The Impact of Redistribution Mechanisms in the Vote with the Wallet Game: Experimental Results

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Abstract

We use the Vote-with-the-Wallet game (VWG) to model socially or environmentally responsible consumption, an increasingly relevant but still under-researched phenomenon. Based on a theoretical model outlining game equilibria and the parametric interval of the related multiplayer prisoners' dilemma (PD) we evaluate with a controlled lab experiment players' behaviour in the game and test the effects of an ex post redistribution mechanism between defectors and cooperators. Our findings document that the redistribution mechanism interrupts cooperation decay and stabilizes the share of cooperators at a level significantly higher, even though inferior to the Nash equilibrium.

Keywords: vote with the wallet, prisoner's dilemma, lab experiment.

JEL Classification: C72 (Noncooperative games), C73 (Repeated games), C91 (Laboratory, Individual behaviour), M14 (Corporate culture, Social responsibility).

1 Introduction

According to the Boston Consulting Group "Responsible consumption" products accounted for at least 15 percent of all grocery sales – or a \$400bn global market in 2014.[1] This implies that consumers face more and more the choice of purchasing a "responsible product" (or a product which is advertised as such) vis-á-vis a conventional alternative and that the vote with the wallet is a phenomenon of increasing relevance in contemporary markets even though it is far under-researched. In our paper we explain why the vote with the wallet can be modelled as a particular type of prisoner's dilemma in a modified all-or-nothing voluntary contribution mechanism. We then analyse with a randomized experiment the effects of two types of 'soft' (frames) and 'hard' (balanced budget) redistribution policies on the solution of the dilemma.

[1]The Boston Consulting Group (2014), "When Social Responsibility Leads to Growth: An Imperative for Consumer Companies to Go Green". Downloadable at http://us5.campaign-archive2.com/?u=a102a1f840f67f04e25a7fc97&id=b8f2d07db0.

The theoretical and empirical literature presents countless contributions to the understanding of the behavioural logic surrounding the prisoner's dilemma (PD). The evolution of economic life however continuously generates new varieties of the basic situation described by the PD game, which become empirically and politically relevant and worth of specific investigation. In this paper we explore theoretically and experimentally the phenomenon of socially and environmentally responsible consumption and how social and environmental responsibility may affect consumers' and investors' choices. We as well argue that the proper way to describe the phenomenon is using a "hybrid contribution-prisoners' dilemma" (Arce and Sandler, 2005), which we define as *Vote-with-the-Wallet game* (VWG henceforth).

The idea of the game comes from the fact that in the nowadays' complex and globalized economic system, consumers and investors face more and more frequently the alternative between buying a standard product vis-á-vis an equivalent socially or environmentally responsible product. When doing so they may (or may not) intrinsically enjoy the act of buying the responsible product per se if they have other-regarding preferences but they often have to pay an extra cost for it. Moreover, if they "coordinate" with a high number of other consumers in choosing the "responsible" product, the share of those buying responsibly grows and produces a positive externality for everyone in terms of more socially and environmentally responsible corporate conduct. However, and this is the core of the problem, the non-rivalrous, non-excludable (public good) component of this externality makes free riding more convenient, under a wide range of reasonable parametric conditions, and more so if the number of players gets large as is usually the case in consumer markets. As a consequence, exactly as in a typical PD, if everyone follows the dominant strategy of buying the standard product, the positive externality is not produced and each player is worse off with respect to the alternative equilibrium where everyone buys the responsible product.

In this paper we start by describing the VWG, and its potential real-life economic applications, outlining its equilibria and the area of the PD under reasonable parametric conditions. We then discuss some potential solutions to it in terms of an ex post redistributive mechanism, whose impact is tested with a controlled lab experiment. To this purpose we devise an experimental design where participants play both the basic VWG and its modified version incorporating a mechanism that redistributes ex post a share of payoffs from defectors to cooperators. To model such mechanism we focus on a feasible (balanced budget) redistribution where all defectors (participants buying the standard product) pay ex post a lump sum tax, which finances a fund that is in turn equally divided among cooperators (participants buying the responsible product).

A fundamental reference for our paper is Fehr and Gächter (2000)[2] public good game experiment in which participants may costly punish defectors. The dynamic structure of our experimental task – 20 rounds where the same players play two different versions of the same game (in alternating order) – is similar to theirs. However, while their "policing" mechanism (private costly punishment) may be apt to mimic situations in which defectors actually infringe laws (or strong social norms), it is less apt to describe those situations (as ours) where defectors choose between two perfectly legal actions (buying the standard product or, alternatively, buying the responsible product which, we assume, may contribute more to society well-being). For this reason we choose a policy measure that resembles more closely to what actually occurs in our field of interest: an ex post government redistribution which can be applied when players' choices to defect or cooperate are observable. A valuable example of how this occurs in the economic reality are tax deductions for individuals opting for renewable sources of energy (i.e., installing solar panels) which are charged on the bills of those who remain on non-renewable sources. This approach is widely adopted in feed-in schemes that are currently followed in 64 jurisdictions worldwide (Couture and Gagnon, 2010). A similar approach to that studied in our experiment may be adopted in other fields such as green consumption taxes, which differ from standard Pigouvian taxes since they work on the demand and not on the supply side.[3]

The fact that these schemes are explicitly or implicitly implemented in some fields, and the ongoing debate for their extension to new fields, confirm the relevance of our research aimed at verifying how much they can alter the balance between defectors and cooperators in social dilemmas such as the VWG.

From another perspective our experiment contributes to the growing literature of differentiation in social and/or environmental sustainability (see Bagnoli and Watts, 2003; Windsor, 2006, and Lyon and Maxwell, 2002 as a more general reference to these issues). This literature has produced a few valuable experimental studies that simulate the behaviour of demand and supply under price competition in order to test the effect of green certification (Cason and Gangadharan, 2002) or the presence in equilibrium of a price differential between CSR (Corporate Social Responsibility) and non CSR products (Rode et al., 2008, and Vasileiou and Georgantzis, 2015). A common element of these contributions is the in-

[2]Fehr and Gächter (2000) document that their decentralized punishment mechanism has a large impact on cooperation in a public good game. Several contributions extend their work in various directions focusing on nonpecuniary sanctions (Masclet et al., 2003, and Noussair and Tucker, 2005), effectiveness of punishment (Nikiforakis and Normann, 2008) and the price of punishment (Anderson and Putterman, 2006, and Carpenter, 2007).

[3]The case for a EU VAT reform in this direction is strongly supported by Albrecth (2006). Along this line Marconi (2010) shows that in a two-country general equilibrium model with endogenous growth and trade a unilateral green consumption tax changes demand patterns and increases technological progress in direction of pollution abatement in both countries. A more specific application of a green consumption tax is proposed by Säll and Gren (2012) who calculate that the introduction of the latter on meat could decrease emissions of greenhouse gas, nitrogen, phosphorus and ammonia by a significant amount.

terplay of supply and demand while our choice is to isolate demand side dynamics in CSR by taking prices and characteristics of supply as exogenous. In this way we may analyse the strategic complementarity between consumers' choices by controlling for the influence of other concurring factors. Another original element of our contribution with respect to the above mentioned literature is in testing on the consumer side the impact of a very specific (balanced budget) policy where taxes collected from defectors (buyers of the traditional product) subsidise cooperators (buyers of the CSR product). In this respect our subsidy is different from that modelled by Vasileiou and Georgantzis (2015) in their experiment design. A final novelty of the paper is that of testing the role of a CSR frame that does not add anything to the structure of the game. We in fact model the "sustainable" product as producing a positive externality for all participants to the session as in Vasileiou and Georgantzis (2015) without specifying else but, in the treatment which follows, we can test the differential impact of the frame net of the positive externality effect. Our vote with the wallet experiment is as well different from a Voluntary Contribution Public Good game (VCPG) where players can choose the level of donation and closer to what happens in the market where consumers instead of a continuum of choices face a more drastic alternative between a standard and a responsible product. In this framework, we apply the idea of a simple self-financing scheme to our experiment with a dichotomous CSR/non CSR choice, as Falkinger et al. (2000) does to the VCPG with a continuum of choices. In our experiment we test whether such mechanism has the same desirable property of rising the aggregate contribution to the public good without having private information.

Our paper is divided into six sections (including introduction and conclusion). In the second section we provide synthetic evidence of the growing relevance of the "vote-with-the-wallet" phenomenon. In the third section we illustrate the VWG and its equilibria conditional on reasonable parametric assumptions. In the fourth section we describe our experiment design, while in the fifth section we present our empirical findings. The final section concludes.

2 The empirical relevance of the VWG

Specific domains where the vote with the wallet phenomenon is particularly relevant are those of socially responsible (SR) investment funds on the investor side and fair-trade products on the consumer side. According to the Eurosif European SRI Study (2014),[4] SR investment funds using exclusion criteria and therefore voting with their wallet in financial markets accounted for around 41 percent (€6.9 trillion) of European professionally

[4]Eurosif (2014), "European SRI Study 2014". Downloadable at http://www.eurosif.org/our-work/research/sri/european-sri-study-2014/.

managed assets in Europe in 2013, with a growth of around 91 percent between 2011 and 2013. Sustainable, responsible and impact investing has seemingly grown in the United States where the USSIF reports a 76 percent increase from $3.74 trillion at the start of 2012 to $6.57 trillion at the start of 2014 and a market share of around one sixth (Report on US Sustainable, Responsible and Impact Investing Trends 2014).

