Clean up your own mess:
An experimental study of moral responsibility and efficiency[a]

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Abstract

Although market-based environmental policy instruments feature prominently in economic theory and are widely employed, they often face public resistance. We argue that such resistance may be driven by moral responsibility, where citizens prefer to tackle the environmental problems that they have caused by themselves, rather than delegating the task to others by means of a market mechanism. Using a laboratory experiment that isolates moral responsibility from alternative explanations, we show that moral responsibility induces participants to take inefficient actions that reduce the earnings of the whole group of participants. We discuss the implications of this finding for the design and implementation of environmental policies.

JEL classification: H23, Q54, Q58

Keywords: Laboratory Experiment, Moral Responsibility, Environmental Policy, Market Mechanism, Climate Change

1. Introduction

There is a long-standing tradition in economics emphasizing the merits of market-based policies, such as pollution taxes and tradable permit schemes, as a means to curb environmental externalities and limit the over-use of exhaustible resources (Baumol and Oates 1988). By providing incentives to reduce pollution or resource use, these policies are generally more efficient in economic terms than traditional command-and-control measures, such as product or performance standards. Hence they offer the possibility of yielding better economic outcomes for everyone, that is, Pareto improvements.

In recent decades, these theoretical insights have made their way into policy making. Tradable quotas are frequently employed to ensure sustainable management of fisheries (Arnason 2012). Pollution taxes, for example on transport fuels, are applied throughout the OECD as well as in numerous developing countries (OECD 2015). To date, 40 countries and over 20 cities, states, and regions have introduced a price on carbon emissions, either in the form of taxes or in the form of tradable permit schemes (World Bank et al. 2016). In addition, voluntary offset mechanisms to compensate for individual emissions have become popular, particular in OECD countries (World Bank et al. 2016).

Nevertheless, market-based environmental policies, such as emissions trading schemes, have repeatedly faced criticism from various sides. In economics, such criticism has pointed out that emissions trading schemes are likely to face real-world constraints (e.g. related to monitoring requirements and the definition of baselines on how emissions would evolve in the absence of the scheme) that may lower their environmental effectiveness (Wara 2007; Schneider and Kollmuss 2015) and economic efficiency (Michaelowa and Jotzo 2005; Krey 2005). More fundamental criticisms have been raised by philosophers, climate scientists, environmental activists, and the Church (see for example Caney 2010 and Page 2011 for discussions of such criticisms). These types of criticisms often rely on a moral critique equating the trading of emission permits with the medieval practice of paying money to be cleared from one's sins, as put succinctly in the Earth Island Journal (Smith 2009):

> *'Congress's new cap-and-trade scam would put the Church's indulgence scheme to shame.'*

In his book 'Storms of my Grandchildren', the prominent climate scientist James Hansen (2009) expresses a similar concern:

> *'A successful new policy cannot include any offsets.[i.e., emissions trading] […] The public must be firm and unwavering in demanding "no offsets", because this sort of monkey business is exactly the type of thing that politicians love and will try to keep. Offsets are like the indulgences that were sold by the church in the Middle Ages'.*

A related argument sees carbon offsets that are used to compensate for greenhouse gas emissions as a way to ease one's conscience without changing behavior. As George Monbiot (2006) writes in "The Guardian":

> '*Our guilty consciences appeased, we continue to fill up our SUVs and fly round the world without the least concern about our impact on the planet … it's like pushing the food around on your plate to create the impression that you have eaten it*'.

The Catholic Church has also taken a critical stance on emissions trading, most notably in Pope Francis's (2015) widely discussed encyclical 'Laudato Si':

> *'The strategy of buying and selling "carbon credits" can lead to a new form of speculation which would not help reduce the emission of polluting gases worldwide. This system seems to provide a quick and easy solution under the guise of a certain commitment to the environment, but in no way does it allow for the radical change which present circumstances require. Rather, it may simply become a ploy which permits maintaining the excessive consumption of some countries and sectors'. (Para. 171)*

These statements capture the two types of criticisms of emissions trading established by Page (2011). First, emissions trading may fail to bring about long-term behavioral change required for successful climate change mitigation and undermine intrinsic incentives for environmentally friendly behavior. Second, it may violate non-consequential objectives of justice and fairness (see also Caney 2010).

Hence, there appears to be a strong presumption that monetarily compensating for an environmental externality is not morally equivalent to changing one's behavior to avoid the externality, even if both courses of action result in identical outcomes. This raises the question

of *why* people object to such compensation-based mechanisms. In this study, we hypothesize that people may have a preference to 'clean up their own mess,' that is, prefer to personally eliminate environmental externalities they are responsible for. For example, they may feel responsible for their or their country's role in climate change, and would prefer to directly fight climate change by reducing their own emissions. As a result, people may consider it immoral to buy their way out of this responsibility, even if doing so would result in a Pareto improvement. In this study we refer to such a motive to adopt behavior that is morally recommended or socially desirable as a 'moral responsibility.'[1] This moral responsibility can be understood as a preference for cleaning up (environmental) externalities that one is responsible for, instead of delegating the task to someone else.

While the aforementioned quotes of environmental activists and the Church are suggestive of moral responsibility, they are not conclusive. For example, they may also reflect a lack of understanding of the benefits of market mechanisms (as argued by Nordhaus 2015), or the view that the assumptions of economic models demonstrating the superiority of trading schemes do not hold. To separate these alternatives from moral responsibility, we use a laboratory experiment. This allows us to eliminate potential confounding factors and directly identify the role of moral responsibility.

In the experiment, we let some of our participants engage in a real-effort task that involves throwing chickpeas into a bowl. Very few chickpeas hit their target, leaving a large number of chickpeas on the floor (an externality), for which these participants are responsible. We then ask the participants to either clean up the chickpeas themselves or delegate the cleaning task to another participant. We construct the experiment such that delegation is Pareto optimal. However, moral responsibility may push participants to clean up themselves, creating a trade-off between efficiency and moral responsibility.

