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Hidden Costs of Control: Three Repetitions and an Extension*

Matteo Ploner† Katrin Schmelz‡ and Anthony Ziegelmeyer§

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Abstract

We report three repetitions of Falk and Kosfeld's (2006) low and medium control treatments with 364 subjects. Each repetition employs a sample drawn from a standard subject pool of students and demographics vary across samples. Our results largely conflict with those of the original study. We mainly observe hidden costs of control of low magnitude that lead to low-trust principal-agent relationships. Our subjects were asked, at the end of the experimental session, to complete a questionnaire in which they had to state their work motivation in hypothetical scenarios. Our questionnaires are identical to the ones administered in Falk and Kosfeld's (2006) questionnaire study. In contrast to the game play data, our questionnaire data are similar to those of the original questionnaire study. In an attempt to solve this puzzle, we report an extension with 228 subjects where performance-contingent earnings are absent i.e. both principals and agents are paid according to a flat participation fee. We observe that hidden costs outweigh benefits of control which shows that control aversion is more prevalent under hypothetical than under real incentives. Still, in the low control treatment, we observe much weaker negative responses to control in our extension than in the original study. This observation, the fact that the original study uses real incentives, and preliminary findings on the relationship between demographics and the degree of control aversion lead us to conclude that Falk and Kosfeld's (2006) experimental regularities originate from the characteristics of their subjects.

KEYWORDS: Control, Demographics, Experimental Economics, Incentives, Intrinsic Motivation.

JEL CLASSIFICATION: C81, C91, M52.

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1 Introduction

A core principle of economics is that individuals respond to incentives. Following the standard economic view, employers implement monitoring and incentive pay policies since the imposition of extrinsic incentives invariably leads to profitable increases in employees' effort. Some empirical studies on the effects of incentive pay on performance confirm that employees respond to extrinsic incentives just as economic theory predicts (Lazear, 2000; Bandiera, Barankay, and Rasul, 2007). However, since the work by Deci (1971), psychologists and sociologists have argued that providing extrinsic incentives for employees can be counterproductive, because it may reduce the employees' intrinsic motivation. Not only do many economists consider that the idea of motivation crowding-out effects holds some intuitive appeal, but several economic explanations have been suggested for the phenomenon and economists have demonstrated that these effects are empirically well-founded (Frey and Jegen, 2001). A broader view of human motivation therefore suggests that, in many employment relationships, the imposition of extrinsic incentives is likely to have two opposite effects on the employees' performance: a disciplining effect and a crowding-out effect. Whether imposing extrinsic incentives is beneficial to the employer depends on the relative size of the two countervailing effects. A worthwhile goal for economic research is to identify the individual and situational conditions under which crowding-out effects arise, and under which they are predicted to dominate disciplining effects.

In a recent contribution to this research agenda, Falk and Kosfeld (2006, henceforth F&K) have conducted a laboratory study to assess the relative importance of implicit incentives, explicit incentives and their interaction effects. F&K's experimental work consists of three main treatments, three robustness treatments and a questionnaire study (details are provided in sections 2 and 6). In their main treatments, F&K implement a principal-agent game where the principal either restricts the agent's choices by setting $x_{\min} = \underline{x}$ or leaves the agent's choices unrestricted by setting $x_{\min} = 0$ and the agent then chooses a transfer $x \in \{x_{\min}, x_{\min} + 1, \ldots, 119, 120\}$ resulting in a (monetary) payoff of $2x$ and $120 - x$ experimental currency units (ECUs) for the principal and the agent respectively. The minimum transfer requirement $\underline{x} \in \{5, 10, 20\}$ is a treatment variable which corresponds to the principal's degree of control. Experimental results show that control entails *hidden costs* caused by the existence of pro-social agents who choose a lower transfer if controlled than otherwise.

Hidden costs of control do not constitute a new phenomenon. As explained by Frey (1997), extrinsic incentives affect norms internalized by individuals and not only narrowly defined intrinsic motivation (see also Gagné and Deci, 2005). The negative reaction to control observed in F&K's study is a mere motivation crowding-out effect of ruling out the most opportunistic choices. Evidence for this effect is provided by, among others, Eckel, Grossman, and Johnston (2005) who study how subjects, endowed with US$20, play a single dictator game with a charity as the recipient and observe slightly higher adjusted donations when experimenters force subjects to donate at least US$2 than when experimenters force subjects to donate at least US$5 (in the US$2 forced donation treatment, donations less than US$5 are set to US$5). Hidden costs of control are in accordance with considerable laboratory research in social psychology that has shown that minimizing control is important for maintaining intrinsic motivation (Deci, Koestner, and Ryan, 1999) and with the field evidence by Nagin, Rebitzer, Sanders, and Taylor (2002) which shows that a non-negligible fraction of employees do not exploit reductions in monitoring to their own advantage (though a significant fraction of employees respond opportunistically to a reduction in monitoring). What is remarkable in F&K's study is that *most* agents react negatively to the implementation of control which in turn implies that imposing explicit incentives is detrimental to the principal (except for the highest level of control). This striking and economically relevant finding is surprising given the anonymity of

subjects' one-shot interactions and the nature of the decision environment.[1]

Indeed, a large body of evidence in psychology indicates that job characteristics and work climates strongly interact to influence employees' reactions to control and that distant principal-agent relationships are often characterized by a controlling leadership style whereas agents' autonomy is more prominent in close principal-agent relationships (Antonakis and Atwater, 2002 as well as Stanton, 2000 and the references therein). This evidence has been recently confirmed by economists and it suggests that a rather low crowding-out effect of control should be observed in the anonymous distant relationship treatments implemented by F&K (see our general discussion). Moreover, agents have the undeserved power to determine almost entirely the two players' payoffs in the implemented principal-agent game. This strongly asymmetric distribution of decision power should lead agents to easily forgive the implementation of control (Exline, Worthington, Hill, and McCullough, 2003). F&K's finding that hidden costs of control are substantial enough to undermine the effectiveness of weak extrinsic incentives is not only surprising but, if robust, it also has important implications for the design of employment contracts. Contrary to what standard economic theory predicts, the elimination of agents' most opportunistic choices may actually decrease the principal's payoff. Therefore, monitoring strategies would not only have to regulate the margin of employees who are opportunistic but they should also sustain the motivation of the large fraction of employees disinclined to shirk. For all these reasons, F&K's experimental results call for a check since scientific findings rest upon replication and only corroborated findings provide a sound foundation for the development of economic theory.[2]

In this paper, we first report three *repetitions* of F&K's C5 and C10 treatments. Our three repetitions reproduce as faithfully as possible the conditions of the original study with different subject pools (like F&K each of our experiments employs a standard subject pool of students). The primary purpose of the three repetitions is to check the robustness of F&K's experimental results to small changes in subject pools. In the first repetition, subjects are students from the University of Jena (federal state of Thuringia, Germany) and the sample composition according to educational background is (almost) equally distributed among the primary fields of academic study. In the second repetition, subjects are students from the University of Trento (province of Trento, Italy) and business and economics majors are over-represented (about 60% of the sample). In the third repetition, subjects are students from the University of Jena and the sample composition according to educational background is identical to the one of the second repetition. In each repetition the gender composition of the sample is approximately balanced. Overall, the findings of our first three experiments question the robustness of F&K's findings in their principal-agent game. We mainly observe hidden costs of control of *low* magnitude which lead to low-trust principal-agent relationships. The crowding-out effect rarely dominates the disciplining effect which is in line with the evidence on distant principal-agent relationships mentioned above.

[1] Fehr and Rockenbach (2003) as well as Fehr and List (2004) already found that imposing explicit incentives is detrimental to the principal. As explained by F&K, these two earlier studies share a fundamentally different setting than the one they consider. In these trust games it pays for the first mover to trust because trusting enhances the second mover's payoff which is reciprocated whereas in F&K's principal-agent game trust pays only because some of the intrinsically pro-socially motivated agents react negatively to the implementation of control. In other words, the notion of reciprocity provides a natural interpretation of the observation that imposing explicit incentives is detrimental to the first mover in these earlier studies. Conversely, in F&K's setting, theories of social preferences only based on payoff distributions as well as theories which take players' intentions into account rule out the possibility that, in equilibrium, the principal leaves transfers unrestricted and the agent responds positively to the principal's trusting decision. F&K's experimental design carefully separates trust from payoff-driven reciprocity.

[2] F&K do not argue that hidden costs *always* prevail over benefits of control (whenever high efforts are easy to enforce one expects the benefits to outweigh the costs of control). However, they conclude that their experimental results suggest important implications for the design of organizations. We feel that this conclusion is premature and we are skeptical about suggesting far-reaching recommendations based on the findings of a single laboratory study.

Once game play data had been collected, subjects were asked to complete a questionnaire in which they had to state their work motivation in hypothetical scenarios. Our questionnaires are identical to the ones F&K administered in their questionnaire study. In contrast to the game play data, our questionnaire data are similar to F&K's questionnaire data. Like in the original questionnaire study, we observe that in each scenario stated work motivation is statistically significantly lower in the condition where the principals controls or uses explicit incentive devices than in the condition where the principals trusts the employee. Hence, we observe little revealed control aversion but stated work motivation is significantly reduced by the implementation of control in our first three experiments. F&K, on the other hand, observe that control has a similar influence on revealed and stated work motivation. In an attempt to solve this puzzle, we decided to investigate the influence of hypothetical incentives on the play of the principal-agent game. In the second part of the paper, we report an extension of F&K's C5 and C10 treatments where performance-contingent earnings are absent i.e. both principals and agents are paid according to a flat participation fee. We observe that hidden costs outweigh benefits of control in C5 and C10 which shows that control aversion is more prevalent under hypothetical than under real incentives.

As a by-product of our study, we investigate whether the magnitude of hidden costs of control varies according to subjects' demographics. This investigation is motivated by the following two observations on agents' behavior. First, the findings of our third repetition (conducted in Jena with 60% of business and economics majors) are more in line with F&K's findings than the ones from our first repetition (conducted in Jena and majors balanced). This suggests that business and economics majors are more control-averse than other majors. Second, the findings of our third repetition are more in line with F&K's findings than the ones from our second repetition (conducted in Trento with 60% of business and economics majors). This suggests that Italian students in the province of Trento are less control-averse than German students in the state of Thuringia. In our general discussion, we test the reliability of these two suggestions with the help of regressions using experimental data from our second and third repetition as well as those from our extension. In these three experiments we collected information from subjects on basic demographic characteristics (academic major, age and gender) which enables us to compare the degree of control aversion across different sub-samples. Statistical results show that our Italian students with economics training are statistically significantly less control-averse than our German students with economics training but that the degree of control aversion of non-economists does not statistically significantly differ between the two regions. Additionally, none of the comparisons between sub-samples leads to the conclusion that economists are statistically significantly more control-averse than non-economists. Finally, in the absence of performance-contingent earnings, female non-economists are statistically significantly more control-averse than male non-economists. This gender effect is the only statistically significant one out of six comparisons.

The remainder of this paper is organized as follows. Section 2 summarizes F&K's experimental findings in their main treatments. Sections 3 to 5 discuss our three repetitions. Section 6 analyzes our questionnaire data and Section 7 presents our extension. We provide a general discussion in Section 8.

2 Falk and Kosfeld's main treatments

In their main treatments F&K implement a principal-agent game to study the potential interaction between control and effort in a parsimonious way. An agent has an endowment of 120 ECUs and chooses a transfer of x ECUs to the principal. For every ECU that the agent gives up, the principal receives two ECUs. Before the agent decides how much to transfer, the principal decides whether or

not to impose a compulsory transfer of $\underline{x}$ ECUs. F&K consider a low ($\underline{x} = 5$), a medium ($\underline{x} = 10$), and a high ($\underline{x} = 20$) control treatment (referred to as C5, C10 and C20 respectively) where principals and agents interact only once and they make use of the strategy method to elicit agents' transfers. Concretely, each agent is asked to choose a pair of transfers $\left(x^C, x^{NC}\right)$ where $x^C \in \{\underline{x}, \underline{x}+1, ..., 120\}$ is payoff-relevant in case the principal imposes a compulsory transfer and $x^{NC} \in \{0, 1, ..., 120\}$ is payoff-relevant in case the principal does not impose a compulsory transfer. A total of 140, 144, and 134 subjects participated in the control treatments C5, C10, and C20 respectively.

For a given agent's pair of transfers $\left(x^C, x^{NC}\right)$, the decision to impose a compulsory transfer has a total effect on the principal's payoff which is given by $2\left(x^C - x^{NC}\right)$. This total effect can be expressed as the sum of the direct and indirect effects of control which are given by $2\max\{\underline{x} - x^{NC}, 0\}$ and $2\left(x^C - \max\{\underline{x}, x^{NC}\}\right)$ respectively (see Schnedler and Vadovic, 2007). Hence, there is an indirect effect of control (or psychological reaction to control) if (i) $x^C \neq x^{NC}$ and (ii) $x^{NC} \leq \underline{x}$ implies $x^C > \underline{x}$. The indirect effect of control is positive if either $x^C > \underline{x} > x^{NC}$ or $x^C > x^{NC} \geq \underline{x}$, in which case there are hidden benefits of control. The indirect effect is negative, i.e., there are hidden costs of control, if $x^{NC} > x^C$. For a given distribution of pairs of transfers, statistically significant hidden costs of control are observed whenever negative psychological reactions to control "dominate" positive psychological reactions to control.