One of the most well-known and pioneering approaches to responsible consumption in the food and textile industry is Fair-trade. Fair-trade products originate from a product chain with specific socially and environmentally responsible characteristics (pre-financing of primary product producers, price stabilisation, price premium reinvested in local public goods, investment for innovation in productive processes, etc.). What matters here for us is not whether fair-trade achieves what it promises[5] but that its popularity is growing and has created contagion and many imitations in terms of similar alternative product chains advertised as more socially and environmentally responsible to consumers. In times of stagnating consumption such as 2012, fair-trade sales registered a 33 percent yearly growth in Germany, 26 percent in The Netherlands, 28 percent in Sweden, 25 percent in Switzerland and 16 percent in the UK. The fair trade "vote with the wallet" proposal is well known to UK consumers since the 2013-14 Fair-trade Annual Report documents that 31 percent shoppers seeked fair-trade products in 2013, while 77 percent know the fair-trade trademark.[6] The action of fair-trade not-for-profit pioneers triggered imitation of profit maximizing incumbents. Valuable examples are Nestlé,[7] Tesco, Sainsbury, Ben & Jerry (Unilever),[8] Starbucks, Mars,[9] and Ferrero.[10]

3 The Vote-with-the-Wallet Game (VWG)

Following Becchetti and Salustri (2015), in the simplest two-player VWG player's utility conditional to the choice of voting for the responsible product (vR) or voting for the

[5]For the literature on the economic impact of fair-trade see, among others, LeClair (2002), Becchetti et al. (2014), Maseland and de Vaal (2002), Moore (2004), Hayes (2004), and Redfern and Sneker (2002).

[6]Fair-trade Annual Impact Report 2013-14.

[7]Wason, E. (2005, October 7th), "Nestle introduces fairtrade coffee, eco-friendly product goes mainstream", *Mongabay.com*. Downloadable at http://news.mongabay.com/2005/1007-reuters.html.

[8]Gunther, S. (2010, October 25th), "Ben & Jerry announces big move into fair trade", *Mother Nature Network*. Downloadable at http://www.mnn.com/earth-matters/wilderness-resources/blogs/ben-jerry-announces-big-move-into-fair-trade.

[9]Mars (2011, September), "Mars and Fairtrade International announce collaboration". Downloadable at http://www.mars.com/global/press-center/press-list/news-releases.aspx?SiteId=94&Id=3182.

[10]Nieburg, O. (2014, March 20th), "Ferrero makes Fairtrade cocoa commitment after rule change", *Confectionery news*. Downloadable at http://www.confectionerynews.com/Commodities/Ferrero-makes-Fairtrade-cocoa-commitment-after-rule-change.

conventional product (vC) can be written as

$$U^i(S) = \begin{cases} \beta + \alpha - \gamma & \text{if } S = (vR, vR) \\ \frac{1}{2}\beta + \alpha - \gamma & \text{if } S = (vR, vC) \\ \frac{1}{2}\beta & \text{if } S = (vC, vR) \\ 0 & \text{if } S = (vC, vC) \end{cases}$$

where $S := (S^i, S^{-i}) \in \{vC, vR\}^2$ indicates the strategy profile.

The parameter $\beta \in [0, +\infty)$ measures the externality arising from the voting choice that induces corporations to a more social, environmental and fiscally responsible stance, the intensity of the effect depending on the share of players choosing the vR strategy. The parameter $\alpha \in [0, +\infty)$ measures the positive effect generated by strategy vR in case of players' nonzero other-regarding preferences. The parameter $\gamma \in [0, +\infty)$ measures the cost differential between the vR strategy (buying the SR product) and the vC strategy (buying the equivalent non SR product). Players are assumed as being not income constrained in the game.[11]

As shown by Becchetti and Salustri (2015), with $G = (N, (S^i)_{i \in N}, (U^i)_{i \in N}, N = 1, 2$ and $S^i = \{vR, vC\}$, the unique Nash equilibrium of the game in dominant strategies is (vC, vC) if $\frac{1}{2}\beta + \alpha < \gamma$ and (vR, vR) otherwise, and we are in the PD area for intermediate values of γ where $\frac{1}{2}\beta + \alpha < \gamma < \beta + \alpha$. In this parametric interval the unique Nash equilibrium (i.e., (vC, vC)) is Pareto dominated by the strategy pair (vR, vR).

In the multiplayer version of the game we have that $n > 2$, $G_n = (N, (S^i)_{i \in N}, (U^i)_{i \in N})$, $N = \{1, \ldots, n\}$, and $S^i = \{vR, vC\}$ for each $i \in N$.

The payoff function now becomes

$$U^i(S^i, S^{-i}) = \begin{cases} \frac{j+1}{n}\beta + \alpha - \gamma & \text{if } S^i = vR \\ \frac{j}{n}\beta & \text{if } S^i = vC \end{cases}$$

where j measures the number of players choosing the vR strategy in S^{-i}. The multiplayer game has mutual conventional voting (i.e., each players chooses vC) as a unique Nash equilibrium in dominant strategies if $\frac{1}{n}\beta + \alpha < \gamma$ and mutual responsible voting (i.e., each player chooses vR) otherwise. What has to be noted is that a higher number of players clearly makes the PD region larger since the parametric interval of γ in which we are in presence of a PD is $(\frac{1}{n}\beta + \alpha, \alpha + \beta)$. This implies that, in global consumer and investor markets, the PD problem of the VWG is highly relevant.

[11] Said in other terms this implies that only players without income constraints (income at least equal or above the full cost of the responsible product) can participate to the game.

The above described characteristics of the VWG imply that it is close to a Voluntary Contribution Mechanism (VCM) with some original qualifications. The standard VCM has a fixed return for all participants for any euro/token invested by a given player. In the VWG the choice of the conventional product implies a monetary disbursement that does not create any externality for other players. Equivalence with the VCM may be established if considering only the difference in costs between the two choices and calculating the rate of return for each euro 'invested' when choosing the responsible product (5 euro extra expenditure that produces a benefit of 3 euros). Second, in the our VWG players have a dichotomous choice between the responsible or the conventional product and not an endowment from which they can contribute with choices ranging from zero up to the maximum of their total endowment. Moreover, we explicitly avoid that players can use all their endowment as it usually happens in most consumption choices where savings are nonnegative and not all the budget is used. Our VWG is therefore close to a small subset of "all-or-nothing" VCMs (see among others van de Kragt et al., 1983; Rapoport and Eshed-Levy, 1989 and Cadsby and Maines, 1999) that are however regarded in this literature as having weaker external consistence with characteristics of voluntary contributions in the economic reality (Cadsby and Maines, 1999) due to the lack of a free offer. The above mentioned different characteristics between the VWG and the VCM make our experiment more externally consistent with a consumption choice and the VCM to a donation/contribution for a public good.

4 The Experiment

4.1 Design

We experimentally investigate choice behaviour in the VWG both with and without the redistribution mechanism. Our design is based on two finitely repeated versions of the game: in the base version a group of 10 players chooses repeatedly, independently and anonymously between two goods, A and B. In each round each player receives an endowment of 20 tokens and has to buy one of the following two goods: good A, which costs 10 tokens, or good B, which costs 5 tokens. Irrespectively of the good purchased, each player receives a benefit of 3 tokens for each player who buys good A (see Table 1). This characteristic of the game is intended to reproduce the positive externality generated by the purchase of the more expensive (responsible) good B. The above described payoff structure creates a free-riding problem since, for any given share of players choosing good A, buying good B is the dominant strategy (see Table 2). An obvious advantage of the lab experiment setting is that we make players focus only on price and public good features of the problem, while controlling for all other concurring factors (such as quality differentials) which in real life affect consumer choices among different types of products.

Table 1 and 2 about here

We consider also a second version of the VWG in which a redistributive mechanism is introduced. This "redistribution" version differs from the basic version since 2.5 tokens are transferred from players who choose the good B to a common fund at the end of each round . Before the new round begins the fund is equally allocated among all the players who have chosen the good A. The payoffs of this second version of the game are described in Tables 3 and 4. In the game with redistribution buying good A becomes the relevant strategy.

Table 3 and 4 about here

We consider also two additional variants of the base and redistribution games. In these two frames the goods are explicitly described as socially responsible goods. More specifically, they are identified as two "electricity supply contracts" provided by two companies: a socially responsible company, selling good A, and a second unspecified company selling good B. Frame 1 describes social responsibility in terms of the company's commitment to the development of the local economy, while Frame 2 describes it in term of the company's funding social innovative projects on a larger, national scale. The two frames differ, in our intentions, for the different distances that they impose between the indirect potential benefits that the player may get from the company's socially responsible activities.[12]

In each treatment we consider the VWG in its base and redistribution versions. Treatments differ for the order in which the two versions are played (base followed by the redistribution treatment or vice versa) and the frame used (Base without frames, BaseFrame1 and BaseFrame2) (see Table 5).

Each player plays 10 rounds of the basic game and 10 rounds of the redistribution game (in direct or reverse order) and then completes a questionnaire. At the end of each round the number of cooperators is revealed to the group but their identity is kept anonymous.

Table 5 about here

By applying the theoretical framework described in section 3 to the parametric case of our game without redistribution we find that $n = 10, \beta = 30, \gamma = 5$, and $\alpha = 0$ for simplicity

[12]For a specific description of the two frames see Appendix 1.

