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Person-Organization Fit and Incentives: A Causal Test

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Person-Organization Fit and Incentives: A Causal Test ^{*}

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Abstract

We investigate the effects of organizational culture and personal value orientations on performance under individual and team contest incentives. We develop a model of regard for others and in-group favoritism predicting interaction effects between organizational culture and personal values in the contest games. The predictions are tested in a computerized lab experiment with exogenous control of both organizational culture and incentives. In line with our theoretical model we find that prosocial (proself) orientated subjects exert more (less) effort in team contests in the primed prosocial organizational culture condition, relative to the neutrally primed baseline condition. Further, when the prosocial organizational culture is combined with individual contest incentives, prosocial subjects no longer outperform their proself counterparts. These findings provide a first, affirmative, causal test of person-organization fit theory. They also suggest the importance of a 'triple-fit' between personal preferences, organizational culture and incentive mechanisms for prosocially orientated individuals.

Keywords: tournaments; organizational culture; personal values; person-organization fit; teams; economic incentives

JEL: C91, D02, D23, J33, M52

1 Introduction

A long tradition in organizational research on person-organization fit supports the notion that employees whose values align with those of their organization are more productive, engage more in organizational citizenship behavior, and are more committed to the organization (see e.g., Hoffman and Woehr, 2006; Verquer, Beehr and Wagner, 2003; Schneider, 1987). Erez and Earley (1993) and Gerhart (2009) among others argue that personal preferences, culture, and management practices interact to impact work behavior and performance. This view suggest that the benefits of the fit

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between personal and organizational values could be reinforced by adopting well-suited management practices, most notably the compensation schemes of the employees.

Economists for their part have intensively studied the effects of compensation schemes on performance (Prendergast 1999; Lazear, 1999; Harbing and Irlenbusch, 2011). Yet, the potential moderating effect of person-organization fit has received little attention, presumably because of the believed elusiveness of identifiable variation in organizational culture and the lack of behavioral incentivized measures of value-related preferences on which economists traditionally focus. Nevertheless, there is a rising interest, especially among experimental economists, in studying the impact of culture, including corporate culture, on economic outcomes (Weber and Camerer, 2003; Kosfeld and von Siemens, 2011; van den Steen, 2010). Even the question of matching the right kind of personalities with the right projects and firms is receiving both theoretical and experimental attention (Besley and Ghatak, 2005, 2013).

In this paper, we attempt to identify a *causal* effect of person-organization fit. In particular, we develop arguments that organizations need to align incentive schemes both with organizational culture and with employee personal preferences to optimally motivate employee performance. Firstly, we do so applying a variant of the model of Chen and Li (2009) featuring regard for others and in-group favoritism to a setting where teams compete to win a prize (a team contest game, see Orrison et al 2004). The model predicts interaction effects between specific configurations of prosocial or proself organizational cultures and personal preferences on performance. We then conduct a controlled laboratory experiment where we use an innovative priming procedure to simulate organizational culture in the laboratory. Personal value orientation and other-regarding preferences are measured and the effects of the primed culture on individual effort in the teams contest are studied for other- and self-regarding individuals. The design allows for the identification of a causal effect of the fit between personal and organizational values on performance in team contests. In line with the theoretical model, we find that prosocial individuals react differently to a prosocial organizational culture than proself oriented ones. In particular, prosocial individuals exert more effort in an organizational culture that matches their preferences. Proself oriented individuals, in contrast, exert less effort in prosocial organizational cultures.

There are two interrelated reasons for the focus on team contests. First, relative performance schemes are found in a majority of hierarchical organizations (Bull et al., 1987; Baker et al., 1988) and several studies report evidence of the increasing importance of team incentives. Ledford (1995), for instance, shows that team incentives are present in a majority of US firms. Lazear and Shaw (2007) point out that since the late 1990's, teamwork has become prevalent in many large firms; even in academia, team incentives are receiving more attention (Wuchty et al., 2007). Second, previous evidence suggests that organizational values supportive of consideration for others may particularly facilitate team effectiveness (Mathieu et al., 2008). Hence, we specifically primed a supportive, prosocially-oriented culture by priming self-transcendence values such as benevolence and universalism, which we contrast with a self-interest oriented culture by priming self-enhancement values such as achievement and power (see Schwartz, 1992). We also introduce a neutral control condition where subjects receive a none-associative prime. Primes were designed building on Schwartz's theory of human values (Schwartz, 1992) and using well-established supraliminal priming techniques (e.g. Bargh and Chartrand, 2000). To our knowledge, organizational values have not been examined in economic experiments to date.

Regarding personal value orientation, we pre-measure prosocial preferences using behavioral measures such as the dictator game transfer (the subject divides a sum of money between himself/herself and an anonymous recipient), and for robustness checks, trust game backtransfer, and the ultimatum game acceptance threshold (Camerer, 2003). We also use a psychological measure, the Personal Value Questionnaire (PVQ) survey tool (Schwartz et al., 2001). With an exogenous control of organizational values and incentives, and with knowledge of individual pro-sociality, we can study which match of personal characteristics and organizational values induces the highest effort in the competing teams contest.

The priming method that we exploit in causal identification is also applied in psychological studies of the impact of both personal and national cultural values (e.g., Maio et al., 2009, Oyserman and Lee, 2008, Oyserman, 2011). Organizational values, for their part, are widely accepted as a core element of organizational cultures (Ashkanasy et al., 2000; Cameron and Quinn, 2011; Hofstede, 1985; O'Reilly et al., 1991) and value congruence is the dominant dimension along which person-organization fit is evaluated (e.g. Hoffman and Woehr, 2006; O'Reilly et al., 1991). Within management and organizational psychology, there is only limited research investigating priming and subconscious goals in the workplace and their effect on work-related outcomes such as expenditure of effort in a performance task. The only workplace-related research using priming investigated how (certain picture) primes lead to the subconscious activation of primed goals (typically achievement), which subsequently have an impact on performance (e.g. call center agents being more successful in fundraising; e.g. Shantz and Latham, 2009). However, our theoretical model leads us to prefer value primes over pictures as primes as we specifically hypothesize person-organization value-*fit* as the key mechanism in our experiment.

We feel that the use of the procedure for priming organizational culture is justified for a number of reasons. First, notwithstanding if researchers use primes to prime personal or national culture values, they agree that primes are situational stimuli which temporarily enhance the accessibility of individual values. Our argument is that outside the lab, within an organization, organizational culture acts as a situational stimulus that members of an organization are exposed to and which primes their values. Second, although we are unaware of other studies using value primes to prime organizational culture, they have been used to prime national culture (e.g. Oyserman, 2011). National and organizational culture, albeit not identical, bear strong similarities and are closely interlinked (see House et al., 2004 for evidence). We attribute the lack of past research using priming to simulate the effects of organizational culture to the fact that past priming research has predominantly been conducted by social psychologists who are not concerned with exploring organizational culture; this contrasts with management and organizational researchers for whom organizational culture is an important concept. Finally, the use of value primes, more generally, is consistent with the literature on organizational culture, which emphasizes values as a core aspect of organizational culture (e.g., Ashkanasy et al., 2000; Cameron and Quinn, 2011; Hofstede, 1995; O'Reilly et al., 1991).

We also bridge the person-organization fit literature with that of economic analysis into the effects of incentives on performance (Prendergast, 1999) and the related literature on pay for performance in management (e.g., Gerhart et al., 2009). In other words, we study the importance of fitting a third dimension, the right incentive structure, with congruing personal preferences and organizational values. To study this, we run a set of complementary experiments where individuals

instead of teams compete to win a prize and study which person-organization value constellations lead to the highest performance. This is done in a controlled manner keeping much of the structure (in particular the game theoretic Nash-equilibrium prediction) from team competition intact, yet dropping the teams structure as in Orrison et al. (2004). Under this incentive structure, we find that the link between prosocial individuals and a prosocial organizational culture vanishes. This finding extends the person-organization fit theory by pointing out the importance of a 'triple-fit' of preferences, organizational culture and incentive mechanisms for those who are prosocially oriented.

Despite an impressive existing literature, the empirical research on the benefits of the fit between personal preferences, organizational culture, and management practices has been impeded by at least four factors. First, organizational culture is often operationalized as an overarching concept embracing both organizational values and human resource management practices, which does not allow us to identify any potential interaction effects. However, a mismatch between organizational cultures and management practices, as is often evident during organizational change processes or mergers, may have particularly severe performance implications and thus warrants investigation. Second, organizational values and compensation schemes are most likely to be correlated, such that organizations adopt compensation schemes that are consistent with their values. The lack of exogenous and independent variation impedes causal identification and might explain why there are no studies exploiting happenstance data. Third, even though person-organization fit theory is widely acknowledged in management research (e.g. Edwards, 2008), it is mainly supported by correlational evidence. To the best of our knowledge, we provide a first causal test where organizational values and compensation schemes vary exogenously and independently. Finally, management researchers and economists alike often search for universally effective management practices - across countries, industries and organizations (e.g. Bloom and van Reenen, 2010). However, organizational cultures are influenced by national cultures as are the preferences of individuals (e.g., House et al., 2004). Thus, it seems worthwhile to investigate whether one size indeed fits all or whether management practices may be differentially effective, depending on organizational values and individual preferences.

In principle, the method of controlled experimentation that we exploit has the capacity of overcoming all these challenges if the experimental proxies for incentive schemes and organizational values do not compromise on external validity. While lab-experimental studies can be criticized for relying heavily on the extrapolation from the lab to the field, experimentation in general has the decisive advantage of direct control which allows for strong causal conclusions. Moreover, the concerns for external validity might be mitigated by the remark that if any effect can be identified in our laboratory setting with a very weak notion of teams and culture, then the effect is also likely to be of importance in environments where, contrary to our minimal group treatment and priming method, the group and the organizational values are more vividly and concretely present. The experimental teams and individual contests, on the one hand, and the priming method, on the other, provide novel tools to proxy incentives and organizational values and thus to study their causal effects. Happenstance exogenous variation in organizational culture is rare and thus, laboratory settings provide an interesting complementary avenue.

The rest of the paper is organized as follows. Section 2 discusses in more depth some related studies to highlight how we complement the existing literature. Section 3 gives a theoretical overview presenting the contest game, the values and priming theory, and the application of the model of

Chen and Li (2009) to the value priming in the teams contest. Section 4 explains the experimental set-up. Section 5 contains the experimental analysis and Section 6 discusses the results and Section 7 concludes.

2 Related literature

In this section, we present some related studies from different disciplines to underscore the interdisciplinary nature of our work and to exemplify how we differ, complement, and draw from the existing literature. We first motivate our focus on tournament incentives. Next, we highlight the specific nature of tournaments relative to other game structures. Then, we discuss related experimental work on organizational culture and cooperation.