We then isolate moral responsibility from other explanations using a control treatment where a third party is responsible for the chickpeas on the floor. In our main treatment, 60 percent of participants decided to clean up their own chickpeas, even though this was economically inefficient. Importantly, this is significantly higher than the 30 percent of people cleaning up in the control treatment. This implies that participants in the experiment are willing to accept

[1] We use the term in a broad sense that does not distinguish between conventions, social norms, or moral obligations (see Southwood 2011 for a detailed discussion).

real losses in return for behaving in a morally responsible way, even if the result is economically inefficient.

To our knowledge, we are the first to present direct experimental evidence of a revealed preference for personally cleaning up an environmental externality (i.e., the chickpeas) instead of delegating the task to someone else, which we refer to as a moral responsibility. In the experiment, moral responsibility comes with a substantial cost, reducing economic efficiency (i.e., total payments) by approximately 20 percent. These results suggest that market-based policies may be met with opposition when they contradict what is considered morally responsible behavior. Our results therefore shed light on behavioral constraints that may affect the optimal design of environmental policies.

2. Literature Review

Understanding the effects of different policy instruments that can be employed to mitigate externalities has been a central concern in public economics and particularly in environmental economics. One strand of the literature on efficient policy design focuses on the differences between market-based and command-and-control measures (Fischer et al. 2003, Goulder et al. 2016). A second strand of investigation concerns the question of whether to prefer quantity- or price-based mechanisms to regulate pollutants such as carbon emissions (Weitzman 1974, Pizer 2002, Hepburn 2006). Even though there is general agreement in this literature that market-based instruments should be preferred over command-and-control policies, none of these contributions discusses the role of moral responsibility.

Various policy instruments have been studied with the help of laboratory and field experiments (e.g., Ambec et al. 2014). An important example is the literature on market design in emissions trading (see Cason 2010 for a survey) that uses laboratory experiments to study different trading rules, such as discriminatory pricing (Cason 1995). In connection with this, field experiments have been employed to develop and test specific policy instruments for consumers' responses to social information (Allcott and Rogers 2014) and for consumers' biases (Allcott and Taubinsky 2015). In contrast, our experiment does not aim to simulate a specific market environment or test a specific policy. Instead, we use a stylized setup to isolate an aspect that could be crucial for the social acceptance of market-based policies, namely a moral responsibility to clean up an externality one is responsible for.

Minimizing environmental externalities can also be thought of as a special case of a public good or common pool problem. Public good games and common pool problems have been studied extensively in experimental economics (e.g., Isaac and Walker 1988, Andreoni 1988, 1995a, 1995b; for surveys see Ledyard 1995 and Chaudhuri 2011). These experiments demonstrate that collective action problems may be less severe than expected under the assumption of self-interested agents. In particular, the experiments demonstrate that many people are willing to give up some of their own payoffs in order to help another participant, to decrease inequality amongst participants, or to repay earlier actions (e.g., Rabin 1993; Fehr and Schmidt 1999; Bolton and Ockenfels 2000). In addition, this literature points to informal and formal institutions that can increase and sustain cooperation (Ostrom 1990, Fehr and Gaechter 2000). Early applications in environmental economics include Mason and Phillips (1997) and Casari and Plott (2003), who study the role of cooperation and punishment institutions in reducing environmental externalities.

Our experiment differs from public good experiments in that there is no conflict between individual and collective rationality. Instead we focus on a situation where individual and collective rationality are perfectly aligned but where moral responsibility may prevent participants from implementing the payoff maximizing allocation. This is related to Erat and Gneezy (2012) who find that participants are unwilling to increase their own payoff and the payoff of another person if doing so involves telling a lie.

The literature on non-selfish motives predominantly focuses on outcomes rather than the process by which the outcome is obtained. Our experiment also contributes to studies emphasizing that an action is not exclusively judged by a consequentialist logic (Messick 1999). Certain markets, such as prostitution or trade in organs, are often regarded as morally unacceptable, even if the parties involved voluntarily engage in the transactions (Roth 2007). A similar concern over *how* the results are obtained is at the core of the situation studied in this paper.

Our study also relates to the literature investigating how concerns for identity can affect economic outcomes (Akerlof and Kranton 2000). Brekke et al. (2003) and Bénabou and Tirole (2006) extend these insights to study the provision of public good and pro-social behavior; Nyborg et al. (2006) explicitly apply them to green consumption. Eyckmans and Kverndokk (2010) construct a model in which concerns for a 'green' identity can hamper the establishment of a market for tradable emission permits. Moral responsibility may be part of

such a green identity, and may therefore be triggered in settings where the green identity is salient.

Our work is also related to recent literature arguing that markets propagate instrumental motives and thus do not foster intrinsic values and motives (Anderson 1993), or even destroy such values (Sandel 2012). A growing empirical literature has emerged on the question of whether markets erode social responsibility and moral concerns (Falk and Szech 2013; Bartling, Weber and Yao 2015; Pigors and Rockenbach 2016a, 2016b). We take the opposite perspective and ask whether moral concerns can reduce the efficiency of markets.

There is relatively little empirical evidence for the role of moral motives in environmental behavior. In a survey of Polish households, Czajkowski et al. (2014) find that people express a preference for sorting waste themselves, instead of relying on a specialized sorting facility, even though they apparently understand that their choice is economically inefficient. Using representative survey data from Germany and the US, Schwirplies and Ziegler (2015) find that a 'green identity' and social norms are important motivations for pro-environmental behavior.

Perhaps most closely related to our study is an experiment by Braaten et al. (2015). They study emissions trading using a common pool resource, where the benefits of extraction vary across the participants, but extraction always lowers the payoff of the group. To mimic emissions trading, each participant is assigned extraction rights that can be sold to other participants. The authors find that even though the majority of participants oppose emissions trading in the real world, they do not oppose the concept of "trading the right to do something wrong" in the experiment. The authors argue that one possible explanation for this result is that the monetary externalities involved were too abstract to incite moral concerns. This suggests that it may be important to use a task that more closely resembles an environmental externality, and is therefore more likely to raise feelings of moral responsibility. This is what we attempt to do in this study.

