Auctioning Payments for Ecosystem Services (PES) Contracts: A Behavioural and Experimental Economic Analysis

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Summary

Ecosystems provide services that contribute to human welfare. Nevertheless, the majority of these services are subject to degradation. This raises the question of designing and implementing effective and sustainable policies for addressing the challenge of conserving (i.e. reducing the degradation of) natural resources and ecosystem services. Environmental and natural resource economics provides a range of policy instruments for managing natural resources. Economic incentive instruments such as PES schemes have become very popular recently. The PES approach consists of translating the external, non-market values of the environment into real financial incentives for local actors (farmers or landholders) and thus stimulate the provision of ecosystem services.

PES programs are widely implemented in both developing and industrialized countries, and include carbon sequestration projects, watershed and biodiversity protection projects, and agri-environmental programs. Although PES has become a popular approach, it is not a panacea, as other environmental policy instruments may be optimal, depending on the policy context. In addition, even if PES might be the appropriate policy instrument to be implemented in a specific context, there are still a number of more detailed policy design issues that should be considered within this PES approach, including payment design and cost targeting features. It remains the norm in most PES programs to offer a single and fixed payment for adopting environmentally friendly practices and make payment contracts available on a "first-come, first served" basis. However, such payment schemes neglect the variation in recipients' opportunity costs. The recipient's opportunity cost is private information that can be used to collect an informational rent (the difference between the payment the recipient receives and her opportunity cost) and this can jeopardize the cost-effectiveness of conservation programs. To address this problem, the use of conservation auctions has been proposed as an alternative approach to allocate PES contracts.

A conservation auction is a process through which a buyer of ecosystem services invites bids from suppliers of ecosystem services for a specified contract and then buys the contracts with the lowest bid. In conservation auctions, the auctioneer may look for allocative efficiency, that is, the selection of farmers with the highest benefit-cost ratio, and budgetary cost-effectiveness, i.e. buying the most units of ecosystem service with a given budget. Auction theory provides some arguments for the adoption of an auction as an instrument for allocating PES contracts.

Experimental studies also provide evidence that conservation auctions to allocate PES contracts are superior to the use of fixed-price schemes. However, a commonly neglected aspect in the conservation auction literature is the analysis of behavioral biases and non-standard preferences of bidders that may influence the performance of conservation auctions. The goal of this PhD thesis is to investigate the role of behavioral economics considerations for the performance of conservation auctions.

This PhD thesis includes, in addition to an introductory chapter, three scientific articles, covering the following content:

(1) Chapter 2 presents the first article, which is called *Self-selection into conservation auctions*. The goal of this paper is to know whether social (distributional) preferences affect subjects' preferences over PES allocation mechanisms like conservation auctions and fixed-price schemes. This article combines both theoretical and experimental approaches to predict and test the research hypotheses on subjects' preferences. The major contribution of this article is to show that conventional and behavioral economics considerations may both play a role for subjects' preferences over PES allocation mechanisms. We find that inequality averse subjects are less likely to sort into conservation auctions than efficiency-minded (altruistic) subjects. We also find that the level of opportunity cost and subjects' fairness perceptions affect sorting into conservation auctions. These results suggest that the analysis of the preconditions for the appropriateness of PES should also involve the acceptance of the allocation mechanism by environmental service providers. In fact, PES is a voluntary approach, which means some farmers may decide to join a PES program and others not, based on their characteristics. Neglecting the analysis of the acceptance of the chosen allocation mechanism may compromise the attractiveness and effectiveness of PES.

(2) Chapter 3 presents the second article, which is called *Impact of fairness considerations for the selection of PES allocation mechanisms*. This paper examines farmers' preferences over PES allocation mechanisms like conservation auctions and fixed-price schemes, and the role of fairness therein. Particularly, the paper distinguishes different fairness dimensions that can influence the acceptance of conservation auctions, such as distributional and procedural fairness. This article presents the results of a field experiment and a survey conducted with 250 farmers in Tori-Bossito, Benin, in the context of payments for agrobiodiversity conservation services (PACS), an agriculture-

related PES. We show that procedural fairness is the most important fairness dimension that is considered by farmers for the acceptance of conservation auctions. We find that farmers who perceive the auction as procedurally fair are more likely to sort into the auction. This result suggests that the choice and implementation of the allocation mechanism of PES should consider the procedural fairness perception of that allocation mechanism. Such perceptions may well vary across different contexts. Thus, it seems important to follow a bottom-up approach identifying fairness perceptions of potential PES recipients before scheme implementation.

(3) Chapter 4 presents the third article with the following title: *Does the performance of conservation auctions depend on the pre-existing institution?* This paper investigates the impact of a pre-existing environmental service price on the economic performance of conservation auctions. It presents the results of a laboratory experiment where subjects experience a fixed-price scheme before participating in a conservation auction. Subjects' behaviors are compared to a control treatment with no pre-existing fixed-price scheme. The contribution of this paper is to show that the pre-existing institution and/or the pre-existing environmental service price can matter for the budgetary cost-effectiveness of conservation auctions, especially in the case of one-shot conservation auctions. We find that the reference price provided by the fixed-price scheme influences subjects' bids in conservation auctions. Moreover, we find that the pre-existence of the fixed-price scheme increases the amount spent per unit of ecosystem service in conservation auctions in the first round. These results suggest that the choice of the allocation mechanism of PES contracts should consider the pre-existing institution. Conservation auctions are more useful when there is no pre-existing fixed-price scheme. In addition, if auction is the chosen allocation mechanism of PES contracts, its implementation should consider a repetitive setting. A repeated-round conservation auction is more likely to perform well, even if there is a pre-existing fixed-price scheme.

This PhD thesis sheds light on the potential of auctions as an institution for allocating PES contracts with a focus on behavioral economics considerations. The findings of these three scientific articles suggest that behavioral economics considerations like social (distributional) preferences and reference-dependent preferences may affect the attractiveness and economic performance of conservation auctions. Policymakers and practitioners should keep them in mind when selecting, designing and implementing PES as policy instrument for conserving natural resources and ecosystem services.

Zusammenfassung

Ökosysteme erbringen wichtige Dienstleistungen, die zum menschlichen Wohlergehen beitragen. Dennoch werden viele dieser Ökosysteme degradiert oder zerstört. Dies wirft die Frage auf, wie eine effektive und nachhaltige Politik zur Bewältigung der Herausforderung der Erhaltung (d.h. der Verringerung der Degradation) von natürlichen Ressourcen und Ökosystemleistungen konzipiert und umgesetzt werden kann. Umweltökonomische Politikinstrumente zum Schutz und Förderung von Ökosystemleistungen insbesondere ökonomische Anreizmechanismen wie Payments for Environmental Services (PES) haben in der Vergangenheit an Bedeutung gewonnen. Der PES-Ansatz besteht darin, die externen, nicht marktwirtschaftlichen Werte der Umwelt in echte finanzielle Anreize für lokale Akteure (Landwirte oder Landbesitzer) zu übersetzen und damit den Schutz und die Bereitstellung von Ökosystemdienstleistungen zu fördern.

PES-Programme werden sowohl in Entwicklungs- als auch in Industrieländern in großem Umfang durchgeführt und umfassen Projekte zur Kohlenstoffbindung, Projekte zum Schutz von Wassereinzugsgebieten und der biologischen Vielfalt sowie klassische Agrarumweltprogramme. Obwohl PES ein sehr populärer Ansatz ist, stellen PES Programme keineswegs ein Allheilmittel dar, da je nach politischem Kontext auch andere umweltpolitische Instrumente optimal sein können. Auch wenn PES das geeignetste politische Instrument für einen bestimmten Kontext sein kann, gibt es immer noch eine Reihe von Ausgestaltungsoptionen welche im Detail betrachtet werden müssen. Dazu zählen unter anderem die Höhe der Geldzahlung und auch das Kosten-Targeting. Letzteres bedeutet eine Verteilung der Gelder auf solche Landwirte, welche möglichst kostengünstig eine Ökosystemleistung bereitstellen können. Im Widerspruch dazu nutzen die meisten PES Programme derzeit standardisierte Fixpreiszahlungen welche nach dem Prinzip "Wer zuerst kommt, mahlt zuerst" vergeben werden. Solche Zahlungsmodelle vernachlässigen jedoch die Variation der Opportunitätskosten der Landwirte welche sich durch die heterogenen Bedingungen der Bewirtschaftung von Ökosystemen in bestimmten Gebieten und durch bestimme Landwirte ergeben. Die Opportunitätskosten des Landwirte sind jene Kosten die aufgrund der Bereitstellung der Ökosystemdienstleistung entstehen. Oftmals ist es jedoch sehr schwierig, die Höhe der Opportunitätskosten einzelner Landwirte zu bestimmen, da diese sehr selten offengelegt werden. Unzureichende Kenntnis über die tatsächlichen Opportunitätskosten kann u.a. dazu führen, dass Zahlungen zu hoch angesetzt werden und somit die Kosteneffizienz

von PES-Programm verschlechtert. Um dieses Problem zu lösen, können Auktionen eingesetzt werden, um eine kosteneffiziente Vergaben von Mitteln und Zahlungen sicher zu stellen.

In einer Naturschutzauktion geben die Landwirte selbst Angebote für die Zahlungen ab und legen damit ihre Opportunitätskosten offen. Diese Vorgehensweise führt zu differenzierten Zahlungen und zur Auswahl jener Landwirte mit dem höchsten Kosten-Nutzen-Verhältnis. Damit kann schlussendlich die Kosteneffizienz eines PES-Programmes gesteigert werden. Die Auktionstheorie liefert damit einige Argumente, welche für den Einsatz von Auktionen als Instrument zur Zuteilung von PES-Zahlungen sprechen. Experimentelle Studien haben ebenfalls bereits gezeigt, dass Auktionen zur Vergabe von PES-Zahlungen effizienter sind als Fixpreismodelle. Ein in der Literatur noch vernachlässigter Aspekt ist, wie verhaltensökonomische Aspekte die Durchführung von Naturschutzauktionen beeinflussen können. Das Ziel dieser Doktorarbeit ist es, die Rolle verhaltensökonomischer Erkenntnisse für die Durchführung von Naturschutzauktionen zur Verteilung von PES-Zahlungen zu untersuchen.

Diese Dissertation umfasst, zusätzlich zu einem einleitenden Kapitel, drei wissenschaftliche Artikel mit folgendem Inhalt:

(1) In Kapitel 2 wird der erste Artikel vorgestellt, welche die Wirkung von sozialen Präferenzen auf die ökonomisch effiziente Ausgestaltung von Auktionen in einem Laborexperiment betrachtet. Das Ziel dieses Artikels ist es herauszufinden, welche Rolle soziale Präferenzen für die Wirkungsweise unterschiedlicher Zuteilungsmechanismen wie eben Naturschutzauktionen als auch Fixpreismodelle, welche nach dem Prinzip ‚Wer zuerst kommt, mahlt zuerst‘ vergeben werden, zu untersuchen. Dieser Artikel kombiniert sowohl theoretische als auch experimentelle Ansätze, um Forschungshypothesen abzuleiten und diese empirisch zu testen. Der Hauptbeitrag dieses Artikels besteht darin, zu zeigen, dass sowohl konventionelle als auch verhaltensökonomische Überlegungen wie soziale Präferenzen für die Wirkungsweisen von Zuteilungsmechanismen eine Rolle spielen können. Wir stellen fest, dass Individuen mit ausgeprägter Ungleichheitsaversion Naturschutzauktionen weniger präferieren als effizienzorientierte (oder sogenannte altruistische) Individuen. Weitere wichtige Determinanten, welche die Akzeptanz unterschiedlicher Zuteilungsmechanismen beeinflussen, sind die Höhe der Opportunitätskosen sowie auch die empfundene Fairness. PES ist ein freiwilliger Ansatz, was bedeutet, dass

Landwirte sich aufgrund ihrer Präferenzen entscheiden können, einem PES-Programm beizutreten oder nicht. Wir schließen aus unseren Ergebnisse, dass Auktionen von Landwirten mit bestimmten Präferenzen bevorzugt werden und dies wiederum die Attraktivität und Wirksamkeit von PES-Programmen im Allgemeinen beeinflussen kann.

(2) In Kapitel 3 wird der zweite Artikel vorgestellt, welcher die Rolle von Fairnesspräferenzen und der wahrgenommen Fairness auf die ökonomische Effizienz von Auktionen in einem Feldexperiment betrachtet. Das Feldexperiment wurde mit 250 Landwirten in Benin, Westafrika im Kontext von einem PES-Programm zur Erhaltung der Agrobiodiversität durchgeführt. Insbesondere beleuchtet dieses Papier verschiedene Dimensionen der Fairness, u.a. distributive und prozedurale Gerechtigkeit, welche die Akzeptanz von Naturschutzauktionen beeinflussen können. Wir stellen fest, dass Landwirte, welche die Auktion als prozedural gerecht empfinden, eher eine Auktion zustimmen. Wir schließen daraus, dass Fairnesswahrnehmungen unterschiedlicher Zuteilungsmechanismen eine wichtige Rolle in der Praxis haben und schlagen vor, solche Fairnesswahrnehmungen potenzieller PES-Empfänger vor der Implementierung eines PES-Programms zu ermitteln, um möglichen Akzeptanzproblemen vorzubeugen.

(3) Kapitel 4 präsentiert den dritten Artikel, welcher die Auswirkungen eines bereits eingeführten Fixpreismodelles und damit einer bereits fixierten Zahlung auf die ökonomische Effizienz von Naturschutzauktionen, welche später eingeführt werden, untersucht. Es wird ein Laborexperiment durchgeführt, in welchen die Probanden vor der Teilnahme an einer Naturschutzauktion zuerst Teil eines PES-Programms mit einem Fixpreismodell sind. Wir vergleichen dieses Verhalten mit einer Kontrollbedingung, in welcher kein Fixpreismodell eingeführt wird. Der Beitrag dieses Papiers besteht darin, zu zeigen, dass ein bereits bestehender Preis für PES für die Kosteneffizienz von Naturschutzauktionen von Bedeutung sein kann. Wir stellen fest, dass der durch das Fixpreismodell vorgegebene Referenzpreis die Gebote der Probanden bei Naturschutzauktionen beeinflusst. Darüber hinaus stellen wir fest, dass die Existenz eines Fixpreismodelles insbesondere Gebote in der ersten Runde erhöht. Dieses Resultat lässt sich u.a. durch referenz-abhängige Präferenzen erklären. Diese Ergebnisse legen nahe, dass bei der Einführung einer Auktion der bereits bestehende Preis berücksichtigt werden sollte. Naturschutzauktionen sind nützlicher, wenn es kein

bereits bestehendes Fixpreismodell gibt. Desweiteren empfehlen wir eine Wiederholung von Auktionen, da unter dieser Voraussetzung Naturschutzauktionen besser funktionieren, selbst wenn es ein bereits bestehendes Fixpreismodell gibt.

Diese Doktorarbeit beleuchtet das Potenzial von Auktionen als Mechanismus für die Vergabe von PES Zahlungen und betrachtet vor allem den Einfluss von verhaltensökonomischen Erkenntnissen auf die Ausgestaltung von Auktionen. Die Ergebnisse der drei wissenschaftlichen Artikel legen nahe, dass verhaltensökonomische Überlegungen insbesondere soziale und referenzabhängige Präferenzen, die Attraktivität und Effizienz von Naturschutzauktionen beeinflussen können. Politiker und Praktiker sollten diese bei der Ausgestaltung und Umsetzung von PES-Programmen im Auge behalten.

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Chapter 1: General Introduction

This PhD thesis sheds light on the role of behavioral economics considerations for the performance of conservation auctions, with a focus on social (distributional) preferences and reference-dependent preferences. For both cases, I use the experimental approach: (*i*) two laboratory experiments with university students in Germany, and (*ii*) a field experiment with farmers in Benin. The aim of this chapter is to first provide an overview of the Payments for Ecosystem Services[1] (PES) concept as a promising approach for environmental conservation. I then provide an overview of conservation auctions as a mechanism to allocate PES contracts and the role of behavioral economic considerations for auction design. Finally, the outline of the thesis, in terms of research questions and objectives, methodology and results is presented.

1.1. Payments for Ecosystem Services (PES)

Ecosystems provide services that contribute to human welfare (IPBES, 2019; Millennium Ecosystem Assessment, 2005). Nevertheless, the majority of these services are subject to degradation (ibid). This raises the question of designing and implementing effective and sustainable policies for addressing the challenge of conserving (i.e. reducing the degradation of) natural resources and ecosystem services. Environmental and natural resource economics provides a range of policy instruments for managing natural resources (Sterner & Coria, 2012). Among these, economic incentive instruments have become very popular recently (Baylis et al., 2008; Schomers & Matzdorf, 2013; Wunder et al., 2008). This thesis focuses on the approach of payments for ecosystem services (PES), as a positive economic incentive approach for environmental conservation.

1.1.1. The PES approach

Figure 1.1 illustrates the logic of the PES approach. Ecosystem managers (farmers or landholders) usually have the choice between alternative land uses or land use practices. For illustration, consider the choice between conversion to pasture (or cropland) and forest conservation. As benefits from conversion to pasture are often higher than those from forest conservation, we expect that ecosystem managers, in the absence of policy intervention, opt for forest conversion. They usually do not consider the environmental costs (such as reduced water services, loss of biodiversity, carbon emissions) of their land use choice to society

[1] In this thesis I use the terms “Ecosystem Services” and “Environmental Services” interchangeably as I refer to PES. Therefore, I do not enter into a debate about semantic differences.

because there is no market price for most environmental goods and ecosystem services. The PES approach consists of translating the external, non-market values of the environment into real financial incentives for local actors (farmers or landholders) and thereby stimulate the provision of ecosystem services (Engel et al., 2008). In other words, the PES approach aims to deal with the problem of market failure resulting from the existence of external effects of production that are not taken into account by ecosystem services providers (Sterner & Coria, 2012). By internalizing the value of ecosystem services into local actors' private land-use decisions via payments[2], PES help incentivize land users to conserve or reduce the degradation of natural resources and provide or move closer to the socially optimal level of ecosystem services. The exact definition of PES is still subject to debate. Here we adopt Wunder's revisited PES concept and define PES as voluntary transactions between service users and service providers that are conditional on agreed rules of natural resource management for generating offsite services (Wunder, 2014).

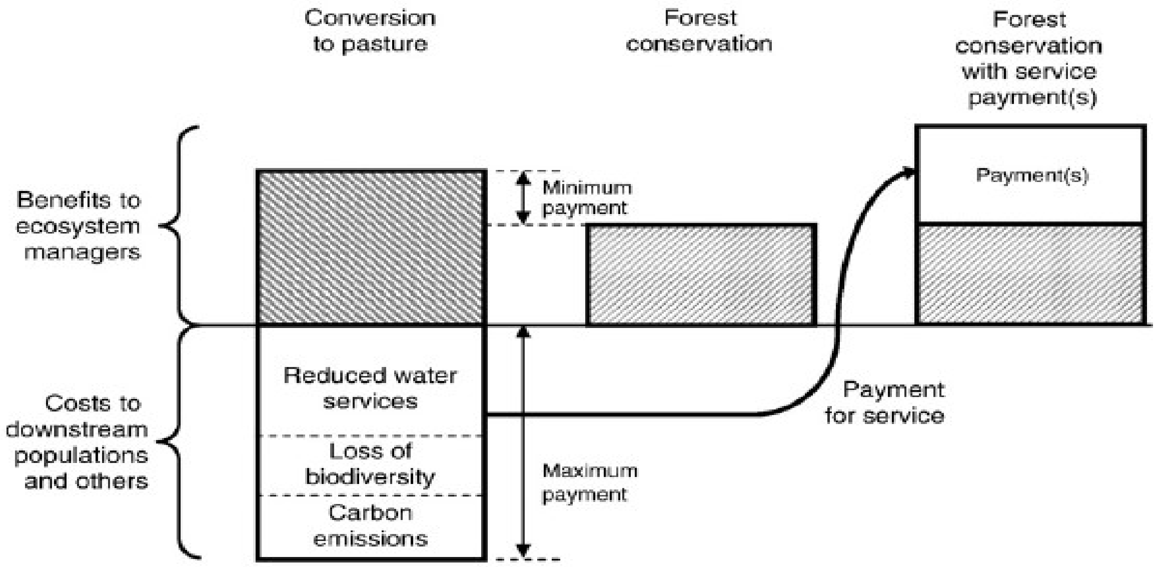

Figure 1.1: The logic of payments for ecosystem services.

Source: Engel, Pagiola, & Wunder (2008).

PES programs are widely implemented in both developing and industrialized countries. In developing countries, the two most well-known PES programs were established in Costa Rica

[2] Payments may come from either beneficiaries (ecosystem service providers), the government, or an international organisation.

and Mexico, in 1997 and 2003, respectively (Alix-Garcia et al., 2009; Porras et al., 2013). In Europe, the Common Agricultural Policy (CAP) now provides payments to farmers choosing to implement conservation efforts that improve the environment and/or maintain the countryside (Schomers & Matzdorf, 2013). In the USA, the Conservation Reserve Program (CRP) has been expanded to focus additionally on wildlife conservation and water and air quality (Hanley et al., 2012). PES programs include carbon sequestration projects, watershed and biodiversity protection projects, (Jindal et al., 2008; Schomers & Matzdorf, 2013; Wunder et al., 2008) and agri-environmental programs (Baylis et al., 2008; Schomers & Matzdorf, 2013). Many carbon sequestration projects stem from voluntary carbon markets, the UN-REDD program, and the clean development mechanism (Jindal et al., 2008; Schomers & Matzdorf, 2013).

PES programs also vary in some characteristics like ecosystem service buyers and sellers, type of activity and performance measures, and payment amount and mode (Engel et al., 2008). When ecosystem service buyers are governments and international organizations, PES programs are called government-financed PES programs. These have been referred to as the Pigouvian concept of PES (Schomers & Matzdorf, 2013), because they resemble a Pigouvian subsidy (Engel et al., 2008). On the other hand, when ecosystem service buyers are the actual users of the ecosystem services, PES programs are called user-financed PES programs. User-financed PES programs have been referred to as the Coasean concept of PES (Schomers & Matzdorf, 2013), because in their purest form they can resemble a Coasean negotiation (Engel et al., 2008). Conditionality is often seen as a defining feature of PES (Ferraro & Kiss, 2002; Wunder et al., 2018). It means that payments should be delivered if and only if the ecosystem service provider secures the service provision or adopts an activity thought to lead to service provision (Wunder, 2007b). However, in practice, PES programs are often weakly monitored (Hart & Latacz-Lohmann, 2005; Naeem et al., 2015; Wunder et al., 2018). Many PES are made on the basis of a particular land use practice rather than actual service provision (Wunder et al., 2008; Engel, 2016). In addition, payments are most commonly made as fixed amounts (e.g. per hectare on which an activity was implemented) (Hanley et al., 2012).

Although PES has become a popular approach, it is not a panacea, as other environmental policy instruments may be optimal, depending on the policy context (Sterner & Coria, 2012). Engel (2016) lists the main considerations for the appropriateness of PES (see figure 1.2). First, PES is an approach to address an environmental externality in situations where the social benefit from the provision of ecosystem services is higher than the costs to ecosystem services providers. Second, PES is appropriate when a 'beneficiary pays' principle is considered appropriate, property rights are clearly defined and well enforced, and monitoring and enforcement capacities are sufficient. However, even if PES might be the appropriate policy instrument to be implemented in a specific context, there are still a number of more detailed policy design issues that should be considered within this PES approach.

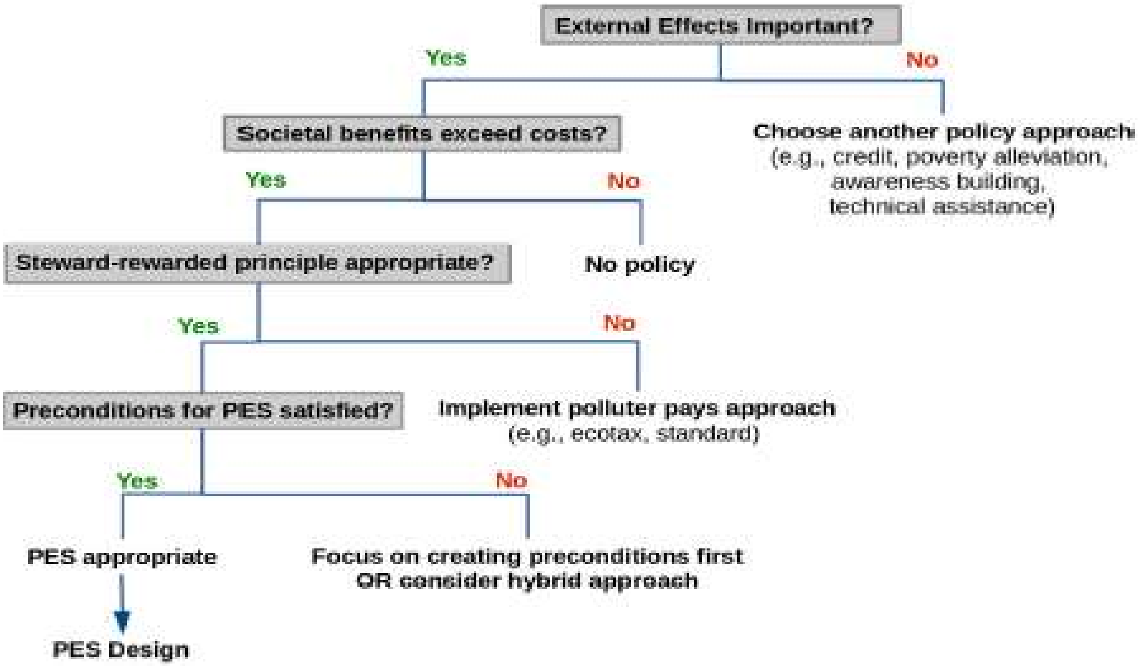

Figure 1.2: Decision guide for determining if PES is the appropriate policy instrument.

Source: Engel (2016).

