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Abstract

The Coase theorem posits: If [1] property rights are perfect, [2] contracts are enforceable, [3] preferences are common knowledge, and [4] transaction costs are zero, then the initial allocation of property rights only matters for distribution, not for efficiency. In this paper we claim that condition [1] can be dropped and show experimentally that this is also empirically true. This also holds when we frame taking as "stealing", and when the initial possessor has to work for the good.

JEL: C91, D01, D02, D03, D47, D61, K11
Keywords: Coase theorem, absolute vs. relative right, bargaining, efficiency, distribution, fairness

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1. Introduction

The Coase theorem (Coase 1960) is widely perceived to be a provocative, counterfactual thought experiment (for a survey of the literature see Korobkin 2014). *If* [1] property rights are perfect, [2] contracts are perfectly enforceable, [3] preferences (valuation) are common knowledge, and [4] transaction costs are zero, *then* the initial allocation of property rights only matters for distribution, not for efficiency. Through ex post bargaining, the asset ends up with the individual who values it most highly. Legal scholars have mainly used the theorem to explain why the initial allocation of property rights *does* matter: none of the four conditions is likely to be fulfilled in most practical applications (see only Samuels 1974, Ellickson 1989, Merges 1994, Medema 1997).

We drop condition [1], but retain condition [2], namely bilateral contractual commitments are perfectly enforced. (We also retain conditions [3] and [4].) The question we explore can be restated as follows: Does the Coase Theorem require absolute property rights, i.e., in rem rights against any non-owner?[1] Or are relative property rights – rights only against other parties to a contract – sufficient? A game-theoretic analysis suggests that relative property rights are sufficient (cf. Kaplow and Shavell 1996). Indeed, some formulations of the Coase theorem omit condition [1] entirely. In this paper, we provide an experimental test for this theoretical prediction. We focus on the simple-most case: a society of only two individuals, a single good, a single round of negotiations, and allocation of all the bargaining power to one of the negotiating parties. Yet the approach may be extended to larger societies (Bar-Gill and Persico 2013). Note that in a 2-person framework, the difference between absolute rights and relative rights is smaller. Yet, the core challenge of Coasean bargaining changes when condition [1] is removed.

In the textbook version of the Coase Theorem (with condition [1]), the main challenge to efficient allocation arises when the good is initially allocated to the lower-valuation user. Efficiency requires that the higher-valuation user buy the good from its initial owner. In our version of the theorem, with no absolute property rights, the challenge to efficient allocation is different: In the absence of property, even if the good is initially allocated to the higher-valuation user, it might be taken by the lower-valuation user. Efficiency requires that the higher-valuation user acquire a contractual right that would prevent the lower-valuation user from exercising her “right” to take.

Will the higher-valuation user acquire the contractual right and ensure an efficient outcome? How will the bargaining interaction unfold when there is no property? A series of well-studied behavioural regularities, such as the endowment effect, respect for entitlements or initial allocations, fairness and equality norms, have been shown to prevent efficient Coasean bargaining. Implicitly, these behavioural effects also challenge condition [3] above – that preferences (valuations) are common knowledge. This even holds in the lab. While the valua-

1 We use the terms “absolute property right” and “property” exchangeably.

tion of goods may be exogenously induced and communicated to all participants, the same cannot be done with behavioural traits. Participants bring them to the lab, and with respect to many behavioural traits there is discernible heterogeneity. Do these behavioural phenomena have similar effects when Coasean bargaining proceeds in the absence of property?

Our research question is practically relevant. Many valuable assets are not legally protected. If a chef has a brilliant idea for a new dish, the restaurant next door may copy it. Same for a fashion designer who invents a new cut, or for a TV producer who develops a new format (more from Raustiala and Sprigman 2012). But if the two chefs – or two designers or two producers – conclude a contract in which they promise not to copy each other's inventions, this contract is enforceable (antitrust concerns notwithstanding). Another example from intellectual property law rests on the territoriality principle. The raw material of IP law is intellectual achievement. Yet in copyright, the same achievement leads to a separate property right in all legal orders. And in patent, the invention is only protected in legal orders where the inventor applies for a patent, and successfully concludes the procedure. Consequently, the achievement may well only be protected in some, not in other legal orders, or the degree of protection may differ across legal orders. Nonetheless a contract is enforceable where one individual promises not to exploit the fact that the achievement is unprotected in another legal order, say by promising not to export an imitation to that market. In a broader sense, we may also invoke the many instances when property rules are ambiguous. In such instances, a contract can create a bright line entitlement and thus eliminate costly legal uncertainty.

These examples motivate the broader research question. But as a first step of a larger research agenda, this paper is confined to the most simple, bilateral case. By contrast the previous examples are multilateral. Yet there are also bilateral applications. Actually these applications are fairly close to the original examples used by Ronald Coase to develop his theorem. Must a neighbour accept vibrations, or noise, or smell? Often such externalities are indeed bilateral: only a single neighbour is affected. Legal orders differ in where they draw the line, i.e., in the way they define and enforce the neighbours' property rights. But a contract where one neighbour promises not to use her land in a way that would be perfectly legal is always enforceable.

Our research question also matters for the experimental literature. Since the eighties, implications of the Coase theorem have been tested in the lab.[2] The efficient outcome quite frequently obtains. But there are noticeable exceptions. And prices substantially diverge from the theoretical predictions. The main explanation given for these results are fairness preferences, and outcome fairness in particular. The Coase theorem has typically been tested with a bargaining protocol that gives one side all the bargaining power. This side, the proposer, makes a take-it-or-leave-it offer to the other side, the responder. In the experimental literature, such negotiations are known as ultimatum games (Güth, Schmittberger et al. 1982, Cooper and Dutcher 2011). If the responder is likely to reject offers she considers (grossly) unfair, the proposer faces strategic uncertainty. She must predict which offer the responder is sufficiently likely to

2 For detail see hypothesis section.

accept. For a series of reasons that we develop in the hypothesis section, the absence of property rights makes this prediction more demanding. Our experiment tests whether the increase in strategic uncertainty increases the incidence of bargaining failure.

Game theory predicts 100% efficiency regardless of whether or not possession is protected by an (absolute) property right. Our experiment tests this indifference result. But, as noted above, the 100% efficiency prediction has already been refuted in the experimental literature with respect to the standard Coase theorem (with property). And we have no reason to expect 100% efficiency in our version of the theorem (without property). The question is whether the behavioural effects and the resulting strategic uncertainty have a different impact on efficient trade with and without (absolute) property rights – whether the cost in terms of lost trades is the same with and without (absolute) property rights. The real purpose of our experiment is, therefore, to test the equivalence between absolute and relative property rights in securing efficient trade, but not necessarily the game theoretic equivalence at 100% efficient trading.

We stress that this is not an experiment designed to test any particular behavioural impediment to efficient trading. Our goal is not the advancement of behavioural theory. We are interested in testing the equivalence hypothesis, and its resulting implication for institutional design - that if, for whatever reason, property is not protected, this does not diminish welfare.

All experiments are artificial. They abstract from many features of the real world phenomenon one is interested in, in the interest of cleanly identifying a causal relationship. In our case, the price we have to pay for this is fairly small, though. We test a formal model, and thus borrow the limiting assumptions from economic theory. We have a chance in the lab to fix transaction costs at zero, and to make valuations common knowledge, so that we can isolate the effect of the existence, or absence, of property. This would be impossible to do in the field. Last, but not least, we can randomly assign participants to either a situation that represents the original version of the theorem (with property), and others to a situation representing our alternative version (without property). If we find differences, we can causally attribute them to the difference between the two versions of the theorem.

If relative property rights are as effective as absolute property rights, the individual with a higher valuation for a good must be similarly willing to buy it from the individual with lower valuation (absolute right) and to pay for the other individual not to take her good (relative right). This is what we test with our first treatment. Ultimately, in a world without property, taking is pointless. The original possessor could always take the good back, and so forth (see Kaplow and Shavell 1996). Ultimately, in such a world the only technology for creating value is contract. We capture this dimension of the absolute vs. relative right distinction by our second treatment. In this treatment, the good is destroyed unless the parties agree on secured possession.

