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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 values on performance under individual and team contest incentives. We develop a model of regard for others and in-group favoritism that predicts interaction effects between organizational values and personal values in contest games. These predictions are tested in a computerized lab experiment with exogenous control of both organizational values 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 values condition, relative to the neutrally primed baseline condition. Further, when the prosocial organizational values are combined with individual contest incentives, prosocial subjects no longer outperform their proself counterparts. These findings provide, to our knowledge, a first, affirmative, causal test of person–organization fit theory. They also suggest the importance of a "triple-fit" between personal preferences, organizational values, and incentive mechanisms for prosocially orientated individuals.

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Keywords: tournaments • organizational culture • personal values • person–organization fit • teams • economic incentives

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 (e.g., Hoffman and Woehr 2006, Verquer et al. 2003, Schneider 1987). Erez and Earley (1993) and Gerhart (2009), among others, argue that personal preferences, organizational culture, and management practices interact and affect work behavior and performance. This view suggests that the benefits of the fit between personal and organizational values could be reinforced by adopting well-suited management practices, most notably the compensation schemes of employees.

Economists, for their part, have intensively studied the effects of compensation schemes on performance (Prendergast 1999, Lazear 1999, Harbring 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 values 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 value fit. In particular, we develop arguments that organizations need to align incentive schemes both with organizational values and with employee personal values to optimally motivate employee performance.

First, we use the values of universalism and benevolence to characterize prosocial cultures, and we use the values of power and achievement to characterize proself cultures (see Schwartz 1992, 2012). To formalize mathematically the impact of personal and organizational values on performance, we advance a variant of the model by Chen and Li (2009), which features differential regard for others in the in-group and in the out-group, befitting a setting where teams compete to win a prize (a team contest game; see Orrison et al.

2004). This mathematical model of regard for others captures the distinction between the prosocial values of benevolence (regard for the in-group) and universalism (regard for general others) in the Schwartz (1992, 2012) personal value classification. Our game-theoretical model predicts interaction effects between specific configurations of prosocial or proself organizational values and personal values on performance. We then conduct a controlled laboratory experiment where we use an innovative priming procedure to simulate organizational values in the laboratory. We measure individuals' personal value orientation and other-regarding preferences and study the effects of primed organizational values on individual effort in the team contests 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 prosocial organizational values than their proself-oriented counterparts. In particular, prosocial individuals exert more effort in an organizational culture that matches their preferences. By contrast, proself-oriented individuals 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 the 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 et al. (1995), for instance, show that team incentives are present in the majority of U.S. firms. Lazear and Shaw (2007) point out that since the late 1990s, 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 prosocial values such as benevolence and universalism, which we contrast with a proself-oriented culture by priming proself values such as achievement and power (see Schwartz 1992). We also introduce a neutral control condition where subjects receive a nonassociative 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 premeasure prosocial preferences using a battery of incentivized behavioral measures such as the dictator game transfer (the subject divides a sum of money between him- or herself and an anonymous recipient). For robustness checks, we also include the trust game back-transfer and the ultimatum game acceptance threshold (Camerer 2003). We also use a psychological self-report measure, the Portrait Values Questionniare (PVQ) survey tool (Schwartz et al. 2001). With exogenous control of organizational values and incentives, and with knowledge of individual prosociality, 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 employ to proxy organizational values has also been applied in psychological studies on 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). However, within management and organizational psychology, there is only limited research that investigates 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.

The procedure for priming organizational culture used in this study is justified for a number of reasons. First, notwithstanding that researchers use primes to prime personal or national culture values, they agree that primes are situational stimuli that temporarily enhance the accessibility of individual values. Our argument is that outside the lab, within an organization, organizational values act as situational stimuli that members of an organization are exposed to and that prime their personal values. Second, although we are unaware of other studies using value primes to prime organizational culture, such studies have been used to prime national culture (e.g., Oyserman 2011). National and organizational cultures, albeit not identical, bear strong similarities and are closely interlinked (House et al. 2004). We attribute the lack of past research using priming to simulate the effects of organizational values to the fact that priming and

experimental research, more generally, are rare in management or organizational psychology. Past priming research has predominantly been conducted by social psychologists who are not concerned with exploring organizational values; this contrasts with management and organizational researchers for whom organizational values are 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 1985, O'Reilly et al. 1991).

A further novelty of our research is that we bridge the person–organization fit literature with the economic literature on 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 to the ones described above where individuals instead of teams compete to win a prize. This is done in a controlled manner, keeping much of the structure (in particular, the game-theoretic self-regarding and risk-neutral 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 prosocial organizational values vanishes. This finding extends person–organization fit theory by pointing to the importance of a "triple-fit" of personal preferences, organizational values, and incentive mechanisms for those who are prosocially oriented.

Despite an impressive existing literature, empirical research on the benefits of the fit between personal preferences, organizational values, and management practices has been to date impeded by at least four factors. First, organizational culture is often operationalized as an overarching concept that embraces both organizational values and human resource management practices, which does not allow us to identify any potential interaction effects. However, a mismatch between organizational culture 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, Schwartz 1992). 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 external validity. Whereas 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, incentives, and culture, then the effect is also likely to be of importance in environments where the group, performance management schemes, and organizational values are more vividly and concretely present.[1] The experimental teams and individual contests, on the one hand, and the priming method, on the other hand, provide novel tools to proxy incentives and organizational values and thus to study their causal effects. Happenstance exogenous variation in organizational values is rare, and thus laboratory settings provide an interesting complementary avenue.

The rest of the paper is organized as follows. Section 2 discusses related studies in more depth to highlight how we complement the existing literature. Section 3 gives a theoretical overview by presenting the contest game, values and priming theory, and the application of the model of Chen and Li (2009) to value priming in the teams contest. Section 4 explains the experimental setup. Section 5 contains the experimental analysis. Section 6 discusses the results and concludes.