F&K's key findings can be summarized as follows: (i) Statistically significant hidden costs of control are observed in all control treatments (C5, C10 and C20); (ii) A clear majority of agents choose a pair of transfers such that $x^C - x^{NC} < 0$ in C5 and C10. Almost identical proportions of agents exhibit positive and negative reactions to control in C20; (iii) The average transfer by agents is higher when agents are not controlled than when they are ($\bar{x}^C - \bar{x}^{NC} < 0$). These differences are statistically significant in C5 and C10 but not in C20. In other words, in C5 and C10, principals earn less when they control their agents than when they do not control them, which implies that the hidden costs of control outweigh the benefits; and (iv) A clear majority of principals anticipates the adverse effect of control and choose to leave transfers unrestricted in C5 and C10. About half of the principals choose to leave transfers unrestricted in C20.[3]

Note that most theoretical models confirm the conventional economic view that the principal's ability to restrict the agent's choice set makes her better off. As F&K explain in their paper, payoff-maximizing principals will force agents to transfer at least $\underline{x}$ ECUs if all agents are payoff-maximizers, and also if some agents are fair in the sense that they have a concern for equity and/or efficiency. Even some applications based on psychological games, which take players' intentions into account, rule out the possibility that, in equilibrium, the principal leaves transfers unrestricted and the agent responds positively to the principal's signal of kindness (Rabin, 1993; Dufwenberg and Kirchsteiger, 2004; Falk and Fischbacher, 2006). However, two recent signalling models provide an economic explanation for the large motivation crowding-out effects observed in F&K's C5 and C10 treatments (Sliwka, 2007; Ellingsen and Johannesson, 2008). These two theoretical rationalizations exhibit a separating equilibrium in which some principals trust their agent and some agents react negatively to the principal's implementation of control but *they do not rule out the standard economic prediction.*

[3] F&K ran three robustness treatments for which treatment C10 served as a basis of comparison. First, they checked whether their results were an artifact of the strategy method by applying the specific response method in treatment SR10 (agents decided only after knowing the principal's decision). Their results do not indicate any effect of using the strategy method versus the specific response method in the considered principal-agent relationship. Second, they established that control only hurts the agent's motivation when the principal has a choice whether to impose control or not. Indeed, in treatment EX10 which is the subgame of treatment C10 following an exogenously imposed control decision, meaning that the agent always has to choose a transfer of x ECUs in the set $\{10, 11, \ldots, 120\}$, the negative effect of control vanishes. Third, F&K explored the validity of their results in a more general economic setting by implementing a gift-exchange treatment (GE10) where the principal not only determines the agent's minimum level of x, but also pays the agent a wage. They found that the reciprocal relationship between wages and effort is weaker in the presence of control in treatment GE10.

3 Experiment 1: Repeating F&K's C5 and C10 treatments

F&K observe hidden costs of control in all their main treatments. However, hidden costs outweigh benefits of control only in C5 and C10. In the C20 treatment, where principals have the possibility to impose a higher level of control, the motivation crowding-out effect and the disciplining effect break even. Thus, the striking nature of F&K's experimental findings is confined to the low and medium control treatments and a robustness check of the high control treatment's results seems of little interest.

Our first experiment repeats F&K's C5 and C10 treatments to assess the robustness of the overall negative effect of control on effort. From now on, we refer to the two principal's choices as the control and no-control conditions.

3.1 Experimental design and practical procedures

The four sessions of the computerized experiment were conducted at the Experimental Laboratory of the Max Planck Institute of Economics (ELMPIE) in Jena. The experiment was programmed with the software z-Tree (Fischbacher, 2007) and we used a slightly modified version of the code employed by F&K (we enlarged the fonts in order to facilitate subjects' reading of the computer screens and we added a questionnaire). Subjects were invited using the ORSEE recruitment system (Greiner, 2004). All 118 subjects were students from the University of Jena.[4] In each session the gender composition was approximately balanced. No subject participated in more than one session. Some subjects had participated in earlier economics experiments, but all were inexperienced in the sense that they had never taken part in an earlier session of this type. The top panel of Table 1 shows the sample composition according to educational background which is partitioned into four categories: business administration & economics, other behavioral & social sciences (education sciences, law, political science, psychology, and sociology), humanities, and engineering, life & natural sciences. Each category is well represented in both treatments. Subjects interacted only once, each session lasted on average 40 minutes and the average earnings per subject were about 9.25 euros including a 2.50 euros show-up fee.

ECUs were converted to euros in the calculation of subjects' final earnings at a conversion rate of 10 ECUs to 1 euro. F&K used an almost equivalent conversion rate but paid their subjects (the equivalent of) a 6.50 euros show-up fee. In the ELMPIE, subjects are usually paid a 2.50 euros show-up fee in addition to their performance-contingent earnings. We decided to not deviate from the usual laboratory practice and therefore put more emphasis on the performance-contingent earnings compared to F&K.

At the beginning of each session subjects randomly drew a cubicle number. Half of the subjects were assigned the role of a principal and the other half the role of an agent, depending on their cubicle number. Cubicles were visually isolated from each other and communication between the subjects was strictly prohibited. Once all subjects sat down in their cubicles, instructions were distributed. Principals and agents were given different sets of instructions. We used the exact same instructions as F&K but modified them slightly to account for the change of currency, show-up fee and funding institution (see Appendices A and B of the supplementary material).

Each subject first read his own instructions silently and then, to ensure common information, a monitor read a summary of the two sets of instructions aloud (the monitor was a native German speaker who did not know the purpose of the experiment). Questions were answered privately.

[4] More precisely, almost all the subjects who took part in the experiments we conducted in Jena were students from the Friedrich Schiller University of Jena and a few (less than 10%) were students from the University of Applied Sciences Jena.

	C5	C10	Pooled
Experiment 1			
Business Administration & Economics	26.56	31.25	28.91
Other Behavioral & Social Sciences	21.88	20.31	21.09
Humanities	20.31	25.00	22.66
Engineering, Life & Natural Sciences	31.25	18.75	25.00
Unknown	0.00	4.69	2.34
Experiment 2			
Business Administration & Economics	50.00	63.33	56.90
Law	28.57	16.67	22.41
Other Behavioral & Social Sciences	5.36	5.00	5.17
Humanities	7.14	8.33	7.76
Engineering, Life & Natural Sciences	8.93	6.67	7.76
Experiment 3			
Business Administration & Economics	59.37	63.63	61.54
Law	21.88	12.12	16.92
Other Behavioral & Social Sciences	9.37	9.09	9.23
Humanities	6.25	7.58	6.92
Engineering, Life & Natural Sciences	3.13	7.58	5.39
Experiment 4			
Business Administration & Economics	20.17	24.56	22.37
Other Behavioral & Social Sciences	26.32	32.46	29.38
Humanities	31.58	20.17	25.88
Engineering, Life & Natural Sciences	21.93	22.81	22.37

Table 1: Subjects' educational background (percentages).

Note: In Experiment 1, subjects' educational background has not been elicited directly. The percentages indicated in the table have been computed with the help of the lists of registered subjects generated by the online recruitment system. In treatment C10, the educational background of three of the registered subjects is missing and the category 'Unknown' is added.

Understanding of the payoff functions was assured by a control questionnaire. Subjects had to calculate the two players' payoffs in three exercises with hypothetical decisions. After all subjects had answered correctly the control questionnaire, subjects played the principal-agent game once. At the end of the one-shot interaction, subjects' payoffs were displayed on their screens.

Before leaving the laboratory, subjects were asked to complete a questionnaire in which they had to state their work motivation in five hypothetical scenarios (subjects did not know at the beginning of the session that they would have to answer a questionnaire). Different subjects answered different versions of the questionnaire, the two versions being identical to the ones F&K administered in their questionnaire study. For each scenario, there is a condition in which the principal trusts the agent and a condition in which the principal controls the agent. Each subject went through only one of the two conditions for a given scenario (see Appendices C and D of the supplementary material). Answering the questionnaire was mandatory but not incentivized and subjects were informed that their answers to the questionnaire would not affect their previous earnings. After completion of the questionnaire, subjects privately retrieved their final earnings (including the show-up fee).

The top panel of Table 2 shows some of the features of our first experiment.

	C5	C10	Pooled
Experiment 1			
Location	Jena		
Number of sessions	2	2	4
Number of subjects	60	58	118
Gender (% female)	50.00	53.12	51.56
Average age (std. dev.)	24.20 (2.35)	24.10 (2.05)	24.15 (2.19)
Agents' average earnings	10.50 (1.31)	10.52 (1.07)	10.51 (1.19)
Principals' average earnings	3.00 (2.63)	2.97 (2.13)	2.98 (2.38)
Experiment 2			
Location	Trento		
Number of sessions	3	3	6
Number of subjects	56	60	116
Gender (% female)	51.79	50.00	50.86
Average age (std. dev.)	21.21 (2.34)	21.75 (2.18)	21.49 (2.27)
Agents' average earnings	10.83 (1.33)	10.91 (0.70)	10.87 (1.04)
Principals' average earnings	2.34 (2.67)	2.19 (1.40)	2.26 (2.09)
Experiment 3			
Location	Jena		
Number of sessions	4	4	8
Number of subjects	64	66	130
Gender (% female)	51.56	50.00	50.77
Average age (std. dev.)	23.97 (2.87)	23.76 (2.63)	23.86 (2.74)
Agents' average earnings	10.08 (1.97)	10.53 (1.12)	10.31 (1.60)
Principals' average earnings	3.83 (3.95)	2.94 (2.25)	3.38 (3.20)
Experiment 4			
Location	Jena		
Number of sessions	5	5	10
Number of subjects	114	114	228
Gender (% female)	48.25	50.88	49.56
Average age (std. dev.)	24.71 (2.90)	24.13 (2.89)	24.42 (2.90)
Agents' average earnings	10.00 (0.00)	10.00 (0.00)	10.00 (0.00)
Principals' average earnings	10.00 (0.00)	10.00 (0.00)	10.00 (0.00)

Table 2: Experimental conditions.

Notes: Earnings are stated in euros net of the show-up fee with standard deviations in parentheses. There was no show-up fee in Experiment 4.

3.2 Results

In this section, we present our main findings concerning the behavior of the agents and the principals in the one-shot interaction. Subjects' answers to the questionnaire are discussed in Section 6. We report the results of two-sided tests throughout the paper and acceptance or rejection of the null hypothesis is always based on a 5 percent level of significance.

Our first result concerns the hidden costs of control and is qualitatively in line with F&K's first result.

Result 1.1: *We observe hidden costs of control.*

Support: Figure 1 shows the cumulative distribution of agents' transfers given the decision of the principal in each control treatment. For each value of $x \geq \underline{x}$, $\underline{x} \in \{5, 10\}$, there are always strictly

more agents who transfer at least x if the principal leaves transfers unrestricted than if the principal forces the agent to transfer at least $\underline{x}$ ECUs. In order to test whether the difference between the two distributions is statistically significant, we follow F&K's procedure and modify the distribution of transfers in the no-control condition such that any transfer strictly lower than $\underline{x}$ is set equal to $\underline{x}$. We reject the null hypothesis that the modified distribution in the no-control condition is the same as the distribution in the control condition (exact Wilcoxon signed rank tests for paired observations, $p - value < 0.01$ in C5 and $p - value = 0.016$ in C10). □

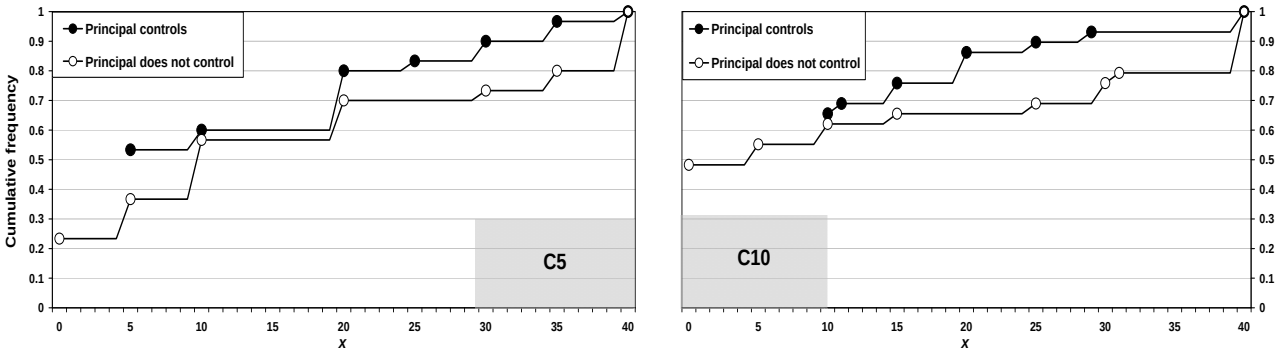

Figure 1: Cumulative distributions of agents' transfers in Experiment 1.

Hidden costs of control should be taken seriously only if they are substantial enough to undermine the effectiveness of economic incentives.[5] Our second result establishes that in none of the control treatments do the hidden costs outweigh the benefits of control. This result contradicts F&K's second result.

Result 1.2: *Hidden costs do not outweigh benefits of control. In C5, the average number of ECUs transferred by the agent to the principal is not statistically significantly higher in the no-control condition than in the control condition. In C10, the average number of ECUs transferred by the agent to the principal is not statistically significantly higher in the control condition than in the no-control condition.*

Support: Table 3 shows agents' transfers as a function of the principal's decision in treatments C5 and C10. In each of the five panels, the first row reports average transfers for each condition followed by the impact of control, $(\bar{x}^C - \bar{x}^{NC})/\bar{x}^{NC}$, measured in percentage terms. The second row reports standard deviation followed by 1st quartile followed by median followed by 3rd quartile for each condition. Agents' transfers in our first experiment are summarized in the top panel. In C5, both the average and the median number of ECUs transferred by the agents to the principals are higher in the no-control condition than in the control condition but not statistically significantly so (exact Wilcoxon signed rank test for paired observations, $p - value = 0.106$).[6] In C10, both the average and the median number of ECUs transferred by the agents to the principals are higher in the control

[5] As already mentioned, for a given agent's pair of transfers $\left(x^C, x^{NC}\right)$, the principal's decision to control entails hidden costs if $d = x^C - \max\{\underline{x}, x^{NC}\} < 0$ i.e. the agent's indirect choice shift due to control is negative. According to F&K's procedure, for a given sample of agents' choices, we observe hidden costs of control if the sum of the ranks of the negative d's is sufficiently larger than the sum of the ranks of the positive d's. Hidden costs of control can therefore be observed in samples which comprise mostly selfish agents since the choices of such agents are not taken into account by the procedure.

