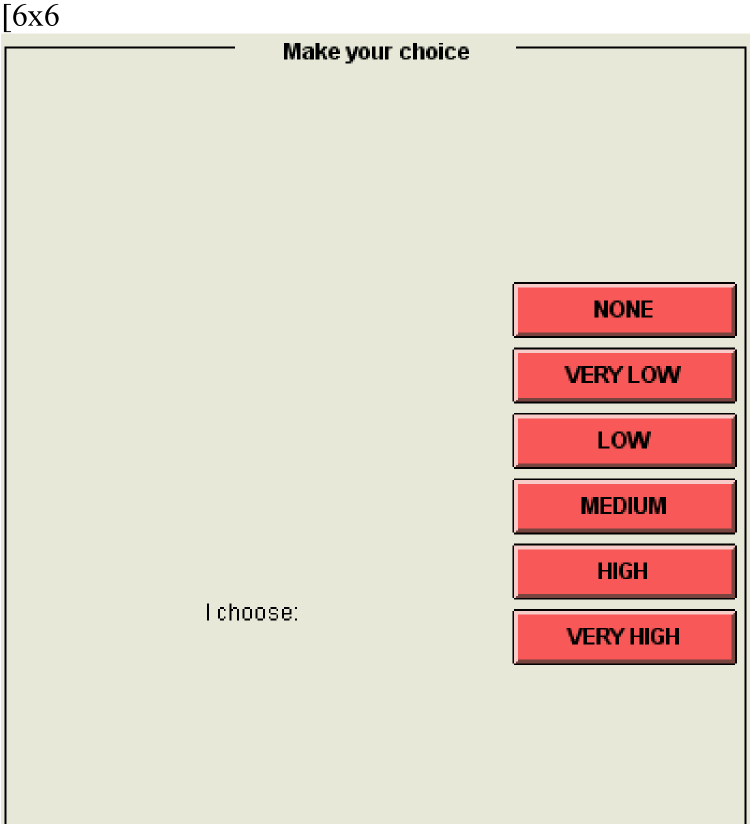
See the example in **Figure 6**.

Figure 6

[3x3

3x3]

[6x6

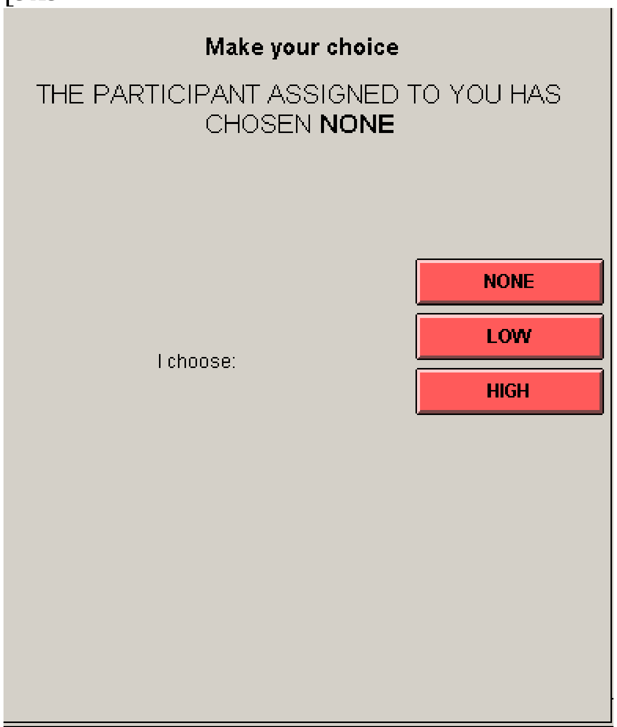


6x6]