2. Related Literature

In this section, we present 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 compared with other individuals in an organization. Because of internal promotion, such practices are implicitly present in most organizations. Whereas Matsumura and Shin (2006) find such incentive schemes to be generally effective, the 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.[2] 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 of effort on fellow team members countervailing the negative externality on the members of the competing team. In this setting, the intrateam positive externality may well more than offset the interteam negative externality. More specifically, team members with particularly strong benevolence values (Schwartz 1992, 2012) will arguably tend to be nicer to the members of their in-group than their out-group (Billig and Tajfel 1973, Chen and Li 2009) or may even be hostile to their out-group as in parochial altruism (Choi and Bowles 2007, Rand et al. 2009).

It is important to analyze both competition and cooperation at the workplace because they are not necessarily polar opposites. The strategic nature of contests (e.g., Shreremeta 2011) differs from that of social dilemmas and public good games in three respects. First, 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, however, a positive externality benefiting one's own teammates in the teams contest.

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 significantly differ from dilemma and public goods games. Group optima in contests differ qualitatively from group optima in social dilemmas. Whereas classic public-good provision is often crucial at many workplaces, we believe that our contest games are equally important in understanding strategic features of workplace incentives.

There are few related experimental studies investigating organizational culture and cooperation. Chatman and Barsade (1995) assess students' disposition to cooperate, following the random assignment to either collectivist or individualist organizational cultures. They were interested in understanding how personal cooperativeness (a 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, which is consistent with their personality. We find similar effects in regard to matching such that proselfs underperform in the prosocial culture when exposed to a team contest incentive scheme. Chatman and Barsade (1995) 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 no longer display a higher performance. Thus, unlike Chatman and Barsade, we separate out different dimensions of organizational values, individual preferences, and incentive schemes, 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 the United States and conclude that group norms reinforce cooperation in the collectivist Chinese culture but not in the individualist American culture. Drouvelis et al. (2010) find that, compared with 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 proself 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 the personal values of the subjects, and it might be that their results are driven by the choices of the most prosocial 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 on the study of postmerger 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 in 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 their own prosocial preferences. In a similar way, in an experimental labor market, Cabrales et al. (2010) 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, explicit control of organizational values.

Laboratory experiments offer a means of circumventing these challenges 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 intrateam and interteam communication on performance. They find that intrateam communication leads to higher performance. Yet interteam 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, 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 effect varies with 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 game-theoretical model where we draw on the model of in-group favoritism in Chen and Li (2009) to introduce and formalize the effect of priming. We also discuss the connections between this game-theoretical model and 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 $i = 1, 2, \ldots, 6$. 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 $\mathbf{e} = (e_1, e_2, \ldots, e_6) \in [0, 100]^6$ be the corresponding strategy profile of all six participants. 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 4,800 experimental currency units (ECUs), which are equally distributed so that every member of the winning team gets $M = 1{,}600$ ECUs. Team members of the losing team each receive $m = 600$ ECUs. The individual profit function (for a risk-neutral player) is given by

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

where $\Pr\{X_j > X_{-j} \text{ for } i \in j \mid \mathbf{e}\}$ 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 to $e_i > \hat{e}$. Given the uniform distribution, this raises the 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 a 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 $e^* = (c/(2q))(M - m) = 250/3$ (for theoretical considerations, see Orrison et al. 2004 and Sutter and Strassmair 2009).

It is a priori not straightforward how other-regarding preferences affect behavior in this game. In the following section we use value theory in social psychology to understand prosocial or proself personal values and how the accessibility of these values can be influenced by external primes and other cues, notably those associated with organizational values as we argue in this paper. Combining insights from value theory and in-group favoritism, we then establish a behavioral game-theoretic Nash equilibrium prediction in the teams contest game using a simple social preference model where players are more prosocial toward in-group members than toward other people—out-group members in particular. Finally, we analyze how this prediction is affected by priming, and from this we derive hypotheses.

3.2. Value Theory and Prosocial Behavior

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 the essential part of an individual's personality that is relevant to motivation (Roccas et al. 2002). Values motivate behavior, are decision-making standards, and 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 and seek recognition, wealth, and authority. Schwartz's (1992, 2012) theory of basic human values proposes 10 such value types organized into two higher-order dimensions. Furthermore, the theory posits that values show a systematic pattern of conflict and compatibilities. Whereas valuing power is compatible and indeed associated with valuing achievement (i.e., seeking personal success through demonstrating competence according to social standards), power conflicts with universalism (i.e., understanding, appreciation, tolerance and protection for the welfare of all people and for nature) and benevolence (i.e., caring about the welfare of people to whom one is close). Past research widely supports value theory. The structure and proposed pattern of relations of the 10 value types have been replicated across more than 80 cultures (e.g., Schwartz 2005). Associations of values with various outcomes including prosocial behaviors (e.g., Schwartz 2005, 2010) as well as the stability of values over time have been robustly 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) versus self-transcendence values (universalism and benevolence). Self-enhancement values reflect 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 2010).

In-group bias is closely related to the prosocial value of benevolence. Schwartz (2012), when defining values in terms of the broad goals that they target, states that the defining goal of *benevolence* is "preserving and enhancing the welfare of those *with whom one is in frequent personal contact* (*the 'in-group'*)" (Schwartz 2012, p. 7, emphasis added). This suggests a close correspondence between in-group bias and benevolence. Similarly, the defining goal of universalism is "understanding, appreciation, tolerance, and protection for the welfare of *all people* and for nature." (Schwartz 2012, p. 7, emphasis added). This suggests that universalism measures a positive concern for others, irrespective of their group membership. In our correlation matrix in the appendix (Table A.1), we find a strong and significant positive correlation between the PVQ measures of universalism and benevolence.

The polar opposite to prosocial values are the proself (self-enhancement) values. The defining goals of proself values of achievement and power are stated by Schwartz (2012, p. 5) as follows: "Both power and achievement values focus on social esteem. However, achievement values (e.g., ambitious) emphasize the active demonstration of successful performance in concrete interaction, whereas power values (e.g., authority, wealth) emphasize the attainment or preservation of a dominant position within the more general social system." In line with these theoretical predictions, our data show that achievement and power are positively correlated and, moreover, that each of them is negatively correlated with universalism and benevolence (see Table A.1). These empirical patterns replicate previous findings in numerous studies eliciting the PVQ measures (Schwartz 2005, 2010).