3. Experimental Design

The goal of our experiment is to capture the trade-off between efficiency and moral responsibility and to separate moral responsibility from alternative explanations. For this purpose, we use a simple and stylized setup that focuses on the revealed preference to clean up an externality one is responsible for (that is, a moral responsibility), instead of emulating a

particular market mechanism. Using such a stylized experimental setting provides us with clean evidence of a moral responsibility that may parallel the moral opposition to emissions trading observed in the real world.

3.1 Overview of the experiment

The experiment was set up to study a conflict between economic efficiency and moral responsibility. Participants in the experiment were paired and in the first part of the experiment, both participants in each pair engaged in individual real-effort tasks. We sought to induce moral responsibility by asking the first participant (A) to engage in a chickpea-throwing task, while the second participant (B) performed a standard real-effort task with piece-rate incentives (solving addition problems).

For the chickpea task, participant A was given a bowl containing 300 chickpeas and had four minutes to throw them into a deep plate from several feet away (see Figure A1 in the Appendix). We expected most chickpeas to miss the target, resulting in a considerable mess. Indeed, the goal of part one was to induce participant A to feel morally responsible for the chickpeas on the floor (an externality), allowing us to study the effect of moral responsibility on efficiency in part two of the experiment. Hence, the chickpea task in part one is a crucial part of our design.

The goal of part two was to identify the effect of moral responsibility on efficiency. For this purpose, participants worked in pairs with perfectly aligned incentives. Each pair had two tasks: cleaning up the chickpeas, and moving sliders on a computer screen (the slider task). Each pair was paid 20 cents for every slider completed by participant A, and 10 cents for every slider completed by participant B, split evenly across the two participants. Importantly, failing to clean up the chickpeas meant that neither participant earned anything for this part of the experiment. Given these incentives, the payoff-maximizing strategy was for both participants to complete as many sliders as possible, and for participant B to clean up the mess. However, if participant A felt responsible for imposing the externality, it was possible that they would elect to personally clean up the chickpeas, reducing the pair's payoff. Note that when throwing the chickpeas, participant A did not know that either they or participant B would have to pick them up later.[2]

[2] We chose this design to parallel the situation that developed countries have emitted large amounts of greenhouse gases in the past decades without anticipating that this behavior would negatively affect the climate.

3.2 Experimental procedures

Each session consisted of exactly four participants. To make sure that enough people attended, we invited six participants per session, randomly selecting four of them, and dismissing any extra participants after paying them a five euro show-up fee. Upon being selected to participate, participants were randomly assigned to a computer desk, anonymously paired and assigned to one of the two roles. Participants were then given printed instructions that were identical for all participants. They were also informed that the experiment consisted of two parts, and that they would receive instructions for part two after completing part one. After having read the instructions, participants were asked to raise their hand so we could answer any remaining questions.

Part one began by moving the two participants A to separate rooms (one for each participant), located next to the main laboratory. They were given a bowl with 300 chickpeas and had four minutes to throw as many as they could into a deep plate from several feet away, see Figure A1 in the Appendix. The plate was placed on the ground ("A" in Figure A1), and the minimum throwing distance was clearly indicated by a line on the floor ("B" in Figure A1). We placed several tissues in the plate to lower the probability that chickpeas hitting the target would bounce out. During this part of the experiment, participants were not allowed to pick up any chickpeas from the floor, and therefore had only one chance to hit the target with each chickpea. A research assistant, who remained in the background, was present in each separate room to make sure the participants followed the instructions and to keep track of the remaining time.

After four minutes, the research assistant counted the number of chickpeas in the plate, and recorded this number on a sheet of paper. The participants were asked to pay close attention, to ensure that they were correctly counted. Participants A earned 10 cents for each chickpea in the plate at the end of the four-minute period. We then entered their score into the database, and escorted the participants back to the main laboratory.

Meanwhile, participants B remained in the main laboratory and had to add sets of three two-digit numbers for four minutes. This task was computerized using z-Tree (Fischbacher, 2007). Participants received 10 cents for each exercise completed successfully. After all participants A had completed the chickpea task and had returned to the main laboratory, every participant received feedback about their own earnings as well as the earnings of their teammate.

The experiment then moved to its second part, for which participants were given a second set of instructions, and again had to work on a real-effort task. Since we did not want to use the same task as in part one to avoid differential learning effects for the two participants, we used the slider task (Gill and Prowse, 2012). Participants were confronted with 40 sliders presented on a computer screen. The position of each slider was randomized; the goal of the task was to move each slider to the middle (50-50) position. The group received 20 cents (10 cents) for each slider correctly positioned by participant A (B). We made sure that both participants understood this information. Total earnings for part two were divided equally between the two group members.

Importantly, the earnings in part two were only paid out if one of the team members cleaned up the room.[3] Otherwise, both participants earned zero for part two of the experiment. This was emphasized in the instructions, ensuring that all subjects were informed about this. Participants who decided to clean up the room had to do the cleaning first, and could only start the slider task after they had finished the cleaning. This ensured that all cleaning had to be done by one person, i.e., the burden could not be split.

After reading through the instructions, participants had the opportunity to practice the slider task with 10 sliders on their computer. After all participants had completed the practice task and no one had any remaining questions, we moved the participants A back to their respective room from part one, in which the floor was still covered with chickpeas. We directed participants A to a computer terminal. The screen informed them that they had to decide whether to clean up the chickpeas from the floor or start working on the slider task on the computer right away (thus leaving the chickpeas on the floor to participant B). Irrespective of their choice, we asked participants to press a 'start' button on the computer screen when they were ready.

Participants who decided to clean up first were instructed to pick up *all* the chickpeas from the floor and put them into a bowl. The research assistant informed the participant when the cleaning was done, after which the participant was allowed to proceed with the slider task for what remained of the four minutes.