(where γ is the cost differential between the two strategies). As a consequence the payoff function becomes[13]

$$U^i(S^i, S^{-i}) = \begin{cases} \frac{j+1}{10} \cdot 30 - 5 & \text{if } S^i = vR \\ \frac{j}{10} \cdot 30 & \text{if } S = vC \end{cases}$$

with j being the number of players choosing the vR strategy in S^{-i}. The multiplayer game has mutual conventional voting as a unique (inefficient) Nash equilibrium since $\frac{1}{n}\beta + \alpha < \gamma < \beta + \alpha$ (i.e., $3 < 5 < 30$).

Note that we prefer to set $\alpha = 0$ in our payoff designs in order to make our game more directly comparable with standard contribution PD games (Arce and Sandler, 2005). In this way the existence of other-regarding preferences becomes actually one of the interpretation for a nonzero share of co-operators in the game. By looking at Table 2 it is clear that only with $\alpha > 2$ players can find it optimal to vote with the wallet. As well, in the comparison between the basic and the framed version of the game we implicitly check whether participants' other-regarding preferences stimulated by the frame may modify the average share of contributors.

4.2 Hypotheses

From our design (see Table 5) several empirical static hypotheses can be inferred and tested by confronting subjects' behaviour in the different treatments of the game. Let $C_{i,t\,(G)}$ be the strategy chosen by the i-th player in round t of game G, with $C_{i,t\,(G)} \in \{vR, vC\}$ where vR (voting for the responsible product) is the purchase of good A, while vC (voting for the conventional product) is the purchase of good B, and $G \in \{$*Base, BaseFrame1, BaseFrame2, Redistribution, RedistributionFrame1, RedistributionFrame2*$\}$ indicates the version of the game.

More formally, by conveniently setting the choice $vR = 0$ and the choice $vC = 1$, we can test

Hypothesis 1: (No policy effect)

$$\begin{aligned} H_0 : E[C_{i,t\,(Base)}] &= E[C_{i,t\,(Redistribution)}] \\ H_A : E[C_{i,t\,(Base)}] &\neq E[C_{i,t\,(Redistribution)}] \end{aligned}$$

Under the null of hypothesis 1 the ex post redistribution mechanism does not affect the share of voting choices that are not significantly different in the *Base* and *Redistribution* treatments.

[13] Players' endowment do not make any difference between the two choices and are omitted for simplicity.

The two hypotheses that follow are closely related and test whether the ex post redistribution mechanism significantly affects voting choices when the treatments are framed (that is, when we explicitly describe in experiment instructions the more expensive product A as a socially responsible product).

Hypothesis 2: (No policy effect under Frame 1)

$$\begin{aligned} H_0 : E[C_{i,t\,(BaseFrame1)}] &= E[C_{i,t\,(RedistributionFrame1)}] \\ H_A : E[C_{i,t\,(BaseFrame1)}] &\neq E[C_{i,t\,(RedistributionFrame1)}] \end{aligned}$$

Hypothesis 3: (No policy effect under Frame 2)

$$\begin{aligned} H_0 : E[C_{i,t\,(BaseFrame2)}] &= E[C_{i,t\,(RedistributionFrame2)}] \\ H_A : E[C_{i,t\,(BaseFrame2)}] &\neq E[C_{i,t\,(RedistributionFrame2)}] \end{aligned}$$

We remark here that null hypothesis 1 is actually against what predicted by Nash equilibrium since, given payoffs in redistribution treatments, the dominant strategy for players is to choose the conventional product in the Base treament and the responsible product in the Redistribution treatment (even though the payoff gain in choosing responsible versus conventional in the Redistribution treatment falls as far as the number of cooperative players gets higher). We therefore expect rejection of the null under the assumption of purely self-regarding preferences. Non rejection of the null, or even less than 100 percent choices of the responsible product would imply misperception or, in case of decay of cooperation over time, private costly punishment à la Fehr and Gächter (2000) of non cooperators since non cooperative choices of other players in the same treatment reduce gains of cooperative players.

Our fourth and fifth hypotheses test whether the two frames significantly affect per se voting choices vis-á-vis the *Base* treatment.

Hypothesis 4: (No Frame 1 effect)

$$\begin{aligned} H_0 : E[C_{i,t\,(Base)}] &= E[C_{i,t\,(BaseFrame1)}] \\ H_A : E[C_{i,t\,(Base)}] &\neq E[C_{i,t\,(BaseFrame1)}] \end{aligned}$$

Hypothesis 5: (No Frame 2 effect)

$$\begin{aligned} H_0 : E[C_{i,t\,(Base)}] &= E[C_{i,t\,(BaseFrame2)}] \\ H_A : E[C_{i,t\,(Base)}] &\neq E[C_{i,t\,(BaseFrame2)}] \end{aligned}$$

These two null hypothesis are theoretically funded under the benchmark assumption of purely self-regarding preferences being common knowledge (that is, each player assumes that all the other players are purely self-regarding). In this case the frame does not affect players' choices that are exclusively based on prices. Hence, we expect that the share of cooperators is zero under both the Base than under the BaseFrame1 and the BaseFrame2 treatments.

Rejection of these two null hypotheses could on the contrary be explained, according to the main rationales advanced in the literature, by: i) the influence of frames on player's preferences (with the implication that preferences are frame-dependent) (Iturbe-Ormaetxe et al. 2011); ii) a shift-in-beliefs effect (Dufwenberg et al. 2011; Ellingsen et all, 2012) or iii) the effect of frames on misperceptions. Note however that the first rationale is more likely to matter when the frame induces player to misunderstand the structure of the game (i.e., a public good game when players are asked about 'taking' from the pool instead of 'giving' to the pool) (Fosgaard et al. ,2017). Given the characteristics of our frame, merely a different description of the characteristics of the two products (without changing words in the part of instructions where payoffs related to the choice of the two products are described), the misperception rationale is highly unlikely to apply. As well, in non redistribution framed treatments the shift-in-belief explanation can not apply as well (since purely self regarding individuals still find it optimal to buy the conventional product, even when they believe that some of the other players have frame-related preferences and will change their choice due to the frame). To conclude, rejection of the null would most likely indicate the presence of frame-related preferences.

Last, our sixth and seventh hypotheses test whether the framed/non framed ex post redistribution mechanisms produce different shares of voters.

Hypothesis 6: (No Frame 1 effect under Redistribution)

$$H_0 : E[C_{i,t\,(Redistribution)}] \quad = E[C_{i,t\,(RedistributionFrame1)}]$$
$$H_A : E[C_{i,t\,(Redistribution)}] \quad \neq E[C_{i,t\,(RedistributionFrame1)}]$$

Hypothesis 7: (No Frame 2 effect under Redistribution)

$$H_0 : E[C_{i,t\,(Redistribution)}] \quad = E[C_{i,t\,(RedistributionFrame2)}]$$
$$H_A : E[C_{i,t\,(Redistribution)}] \quad \neq E[C_{i,t\,(RedistributionFrame2)}]$$

Reasoning made on null hypotheses 1 and 2 should apply also here. Findings on hypotheses 6 and 7 dissimilar from those on hypotheses 1 and 2 could then be interpreted in terms of interaction between soft (frame) and hard (redistribution) policies.

Note that, while hypotheses 1-3 test within effects, hypotheses 4-7 test between effects.

4.3 Experimental Procedures

We recruited 180 participants (90 females and 90 males) among the volunteers of the BERG (Behavioural Economics Research Group) of the University of Cagliari (Italy), mainly students, from a wide range of disciplines. The 18 experimental sessions took place in Cagliari in June 2015. Upon arrival in the lab, participants, ten per session, were randomly assigned, to a computer terminal. General instructions were read aloud and subjects were informed that the experiment consisted of two phases, but they received only the specific instructions for phase one. Questions about the structure of the game, the procedures and the payment rules were then answered privately. Participants played the first ten rounds of the game. When everyone had completed phase one, subjects were given phase two instructions, which were read aloud. When the second phase ended all the participants completed a post-experimental questionnaire about their socio-demographic characteristics, general values and their attitude about corporate social and environmental responsibility (see Appendix 2).

One round among the twenty played by each participant was picked at random and paid privately in cash, in addition each participant received 5 tokens as show-up fee (conversion rate 2 tokens = 1 euro). The sessions lasted approximately one hour and earnings averaged about 16 euros. The experiment, other than for the experimental instructions, was computerised using the software z-Tree platform (Fischbacher, 2007).

5 Descriptive findings and results on hypothesis testing

Given the dynamic structure of our multi-player treatments, observations are not independent within treatments and across rounds. The results we present here are therefore purely descriptive when we compare observations that are not independent across groups, while we report tests when independence across groups is met. Testing of the hypotheses described in section 4.2 will be completed in the econometric analysis that follows where the effect of each individual factor (frame, redistribution) can be measured controlling for all concurring effects. In order to analyse in depth and from different perspectives players' behaviour in the VWG we focus on individual round choices jointly considered (Table 1 and Figures 1 and 2 for confidence intervals), first and last round differences only (Table 2), and mean 10-round choices by treatment with matched-pairs Wilcoxon tests (Table

3). Dynamic properties of our findings across rounds will be analysed and commented in section 5.1.

Tables 1–3 and Figures 1 and 2 about here

When using all round information (Table 1) we find that the average share of voting with the wallet decisions is much smaller in the *Base* than in the *Redistribution* treatment (25.6 percent against 59.3 percent, Table 1) against what postulated by hypothesis 1 (no policy effect).