1.1.2. Issues in PES design

Engel (2016) reviews the main issues in and lessons on the design of PES programs, including, among others, payment design, conditionality, site selection (targeting), and spatial coordination. As this PhD thesis is about conservation auctions as mechanisms to allocate PES contracts, this section focuses on payment design and cost targeting features. Engel (2016) highlights different issues related to these. The first issue in payment design is the choice of the payment amount. In theory, the minimum payment should cover provision and transaction cost, while the maximum payment would encompass the full value of the provision of ecosystem services provided (Engel et al., 2008; see also figure 1.1). However, in practice, due to methodological difficulties in measuring the value of ecosystem services provided, payments are often close to an estimate of ecosystem service providers' opportunity cost (Engel, 2016; Wunder et al., 2008). While most PES are made in cash (Engel et al., 2008; Engel, 2016; Wunder et al., 2008), some PES programs have also implemented in-kind payments or a mixed approach (cash plus in-kind payments) (Wunder et al., 2008). In-kind payments may be suitable when payments are made to groups, in order to avoid elite capture (Engel, 2016) or when the local context does not allow environmental service providers to use cash payments to improve their welfare (Asquith, Vargas, & Wunder, 2008). Payment differentiation is also an important issue concerning payment design. For instance, many existing large-scale PES programs such as the ones targeting tropical deforestation in Costa Rica and Mexico are making fixed payments for practical reasons (Wunder et al., 2008), yet many papers argue that payment differentiation on the basis of provision costs or environmental benefits can potentially improve upon cost-efficiency of such programs and thus might be more desirable from a social welfare perspective (Alix-Garcia et al., 2009; Hanley & White, 2014; Porras et al., 2013; Wünscher et al., 2008). In practice, payment differentiation is desirable if there is considerable variation in provision costs or in environmental benefits (Engel, 2016). Finally, in the design of PES, the environmental agency has the choice to make payments temporarily or indefinitely. Temporary payments are useful if the implementation of the PES program implies short-run costs and becomes profitable later (Pagiola et al., 2004). Otherwise payments should be permanent in order to incentivize ecosystem service providers. Engel (2016) highlights three approaches to secure long-term funding of PES programs: involve firms that benefit from the environmental service provided, link payments to earmarked revenues from user fees or taxes, and invest the available funding in a trust fund and make payments from the interest earned. These approaches have been implemented in France, Costa Rica, and Ecuador, respectively (Pagiola, 2008; Wunder et al., 2008).

Regarding cost targeting features, one issue is how to target farmers' provision costs in face of asymmetric information and reduce farmers' informational rents (the difference between the payment they receive and their provision costs). Ferraro (2008) indicates that conservation agents can take three approaches: (1) acquire information on observable farmers attributes that are correlated with provision costs; (2) offer farmers a menu of screening contracts; or (3) allocate contracts through auctions. Although no single approach dominates, there is a growing literature on the use of auctions to allocate PES contracts (Latacz-Lohmann & Schilizzi, 2005). If an auction is the chosen approach, another issue is its design. It is important to note that an auction can also either make fixed payments (uniform price auction) or differentiate payments (discriminative price auction) (Latacz-Lohmann & Schilizzi, 2005). In both cases, the goal is still to target provision costs, although the understanding of the rules may depend on the type of subjects that participate in the auction. It is also important to take into account the social dimension of auctions, as some farmers seem to like the transparent way by which the auction process identifies recipients of contracts (Jindal et al., 2013). Conservation auctions are discussed in the next section.

Although provision cost targeting may be seen as a single issue, many papers suggest that both provision cost targeting and environmental benefit targeting should be considered. In fact, a combined cost-benefit targeting approach, often also combined with payment differentiation, can improve the cost-effectiveness of a PES program (Engel, 2016; Wünscher et al., 2008).

1.2. Conservation auctions

Section 1.1 had shown that in the contractual relationship between ecosystem service providers and ecosystem service buyers, the PES approach may use different mechanisms to setup payments and allocate contracts. It remains the norm in most PES programs to offer a single and fixed payment for adopting environmentally friendly practices (Jindal et al., 2008; Latacz-Lohmann & Schilizzi, 2005; Porras et al., 2013) and make payment contracts available on a "first-come, first served" basis. (Alpizar et al., 2017; Hanley et al., 2012). However, such payment schemes neglect the variation in recipients' opportunity costs. The recipient's opportunity cost is private information that can be used to collect an informational rent (the difference between the payment the recipient receives and her opportunity cost) and jeopardize the cost-effectiveness of conservation programs (Ferraro, 2008). To address this problem, the use of conservation auctions has been proposed as an alternative approach to allocate PES contracts. The most prominent conservation auction is the U.S. Conservation Reserve Program (CRP), which compensates landowners for removing sensitive cropland from production (Hanley et al., 2012). Other conservation auctions have also been implemented in Australia, Bolivia, Canada, Germany, Ghana, Kenya, Malawi, Peru, Tanzania, UK, etc. (Latacz-Lohmann & Schilizzi, 2005). In this section, I highlight the theory behind auctions and the usefulness of auctions for allocating PES contracts.

1.2.1. Auction theory

Auctions may be seen as trading mechanisms, which are used for allocating resources and purchasing services. Governments are using auctions for selling mobile-phone licenses, privatizing companies, and operating decentralized electricity markets (Klemperer, 1999, 2002). Private actors like antiquarians and online platforms are also using auctions for selling artwork and other goods. Auctions may be designed as either selling auctions or procurement auctions (Klemperer, 2004). In the first case, many buyers compete for buying few units of good or service, whereas, in the second case, many sellers compete for selling few units of good or service.

The literature on auctions mentions four basic auction forms: the ascending-price (English) auction, the descending-price (Dutch) auction, the first-price auction and the second-price auction (Klemperer, 1999; Wolfstetter, 1996). The first two are oral auctions, whereas the latter two are written and sealed-bid auctions (see table 1.1). Wolfstetter (1996) provides definitions of the four basic auction forms. In the ascending-price (English) auction, bids are increasing until all but the highest bidder are eliminated. The item is awarded to the remaining bidder and she pays the indicated price. The descending-price (Dutch) auction is the reverse of the ascending-price auction. The auctioneer starts by asking a specific price, and gradually lowers it until a bidder says ‘Mine’ to claim the item. The item is awarded to the first bidder who claims the item and she pays the indicated price. In a written auction, bidders are invited to submit their sealed bids. The item is awarded to the highest bidder. Under the first-price sealed-bid auction, the winner pays the highest bid, whereas, under the second-price sealed-bid auction, the winner pays the second highest bid.

Auction theory has received many contributions (see Klemperer (1999) for an overview). The early literature on auction theory has focused on the strategic equivalence and the equilibrium of the basic auction forms, using concepts from game theory and some specific assumptions (Griesmer et al., 1967; Vickrey, 1961). For instance, the most used auction model, the symmetric independent private values model, has been solved as a non-cooperative game under incomplete information, and assumes that bidders are risk neutral, only know their own valuations, and unknown valuations are independent and identically distributed (i.i.d) and continuous random variables, while the common value model assumes that the object for sale has the same value for each bidder (Wolfstetter, 1996). The analysis of these models has led to some basic results: the strategic equivalence between the descending-price (Dutch) auction and the first-price auction, the strategic equivalence between the ascending-price (English) auction and the second-price auction, and the revenue equivalence theorem (Wolfstetter, 1996). Therefore, the analysis of the four basic auction forms may be reduced to the analysis of two basic auction forms (the first-price and second-price auctions), and that the four basic auction forms lead to the same expected income for the auctioneer. Another key result of the auction theory indicate that the 'truth-revealing' strategy, which consists of bidding your own valuation, is a (weakly) dominant strategy in the ascending-price (English) and second-price auctions.

Table 1.1: The most popular auctions

Oral	Seal-Bid
ascending price (English)	second-price
descending price (Dutch)	first-price

Source: Wolfstetter (1996).

The recent literature on auction theory has focused on a range of topics, relaxing some assumptions of the early literature. These topics include risk aversion (Maskin & Riley, 1984), correlation and affiliation (Cremer & McLean, 1985; Myerson, 1981), information asymmetry (Milgrom, 1981; Milgrom & Weber, 1982), entry cost and the number of bidders (Levin & Smith, 1994; McAfee & McMillan, 1987), collusion (McAfee & McMillan, 1992; Robinson, 1985), etc. (see Klemperer (1999) for an overview). Although this extensive literature is valuable from a theoretical point of view, some issues are of second-order importance for practical auction design. Klemperer (2002) lists some critical issues in auction design and discusses how to deal with them. Among others, the major concerns for practical auction design are collusion and entry deterrence. The former refers to the fact that bidders may collude to avoid higher bids, whereas the latter refers to the fact that few bidders imply a low level of competition. Both cases imply unprofitability for the auctioneer. For instance, some laboratory experiments looked at tacit collusion in auctions by incorporating a number of facilitating factors: small and same numbers of bidders, pre-play communication and knowledge about the number of units for sale. They found that these factors reduced average prices significantly (Goswami et al., 1996; Philipps et al., 2003; Sherstyuk, 2002). Other experimental studies looked at self-selection into auctions and found that bidding in the auction is lower with endogenous entry (Palfrey & Pevnitskaya, 2008). However, making auctions more robust, using sealed-bid auctions, and enforcing antitrust may help reduce the problems and increase the auctioneer's income (Klemperer, 2002). In most cases, tailoring the design of the auction to the context might be the best way of good auction design (Klemperer, 2002)

In summary, auction theory has received several contributions, from the analysis of basic auction forms to the analysis of issues that matter for practical auction design. Among the theoretical findings, two have received attention in the literature on PES, as they help deal with the problem of asymmetric information between farmers and the conservation agency (Ferraro, 2008). I highlight these here again. First, the 'truth-revealing' strategy, which consists of bidding your own valuation, is a (weakly) dominant strategy in the ascending-price (English) and second-price auctions. Second, even if there is no dominant strategy in other types of auctions, a large number of bidders increases competition and helps reduce bids. Accordingly, using an auction as an instrument for allocating PES contracts may help either to reveal farmers' true opportunity costs or reduce their informational rents (the difference between the payment farmers receive and their opportunity costs) through competition.

1.2.2. Auction as an instrument for allocating PES contracts: theory and experiments

A conservation auction is a process through which a buyer of ecosystem services invites bids (tenders) from suppliers of ecosystem services for a specified contract and then buys the contracts with the lowest bid (Ferraro, 2008a). In conservation auctions, the auctioneer may look for allocative efficiency, that is, the selection of farmers with the highest benefit-cost ratio, and budgetary cost-effectiveness, i.e. buying the most units of ecosystem service with a given budget (Latacz-Lohmann & Schilizzi, 2005). Although the use of conservation auctions is drawn from auction theory, it is important to point out that conservation auctions differ from basic auction forms in many respects (Latacz-Lohmann & Schilizzi, 2005). First, conservation auctions are often multi-item procurement auctions: the conservation agency seeks to buy multiple units of parcels of land (or ecosystem service). Second, the items being traded are often heterogeneous: different parcels of land have different conservation values (or environmental benefits). Third, either the budget is fixed or the number of units of the conservation good (or ecosystem service) to be traded is fixed: in the first case, the budget size is decided upon and known, and in the second case, the number of contracts or hectares of land to come under contract is decided upon and known. Fourth, bidders can bid more than one time (sequential): tenders for the same contracts are invited in a sequence of bidding rounds. Fifth, payment formats may be either discriminatory or uniform: a discriminatory price auction is the multi-unit version of the first-price sealed-bid auction (winners receive as they bid), whereas a uniform price auction is the multi-unit version of the second-price sealed-bid auction (winners receive the same payment, which is either the lowest rejected bid or the highest accepted bid).

Finally, the auctioneer (the conservation agency) is free to set an explicit reserve price: In conservation auctions, the reserve price is the maximum amount of money the auctioneer is willing to pay for a unit of the conservation good (or ecosystem service) being traded (Ferraro, 2008; Latacz-Lohmann & Schilizzi, 2005).

Conservation auctions have been analyzed using theoretical models and laboratory and field experiments (Latacz-Lohmann & Schilizzi, 2005; Schilizzi, 2017). First, some papers analyze bidding behavior in conservation auctions. For instance, Latacz-Lohmann & Van der Hamsvoort (1997) develop a model of optimal bidding for conservation contracts and apply it to a hypothetical conservation program. Assuming that farmer's bidding strategy is guided by the notion of maximum acceptable payment level, above which no payments will be accepted, they find that farmers' optimal bidding behavior is to bid above their opportunity costs and collect an informational rent (the difference between the payment they receive and their opportunity costs). Glebe (2008) furthers the analysis of bidding behavior in conservation auctions, assuming that the bidding strategy is guided by the maximal level of a scoring index that aggregates financial bids and environmental effects. He also confirms that farmers' optimal bidding behavior is to overbid.

Second, some papers analyze the (cost-) effectiveness of conservation auctions. Much research compares conservation auctions with traditionally used fixed-price schemes for allocating PES contracts. For instance, in a laboratory experiment, Schilizzi & Latacz-Lohmann (2007) find that budget-constrained and target-constrained conservation auctions outperform fixed-price schemes, with respect to budgetary cost-effectiveness and informational rent. However, they find that, in repetition, conservation auctions lose their advantage. The finding is also confirmed by Hailu & Schilizzi (2004), using an agent-based model. In a field experiment that allocates subsidies for afforestation on private land in Malawi, Jack (2013) finds that a uniform conservation auction yields better environmental outcomes at lower costs than a comparable fixed-price scheme.

Finally, some studies focus on the evaluation of different design features (payment format, information provision, communication, dynamic entry) that may influence the performance of conservation auctions. For example, Cason & Gangadharan (2005) compare a uniform price auction design to a discriminatory price one and find that a discriminatory price auction design outperforms uniform auction designs with respect to cost-effectiveness, even though a uniform price tends to have better cost-revelation incentives. The role of information (provision) for the performance of conservation auctions has been analyzed using theoretical models and laboratory experiments. Glebe (2013) develops a theoretical model that demonstrates that concealing information about conservation benefits is preferable if entry decisions are not relevant. Cason et al. (2003) confirm this result in a laboratory experiment, showing that information provision about the environmental benefits of conservation actions lowers the cost-effectiveness of the conservation auction used to allocate conservation contracts. However, in another laboratory experiment, Conte & Griffin (2017) show that, in the context of a two-dimensional bid-scoring conservation auction (where landholders bid a conservation action and a price at which they would be willing to undertake that action), information provision about conservation benefits allows sellers to identify and submit higher-quality conservation actions, which improves the cost-effectiveness of conservation auctions. When spatial coordination is needed, some laboratory experiments show that neither information about the spatial goal implemented by the scoring rule nor communication between landholders affect the cost-effectiveness of conservation auctions (Banerjee et al., 2015; Krawczyk et al., 2016). Regarding dynamic entry in conservation auctions, Fooks, Messer, & Duke (2015) find that a multiple-round auction including entry decisions weakens the performance of the conservation auction, in terms of social benefits, when compared to a static setting.

In summary, auction theory provides some arguments for the adoption of an auction as an instrument for allocating PES. Experimental studies also provide evidence that conservation auctions to allocate PES contracts are superior to the use of fixed-price schemes. However, a commonly neglected aspect is the analysis of behavioral biases and non-standard preferences of bidders that may influence the performance of conservation auctions.

1.3. Research issue

Models of agent behavior in environmental and natural resource economics are based on the standard neoclassical economics approach (Shogren & Taylor, 2008), which is a framework that follows core assumptions and offers clear, testable predictions (Dhami, 2016). In recent years, there has been considerable empirical evidence showing that human behavior may deviate from the neoclassical models, leading to a parallel development of a new paradigm: behavioral economics (Thaler, 2016). Behavioral economics is a field of economics that draws on insights from psychology, biology, anthropology, sociology and other social sciences (Dhami, 2016). Insights from behavioral economics are potentially important for conservation auctions, in the sense that behavioral biases and non-standard preferences may influence the performance of conservation auctions. This section presents the research issue. It first highlights the role of behavioral economics for environmental policy design, and identifies the gaps in the literature with respect to the design of conservation auctions. I then present the research objectives of and questions addressed in the thesis.

1.3.1. Behavioral economics and environmental policy

Insights from behavioral economics may affect environmental policy design. Carlsson & Johansson-Stenman (2012) and Shogren & Taylor (2008) provide a survey on implications of insights from behavioral economics for environmental policy. I follow the same approach and highlight some behavioral anomalies that are relevant to this PhD thesis. Environmental and resource economics points out a number of advantages of market-based policy instruments, as compared to command-and-control policy instruments. However, behavioral economics may bring a new perspective into this choice. Insights from behavioral economics indicate that economic incentive instruments such as subsidies (and taxes) may be counterproductive or less effective than expected (Noussair & van Soest, 2014). Particularly, it has been shown that economic incentives may adversely affect individuals' intrinsic motives to contribute to a better environment or serve the public (Carlsson & Johansson-Stenman, 2012; Noussair & van Soest, 2014). In fact, societies in which formal institutions are missing or weak develop and rely on prosocial norms and social preferences to conserve and protect the environment (Cardenas & Carpenter, 2008). Social preferences mean that individuals do not only care about their own welfare but also about other people's welfare (Kerschbamer, 2015). These include evidence for altruism, reciprocity and fairness preferences (or inequality aversion) (Rabin, 1993, Fehr & Schmidt, 1999; Fehr & Schmidt, 2006). Introducing a formal institution like an economic incentive may crowd out these prosocial norms and social preferences (Bowles & Polania-Reyes, 2012). For instance, Reeson & Tisdell (2010) conduct a laboratory experiment with university students and find that social preferences are not maintained in the presence of a market institution. This has implications for the choice and implementation of PES as policy instrument. So far the literature on PES considers PES as an approach for addressing environmental externalities in situations where the societal benefits exceed the costs to ecosystem service providers (Engel, 2016). The PES approach is also chosen when the 'beneficiary pays' principle is appropriate (Engel et al., 2008) and basic preconditions like property rights enforcement and monitoring capacity are satisfied (Engel, 2016). The existence of possible crowding out effects (Rode et al., 2015) raises the importance to identify the pre-existing social norms and preferences, in order to avoid a counterproductive effect of a new policy instrument like PES. An ex-ante evaluation of potential impacts of PES should be conducted that accounts for potential behavioral biases and non-standard preferences. This may be done by running economic experiments (Colen et al., 2016).

Insights from behavioral economics may also be relevant to the design and effectiveness of PES programs. Fairness concerns, especially fairness perceptions[3] and fairness preferences are particularly important to the acceptance and effectiveness of policy instruments. First, people might be more willing to accept a specific policy instrument if it is considered fair in terms of the allocation of social burdens. For instance, Oberholzer-Gee, Bohnet, & Frey (1997) find that the price system (the market) as a mechanism to decide on the siting of nuclear waste facilities is only accepted by the public if the price system is perceived as fair. Second, fairness preference (or inequality aversion), which may be defined as a preference for more equal monetary payoff for all players (Fehr & Schmidt, 2006), can increase cooperation within a policy instrument. For instance, assuming that countries have fairness preferences in the context of climate change, Lange & Vogt (2003) demonstrate that cooperation of a large fraction or even of all countries can establish a Nash equilibrium. Translating into our PES setting, fairness perceptions and fairness preferences have the potential to negatively and positively affect the acceptance and effectiveness of PES programs (Pascual, Muradian, Rodriguez, & Duraiappah, 2010; Pascual et al., 2014) . Research on PES design should help identify key elements that make PES fair. A few recent studies have started to examine these issues with respect to payment distribution and the role of stakeholder participation (Lliso et al., 2020a; Lliso et al., 2020b). They demonstrate that what is perceived as fair is highly context-specific. No studies have examined fairness concerns in relation to PES allocation mechanisms such as auctions.

In summary, behavioral economics may bring a new perspective into environmental policy, especially the choice, design and implementation of PES as environmental policy instrument.

1.3.2. Identifying the gaps in the literature on conservation auctions

A commonly neglected aspect in the conservation auction literature, is the analysis of behavioral economics considerations that could influence the performance of conservation auctions. While the theory of conservation auctions assumes that farmers behave in a rational and self-interested manner (Glebe, 2008; Latacz-Lohmann & Van der Hamsvoort, 1997),

[3] The literature uses the terms “fairness views” and “fairness perceptions”. I do not enter into a debate about semantic differences and use the term fairness perceptions.

insights from behavioral economics indicate that people may exhibit non-standard preferences and, given bounded rationality, may follow non-standard decision-making including among others the use of decision heuristics (Della Vigna, 2009).

First, social preferences could be potentially important for the attractiveness (or acceptance) of conservation auctions by local communities, in the sense that conservation auctions imply different payment rules (i.e. flat fee versus differentiated payments) and a specific selection scheme (i.e. first-come-first serve versus auction), which may attract different types of farmers, based on their social preferences. For instance, in an experiment where subjects face the choice between a fixed and a variable payment scheme, Dohmen & Falk (2011) show that trust[4] affects sorting decisions. Subjects who trust others are likely to prefer the variable payment scheme. In another experiment, Bartling et al. (2009) find that distributional fairness (inequality aversion) also affects sorting decisions. Particularly they find that inequality averse women are less inclined to self-select into competitive environments. To our knowledge, no study has extended this analysis to investigate the role of social preferences for the attractiveness (or acceptance) of conservation auctions.

Besides distributional fairness concerns (inequality aversion as a type of social preferences), the procedural fairness dimension may also be important for the attractiveness of conservation auctions. In a conceptual paper, Pascual et al. (2014) show that PES schemes affect a range of fairness dimensions, including procedural fairness, which may potentially feedback on ecological outcomes. Contrary to the commonly used fixed-price scheme, which allocates PES contracts on a "first-come, first served" or random basis (Alpizar et al., 2017; Jack, 2013), conservation auctions use a competitive selection rule. Which type of allocation mechanism is chosen by the implementing organization thus has potential implications for the distributional and procedural fairness perceptions of the ecosystem service (ES) providers. Likewise, which type of allocation mechanism is preferred by ES providers has potential implications for the effectiveness of the PES program. Understanding those fairness dimensions, including their relative importance, and how farmers' preferences over PES allocation mechanisms are affected by fairness perceptions and fairness preferences could thus be helpful in the design and implementation of PES programs to avoid undermining their social desirability and success.

[4] In this experiment, trust is measured by the amount of money a subject sends to a matched person. It may also be interpreted as conditional altruism.

Second, as noticed in the previous section, behavioral economics may also bring a new perspective into the choice of PES as environmental policy instrument, especially the choice of auctions to allocate PES contracts. In this context, reference-dependent preferences (Camerer & Loewenstein, 2004; Kahneman & Tversky, 1979) could be important for the economic performance of conservation auctions. Reference-dependent preferences refer to the dependence of preferences on a reference point (Camerer & Loewenstein, 2004), and have been found to (adversely) affect many economic behaviors, including, among others, the declaration of taxes (Bruttel & Friehe, 2014), trust and cooperation (Bohnet and Huck, 2004; Bruttel and Friehe, 2011), valuation of housing prices (Einiö, Kaustia, & Puttonen, 2008; Genesove & Mayer, 2001), the time investors hold on to stocks (Weber & Camerer, 1998), and bidding behaviour (Dijk et al., 2013). Reference points may become relevant for the performance of conservation auctions when an implementing agency of a PES program decides to switch from traditional allocation of PES contracts via a fixed-price scheme to the allocation via conservation auctions. In such a case, program participants may use the pre-existing environmental service price as reference point, which in turn may affect their bidding behavior in a subsequent conservation auction. To our knowledge, no study has investigated the impact of a pre-existing environmental service price on the economic performance of conservation auctions.

This PhD thesis aims to address these research gaps. In section 1.3.3, the objectives and research questions of the PhD thesis are provided.

1.3.3. Research objectives of and research questions addressed in the thesis

The PhD thesis investigates the potential of auctions as an institution for allocating PES contracts with a focus on behavioral economics considerations. Figure 1.3 gives an overview of the objectives and research questions.

The **general objective** of the PhD thesis is to investigate the role of behavioral economics considerations for the performance of conservation auctions. It is guided by two specific objectives that focus on social (distributional) preferences and reference-dependent preferences.

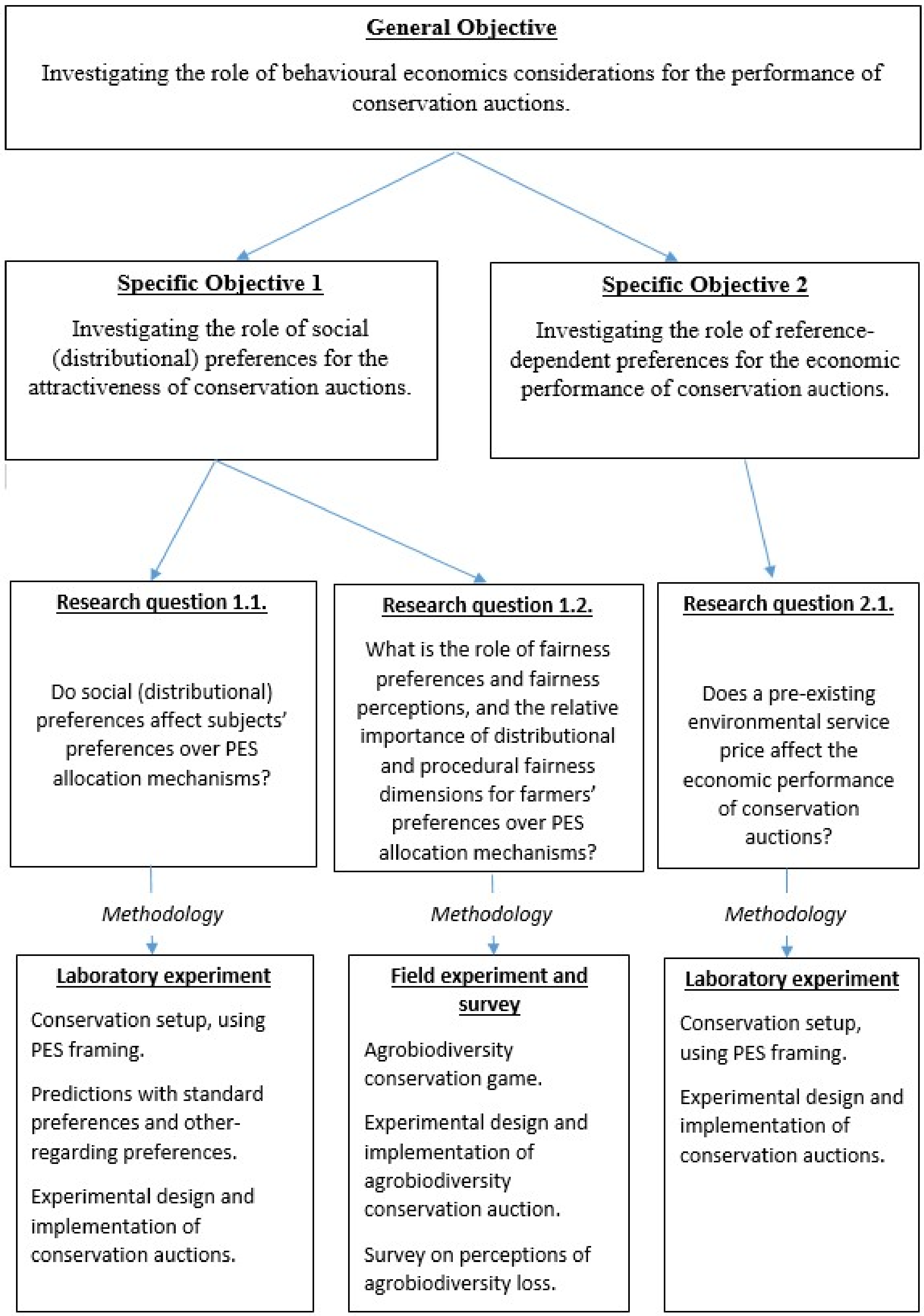

Figure 1.3: Research summary.