We have a striking result. Irrespective of treatment, trade occurs in about 75% of all cases. Neither treatment affects efficiency. We also do not find any significant difference in the price the individual with low valuation asks for selling the good, refraining from taking the good, or agreeing that the good is not destroyed. The theoretical equivalence of absolute and relative property rights is completely supported by the data. None of the behavioural qualifications matter.

One might object that our experimental design undermines the difference between absolute and relative property rights. Participants might see the good as just a cover story for a bargaining experiment. Or they might view the initial allocation of the good to one party as normatively irrelevant. In two complementary ways, we investigate whether this objection is critical. First we add a treatment where we call taking "stealing". This framing manipulation has no discernible effect. Second we have the initial possessor earn the good in a real effort task. This has a small, weakly significant negative effect on the probability that the good trades. If we combine the earning and the framing manipulations, takers demand less (but still more than 50% of the value of the good), yet the probability of trade remains unaffected.

The remainder of the paper is organised as follows: Section 2 derives hypotheses from theory, and from the existing experimental literature. Section 3 introduces the design of the experiment. Section 4 reports results from the main experiment. Section 5 reports on the framing manipulation. Section 6 discusses the earning manipulation. Section 7 concludes.

2. Hypotheses

2.1 Standard Theory

If individuals hold standard preferences, this is common knowledge and the society has a size of two individuals, it is straightforward to show that the efficient outcome is always reached if only contracts are enforceable. To see this, first assume that the good is initially allocated to individual h who values it more than individual l. If the initial owner is protected by a property right, she keeps the good. There is no mutually acceptable price at which the good trades. If the initial owner is not protected, l credibly threatens to take the good. To avoid this inefficient taking, h pays a "bribe" that is determined by the parties' respective valuations and their relative bargaining power. Assuming standard preferences, h is happy to pay since contract is the only available technology for securing the benefits from the good. Once the deal is struck, h no longer has anything to fear[3].

Now consider the opposite case. l to whom the good is initially allocated values it less than h. If this allocation is protected with a property rule, h will buy the good, at a price that is determined by the parties' respective valuations and their relative bargaining power. If the initial owner is not protected, one might think that h just takes the good. Yet since there is no property right, l would come by and take the good back. In anticipation, h offers l to acquire a

3 Of course, since contract only gives her a relative right, in a larger society the individual that values the good more than all others must conclude contracts with all of them.

contractual right to hold the good. Since l values the good less and valuations are common knowledge, l agrees. Price is again determined by the difference in valuation and relative bargaining power.[4]

More formally assume a society of size $N = 2$. Individuals h and l are both endowed with e units of a perfectly transferable, scalable and protected means of payment. There is a single unit of a good. Without loss of generality we assume that the value of this good to individual h is $v_h > 0$, and the value of the good to individual l is $v_l = 0$. Valuations are common knowledge. We further assume that $e > v_h > v_l = 0$, so that there is no budget constraint. The individual who currently possesses the good is free to sell it. Contracts are perfectly enforceable. Concluding and enforcing contracts is costless. The act of taking the good is similarly costless. l is allowed to make a take-it-or-leave-it offer, and hence has all the bargaining power.

We consider possible variations on the following dimensions:

(1) The identity of the original possessor: Individual h or individual l.

(2) The level of protection: We consider two alternative assumptions: (i) Property rule (ii) No protection.

The outcomes in each of the four permutations are summarized below:

<u>Original Possessor:h; Property Rule Protection</u>
There is no bargaining; the asset stays with h.

<u>Original Possessor:h; No Protection</u>
l threatens to take. h and l sign a contract, by which h pays l a bribe of $v_h[-\varepsilon]$ and l commits not to take.

<u>Original Possessor: l; Property Rule Protection</u>
l sells the asset to h for $v_h[-\varepsilon]$.

<u>Original Possessor: l; No Protection</u>
h and l sign a contract, by which h pays l a price of $v_h[-\varepsilon]$ and gets the good (and l commits not to take it back).

With property, bargaining takes place (and bargaining power matters) only when the original possessor is l. With no protection, it is "as if" the entitlement is not allocated to either party. (If l takes the good, then h can take it back, l can retake, etc'.) In other words, both parties have an outside option that is worth zero. In this case, efficient allocations obtain regardless, and the distributional outcome is determined by the allocation of bargaining power.

4 Of course, in a larger society individual h must also conclude contracts with all others who value the good less, to safely enjoy the good.

In the first situation (property right for h), no trade is to be expected. We experimentally test the remaining situations where efficiency requires trade. Based on game theory, our null hypothesis reads:

H_0: a) The asset always ends up with h.
b) h pays $v_h[-\varepsilon]$.

2.2 Behavioral Qualifications

2.2.1 General

Coasean bargaining has been the subject of multiple experimental studies. The basic efficiency result has been confirmed by most studies: bargaining guarantees efficient allocations (Rhoads and Shogren 2001, Prante, Thacher et al. 2007). These studies cover a variety of settings, including bargaining among more than two parties (Hoffman and Spitzer 1982, Hoffman and Spitzer 1986), settings where one party must obtain consent by another party to exploit a wealth-improving opportunity (Schwab 1988), settings where the valuation function of one individual is non-linear in the amount of the good (Shogren, Moffett et al. 2002), and settings where the object of negotiation is physical discomfort (Coursey, Hoffman et al. 1987). Of course, the Coase Theorem predicts efficient outcomes only in the absence of transaction costs. In a meta-study that includes also experiments where transaction costs were explicitly introduced, Prante, Thacher et al. (2007):4 find inefficient outcomes in 31% of the 2052 bargaining dyads that they consider.

Some experimentally-identified sources of inefficiency are excluded by our experimental design. We fix transaction costs at zero (cf. King 1994, Shogren 1998, Rhoads and Shogren 1999, Croson and Johnston 2000, Cherry and Shogren 2005). Preferences (valuations) are common knowledge (cf. Prudencio 1982)[5] (Hoffman and Spitzer 1982, Ayres 2010). Participants are not under time pressure (cf. Prudencio 1982, Harrison, Hoffman et al. 1987). Interaction is completely anonymous, and communication is exclusively through the negotiation protocol (cf. Hoffman and Spitzer 1982, Prudencio 1982). There is no shadow of the future, and hence no room for establishing a relationship or for reputational concerns (cf. Hoffman and Spitzer 1982, Harrison and McKee 1985). Contracts are perfectly enforced (cf. Harrison and McKee 1985). In the few experimental papers that have studied Coasean bargaining with imperfectly protected property rights, at least one of these sources of inefficiency is present (Croson and Johnston 2000, Cherry and Shogren 2005, Ayres 2010).[6]

Still, there are potentially important behavioural forces that might interfere with efficiency in our framework. These forces, and their expected effects on bargaining dynamics and on efficiency, are described below.

5 The critique by Norton and Patrick (1985) does not concern this point.

6 Aivazian, Callen et al. (2009) also find an effect of „ill-defined“ property rights. But by the term they mean a situation where the group has a size bigger than two, and no property rights are assigned.

2.2.2 A Simple Model

We adopt a simple bargaining protocol, l makes an offer and h decides whether to accept or reject the offer. In describing possible behavioural effects it is useful to address these two stages in reverse order.

First, what is the maximum amount that h would be willing to pay? This maximum amount constitutes h's reservation price, which we denote by R. In the game-theoretic analysis, $R = v_h$. In a behavioural model, h's reservation price includes additional dimensions. Consider the situation where the good is initially allocated to party *l* and *l*'s entitlement is protected with a property rule (this situation triggers what we refer to as the standard Coase Theorem). The maximal amount that *h* would be willing to pay, to purchase the good from *l*, is affected by h's preferences for fairness. As an entire literature on ultimatum games has demonstrated, offers deemed grossly unfair are often rejected (Güth, Schmittberger et al. 1982, Oosterbeek, Sloof et al. 2004, Hoffman, McCabe et al. 2008, Cooper and Dutcher 2011). This in particular holds for offers that leave the responder less than half of the surplus (Hoffman and Spitzer 1982, Prudencio 1982, Harrison and McKee 1985, Shogren 1992). Formally, let *x* denote the strength of *h*'s fairness concerns. The maximal amount that *h* would be willing to pay, in this behavioural model, is: $R = v_h - x$.