The irrelevance of the ex post redistribution mechanism seems not to stand as well under the two framed designs (hypotheses 2 and 3). Note however that the share of cooperative choices is higher under *BaseFrame1* and *BaseFrame2* treatments than under the *Base* treatment, with around 41 and 46 percent players choosing the responsible product, respectively, and a very narrow difference between the two frames. Hence, the local qualification of social responsibility under the first frame versus the national in the second frame do not seem to affect players' choices. Our empirical findings on the difference between framed/non framed base treatments are consistent with the literature documenting that frames matter. As explained in the section outlining our theoretical hypotheses the most likely explanations for this result is the influence of frames on player's preferences (with the implication that preferences are frame-dependent) (Iturbe-Ormaetxe et al. 2011).

As well, the gain in terms of cooperative choices after the introduction of the redistribution mechanism is relatively lower with respect to what happens under the *Base* treatment (14 and 20 percent respectively in *RedistributionFrame1* and *RedistributionFrame2*). Given what said above is no wonder that hypotheses 4 and 5 are rejected since *BaseFrame1* and *BaseFrame2* treatments produce a significantly higher share of purchases of the responsible product than the Base treatment (χ^2 33.05, p-value 0.000 and χ^2 53.94, p-value 0.000, respectively). Our findings on hypotheses 6 and 7 show that the nulls are in this case not rejected documenting that there is no significant difference in the impact of the frame on the share of responsible players in the treatments with redistribution, that is, when the redistribution treatment varies from the *Redistribution* to the *RedistributionFrame1* and *RedistributionFrame2* treatments.

In order to increase homogeneity of the aggregated round treatments we decompose our previous descriptive findings by separately considering the (framed and not framed) base/redistribution and the (framed and not framed) redistribution/base sequences. The hypothesis of no difference in the share of cooperative choices between the *Base* and the *Redistribution* treatments seems strongly contradicted by our descriptive evidence as well when we look at homogeneous sequences separating cases in which redistribution comes first (after) and base treatment comes after (first). The only exception is under Frame 2 when *Redistribution* comes after. (Table 1, row 8).

Taking into account that observations in our tests above are not independent over time, we focus on differences between the first round and the last round. Table 2 clearly show that last round findings are much sharper than first round findings. This suggests that the dynamics of the game plays an important role for redistribution mechanisms to produce the expected effects. More specifically, the difference between base and redistribution treatments in the first round is strong only when aggregating all treatments (without considering frame/non frame and ex ante/ex post session specific differences) or considering non framed treatments only . On the contrary, the base vs redistribution difference is sharper in last round findings, while the relevance of the policy design in Frame1 and Frame2 treatments is weaker in last round findings. The magnitude of the effect is consistently much stronger in last round (62.2 vs 25 percent share of cooperators in the aggregate base vs redistribution test) than in first round findings (78.3 vs 62.2 percent).

Overall our results imply five general considerations. First, we find a significant share of cooperative choices (in framed/non framed base treatments) even in absence of the redistribution mechanism. This non zero share of cooperative choice is not consistent with the assumption of purely self regarding preferences. Second, the share of cooperative choices is higher in framed versus non framed base treatments. Third, the redistribution treatment adds a significant share of cooperative choices out of total choices even though we are below the Nash equilibrium prediction of 100 percent cooperative choices. Fourth, a significant share of non cooperative choices remains also in the *Redistribution* treatment even when considering last round findings only. Fifth, the share of cooperators is much higher in first than in last round findings, presumably indicating a decay in cooperation which will be investigated further in depth in the dynamic analysis which follows. The first round–last round difference is somewhat averaged in mean by session-treatment findings (Table 3) that provide further evidence against our null hypotheses 1–3 and rejection of hypotheses 4–5 in matched-pairs Wilcoxon tests.

5.1 Dynamic findings

In what discussed above we did not investigate how players' behaviour evolve across rounds. We do it here, firstly, by plotting the share of players choosing the responsible product for each of the 20 rounds in homogeneous sessions.

Figures 3 and 4 about here

We start with the first three sessions where players play the *Base* treatment for the first 10 rounds and the *Redistribution* treatment for the following 10 rounds (Figure 3). Four main results emerge. First, the break in the share of the responsible choices following the introduction of the redistribution mechanism is sharp (from 17.7 in the last (10^{th}) round without redistribution to 77.7 percent in the 11^{th} round when the redistribution mechanism is first applied). Second, the negative slope of the share of responsible players in the 10 rounds of the *Base* treatment documents a decay of cooperative choices showing that players tend to move toward the Nash equilibrium of zero cooperators in absence of the redistribution mechanism. More specifically, we start from a share of 63.3 percent of cooperators in the first round and fall down to 17.7 percent in the 10^{th} round. In absence of further rounds we cannot however say whether the zero cooperators share will be achieved or if a share of unconditional cooperators will persist. Third, the same downward movement after the redistribution break is not observed in the following 10 rounds since the difference between the 11^{th} and the 20^{th} round is smaller and not significant in terms of 95% confidence intervals. Fourth, the share of responsible voters is in general below the 100% share of the new (redistribution design) Nash equilibrium since the latter ranges from a maximum of 76.6 percent to a minimum of 46.6 percent (ending up with a share of 60 percent in the 20^{th} round).

In sessions 4 to 6 we invert the *Base–Redistribution* sequence (the first 10 rounds with redistribution are followed by 10 rounds with no redistribution) and find that the break after the 10^{th} round is less strong than in treatments 1–3, even though still remarkable (the share of cooperators falls from 63.3 to 30 percent from the 10^{th} to the 11^{th} round). The downward slope in the first ten rounds of the base treatment is still relevant (from 30 to 6.6 percent cooperators from the 11^{th} to the 20^{th} round). The share of players buying the responsible product ranges between 46.6 and 80 percent in the first ten redistribution rounds and is in any case below the redistribution design Nash equilibrium.

Results from the other groups of homogeneous designs in which we apply the two frames display similar properties. In the *BaseFrame1–RedistributionFrame1* design the downward slope in the first 10 rounds is steep. We start from 90 percent responsible voters in the first round and end up to 33.3 percent in the 10^{th} round. The break of the 11^{th} round in which we begin to introduce the redistribution mechanism brings back the cooperators' share to 70 percent. The share remains quite stable going to a minimum of 57.46 percent and ends up to 60 percent in the last round, well below the Nash equilibrium in presence of the redistribution mechanism. When we invert the sequence between the *BaseFrame1* and the *RedistributionFrame1* treatment results are less clear-cut. The main difference here is that the break between the 10^{th} and the 11^{th} round is much smaller (from 57.6 to 47.6 percent cooperative choices) and the slope is negative as well in the first 10 periods of the redistribution mechanism (from 83.3 to 56.6 percent cooperative choices).

When looking at the first sequence of the second frame (*Frame 2–RedistributionFrame2*) treatment we find again that the latter has a very strong initial effect also in absence of the redistribution mechanism. In the first round 86.6 percent of players buy the responsible product. However the share of cooperators declines across rounds down to 46.6 percent in the last (10^{th}) round without redistribution mechanism. The 11^{th} round break after the introduction of the redistribution mechanism is remarkable and brings the share of responsible voters up to 73.3 percent. The final share of cooperators in the 20^{th} round is not much lower (63.3 percent) but still below the redistribution design Nash equilibrium.

In the last group of homogeneous sessions (where we switch from the *RedistributionFrame2* to *BaseFrame2* treatment) we find that the share of responsible voters is very high at the beginning (86.6 percent) moving down to 70 percent at period 10 (Figure 4). The break is not remarkable since 56.6 percent of players choose the responsible product at the 11^{th} round when the redistribution mechanism is eliminated and the final share of responsible voters is down to 23.3 percent in the last round.

5.2 Regression findings

Econometric estimates may help us to shed further light on the determinants of our findings. In a perfect information vote-with-the-wallet theoretical model we know that two of the three parameters are fixed and common to each player (the extra cost of voting for the responsible product γ and the public good benefit β). Our experiment faithfully reproduces these two characteristics. However there is a third parameter (the self-regarding preference component), which is unknown and likely to be heterogeneous among players. This parameter accounts for differences in players' choices and in our *Base* treatment it must worth more than 2 tokens to produce a switch from the conventional to the responsible product given the payoff structure shown in Table 2.

By regressing players' choices on a set of socio-demographic variables we may understand which factors affect the above mentioned other-regarding preference parameter.

We start with the following pooled data logit specification

$$\begin{aligned} RespChoice_{i,t,s} = \beta_0 + \beta_1 Redistribution_{t,s} \\ +\beta_2 Frame\ 1_{t,s} + \beta_3 Frame\ 2_{t,s} + \beta_4 Round \\ +\beta_5 Round \times Redistribution + \sum \delta_i SocioDem_i + \varepsilon_{i,t,s} \end{aligned} \quad (1)$$

where $RespChoice_{i,t,s}$ is a dummy taking value 1 if the i-th individual purchases the good A (the buying responsible choice) and 0 otherwise in session s at round t. $Redistribution_{t,s}$ is a (0/1) dummy equal to 1 if the redistribution mechanism is applied in session s at round t, $Frame\ 1_{t,s}$ ($Frame\ 2_{t,s}$) is a (0/1) dummy equal to 1 if *BaseFrame1* (*BaseFrame2*) treatment applies. The variable *Round* measures the round number thereby controlling for the time effect within the treatment. We also control for the interaction using the variable *Round*$\times$*Redistribution* (dummy *Redistribution* times the *Round* variable) to test whether the round effect changes when the redistribution mechanism is applied. $SocioDem_i$ represents the socio-demographic variables added as controls in the estimates[14] (age, sex, mother education, father education, mother professional status, father professional status, and volunteer activities). In order to control for the possible dependency of data coming from the same session the regression is repeated by clustering the standard errors at session level (Table 9, column 2).