The **first specific objective** is to investigate the role of social (distributional) preferences for the attractiveness of conservation auctions. We are interested to answer the following questions:

- **RQ 1.1**: Do social (distributional) preferences affect subjects' preferences over PES allocation mechanisms?
- **RQ 1.2**: What is the role of fairness preferences and fairness perceptions, and the relative importance of distributional and procedural fairness dimensions for farmers' preferences over PES allocation mechanisms?

The **second specific objective** is to investigate the role of reference-dependent preferences for the economic performance of conservation auctions. We would like to answer the following question:

- **RQ 2.1**: Does a pre-existing environmental service price affect the economic performance of conservation auctions?

In order to answer these questions, Chapter 2 presents a theoretical model and conducts an economic lab experiment to test the research hypotheses from (i) standard preferences (neoclassical economic theory), (ii) an alternative assumption of social (distributional) preferences (behavioral economic theory) and (iii) previous experiments. This choice is motivated by the fact of using neoclassical and behavioral economic theories as complementary approaches. However, although the study helps measure the relationship between social (distributional) preferences and preferences over PES allocation mechanisms, it uses university students as subject pool. In addition, it does not focus on a specific type of social preference that may be relevant to the analysis of preferences over PES allocation mechanisms. Chapter 3 furthers the previous study by using farmers as subject pool and focusing on fairness preferences and fairness perceptions and distinguishing between distributional and procedural fairness. We made this choice because the previous study showed that fairness preference (inequality aversion) and fairness perceptions play a role for subjects' preferences over PES allocation mechanisms. In Chapter 3, we disentangle the effect of fairness perceptions, focusing on the distributional and procedural dimensions. The study was conducted with farmers in Benin in the context of payments for agrobiodiversity conservation services, which is an agriculture-related PES. Finally, in chapter 4, we conduct a controlled laboratory experiment with university students, which provides us the opportunity to exogenously vary the existence and level of a pre-existing fixed-price scheme and assess its impact on the economic performance of conservation auctions. In our induced-value and decontextualized experiment we exogenously vary the pre-existing environmental service price, using the midpoint and the upper bound of the distribution of opportunity costs as realistic benchmarks for possible policy price settings. We do so because we assume that policymakers or the implementing conservation agency will have some information about the upper bound and the midpoint of the distribution of opportunity costs, and will use it as starting point to set the payment level.

1.4. Methodology

Our research strategy combines both theoretical and empirical analyses. In this section, I present the method used for collecting our data: the experimental method. I also present the essential elements of our designs of laboratory and field experiments, including the conservation setup used to mimic PES.

1.4.1. The experimental method

Economics has been a non-experimental discipline for a long time (Serra, 2012). However, in the last decades, the experimental method has become an important tool for economic analysis (Plott, 1991). Experiments in economics are usually conducted either in a laboratory or in a field context. In the field context, experiments take the form of social experiments and natural experiments (Harrison & List, 2004; Levitt & List, 2009). In this subsection, I highlight the importance of the experimental method in economics and its main uses.

In most tests of economic models, the economist's goal is to find that a specific variable has a causal effect on another variable. The most common approach is to run an econometric regression of the dependent variable on the independent variable, holding other factors fixed. However, as noticed by Wooldridge (2013), the key question in most econometric studies is: Have enough other factors been held fixed to make a case for causality? The experimental method helps deal with this problem of inferring causality, in the sense that it creates a manageable environment where control can be maintained and accurate measurement of relevant variables guaranteed (Smith, 1982). Experiments may be seen as microeconomic systems, which are divided into two components: the environment and the institution. The environment refers to the set of initial circumstances that cannot be altered by the agents or the institutions in which they interact, whereas the institution defines the rules of the game (Smith, 1982). Accordingly, by fixing the environment and varying the institution or fixing the institution and varying the environment, experiments are more likely to make a case for causality.

Experiments in economics are usually conducted for three purposes (Eber & Willinger, 2005; Smith, 1994). First, experiments help test a theory or discriminate between theories. Theories usually provide predictions, using game theory and decision theory. By varying the environment and fixing the institution, experiments help test theoretical predictions. For instance, Kahneman & Tversky (1979) present a critique of the expected utility model by running experiments that demonstrate several phenomena that violate the expected utility theory. Second, experiments may have an exploratory goal. They help find empirical regularities, in order to use them as a basis for a new theory. For instance, some experiments show that some people may have other-regarding preferences (Dhami, 2016; Henrich et al., 2001; Levitt & List, 2008). Finally, experiments are also used for decision support. They help test the effectiveness of policies. Ex-ante evaluations, which compare different policy options, are conducted with laboratory and (framed) field experiments, while ex-post evaluations, which measure the net impact of a policy, may be done by running randomized controlled trials. For instance, Glennerster & Takavarasha (2013) provide a list of randomized evaluations that test the effectiveness of specific approaches to reducing poverty from Britain to Burkina Faso.

In this PhD thesis, laboratory and field experiments are conducted to discriminate between neoclassical and behavioral economic theories of preferences over PES allocation mechanisms, and to test the economic performance of conservation auctions in the context of a pre-existing environmental service price.

1.4.2. Laboratory experiments

In this PhD thesis, we use two laboratory experiments to answer the first and third research questions. The goal is to design experiments that capture the conservation situation and allow us to observe subjects' behaviors. To capture the conservation situation, we setup a conservation game, similar to the approach used in other papers (Cason & Gangadharan, 2005; Fooks et al., 2015; Schilizzi & Latacz-Lohmann, 2007). Our conservation setup includes two states (the status quo and PES) and two activities (the conventional activity and the environmentally friendly activity). We consider the representative farmer, who can obtain a profit π_0 by implementing the conventional activity. The farmer may switch to an environmentally friendly activity which yields a profit equal to π_1. However, $\pi_0 > \pi_1$, and hence the representative farmer's opportunity cost of replacing the conventional activity with the environmentally friendly one is $\pi_0 - \pi_1$. In order to promote the environmentally friendly activity, the implementing agency can incentivize agents to adopt the environmentally friendly activity through a compensation payment, which we refer to as *PES*. The implementing agency can distribute such payments either via a fixed-price scheme or via a discriminatory auction. This basic setup was the basis for all experiments designed in the course of this thesis. While lab experiments used neutral framing, the field experiment used an agrobiodiversity conservation framing.

Chapter 2 provides an overview of the design and implementation of the first laboratory experiment. In this experiment, we measure subjects' preferences over PES allocation mechanisms. Subjects have to make a choice between two PES allocation mechanisms (a fixed-price scheme vs a discriminatory auction). A decontextualized (abstract) experimental framing is used, in order to avoid that environmental preferences affect subjects' decisions. The experiment consists of three main parts, and is aligned to several empirical studies analyzing sorting of relatively productive agents into different payment schemes (see, for instance, Dohmen and Falk (2011), Larkin & Leider (2012)). In the first part, we elicit risk and social (distributional) preferences, using the certainty equivalence test (CET; Dohmen & Falk, 2011, and Dohmen, Falk, Huffman, & Sunde, 2010) and the equality equivalence test (EET; Kerschbamer, 2015), respectively. We made this choice to measure how risk and social (distributional) preferences may affect subjects' entry choices, as auctions imply a risky choice (subject's bid may lose the auction.) and a specific payment rule non-egalitarian: winners receive as they bid).

Chapter 4 provides an overview of the design and implementation of the second laboratory experiment. In this experiment, subjects experienced the fixed-price scheme before participating in the auction. Comparing behavior to a control treatment with no pre-existing fixed-price scheme, we can test whether a pre-existing environmental service price affects the economic performance of conservation auctions. As in Chapter 2, we use a decontextualized (abstract) experimental framing, in order to avoid that environmental preferences affect subjects' decisions. The experiment consists of two stages. Stage 1 consists of a one-time allocation of PES contracts using a fixed-price scheme, and stage 2 implements the conservation auction. Our two main treatments T1 and T2 only differ in the level of the fixed price offered – either the upper bound or the midpoint of the distribution of opportunity costs, respectively. We do so because – in line with existing PES schemes (Wunder, Engel, & Pagiola,

2008) – we assume that policymakers or the implementing conservation agency will have some information about the upper bound and the midpoint of the distribution of opportunity costs, and will use it as starting point to set the payment level.

Both laboratory experiments took place at the laboratory for economics research (LaER), University of Osnabrück, Germany, using SoPHIE (Hendriks, 2012) as software platform and university students as subjects. In the first experiment, that is Chapter 2, we ran 7 sessions with 20 subjects in each session, which imply a total of 140 subjects (14 groups of 10 subjects), whereas in the second experiment, that is Chapter 4, we conducted 9 sessions with 20 subjects in each session, which imply a total of 180 subjects (i.e. 18 groups of 10 subjects).

1.4.3. Field experiment

In chapter 3, we use a framed field experiment to answer the second research question. The study was conducted in Tori-Bossito, Benin, in the context of payments for agrobiodiversity conservation services (PACS), an agriculture-related PES. Our research strategy consisted of three stages. In the first stage, I conducted a needs assessment, in order to know whether the context was suitable for our field experiment. Based on interviews with local scientists and focus groups with farmers, we came to the conclusion that some villages in Tori-Bossito were facing agrobiodiversity loss, and a PACS program could help incentivize farmers to plant and conserve some neglected crop varieties. Second, we designed the field experiment, which I conduced in Benin. The design started with the conception of a framed conservation game, which captures the context of agrobiodiversity loss. Then, the experiment consisted of three parts: an entry survey, the main experiment, and an exit survey. The entry survey was similar to that of our laboratory experiments, eliciting risk and social (distributional) preferences. However, we used elicitation procedures that were adapted to illiterate farmers (Binswanger, 1980; Kerschbamer, 2015). I also interviewed farmers on their household and agricultural profiles. In the main experiment, farmers received an endowment, which represented the profit from the conventional crop variety and were asked to state the amount they would like to receive for adopting the neglected crop variety. Bids would be ranked and selected from the lowest to highest until the budget was exhausted. Winners would receive their bids, whereas losers would keep their endowment. After experiencing the auction, farmers were asked to vote for their preferred allocation mechanism: auction versus fixed-price scheme. The fixed-price scheme would randomly select winners and would give them the same payment level, whereas auction would select winners on the basis of competition (bids) and would give them different payment levels. Finally, in the last part, I ran an exit survey to collect information about farmers' views of allocation mechanisms (including perceived distributional and procedural fairness) and their perceptions of agrobiodiversity loss. Wale (2011) implemented a similar approach to elicit farmers' perceptions of agrobiodiversity loss in Ethiopia.

The field experiment took place in 5 villages, using CSPRO as software platform and farmers as subjects. The villages and crop varieties were selected in close collaboration with local scientists and agricultural extension experts. We conducted 5 sessions of 10 farmers in each village, implying a total of 250 farmers. Farmers were selected using a simple random sampling strategy. Power calculations and the budget determined the sample size.

1.5. Discussion and conclusions

This section begins by summarizing the results of our research, and drawing the implications for the use of conservation auctions as mechanisms to allocate PES contracts. Then, I consider some of the limitations of the methodology and highlight directions for further research.

1.5.1. Results and policy (design) implications

Research question 1.1 (role of social preferences for subjects' preferences over PES allocation mechanisms).

In Chapter 2, we find that inequality averse subjects are less likely to sort into conservation auctions than efficiency-minded (altruistic) subjects. In addition, we find that the level of opportunity cost and subjects' fairness perceptions also affect sorting into conservation auctions. Our results suggest that conventional (neoclassical) and behavioral economics considerations both play a role for the attractiveness of conservation auctions. This is in line with other papers that study sorting into competitive environments (Bartling et al., 2009; Dohmen & Falk, 2011). Our results have potential implications for the choice and design of PES as environmental policy instrument. The analysis of the preconditions for the appropriateness of PES should also involve the acceptance of the allocation mechanism by environmental service providers. In fact, PES is a voluntary approach (Engel et al., 2008), which means that some farmers may decide to join a PES program and others not, based on their characteristics. Neglecting the analysis of the acceptance of the chosen allocation mechanism may compromise the attractiveness and effectiveness of PES.

Research question 1.2 (role of fairness preferences and fairness perceptions, and the relative importance of distributional and procedural fairness dimensions for farmers' preferences over PES allocation mechanisms).

In chapter 3, we find that only the procedural fairness dimension, especially the procedural fairness perception of PES allocation mechanisms, affects farmers' preferences over PES allocation mechanisms. Farmers who perceive the auction as procedurally fair are more likely to sort into the auction. Our results suggest that procedural fairness is the most important fairness dimension that is considered by farmers for the acceptance of conservation auctions. This is in line with Lliso et al. (2020a) who find procedural fairness to be the most important dimension of fairness in a choice experiment on PES design. Our results have potential implications for the design of PES as environmental policy instrument. The choice and implementation of the allocation mechanism of PES should consider the procedural fairness perception of that allocation mechanism. Such perceptions may well vary across different contexts. Thus, it seems important to follow a bottom-up approach identifying fairness perceptions of potential PES recipients before scheme implementation.

Research question 2.1(impact of a pre-existing environmental service price on the economic performance of conservation auctions).

In chapter 4, we find that the reference price provided by the fixed-price scheme influences subjects' bids in conservation auctions. Moreover, this effect on bids persists by repeating the auction. Our result is in line with studies that find evidence in favor of the price following hypothesis (Corrigan & Rousu, 2006; List & Shogren, 1999). This hypothesis (also called the shaping hypothesis) refers to the fact that agents adjust their bids towards the price observed in a previous market period (Loomes et al., 2003). Moreover, we find that the pre-existence of the fixed-price scheme increases the amount spent per unit of ecosystem service in conservation auctions in the first round. However, all of these effects are significant only when the fixed price is set at a high level, in our case at the upper bound of opportunity costs. Our results suggest that the pre-existing institution and/or the pre-existing environmental service price can matter for the budgetary cost-effectiveness of conservation auctions, especially in the case of one-shot conservation auctions. This has implications for the design of PES as policy instrument. First, the choice of the allocation mechanism of PES contracts should consider the pre-existing institution. Conservation auctions are more useful when there is no pre-existing fixed-price scheme. Second, if auction is the chosen allocation mechanism of PES contracts, its implementation should consider a repetitive setting. A repeated-round conservation auction is more likely to perform well, even if there is a pre-existing fixed-price scheme.

In summary, this PhD thesis sheds light on the potential of auctions as an institution for allocating PES contracts with a focus on behavioral economics considerations. The findings of these three papers suggest that behavioral economics considerations like social (distributional) preferences and reference-dependent preferences may affect the attractiveness and economic performance of conservation auctions. Policymakers and practitioners should keep such considerations in mind when designing and implementing PES as policy instrument for conserving natural resources and ecosystem services.

1.5.2. Limitations and future avenues of research

The methodologies used in this research have several limitations. I discuss the specific limitations of each methodology in more detail in the individual chapters, and highlight future avenues of research. Here the main points are summarized.

Chapter 2 combines both theoretical and empirical approaches to answer the question of the role of social (distributional) preferences for subjects' preferences over PES allocation mechanisms. While the theoretical approach is useful to draw theoretical predictions and research hypotheses, it is important to note that the empirical approach uses a laboratory experiment, which is not designed to capture all aspects of the real situation that farmers face. For instance, the conservation setup assumes that the representative agent has the option to choose between applying to a fixed-price conservation scheme or participate in a conservation auction. We take this approach in order to elicit preferences over the two allocation mechanisms. Yet, in reality, land users will usually be faced with only one allocation mechanism, which they can then decide to participate in or not. In addition, the laboratory experiment does not meet all assumptions of the theoretical model. For instance, the theoretical model assumes that landholders are smoothly distributed over the range of opportunity cost, while the laboratory experiment randomly assigns opportunity costs to subjects. Opportunity costs are uniformly drawn from a specific interval. We take this approach as it is commonly used in the literature on laboratory experiments on conservation auctions (Schilizzi & Latacz-Lohmann, 2007). Finally, the laboratory experiment uses a subject pool of university students and decontextualized (abstract) experimental framing rather than real actors like farmers and field context in the instructions. This approach is sometimes chosen to avoid that environmental preferences affect subjects' behaviors. Further empirical research should examine the choice of allocation mechanisms for real actors like farmers, and in a real context of PES. To some degree this is done in chapter 3. Also, in future research, a laboratory experiment could be designed to deliver a rigorous test of the empirical validity of the theoretical model. It seems also promising to look more closely into the role of fairness perceptions, their determinants and their consequences for behavior under alternative allocation mechanisms. Such effects may be stronger or different among actual farmers than among students. Again, to some degree this is examined in chapter 3, albeit only in one specific context.

Chapter 3 uses a field experiment to answer the question of the relative importance of distributional and procedural fairness dimensions for farmers' preferences over PES allocation mechanisms. Although this study seems to address some limitations of the previous study, especially the analysis of the choice of allocation mechanisms for real actors like farmers, and in the context of a potential PES program, it still has some limitations. First, farmers know that they are participating in an experiment and their behaviors are observed. This may be an incentive to hide their true preferences, although their choices are linked to real payments. Second, farmers will not undertake PES activities on their land. This may limit the stakes of getting selected by a specific allocation mechanism in the field experiment. A real PES program may help increase the stakes and reveal farmers' real behaviors. Third, although fairness perceptions are found to play a role for farmers' preferences over PES allocation mechanisms, we do not directly measure whether this has implications for the success or failure of a PES program. Further research could explore how fairness perceptions of PES allocation mechanisms could feedback on ecological outcomes and affect the effectiveness of PES programs.

Finally, Chapter 4 uses a laboratory experiment to answer the question of the impact of a pre-existing environmental service price on the economic performance of conservation auctions. Although this study raises the importance for policymakers to take the pre-existing institution into account when evaluating the opportunities for implementing a conservation auction, especially a one-shot conservation auction, it has some limitations. First, as in most laboratory experiments, the study uses a subject pool of university students and decontextualized (abstract) experimental framing rather than real actors like farmers and field context in the instructions. As mentioned in Chapter 2, this approach is sometimes chosen to avoid that environmental preferences affect subjects' behaviors. Nevertheless, further empirical research should test the same experimental design with real actors like farmers, e.g. comparing contexts with and without a pre-existing fixed-price scheme. Second, the parametric setting, although close to Schilizzi & Latacz-Lohmann (2007), is not inspired by a real case study, which limits the scope of the findings. Some research could examine the impact of pre-existing fixed-price schemes on conservation auctions in a real case study. This could increase the behavioral effects found in this chapter. It remains to be studied whether the effect can be large enough to override the general performance gains from implementing auctions. In our setting, the auction seemed to still perform better than the fixed price scheme, despite the reference dependence.

Chapter 2: Self-selection into conservation auctions

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2.1. Introduction

Payments for ecosystem services (PES) have become a popular approach to promote the provision of ecosystem services (Engel, Pagiola, & Wunder, 2008). It has been argued that they are able to provide potentially large gains in cost-effectiveness compared to other regulatory policy approaches (Ferraro & Simpson, 2002; Wunder, 2005). Others have argued that many PES schemes lack in terms of realizing these potential gains in cost-effectiveness (Alix-Garcia, de Janvry, & Sadoulet, 2008; Engel, 2016; Hanley, Banerjee, Lennox, & Armsworth, 2012; Wunder et al., 2018; Wünscher, Engel, & Wunder, 2008).

One source of inefficiency in PES programs is due to severe information asymmetries where ecosystem service providers have better information on their opportunity cost of supplying contracted services than do buyers of those ecosystem services (Ferraro, 2008a). This leads to informational rents in the form of payments above the minimum payment necessary to induce conservation activities. One mechanism to address this asymmetry is the use of conservation auctions, instead of the more common fixed price schemes (ibid; Porras, Barton, Miranda, & Chacón-Cascante, 2013) to allocate PES contracts. Informational rents obtained by low-opportunity costs service providers are expected to be lower in conservation auctions than with fixed-price conservation contracts. If the bid is accepted, overbidding results in higher payments in discriminatory price auctions, but overbidding also decreases the probability of one's bid getting accepted (Ferraro, 2008).

However, although conservation auctions may outperform fixed-price schemes in terms of cost-effectiveness (Hailu & Schilizzi, 2004; Latacz-Lohmann & Van der Hamsvoort, 1997; Schilizzi & Latacz-Lohmann, 2007), a commonly neglected aspect concerns the fact that different PES allocation mechanisms (i.e., fixed-price vs. auction) may attract providers with different characteristics. This in turn may have an important bearing on a scheme's overall environmental as well as cost effectiveness. The voluntary nature of PES makes self-selection into a program, based on a specific allocation mechanism, one of the factors determining its successfulness. Some empirical field studies indicate that PES allocation mechanisms, even in settings where those mechanisms are theoretically equivalent, systematically affect who and how many participate in the PES program and how successful those providers are in securing ecosystem service provision (Jack, 2013; van Soest et al., 2018). Differences in outcomes between those equivalent mechanisms have been attributed to behavioral factors including differences in risk and time preferences as well as cognitive ability (van Soest et al., 2018). Other studies indicate that participation rates in conservation auctions are generally low, and especially so in developing countries (Rolfe et al., 2018). Low participation rates may be related to attitudes, opinions or the presence of social norms (ibid).

This paper presents a theoretical model and a laboratory experiment to systematically study the impact of several characteristics of the potential participants of a PES program on sorting decisions into a conservation auction vs. a fixed-pay scheme, and how these effects depend on an important scheme characteristic: the available budget. We adapt the theoretical model by Latacz-Lohmann & Van der Hamsvoort (1997) and use it to develop hypotheses about how the preferred allocation mechanism depends on individual opportunity costs and the conservation budget – and hence on the level of competition for PES contracts. We also discuss how the preference for a specific allocation mechanism is likely to depend on potential PES participants' individual social (distributional) preferences.[5] We use data from a lab experiment conducted with students to examine whether the empirical analysis supports our theoretical hypotheses. Our empirical analysis also examines the impact of further characteristics of potential PES participants, such as fairness perceptions regarding the allocation mechanisms, gender, risk attitudes and personality characteristics.

We find that the level of competition influences the propensity of low- and high-opportunity-cost subjects to select into the conservation auction rather than opting for the fixed-price

[5]Social (distributional) preferences are a type of preference studied in behavioural and experimental economics describing an individual's positive or negative concern for the material well-being or payoff of others. To elicit individual social preferences we use a methodology developed by Kerschbamer (2015) which allows classifying individuals' preferences as efficiency-minded (i.e. positive concern for others' well-being), inequality averse (i.e. positive concern for those who have less; negative concern for those who have more), inequality loving (i.e. positive concern for those who have more; negative concern for those who have less), and spiteful (i.e. negative concern for others' well-being).

conservation contract. With respect to the impact of social preferences, our central finding is that, if relatively few contracts can be awarded (so that competition among landowners is likely to be fierce), inequality averse subjects are significantly less likely to prefer the auction over a fixed-price scheme than, for example, efficiency-minded (altruistic) subjects. We find no differences between male and female subjects in their propensity to select the auction even in highly competitive settings. We also find that risk attitudes and personality characteristics do not affect the propensity to select into the auction, while perceived unfairness of auctions negatively affects the likelihood to choose an auction. Our paper suggests that both economic and behavioral considerations may play a role in determining the relative attractiveness of different PES allocation mechanisms and potentially the effectiveness of PES programs, as the allocation mechanism used attracts different types of subjects. We recommend that policymakers and practitioners account for that when designing and implementing conservation auctions.

The remainder of the paper is organized as follows. Section 2 summarizes related literature. Section 3 introduces the experimental approach and design. Section 4 summarizes our theoretical predictions and research hypotheses. Section 5 presents the experimental results. Finally, in section 6, we discuss the results and conclude the paper.

2.2. Related literature and contribution

A growing body of experimental studies investigate the impact of individual characteristics and preferences on sorting or self-selection[6] of individuals into different economic environments (Lazear et al., 2012). For example, such sorting has been studied for the choice of reward and punishment institutions in social dilemma situations (Bohnet & Kübler, 2005; Decker et al., 2003; Ertan et al., 2009; Guillen et al., 2006; Gürerk et al., 2006; Sutter et al., 2010), labor incentive contracts (Dohmen & Falk, 2011; Eriksson & Villeval, 2008; Bulte et al., 2019) and entry into competitive environments (Balafoutas et al., 2012; Bartling et al., 2009; Camerer & Lovallo, 1999; Niederle & Vesterlund, 2007).

