On the Efficiency and Effectiveness of Policy Instruments for the Procurement of Environmental Services

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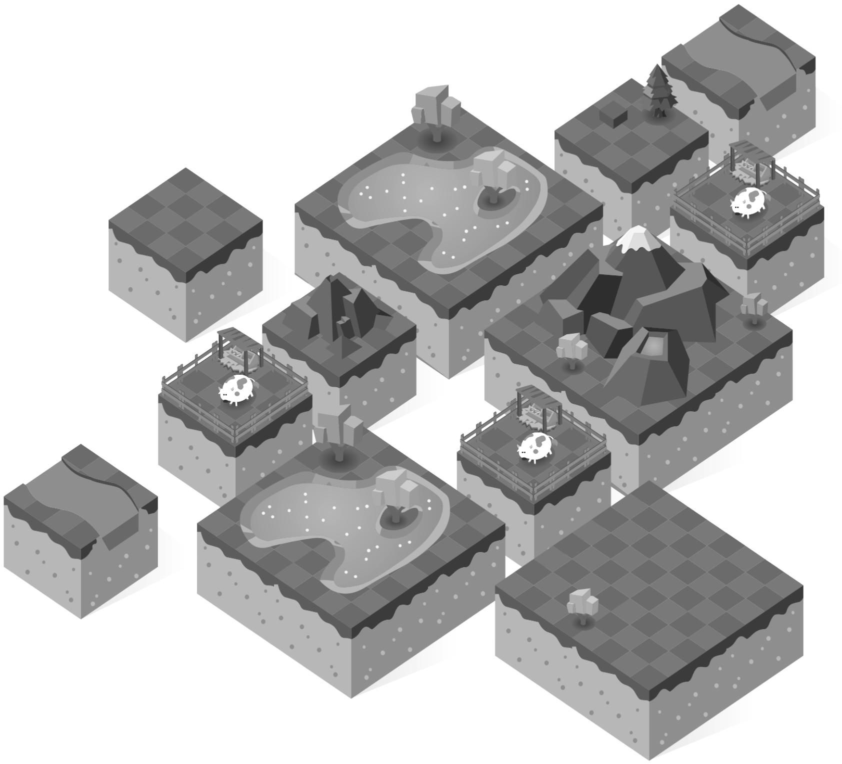
CHAPTER SIX

Trust and Control



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6.1 Introduction

Trust and control are perhaps the two most influential factors that determine risk in agency problems under asymmetric information – settings in which a principal grants decision making power to an agent who may or may not have conflicting interests in carrying out tasks for the principal (*cf.* Fama 1980, Alston 1981, Alston and Higgs 1982, Falk and Kosfeld 2006). To align interests, the principal typically monitors the agent, sets minimum requirements, offers incentives for desired actions or fines for unwanted behavior, *etc.* In contrast to these tangible examples of control, the principal can also take a risk and trust the agent by not imposing fines or not setting performance standards. Trusting the agent and refraining from control can increase the agent's willingness to act in the principal's interest if the agent is intrinsically motivated to act, making control, perceived as distrust by the agent, a counterproductive activity. For instance, Luhmann (1979) argues that distrust can be "objectively inexplicable" to the subject. In the act of controlling an agent, the latter considers himself relieved of his moral obligations to pursue the principal's objectives and has the freedom to act in his own interests and/or take revenge for an undeserved signal of distrust (Luhmann 1979, p. 74). This observation is typically referred to as the "crowding-out effect" of intrinsic motivation by extrinsic stimuli (*cf.* Frey 1993, 1997). Whether a principal should be more inclined to trust the agent thus depends on the existing level of motivation of the agent, the (non)availability of other alternatives, and also on the risk attitude of the principal, *e.g.* choosing to trust but still enforce a low level of control to put a lower bound on the level of potential disappointment (Luhmann 1979, p. 89).

Settings in which trust can be an efficient substitute for control are numerous – think of the regulation of working hours, minimum output/quality requirements or even refraining from bargaining about the concrete terms of a marriage (Fehr and List 2004). In these instances, the (often) implicit assumption is that both the principal and the agent are aware of how beneficial the service is to the principal, and also how costly it is for the agent to provide the service. In this chapter, we revisit the issue of trust versus control in a setting where there is full information about the agent's costs of providing the service, but where the principal has better information about how valuable the service is to her at a particular moment in time.[104] Catalyzing climate change adaptation in the private sector is a clear example. Private parties often do not have access to sufficient information for adaptation action and governments can step in to generate and distribute such knowledge (*cf.* Berkhout 2005).[105] Clearly, due to the typical asymmetric information problem concerning the costs of private adaptation action, there exists a *relational* risk between the principal and the agent in this context. But given the uncertainty that surrounds the benefits and effectiveness of adaptation measures (Füssel 2007, Adger *et al.* 2009, van Vuuren *et al.* 2011), the climate change adaptation context also exhibits a *performance* risk (Das and Teng 2001). In other words, the uncertainties surrounding climate change complicate the formation of beliefs and thus the perceived public benefits of private adaptation to climate change over time.

To the best of our knowledge, the potential trade-off between trust and control has not been studied in these settings in which the principal has better information about the cost-benefit ratio of the agent's actions than the agent himself, and in which the principal is unable to impose the first-best level of regulation. We hypothesize that a principal can benefit from removing the reverse information asymmetry because (fully) informed agents are more prone to take actions that are in line with the preferences of the principal.