We now turn to l: what amount will l offer? A perfectly rational (and risk neutral) l who seeks to maximize her absolute monetary payoffs will try to predict *R* and make an offer that maximizes the expected payment (magnitude of offer multiplied by the probability of acceptance). Formally, *l* would form a prediction: $\hat{R} = v_h - \hat{x}$, where $\hat{x} = x + \varepsilon_x$ and the error term ε_x is distributed according to the cumulative distribution function $F(\varepsilon_x)$. l would then offer an amount *m* that maximizes $Prob\left(m \leq \hat{R}\right) \cdot m$, or $Prob(\varepsilon_x \leq v_h - x - m) \cdot m$. With this chosen *m*, there is a probability $Prob(\varepsilon_x \geq v_h - x - m)$ that efficient trade would not occur. The source of the inefficiency is prediction error and the likelihood of bargaining failure increases in the variance of the error distribution $F(\varepsilon_x)$ (cf. Bebchuk 1984).

Next, consider the situation where the good is initially allocated to party *h* but *h*'s entitlement is not protected (this situation triggers what we refer to as the our version of the Coase Theorem). This situation would be expected to trigger additional behavioural effects, in addition to the fairness concerns discussed above. In particular, loss aversion may further reduce *h*'s reservation price, *R*, as the initial allocation of the good alters the reference point against which *h* measures any bargaining outcome (Kahneman, Knetsch et al. 1991, Tversky and Kahneman 1991, Thaler, Tversky et al. 1997, Köszegi and Rabin 2006).[7] Let *y* denote this additional be-

7 Other explanations for an endowment effect effect (Kahneman, Knetsch et al. 1990, Kahneman, Knetsch et al. 1991, Rachlinski and Jourden 1998, Tversky and Griffin 2000, Korobkin 2003) are ruled out by our experimental design. Specifically, we use a mere token; and no endowment effect has been found with tokens (Kahneman, Knetsch et al. 1990). Attachment, which is one potential source of the Endowment effect, is unlikely to form with tokens (cf. Kelman 1978, Brosnan, Jones et al. 2007). And regret, another potential source of the Endowment effect, is unlikely to be triggered, since the token is originally assigned by the experimenter (cf. Gilovich and Medvec 1995).

havioural effect. We thus have $R = v_h - x - y$. And, correspondingly, l's prediction of h's reservation price becomes: $\hat{R} = v_h - \hat{x} - \hat{y}$, where $\hat{y} = y + \varepsilon_y$ and the error term ε_y is distributed according to the cumulative distribution function $G(y)$. l would offer an amount m that maximizes $Prob(m \leq \hat{R}) \cdot m$, or $Prob(\varepsilon_x + \varepsilon_y \leq v_h - x - y - m) \cdot m$. With this chosen m, there is a probability $Prob(\varepsilon_x + \varepsilon_y \geq v_h - x - y - m)$ that efficient trade would not occur. The source of the inefficiency, as explained above, is prediction error. In our version of the theorem, l needs to estimate two variables, x and y, whereas there was only a single variable to estimate in the standard version. Prediction thus becomes more difficult, and prediction error and inefficiency become more likely. Note also that the introduction of a new variable, y, that reduces the reservation price, R, reduces l's optimal offer, m.

Furthermore, in our version of the theorem disjunction between bargaining power and the initial allocation of the good might further increase the likelihood of prediction error. In the standard version of the theorem, the same party, l, both gets the good and enjoys all the bargaining power. In our version, bargaining power is allocated to one party, l, while the good is initially allocated to the other party, h. Is l entitled to a larger share of the surplus, since she was granted all the bargaining power? Is h entitled to a larger share of the surplus, since the good was initially allocated to him? What is the relative strength of these competing claims? l would be uncertain about how h resolves these issues, and this uncertainty increases the likelihood of prediction error, and of inefficiency.

We have thus far assumed a perfectly rational l who seeks to maximize her absolute monetary payoffs. How does the analysis change, when l is a behavioural actor? In the standard version of the theorem, the only behavioural effect involved fairness concerns. We have seen the implications of h's fairness concerns. But l may herself care about fairness. And this could reduce the amount that l demands and thus the likelihood of an inefficient failure to trade. Formally, let $\widetilde{m}$ denote the offer that l deems fair, and distinguish it from the offer m that a rational l with no fairness concerns of her own would make (as derived above). l would then offer: $min(m, \widetilde{m})$.

In our version of the theorem, there is a second behavioural effect: loss aversion pushes h's reservation price further down. But, also, l herself may be averse to taking, perceived as stealing. Experiments find (limited) hesitance to steal (Falk and Fischbacher 2002, Schildberg-Hörisch and Strassmair 2012, Engel and Nagin 2015) (cf. also Prudencio 1982, Lewinsohn-Zamir 2012). And this hesitance could reduce the amount that l demands and thus the likelihood of an inefficient failure to trade. Again, we could let $\widetilde{m}$ denote the offer that l deems appropriate. And, again, l's offer would be: $min(m, \widetilde{m})$.

In both situations – in the standard version of the theorem and in our version – there is a behavioural effect(s) that pushes h's reservation price down and a corresponding effect that pushes l's offer down. We expect the effect on h's reservation price to be stronger than the effect on l's offer: when h is left with a smaller share of the surplus, this would likely be more painful to h than it would be to l. Similarly, the loss that h experiences from a taking, perceived as stealing, would likely be more painful to h than to l. Therefore, we expect that $min(m, \tilde{m}) = m$.[8] Accordingly, the predictions derived under the assumption of a perfectly rational l who seeks to maximize her absolute monetary payoffs should continue to hold.

We stress that our experiment is not meant to test a particular behavioural theory, or to discriminate between different behavioural effects. All we need, and all that we test, is the prediction that in our version of the theorem the likelihood of a successful bargain is lower and l's offer is lower.

In particular, we test the following alternative hypothesis:

H_1: a) h does not always obtain the good.
b) Bargaining failure is more frequent if l sells her right to take the good.
c) The price demanded by l is lower if l sells her right to take the good.

3. Design

Our baseline is the textbook version of *Coase*'s theorem: an individual with low valuation for a good (which we fix at 0) holds an absolute right in the good. The individual with high valuation may buy the good from her.

We compare this baseline with two treatments. Each treatment is meant to capture a different potential downside of not protecting property. The first *Take* treatment tests the willingness of the high-valuation user to pay for the low-valuation user not to take the good. To test this key aspect of the theorem, we simplify the situation. We initially assign the good to the high-valuation user, and make it possible for the low-valuation user to take the good.

The first version of the experiment allows for only one possible taking (by the low-valuation user). In theory, in the absence of property rights, the high-valuation user would be able to take the good back, the low-valuation user would be able to take again, etc. (Kaplow and Shavell 1996). Given the possibility of reciprocal takings, the good is worthless in the absence of contract. Any value requires contract. To capture this aspect of the theory, we conduct a second version of the experiment, the *Destroy* treatment. In this version, the experimental protocol provides for the destruction of the good unless the parties conclude a contract. One may also say: in this treatment, participants bargain over the right to undisturbed use. While this protocol captures the reciprocal takings problem implied by the absence of property, one may wonder whether it has additional behavioural implications. Each version of the experiment thus captures a separate dimension of bargaining in the absence of an absolute property right.

8 The asymmetry between aversion to inflicting harm on others, and aversion to being the victim of harm, is also characteristic of the canonical model of inequity aversion by Fehr and Schmidt (1999), which assumes $\alpha > \beta$. This is also what one predominantly finds empirically (Blanco, Engelmann et al. 2011).

The examples from legal practice listed in the introduction map more directly to the *Take* treatment: a competitor may just use the same recipe, cut or media format; she may just sell a copy of my product in a country where intellectual property is not protected; a neighbour may just annoy me with her noise, smell or vibrations (and thereby appropriate some of the enjoyment of my land). But there are also illustrations that are closer to the *Destroy* treatment. Not so rarely, a good only has value as long as it is unique. A classic illustration is a secret. If somebody else knows my secret, or is able to learn it, it becomes worthless unless the parties are able to reach a contractual arrangement. Another person may also have naked threat power because she is able to (legally) inflict harm on me, say by exercising a right that has no intrinsic value to her. Divorced parents fighting over the right to live with their child might be taken as an illustration of a feud that destroys what both hopefully care most about: their child's well-being.