Concerning the selection into incentive contracts, Dohmen & Falk (2011) provide evidence that different incentive schemes (fixed wage schemes, but also variable pay regimes such as piece rate, tournament and revenue sharing) attract individuals with different characteristics as productivity, gender, and willingness to take risks.

[6] We use the term sorting throughout the paper. But it is important to note that other papers use the terms sorting and self-selection interchangeably.

Concerning the selection into competitive environments, evidence on the role of gender seems inconclusive. Niederle & Vesterlund (2007) document that women are less prone to select competitive environments because of attitudes towards competition and lower levels of self-confidence. Kamas & Preston (2010) show that this result continues to hold even when controlling for social preferences, while Balafoutas et al. (2012) show that the gender gap in the willingness to compete disappears when additionally controlling for risk preferences and past performance. Bartling et al. (2009) classify female participants into egalitarian and non-egalitarian preference types and find that egalitarians are less likely to choose a tournament incentive scheme over a simple piece rate. This implies that females who are more pro-social are generally less eager to select into competitive environments.

So far, few have extended such an analysis to other competitive environments such as (conservation) auctions[7], with three exceptions. Fooks et al. (2015) compare self-selection and bidding behavior in a multi-period PES auction with that in a single-round auction. They find that participation decisions differ between dynamic and static conservation auctions. With repetition, more and more participants drop out from the auction, and the subsequent reduction in competition results in increasing informational rents. Although this study investigates subjects' willingness to participate into PES auctions, it does not analyze the impact of individual characteristics and attitudes like risk and social preferences therein, nor does it compare auctions with fixed-price schemes.

Jack (2013) provides field experimental evidence from Malawi in which she compares participant's self-selection into conservation auctions versus a standard fixed-price scheme for promoting afforestation. She finds that landholders with high tree survival outcomes are more likely to self-select into a uniform price auction. Thus a conservation auction is found to yield better environmental outcomes at lower costs than a comparable fixed-price scheme. This suggests that conservation auctions may serve as an effective targeting device to attract landholders who are more "productive" in the long-run. However, Jack (2013) did not test whether self-selection depends on other individual characteristics.

In a study conducted in Ghana, van Soest et al. (2018) build on Jack's (2013) experimental design, with the purpose of explaining the difference in take-up rates between uniform auctions

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[7] It is important to note that conservation auctions differ from basic auction forms in several ways. In other words, even if we fill the research gap related to basic auction forms, the results may not be generalized to conservation auctions.

and theoretically equivalent fixed-price schemes. They find that this difference may be explained by behavioral factors like risk aversion, impatience, impulsiveness, self-determination and loss aversion. Their study is most closely related to ours. However, in our study we consider the impact of various characteristics not considered in van Soest et al. (ibid) in explaining the difference in take-up rates, including both more conventional (neoclassical) and behavioral economic considerations. Indeed, recent economic experiments reveal a mixture of self-regarding and other-regarding preferences that may explain people's behavior (Dhami, 2016).

Our study contributes to the literature in several ways. In light of the above cited literature on sorting into competitive environments, we hypothesize that and test whether social preferences, opportunity costs, gender and fairness perception may also be important explanatory factors. Moreover, we vary budget size to assess how the impact of various characteristics of potential PES participants depends on the level of competition in the PES program. Furthermore, we also develop a theoretical model that allows us to derive predictions on some of the characteristics, such as opportunity costs, and their interplay with the PES program's budget size.

2.3. Theoretical predictions and research hypotheses

We derive theoretical predictions for a model considering standard preferences, that is, a rational and payoff-maximizing agent. Afterwards, we use the model as basis for formulating some research hypotheses, and also formulate additional hypotheses based on the behavioral economics literature.

2.3.1. Theoretical predictions with standard preferences

The predictions with standard preferences are based on a formal model, inspired by Latacz-Lohmann & Van der Hamsvoort (1997) and Glebe (2008), to analyze under what circumstances a landowner prefers to participate in a conservation auction, and when she prefers a fixed-price conservation contract. We use backward induction to derive the predictions about a subject's bid and choice of PES allocation mechanism.

Whether a landowner prefers to participate in a conservation auction or in the fixed payment scheme depends on the expected utility derived from each. Let us use π_0 to denote a landowner's payoffs associated with the conventional activity, and π_1 to denote the direct financial returns she receives when implementing the environmentally friendly activity, with

$\pi_0 > \pi_1$. The opportunity costs of participating in any of the two conservation schemes are hence equal to $\pi_0 - \pi_1 > 0$, and the landowner prefers to continue implementing the conventional activity unless she is offered an additional payment that is at least as high as her opportunity costs of participating. Let N denote the number of landowners.

We assume that landowners have the option to choose between applying to a fixed-price conservation scheme or participate in a conservation auction, but not both.[8] The fixed-price scheme is straightforward. Contracts are offered in which each participating landowner receives a payment equal to z if she implements the environmentally friendly activity. We assume that $z \geq \pi_0 - \pi_1$ for all landowners, so that it is ex ante interesting for each landowner to participate. However, the available budget, B, is limited. In fact, we have $B \leq Nz$, and participation in the scheme is determined on the basis of a lottery. The probability of being accepted is $p_z = B/Nz$. A successful applicant receives payment z if she implements the environmentally friendly activity; all applicants who are not accepted into the program, are not eligible to receive any compensation, and hence implement the conventional activity on their land. A landowner's expected benefits of applying for the fixed-price contract V^{FP} are equal to

$$V^{FP} = p_Z U(\pi_1 + z) + (1 - p_Z) U(\pi_0), \quad (1)$$

where $U(\cdot)$ denote a twice-differentiable *von Neumann-Morgenstern* utility function which is increasing in conservation payoffs.

The landowner can also decide to apply for the conservation program via the auction mechanism. Here, each participant is asked to submit a bid, b; if accepted, the landowner will be paid her bid (b) if she meets the contract requirements regarding the implementation of the environmentally friendly activity. Having received the bids of all auction participants, the auctioneer orders all bids from lowest to highest. Starting from the lowest, she accepts all bids until the budget is exhausted. The expected benefits of participating in the conservation auction depend on both the bid she submits as well as on the probability of her bid getting accepted. Let us use p_b to denote the probability that a bid b gets accepted. The expected benefits of participating in the auction are then equal to

$$V^{AUC} = p_b U(\pi_1 + b) + (1 - p_b) U(\pi_0) \quad (2)$$

[8] Although this is not what we would expect in practice, we take this approach in order to elicit preferences over the two allocation mechanisms.

Two conditions need to hold for a landowner to prefer participating in either the conservation auction or in the fixed-price program: (i) $V^j > U(\pi_0)$, and (ii) $V^j > V^i$, with $i, j = \{AUC, FP\}, j \neq i$. For the conservation auction, participation constraint (i) is trivially met, because it is never in the interest of a landowner to submit a bid that is smaller than the opportunity costs of participating $(\pi_0 - \pi_1)$. In the experiment we ensure that (i) is also met for the fixed-price scheme, by making sure that *z* is larger than the opportunity costs of the highest-cost landowner. We now turn to identifying the conditions under which $V^{AUC} > V^{FP}$, or vice versa. To determine whether $V^{AUC} > V^{FP}$ for different land owners, we first need to determine the optimal bidding strategy. We follow Latacz-Lohmann and van der Hamsvoort (1997) and assume that a landowner's bidding strategy is guided by the notion of *maximum acceptable payment level* β, above which no bids will be accepted. The maximum acceptable bid is unknown to the landowners, but they can form expectations about its value. Let us use $\bar{\beta}$ to denote the landowner's upper limit of what β might be. Her (subjective) probability that a bid *b* is accepted is:

$$p_b(b) = P(b \leq \beta) = \int_b^{\bar{\beta}} f(b)db = 1 - F(b), \quad (3)$$

where $f(.)$ and $F(.)$ are the density and distribution functions of the landowner's expectations about β, respectively. The landowner then chooses bid *b* to maximize her expected utility:

$$\max_b P(b \leq \beta)\, U(\pi_1 + b) + [1 - P(b \leq \beta)]\, U(\pi_0) \quad (4)$$

Rather than solving (4) directly, we follow Latacz-Lohmann and van der Hamsvoort (1997) by rewriting (4) to maximize the landowner's certainty equivalent income associated with participating in the conservation auction, which is equal to the expected returns from participating in the auction minus a risk premium. Rewriting (4) in certainty-equivalent payment levels, we have

$$\max_b P(b \leq \beta)\, (\pi_1 + b) + [1 - P(b \leq \beta)](\pi_0) - RP^{AUC}(b, X)\,, \quad (5)$$

where RP^{AUC} represents the value of the risk premium which, in turn, does not only depend on the bid (*b*) itself, but also of other characteristics of the land owner (including among others her risk preferences and her attitude towards competition), which are captured by vector *X*. Solving (5) we have

$$b^* = \pi_0 - \pi_1 + \frac{1 - F(b^*)}{f(b^*)} - \frac{\partial RP^{AUC}(b^*, X)}{\partial b}\,; \quad (6).$$

Substituting (6) into (5), we have that

$V^{AUC} > V^{FP}$ if and only if

$$p_b(b^*)(\pi_1 + b^*) + [1 - p_b(b^*)]\,\pi_0 - RP^{AUC}(b^*, X) > p_f(\pi_1 + z) + [1 - p_f]\,\pi_0 - RP^{FP}(z, X), \quad (7)$$

where $RP^F(z, X)$ is the risk premium associated with participating in the fixed price conservation lottery.

We now discuss the implications of (7) when the landowner is risk neutral and only cares about the financial consequences of participating in either of the two programs.;.

If the land owner is risk neutral and only cares about the financial consequences of participating in either of the two programs, $RP^{AUC}(b^*, X) = RP^{FP}(z, X) = 0$. In that case, a landowner's optimal bid is

$$b^* = \pi_0 - \pi_1 + \frac{1-F(b^*)}{f(b^*)}. \quad (8)$$

From (8) we have that (i) the optimal bid is always as least as high as the opportunity costs of implementing the environmentally friendly option, and (ii) the optimal bid is an increasing and concave function of these opportunity costs of conservation; see also Latacz-Lohmann and van der Hamsvoort (1997).[9] Furthermore, substituting (8) into (7) and setting $RP^F(f, X) = RP^B(b^*, X) = 0$, we have $V^{AUC} > V^{FP}$ if and only if:

$$\pi_0 - \pi_1 > z - \frac{(1-F(b^*))^2}{f(b^*)p_z} \quad (9)$$

Equation (9) shows – possibly counterintuitively – that the probability of choosing the conservation auction over the fixed price scheme is declining in the opportunity costs of conservation. The intuition is, however, straightforward. For the same budget, the quantity of contracts awarded is larger with discriminatory price auctions, because the informational rents for the landowners with the lowest opportunity costs is always higher with a fixed price scheme. And because of this, the probability of getting accepted for the high-opportunity cost

[9] This follows directly from totally differencing (8) with respect to b and $(\pi_0 - \pi_1)$. The intuition is that the "excess bid" is declining in the probability of not winning the contract. In addition, if we follow Latacz-Lohmann & Van der Hamsvoort (1997) and assume that the bidder's expectation about the bid cap is uniformly distributed in the range $[40.2, 93.8]$, then the simulation of the optimal bid gives an increasing and concave function of opportunity cost.

landowners is higher in the auction, and hence they prefer the auction to the fixed price scheme. This is also illustrated graphically in Figure 2.1.

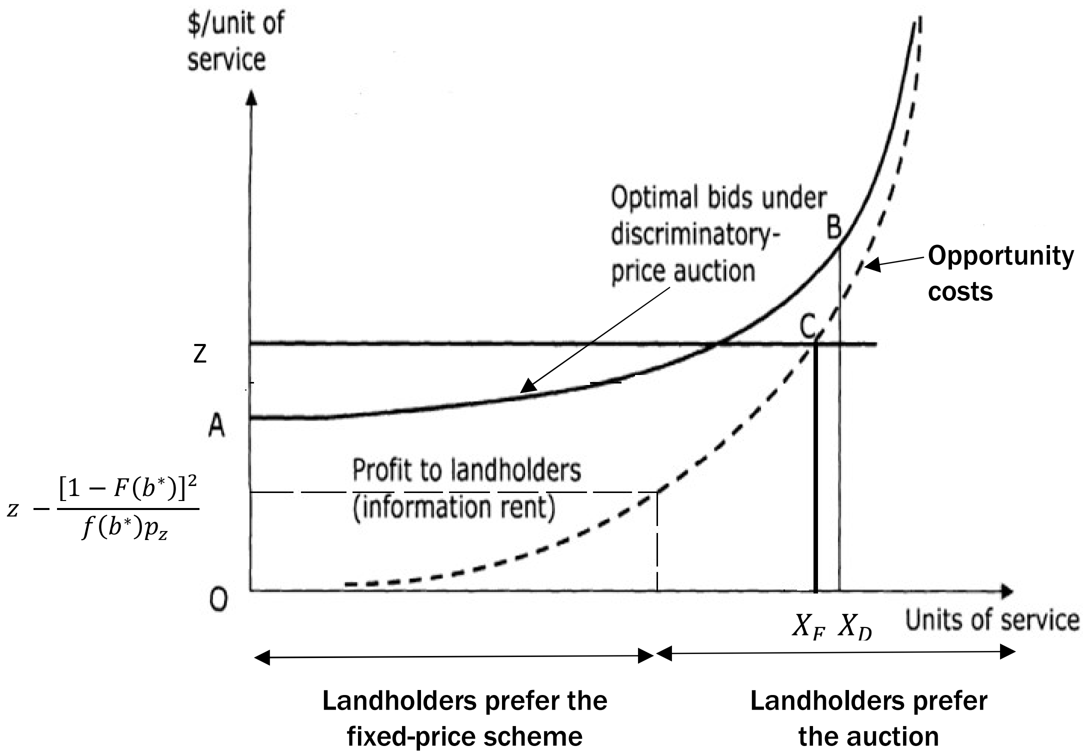

Figure 2.1: Conceptual framework for risk-neutral landholders' preferences over PES allocation mechanisms.

Source: Adapted from Schilizzi & Latacz-Lohmann (2007).

This Figure illustrates the case of risk-neutral landowners, based on inequality (9) and the conceptual framework provided by Schilizzi & Latacz-Lohmann (2007). Assuming that each farmer represents a unit of ecosystem service, with a fixed payment z, X_F farmers (units of ecosystem service) will be selected. The total budget is then represented by the area O-Z-C-X_F. Under the discriminatory price auction, farmers overbid in order to get an informational rent, as predicted by Latacz-Lohmann & Van der Hamsvoort (1997). However, as bidders are accepted in the order of their bids until the budget is exhausted, the auction creates a competitive environment that helps reduce the informational rent farmers can get in the fixed-price scheme. Therefore, assuming that we use the same budget (area O-Z-C-X_F = area O-A-B-X_D), the discriminatory price auction helps selecting more farmers into the program, and hence more ecosystem services (X_D) are obtained with the same budget. Regarding farmers' preferences over PES allocation mechanisms, according to equation (9), figure 2.1 shows a cut-off level of opportunity cost in risk-neutral subjects' preferences. Equation (9) may also help analyze the variation in the share of risk-neutral subjects who choose the discriminatory auction, which may depend on the budget size. For example, if the budget size, which determines the number of contracts to be allocated by the fixed-price scheme, is smaller, $\bar{p}$ is also smaller, so the last term on the right-hand side of equation (9) may increase. Accordingly, the total value of the term on the right-hand side of equation (9) may decrease. However, a smaller budget size may also imply a lower probability that a subject's bid is accepted, which may also increase the total value of the term on the right-hand side of equation (9). Therefore, the share of risk-neutral subjects who choose the discriminatory auction may or may not be affected by the budget size, depending on whether and which one of the two effects dominates: the change in the probability of being selected by the fixed-price scheme or the change in the probability that a subject's bid is accepted.

2.3.2. Research hypotheses

We now formulate four hypotheses which are motivated by (i) the above model based on standard preferences (neoclassical economic theory), (ii) an alternative assumption of other-regarding preferences (behavioral economic theory) and (iii) previous experiments.

Hypothesis 1: Assuming risk neutrality, subjects overbid, whatever the level of opportunity cost.

Our theoretical predictions show that a risk-neutral subject's optimal bid is equal to her opportunity cost plus a premium, so she overbids. Likewise, a risk-neutral subject's optimal bid is a linearly increasing function of the bidder's opportunity cost. In other words, high-opportunity-cost risk-neutral subjects' optimal bids may be higher than low-opportunity-cost subjects' ones, but both overbid. This hypothesis is intuitive in the sense that the best to maximize the payoff is to include an informational rent in the bid.

Hypothesis 2: Assuming risk neutrality, low-opportunity-cost subjects are more likely to prefer the fixed-price scheme, while high-opportunity-cost subjects are more likely to prefer the auction.

Our theoretical predictions show that, under the assumptions of risk neutrality and smoothly distributed subjects over the range of opportunity costs, there is a cut-off level of opportunity cost in subjects' decisions. Below this cut-off level, risk-neutral subjects prefer the fixed-price scheme, while the others prefer the discriminatory auction. This hypothesis is intuitive, in the sense that high-opportunity-cost subjects are close to the fixed-price level. Therefore, they can get more informational rent if they are selected by the auction, while, in the fixed-price scheme, low-opportunity-cost subjects' information rent is higher than that of high-opportunity-cost subjects.

The first two hypotheses are based on standard preferences (neoclassical economic theory). However, recent economic experiments indicate that subjects may have other-regarding preferences, especially social (distributional) preferences (Dhami, 2016). In other words, subjects care about other people's well-being. A large body of literature also indicates that some landholders or communities follow (have) social norms (preferences) like altruism, fairness, and reciprocity (Rode et al., 2015), which may affect their preferences over PES allocation mechanisms. For example, our literature review shows that social (distributional) preferences affect subjects' self-selection into competitive environments (Bartling et al., 2009). Thus, we formulate:

Hypothesis 3: Subjects' preferences over PES allocation mechanisms are affected by their social (distributional) preferences.

Apart from the neoclassical and behavioral economic theories, our literature review shows that gender may play a role for subjects' preferences over PES allocation mechanisms. For example, Niederle & Vesterlund (2007) finds that women shy away from competition and men embrace it. Although our subjects do not compete in a real effort task[10] (as in Niederle & Vesterlund (2007)), it may be relevant to consider the role of gender for subjects' preferences over PES allocation mechanisms, as auction may be perceived as a competitive environment. Thus, we also formulate

Hypothesis 4: Subjects' preferences over PES allocation mechanisms are affected by gender.

2.4. Experimental approach and design

2.4.1. The Conservation Set-up

While our design was presented to subjects in an abstract way to avoid uncontrollable framing effects in the lab, our design is motivated by the challenges posed by implementing PES in the field. Therefore, we explain our design here by using the PES framing. We consider a

[10] In our study, subjects compete for getting their bids selected by the auction mechanism. And opportunity costs are randomly allocated.

representative agent (or farmer), who can obtain a profit π_0 by implementing a conventional activity. The agent may switch to an environmentally friendly activity which yields a profit equal to π_1. However, $\pi_0 > \pi_1$, and hence the representative agent's opportunity cost of replacing the conventional activity with the environmentally friendly one is $\pi_0 - \pi_1$. In order to promote the environmentally friendly activity, an implementing agency can incentivize agents to adopt the environmentally friendly activity through a compensation payment, which we refer to as *PES*. The implementing agency can distribute such payments either via a fixed price scheme or via a discriminatory price auction. Both schemes are budget constrained.[11] The possible monetary consequences of the agent's payoff under one of the two allocation schemes can be seen in Figure 2.2. An agent receives $\pi_1 + b$ if under the discriminatory auction and able to win a compensation payment, where b denotes her bid. Under the fixed-price scheme, the agent receives $\pi_1 + PES$ if a random draw assigns her a PES contract, where PES is the proposed fixed compensation payment. Otherwise the agent obtains π_0, i.e.the payoff of the conventional activity.

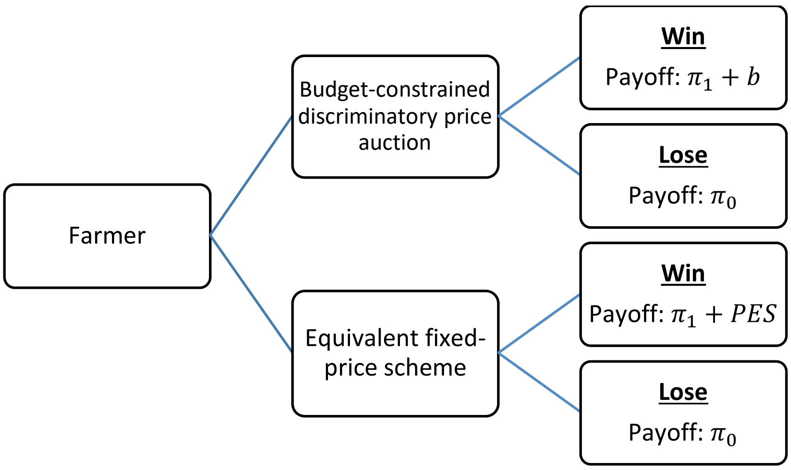

Figure 2.2: Game structure.

2.4.2. Experimental design

The aim of the experiment is to observe how individual attitudes and characteristics affect the sorting decision into conservation auctions vs. a fixed pay scheme and how this is impacted by the level of competition for conservation contracts, which we simulate via the size of the

[11] Here we consider an ex-ante equivalence. This means both allocation mechanisms have the same budget size, that is, a potentially equal budget expenditure.

available budget. The experiment consists of three main parts and subjects go through a total of 8 stages[12] (table A1 in the appendix provides an overview).

Our theory posits that the decision to participate in either of the two schemes depends on a series of preferences. Each experimental session started by eliciting our participants' risk and social preferences. We do so at the start of the session, because these measures are then still unaffected by the outcomes of the conservation game played in part 2.

We use the certainty equivalence test (CET; Dohmen & Falk, 2011, and Dohmen, Falk, Huffman, & Sunde, 2010) to measure risk preferences. In the CET, subjects make 10 choices between either receiving a certain payment or playing a lottery (see Table A2 in the Appendix). The risky lottery remains unchanged for each of the ten choice pairs, but the payment one receives with certainty is increased over the sequence. If subjects' preferences are monotonic, they switch maximally just once, and the later in the sequence the switch happens, the more risk loving (or less risk averse) they are documented to be.

Social preferences are measured using the equality equivalence test (EET; Kerschbamer, 2015). The EET is similar to the CET (see Table A3 in the Appendix). Here, subjects are offered the choice between two proposed allocations of money between themselves and another subject. One such allocation remains unchanged over all ten choice pairs – both the subject herself and the other subject each receive the same number of points. The other allocation changes over the sequence, with the amount allocated to the decision maker increasing over the sequence, and with a fixed amount allocated to the other subject which is either high (in the first 5 choice options), or low (in the last 5 choice options) – allowing the researcher to measure the subject's tolerance for advantageous and disadvantageous payment inequality. The switching pattern in the EET allows classifying participants into major social preference types discussed in the literature including efficiency-minded, inequality averse, inequality-loving, and spiteful. We also ensure that play in part 1 does not affect play in part 2 by only informing the participants of the payoff outcomes of CET and EET at the end of each session.

Part 2 of the experiment introduces the main experiment consisting of the conservation set-up as described in section 2.4.1 and using an experimental auction design adapted from Schilizzi

[12] The experimental sequence is aligned to several empirical studies analyzing sorting of relatively productive agents into different incentive schemes (see, for instance, Dohmen and Falk (2011); Larkin & Leider (2012)).

& Latacz-Lohmann (2007). We use the same distribution of opportunity costs and keep the same degree of competition as Schilizzi and Latacz-Lohmann (2007) but use a decontextualized (abstract) experimental framing. At the beginning of this part, subjects were assigned to groups of 10 participants in order to keep the number of competitors constant across experimental sessions. Each subject was then randomly assigned a different opportunity cost in the form of an individual "Chip" cost. Chip costs were uniformly drawn from an interval between 5 and 264. Subjects were told that chip costs were spread uniformly along the given interval and then asked to make decisions in various stages.

- Stage 2.1: Exogenous Auction – high level of competition. Subjects were asked to submit how much they want to receive in return for their Chips. If a subject's bid is accepted, she is paid the amount she asked for, and bids would be accepted from lowest to highest until the budget was exhausted. Subjects were informed that the available budget was 975 points and that hence not all bids could be accepted. If their claim was accepted, they were paid the difference between their claim and their individual Chip cost.
- Stage 2.2: Endogenous Choice of Auction vs Fixed Pay with strong competition. In this stage, every group member could vote whether she would prefer a rule selecting winners via an auction (i.e., paying subjects their bids starting from the lowest bid, until the budget was exhausted), or a random selection mechanism in which each subject, if selected, was equally compensated with 265 points (i.e. the fixed-pay scheme allocated via a lottery to the number of participants that could be included with the same budget). To determine the payoff-relevant selection rule, a simple majority rule at the group level was applied, while in case of a tie, the selection rule was randomly chosen by the computer. The result was announced only at the end of the experiment.
- Stage 2.3: Exogenous Auction – low level of competition. This stage was identical to Stage 2.1 except that the available budget amounted to 2120 (instead of only 975) and thus the situation in this stage was characterized by a low level of competition among subjects.
- Stage 2.4: Endogenous Choice of Auction vs Fixed pay with low level of competition. This stage is identical to Stage 2.2 except that the available budget amounts to 2120 (instead of only 975).

- Stage 2.5: Repetition of Stage 2.1 to check for order effects. In this stage, we repeat Stage 2.1 to check for potential order effects, as the exogenous auction starts with strong competition. In fact, even if we expect that subjects' behaviors may change under a low level of competition, the way their behaviors change may depend on the first treatment. We test for this potential nuisance by comparing subjects' behaviors (bids) in stage 2.1 with stage 2.5.

To minimize the impact of wealth on decisions and also to minimize the impact of possible ordering effects, subjects were only informed of the (financial) outcomes of each of the 5 stages in part 2 at the end of the session.