We study the relationship between trust, control and reverse asymmetric information in a repeated principal-agent setting. Our methodology involves economic laboratory experiments with students and water management professionals from the Netherlands. The experimental game is due to Falk and Kosfeld (2006), and we implement a full factorial two-by-two between-subject design with the following design factors: (i) full information or limited information about the marginal benefits of the

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[104] The bulk of the literature on asymmetric information focuses on the case in which the agent has better information about either the benefits of his/her action to the principal, or the costs he/she incurs when undertaking the desired action (see for example Holmström 1979, Laffont and Tirole 1993, Macho-Stadler and Perez-Castrillo 2001, Salanié 2005); see also Chapters 3-5. But the case of so-called *reverse* asymmetric information is also highly policy relevant, especially in rapidly changing environments (see for example Jakobsen and Meyer 2008). Banerjee *et al.* (2014) make a similar assumption in an experimental setting and state: "*Usually the regulator has more accurate knowledge about the environmental benefit of a project on a particular private property than its owner.*"

[105] Other examples that come to mind are information campaigns that increase environmental awareness (Endres 1997, Mees *et al.* 2014; see also Chapter 2) or a move towards self-regulation (*e.g.* participatory monitoring or joint problem solving) and away from traditional command and control instruments (see for instance Sinclair 1997, Forester 1999, Gunningham and Sinclair 1999, Gouldson *et al.* 2008, Brown *et al.* 2012).

provided service, and (ii) an option or no option to set a minimum performance requirement. This design allows us to determine the effect of offering the principal the option to control the agent's actions, the effect of extending to the agent full information on the marginal benefits of the provided service, or the combined effect of offering both control and full information. These effects are measured by comparing them against a benchmark treatment in which the subject with the role of the principal is not able to set a minimum performance requirement and the subject acting as the agent receives only limited information on the marginal benefits of the provided service. All treatments were implemented with Dutch students from Tilburg University, and repeated with groups of Dutch water management professionals.

We find that informed agents have a higher propensity to supply services to the principal. For our student subjects, we find that information is a substitute for control. Extending information on the marginal benefits has a positive effect on the agent's propensity to supply the service the same way setting a minimum performance requirement has. Offering the principal the option to set a minimum performance requirement (*i.e.*, partially control the agent's action) when the agent already receives information (or vice versa) does not further increase agent performance with student subjects. The intrinsic motivation of students to cater for the needs of the principal is low – questionnaire results show that for them greed is the main driver. Professionals are more inclined to show crowding-out of intrinsic motivation when control is applied. Professional principals who choose high levels of control are thus more likely to decrease intrinsically high service levels than students playing the role of the principal. When asked for their considerations to exert effort in the role of the agent, professionals are also more likely than students to say that receiving information on the marginal benefits has a positive effect on their propensity to provide the service. For most professionals, efficiency and also fairness considerations are more important than signals of (dis)trust when deciding on their level of service provision.

Related work on crowding-out dates back to the well-known example of the effect of paying for blood donations (Titmuss 1970), and other seminal works showing that formal interventions reduce the 'warm glow' of voluntary action (Andreoni 1993) and weaken moral sentiments (Bowles 2008). As mentioned before, formal interventions are counterproductive if they are interpreted as signaling distrust (Luhmann 1979, Fehr and Fischbacher 2005). In particular, the results for our student subjects are very similar to those of Ziegelmeyer *et al.* (2012) who find that control does not have substantial negative effects on agents' performance levels, and are thus also not able to reproduce a significant crowding-out effect as is found in Falk and Kosfeld (2006). Most striking is that when asked about their considerations, our students respond that greed is their main driver, while Falk and Kosfeld's questionnaire results show that distrust and lack of autonomy are the most important drivers to transfer (fewer) points to the principal. Yet the results for our sample of water management professionals are very similar to those of Fehr and List (2004). In the latter, the authors compare the behavior of CEOs with the behavior of students in a trust game and also find that while on average only few refrain from control, CEOs do so more often than students as they seem to better understand that it can be in their own interest to reciprocate trustworthy behavior with less control.[106] We add to the results of Fehr and List (2004) in that professionals not only better understand that refraining from (high levels of) control can be beneficial, but also that higher efficiency levels can be reached by putting to use the information on productivity if it is available to them. If we would be permitted to alter Gneezy and Rustichini's (2000) well-known proverb "Pay enough or don't pay at all", and apply it to our subsample of professionals, we would conclude: "Control a little, but whatever you do, inform your agent".

6.2 The game and the experimental design

The game designed by Falk and Kosfeld (2006) can be summarized as follows. The agent can provide a service, at positive cost to himself, that benefits the principal. At the beginning of a period the agent receives a

[106] Fréchette (2011) includes Fehr and List (2004) in his review on experimental studies that feature subject pools of both students and professionals. He finds that only in 1 out 13 studies students and professionals behave different with respect to the main theoretical predictions of these studies. In fact, he argues that Fehr and List (2004) is 1 out of 9 in which the results between the subject pools are very similar – although CEOs refrain from control more often, the main insights from the trust game experiments using student subjects are supported by experiments with CEOs – senders send money and responders send back money in both subject pools. In this sense, our main result is also similar between subject pools: informed agents have a higher propensity to supply services to the principal. And while the professionals show a reduction in intrinsic motivation for high levels of control, refraining from control reduces average transfers in both subject pools.

number of points, $W > 0$, that he can keep himself, or that he can transfer to the principal ('the service'). Any unit transferred reduces the agent's earnings by one unit, but it increases the principal's earnings by $\alpha > 1$ units. Aggregate payments are maximized if the agent transfers his entire endowment of W points to the principal in every period. If the agent is rational and only cares about his own payoffs, he will transfer zero points to the principal, independent of the number of periods in which the interaction takes place.

To assess the relationship between trust and control, Falk and Kosfeld (2006) implemented essentially two treatments – the one described above, and a second in which the principal has the option to impose a lower bound on the agent's transfer, T, where $0 \leq T \ll W$. Given the minimum transfer T imposed, the agent can then decide to transfer any number of points between T and W, and the principal's earnings in that period are equal to α times the number of points the agent decided to transfer. If agents only care about their own payoffs, it is in the principal's interest to impose threshold T. If, on the contrary, agents are intrinsically motivated to provide the service, they may perceive the principal imposing a minimum service provision level as a signal of mistrust, and they may provide fewer services than absent the minimum contribution level.

