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How (Not) To Decide: Procedural Games

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How (Not) To Decide: Procedural Games*

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Abstract

Psychologists and experimental economists find that people's behavior is shaped not only by outcomes but also by the procedures through which these outcomes are reached. Using Psychological Game Theory we develop a general framework allowing players to be motivated by procedural concerns. We present two areas in which procedural concerns play a key role. First, we apply our framework to policy experiments and show that if subjects exhibit procedural concerns, the way in which researchers allocate subjects into treatment and control groups influences the experimental results. The estimate of the treatment effect is always biased as compared to the effect of a general introduction of the treatment. In our second application we analyze the problem of appointing agents into jobs that differ in terms of their desirability. Because of procedural concerns the principal's choice of appointment procedure affects the subsequent effort choice of agents. We test this theoretical hypothesis in a field experiment. The results are consistent with our predictions.

Keywords: Procedural concerns, Psychological game theory, Policy experiments, Appointment procedures.
JEL Classification: A13, C70, C93, D63.

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1 Introduction

Among psychologists a broad consensus exists that the way in which decisions are made - and not only the expected outcomes of these decisions - shape human behavior. People make different choices in outcomewise-identical situations depending on the decision-making procedures which led to these situations [e.g. Thibaut and Walker (1975), Lind and Tyler (1988), Collie et al. (2002), Anderson and Otto (2003) and Blader and Tyler (2003)]. For example, reactions to promotion decisions, bonus allocations, and dismissals strongly depend on the perceived fairness of the selection/allocation procedures [e.g. Lemons and Jones (2001), Konovsky (2000), Bies and Tyler (1993), Lind et al. (2000) and Roberts and Markel (2001)]. Procedures seem to matter because they affect the beliefs that people hold about each others' intentions and expectations which subsequently influence their behavior.

The results of laboratory experiments in economics also indicate that people care about decision-making procedures [Blount (1995), Bolton et al. (2005), Charness (2004), Brandts et al. (2006), Charness and Levine (2007), Falk et al. (2008), Kircher et al. (2009)].[1] Brandts et al. (2006), for example, show that selection procedures matter in a three-player game in which one player has to select one of the other players to perform a specific task. They find that the selected player behaves differently in her subsequent task depending on the procedure which was used to select her.

Traditionally, economic theory assumes that agents only care about the consequences of decisions. Although this *consequentialism* allows that an agent cares about the payoffs of other players, e.g. that she is altruistic or envious, it inherently implies that people behave identically in outcomewise-identical situations, regardless of the decision-making procedures that led to these situations. Thus, consequentialism is at odds with the aforementioned evidence. As an example, consider the following principal-agent relation: a profit-maximizing principal has to assign two equally skilled agents to two different jobs. The first job, *controller*, is more desirable than the second, *typist*.[2] The principal first chooses between two possible procedures by which the typist is chosen: (i) she can directly allocate the task, or (ii) she can choose a verifiable appointment procedure giving both agents an equal chance to get either job (e.g. a publicly observable coin toss). After the tasks are allocated, the appointed typist chooses her effort. The appointment procedures (i) and (ii) differ with regard to the ex-ante probabilities that they attach to

[1]One of the first papers in economics to discuss procedural utility is Frey et al. (2004), which underlines the importance of institutions that affect the feeling of self-worth of individuals. Empirically, Frey and Stutzer (2005) find that regardless of the outcome, citizens enjoy higher subjective well-being from being able to participate in political decision-making processes. Our paper differs from this approach in one key aspect: in our framework, utility is belief-dependent and procedural concerns (i.e. economic behavior being affected by procedures) are an *outcome* of this belief dependence, whereas in Frey et al. (2004) the procedural concerns manifest themselves in the measures of subjective well-being.

[2]This terminology of *controller* and *typist* refers to the field experiment in section 4.

specific outcomes. Procedure (i) puts probability 1 on one of the agents. Procedure (ii), on the other hand, puts the ex-ante probability 0.5 on each of the agents. Obviously, if the typist cares only about final outcomes, her effort choice should be independent of the selection procedure.

However, the aforementioned evidence from psychology and controlled laboratory experiments suggests that the typist's effort is higher when the principal uses the unbiased random assignment procedure, i.e. the typist cares about the decision-making procedure. Sebald (2010) suggests that *procedural concerns* can be conceptualized by assuming that people have belief-dependent reciprocal preferences à la Dufwenberg and Kirchsteiger (2004), where agents care not only about final outcomes, but also about the principal's (un)kindness.[3] The typist's perception about the principal's (un)kindness towards her depends on the procedure that the principal uses to make the appointment decision. If the principal chooses the typist directly, the chosen agent interprets the principal's decision as intentionally directed against her. If, on the contrary, the principal uses a random appointment procedure, the agent interprets the outcome as pure chance rather than an intentional act of the principal. The chosen agent thus considers the principal's choice of the random appointment procedure as a 'kinder' one and subsequently exerts higher effort (as compared to the situation in which she is chosen to be the typist directly).

Sebald (2010) relies on a specific form of belief-dependent preferences, i.e. reciprocity. However, reciprocity is just one possible belief-dependent motivation. A lot of other types of belief-dependent emotions (e.g. regret, disappointment, guilt) that are important in real life have been studied in the economic literature. For example, Charness and Dufwenberg (2006) and Battigalli and Dufwenberg (2007) study the strategic interaction of agents that are guilt-averse. Ruffle (1999) presents a model in which surprise, disappointment, and embarrassment enter into the interaction of emotional agents.

To encompass all kinds of belief-dependent preferences, we first introduce a class of general procedural games in which agents

(i) are motivated by belief-dependent preferences in general, and

(ii) can choose between possibly stochastic decision-making procedures.

To formalize this, we rely on the model of dynamic psychological games of Battigalli and Dufwenberg (2009). Using their setting, we define decision-making procedures and provide an analytical framework for the impact of procedural choices on the interaction of agents that are motivated by belief-dependent preferences. Our framework emphasizes two important aspects of procedures and procedural concerns. First, procedures can be

[3] Several alternative approaches have been recently suggested to accommodate *procedural concerns*. All of them use models of other-regarding preferences. For example, Trautmann (2009) and Krawczyk (2007) extend models of inequality aversion by assuming that agents care ex post about the ex-ante probability of random processes. Borah (2010) also argues that people have preferences over procedures.

viewed as possibly stochastic decision-making mechanisms determining the ex-ante probabilities for situations in which agents can find themselves in ex-post. Second, procedural concerns mean that these ex-ante probabilities have an impact on agents' decisions even ex-post, that is after the resolution of the uncertainty inherent in decision-making procedures. This means that agents that exhibit procedural concerns are not consequentialist, i.e. for them 'bygones are not bygones'.

Our second contribution is the application of this framework to policy experiments widely used in empirical work in labor, public, and development economics. We demonstrate how the choice of the selection procedure which is used to allocate people into treatment and control groups might lead to biased predictions concerning the effectiveness of the policy treatment. Typically, policy experiments are used to evaluate ex-ante the effect of a general introduction of a governmental or NGO program on a particular social or economic outcome. The evidence from policy experiments outside economics (e.g. Schumacher et al. 1994) indicates that predictions concerning the effectiveness of the tested programs might be biased due to the existence of procedural concerns. In line with this, we theoretically show that the procedures which are used to allocate subjects into the treatment and control groups have an impact on the behavior of agents that are motivated by belief-dependent emotions. Furthermore, random selection (as normally used in policy experiments) leads to a biased estimation of the treatment effects.

Subsequently, we apply our framework with procedural choices and belief-dependent preferences to another area in which procedural concerns are important: human resource management. Formalizing the simple principal-agent model sketched above, we find that appointing the typist by explicit randomization induces higher effort from the agent than appointing her directly. We tested this prediction in a field experiment. For an ongoing data-building project we hired undergraduate students as research assistants and allocated them to two different jobs, typists and controllers. The typists' work consisted of inserting data, while that of controllers consisted of verifying the data inserted by the typists. The controllers' wage was 50% higher than that of the typists. The experiment had two treatments. In the first treatment, we allocated subjects to jobs directly (i.e. via hidden randomization), whereas in the second treatment, the allocation was randomized explicitly. Our field-experimental findings support our theoretical hypothesis: typists in the explicit-randomization treatment exerted more effort than their directly appointed counterparts. Furthermore, male typists under explicit-randomization allocation exerted higher effort both in terms of quantity (i.e. cells encoded) and quality (i.e. number of mistakes) . Female typists, on the other hand, exerted higher effort only in terms of quality. These findings relate to and complement the existing literature on gender differences in social preferences (see Croson and Gneezy 2009).

The organization of the paper is as follows. Section 2 presents our general framework with procedural choices and belief-dependent preferences, as well as a solution concept. In Section 3, we analyze the impact of procedural concerns on the validity of policy experiments. Section 4 presents the application to appointment procedures and our field experiment. Section 5 discusses the general implications of our findings, some avenues for future work, and concludes.

2 Procedural Games: A General Framework

In this section we define procedural games with belief-dependent preferences and a solution concept for this class of games. Intuitively, a procedural game is a game in which players do not choose actions but decision-making procedures which characterize the way in which actions are chosen. Technically, our class of procedural games is a special case of the class of dynamic psychological games with moves of chance defined by Battigalli and Dufwenberg (2009). Our framework allows

i) to highlight how moves of chance can be used to formalize decision-making procedures, and

ii) to isolate the impact of procedural choices on the strategic interaction of agents with belief-dependent preferences.

Let the set of players be $\mathcal{N} = \{1, ..., N\}$. Denote as $\mathcal{H}$ the finite set of histories h, with the empty sequence $h^0 \in \mathcal{H}$, and as Z the set of end-nodes. Histories $h \in \mathcal{H}$ are sequences that describe the choices that players have made on the path to history h. At each non-terminal history each player $i \in \mathcal{N}$ disposes of a nonempty, finite set of feasible actions $\mathcal{A}_{i,h}$ with $a_{i,h} \in \mathcal{A}_{i,h}$. $\mathcal{A}_{i,h}$ can be a singleton, meaning that player i is inactive at history h. Given this standard game form we can define procedural games.

This standard game form is transformed into the game form of a procedural game by the feature that at each non-terminal history $h \in \mathcal{H}$ every player $i \in \mathcal{N}$ does not choose an action $a_{i,h} \in \mathcal{A}_{i,h}$ directly. Rather, she chooses a decision-making procedure, which determines for every action $a_{i,h} \in \mathcal{A}_{i,h}$ the probability that $a_{i,h}$ is implemented. In other words, in a procedural game the players choose decision-making procedures that characterize the way in which decisions are made, rather than the decisions themselves, which are made by chance.