Finally, in Part 3, subjects were asked to answer a short socio-demographic survey (including gender, age, education, income). In addition, subjects were asked to indicate their relative fairness perception of both allocation mechanisms and also completed a personal attitudes test in the form of the Big Five (John, Donahue, & Kentle, 1991; John, Naumann, & Soto, 2008)[13].

2.4.3. Procedural details

The experimental sessions took place at the laboratory for economics research (LaER), University of Osnabrück, Germany, using SoPHIE[14] (Hendriks, 2012) as software platform and university students as subjects. We ran 7 sessions with 20 subjects in each session, which imply a total of 140 subjects (14 groups of 10 subjects). Likewise, only one randomly selected stage was relevant for cash payments at the end of the experiment. A typical session lasted about 1 hour and the average cash payment was to 12.90 Euros.

2.5. Empirical Results

Section 2.5.1 presents overall descriptive statistics on the main outcome variables. Section 2.5.2 analyses low and high opportunity cost types' behavior in the auction under low and high level of competition. Subsequently, in section 2.5.3, we analyze subjects' self-selection into the auction and the impact of subjects' preferences and personal characteristics. . We present this analysis across our treatment conditions (low and high level of competition).[15]

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[13] The Big Five personality test is used to characterize participants according to five personality traits including openness (i.e. tendency to be open to new aesthetic, cultural, and intellectual experiences), conscientiousness (i.e. tendency to be organized, responsible, and hardworking), extraversion (i.e. tendency to be more outgoing, gregarious, sociable, and openly expressive), agreeableness (i.e. tendency to act in a cooperative and unselfish manner) and neuroticism (i.e. emotional instability).

[14] Software Platform for Human Interaction Experiments.

[15] Please note that we find that subjects' bids under the low level of competition (stage 2.3) are always higher than subjects' bids under the high level of competition (stages 2.1 and 2.5), i.e., regardless of whether we

2.5.1. Descriptive statistics

Among the 140 subjects who participated in the experiment, nine made inconsistent choices in the risk-preference elicitation task and nine made inconsistent choices in the social preference elicitation task. This leaves a total of 124 subjects with consistent choices in both tasks, which we use for our analysis.[16]

Table 2.1 presents the main descriptive statistics. Panel A reports individual risk preferences and Panel B shows the frequency of social (distributional) preference types. Panel C shows average opportunity costs. Panels D and E show our main outcome variables with respect to auction behavior such as average bids, percentage of winners, and percentage of subjects who vote in favor of an auction. Also, note that all variables are reported for all subjects as well as for male and female participants separately.

The percentage of risk-averse subjects amounts to 84.68% (Panel A), meaning that the majority of our participants can be classified as risk-averse.[17] Comparing risk-aversion across gender, we find no differences between men and women (chi2 test, p = 0.890). We also provide a continuous risk index[18] which ranges between 0.1 for those subjects who always choose the safe payoff in each row of the risk preference experiment and 1 for those subjects who only choose the safe payoff in the last row. The lower the score, the higher the degree of individual risk aversion. The mean value amounts to 0.52 in our sample, while we also find no differences between men and women (Mann-Whitney U test, two-sided, p = 0.425).

Panel B shows the distribution of subjects by social-preference type. We find the majority of our subjects to be efficiency-minded (altruistic), namely 61.29%. Both results (risk aversion and altruism) are not unusual (see table A4 in the appendix). For example, in Balafoutas et al. (2012), subjects have a high degree of risk aversion, and 71.2% of subjects are efficiency-minded. We also find a low proportion of inequality loving subjects (8.87%) and some inequality averse (17.74%) and spiteful (12.10%) subjects in our experiment.

start or end with the high level of competition. We also find no difference in bids between stage 2.1 and stage 2.5. This implies that subjects' (bidding) behavior under the high level of competition is not dependent on the ordering.

16 Results remain robust including also the subjects with inconsistent choices.

17 The switching point from the lottery to the safe payoff is informative of a subject's risk aversion. Since the expected value of the lottery is 50 points, weakly risk-averse subjects should prefer the safe payoffs that are lower than or equal to 50 points.

18 We use the task number where the safe payoff is chosen for the first time to construct a risk index, as used by Balafoutas et al. (2012). We divide the task number in which the subject chooses the safe payoff for the first time by 10.

2.5.2. Auction behavior under low and high level of competition

Table 2.1 also shows average bids. We find that these are not statistically different between male and female subjects, neither in the high (Mann-Whitney U test, two-sided, p = 0.996) nor in the low competition treatment (Mann-Whitney U test, two-sided, p = 0.844). Next, we analyze subjects' bidding behavior. We define low-opportunity-cost subjects as those with below-median opportunity costs[19]. According to hypothesis 1, we expect that subjects overbid, whatever the level of opportunity cost, and that low-opportunity-cost subjects overbid more than high-opportunity-cost subjects. This is supported by the data.

RESULT 1: Subjects overbid, whatever the level of opportunity cost, and low-opportunity-cost subjects overbid more than high-opportunity-cost subjects.

Support for Result 1. Consistent with the theory presented in section 3, we find that subjects overbid, whatever the level of opportunity cost and the level of competition. In other words, subjects' bids are higher than their opportunity costs, whatever the level of opportunity cost and the level of competition (low-opportunity-cost and high level of competition: Wilcoxon matched-pairs signed-ranks test: two-sided test, p = 0.0000; low-opportunity-cost and low level of competition: two-sided test, p = 0.0000; high-opportunity-cost and high level of competition: Wilcoxon matched-pairs signed-ranks test: two-sided test, p = 0.0000; high-opportunity-cost and low level of competition: two-sided test, p = 0.0000).We find that the informational rent (the difference between a subject's bid and her opportunity cost) is higher under the low level of competition than under the high level of competition (two-sided Wilcoxon matched-pairs signed-ranks test, p = 0.016). In addition, as can be seen in figure 2.3, low-opportunity-cost subjects' informational rent is higher than that of high-opportunity-cost subjects, whatever the budget size (high level of competition: two-sided Mann-Whitney U test, p = 0.000; low level of competition: two-sided Mann-Whitney U test, p = 0.000).

[19] The median is 128.

Table 2.1: Descriptive statistics of our experimental subjects.

	All	Men	Women
Panel A: Risk attitude			
Risk index	0.52	0.53	0.52
% of risk averse subjects	84.68	37.10	47.58
Panel B: Social (distributional) preferences			
% of efficiency-minded (altruistic) subjects	61.29	28.23	33.06
% of inequality loving subjects	8.87	6.45	2.42
% of inequality averse subjects	17.74	7.26	10.48
% of spiteful subjects	12.10	1.62	10.48
Panel C: Average Opportunity Cost	124.37 (71.24)	126.96 (74.12)	122.37 (69.42)
Panel D: Auction performance under high competition			
Average bids	206.64 (85.36)	206.5 (76.77)	206.76 (91.99)
% of winners	58.06	23.39	34.67
% of subjects who choose the auction	57.26	27.42	29.84
Panel E: Auction performance under low competition			
Average bids	310.83 (212.34)	294.94 (110.72)	323.09 (265.72)
% of winners	77.42	33.87	43.55
% of subjects who choose the auction	45.16	17.74	27.42

Note: Standard deviations in parentheses. Risk index captures subjects' risk aversion. The lower the score, the higher the degree of individual risk aversion.

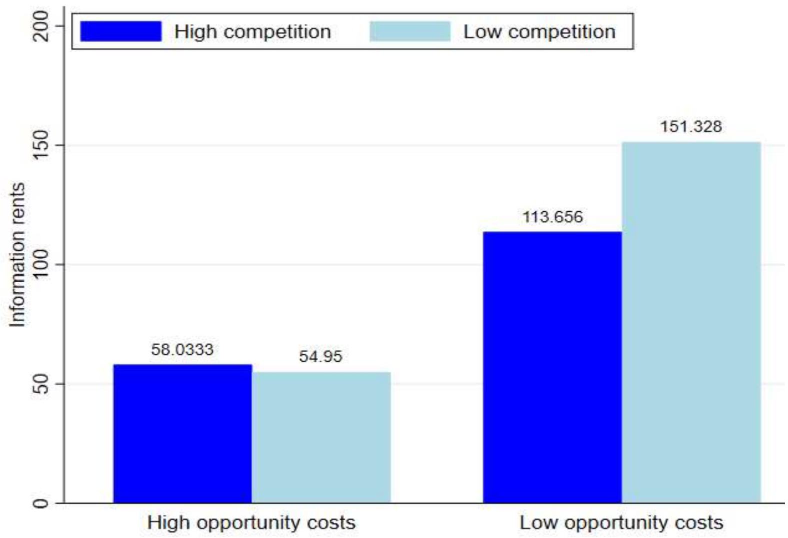

Figure 2.3: Average information rents per type of opportunity cost and level of competition.

2.5.3. Self-selection into the auction and the impact of subjects' preferences and personal characteristics

The impact of opportunity costs under low and high level of competition

We first study the role of individual opportunity costs for the choice in favor or against entering an auction. According to hypothesis 2, one should expect, under the assumption of risk-neutrality, that the low-opportunity-cost subjects are less likely to prefer the auction. This is, however, not supported by the data.

RESULT 2.1: Under the high level of competition, low-opportunity-cost subjects are more likely to prefer the auction than the fixed-price scheme, while high-opportunity-cost subjects are equally or more likely to prefer the fixed-price scheme than the auction.

Support for Result 2.1. Figure 2.4 compares subjects' preferences in the lower half of the distribution of opportunity costs with the higher half of the distribution, under high level of competition. We find that, in the lower half of the distribution of opportunity costs (equal or below the fifth decile or median, 128), the proportion of subjects who choose the auction (64%) is significantly higher than the proportion of subjects who choose the fixed-price scheme (36%) (Binomial test: two-sided test, $p = 0.033$). In contrast, in the higher half of the distribution of opportunity costs (above the fifth decile or median, 128), the proportions of subjects who choose each allocation mechanism are around 50%. We also find that, among the subjects who choose the auction, the proportion of low-opportunity-cost subjects (58%) is higher than the proportion of high-opportunity-cost subjects (42%), although the difference is not statistically significant (Binominal test: one-sided test, p = 0.118). In addition, we also find that, among the subjects who choose the fixed-price scheme, the proportion of low-opportunity-cost subjects is lower (43%), although the difference is not statistically significant (Binomial test: one-sided test, p = 0.205).[20] This result suggests that the proportion of subjects who prefer the auction is higher among low-opportunity-cost subjects than among high opportunity cost subjects. Furthermore, an econometric analysis of the impact of the level of opportunity cost on subjects' preferences over both allocation mechanisms (auction and fixed-price scheme) confirms our result. Using Probit regression models (see Table 2.2, Models 1 and 2), we find that the coefficient estimates for *Opportunity Cost* indicate that subjects who have higher opportunity costs are significantly less likely to sort into the auction.

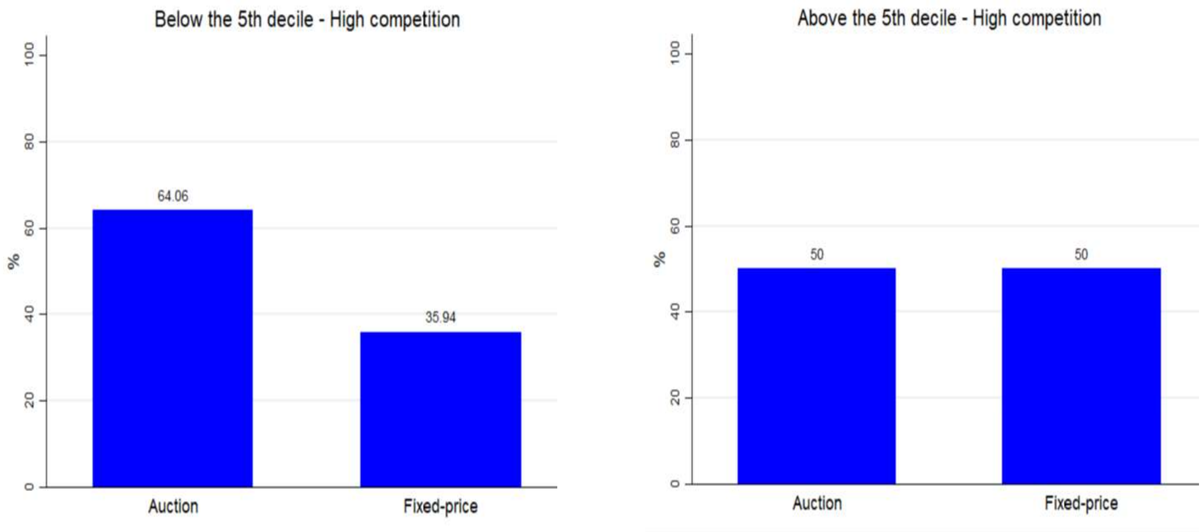

Figure 2.4: Subjects' preferences over allocation mechanisms under high level of competition below and above the median of opportunity costs.

[20] Our results are similar when we analyse all other deciles below and above the median: below the fourth and third deciles, the proportions of subjects who prefer the auction (69% and 79%, respectively) are significantly higher than the proportions of subjects who prefer the fixed-price scheme (binomial test: two-sided test, p = 0.0109 and 0.0004, respectively); below the second and first deciles, the proportions of subjects who choose the auction (81% and 87%, respectively) are significantly higher than the proportions of subjects who choose the fixed-price scheme (binomial test: two-sided test, p = 0.0024 and 0.0073, respectively); above the sixth and seventh deciles, the proportions of subjects who prefer the auction (43 % and 36%) are lower than those who prefer the fixed-price scheme (binomial test: one-sided test, p = 0.1958 and 0.0662); above the eighth and ninth deciles, the proportions of subjects who prefer the auction (30% and 42%, respectively) are lower than those who prefer the fixed-price scheme (binomial test: one-sided test, p = 0.0465 and 0.3872, respectively).

Our results lead us to reject our second research hypothesis. However, as stated in the section on research hypotheses, we may have a different result in the context of risk aversion. So given that the majority of our subjects are risk-averse, our result is not in contradiction to the theoretical model.

Table 2.2: Self-selection into Auctions: Regression Models.

	High competition		Low competition	
	Model 1	*Model 2*	*Model 3*	*Model 4*
Opportunity cost	-0.002***	-0.002***	-0.0004	-0.0002
	(0.000)	(0.000)	(0.001)	(0.001)
Gender	0.131	0.097	-0.068	-0.073
	(0.092)	(0.111)	(0.091)	(0.104)
Risk aversion Index	-0.577	-0.327	-0.679*	-0.539
	(0.354)	(0.384)	(0.358)	(0.377)
IAV		-0.296**		-0.115
		(0.124)		(0.124)
ILO		0.002		0.214
		(0.173)		(0.162)
SPI		-0.122		0.031
		(0.161)		(0.150)
Fairness perception		0.232**		0.074
		(0.095)		(0.098)
Personality traits	NO	YES	NO	YES
Constant	YES	YES	YES	YES
Observations	124	124	124	124
Pseudo R-squared	0.086	0.164	0.028	0.052
Prob > chi2	0.002	0.006	0.196	0.719

Note: The table reports Probit marginal effect estimates (standard errors in parentheses). The dependent variable is a binary variable that takes the value 1 if a subject perceives the auction fairer than the fixed-price scheme and 0 otherwise. Opportunity cost refers to the amount of subjects' opportunity costs. Gender is a binary variable that takes the value 1 if the subject is male and 0 otherwise. Risk aversion index captures subjects' risk aversion. The lower the score, the higher the degree of individual risk aversion. IAV, ILO and SPI are dummy variables for inequality averse, inequality loving and spiteful subjects, respectively. The coefficients indicate changes from the baseline type (efficiency loving). Fairness perception is a binary variable that takes the value 1 if the subject perceives the auction as fair and 0 otherwise. Personality traits refer to whether the Big Five traits (openness, conscientiousness, extraversion, agreeableness and neuroticism) are included as control variables. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

The impact of budget size (or the level of competition)

We examine the impact of budget size on subjects' preferences over PES allocation mechanisms. Our theoretical model does not permit a conclusion on the direction of the effect. Our empirical results indicate the following.

RESULT 2.2: The proportion of subjects choosing the auction is higher under the high level of competition than under the low level of competition.

Support for Result 2.2. We find that, in total, 57% of all subjects chose the auction in a high competitive setting (binomial test: one-sided test, $p = 0.063$), while 45% choose the auction in a low competitive setting (binomial test: one-sided test, $p = 0.879$). The proportion of subjects preferring the auction in the high competitive treatment is weakly significantly higher (at 10% level) than the proportion who prefer the auction in the low competitive treatment (McNemar test, one-sided, $p = 0.059$).

Next, we show how the subjects who prefer the auction can be characterized with respect to individual opportunity costs, distributional and risk preferences, as well as gender and personality traits.

RESULT 2.3. Under the low level of competition, the proportions of subjects choosing the auction and the fixed-price scheme are equal, both for low- and high-opportunity-cost subjects.

Support for Result 2.3. Figure 2.5 compares subjects' preferences in the lower half of the distribution of opportunity costs with the higher half of the distribution under low level of competition below and above the median opportunity cost (128). We find that, in the lower half of the distribution of opportunity costs, the proportion of subjects who choose the auction (48%) is not statistically different from those who choose the fixed-price scheme (52%) (Binomial test: two-sided test, $p = 0.901$). Likewise, in the higher half of the distribution of opportunity costs, the proportion of subjects who prefer the auction (42%) is not statistically different from the proportion of subjects who prefer the fixed-price scheme (58%) (Binomial test: two-sided test, $p = 0.245$).[21] These results indicate that subjects are indifferent between

[21] Results are similar across other decile comparisons: below the fourth, third, second and first deciles, the proportions of subjects who prefer the auction are not statistically different from those who prefer the fixed-price scheme (binomial test: two-sided test, $p = 0.7797$, 1.0000, 0.8450 and 0.3017, respectively). Likewise, above the sixth, seventh, eighth and ninth deciles, the proportions of subjects who choose the auction are not

both allocation mechanisms, whatever their opportunity costs. Using Probit regression models, we also find that the opportunity cost no longer affects subjects' preferences over PES allocation mechanisms under the low level of competition (see Table 2, Models 3 and 4).

The impact of social (distributional) preferences, gender and general fairness perception

Other potential drivers apart from opportunity costs may affect individual self-selection into auctions. We present results on the self-selection of subjects with respect to their social (distributional) preference type, gender and general fairness perception.

Social (distributional) preferences

We first discuss our hypothesis 3 that social preferences affect sorting decisions.

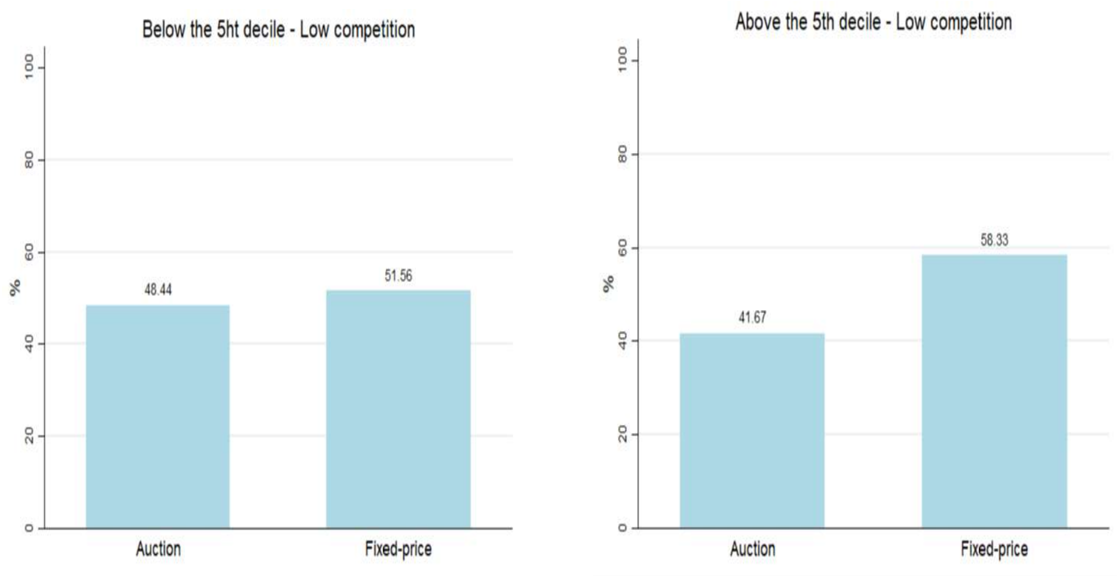

Figure 2.5: Subjects' choices in % over allocation mechanisms under low level of competition below and above the median of opportunity costs.

RESULT 3: Under the high level of competition, inequality averse subjects are significantly less likely to prefer the auction than efficiency-minded subjects. This effect is not significant under the low level of competition.

Support for Result 3: Table 2.3 displays the distribution of social preferences types broken down by treatment and preference for one over the other allocation mechanism. We find that,

statistically different from those who choose the fixed-price scheme (binomial test: two-sided test, p = 0.2528, 0.2429, 1.0000, and 0.3876, respectively).

under the high level of competition, inequality averse subjects are less likely to choose an auction (36%) rather than a fixed-price scheme, but this result is not statistically significant (Binomial test: two-sided test, p = 0.286). However, inequality averse subjects are significantly less likely to prefer the auction than efficiency-minded subjects, which constitute the majority of our subjects. The Probit regressions in Table 2.2 include dummy variables for inequality averse, inequality loving and spiteful types, while efficiency-minded (altruistic) types constitute the reference group. The regressions confirm result 5. The coefficient estimates for inequality averse subjects indicate that those subjects are significantly less likely to prefer auctions in high competitive settings (Model 2) than efficiency-minded ones. We find no such differences for the low competition setting (Model 4). These results confirm our hypothesis 3. However, the type of social preference that drive subjects' preferences over PES allocation mechanisms may depend on the level of competition and reference group.

Table 2.3: Social (distributional) preferences and allocation mechanism preference across treatments.

	High level of competition		
	Auction	Fixed-price scheme	Total
Efficiency loving	48 (63%)**	28 (37%)	76
Inequality averse	8 (36%)	14 (64%)	22
Inequality loving	7 (64%)	4 (36%)	11
Spiteful	8 (53%)	7 (47%)	15
Total	71 (57%)*	53 (43%)	124
	Low level of competition		
	Auction	Fixed-price scheme	Total
Efficiency loving	34 (45%)	42 (55%)	76
Inequality averse	7 (32%)*	15 (68%)	22
Inequality loving	7 (64%)	4 (36%)	11
Spiteful	8 (53%)	7 (47%)	15
Total	56 (45%)	68 (55%)	124

Note: The table reports subjects' preferences over PES allocation mechanisms, across social preference types. We use a two-sided binomial test to examine, for each type of social preference, whether the proportion of subjects who choose the auction is different from those who choose the fixed-price scheme. ***Significant at the 1 percent level. ** Significant at the 5 percent level. *Significant at the 10 percent level.

Gender

We proceed to discuss our hypothesis 4 that gender affects sorting decisions.

RESULT 4: Subjects' preferences over PES allocation mechanisms are not affected by gender.

Support for Result 4. According to hypothesis 4, subjects' preferences over PES allocation mechanisms are driven by gender. In fact, some studies have found evidence that women are less likely to enter competitive environments[22] (for instance, Niederle & Vesterlund, 2007). Such differences have been largely related to women being more risk averse (Dohmen & Falk, 2011) and social (distributional) preferences, which differ largely between men and women (Balafoutas et al., 2012). Looking at the data, we find that the share of men and women choosing the auction is very similar under the high level of competition (27% men, 29% women), while women are more likely to choose the auction under the low level of competition (17% men, 27% women) (McNemar test, one-sided, p = 0.023). Our regressions controlling also for opportunity costs, social (distributional) preferences, risk attitudes, personality traits and fairness perception show no effect of gender across specifications, which confirms that gender may affect entry into competitive environments through factors like risk aversion, social (distributional) preferences and personality traits.

In addition, we find that women's preferences over PES allocation mechanisms are not affected by the level of competition (two-sided McNemar test, p = 0.612), while men show a tendency to change their preferences from auction to fixed-price scheme when the level of competition decreases (two-sided McNemar test, p = 0.023). This seems to indicate that men are more willing to compete when the level of competition is high. This may also explain why men are more willing to select the competitive environment in some contexts, especially in highly competitive contexts.

Fairness perception

Interestingly, a simple perception question which of the two mechanisms subjects perceive to be fairer is a strong predictor of subject's entry into auctions. In fact, subjects who indicate the auction as the fairer mechanism are significantly more likely to sort into a conservation auction than subjects who perceive the fixed-price scheme as the fairer mechanism. This effect is large;

[22] Please note that in Niederle & Vesterlund (2007) subjects compete in a real effort task, while, in our study, opportunity costs are randomly allocated and subjects compete for getting their bids selected by the auction mechanism.

subjects who perceive the auction as the fairer mechanism are about 20% more likely to select into an auction.

2.6. Discussion and conclusions

We find that, as predicted by theory, subjects overbid in the auction. As to subjects' preferences over alternative allocation mechanisms, our results suggest that conventional and behavioral economics considerations may both play a role. Budget size affects the level of competition and has an influence on sorting decisions. The proportion of subjects choosing the auction is higher under the high level of competition than under the low level of competition. Moreover, under a high level of competition, subjects' preferences over alternative allocation mechanisms depend on their level of opportunity cost. Specifically, under a high level of competition (low budget size), low-opportunity-cost subjects are more likely to prefer the auction, while high-opportunity-cost subjects are more likely to prefer the fixed-price scheme.

Social (distributional) preferences also affect subjects' choice of conservation auctions. Specifically, we find such an effect for the setting with a high level of competition only. There, inequality averse subjects are more willing to enter auctions than efficiency-minded (altruistic) subjects. Furthermore, we do not find a gender effect on subjects' preferences over PES allocation mechanisms, but a fairness perception effect. Subjects who indicate the fixed-price scheme as the fairer mechanism are significantly less likely to prefer a conservation auction than subjects who perceive the auction as the fairer mechanism.