[6] Unlike the procedure employed to obtain Result 1.1, no transformation is applied to the distribution of transfers in the no-control condition to obtain Result 1.2.

condition than in the no-control condition but not statistically significantly so (exact Wilcoxon signed rank test for paired observations, $p - value = 0.307$). Interestingly, in both control treatments, the variance of transfers is higher in the no-control condition than in the control condition. Finally, agents' transfers in our sample are statistically significantly lower than agents' transfers in F&K's sample in the no-control condition (exact Wilcoxon signed rank tests, $p - value = 0.028$ in C5 and $p - value = 0.015$ in C10). In the control condition, agents' transfers do not statistically differ among the two samples (exact Wilcoxon signed rank tests, $p - value = 0.695$ in C5 and $p - value = 0.278$ in C10). □

	C5			**C10**		
	Control condition	No-control condition	Impact of control (%)	Control condition	No-control condition	Impact of control (%)
Experiment 1	**13.83** (11.35;5;5;20)	**16.67** (15.44;5;10;34)	-17.04	**14.66** (8.68;10;10;15)	**13.83** (16.81;0;5;30)	6.00
Experiment 2	**12.07** (12.56;5;5;13)	**11.21** (14.39;0;5;20)	7.67	**12.50** (5.26;10;10;12)	**9.53** (11.96;0;4;18)	31.16
Experiment 3	**15.53** (17.99;5;10;20)	**20.50** (20.49;5;20;34)	-24.24	**13.82** (7.87;10;10;15)	**15.97** (14.47;0;20;25)	-13.46
Experiment 4	**16.81** (23.00;5;5;20)	**21.42** (24.64;0;20;40)	-21.52	**20.58** (12.92;10;15;40)	**27.79** (20.15;20;30;40)	-25.94
Falk & Kosfeld	**12.19** (10.98;5;5;15)	**25.11** (19.34;10;20;40)	-51.45	**17.53** (13.57;10;10;20)	**22.99** (17.97;10;20;40)	-23.75

Table 3: Agents' transfers as a function of the principal's decision.

An interesting aspect of F&K's experimental design is that agents' heterogeneity in the behavioral reaction to control can be analyzed. The use of the strategy method permits to quantify the number of agents who react positively, neutrally, or negatively to the principal's implementation of control. Result 1.3 summarizes our findings about the heterogeneity of agents' behavioral reaction to control. Negative reactions to the implementation of control are less prevalent than in F&K's C5 and C10 treatments.

Result 1.3: *There is heterogeneity among the agents with regard to their behavioral reaction to control. In C5, less than a quarter of the agents reacts positively to the implementation of control with the rest dividing almost equally between negative and neutral reactions, the average negative response to control equals -10.00 ECUs and the average positive response to control equals 5.00 ECUs. In C10, a clear majority of agents reacts positively to the implementation of control, the average negative response to control equals -16.50 ECUs and the average positive response to control equals 9.17 ECUs.*

Support: The top and bottom panels of Table 4 summarize the agents' behavioral reaction to control in our first experiment and in F&K's C5 and C10 treatments respectively. The relative share of agents who are averse to being controlled is much lower in our first experiment (40.00% and 27.59% in C5 and C10 respectively) than in F&K's C5 and C10 treatments where control-averse agents are the clear majority (64.29% and 56.94% in C5 and C10 respectively). In C10, the difference in the magnitude of hidden costs of control observed by F&K and the one we observe is entirely driven by the difference in the fraction of control-averse agents (negative and positive reactions to control are

of similar strength). However, in C5, the difference in the fraction of control-averse agents observed by F&K and the one we observe is only partly responsible for the difference in the magnitude of hidden costs of control. The control-averse agents in F&K's C5 treatment exhibit a much stronger negative response to control than our control-averse agents. □

	C5			C10		
	Positive	Neutral	Negative	Positive	Neutral	Negative
Experiment 1						
Relative share (%)	**23.33**	**36.67**	**40.00**	**58.62**	**13.79**	**27.59**
Mean control transfer	5.00	16.82	16.25	10.35	28.75	16.75
Mean no-control transfer	0.00	16.82	26.25	1.18	28.75	33.25
Experiment 2						
Relative share (%)	**50.00**	**21.43**	**28.57**	**60.00**	**16.67**	**23.33**
Mean control transfer	8.00	19.17	13.88	10.33	14.80	16.43
Mean no-control transfer	0.64	19.17	23.75	1.39	14.80	26.71
Experiment 3						
Relative share (%)	**25.00**	**34.38**	**40.62**	**39.40**	**15.15**	**45.45**
Mean control transfer	5.62	23.64	14.77	10.00	22.40	14.27
Mean no-control transfer	0.62	23.64	30.08	0.77	22.40	27.00
Experiment 4						
Relative share (%)	**33.33**	**22.81**	**43.86**	**21.06**	**33.33**	**45.61**
Mean control transfer	5.84	42.69	11.68	11.25	36.32	13.38
Mean no-control transfer	0.84	42.69	26.00	0.42	36.32	34.19
Falk and Kosfeld						
Relative share (%)	**20.00**	**15.71**	**64.29**	**25.00**	**18.06**	**56.94**
Mean control transfer	10.21	22.27	10.33	11.11	22.69	18.71
Mean no-control transfer	4.79	22.27	32.13	1.94	22.69	32.32

Table 4: Agents' behavioral reaction to control.

In C5, both the average number of ECUs transferred and the variance of transfers in the no-control condition are larger than those in the control condition. Assuming that principals correctly anticipate the ambiguous effects of implementing control, the optimal decision will typically vary with their degree of risk-aversion. In C10, on the contrary, it is clearly optimal for principals to control their agent (controlling the agent is at least as rewarding in monetary terms and safer than leaving the transfer unrestricted). Our fourth result is in line with the hypothesis that most principals took an optimal decision anticipating quite well the distributions of conditional transfers. The same hypothesis is supported by F&K's findings but the consequences are different in the two samples. The substantial prevalence of control in Experiment 1 contradicts F&K's fourth result.

Result 1.4: *In C5, half the principals chooses to control. In C10, the large majority of principals chooses to control.*

Support: 50% and 83% of our principals force the agent to transfer at least $\underline{x}$ ECUs in C5 and C10 respectively. Judging by a binomial test, the proportion of principals who leave transfers unrestricted is statistically significantly lower than the proportion of principals who control in C10 ($p - value < 0.01$). In F&K's C5 and C10 treatments, respectively 26% and 29% of the principals choose to control which is the least rewarding decision since average transfers are respectively 106% and 31% higher in the no-control condition. □

After having made their decision, principals were asked to state their expectation about the number of ECUs transferred by the agent (expectations were not incentivized). We find that principals who control have lower expectations about the agent's transfer than principals who leave transfers unrestricted (8.07 vs. 18.93 and 17.21 vs. 28.00 in C5 and C10 respectively). The two distributions statistically differ in C5 (exact Wilcoxon signed rank test, $p - value = 0.020$) but they do not in C10 (exact Wilcoxon signed rank test, $p - value = 0.153$) with the caveat that there are only 5 expectations in C10's no-control condition. In F&K's C5 and C10 treatments, the two distributions of expectations statistically differ with principals who control being more pessimistic. Note that since expectations were elicited after decisions in both experiments, stated expectations could be mere rationalizations for behavior after the fact, i.e., the expectation might be induced *by* the decision rather than reflecting the reasons *for* the decision.

3.3 Discussion

In assessing the reproducibility of F&K's experimental findings, we have confirmed the existence of hidden costs of control but we have also shown that these costs are not substantial enough to undermine the effectiveness of economic incentives in our sample. In the low control treatment, the difference in the magnitude of hidden costs of control observed by F&K and the one we observe is driven both by the difference in the fraction of control-averse agents and the strength of their negative response to control. In the medium control treatment, the difference in the magnitude of hidden costs of control is entirely driven by the difference in the fraction of control-averse agents. Our sample of agents is less inclined to express a negative reaction to the implementation of control than F&K's sample of agents. In both experiments most principals seem to have well anticipated the distributions of conditional transfers. Consequently, our principals predominantly control their agent while in F&K's C5 and C10 treatments agents are trusted to be generous.

Intrigued by the discrepancy between F&K's results and the results of our first experiment, anonymous referees reasoned that this discrepancy might be due to cultural differences between the two subject pools. In particular, it has been argued that students in Jena, which was part of the communist country East Germany, might be less bothered by the imposition of control than Zurich students because East Germany scores lower on self-expression than Switzerland and a low score implies, in turn, a relatively favorable attitude towards authority (the argument builds on Inglehart, 2000). Anonymous referees seem to have conjectured that a *regionality effect* is mainly responsible for the observed subject-pool differences.

In our first experiment, subjects were recruited by "convenience sampling" meaning that they are not statistically representative of the East German population. Subjects in F&K's study were recruited in the same way meaning that they are not statistically representative of the Swiss German population. Both experiments employ a standard subject pool of students which implies that the two samples are comparable according to certain demographics (e.g. age and educational attainment) but they might still differ in other demographics (e.g. educational background). Table 1 and Table 2 show that, in our first experiment, the sample composition according to educational background is (almost) equally distributed among the primary fields of academic study and the gender composition is approximately balanced. Since we do not possess any information concerning the distribution of demographics in F&K's sample,[7] the cross-region comparability of the two samples is not guaranteed. Consequently, the discrepancy in behavior between the two samples in F&K's principal-agent game can be the product of *any* difference between the two samples (Botelho, Harrison, Hirsch, and Rutström, 2005; Bardsley, Cubitt, Loomes, Moffatt, Starmer, and Sugden, 2010, p. 188). The

[7] So far we were unable to obtain this information.

behavioral differences between the two samples are weak evidence that regionality affects control aversion.

We complement Experiment 1 with two additional repetitions of F&K's C5 and C10 treatments to further check the generalizability of F&K's findings to other subject pools of students and to offer preliminary insights on individual determinants of hidden costs of control.

4 Experiment 2: Repeating Experiment 1 in Trento

Our second experiment repeats Experiment 1 in Trento partly to address the potential issue of cross-regional differences (Jena and Trento are two cities of comparable size). We follow the suggestion of anonymous referees and investigate the relationship between the experimentally observed degree of control aversion and self-expression values. The self-expression dimension is extracted from the World Values Survey, a large investigation of attitudes, values and beliefs around the world.[8] According to the most recent wave of the survey (2005-2008), Italy's score on self-expression is more than twice the score of East Germany and about one-third of Switzerland's score. If the score on the self-expression dimension positively correlates with control aversion then the magnitude of hidden costs of control should be larger in Experiment 2 than in Experiment 1. This being said, the main purpose of Experiment 2 is not to investigate whether self-expression scores are a good predictor of control aversion. Rather, Experiment 2 allows us to further assess the robustness of F&K's experimental results by rerunning Experiment 1 on a new pool of subjects and it constitutes a first step in exploring which demographics influence the magnitude of hidden costs of control.

Experimental design and practical procedures

The six sessions of the computerized experiment were conducted at the Computable and Experimental Economics Laboratory (CEEL) of the University of Trento. All 116 subjects were students from the University of Trento and in each session the gender composition was approximately balanced. The second panel of Table 1 shows the sample composition according to educational background where law is shown separately from the category 'other behavioral & social sciences'. Contrary to Experiment 1, the category 'business administration & economics' is excessively represented and gathers about 60% of the subjects. Business and economics majors are over-represented in experiments conducted at CEEL since students from other majors are less aware of the possibility to participate in economic experiments and the laboratory is located near the faculty of economics. Experimental procedures are identical to those of Experiment 1 except that once all subjects had played the principal-agent game and had answered the questionnaire they were asked to report their year of birth, their gender, and their academic major.

To control for potential experimental artifacts, we followed Roth, Prasnikar, Okuno-Fujiwara, and Zamir (1991) on appropriate designs for cross-country experiments. In particular, we did our best to control for currency, experimenter, and language effects. First, the currency effect is of no concern since both Germany and Italy belong to the Euro zone and the price levels in the two countries are equivalent (according to Eurostat, the statistical office of the European Union, the 2007 price level indices equal 101.9 and 102.9 in Germany and Italy respectively).[9] Second, our first two repetitions were conducted under the supervision of the third author with the help of the first author. Third, the German instructions were translated into Italian by the first author with the help of a professional translator and then the Italian instructions were back-translated to German

[8] See http://www.worldvaluessurvey.org/.

[9] In Switzerland, the 2007 price level index equals 124.5 and it has been about 140 in the period during which F&K conducted their experiment.

and compared to the original instructions. Italian instructions were fine-tuned until the rules of the experiment were explained as similarly as possible in both languages.

The second panel of Table 2 shows some of the features of Experiment 2. Compared to Experiment 1, two additional sessions had to be conducted in Experiment 2 in order to collect the same number of observations since the CEEL has a capacity of 20 participants whereas the ELMPIE has a capacity of 32 participants.

Results

Similarly to Experiment 1, we present here our main findings concerning the principal-agent game data and subjects' answers to the questionnaire are discussed in Section 6.

Result 2.1 summarizes our findings concerning the behavior of agents in Experiment 2 which are even less in line with F&K's findings than the ones from Experiment 1.