Definition 1 *A procedure for player $i \in \mathcal{N}$ in history $h \in \mathcal{H} \backslash Z$ is a tuple:*

$$\tau_{i,h} \equiv \langle \omega_{i,h}, \mathcal{A}_{i,h} \rangle ,$$

where $\omega_{i,h}$ is an explicit probability distribution defined on $\mathcal{A}_{i,h}$.

Analogously to the sets of actions, the sets of procedures $\mathcal{T}_{i,h}$ are exogenously given for each history and for each player.[4] $\mathcal{T}_{i,h}$ is assumed to be nonempty and finite, implying that not all probability distributions are feasible as procedures. The feasibility of procedures depends on the specific economic situation analyzed by using a procedural game. Note further that this framework allows for 'degenerate' probability distributions which attach probability 1 to a particular feasible action. Obviously, such a procedure is equivalent to choosing this action directly.

$\tau_i = (\tau_{i,h})_{h \in \mathcal{H} \setminus Z}$ denotes a pure procedural strategy of player i, and $\mathcal{T}_i = \prod_{i \in \mathcal{N}} \mathcal{T}_{i,h}$ his set of pure procedural strategies. $\tau = (\tau_i)_{i \in \mathcal{N}}$ denotes a procedural strategy profile, and $\mathcal{T} = \prod_{i \in \mathcal{N}} \mathcal{T}_i$ the set of procedural strategy profiles. Obviously, each feasible profile of procedural strategies τ induces a probability distribution over the set of endnodes Z.

As an example, in our introductory story the principal, p, first has to choose a procedure τ_{p,h^0} in the initial history h^0. She can choose between two types of procedures: she can decide herself by choosing a procedure that puts probability 1 on one of the agents or she can use a decision-making procedure giving each agent a verifiable chance of 50% to get either job. Given the principal's choice, the actual decision is made by chance. By allowing for moves of chance we can formalize strategic environments in which people have the possibility to choose between different decision-making procedures.

This concludes our definition of an extensive form in which players choose decision-making procedures. To formalize belief-dependent payoffs, we follow Battigalli and Dufwenberg (2009) and assume that in each history $h \in \mathcal{H}$ players $i \in \mathcal{N}$ hold conditional beliefs about the procedural strategies $\tau_j = (\tau_{j,h})_{h \in \mathcal{H} \setminus Z}$ played by the other players $j \in \mathcal{N}$ with $j \neq i$. Furthermore, players $i \in \mathcal{N}$ hold conditional beliefs about the beliefs that these other players hold about their procedural strategy $\tau_i = (\tau_{i,h})_{h \in \mathcal{H} \setminus Z}$, conditional beliefs about these other players' beliefs about their beliefs, etc. In other words, in every history $h \in \mathcal{H}$ players hold hierarchies of conditional beliefs that capture their beliefs about the procedural strategies and beliefs of all other players. We assume that these hierarchies of conditional beliefs are 'collectively coherent', meaning that (i) beliefs of different orders do not contradict each other, and (ii) players do not believe that others hold incoherent beliefs. Finally, we assume that, wherever possible, players update their beliefs according to the Bayes rule as play unfolds.[5] At the initial history h^0, players might not know the true profile of procedural strategies and the beliefs of their opponents. But at any later history h every player has updated her beliefs such

[4]We do not exclude the possibility that players use procedures to choose between procedures and procedures that choose between procedures that choose between procedures, etc. Procedures, $\tau_{i,h} \in \mathcal{T}_{i,h}$, rather have to be understood as *reduced procedures*. The explicit probability distribution associated with a reduced procedure subsumes the probability distributions of procedures of all levels into one explicit distribution indirectly defined on $\mathcal{A}_{i,h}$.

[5]For an explicit definition of *collectively coherent hierarchies of beliefs* see Battigalli and Dufwenberg (2009). For topological details, proofs, and further references see Brandenburger and Dekel (1993) and Battigalli and Siniscalchi (1999).

that she knows for sure which procedures have been chosen on the way to h, such that she knows for sure that all other players know for sure all the procedures on the way to h, etc. Note that the updating of beliefs refers to the procedures chosen, and not to the randomly determined outcome of the procedures. In the context of agents with belief-dependent preferences this implies that players do not hold themselves and others responsible for the outcomes of moves of chance. Players evaluate their and the others' responsibility for outcomes only on the basis of the procedural choices made. For example, in our principal-agent relation agents do not 'blame' the principal for any decision, if she makes the decision by using a procedure credibly giving each of them an equal chance to get either job. On the other hand, if the principal makes the decision directly, agents hold her responsible for the outcome and might, for example, reciprocate by choosing low effort in the subsequent period.

Denote the set of all possible *collectively coherent hierarchies of conditional beliefs* of player i by $\mathcal{M}_i$. The set of collectively coherent beliefs of the opponents $-i$ is $\mathcal{M}_{-i}$ and $\mathcal{M} = \prod_{j\in\mathcal{N}} \mathcal{M}_j$. A typical element of $\mathcal{M}$ is denoted by m.

Definition 2 *Player i exhibits belief-dependent utilities iff her preferences can be represented by*

$$u_i : \mathcal{T} \times \mathcal{M} \to \mathbb{R},$$

with $u_i(\tau, m)$ being i's utility from a procedural strategy profile τ when the conditional beliefs are given by m.

Battigalli and Dufwenberg (2009) adapt Kreps and Wilson's (1982) concept of sequential equilibrium to their class of dynamic psychological games with moves of chance. They do so by characterizing *consistent assessments* that do not only consist of first-, but also of higher-order beliefs and defining sequential equilibria as sequentially rational consistent assessments. Their equilibrium concept refers to randomized choices. However, following Aumann and Brandenburger (1995), they interpret player i's randomized choice as a conjecture on the part of her opponents as to what player i will do. They denote a behavioral procedural strategy of player i as $\sigma_i = (\sigma_{i,h})_{h\in\mathcal{H}} \in \Sigma_i$, where Σ_i is the set of all behavioral procedural strategies of player i. Note that the behavioral choice $\sigma_{i,h} \in \Sigma_i(h)$ in h has to be understood as an implicit randomization over the set of procedures $\mathcal{T}_{i,h}$ in history h and interpreted as an array of common conditional first-order beliefs held by i's opponents. In contrast to this, a procedural choice $\tau_{i,h}$ is an explicit/verifiable randomization commonly known to all players $i \in \mathcal{N}$. This means that the behavioral strategy σ_i is part of an assessment $(\sigma, \mu) = (\sigma_i, \mu_i)_{i\in\mathcal{N}}$ of behavioral procedural strategies σ_i and hierarchies of conditional beliefs μ_i. This assessment is a sequential equilibrium if it is consistent as defined by Battigalli and Dufwenberg (2009) and for all $i \in \mathcal{N}$, $h \in \mathcal{H}$, $\tau^*_{i,h} \in \mathcal{T}_{i,h}$ in the support of $\sigma_{i,h}$ it holds that

$$\tau^*_i \in argmax_{\tau_i\in\mathcal{T}_i(h)} E_{\tau_i,\mu} [u_i|h] \,,$$

where $E_{\tau_i,\mu}[u_i|h]$ is the expected utility of player i from choosing τ_i conditional on history h and given the system of hierarchies of conditional beliefs μ. Battigalli and Dufwenberg (2009) show that if utility functions u_i are continuous, a sequential equilibrium exists.

In the next sections we use our class of procedural games and the solution concept to analyze the impact of procedural choices on the strategic interaction of agents with belief-dependent preferences in specific contexts.

3 Policy Experiments and Selection Procedures

Policy experiments have become a standard tool for applied economists in labor, development, and public economics. Researchers use policy experiments to evaluate, for instance, the effect of conditional cash transfers to poor families on education and health outcomes of children [in Mexico, see Schultz (2004), Gertler (2004)], the effect of vouchers for private schooling [in Colombia, see Angrist et al. (2002), Angrist et al. (2006)], the effect of publicly released audits on electoral outcomes [in Brazil, see Ferraz and Finan (2008)], the effect of incremental cash investments on the profitability of small enterprises [in Sri Lanka, see De Mel et al. (2008)], the effect of income subsidies on work incentives [in Canada, see Michalopoulos et al. (2005), Card and Robins (2005), Card and Hyslop (2005)], and the effect of saving incentives on the saving decisions of low- and middle-income families [in the United States, see Duflo et al. (2006)]. Many applied economists consider policy experiments as 'the only clean way of identifying impact, as it appears to avoid untestable identifying assumptions based on economic theory or other sources. They view non-experimental methods as (by and large) unscientific and best avoided' (Ravallion 2009).

Typically, such experiments are used for ex-ante program evaluation purposes. To evaluate ex ante the effect of a general introduction of government or developmental (NGO) program on some social or economic outcome, researchers allocate individuals (or other units under study, such as schools or villages) into a treatment and a control group. The individuals in the treatment group receive the policy 'treatment' and then their behavior is compared to that of the individuals in the control group. The observed difference between the outcomes in the treatment and the control group is used as a predictor for the effect of a general introduction of the program. Based on the experimental results, the program might be generally adopted or not.[6]

The validity of the outcomes of policy experiments depends on two crucial assumptions. First, the treatment and control groups must not differ from the general population for which the program is designed. Second, the selection into the treatment and the control groups should have no impact on the behavior of the participants of the experiment.

[6]See e.g. Duflo (2004) for a description of the randomized trial and the subsequent general implementation of PROGRESA, a Mexican program of monetary incentives for school attendance.

To guarantee the validity of the first assumption, the selection into the treatment and the control groups is typically done randomly. Sometimes, the experimental administrators use an explicit randomization procedure, i.e. a public lottery [see e.g. Ferraz and Finan (2008)]. However, in most instances they carry out the randomization privately, i.e. 'behind closed doors', with subjects in both groups often being aware that a treatment and a control group exists [see e.g. Angrist et al. (2002) and De Mel et al. (2008)]. The proponents of such experiments claim that because individuals are selected into the two groups randomly, any bias in estimating the effect of the program that can occur in non-experimental studies is eliminated, as the individuals in treatment and control groups are comparable in every respect except the treatment (the so-called internal validity).

However, the selection procedure *itself* can have an impact on the behavior of the agents participating in the experiment. When agents are motivated by procedural concerns, how people are allocated into the treatment and control group matters for the behavior of subjects in the experiments and, hence, for the empirical findings. The credibility of random selection procedures might influence people's perceptions of gratitude and privilege, if they are selected into the treatment group, and their feelings of demoralization and resentment, when they are allocated into the control group. These feelings then might have an impact on people's subsequent behavior.