Individual relative performance incentives reward those who perform best as compared to other individuals in an organization. Due to internal promotion, such practices are implicitly present in most organizations. While Matsumura and Shin (2006) find such incentive schemes to be generally effective, the latter authors note that a sense of unfairness by the employees diminishes their effectiveness. In particular and as suggested by field evidence (Bandiera et al., 2005), relative individual evaluation may render the performance of close-knit groups suboptimal if other-regarding group members internalize the negative externality of their effort on others. In related research, Bandiera et al. (2010) conduct a field experiment to investigate the effect of social ties to other workers on productivity under absolute performance measures. They find that overall, there is a positive effect of social ties on aggregate productivity. Measuring the aggregate performance within small teams and rewarding teams relative to the performance of other teams may help alleviate the problem (Orrison et al., 2004; Nalbantian and Schotter, 1997; Hamilton et al., 2003; Eckel and Grossman, 2005). Indeed, team incentives induce a positive externality on fellow team members countervailing the negative externality on the members of the competing team. In this setting, the intra-team positive externality may well more than offset the inter-team negative externality given the tendency for parochial altruism, the preference for being nice to in-group members and neutral or even hostile to out-group members (Choi and Bowles, 2007; Billig and Tajfel, 1973; Rand et al. 2009, Chen and Li, 2009).

It is important to analyze both competition and cooperation at the workplace which are not necessarily polar opposites. The strategic nature of contests (e.g., Shreeremeta, 2011) differs from that of social dilemmas and public good games in three respects. First, in order to cooperate in contests, contestants should collude and refrain from exerting effort so that prizes can be won at a lower cost. Second, in contest games, it is in each participant's private interest to contribute resources to the race. Finally, contributing more than the private optimum undermines both the total and the private surplus. Increasing one's contribution marginally generates a negative externality on other contestants (or members of other teams). There is a positive externality benefitting one's own team-mates (in the teams contest), however.

Sagiv et al. (2011) study a setting where individuals cooperate in a social dilemma game, on the one hand, and where teams compete in threshold-public-good production, on the other hand. They find evidence that universalism and benevolence values as opposed to achievement and power values promote both individual cooperation and within-team cooperation. Yet, as pointed out above, our contest games reflecting two alternative competitive incentive schemes greatly differ from dilemma

and public goods games. Group optima in contests differ qualitatively and importantly from group optima in social dilemmas. Although classic public good provision is also crucial at many work places, we believe that our contest games are equally important in understanding strategic features of work place incentives.

There is few related experimental research studying organizational culture and cooperation. Chatman and Barsade (1995) assessed students' disposition to cooperate, following the random assignment to either collectivist or individualist organizational cultures. They were interested in understanding how personal cooperativeness (personality characteristic) and an organization's emphasis on collectivistic or individualistic values interact to influence members' cooperative behavior in a business simulation game. A collectivist culture was induced by placing higher rewards on cooperation and teamwork. Thus, the variation in incentives and organizational values is not independent in their study. They find that individualists in the collectivist culture display less cooperative behavior, consistent with their personality. We find similar effects with regard to matching such that proselves underperform in the prosocial culture when exposed to a team contest incentive scheme. They also find that individuals with a high disposition to cooperate show relatively low levels of cooperation in individualist organizational culture. We find that prosocials in the prosocial culture with individual contests do no longer display a higher performance. Thus, unlike Chatman and Barsade we separate out organizational culture on the one hand and incentive scheme on the other, and we investigate performance in contests instead of cooperative behavior in social dilemmas.

Chen et al. (2007) run voluntary contribution game sessions in China and US and conclude that group norms reinforce cooperation in the collectivist Chinese culture, but not in the individualist American one. Drouvelis et al. (2010) find that, compared to a neutral prime, prosocial priming increases the effort in a one-shot public goods game. Although their findings are supportive of ours, we do not find any difference in the average effort between the no priming and prosocial priming condition - we only find the differential effect of the prosocial prime on prosocial and prosself individuals. However, like Chatman and Barsade (1995), both Chen et al. (2007) and Drouvelis et al. (2010) are interested in cooperation and voluntary contributions to a public good as opposed to performance in two alternative types of contests (Bull et al., 1987; Orrison et al., 2004). Moreover neither Chen et al. (2007) nor Drouvelis et al. (2010) control for personal values of the subjects and it might be that their results are driven by the most pro-social individuals.

There is also a related, small experimental literature studying mergers of two corporate cultures springing from the seminal contribution by Weber and Camerer (2003). Instead of organizational values, the focus of this line of research lies in the study of post-merger adaptation processes when the behavioral norms for coordination may be conflicting in the two merging organizations (e.g., Camerer and Weber, 2008)).

Kosfeld and von Siemens (2011) present a model where workers differ with regard to their prosocial preferences, worker preference type is private information, and firms compete for workers by offering wage contracts that can provide monetary incentives for individual effort but not team effort. Their results show that there is no pooling in equilibrium, but that workers endogenously sort into firms whose incentives are best aligned with own prosocial preferences. Relatedly, in an experimental labor market, Cabrales et al. (2009) find that employers and employees with similar social preferences self-select into a commonly preferred incentive platform. However, a more accurate explanation of observed levels of teamwork is difficult without careful control of worker

preferences, the degree of incentives, and also, as the present paper argues, explicitly organizational culture.

Laboratory experiments provide a means of circumventing this challenge by providing tighter control. Thus, it is not surprising that there has been a recent surge of experimental research studying the dynamics of teams. For instance, collaborative decision making within confronting teams has been shown to induce more aggressive behavior closer to the predictions of rationality (Cooper and Kagel, 2005). Using a contest structure similar to ours, Sutter and Strassmair (2009) study the effects of intra-team and inter-team communication on performance. They find that intra-team communication leads to higher performance. Yet, inter-team communication leads to the contrary. Even increasing the mere saliency of team membership in such situations suffices for an increased aggressive stance to arise (Charness et al., 2007 and Sutter, 2009).

To our knowledge, there is no other study examining the effect of priming (a prosocial or proself organizational culture) on performance in team contests and showing how this relates to individual preferences.

3 Theoretical considerations

In this section, we first present the contest game played by subjects and study its game-theoretical equilibria when contestants are self-interested. Then, we discuss the psychological values theory and the received understanding of the impact of external prosocial and proself primes or cues on the accessibility to individual values. We then propose a simple model of behavioral game theory of in-group favoritism drawing on Chen and Li (2009) and formalize the effect of priming in this theoretical framework. We also discuss the connections of this game theory model to values theory in psychology.

3.1 The contest game

We use the teams variant of the contest game first presented in Orrison et al. (2004). For expositional purposes we do not present the general model. Instead, we focus on the particular game actually played by subjects in the subsequent experiment. The game has six participants $i = 1, 2, \dots, 6$. The participants are equally divided into two teams $j = A, B$. Without loss of generality, we let $\{1, 2, 3\} = A$ and $\{4, 5, 6\} = B$. The strategy for each player i is to choose a level of effort $e_i \in [0, 100]$. Let $\mathbf{e} \in [0, 100]^6$ be the corresponding strategy profile. Exerting effort is associated with a cost $c(e_i) = e_i^2/(2c)$ with $c = 10$. Output is measured at the team level and is given by the sum of team members' efforts plus a random term, $X_j = \sum_{i \in j} e_i + \varepsilon_j$, where each ε_j is independently and uniformly distributed on the interval $[-q, q]$ where $q = 60$. The team with the highest output wins 4800 ECUs which are equally distributed so that every member of the winning team gets $M = 1600$ ECUs. Team members of the losing team each receive $m = 600$ ECUs. The individual profit function (for a risk-neutral player) is given by:

$$\pi_i(\mathbf{e}) = \Pr\{X_j > X_{-j} \text{ for } i \in j | \mathbf{e}\}M + (1 - \Pr\{X_j > X_{-j} \text{ for } i \in j | \mathbf{e}\})m - c(e_i) \quad (1)$$

where $\Pr\{X_j > X_{-j} \text{ for } i \in j | \mathbf{e}\}$ is the probability that team j to which i belongs has a higher output than the opposing team $-j$ conditional on the effort profile \mathbf{e} .

Consider a symmetric effort profile with effort level \hat{e} and a deviation by player i by $e_i > \hat{e}$. Given the uniform distribution, this approximately has an impact on winning probability by $(e_i - \hat{e})/(2q)$ and increases the private cost of effort by $(e_i - \hat{e})\hat{e}/c$. There are two alternative outcomes. Either player i 's team wins in which case i receives prize M or i 's team loses in which case the reward equals m . On the margin, a larger effort has a positive impact on the probability of being among the winners and negative impact on the probability of being among the losers. These marginal effects are of the same magnitudes but of opposite signs. The pecuniary relative benefit of increasing the probability of winning is thus of magnitude $M - m$. In the unique Nash equilibrium, players trade off the marginal benefits and costs and the corresponding symmetric equilibrium effort equals $e^* = \frac{c}{2q}(M - m) = 250/3$ (see Orrison et al., 2004 and Sutter and Strassmeier, 2009 for theoretical considerations).

It is less clear how other-regarding preferences affect the behavior in this game. In the following section we present the values theory of social psychology to understand prosocial or proself personal values and how their accessibility can be influenced by external primes and other cues, even by those associated with organizational values as we argue in this paper. Then we pin down a behavioral-game-theoretic Nash equilibrium prediction in the teams contest game using a simple social identity preference model where players are more altruistic towards ingroup members than towards other people, and outgroup members in particular.

3.2 Values theory

In psychology, values are considered to be desirable, stable, transsituational goals that vary in importance and serve as guiding principles in people's lives (e.g. Schwartz, 1992). They capture an essential part of an individual's personality relevant to motivation (Roccas et al., 2002). Values motivate behavior, are decision-making standards as well as guide attention and the interpretation of situational cues (e.g. De Dreu and Nauta, 2009; Maio et al., 2009; Schwartz, 1992; Schwartz et al., 2000).

Values differ in their motivational goal, for instance the value of power motivates behaviors to dominate others, seek recognition, wealth and authority. Schwartz's theory of basic human values proposes ten such value types organized in two higher-order dimensions. Furthermore, the theory posits that values show a systematic pattern of conflict and compatibilities. While valuing power is compatible and indeed associated with valuing achievement (i.e. seeking personal success through demonstrating competence according to social standards), power is conflicting with universalism (i.e. understanding, appreciation, tolerance and protection for the welfare of all people and for nature) and with benevolence (i.e. caring about the welfare of people to whom one is close). Past research widely supports the value theory. The structure and proposed pattern of relations of the ten value types could be replicated across more than seventy cultures (e.g. Schwartz, 2005). Associations of values with various outcomes including prosocial behaviors (e.g. Schwartz, 2005, 2009) as well as the stability of values over time have been demonstrated (Bardi et al., 2009).