Result 2.1: *We do not observe hidden costs of control. In C5, the average number of ECUs transferred by the agent to the principal is not statistically significantly higher in the control condition than in the no-control condition. In C10, the average number of ECUs transferred by the agent to the principal is statistically significantly higher in the control condition than in the no-control condition. The majority of agents reacts positively to the implementation of control.*

Support: Agents' transfers in Experiment 2 are summarized in the second panel of Table 3. We never reject the null hypothesis that the modified distribution in the no-control condition is the same as the distribution in the control condition (exact Wilcoxon signed rank tests for paired observations, $p - value = 0.177$ in C5 and $p - value = 0.068$ in C10).[10] The impact of control on transfers is always positive (it equals 7.67% in C5 and 31.16% in C10) but it is statistically significantly so only in C10 (exact Wilcoxon signed rank tests for paired observations, $p - value = 0.720$ in C5 and $p - value = 0.046$ in C10). Like in Experiment 1, agents' transfers in Experiment 2 are statistically significantly lower than agents' transfers in F&K's sample in the no-control condition (exact Wilcoxon signed rank tests, $p-value < 0.01$ in both treatments), and, in the control condition, agents' transfers do not statistically differ among the two samples (exact Wilcoxon signed rank tests, $p-value = 0.345$ in C5 and $p-value = 0.226$ in C10). Finally, as shown in the second panel of Table 4, the majority of agents reacts positively to the principal's implementation of control (50.00% and 60.00% in C5 and C10 respectively), and the control-averse agents in F&K's C5 and C10 treatments exhibit a stronger negative response to control than the control-averse agents in Experiment 2. □

Like in Experiment 1, the variance of transfers is always higher in the no-control condition than in the control condition and this difference is large in C10. Consequently, the optimal choice for principals who anticipate well-enough the distributions of conditional transfers is to control their agent both in C5 and in C10. Like in F&K and Experiment 1, most principals in Experiment 2 seem to have correctly anticipated the effects of controlling their agents. Consequently, a clear majority of principals controls in Experiment 2.

Result 2.2: *The majority of principals chooses to control the agent.*

Support: 64% and 77% of our principals force the agent to transfer at least $\underline{x}$ ECUs in C5 and C10 respectively. Judging by binomial tests, the proportion of principals who leave transfers unrestricted

[10] In C5 we observe the transfer pair $\left(x^C, x^{NC}\right) = (40, 0)$ which is likely to be the result of a typing error. Excluding this transfer pair from the sample leads to statistically significant hidden costs of control.

is statistically significantly lower than the proportion of principals who control in C10 ($p - value < 0.01$) but the difference is statistically insignificant in C5 ($p - value = 0.185$). Additionally, we find that principals who control have lower expectations about the agent's transfer than principals who leave transfers unrestricted in C10 (12.61 vs. 25.14) whereas in C5 expectations are very similar in both conditions (15.89 vs. 14.20). The two distributions never differ statistically (exact Wilcoxon signed rank tests, $p - value = 0.841$ in C5 and $p - value = 0.411$ in C10). □

Discussion

In Experiment 2, we observe a positive impact of control in both treatments which implies that principals are better off controlling their agent even in the case of extremely weak extrinsic incentives. The substantial hidden costs of control observed by F&K in the low and medium control treatments do not extend to our student populations from neighboring countries. Additionally, we find that Trento students are less bothered by the imposition of control than Jena students with the caveat that the two samples differ according to educational background. This finding does not support the conjecture that the score on the self-expression dimension positively correlates with the degree of control aversion observed in the laboratory.

5 Experiment 3: Repeating Experiment 1 with mostly economists

Contrary to Experiment 1, we do not observe statistically significant hidden costs of control in Experiment 2. This difference in results cannot be attributed solely to cross-regional differences in subjects' attitudes since the subject samples also differ according to educational background. The majority of subjects in Experiment 2 are business and economics majors while such students represent less than 30% of Experiment 1's sample. This difference in educational background between the two samples confounds with cross-regional differences but the severity of this confound is difficult to appreciate given the existing evidence. Indeed, though a few empirical studies have argued that economists behave in a more self-interested way than other people, the results are inconclusive and vary with the context (see Faravelli, 2007 and the references therein). Moreover, Fehr, Naef, and Schmidt (2006) have shown that economics majors value efficiency much more than non-economics majors in simple dictator game experiments. Finally, and most importantly, economists' reaction to the implementation of control has not been documented to the best of our knowledge. In summary, the current evidence does not clearly suggest whether economics majors will behave differently than non-economics majors in F&K's principal-agent game, and, if so, whether they will decrease or increase the magnitude of hidden costs of control. To assess the severity of the confound and offer additional insights on the individual determinants of control aversion, our third experiment repeats Experiment 1 with a sample of Jena students whose composition according to educational background closely resembles the one of Experiment 2.

Experimental design and practical procedures

The eight sessions of the computerized experiment were conducted at the ELMPIE in Jena. All 130 subjects were students from the University of Jena and in each session the gender composition was approximately balanced. The third panel of Table 1 shows the sample composition according to educational background which closely resembles the one of Experiment 2 with business administration & economics gathering about 60% of the subjects. Experimental procedures are identical to those of Experiment 2. Experiment 3 was conducted under the supervision of the third author with the help of the second author.

The third panel of Table 2 shows some of the features of Experiment 3. To extend the equivalence of experimental procedures between the two regions, we aimed at conducting sessions with the same number of subjects as in Experiment 2. Unfortunately, we suffered from many no-shows in Experiment 3 since the sessions took place at the end of the term break. Consequently, we had to conduct a total of 8 sessions in Experiment 3, two more than in Experiment 2.[11]

Results

Similarly to Experiments 1 and 2, we present here our main findings concerning the principal-agent game data and subjects' answers to the questionnaire are discussed in Section 6. For the sake of brevity, we focus on agents' transfers. The behavior of principals is again in line with the hypothesis that most of them have well anticipated the distributions of conditional transfers.[12]

Result 3: *We observe hidden costs of control. In C5, hidden costs outweigh benefits of control since the average number of ECUs transferred by the agent to the principal is statistically significantly higher in the no-control condition than in the control condition. In C10, hidden costs do not outweigh benefits of control since the average number of ECUs transferred by the agent to the principal is not statistically significantly higher in the no-control condition than in the control condition. The largest group of agents shows a negative reaction to the implementation of control.*

Support: Agents' transfers in Experiment 3 are summarized in the third panel of Table 3. We always reject the null hypothesis that the modified distribution in the no-control condition is the same as the distribution in the control condition (exact Wilcoxon signed rank tests for paired observations, $p-value < 0.01$ in both treatments). The impact of control on transfers is always negative (it equals -24.24% in C5 and -13.46% in C10) but it is statistically significantly so only in C5 (exact Wilcoxon signed rank tests for paired observations, $p - value < 0.01$ in C5 and $p - value = 0.3026$ in C10). Finally, as shown in the third panel of Table 4, the largest group of agents reacts negatively to the principal's implementation of control (40.62% and 45.45% in C5 and C10 respectively). □

Experiment 3's findings concerning the behavior of agents are more in line with F&K's findings than the ones from Experiment 1 and they are less in line with Experiment 2's findings than the ones from Experiment 1. This in turn suggests the following two implications. First, our Trento subjects are less bothered by the imposition of control than our Jena students after controlling for educational background. This regionality effect is statistically significant in C5 where agents' transfers in the no-control condition are statistically significantly lower in Experiment 2 than in Experiment 3 (exact Wilcoxon signed rank test, $p - value = 0.041$) whereas in C10 agents' transfers in the no-control condition do not statistically differ between the two experiments (exact Wilcoxon signed rank test, $p-value = 0.101$). Not surprisingly, agents' transfers in the control condition never statistically differ (exact Wilcoxon signed rank tests, $p - value = 0.101$ in C5 and $p - value = 0.899$ in C10). Second, a subject sample which mostly consists of business and economics majors seems to be more control-averse than a subject sample with a more balanced distribution of majors in F&K's principal-agent

[11] Contrary to Experiment 1 and 2, there is a non-negligible variability in the cohort size in Experiment 3. We cannot rule out the possibility that the size of the cohort influences the magnitude of hidden costs of control observed in the session. Indeed, in sessions with cohort size 12 or 14 we observe a positive impact of control whereas in sessions with cohort size 18 or 20 we observe a negative impact of control. However, the magnitude of hidden costs of control observed for a given cohort size also drastically varies between sessions. For example, in C10 and a cohort size of 14, the impact of control varies from 16.47% to 114.29%.

[12] 53% and 64% of our principals force the agent to transfer at least $\underline{x}$ ECUs in C5 and C10 respectively. Judging by binomial tests, the proportion of principals who leave transfers unrestricted is never statistically significantly lower than the proportion of principals who control ($p - value = 0.860$ in C5 and $p - value = 0.163$ in C10).

game. This educational effect is statistically insignificant since agents' transfers never statistically differ between Experiment 1 and Experiment 3 (exact Wilcoxon signed rank tests, p-values for the no-control condition are 0.602 and 0.519 in C5 and C10 respectively, and for the control condition they are 0.688 and 0.932 in C5 and C10 respectively). Needless to say, both effects could just be the reflection of sample differences in terms of other non-observed demographics. Though our main purpose is not to investigate extensively subject pool differences, both implications are further discussed in the concluding section.

Discussion

Like in Experiment 1, Experiment 3's findings in C10 confirm the existence of hidden costs of control but they also establish that the crowding-out effect does not dominate the disciplining effect. On the contrary, in C5, Experiment 3's findings confirm F&K's striking observation that the implementation of control is detrimental to the principal. Still, agents' data in Experiment 3 are quantitatively quite different from those in F&K's experiment. Indeed, only 40.62% of our agents react negatively to the implementation of control compared to 64.29% in F&K and their negative response ($\bar{x}^C - \bar{x}^{NC}$) is lower than the one observed by F&K (-15.31 vs. -21.80). Overall, the impact of control in Experiment 3's C5 treatment is less than half the impact of control in F&K's C5 treatment (-21.52% vs. -51.45%).

In summary, the findings of our first three experiments question the robustness of F&K's findings in their principal-agent game. Contrary to F&K, we mainly observe hidden costs of control of *low* magnitude which lead to low-trust principal-agent relationships. We now discuss our subjects' answers to the questionnaires that F&K administered in their questionnaire study.

6 Questionnaire results

With the help of a questionnaire in which they presented 403 subjects with vignettes, F&K studied how control and explicit incentives affect motivation in typical work environments. They constructed five different workplace scenarios, and, for each scenario, they presented a condition where the principal trusts the employee and a condition where the principal controls or uses explicit incentive devices. Each subject went through all five scenarios but faced only one of the two conditions for a given scenario. Based on a sample of 2015 responses, F&K observe that, in all scenarios, stated work motivation is statistically significantly lower in the condition where the principal controls or uses explicit incentive devices than in the condition where the principal trusts the employee.

Like in F&K's questionnaire study, our subjects had to indicate their work motivation on a five-level scale ranging from "very low" to "very high" for each of the five hypothetical scenarios. Our sample of 1820 subjects' answers is similar to F&K's sample: in each scenario, the cumulative frequencies of subjects indicating that they have at most a medium motivation are always higher in the control than in the trust condition (for details see Appendix E of the supplementary material). Like F&K we observe that in all scenarios stated work motivation is statistically significantly lower in the condition where the principals controls or uses explicit incentive devices than in the condition where the principals trusts the employee (exact Wilcoxon signed rank tests, $p-value < 0.01$ in each scenario). Even when restricting the sample to Trento subjects, for whom the impact of control is positive, we observe that in all scenarios except the fifth one stated work motivation is statistically significantly lower in the control than in the trust condition (exact Wilcoxon signed rank tests, $p-value = 0.278$ in scenario 5 and $p-value < 0.01$ for all remaining scenarios).

In a nutshell, little revealed control aversion is inferred from the game play data of our first three experiments but control substantially reduces stated work motivation according to our questionnaire

data. This dissimilar influence of control on revealed and stated work motivation contrasts sharply with F&K's findings who conclude from their game play and questionnaire data "... results we obtained in stylized principal-agent relations in the lab carry over to quite realistic, everyday workplace scenarios." and additionally argue that their questionnaire data illustrate "the variety of applications of hidden costs of control, as well as their economic importance." (Falk and Kosfeld, 2006, p. 1627). We put forward two non-exclusive explanations for the concomitance of dissimilar game play data and similar questionnaire data with respect to F&K. First, the implemented principal-agent game might not incorporate essential features of employment relationships which generate strong negative reactions to control in our samples of subjects. Second, hypothetical incentives are conducive to substantial hidden costs of control in our samples of subjects. We decided to test the second rather than the first explanation. Though an empirical assessment of hidden costs of control in rich decision environments seems an important direction for future research (see our general discussion), we strongly believe that results motivated by hypothetical incentives should be interpreted with caution. Below, we present the results of an extension of F&K's C5 and C10 treatments where performance-based monetary rewards are absent i.e. both principals and agents are paid according to a flat participation fee.

7 Experiment 4: Hypothetical choices in Experiment 1

Experiment 4 aims at understanding the dissimilar influence of control on revealed and stated work motivation in our first three experiments. This dissimilarity in influence is puzzling since it contrasts sharply with F&K's findings. Based on our questionnaire results, we conjectured that Jena students are more control-averse in the principal-agent game under hypothetical incentives. Experiment 4 extends our first experiment by asking agents and principals to make hypothetical choices for a flat fee, and the samples used in both experiments have similar observed characteristics. The findings of Experiment 4 confirm our conjecture: Hidden costs outweigh benefits of control in C5 and C10.