These behavioral effects are not just hypothetical. A selection-induced change in the behavior of the control group is well-known in psychology under the heading 'resentful demoralization'.[7] Take as an example the Baltimore Options Program [Friedlander et al. (1985)], which was designed to increase the human capital and, hence, the employment possibilities of unemployed young welfare recipients in the Baltimore Country. Half of the potential recipients were randomly assigned to the treatment group and half to the control group. The treatment group individuals received tutoring and job search training for one year. The control group members, aware of not having received the (desirable) treatment, became discouraged and thus performed worse in the outcome measure than they would have performed if the treatment group did not exist. This bias leads to an overestimation of the effectiveness of the program. In fact, researchers found that the earnings of the treatment group increased by 16 percent, but that the overall welfare claims of program participants did not decrease [Friedlander et al. (1985)]. This implies that some of the control-group individuals that would have normally moved out of welfare stayed longer on welfare because of the experiment.

Another example where this demoralization effect played a key role is the Birmingham Homeless Project (Schumacher et al. 1994), aimed at the homeless drug-takers in Birmingham, Alabama. The randomly selected subjects of the treatment group received more frequent and therapeutically superior treatment, as compared to those in the control group. Schumacher et al. (1994) note that '11 percent increase in cocaine relapse rates for usual care clients [i.e. the control group] was revealed' (p. 42). They conclude,

[7] It was first described in detail by Cook and Campbell (1979). See Onghena (2009) for a short survey.

'demoralization represented a potential threat to the validity of this study [...] If the worsening of the usual care clients [control group] from baseline to the two-month follow-up point was related to demoralization, there exists a potential for an overestimation of treatment effects of the enhanced care program' (p. 43-44).

Given this evidence, one wonders whether any selection procedure used in a policy experiment can avoid these behavioral biases. The simple model that we develop below addresses this question.

3.1 A simple model of policy experiments

Consider a policy treatment that entails some benefits (in-cash or in-kind). Denote the overall population of agents (e.g. children in low-income families) by N. n of these agents are subject to the treatment, and $q = \frac{n}{N} \in [0, 1]$ denotes the fraction of agents getting the treatment. To concentrate on the impact of the selection procedures, we abstract from any idiosyncratic differences between the agents. Thus, all agents are identical except for their treatment status. We assume that the experimenter can choose between two procedures for selecting individuals into the treatment and the control group: (i) She can select the n treatment agents directly. This also models a closed-doors random selection procedure, when the agents do not believe in the randomness of the selection. (ii) The experimenter can choose an explicit randomization procedure observable to the agents, such that each agent has the same probability q of receiving the treatment. This also models a closed-doors random selection procedure, when the subjects to not doubt the randomness of the selection. Since we are interested in the impact of the selection procedure, we will not analyze the experimenter's equilibrium choice as if she were a player. Rather, we will compare the reaction of the agents to the two selection procedures.

Formally, any subset of the overall population with n agents is a feasible action of the experimenter. The set of feasible procedures is given by all degenerate probability distributions that choose an action for sure (i.e. direct appointment of the n treatment agents), and by the procedure that gives equal probability to every feasible action (i.e. the experimenter chooses the n treatment agents with the help of a public lottery). Note that since all agents are equivalent, all these 'degenerate' procedures where the treatment agents are picked directly induce the same choices of the 'treated' as well as of the 'untreated' agents. Therefore, we restrict the analysis to a typical element of this class of procedures, denoted by d. Denoting the public randomization procedure by r, experimenter's set of procedures is given by $P = \{d, r\}$ with p denoting a typical element of this set. Upon selection, the selected agents receive the treatment, whereas the other individuals do not receive it.

Next, all agents choose simultaneously an effort level[8] $e \in [0, 1]$.

[8] For our general framework we have assumed that the action spaces are finite. For expositional ease,

The success of the program depends not only on the treatment itself, but also on the effort level of the agents. Let us assume that the marginal success of effort is constant, and denote it with t. For analytical simplicity, we assume that $t = 1$ for agents that receive the treatment and $t = \frac{1}{2}$ for the other agents. Thus, the treatment makes it easier for participants to be successful. We use the variable $t \in \{\frac{1}{2}, 1\}$ to denote also whether an agent is in the control group ($t = \frac{1}{2}$) or in the treatment group ($t = 1$). We denote with (t, p) the *type* of the agent who is put into group t by the selection procedure p. We restrict our attention to symmetric equilibria where all agents of the same type (t, p) choose the same effort level $e(t, p)$. Together with the (lack of) the treatment, this effort determines the success of an agent with respect to, for example, finding a job or stopping drug consumption. Formally, the success of a (t, p)-agent is given by

$$s = te(t, p). \tag{1}$$

As already mentioned, we do not analyze the experimenter's equilibrium choice as if she were a player. However, to determine the reaction of the agents to the selection procedure, we have to specify the goal of the experimenter *as perceived by the agents*. In almost every policy experiment, the subjects do not know that the goal of the researcher is to evaluate the effectiveness of the policy intervention by comparing the outcomes of the treatment and control groups[9]. It is thus reasonable to assume that subjects consider the overall success, denoted by π_x, as the goal of the experimenter. It depends on the effort levels chosen by the agents (which, in turn, depends on the selection procedure), and on the group sizes:

$$\pi_x = ne(1, p) + (N - n)\frac{1}{2}e(\frac{1}{2}, p). \tag{2}$$

We assume that the agents are motivated by their individual success: unemployed want to find a job, the drug users want to get clean, etc. Furthermore, each agent has to bear the cost of effort, which we assume to be quadratic. Disregarding any psychological payoff, a (t, p)-agent's direct (or 'material') payoff is

$$\pi(t, e(t, p)) = te(t, p) - e(t, p)^2. \tag{3}$$

However, as we argue above, agents care not only about their material but also about their psychological payoffs. The psychological payoff arises from belief-dependent

the set of feasible effort levels are continua in the applications. While this is slightly contradictory, all the results of the applications can be also generated with finite sets of feasible effort levels, provided that equilibrium effort levels of the continuous model are also feasible in the model with finite feasible effort levels.

[9] If the agents would know that the effectiveness of the program is tested and that the experimental results determine the long-run feasibility and shape of the program, the agents' long-term strategic interests would jeopardize the validity of the experimental results. To give the randomized experiments the best shot, we abstract from such effects by assuming that the agents are unaware of the experimental character of the program.

psychological motives such as reciprocity, encouragement, or resentment. If an agent feels treated badly (via the selection procedure), she may resent the experimenter, feel discouraged, and hence, be less willing to provide effort (as compared to the selection procedure under which she would not feel treated badly). On the other hand, if the agent feels treated particularly well, she might feel encouraged, may want the program to be a success, and hence provide higher effort. Crucially, whether the agent feels treated well or badly depends on how much material payoff she *thinks* that the experimenter *intends* to give her relative to a 'neutral' material payoff.

To model these belief-dependent psychological payoffs, we need to introduce first- and second-order beliefs into the utility functions. For any t, t' and p, p', denote by $\overline{e}^{t,p}(t', p')$ the *first-order belief* of a (t, p)-agent about the effort choice of a (t', p')-agent. $\overline{e}^{t,p}(t, p)$ is the belief of a (t, p)-agent about the effort choice of the other agents of her own type. The first-order beliefs of a (t, p)-agent are summarized by

$$\overline{e}^{t,p} = (\overline{e}^{t,p}(1, d), \overline{e}^{t,p}(\frac{1}{2}, d), \overline{e}^{t,p}(1, r), \overline{e}^{t,p}(\frac{1}{2}, r)).$$

$\overline{\overline{e}}^{t,p}(t', p')$ denotes the *second-order belief* of a (t, p)-agent about the experimenter's belief about the effort choice of a (t', p'). The second-order beliefs of a (t, p)-agent are then summarized by

$$\overline{\overline{e}}^{t,p} = (\overline{\overline{e}}^{t,p}(1, d), \overline{\overline{e}}^{t,p}(\frac{1}{2}, d), \overline{\overline{e}}^{t,p}(1, r), \overline{\overline{e}}^{t,p}(\frac{1}{2}, r)).$$

Denote by $\pi_x(e(t, p), \overline{e}^{t,p})$ the level of success of the program that a (t, p)-agent intends for the program if she chooses $e(t, p)$ and she believes that the others choose $\overline{e}^{t,p}$. It is given by

$$\pi_x(e(t, p), \overline{e}^{t,p}) = \begin{cases} e(1, p) + (n-1)\overline{e}^{1,p}(1, p) + (N-n)\frac{1}{2}\overline{e}^{1,p}(\frac{1}{2}, p) & \text{if } t = 1 \\ \frac{1}{2}e(\frac{1}{2}, p) + n\overline{e}^{\frac{1}{2},p}(1, p) + (N-n-1)\frac{1}{2}\overline{e}^{\frac{1}{2},p}(\frac{1}{2}, p) & \text{if } t = \frac{1}{2} \end{cases} \tag{4}$$

Note that $\pi_x(e(t, p), \overline{e}^{t,p})$ does not depend on the actual effort of the other agents, but on the agent's belief about the other agents' effort. Any change of $e(t, p)$ does not change what the particular (t, p)-agent thinks the other agents will contribute to the overall success. This is reflected by $\frac{\partial \pi_x(e(t,p), \overline{e}^{t,p})}{\partial e(t,p)} = t$.

$\pi(\overline{\overline{e}}^{t,p})$ denotes the belief of a (t, p)-agent about the expected material payoff the experimenter intends to give her. The agent does not hold the experimenter responsible for the outcome of the move of chance associated with her procedural choice. Hence, $\pi(\overline{\overline{e}}^{t,p})$ is given by

$$\pi(\overline{\overline{e}}^{t,p}) = \begin{cases} q(\overline{\overline{e}}^{t,r}(1, r) - \overline{\overline{e}}^{t,r}(1, r)^2) + (1-q)(\frac{1}{2}\overline{\overline{e}}^{t,r}(\frac{1}{2}, r) - \overline{\overline{e}}^{t,r}(\frac{1}{2}, r)^2) & \text{if } p = r \\ t\overline{\overline{e}}^{t,d}(t, d) - \overline{\overline{e}}^{t,d}(t, d)^2 & \text{if } p = d \end{cases} \tag{5}$$

Note that $\pi(\overline{\overline{e}}^{1,r}) = \pi(\overline{\overline{e}}^{\frac{1}{2},r})$ whenever $\overline{\overline{e}}^{1,r} = \overline{\overline{e}}^{\frac{1}{2},r}$. When the public randomization procedure is used and the agent's second-order beliefs are independent of her group t, the agent's beliefs about the payoff that the experimenter intends to give to the agent are not influenced by t. Furthermore, $\pi(\overline{\overline{e}}^{t,p}) \in [-\frac{1}{2}, \frac{1}{4}]$ since $e \in [0,1]$.