Of particular interest for the present research are four values that constitute the higher-order dimension of self-enhancement (power and achievement) vs. self-transcendence value (universalism and benevolence). Self-enhancement value orientation reflects proself interest, a focus on extrinsic motivation, achievement, outperformance and, dominance; self-transcendence reflects prosocial motivation, a focus on intrinsic motivation and other-regarding interest (Schwartz, 2009).

A priori it is not clear whether prosocial values promote the effectiveness of team contests. First, prosocial values could make individuals do their best for their teams and work harder for own team success. Yet, these values might also motivate inter-team cooperation so as to give everyone equal chances of winning with the least effort. Proself values could further encourage team members to outperform the members of the opposing teams, or they may discourage performance due to the lack of within-team competition.

3.3 Social identity and in-group favoritism

To make better sense of how prosocial preferences and prosocial organizational culture may impact the behavior in team contests, let us cast the contest game in a simple model of group identity and ingroup-bias. This game-theoretic model is based on Chen and Li (2009) and has its origins in Tajfel and Turner’s (1979) social identity theory in social psychology. We note that values are a key part of individuals’ social identity and impact the way team and organizational goals are internalized, for instance (Johnson et al. 2010).

Chen and Li (2009) estimate other-regarding preferences and in-group bias in a large number of two-player interactions in a setting where the two parties’ group-membership is exogenously varied. In this economic decision making experiment, they provide evidence that many other-regarding concerns, such as charity and envy, depend on whether the other party belongs to the same group or not. Altruistic concerns for ingroup members, for instance, are more important than for outgroup members.

It is fairly straightforward to apply a simplified version of the Chen-Li model to the teams contest game framework. Let (without loss of generality) player i belong to team A . A simple model of group identity holds that the other-regarding payoff of player i is a weighted sum of all parties’ payoffs; π_k , $k = 1, \dots, 6$

$$\hat{\pi}_i(\mathbf{e}) = \mu\pi_i(\mathbf{e}) + (1 - \mu)[\omega \sum_{k \in A, k \neq i} \pi_k(\mathbf{e}) + (1 - \omega) \sum_{k \notin A} \pi_k(\mathbf{e})].$$

Altruism decreases with μ and pure self interest is captured by $\mu = 1$. In line with the findings of Chen and Li (2009), we assume that $\omega \geq 1/2$, i.e. altruistic concerns towards in-groups are more important than concerns for out-groups. The special case of no ingroup-bias corresponds to $\omega = 1/2$.

A competitive individual seeking to outperform others would have $\mu > 1$, implying a negative coefficient on others, $1 - \mu < 0$. As another special case, notice that a model of parochial altruism (Choi and Bowles, 2007) would hold that $0 < \mu < 1$ and $\omega > 1$ and thus that $0 > 1 - \omega$ so that i is altruistic towards ingroups and spiteful towards outgroups. Competitiveness and parochial altruism may be less transsituational than other values and more triggered by contest-like settings (Tajfel and Turner, 1979).

Clearly, benevolence (caring about the welfare of people to whom one is close) seems associated with ingroup-altruism ω . Universalism (protection of the welfare of all people) and self-transcendence in general seem associated with low values of μ (those below one and close to $1/2$ or even below) and self-enhancement (proself values related to achievement, outperformance, and dominance) would correspond to values of μ close to or above one.¹

¹One should consider these correspondencies as suggestive ways of organizing thoughts, the correlations between

Values theory suggests that priming has an impact on the accessibility to the corresponding personal value, if the value is a predominant one. In other words, the priming should impact those scoring high on the corresponding value. In our model, the parameter of altruism for in-groups, ω , and the weight on individualism, μ , are considered to be continuously differentiable functions of organizational culture, i.e. WE (prosocial prime) and ME (proself prime). Prosocial priming, for instance, should impact positively the ω -weights of those with a high ω , that is $\frac{\partial \omega}{\partial WE} > 0$ iff $\omega \geq \bar{\omega}$ where $\bar{\omega}$ is some threshold type. Proself priming should impact positively the μ -weights of those with high μ , that is $\frac{\partial \mu}{\partial ME} > 0$ iff $\mu \geq \bar{\mu}$ where $\bar{\mu}$ is some threshold type. More speculatively given that universal prosocial motivation can be considered as corresponding to values of μ below one and closer to 1/2, the prosocial prime could have a negative impact on the μ -weights of those with particularly low μ , i.e. $\frac{\partial \mu}{\partial WE} < 0$ iff $\mu < \underline{\mu}$ where $\underline{\mu}$ is some threshold type.

Subsection 3.1 derived the symmetric equilibrium of the teams contest game when $\mu = 1$. Let us now study the equilibrium in the general case. In addition to the pecuniary effects discussed earlier for the case when $\mu = 1$, there are non-pecuniary benefits that depend on the weights of other-regarding preferences. In the teams contest, for instance, the gross benefit to oneself also accrues to one's two teammates and thus positive effects are multiplied by the factor $\mu + 2(1 - \mu)\omega$. Yet, there is an opposite effect on the three members of the opposing team associated with a coefficient $-3(1 - \mu)(1 - \omega)$.

In a symmetric equilibrium, the marginal benefit of exerting effort must equal the marginal cost. So in a teams contest,

$$\frac{\mu e^*}{c} = \frac{(M - m)(\mu + (1 - \mu)(2\omega - 3(1 - \omega)))}{2q},$$

or, equivalently,

$$e^* = \frac{c(M - m)}{2q} \cdot \left(1 + \frac{(1 - \mu)(5\omega - 3)}{\mu}\right).$$

This implies that altruists with an in-group bias provide more effort than the self-interested with no other-regarding motives. Notice also that $\frac{\partial e^*}{\partial \mu} = -\frac{c(M - m)}{2q} \frac{5\omega - 3}{\mu^2}$, $\frac{\partial e^*}{\partial \omega} = \frac{c(M - m)}{2q} \frac{5(1 - \mu)}{\mu}$, and $\frac{\partial e^*}{\partial \omega \partial \mu} = -\frac{c(M - m)}{2q} \frac{5}{\mu^2}$. In words, effort is increasing in altruism when in-group favoritism is strong, i.e. if $\omega > 3/5$; and the stronger is the in-group bias, the stronger is the positive association of effort and altruism. Second, altruists' effort is increasing in in-group favoritism; the stronger is altruism, the stronger is the association between in-group favoritism and effort. Thus, the effects of altruism and in-group favoritism invigorate each other.

The effects of pro-self motivation on effort tend to be negative and smaller in absolute terms than on the prosocial side. Intuitively, self-interest tends to erode any motivation to sacrifice for one's team. Moreover, the effects may have an ambiguous sign. Stronger self-interest, for instance, promotes the effort of those without much in-group bias, $\mu = 3/5 - \varepsilon$, but erodes the effort of those with in-group bias, $\mu = 3/5 + \varepsilon$.

Let us now consider the impact of priming on the effort in our team contest model.² The effect

the values theory survey items and the behavioral measures (such as dictator giving or trustee backtransfer) have the right signs but they might not be strong enough to allow for definite conclusions. See the next section on experimental design and Table 6 in the Appendix.

²To keep the model simple and tractable, we have considered a symmetric equilibrium of a model with homogenous agents. This is admittedly a shortcoming and, ideally, one would consider the effects in an equilibrium model where all types are present at the same time.

of prosocial priming, WE, on equilibrium effort is proportional to

$$\frac{(1-\mu)5}{\mu} \frac{\partial \omega}{\partial WE} - \frac{(5\omega-3)}{\mu^2} \frac{\partial \mu}{\partial WE}. \quad (2)$$

Above we argued that $\frac{\partial \omega}{\partial WE} > 0$ iff $\omega \geq \bar{\omega}$ and, potentially, $\frac{\partial \mu}{\partial ME} < 0$ iff $\mu < \underline{\mu}$. Both effects in (2) are positive for prosocials if $\mu < 1$ and if $\omega \geq 3/5$. These latter two should simultaneously hold since μ and ω are negatively correlated (see the theoretical arguments in Section 3.2. and Table 6 in the Appendix for supportive evidence). Moreover, due to the negative correlation and $\frac{\partial e^*}{\partial \omega \partial \mu} = -\frac{5}{\mu^2}$, prosocial priming is expected to be particularly effective in team contests.

Let us then formalize the effect of proself priming, ME, on performance in the teams contest. This is negative iff

$$\frac{(1-\mu)5}{\mu} \frac{\partial \omega}{\partial ME} - \frac{(5\omega-2)}{\mu^2} \frac{\partial \mu}{\partial ME} < 0. \quad (3)$$

As argued above, $\frac{\partial \mu}{\partial ME} > 0$ iff $\mu \geq \bar{\mu}$ where $\bar{\mu}$ is some threshold type closer to one. Thus, the first effect in (3) should be approximately zero and of an ambiguous sign, since due to individual heterogeneity among the proselfs, μ varies on both sides of one. Moreover, the sign of the second effect is not expected to be particularly strong either. This is due to the fact that the negative correlation of μ and ω implies that ω conditional on $\mu \geq \bar{\mu}$ takes values possibly on both sides of $3/5$ and closer to $1/2$, so that the effect is ambiguous and small.

We summarize our theoretical results in the following paragraph.

Results: *Prosocial priming is expected to have a positive effect on performance in the teams contest due to its magnifying impact on prosocial individuals' concern for others, especially for the team mates. The impact of proself prime on performance, on the other hand, should be of insignificant and of ambiguous sign.*

4 Experimental design and procedures

In order to causally study person-organization fit in a controlled environment, we resort to an experimental design with three core building blocks. The first key element is an exogenous manipulation of values, i.e. priming, of which subjects are unaware. The priming condition serves as a proxy for the organizational culture. This part is operationalized by using word-puzzles with connotative words - a standard procedure in social psychology (Bargh et al., 2000) and further described below. The second pillar is the team contest game, which immediately follows the manipulation of the organizational culture and replicates the competing teams design of Orrison et al. (2004) and Sutter and Strassmair (2009). The core feature of this game is that a higher performance is monetarily more costly but leads to a greater likelihood of winning a monetary prize (see Section 3.1). The third pillar is an elicitation procedure for measuring personal values. To achieve this we utilize two complementary methods: first, a battery of choices in simple incentivized social interactions frequently used by economists; second, a PVQ survey tool, typically used by social psychologists (Schwartz et al., 2001). In what follows we discuss the implementation of these three pillars in detail.