Experimental design and practical procedures

The ten sessions of the computerized experiment were conducted at the ELMPIE in Jena. All 228 subjects were students from the University of Jena and in each session the gender composition was approximately balanced.[13] The fourth panel of Table 1 shows the sample composition according to educational background which closely resembles the one of Experiment 1. Experimental procedures are identical to those of Experiment 1 except that (i) Once all subjects had played the principal-agent game and had answered the questionnaire they were asked to report their year of birth, their gender, and their academic major; and (ii) Subjects were paid a flat participation fee of 10 euros for completing the experiment.[14] Experiment 4 was conducted under the supervision of the third author with the help of the second author.

The fourth panel of Table 2 shows some of the features of Experiment 4.

Results

Result 4 summarizes our findings concerning subjects' behavior in Experiment 4 which are qualitatively in line with F&K's results 1 to 4.

[13] Since hypothetical incentives usually increase the "noise" in subjects' decisions, we collected twice the amount of data in Experiment 4 compared to Experiment 1.

[14] Sessions in which subjects make hypothetical choices for a flat fee are the exception at the ELMPIE. In such cases, no show-up fee is paid meaning that the usual 2.50 euros show-up fee is included in the participation fee.

Result 4: *We observe hidden costs that outweigh the benefits of control. The largest group of agents reacts negatively to the implementation of control. The majority of principals chooses not to control the agent.*

Support: Agents' transfers in Experiment 4 are summarized in the fourth panel of Table 3. We always reject the null hypothesis that the modified distribution in the no-control condition is the same as the distribution in the control condition (exact Wilcoxon signed rank tests for paired observations, $p-value < 0.01$ in both treatments). The impact of control on transfers is negative (it equals -21.52% in C5 and -25.94% in C10) and statistically significantly so (exact Wilcoxon signed rank tests for paired observations, $p-value \leq 0.01$ in both treatments). As shown in the fourth panel of Table 4, the largest group of agents reacts negatively to the principal's implementation of control (43.86% and 45.61% in C5 and C10 respectively). Only 42% and 44% of our principals force the agent to transfer at least $\underline{x}$ ECUs in C5 and C10 respectively. Judging by binomial tests, the proportion of principals who leave transfers unrestricted is not statistically significantly higher than the proportion of principals who control ($p-value = 0.289$ in C5 and $p-value = 0.427$ in C10). □

Though Experiment 4 reproduces F&K's striking finding that the implementation of control is detrimental to the principal, much stronger negative responses to control are observed in F&K's C5 treatment compared to Experiment 4's C5 treatment (in C10, agents' responses to control are similar in both experiments). Figure 2 illustrates the distributions of transfer pairs from agents to principals in Experiment 1, Experiment 4 and in F&K's low and medium control treatments (see Appendix F of the supplementary material for the distributions in Experiments 2 and 3). Bubble plots on the left (respectively right) represent distributions of transfer pairs in C5 (respectively C10). On the horizontal axis we denote the amount transferred in the no-control condition and on the vertical axis we denote the amount transferred in the control condition. The size of bubbles is proportional to the number of transfer pairs. Black, grey and white bubbles correspond to negative, neutral and positive reactions to control respectively and histograms with the relative frequencies of behavioral reactions to control are added in the left upper corner of each bubble plot. The few transfer pairs that include a transfer of more than 40 ECUs are clustered on the right side of the plot.

Compared to Experiment 1, the absence of performance-based monetary rewards in Experiment 4 has induced a clear downward-right shift in the distribution of transfer pairs. In particular, agents whose negative response to control is strictly lower than $(-\underline{x})$ are twice as many in Experiment 4 compared to Experiment 1 (26% vs. 13% and 30% vs. 14% in C5 and C10 respectively). Moreover, the shift in the distribution of transfer pairs in C10 has been sufficiently strong so that behavioral reactions to control observed in Experiment 4 are comparable to the ones observed by F&K (the proportion of agents whose negative response to control is strictly lower than (-10) equals 24% in F&K's C10 treatment). In C5, on the contrary, the shift is insufficient to have similar reactions to control in both experiments with 53% of the agents exhibiting a negative response to control strictly lower than (-5) in F&K's sample. This notable difference mainly explains that the impact of control in Experiment 4's C5 treatment is less than half the impact of control in F&K's C5 treatment.

Discussion

We observe that hidden costs outweigh benefits of control in C5 and C10 which shows that our subjects are more likely to express control aversion in F&K's principal-agent game under hypothetical than under real incentives. This new evidence helps us understanding why little revealed control aversion has been inferred from the game play data of our first three experiments but control substantially reduced stated work motivation according to our questionnaire data. Interestingly, a few transfer pairs that indicate some likely confusion with the game, $max\{x^C, x^{NC}\} > 40$, are observed

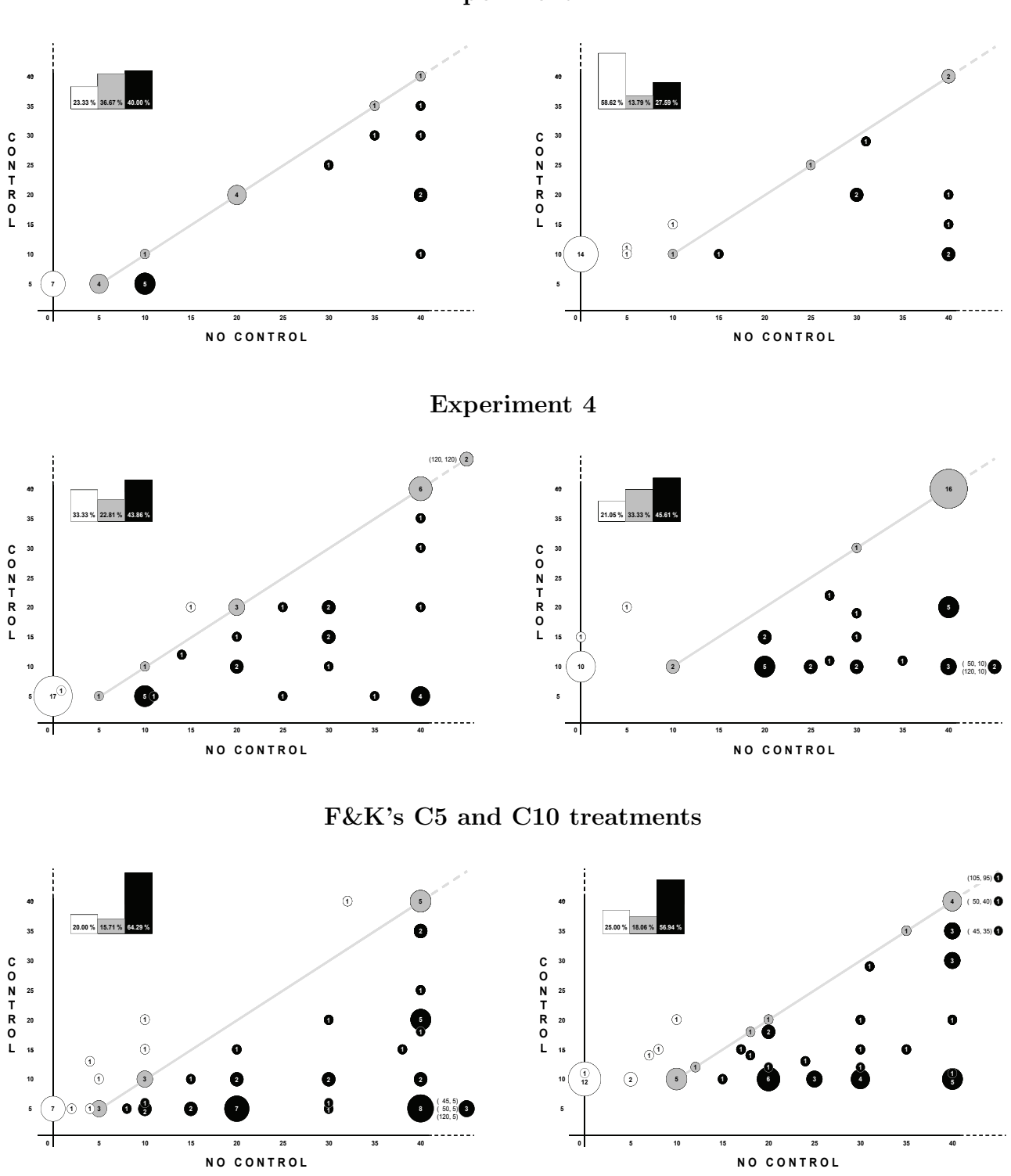

Figure 2: Distributions of transfer pairs from agents to principals.

in Experiment 4 as well as in F&K's C5 and C10 treatment (about 4% in each experiment) but such "confused" transfer pairs are basically absent in our first three experiments (about half a percent).

8 General discussion

In this concluding section, we first provide a summary of our experimental findings and review briefly some of the related literature. Second, we present the results of statistical analyses which assess the effects of demographics and experimental conditions on control aversion. Third, we complement the results of Section 4 by considering two other societal norms/values which according to us are more likely to correlate with the experimentally observed behavior. Finally, we sketch a research agenda for the future.

Summary and related literature

The evidence collected in our three first experiments largely confirms that, though hidden costs of control exist, the crowding-out effect of monitoring does not dominate the disciplining effect in the laboratory when complementarities between effort discretion and other elements of the employment strategy are absent. Among others, Dickinson and Villeval (2008) establish that a personal relationship between principal and agent is a major condition for the crowding-out effect to emerge (as suggested by Frey, 1993 and supported by the field evidence in Barkema, 1995). Bartling, Fehr, and Schmidt (2010) show that the trust strategy is optimal only when the possibility to condition the job offer on the employees' reputation exists (like F&K, the latter study used students from the University of Zurich and the Swiss Federal Institute of Technology in Zurich as subjects). In line with these findings, two replication and extension studies of F&K's medium control treatment, Hagemann (2007) and Schnedler and Vadovic (2007), validate our experimental results in their replication treatments.

The evidence collected in Experiment 4 shows that our subjects are more likely to express a negative reaction to the implementation of control in F&K's principal-agent game under hypothetical than under real incentives. This finding solves the puzzle of little revealed control aversion in our first three experiments but substantially reduced stated work motivation due to control in our questionnaire data. A plausible explanation for the observation of low magnitude hidden costs of control in Experiment 1 and high magnitude hidden costs of control in Experiment 4 is that the use of performance-based monetary rewards causes subjects to invoke monetary-marketplace frames and norms which in turn leads to low negative effects of control on motivation whereas the use of a flat fee suggests a social-market relationship where effort is shaped by altruism and control has substantial negative effects on motivation (for more details see Heyman and Ariely, 2004).

In contrast to our three repetitions and other experiments using real incentives, most agents react negatively to the implementation of control in F&K's C5 and C10 treatments which in turn implies that imposing explicit incentives is detrimental to the principal. These laboratory results carry over to hypothetical workplace scenarios. Moreover, though agents' responses to control are similar in F&K's and Experiment 4's C10 treatments, much stronger negative responses to control are observed in F&K's C5 treatment compared to Experiment 4's C5 treatment. This observation and the fact that the original study uses real incentives suggest that F&K's experimental regularities originate from the characteristics of their subjects.[15] We complement this insight in the next two subsections.

[15] As already mentioned, we put more emphasis on the performance-contingent earnings in our three first experiments compared to F&K. It seems unlikely that this distinct implementation feature drives the observed discrepancy in the game play data between our three first experiments and F&K's C5 and C10 treatments. Interestingly, Schnedler and Vadovic (2007) use the same conversion rate as we do but a larger show-up fee of 4 euros.

Individual determinants of control aversion

F&K's experimental work has informed us about some of the situational factors that influence control aversion (e.g. the level of control). Experiment 4 has complemented this work by showing that our subjects are more control-averse in the considered principal-agent game under hypothetical than under real incentives. With the help of the information we collected from our subjects in Experiments 2, 3 and 4 on a few demographic characteristics, we now offer a preliminary discussion of which individual factors might influence control aversion. This discussion is based on the results of statistical analyses which are detailed in Appendix G of the supplementary material.

Before discussing the behavioral effects of demographics, we summarize the impact of experimental conditions on control aversion. This discussion relies on the results of a linear regression in which the degree of control aversion, measured as $x^{NC} - x^{C}$, is regressed against five experimental conditions: F&K's C5 and C10 treatments and our four experiments (the regression analysis uses data from both control levels). To compare the estimated coefficients of the different experimental conditions, a series of 10 comparisons based on the F-test between the estimated model and linearly restricted models are performed. We conclude that the degree of control aversion in F&K's C5 and C10 treatments is statistically significantly higher than the degree of control aversion in Experiment 1 ($p - value < 0.01$), Experiment 2 ($p - value < 0.01$) and Experiment 3 ($p - value = 0.018$) but it is not statistically significantly different from the degree of control aversion in Experiment 4 ($p - value = 0.054$). Additionally, the degree of control aversion in Experiment 4 is statistically significantly higher than the degree of control aversion in Experiment 1 ($p - value < 0.04$). Finally, the degree of control aversion in Experiment 3 is statistically significantly higher than the degree of control aversion in Experiment 2 ($p - value < 0.01$) but it is not statistically significantly different from the degree of control aversion in Experiment 1 ($p - value = 0.198$). The results of these comparisons are in line with the non-parametric analyses reported in the previous sections of the paper and they confirm, among other things, the presence (respectively absence) of a regionality (respectively educational) effect.

According to a separate regression analysis which uses data only from the medium control level, the degree of control aversion in the replication treatment of Schnedler and Vadovic (2007) is statistically significantly lower than the degree of control aversion in F&K's C10 treatment ($p - value = 0.01$) but it is not statistically significantly different from the degree of control aversion in Experiment 1's C10 treatment ($p - value = 0.971$).