We also have to specify the 'neutral' payoff $\widehat{\pi}$ that the experimenter has to intend for an agent for inducing the agent to regard the selection procedure as being neutral, i.e. neither favoring nor discriminating against the agent.[10] Below we will specify the psychological payoff in such a way that whenever the agent thinks that the experimenter intends to give her $\widehat{\pi}$ she simply maximizes her material payoff. Note that the expected material payoff of an agent is maximized when she is directly assigned to the treatment group. It is minimized when the agent is directly assigned to the control group. Therefore, we assume that $\widehat{\pi}$ as a weighted average between the payoff that the agent thinks that the experimenter intends to give to someone directly selected into the treatment group and the intended material payoff for an agent directly selected into the control group. The weights are denoted by λ and $1-\lambda$, respectively, with $\lambda \in [0,1]$:

$$\widehat{\pi}(\overline{\overline{e}}^{t,p}) = \lambda(\overline{\overline{e}}^{t,p}(1,d) - \overline{\overline{e}}^{t,p}(1,d)^2) + (1-\lambda)(\frac{1}{2}\overline{\overline{e}}^{t,p}(\frac{1}{2},d) - \overline{\overline{e}}^{t,p}(\frac{1}{2},d)^2), \tag{6}$$

with $\widehat{\pi}(\overline{\overline{e}}^{t,p}) \in [-\frac{1}{2}, \frac{1}{4}]$ since $e \in [0,1]$.

The weight λ depends on the fraction of agents that are subject to the treatment, q. Whenever an experiment is conducted, i.e. if $q \in (0,1)$, the agents take the existence of both groups into account, i.e. $\lambda \in (0,1)$. In the extreme cases when nobody (everybody) is subject to the treatment, i.e. when $q = 0$ ($q = 1$), the agents are aware of it, i.e. $\lambda = 0$ ($\lambda = 1$). Moreover, for $q \in (0,1)$ it seems natural to assume that $\lambda = q$. However, it is well-known that people's perception about what they deserve is often self-serving. For instance, most people regard themselves as being more talented than the average (the so-called 'Lake Wobegon effect'; see Hoorens 1993). Therefore, most individuals in the policy program might think that they deserve the treatment more than the others. This 'superiority bias' would imply $\lambda > q$. On the other hand, we also allow for an 'inferiority bias', i.e. $\lambda < q$.

The psychological payoff is such that the higher the payoff $\pi(\overline{\overline{e}}^{t,p})$ that the agent believes the experimenter intends to give her (as compared to the neutral payoff $\widehat{\pi}(\overline{\overline{e}}^{t,p})$), the more the agent wants the program to be successful (and the less she is subject to resentful demoralization). Denoting with $v(\pi_x(e(t,p), \overline{e}^{t,p}), \pi(\overline{\overline{e}}^{t,p}), \widehat{\pi}(\overline{\overline{e}}^{t,p}))$ the psychological payoff in the agent's utility, a simple way to capture these motives is by assuming that

$$\frac{\partial v(\pi_x(e(t,p), \overline{e}^{t,p}), \pi(\overline{\overline{e}}^{t,p}), \widehat{\pi}(\overline{\overline{e}}^{t,p}))}{\partial \pi_x} = \pi(\overline{\overline{e}}^{t,p}) - \widehat{\pi}(\overline{\overline{e}}^{t,p}). \tag{7}$$

[10] $\widehat{\pi}$ plays a role similar to the 'equitable' payoffs in Dufwenberg and Kirchsteiger (2004).

For simplicity, we denote $\frac{\partial v(\pi_x(e(t,p),\overline{e}^{t,p}),\pi(\overline{\overline{e}}^{t,p}),\widehat{\pi}(\overline{\overline{e}}^{t,p}))}{\partial \pi_x}$ by $v_{\pi x}^{t,p}$. Since $\pi(\overline{\overline{e}}^{t,p})$ and $\widehat{\pi}(\overline{\overline{e}}^{t,p}) \in [-\frac{1}{2}, \frac{1}{4}]$, $v_{\pi x}^{t,p} \in [-\frac{3}{4}, \frac{3}{4}]$.[11]

The overall utility of a (t,p)-agent is the sum of the material and the psychological payoffs:

$$u^{t,p}(e(t,p), \overline{e}^{t,p}, \overline{\overline{e}}^{t,p}) = te(t,p) - e(t,p)^2 + v(\pi_x(e(t,p), \overline{e}^{t,p}), \pi(\overline{\overline{e}}^{t,p}), \widehat{\pi}(\overline{\overline{e}}^{t,p})). \qquad (8)$$

Denote with $e^*(t,p)$ the equilibrium effort level of a (t,p)-agent. There exists an equilibrium in pure strategies of the agents.

Proposition 1 *The game exhibits a sequential equilibrium in pure strategies. The equilibrium effort levels are in the interior, i.e.* $0 < e^*(t,p) < 1$ *for all* t, p.

Proof: See Appendix

Next we show that the effort levels of agents in both groups depend on whether the treatment agents are chosen directly or by public randomization.

Proposition 2 *For any* $q \in (0,1)$:

$$e^*(1,d) > e^*(1,r) > e^*(\frac{1}{2}, r) > e^*(\frac{1}{2}, d).$$

Proof: See Appendix

In a policy experiment the treatment-induced differences in effort between the two groups are larger when the allocation into the two groups is done directly than when it is done using public randomization procedure. The effort is highest among directly selected members of the treatment group and lowest among members of the directly selected control group. The effort levels of randomly selected agents is less extreme, with the effort of treatment-group agents still being higher than that of control-group agents. This shows that the randomization procedure has an impact on the observed effectiveness of the treatment. On the one hand, agents feel more privileged if they feel deliberately chosen to get the treatment. On the other hand, agents are more discouraged when they feel deliberately selected into the control group. This result holds for any fraction of people that are allocated into the treatment group $q \in (0,1)$.

The previous proposition shows that randomization procedures have an impact on the behavior of agents in policy experiments. The key question then is: which procedure provides a correct prediction of the effect of a general introduction (scale-up) of the treatment, and under which circumstances does this occur?

[11] For $\lambda = \frac{1}{2}$ this specification of the psychological payoff is equivalent to the psychological payoff of the reciprocity models of Rabin (1993) or Dufwenberg and Kirchsteiger (2004).

In our setting, the effect of the program scale-up to the entire population is the difference between the effort level of agents in the situation when the treatment is applied to everyone and the effort in the situation when the treatment is applied to nobody, i.e. between $q = 1$ and $q = 0$. We need to compare this difference to the difference in effort levels between agents in the treatment and control groups, under the two randomization procedures.

Proposition 3 *i) If the treatment is applied to everybody, i.e. if $q = 1$, then $e^*(1, d) = e^*(1, r) = \frac{1}{2}$. ii) If the treatment is applied to nobody, i.e if $q = 0$, then $e^*(\frac{1}{2}, d) = e^*(\frac{1}{2}, r) = \frac{1}{4}$.*

Proof: See Appendix

Proposition 3 shows that if nobody or everybody is selected, the 'selection procedure' does not affect the effort and the effort chosen by an agent is as if she were motivated only by her material payoff.

Direct selection *always* leads to an overestimation of the impact of the treatment as the following proposition shows.

Proposition 4 *For any $q \in (0, 1)$,*

$$e^*(1, d) > \frac{1}{2} \text{ and } e^*(\frac{1}{2}, d) < \frac{1}{4}.$$

Proof: See Appendix

With direct selection, the effort level of the control group is always smaller than the effort level realized when the entire population does not receive the treatment. The effort level of the treatment group is always larger than the one realized when the entire population receives the treatment. Therefore, any estimate of the effect of a general introduction of the treatment based on an experiment with direct appointment (or with private randomization) is biased upwards. A policy-maker scaling up the program on the basis of such a randomized evaluation faces the risk of introducing a non-effective program to the entire population.

One might hope that with an explicit randomization procedure the treatment-induced differential effort in the policy experiment is the same as the one induced by a general introduction of the treatment. However, as shown in the following proposition, this holds only for knife-edge special cases.

Proposition 5 *i) For any $\lambda \in (0, 1)$, there exists at most one q such that $e^*(1, r) - e^*(\frac{1}{2}, r) = e_1^* - e_0^* = \frac{1}{4}$. ii) If $\lambda = q \in (0, 1)$, $e^*(1, r) - e^*(\frac{1}{2}, r) \neq \frac{1}{4}$.*

Proof: See Appendix

Explicit randomization does not solve the problem of the bias in the estimate of the treatment impact under a general introduction of the treatment. Remember that in terms of the material payoffs the agent is best off when she is directly selected into the treatment group, and worst off when she is directly selected into the control group. There is no reason why the resulting neutral payoff should equal the expected material payoff of an agent subject to explicit randomization. Hence, even under explicit randomization the experimental results do not reflect the true benefits of a general introduction of the treatment. This is true even for the natural case of $\lambda = q$ (i.e. when agents do not suffer from any self-serving bias).

Moreover, while policy experiments with private randomization always lead to an over-estimate of the true benefits of a treatment, in policy experiments with public randomization, the estimate from the experiment can be either bigger or smaller than the true effect. Consider a numerical example. Let $\lambda = \frac{1}{2}$ and $q = \frac{1}{4}$. Using the first-order conditions (17), (18), (19), and (20), one obtains $e^*(1, r) = 0.49261$, $e^*(\frac{1}{2}, r) = 0.246305$, implying that $e^*(1, r) - e^*(\frac{1}{2}, r) < \frac{1}{4}$, an under-estimate of the true effect. However, letting $\lambda = q = \frac{1}{2}$, one obtains $e^*(1, r) = 0.5012$, $e^*(\frac{1}{2}, r) = 0.2506$, implying that $e^*(1, r) - e^*(\frac{1}{2}, r) > \frac{1}{4}$, an over-estimate of the true effect.

Our results show that procedural concerns can severely compromise the usefulness of policy experiments because the randomization procedures used to allocate agents into treatment and control groups influence the empirical results obtained from the experiments. Compared to the effect of a general introduction of the treatment, private selection leads to an over-estimation of the treatment effect when the agents doubt that the selection is done randomly (even when selection *is* random). If an explicit randomization mechanism is used (or if the agents believe that the selection 'behind closed doors' is truly random), the over-estimation problem is reduced. However, there is no *a-priori* guarantee that the effect observed in the experiment coincides with the effect of a general introduction of the treatment. In this case the experimental effect might even underestimate the 'true' effect of a general introduction of the treatment. Thus, when public randomization is used, even the sign of the bias is unclear.

These negative results should not come as a surprise considering that in other sciences researchers put a lot of effort into trying to minimize the impact of the selection procedure. For instance, in medicine researchers use placebos to make sure that none of the participants knows whether she is subject to the treatment or not. Of course, economics and medicine differ fundamentally in that deliberate decisions are crucial for the former, while largely unimportant for the latter. However, this difference suggests that the unawareness of whether one receives the treatment or not would be even more important for economic experiments than for medical ones.