At the end of the four minutes, we moved the participants A back to the main laboratory. When both participants A had returned, we guided their teammates (participants B) to their

[3] In the instructions, we did not use the word 'cleaning'. Instead, we used 'picking up chickpeas' and 'returning the room to its initial state'.

team's respective room. Once again, participants were directed to the terminal and asked to start the four-minute timer before working on the task(s). Naturally, participants could only opt to clean the room if their teammate had not already done so.

When their time was up, we moved participants B back to the main laboratory. All participants were subsequently notified of their earnings in part two and their total earnings. Total earnings per person consisted of a show-up fee (5 euros), the individual earnings from part one and the group earnings from part two divided equally between the two participants. Participants were then asked to answer a small questionnaire with basic demographic questions as well as several questions concerning their attitudes towards emissions trading, the environment, and the market economy (see Appendix). After completing the questionnaire, participants were paid individually and left the laboratory. In total, each session lasted about 35 to 40 minutes.

3.3 Additional treatments

The payoff-maximizing strategy in the experiment is for participant A to work only on the slider task and to let participant B clean up the chickpeas. This is a corner solution, from which deviations may be observed for a number of different reasons. Participants A may, for example, fear that B will refuse or forget to do the cleaning. They may also be afraid that participant B will not have enough time to finish all the cleaning, or they may dislike the slider task enough to be willing to give up money in order to clean instead. Cleaning up can also be rational for participants A who believe that their teammates are at least twice as good at the slider task, which would fully offset the difference in piece rates. In addition, participants A may also clean up by mistake, for example because they misread the instructions.

In order to separate these reasons from moral responsibility, we ran a control treatment (ThirdParty). In this treatment, *both* participants solved addition problems in part one; the chickpeas were thrown on the floor by a third party (a research assistant). As a result, participant A was no longer responsible for the chickpeas on the floor. This treatment therefore eliminated the role of moral responsibility, whilst not affecting the alternative mechanisms mentioned above. As a result, it allows us to distinguish between these alternative mechanisms and the moral responsibility that forms the main focus of the paper.

In the first five sessions of the main treatment, the majority of participants A used up all their chickpeas, hitting approximately 20 on average. In treatment ThirdParty, we therefore asked the research assistants to also use all 300 chickpeas and put approximately 20 chickpeas in the deep plate. This ensured that the number of chickpeas on the floor was similar across the two treatments. Otherwise, all procedures were identical to the main treatment.

Finally, we ran a third treatment (BFirst) where we reversed the order of moves in part two. In this treatment, participants B were therefore the first ones to decide whether to pick up the chickpeas. Note that, as participants A had thrown them, participants B were not responsible for the chickpeas on the floor. This implies that they were unlikely to be influenced by moral responsibility, and could as a result have been more likely to choose the efficient allocation (i.e., clean up themselves). On the other hand, participants B could be unwilling to clean up if they felt that cleaning was A's responsibility. This treatment therefore allows us to see whether any efficiency loss that occurs in the main treatment is mitigated by changing the order of moves. The three treatments are summarized in Table 1.

	Main	**ThirdParty**	**BFirst**
Chickpeas thrown by	Participant A	Research Assistant	Participant A
Decision to clean by	Participant A	Participant A	Participant B

Table 1: Overview of the three treatments.

Participants were recruited using ORSEE (Greiner, 2015). We ran 45 sessions at the TU-WZB lab in Berlin between May and September 2015, with a total of 180 participants (60 in each treatment, 46% women). All participants in a given session were in the same treatment. We first ran five sessions of both the main treatment and treatment BFirst to establish a baseline for the number of chickpeas on the floor. The remaining 35 sessions were randomly assigned to the three treatments. The average age was 25, and 96% of participants were students, with the largest proportions majoring in engineering (22%), science (19%) and double majoring in economics and engineering (19%). The average participant earned approximately €14.40 (including the show-up fee), with a minimum of €5.90 and a maximum of €20.70.[4] As shown in Table A1 in the Appendix, the three treatments are well balanced with respect to all demographic variables in the sense that none of the demographic variables differ significantly across treatments.

[4] We omit the data from one pilot session (four participants) with 200 chickpeas (instead of 300). We also omit the data from a session in which participants were mistakenly given the instructions for the wrong treatment in part two.

4. Results

In this section, we first present the performance of the subjects in the three different tasks (solving additions, throwing chickpeas, and setting sliders). We then focus on the cleaning decisions. These decisions allow us to identify the importance of moral responsibility, and to evaluate the efficiency of the outcome with respect to the earnings of the participants.

	Overall	Main	ThirdParty	BFirst
Addition task score	16.2	16.0	16.2	16.5
	(6.0)	(6.1)	(5.9)	(6.3)
Chickpea task score	21.3	22.4	NA	20.1
	(14.7)	(16.1)	(NA)	(13.2)
Slider task score (with cleaning)	27.7	26.9	29.1	27.0
	(12.1)	(14.5)	(11.8)	(9.6)
Slider task score (no cleaning)	48.2	41.2	54.7	48.4
	(20.9)	(12.5)	(26.4)	(19.3)
Fraction of pairs who cleaned	.97	1	.93	.97
	(.18)	(.0)	(.25)	(.18)
Cleaning time (seconds)	86	84	79	94
	(30)	(30)	(26)	(32)
Observations	180	60	60	60
Pairs	90	30	30	30

Table 2: Descriptive statistics.

Notes: The table displays means; standard deviations are shown in brackets. "Addition task score" is the average number of correct answers in the addition task. "Chickpea task score" is the average number of chickpeas that hit the target in the chickpea task. "Slider task score" is the average number of sliders moved in the slider task, given separately for those who cleaned up the room, "(with cleaning)", and those who did not, "(no cleaning)". "Fraction of pairs who cleaned" is the fraction of pairs for which either participant A or participant B cleaned the room. "Cleaning Time" is the average time used to clean up the room (in seconds), as recorded by the research assistants. This variable was not recorded for the first three sessions, and is not defined for pairs that did no cleaning.