4.1 The priming procedure

Immediately before the contest game, a word scramble is used to prime subjects into two alternative organizational culture conditions: a prosocial, self-transcendent organizational culture (WE) where universalism and benevolence values are the main dimensions and a competitive, proself, self-enhancement organizational culture (ME), where power and achievement values are the central dimensions. Finally, as a control, we have a third organizational culture condition, labeled as Neutral (N), where the words in the scramble tasks had no special connotation.

We use value-laden word-scrambles, which is a well-established, robust and widely used priming method (Bargh, 2006; Bargh and Chartrand, 2000; Maio et al., 2009).³ The word-scramble priming has the advantage of not requiring specialist technological equipment. It is a so-called supraliminal priming technique, where subjects are aware of the task itself, but are not aware that the pattern of words primes values. In more detail, the priming procedure requires subjects to construct a meaningful and grammatically correct sentence using four of the five words with which they are presented. We follow the procedures described in Bargh and Chartrand (2000) and also in Bargh et al., (2001). The subjects have to solve 30 items, i.e. scrambled sentences, 15 of which in the WE and ME condition are primed according to the WE or ME prime, respectively. We also have a neutral condition without any primed value (denoted N). The other 15 items in each condition represented neutral sentences. Examples are 'be want I helpful to would' (I want to be **helpful**; WE-item), 'target goals my for I' (I target my **goals**; ME-item), and 'am I today here would' (I am here today; N-item). Prime-words were taken from the Schwartz Value Survey (Schwartz, 1992), which is a theory-based and well-validated instrument to measure cultural and personal values and which lists a series of synonymous or specifying words for each value. For example, the prime words for WE were reliable, responsible, helpfulness, honest, loyal, forgiving, sincere, tolerant, just, wisdom, equality, peace, preserving nature, broad-minded, environmentally-conscious. Prime words were taken from the German version of the Schwartz Value Survey to circumvent translation problems.

Participants were presented with the scrambled sentences on a sheet of paper and were given an example of how to solve this 'word-puzzle' task. Prime-items and neutral items were alternated, in order to limit the likelihood that subjects become aware of the prime content. In the ex post questionnaire, subjects were asked a series of 'funneled' questions after the experiment (see e.g., Bargh et al., 2001). More specifically, subjects were asked: what they think the experiment tried to capture, whether they think their behavior in one task was influenced by another experimental task, if so what those influences were, whether they noticed something unusual in the word puzzle, whether they noticed some kind of pattern or common topic in the word puzzle items and, if so, what kind of pattern or common topic they noticed. Sixty-two respondents of the total 460 were excluded from the analysis as they recognized a common theme among the scrambled sentences (e.g. social justice, achievement, success, power). These respondents are uniformly distributed over the sessions and treatments and the results are robust to the inclusion of these respondents.

4.2 The contest game

Immediately following the priming, subjects switched to the contest game. The subjects were randomly matched to groups of six and further to teams of three to make effort choices in the

³For different ways to prime values see e.g., Bargh and Chartrand, (2000), Oyserman (2011) and Oyserman and Lee (2008).

contest. There were five groups, thus ten teams and thirty subjects in each session. The contest was repeated ten times, keeping the matching fixed. After each round, the subject learned whether she had won the prize and was reminded of her effort in that round. Once all ten rounds of the experiment had been completed, we handed out a questionnaire which, indirectly, inquired whether participants grasped the purpose of the experiment and the purpose of the word puzzle in particular (see the 'funneled' questions described in the previous paragraph 4.1). This is the standard procedure in priming experiments (see Bargh and Chartrand, 2000).

4.3 Elicitation of preferences and values

Once the questionnaire regarding the understanding of the priming condition had been completed (as described in Section 4.1), we started a new questionnaire with 19 questions on self-transcendence and self-enhancement values, a subset of the 40-question PVQ questionnaire (Schwartz et al., 2001, explained in the Appendix). Thereafter we elicited the preferences of the subjects using behavioral methods. First, risk preferences were evaluated through a battery of nine questions using the standard Holt and Laury (2002) risk-aversion elicitation method. Second, we elicited subjects' choices in three social interactions where "sharing" is an integral dimension: a Dictator game, an Ultimatum game and a Trust game. We elicited behavior in both roles for the two latter games (the instructions are available upon request). This amounts to 14 decisions. The subjects were told that one of the choices would be drawn as payoff-relevant and matched to a randomly drawn opponent's choice and paid-out accordingly at the end of the lab-part of the experiment.

One week and a half before the actual lab experiment, the subjects in a subset of the experimental sessions made choices in the same fourteen incentivized tasks (test-retest design). These choices were also incentivized. This pre-elicitation was conducted as a robustness check. We wanted to ensure that the elicitation of personal preferences through simple interactive tasks would not be influenced by the subjects' experiences during the contest phase of the experiment. In section 5, we show that our results are robust to using the pre-elicited values. The values survey was only administered after the performance task since in a pilot study, we learned that it contaminates the word puzzle manipulation if elicited *ex ante*.

4.4 Laboratory procedures

Subjects were recruited using ORSEE software (Greiner, 2004) and the performance task in the experiment was programmed and conducted using z-Tree software (Fischbacher, 2007).

In the laboratory, we were careful to directly control many other possible factors that might bias behavior or add noise. The temperature in the lab was set to 22°C and we always kept the curtains closed in an attempt to exclude or minimize the influence of temperature or weather. The sessions were always at 13h to provide maximum control of hour-of-day selection across prime comparisons. (Levitt and List, 2011 highlight the importance of such considerations.) One neutral priming session had to be run at a different point in time since all sessions had to be run within a week. We have control over experimenter effects since there was always one given staff member communicating with the subjects in exactly half of each of the priming conditions (implying a balanced sample when it comes to experimenter effects). Psychology students were excluded from the subject sampling frame, since they were likely to be familiar with priming studies. Also those

with any previous participation in priming experiments were excluded.

We checked the identity of the subjects and randomly allocated them to a visually isolated cubicle in the laboratory. They received a hard-copy of the instructions, written in German, and were told that everyone would get an identical copy. Once participants had read through the instructions, they received the word-puzzles with connotations of words depending on the experimental treatment (WE, ME, N). This served two purposes: a manipulation of organizational culture and a language comprehension test. In total, three subjects of those invited in 2009 were substituted by reserve subjects, because of lacking language skills. Word puzzles were always correctly filled out in the sessions in July 2011. Once two thirds of the subjects had completed the puzzle, the experimental contest game was started and subjects proceeded at an individual pace to complete the first round of the game. Each subject was instructed to first complete the puzzle before typing in their effort choice in the contest game.

Once all decisions had been completed, public draws of payoff-relevant tasks were made and subjects were paid individually according to their pre-elicitation and laboratory choices. This payout stage lasted 20 to 25 minutes. The actual lab-experiment lasted on average 1 hour and 10 minutes. The experimental timeline can be found in the Appendix.

5 Experimental Analysis

The data was collected over the period 2009-2011. We ran the first sessions in 2009 and the initial results encouraged us to collect more data to increase the statistical power needed for the analysis of interaction effects. We also changed the timing of the elicitation of the behavioral measure to ensure and verify a truly exogenous variation in that dimension as well. That is, we elicited the dictator giving and other interaction choices measuring individual prosociality (trust game, ultimatum game) both 1 1/2 weeks before the lab experiment (over the internet) and then again after the contest game. The choices were incentivized and paid out at the end of the laboratory experiment. We find no significant differences in dictator giving across these two timing designs, and our results are robust to the alternative timing .

Appendix A4 provides a detailed description of the experimental time line. Before we embark on the analysis, we provide some descriptive statistics of the behavioral measures that we use in the ensuing OLS regressions that strive to explain individual effort choices. In Table 1, summary statistics and a short description of the variables used in the analysis are presented.

As the measures in Table 1 try to elicit the same underlying preferences, it is also worth noting that they correlate in an expected manner. In the Appendix 6we provide a pairwise correlations along with a discussion. We also show that there is no statistically significant difference in the elicited measures over the different treatments (with the exception of Universalism) which indicates that randomization worked and that the prime did not spill over to these measures. Most importantly, the Dictator variable which is the main focus of our subsequent analysis does not differ significantly between treatments. We use OLS regressions to analyze the data and cluster the standard errors over the groups of subjects that interacted. In what follows we start by analyzing behavior in a team contest and then move on to robustness checks using alternative measures of prosocialty (section 5.2) and varying incentive design (section 5.3).

TABLE 1: DESCRIPTION OF STUDY AND CONTROL VARIABLES

Variable	Mean	s.d	Min	Max	Notes:
Dictator	0.30	0.21	0	1	How much the subject gave to a receiver in the dictator game
Benevolence	4.62	0.68	2	6	How strongly the subject agreed on the survey questions regarding this measure
Universalism	4.32	0.80	2	6	How strongly the subject agreed on the survey questions regarding this measure
Achievement	3.87	1.00	1	6	How strongly the subject agreed on the survey questions regarding this measure
Power	2.82	0.95	1	6	How strongly the subject agreed on the survey questions regarding this measure
Risk	6.60	1.59	1	10	When the subject first switched to a risky lottery in the Holt Laury list.
Trustee	0.29	0.20	0	1	Fraction of amount returned to the trustor in the Trust game
Trustor	0.55	0.50	0	1	Dummy variable for trusting the trustee (sending money) in the Trust Game
UltimatOf	0.43	0.10	0	1	Fraction offered to the responder in the Ultimatum Game
UltimatRe	0.340	0.17	0	1	Minimum amount accepted offer in a Ultimatum Game

5.1 Person-Organization Fit in Team Contest

A total of 231 subjects took part in the team contest sessions ($\#N = 88$, $\#WE = 70$ and $\#ME = 73$).

Table 2 presents the results of a set of different OLS regressions where individual effort is the dependent variable. In these specifications we systematically add independent variables. Model 4 tests the hypothesized interaction effects of person-organisation fit under team tournament incentives by interacting the Dictator variable with the priming condition (WE and ME primes with the Neutral prime as baseline) - with various control variables added in Model 5 and 6. We note that the coefficient on the interaction term $WE \times Dictator$ is positive and highly significant in all three models (4 to 6), indicating that more pro-social individuals tend to exert more effort when primed with a pro-social prime (independent of the specific set of control variables). On the other hand, the corresponding coefficient for the prosocial prime is not significantly different from zero. These effects are in line with the predictions of our group-identity model with ingroup-bias in altruism, see Section 3.3. We also estimated the model using a pre-elicited measure of giving in the Dictator game. As can be seen in Table 11 in the Appendix, this does not affect the main results.