In their 2006 paper, Falk and Kosfeld set W equal to 120, α was equal to 2, the interaction took place just once, and in the treatments where the principal could impose a minimum threshold level, T was set at 5, 10 or 20. We extend their analysis by randomly varying α (which is equal to either 2 or 4, each with a 50 percent probability), and by introducing the possibility of a (reverse) information asymmetry – the principal is always informed about the actual value of α, but depending on the treatment the agent may or may not be informed. Apart from this (lack of) information on the marginal benefits, our design differs from that of Falk and Kosfeld in several respects. Falk and Kosfeld implemented a one-shot design, while in our experiment the interaction between the principal and the agent lasts ten periods – as is natural in our design with random realizations of the marginal benefit parameter. Also, whereas in Falk and Kosfeld the choice of the minimum transfer is a binary one (in a treatment with the option to impose a minimum transfer of T points, the principal could set the threshold at either 0, or T), in our experiment the principal could choose any integer threshold between 0 and T. We opted for a range of minimum threshold choices because we were interested in the dynamics of control over time.

Our two key treatment variables are the following. One treatment variable has the agent either (i) informed on whether the value of α is equal to 2 or 4 *before* he chooses the level of service provision, or (ii) aware that α equals 2 or 4 with a 50 probability but only informed about the parameter's actual realization *after* he chose the level of service provision. The second treatment variable is whether the principal is able to impose a minimum level of service provision (between 0 and T) on the agent, or not. Using 0 and 1 to denote whether information provision (I) and control (C) are either off or on, we have four key treatments in our 2x2 full factorial between-subject design: I0C0, I1C0, I0C1 and I1C1; see Table 6.1.

Table 6.1. The four key treatments.

	No control	**Control**
No full information	Treatment I0C0: Marginal benefit of 2 or 4 with a 50% chance, agent receives exact information only *after* transfer decision is made; principal is not able to impose a minimum transfer.	Treatment I0C1: Marginal benefit of 2 or 4 with a 50% chance, agent receives exact information only *after* transfer decision is made; principal is able to impose a minimum transfer with a maximum of 20 points.
Full information	Treatment I1C0: Agent knows whether principal's marginal benefit of receiving a transfer is either 2 or 4 at the moment he chooses his transfer; principal is not able to impose a minimum transfer.	Treatment I1C1: Agent knows whether principal's marginal benefit of receiving a transfer is either 2 or 4 at the moment he chooses his transfer; principal is able to impose a minimum transfer with a maximum of 20 points.

Our benchmark is I0C0 – the treatment in which the agent does not have exact information about the benefits to the principal if the agent transfers an additional unit to her (no full information), and in which the principal is not able to set a minimum transfer level (no control). We compare the outcomes of this base treatment with those of three other treatments. In I0C1 the agent does not have exact information about the principal's benefits of receiving transfers at the time he chooses his transfer, but the principal is able to impose a minimum transfer (no full information, but with the option of control). In I1C0 the agent does have exact information about the marginal benefits of transferring units at the moment he decides about the size of the transfer, but the principal is not able to impose a minimum transfer level (full information, no control). And in I1C1 the agent has full information about how valuable it is to the principal to receive transfers while the principal is able to impose a minimum transfer level (full information, with the option of control). Comparing the outcomes of I0C0 and I1C0 gives insight into the impact of eliminating the information asymmetry on the agent's propensity to provide the service, while comparing the outcomes of I0C0 and I0C1 gives insight into the effectiveness of controlling the agent's transfer in the presence of information asymmetries. Finally, the outcomes of I1C1 provide insight into how information and control interact with each other (whether they are complements, or substitutes).

The key treatments are summarized in Table 6.1, and they were implemented using both student subjects and professionals. In addition, we ran I0C1 and I1C1 with student subjects using $T = 10$ (as opposed to $T = 20$ in the key treatments). We refer to these treatments as I0C1LT and I1C1LT, where LT indicates the use of a low threshold. These treatments were implemented to check whether the range of minimum thresholds that principals could choose from had an impact on the decisions made by the agent and the principal. We conjecture that in line with Falk and Kosfeld's results, the impact of control will depend on the possible level of control the principal can implement. Hence, while the positive effect of being able to set low levels of control is probably small, the hidden costs of control may be substantial. For instance, if we dub the 1 to 10 range "low control", and the 11 to 20 range "high control", assuming that the hidden costs of control exists, low control in I1C1LT might generate a lower payoff in I1C1LT than opting for no threshold or low control in I1C1.

We predict that, as suggested by the literature on other regarding preferences (*cf.* Kahneman *et al.* 1982, Fehr and Gächter 2000, Camerer 2003, Camerer and Fehr 2006), our subjects will not act in a purely selfish manner when deciding to transfer points to the principal. However, in light of the results of Fehr and List (2004) and Ziegelmeyer *et al.* (2012), we expect that our student subjects acting as the agent will exhibit less intrinsic motivation to provide services to the principal than the professional subjects, since the former are expected to be more inclined to attend to selfish preferences. To the best of our knowledge there are no experimental studies on the effect of providing information on the marginal benefits of a productive activity in (variants of) trust games, and hence, we have to make an educated guess that participants will transfer more points to the principal when the marginal benefits are higher, but the considerations for transferring few or many points are numerous (see Falk and Kosfeld 2006, p. 1624). Subjects might respond to information on high marginal benefits in a particular period with feelings of spite or unfairness, they could feel inclined to transfer more points when it is more efficient to do so, *etc.* Therefore, a questionnaire was conducted at the end of every session in which subjects were asked to state what their considerations were regarding the number of points transferred, possibly in response to a threshold set by the principal.[107]

All treatments were implemented with Dutch students from Tilburg University, and repeated with groups of Dutch water management and climate change professionals. In total, 210 students and 86 professionals participated in the experiment, with approximately 30-40 students and 20 professionals in every treatment (see Table 6.2 for details). The professionals were civil servants (climate change and water management experts) recruited from the Province of North Brabant and employees recruited from Deltares Utrecht (an independent policy research institute in the fields of water, subsurface resources and infrastructure). The students at Tilburg University had different academic backgrounds: economics, legal studies, management and social sciences. The student sessions were performed in the months of February, March and June of 2014, while the sessions with the professionals took place in the months of April, July and September of the same year. The instructions were written in Dutch and are available upon request (an English translation is also available). We were careful to frame the treatments in a neutral matter, *e.g.* agent subjects were referred to as "participant A" and principals as "participant B", and the marginal benefit of the transfer was called the "multiplication factor". No subject participated in more than one treatment. Session lasted on average 40-55 minutes. To account for differences in the marginal value of money, student subjects earned €1.00 for every 100 points and received a shop-up fee of €5.00, while professionals earned €1.00 for every 50 points and received a shop-up fee of €10.00. On average, students left the experiment €13.61 and professionals

[107] We differ from Falk and Kosfeld here as we avoid their value-laden question "What do you feel if participant B forces you to transfer at least $\underline{x}$ points?" (p. 1624).

earned €30.57 (including the show-up fee). The experiment was programmed and conducted with z-Tree software (Fischbacher 2007).