4.1 Aggregate performance

The performance in the tasks we employed was measured by the number of additions solved correctly, the number of chickpeas that hit the target, and the number of sliders positioned correctly. Table 2 presents summary statistics for each treatment separately as well as for all treatments combined. For each of the three tasks, the performance levels are similar across treatments. In addition, the performance in the chickpea task was similar in both rooms (not displayed in the table, p=0.27, t-test). Participants threw 272 out of 300 chickpeas, on average, with most participants (67%) using all 300.[5] Comparing the chickpea and the

[5] Among the participants who used all chickpeas, the performance was (by construction) perfectly negatively correlated with the number of chickpeas on the floor. However, the participants who failed to use up all the chickpeas performed significantly worse in the chickpea task (11.8 versus 25.9 chickpeas; p=.003, t-test). As a

addition task, the *median* performance in the two tasks was nearly identical with 16 and 16.5 respectively (not displayed in the table; p=0.351, Mann-Whitney). However, the *average* performance was better in the chickpea task (average: 21.3) than in the addition task (average: 16.2, p=0.016, t-test). As we will show in the discussion section, this difference is driven by the upper part of the distribution, which is highly skewed for the chickpea task but not for the addition task.

4.2 Cleaning Decisions

Given that participants A and B are randomly assigned to their roles and hence do not systematically differ in their ability in the two tasks, payoff maximization requires that participant A delegates the cleaning task to participant B in the main treatment. However, this is not what we observe. In the main treatment, 60% of participants A decided to pick up the chickpeas. Thus, 60% of participants A in the main treatment were willing to forego part of their earnings in order to do the cleaning themselves. This is consistent with our hypothesis that participants A feel a moral responsibility to clean up, and it seems inconsistent with the idea that participants solely wish to maximize their income.

While our results can be explained by moral responsibility, there are other mechanisms that may also have played a role, such as participant A's fear that participant B may refuse to clean up. Treatment ThirdParty eliminates moral responsibility while retaining the potential for the other reasons that might motivate A to pick up the chickpeas. If moral responsibility is important, we would therefore expect fewer participants A to elect to clean up in treatment ThirdParty. Indeed, we find that only 30% of participants A pick up the chickpeas in this treatment. The difference relative to the main treatment is statistically significant (p=0.019, test of proportions). Taking these results together, the data from treatment ThirdParty suggest that around 30% of participants A elected to clean up for reasons unrelated to moral responsibility. The comparison between the two treatments suggests that a further 30% of participants A picked up the chickpeas out of a moral responsibility.

result, the correlation between performance and the number of chickpeas on the floor over the whole sample is not significant (r=0.12, p=0.36).

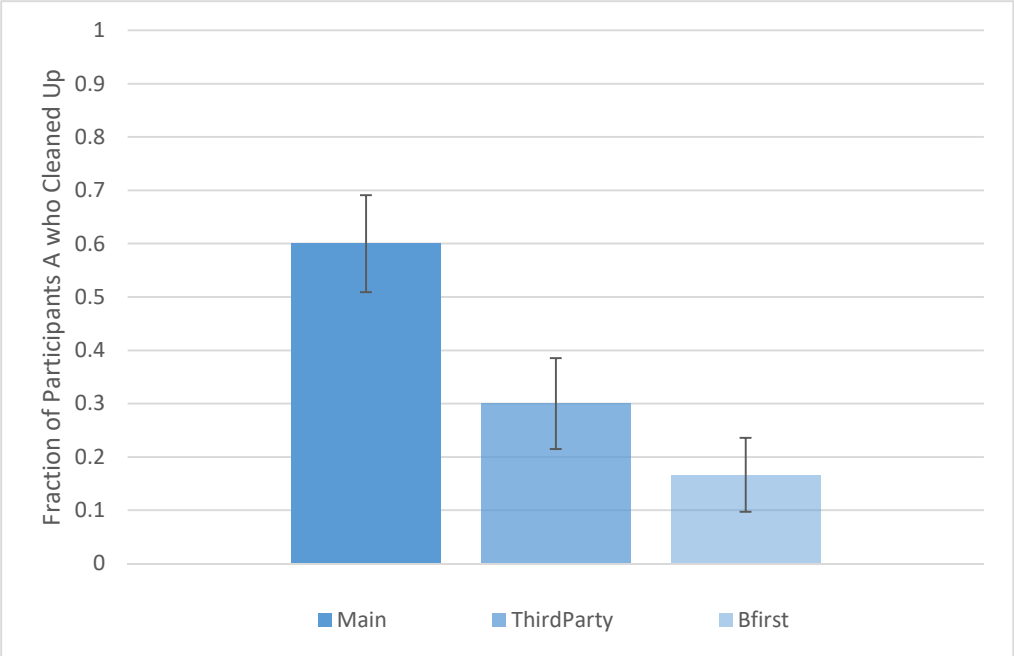

Figure 1: Fraction of participants A who cleaned up in the different treatments.
Note: The whiskers indicate one standard error.

In order to investigate the motives of participants B, we ran our third treatment, BFirst, where participant B was the first to decide whether to pick up the chickpeas. In this treatment, 25 out of 30 participants B (83%) decided to clean up themselves. As a result, participants A cleaned up significantly less often in this treatment, relative to the main treatment ($p<0.001$, test of proportions), but not relative to treatment ThirdParty ($p=0.222$, test of proportions).

Thus, participants B were not averse enough to cleaning up the chickpeas thrown by participant A to accept the loss in payoffs from leaving the chickpeas on the floor. This may in part be explained by the fact that it was the experimenter who instructed participant A to throw chickpeas in the first part of the experiment. Thus, while participant A's behavior is the source of the chickpeas on the floor, it could be argued that their action was not irresponsible, especially since participant A did not know that either they or participant B would have to pick up the chickpeas later. The experimental results imply that in this situation, a more efficient outcome is obtained when participant B, i.e., the person who did not throw the chickpeas, has the ability to decide first.