4 Appointment Procedures

Procedural concerns seem to have a pervasive influence also in human resource management. Researchers have found that people's reactions to promotion decisions, bonus allocations, and dismissals strongly depend on the perceived fairness of the selection/allocation procedures [e.g. Lemons and Jones (2001), Konovsky (2000), Bies and Tyler (1993), Lind et al. (2000), and Roberts and Markel (2001)]. More specifically, people seem to be demotivated less after failing to be promoted when the promotion procedure is unbiased, i.e. when it gives them a fair chance to be promoted. Analogously, people react less negatively to failing to get a bonus or to being laid off, when the process leading to this decision gives them a fair chance. As we show, these procedures matter because they affect the beliefs that people hold about each others' intentions and expectations which subsequently influence their behavior.

In reality, many promotions are based on merit on effort. At the first glance, it seems as if such promotion procedures are not covered by our framework, where the procedures are only characterized by the promotion probabilities. But the acceptability of a merit- or effort-based decision procedure depends crucially on the ex-ante promotion probability before the work effort is actually provided. If merit or effort is measured such that it is ex ante clear that only one of the suitable candidates will 'win' the contest, the unpromoted will be demotivated. If, on the other hand, the definition of merit implies that ex ante each of the suitable candidates has a fair chance of winning the contest, even the losing candidates will regard the procedure as acceptable. So the perceived fairness of such merit- or effort- based promotion procedures depends on the ex-ante probabilities of getting promoted.

In the following we present a principal-agent setting in which a principal has to allocate two different jobs that differ in terms of their desirability. We theoretically show that the agent allocated to the less desirable job works harder subsequently, i.e. is less demotivated, if she was picked by an unbiased appointment procedure than if she was allocated the less desirable job directly. Next, we test this theoretical hypothesis in a field experiment.

4.1 A simple model of appointment procedures

Consider a two-stage game with a principal and two agents, i and j, which are ex-ante identical. The principal has two jobs to allocate: a good job with high earnings and a bad job with lower earnings. To make the link with our field-experimental test more explicit, let's call the good job 'controller' and the bad one 'typist'. The principal has to fill both jobs, thus she can appoint agent i as typist (and hence agent j as controller), or the other way round. To carry out the appointment, the principal can choose between the following appointment procedures: a direct appointment of i as typist (procedure d_i), a direct appointment of j as typist (procedure d_j), and a random appointment procedure where both agents have an equal chance of serving as typist (procedure r).

After the tasks are allocated, the appointed typist chooses her effort, $e \in [0, 1]$. Effort is costly, with a (continuous and twice-differentiable) effort cost function $c(e) \geq 0$ such that $c' > 0$, $c'' > 0$, $c'(0) = -\infty$, and $c'(1) = \infty$. Effort costs are independent of the identity of the typist. We assume that the typist provides at least some effort voluntarily: the cost of effort is minimized at some $\widetilde{e}$ with $0 < \widetilde{e} < 1$.

Given that we concentrate on the impact of the appointment procedure on the effort of the typist, we do not model the effort choice of the controller. The revenue of the principal is assumed to be equal to output, which is linear in effort of the typist. Thus, the profit of the principal is given by

$$\pi_p = e - w_c - w_t,$$

where w_c and w_t denote the wages of the controller and the typist. The principal is a pure profit maximizer.

Since the controller does not provide any effort, her material payoff is w_c. The typist gets a wage lower than the controller, i.e. $w_t < w_c$. We assume that the wage difference is larger than the largest possible effort cost difference, i.e. $w_c - w_t > \max_e \left(c(e) - c(\widetilde{e})\right)$.

Agent i becomes a typist and has to choose her effort level whenever either the principal has chosen d_i, or the principal has chosen r and chance has appointed i as typist. Denote by $e_i(s)$, $s \in \{d_i, r\}$, the effort choice of agent i if the principal chooses s and if chance appoints i in case of r. Disregarding any psychological payoff, agent $i's$ material payoff as a typist is given by

$$\pi_i(s, e_i(s)) = w_t - c(e_i(s)).$$

We assume that the agents cannot quit their jobs, which is equivalent to assuming that their outside options are sufficiently low.

As in the previous sections, we assume that agents care not only about their material payoffs, but are also motivated by belief-dependent psychological payoffs. If an agent feels treated badly, this triggers negative feelings in her, and thus she is demotivated to increase the principal's profit. On the other hand, if she feels treated well, she is motivated to increase the principal's payoff. Similar to the previous section, we first have to specify the profit $\overline{\pi}_p$ that typist i appointed by procedure s intends to give to the principal by choosing $e_i(s)$. It is given by

$$\pi_p(e_i(s)) = \begin{cases} e_i(r) - w_c - w_t & \text{if } s = r \text{ and chance has chosen } i \\ e_i(d_i) - w_c - w_t & \text{if } s = d_i. \end{cases}$$

This implies that $\frac{\partial \pi_p(e_i(s))}{\partial e_i(s)} = 1$.

We also have to specify the belief of typist i about the material payoff the principal intends to give her. To derive this belief, one needs the second-order beliefs of agent i about the principal's belief about agent i's effort choice when the principal chooses s, $s \in \{d_i, r\}$, and when chance appoints i in case of r. Let's denote this second-order belief by $\overline{\overline{e}}_i(s)$. The agent does not hold the principal responsible for chance moves. Hence, $\pi_i(\overline{\overline{e}}_i(s))$ is given by

$$\pi_i(\overline{\overline{e}}_i(s)) = \begin{cases} \frac{1}{2}w_c + \frac{1}{2}(w_t - c(\overline{\overline{e}}_i(r)) & \text{if } s = r \\ w_t - c(\overline{\overline{e}}_i(d_i)) & \text{if } s = d_i. \end{cases}$$

Since $w_c - w_t > \max_e (c(e) - c(\widetilde{e}))$, $\pi_i(\overline{\overline{e}}_i(r)) > \pi_i(\overline{\overline{e}}_i(d_i))$ for any $\overline{\overline{e}}_i(r)$, $\overline{\overline{e}}_i(d_i)$.

Finally, we specify a 'neutral' material payoff $\widehat{\pi}$ under which the agent is neither happy nor unhappy if the agent thinks that principal intends to give her $\widehat{\pi}$. Similar to $\pi_i(\overline{\overline{e}}_i(s))$, $\widehat{\pi}$ also depends on the second-order beliefs. Since $w_c - w_t > \max_e (c(e) - c(\widetilde{e}))$, the expected material payoff of an agent is maximized when she is directly chosen to be the controller (in that case, it equals w_c). The minimum expected payoff is realized when the agent is directly chosen to be the typist. As in the previous section, $\widehat{\pi}$ is assumed to be a weighted average between the minimum and maximum expected payoffs. Again, the weights are denoted by λ and $1 - \lambda$, respectively, with $\lambda \in (0, 1)$. Then,

$$\widehat{\pi}(\overline{\overline{e}}_i(d)) = \lambda w_c + (1 - \lambda)(w_t - c(\overline{\overline{e}}_i(d))). \tag{9}$$

In this application, it seems natural to assume that $\lambda = \frac{1}{2}$. However, our model can easily accommodate the case of a 'superiority bias' in which an agent (wrongly) believes that she deserves the controller's position more than the other agent ($\lambda > \frac{1}{2}$), as well as the case of an inferiority bias ($\lambda < \frac{1}{2}$).

The more the agent believes that the principal intends to give her $\pi_i(\overline{\overline{e}}_i(s))$ vis-à-vis the agent's neutral payoff $\widehat{\pi}(\overline{\overline{e}}_i(d))$, the more it is likely that the agent wants to reciprocate by increasing the principal's payoff in return. Denoting by $v(\pi_p(e_i(s)), \pi_i(\overline{\overline{e}}_i(s)), \widehat{\pi}(\overline{\overline{e}}_i(d)))$ the psychological part of the agent's utility, a simple way of capturing this idea is to assume that

$$\frac{\partial v(\pi_p(e_i(s)), \pi_i(\overline{\overline{e}}_i(s)), \widehat{\pi}(\overline{\overline{e}}_i(d)))}{\partial \pi_p} = \pi_i(\overline{\overline{e}}_i(s)) - \widehat{\pi}(\overline{\overline{e}}_i(d)).$$

From now on, $v^s_{\pi p}$ denotes $\frac{\partial v(\pi_p(e_i(s)), \pi_i(\overline{\overline{e}}_i(s)), \widehat{\pi}(\overline{\overline{e}}_i(d)))}{\partial \pi_p}$.[12]

As in the previous section, we assume that the overall utility of a typist i selected by procedure s is the sum of her material and psychological payoffs:

$$u_i(s, e_i(s), \overline{\overline{e}}_i(s)) = w_t - c(e_i(s)) + v(\pi_p(e_i(s)), \pi_i(\overline{\overline{e}}_i(s)), \widehat{\pi}(\overline{\overline{e}}_i(d))).$$

Using this type of psychological motivation, we get the following result:

[12] For $\lambda = \frac{1}{2}$, this specification of the psychological payoff is equivalent to the psychological payoff of the reciprocity models of Rabin (1993) and Dufwenberg and Kirchsteiger (2009).

Proposition 6 *In any sequential psychological equilibrium,* $e_i(r) > e_i(d_i)$*.*

Proof: See Appendix

We have thus shown that whenever agents have belief-dependent preferences as defined above, the typist picked by a random mechanism works harder than the one appointed directly. The intuition is straightforward: If picked by the explicit random mechanism, the agent attributes 'better' intentions to the principal's choice (i.e. $\pi_i(\overline{\overline{e}}_i(r)) > \pi_i(\overline{\overline{e}}_i(d_i))$, since she does not hold the principal responsible for being appointed as the typist. Therefore, she is willing to work harder.

4.2 Field-experimental test

4.2.1 Setup

To test the impact of the principal's procedural choice on the effort choice of the disadvantaged agent, we conducted a field experiment at the University of Namur. We hired research assistants for an ongoing research project that involves constructing a large dataset on the evolution of family structures in XIX-XXth century Russia and Kazakhstan. Half of the research assistants (the 'typists') had to type numerical data into a Microsoft Excel worksheet from scanned paper copies of the statistical publications of the Russian Empire. The others (the 'controllers') had to check whether the data typed in was correct. All research assistants were employed for two hours. The typists received a flat hourly wage of €10, whereas the controllers received a flat hourly wage of €15. There is no obvious difference in terms of intrinsic (dis)utility of labor for both jobs. If anything, the controllers's task seems to be less unpleasant. Taking the wage difference into account, the typist's job is clearly less attractive than that of the controller.