3.1 Baseline and Treatments

3.1.1 Baseline

In the *Baseline* a token good is originally assigned to individual l. If, at the end of the experiment, l is still in possession of the good, the good has $v_l = 0$. If, by contrast, h possesses the good, the experimenter buys the good at $v_h < e$. Property is perfectly protected, meaning that the only way for h to possess and use the good is by concluding a contract with l. l is allowed to make a take it or leave it offer. The parties are free to choose any price $\in [0, e]$. If a deal is struck, the good is transferred to h; l receives the price. There are no negotiation or enforcement costs, and no time limit.

3.1.2 Take Treatment

At Stage 1, we assign the token good to individual h. At Stage 3, individual l can unilaterally take the good from individual h. At Stage 2, before individual l gets the chance to unilaterally take the good, the parties can bargain, and individual l may commit not to take the good. The bargaining protocol is as follows: individual l makes a take-it-or-leave-it offer to individual h. There are no negotiation or enforcement costs, and no time limit. If a contract is reached, the good stays with individual h and the game ends (we do not proceed to Stage 3). If a contract is not reached, we proceed to Stage 3 and individual l decides whether to take the good. And then the game ends. If individual h possesses the good at the end of the experiment, she receives her redemption value v_h from the experimenter. If individual l possesses the good at the end of the experiment, she receives her redemption value $v_l = 0$ from the experimenter.

3.1.3 Destroy Treatment

At Stage 1, the good is originally assigned to individual h. Property is not protected, meaning that l is free to take the good, or to threaten to do so. The only way for h to secure possession and use of the good is by concluding a contract with l. Specifically, at Stage 2, l is allowed to make a take it or leave it offer. If h rejects, the good is not protected. If a deal is struck, one of the parties gains secure possession; the other receives a price. There are no negotiation or enforcement costs, and no time limit. In order not to artificially constrain the negotiation space, contracts that give the good to l are permitted. At the end of the experiment, the good is bought by the experimenter at its redemption value, but only if the parties concluded a contract.

In our model, the good could go endlessly back and forth between the two parties as long as no contract has been concluded. Directly translating this into an experimental protocol would be impractical; participants know for sure that any experiment ends within reasonable time. Other experiments have created quasi-infinity by a rule that the experiment in every period ends (after a sequence of initial periods) with a small probability. But if we had used that design option, there would have been more than one difference between this and the *Take* treatment: one shot vs. repeated (with uncertain end), and a single option vs. the option to take and to take back. Yet the critical element of the model is not the actual taking, and possible taking back. The critical element is that, in the absence of a contract, the good is worthless. This we implement by the following rule: if l does not make an offer, or if h rejects the offer, the good is destroyed.[9] Table 1 summarizes our treatments.

	Coase	Take	Destroy
good originally allocated to	l	h	h
property right	yes	no	no
bargaining power	l	l	l
effect of bargaining failure	good stays with l	l may take good	good is destroyed

Table 1
Treatments

3.2 Motives for Design Choices

We are interested in testing whether the textbook version of the Coase theorem and our alternative version yield different outcomes, despite the fact that standard theory predicts no difference. We therefore focus on a situation for which this theoretical equivalence holds. One might criticize this approach since theoretical equivalence requires two differences in design: (1) presence vs. absence of a property right and (2) original assignment of possession to l vs. h. Yet if we only change one of these two features in isolation, standard theory predicts fundamentally different outcomes (at least for the Take treatment). For testing our hypothesis, we cannot change one of these elements in isolation. In essence, we are testing whether giving a property right to l is different from giving l the option to take the good from h. Standard theory says that there is no difference. The behavioural analysis suggests several possible differ-

9 Since the game remains one-shot, we also have no reason to consider the possible additional complication resulting from the need to predict time preferences of the negotiation partner.

ences. (Even standard theory would predict a difference between Take and Destroy when $v_l > 0$. But when individual l values the asset at zero, these differences disappear.)

In the field, individuals who value a good less usually do not regard the good to be completely worthless. In the interest of increasing external validity, it might therefore have been attractive to set $v_l > 0$. Yet in the *Destroy* treatment, the good is destroyed if there is no trade. We want to be in a position to compare the *B*aseline with both the *Take* treatment and the *Destroy* treatment, and both treatments with each other. Now if bargaining fails in the *Baseline* and *Take* versions, l is still in possession of the good. Consequently had we chosen $v_l > 0$, there would have been a confounding difference between the *Destroy* treatment and the remaining two versions of the experiment: a difference in payoffs in case negotiations fail ($v_l > 0$ vs. 0). This confound would have prevented us from meaningfully comparing the different versions of the experiment.

3.3 Timeline

In both versions of the experiment, participants are randomly matched to groups of two. All are endowed with $e = 40$ units of an experimental currency ECU. There is one unit of a good. Valuations for this good are randomly assigned. Individual h values the good at $v = 30ECU$, individual l values the good at $v = 0ECU$. Valuations are common knowledge. In all versions of the experiment, the timeline is as follows:

t_1: groups are formed, valuations are assigned, treatment conditions are defined.

t_2: individual l who has all the bargaining power makes an offer.

t_3: individual h decides whether she accepts.

t_4: if a contract is made,
Baseline: the good is transferred to individual h.
Take: the good stays with individual h.
Destroy: the good either stays with individual h or is transferred to individual l.

t_5: if a contract is not made,
Baseline: the good stays with individual l.
Take: the taker decides whether to take the good from its current owner.
Destroy: the good is destroyed

t_6: the individual who eventually possesses the good sells it to the experimenter, at her valuation.

3.4 Post-experimental tests

Our experiment tests for possible failures in Coasean bargaining and, especially, whether such failures are more frequent in the absence of property rights. The source of these possible bargaining failures lies in a series of behavioural regularities (see Section 2.2). To test whether these regularities affect choices in our bargaining game, within subjects we run three incentivized post-experimental tests and administer a non-incentivized questionnaire. At the beginning of the experiment, participants only learn that the experiment has several parts, but do not know what these parts are about. That way the results of the main experiment cannot be affected by the anticipation of later parts.

The first test is a standard ultimatum game (Güth, Schmittberger et al. 1982), with individual l in the role of proposer, and individual h in the role of responder. Participants receive a new endowment of 20 ECU. Groups are rematched within matching groups of unannounced size 6. This standard procedure is meant to guarantee independent observations, without inducing participants to second guess group composition (see, e.g., Charness 2000, Montero, Sefton et al. 2008).

The second test is a simple stealing game (Falk and Fischbacher 2002, Engel and Nagin 2015). Individual h is endowed with one unit of a good, and is randomly paired with another individual l (again from the same unannounced matching group). l may take the good. Whoever possesses the good at the end of this part of the experiment receives 20 ECU from the experimenter.

The third test is a standard instrument to measure social value orientation, the so-called ring measure (McClintock and Liebrand 1988). In a non-incentivized questionnaire, we finally administer the Big Five instrument (Rammstedt and John 2007), ask four trust questions taken from the German Socio Economic Panel, and ask for basic demographic information.

The Experimental Lab of the University of Hamburg Economics Department was kind enough to run the experiment for us. The experiment was programmed in zTree (Fischbacher 2007). Participants were invited using hroot (Bock, Baetge et al. 2014). Participants never learned with which other participant they interacted. This was announced in the instructions. In each treatment, we had 72 student participants of various majors. 116 (53.7%) were female. Mean age was 25.58 years. The experiment approximately lasted one hour. Participants on average earned 11.76 € ($14.66 at the days of the experiment), range [4€, 21€].