Intuitively, the influence of prosocial and proself values on team contest play is ambiguous. Prosocial values could motivate individuals to do their very best for their team and work harder for own team success. At the same time, these values may also motivate interteam cooperation so as to give everyone an equal chance of winning with the least effort expended. Likewise, proself values could further encourage team members to outperform the members of the opposing team, or they may discourage performance as a result of the lack of within-team competition.

3.3. Formalization and Predictions Using Behavioral Game Theory

To advance our understanding of how personal prosocial preferences and prosocial organizational values may impact the behavior in team contests, let us cast the contest game in a simple other-regarding model

where the extent of regard for others depends on whether the other person is a member of the same group as the agent. Group membership is a key feature in team contests where two groups compete against each other.

Chen and Li (2009) pioneer such models and provide experimental evidence in favor of such preferences.[3] In particular, 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. Their economic decision-making experiment provides 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 in-group members, for instance, are more important than for out-group members. As argued in the previous section, personal values relate to personal goals, social identity, and in-group bias (Schwartz 2012), and personal values and preferences impact the way team and organizational goals are internalized (for instance, Johnson et al. 2010).

It is fairly straightforward to apply a simplified version of the model in Chen and Li (2009) to the teams contest game framework. Without loss of generality, let 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; $\pi_k, k = 1, \ldots, 6$:

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

The first term in the sum is player i's own payoff, which receives weight μ. Altruism decreases with μ, and pure self-interest is captured by $\mu = 1$, in which case the residual terms in the sum are 0. The second term sums over monetary payoffs of the teammates of i. This term receives a weight $(1-\mu)\omega$. The last term sums over monetary payoffs of the members of the rival team and receives a weight of $(1-\mu)(1-\omega)$. In line with the findings of Chen and Li (2009), we assume that $\omega \geq 1/2$; i.e., altruistic concerns toward in-groups are more important than concerns for out-groups. The special case of no in-group 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 toward in-groups and spiteful toward out-groups. Competitiveness and parochial altruism may be less transsituational than other values and more triggered by contest-like settings (Tajfel and Turner 1979).

3.3.1. Values, Goals, and Other-Regarding Payoffs. In-group bias and benevolence will be theoretically captured by high ω in our model. Universalism, for its part, is theoretically captured by low μ and thus by large positive concern, $1-\mu$, for all other participants in the contest. Proself values of power and achievement are the polar opposite of prosocial values; thus, proself values are represented by high μ (close to 1 or above 1) and low ω in our model.

Our behavioral measures of prosocial preferences (dictator giving and trust game back-transfer) are positively correlated with benevolence (in-group bias) and universalism (positive concerns for general others) and negatively correlated with the proself values of power and achievement (see Table A.1 in the appendix). Thus, the participants with high dictator giving, for instance, are expected to behave as agents with low μ:s and with high ω:s in the group contest. (Indeed, a simple formalization of the dictator and trust games would reveal that the lower μ is, the more an agent would give in the dictator game and transfer back in the trust game.) In addition, the participants with low dictator giving are expected to behave as agents with large high μ:s and with low ω:s in the group contest. Before pinning down the theoretical predictions in the teams contest game with such other-regarding concerns, let us study the impact of the organizational values on these personal concerns for others, i.e., the effect of the value primes on personal values.

3.3.2. The Effect of Priming on the Other-Regarding Parameters. Values theory suggests that priming has an impact on the accessibility to the corresponding personal value (see the introduction), particularly if the value is a predominant one.[4] In other words, experimentally priming a specific value should especially impact those scoring high on the corresponding value. In our model, the weight for in-group bias, ω, and the weight on own payoff, μ, are considered to be continuously differentiable functions of organizational values, which we proxy with the prosocial prime and the proself prime.

Prosocial priming, for instance, should impact positively the ω-weights of those with a high ω; that is, $d\omega > 0$ iff $\omega \geq \bar{\omega}$, where $\bar{\omega}$ is some threshold type. Second, given that universal prosocial motivation can be considered as corresponding to values of μ below 1 and closer to 1/2, prosocial priming should have a negative impact on the μ-weights of those with particularly low μ; i.e., $d\mu < 0$ iff $\mu < \underline{\mu}$, where $\underline{\mu}$ is some threshold type.

Proself priming should impact positively the μ-weights of those with high μ—that is, $d\mu > 0$ iff $\mu \geq \bar{\mu}$, where $\bar{\mu}$ is some threshold type—and negatively the ω values of those with low ω.

3.3.3. The Effect of the Personal Preferences and Organizational Values on Effort. Section 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 other-regarding preference weights. In the teams contest, for instance, the gross benefit to oneself also accrues to one's two teammates; 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,

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

Since the term $(1-\mu)(5\omega-3)$ is positive when $\omega > 3/5$ and $\mu < 1$, it is the altruists with an in-group bias who provide more effort than the self-interested with no other-regarding motives. Notice also that $\partial e^*/\partial\mu = -(c(M-m)/(2q))((5\omega-3)/\mu^2)$, $\partial e^*/\partial\omega = (c(M-m)/(2q))(5(1-\mu)/\mu)$, and $\partial^2 e^*/\partial\omega\partial\mu = -(c(M-m)/(2q))(5/\mu^2)$. In other words, effort is increasing in altruism (recall that μ falls when altruism gets stronger) when in-group favoritism is strong, i.e., if $\omega > 3/5$; the stronger the in-group bias, the stronger the positive association of effort and altruism. Second, altruists' effort increases in in-group favoritism; the stronger altruism is, the stronger the association between in-group favoritism and effort. Thus, the effects of altruism and in-group favortism reinforce one another.