We now discuss the impact of demographics on control aversion. This discussion relies on the results of a linear regression which uses data from Experiment 2, Experiment 3 and Experiment 4 since information from subjects on demographic characteristics is available to us only in those experiments. The dependent variable $x^{NC} - x^{C}$ is regressed against the three experimental conditions as well as a gender dummy and the variable *Economist* which equals 1 when the subject is a business or economics major and 0 otherwise. We control for age and the cohort size of the session and the estimated model also includes some interactions between dummy variables. A series of 66 F-tests on linear restrictions of the estimated model permits us to compare the degree of control aversion across all sub-samples where a sub-sample is characterized by the experimental condition, the gender and whether the academic major is business/economics or not (12 sub-samples in total). We conclude that female and male economists in Experiment 3 are statistically significantly more control-averse than female and male economists in Experiment 2 ($p - value < 0.01$). However, non-economists in Experiment 3 are more control-averse than non-economists in Experiment 2 but not statistically significantly so ($p - value = 0.08$ for female and $p - value = 0.23$ for male). The regionality effect is mainly driven by economists which reminds us that it is often incorrect to equate subject-pool effects with the effect of a simple country (region) dummy variable (Botelho, Harrison, Hirsch, and Rutström,

2005). Additionally, we observe a gender effect in only one out of 6 comparisons. In Experiment 4, female non-economists are statistically significantly more control-averse than male non-economists ($p - value < 0.01$). Finally, none of the comparisons leads to the conclusion that economists are statistically significantly more control-averse than non-economists. In fact, in Experiments 2 and 4, female non-economists are statistically significantly more control-averse than female economists ($p - value = 0.03$ and $p - value = 0.01$ respectively).

Exact Wilcoxon signed rank tests confirm that economists in Experiment 3 are statistically significantly more control-averse than economists in Experiment 2 ($p - value < 0.01$), that non-economists in Experiment 3 are not statistically significantly more control-averse than non-economists in Experiment 2 ($p - value = 0.39$), that in Experiment 4 female non-economists are statistically significantly more control-averse than male non-economists ($p - value < 0.01$), and that in Experiment 2 female non-economists are statistically significantly more control-averse than female economists ($p - value = 0.02$).

Experimentally observed control aversion and societal norms/values

Assuming that societal-level variables exist which correlate with experimentally observed control aversion, we complement the results of Section 4 and further investigate whether F&K's observed regularities are likely to originate from the regional affiliation of their subjects.

In F&K's study, most agents who react negatively to the implementation of control perceive the principal's controlling decision as a signal of *distrust* or a limitation of their *choice autonomy* (pages 1623-1625). As societal-level measures of choice autonomy and distrust, we select two variables from the World Values Survey that we call *freedom of choice* and *trustworthiness* (for details see Appendix H of the supplementary material). Freedom of choice is the average score of the region's population on the question "Some people feel they have completely free choice and control over their lives, while other people feel that what they do has no real effect on what happens to them. Please indicate how much freedom of choice and control you feel you have over the way your life turns out." where answers are on a 10-point scale with 1 being 'no choice at all' and 10 being 'a great deal of choice'. Trustworthiness is the average score of the region's population on the question "Do you think most people would try to take advantage of you if they got a chance, or would they try to be fair?" where answers are on a 10-point scale with 1 being 'take advantage' and 10 being 'fair'. We hypothesize that hidden costs of control of larger magnitude prevail in regions where the average citizen expects more trustworthiness and/or his perceived freedom of choice is higher. Our hypotheses are not fully confirmed by the World Values Survey evidence. On the one hand, we observe that the region around Zurich scores much higher than the region around Jena and the region around Trento on both variables. In fact, cross-societal data show that freedom of choice and trustworthiness are extraordinarily prevalent in the region around Zurich (only Norway and Sweden reach slightly higher scores on both variables) which provides a potential explanation for F&K's striking findings. On the other hand, the comparison of the field evidence for the regions around Jena and Trento shows that scores on freedom of choice are similar but the score on trustworthiness for the region around Trento is higher than the score on trustworthiness for the region around Jena. This latter difference in scores contrasts with the experimentally observed degree of control aversion since non-economists (respectively economists) are (significantly) more control-averse in Experiment 3 than in Experiment 2. We draw the same conclusion in case we use the percentage in a region's population saying that most people can be trusted instead of the scale trustworthiness since both scales are highly positively correlated.

Our weak support for a region-based explanation is in line with the findings of many studies which tried to find a correlation between societal-level variables and experimentally measured behavior in

standard subject pools of students (among others Oosterbeek, Sloof, and van de Kuilen, 2004, and Cameron, Chaudhuri, Erkal, and Gangadharan, 2005). An exception is Herrmann, Thöni, and Gächter (2008) who observe highly significant correlations between anti-social punishment in public goods experiments and the societal norm of civic cooperation.

Future research agenda

F&K's principal-agent game seems a good starting point for the development of a valid method to measure control aversion and for studying the relative importance of implicit incentives, explicit incentives and their interaction effects. In the future, experimental studies should move away from F&K's single-shot trial environment by implementing a repetitive trial environment meaning that subjects should play the game repeatedly. The fact that F&K's study and our three repetitions made use of a single-shot trial environment is likely to account for part of the observed variability in subjects' behavior. A repetitive trial environment will allow subjects to gain experience with the interactive situation and experienced behavior in the control game is likely to be a more reliable measure of control-aversion (a matching protocol which best preserves the nature of one-shot interactions might be favored). Laboratory experiments should be combined with field experiments employing heterogeneous populations within a given country as well as across countries and game play data should be complemented with survey data on a large spectrum of individual characteristics, subjective attitudes to control and information about the workplace environment. The evidence gathered with the help of these future studies would greatly improve the assessment of the individual determinants of control aversion which, in turn, would improve our understanding of the relative importance of the disciplining and crowding-out effects of monitoring on employees' effort.

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Supplementary material

Appendices A and B contain translated versions of the instructions for Experiment 4 (originally in German). The instructions for the first three experiments are identical except for the blue paragraphs which are omitted. Appendices C and D contain translated versions of the questionnaires (originally in German). Note that for each scenario, we indicate whether the condition is "control" or "no-control". Obviously, this was not indicated in the original questionnaires. Appendix E summarizes the questionnaire data. The distributions of transfer pairs from agents to principals in Experiments 2 and 3 are displayed in Appendix F. Appendix G documents the statistical analyses on the impact of demographics and experimental conditions on control aversion. Finally, Appendix H documents the materials and methods used to investigate whether cross-regional differences in subjects' degree of control aversion relate to societal norms/values.

Appendix A. Instructions for the agents (participants A)

You are now participating in an economic experiment which has been funded by the Max Planck Institute of Economics.

Please read the following instructions carefully. The instructions will provide you with all the information you require for participation in the experiment. Please ask for assistance if there is something that you do not understand. Your question will be answered at your workplace. There is a strict prohibition of communication during the experiment.

The decisions in this experiment are purely hypothetical. All participants are paid a fixed amount of 10 euros for making their decisions in this experiment. Nevertheless, please think carefully about what you would decide in the following situation:

You will receive an initial endowment of 2.50 euros at the beginning of the experiment. You can earn additional money over the course of the experiment by collecting points. All of the points which you accrue over the course of the experiment will be converted to euros at the end of the experiment. Please note that:

1 point = 0.10 euros

At the end of the experiment, you will receive the income which you earned over the course of the experiment plus the 2.50 euros of initial endowment in cash.

The experiment

In this experiment, each participant A is associated with a participant B in a group of two. No participant knows with whom he is associated, meaning that all decisions are made anonymously.

You are a participant A.

Participant A receives an amount of 120 points at the beginning of this experiment. Participant B receives no points.

Participant A's decision:

Participant A can decide how many points he wants to transfer to participant B. The experimenter doubles each point which A transfers to B. Thus, each point which A transfers to B reduces A's income by one point and increases B's income by two points.

The formula for calculating income is as follows:

Participant A's income: 120 - transfer

Participant B's income: 0 + 2×transfer

The following examples will clarify the income formulas:
Example 1: A transfers 0 points to B. The incomes are then 120 for A and 0 for B.
Example 2: A transfers 20 points to B. The incomes are then 100 for A and 40 for B.
Example 3: A transfers 80 points to B. The incomes are then 40 for A and 160 for B.

Participant B's decision:

Before A decides how many points he wishes to transfer to B, B can determine a minimum transfer. In particular, B can constrain his associated participant A to transfer him at least 10 points. However, he can also decide not to limit participant A and thus leave his transfer decision completely free.

Therefore, there are two cases:

Case 1: Participant B constrains participant A to transfer at least 10 points to him. In this case, participant A can transfer any (integer) amount **between 10 and 120** to B.

Case 2: Participant B allows participant A to decide on his transfer freely and does not constrain him to transfer at least 10 points to him. In this case, participant A can transfer any (integer) amount **between 0 and 120** to B.

Therefore, the experiment consists of two stages:

Stage 1:

In stage 1, B decides if he will constrain A to transfer at least 10 points to him, or if he will allow A to decide freely.

Stage 2:

In stage 2, A decides which amount he will transfer to B. This amount lies

- between 10 and 120, if B constrains A to transfer at least 10 points;

  or

- between 0 and 120, if B does not constrain A to transfer at least 10 points.

The experiment is completed as soon as A has decided how many points he will transfer to B.

Please note that you, as participant A, must decide which amount you will transfer to B **before you know whether or not B will constrain you to transfer at least 10 points.** This means that you must make two decisions. You can make your entries with the help of this monitor:

Therefore, you indicate how many points you transfer to B when B constrains you to transfer at least 10 points (case 1) and how many points you will transfer when you are free to decide (case 2).

Which of the decisions is relevant for you depends on what B decides. If he constrains you to transfer at least 10 points, the decision you give under case 1 applies. If he leaves you free in your decision, the point amount which you indicate under case 2 applies.

A final income monitor will inform you of the decisions and the resulting incomes.

Your point income will be converted to euros and paid out in cash to you together with your initial endowment.

At the end of the experiment you will be paid a fixed amount of 10 euros in cash. We would be very grateful if you still think about your decisions carefully.

Do you have any questions?

Please solve the following control questions. They have no consequence on your income and only serve to check if all participants in the experiment have understood the rules.

Question 1: Assume that participant B allows A to decide freely. A transfers 22 points. What are the incomes?

Income for A:

Income for B:

Question 2: Assume that participant B constrains A to transfer at least 10 points. A transfers 12 points. What are the incomes?

Income for A:

Income for B:

Question 3: Assume that participant B allows A to decide freely. A transfers 6 points. What are the incomes?

Income for A:

Income for B:

Please raise your hand when you have solved the control questions.

Appendix B. Instructions for the principals (participants B)

You are now participating in an economic experiment which has been funded by the Max Planck Institute of Economics.

Please read the following instructions carefully. The instructions will provide you with all the information you require for participation in the experiment. Please ask for assistance if there is something that you do not understand. Your question will be answered at your workplace. There is a strict prohibition of communication during the experiment.

The decisions in this experiment are purely hypothetical. All participants are paid a fixed amount of 10 euros for making their decisions in this experiment. Nevertheless, please think carefully about what you would decide in the following situation:

You will receive an initial endowment of 2.50 euros at the beginning of the experiment. You can earn additional money over the course of the experiment by collecting points. All of the points which you accrue over the course of the experiment will be converted to euros at the end of the experiment. Please note that:

1 point = 0.10 euros

At the end of the experiment, you will receive the income which you earned over the course of the experiment plus the 2.50 euros of initial endowment in cash.

The experiment

In this experiment, each participant A is associated with a participant B in a group of two. No participant knows with whom he is associated, meaning that all decisions are made anonymously.

You are a participant B.

Participant A receives an amount of 120 points at the beginning of this experiment. Participant B receives no points.

Participant A's decision:

Participant A can decide how many points he wants to transfer to participant B. The experimenter doubles each point which A transfers to B. Thus, each point which A transfers to B reduces A's income by one point and increases B's income by two points.

The formula for calculating income is as follows:

Participant A's income: 120 - transfer

Participant B's income: 0 + 2×transfer

The following examples will clarify the income formulas:
Example 1: A transfers 0 points to B. The incomes are then 120 for A and 0 for B.
Example 2: A transfers 20 points to B. The incomes are then 100 for A and 40 for B.
Example 3: A transfers 80 points to B. The incomes are then 40 for A and 160 for B.

Participant B's decision:

Before A decides how many points he wishes to transfer to B, B can determine a minimum transfer. In particular, B can constrain his associated participant A to transfer him at least 10 points. However, he can also decide not to limit participant A and thus leave his transfer decision completely free.

Therefore, there are two cases:

Case 1: Participant B constrains participant A to transfer at least 10 points to him. In this case, participant A can transfer any (integer) amount **between 10 and 120** to B.

Case 2: Participant B allows participant A to decide on his transfer freely and does not constrain him to transfer at least 10 points to him. In this case, participant A can transfer any (integer) amount **between 0 and 120** to B.

Therefore, the experiment consists of two stages:

Stage 1:

In stage 1, B decides if he will constrain A to transfer at least 10 points to him, or if he will allow A to decide freely.

Stage 2:

In stage 2, A decides which amount he will transfer to B. This amount lies

- between 10 and 120, if B constrains A to transfer at least 10 points;

  or

- between 0 and 120, if B does not constrain A to transfer at least 10 points.

The experiment is completed as soon as A has decided how many points he will transfer to B.

The decisions of A and B will be entered on the monitors at the computers. A final income monitor will inform you of the decisions and the resulting incomes.

Your point income will be converted to euros and paid out in cash to you together with your initial endowment.

At the end of the experiment you will be paid a fixed amount of 10 euros in cash. We would be very grateful if you still think about your decisions carefully.

Do you have any questions?

Please solve the following control questions. They have no consequence on your income and only serve to check if all participants in the experiment have understood the rules.

Question 1: Assume that participant B allows A to decide freely. A transfers 22 points. What are the incomes?