[SEQ

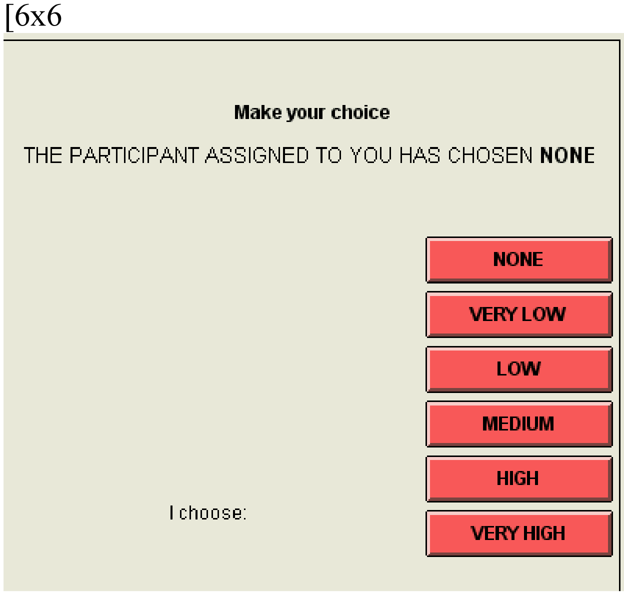
The example in **Figure 6** shows how a Type 1 participant, who will move first, will make a choice. In Figure 7 is shown how a Type 2 participant, who will move second, will make a choice. As you can see in Figure 7, a Type 2 participant sees the choice that the Type 1 participant assigned to him or her for that round has made.

Figure 7
[3x3



3x3]

[6x6



6x6]
SEQ]

You can see your possible earnings and the possible earnings of the participant assigned to you for that round in the Earnings Table on the paper with the title **"YOUR EARNINGS TABLE"** which you find on your desk.

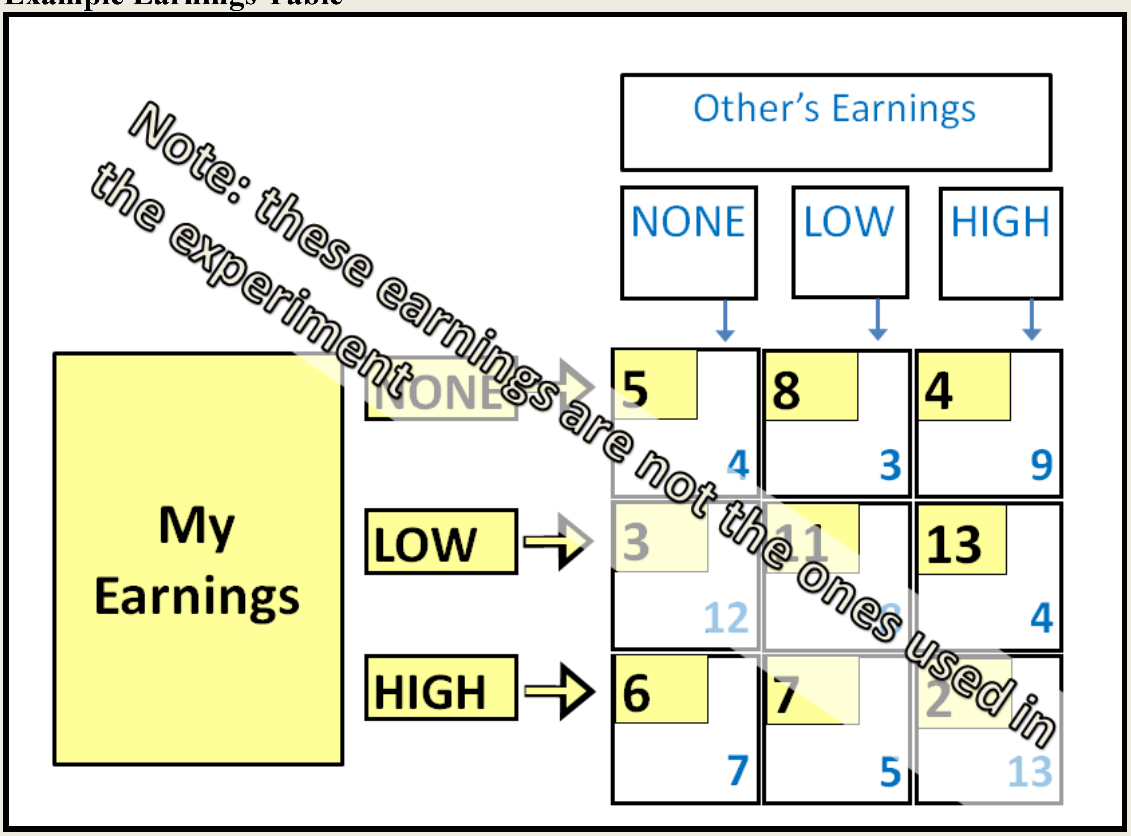
Your payoffs are in bolded black numbers on yellow background in the upper left corners of each cell of the Earnings Table. The payoffs of the participant assigned to you for that round are in blue numbers on a white background in the lower right corner of each square of the Earnings Table. To repeat, your earnings in each round will depend both on your choice and that of the person that is assigned to you for that round.