Regression findings in Table 9 (column 1) reported in terms of average marginal effects document that the redistribution mechanism produces a positive and significant effect (0.199) on the probability of buying the responsible product vis-á-vis the *Base* treatment that represents the omitted benchmark (thereby rejecting the null of hypothesis 1. Frame 1 and Frame 2 effects are also positive and significant, with the latter (0.213) being stronger in magnitude than the former (0.136). These results confirm the direction of findings of our descriptive analysis and are consistent with what we observed in Figures 3 and 4.[15]

Another relevant result is the negative and significant effect of the *Round* variable (-0.020) confirming that the repeated game tends to bring players toward the non cooperative Nash equilibrium as shown by Figure 3. However our descriptive findings documented as well that this effect occurred mainly in no redistribution rounds, while being almost absent when the redistribution mechanism is at work. This evidence is confirmed and supported by the positive and significant effect (0.016) of the interacted *Round* $\times$ *Redistribution* dummy variable. The combination of the two results documents that the introduction of the redistribution mechanism interrupts the "entropic" effect that brings players away from cooperation. A rationale to explain the decay in cooperation in no redistribution treatment is provided by the theoretical benchmark set forth by Figuières et al. (2013) where agents decide their choice based on a mix of morally ideal contribution and observations of what others do. Weak morality means that they follow only in part the first decision principle. This is consistent with the decline of cooperative choices over time in no redistribution treatments and, as well, it can explain the fact that the share of responsible choices is significantly higher in framed versus non framed non redistribution treatments (where the principle of morally ideal contribution takes a higher weight with respect to the observation

[14]For further details on the socio-demographic variables and their impact see questions 1–11 of the Questionnaire in Appendix 2 and detailed descriptive and econometric findings in Appendix 2.

[15]Similar results can be shown when marginal effects are computed at the means of our regressors and are available upon request.

of what others do).

In order to investigate more in depth the role of time in our repeated experiment we propose the following dynamic logit estimate in which we test for the existence of conditional cooperation and/or conformity.

$$\begin{aligned} ResponsibleChoice_{i,t,s} = {} & \beta_0 + \beta_1 Redistribution_{t,s} + \beta_2 Frame\ 1_{t,s} \\ & + \beta_3 Frame\ 2_{t,s} + \beta_4 Round + \beta_5 Round \times Redistribution \\ & + \beta_6 AvgRespChoice_{i,t-1,s} + DiffFromOthers_{i,t-1,s} \\ & + \sum \delta_i SocioDem_i + \varepsilon_{i,t,s} \end{aligned} \tag{2}$$

More specifically, the introduction of time in our estimate allows us to add the lagged average share of responsible voters, namely $AvgRespChoice_{i,t-1,s}$. We also control for the difference between the choice of player i and the average share of responsible choice of the others players in session s at round $t-1$ ($DiffFromOthers_{i,t-1,s}$).

The policy effect is stable and robust to the introduction of a both a random-effect and a fixed-effect logit model (Table 9, column 3–5). Similar results are also shown for the round effect. To analyse the conformity effect, Table 9 (column 3) shows that *AvgRespChoice* and *DiffFromOthers* are both positive and significant indicating the presence of conditional cooperation and/or conformity (0.111 and 0.169, respectively). When considering the fixed-effect model, we find that the conformity effect is mainly due to the difference between your own choice and the share of others' responsible choice (Table 9, column 4 and 5).

6 Conclusions

More and more, millions of consumers and investors face everyday the alternative between buying/investing in a product, which they consider as more socially and environmentally responsible, and an alternative conventional product. Most of times this choice is accompanied by a trade-off between social responsibility and prices. We reproduce the essential features of the dilemma in a model and test empirically the behaviour of players facing the dilemma in a randomised experiment looking more specifically at the effect of frames and ex post redistribution mechanisms between cooperators and defectors. Our main research question was which policies could bridge the gap between private and social optimum avoiding that players in the VWG choose the conventional product with a "coordination failure" that brings them away from the Pareto superior socially optimal outcome of the game. The interest of the question is particularly relevant in our 10-player setting where the large number of participants widens the parametric interval of the Prisoner's dilemma, thereby making coordination more difficult to achieve. In this respect we look at the specific contribution of two distinct policies: the first (symbolic and costless) is a socially responsible frame, the second (costly for players, but not for the experimenter that chooses a balanced budget mechanism) is a redistribution mechanism where each player choosing the conventional product pays a tax and the final total amount is redistributed among players choosing the responsible product.

In the empirical section of the paper we formulate null hypotheses on players' behavior based on the standard assumption of purely self-regarding preferences being common knowledge. Under this benchmark theoretical predictions are clear-cut. The introduction of the frame in the base (no redistribution) treatment dos not change the Nash Equilibrium were all players choose the conventional product. The redistribution mechanism (in both framed and non framed treatments) is built in such a way that the Nash equilibrium becomes, on the contrary, the set of strategies where all players choose the responsible product.

Our empirical findings show that these theoretical predictions are violated in four ways: i) nonzero (even though decaying over time) shares of responsible product choices under base treatments; ii) significantly higher share of responsible product choices under framed versus non framed base treatments; iii) significantly higher share (non decaying over time) of responsible choices under redistribution versus non redistribution treatments, even though below the predicted 100 percent share; iv) relatively lower growth of responsible choices in the switch between base and redistribution treatments under framed versus non framed treatments. We interpret facts i) and ii) as evidence of deviation from purely self-regarding preferences, fact ii) of frame-related non purely self-regarding preferences, fact iii) of misperception, while fact iv) as showing that the effectiveness of "hard" redistribution policies is relatively lower in presence of frame related non purely self-regarding preferences where "soft" policies are also partially effective.

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A Table and Figures

Table 1: Costs and benefits in the Base Vote-with-the-wallet game (VWG) experiment.

	Payoff	
Endowment	20	20
Your Choice	Product A	Product B
Cost	-10	-5
Benefit (from the choice of the other players)	+3 for each player choosing product A	+3 for each player choosing product A

Table 2: Players' payoff in the Base VWG experiment conditional to other players' choices.

	When you buy good A				When you buy good B			
How many players choose good A	Endowment	Cost	Benefit	**TOTAL**	Endowment	Cost	Benefit	**TOTAL**
			3 X *n* =				3 X *n* =	
10	20	-10	30	**40**	-	-	-	-
9	20	-10	27	**37**	20	-5	27	**42**
8	20	-10	24	**34**	20	-5	24	**39**
7	20	-10	21	**31**	20	-5	21	**36**
6	20	-10	18	**28**	20	-5	18	**33**
5	20	-10	15	**25**	20	-5	15	**30**
4	20	-10	12	**22**	20	-5	12	**27**
3	20	-10	9	**19**	20	-5	9	**24**
2	20	-10	6	**16**	20	-5	6	**21**
1	20	-10	3	**13**	20	-5	3	**18**
0	-	-	-	-	20	-5	0	**15**

Table 3: Costs and benefits in the VWG experiment with Redistribution.

	Payoff	
Endowment	20	20
Your Choice	Product A	Product B
Cost	-10	-5
Benefit (from the choice of the other players)	+3 for each player choosing product A	+3 for each player choosing product A
Redistribution effect	2.5 tokens times the number of players who choses product B, divided by the number of those who choses product A	-2.5

Table 4: Players' payoff in the VWG experiment with Redistribution, conditional to other players' choices.

	When you buy good A					When you buy good B				
How many players choose good A	Endowment	Cost	Benefit	Redistribution	**TOTAL**	Endowment	Cost	Benefit	Redistribution	**TOTAL**
			3 X *n* =					3 X *n* =		
10	20	-10	30	-	**40.0**	-	-	-		-
9	20	-10	27	0.3	**37.3**	20	-5	27	-2.5	**39.5**
8	20	-10	24	0.6	**34.6**	20	-5	24	-2.5	**36.5**
7	20	-10	21	1.1	**32.1**	20	-5	21	-2.5	**33.5**
6	20	-10	18	1.7	**29.7**	20	-5	18	-2.5	**30.5**
5	20	-10	15	2.5	**27.5**	20	-5	15	-2.5	**27.5**
4	20	-10	12	3.8	**25.8**	20	-5	12	-2.5	**24.5**
3	20	-10	9	5.8	**24.8**	20	-5	9	-2.5	**21.5**
2	20	-10	6	10.0	**26.0**	20	-5	6	-2.5	**18.5**
1	20	-10	3	22.5	**35.5**	20	-5	3	-2.5	**15.5**
0	-	-	-	-	-	20	-5	0	-2.5	**12.5**

Table 5: Treatments and sessions.

Treatment	Phase 1 (10 rounds)	Phase 2 (10 rounds)	Phase 3	Subjects
BR	Base	Redistribution	Questionnaire	30
RB	Redistribution	Base	Questionnaire	30
BR1	BaseFrame1	RedistributionFrame1	Questionnaire	30
RB1	RedistributionFrame1	BaseFrame1	Questionnaire	30
BR2	BaseFrame2	RedistributionFrame2	Questionnaire	30
RB2	RedistributionFrame2	BaseFrame2	Questionnaire	30

Table 6: Hypothesis testing, Individual choices.