Income for A:

Income for B:

Question 2: Assume that participant B constrains A to transfer at least 10 points. A transfers 12 points. What are the incomes?

Income for A:

Income for B:

Question 3: Assume that participant B allows A to decide freely. A transfers 6 points. What are the incomes?

Income for A:

Income for B:

Please raise your hand when you have solved the control questions.

Appendix C. Questionnaire 1

In the following, we present several scenarios. For each scenario, please imagine how you would react if you found yourself in the described situation and answer conscientiously the corresponding question.

Scenario 1 (no-control condition): You began a new vacation job in a supermarket. Your task is to check the balances in the cash registers in the evening, meaning that you examine whether the amounts of money in the cash registers agree with the entries. In principle, you could easily swindle the supermarket by simply removing money from the cash registers. You examined the cash registers conscientiously and without cheating, and reported the results honestly. The manager believes that you reports are honest and does not double check the balances in the cash registers.

How high is your work motivation the next day?

Very high High Medium Low Very low

Scenario 2 (control condition): You have a new job. Your boss explains your tasks to you as well as the amount of work expected of you. Before starting your work, you have to sign a binding agreement. This defines your working times exactly.

How high is your work motivation at your new workplace?

Very high High Medium Low Very low

Scenario 3 (control condition): During a job interview, you presented your knowledge, experience, and qualifications truthfully. You provide your previous employer as a reference who could confirm your information. The new employer does not hire you until he has gathered information about you from your previous employer and confirmed the accuracy of your information.

How high is your work motivation?

Very high High Medium Low Very low

Scenario 4 (no-control condition): During your studies, you start working in a small family-owned company. The head of the company explains to you that for cost reasons it is not allowed to use the photocopier or the printer for private purposes. You clearly plan to adhere to this directive. The room where the photocopier and the printer are located stands open.

How high is your work motivation?

Very high High Medium Low Very low

Scenario 5 (control condition): You do various administrative tasks for a small company. Recently, an Internet access has been provided on all PCs, but this access may be used only for business purposes. In order to limit potential abuses, the management installed special software, which lists all Internet sites the employees have visited.

How high is your work motivation?

Very high High Medium Low Very low

Appendix D. Questionnaire 2

In the following, we present several scenarios. For each scenario, please imagine how you would react if you found yourself in the described situation and answer conscientiously the corresponding question.

Scenario 1 (control condition): You began a new vacation job in a supermarket. Your task is to check the balances in the cash registers in the evening, meaning that you examine whether the amounts of money in the cash registers agree with the entries. In principle, you could easily swindle the supermarket by simply removing money from the cash registers. You examined the cash registers conscientiously and without cheating, and reported the results honestly. You realize on the way home that you forgot your umbrella. When you enter the supermarket, you see that the manager is again examining the amounts in the cash registers.

How high is your work motivation the next day?

Very high High Medium Low Very low

Scenario 2 (no-control condition): You have a new job. Your boss explains your tasks to you as well as the amount of work expected of you. Your boss asks you to follow the work times exactly.

How high is your work motivation at your new workplace?

Very high High Medium Low Very low

Scenario 3 (no-control condition): During a job interview, you presented your knowledge, experience, and qualifications truthfully. You provide your previous employer as a reference who could confirm your information. The new employer believes your information and hires you.

How high is your work motivation?

Very high High Medium Low Very low

Scenario 4 (control condition): During your studies, you start working in a small family-owned company. The head of the company explains to you that for cost reasons it is not allowed to use the photocopier or the printer for private purposes. You clearly plan to adhere to this directive. The room where the photocopier and the printer are located is locked, meaning that in general you first have to get the key from your boss.

How high is your work motivation?

Very high High Medium Low Very low

Scenario 5 (no-control condition): You do various administrative tasks for a small company. Recently, an Internet access has been provided on all PCs, but this access may be used only for business purposes. During a meeting, the management asks all employees to respect this rule.

How high is your work motivation?

Very high High Medium Low Very low

Appendix E. Questionnaire data (relative frequencies in percentages)

Table E1: Questionnaire data for Experiment 1

	Scenario 1		Scenario 2		Scenario 3		Scenario 4		Scenario 5	
Work Motivation	Control	Trust	Control	Trust	Control	Trust	Control	Trust	Control	Trust
Very low	1.79	0.00	0.00	1.79	1.61	1.79	1.79	0.00	6.45	1.79
Low	10.71	1.61	8.06	0.00	8.06	0.00	8.93	1.61	16.13	1.79
Medium	41.07	16.13	45.16	23.21	40.32	1.79	35.71	25.81	35.48	37.50
High	26.79	53.23	35.48	44.64	38.71	35.71	35.71	54.84	30.65	41.07
Very high	19.64	29.03	11.29	30.36	11.29	60.71	17.86	17.74	11.29	17.86
N observations	56	62	62	56	62	56	56	62	62	56

Table E2: Questionnaire data for Experiment 2

	Scenario 1		Scenario 2		Scenario 3		Scenario 4		Scenario 5	
Work Motivation	Control	Trust	Control	Trust	Control	Trust	Control	Trust	Control	Trust
Very low	12.07	0.00	0.00	0.00	1.72	0.00	3.45	0.00	6.90	1.72
Low	22.41	5.17	6.90	0.00	12.07	1.72	24.14	3.45	20.69	13.79
Medium	50.00	13.79	43.10	15.52	31.03	3.45	46.55	29.31	31.03	31.03
High	12.07	29.31	31.03	56.90	29.31	44.83	20.69	48.28	27.59	48.28
Very high	3.45	51.72	18.97	27.59	25.86	50.00	5.17	18.97	13.79	5.17
N observations	58	58	58	58	58	58	58	58	58	58

Table E3: Questionnaire data for Experiment 3

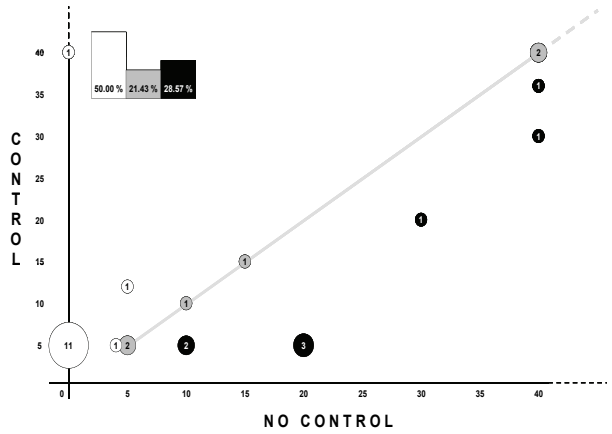
	Scenario 1		Scenario 2		Scenario 3		Scenario 4		Scenario 5	
Work Motivation	Control	Trust	Control	Trust	Control	Trust	Control	Trust	Control	Trust
Very low	2.50	0.00	2.00	1.25	2.00	1.25	1.25	0.00	6.00	1.25
Low	21.25	2.00	6.00	5.00	14.00	0.00	16.25	2.00	30.00	6.25
Medium	32.50	16.00	40.00	21.25	36.00	7.50	33.75	22.00	28.00	31.25
High	36.25	40.00	32.00	42.50	40.00	40.00	37.50	40.00	28.00	47.50
Very high	7.50	42.00	20.00	30.00	8.00	51.25	11.25	36.00	8.00	13.75
N observations	80	50	50	80	50	80	80	50	50	80

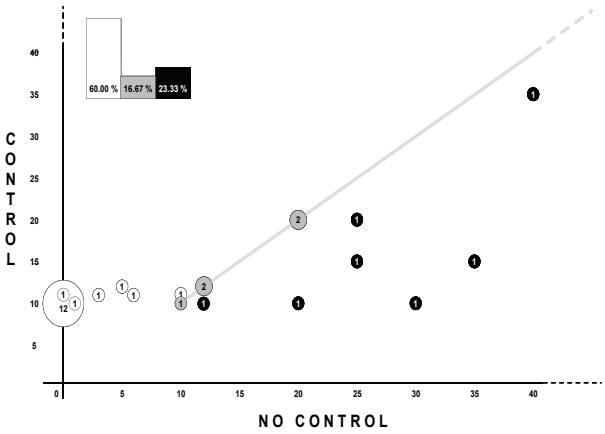
Table E4: Questionnaire data for Experiment 4

	Scenario 1		Scenario 2		Scenario 3		Scenario 4		Scenario 5	
Work Motivation	Control	Trust	Control	Trust	Control	Trust	Control	Trust	Control	Trust
Very low	1.96	0.00	0.79	0.00	2.38	0.00	0.00	0.00	8.73	0.98
Low	22.55	1.59	8.73	3.92	8.73	1.96	8.82	5.56	25.40	5.88
Medium	35.29	17.46	38.10	18.63	34.13	4.90	38.24	17.46	31.75	41.18
High	29.41	41.27	40.48	46.08	41.27	31.37	33.33	51.59	25.40	42.16
Very high	10.78	39.68	11.90	31.37	13.49	61.76	19.61	25.40	8.73	9.80
N observations	102	126	126	102	126	102	102	126	126	102

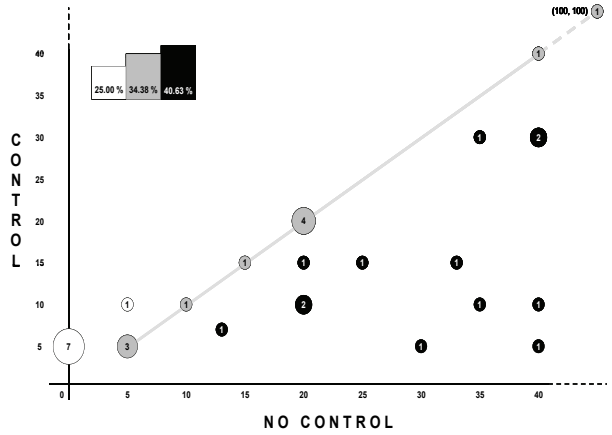
Appendix F. Distributions of transfer pairs in Experiments 2 and 3

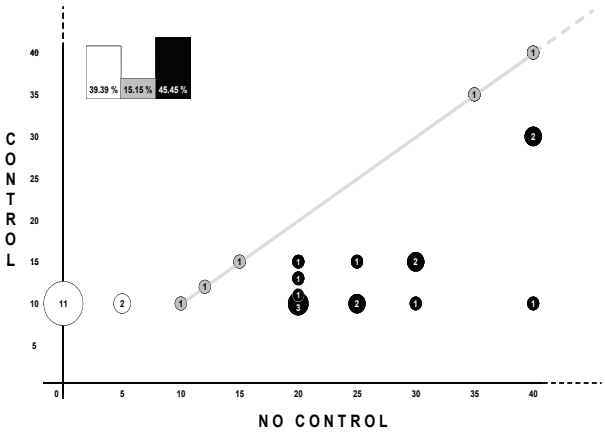
Experiment 2





Experiment 3





Appendix G. The impact of demographics and experimental conditions on control aversion

In this appendix we detail the statistical analyses on the degree of control aversion in F&K's principal-agent game whose results are discussed in our concluding section. These statistical analyses use all experimental data available to us.

The degree of control aversion is captured by the difference between transfers in the no-control condition (x^{NC}) and transfers in the control condition (x^{C}). In most analyses, transfers in treatments C5 and C10 are pooled together. We restrict our analyses to transfer pairs such that $max\{x^{C}, x^{NC}\} \leq 40$ since transfers strictly larger than 40 indicate some likely confusion and are very few in number (in each analysis, they account for less than 3% of the working sample). The exclusion of transfer pairs such that $max\{x^{C}, x^{NC}\} > 40$ also implies that the difference $x^{NC} - x^{C}$ is not subject to censoring. Note that, due to different minimum transfer requirements, treatments C5 and C10 are different data-generating processes. To control for potential heteroscedasticity in the data, heteroscedasticity consistent estimations of the covariance matrix (MacKinnon and White, 1985) are systematically implemented and we report the corresponding robust standard errors. Analyses are conducted in the *R* Environment (R Development Core Team, 2009) and they rely upon the estimator HC_3 of the *sandwich* package (Zeileis, 2004).

The impact of experimental conditions on control aversion

Table G1 summarizes the results of a linear regression (OLS) in which the dependent variable $x^{NC} - x^{C}$ is regressed against a set of explanatory factors identifying alternative experimental conditions (11 observations are dropped from the regression analysis due to the exclusion of transfer pairs such that $max\{x^{C}, x^{NC}\} > 40$). All the independent variables are dichotomous variables. *FK* identifies data from F&K's C5 and C10 treatments. *Exp1* (respectively *Exp2*, *Exp3* and *Exp4*) identifies data from our first (respectively second, third and fourth) experiment. The estimated coefficients of *FK*, *Exp3* and *Exp4* are significantly positive whereas the estimated coefficients of *Exp1* and *Exp2* are not significantly different from zero. We conclude that there is significant control aversion in F&K's C5 and C10 treatments as well as in our third and fourth experiments but no significant control aversion in our first and second experiments. We also conducted a separate analysis of treatment C10 to include the data from the replication treatment of Schnedler and Vadovic (2007). The results of this analysis are briefly discussed in our concluding section (and details are available from the authors upon request).

To complement the pieces of information obtained from the regression coefficients, a series of 10 comparisons based on the *F*-test between the estimated model and linearly restricted models are performed. The tests aim at comparing the estimated coefficients for different sub-samples where a sub-sample is characterized by the experimental condition (5 sub-samples in total). For example, we test whether the degree of control aversion in Experiment 1 significantly differs from the degree of control aversion in F&K's C5 and C10 treatments by comparing the model with the restriction *Exp1=FK* to the estimated model. The results of these comparisons are discussed in our concluding section (and details are available from the authors upon request).