We also note that the coefficient on the mere effect of prosocial priming becomes negative and significant when the measure of personal pro-sociality, Dictator, is allowed to interact with the prosocial priming, WE. This is intuitive as it indicates that those with prosocial values who give nothing to the opponent in the dictator game react to prosocial priming by providing less effort than when neutrally primed - an indication of a negative effect of mismatch of the person and the organization. Yet, this negative effect falls outside the scope of our model in Section 3.3 and, in fact, outside the values theory in social psychology which predicts that priming effects increase the accessibility to the primed value, if this value is a predominant personal value.

Models 3 and 6 are robustness checks using the self-reported PVQ value measures. Here we add all values at once, in the appendix A.2 we present regressions adding them one by one, but this does not change the results. We excluded the behavioral measures from the Trust game and the Ultimatum game in the main specifications presented in this section since they did not add any explanatory power in the OLS regressions. In section 5.2 we provide an analysis using both the ultimatum game acceptance threshold and the trustee back-transfer in turn as an alternative measure of prosociality instead of dictator giving. The regression results provide equally strong support for our conclusions.

Interestingly, we note that the Dictator variable is significant in Model 2, i.e. when no interaction term is added. The effect is positive indicating that overall under team tournament incentives, the more pro-social individuals exert higher effort as predicted by the model in Section 3.3. However, this effect disappears in subsequent models.

We further note that overall there seems to be no significant effect of gender, a finding in line with the recent study of Healy and Patt (2011) suggesting a smaller gender gap in team contests. Nor do we find any risk-preference effects which may be puzzling given the results of Shreemeta (2011), for instance. Yet, in team contests, the team members rely on each other and in a well-functioning team, each member trusts that others also contribute. From this perspective, our findings are in line with those of Eckel and Wilson (2004) finding no links between decisions to trust and decisions to take risk.

To get a better understanding of how the Dictator variable interacts with the priming conditions

TABLE 2: OLS REGRESSIONS TEAM TOURNAMENT

	(1)	(2)	(3)	(4)	(5)	(6)
WE	-2.205	-2.517	-0.499	-21.67***	-16.77**	-17.79**
	[3.669]	[3.604]	[3.365]	[7.312]	[7.696]	[7.292]
ME	-2.212	-2.229	-2.045	-4.106	-4.159	-4.440
	[3.668]	[3.587]	[3.386]	[6.637]	[6.595]	[6.410]
Dictator		16.66**	11.61	-3.306	-2.327	-5.926
		[7.768]	[7.821]	[10.19]	[9.886]	[9.874]
WE×Dictator				62.13***	51.11***	53.95***
				[15.97]	[17.20]	[16.70]
ME×Dictator				6.396	5.904	7.494
				[15.93]	[16.05]	[15.98]
Benevolence			2.608			3.323*
			[1.826]			[1.726]
Universalism			-1.240			-1.357
			[2.052]			[1.898]
Achievement			-0.963			-1.368
			[1.913]			[1.940]
Power			-0.408			-0.144
			[1.624]			[1.682]
Period			-0.604**		-0.604**	-0.604**
			[0.273]		[0.273]	[0.273]
Female			-4.153		-1.840	-2.153
			[3.289]		[3.164]	[3.315]
Risk			-0.941		-0.531	-0.571
			[0.875]		[0.887]	[0.901]
Constant	64.61***	59.69***	71.21***	65.59***	73.20***	70.77***
	[2.075]	[3.268]	[13.85]	[4.035]	[5.946]	[12.67]
Observations	2,310	2,310	2,280	2,310	2,280	2,280

Note: Effort is the dependent variable. N prime is the baseline. Period is a linear time trend and Female is a dummy indicating the gender of the subject. Robust standard errors in brackets (clustered at the group level). p<0.01, ** p<0.05, * p<0.1.

we have plotted the marginal effect of the prime dummy on each level of dictator giving, along with the corresponding 95 percent confidence interval. Figure 1 illustrates the strong match and mismatch effects of prosocial priming on subjects with different degrees of individual prosocial preferences as captured by the dictator variable. Those who are most prosocial are estimated to exert a 40 point higher effort under the prosocial (WE) than under the neutral prime, while the most proself individuals provide 20 points less effort. The modal person with a prosocial orientation of 1/2 is estimated to provide about 10 points more effort under the prosocial prime. Recall that the monetary opportunity cost on the upside is higher given the convex monetary cost of effort.

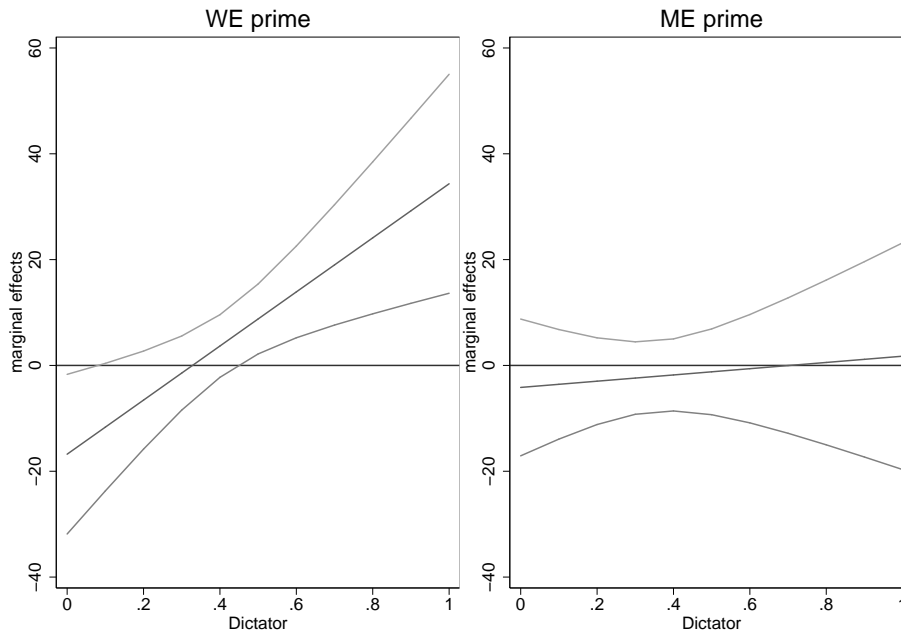


FIGURE 1: INTERACTION EFFECTS BETWEEN DICTATOR GIVING AND THE PRIMING CONDITION WITH 95% CONFIDENCE INTERVAL.

Non-parametric tests also support our findings. We first divide the group of participants into a pro-social half and a proself half at the median of dictator giving. Then we study the effect of priming on individual effort in the team contest. The effect of the WE prime on the effort of the prosocials is positive (p-values 0.077 and 0.028 for first period effort and average effort, respectively) but there is no significant effect of the ME-prime on the effort in this group (p-values 0.462 and 0.434 for first period effort and average effort, respectively). The effect of the WE prime on the effort of the proselfs is negative (p-values 0.001 and 0.02 for first period and average effort, respectively) and there is no significant effect of the ME-prime among the proselfs (p-values 0.256 and 0.727 for the first period effort, respectively).

The interaction effect indicates that the effect of prosocial priming is highly heterogeneous over the population and the average effect is ambiguous and will depend on the composition of the group with regard to prosocial individuals. This explains why we do not find any average treatment effects as shown in models 1-3 in Table 2. To complement this result Table 3 gives the per subject average exerted effort by treatment. A non-parametric Mann-Whitney U-test on the average effort choice in each group confirms that there is no difference between treatments. (Since subjects were matched

in fixed groups during the entire contest, these averages are statistically independent.)

TABLE 3: SUMMARY STATISTICS

	N	WE	ME	Total
Mean effort	64.61	62.406	62.399	63.243
s.d.	20.538	21.902	20.882	21.002

In sum, the regressions show a statistically highly significant interaction effect between personal preferences (using the dictator game giving as a behavioral measure for pro-social personal values) and pro-socially primed organizational culture. The interaction graph in Figure 1 further illustrates that not only is there a statistically significant positive effect of a pro-social organizational culture on the performance of the most pro-social types but further, the most pro-self orientated individuals under-perform under the same organizational culture. The fact that we get a lower effort on average is due to the average dictator giving being fairly low, about 0.3, and an important fraction giving nothing, in our sample. We do not observe the corresponding result for the prosel priming condition; once more in line with the model in Section 3.3.

A question that arises is whether the positive link between prosocial preferences and the prosocial prime is robust. In Sections 5.2 and 5.3 we perform two important robustness tests. The first is with respect to using different measures of prosociality, and the second is with respect to the underlying incentive structure, i.e. the triple-fit hypothesis.

5.2 Robustness Check I: Alternative measures of prosociality

In this section, we consider alternative measures of prosocial orientation. Instead of dictator giving, we interact the priming variables with ultimatum game acceptance thresholds (UltimatRe) and trust game backtransfers (Trustee). These are positively correlated with dictator game giving and higher values of these choice variables constitute more substantial deviations from the predictions of self-interested rationality (see Table 6 in the Appendix and Camerer (2003, pp. 43-117) for a detailed description of these games). For the sake of comparison, we also provide the corresponding interaction models using the first mover decisions in the trust and ultimatum games, the variables Trustor and UltimatOf, respectively. These first-mover choices are not only motivated by other-regarding concerns, but also by the expectations about the second-mover decisions. Not surprisingly, the interaction effects vanish.

Table 4 summarizes the findings from running OLS regressions with individual effort as dependent variable, using the alternative behavioral measures. The first column in the table replicates the results of Model 4 of Table 2 for a comparison. The full regression results of the corresponding models are available in Table 9 in the Appendix. Each column in Table 4 represents a regression where we have interacted the behavioral measure at hand with the treatment prime. The “Main” variable presents the coefficient of the main effect and the two “Interact” variables present the interaction coefficients.