EXAMPLE BOX

In this EXAMPLE BOX we will explain how your choices and the choices of the participant that is assigned to you determine your earnings.

The Example Earnings Table in this EXAMPLE BOX is NOT the earnings table used in the experiment. In the experiment a different Earnings Table will be used: the one on your table with the title "YOUR EARNINGS TABLE".

Example Earnings Table

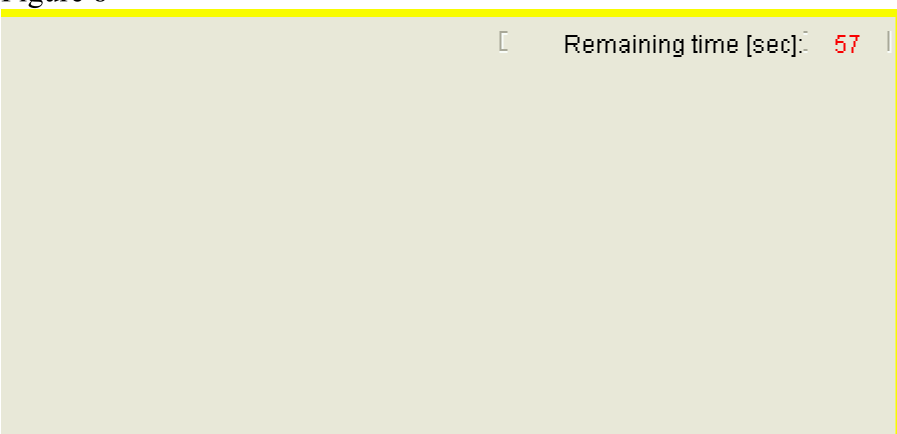


If the **Example Earnings Table** would be the relevant Earnings Table, then if the participant assigned to you chose NONE, your earnings will be 5 if you choose NONE, 3 if you choose LOW, and 6 if you choose HIGH. If the participant assigned to you chose LOW, then your earnings will be 8 if you choose NONE, 11 if you choose LOW, and 7 if you choose HIGH.

The earnings of the participant that is assigned to you are determined in a similar manner, with their earnings shown in the lower right corner of each square of the Example Earnings Table.

To make your choice you have one minute; if you have not made a choice during that time, the computer will assign you the choice of NONE. This is the standard procedure for all decisions in this experiment. You can see the time you have left to make a choice in the upper right corner of the screen (“Remaining time”), see Figure 8 for an example.