The effects of proself motivation on effort tend to be negative and smaller in absolute terms than on the prosocial side: changes in ω have little impact on effort if μ is close to 1, and changes in μ have little impact on effort if ω is close to 3/5. Intuitively, self-interest tends to erode any motivation to sacrifice for one's team. Moreover, the effects may have an ambiguous sign. Stonger self-interest, for instance, slightly promotes the effort of those without much in-group bias, $\omega = 3/5 - \varepsilon$, but erodes the effort of those with a little more in-group bias, $\omega = 3/5 + \varepsilon$, for any small but positive ε. Similarly, stronger in-group bias slightly promotes the effort of those with a little altruism $\mu = 1 - \varepsilon$ but erodes the effort of the slightly competitive ones, $\mu = 1 + \varepsilon$.

Let us now consider the impact of priming on effort in our team contest model.[5] The effect of prosocial priming on equilibrium effort is proportional to

$$\frac{(1-\mu)5}{\mu}\, d\omega - \frac{5\omega - 3}{\mu^2}\, d\mu. \tag{3}$$

Above, we argued that the impact of the prosocial priming is positive, i.e., $d\omega > 0$, iff $\omega \geq \bar{\omega}$ and $d\mu < 0$ iff $\mu < \bar{\mu}$. Indeed, both effects in (3) are positive for prosocials if $\mu < 1$ and if $\omega \geq 3/5$. These latter two are likely to simultaneously hold since μ and ω are negatively correlated (for supportive evidence on the negative correlation, see Table A.1 in the appendix; for theoretical arguments, see Sections 3.2 and 3.3.3, after Equation (2)).

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

$$\frac{(1-\mu)5}{\mu}\, d\omega - \frac{5\omega - 3}{\mu^2}\, d\mu < 0. \tag{4}$$

As argued above, $d\mu > 0$ iff $\mu \geq \bar{\mu}$, where $\bar{\mu}$ is some threshold type closer to 1. Moreover, proself values are negatively associated with ω. Thus, if proself priming has an impact on ω, this impact will be negative; $d\omega < 0$ iff $\omega \leq \underline{\omega}$, where $\underline{\omega}$ is some threshold type below $\bar{\omega}$.

The first effect in (4) should be approximately 0 and of an ambiguous sign since, because of individual heterogeneity among the proselfs, μ varies on both sides of 1 (self-interest and competitiveness). Moreover, the sign of the second effect is not expected to be particularly strong either. This is because 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 again ambiguous and small.

We summarize our theoretical results in the following proposition.

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

4. Experimental Design and Procedures

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 values. This part is operationalized by using word puzzles with connotative words—a standard procedure in social psychology (Bargh and Chartrand 2000) and further described below. The second pillar is the teams

contest game, which immediately follows the manipulation of the organizational values 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 preferences and values. To achieve this, we utilize two complementary methods: first, a battery of choices in simple incentivized social interactions frequently used by economists; second, the PVQ survey tool, which is 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: (1) a prosocial organizational culture (WE) where universalism and benevolence values are the main dimensions and (2) a competitive, proself 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).[6] 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. Specifically, 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 or ME conditions are primed according to the WE or ME prime, respectively. We also have a neutral condition without any primed value (denoted as N); the other 15 items in each condition represent 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 (such as those italicized in the example sentences above) were taken from the Schwartz Value Survey (Schwartz 1992), which is a theory-based and well-validated instrument for measuring 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 as follows: reliable, responsible, helpfulness, honest, loyal, forgiving, sincere, tolerant, just, wisdom, equality, peace, preserving nature, broad-minded, and 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 to limit the likelihood that subjects would become aware of the prime content. In the ex post questionnaire, subjects were asked a series of "funneled" questions after the experiment (see Bargh et al. 2001). More specifically, subjects were asked the following: What do you think the experiment tried to capture? Do you think your behavior in one task was influenced by another experimental task, and if so, what were those influences? Did you notice something unusual in the word puzzle? Did you notice some kind of pattern or common topic in the word puzzle items, and if so, what kind of pattern or common topic did you notice? Sixty-two respondents of the total 460 were excluded from the analysis because 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 into groups of six and further to teams of three to make effort choices in the contest. There were five groups, thus 10 teams and 30 subjects in each session. The contest was repeated 10 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. After all 10 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 of Section 4.1). This is the standard procedure in priming experiments (Bargh and Chartrand 2000).

4.3. Elicitation of Personal 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 prosocial and proself personal 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.

A week and a half before the actual lab experiment, the subjects in a large subset of the experimental sessions made choices in the same 14 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 1 P.M. 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 the 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, or 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. After 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 complete the puzzle before typing in their effort choice in the contest game.

After 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 one hour and 10 minutes.

5. Experimental Analysis

The data were collected over the period 2009–2011 at the Max Planck Institute of Economics in Jena, Germany. 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 one and a half 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 A.4 provides a detailed description of the experimental timeline. Before we embark on the analysis, we provide some descriptive statistics of the behavioral measures that we use in the ensuing ordinary least squares (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.

Because the measures in Table 1 try to elicit closely related underlying preferences, it is also worth noting that they correlate in an expected manner. In the appendix we provide pairwise correlations along with a discussion. In short, Table A.1 in the appendix shows that both universalism and benevolence are typically significantly positively correlated with our

Table 1. Description of Study and Control Variables

Variable	Mean	SD	Min	Max	Description
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 and Laury (2002) 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	0.34	0.17	0	1	Minimum amount accepted offer in the ultimatum game

behavioral measures of prosociality—the dictator giving and the trustee back-transfer. Moreover, power and achievement are significantly negatively correlated with these behavioral measures. This suggests that the interpersonal variation in dictator giving and trustee back-transfers constitutes valid behavioral correlates of the prosocial–proself (self-transcendence versus self-enhancement) personal value dimensions. 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).

5.1. Person–Organization Fit in Team Contest

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

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–organization 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 Models 5 and 6. We note that the coefficient on the interaction term $WE \times Dictator$ is positive and highly significant in all three models (4–6), indicating that more prosocial individuals tend to exert more effort when primed with a prosocial prime (independent of the specific set of control variables). On the other hand, the corresponding coefficient for the proself prime is not significantly different from 0. These effects are in line with the predictions of our group-identity model with in-group 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 A.6 in the appendix, this does not affect the main results.