4. Results of the Main Experiment

4.1 Treatment Effects

Figure displays the results graphically. In the *Baseline* and in the *Take* treatment, the exact same number of offers is rejected (8/36). In the *Destroy* treatment, 5 of those 33 offers are rejected that propose to give the good to h, plus 2 of the 3 offers that propose to give the good to l. Given these numbers, we obviously do not find any significant treatment differences, whether we use non-parametric Mann-Whitney tests or estimate a parametric Logit model, and whether or not we include the offers (in the *Destroy* treatment) that propose to give the good to l. We also do not find any treatment differences if we condition the acceptance decision on the amount the l player requests. This rejects hypothesis $\mathbf{H_{1b}}$ and gives us

> *Result 1: Coasean bargaining is no less efficient in the absence of absolute property rights.*

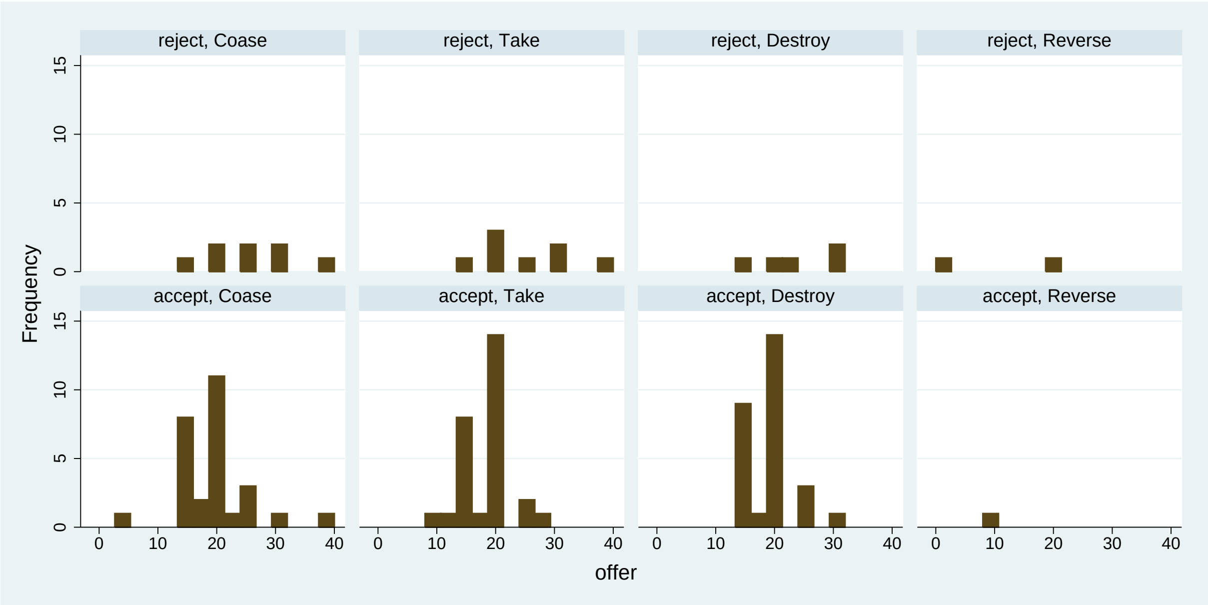

Figure 1
Descriptives

histogram of acceptance / rejection, conditional on treatment, size of the offer and (for the *Destroy* treatment) whether offer was about transfer from l to h or in the reverse direction
bar size indicates frequency

Strictly speaking this result only shows that, with our data, we are not able to reject our null hypothesis of no treatment difference. This is not the same as showing that the null hypothesis is actually true. Power calculations indicate how much trust we can put in this non-result. In each treatment, we have 36 independent observations. It is conventional to accept a β-error of .2. If we follow this convention, we may be safe to detect an effect of standardized size .6696. Since all our hypotheses are directed, we are justified to use a one-sided test. We may therefore even safely detect an effect of standardized size .5918.

Descriptively, offers also look very similar across treatments in Figure . Again we do not find any significant differences, whether we use a nonparametric Mann Whitney test or OLS, and whether or not we include offers (in treatment *Destroy*) that propose to give the good to l.[10] This rejects hypothesis $\mathbf{H_{1c}}$ and gives us

> *Result 2: In the two versions of the Coase Theorem, individual l makes indistinguishable price offers.*

4.2 Testing Standard Theory

While we do not find any treatment differences, we also do not support our $\mathbf{H_0}$, which was based on standard theory. With common knowledge of rationality, we should not have found 77.78% (*Coase* and *Take*) or 84.85% (*Destroy* if good is to stay with h) of acceptance; rather the acceptance rate should be (close to) 100%. Using a binomial test, at conventional levels we can exclude that this probability is larger than 89% in treatments *Coase* and *Take*, and larger than 90% in treatment *Destroy*.[11] This supports hypothesis $\mathbf{H_{1a}}$.

By the same token, with common knowledge of rationality, the mean offer should not have been 20.97 ECU in *Coase*, 19.97 ECU in *Take*, and 19.03 ECU in *Destroy*. Rather, irrespective of treatment, offers should have been close to $v_h = 30$, since the design gives l all the bargaining power. Yet using a one sample signed rank test, in the *Coase* treatment we can exclude that the offer is larger than 22 ECU.[12] In the *Take* and *Destroy* treatments we can exclude that the offer is larger than 20 ECU.[13]

4.3 Explanations

We do not find any treatment differences, suggesting that behavioral forces do not play a meaningful, differential role in our setup, namely, they do not have a meaningfully different effect in our version of the theorem as compared to the standard version. Our findings do not support standard game theoretic predictions either: behavioral forces play a role, just not a differential role. Revisiting Figure 1, we observe that offers giving l more than 20 are very likely to be rejected. Irrespective of treatment, offers peak at 15 ($\frac{v_h}{2}$) and at 20 ($\frac{e}{2}$). We turn to the data from our post-experimental tests – to understand these findings.

The fact that we do not find treatment differences in offer amounts bears on the importance of the behavioral effects discussed in Section 2.2. In particular, loss aversion or respect for ownership led us to hypothesize lower offers in the treatment ($\mathbf{H_{1c}}$), and we do not find such lower

10 Since sample size is the same as for testing the frequency of trade, the minimum standardized effect size we are safe to detect is also identical: .6696 for a two-sided test, or .5918 for a one-sided test.

11 We avoid testing at the limit of the support by repeating the test of all probabilities between 75% and 99%, and report the lowest percentage at which a two-sided test still rejects at conventional levels.

12 We face the same problem of testing at the limit of the support as with acceptance decisions, and tackle it the same way. We test whether the mean offer, in this treatment, is at any value between 1 and 39 ECU. We report the highest value at which the test still rejects, i.e. 23 ECU, p = .0059.

13 The highest value at which the test rejects is 21 ECU, p = .0256 in treatment *Take*, and p = .0085 in treatment *Destroy*.

offers. Results from the post-experimental stealing game show that respect for ownership did not play a meaningful role in our setup: only 6 out of 108 participants decided not to take the good in this game. (It would not be surprising to find minimal stealing in the post-experimental test, and still find that loss aversion or respect for ownership lowers the offers in the Take and Destroy versions of the main experiment: In the post-experimental stealing game the taker gained positive value from taking the good, and this monetary incentive may have overcome any respect for ownership. In contrast, in the Take and Destroy treatments the taker stood to gain nothing from taking.)[14]

The post-experimental ultimatum game sheds further light on the importance of behavioral forces in our framework. This post experimental test replicates results from previous ultimatum experiments. Fairness norms prevent the offeror from making very high offers (namely, from keeping almost all of the surplus to himself). And some inefficiency results when the offeror thinks that an amount is sufficiently fair and the offeree thinks otherwise. These considerations affect both the Coase baseline and the treatments in the main experiment. Accordingly, they explain why results from our main experiment are not in line with standard game theoretic predictions.

It is interesting that offer amounts in the ultimatum game do not significantly explain offer amounts in the main experiment – neither in the baseline nor in the treatments (see Figure). This holds whether or not we control for treatment in the main experiment, and whether or not we interact treatment with the amount demanded in the ultimatum game. It seems that the bargaining game – ultimatum vs. baseline vs. treatments – has an effect on how fairness norms manifest, but that this effect is not systematic; it varies among individuals.

14 We get more stealing than is common in the literature (see, e.g., Schildberg-Hörisch and Strassmair 2012). This is likely because we present subjects with a binary choice – steal (you get everything and the other party gets nothing) or not steal (you get nothing and the other party gets everything). Other stealing experiments allow for a continuous stealing decision – the subjects decide how much to steal.