TABLE 4: SUMMARY TABLE: INTERACTION EFFECTS OF ALTERNATIVE MEASURES OF PROSOCIALITY WITH PRIMED ORGANISATIONAL CULTURE (WE, ME)

	Dictator	UltimatRe	Trustee	UltimatOf	Trustor
Main	-3.306	-10.41	15.90	-7.200	5.897
Interact×WE	62.13***	62.90**	51.24**	65.81	10.01
Interact×ME	6.396	5.430	-29.05*	34.19	-0.876

From this table, we see that both UltimatumRe and Trustee interactions with the prosocial prime (WE) show highly similar results as for the Dictator variable. The reason for the non-significant result of UltimatumOf may be that behavior in the first-mover Ultimatum proposer position can be driven by both pro-social preferences and a fear of being rejected by the responder, for instance. On the contrary, the behavior of the responder (UltimatRe) is more clearly and purely driven by pro-social concerns when the strategic uncertainty about first-mover behavior has been resolved. On similar grounds, the second-moving Trustee’s behavior in the trust game (i.e. the amount returned to the Trustor) is more clearly driven by pro-social preferences than a first-moving Trustor’s behavior. We also see that the Trustee interacts negatively with the self-interest (ME) prime, albeit only at the 10% level, indicating a negative effect of misalignment of prosocial personal values and the prosocial prime. This is once more in line with the person-organization misfit conjecture, not yet predicted by the model of Section 3.3. However, since this effect is not present for the other measures, we refrain from extensively interpreting this result. Overall, we conclude that our results of a positive interaction between individual pro-sociality and the WE prime are robust, particularly for pure measures of pro-sociality.

We also ran analogous regressions using the PVQ-measures of prosociality and their interaction with the prime but the results are statistically insignificant. This may be due to the fact that we had to elicit the PVQ-measures ex post to prevent the subjects from gaining insight into the role of the word-puzzle in the experiment. See appendix A4.

5.3 Robustness Check II: Alternative Incentive Structure

In this section we check if the effect of priming in team contests carries over to individualistic incentive structures, where team motivation and in-group bias are absent. In particular, we conduct identical experiments as described above except that we now let individuals rather than teams compete. In total, 167 subjects participated ($\#N = 76$, $\#WE = 50$ and $\#ME = 41$) in this experiment that took place in July 2011. The contest is constructed in the following manner. Individual subjects now compete for three prizes in a group of six contestants. Players are ranked according to their individual output and the top three players each win a prize of 1600 ECUs. Thus, the private value of winning the contest coincides in this contest and the team contest. There is also the same number of winning players and the same number of losing players in this contest and the team contest.

Player i ’s output is given by $e_i + \varepsilon_i$. We keep the strategy set, the cost function, the size of the prize and the distribution of ε the same as in the team contest game. This set-up is very similar to the team contest except for the team formations. Indeed, Orrison et al. (2004) showed that the equilibrium effort level is invariant to the modifications and thus coincides in the two contest types.

We extend the results of Orrison et al. to allow for other-regarding concerns applying the model of Section 3.3. There are no salient groups in the competing individuals contest and the altruism weight for other players is $(1 - \mu)w$ for all players. Given monetary compensation π_k for $k = 1, \dots, 6$, the other-regarding payoff function of player i reads

$$\hat{\pi}_i(\mathbf{e}) = \mu\pi_i(\mathbf{e}) + (1 - \mu)w \sum_{j \neq i} \pi_j(\mathbf{e}).$$

We assume that $w = 1/2$ so that in-groups in team contests are assumed to have a higher altruistic weight than general others in the individual contest, $\omega \geq w$ (see Section 3.3. above). The general others then once more have a higher altruistic weight than out-groups, $w \geq 1 - \omega$. These assumptions are in line with the model and findings of Chen and Li (2009).

In a symmetric equilibrium of the competing individual contest, the expression for equilibrium effort satisfies

$$e^* = \frac{c(M - m)}{2q} \left(1 - \frac{5(1 - \mu)}{2\mu}\right) \quad (4)$$

and thus, an altruistic i is less willing and a competitive i is more willing to put in more effort in the individual contest than a self-interested i exactly as suggested by Bandiera et al. (2005) and Matsumura and Shin (2006) and as evident in the regression Models 3 and 4 in Table 5.

The self-interested i exerts exactly the same effort in the individual contest and the competing teams contest as shown by Orrison et al. (2004). In essence this means that we have a tight control over the incentive structure and hence, differences between the two contests cannot be attributed to differences in equilibrium behavior by self-interested, risk-neutral, and rational players (in the sense of a Nash equilibrium).

Taking the derivative w.r.t., μ reveals that the effect of proself-priming on equilibrium effort is proportional to $\frac{5}{2\mu^2} \frac{\partial \mu}{\partial ME}$. According to Section 3, the effect should apply to those scoring high on proself values, i.e. those with a high μ . This effect is positive but decreasing in μ since a high μ also implies a stronger emphasis on the private cost over any effects on the benefit side. This implies that one should perhaps expect weaker effects of proself priming than on the prosocial side.

The effect of prosocial priming, capitalizing through ω , is absent in the competing individuals model where in-group effects are absent. The potential effect $\frac{\partial \mu}{\partial ME} < 0$ iff $\mu < \underline{\mu}$ would have a negative impact on effort. Thus, we would expect a weak negative or no interaction effect between the Dictator variable and WE prime.

In Table 5, we neither find an effect of WE priming nor of ME priming. The lack of an interaction between WE prime and Dictator giving with individualistic incentives and the presence of the positive interaction effect with team incentives highlights the importance of the triple fit between personal preferences, organizational culture and the underlying incentive structure.

TABLE 5: OLS REGRESSIONS INDIVIDUAL CONTESTS

	(1)	(2)	(3)	(4)	(5)	(6)
WE	-4.023	-4.120	-1.314	-2.464	-0.328	-1.528
	[3.593]	[3.218]	[3.534]	[4.186]	[4.853]	[5.458]
ME	-3.254	-2.643	1.300	-6.319	1.635	1.235
	[4.843]	[4.648]	[4.572]	[7.303]	[4.717]	[5.222]
Dictator		-11.15**	-15.66**	-12.62	-14.49	-15.98
		[5.522]	[6.733]	[9.301]	[12.78]	[14.19]
WE×Dictator				-6.041	-3.678	0.722
				[11.75]	[15.77]	[17.80]
ME×Dictator				12.34	-1.708	0.216
				[14.44]	[14.73]	[15.44]
Benevolence			-0.262			-0.275
			[2.639]			[2.730]
Universalism			1.511			1.526
			[1.793]			[1.872]
Achievement			1.781			1.790
			[1.587]			[1.703]
Power			0.767			0.770
			[1.813]			[1.840]
Period			0.463*		0.463*	0.463*
			[0.239]		[0.239]	[0.239]
Female			1.803		1.497	1.793
			[2.947]		[3.100]	[3.063]
Risk			1.039		0.761	1.047
			[0.876]		[0.874]	[0.937]
Constant	79.72***	82.40***	55.95***	82.87***	72.26***	55.96***
	[2.110]	[2.134]	[12.22]	[2.676]	[6.656]	[12.51]
Observations	1,670	1,610	1,360	1,610	1,360	1,360

Note: Individual effort is the dependent variable. N prime is the baseline. Period is a linear time trend and Female is a dummy indicating the gender of the subject. Robust standard errors in brackets (clustered at the group level). $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

6 Discussion

The paper combines two strands of research, one on organization-person fit (e.g., Hoffmann and Woehr, 2006; Schneider, 1987) and the other on the optimal design of incentives in the economics (Prendergast, 1999) and management literatures (Gerhardt et al., 2009). Person-organization fit theory has so far mostly considered the match of people’s value preferences to the organizational culture without paying attention to the incentives used in the organizations. Similarly, economics and management research discuss incentive mechanisms generally or selection into these based on personal dispositions without considering the match with the wider organizational context such as the organizational culture.

In this study we have provided new causal evidence of the importance of person-organization fit (e.g., Hoffmann and Woehr, 2006; Schneider, 1987) and extend person-organization fit theory by demonstrating how the effect of P-O fit on performance is moderated by the incentive structure. In our experiment subjects are randomly assigned to three alternative priming conditions, proxying the organizational culture and two alternative incentive conditions. By controlling for pre-elicited

measures of personal preferences as well as measures of personal values, we can study causal interaction effects of preferences and organizational cultures on performance in various incentive schemes (team, individual).

We find that when subjects work in teams competing with other teams, there is a strong interaction between prosocial personal preferences and organizational culture - those who are prosocially oriented perform significantly better and those who are proself oriented perform worse than in the neutral baseline condition. Thus, our study provides experimental support for the notion that other-regarding organizational values facilitate team effectiveness (Mathieu, Maynard et al., 2008), at least for prosocially oriented individuals.

There are evolutionary reasons to expect that group members are more altruistic to ingroups than to out-groups. In fact Choi and Bowles (2007) show that a combination of altruism towards ingroups and hostility towards outgroups is persistent under evolutionary pressures. Such evolutionary arguments support the theoretical other-regarding preference model that we use in Section 3 to explain the differences in competing teams treatment. If the prosocial prime impacts the altruistic concerns for ingroups at a different rate (let alone to the opposite direction) than for outgroups, we should expect priming to generate the observed effects in the teams contest.

Practically, our findings suggest that organizations characterized by a significant presence of prosocial organizational values and prosocially motivated employees stand to reap measurable productivity gains from the use of team tournament incentives instead of the more traditionally used individual tournament incentives. This insight is particularly applicable to public sector, nonprofit and social enterprise organizations, who disproportionately attract and select managers and employees with a strong prosocial value orientation, other-regarding interests and predisposition to contribute to the public good, relative to pure for-profit businesses (Buurman et al., 2012; Besley and Ghatak, 2013). At the same time, our findings underpin the benefits to these organizations from not only carefully screening new employees for their social preferences, but also sustaining and reinforcing incumbent employees' prosocial preferences (Frey, Homberg and Osterloh, 2013). Having proself individuals working at a prosocial oriented organization under team tournament incentives effectively backfires, resulting in suboptimal performance by these individuals. It is as if proselfs are then put in a cheater mode, which can be very detrimental to the organization. By offering lower salaries or through other screening devices, prosocial organizations can effectively try to deter the 'cheaters' from joining, pretending to be prosocial. Once on the job, instances when a leader makes personal sacrifices (Fehr and Gintis, 2007) or hierarchical control is executed for the sake of the community rather than selfish interests lead to employee perceptions of higher organizational support and increased prosocial motivation (Osterloh and Frey, 2013)

Lastly, the ideas in this paper are also applicable to organizations that are turning away from hierarchical structures and towards flatter, more group-based structures, requiring employees to have increased interpersonal interaction and rely more on their coworkers (Grant and Berg, 2010). For these organizations, prosocial motivation may become a more significant source of employee motivation, and potentially play a bigger role in productivity. Our study highlights one specific strategy to this effect, namely for organizations to cultivate a more prosocial organizational culture and deploy team tournament incentives. More implicitly, our findings subscribe to the notion that coherent managerial decision-making across the domains of incentive design, recruitment and organizational culture is particularly instrumental to overall firm productivity, specifically when

prosocial employee motivations prevail (Ben-Ner, 2012).