These results are relevant to researchers as well as practitioners. From an academic perspective, the results contribute to understanding an anomaly found in the literature on PES: the difference in outcomes (take-up rates and tree survival rates) between auctions and fixed-price schemes, even when the two allocation mechanisms are theoretically equivalent (Jack, 2013; van Soest et al., 2018). Our results indicate that conventional (neoclassical) and behavioral economics considerations, including social preferences and fairness perceptions, may matter for the explanation of this anomaly. Moreover, budget size affects the level of competition in the auction as well as the probability of being selected in the fixed price scheme. Social preferences and opportunity costs seem to matter for behavior under a high level of competition, but not under a low one. It is important to note that our research does not consider theoretically equivalent PES allocation mechanisms, but budgetarily equivalent ones. Indeed, considering theoretically equivalent PES allocation mechanisms like uniform price auction and equivalent fixed-price scheme (Jack, 2013) may bias our analysis, in the sense that the use of the auction clearing price as the fixed-price level may preclude some high-opportunity-cost subjects to prefer the fixed-price scheme, as their opportunity cost level may be higher than the fixed-price level.

From a practitioner perspective, the results confirm auction theory in that information rents decrease with competition. More importantly, the results suggest that the types of subjects who may be attracted to a PES program depends on the allocation mechanism. In fact, acceptance of PES programs by relevant stakeholders, especially environmental service providers, is considered important due to the voluntary nature of PES (Schomers & Matzdorf, 2013). Our results indicate that when budgets are highly constrained, so that only a small proportion of potential applicants can be included in the PES scheme, more efficiency-minded individuals and those with lower opportunity costs prefer an auction, while more inequality-averse individuals and those with higher opportunity costs prefer a fixed-price scheme. Three caveats are in order when interpreting this result. First, the result on opportunity costs is likely influenced by the high share of risk averse individuals in our sample, although we would expect similarly high shares in populations of actual land users. Second, when budgets are less constrained, as for example currently in European agri-environmental policy, our results suggest that sorting effects seem to be less of a concern. Third, our experiment captures the choice between allocation mechanisms, while in practice, implementing organizations choose and offer only one mechanism. A further obvious limitation of our study is that it is based on a subject pool of students rather than actual farmers or land users. Our results suggest that population characteristics affect outcomes.

Further empirical research should examine the choice of allocation mechanisms for actual farmers. It seems also promising to look more closely into the role of fairness perceptions, their determinants and their consequences for behavior under alternative allocation mechanisms. Such effects may be stronger or different among actual farmers than among students.

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Chapter 3: Impact of fairness considerations for the selection of PES allocation mechanisms

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3.1. Introduction

In the search for effective policy instruments to address the challenge of conserving or reducing the degradation of natural resources, payments for ecosystem services (PES) have become a popular approach (Naeem et al., 2015; Schomers & Matzdorf, 2013). Where the societal benefits from ecosystem services provision exceed the costs to ecosystem services providers, PES helps translate the non-market value of the environment into positive economic incentives for local actors (Engel et al., 2008; Wunder, 2005). PES has also been argued to provide potentially large gains in cost-effectiveness compared to other regulatory policy approaches (Ferraro & Kiss, 2002; Ferraro & Simpson, 2002). Whether this potential is fulfilled, however, depends on whether important design lessons are implemented (Börner et al., 2017; Engel, 2016; Wunder et al., 2018).

Despite the increasing implementation of PES programs in both developed and developing countries (Schomers & Matzdorf, 2013), the literature on PES fails to agree on the potential role of the social dimension for the performance of PES programs. Many authors have highlighted the fact that PES programs should focus on their environmental outcomes as primary goal, therefore social goals may need to be traded off (Engel et al., 2008; Wunder, 2007). Others have argued that the relationship between the environmental and social dimensions of PES are context-dependent[23], and it may be relevant to analyze the distributional implications of PES, especially in developing countries (Pagiola et al., 2005). A third strand of literature highlights that fairness or equity[24] considerations are important in PES design, not just for ethical reasons, but also for effectiveness and efficiency reasons (Alpizar et al., 2017;

[23] This, among others, depends on the ecosystem service being sought, the degree to which PES programs are spatially targeted, where the program is implemented, income levels of ES providers and ES beneficiaries.

[24] Both terms are used in the PES literature. We use the term 'fairness' throughout most of the paper and do not enter into a debate about semantic differences.

Pascual et al., 2010; Lliso et al, 2020a). They argue that if PES is perceived as inequitable, this can negatively affect program compliance and support and thereby environmental outcomes (Corbera & Pascual, 2012). In fact, PES may affect a range of fairness dimensions, including procedural equity (i.e. inclusiveness of rule and decision-making), distributive equity (i.e. distribution of costs and benefits), and recognition (i.e. accounting for stakeholder knowledge, norms, and values) (Pascual et al., 2014; Pascual et al., 2010). All in all, this strand of the literature implies a potential role for fairness considerations in the design and implementation of PES programs.

So far the analysis of the role of fairness for the design and implementation of PES programs is limited to conceptual papers and qualitative research methods (Calvet-Mir et al., 2015; Pascual et al., 2010). For instance, Pascual et al. (2010) introduce the concept of the 'efficiency-equity interdependency curve' to illustrate potential combinations between equity and efficiency in PES. Few studies have tried to complement the analysis with a quantitative analysis approach (Lliso et al., 2020a). Likewise, few studies have attempted to address this gap by considering the multidimensional aspect of fairness in PES. Lliso et al. (2020a) run a deliberative choice experiment in an indigenous community in Colombia and find that local land users value various equity dimensions in PES. Narloch et al. (2013) provide insights into the multiple dimensions of fairness in PES based on results of pilot agrobiodiversity conservation auctions in Bolivia and Peru. They find that discriminatory, uniform and conditional payment rules, which incorporate alternative principles of fairness, also result in varying conservation outcomes. However, their analysis does not consider farmers' perceptions (judgements) of fairness of payment rules nor the impact of these fairness perceptions on conservation outcomes. We add to this literature by conducting a quantitative analysis, focusing on different PES allocation mechanisms and explicitly considering the distributional and procedural fairness dimensions. We also add to the literature by distinguishing between fairness perception (judgement about how fair a design feature is) and fairness preference (whether a person cares about fairness, measured as inequality aversion). In practice, PES contracts may be allocated via auctions and fixed-price schemes[25] (Jack, 2013; Jindal et al., 2013; Stoneham et al., 2003). While fixed-price schemes are the predominantly used allocation

[25] In auctions, farmers compete to get selected. In discriminatory auctions, winners receive as they bid, whereas in uniform auctions winners receive the same payment. In fixed-price schemes, farmers are randomly selected, and winners receive the same payment.

mechanism in practice, economists have argued for auctions in order to enhance environmental- and cost-effectiveness of PES programs (Ferraro, 2008; Jack, 2013; Latacz-Lohmann & Van der Hamsvoort, 1997; Schilizzi & Latacz-Lohmann, 2007). However, which type of allocation mechanism is chosen by the implementing organization has potential implications for the distributional and procedural fairness perceptions of the ecosystem service (ES) providers. Likewise, which type of allocation mechanism is preferred by ES providers has potential implications for the effectiveness of the PES program. Understanding those fairness dimensions and farmers' preferences over PES allocation mechanisms in the design and implementation of PES programs could thus be helpful to avoid undermining their social desirability and success. The goal of our field study in Benin is therefore to examine farmers' preferences over PES allocation mechanisms and the role of fairness therein. Our study is motivated by and builds on a previous lab experiment with students in Osnabruck, Germany, which showed that preferences over allocation mechanisms are influenced by opportunity costs and social preferences and which indicated that fairness perceptions also appear to play a significant role (Kouakou et al., 2020). In this paper, we are interested to answer the following questions: What are farmers' preferences over alternative PES allocation mechanisms (auction vs. fixed price)? Are these preferences and farmers' behavior in a PES auction affected by fairness considerations (specifically inequality aversion and fairness perceptions)? If so, what is the relative importance of distributional and procedural fairness dimensions therein?

3.2. Results

Our main analysis is based on the experimental economic approach. We use data from a framed field experiment, conducted with 250 farmers from 5 villages in Benin in the context of agrobiodiversity conservation. The field experiment is divided into three parts (see the section on methods for a detailed description): an entry survey, the main experiment and an exit survey. The entry and exit surveys measure farmers' inequality aversion and fairness perceptions of allocation mechanisms, respectively. The main experiment is an incentivized economic game in which farmers participate in a conservation auction. The goal of the conservation auction is to allocate payments to farmers to incentivize the replacement of a conventional crop variety with a neglected one. In the auction process, subjects compete for getting their bids selected. After experiencing the auction, farmers are asked to vote for a specific allocation mechanism: auction vs fixed-price scheme. In the fixed-price scheme, winners are randomly selected and receive the same payment. The mechanism with the majority vote is implemented for actual payoffs. We thus measure farmers' revealed preferences over allocation mechanisms.

As a measure of fairness preferences, individual level of inequality aversion is elicited through incentivized choices via the Equality Equivalence Test (Kerschbamer, 2015). Farmers are offered four choices between two proposed allocations of money between themselves and another farmer. The first allocation is an egalitarian one and remains unchanged, whereas the second allocation is non-egalitarian and changes. Farmers who select the egalitarian allocation of money in all four choices are classified as inequality averse subjects, whereas the others are classified as non-inequality averse subjects. Among non-inequality averse subjects, it is possible to identify other social preference types like spiteful, altruist and inequality loving.

Fairness perception of each allocation mechanism is measured via a self-reported ten-point Likert scale: 1 indicates that the allocation mechanism is perceived extremely unfair, whereas 10 indicates that it is seen as extremely fair[26]. Furthermore, we elicit on separate 10-point Likert scales the degree to which fairness perceptions are shaped by distributional and/or procedural fairness considerations.[27]

We find that (1) farmers vary in their revealed preferences over PES allocation mechanisms, and revealed preferences are not influenced by individual-level inequality aversion, but are influenced by self-stated procedural fairness perceptions; and (2) farmers' bidding behavior in PES auctions is not impacted by fairness considerations, neither by individual level-inequality aversion nor by self-reported fairness perceptions.

Our results indicate that farmers who perceive an allocation mechanism as procedurally unfair, are less likely to choose this mechanism. In light of other studies, this could imply an impact

[26] For instance, in the case of fairness perception of the auction, the question is as follows: *On a scale from 1 to 10, where 1 is not at all and 10 is very much, how fair do you think it is to use the ranking rule to select winners and determine earnings?* Regarding the fairness perception of the fixed-price scheme, we ask the following question: *On a scale from 1 to 10, where 1 is not at all and 10 is very much, how fair do you think it is to use the random rule to select winners and determine earnings?*

[27] For instance, in the case of the procedural fairness perception of the auction, the question is as follows: *On a scale from 1 to 10, where 1 is not at all and 10 is very much, when you think about how fair the ranking rule is, how important is it in your view that winners are selected by competition?* In the case of the distributional fairness perception of the auction, the question is as follows: : *On a scale from 1 to 10, where 1 is not at all and 10 is very much, when you think about how fair the ranking rule is, how important is it in your view that the fund is not equally distributed among winners?*

Regarding the procedural fairness perception of the fixed-price scheme, we ask the following question*: On a scale from 1 to 10, where 1 is not at all and 10 is very much, when you think about how fair the random rule is, how important is it in your view that winners are randomly selected?* In the case of the distributional fairness perception of the fixed-price scheme, the question is as follows: *On a scale from 1 to 10, where 1 is not at all and 10 is very much, when you think about how fair the random rule is, how important is it in your view that the fund is equally distributed among winners?*

on PES effectiveness. However, we find that fairness considerations do not impact bids. In this sense, we do not find evidence that fairness perceptions affect the performance of the auction.

Farmers vary in their preferences over PES allocation mechanisms and in their fairness preferences (inequality aversion), but preferences over PES allocation mechanisms are not influenced by inequality aversion.

We find that a significant majority of Beninese farmers, namely 55.6%, prefer the auction (binomial test: two-sided test, p = 0.014), while the remaining 44.4% prefer the fixed-price scheme. Regarding fairness preferences, we find that most Beninese farmers are inequality loving (42.40%). Table 3.1 shows the distribution of fairness preferences in more detail. We do not find any effect of inequality aversion on farmers' revealed preferences over allocation mechanisms (see Table 3.2, models (1) and (2)).

Table 3.1: Distributions of fairness preferences among Beninese farmers.

Inequality averse subjects	Non-inequality averse subjects		
	Spiteful subjects	Altruistic subjects	Inequality loving subjects
15.60%	4.80%	37.20%	42.40%

Note: We use the Equality Equivalence Test to measure fairness preferences (Kerschbamer, 2015). It helps classify individuals as inequality averse (i.e. positive concern for those who have less; negative concern for those who have more), altruistic (i.e. positive concern for others well-being), inequality loving (i.e. positive concern for those who have more; negative concern for those who have less), and spiteful (i.e. negative concern for those who have less; negative concern for those who have more).

Farmers' preferences over PES allocation mechanisms are affected by fairness perceptions.

We also analyze the relationship between fairness perception and farmers' preferences over allocation mechanisms. We include fairness perceptions of allocation mechanisms in the Probit regression of subjects' preferred allocation mechanism (dependent variable = 1 if auction is preferred), controlling for inequality aversion and other variables. We find that fairness perceptions of fixed-price scheme and auction affect farmers' preferences over allocation mechanisms (see Table 3.2, model (1)).

Table 3.2: Probit regressions of farmers' preferences and log-linear of farmers' bids.

		Y = vote for auction (0/1)		Y = log (auction bid)	
		(1)	*(2)*	*(3)*	*(4)*
Fairness preference (inequality aversion)		0.321 (0.197)	0.068 (0.349)	-0.364 (0.287)	-0.393 (0.227)
Perceived fairness of auction		0.062*** (0.018)		-0.007 (0.017)	
Perceived distributional fairness of auction	Arg 1: The fact that the fund is not equally distributed		-0.009 (0.019)		-0.002 (0.004)
	Arg 2: The fact that winners receive as they bid		-0.013 (0.021)		-0.004 (0.013)
Perceived procedural fairness of auction	Arg 3: The fact that winners are competitively selected		0.066*** (0.020)		-0.025 (0.034)
Perceived fairness of fixed-price scheme		-0.039*** (0.015)			
Perceived distributional fairness of fixed-price scheme	Arg 1: The fact that the fund is equally distributed		-0.0004 (0.021)		
	Arg 2: The fact that winners receive the same payment		-0.006 (0.022)		
Perceived procedural fairness of fixed-price scheme	Arg 3: The fact that winners are randomly selected		-0.045*** (0.017)		
Individual and village-level controls		YES	YES	YES	YES
N		239	239	250	250
(Pseudo) R2		0.279	0.293	0.408	0.447
Prob > chi2		0.000	0.000		

Note: Columns 1 and 2 report probit marginal effect estimates evaluated at the means of all covariates.. Columns 3 and 4 are OLS regressions with ln (bid) as the dependent variable. Standard errors in bracket are cluster-robust at the village level. Fairness preference (inequality aversion) is a binary variable that takes the value 1 if the farmer is inequality averse and 0 otherwise. Perceived fairness is a ten-point Likert scale. 1 indicates that the allocation mechanism is extremely unfair and 10 indicates that the allocation mechanism is extremely fair. Individual and village-level controls include opportunity cost, age, gender, income, risk preferences, village and village chief, etc. ***Significant at 1 percent level. **Significant at 5 percent level. *Significant at 10 percent level. We also tested combining Arg 1 and Arg 2 into one (average) variable and found that the results were unchanged (see the supplementary information).

The more a farmer perceives the auction as fair, the more likely he is to choose the auction. Also, the more a farmer perceives the fixed-price scheme as fair the less likely he is to select the auction; in other words, the more likely he is to prefer the fixed-price scheme. The result suggests that farmers' fairness perceptions are more likely to play a role for the desirability (or the preference) of a specific PES allocation mechanism.

Farmers' behavior in PES auctions is not affected by fairness considerations (neither inequality aversion nor fairness perceptions).

We analyze the relationship between bids under the auction and fairness considerations (inequality aversion and fairness perceptions of PES allocation mechanisms). Average bids do not differ between inequality averse subjects and non-inequality averse subjects (Mann-Whitney U test: two-sided test, $p = 0.460$). When we control for other characteristics, using a log-linear regression, we also do not find an effect of fairness considerations (inequality aversion and fairness perception of the auction) on farmers' bids (see table 3.2, models (3) and (4)). Thus, neither fairness preferences (inequality aversion) nor fairness perceptions are found to affect farmers' behavior in PES auctions.

Although farmers consider both distributional and procedural dimensions to judge the fairness of PES allocation mechanisms, their preferences over allocation mechanisms are more affected by the procedural dimension.

We further our analysis on the role of fairness perceptions by distinguishing between distributional and procedural fairness dimensions. Distributional fairness refers to a fair distribution of costs and benefits (Pascual et al., 2010, 2014). In our case, this is translated into the fact that the reward is equally distributed among recipients (see table 2, Arg 1) and they receive the same payment level (see table 2, Arg 2). Procedural fairness focuses on the inclusiveness of rule and decision-making (Pascual et al., 2014). It may be reached when groups have an opportunity to participate in the design of the payment schemes, or at least their interests are taken into account (Narloch, Drucker, & Pascual, 2011). In our context, an allocation mechanism is procedurally fair if winners participate in the selection process. We invited farmers to judge the procedural fairness perception of PES allocation mechanisms through the way they participate in the selection process (competition vs randomness) (see table 2, Arg 3). First, we analyze which dimension(s) affect the perceived fairness of the auction and the fixed-price scheme. We find that both dimensions are significantly correlated with farmers' fairness perceptions of PES allocation mechanisms (see the appendix for Spearman and Pearson correlation matrices).

Table 3.3: Tobit regressions of farmers' fairness perceptions of PES allocation mechanisms.

		Y= Fairness perception of auction	**Y= Fairness perception of fixed-price scheme**
Fairness preference (inequality aversion)		0.469 (0.467)	-0.453 (0.450)
Distributional fairness perceptions of auction	Arg 1: The fact that the fund is not equally distributed	0.156** (0.073)	
	Arg 2: The fact that winners receive as they bid	0.063 (0.087)	
Procedural fairness perceptions of auction	Arg 3: The fact that winners are competitively selected	0.544*** (0.077)	
Distributional fairness perceptions of fixed-price scheme	Arg 1: The fact that the fund is equally distributed		0.285*** (0.082)
	Arg 2: The fact that winners receive the same payment		-0.106 (0.081)
Procedural fairness perceptions of fixed-price scheme	Arg 3: The fact that winners are randomly selected		0.687*** (0.061)
Individual and village-level controls		YES	YES
N		239	239
(Pseudo) R2		0.110	0.163
Prob > chi2		0.000	0.000

Note: The table reports Tobit estimates (standard errors in parentheses). The dependent variable is a discrete variable that could take any value between 1 and 10. Higher values imply greater fairness perceptions. Individual controls include age, gender, income, risk preferences, village, village chief, etc. ***Significant at 1 percent level. **Significant at 5 percent level. *Significant at 10 percent level.

In other words, farmers who perceive auction and fixed-price schemes fairer tend to perceive them as distributionally and procedurally fairer. Using a tobit regression of fairness scores of PES allocation mechanisms on distributional and procedural fairness scores (see table 3.3), confirms that both distributional and procedural fairness perceptions of PES allocation mechanisms are more likely to affect the general fairness perceptions of those PES allocation mechanisms. In other words, farmers consider both distributional and procedural dimensions to judge the fairness of PES allocation mechanisms.

Second, we analyze the relative importance of distributional and procedural fairness perceptions for farmers' preferences over PES allocation mechanisms. Disentangling different dimensions of fairness perceptions in the probit regression (table 3.2, model (2)), we find that the procedural fairness perceptions of both PES allocation mechanisms affect farmers' preferences over these allocation mechanisms. This result suggests that farmers' preferences over PES allocation mechanisms are more affected by the procedural fairness dimension, although both distributional and procedural dimensions matter for their judgement of the fairness of PES allocation mechanisms.

3.3. Discussion

Our study examines farmers' preferences over PES allocation mechanisms and the role of fairness considerations therein. Our results do not confirm the findings from the laboratory experiment of Kouakou et al. (2020) with regards to the significant impact of fairness preferences (inequality aversion). This may be due to the fact that that study was conducted with university students in Osnabruck, who exhibited more inequality aversion than the farmers in Benin (in the lab experiment 17.74% of students were inequality averse). With respect to fairness perceptions, our study confirms that fairness perceptions play an important role for preferences over PES allocation mechanisms. Further differentiating different fairness dimensions, we find that the procedural fairness dimension appears to be most important. This is in line with Lliso et al. (2020a) who found procedural fairness to be the most important dimension of fairness in a choice experiment on PES design. Thus, in search of social desirability of PES allocation mechanisms in those villages, practitioners should care particularly about the procedural dimension: the way winners (recipients) are selected.

Two caveats are in order when interpreting our results. First, our experiment is a framed field experiment. Although there is field context in the information set that farmers could use, they still know that they are participating in an experiment. Likewise, farmers will not really grow the neglected crop variety on their land. This may limit the stakes of getting selected by a specific allocation mechanism in the field experiment. A real PES program may help increase the stakes and reveal farmers' real behaviors. Second, although fairness perceptions are found to play a role for farmers' preferences over PES allocation mechanisms, we did not directly measure whether this has implications for the success or failure of a PES program.

Methods

Framed Field Experiment. *Selection of the study site and crop varieties*. The field experiment was implemented in Tori-Bossito, Benin. The study site was selected in close collaboration with our local partners. First, a feasibility study of the field experiment was conducted in October 2016, including a needs assessment and interviews with farmers' organizations and local scientists. Second, based on an analysis on crops' risk status, local scientists and experts from Bioversity International selected 5 villages and 2 cowpea varieties for our study. The first cowpea variety was a local and traditional variety, named *Sewe*. Due to climate and market conditions, farmers were neglecting this variety in favor of an improved variety, called *Assienon gbehami*. This improved variety was advertised and recommended by development organizations, which might imply a loss of agrobiodiversity if farmers only planted and used the improved variety. *Entry survey and main experiment*. Between September and December 2018, data for the study were collected through a household questionnaire and experimental sessions. Based on the sampling frame, we randomly selected and surveyed 250 farmers[28]. The survey included questions on household and agricultural profiles (Jack, 2013; Rohit Jindal et al., 2013), risk preferences and fairness preferences, especially inequality aversion. Risk preferences were elicited through an ordered lottery selection (OLS) design (Binswanger, 1980, 1981). Each subject was presented with the choice of six lotteries, and asked to pick one. Each lottery had two possible outcomes: a low payoff and a high payoff. The average payoff was increasing from one lottery to another lottery, but with increasing variance around that payoff. Once farmers chose a lottery, a coin was tossed. They got the low payoff if Head appeared and the high payoff if Tail appeared. Regarding fairness (social) preferences, we used a reduced version of the equality equivalence test (EET) developed by Kerschbamer (2015). Subjects were offered the choice between two proposed allocations of money between themselves and another subject. One such allocation remained unchanged over all 4 choice pairs – both the subject herself and the other subject each received the same amount of money. The other allocation changed over the sequence, with the amount allocated to the decision maker increasing over the sequence, and with a fixed amount allocated to the other subject which was either high (in the first 2 choice options), or low (in the last 2 choice options) – allowing the researcher to measure the subject's tolerance for advantageous and disadvantageous payment

[28] We selected 50 farmers in each village. Power calculations and the budget available for data collection and payments determined the sample size.

inequalities. The switching pattern in the EET allowed classifying participants into major social preference types discussed in the literature including inequality averse, spiteful, altruist and inequality loving. Then, farmers were invited to participate in an experiment on their preferences over allocation mechanisms that could be used by a payments for agrobiodiversity conservation services (PACS) program, which is an agriculture-related PES. In each village, farmers were assigned to a group of 10 subjects. First, they went through a mock auction in order to understand the auction mechanism. They received a banana and were asked to state the minimum amount of money they would accept in return for giving their bananas to the experimenter. Bids were selected from the lowest to the highest until the experimenter's budget was exhausted. Winners got their bids and losers kept their bananas. After experiencing the auction mechanism, farmers received an endowment (a specific amount of money), which represented the profit of the improved cowpea variety. The endowment levels were similar to the market prices and were randomly drawn from a set of three amounts (FCFA 500, FCFA 800 and FCFA 1,000). Each farmer was aware of her endowment level. To keep things very simple, we also assume that the profit from planting the local/traditional variety is null[29]. In other words, farmer's opportunity cost of replacing the improved variety with the local/traditional one is her endowment. Third, farmers were asked to place bids on the minimum amount of money they would accept to plant (or conserve) the local/traditional neglected cowpea variety on their land. Finally, farmers were asked to choose/vote for their preferred allocation mechanisms, as all farmers could not be selected for getting the payments. The allocation mechanisms were explained to them through pictures and posters. The auction selected winners through a competitive mechanism and they got as they bid, whereas the fixed-price scheme selected winners randomly and they got the same payment. *Exit survey*. After the experiment, farmers were asked to state their perceptions of agrobiodiversity loss in their villages and their fairness perceptions of PACS allocation mechanisms: auction and fixed-price scheme. Perceptions of agrobiodiversity loss were collected through a questionnaire developed by Wale (2011), whereas fairness perceptions of allocation mechanisms were measured through a ten-point Likert scale. 1 indicated that the allocation mechanism is extremely unfair, whereas 10 indicated that it was extremely fair. In addition, we measured the distributional and procedural fairness perceptions of each allocation mechanisms, using a ten-point Likert scale.

[29] In the short run, it may be difficult to sell the local/traditional variety in the market.