We also note that the coefficient on the mere effect of prosocial priming (WE) becomes negative and significant when the interaction $WE \times Dictator$ is included. This is intuitive, because it indicates that those with proself 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.

Models 3 and 6 are robustness checks using the self-reported PVQ-value measures. As a robustness check, we also run regressions, adding them one by one, but this does not change the results (see Appendix A.2). 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 when already controlling for dictator giving. 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 prosocial 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. This is a finding in line with the recent study of Healy and Patt (2011), which suggests a smaller gender gap in team contests. In addition, we do not find any risk-preference effects, which may be puzzling given the results, for instance, of Shreremeta (2011). Yet, in team contests, the team

Table 2. OLS Regressions Team Tournament

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 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

Notes. *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 are in brackets (clustered at the group level).

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

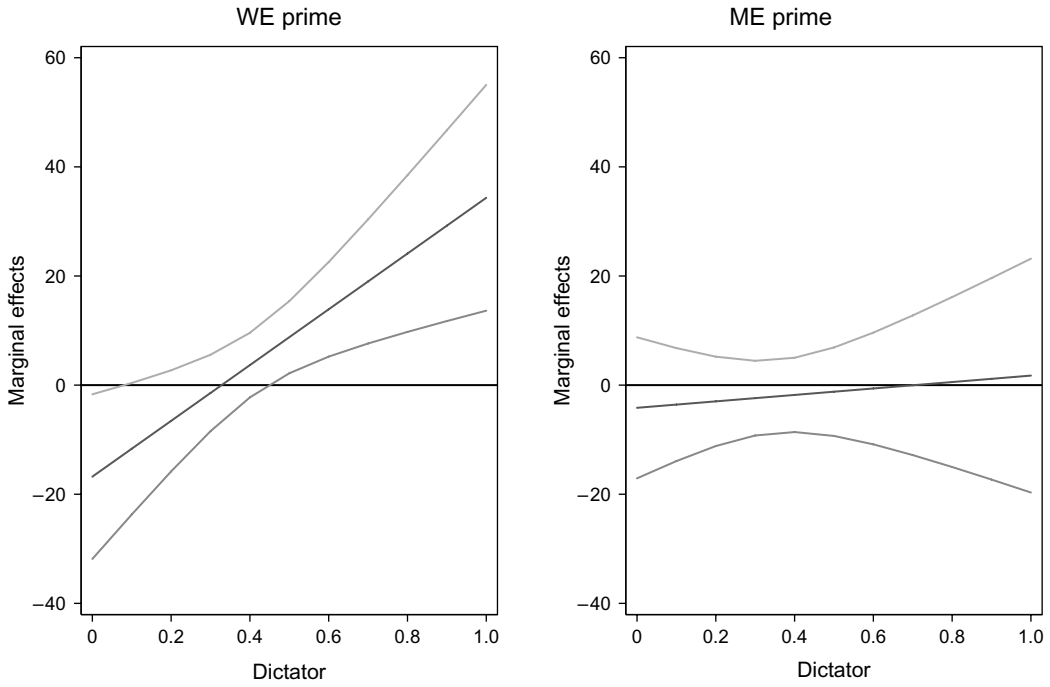
members rely on each other, and in a well-functioning team, each member trusts that others also contribute. From this perspective, our findings are in line with those of Eckel and Wilson (2004),who find no links between decisions to trust and decisions to take risks.

To get a better understanding of how the *Dictator* variable interacts with the priming conditions, we plotted the marginal effect of the prime dummy on each level of dictator giving, along with the corresponding 95% 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 in WE than in N, whereas the most proself individuals (who give nothing in the dictator game) provide 20 points less effort. The modal person, who donates half of the windfall earnings to a random stranger in the dictator game, is estimated to provide about 10 points more effort in WE. Recall that the monetary opportunity cost on the upside is higher given the convex monetary cost of effort.

Nonparametric tests also support our findings. We first divide the group of participants into a prosocial 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 of 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 of 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 of 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 of 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 nonparametric Mann–Whitney *U*-test on the average effort choice in each group confirms that

Figure 1. Interaction Effects Between Dictator Giving and the Priming Condition with 95% Confidence Interval



there is no difference between treatments. (Since subjects were matched in fixed groups during the entire contest, these averages are statistically independent.)

In sum, the regressions show a statistically highly significant interaction effect between personal preferences (using the dictator game giving as a behavioral measure for prosocial personal values) and prosocially primed organizational values. The interaction graph in Figure 1 further illustrates that not only is there a statistically significant positive effect of a prosocial organizational value on the performance of the most prosocial types, but further, that the most proself-orientated individuals underperform under the same organizational value prime. 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 proself priming condition, which is 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 prosocial preferences, and the second is with respect to the underlying incentive structure.

Table 3. Summary Statistics

	N	*WE*	*ME*	Total
Mean effort	64.61	62.406	62.399	63.243
SD	20.538	21.902	20.882	21.002

5.2. Robustness Check I: Alternative Measures of Prosocial Preferences

In this section, we consider alternative measures of prosocial preferences. Instead of dictator giving, we interact the priming variables with ultimatum game proposals (*UltimatOf*) and acceptance thresholds (*UltimatRe*), trust game transfers (*Trustor*) and back-transfers (*Trustee*).[7] It should be noted that these alternative measures are not expected to be perfect substitutes for dictator giving as a measure of prosocialness. For example, ultimatum game acceptance thresholds may be driven by reciprocal and spiteful preferences. Yet higher values of these choice variables constitute more substantial deviations from the predictions of self-interested rationality, and Table A.1 in the appendix shows that these alternative measures are, in fact, all positively correlated with dictator game giving.

Table 4 summarizes the findings from 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 A.4 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 represents the coefficient of the main effect, and the two *Interact* variables represent the interaction coefficients.