Treatment (1) vs (2)	**Obs.**	**Share of cooperators (%)** (1) vs (2)
Base vs Redistribution	3,600	37.7 (0.011) vs 63.7 (0.011)
Base vs Redistribution (No Frame)	1,200	25.6 (0.018) vs 59.3 (0.020)
Base vs Redistribution (Frame 1)	1,200	41.3 (0.020) vs 65.6 (0.019)
Base vs Redistribution (Frame 2)	1,200	46.0 (0.019) vs 66.0 (0.019)

Treatment (1) vs (2)	**Obs.**	**Share of cooperators (%)** (1) vs (2)	χ^2	**P-value**
No Frame vs Frame 1 (Base)	1,200	25.6 vs 41.3	33.053	0.000
No Frame vs Frame 1 (Redistribution)	1,200	59.3 vs 65.6	5.134	0.023
No Frame vs Frame 2 (Base)	1,200	25.6 vs 46.0	53.944	0.000
No Frame vs Frame 2 (Redistribution)	1,200	59.3 vs 66.0	5.699	0.017

Row 1. Base vs Redistribution shows the average share of cooperation and the standard errors among the subjects in each of the eighteen Base treatments (six with no frame, six with Frame 1, and six with Frame 2) versus the corresponding eighteen Redistribution sessions (six with no frame, six with Frame 1, six with Frame 2); *Rows 2–4.* Base vs Redistribution (No Frame/Frame 1/Frame 2) show the average share of cooperation among the subjects in each of the six Base sessions (with no frame/with Frame 1/with Frame 2) versus the corresponding six Redistribution sessions (with no frame/with Frame 1/with Frame 2); *Rows 5–6.* No Frame vs Frame 1 (Base/Redistribution) tests the average share of cooperators among the subjects in each of the six Base/Redistribution sessions with no frame versus the corresponding six Base/Redistribution sessions with Frame 1; *Rows 7–8.* No Frame vs Frame 2 (Base/Redistribution) tests the average share of cooperators among the subjects in each of the six Base/Redistribution sessions with no frame versus the corresponding six Base/Redistribution sessions with Frame 2.

Rows 1–4 compare dependent observations as they are choices of the same subjects. *Rows 5–8* compare independent observations as they are choices of the different subjects. We use the chi-squared test.

Table 7: Hypothesis testing, First and Last round (Chi-squared test).

Treatment (1) vs (2)	**Obs.**	**Share of cooperators (%)** (1) vs (2)
First round		
Base vs Redistribution	360	62.2 (0.036) vs 78.3 (0.031)
Base vs Redistribution (No Frame)	120	46.7 (0.065) vs 78.3 (0.054)
Base vs Redistribution (Frame 1)	120	68.3 (0.061) vs 76.7 (0.055)
Base vs Redistribution (Frame 2)	120	71.7 (0.059) vs 80.0 (0.052)
Last round		
Base vs Redistribution	360	25.0 (0.032) vs 62.2 (0.036)
Base vs Redistribution (No Frame)	120	11.7 (0.042) vs 61.7 (0.063)
Base vs Redistribution (Frame 1)	120	28.3 (0.059) vs 58.3 (0.064)
Base vs Redistribution (Frame 2)	120	35.0 (0.062) vs 66.7 (0.061)

Treatment (1) vs (2)	**Obs.**	**Share of cooperators (%)** (1) vs (2)	χ^2	**P-value**

No frame vs Frame 1 (Base)	120	46.7 vs 68.3	5.763	0.016
No frame vs Frame 2 (Base)	120	46.7 vs 71.7	7.761	0.005
No frame vs Frame 1 (Redistribution)	120	78.3 vs 76.7	0.048	0.827
No frame vs Frame 2 (Redistribution)	120	78.3 vs 80.0	0.051	0.822
Last round				
No frame vs Frame 1 (Base)	120	11.7 vs 28.3	5.208	0.022
No frame vs Frame 2 (Base)	120	11.7 vs 35.0	9.130	0.003
No frame vs Frame 1 (Redistribution)	120	61.7 vs 58.3	0.139	0.709
No frame vs Frame 2 (Redistribution)	120	61.7 vs 66.7	0.326	0.568

Row 1. Base vs Redistribution show the average share of cooperation and the standard errors (in parenthesis) among the subjects in the first rounds of each of the eighteen Base treatments (six with no frame, six with Frame 1, and six with Frame 2) versus the corresponding first rounds of eighteen Redistribution sessions (six with no frame, six with Frame 1, six with Frame 2); *Rows 2–4.* Base vs Redistribution (No Frame/Frame 1/Frame 2) show the average share of cooperation and the standard errors (in parenthesis) among the subjects in the first rounds of each of the six Base sessions (with no frame/with Frame 1/with Frame 2) versus the corresponding first rounds of six Redistribution sessions (with no frame/with Frame 1/with Frame 2); *Rows 5–8* replicate Rows 1–4 with respect to the last round observations. *Rows 9–10.* No Frame vs Frame 1 (Base/Redistribution) tests the average share of cooperators among the subjects in the first rounds of each of the six Base/Redistribution sessions with no frame versus the corresponding six Base/Redistribution sessions with Frame 1; *Rows 11–12.* No Frame vs Frame 2 (Base/Redistribution) tests the average share of cooperators among the subjects in the first rounds of each of the six Base/Redistribution sessions with no frame versus the corresponding six Base/Redistribution sessions with Frame 2. *Rows 13–16* replicate Rows 9–12 with respect to the last round observations.

Table 8: Hypothesis testing (Matched-Pairs Wilcoxon test).

Treatment (mean by session-treatment)	**Obs.**	**Share of cooperators**	z	**P-value**
Base vs Redistribution	36	37.7 vs 63.7	−5.124	0.000
Base vs Redistribution (No Frame)	12	25.7 vs 59.3	−3.066	0.002
Base vs Redistribution (Frame 1)	12	41.3 vs 65.7	−3.076	0.002
Base vs Redistribution (Frame 2)	12	46.0 vs 66.0	−2.516	0.012
No frame vs Frame 1 (Base)	12	25.7 vs 41.3	−2.831	0.005
No frame vs Frame 2 (Base)	12	25.7 vs 46.0	−2.831	0.005
No frame vs Frame 1 (Redistribution)	12	59.3 vs 65.7	−2.280	0.023
No frame vs Frame 2 (Redistribution)	12	59.3 vs 66.0	−2.366	0.018

Row 1. Base vs Redistribution tests the average share of cooperation among the subjects in each of the eighteen Base treatments (six with no frame, six with Frame 1, and six with Frame 2) versus the corresponding eighteen Redistribution sessions (six with no frame, six with Frame 1, six with Frame 2); *Rows 2–4.* Base vs Redistribution (No Frame/Frame 1/Frame 2) tests the average share of cooperation among the subjects in each of the six Base sessions (with no frame/with Frame 1/with Frame 2) versus the corresponding six Redistribution sessions (with no frame/with Frame 1/with Frame 2); *Rows 5–6.* No Frame vs Frame 1 (Base/Redistribution) tests the average share of cooperators among the subjects in each of the six Base/Redistribution sessions with no frame versus the corresponding six Base/Redistribution sessions with Frame 1; *Rows 7–8.* No Frame vs Frame 2 (Base/Redistribution) tests the average share of cooperators among the subjects in each of the six Base/Redistribution sessions with no frame versus the corresponding six Base/Redistribution sessions with Frame 2.

Table 9: Econometric findings (average marginal effects).

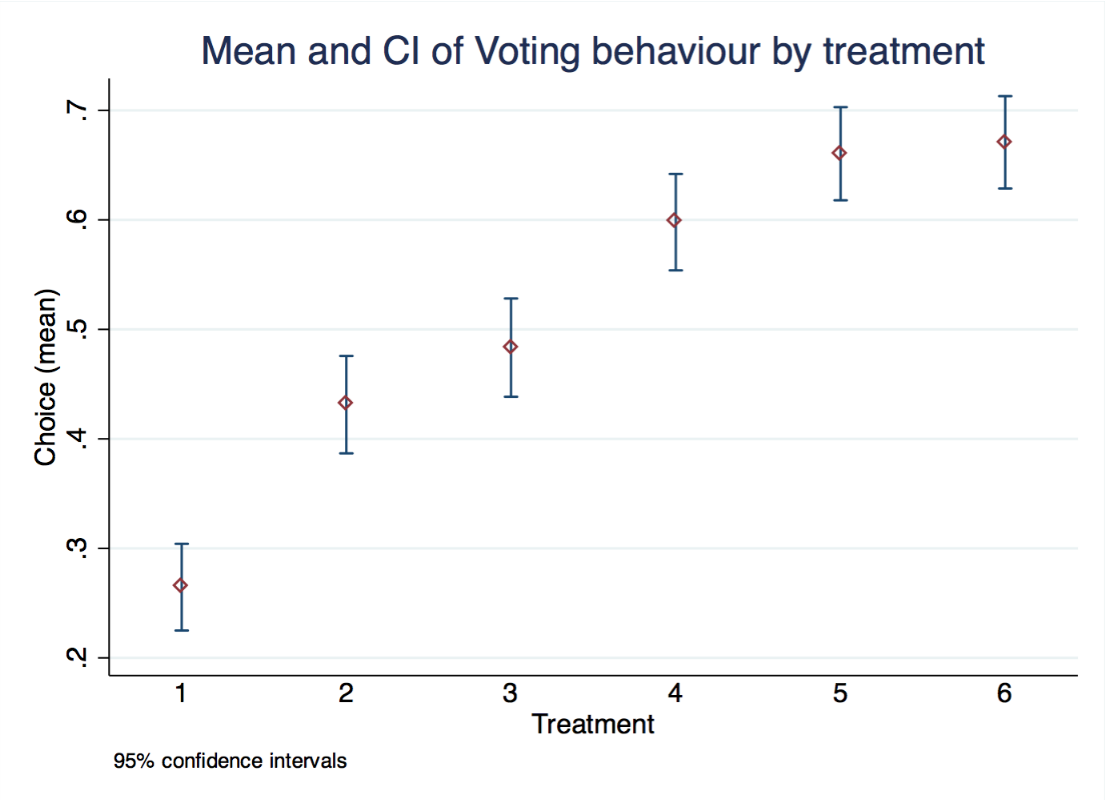
VARIABLES	(1) Pooled Resp Choice	(2) Pooled Cluster SE Resp Choice	(3) RE Resp Choice	(4) FE Resp Choice	(5) FE Cluster SE Resp Choice
Redistribution	0.199***	0.199***	0.220***	0.226***	0.226***
	(0.038)	(0.065)	(0.057)	(0.067)	(0.066)
Frame 1	0.136***	0.136**	0.114***	0.103**	0.103
	(0.029)	(0.064)	(0.037)	(0.041)	(0.065)
Frame 2	0.213***	0.213***	0.162***	0.133***	0.133*
	(0.028)	(0.056)	(0.038)	(0.043)	(0.067)
Round	-0.0201***	-0.0201***	-0.0144***	-0.0156***	-0.0156***
	(0.00194)	(0.00374)	(0.00301)	(0.00429)	(0.00382)
Round×Redistribution	0.0157***	0.0157***	0.0122**	0.0126*	0.0126***
	(0.00286)	(0.00486)	(0.00473)	(0.0067)	(0.0049)
Avg Resp Choice$_{t-1}$			0.111**	0.073	0.073*
			(0.050)	(0.052)	(0.043)
Diff from Others$_{t-1}$			0.169***	0.101***	0.101***
			(0.020)	(0.021)	(0.036)
Socio Dem	Yes	Yes	Yes	Yes	Yes
Age dummies	Yes	Yes	Yes	Yes	Yes
Observations	3,600	3,600	3,420	3,116	3,116
Number of id			180	164	