To test for procedural concerns, we concentrate our experiment on the performance of the typists.[13] If procedural concerns play no role, the performance of typists (in terms of the amount of data typed in and of typos made) should be independent of the procedure through which they are appointed. We used two different mechanisms to appoint the typists and the controllers: a direct (DA) and a random appointment (RA) mechanism. In the DA treatment, we conducted a randomization 'behind closed doors' and did not announce to the research assistants that they were appointed to jobs randomly. We did not give any justification for the appointment to a given role. In the RA treatment, each research assistant drew a card from a bowl to determine her/his role. So each research assistant had the same chance to become typist or controller.

Under either procedure, one-half of research assistants were appointed as typists and the one-half as controllers. We made all the research assistants aware of the wage difference, as well as of the fact that one-half of them were controllers and the other half were

[13] It is unclear how the controllers' performance can be measured, since the files to be controlled vary substantially in size as well as in the number of errors.

typists (see the instructions that we read out to the research assistants in the Appendix). None of the participants was made aware of the fact that this was not only a real research assistant's job but also an experiment.

We hired freshmen and sophomore undergraduate students of the University of Namur studying in six different faculties (Economics/Business, Law, Science, Computer Science, Philosophy, and Medicine). For each research assistant, we know which faculty and year he or she studies in, whether the student is foreign-born, and the student's gender. As a few registered students did not show up on the date of the experiment, the numbers of research assistants subject to the two appointment procedures differs slightly.

4.3 Summary statistics

Table 1 presents the distribution of typists across treatments and gender. Overall, we have 43 typists: 24 in the DA, and 19 in the RA treatment. 25 subjects were men and 19 were women. The number of male subjects in two treatments was roughly equal (13 in DA, 12 in RA), whereas for women there was a slight over-representation in the DA treatment (11 versus 7).

[Insert Table 1 about here]

Table 2 describes the summary statistics for our three measures of performance. On average, in two hours of work, a typist encoded 3675 cells in the Excel worksheet. There is substantial variation in the number of cells typed in: the standard deviation is 1031 cells, with the minimum equal to slightly over 2000 cells and the maximum over 6800 cells.

[Insert Table 2 about here]

We also measured the number of cells inserted incorrectly ('typos'), using the information on typos detected by controllers, which was also cross-checked by another research assistant not participating in the experiment. On average, a typist made 6.74 typos in two hours. Again, the performance varied substantially: the standard deviation was 5.42, with some typists making zero mistakes, while some making as many as 20 typos.

Clearly, a typist typing in more data is also likely to make more typos. To account for this, we also measure the error rate. This is a common measure of performance in statistical quality control (see Montgomery 2008). On average, a typist inserted 0.19% of cells incorrectly. There was an important variation in the error rate: the standard deviation was 0.16%, with the minimum error rate of zero and the maximum error rate of 0.6%.

4.4 Experimental results

Tables 3 to 5 present our experimental results.

[Insert Tables 3 to 5 about here]

As one can see from Table 3, an average typist inserted 3470 cells in the DA and 3934 cells in the RA treatment. On average, a male typist inserted 3892 cells, while a female typist typed in 3375 cells.

The results across treatments are strikingly different for men and women. Women inserted on average 3444 cells in DA and a somewhat lower number (3267) in RA. Men, on the contrary, inserted substantially more cells in RA (on average 4324) than in DA (on average 3494). Thus, in terms of the number of cells typed in, there is an important effect of procedures on the performance of male typists, with random appointment inducing higher performance, while for female typists the effect is basically absent (in any, it goes in the opposite direction).

Table 4 presents the results on the raw number of typos. Contrary to the findings on the quantity of cells inserted, men and women are similar with respect to the typos. In the RA treatment male and female typists made fewer typos (4.8 and 4.6, respectively) as compared to the DA treatment (9.3 and 7.2, respectively). Overall there are 4.7 typos in RA versus 8.3 typos in DA.

Finally, the error rates are presented in Table 5. The findings are similar to those on the number of typos. On average, RA typists had an error rate of 0.12%, while DA ones had an error rate twice as high, namely 0.24%. For men, the corresponding error rates were 0.11% versus 0.26%, whereas for women the rates are 0.14% versus 0.23%.

4.5 Regression results

We now proceed to a more rigorous statistical analysis exploiting the information on the individual characteristics of workers. We estimate the following econometric model:

$$y_i = \alpha + \beta \mathbf{I}_i(r = 1) + \gamma \mathbf{X}_i + \varepsilon_i, \tag{10}$$

where y_i is the measure of individual performance of typist i, $\mathbf{I}_i(r = 1)$ is the indicator variable that takes value 1 for RA typists and 0 for DA typists. $\mathbf{X}_i$ is a vector of individual characteristics, and ε_i is the error term assumed to be normally distributed with zero mean and a constant variance. Our theoretical model predicts that the coefficient β is positive and significantly different from zero.

The descriptive results above suggest that typists of different gender might respond differently to the same procedure. Hence, we also estimate an amended model:

$$y_i = \alpha + \beta \mathbf{I}_i(r = 1) + \delta \mathbf{G}_i(f = 1) + \mu[\mathbf{I}_i(r = 1) * \mathbf{G}_i(f = 1)] + \gamma \mathbf{X}_i + \varepsilon_i, \tag{11}$$

where $\mathbf{G}_i(f = 1)$ is the indicator variable which takes value 1 if the typist is a woman and 0 otherwise. The differential-by-gender response to the RA procedure is thus captured by the coefficient μ. Finding a positive (negative) and statistically significant μ would mean that once we hold other observable individual characteristics constant, the women in the RA procedure exhibit higher (lower) performance than the women in the DA procedure.

Table 6 presents the estimation results of models (10) and (11).

[Insert Table 6 about here]

Columns (1)-(3) show results with the number of cells inserted as the measure of individual performance. Column (1) reports the results of the estimation with only the treatment status as a regressor. On average, RA typists appointed using 'random appointment' encode 464 cells more than DA typists, but this difference is not statistically significant. Column (2) reports the results of the estimation of the amended model (11) without additional controls. A male RA typist inserts 830 cells more than a DA typist, and this difference is statistically significant at 5% level. However, a female RA typist inserts 226 cells less ($830 - 50 - 1006 = -226$) than a female DA typist (and the difference is not statistically significant).

There is substantial variation in individual performance, a part of which is captured when we add the additional controls that might be correlated with unobservable differences in skills. Column (3) presents the results of the estimation of the model (11) with these additional regressors. The coefficients β and μ both increase in absolute value and are more precisely estimated (both are significant at 1%). Moreover, the adjusted-R^2 of the model is the highest among the three specifications. Thus, a male RA typist encodes 1327 cells more his DA counterpart. This is a quantitatively large effect, about 1.3 times the standard deviation. For women the effect remains insignificant and negative: $1327 + 341 - 1970 = -302$ cells.

Columns (4)-(6) and (7)-(9) present the results of the estimation with, respectively, the number of typos and the error rate as the measure of performance. In both cases, the effect of the appointment procedure is present and similar for male and female typists (the coefficient μ is not significantly different from zero in any specification). The results are similar across all specifications. Using the model with the best fit (as measured by the adjusted-R^2), we can state that a RA typist makes 3.6 typos less and has half the error rate (i.e. 0.12% less) than a DA typist. This effect is quantitatively large: it equals 2/3 of the standard deviation in the case of typos and 3/4 in the case of the error rate.

In line with proposition 6, our experimental results suggest that the appointment procedure has a large significant effect on individual performance. However, the form of the effect differs for men and women. Male RA typists exhibit higher performance both in terms of quantity and quality of output. Female RA typists increase the quality of their output but not the quantity.

Overall, these findings provide robust support for our theoretical hypotheses. They indicate that the allocation procedure has a substantial effect on the workers' effort in terms of quality and quantity.

Our findings also complement the existing literature on the gender differences in social preferences. Croson and Gneezy (2009, section 3) argue that women seem to be more sensitive to the experimental context. Our results qualify this argument: in the principal-agent field-experimental setting, both women and men seem to exert less effort if the procedure chosen by the principal is considered less fair; however, women carry out this reduction of effort in a subtler way than men.

5 Conclusion

In this paper we have introduced a class of procedural games capable of dealing with procedural concerns. Procedural concerns arise because procedural choices influence peoples beliefs' about other people's intentions, expectations, etc. We have used this framework to analyze procedural concerns in two applications.

First, we have investigated the impact of procedural concerns on the results of policy experiments. We theoretically show that if experimental subjects are motivated by belief-dependent preferences, the way in which experimenters allocate them into the treatment and control groups influences their behavior, and thus the size of the treatment effect in the experiment. Moreover, we show that the estimate of the treatment effect is always biased as compared to the effect of a general introduction of the treatment.

In the second application, we analyze the impact of procedural concerns in a situation in which a principal has to appoint two agents into two different jobs that differ in terms of their desirability. Our model predicts that whenever agents are motivated by procedural concerns, the principal's choice of appointment mechanism is crucial for the subsequent effort choice of the agents. We test this hypothesis in a field experiment, and its results are consistent with our predictions. Moreover, we establish a novel result on gender differences in the agents' reaction to different appointment procedures.

While our paper provides a general framework for procedural concerns, the application to policy experiments and principal-agent relations concentrate on specific belief-dependent preferences. However, procedural concerns are not confined to these specific belief-dependent motivations, but also arise under other belief-dependent psychological incentives (guilt, disappointment, etc.). The analyses of the impact of procedural choices on the interaction of agents with these other types of belief-dependent motivations is left for future research.