The impact of demographics on control aversion

Our next statistical analysis aims at assessing the behavioral effects of demographics in F&K's principal-agent game. To do so, we complement the previous set of explanatory factors, experimental conditions, with the three demographics controls we collected information on: academic major, age and gender. Since information from subjects on these three demographic characteristics were only collected in our last three experiments, our next statistical analysis uses experimental data only from Experiment 2, Experiment 3 and Experiment 4 (5 observations are dropped from the analysis due to the exclusion of transfer pairs such that $max\{x^C, x^{NC}\} > 40$).

Table G2 summarizes the results of a linear regression (OLS) in which the dependent variable $x^{NC} - x^C$ is regressed against an intercept, the dummy variable *Economist* (equals 1 when the subject is a business or economics major and 0 otherwise), the dummy variable *Female* (equals 1 when the subject is a female and 0 otherwise), the variable *Age* which measures the subject's age in the year of participation to the experiment (coded as a continuous variable), the dummy variable *Small cohort* (equals 1 when the cohort size of the session is less than or equal to 16 subjects and 0 otherwise), *Exp2* and *Exp3*. The estimated model also includes some interactions between dummy variables.[1] We included the dummy variable *Small cohort* as we observed in Experiment 3 that the size of the cohort has some influence on the degree of control aversion in the session. The way the dummy variable *Small cohort* is coded allows the inclusion of an interaction effect with *Exp2* in the estimated model and assigns approximately half of the sessions in Experiment 3 to each cohort level (the conclusions derived from this analysis are basically unchanged when the dummy variable *Small cohort* is omitted).

The estimated coefficient of *Female* is significantly positive which leads to the conclusion that female non-economists in Experiment 4 are more control averse than male non-economists in Experiment 4. The estimated coefficients of *Exp2*×*Female* and *Exp3*×*Female* are significantly negative which implies that gender effects are weaker in Experiments 2 and 3 than in Experiment 4. All remaining estimated coefficients are not significantly different from zero. The information shown in Table G2 is complemented by a series of 66 F-tests on linear restrictions of the model aimed at comparing the regression intercepts for different sub-samples where a sub-sample is characterized by the experimental condition, the gender and the academic major being economics or not (12 sub-samples in total). The results of these comparisons are discussed in our concluding section (and details are available from the authors upon request).

Remark: For the two regressions summarized in Table G1 and Table G2 the dependent variable ranges from -40 to +35, with first, second and third quartile of the distribution equal to -5, 0, and 10, respectively. The distribution of the dependent variable in the first regression has a slightly higher first and second moment than that of the second regression (mean=4.145 vs. 2.763; std. dev.= 12.129 vs. 11.415).

[1] This specification was preferred to the one without interactions on the basis of an Akaike Information Criterion (AIC) comparison. The value of the AIC for the no-interactions model is 1779.611 while the same value for the reported model is 1766.373.

Table G1: Effects of Experimental Conditions on $x^{NC} - x^{C}$

	OLS
$x^{NC} - x^{C}$	Coefficient (robust std. err.)
FK	7.853 (1.124)***
Exp1	1.034 (1.406)
Exp2	-1.948 (1.270)
Exp3	3.594 (1.399)*
Exp4	4.764 (1.136)***
Obs	427
R^2	0.171

***(0.1%); **(1%); *(5%); .(10%) significance level

Table G2: Effects of Demographics on $x^{NC} - x^{C}$

	OLS
$x^{NC} - x^{C}$	Coefficient (robust std. err.)
Intercept	-0.701 (9.793)
Exp2	0.081 (3.357)
Exp3	4.998 (3.520)
Economist	-2.943 (2.729)
Female	6.810 (2.355)**
Age	0.121 (0.400)
Small cohort	1.915 (3.021)
Exp2×*Economist*	0.498 (3.650)
Exp3×*Economist*	6.320 (3.536).
Exp2×*Female*	-7.860 (3.614)*
Exp3×*Female*	-7.279 (3.432)*
Economist×*Female*	-4.361 (2.976)
Exp2×*Small cohort*	-7.400 (5.973)
Exp3×*Small cohort*	-11.433 (3.859)**
Obs	232
R^2	0.197

***(0.1%); **(1%); *(5%); .(10%) significance level

Appendix H. Differences in control aversion and societal norms/values

In their study, F&K asked agents to indicate their perception of the principal's controlling decision and they partitioned the answers into six categories.[2] The answers of most agents who react negatively to control fall into the categories "distrust" and "lack of autonomy". Consequently, societal-level variables which capture the notions of distrust or choice autonomy are likely to relate to the agents' behavior (assuming that societal norms/values that correlate with the observed degree of control aversion exist). We now present two variables of the WVS which, according to us, capture well the notions of distrust and choice autonomy.

The category "distrust" encompasses agents who "explicitly mentioned a feeling of not being trusted to transfer a positive amount" (Falk and Kosfeld, 2006, p. 1624). Accordingly, the principal trusts (distrusts) the agent's pro-sociality if he leaves the transfer unrestricted (controls). We capture the notion of distrust with the help of the variable TRUSTWORTHINESS which is the average score of the region's population on the question "Do you think most people would try to take advantage of you if they got a chance, or would they try to be fair?" where answers are on a 10-point scale with 1 being 'people would try to take advantage of you' and 10 being 'people would try to be fair'.

The category "lack of autonomy" represents answers of agents who "expressed a negative feeling of being restricted in [their] freedom of choice" (Falk and Kosfeld, 2006, p. 1624), or who "say that they feel a lack of autonomy and a reduced opportunity for determining the outcome at their own will" (Falk and Kosfeld, 2006, p. 1624, footnote 13). We capture the notion of choice autonomy with the help of the variable FREEDOM OF CHOICE which is the average score of the region's population on the question "Some people feel they have completely free choice and control over their lives, while other people feel that what they do has no real effect on what happens to them. Please use this scale where 1 means 'no choice at all' and 10 means 'a great deal of choice' to indicate how much freedom of choice and control you feel you have over the way your life turns out."

We expect both variables to correlate positively with the magnitude of hidden costs of control in a given region. We now detail our method of data collection and discuss whether the two considered variables relate to the agents' behavior.

We collected evidence from the fifth wave of the WVS which has been carried out in 57 countries between 2005 and 2008 (Italy 2005, Germany 2006 and Switzerland 2007). Below, we report the values of our two variables at the level of the country (Germany, Italy and Switzerland), at the level of a broad geographical region around each city (Thuringia, Saxony-Anhalt and Saxony for Jena; Trentino-Alto Adige, Lombardy and Veneto for Trento; German Switzerland for Zurich), and at the level of the smallest region around Jena and Trento for which survey data are available (Thuringia for Jena; Trentino-Alto Adige for Trento; no data are available for a region around the city of Zurich smaller than German Switzerland). Since our experimental data have been collected in only one city per country, and since within-country differences might be of the same magnitude as between-country differences (Oosterbeek, Sloof, and van de Kuilen, 2004), we favor survey data of a region around the city rather than survey data at the country level. Unfortunately, we cannot rely on the survey data of the small

[2] After the agent has decided about her transfers but before feedback about the principal's decision and payoffs are given, she is asked: "What do you feel if participant B (principal) forces you to transfer at least $\underline{x}$ ECUs?" The answer is given in free form.

regions around the three cities because there are very few observations for Trentino-Alto Adige in the WVS. Consequently, we investigate the relation between the agents' behavior in F&K's principal-agent game and the values of our two variables at the level of a broad region around Jena (henceforth TSAS), around Trento (henceforth TLV), and around Zurich (henceforth GS).

Table H1 summarizes the evidence collected from the WVS. The first column lists the regions, the second column reports the population size of the different regions, the third column shows the number of survey respondents per region for each of our two variables, and the last two columns report the values of our two variables in each region. Note that the country-level values are taken from the data analysis offered on the website of the WVS whereas the remaining values have been computed from the official data file of the WVS (World Values Survey Association, 2009).[3]

Table H1: Regional scores on TRUSTWORTHINESS and FREEDOM OF CHOICE

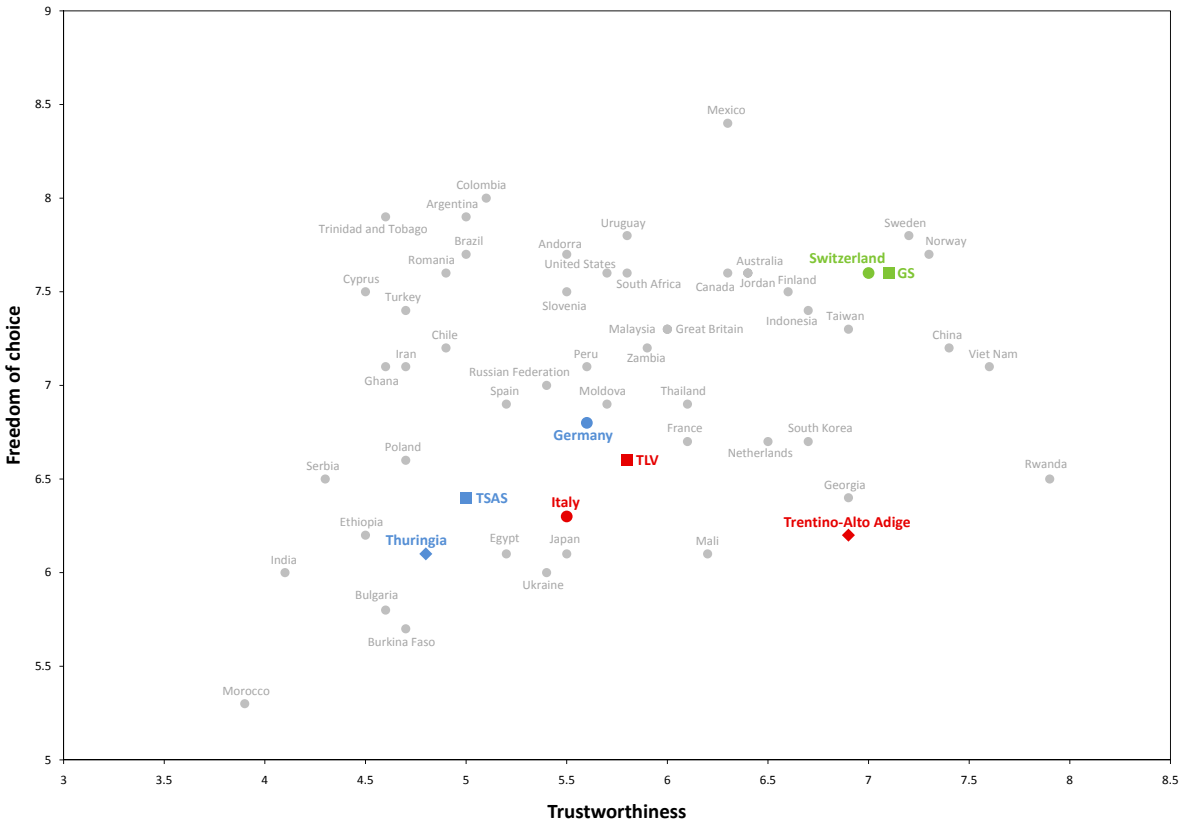
Region	Population (millions)	Survey respondents	TRUST-WORTHINESS	FREEDOM OF CHOICE
Jena				
Thuringia	2.3	178-179	4.8	6.1
TSAS	8.8	664-666	**5.0**	**6.4**
Germany	82.0	2022-2037	5.6	6.8
Trento				
Trentino-Alto Adige	1.0	17-18	6.9	6.2
TLV	15.5	249-253	**5.8**	**6.6**
Italy	58.1	986-988	5.5	6.3
Zurich				
GS	4.6	880-887	**7.1**	**7.6**
Switzerland	7.7	1226-1239	7.0	7.6

The fourth column of Table H1 reveals that GS ranks first on TRUSTWORTHINESS (7.1), followed by TLV (5.8) and TSAS (5.1). The ranking of GS on TRUSTWORTHINESS is consistent with the striking proportion of control-averse agents in Zurich. On the contrary, the ranking of the remaining two regions is negatively correlated with the agents' degree of control aversion in our experiments. Incidentally, Trentino-Alto Adige scores very similar to GS, but there are not sufficient observations in the WVS to rely on. Although we favor the question examined above, we could have relied on the frequently used general trust question of the WVS: "Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?" The percentages of residents answering "most people can be trusted" reveal that qualitatively, we would still observe the same result. It turns out that both variables on distrust are highly correlated.

The fifth column of Table H1 shows the values of the variable FREEDOM OF CHOICE. The same ranking is observed as for TRUSTWORTHINESS: GS ranks first (7.6), followed by TLV (6.6) and TSAS (6.4). The ranking of GS on FREEDOM OF CHOICE is again consistent with the high magnitude of hidden costs of control in Zurich. TLV and TSAS have similar scores which do not reflect the difference

[3]Too few data are available to base our analysis on the students' population.

Figure H1: Cultural map of societies according to TRUSTWORTHINESS and FREEDOM OF CHOICE



observed in our experiments between the agents' degree of control-aversion in Jena and in Trento.[4]

Figure H1 shows a cultural map of societies where locations reflect scores on TRUSTWORTHINESS and FREEDOM OF CHOICE for 2005-2008. GS is clearly different from TSAS and TLV. The differences between TSAS and TLV are less pronounced.

[4]The variable FREEDOM OF CHOICE is a subjective measure of the degree of choice autonomy. We would have liked to rely on a more objective measure like 'effective democracy' which reflects the extent to which formally institutionalized civil and political liberties are actually practiced. According to Inglehart and Welzel, effective democracy indicates "how much free choice people really have in their lives" (Inglehart and Welzel, 2005, p. 154). Unfortunately, effective democracy is only measured at the country-level with Switzerland scoring higher than Germany which in turn scores higher than Italy (personal communication with Christian Welzel whom we thank).

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