While a lab-experimental study can always be criticized for relying heavily on extrapolation from the lab to the field, the methodology avoids some of the weaknesses of previous survey studies in real organizations (see Vogel and Feldman, 2009, Section 5 for instance). First, the method puts a firmer finger on causation. Second, by means of indirect and direct control, it avoids the potential for an omitted variable bias often driven by underlying selection effects. For instance, Vogel and Feldman (2009) point out that some of the benefits of person-organization fit are, in fact, driven by person-vocation fit, i.e. self-selection of individuals into occupations. Third, our experiment establishes a strong effect on a behavioral outcome measure, performance, the effects on which have been considered to be weaker and more contestable than those on attitudinal measures (Hoffman and Woehr, 2006). Fourth, we elicit an objective measure of prosocial orientation (dictator game) by using choices in simple social interactions as proxies. We also have a controlled exogenous variation in incentives and organizational culture, the two organizational variables of interest. These objective measures and exogenous variation allow for a more objective identification of a match between a person and an organization.

Finally, we suggest that our study also contains a methodological innovation. Methodologically, priming is only recently used in a few pioneering economics experiments, such as Ahmed and Salas (2011) using religious primes and Boschini et al. (2012) using gender primes. Our study can be seen to complement this growing literature. To our knowledge, there is only one other economic experiment studying the effect of prosocial priming on behavior. Drouvelis et al. (2010) find that, compared to a neutral prime, prosocial priming increases the effort in a one-shot public goods game. Although their findings are supportive of ours, we do not find any difference in average effort between the no priming and the prosocial priming condition - we only find the differential effect of the prosocial prime on prosocial and proself individuals. However, their study differs from ours in many aspects: first, the public goods game they consider has a different strategic structure as compared to contests and team contests. In public good games, the equilibrium efforts are inefficient whereas, in our case, deviating and contributing more than in the equilibrium decreases efficiency. In public good games, increasing the effort from equilibrium increases the expected payoffs from all other participants, whereas in ours, it has a positive effect on own team members only and a negative effect on others. Second, they study a one-shot interaction whereas we have a repeated situation studying more persistent priming effects. Previous studies on public goods games exhibit deteriorating contributions over time (Fischbacher and Gächter, 2010), thus validating this concern. Third, they do not control for individual prosocial preferences, which we show to constitute an important moderator of the prosocial priming effect. Rather than public good provision, the focus in this paper is to consider the contest nature of the workplace in an explicit manner comparing the two contest types. These are two related but separate questions due to the very different strategic nature of pure public good provision, on the one hand, and the contests, on the other hand (see Section 2).

In conclusion, our paper contributes a first causal test of person-organization fit theory, thereby re-affirming its validity. Furthermore, we extend person-organization fit theory by demonstrating that its effects are contingent on the dominant incentive scheme - and vice versa, that the effectiveness of tournament incentive schemes is contingent on organizational culture and pro-social individual preferences.

Appendix

A1. Descriptives

Table 6 provides pairwise correlations between our elicited measures. These indicate expected positive associations among the behavioral measures of prosociality, particularly dictator, ultimatum game responder (ultimatre) and trustee in the trust game. These measures also tend to show the expected positive relationships with benevolence and universalism - the two self-reported values capturing prosociality - as well as negative correlations with the two self-reported values capturing self-interest (achievement and power). The correlations among the self-reported values are furthermore consistent with Schwartz' value theory, in particular benevolence and universalism show high positive correlations with each other, as do power and achievement; whilst the correlations of prosocial values (universalism and benevolence) are negatively correlated with achievement and power.

Table 7 shows average values for all personality measures by treatment together with Kruskal-Wallis p-values. We note that there is no statistically significant difference which indicates that randomization worked and the prime did not spill over to these measures.

A2. Additional regression results for the team contest.

Table 8 is an extension of Table 2. In this table, where individual effort is the dependent variable, we add personal-value-orientation measures one-by-one in each of the columns.

Table 9 reports the results from OLS regressions, with individual effort as dependent variable, interacting the behavioral measures separately with the priming conditions. In this table the "Main" coefficient comes from the main effect of the corresponding column variable (e.g. Dictator). The "Interact" variable gives the interaction coefficient of the corresponding column variable with respective prime (e.g. Dictator \times ME for the third column).

Table 10 reports the results from OLS regressions, with individual effort as dependent variable, interacting the PVQ-measures separately with the priming conditions. In this table the "Main" coefficient comes from the main effect of the corresponding column variable (e.g. Benevolence). The "Interact" variable gives the interaction coefficient of the corresponding column variable with respective prime (e.g. Benevolence \times ME for the third column). In Table 11 we report OLS estimations using the pre-elicited Dictator variable, with individual effort at dependent variable. The number of observations is lower here since we did not pre-licit this measure in the 2009 experiments.

A3. Instructions

A3.1. Pre-elicitation Online Questionnaire

We here provide a shortened version of the exact instructions (more details are available from the authors upon request): "This is a study on decision-making behavior and personality. Our study

TABLE 6: CORRELATION TABLE FOR BEHAVIORAL MEASURES AND PVQ-MEASURES

	Dictator	UltimatOf	UltimatRe	Trustee	Risk	Trustor	Benevolence	Universalism	Achievement	Power
Dictator	1.000									
UltimatOf	0.250 (0.000)	1.000								
UltimatRe	0.209 (0.001)	0.401 (0.000)	1.000							
Trustee	0.751 (0.000)	0.277 (0.000)	0.199 (0.001)	1.000						
Risk	-0.033 (0.532)	0.124 (0.044)	0.034 (0.581)	0.018 (0.771)	1.000					
Trustor	0.338 (0.000)	0.153 (0.012)	0.184 (0.002)	0.408 (0.000)	-0.112 (0.068)	1.000				
Benevolence	0.148 (0.003)	-0.012 (0.850)	-0.033 (0.583)	0.104 (0.087)	0.046 (0.381)	-0.004 (0.944)	1.000			
Universalism	0.206 (0.000)	0.052 (0.391)	0.033 (0.594)	0.200 (0.001)	-0.035 (0.508)	0.088 (0.149)	0.514 (0.000)	1.000		
Achievement	-0.144 (0.004)	-0.068 (0.262)	-0.094 (0.123)	-0.261 (0.000)	-0.090 (0.087)	-0.128 (0.035)	-0.116 (0.021)	-0.188 (0.000)	1.000	
Power	-0.200 (0.000)	-0.114 (0.060)	-0.029 (0.633)	-0.210 (0.001)	-0.076 (0.150)	-0.062 (0.311)	-0.215 (0.000)	-0.314 (0.000)	0.611 (0.000)	1.000

TABLE 7: AVERAGE VALUES OF BEHAVIORAL MEASURES AND PVQ-MEASURES BY PRIME. P-VALUES FROM THE KRUSKAL-WALLIS TEST

	Mean(N)	Mean(WE)	Mean(ME)	p-value
Dictator	0.303	0.301	0.296	0.949
Benevolence	4.561	4.642	4.667	0.638
Universalism	4.196	4.450	4.357	0.070
Achievement	3.959	3.758	3.844	0.361
Power	2.896	2.719	2.807	0.491
Risk	6.453	6.724	6.658	0.560
Trustee	0.289	0.289	0.283	0.996
Trustor	0.562	0.495	0.591	0.512
Ultimateof	0.435	0.437	0.432	0.826
Ultimate	0.324	0.336	0.348	0.646

TABLE 8: OLS REGRESSION ADDING PVQ-MEASURES ONE-BY-ONE

	Benevolence	Universalism	Achivement	Power
WE	-22.34*** [7.280]	-21.72*** [7.312]	-22.19*** [7.074]	-22.10*** [7.187]
ME	-4.445 [6.553]	-4.145 [6.563]	-4.246 [6.667]	-4.403 [6.646]
Dictator	-5.533 [10.63]	-3.452 [10.12]	-4.874 [10.12]	-4.853 [10.15]
WE×Dictator	64.23*** [16.35]	62.25*** [15.97]	63.25*** [15.45]	62.99*** [15.79]
ME×Dictator	8.028 [16.12]	6.533 [15.85]	6.849 [15.96]	7.250 [15.95]
PVQ-measure	2.322 [1.844]	0.154 [1.777]	-1.272 [1.446]	-1.317 [1.238]
Constant	55.41*** [9.217]	64.96*** [8.924]	70.96*** [7.103]	69.72*** [5.822]
Observations	2,310	2,310	2,310	2,310

Note: Individual effort is the dependent variable. N prime is the baseline.

Robust standard errors in brackets(clustered on the group level). *** p<0.01, ** p<0.05, * p<0.1

TABLE 9: OLS REGRESSIONS WITH BEHAVIORAL MEASURES FOR TEAM TOURNAMENTS

	Dictator	UltimatOf	UltimatRe	Trustee	Trustor
WE	-2.517 [3.604]	-2.814 [4.666]	-3.333 [4.974]	-2.674 [4.394]	-1.535 [4.771]
ME	-2.229 [3.587]	5.580 [3.916]	5.139 [3.880]	5.300 [3.866]	5.499 [3.947]
Main	16.66** [7.768]	25.48 [19.72]	7.265 [11.53]	21.84** [9.945]	8.671** [3.379]
Interact×WE	62.13*** [15.97]	65.81 [43.03]	62.90** [26.90]	51.24** [21.03]	10.01 [8.672]
Interact×ME	6.396 [15.93]	34.19 [35.55]	5.430 [19.46]	-29.05* [16.92]	-0.876 [7.095]
Constant	59.69*** [3.268]	52.23*** [8.923]	61.22*** [4.295]	57.06*** [4.126]	60.11*** [4.189]
Observations	2,310	1,520	1,520	1,520	1,520

Note: Individual effort is the dependent variable. The “Main” variable is the main effect coming from the column variable (e.g. Dictator). The “Interact” variable interact the column variable with each of the primes. N prime is the baseline. Robust standard errors in brackets(clustered on the group level). *** p<0.01, ** p<0.05, * p<0.1

TABLE 10: OLS REGRESSIONS WITH PVQ-MEASURES FOR TEAM TOURNAMENTS

	Benevolence	Universalism	Power	Achievement
WE	-2.253 [3.585]	-2.224 [3.662]	-2.434 [3.677]	-2.445 [3.630]
ME	-2.075 [3.616]	-2.210 [3.658]	-2.220 [3.683]	-2.271 [3.687]
Main	2.227 [2.087]	0.338 [1.944]	-1.477 [1.511]	-1.712 [1.334]
Interact×WE	-1.106 [5.564]	-1.371 [5.070]	0.158 [3.375]	-0.907 [3.632]
Interact×ME	-0.443 [4.054]	6.812* [3.709]	5.153 [3.434]	3.315 [2.734]
Constant	54.222*** [10.31]	63.14*** [9.033]	70.30*** [6.151]	69.38*** [4.383]
Observations	2,310	2,310	2,310	2,310

Note: Individual effort is the dependent variable. The “Main” variable is the main effect coming from the column variable (e.g. Dictator). The “Interact” variable interact the column variable with each of the primes. N prime is the baseline. Robust standard errors in brackets(clustered on the group level). *** p<0.01, ** p<0.05, * p<0.1.