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7 Appendix

Proof of proposition 1

Recall that $\pi(\overline{\overline{e}}^{t,p})$ and $\widehat{\pi}(\overline{\overline{e}}^{t,p})$ depend only on the agent's second-order beliefs about the effort (and not on the effort level itself) and that $\frac{\partial \pi_x(e(t,p),\overline{e}^{t,p})}{\partial e(t,p)} = t$. Hence,

$$\frac{\partial u^{t,p}(e(t,p),\overline{e}^{t,p},\overline{\overline{e}}^{t,p})}{\partial e(t,p)} = t(1+v_{\pi x}^{t,p}) - 2e(t,p), \tag{12}$$

$$\frac{\partial^2 u^{t,p}(e(t,p),\overline{e}^{t,p},\overline{\overline{e}}^{t,p})}{\partial e(t,p)^2} = \frac{\partial^2 v(\pi_x(e(t,p),\overline{e}^{t,p}),\pi(\overline{\overline{e}}^{t,p}),\widehat{\pi}(\overline{\overline{e}}^{t,p}))}{(\partial \pi_x)^2} t^2 - 2. \tag{13}$$

Since $\frac{\partial^2 v(\pi_x(e(t,p),\overline{e}^{t,p}),\pi(\overline{\overline{e}}^{t,p}),\widehat{\pi}(\overline{\overline{e}}^{t,p}))}{(\partial \pi_x)^2} = 0$,

$$\frac{\partial^2 u^{t,p}(e(t,p),\overline{e}^{t,p},\overline{\overline{e}}^{t,p})}{\partial e(t,p)^2} < 0 \quad \text{for all } t,p. \tag{14}$$

Because $|v_{\pi x}^{t,p}| \leq \frac{3}{4}$, it is easy to check that

$$\begin{aligned} \left.\frac{\partial u^{t,p}(e(t,p),\overline{e}^{t,p},\overline{\overline{e}}^{t,p})}{\partial e(t,p)}\right|_{e(t,p)=0} &> 0 \quad \text{for all } t,p, \\ \left.\frac{\partial u^{t,p}(e(t,p),\overline{e}^{t,p},\overline{\overline{e}}^{t,p})}{\partial e(t,p)}\right|_{e(t,p)=1} &< 0 \quad \text{for all } t,p. \end{aligned} \tag{15}$$

Because of (14) and (15), each of the equations

$$\frac{\partial u^{t,p}(e(t,p),\overline{e}^{t,p},\overline{\overline{e}}^{t,p})}{\partial e(t,p)} = 0 \tag{16}$$

has a unique interior solution for each t,p for any first- and second-order belief $\overline{e}^{t,p},\overline{\overline{e}}^{t,p}$. These solutions characterize the optimal effort choices of all types of agents for given first- and second-order beliefs. In equilibrium, the beliefs of first- and second-order have to be the same, i.e. $\overline{e}^{t,p} = \overline{\overline{e}}^{t,p}$ for all t,p. The solution of (16) can be rewritten as a function

$$e_{opt}^{t,p} : [0,1]^4 \to [0,1]^4,$$

with $e_{opt}^{t,p}(\overline{e}^{t,p})$ being the optimal effort choice of an (t,p)-agent who holds the same first- and second-order beliefs $\overline{e}^{t,p} = \overline{\overline{e}}^{t,p}$. Since $u^{t,p}(e(t,p),\overline{e}^{t,p},\overline{\overline{e}}^{t,p})$ is twice continuously differentiable, $e_{opt}^{t,p}$ is also continuous. Brower's fixed-point theorem guarantees the existence of a fixed point:

$$\exists e^* \in [0,1]^4 : e_{opt}^{t,p}(e^*) = e^*(t,p) \text{ for all } t,p.$$

The effort levels characterized by this fixed point maximize the agents' utilities for first- and second-order beliefs which coincide with the utility maximizing effort levels, i.e. for correct beliefs. Hence, e^* fulfills the conditions for an equilibrium.

Finally, the experimenter is assumed to be motivated by the success of the program. And since her set of feasible procedures is finite, an optimal procedure for the experimenter exists. Hence, the procedural game exhibits an equilibrium in pure strategies.■

Proof of proposition 2

By proposition 1, the equilibrium effort levels are in the interior. Hence, they are fully characterized by the first-order conditions (FOCs):

$$\begin{aligned}
1 - 2e(1,d) + v_{\pi x}^{1,d} &= 0, && (17)\\
\frac{1}{2} - 2e(\frac{1}{2},d) + v_{\pi x}^{\frac{1}{2},d}\frac{1}{2} &= 0, && (18)\\
1 - 2e(1,r) + v_{\pi x}^{1,r} &= 0, && (19)\\
\frac{1}{2} - 2e(\frac{1}{2},r) + v_{\pi x}^{\frac{1}{2},r}\frac{1}{2} &= 0. && (20)
\end{aligned}$$

In equilibrium, the beliefs have to be correct. The FOCs hold with $\overline{\overline{e}}^{t,p}(t',p') = \overline{e}^{t,p}(t',p') = e(t',p')$.

To prove the proposition, we first show that $e^*(1,r) > e^*(\frac{1}{2},r)$. Since in equilibrium $\overline{\overline{e}}^{\frac{1}{2},r}(t',p') = \overline{\overline{e}}^{1,r}(t',p') = e(t',p')$, $\overline{\overline{\pi}}_a^{1,r}(\overline{\overline{e}}^{1,r}) = \overline{\overline{\pi}}_a^{\frac{1}{2},r}(\overline{\overline{e}}^{\frac{1}{2},r})$. Because of this equality, $v_{\pi x}^{1,d} = v_{\pi x}^{\frac{1}{2},d}$. Using this and comparing the FOCs (19) and (20) reveal that $e^*(1,r) = 2e^*(\frac{1}{2},r) > e^*(\frac{1}{2},r)$.

Second, we prove that

$$e^*(1,r) - e^*(1,r)^2 > \frac{1}{2}e^*(\frac{1}{2},r) - e^*(\frac{1}{2},r)^2. \tag{21}$$

Inserting $e^*(1,r) = 2e^*(\frac{1}{2},r)$ and rearranging terms, (21) becomes

$$\frac{3}{4}(e^*(1,r) - e^*(1,r)^2) > 0,$$

which holds for any $e^*(1,r) \in (0,1)$.

Third, it has to be shown that $e^*(1,d) > e^*(1,r)$. Because of equations (5), (7) and (21) it is true that

$$\begin{aligned}
v_{\pi x}^{1,d} - v_{\pi x}^{1,r} &= e(1,d) - e(1,d)^2 - q(e(1,r) - e(1,r)^2) - (1-q)(\frac{1}{2}e(\frac{1}{2},r) - e(\frac{1}{2},r)^2)\\
&> e(1,d) - e(1,d)^2 - e(1,r) + e(1,r)^2.
\end{aligned}$$

Comparing (17) to (19), one sees that

$$v_{\pi x}^{1,d} - v_{\pi x}^{1,r} = 2(e(1,d) - e(1,r)), \tag{22}$$

implying that

$$e(1,d) - e(1,r) > -e(1,d)^2 + e(1,r)^2. \tag{23}$$

However, this condition can only hold for $e^*(1,d) > e^*(1,r)$.

Finally, it remains to show that $e^*(\frac{1}{2},r) > e^*(\frac{1}{2},d)$. Because of equations (5), (7) and (21), it holds that

$$\begin{aligned} v_{\pi x}^{\frac{1}{2},r} - v_{\pi x}^{\frac{1}{2},d} &= q(e(1,r) - e(1,r)^2) + (1-q)(\frac{1}{2}e(\frac{1}{2},r) - e(\frac{1}{2},r)^2) - \frac{1}{2}e(\frac{1}{2},d) + e(\frac{1}{2},d)^2) \\ &> \frac{1}{2}(e(\frac{1}{2},r) - e(\frac{1}{2},d)) - e(\frac{1}{2},r)^2 + e(\frac{1}{2},d)^2). \end{aligned}$$

Comparing (18) to (20), one gets

$$v_{\pi x}^{\frac{1}{2},r} - v_{\pi x}^{\frac{1}{2},d} = 4(e(\frac{1}{2},r) - e(\frac{1}{2},d)),$$

implying that

$$\frac{7}{2}(e(\frac{1}{2},r) - e(\frac{1}{2},d)) > -e(\frac{1}{2},r)^2 + e(\frac{1}{2},d)^2.$$

However, this condition can only hold for $e^*(\frac{1}{2},r) > e^*(\frac{1}{2},d)$. ■

Proof of proposition 3

i) $q = 1$ implies that $\lambda = 1$. Therefore, $\pi(\overline{\overline{e}}^{1,d}) = \widehat{\pi}(\overline{\overline{e}}^{1,d})$ and $v_{\pi x}^{1,d} = 0$. From (17) follows that $e^*(1,d) = \frac{1}{2}$. Since the beliefs have to be correct in equilibrium, we get that $\widehat{\pi}(\overline{\overline{e}}^{1,r}) = \frac{1}{4}$. By substiting into (19) we get

$$1 - 2e(1,r) + (e(1,r) - e(1,r)^2 - \frac{1}{4}) = 0, \tag{24}$$

given that the beliefs have to be correct. The unique solution to (24) is $e^*(1,r) = \frac{1}{2}$.

ii) $q = 0$ implies that $\lambda = 0$. Therefore, $\pi(\overline{\overline{e}}^{\frac{1}{2},d}) = \widehat{\pi}(\overline{\overline{e}}^{\frac{1}{2},d})$ and $v_{\pi x}^{1,d} = 0$. From (18) follows that $e^*(1,d) = \frac{1}{4}$. Since the beliefs have to be correct in equilibrium, we get that $\widehat{\pi}(\overline{\overline{e}}^{1,r}) = \frac{1}{16}$. By substiting into (20) we get

$$\frac{1}{2} - 2e(\frac{1}{2},r) + \frac{1}{2}(\frac{1}{2}e(\frac{1}{2},r) - e(\frac{1}{2},r)^2 - \frac{1}{16}) = 0, \tag{25}$$

given that the beliefs have to be correct. The unique solution to (25) is $e^*(\frac{1}{2},r) = \frac{1}{4}$. ■

Proof of proposition 4

We first show that in equilibrium $v_{\pi x}^{1,d} > 0 > v_{\pi x}^{\frac{1}{2},d}$. Inserting (5) and (6) into (7) gives

$$\begin{aligned} v_{\pi x}^{1,d} &= (1-\lambda)(e(1,d) - e(1,d)^2 - \frac{1}{2}e(\frac{1}{2},d) + e(\frac{1}{2},d)^2), \qquad (26) \\ v_{\pi x}^{\frac{1}{2},d} &= -\lambda(e(1,d) - e(1,d)^2 - \frac{1}{2}e(\frac{1}{2},d) + e(\frac{1}{2},d)^2) \end{aligned}$$

Both equations together can only hold for either $v_{\pi x}^{1,d} = v_{\pi x}^{\frac{1}{2},d} = 0$ or for $v_{\pi x}^{1,d}$ and $v_{\pi x}^{\frac{1}{2},d}$ having opposite signs.

Take first the case of $v_{\pi x}^{1,d} = v_{\pi x}^{\frac{1}{2},d} = 0$. In this case, the equilibrium effort levels would be $\frac{1}{2}$ and $\frac{1}{4}$, respectively (see FOCs (17) and (18)). Inserting these values and (5) and (6) into (7), one obtains that $v_{\pi x}^{1,d} > 0 > v_{\pi x}^{\frac{1}{2},d}$ - a contradiction.

Hence, $v_{\pi x}^{1,d}$ and $v_{\pi x}^{\frac{1}{2},d}$ must have opposite signs. Assume that $v_{\pi x}^{1,d} < 0 < v_{\pi x}^{\frac{1}{2},d}$. This inequality together with the FOCs (17) and (18) implies that $e(1,d) < \frac{1}{2}$ and $e(\frac{1}{2},d) > \frac{1}{4}$. Since $e(1,d) > e(\frac{1}{2},d)$, this implies that $e(t,d) \in (\frac{1}{4},\frac{1}{2})$ for $t = 1, \frac{1}{2}$.