6.3 Experimental results

Tables 6.2 and 6.3 present our main results. We find large differences in transfers between treatments and between students and professionals. In each cell we present the mean number of points transferred (averaged over all periods), its standard deviation, and the number of participants – with the professionals' (students') data being depicted in bold (normal) font

Let us first analyze the results for the student subjects. Treatment I0C0 is our benchmark treatment in which the agents receive only probabilistic information on the marginal benefits of the transfer in each period and principals are not able to set a minimum threshold for the transfer of the agent. Moving from treatment I0C0 to I1C0 we find that providing agents with full information on the marginal benefits transfers significantly increases the average transfer from the agent to the principal (Mann-Whitney U test, $p = 0.032$). Moving from I0C0 to I0C1 we find that having the possibility to control also significantly increases the average transfer of the agent to the principal (Mann-Whitney U test, $p = 0.001$). However, adding control when full information is provided (comparing I1C0 and I1C1), or providing agents with full information when control is already available (comparing I0C1 and I1C1) does not significantly increase average transfers (the p-values of the associated Mann-Whitney U tests are equal to 0.704 and 0.812, respectively). In other words, the relationship between control and information in raising average transfers is one of substitutes; control does not complement information, or vice versa.

Table 6.2. Summary of transfers across treatments.

	No full information	*Full information*
No control	10.9 (7.50)	16.6 (9.37)
(I0C0, I1C0)	**25.6 (15.7)**	**42.0 (39.3)**
	n = 44, **n = 22**	n = 34, **n = 22**
Control	20.5 (5.71)	19.7 (1.71)
(I0C1, I1C1)	**44.5 (40.6)**	**29.8 (12.5)**
	n = 36, **n = 20**	n = 36, **n = 22**
Low levels of control	14.7 (7.20)	15.3 (7.58)
(I0C1LT, I1C1LT)	-	-
	n = 30	n = 30

Professionals' data in bold. Standard deviations are in parentheses. Transfers in points, averaged over all agents and over all periods, standard deviations are in parentheses, and *n* is the number of subjects participating in the treatments.

To check whether the range of possible thresholds (*i.e.* the level of control that can be enforced) had an effect on these results, we ran two additional treatments with our student subjects (I1C1LT and I0C1LT) in which the minimum threshold range was restricted to [0,10]. Here, we find that when the principal's range of minimum thresholds is restricted, agents transfer significantly fewer points (Mann-Whitney U tests, $p = 0.029$ for I1C1LT *vs.* I1C1, and $p = 0.014$ for I0C1LT *vs.* I0C1). These results suggest that students do not show hidden costs of control, and that private payoff maximization is their main driver.[108] Consistent with this, when comparing treatment I0C0 with I0C1LT, we do not find that low levels of control significantly improve transfers (Mann-Whitney U test, $p = 0.201$).

If we compare the behavior of the professionals to the behavior of our student subjects, we find that professionals transfer significantly more points than students in all treatments; the relevant Mann-Whitney U tests yield $p < 0.01$ for treatments I0C0 and I1C1 and $p < 0.05$ for treatments I1C0 and I0C1. Yet the impact of the two treatment variables, information and control, on the behavior of professionals is qualitatively identical to that when using student subjects. While none of the treatment differences are significant using standard cut-off values (due to the relative small number of playing

[108] Since student subjects did not show substantial hidden costs of control at any level of control in Ziegelmeyer *et al.*'s (2012) repetitions of Falk and Kosfeld (2006), we conjectured that we also needed additional treatments for our student subjects to check whether crowding-out might occur for lower ranges of control. We did not run these additional treatments for our subsample of professional because we already find evidence for hidden costs of (low) control in I1C1 for them.

pairs), we do observe that the average transfers in I0C1 and I1C0 are much larger than in I0C0, and that they are also roughly equally large. Viewing the results of the experiments with subjects with real-world regulatory experience, we come to the same conclusion as when using student subjects – information and control are substitutes, not complements (as the average transfers in I1C1 are, if anything, not larger than those in I0C1 or in I1C0).

The above results come from non-parametric tests based on average play (averaged over all agents and over all periods), and hence they ignore the dynamics of the interaction – if any. In Table 6.3, we show the results of regression analyses in which we control for reciprocity and learning (*cf.* Gächter *et al.* 2009, Brañas-Garza *et al.* 2011). This table shows the estimation results of System General Method of Moments (SGMM) models for the sample of 118 agents in the four key treatments, over the 10 periods of interactions. We added the estimation results of standard (but naïve) OLS models for comparison.[109] To control for heteroskedasticity, we use robust standard errors (clustered at the individual level) in the estimation of the OLS models, and apply the Windmeijer-corrected cluster-robust errors (idem) in the two-step estimation of the SGMM models (Windmeijer 2005). Following Roodman's (2009) advice on the dangers of instrument proliferation in GMM methods, we resort to using 'collapsed instruments' in the SGMM models that are presented below (*cf.* Beck and Levine 2004, Calderón *et al.* 2002, Carkovic and Levine 2005).[110]

[109] Applying OLS (or GLS) to a model that includes a lagged dependent variable, such as 'Transfer(lagged)' in Table 6.3, can cause 'dynamic panel bias' if the lagged dependent variable is correlated with the fixed effects in the error term (Nickel 1981, Bond 2002, Baum 2006). Since we want to add a lag of the transfer made by the agent to control for reciprocity and learning, we chose to employ system GMM estimators. These estimators are found to exhibit the smallest bias and variance in dynamic panel data models (Arellano and Bond 1991). Note that we follow Arellano and Bover (1995) and Blundell and Bond (1998) in reporting the results of *system* GMM estimators here. The reason is that time invariant regressors, such as the treatment effects we are interested in, would drop out of the estimation equations when using the *difference* GMM method.