Figure 8



[SEQ
To repeat, Type 1 will make a decision first (“move first”). The choice of Type 1 is then communicated to Type 2. Type 2 will then make a decision (“move second“).
SEQ]

[SIM
To repeat, you will not know beforehand what the other participant chooses and the other participant will not know beforehand what you choose .
SIM]

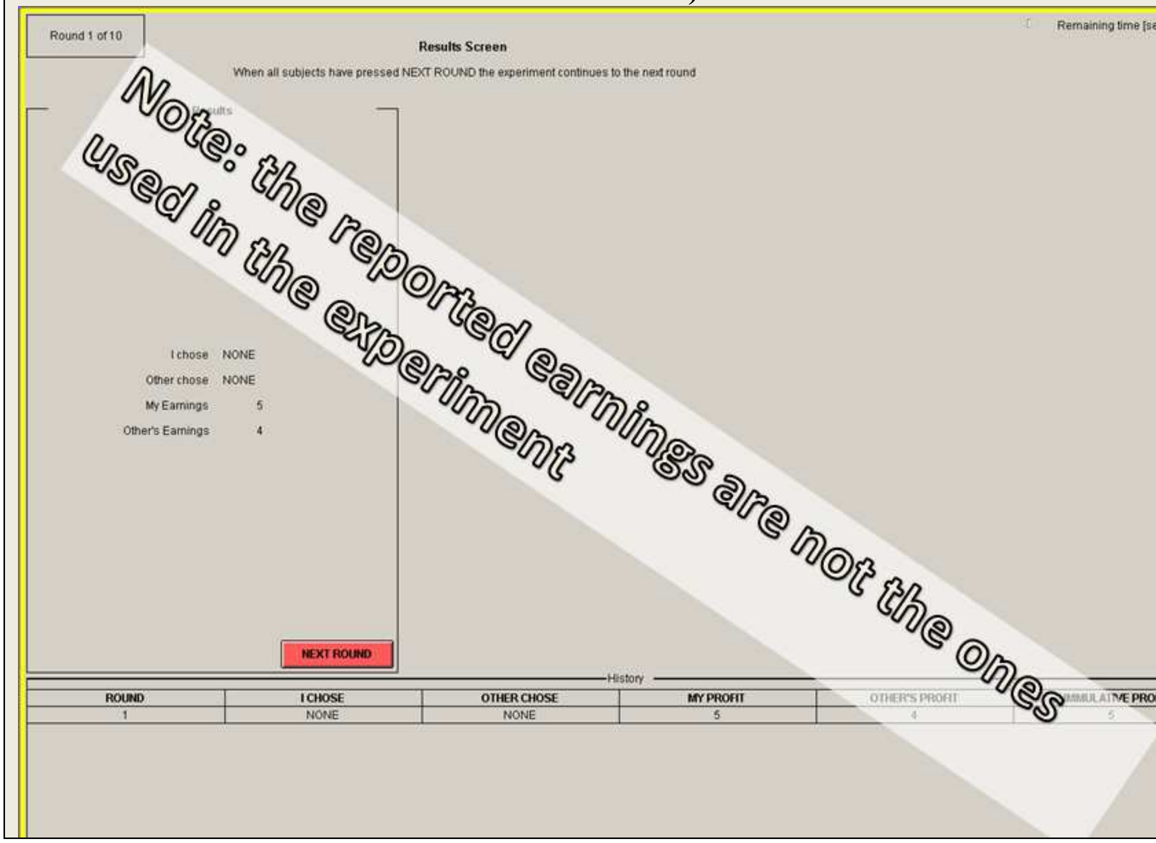
After all participants have made their decisions, or if one minute has expired, the computer will calculate your earnings.

Results Screen
You will next see a Results Screen. The Results Screen will show your choice and the choice of the participant that is assigned to you for that round. The Results Screen will also show your and the other participants’ earnings.

EXAMPLE BOX

In the example in Figure 3 you and the other participant chose NONE. In the example in Figure 3 your earnings are thus 5 and that of the other participant are 4
(**to repeat: in the experiment a different Earnings Table will be used: the one on your**

desk with the title “YOUR EARNINGS TABLE”).



You have one minute to inspect the outcomes. (This is the standard time you have for inspecting results). When you need less time to inspect the outcomes, then click the NEXT ROUND button. Once all participants have clicked the NEXT ROUND button, the experiment continues with the next round. Note that the Results Screen will be visible until all participants have clicked on the NEXT ROUND button.