Figure 2
Offers in Main Experiment and in Ultimatum Game

marker size indicates frequency
offers in the ultimatum game are recoded as the share the proposer wants to keep for herself, to increase comparability between the main experiment and this post-experimental test

We do not find any significant effects when attempting to explain choices with data from the social value ring measure, or from the questionnaire.

5. Stealing Frame

As a first robustness check, we repeat the *Take* treatment from the main experiment, but now call taking "stealing". The new *Frame* treatment was run in the same lab, with the same software. We had 66 new participants,[15] half of whom assumed the role of *h* and half of whom the role of *l*. 48.49% were female. Mean age was 25.03 years. Participants on average earned 15.19€.

Figure compares the *Take* treatment without and with the stealing frame. If we add the frame, descriptively a few more deals fail (63.63% of all goods trade, rather than 77.78% without the frame). Yet this difference is not significant, whether we use a non-parametric Mann Whitney test or a logistic regression, and whether or not we control for or interact with the size of the offer. The mean amounts demanded by player *l* are virtually identical (19.97 in the *Take* treatment vs. 20,00 in the *Frame* treatment). Unsurprisingly, we also do not find any statistical difference regarding the amount requested, neither non-parametrically nor parametrically.

15 In one of the 3 sessions, so many invited participants did not show up that we could not fill one matching group of 6 participants.

Of course, these are again only null effects. But we had enough statistical power to detect an effect of standardized size .6851 in a two-sided test, and .6054 in a one-sided test.[16]

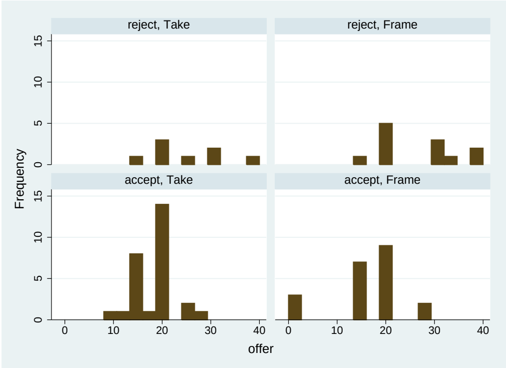

Figure 3
Choices in Take vs. Frame Treatment
(Good Assigned by Experimenter)

6. Earned Good

In the final robustness check, whoever initially receives the good has to earn it. We use a simple, but annoying real effort task (it has for instance been used by Falk and Huffman 2007): the participant has to correctly count the number of 1s in 10 tables of size 10x10 with 1s and 0s. With this change we repeat the *Coase*, *Take* and *Frame* treatments. These additional treatments were run in the same lab, using the same software.[17] We had 204 new participants. 59.8% of the additional participants were female. Mean age was 25.39 years. Participants on average earned 15.63€.

As Figure shows, this manipulation changes outcomes, but only slightly. If the good is assigned (in the main experiment), 77.78% of all goods trade in the *Coase* and in the *Take* treatments, and 63.63% trade in the *Frame* treatment. If the good is earned, in the *Coase* treatment, even more goods trade (83.33%), while less goods trade in the *Take* treatment (63.63%). The comparison between the *Coase* and the *Take* treatments, when the good is earned, is the only weakly significant effect (Mann Whitney, N = 69, p = .0646). And we are reluctant to read too much into this result, since the offer size in Earned Take (19.52 ECU) is not significantly different from the offer size in Earned Coase (20.78 ECU). Moreover, all other comparisons of accepting offers are insignificant, whether we use non-parametric or parametric statistics, and whether or not we either control for or interact with the size of the offer. Note in particular that even more goods (78.79%) trade if the good is earned, and the taking manipulation is combined with the stealing *Frame*.

16 Power (slightly) differs from calculations in the main experiment since we miss 6 participants (3 responders) in the *Frame* treatment.

17 We do not repeat the *Destroy* treatment since it did not significantly differ from the *Take* treatment in the main experiment. In the *Earned Take* and in the *Earned Frame* treatments, we could not fill one matching group of 6 participants each.

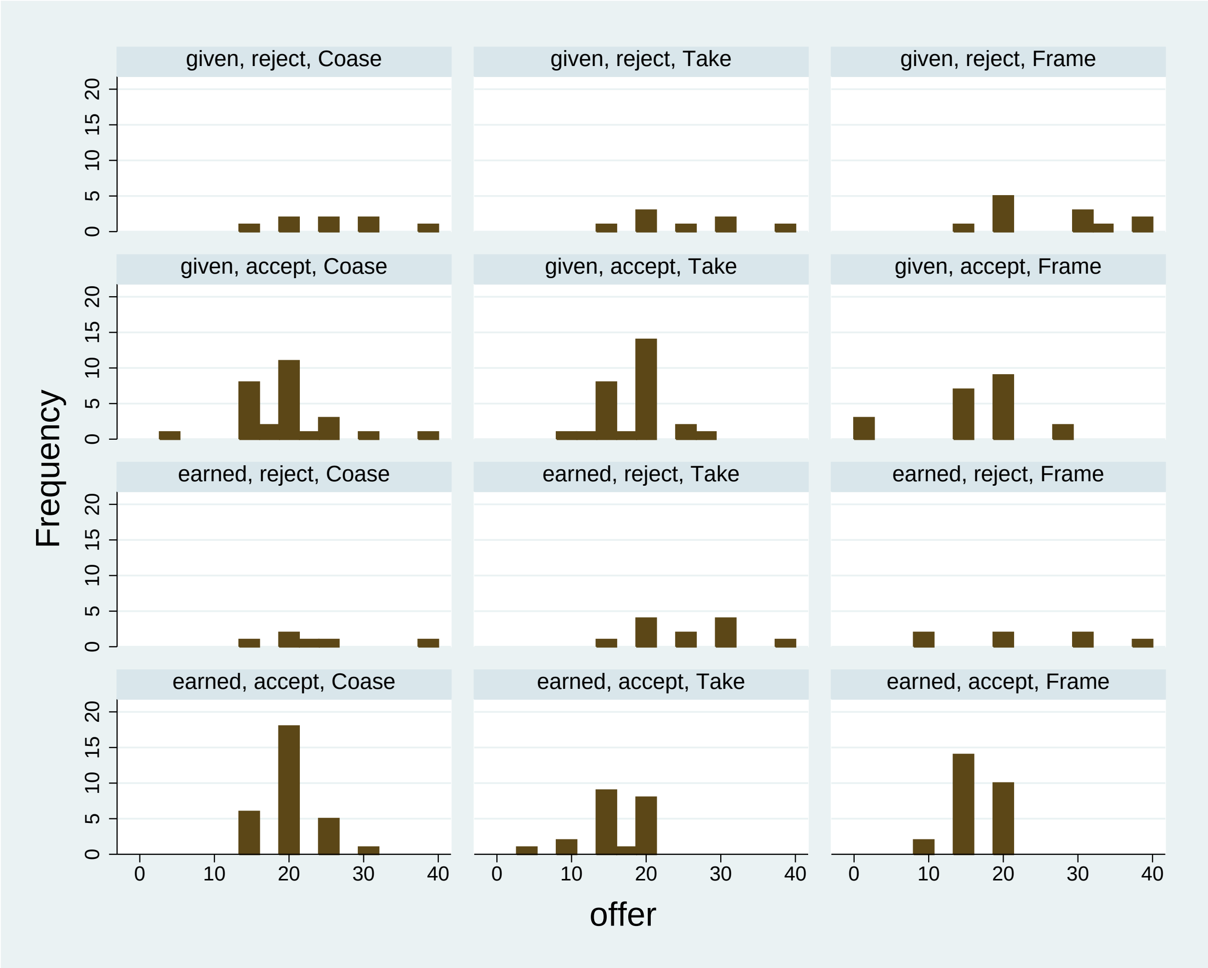

Figure 4
Choices in Treatments with Assigned vs. Earned Goods

This is all the more remarkable since we do find a significant effect on offer size in the *Earned Frame* treatment. The average offer size in the Earned Frame treatment is 17.91 ECU, as compared to 20.78 ECU in the *Earned Coase* treatment – a statistically significant difference (Mann Whitney, N = 69, p = .0028). The *Earned Frame* treatment combines the earning manipulation with the framing manipulation. It thus puts our hypothesis to the hardest test. The results suggest that proposers are sensitive to these manipulations. They deem it fair to leave the other participant a larger share. But this share is on average still less than half the value of the good (which is 30). Focusing on efficiency, the lower offer size does not translate into more deals. It seems that proposers offer to leave possessors with a larger share, because they accurately anticipate that possessors will have a higher reservation price in this treatment. And, in any event, we don't see fewer trades, or more inefficiency, as we did in *Earned Take*.