[110] Since no lags are actually dropped, collapsing the instruments has the advantage of retaining more information. The method creates one instrument for each endogenous variable (only the lag of the transfer here) and its lag distances, rather than one for each available time period, endogenous variable and its lag distances. For instance, SGMM(All) in Table 6.3 has 'Transfer(lagged)' contributing 9 moment conditions: 8 moments from collapsing the available instruments in periods 3,4, …, 10 and one instrument for the level of 'Transfer(lagged)'. The remaining (exogenous) explanatory variables and the intercept each contribute one moment condition resulting in a total of 14 instruments.

Table 6.3. OLS and SGMM estimation results for agents' transfers.

Dependent variable: Transfer	OLS (1)	OLS (2)	SGMM (All)	SGMM (Studs.)	SGMM (Profs.)
Transfer(lagged)	-	.4546 ***	-.1425	-.0692	-.2553 ***
		(.1175)	(.0934)	(.1410)	(.0935)
Professionals	18.35 ***	10.08 ***	20.34 ***	-	-
	(4.407)	(1.802)	(6.071)		
Treatment I1C0	9.672 *	4.798 **	12.37 **	5.475 *	24.11
	(4.946)	(2.376)	(6.265)	(3.014)	(16.34)
Treatment I1C1	6.828 ***	4.800 ***	9.797 ***	10.04 ***	6.913
	(2.455)	(1.685)	(2.972)	(2.106)	(6.898)
Treatment I0C1	12.81 ***	7.042 ***	16.10 ***	9.554 ***	26.26
	(4.808)	(2.371)	(5.798)	(1.879)	(17.56)
Constant	9.725 ***	5.149 **	9.752 ***	11.11 ***	28.00 ***
	(2.163)	(1.988)	(2.681)	(2.274)	(6.232)
N	1180	1062	1062	675	387
R^2	.1412	.3169	-	-	-
χ^2	-	-	21.49	31.86	14.76
(Collapsed) instruments	-	-	14	13	13
AB-test for AR(1)	-	-	0.000	0.024	0.005
AB-test for AR(2)	-	-	0.161	0.955	0.335
Hansen-J	-	-	0.501	0.579	0.622

Standard deviations are in parentheses. Average transfers in points. *** indicates significance at the 1% level, ** at the 5% level, and * at the 10% level.

The models in Table 6.3 have dummy variables for treatments I1C0, I1C1, and I0C1 and thus show treatment effects compared to the omitted dummy for the benchmark treatment. In the models presented in the first three columns, a dummy for professionals is added to the intercept to account for the higher level of transfers made by this subsample. The last two columns offer separate SGMM estimation results for the subsamples of students and professionals and thus do not include this dummy. At the bottom of the table, we present several results of testing for the validity of the SGMM models. Arellano and Bond (1991) show that an unbiased GMM estimator requires that there is first-order serial correlation, but that there is no second-order serial correlation in the error terms. For all models we find that this holds: the results of the Arellano-Bond tests show that the null hypothesis of no AR(1)-process in the error terms is rejected, and that the null hypothesis of no AR(2)-process in the error terms is not rejected. The Hansen-J-statistic tests the validity of the instruments and we find that we cannot reject the null hypothesis of valid instrumentation in the models presented above. Tests for subsets of instruments (so-called difference-in-Hansen tests) also do not provide enough evidence to reject the null hypothesis of exogeneity of any subset of GMM-instruments or standard IV instruments.

The simple OLS(1) model generally supports the outcomes of the non-parametric tests presented above. Providing the principal with the option to control (I0C1), making full information on the marginal benefits available to the agent (I1C0), or both (I1C1) has a significant positive effect on the transfer made by the agent. Also, professionals transfer significantly more points to the principal. Furthermore, adding control when full information is provided, or providing agents with full information when control is already available, does not cause transfers to increase – the coefficients on I1C0 and I0C1 are respectively about 1 and 1.5 standard deviations larger than the coefficient on I1C1. Since we do not know how feedback is processed or how learning throughout the experiment affects transfers directly (but see Table 6.5), we control for feedback and learning effects in OLS(2) by adding a lagged term for the transfer made by the agent. The idea is that the transfer made in previous periods implicitly captures how an agent responds to feedback on earnings of the principal, his/her own earnings and on the possible threshold set by the principal in a previous period, while learning is implicitly controlled for since a transfer made in one period is now explained by how an agent responded in a previous period. We find that controlling for feedback/learning effects in OLS(2) decreases the size of the coefficients on the treatment and subsample dummy variables, but that the qualitative results remain unchanged.

Let us now discuss the estimation results of the SGMM models to see whether the results of OLS models are biased (see Footnote 109). For SGMM(All) we find that the results for information and control as substitutes are more pronounced than before – the size of the coefficients are similar for treatments I1C0 and I0C1, but much larger than the same coefficients in OLS(2). The results for information and control as substitutes carry over to the subsample of students in SGMM(Studs), albeit that adding control when full information is provided does slightly increase the transfer that is made by the agent. In the last column of Table 6.3, we see that for the professionals the intercept is much larger than for the students. Also the coefficients on treatment I1C0 and I0C1 have much larger positive values here than the coefficients on these treatment effects for the student subsample, but the associated *p*-values are equal to 0.135, and larger – again, probably due to the relative small number of (independent) observations. Finally, we find that a nonzero transfer made in previous periods has a significant negative effect on the transfer in the current period for the subsample of professionals. Although 'Transfer(lagged)' is not significant in the other SGMM models in Table 6.3, the consistent negative sign shows that the positive sign in the OLS models biased the estimation results; the treatment effects estimated in the SGMM models are preferred.

The results of Fehr and List (2004) and Ziegelmeyer *et al.* (2012) suggest that the hidden costs of control are negligible for students, and in particular Fehr and List's results show that professionals are more inclined to respond negatively to control as this can cause (high) intrinsic motivation to transfer point to decrease. Aside from the small sample size of the subsample of professionals, this may be the reason why we do not find significant treatment effects for the control treatments. In Tables 6.4 (and 6.5), we therefore analyze these hidden costs of control in more detail for the two subsamples.