Analytic methods. In examining farmers' preferences over PES allocation mechanisms and the role of fairness considerations therein, we faced two challenges: (1) identifying key variables that affect preferences and (2) identifying the dimensions of allocation mechanisms that play a role for farmers' fairness perceptions. In order to address the first challenge, we used a probit regression, as the dependent variable takes the value 1 if subjects (or farmers) choose the auction and 0 if they prefer the fixed-price scheme. The marginal effects helped measure the impact of each independent variables on the probability to prefer the auction. Second, a tobit regression helped us identify the dimensions of allocation mechanisms that play a role for preferences, as we use used a ten-point Likert scale (fairness perceptions) as dependent variable. In fact, the ten-point Likert scale may be seen like a censored variable.

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Chapter 4: Does the performance of conservation auctions depend on the pre-existing institution?

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4.1. Introduction

Conservation auctions are viewed as an efficient way of allocating conservation contracts to farmers, in the sense that they use competition to allocate conservation contracts (Jack, 2013; Rolfe et al., 2009; Stoneham et al., 2003; Ulber et al., 2011). They are also a form of payments for environmental services (PES), which help translate the non-market value of the environment into incentives for local actors to provide environmental services (Engel et al., 2008). One of the main advantages of conservation auctions over more frequently used fixed-price schemes to allocate conservation contracts is that auctions are able to deal with the problem of asymmetric information[30] arising between the conservation agency and farmers/landholders (Ferraro, 2008; Latacz-Lohmann & Van der Hamsvoort, 1997) and ultimately can enhance cost-effectiveness of conservation programs (Jack, 2013; Schilizzi & Latacz-Lohmann, 2007, Windle & Rolfe, 2008). A number of scientific studies have explored and found the superiority of conservation auctions over traditionally used fixed-price schemes (Jack, 2013; Latacz-Lohmann & Van Der Hamsvoort, 1997; Schilizzi & Latacz-Lohmann, 2007; Windle & Rolfe, 2008). However, others have also found potential pitfalls in the design of conservation auctions, specifically that conservation auctions may lose their competitive edge in case of repetition (Schilizzi & Latacz-Lohmann, 2007; Windle & Rolfe, 2008). Furthermore, Lundberg et al. (2018) argue that the relative effectiveness compared to fixed-price schemes depends largely on the context in which auctions are implemented, including variability in the correlation between ecosystem services and landscape characteristics, cost heterogeneity and available budget size of the program. In this paper, we study whether the

[30] The problem of asymmetric information describes the case in which a conservation agency know less than farmers or landowners as farmer's opportunity cost of participating in a conservation programs is private information.

cost-effectiveness of a conservation auction is affected by the previous implementation of a fixed-price scheme, that is whether a kind of path-dependence in provision of conservation activities by landowners exists.

Many existing large-scale conservation programs such as the ones targeting tropical deforestation in Costa Rica and Mexico as well as agri-environmental programs in Europe traditionally use fixed-price contracts for contract allocation (Wunder, Engel, & Pagiola, 2008). Several studies have called for shifting these schemes towards allocating conservation contracts with auctions (Alix-Garcia et al., 2009; Hanley et al., 2012; Porras et al., 2013). Yet, these studies argue for the implementation of auctions without considering potential behavioral biases that may be induced by such a switch from a pre-existing to a new institution. We argue that, from a behavioral economics perspective, the existence and level of a pre-existing fixed-price scheme could continue to influence bids in conservation auctions despite being no longer rationally relevant for an individual's decision problem. This paper therefore explores the impact of a pre-existing fixed-price scheme on the performance of conservation auctions. We hypothesize that a pre-existing environmental service price may affect the benchmark against which participants perceive and evaluate conservation auctions, that is, a participant may use such prices as reference points.

Reference points have been found to (adversely) affect many economic behaviors including, among others, the declaration of taxes (Bruttel & Friehe, 2014), trust and cooperation (Bohnet and Huck, 2004; Bruttel and Friehe, 2011), valuation of housing prices (Einiö et al., 2008; Genesove & Mayer, 2001), and the time investors hold on to stocks (Weber & Camerer, 1998). Yet, other studies have shown that repeated decision-settings and market competition may eliminate behavioral anomalies such as the impact of non-conscious reference points (e.g. Braga et al., 2009; Dijk et al., 2013; Knetsch et al., 2001; List, 2003; Loomes et al., 2003; Tufano, 2010). Translated to our setting, such reference-dependent or path-dependent[31] behavior may affect subjects' bids and thus possibly compromise the performance of conservation auctions and ultimately even eliminate the relative cost-effectiveness advantage of the auction.

[31] Reference-dependence refers to the dependence of preferences on a reference point, while path dependence means that people's decisions depend on previous decisions made. Here we use the terms 'reference-dependent' and 'path-dependent' interchangeably and thus do not enter into a debate about semantic differences.

To answer the question how conservation auction performance is affected by the previous existence of a fixed-price scheme, we conduct a controlled laboratory experiment with university students, which provides us the opportunity to exogenously vary the existence and level of such the fixed price and assess its impact on the economic performance of conservation auctions. In our induced-value and decontextualized experiment we exogenously vary the pre-existing environmental service price, using the midpoint and the upper bound of the distribution of opportunity costs as realistic benchmarks for possible policy price settings (Wunder, Engel, & Pagiola, 2008). In addition, we implement a baseline (control) treatment in which no such previous service price exists.

Our results indicate that the performance of conservation auctions depends on the level of the pre-existing environmental service price. When the price in a pre-existing fixed-price scheme is high, participants' average bids increase and the budgetary effectiveness of the conservation auction decreases in the first round. However, these effects are not statistically significant for the case where the fixed-price scheme was based on average opportunity costs. Likewise, repeated rounds improve the performance of conservation auctions. Our paper thus provides evidence that the performance of conservation auctions can depend on the institutional context in which the auction is being implemented and the familiarity with (the repetition of) the auction.

The remainder of the paper is organized as follows. Section 4.2 summarizes related literature. Section 4.3 presents the experimental design. Section 4.4 details our research hypotheses. Section 4.5 presents the results. Finally, in section 4.6, we discuss our findings and conclude the paper.

4.2. A review of related literature

Our paper explores the potential of reference- or path-dependent behavior in conservation auctions. A number of behavioral economic studies in other markets and trade settings have analyzed related issues. For example, Genesove & Mayer (2001) and Einiö et al. (2008) find sellers' behavior in the housing/apartment market to be influenced by the previous nominal purchase price which sellers seem to use as reference point. Both studies indicate that the purchase price of the old apartment strongly influences decisions to move and acquire new apartments. Prospect Theory (Kahneman and Tversky, 1979) is a potential explanation, as it stipulates that loss-averse agents might consider the original purchase price to be a reference point in their value function. Another example includes research indicating that reference points can affect investors' choices of selling and keeping stocks (Grinblatt & Keloharju, 2001;

Odean, 1998; Weber & Camerer, 1998). In particular, many investors have been found to hold on to collapsing assets too long in their quest to avoid losses. There is also a broader literature in experimental auctions that shows that providing participants with a tangible good as endowment significantly influences their valuation of that good (e.g. Horowitz & McConnell, 2002; Kahneman & Tversky, 1979). This phenomenon is referred to as the endowment effect (Tversky & Kahneman, 1991) and usually explained by Prospect Theory and loss aversion. With respect to conservation auctions Dijk et al. (2018) confirm the existence of an endowment effect in a laboratory procurement auction.

The aforementioned studies examine the possibility of reference points impacting economic decision-making and bidding behavior. As we also plan to investigate the effect of repeated market trials or a repeated-round auction, we also consider evidence for the mitigation of behavioral anomalies via repeated market exposure (Braga et al., 2009; Corrigan et al., 2011; Cox & Grether, 1996; Knetsch et al., 2001; List & Shogren, 1999; Loomes et al., 2003; Plott, 1996; Dijk et al., 2018). In fact, most of these studies test two hypotheses: the price following hypothesis (also called the shaping hypothesis), which refers to the fact that agents adjust their bids towards the price observed in a previous market period (Loomes et al., 2003), and the market experience hypothesis (also called the market discipline hypothesis or the refining hypothesis), which refers to the fact that market experience over time may induce individuals to reveal or understand their real preferences and eliminate anomalies over time (Loomes et al., 2003; Plott, 1996).

Results are mixed. While some studies find evidence in favor of the price following hypothesis (Corrigan & Rousu, 2006; List & Shogren, 1999) or the market experience hypothesis (Cox & Grether, 1996; Loomes et al., 2003), others do not (Braga et al., 2009; Knetsch et al., 2001). In the context of conservation auctions, Dijk et al. (2018) find support for the market experience hypothesis, showing that repetition is able to attenuate the endowment effect, i.e. the effect of initial endowment and its effect on auction results. In contrast to our experiment, van Dijk et al. (2018) investigate the ownership of a good, in their case, subjects are able to participate in an auction and sell an item they just have been endowed with. In our experiment, we are interested in the phenomenon if the pure possibility of obtaining a contract via the participation in a fixed-price scheme in a previous round has an impact on auction efficiency later on.

Finally, our paper links to a broader literature on using laboratory and field experiments to study conservation auction design questions and their effect on auction performance. Most of the literature has focused on a direct comparison of conservation auctions to fixed price schemes (Schilizzi & Latacz-Lohmann, 2007; Windle & Rolfe, 2008; Stohneman et al., 2003). Others have focused on studying more precisely the effect of information provision and entry conditions on the performance of auctions (Cason et al., 2003; Fooks, Messer, & Duke, 2015). We are, to our knowledge, the first study to examine the impact of a pre-existing fixed-price scheme on participant's bids and auction performance.

4.3. Experiment

We first describe our general conservation set-up and explain our experimental design and treatments. We also provide details on the implementation of the experiment. The appendices contain a translated version of the experimental instructions.

4.3.1. The Conservation set-up

As our research is motivated by the use of auctions to allocate PES contracts, we present the set-up of our experiment here using typical PES framing. Note, however, that all experiments were conducted in a context-free environment, using neutral language.

We consider a representative agent (or farmer), who runs a conventional activity and obtains a profit π_0. The conventional activity may be replaced with an environmentally friendly one yielding profit π_1, with $\pi_0 > \pi_1$. In other words, $\pi_0 - \pi_1$ represents the agent's cost of replacing the conventional activity with the environmentally friendly one. The implementing agency has a budget, B, which it can use to incentivize agents to adopt the environmentally friendly activity by means of a PES payment. The implementing agency can allocate such payments either via a fixed price scheme or via a discriminatory auction.[32] We set parameters such that ex-ante the available budget is the same in both allocation mechanisms. To be able to study the impact of the sequencing of institutions on the performance of conservation auctions, our two main treatments (T1 and T2) first implement the fixed-price scheme in a session, followed by a discriminatory auction; see Figure 1 for a graphical illustration. Treatments T1 and T2 only differ in the level of the fixed price offered – either the upper bound or the midpoint of the distribution of opportunity costs, respectively. We do so because we assume that policymakers or the implementing conservation agency will have some information about the upper bound and the midpoint of the distribution of opportunity costs, and will use it as starting point to set the payment level. This is in line with evidence on existing PES (Wunder, Engel,

[32] Note that we implement a reverse auction. The discriminatory auction is the generalization of the first-price sealed-bid auction. In the first-price sealed-bid auction, bidders are invited to submit a sealed bid, with the understanding that the winner has the lowest bid. In our case, winners are selected from the lowest to the highest bid until the budget is exhausted.

& Pagiola, 2008). As a control treatment, we include a third treatment, T3, in which the discriminatory price auction is *not* preceded by the fixed price scheme.

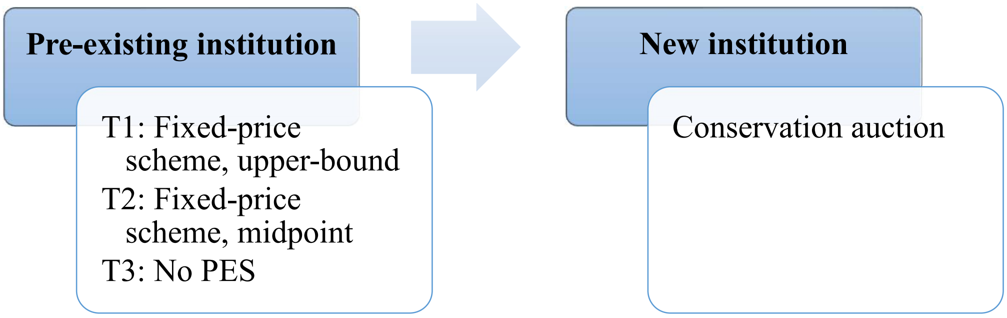

Figure 4.1: Sequential implementation of different PES allocation mechanisms.

4.3.2. The experimental design

The main aim of the experiment is to answer whether a pre-existing fixed price scheme affects participants' bids and hence the performance in a subsequent conservation auction. To establish a clean causal relationship, we also aim to control for a range of participant characteristics and preferences measurements which have been shown to affect participant bidding in auction environments (Isaac & James, 2000; Latacz-Lohmann & Van der Hamsvoort, 1997; Van den Bos et al., 2013). Before the start of the main experiment, we collected incentivized measures of individual risk and social (distributional) preferences (part 1 and 2 of the experiment). The details of the measures used are provided in Appendix A. In addition to these two financially incentivized preference elicitation games we also asked participants to fill out a short exit survey at the end of each experimental session consisting of, among others, a personality type test in the form of the Big Five (John et al., 1991; John et al., 2008), and questions to assess our subjects' perceptions vis-à-vis the fairness of the auction mechanism.

For the conservation auction (part 3), we implement a partner matching with group sizes of 10 throughout the auction. We apply an induced-value[33] set-up, inspired by Schilizzi & Latacz-Lohmann (2007): this means, each subject was randomly assigned an individual 'Chip' cost. The individual chip cost represents the opportunity cost of participating in the environmentally friendly program, denoted $\pi_0 - \pi_1$. Subjects were informed that Chip costs for each individual

[33] In an induced-value experiment, experimental subjects are given pre-assigned values for a fictitious good. In our setting, this means that participants are pre-assigned opportunity cost for a parcel of land. This method stands in contrast to other experimental markets for real goods in which participants are assigned field goods such as coffee mugs.

participant were drawn from a uniform distribution with support [10,300], and they were also informed about the cost that was assigned to their Chip.

Table 4.1 summarizes our experimental design. For treatments T1 and T2 there are two stages. Stage 1 consists of a one-time allocation of conservation contracts using a fixed-price scheme, and stage 2 implements the conservation auction. In the fixed price scheme contracts are allocated randomly until the budget is exhausted. Subjects get a positive payoff (the difference between the fixed price and their Chip costs) if they are selected. Otherwise they get nothing. Under the upper bound condition, all subjects are eligible, whereas under the midpoint condition, only subjects who have a Chip cost below or equal to the fixed price are eligible. In the conservation auction all bids submitted are lined up from lowest to highest, and the auctioneer accepts bids starting with the lowest until the budget is exhausted. If their bid is accepted, subjects receive the difference between the price they asked and the cost of their Chip; otherwise they get nothing. Stage 2 is repeated 10 times. Note that after completion of stage 1 and before the start of the first round of stage 2, participants in T1 and T2 are informed about the outcomes of stage 1 including whether they were randomly selected or not. Note also that stage 1 does not involve an active decision of the participants. T3 is implemented as a control treatment without pre-existing fixed price scheme. T3 thus only implements the conservation auction, identical to the one implemented in stage 2 of T1 and T2. Comparing T1 and T2 to T3 will allow us to test whether bids in the conservation auction are affected by the presence of a pre-existing fixed price scheme.

Using a between-subject design, our experiment thus varies the pre-existence of a fixed-price environment as well as the price that is offered therein using (i) a control condition with no previous-fixed price scheme (T3), (ii) an upper-bound condition in which the stage-1 price-level of the fixed price scheme is set at 300 (i.e. the upper bound of the distribution of Chip costs; T2), and (iii) a midpoint condition in which the price-level of the stage 1 fixed-price scheme is set at 155 (T1). In our set-up the budget of the mechanisms (the conservation auction and the fixed price mechanism, using either high or medium prices) is always equal to 1200.[34] At the start of the session subjects are informed about their Chip costs, the distribution from which the Chip costs are drawn, the size of the budget, and that they will remain matched with the same nine (anonymous) other members of their group for the entire session.

[34] This means we can keep a similar degree of competition as in the original paper of Schilizzi & Latacz-Lohmann (2007). Please also note that, in T2, with 10 independent draws the average Chip cost will be close to the midpoint. Therefore, we expect to have few non-eligible participants under the fixed-price scheme.

Table 4.1: Experimental stages and treatments.

Treatment / Stage	Stage-1	Stage-2
Control (T3; N=60)	No PES	Conservation auction
Midpoint (T2; N=60)	Fixed price = 155	Conservation auction
Upper-Bound (T1; N=60)	Fixed price = 300	Conservation auction

Note: N is the number of participants. In each treatment, we have 6 groups of 10 participants.

4.3.3. Procedural details

The experimental sessions took place at the laboratory for economics research (LaER), University of Osnabrück, Germany, using SoPHIE (Hendriks, 2012) as software platform and university students as subjects. We ran 9 sessions with 20 subjects in each session, which implies a total of 180 subjects (i.e. 18 groups of 10 subjects). Only one randomly selected part (either preference experiments – part 1 or part 2 - or auction experiments – part 3) was relevant for cash payments at the end of the experiment.[35] A typical session lasted about one hour and the average payment amounted to 10.22 Euros.

4.4. Hypotheses

We formulate our research hypotheses based on the literature review (presented in Section 2) on reference- and path-dependent behavior and the impact of repeated trials on bidding in auctions.

First, evidence on the impact of reference points in price (e.g., Genesove & Mayer, 2001) and market settings (e.g., List & Shogren, 1999) in which price bids follow the price observed in a previous market period, suggests that subjects may adjust their bids in the conservation auctions according to the price-level observed in the fixed-price scheme. Also, evidence on repeated market and auction trials posit that multiple bidding rounds may attenuate the effect of subjects adjusting their bids according to the price-level observed in the fixed-price scheme.

H1.1 ('reference point')*: If there is a pre-existing fixed-price scheme, subjects use the pre-existing price as reference point for bidding in the conservation auction.*

H1.2 ('market experience')*: Multiple bidding rounds attenuate such adjustments in bidding over the interaction.*

[35] If the preference experiments were selected, either the risk-preference experiment or the social-preference experiment was randomly chosen as payoff relevant. If the auction experiments were selected, one round was randomly chosen as payoff relevant.

Second, according to the reference point effect (Genesove & Mayer, 2001; Loomes et al., 2003), we expect that conservation auctions perform less well when there is a pre-existing fixed-price scheme than when there is no such scheme. In evaluating the performance of conservation auctions, three criteria are used: the proportion of contracts awarded, the informational rents obtained, and the budgetary cost-effectiveness. The definition of these measures will be provided in section 4.5.

H2.1 ('overall performance'): *When there is a pre-existing fixed-price scheme, conservation auctions perform less well than when there is no pre-existing fixed-price scheme.*

H2.2 ('repetition effect'): *Repeated-round conservation auctions do not eliminate behavioral anomalies, meaning that conservation auctions still perform less well when they are preceded by a fixed-price scheme.*

4.5. Results

We first present descriptive statistics on subject's behavior in the different parts of the experiment (section 4.5.1). We then test our research hypotheses (sections 4.5.2 and 4.5.3).

4.5.1. Descriptive statistics

Among the 180 subjects who participated in our experiment, eight subjects made inconsistent choices in the risk-preference elicitation task and seven subjects made inconsistent choices in the social preference elicitation task. This leaves a total of 168 subjects with consistent choices in both tasks. We focus our analysis on these 168 subjects.[36]

Table 4.2 presents a summary of descriptive statistics of key characteristics of the participants in each of the three treatments. Part 1 reports individual risk preferences and Part 2 shows the frequency of social preference types. Part 3 shows the number of subjects and the average opportunity cost assigned in the induced-value experiment. With respect to risk-aversion, we find that across treatments, the majority of our subjects can be classified as risk-averse (Chi-square test for 3 independent samples: p=0.570). Likewise, the majority of our subjects can be classified as efficiency loving (i.e. altruistic) (71% for treatment 1, 67% for treatment 2 and 66% for treatment 3) (Chi-square test for 3 independent samples: p=0.838). This is in line with Balafoutas et al. (2012) who find that 71% of their subjects are efficiency minded. Inspection of the average level of assigned opportunity costs suggests that they are somewhat higher in

[36] However, our results remain robust including also the subjects with inconsistent choices; results are available upon request.

T1 compared to the other treatments, although the difference is not statistically significant (Kruskal-Wallis one-way analysis of variance: p=0.1057). . Nevertheless, to address the potential bias, we control for differences in opportunity cost by including it as an independent variable in our econometric analysis (see Section 4.5.2).

Table 4.2: Descriptive statistics.

Part 1: Risk preferences	**Upper-bound (T1)**	**Mid-point (T2)**	**Control (T3)**	**p-value**
Risk index*	0.51 (0.12)	0.49 (0.16)	0.48 (0.12)	0.434
% of risk averse subjects	87	87	93	0.570
Part 2: Social preferences				
% of efficiency-minded (altruistic) subjects	71	67	66	0.838
% of inequality averse subjects	15	16	18	0.891
% of spiteful subjects	9	12	11	0.862
% of inequality loving subjects	5	5	5	0.999
Part 3: Auction				
Number of subjects (N)	55	57	56	
Budget size (points)	1200	1200	1200	
Average opportunity cost (standard deviation)	158.8 (80.2)	133.2 (77.5)	129.5 (73.7)	0.1057

Note: Standard deviations in parentheses; p-values of the difference in distributions based on a Chi2 test. We use the Kruskal-Wallis one-way of analysis of variance by ranks for risk index and average opportunity cost. *Risk index: For rational subjects, the risk index ranges from 0.1 (if a subject always chooses the safe payoff) to 1.0 (if a subject chooses the safe payoff only in the last decision problem). A higher value of the risk index means a lower degree of risk aversion. The proportion of risk averse subjects indicates the share of rational subjects who prefer safe options that are lower than or equal to the expected value of the lottery (50 points).

4.5.2. Impact of pre-existing fixed-price schemes on bids and information rents

We first examine the impact of our treatments on subjects' bids (hypotheses 1.1. and 1.2). We find the following.

RESULT 1.1 ('reference point')*: Exogenously provided reference prices influence subjects' bids in conservation auctions (in round 1 and across all 10 rounds). This effect is, however, statistically significant only in the case where the fixed price is the upper-bound of the distribution of opportunity costs.*

Support for Result 1.1. First, we compare bids in round 1 between treatment and control conditions. We find that bids in round 1 of treatment 'Upper bound' are not significantly different from the bids in the control condition (Mann-Whitney U test, two-sided, $p = 0.207$; N1 = 55, N3 = 56). Likewise, the bids in round 1 of treatment 'Midpoint' are not statistically different from the control condition (Mann-Whitney U test, two-sided, $p = 0.906$; N2 = 57, N3 = 56). Second, we compare average bids across all rounds. We find that the average bids in treatment 'Upper bound' (237) and treatment 'Midpoint' (220) are higher than the average bid in our control condition (208), although the difference is significant only in case of the upper-bound condition (Mann-Whitney U test, two-sided; control vs upper bound: $p = 0.0374$, N1 = 6, N3 = 6; control vs mid-point: $p = 0.6310$).[37] Figure 4.2 provides further evidence in support of result 1.1. We note however, that higher bids in T1 could be potentially due to the fact that opportunity costs were also slightly (yet not significant) higher in that treatment, compared to T2 and T3. We use OLS regressions[38] to check the robustness of our result and control for opportunity costs in those regressions (see table 4.3) The full panel regression of subjects' bids on opportunity cost, risk preferences, social preferences and BIG5 personality traits is also presented in the appendices. According to it, social preferences and BIG5 personality traits do not affect subjects' bids. When controlling for the level of opportunity cost, the effect of the pre-existence of a fixed-price scheme is statistically significant for the 'Upper bound' and 'Midpoint' treatments. Our regression results thus lead us to confirm H1.1 (*reference point effect*).

RESULT 1.2 ('market experience'): *Multiple bidding rounds do not attenuate subjects' adjustments in bidding over time. To the contrary, the pre-existing fixed price significantly influences bids in later rounds in both treatments (regardless of the level of the fixed price).*

Support for Result 1.2. We compare average bids across the last five rounds between each treatment and the control condition. We find that the average bids over the last five rounds of treatments 'Upper bound' (240) are significantly higher than the control treatment (205), but not so for the 'Midpoint' (227) (control vs upper bound: Mann-Whitney U test, two-sided, p =

[37] In addition, we find that bids in our treatments are statistically different from pre-existing price levels 300 and 155, respectively.
[38] We ran the test of the Box-Cox model to make the choice between a linear model and a log-linear model.

0.025, N1 = 6, N3 = 6; Control vs midpoint: Mann-Whitney U test, two-sided, p = 0.522, N2 = 6, N3 = 6).