Because of (26) and $v_{\pi x}^{1,d} < 0 < v_{\pi x}^{\frac{1}{2},d}$,

$$-e(1,d) + e(1,d)^2 + \frac{1}{2}e(\frac{1}{2},d) - e(\frac{1}{2},d)^2 = -v_{\pi x}^{1,d} + v_{\pi x}^{\frac{1}{2},d} > 0 \qquad (27)$$

For $e(t,d) \in (\frac{1}{4},\frac{1}{2})$ the left-hand side of (27) is decreasing in $e(1,d)$ and $e(\frac{1}{2},d)$. However, even for the limit case of $e(1,d) = e(\frac{1}{2},d) = \frac{1}{4}$ the left hand side of (27) is $-\frac{1}{8}$. Hence (27) cannot hold and $v_{\pi x}^{1,d} < 0 < v_{\pi x}^{\frac{1}{2},d}$ is not possible in equilibrium. Therefore, $v_{\pi x}^{1,d} > 0 > v_{\pi x}^{\frac{1}{2},d}$. This and (26) also implies that $e^*(1,d) - e^*(1,d)^2 > \frac{1}{2}e^*(\frac{1}{2},d) - e^*(\frac{1}{2},d)^2$ - the material payoff from getting a treatment is larger than from not getting a treatment, if the selection is done directly.

Recall that $v_{\pi x}^{t,p} \in [-\frac{3}{4},\frac{3}{4}]$. Hence, $v_{\pi x}^{1,d} \in (0,\frac{3}{4}]$ and $v_{\pi x}^{\frac{1}{2},d} \in [-\frac{3}{4},0)$. Using this and the FOCs (17) and (18) one immediately gets that $e^*(1,d) \in (\frac{1}{2},\frac{7}{8}]$ and that $e^*(\frac{1}{2},d) \in [\frac{1}{16},\frac{1}{4})$. ■

Proof of proposition 5

i) Substracting (20) from (19) reveals that $v_{\pi x}^{1,r} - \frac{1}{2}v_{\pi x}^{\frac{1}{2},r} = 0$, whenever in equlibrium $e(1,r) - e(\frac{1}{2},r) = \frac{1}{4}$. Since $v_{\pi x}^{1,r} = v_{\pi x}^{\frac{1}{2},r}$, this can only hold for $v_{\pi x}^{1,r} = v_{\pi x}^{\frac{1}{2},r} = 0$. Hence, $e(1,r) = \frac{1}{2}$, $e(\frac{1}{2},r) = \frac{1}{4}$ in equilibrium if the difference in equilibrium effort is $\frac{1}{4}$.

In equilibrium, the beliefs have to be correct. From this, $v_{\pi x}^{1,r} = v_{\pi x}^{\frac{1}{2},r} = 0$, and $e(1,r) = \frac{1}{2}$, $e(\frac{1}{2},r) = \frac{1}{4}$, we get that in equilibrium the neutral payoff must be given by

$$\widehat{\pi} = \frac{3q+1}{16}. \qquad (28)$$

Using the definition of $\widehat{\pi}$, (28), and again the fact that the equilibrium beliefs are correct, we get

$$\frac{3q+1}{16} = \lambda\pi(1,d) + (1-\lambda)\pi(\frac{1}{2},d). \tag{29}$$

If in equilibrium $e(1,r) - e(\frac{1}{2},r) = \frac{1}{4}$, then the equation (29) has to hold. Recall that $\pi(1,d)$ and $\pi(\frac{1}{2},d)$ are determined by the joint solution of the FOCs (17) and (18). Since $v_{\pi x}^{t,d}$ is independent of q, $\pi(1,d)$ and $\pi(\frac{1}{2},d)$ do not depend on q. Hence the right-hand side of (29) is independent of q, whereas the left-hand side is strictly increasing in q. Hence, for any given $\lambda \in (0,1)$ there exists at most one q such that $e_1^* - e_0^* = \frac{1}{4}$.

ii) Inserting (28) into (7) and (17) leads to

$$1 - 2e(1,d) + (e(1,d) - e(1,d)^2 - \frac{3q+1}{16}) = 0.$$

By solving this equation one gets

$$e(1,d) = \frac{-2+\sqrt{19-3q}}{4}. \tag{30}$$

Inserting 28) into 7) and 18) leads to

$$\frac{1}{2} - 2e(\frac{1}{2},d) + (\frac{1}{2}e(\frac{1}{2},d) - e(\frac{1}{2},d)^2 - \frac{3q+1}{16})\frac{1}{2} = 0.$$

By solving this equation one gets

$$e(\frac{1}{2},d) = \frac{-7+\sqrt{64-3q}}{4} \tag{31}$$

Given that $\lambda = q$ and because of (31) and (30), (29) becomes

$$\begin{aligned} \frac{3q+1}{16} &= q\left(\frac{-2+\sqrt{19-3q)}}{4} - \left(\frac{-2+\sqrt{19-3q)}}{4}\right)^2\right) \\ &\quad +(1-q)\left(\frac{1}{2}\frac{-7+\sqrt{64-3q}}{4} - \left(\frac{-7+\sqrt{64-3q}}{4}\right)^2\right), \end{aligned} \tag{32}$$

leading to

$$0 = 96q + 8q\sqrt{19-3q} - 16q\sqrt{64-3q} + 16\sqrt{64-3q} - 128. \tag{33}$$

For any $q \in (0,1)$, the right-hand side of (33) is strictly larger than zero. This equation holds only for the limit cases $q = 1$ and $q = 0$. ■

7.1 Proof of proposition 6

When agent i has to make an effort choice, she maximizes her utility for given s and given $\overline{\overline{\pi}}_i$. Formally, for $s \in \{d_i, r\}$ the maximization problems reads

$$\max_{e_i(s)\in[0,1]} w_t - c(e_i(s)) + v(\overline{\pi}_p(e_i(s)), \overline{\overline{\pi}}_i(\overline{\overline{e}}_i(s)), \widehat{\pi}(\overline{\overline{e}}_i(d)))$$

Since $c(1) = -c(0) = \infty$, and $\frac{\partial \overline{\pi}_p(e_i(s))}{\partial e_i(s)} = 1$, the solution of the maximization problem is characterized by the first-order conditions

$$-c'(e_i(d_i)) + v_{\pi p}^{d_i} = 0 \tag{34}$$

and

$$-c'(e_i(r)) + v_{\pi p}^{r} = 0. \tag{35}$$

Recall that $\overline{\overline{\pi}}_i(\overline{\overline{e}}_i(r)) > \overline{\overline{\pi}}_i(\overline{\overline{e}}_i(d_i))$, implying that $v_{\pi p}^{d_i} > v_{\pi p}^{r}$ for any $\overline{\overline{e}}_i(d_i)$. Therefore, $c'(e_i(r)) > c'(e_i(d_i))$, implying that $e_i(r) > e_i(d_i)$. ■

APPENDIX

The following information was read out to subjects at the beginning of each session. The contents were identical in both treatments, except the section marked in italics.

Job description and payment details

We are constructing a dataset on the socio-economic characteristics of extended families and their production and consumption decisions, for a project on the evolution of family structure and collective action in traditional societies. The raw data that we have (that comes from an agricultural census of the Russian Empire of the beginning of the 20th century) exists only in the paper version, and not in electronic format. This means that it is necessary to copy it from the paper version into an Excel worksheet.

[Detailed instructions on how to copy the data from the paper version into the worksheet]

To make sure that the data is inserted correctly, all the files will be crosschecked. This means that there are two different tasks. TYPISTS insert data into Excel worksheets. CONTROLLERS verify the inserted data and correct it wherever necessary.

The hourly wage is 15€ for a controller and 10€ for a typist. In total, you are going to work for 2 hours; thus, a typist will receive 20€ and a controller 30€ at the end of the work.

[TREATMENT 1] Given the lack of time, we cannot verify which of you are better qualified to work as a typist or as a controller. We thus have decided that you are going to work as a TYPIST.

[TREATMENT 2] Given that we do not know which of you are better qualified to work as a typist or as a controller, the tasks are allocated in a random fashion. Each of you had to draw a card from the bowl. If you have picked a card with the word "TYPIST", you are going to work as a typist. If you have picked a card with the word "CONTROLLER", you are going to work as a controller.

In order to avoid losing the data inserted, please make sure that you save your Excel file regularly.

Do you have any questions?

Table 1. Number of observations

	Male	Female	Both
Direct appointment	13	11	24
Random appointment	12	7	19
Both treatments	25	18	43

Table 2. Summary statistics

	Mean	Std. Dev.	Min	Max
Number of cells encoded	3675	1031	2010	6825
Number of typos made	6,74	5,42	0	20
Error rate, in %	0,19	0,16	0	0,6

Table 3. Average number of cells encoded

	Male	Female	Both
Direct appointment	3494	3444	3470
Random appointment	4324	3267	3934
Both treatments	3892	3375	3675

Table 4. Average number of typos made

	Male	Female	Both
Direct appointment	9,3	7,2	8,3
Random appointment	4,8	4,6	4,7
Both treatments	7,2	6,2	6,7

Table 5. Average error rate, in %

	Male	Female	Both
Direct appointment	0,26	0,23	0,24
Random appointment	0,11	0,14	0,12
Both treatments	0,18	0,19	0,19

Table 6. Multiple Regression Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent variable	Number of cells encoded	Number of cells encoded	Number of cells encoded	Number of typos made	Number of typos made	Number of typos made	Error rate, in %	Error rate, in %	Error rate, in %
Constant	3470	3494	2432	8.33	9.31	8.34	0.24	0.26	0.26
	(16.73)***	(12.86)***	(3.59)***	(7.90)***	(6.41)***	(2.21)**	(8.18)***	(6.24)***	(2.35)**
Treatment = Random appointment	464	830	1327	-3.60	-4.47	-3.66	-0.12	-0.15	-0.15
	(1.49)	(2.12)**	(2.95)***	(2.27)**	(2.14)**	(1.46)	(2.75)***	(2.50)**	(2.06)**
Female		-50	341		-2.21	-0.71		-0.03	-0.01
		(0.12)	(0.78)		(0.99)	(0.29)		(0.50)	(0.15)
Female*Random		-1006	-1970		1.86	-0.35		0.06	0.05
		(1.64)	(2.73)***		(0.57)	(0.09)		(0.67)	(0.39)
Controls	No	No	Yes	No	No	Yes	No	No	Yes
Observations	43	43	43	43	43	43	43	43	43
Adjusted R-squared	0,03	0,10	0,15	0,09	0,07	0,05	0,14	0,10	0,03

Absolute value of t-statistics in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Treatment: omitted category = "Direct appointment"
Controls: Faculty, freshman, and foreign-born dummies