Legend: Pooled: pooled estimates of specification (1); RE: random effect estimate of the specification in (2) augmented with dynamic variable; FE: fixed effect estimate of the specification in (2) augmented with dynamic variable. Cluster SE = Standard errors adjusted for 18 clusters in session.

Standard errors in parentheses.

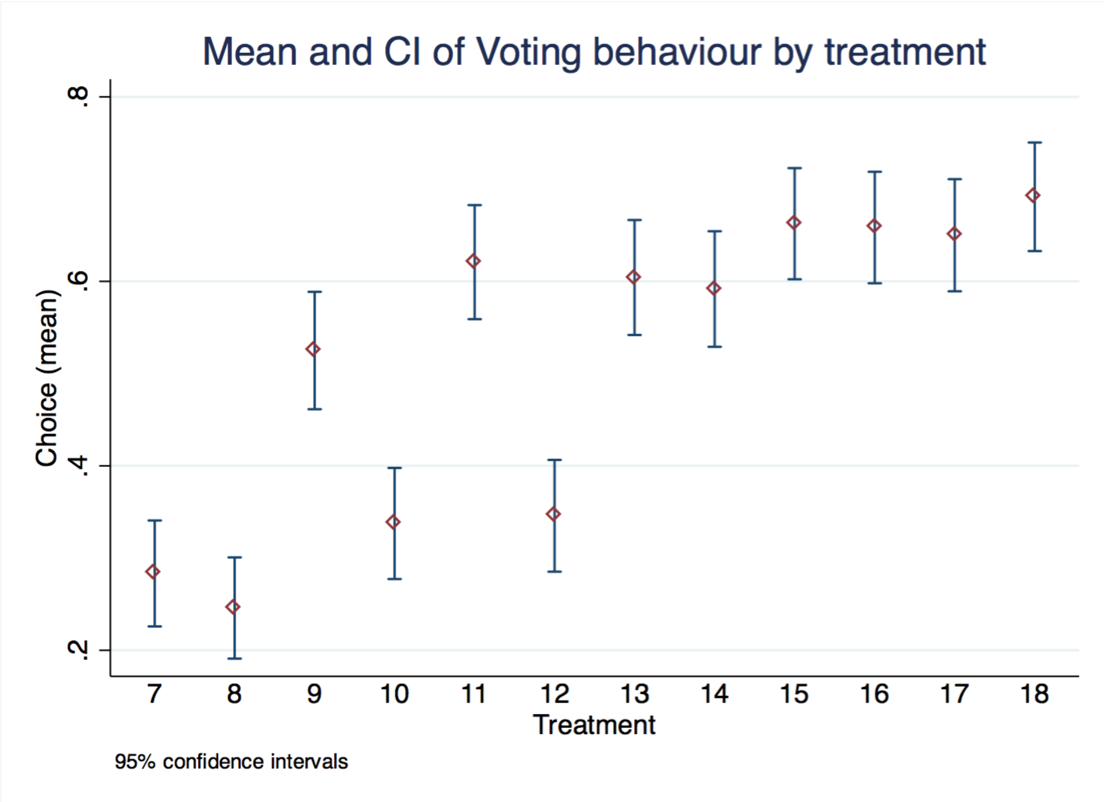
*** p<0.01, ** p<0.05, * p<0.1

Figure 1: Voting Behaviour by Treatment.



Legend (vertical axis: share of players buying the responsible product, horizontal axis numbers and corresponding treatments): (1) *Base*, (2) *BaseFrame1*, (3) *BaseFrame2*, (4) *Redistribution*, (5) *RedistributionFrame1*, (6) *RedistributionFrame2*.

Figure 2: Voting Behaviour by Treatment (disaggregated by order).



Legend (vertical axis: share of players buying the responsible product, horizontal axis numbers and corresponding treatments): (7) Base (in Base-Redistribution sessions), (8) *Base* (in *Redistribution – Base* sessions), (9) *Frame 1* (in *BaseFrame1 – RedistributionFrame1* sessions), (10) *BaseFrame1* (in *RedistributionFrame1 – BaseFrame1* sessions), (11) *Frame 2* (in *BaseFrame2 – RedistributionFrame2* sessions), (12) *BaseFrame2* (in *RedistributionFrame2 – BaseFrame2* sessions), (13) *Redistribution* (in *Base – Redistribution* sessions), (14) *Redistribution* (in *Redistribution-Base* sessions), (15) *RedistributionFrame1* (in *BaseFrame1 – RedistributionFrame1* sessions), (16) *RedistributionFrame1* (in *RedistributionFrame1 - BaseFrame1* sessions), (17) *RedistributionFrame2* (in *BaseFrame2 – RedistributionFrame2* sessions), (18) *RedistributionFrame2* (in *RedistributionFrame2 – BaseFrame2* sessions).

Figure 3: Share of players buying the responsible product (by treatment).

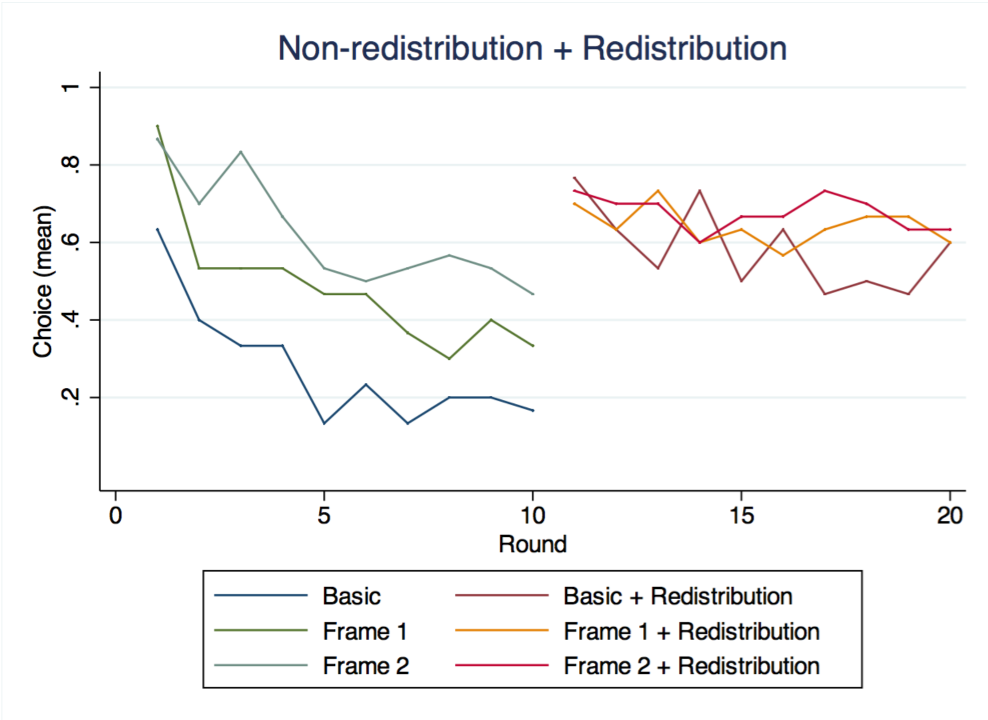
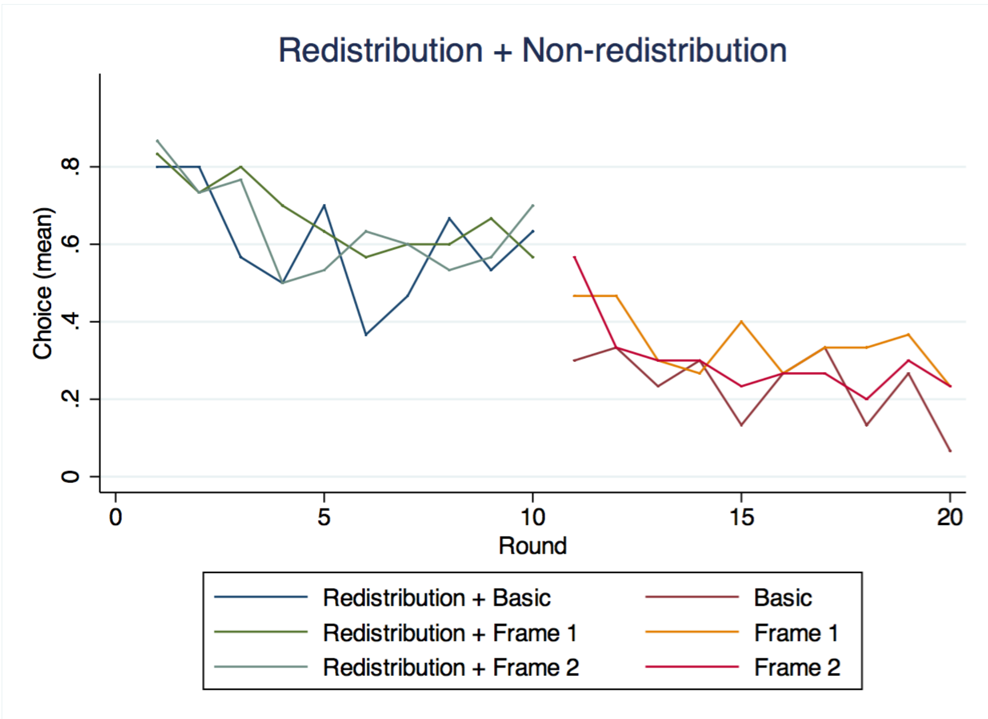