In Table 6.4, we categorize control implemented by the principal in three categories. No control indicates a choice for a threshold of zero, low control is used for a threshold between 1 and 10 points, and a choice for a threshold between 11 and 20 (if available) is dubbed high control. Note that significance levels in this table indicate the results of Mann-Whitney U tests based on the null hypothesis that the transfers are the same for a particular threshold category and its adjacent threshold category. For instance, in the first column of Table 6.4, we can see that opting for high control (third row) rather than low control (second row) significantly increases average transfers from 12.6 to 20.8 (rejection of the null at the 1% level) for students in treatment I1C1.

Let us first look at the top part of Table 6.4 in which the results for the subsample of students are shown. We find that moving from no control to low control, or from low control to high control is always beneficial for students playing the role of the principal. Higher categories of control (almost always) give rise to significantly higher transfers for students in treatments I1C1 and I0C1 (and also significantly higher payoffs for the principal; see the column with "transfer × marginal benefit factor" for I1C1). When students are restricted to choose between low control and no control (in I1C1LT and I0C1LT), we find that low control does not significantly increase transfers, although it is weakly significant in increasing the average payoffs in I1C1LT.[111] Hence, although the results for I1C1LT and I0C1LT hint that there are some hidden costs of control for students, the associated effects are quite small.

Next, let us look at the results for the professional in the bottom part of Table 6.4. Clearly, moving to higher categories of control does not significantly improve average transfers for professionals at conventional confidence levels. Moving from low to high control is also (very) weakly significant in increasing the average payoff in I1C1. It seems that for the professionals there are relatively more individuals who reduce their transfers when faced with higher levels of control. Hence, although we do not have a specific model to explain how individual agents process feedback, these results hint that feedback on specific levels of control has different impacts on the transfer decisions when comparing the two subsamples. In the regressions in Table 6.5, we attempt to clarify the results described above and also take into account the feedback agents receive on the marginal benefits of the provided service.

[111] Note that I0C1 and I0C1LT are omitted in the "Transfer × factor" columns in Table 6.4 as agents in these treatments do not have exact information on the marginal benefit factor when deciding on their transfers – they only know it is equal to either 2 or 4 (each with a 50 percent probability).

Table 6.4. Hidden costs of control for the two subsamples.

Control categories:	Transfer: Treatment I1C1	Transfer: Treatment I0C1	Transfer: Treatment I1C1LT	Transfer: Treatment I0C1LT	Transfer × factor: Treatment I1C1	Transfer × factor: Treatment I1C1LT
STUDENTS						
No threshold	8.60 (4.98)	2.71 (3.50)	9.94 (8.15)	11.9 (17.7)	25.2 (16.6)	26.1 (22.2)
Threshold 1-10	12.6 ++ (3.57)	16.4 ** (12.7)	15.9 ++ (7.96)	14.7 (6.82)	33.3 (7.00)	47.8 * (28.9)
Threshold 11-20	20.8 *** (1.49)	21.6 ** (4.78)	-	-	62.4 *** (6.41)	-
PROFESSIONALS						
No threshold	13.5 (12.2)	25.3 (14.5)	-	-	41.0 (37.2)	-
Threshold 1-10	35.3 + (39.0)	33.2 (37.5)	-	-	117 (165)	-
Threshold 11-20	35.6 (24.3)	45.3 + (40.0)	-	-	123 ++ (103)	-

Standard deviations are in parentheses. Average transfers in points. The Mann-Whitney U tests are based on the null hypothesis that the transfers are the same for a particular threshold category and its adjacent threshold category. *** indicates rejection of the null hypothesis at the 1% level, ** at the 5% level, and * at the 10% level. ++ indicates rejection of the null hypothesis at the 15% level and + at the 20% level.

In the SGMM specifications of Table 6.5, we control for learning and reciprocity by adding variables for period-specific information on the level of control chosen by the principal and the realization of the marginal benefit factor in a particular period. Note that these variables are cross terms: they measure the effect of the level of control, if control is available to the principal, and the effect of the size of the marginal benefits, if full information is given to the agent. These variables thus show control/information effects compared to the omitted benchmark in which control is not available to the principal and information on the size of the marginal benefits is only available to the agent after the transfer decision has been made. Again, we add a dummy for professionals to account for the higher level of transfers made by this subsample in the first two columns, and we offer estimation results for the subsamples of students and professionals in the last two columns

We apply the same estimation technique as before. We use Windmeijer-corrected errors (clustered at the individual level) in the two-step estimation of the SGMM models, and reduce the danger of instrument proliferation by collapsing the instruments used in the SGMM models presented below (see Footnote 110). As compared to the models in Table 6.3, we however replaced the intercept with a dummy for anchoring. The agent's transfer levels in all periods are thus assumed to be anchored to (*i.e.* influenced by) the agent's transfer choice in the first period (*cf.* Kahneman *et al.* 1982, Plott 1996, Corrigan *et al.* 2012). Given that the transfer made in the first period is an independent observation in the statistical sense, we can use it as an instrument for learning: it controls for how individuals change their transfer decision in the following periods relative to how they behaved in the first period.[112] Another difference between the model of Table 6.3 and 6.5 is that we do not add a lag of the transfer, because we assume that the dynamics of the game are now captured by the period-specific variables that account for control/marginal benefit feedback.

[112] The initial value (or anchor) may be suggested by the formulation of the game in the experimental instructions, by the wording and multiple-choice answers of the test questions, or simply the first number that comes to mind. The psychology literature suggests that people do not have immediate valuations but rather construct preferences on the fly when asked questions (see for example Plott 1996). Table 6.5 shows that about 50% of an agent's transfer decision in a particular period is determined by the size of the transfer made in the first period. The results for the subsamples in the last two columns of in Table 6.5 show that anchoring explains less <50% (>50%) of the transfer decision for students (professionals).

Table 6.5. SGMM estimation results for control/information effects.