Do you have any questions at this point?

[Base, 3x3, SRO

YOUR EARNINGS TABLE

		Other		
		NONE	LOW	HIGH
Me	NONE	10 1	14 4	8 6
	LOW	17 7	10 9	0 7
	HIGH	11 13	0 10	0 9

Base, 3x3, SRO]

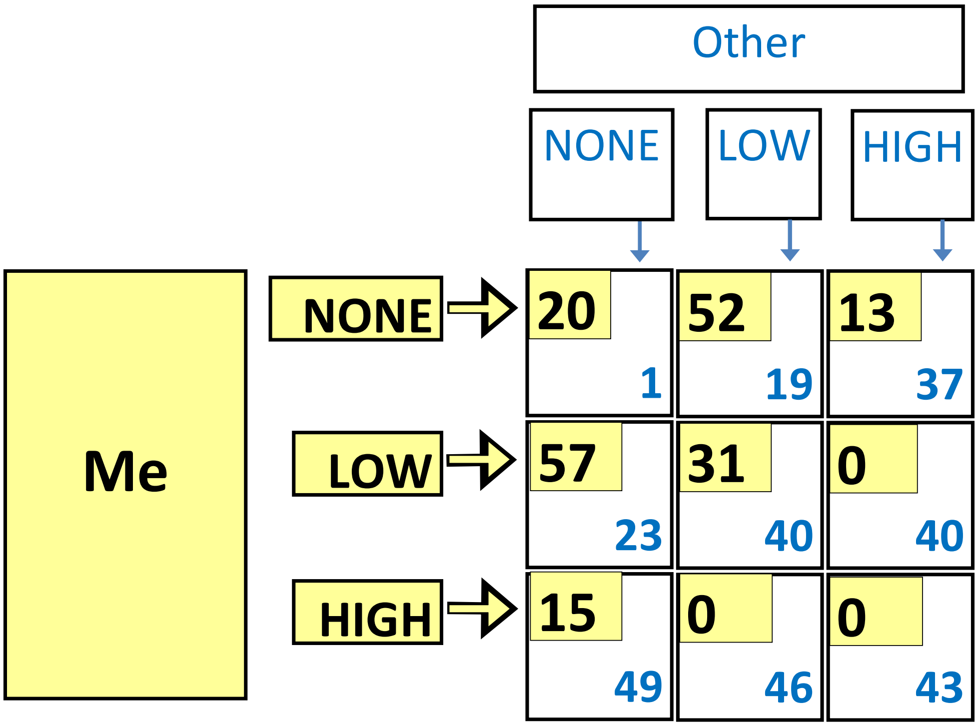
[Base, 3x3, GOV

YOUR EARNINGS TABLE

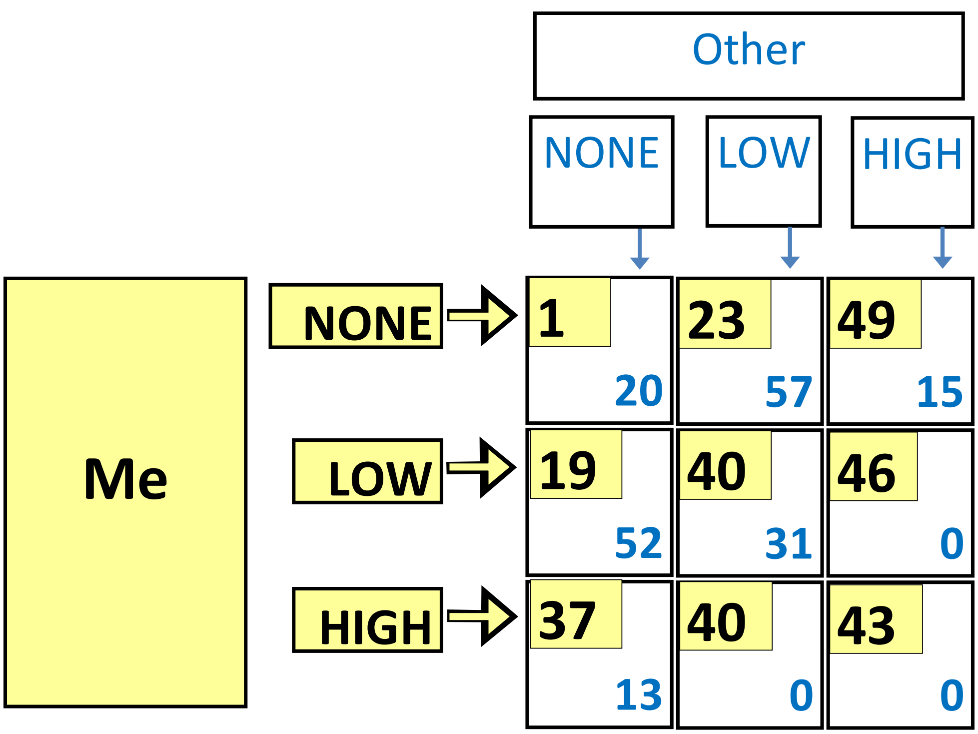
		Other		
		NONE	LOW	HIGH
Me	NONE	1 10	7 17	13 11
	LOW	4 14	9 10	10 0
	HIGH	6 8	7 0	9 0

Base, 3x3, GOV]

[Alt, 3x3, SRO



Alt, 3x3, SRO]