Table 4. Summary Table: Interaction Effects of Alternative Measures of Prosociality with Primed Organisational Culture (WE or 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

All coefficients on the interaction with the WE prime show the expected sign, and the first three are similar in magnitude to the *Dictator* variable; however, only the *UltimatumRe* and *Trustee* interactions are significant. We also see that *Trustee* interacts negatively with the self-interest (ME) prime, albeit only at the 10% level, indicating a negative effect of misalignment of prosocial preferences and the proself prime. This is once more in line with the person–organization misfit conjecture. However, since this effect is not present when other measures are used as regressors, we refrain from extensively interpreting this result.

One plausible explanation for the nonsignificant result of *UltimatOf* may be that behavior in the first-mover ultimatum proposer position can be driven by both prosocial preferences and selfish preferences with a heterogenous fear of being rejected by (inequity-averse or reciprocity-concerned) responders, hence rendering the measure more noisy. On the contrary, a higher acceptance threshold of the responder (*UltimatRe*) is less likely driven by beliefs (selfish preferences) and hence more clearly driven by other-regarding concerns, thus giving a more precise measure of prosocial preferences in general than *UltimatOf*. 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 prosocial preferences than a first-moving trustor's behavior. Table A.1 in the appendix again provides support for these views: the *Trustee* and *Dictator* variables are generally significantly correlated with achievement, power, benevolence, and universalism and with expected signs; yet the *Trustor* and *UltimatOf* variables are generally not significantly correlated with achievement, power, benevolence, and universalism, but when they are, the signs are as expected.[8] Overall, we conclude that our results of a positive interaction between individual prosociality and the WE prime are robust, particularly for pure behavioral measures of prosociality.

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 because 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 A.4.

5.3. Robustness Check II: Alternative Incentive Structure

In this section we check whether 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 (76 for N, 50 for WE, and 41 for ME) 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 1,600 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 setup is very similar to the team contest except for the team formations. Indeed, Orrison et al. (2004) showed that under risk neutrality and self-interest, the theoretical equilibrium effort level is invariant to the modifications and thus coincides in the two contest types.

We extend the results of Orrison et al. (2004) 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 $k = 1, \ldots, 6$, the other-regarding payoff function of player i reads as

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

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). The general others 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

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

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: this is exactly as suggested by Bandiera et al. (2005) and Matsumura and Shin (2006) and in line with regression models 2

Table 5. OLS Regressions Individual Contests

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 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

Notes. 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 are in brackets (clustered at the group level).

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

and 3 in Table 5, where the coefficient of the *Dictator* variable is significantly negative.

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 with respect to μ reveals that the effect of proself-priming (ME) on equilibrium effort is proportional to $(5/(2\mu^2))d\mu$. 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 (WE), capitalizing through ω, is absent in the competing individuals model where in-group effects are absent. The potential effect of prosocial priming through μ on prosocial individuals, $d\mu < 0$ iff $\mu < \underline{\mu}$ (see Section 3), 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.

To get a better idea of the relative strengths of the priming effects, assume that a representative prosocial individual (high universalism and benevolence) has $\mu = 1/2$ (consistent with 50–50 splits in the dictator game) and $\omega = 1$, and a representative proself individual has $\mu = 1$ (consistent with giving nothing in the dictator game) and $\omega = 1/2$. Moreover, assume that the effect of priming on the individual's predominant value weights are always of the same magnitude; i.e., $|d\omega| = |d\mu|$. By substituting these values into the formal expressions, we learn that the positive effect of prosocial priming on the effort of prosocial individuals in the teams contests is more than five times the effect of proself priming on the effort of proself individuals in the individual contests. Hence, the treatment effect is considerably smaller in the individual contest. In line

with these differences in relative impact, we find a significant effect in the teams contest but no significant effect in the individual contests. One should also expect a negative effect of prosocial priming on the effort of prosocials in the individual contest. This effect should be weaker than the effect in the teams contest but still about 3/4 of the latter effect under the current parameter configuration. But with some parochial altruism, $\omega = 1.2$, this ratio would already fall to about 1/2.

In Table 5, we do not find an effect of WE priming or 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 among personal preferences, organizational culture, and the underlying incentive structure.

6. Discussion

This paper combines two strands of research, one on organization–person fit (e.g., Hoffman and Woehr 2006, Schneider 1987) and the other on the optimal design of incentives in the economics (Prendergast 1999) and management literatures (Gerhart 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 discusses 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., Hoffman and Woehr 2006, Schneider 1987) and extend person–organization fit theory by demonstrating how the effect of person–organization fit on performance is moderated by the incentive structure. In our experiment subjects are randomly assigned to three alternative priming conditions, proxying exogenous variation in organizational values 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 values on performance in various tournament incentive schemes.

We find that when subjects work in teams competing with other teams, there is a strong interaction between prosocial personal preferences and organizational values—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 et al. 2008), at least for prosocially oriented individuals. Our findings are generally in line with the theory we formalize to predict the effect of organizational values on individual performance contingent on personal preferences. Our theory applies a variant of the model of in-group favoritism by Chen and Li (2009) capturing the distinction between the prosocial values of benevolence (regard for the in-group) and universalism (regard for general others) value classification of the Schwartz (1992, 2012) PVQ.

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

Recently, it has been emphasized that more attention should be paid to publication and other biases that might lead to false positive findings (e.g., Maniadis et al. 2014). We calculate the effect size for the observed effect of the prosocial prime on prosocial participants (above median dictator giving), yielding a Cohen's d of 0.47, which is considered a medium effect size (Cohen 1992). Using a 5% significance level, a sample size of 79 participants in the relevant treatments, and the fact that the sample is imbalanced (35 observations in WE prime and 44 in N prime), the power of the t-test in finding a difference in effort between the treatments is 70%. Maniadis et al. (2014) use formal modeling to illustrate how the rate of false positives depends not only on the observed significance level and statistical power but also on the prior probability of the hypothesis being true and on the number of independent researchers exploring the question. Assuming that we are the only research group exploring the above question and using prior probabilities of 10%, 50%, and 70%, we estimate the poststudy probability for a true relationship being reported as true at 61%, 93%, and 97%, respectively (see Equation 2 in Maniadis et al. (2014) for details). These calculations indicate that our findings are of practical relevance. Yet one of the main conclusions drawn from the analysis in Maniadis et al. (2014) is that replication will quickly reduce the risk of reporting false positives. We therefore encourage researchers to replicate our study.