Note that the difference between the cleaning decisions of participants A in the main treatment and in treatment ThirdParty may result from two related but distinct motives. First, participants A might clean up because of personal (i.e., 'first-order') feelings of moral responsibility. Second, participants A might expect participants B in the main treatment to

feel that participants A are morally responsible for the chickpeas on the floor. If such 'second-order' moral responsibility is strong enough, participants A may clean up themselves out of a desire to not disappoint participants B, or out of an increased fear that participants B might refuse to clean up the mess altogether.

Since treatment ThirdParty eliminates both first- and second-order moral responsibility, it does not allow us to disentangle between them. However, our data do not provide much support for second-order moral responsibility. Specifically, the majority of participants B (83%) chose to clean up in treatment BFirst; in addition all participants B in the main treatment cleaned up if participants A did not.[6] At the same time, we cannot exclude the fact that participants A mistakenly expected participants B not to clean up in the main treatment, which could explain our treatment effect. In the end, our data are therefore consistent with both first-order and second-order moral responsibility.

In Table 3, we analyze our data using a linear probability model.[7] Column (1) of Table 3 compares the proportion of participants A cleaning up the chickpeas across treatments. Relative to the main treatment, participants A in ThirdParty and BFirst are 30 and 40 percentage points less likely to clean up the chickpeas, respectively. Both coefficients are significant, replicating the results of the non-parametric tests. Column (2) shows that we obtain similar treatment effects (28 and 41 percentage points respectively) when controlling for the first mover's performance (the number of exercises or chickpeas) in part one. (Note that the first mover is A in the main treatment and in ThirdParty, while it is B in BFirst.) The performance variable is significant at the 10% level in the ThirdParty treatment, suggesting that each additional exercise solved by participants A decreased their likelihood of cleaning by 1.4 percentage points. One possible reason for this could be that high performers in the additions task better understand the efficiency implications in treatment ThirdParty, where moral responsibility does not play a role.

[6] Answers by participants in the post-experimental questionnaire also hint at 'first-order responsibility' regarding real environmental problems. For example, one participant stated that "[emissions rights] should not be tradable as the environment would still be damaged and companies with a lot of money could buy themselves out of their responsibility" (translated from German by the authors).

[7] We obtain nearly identical results using probit or logit. The tables are reported in Appendix A.

	(1)	(2)	(3)	(4)
	Dependent Variable: first mover assigns cleaning task to A			
Treatment BFirst	-0.400***	-0.414***	-0.435***	-0.404***
	(0.001)	(0.001)	(0.007)	(0.001)
Treatment ThirdParty	-0.300**	-0.284**	-0.520***	-0.301**
	(0.018)	(0.033)	(0.002)	(0.013)
Performance (Part 1) X Main		0.000	-0.001	
		(0.996)	(0.851)	
Performance (Part 1) X BFirst		-0.008	-0.008	
		(0.369)	(0.367)	
Performance (Part 1) X ThirdParty		-0.014*	-0.006	
		(0.095)	(0.473)	
Female X Main			-0.173	
			(0.377)	
Female X BFirst			-0.156	
			(0.261)	
Female X ThirdParty			0.309*	
			(0.076)	
Female				-0.055
				(0.589)
Age				-0.005
				(0.522)
Governments should support equal income/wealth				-0.061
				(0.183)
Governments should support individual liberty				-0.093*
				(0.067)
Governments should protect the environment				0.023
				(0.678)
Taxes for rich people should be low				0.086*
				(0.080)
Individual responsibility important to prevent climate change				0.141***
				(0.001)
Constant	0.600***	0.600***	0.672***	0.474
	(0.000)	(0.000)	(0.000)	(0.177)
Observations	90	90	90	90
R-squared	0.124	0.139	0.187	0.303

Table 3: Linear regression analysis.

*Notes: The table presents the regression coefficients, p-values are shown in brackets. We only use the initial decision maker in each treatment: participant A in the main treatment and ThirdParty, participant B in BFirst. Performance (Part 1) refers to the number of chickpeas that hit the bowl (Main) or exercises solved (BFirst, ThirdParty) in the first part of the experiment. Age is expressed in years. The bottom five variables are 5-point Likert scale variables elicited as part of the questionnaire. P-values are calculated using robust standard errors. *** p<0.01, ** p<0.05, * p<0.1*

Column (3) also controls for the first mover's gender, separately for each treatment. Female participants A are 31 percentage points more likely to clean up in treatment ThirdParty (p=0.076), but there is no significant gender effect in the other two treatments. As a result, the treatment dummy for treatment ThirdParty (which is now estimated using only the data of men) is larger and significant at the 1% level, suggesting that men were 52 percentage points less likely to clean up in the ThirdParty treatment. Since these effects were not part of our original hypotheses, we only mention them in passing.

Finally, column (4) controls linearly for the age and gender of the first movers (A in treatments Main and ThirdParty and B in BFirst) and their responses to several post-experimental questions. Controlling for these variables does not affect the treatment coefficients, which are similar to columns (1) and (2). When we add a dummy variable for each field of study (not reported in the table), the coefficients for the treatment dummies are similarly unaffected.

Moreover, we find an interesting correlation between one of the questionnaire items and the observed choices. Specifically, the regression analysis shows that a one point increase in agreement with a statement that focused on individual responsibility for climate change (a five-point Likert scale) was related to a 14 percentage points increase in the likelihood of participants A to clean up if they were the ones deciding on the cleaning (i.e., in the main treatment and in treatment ThirdParty).[8] Likewise, participants B who agreed with the above statement were more likely to delegate the task to participant A (in the BFirst treatment).[9] While this correlation is intuitive for the treatments in which A has thrown the chickpeas on the floor (Main and BFirst), it also appears in the ThirdParty treatment, in which none of the participants is responsible. One explanation is that participants A in this treatment feel that it is appropriate to clean up the mess by virtue of being the first to decide. Note, however, that this would induce participant A to clean up in both the main treatment and in ThirdParty and can therefore not explain the observed treatment difference.