Figure 4: Share of players buying the responsible product (by treatment).



B Online Appendix

B 1: Hypothesis testing, Individual choices.

Treatment **(1) vs (2)**	**Obs.**	**Share of cooperators (%) (1) vs (2)**		
Base vs Redistribution (Redistribution after)	600	27.6 (0.026) vs 58.3 (0.029)		
Base vs Redistribution (Redistribution after)	600	23.6 (0.025) vs 60.3 (0.028)		
Base vs Redistribution (Frame 1, Redistribution after)	600	48.3 (0.029) vs 64.3 (0.028)		
Base vs Redistribution (Frame 1, Redistribution before)	600	34.3 (0.027) vs 67.0 (0.027)		
Base vs Redistribution (Frame 2, Redistribution after)	600	62.0 (0.028) vs 67.6 (0.027)		
Base vs Redistribution (Frame 2, Redistribution before)	600	30.0 (0.027) vs 64.3 (0.027)		
Treatment **(1) vs (2)**	**Obs.**	**Share of cooperators (%) (1) vs (2)**	χ^2	**P-value**
No Frame vs Frame 1 (Base before)	600	27.7 vs 48.3	27.193	0.000
No Frame vs Frame 1 (Base after)	600	23.7 vs 34.3	8.289	0.004
No Frame vs Frame 2 (Base before)	600	27.7 vs 62.0	71.490	0.000
No Frame vs Frame 2 (Base after)	600	23.7 vs 30.0	3.065	0.080
No Frame vs Frame 1 (Redistribution after)	600	58.3 vs 64.3	2.277	0.131
No Frame vs Frame 1 (Redistribution before)	600	60.3 vs 67.0	2.882	0.090
No Frame vs Frame 2 (Redistribution after)	600	58.3 vs 67.7	5.606	0.018
No Frame vs Frame 2 (Redistribution before)	600	60.3 vs 66.0	1.022	0.312

Rows 1–6 compare dependent observations as they are choices of the same subjects. *Rows 7–14* test independent observations as they are choices of the different subjects. We use the chi-squared test.

Standard errors in parenthesis.

B 2: Hypothesis testing, First and Last round (Chi-squared test).

Treatment **(1) vs (2)**	**Obs.**	**Share of cooperators (%) (1) vs (2)**
First round		
Base vs Redistribution (Redistribution after)	180	80.0 (0.042) vs 73.3 (0.047)
Base vs Redistribution (Redistribution before)	180	44.4 (0.053) vs 83.3 (0.040)
Base vs Redistribution (No Frame, Redistribution after)	60	63.3 (0.089) vs 76.7 (0.079)
Base vs Redistribution (No Frame, Redistribution before)	60	30.0 (0.085) vs 80.0 (0.074)
Base vs Redistribution (Frame 1, Redistribution after)	60	90.0 (0.056) vs 70.0 (0.085)
Base vs Redistribution (Frame 1, Redistribution before)	60	46.7 (0.093) vs 83.3 (0.069)
Base vs Redistribution (Frame 2, Redistribution after)	60	86.7 (0.063) vs 73.3 (0.082)
Base vs Redistribution (Frame 2, Redistribution before)	60	56.7 (0.092) vs 86.7 (0.063)
Last round		
Base vs Redistribution (Redistribution after)	180	32.2 (0.050) vs 61.1 (0.052)
Base vs Redistribution (Redistribution before)	180	17.8 (0.041) vs 63.3 (0.051)
Base vs Redistribution (No Frame, Redistribution after)	60	16.7 (0.069) vs 60.0 (0.091)
Base vs Redistribution (No Frame, Redistribution before)	60	6.7 (0.046) vs 63.3 (0.089))
Base vs Redistribution (Frame 1, Redistribution after)	60	33.3 (0.088) vs 60.0 (0.091)
Base vs Redistribution (Frame 1, Redistribution before)	60	23.3 (0.079) vs 56.7 (0.092)
Base vs Redistribution (Frame 2, Redistribution after)	60	46.7 (0.093) vs 63.3 (0.089)
Base vs Redistribution (Frame 2, Redistribution before)	60	23.3 (0.079) vs 70.0 (0.085)

Treatment **(1) vs (2)**	**Obs.**	**Share of cooperators (%) (1) vs (2)**

No frame vs Frame 1 (Base before)	60	63.3 vs 90.0	5.963	0.015
No frame vs Frame 1 (Base after)	60	30.0 vs 46.7	1.7626	0.184
No frame vs Frame 2 (Base before)	60	63.3 vs 86.7	4.356	0.037
No frame vs Frame 2 (Base after)	60	30.0 vs 56.7	4.344	0.037
No frame vs Frame 1 (Redistribution after)	60	76.7 vs 70.0	0.341	0.559
No frame vs Frame 1 (Redistribution before)	60	80.0 vs 83.3	0.111	0.739
No frame vs Frame 2 (Redistribution after)	60	76.7 vs 73.3	0.089	0.766
No frame vs Frame 2 (Redistribution before)	60	80.0 vs 86.7	0.480	0.488
Last round				
No frame vs Frame 1 (Base before)	60	16.7 vs 33.3	2.222	0.136
No frame vs Frame 1 (Base after)	60	6.7 vs 23.3	3.268	0.071
No frame vs Frame 2 (Base before)	60	16.7 vs 46.7	6.239	0.012
No frame vs Frame 2 (Base after)	60	6.7 vs 23.3	3.268	0.071
No frame vs Frame 1 (Redistribution after)	60	60.0 vs 60.0	0.000	1.000
No frame vs Frame 1 (Redistribution before)	60	63.3 vs 56.7	0.278	0.598
No frame vs Frame 2 (Redistribution after)	60	60.0 vs 63.3	0.071	0.791
No frame vs Frame 2 (Redistribution before)	60	63.3 vs 70.0	0.300	0.584

B 3: Hypothesis testing (Matched-Pairs Wilcoxon test).

Treatment (mean by session-treatment)	**Obs.**	**Share of cooperators**	z	**P-value**
Base vs Redistribution (Redistribution after)	18	46.0 vs 63.4	−3.426	0.001
Base vs Redistribution (Redistribution before)	18	29.3 vs 63.8	−3.728	0.000
Base vs Redistribution (No Frame, Redistribution after)	6	27.7 vs 58.3	−2.220	0.026
Base vs Redistribution (No Frame, Redistribution before)	6	23.7 vs 60.3	−2.220	0.026
Base vs Redistribution (Frame 1, Redistribution after)	6	48.3 vs 64.3	−2.271	0.023
Base vs Redistribution (Frame 1, Redistribution before)	6	34.3 vs 67.0	−2.220	0.026
Base vs Redistribution (Frame 2, Redistribution after)	6	62.0 vs 67.7	−0.740	0.459
Base vs Redistribution (Frame 2, Redistribution before)	6	30.0 vs 64.3	−2.220	0.026
Base vs Redistribution (Redistribution after)	18	46.0 vs 63.4	−3.426	0.001
Base vs Redistribution (Redistribution before)	18	29.3 vs 63.9	−3.728	0.000
No frame vs Frame 1 (Base before)	6	27.7 vs 48.3	−2.220	0.026
No frame vs Frame 1 (Base after)	6	23.7 vs 34.3	−1.586	0.113
No frame vs Frame 2 (Base before)	6	27.7 vs 62.0	−2.220	0.026
No frame vs Frame 2 (Base after)	6	23.7 vs 30.0	−1.586	0.113
No frame vs Frame 1 (Redistribution after)	6	58.3 vs 64.3	−1.586	0.113
No frame vs Frame 1 (Redistribution before)	6	60.3 vs 67.3	−1.586	0.113
No frame vs Frame 2 (Redistribution after)	6	58.3 vs 67.7	−2.220	0.026
No frame vs Frame 2 (Redistribution before)	6	60.3 vs 64.3	−1.586	0.113