In practical terms, 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 not only from carefully screening new employees for their prosocial preferences but also from sustaining and reinforcing incumbent employees' prosocial preferences (Frey et al. 2013). Having proself individuals working at a prosocially 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 deterimental to the organization. By offering lower salaries or through other screening devices, prosocial organizations can effectively try to deter the "cheaters" from joining and pretending to be prosocial. Once on the job, instances when a leader makes personal sacrifices (Fehr and Gintis 2007) or takes hierarchical control that are 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).

The ideas in this paper are also applicable to organizations that are turning away from hierarchical structures and toward flatter, more group-based structures that require employees to have increased interpersonal interaction and rely more on their coworkers (Grant and Berg 2012). 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).

Whereas 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 Section 5 of Vogel and Feldman 2009, for instance). First, the method puts a firmer grasp 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, that of performance, the effects on which have been considered to be weaker and more contestable than those on attitudinal measures in past correlational research (Hoffman and Woehr 2006). Fourth, we elicit an objective measure of prosocial preferences (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. From a methodological standpoint, priming has only recently been used in a few pioneering economics experiments (Benjamin et al. 2010, Ahmed and Salas 2011, Boschini et al. 2012; see also the brief review in Kamenica 2012). Our study can be seen to complement this growing literature. Perhaps the most related study is Drouvelis et al. (2010), who find that, compared with 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 compared with contests. In public goods games, the equilibrium efforts are inefficiently low, whereas, in our case, deviating and contributing more than in the equilibrium decreases efficiency. In public goods games, increasing the effort from equilibrium increases the expected payoffs for 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 constitute an important moderator of the prosocial priming effect. Rather than public goods provision, the focus of this paper is to consider the contest nature of the workplace in an explicit manner by comparing the two contest types. These are two related but separate questions because of the very different strategic nature of pure public-good provision and the contests (see Section 2).

Our study also has limitations, which point to fruitful avenues for future research. First, as argued in the introduction, relative performance incentive schemes are commonplace in organizations but, of course, not omnipresent. For us to be able to draw more general conclusions about the importance of person–organization fit in organizations, it remains to be seen if our results extend to other incentive schemes commonly employed—a question we save for future research. Second, as we outline in the introduction, there is a lack of research using priming to manipulate organizational values. We hope that future research can build on our work and perhaps start to differentiate the different layers of priming effects (e.g., effects of priming national as opposed to organizational culture) through manipulations checks.

In conclusion, our paper contributes a first causal test of person–organization fit theory, thereby reaffirming 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 prosocial individual preferences.

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Appendix

A.1. Descriptives

Table A.1 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's value theory (1992, 2012); in particular, benevolence and universalism show high positive correlations with each other, as do power and achievement, whereas the correlations of prosocial values (universalism and benevolence) are negatively correlated with achievement and power.

Table A.2 shows average values for all personality measures by treatment, together with Kruskal–Wallis p-values. We note that there is no statistically significant difference indicating that randomization worked and the prime did not spill over to these measures.

A.2. Additional Regression Results for the Team Contest

Table A.3 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 A.4 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 its respective prime (e.g., *Dictator* × *ME* for the third column).

Table A.5 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 its respective prime (e.g., *Benevolence* × *ME* for the third column). In Table A.6 we report OLS estimations using the pre-elicited *Dictator* variable, with individual effort as the dependent variable. The number of observations is lower here, since we did not pre-elicit this measure in the 2009 experiments.

A.3. Instructions

A.3.1. 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 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 week's 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 paid to you is the income earned by you in the randomly selected round and translated as 1,000 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 a request to fill in an anonymous code consisting of the first letter of the respondent's mother's 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 ECUs). The third page had a trust game (from the trustee perspective), and the fourth page had 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. On the seventh page, the ultimatum game was presented from the perspective of the offerer. The eighth page asked for sociodemographic data (year of birth, number of years lived in the focal country,

Table A.1. 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

the highest completed educational level, area of study, and gender). The reference in all games was 444 ECUs. The ninth and final page thanked respondents for their participation and indicated a contact email where they could reach the researchers for more information or concerns.

A.3.2. 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; see Schwartz et al. 1999, 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 0.80 for universalism (six items) and 0.62 for benevolence (four items), 0.87 for achievement (four items) and 0.78 for power (three items). The PVQ presents subjects with short portrayals of different people, each describing an individual's goals, aspirations, or wishes that implicitly point to 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 7 such statements (3 capturing power, 4 achievement) and prosocial orientation with 10 statements (4 for benevolence and 6 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

Table A.2. Average Values of Behavioral Measures and PVQ Measures by Prime

	Mean			
	N	WE	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
UltimatOf	0.435	0.437	0.432	0.826
UltimatRe	0.324	0.336	0.348	0.646

Note. *p*-Values are obtained from the Kruskal–Wallis test.