4.3 Efficiency and Earnings

What is the loss in earnings resulting from participant A collecting the chickpeas from the floor? We calculate the loss in terms of opportunity costs. Cleaning up took around one

[8] Specifically, we asked participants whether or not they agreed with the statement that "The reduction of individual consumption (e.g., eating less meat, less or no air travel) is an important instrument to fight climate change."

[9] If we run this regression separately for each treatment, the coefficients are similar (ranging from 0.10 to 0.14).

minute and 26 seconds on average. As a result, participants who cleaned up solved 27.65 sliders, relative to 48.24 sliders for those who did not clean ($p<0.001$, t-test). Figure 2 shows the cumulative distribution of the number of sliders solved by those who cleaned and those who did not clean.

Given these differences in performance, the pairs in which participant A cleaned up missed out on (48.24 - 27.65) * (€0.2 - €0.1) = €2.06 (€1.03 for each person), or 18% of the average income in part two. Over all treatments, pairs earned more when B did the cleaning ($p=.022$, t-test). This implies that participants in the BFirst and ThirdParty treatments earned significantly more than participants in the main treatment ($p=0.005$ for ThirdParty, $p=0.019$ for BFirst, t-test).

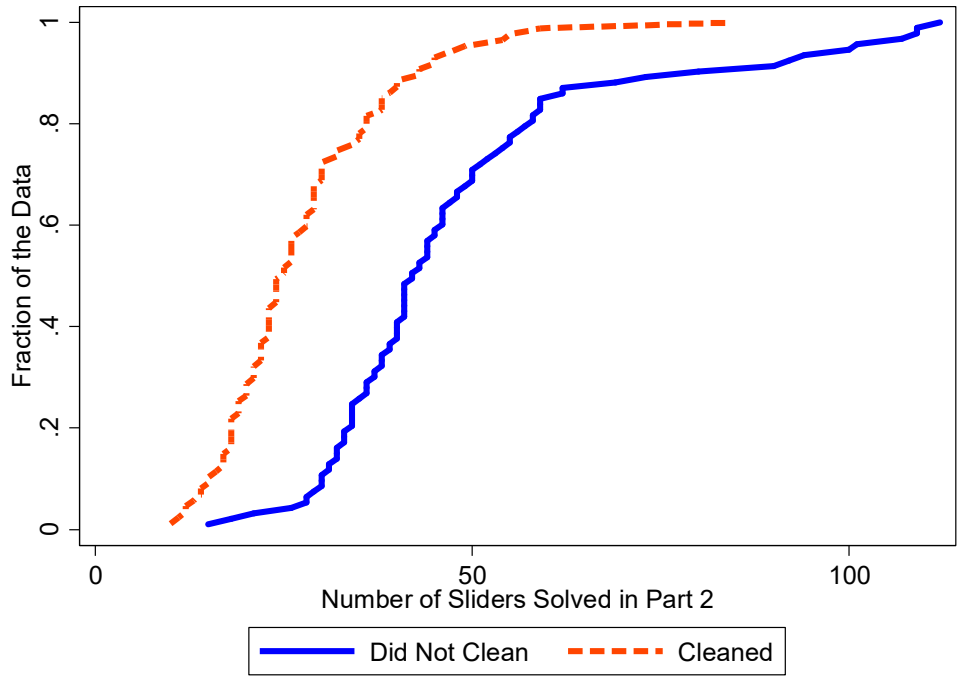

Figure 2: Cumulative distribution function of sliders solved in part two.
Notes: The figure presents the cumulative distribution functions for the number of sliders solved in part two of the experiment, separately for participants who did not clean the room (Did not clean) and those who did (Cleaned). The figure uses data from participants A and participants B across all three treatments.

5. Discussion

The main goal of our design is to create a setting that allows us to cleanly disentangle the effect of moral responsibility from other relevant factors. Using a laboratory experiment allows us to achieve high internal validity due to tight experimental control. However, this also means that our experiment is less well-suited to assess the exact extent to which moral

responsibility will affect behavior within a specific market mechanism, or for a specific environmental externality. For instance, in our experiment, the chickpeas are visible and have a direct effect on the number of sliders that can be placed, while many environmental externalities, such as greenhouse gas emissions, have effects that occur far in the future and are subject to some degree of uncertainty, hence making them 'psychologically distant' (Spence et al. 2012). In addition, participants in our experiment have a rather homogenous educational, social and cultural background, which implies that the results might not be fully representative across social strata or for different countries.

That said, our results are consistent with the wide-spread concern against buying one's way out of moral responsibility reviewed in the introduction. In addition, we find some evidence that the motives we capture in the laboratory may be indicative of behavior outside of the laboratory. In particular, there is a link between the choices in our experiment and participants' attitudes regarding how to fight climate change, as elicited by the post-experimental questionnaire. Nevertheless, extending the study of moral responsibility to specific market institutions both in the lab and in the field could be an important avenue for future research. Studying subject pools from other countries and representative samples of the population could also be a worthwhile endeavor in order to establish the relevance of moral responsibility in other social contexts.

We emphasize that moral responsibility in our experiment is defined rather broadly, and could be an expression of a social norm, a moral obligation, or it could be a mere convention (see also Southwood 2011). A more fine-grained differentiation between these categories might be important for the policy relevance of our results. For instance, behaviors that are connected to people's moral feelings of 'right' and 'wrong' can be expected to be more resistant to change than mere conventions (Steg and Vlek 2009), which are only superficially internalized as an intuitive guide to decision-making (Farrow et al. 2017). Further experiments are needed to shed more light on these possible sources of behavior. Still, the results hint at a moral responsibility that goes beyond a mere convention to clean up, since participants who indicated in the post-experimental questionnaire that individual responsibility is important to fight climate change have a greater tendency to clean up.