TABLE 11: OLS USING THE PRE-ELICITED DICTATOR VARIABLE

	(1)	(2)	(3)	(4)	(5)	(6)
WE	-2.205	-2.074	-0.302	-18.28	-26.35***	-26.23***
	[3.669]	[5.361]	[4.847]	[11.74]	[7.106]	[7.400]
ME	-2.212	6.005	5.201	5.632	4.852	5.111
	[3.668]	[4.000]	[3.423]	[6.044]	[5.798]	[5.562]
Dictator		-4.094	-0.801	-17.59	-16.21	-17.63
		[11.36]	[11.37]	[16.29]	[15.32]	[15.45]
WE×Dictator				50.97	84.42***	82.84***
				[36.55]	[20.04]	[20.42]
ME×Dictator				2.005	1.282	0.727
				[17.92]	[17.15]	[16.65]
Benevolence			-0.238			-0.117
			[2.276]			[2.265]
Universalism			-0.204			-0.140
			[2.914]			[2.711]
Achievement			-3.944*			-3.482
			[2.326]			[2.183]
Power			2.005			1.737
			[1.775]			[1.558]
Period			-0.357		-0.357	-0.357
			[0.321]		[0.321]	[0.322]
Female			0.763		3.190	3.126
			[4.169]		[3.843]	[4.128]
Risk			-2.593*		-1.880	-1.887
			[1.426]		[1.301]	[1.303]
Constant	64.61***	64.08***	93.43***	67.98***	80.05***	90.19***
	[2.075]	[3.798]	[16.76]	[4.751]	[8.614]	[14.52]
Observations	2,310	1,470	1,440	1,470	1,440	1,440

Note: Individual effort is the dependent variable. N prime is the baseline.

Period is a linear time trend and Female is a dummy indicating the gender of the subject.

Robust standard errors in brackets (clustered on the group level). $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

has two parts, part 1 is this web-based survey, in which you will take part over the next 15 min; part 2 will take place in about 1 weeks time in [location of lab experiment mentioned]. We ask you to make 14 decisions in the following survey. Please read the instructions carefully for each of the 14 decisions and then make your decision. Depending on your decision you can earn money. In particular, we will randomly choose one of your 14 decisions for actual payment. Your income is calculated in ECU (Experimental Currency Unit). The amount to be payed to you is the income earned by you in the randomly selected round and translated as 1000 ECU = 15 Euro. We will pay the money to you, as well as any income you earned during part 2, after part 2 of the study next week in [name of location].” This was followed by a technical instruction of how to move through the survey, whom to contact for help and followed by a request to fill in an anonymous code consisting of the first letter of the respondent’s mother first name, the first letter of her father’s name, the second letter of own first name, the first letter of place of birth and the last two digits of own year of birth. The second page of the survey asked for a dictator-choice decision (dividing 444 ECU). The third page had a trust game (from the trustee perspective) and the fourth page an ultimatum game from the receiver’s perspective. On the fifth page, we gave nine questions of the standard Holt and Laury (2002) risk-aversion measure and the sixth page had the trust game from the trustor perspective. Then, on the seventh page the ultimatum game was presented from the perspective of the offers. The eighth page asked for socio-demographic data (year of birth, number of years lived in the focal country, the highest completed educational level, area of study and gender). The reference in all games was 444 ECU. The final ninth page thanked respondents for their participation and indicated a contact email where they could reach the researchers for more information or concerns.

A3.2. Values orientation questionnaire (How similar are you to this person?)

The personal prosocial and proself PVQ-measures were captured with the Portrait Values Questionnaire (PVQ, Schwartz et al., 1999; Schwartz et al., 2001). The PVQ has been widely used in different contexts and shows good psychometric qualities. Psychometric quality refers to the measurement reliability of a self-reported measure in, e.g. psychological research. It is typically estimated with a Cronbach alpha coefficient (e.g., DeVellis, 1991). Cronbach Alpha reliabilities for the present sample were .80 for universalism (6 items) and .62 for benevolence (4 items), .87 for achievement (4 items) and .78 for power (3 items). The PVQ presents subjects with short portrayals of different people, each describing an individual’s goals, aspirations, or wishes that implicitly point at the importance of a single value type (Schwartz et al., 2001). For example, “It is important to Z to be rich. Z wants to have a lot of money and expensive things.” (power) or “E thinks it is important that every person in the world be treated equally. E wants justice for everybody, even for people E doesn’t know” (universalism). Following the protocol of the PVQ, proself orientation was captured with seven such statements (three capturing power, four achievement) and prosocial orientation with ten statements (four for benevolence and six for universalism). Statements were presented in random order. Subject rated the portrayals in response to the question “How much like you is this person?” on the following scale “very much like me”, “like me”, “somewhat like me”, “a little like me”, “not like me”, and “not like me at all”. Answers were coded 6 (very much like me) to 1 (not like me at all) and mean sum scores were calculated for the corresponding items per value.

A4. The experimental procedure

In our first sessions in 2009 we ran the competing teams treatment with prosocial and proself priming of organizational culture. In those sessions, we only had one behavioral measure of prosociality elicited before the contests, namely the dictator game. The data confirmed our hypothesis. This encouraged us to continue with a full-scale design where both competing teams and competing individuals tournaments were used, where (in addition to prosocial and proself priming) also a neutral priming benchmark was introduced. We also included further behavioral measures of prosociality in addition to the dictator game, such as the trust game and the ultimatum game (see Section 5.2), which were elicited 1 1/2 weeks prior to the experiment.

In April 2011, in our first larger-scale attempt, we again found support for our main hypothesis. Yet, the reliability of the data was questionable due to the high rate of subjects who understood the purpose of the priming. We conjectured that the failure was due to the similarity of the words in the pre-elicited values questionnaire and in the word-scrambles used in the priming of organizational values. Therefore, we decided to exclude the April 2011 sample from our analyses.

In July 2011, we ran the main sessions where the pre-elicited values questionnaire was abandoned. Only behavioral measures for other-regarding concerns were elicited beforehand. The fraction of subjects who understood the purpose of the priming was much lower in these sessions.

The July 2011 sessions constitute our main data set. The timeline of our 2011 July experiments was as follows.

1. Pre-elicitation (1 1/2 weeks prior to the experiment): dictator game, trust game, ultimatum game, risk-preferences (Holt and Laury, 2002).
2. ... 1 1/2 weeks passed
3. Subjects came to the laboratory
4. Those who had not completed the pre-questionnaire were set at the back of the queue.
5. Identity was verified.
6. Random seating number was drawn.
7. Instructions were distributed upside down. When everyone had their instructions, subjects were asked to turn them around, read them through carefully, and raise their hand if they had any questions.
8. Once the instructions had been read, the subjects retrieved their personal codes, which they had generated when answering the online pre-elicitation over the internet, and were asked to enter their personal code on the screen of the computer.
9. Subjects then started filling out the word puzzle sheet, which served both as priming the organizational culture and as a language test. Subjects were asked to raise their hand when done. Experimenters verified that the puzzles were correctly filled out. (In July 2011, all word puzzles were competently completed - we had reserved extra subjects as substitutes if the language skills had not turned out to be sufficient. As discussed in Section 4.4. in the 2009 session three subjects were substituted.).

10. When two thirds of the subjects had completed the word puzzle, the on-screen contest experiment was started. Each subject was required to fill out the scramble sheet before starting with the onscreen experiment.
11. After 10 rounds of contest, the subjects received a funneled questionnaire enquiring into what the participants considered the purpose of the study to be . This standard questionnaire in priming experiments was used to check whether subjects understood the purpose of the priming task and whether the priming task might have influenced their behavior (Bargh and Chartrand, 2000).
12. An on-screen personal value questionnaire was filled out (the self-enhancement and self-transcendence items of the PVQ, Schwartz et al., (2001), see Appendix A3.2).
13. The incentivized behavioral tasks were completed (dictator, ultimatum, trust game, risk preferences - i.e. the same elements that had been pre-elicited 1 1/2 weeks prior to coming to the lab).
14. Public transparent lotteries were run to randomly draw the payoff relevant tasks (one lottery for the pre-elicitation task and another for the laboratory tasks) and the lottery outcomes in risk preference tasks. (One subject was asked to come forward and verify the numbering of table tennis balls that were thereafter placed in an urn. The subject first drew a ball that determined the task that was to be paid out. If the task involved exogenous uncertainty (risk-preference measures), another draw was carried out with needed a replacement of table tennis balls.)

The average duration of a session was 1h 10 minutes without payout procedures and 1h 35 minutes until the last subject had received the remuneration. The temperature in the lab was set to 22°C. The curtains were drawn. There was always the same experimenter and two helpers such that we had a balanced sample when it comes to experimenter effects. The main sessions were carried out Mon-Fri, at 10h and at 13h each day. Competing teams sessions were always at 13h and competing individual sessions at 10h. One neutral priming teams session took place on Tue at 16h and one neutral priming teams session on Wed at 16h.

Psychology students were excluded since they are likely to be familiar with priming studies. Also those with any previous participation in priming experiments were excluded.

The procedures used in the collection of the data in May 2009 were identical to the procedures just described apart from the following points:

1. There was no pre-elicitation of behavioral measures of personal values 1 1/2 weeks before the lab experiment.
2. The dictator-variable was elicited as the first task in the lab, before handing out the instructions (after stage 6 and before stage 7 above). The dictator game was considered to be an additional round of the contest when randomly drawing one of the rounds as the payoff-relevant one. The amount shared in the dictator game was 1000 ECUs as opposed to the 444 ECUs in the 2011 experiment. Therefore, we normalized the dictator variable so that it varied between 0 (nothing given to the other) and 1 (everything given to the other). The distribution of normalized dictator giving in 2009 is not statistically significantly different from the distribution of 2011.

3. The Holt-Laury risk aversion measure was elicited immediately after the tournament rounds. The proportions of the stakes in the lotteries were identical to the stakes in the 2011 sessions but somewhat larger in absolute terms. The choice distributions in the 2011 and 2009 experiments are not statistically significantly different.
4. The ultimatum game and trust game choices were not elicited in 2009. These were added to check the robustness of our results and to have a more comprehensive set of proxies for other-regarding concerns (see Section 5.2).

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