Table A.3. OLS Regression Adding PVQ Measures One by One

	Benevolence	*Universalism*	*Achievement*	*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

Notes. *Individual effort* is the dependent variable. *N* prime is the baseline. Robust standard errors are in brackets (clustered on the group level).
***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

Table A.4. OLS Regressions with Behavioral Measures for Team Tournaments

	Dictator		*UltimatOf*		*UltimatRe*		*Trustee*		*Trustor*	
WE	−2.517	−21.67***	−2.814	−31.65*	−3.333	−25.08**	−2.674	−17.11*	−1.535	13.980
	[3.604]	[7.312]	[4.666]	[18.22]	[4.974]	[11.14]	[4.394]	[8.397]	[4.771]	[6.871]
ME	−2.229	−4.106	5.580	−9.490	5.139	3.837	5.300	13.93**	5.499	5.940
	[3.587]	[6.637]	[3.916]	[16.18]	[3.880]	[6.336]	[3.866]	[6.408]	[3.947]	[4.173]
Main	16.66**	−3.306	25.48	−7.200	7.265	−10.41	21.84**	15.90	8.671**	5.897
	[7.768]	[10.19]	[19.72]	[20.71]	[11.53]	[11.30]	[9.945]	[11.95]	[3.379]	[4.528]
Interact × *WE*		62.13***		65.81		62.90**		51.24**		10.01
		[15.97]		[43.03]		[26.90]		[21.03]		[8.672]
Interact × *ME*		6.396		34.19		5.430		−29.05*		−0.876
		[15.93]		[35.55]		[19.46]		[16.92]		[7.095]
Constant	59.69***	65.59***	52.23***	66.64***	61.22***	66.69***	57.06***	58.53***	60.11***	66.01***
	[3.268]	[4.035]	[8.923]	[8.173]	[4.295]	[3.972]	[4.126]	[3.594]	[4.189]	[3.193]
Observations	2,310	2,310	1,520	1,520	1,520	1,520	1,520	1,520	1,520	1,520

Notes. Individual effort is the dependent variable. The *Main* variable is the main effect coming from the column variable (e.g., *Dictator*). The *Interact* variable is the interaction of the column variable with each of the primes. *N* prime is the baseline. Robust standard errors are in brackets (clustered on the group level).

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

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.

A.4. 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 and 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 because of 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.

Table A.5. OLS Regressions with PVQ Measures for Team Tournaments

	Benevolence		*Universalism*		*Power*		*Achivement*	
WE	−2.253	2.925	−2.224	3.882	−2.434	−3.285	−2.445	−0.157
	[3.585]	[26.22]	[3.662]	[23.17]	[3.677]	[13.34]	[3.630]	[10.18]
ME	−2.075	−0.004	−2.210	−31.74*	−2.220	−22.06	−2.271	−11.43
	[3.616]	[20.06]	[3.658]	[16.04]	[3.683]	[13.86]	[3.687]	[8.728]
Main	2.227	2.678	0.338	−1.081	−1.477	−3.208	−1.712	−2.514
	[2.087]	[3.317]	[1.944]	[2.810]	[1.511]	[2.007]	[1.334]	[1.572]
Interact × *WE*		−1.106		−1.371		0.158		−0.907
		[5.564]		[5.070]		[3.375]		[3.632]
Interact × *ME*		−0.443		6.812*		5.153		3.315
		[4.054]		[3.709]		[3.434]		[2.734]
Constant	54.22***	52.11***	63.14***	69.30***	70.30***	76.98***	69.38***	71.62***
	[10.31]	[16.12]	[9.033]	[12.90]	[6.151]	[7.863]	[4.383]	[5.093]
Observations	2,310	2,310	2,310	2,310	2,310	2,310	2,310	2,310

Notes. Individual effort is the dependent variable. The *Main* variable is the main effect coming from the column variable (e.g., dictator). The *Interact* variable is the interaction of the column variable with each of the primes. *N* prime is the baseline. Robust standard errors are in brackets (clustered on the group level).

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

Table A.6. OLS Using the Pre-elicited Dictator Variable

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 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

Notes. *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 are in brackets (clustered on the group level).
***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

1. Pre-elicitation (1 and 1/2 weeks prior to the experiment): dictator game, trust game, ultimatum game, risk preferences (Holt and Laury 2002).
2. … One and a half 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 they 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 Portrait Values Questionnaire was filled out (the self-enhancement and self-transcendence items of the PVQ; Schwartz et al. 2001; see also Appendix A.3.2).
13. The incentivized behavioral tasks were completed (dictator, ultimatum, trust game, risk preferences—i.e., the same elements that had been pre-elicited 1 and 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 a needed replacement of table tennis balls.)

The average duration of a session was 1 hours and 10 minutes without payout procedures and 1 hour and 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 were always the same experimenter and two helpers so that we had a balanced sample when it came to experimenter effects. The main sessions were carried out Monday–Friday, at 10 A.M. and at 1 P.M. each day. Competing teams sessions were always at 1 P.M. and competing individual sessions at 10 A.M. One neutral priming team session took place on Tuesday at 4 P.M. and one neutral priming teams session on Wednesday at 4 P.M.

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 and 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 1,000 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 (Holt and Laury 2002) 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).

Endnotes

[1] As in the classic minimal group paradigm (Tajfel and Turner 1979), our group assignment is anonymous. In contrast to the minimal group paradigm, where preferences over two paintings determine the group assigment and there are no payoff interdependencies, group assignment is here randomized, and the participants engage in strategic interaction with payoff interdependencies. In our setup, the payoff externality is positive toward the in-groups and negative toward the out-groups.

[2] 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.

[3] In social psychology, Tajfel and Turner (1979) provide seminal contributions to the literature.

[4] The effect of priming is commonly explained in reference to network theories of memory. The prime activates concepts related to the prime as well as action repertoires, which lead to the observable response (Bargh 2006, Custers and Aarts 2010). The stronger the networks links between the concepts and action repertoires, the stronger the effect of the prime. Thus, priming has a particularly strong influence on activities aligned with one's needs, motivation, and goals. Thus, prosocial priming should impact particularly those prosocially motivated. Karremans et al. (2006), for instance, demonstrate that subliminal priming of a drink brand name positively affected participants who were thirsty.

[5] To keep the model simple and tractable, we have considered a symmetric equilibrium of a model with homogeneous 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.

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

[7] See Camerer (2003, pp. 43–117) for a detailed description of these games.

[8] Even *UltimatRe* is generally not significantly correlated with achievement, power, benevolence, and universalism, but when it is, the signs are as expected. The weaker correlations may be due to somewhat different other-regarding motivations, such as negative reciprocity and spite, driving the rejection behavior, whereas positive reciprocity and altruism underlie trustee back-transfers and dictator giving.

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