[Alt, 3x3, GOV



Alt, 3x3, GOV]

[Base, 6x6, SRO

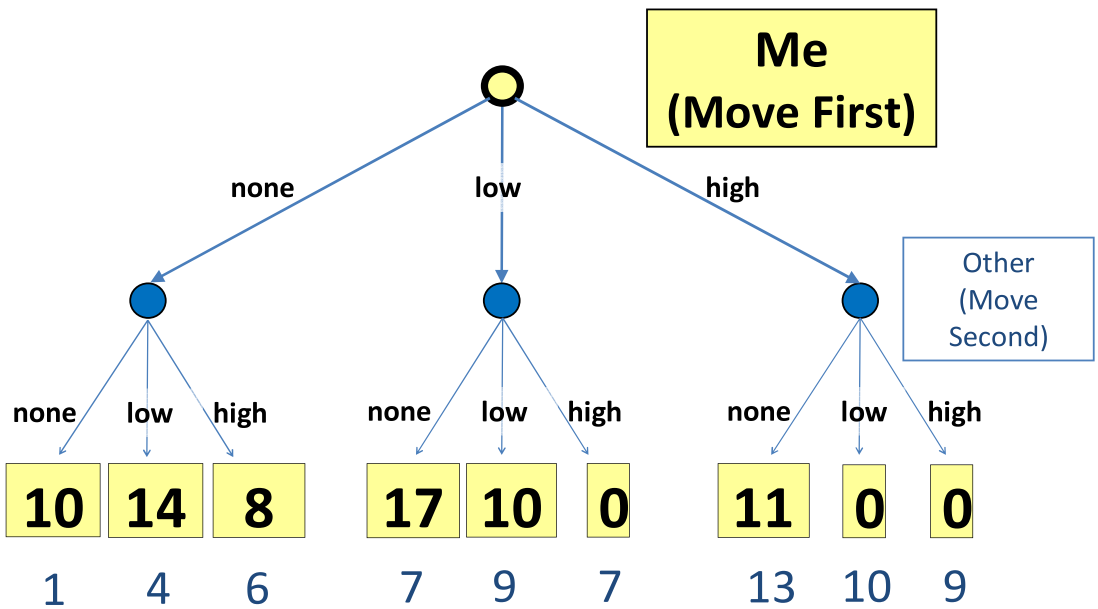
		Other					
		NONE	VERY LOW	LOW	MEDIUM	HIGH	VERY HIGH
Me	NONE	10 / 1	13 / 3	13 / 5	11 / 6	8 / 6	4 / 6
	VERY LOW	15 / 4	15 / 6	13 / 7	9 / 8	4 / 7	0 / 6
	LOW	17 / 7	14 / 9	10 / 9	5 / 9	0 / 7	0 / 7
	MEDIUM	15 / 10	10 / 11	5 / 10	0 / 9	0 / 8	0 / 7
	HIGH	11 / 13	5 / 12	0 / 10	0 / 9	0 / 9	0 / 8
	VERY HIGH	6 / 14	0 / 12	0 / 11	0 / 10	0 / 9	0 / 9