For the interpretation of our results it is important that participants had a good understanding of the experimental procedures, specifically that it was clear to participants A that participants B could also clean up, and that letting participants B do the cleaning would likely result in

higher earnings for both group members. For this purpose we provided participants with detailed instructions that included several clarifying examples and explicitly encouraged participants to ask one of the research assistants if anything remained unclear about the instructions (there were four participants and two research assistants in each session). As a result, when we asked all 60 participants A in the main treatment and treatment ThirdParty to indicate why they chose (not) to clean up the chickpeas in the questionnaire, not one participant mentioned that they did so by mistake. Rather, several participants A stated that they were well aware that cleaning would reduce both participants' payoffs, but nevertheless chose to clean up. In addition, even if confusion was an issue, our key identification strategy relies on comparing cleaning choices in the main treatment to treatment ThirdParty. If any confusion about the experimental procedures was similar in both treatments, such confusion would be captured by the 30% of people who chose to clean up in treatment ThirdParty and cannot explain the treatment difference between the main treatment and ThirdParty (which we interpret as the effect of a moral responsibility).

While participants may have been unwilling to delegate the cleaning task out of a moral responsibility, they may also have been embarrassed about the fact that (on average) less than 10% of their chickpeas ended up hitting the target. Since treatment ThirdParty eliminates both embarrassment and moral responsibility, it does not allow us to disentangle between them. However, our design limits the scope for embarrassment to explain behavior, since both participants received feedback about their teammate's performance at the end of part one. Hence, cleaning up in our experiment did not allow participants to hide a poor performance from their teammate. We also find no evidence that participants who might feel most embarrassed (i.e., those with a large number of chickpeas on the floor) were more likely to delegate the cleaning task ($r=-.11$, $p=.573$). Nevertheless, exploring the role of moral responsibility in a design that further reduces the role of embarrassment could be an interesting avenue for future research.

Finally, participants working on the chickpea task were more likely to end up in the top of the earnings distribution in part one. For example, 11 subjects (18%) earned more in the chickpea task than the best performer in the addition task; see Figure A2 in the Appendix. In contrast, participants in the ThirdParty treatment both worked on the addition task in part one, and therefore earned similar amounts. Thus, it could be argued that the income difference between A and B might provide an alternative explanation for the difference between the cleaning choices observed in these treatments. However, since payoffs were split evenly, participants

could not change the income distribution in part two. Hence, inequality aversion defined over the payoffs cannot explain the treatment differences in cleaning choices.

At the same time, it is conceivable that richer participants feel more willing to clean up if they believe that picking up the chickpeas is associated with a lower utility than working on the slider task alone. To check whether this form of (utility-based) inequality aversion can explain our results, we first examine whether participant A or B was the high earner in each pair, and whether this varied across treatments. It turns out that participant A was the high earner 15 times (50%) in the main treatment, 16 times (53%) in BFirst and 17 times (57%) in ThirdParty. Thus, participants A were not more likely to be the high earner in the main treatment (or BFirst) relative to the ThirdParty treatment. Conditional on A being the high earner, however, the income difference was larger in the main treatment (€1.96) than in the ThirdParty treatment (€0.77; p=0.004, t-test). For this to explain our treatment effect, participants A who were high earners would need to have been more likely to clean than low earners. In fact, we find the opposite: participants A who were the high earner in their group cleaned 40% of the time, compared to 80% for low earners.[10] The reason for this could be that high performers in the chickpea task were those who better understood the efficiency implications, or expected to be more productive in the slider task.

6. Conclusions

Ever since Max Weber's (1930) seminal analysis of the role of protestant ethics for capitalism, social scientists have argued that moral convictions can affect economic outcomes. The experiments presented in this paper show that this hypothesis may also have implications for environmental policy design.

Our main result is that the majority of participants clean up when they are responsible for an environmental externality (i.e., chickpeas on the floor), even though delegating the task would have yielded a higher payoff not only for themselves, but also for their teammate. Specifically, they are willing to accept a payoff that is roughly 20% lower than what they could have earned otherwise. Interestingly, participants who were not responsible for generating the externality were more likely to choose the economically efficient solution. Taken together, these findings suggest that participants take on a responsibility to "clean up

[10] A test of proportions yields p=0.03. However, regressing the choice to clean up by participant A in the main treatment on the difference in payoffs with participant B, the regression coefficient is small and not significant (p=0.41). When also including a quadratic term, the results of both coefficients are significant and suggest that higher relative performance makes cleaning less likely though the effect is weaker for very high performers.

their own mess," but experience little or no reluctance to clean up for someone else if this increases their own and the other participant's payoff.

These results are derived from a novel experimental design that allows us to disentangle the effect of moral responsibility from other relevant factors. This design is not meant to precisely capture any specific market institution. Rather, it zooms in on perhaps the most contentious feature of emissions trading, namely the option to use money to compensate for an environmental externality instead of changing one's behavior, which is often regarded as morally irresponsible.

Our findings have a number of potential ramifications for the implementation of environmental policies. They suggest that Pareto-improving policies, such as cap-and-trade systems, might be resisted because of feelings of moral responsibility and related concerns, not simply because of a lack of understanding of their economic rationale. As a consequence, taking into account the observed reluctance to delegate the responsibility for an externality could support the design of environmental policies that face lower political resistance. Such policies would be more costly to implement than those estimated on the basis of first-best economic analyses. Nevertheless, they would likely yield a better outcome than a policy that is resisted due to moral responsibility and related concerns.

This paper also opens up a new perspective on the long-standing debate on 'prices vs. quantities' for the choice of environmental policies. Previous research (summarized in Goulder and Parry 2008) has identified advantages and drawbacks of price-based (e.g., a tax on pollution) and quantity-based (e.g., tradable permit schemes) environmental policies. This research, however, has not taken into account the role of moral responsibility for instrument choice. Since a trading scheme has the flavor of paying one's way out of reducing emissions, a pollution tax may be seen as more acceptable. Hence, further research into whether moral responsibility introduces an asymmetry between price- and quantity-based policy instruments seems warranted.

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