Dependent variable: Transfer	SGMM (1)	SGMM (2)	SGMM' (Studs.)	SGMM' (Profs.)
Professionals	8.194 ** (3.909)	10.47 *** (2.577)	-	-
Transfer(period1)	.5901 *** (.1345)	.5618 *** (.1063)	.3483 *** (.0969)	.7317 *** (.1173)
Full information × Factor 2	.0533 (3.035)	-1.832 (2.464)	2.572 (1.893)	5.144 (4.854)
Full information × Factor 4	3.480 (3.026)	3.563 (2.463)	6.205 ** (2.820)	9.325 * (4.944)
Control × No threshold	-11.56 (13.28)	-	-	-
Control × Threshold low	2.531 (6.275)	-	-	-
Control × Threshold high	8.223 (6.632)	-	-	-
Threshold [0,20]	-	.7327 *** (.1774)	.8106 *** (.1052)	.6040 * (.3253)
N	1180	1180	750	430
χ^2	129.3	208.6	209.5	71.24
(Collapsed) instruments	33	14	13	13
AB-test for AR(1)	0.000	0.000	0.005	0.001
AB-test for AR(2)	0.018	0.018	0.412	0.027
Hansen-J-test	0.857	0.889	0.778	0.733

Standard deviations are in parentheses. Average transfers in points. *** indicates significance at the 1% level, ** at the 5% level, and * at the 10% level.

The values for the Hansen-J-statistic show that we cannot reject the null hypothesis of valid instrumentation in the models presented above. Difference-in-Hansen tests also do not provide enough evidence to reject the null hypothesis of exogeneity of any subset of GMM-instruments or standard IV instruments. The Arellano-Bond tests reject the null hypothesis of no AR(1)-process in the error terms for all models, as they should, but the null hypothesis of no AR(2)-process in the error terms is also rejected in the models with the subsample of professionals. Hence, only the SGMM'(Studs) specification is consistent here as the tests show that only for this specification the orthogonality conditions of the SGMM estimator are not violated.[113]

[113] Rejecting the hypothesis that there is no second-order autocorrelation in the error terms of the first-difference equation also means rejecting the hypothesis that the error term in the levels equation is not autocorrelated. This indicates that Arellano and Bond (1991)'s orthogonality conditions are not valid – no matter which lags we choose as instruments. Nevertheless, the results of SGMM'(Studs) and SGMM'(Profs) are very similar – the signs and levels of the coefficients and the results of the other tests do not show signs that the application of the SGMM method is invalid here.

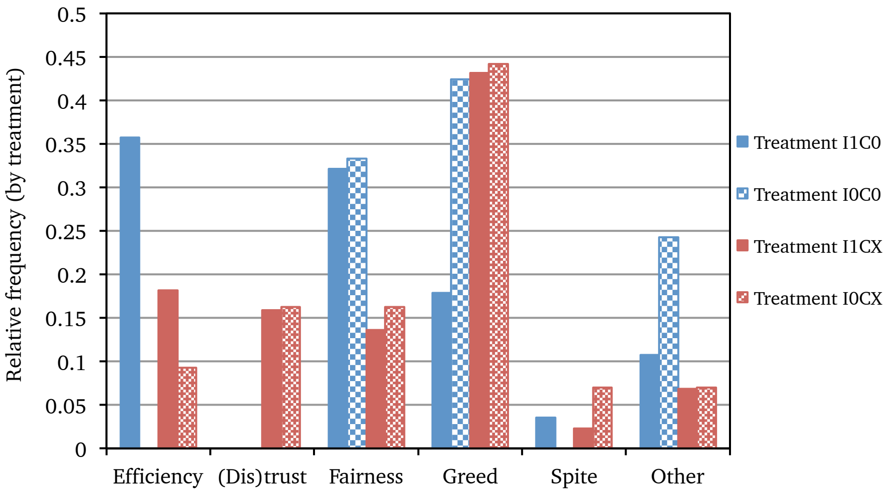
Let us first look at the effect of control. In the SGMM(1) specification, we use dummy variables for the three control categories (no threshold, low threshold, high threshold; see Table 6.4). Since these categories are endogenous variables, they are assigned GMM-type instruments in the estimation procedure. We find that this model does not offer a good fit to the full sample as only the dummy for the higher level of transfers made by the professionals and the anchoring to the transfer choice in period 1 offer significant explanatory power. The dummies for control and marginal benefit feedback are not significant. Note that by using the three control categories, we lose a lot of information on the chosen levels of control. Also, adding these three categories to the estimation equation requires including many instruments to solve for endogeneity. In SGMM(2), we therefore include the level of the threshold set by the principal instead.[114] We find that increasing the level of control is beneficial for the pooled sample of students and professionals. Interestingly enough, when we compare SGMM'(Studs) and SGMM'(Profs), we find that increasing the level of control is more beneficial for the students than for the professionals. On average, for every 1 point increase in the threshold, agents in the student subsample transfer +0,8 points, while agents in the subsample professionals transfer +0,6 points. If we would assume that intrinsic motivation is not present in our sample, the expected coefficient on the level of the threshold would be +1. Hence, the results in Table 6.5 present evidence for hidden costs of control: the coefficient on the level of the threshold is less than 1 in all models. We find that these costs are more outspoken for the professionals, because on average control is less effective for them – professional principals who choose high levels of control are more likely to decrease intrinsically high service levels of agents than students playing in the role of the principal.

Now let us look at the effect of information. In SGMM(2), the coefficients on the full information dummies are not significant. The likely

[114] Introducing the level of the threshold to the estimation equation comes with a caveat: the level of control is zero by definition for the treatments in which the principal does not have the option to control. This means that a choice for zero is now interpreted the same way as not being able to set a threshold. Clearly this is not beneficial for our interpretation of the effect of choosing not to control, but it does offer us much more details on the effect of increasing levels of control on the size of the transfer. Also, it has the benefit of reducing the required number of (collapsed) instruments from 33 in SGMM(1) to 14 in SGMM(2). Adding a dummy for 'Control × No threshold' to SGMM(2) does not change our results, but the coefficient on refraining from control does not turn out to be significant either. This is probably because only few refrained from control in our sample: 1.7% (10%) of the time did students (professionals) choose not to control when the option was available to them.

reason is that the subsamples use the information differently, and the fact that the dummy for full information with a marginal benefit factor of 4 becomes significant when we split the sample is a telltale sign. The coefficients on the full information dummies are much larger for the subsample of professionals. Furthermore, both students and professionals only transfer significantly more points when the marginal benefits are high. We thus see that the response of professionals to information on (high) efficiency is more pronounced than the response of students. In fact, this is also suggested by the questionnaire results.