Base, 6x6, SRO]
[Base, 6x6, GOV

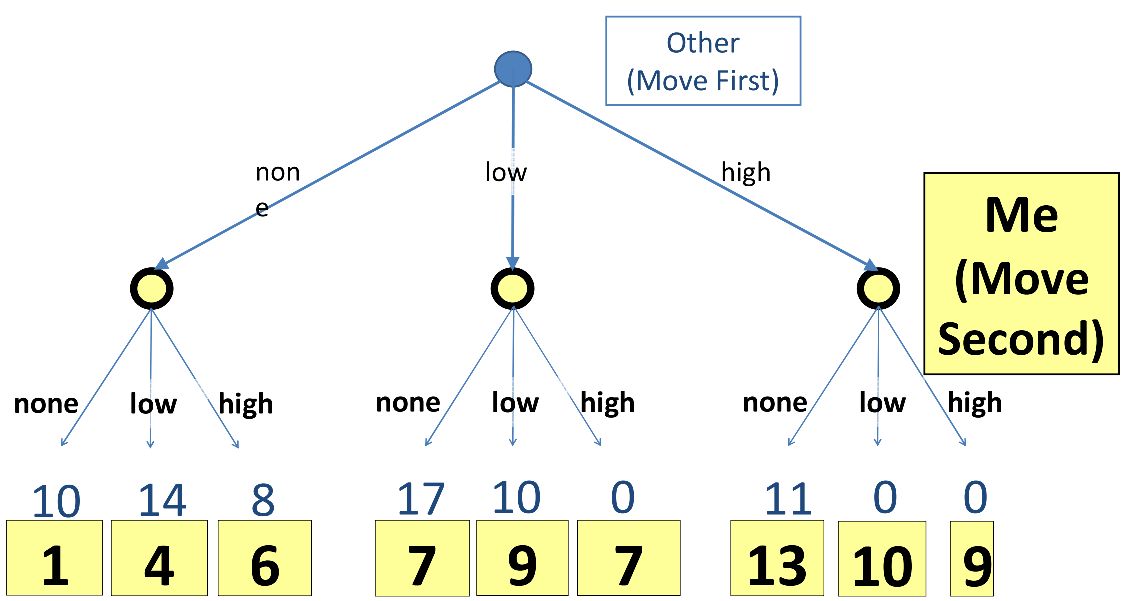
		Other					
		NONE	VERY LOW	LOW	MEDIUM	HIGH	VERY HIGH
Me	NONE	1 / 10	4 / 15	7 / 17	10 / 15	13 / 11	14 / 6
	VERY LOW	3 / 13	6 / 15	9 / 14	11 / 10	12 / 5	12 / 0
	LOW	5 / 13	7 / 13	9 / 10	10 / 5	10 / 0	11 / 0
	MEDIUM	6 / 11	8 / 9	9 / 5	9 / 0	9 / 0	10 / 0
	HIGH	6 / 8	7 / 4	7 / 0	8 / 0	9 / 0	9 / 0
	VERY HIGH	6 / 4	6 / 0	7 / 0	7 / 0	8 / 0	9 / 0

Base, 6x6, GOV]

[Base, Ext, SRO



Base, Ext, SRO]
[Base, Ext, GOV



Base, Ext, GOV]

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