7. Conclusion

For policymakers, the Coase theorem is a source of comfort. They may still see good reason to protect property, and to allocate the absolute right to those individuals who are likely to put the protected good to the most efficient use. This is the logic of a majoritarian default. Yet even if they act with the best of all intentions, policymakers may get it wrong. This problem is exacerbated in a heterogeneous society; general rules inevitably fail to fully capture such heterogeneity. In addition, public choice theory doubts that policymakers benevolently pursue efficiency goals. For all these reasons, ex post bargaining is an appealing remedy for regulatory mistake. The regulator may still be concerned about distribution. It may be afraid that transaction costs are too high, or that there is too much uncertainty about preferences. But at least the welfare loss of an inefficient original allocation of property rights is not as pronounced.

In this paper, we aim at extending the domain of the Coase theorem to a situation that is not infrequent in legal reality. We ask whether efficiency-restoring bargaining works even when the regulator does not protect property at all? When absolute property rights are absent, and only relative, contractual rights are enforceable?

Provided (1) individuals maximize standard preferences and this is common knowledge, (2) transaction costs are zero and (3) preferences are common knowledge, efficiency still obtains via ex post bargaining in a rational-choice, game-theoretic framework. Yet one may wonder whether the equivalence between the textbook version of the Coase theorem and our modification still holds if one takes well established behavioral regularities into account.

To test whether behavioral effects are more detrimental to ex post bargaining in our version of the theorem, we compare three situations in the lab: (1) there is an absolute property right, but it is initially allocated to the individual that values the good less (the standard version of the theorem); (2) the good is initially allocated to the individual that values it more, but the other individual may take it at no cost (our version of the theorem – Take treatment); (3) if the two participants do not strike a deal, the good is destroyed (our version of the theorem – Destroy treatment). We have a striking result: there is no discernible difference between these three versions of Coasean bargaining, neither in terms of efficiency, nor in terms of the price at which the individual with lower valuation agrees to assign the good to the other individual. Coasean bargaining is as effective with mere contractual rights as it is with absolute property rights. The striking result obtains even if we frame taking as stealing.

We test the robustness of our result in a further dimension. In the main experiment, the good was assigned by the experimenter. In three additional treatments, we have the original possessor earn the good. Note that this changes the situation. In these treatments we not only test a situation where property is unprotected, but also expose the proceeds from one's own productive effort for everyone to appropriate. With this additional manipulation we find one weakly significant effect: we find fewer trades and thus more inefficiency in *Earned Take*, as compared to *Earned Coase*. The robustness of this effect is, however, questionable. In particular, it disappears when we frame taking as stealing – a framing that would be expected to intensify the effect. In the *Earned Frame* treatment, we do find a significant effect on the offered price: despite having all the bargaining power, the potential taker leaves the person who originally earned the good a somewhat larger share. But this difference in offer size does not seem to affect the number of trades.

In our experiment, we wanted to compare the *Destroy* treatment with the remaining two treatments (of the main experiment). For that reason, we fixed the redemption value for the l player at 0. Had this value been positive, the fairness assessment might have been even more complicated, and hence more bargaining failure might have ensued. Yet neither our theory nor our results let us predict a treatment difference; of course ultimately this is an empirical question that only a new experiment could answer.

It is of interest to examine whether the equivalence result can be extended to arbitrarily large communities. In a setting with a single owner (h) and many potential takers (ls), efficiency requires that all ls grant the owner (h) a contractual right to possess and use the good. If one assumes common knowledge of rationality, this can be achieved under certain conditions (Bar-Gill and Persico 2013). Yet whether, and under what conditions, the empirical equivalence extends beyond the minimum community of two individuals is a question that we leave to our future work. With this paper all we want to claim is: in bilateral relationships the equivalence does not only hold theoretically, but also empirically.

We conclude by stating that we feel in good harmony with *Ronald Coase* (even if not with all of his later readers). In his seminal paper he writes: “Of course, if market transactions were costless, all that matters (questions of equity apart) is that the rights of the various parties should be well-defined and the results of legal actions easy to forecast” (Coase 1960:19). Note that *Coase* does not speak of absolute rights, only of well-defined rights. A complete absence of property also counts as a well-defined (non-)right. We have confirmed, in our experimental setup, the Coasean irrelevance of property.

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Appendix: Instructions

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Instructions

General Instructions

You are now taking part in an experiment. If you read the following instructions carefully, you can, depending on your decisions, earn a considerable amount of money. It is therefore important that you take your time to understand the instructions.

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In the first three parts of the experiment, we calculate your payoff in Experimental Currency Units (ECU). At the end of the experiment, we will convert 1 ECU into 10 Eurocent. Hence 1 € corresponds to 10 ECU.

[Part 1]

Part 1 of the experiment is only played once and not repeated. You are randomly matched with one other anonymous participant. At the beginning of the experiment, both of you receive an endowment of 40 ECU (Experimental Currency Units). You are - again randomly - assigned a role. One of you will have role A, the other will have role B. There is one unit of a good. At stage 1 of the experiment, the good is given to B. If, at the end of the experiment, B is still in possession of the good, the experimenter buys it from her at a price of 0 ECU (i.e. B gets nothing). If, however, at the end of the experiment, A is in possession of the good, the experimenter buys the good from her at a price of 30 ECU.

At stage 2 of the experiment, B may sell the good to A. If a deal is struck, (i) the good is transferred from B to A, (ii) A pays a sum between 0 and 40 ECU, as stipulated by the contract. A and B decide upon conclusion of a contract according to the following procedure: (1) B is allowed to offer to sell the good at a price determined by her. (2) If B has made an offer, A decides whether she accepts or rejects. If she accepts, the price stated by the contract is transferred from A to B, and the good is transferred from B to A. If A rejects the offer, B keeps the good. After the experiment ends, whoever holds the good sells it to the experimenter at the price specified above (30 ECU if A, and 0 ECU if B).

[Part 2]

In the second part of the experiment, participants keep roles. Otherwise, the second part is unrelated to the first part. You are matched with a new randomly selected participant who held the other role. Participants negotiate over splitting 20 ECU. The participant who had the role of B may offer any split, including (a) 20 ECU for herself and 0 ECU for the participant who had the role of A, and (b) 0 ECU for herself and 20 ECU for A. If A accepts, the new endowment is split accordingly. If no deal is struck, both participants receive 0 ECU.

[Part 3]

In the third part of the experiment, participants keep roles. Otherwise the third part is unrelated to the first and second parts. You are again newly matched at random with a participant who held the other role. The participant who had the role of A in the first part of the experiment is given one unit of a new good. The participant who had the role of B in the first part of the experiment is able to take the good. At the end of this part of the experiment, whoever possesses the good receives 20 ECU.

[Part 4]

*** Social value orientation test ***

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At stage 3 of the experiment, B has a chance to take the good from A. At stage 2 of the experiment, A and B may conclude a binding contract. If a deal is struck, the agreed-upon contract would contain two elements: (i) B would give up the ability to take the good at stage 3 of the experiment, and, in exchange, (ii) A would pay B a sum between 0 and 40 ECU, as stipulated by the contract. A and B decide upon conclusion of a contract according to the following procedure: (1) B is allowed to offer that she will refrain from taking the good at stage 3 of the experiment, provided A pays the price she has asked for. (2) If B has made an offer, A decides whether she accepts or rejects. If she accepts, the price stated by the contract is transferred from A to B and the experiment ends. If A rejects the offer, the experiment proceeds to stage 3 and B decides whether to take the good from A. And then the experiment ends. After the experiment ends, whoever holds the good sells it to the experimenter at the price specified above (30 ECU if A, and 0 ECU if B).