Figure 6.1. Relative frequency of considerations by treatment.



In Figures 6.1 and 6.2, we offer the results of the on screen questionnaires that were performed immediately after the last period of transfer decisions and the announcements of individual experiment earnings were made. We categorized answers in six categories and compare considerations across treatments; answers in treatment I1C1/I1C1LT and I0C1/I0C1LT are pooled here and depicted as treatment I1CX and I0CX. Note that we differ from Falk and Kosfeld (2006) in three categories: "Efficiency", "Fairness" and "Spite" are added while "Lack of autonomy", "Understanding", and "Neutral" are removed.[115] An answer was categorized

[115] Aside from finding different main categories simply due to some considerations evidently being more outspoken in our sample, these categories can also be explained by our more open

as "Efficiency" if the subject indicated that he/she wanted to maximize the joint income of the agent and the principal. "(Dis)trust" represent answers that mainly contain responses to the threshold set by the principal, *e.g.* feelings of being trusted when low thresholds were set and not being trusted when high thresholds were chosen. "Fairness" depicts subjects who indicated that a fair distribution of points was their main concern, and "Greed" shows the subjects who felt that their main motivation was maximizing their own payoff. "Spite" represents subjects who expressed animosity towards the principal, and subjects that did not have a clear motivation for their decision were added to "Other". Note that frequencies (per treatment and per subsample) add up to one since only one category was chosen for every agent.

Figure 6.1 shows that the most important considerations are efficiency, fairness, and greed. It might seem strange that some subjects in treatment I0CX indicate efficiency as their main consideration because full information was not available here, but these subjects assumed that the principal's choices for certain levels of control were signals for the marginal benefit factor in a particular period. Considerations of (dis)trust clearly cause efficiency to become less important when moving from treatment I1C0 to treatments I1CX and I0CX. Furthermore, when comparing treatment I0C0 to I1C0, providing information to the agent seems to decrease feelings of greed and replace them with a motivation to maximize joined payoffs. Furthermore, providing information on the threshold set by the principal (I1CX and I0CX) causes agents to put less weight on the information on the marginal benefit factor and also clearly decreases considerations of fairness.

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ended question: "What were your considerations to transfer certain amounts of points, possibly in response to a threshold set by the principal?"

Figure 6.2. Relative frequency of considerations by subject pool.

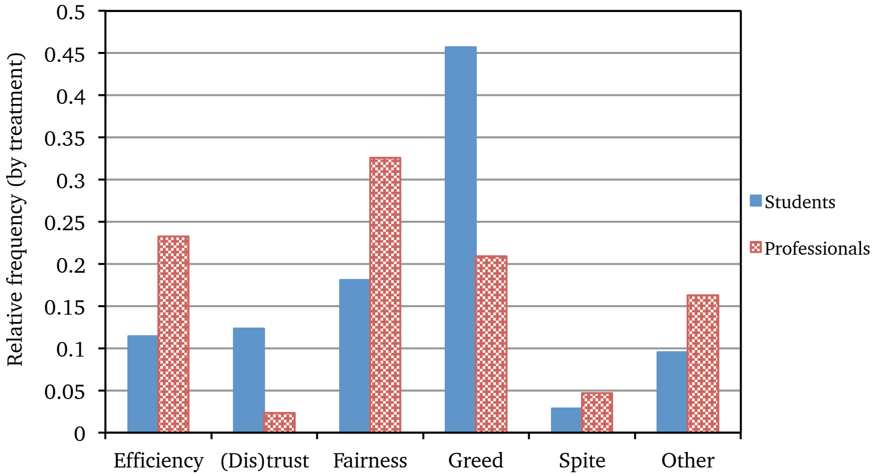

Figure 6.2 supports the results of the regression model in Table 6.5. Professionals were mostly concerned with efficiency and fairness, while students were clearly more motivated to maximize their own payoffs. We were surprised that only a few professionals indicated that (dis)trust was their most important consideration and that efficiency was frequently given as one of the most important drivers for their transfer decision. In fact, nobody indicated that a feeling of being trusted was his or her main consideration. However, given that approximately only 10% of professionals refrained from control and only 7.7% opted for low control in case control was available to the principal, trusting the agent clearly was not considered their go-to choice. Figures of 1.7% for no control and 4.1% for low control for students (in I1C1/I0C1) are no surprise given that students in the role of the agent indicated they were mainly driven by profit maximization, but that most professional still chose high control is paradoxical given the finding of fairness and efficiency to be most important drivers for professionals playing the role of the agent.

6.4 Conclusion

In this chapter, we analyze whether giving agents more or less information on the benefits of an activity – such as actions for climate change adaptation – actually adds to the performance on these actions. In other words, do less strict monitoring arrangements that are based on information, such as participatory monitoring, lead to better implementation and can they be a substitute for control? We find that informed agents have a higher propensity to supply services to the principal. For our student subjects, we find that information is a substitute for control. Extending information on the marginal benefits has a positive effect on the agent's propensity to supply the service the same way setting a minimum performance requirement has. Offering the principal the option to set a minimum performance requirement (*i.e.*, partially control the agent's action) when the agent already receives information (or vice versa) does not further increase agent performance with student subjects. The intrinsic motivation of students to cater for the needs of the principal is low – questionnaire results show that for them greed is the main driver.

Professionals are more inclined to show crowding-out of intrinsic motivation when control is applied. Professional principals who choose high levels of control are thus more likely to decrease intrinsically high service levels of agents than students playing in the role of the principal. When asked for their considerations to exert effort in the role of the agent, professionals are also more likely than students to say that receiving information on the marginal benefits has a positive effect on their propensity to provide the service. For most professionals, such efficiency (but also fairness) considerations are more important than signals of (dis)trust when deciding on the level of service they wish to provide. The results of this chapter can be seen as a stepping stone in the design of arrangements for environmental regulation that are based on providing (more) information to the agent, *e.g.* on the social benefits of private adaptation to climate change.