[Part 2]

In the second part of the experiment, participants keep roles. Otherwise, the second part is unrelated to the first part. You are matched with a new randomly selected participant who held the other role. Participants negotiate over splitting 20 ECU. The participant who had the role of B may offer any split, including (a) 20 ECU for herself and 0 ECU for the participant who had the role of A, and (b) 0 ECU for herself and 20 ECU for A. If A accepts, the new endowment is split accordingly. If no deal is struck, both participants receive 0 ECU.

[Part 3]

In the third part of the experiment, participants keep roles. Otherwise the third part is unrelated to the first and second parts. You are again newly matched at random with a participant who held the other role. The participant who had the role of A in the first part of the experiment is given one unit of a new good. The participant who had the role of B in the first part of the experiment is able to take the good. At the end of this part of the experiment, whoever possesses the good receives 20 ECU.

[Part 4]

*** Social value orientation test ***

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At stage 3 of the experiment, B has a chance to steal the good from A. At stage 2 of the experiment, A and B may conclude a binding contract. If a deal is struck, the agreed-upon contract would contain two elements: (i) B would forego the possibility to steal the good at stage 3 of the experiment, and, in exchange, (ii) A would pay B a sum between 0 and 40 ECU, as stipulated by the contract. A and B decide upon conclusion of a contract according to the following procedure: (1) B may offer that she will refrain from stealing the good at stage 3 of the experiment, provided A pays the price she has asked for. (2) If B has made an offer, A decides whether she accepts or rejects. If she accepts, the price stated by the contract is transferred from A to B and the experiment ends. If A rejects the offer, the experiment proceeds to stage 3 and B decides whether to steal the good from A. And then the experiment ends. After the experiment ends, whoever holds the good sells it to the experimenter at the price specified above (30 ECU if A, and 0 ECU if B).

[Part 2]

In the second part of the experiment, participants keep roles. Otherwise, the second part is unrelated to the first part. You are matched with a new randomly selected participant who held the other role. Participants negotiate over splitting 20 ECU. The participant who had the role of B may offer any split, including (a) 20 ECU for herself and 0 ECU for the participant who had the role of A, and (b) 0 ECU for herself and 20 ECU for A. If A accepts, the new endowment is split accordingly. If no deal is struck, both participants receive 0 ECU.

[Part 3]

In the third part of the experiment, participants keep roles. Otherwise the third part is unrelated to the first and second parts. You are again newly matched at random with a participant who held the other role. The participant who had the role of A in the first part of the experiment is given 20 ECU. The participant who had the role of B in the first part of the experiment has a chance to steal any number of ECU (between 0 and 20). At the end of this part of the experiment, any ECU possessed by one of the participants are redeemed by the experimenter.

[Part 4]

*** Social value orientation test ***

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Part 1

Part 1 of the experiment is only played once and not repeated. You are randomly matched with one other anonymous participant. At the beginning of the experiment, both of you receive an endowment of 40 ECU (Experimental Currency Units). You are – again randomly - assigned a role. One of you will have role A, the other will have role B. There is one unit of a good. At stage 1 of the experiment, the good is given to A. If the experiment ends without destruction of the good and A is in possession of the good, the experimenter buys it from her at a price of 30 ECU. If the experiment ends without destruction of the good and B is in possession of the good, the experimenter buys the good from her at a price of 0 ECU (i.e. B gets nothing).

At stage 2 of the experiment, A and B may conclude a binding contract. If a deal is struck, (i) it determines whether, at the end of the experiment, A or B is in possession of the good, (ii) A pays a sum between 0 and 40 ECU to B, or B pays a sum between 0 and 40 ECU to A, as stipulated by the contract. A and B decide upon conclusion of a contract according to the following procedure: (1) B is allowed to make an offer, specifying who gets the good and for what price. (2) If B has made an offer, A decides whether she accepts or rejects. If she accepts, the contract is executed: either A keeps the good and pays B the contractually specified price, or A transfers the good to B and B pays A the contractually specified price. If either (a) B has not made an offer or (b) A has rejected the offer, the good is destroyed.

[Part 2]

In the second part of the experiment, participants keep roles. Otherwise, the second part is unrelated to the first part. You are matched with a new randomly selected participant who held the other role. Participants negotiate over splitting 20 ECU. The participant who had the role of B may offer any split, including (a) 20 ECU for herself and 0 ECU for the participant who had the role of A, and (b) 0 ECU for herself and 20 ECU for A. If A accepts, the new endowment is split accordingly. If no deal is struck, both participants receive 0 ECU.

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There is one unit of a good. At stage 1 of the experiment, B earns this good. To that end, on the computer screen B receives a series of 10x10 tables, filled with 0s and 1s. B must correctly indicate the number of 1s in 10 such tables. The following is an example of the task:

0	0	1	1	1	0	0	1	0	0
1	0	0	0	1	1	0	1	1	1
1	0	1	0	1	0	0	0	1	0
0	0	0	1	1	0	0	0	1	0
1	0	0	0	1	1	0	1	1	1
0	1	0	1	1	0	1	0	1	1
1	1	1	1	0	0	0	1	0	1
1	1	1	0	0	1	0	1	0	1
0	1	0	1	0	0	1	1	0	0
0	0	1	1	0	0	1	1	0	1

If, at the end of the experiment, B is still in possession of the good, the experimenter buys it from her at a price of 0 ECU (i.e. B gets nothing). If, however, at the end of the experiment, A is in possession of the good, the experimenter buys the good from her at a price of 30 ECU.

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[Part 2]

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[Part 4]

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1	0	0	0	1	1	0	1	1	1
1	0	1	0	1	0	0	0	1	0
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1	0	0	0	1	1	0	1	1	1
0	1	0	1	1	0	1	0	1	1
1	1	1	1	0	0	0	1	0	1
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If, at the end of the experiment, A is still in possession of the good, the experimenter buys it from her at a price of 30 ECU. If, however, at the end of the experiment, B is in possession of the good, the experimenter buys the good from her at a price of 0 ECU (i.e. B gets nothing).

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[Part 2]

In the second part of the experiment, participants keep roles. Otherwise, the second part is unrelated to the first part. You are matched with a new randomly selected participant who held the other role. Participants negotiate over splitting 20 ECU. The participant who had the role of B may offer any split, including (a) 20 ECU for herself and 0 ECU for the participant who had the role of A, and (b) 0 ECU for herself and 20 ECU for A. If A accepts, the new endowment is split accordingly. If no deal is struck, both participants receive 0 ECU.

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1	1	1	1	0	0	0	1	0	1
1	1	1	0	0	1	0	1	0	1
0	1	0	1	0	0	1	1	0	0
0	0	1	1	0	0	1	1	0	1

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At stage 3 of the experiment, B has a chance to steal the good from A. At stage 2 of the experiment, A and B may conclude a binding contract. If a deal is struck, the agreed-upon contract would contain two elements: (i) B would forego the possibility to steal the good at stage 3 of the experiment, and, in exchange, (ii) A would pay B a sum between 0 and 40 ECU, as stipulated by the contract. A and B decide upon conclusion of a contract according to the following procedure: (1) B may offer that she will refrain from stealing the good at stage 3 of the experiment, provided A pays the price she has asked for. (2) If B has made an offer, A decides whether she accepts or rejects. If she accepts, the price stated by the contract is transferred from A to B and the experiment ends. If A rejects the offer, the experiment proceeds to stage 3 and B decides whether to steal the good from A. And then the experiment ends. After the experiment ends, whoever holds the good sells it to the experimenter at the price specified above (30 ECU if A, and 0 ECU if B).

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In the second part of the experiment, participants keep roles. Otherwise, the second part is unrelated to the first part. You are matched with a new randomly selected participant who held the other role. Participants negotiate over splitting 20 ECU. The participant who had the role of B may offer any split, including (a) 20 ECU for herself and 0 ECU for the participant who had the role of A, and (b) 0 ECU for herself and 20 ECU for A. If A accepts, the new endowment is split accordingly. If no deal is struck, both participants receive 0 ECU.

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[Part 4]

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