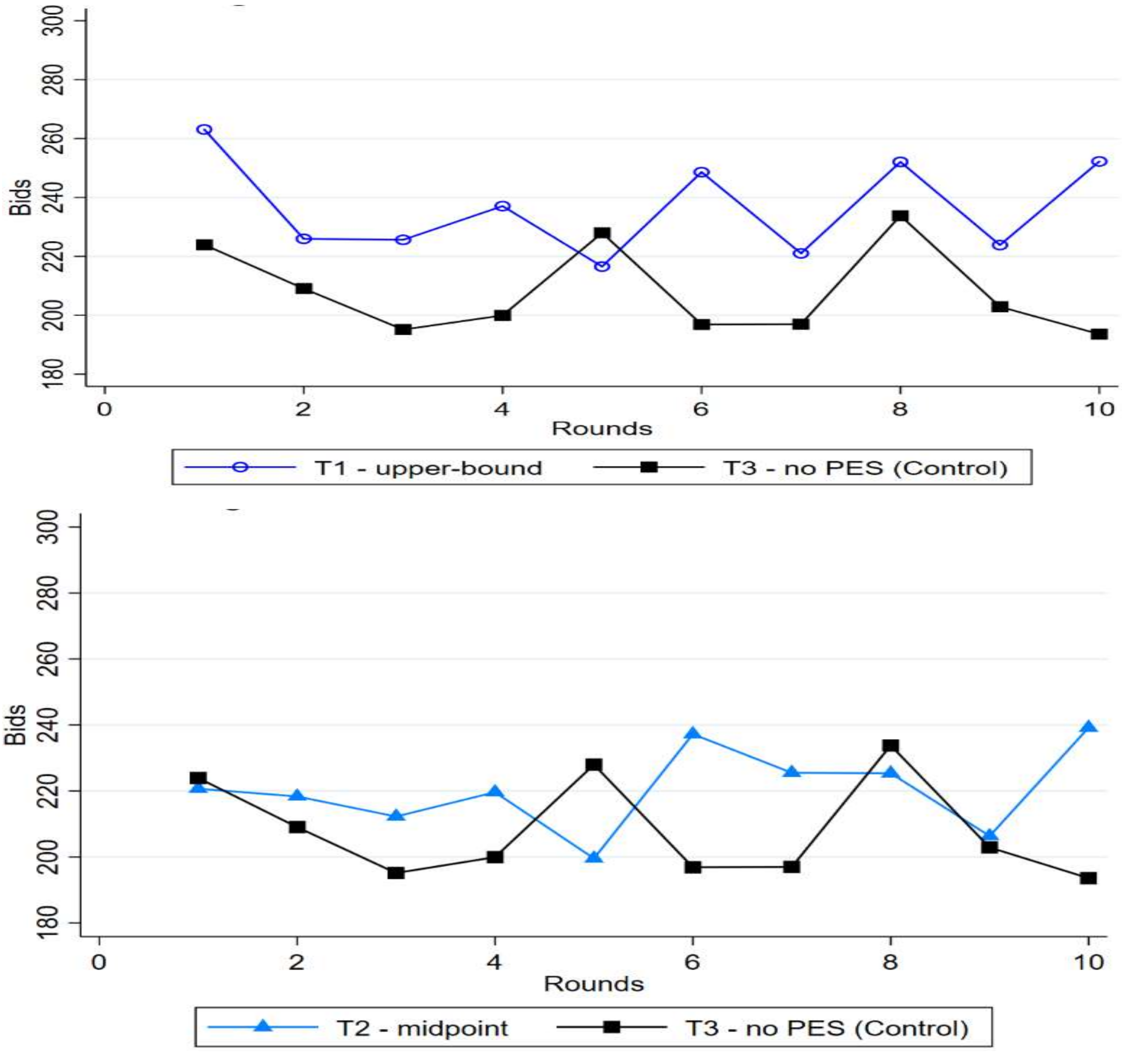

Figure 4.2: Average bids across rounds and treatments.

Second, we compare the average bids of the last round across treatments. We find that the average bids of the last round of treatments 'Upper bound' (252) and 'Midpoint' (239) are significantly higher than that of the control condition (194) (control vs upper bound: Mann-Whitney U test, two-sided, p = 0.001, N1 = 55, N3 = 56 ; Kolmogorov-Smirnov two-sample test, two-sided, p = 0.001. Control vs midpoint: Mann-Whitney U test, two-sided, p = 0.128, N2 = 57, N3 = 56).

Table 4.3: OLS regression of subjects' bids on the pre-existing price level.

	Round 1 (Model 1)		**Round 10 (Model 2)**		**Round 1-10 (Model 3)**	
	Coefficient	Standard errors	Coefficient	Standard errors	Coefficient	Standard errors
Opportunity cost	0.792***	0.094	0.657***	0.202	0.003***	0.000
T1 (Upper-bound)	30.081***	3.457	63.187***	9.092	0.064***	0.015
T2 (Midpoint)	-3.371	7.044	68.102***	6.523	0.084***	0.009
Constant	94.400*	42.304	-96.655	7.672	4.893***	0.104
Individual controls	YES		YES		YES	
Group dummies	YES		YES		YES	
N	168		168		168	
R^2	0.340		0.270		0.607	

Note: The table reports ordinary least squares estimates (heteroskedasticity-robust and cluster-robust (at the group level) standard errors in parentheses). Models 1 and 2 are linear models, which use first-round bid and tenth-round bid as dependent variables, whereas model 3 is a log-linear model, which uses the logarithmic transformation of average bid over 10 rounds as dependent variable. Opportunity cost is subject's opportunity (chip) cost. T1 is a binary variable that takes the value 1 if the pre-existing price is the upper bound of the distribution of opportunity costs and 0 otherwise. T2 is a binary variable that takes the value 1 if the pre-existing price is the midpoint of the distribution of opportunity costs and 0 otherwise. Coefficients reflect differences as compared to the control treatment (no pre-existing price before conservation auctions). Individual controls include age, gender, income, social preference traits, risk index, and fairness perception of the auction. Group dummies are variables that take 1 if the subject belongs to a specific group and 0 otherwise.***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

We also ran the OLS regression for bids in round 10, controlling again for opportunity costs. The regression results confirm that bids are significantly higher in T1 – 'Upper bound' (see table 4.3, model 2). Therefore, we reject H1.2 (*market experience*). Our findings actually suggest that repetition enhances the effect for the 'Midpoint' treatment, so that both treatments show a significant effect of the prior fixed price on bids in later rounds.

4.5.3. Impact of pre-existing fixed-price schemes on performance of conservation auctions

Regarding the performance of conservation auctions, we use the following criteria for our evaluation: the proportion of contracts awarded[39] (i.e. the more contracts awarded, the better[40]), the informational rent (i.e. the lower the informational rent, the better), and the budgetary cost-effectiveness (i.e., the lower the payment per unit of ecosystem service provision, the better). The informational rent is calculated as the difference of the obtained payment from the auction and subject's assigned opportunity costs. Budgetary cost-effectiveness is calculated by dividing the total sum of payments made by the number of chips sold. Table 5.4 summarizes all performance criteria across treatments. To the degree applicable, it also provides comparable values for the performance of the fixed-price schemes across all three treatments.

We first analyze the impact of our treatments on the proportion of contracts awarded. We find the following.

RESULT 2.1.1 ('proportion of contracts – overall performance'): *The pre-existence of a fixed-price scheme does not affect the proportion of contracts awarded by conservation auctions.*

RESULT 2.2.1 ('proportion of contracts – repetition effect)*: Repeated-round conservation auctions do not affect the proportion of contracts awarded.*

Support for Results 2.1.1 and 2.2.1. Table 5.4 reports the proportions of contracts awarded by each allocation mechanism in each treatment in terms of averages across all ten rounds. We compare the proportions of contracts awarded by conservation auctions in each round (round 1 to round 10) in our control treatment (T3) with that in treatment 'upper bound' (T1) and 'midpoint' (T2). We find that, in each round, there is no statistically significant difference[41] (Fisher exact probability test: two-sided; T1 vs T3 for round 1: $p = 0.567$, N1 = 55, N3 = 56. T2 vs T3 for round 1: $p = 0.848$, N2 = 57, N3 = 56. T1 vs T3 for round 10: $p = 0.255$, N1 = 55,

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[39] The percentage of contracts could be interpreted as a proxy of what some authors call P-OCER. P-OCER is calculated by dividing the ratio between the number of contracts awarded and the available budget by the ratio between the number of available contracts and the available budget. See Cason & Gangadharan(2005) and Cason et al. (2003).

[40] We assume that environmental impacts per unit of land conserved are equal across farmers, so that a larger number of contracts implies higher environmental effectiveness (ecosystem service provided).

[41] The result holds for every round.

N3 = 56. T2 vs T3 for round 10: p = 0.849, N2 = 55, N3 = 56). This result leads us to reject H2.1 and H2.2 if we use the proportion of awarded contracts as criterion for evaluating the performance of conservation auctions.

Table 4.4: Performance of conservation auctions across treatments.

	Upper-bound (T1)		**Mid-point (T2)**		**Control (T3)**
	FP	A	FP	A	A
Average bid	-	236.60 (16.23)	-	220.41 (12.42)	208.02 (15.01)
Average # of contracts awarded[42]	22	30 (1.08)	33	35 (2.22)	35 (0.73)
% of contracts awarded	40	54	58	61	62
Average informational rent	137.36 (15.94)	73.11 (10.42)	74.42 (46.73)	76.17 (8.08)	79.07 (4.68)
Budgetary cost-effectiveness*	300	194.03 (7.73)	155	178.61 (7.66)	179.89 (6.46)

Note: Values are averages across all ten rounds, in the case of auctions. Standard deviations in parentheses. % of contracts awarded is the proportion of subjects selected in each treatment. FP means fixed-price; A means Auction. *Payment per unit of ecosystem service.

We then examine the impact of the pre-existence of a fixed-price scheme on the level of information rent collected in conservation auctions. We find that:

RESULT 2.1.2 ('informational rent – overall performance')*: The pre-existence of a fixed-price scheme does not affect the level of informational rent in conservation auctions.*

RESULT 2.2.2 ('informational rent – repetition effect'): *Repeated-round conservation auctions do not affect the level of informational rent.*

[42]The average number of contracts awarded is the sum of contracts awarded over 10 rounds divided by 10 (the number of rounds).

Support for Results 2.1.2 and 2.2.2. Again, we provide a between-treatment comparison to evaluate hypothesis 2, now focusing on informational rents. We find that, in round 1, the information rent collected in auctions without a pre-existing fixed-price scheme (T3) is not different from that of the treatments with a pre-existing fixed price (T1 and T2) (Mann-Whitney U test: two-sided; T3 vs T1: p = 0.349, N1 = 55, N3 = 56. T3 vs T2: p = 0.941, N2 = 57, N3 = 56). Likewise, when we consider the average informational rent collected over 10 rounds, we also find that there is no difference between treatments (T1 and T2) and the control condition (Mann-Whitney U test: two-sided test; T3 vs T1: p = 0.109, N1 = 6, N3 = 6; T3 vs T2: p = 0.630, N2 = 6, N3 = 6). Together, this leads us to formulate result 2.1.2.

For result 3.2.2, we do the same analysis for the last five rounds and for the last round. When we consider the average informational rent collected over the last five rounds, we find that there is no difference between treatments (T1 and T2) and the control condition (Mann-Whitney U test: two-sided test; T3 vs T1: p = 0.109, N1 = 6, N3 = 6. T3 vs T2: p = 0.631, N2 = 6, N3 = 6). Likewise, in the last round, there is no difference between treatments (T1 and T2) and the control condition (Mann-Whitney U test: two-sided test; T3 vs T1: p = 0.403, N1 = 55, N3 = 56. T3 vs T2: p = 0.990, Kolmogorov-Smirnov two-sample test: one-sided test, p= 0.792, N2 = 57, N3 = 56). A further econometric regression (see table 4.5), using panel data models, indicates that treatments 1 and 2 do not affect the informational rent collected. Together, our results lead us to reject H2.1 and H2.2 if we use informational rent as criterion for evaluating the performance of conservation auctions.

Finally, we evaluate the impact of the pre-existence of a fixed-price scheme on the budgetary cost-effectiveness of conservation auctions. For this, we compute budgetary cost-effectiveness in each round as the payment per unit of ecosystem service, i.e., the total payment divided by the number of chips sold. We find that:

RESULT 2.1.3 ('budgetary cost-effectiveness – overall performance')*: The pre-existence of a fixed-price scheme only increases the payment per unit of ecosystem service in conservation auctions in the first round. The effect is not significant for the case where the fixed price is the midpoint of the distribution of opportunity cost.*

RESULT 2.2.3. ('budgetary cost-effectiveness – repetition effect'): *Repeated-round conservation auctions attenuate the effect on the budgetary cost-effectiveness.*

Table 4.5: Pooled FGLS and random-effects panel-data regressions of informational rent.

	Pooled FGLS (Model 1)		Random-effects (Model 2)	
	Coefficient	Standard errors	Coefficient	Standard errors
Opportunity cost	-0.591***	0.023	-0.592***	0.023
T1 (Upper-bound)	-19.279	17.300	-19.685	17.430
T2 (Midpoint)	-23.312	8.666	-23.517	8.469
Constant	138.218***	17.013	138.122***	17.040
Individual controls	YES		YES	
Group dummies	YES		YES	
Round	YES		YES	
N	1,680		1,680	
Prob > Chi2	0.000		0.000	

Note: The table reports feasible generalized least squares estimates (Cluster-robust standard errors in parentheses. We specify an AR(1) error correlation over round for individual *i*, as the model fails to converge with no specific restriction on the error correlation. However, the other error specifications give the same result.) and random-effects estimates The dependent variable is subject's information rent. Opportunity cost is subject's opportunity (Chip) cost. T1 is a binary variable that takes the value 1 if the pre-existing price is the upper bound of the distribution of opportunity costs and 0 otherwise. T2 is a binary variable that takes the value 1 if the pre-existing price is the midpoint of the distribution of opportunity costs and 0 otherwise. Coefficients reflect differences as compared to the control treatment (no pre-existing price before conservation auctions). Individual controls include age, gender, income, distributional (or "social") preference traits, risk index, and fairness perception of the auction. Group dummies are variables that take 1 if the subject belongs to a specific group and 0 otherwise. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

Support for Results 2.1.3 and 2.2.3. First, we focus on first five rounds and all rounds to analyze the overall performance. Second, we focus on last five rounds and the last round to analyze the repetition effect. Our between-treatment comparison indicates that, in round 1, payments per unit of ecosystem service in conservation auctions are higher when there is a pre-existing fixed-price scheme. However, the difference is only significant when the pre-existing fixed price is the upper-bound of the distribution of opportunity cost (Mann-Whitney U test: two-sided test; T1 vs T3: $p = 0.043$, N1 = 55, N3 = 56; T2 vs T3: $p = 0.290$, N2 = 57, N3 =

56). When we consider the average payment across the first five rounds, we do not find significant differences (Mann-Whitney U test: two-sided test; T1 vs T3: p = 0.109, N1= 6, N3 = 6; T2 vs T3: p = 0.749, N2= 6, N3 = 6). The results are also similar when we consider the average payment across all rounds. Payments per unit of ecosystem service in conservation auctions without pre-existing fixed-price schemes are not statistically different from those with pre-existing fixed-price schemes (Mann-Whitney U test: two-sided test; T1 vs T3: p = 0.109, N1 = 6, N3 = 6; T2 vs T3: p = 0.873, N2 = 6, N3 = 6), although the p-value is low (difference is close to significant) when the pre-existing price is the upper-bound of the distribution of opportunity costs.

We do the same analysis for the last five rounds. Using the average payments of the last five rounds, we find no evidence that payments per unit of ecosystem service in conservation auctions are significantly different when there is a pre-existing fixed-price scheme (Mann-Whitney U test: two-sided test; T1 vs T3: p = 0.522, N1 = 6, N3 = 6; T2 vs T3: p = 0.873, N2 = 6, N3 = 6). Likewise, in the last round, there is no statistically significant difference between treatments and the control condition (Mann-Whitney U test: two-sided test; T1 vs T3: p = 0.114, N1 = 27, N3 = 35; T2 vs T3: p = 0.622, N2 = 34, N3 = 35). These results are also confirmed by the regression analysis in table 4.6. In summary, we confirm H3.1 and H3.2 for T1 when using payment per unit of ecosystem service as criterion for evaluating the performance of conservation auctions, while the effect is weaker for T2.

For T1, we also compare the performance of the auction to the performance of the fixed-price scheme to check whether the alleged performance gains of introducing an auction that have been claimed in the literature prevail despite the behavioral biases shown above. For this, we compare the performance indicators for T1 in Table 5 between the fixed-price scheme and the auction. We find that, compared to the fixed-price scheme, under the auction the number and proportion of contracts (and thus ecosystem service provision in our setting) is higher (Wilcoxon matched-pairs signed-ranks test: two-sided, p = 0.004), information rents are lower (Wilcoxon matched-pairs signed-ranks test: two-sided, p = 0.000), and budgetary cost-effectiveness is improved (Wilcoxon matched-pairs signed-ranks test: two-sided, p = 0.000) as compared to the fixed-price scheme. So, while performance may be poorer (especially in the first round) than in a setting where no fixed-price scheme was previously installed, the auction still seems worthwhile implementing.

Table 4.6: Pooled FGLS and random-effects panel-data regressions of payment per unit of ecosystem service.

	Pooled FGLS (Model 1)		**Random-effects (Model 2)**	
	Coefficient	Standard errors	Coefficient	Standard errors
Opportunity cost	-0.610***	0.042	-0.610***	0.043
T1 (Upper-bound)	8.444	17.246	8.640	17.535
T2 (Midpoint)	-0.517	13.557	-0.749	13.629
Constant	186.173***	31.449	186.614***	32.277
Individual controls	YES		YES	
Group dummies	YES		YES	
Round	YES		YES	
N	1,680		1,680	
Prob > Chi2	0.000		0.000	

Note: The table reports feasible generalized least squares estimates (Cluster-robust standard errors in parentheses. We specify an AR(1) error correlation over round for individual *i*, as the model fails to converge with no specific restriction on the error correlation. However, the other error specifications give the same result.) and random-effects estimates The dependent variable is subject's payment per unit of ecosystem. Opportunity cost is subject's opportunity (Chip) cost. T1 is a binary variable that takes the value 1 if the pre-existing price is the upper bound of the distribution of opportunity costs and 0 otherwise. T2 is a binary variable that takes the value 1 if the pre-existing price is the midpoint of the distribution of opportunity costs and 0 otherwise. Coefficients reflect differences as compared to the control treatment (no pre-existing price before conservation auctions). Individual controls include age, gender, income, distributional (or "social") preference traits, risk index, and fairness perception of the auction. Group dummies are variables that take 1 if the subject belongs to a specific group and 0 otherwise. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

4.6. Discussion and conclusions

The purpose of this paper is to examine the impact of pre-existing fixed-price schemes on subjects' bids in and the performance of conservation auctions. We use the midpoint and the upper bound of the distribution of opportunity costs as pre-existing fixed prices, which can be viewed as realistic benchmarks for policy settings. Regarding behavioral responses, we find that the reference price provided by the fixed-price scheme influences subjects' bids in conservation auctions and this effect is not attenuated by repeating the auction. Moreover, we find that this behavioral response has implications for the budgetary cost-effectiveness in the first round of the auction: the pre-existence of the fixed-price scheme increases the amount spent per unit of ecosystem service in conservation auctions in the first round. However, all of these effects are significant only when the fixed price is set at a high level, in our case at the upper bound of opportunity costs. Also, we do not find an impact of a pre-existing fixed price scheme on the number of contracts awarded nor on information rents.

Previous studies have called for allocating conservation contracts by using auctions (Alix-Garcia et al., 2009; Jindal et al., 2013; Porras et al., 2013; Schilizzi & Latacz-Lohmann, 2007; Stoneham et al., 2003; Windle & Rolfe, 2008). Our study indicates that the pre-existing institution and/or the pre-existing environmental service price can matter for the performance of conservation auctions, especially in the case of one-shot conservation auctions. Conservation auctions may perform less well when there is a high pre-existing fixed-price scheme than when there is not. Our results imply that it is important for policymakers to take the pre-existing institution into account when evaluating the opportunities for implementing an auction, especially a one-shot auction. From an academic perspective, our research confirms the price-following hypothesis, especially when the fixed price is high. However, we also find an overall performance gain from implementing the auction compared to a fixed-price scheme, even in the case where there is a pre-existing high fixed price. So while behavioral effects may impact the degree of the cost effectiveness gain that can be achieved through auctions, our results do not contradict the possibility of such a gain *per se*.

Two caveats are in order when considering the results. First, our study was conducted in a laboratory, using university students as subjects. Second, our parametric setting, although close to Schilizzi & Latacz-Lohmann (2007), is not inspired by a real case study. Further empirical research should examine the impact of pre-existing fixed-price schemes on conservation auctions in a real PES program. This could increase the behavioral effects found in this paper. It remains to be studied whether the effect can be large enough to override the general performance gains from implementing auctions.

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Appendices

Appendix A – Appendix to chapter 2

A1: The design of the experiment.

Part 1 – Preference measurements		**Part 2 – Main experiment**					**Part 3 - Questionnaire**
Stage 1.1	**Stage 1.2**	**Stage 2.1**	**Stage 2.2**	**Stage 2.3**	**Stage 2.4**	**Stage 2.5**	**Stage 3.1**
Risk preferences	Social (distributional) preferences	Discriminatory auction (low budget)	Choice of a PES allocation mechanism	Discriminatory auction (high budget)	Choice of a PES allocation mechanism	Discriminatory auction (low budget)	Questionnaire
Certainty equivalence test	Equality equivalence test	Conservation auction	Discriminatory auction versus Fixed-price scheme	Offering the Chip to the buyer by submitting a claim	Discriminatory auction Versus Fixed-price scheme (budget size S2)	Conservation auction	Question on the relative fairness perception of allocation mechanisms and Age, Gender, Field of studies, Personal attitudes test (Big 5)

A2: The Certainty Equivalence Test (CET).

Row Nr.	Alternative LEFT	Your choice		Alternative RIGHT
	You receive			You have a …% chance of receiving 100 points and a …% chance of receiving 0 points
1	**10 points for sure**	LEFT ◯	◯ RIGHT	**50 -- 50**
2	**20 points for sure**	LEFT ◯	◯ RIGHT	**50 -- 50**
3	**30 points for sure**	LEFT ◯	◯ RIGHT	**50 -- 50**
4	**40 points for sure**	LEFT ◯	◯ RIGHT	**50 -- 50**
5	**50 points for sure**	LEFT ◯	◯ RIGHT	**50--50**
6	**60 points for sure**	LEFT ◯	◯ RIGHT	**50--50**
7	**70 points for sure**	LEFT ◯	◯ RIGHT	**50--50**
8	**80 points for sure**	LEFT ◯	◯ RIGHT	**50--50**
9	**90 points for sure**	LEFT ◯	◯ RIGHT	**50--50**
10	**100 points for sure**	LEFT ◯	◯ RIGHT	**50--50**

Source: Adapted from Balafoutas, Kerschbamer, & Sutter (2012), Dohmen & Falk (2011) and Dohmen et al. (2010). Note: Since the expected value of the lottery is 50 points, risk averse subjects should prefer the safe options that are smaller than or equal to 50 points over the lottery. Risk loving subjects should choose the lottery when the safe option is greater than 50 points.

A3: The Equality Equivalence Test (EET).

Row Nr.	LEFT		Your choice		RIGHT	
	You receive	Other person receives			You receive	Other person receives
1	**80 points**	**130 points**	LEFT ◯	◯ RIGHT	**100 points**	**100 points**
2	**90 points**	**130 points**	LEFT ◯	◯ RIGHT	**100 points**	**100 points**
3	**100 points**	**130 points**	LEFT ◯	◯ RIGHT	**100 points**	**100 points**
4	**110 points**	**130 points**	LEFT ◯	◯ RIGHT	**100 points**	**100 points**
5	**120 points**	**130 points**	LEFT ◯	◯ RIGHT	**100 points**	**100 points**
6	**80 points**	**70 points**	LEFT ◯	◯ RIGHT	**100 points**	**100 points**
7	**90 points**	**70 points**	LEFT ◯	◯ RIGHT	**100 points**	**100 points**
8	**100 points**	**70 points**	LEFT ◯	◯ RIGHT	**100 points**	**100 points**
9	**110 points**	**70 points**	LEFT ◯	◯ RIGHT	**100 points**	**100 points**
10	**120 points**	**70 points**	LEFT ◯	◯ RIGHT	**100 points**	**100 points**

Source: Adapted from Balafoutas et al. (2012) and Kerschbamer (2015).

A4: Comparison between papers for the distribution of social preference types.

	Our paper	Balafoutas et al. (2012)
Efficiency loving	61.29%	71.21%
Inequality averse	17.74%	15.91%
Inequality loving	8.87%	0%
Spiteful	12.10%	12.88%

A5: The optimal bid.

The landowner chooses bid b to maximize her expected utility:

$$\max_b P(b \leq \beta)\ U(\pi_1 + b) + [1 - P(b \leq \beta)]\ U(\pi_0)$$

Rather than solving this problem directly, we follow Latacz-Lohmann and van der Hamsvoort (1997) by rewriting it to maximize the land owner's certainty equivalent income associated with participating in the conservation auction, which is equal to the expected returns from participating in the auction minus a risk premium. Rewriting the problem in certainty-equivalent payment levels, we have:

$$\max_b P(b \leq \beta)\ (\pi_1 + b) + [1 - P(b \leq \beta)]\ \pi_0 - RP^{AUC}(b, X)$$

The problem may also be rewritten as follows:

$$\max_b [1 - F(b)]\ (\pi_1 + b) + F(b)\ \pi_0 - RP^{AUC}(b, X)$$

Applying the first order condition, we have:

$$-f(b^*)\ (\pi_1 + b^*) + [1 - F(b^*)] + f(b^*)\ \pi_0 - \frac{\partial RP^{AUC}(b^*, X)}{\partial b} = 0$$

After rearraging the terms, we have:

$$b^* = \pi_0 - \pi_1 + \frac{[1 - F(b^*)]}{f(b^*)} - \frac{\partial RP^{AUC}}{\partial b}(b^*, X)$$

If the land owner is risk neutral and only cares about the financial consequences of participating in either of the two programs, $RP^{AUC}(b^*, X) = RP^{FP}(z, X) = 0$. In that case, a land owner's optimal bid is:

$$b^* = \pi_0 - \pi_1 + \frac{1 - F(b^*)}{f(b^*)}$$

A6: The risk-neutral landholder's preference over PES allocation mechanisms.

The risk-neutral landholder prefers the auction if her expected benefit from the auction is higher than her expected benefit from the fixed-price scheme:

$$V^{AUC} > V^{FP}$$

This inequality may also be rewritten as follows:

$$p_b(\pi_1 + b^*) + (1 - p_b)(\pi_0) > \; p_Z(\pi_1 + z) + (1 - \; p_Z)(\pi_0)$$

Or

$$[1 - F(b)](\pi_1 + b^*) + F(b)\,\pi_0 > \; p_Z(\pi_1 + z) + (1 - \; p_Z)(\pi_0)$$

After replacing b^* with $\pi_0 - \pi_1 + \frac{1-F(b^*)}{f(b^*)}$ and rearranging the terms, we have:

$$\pi_0 - \pi_1 > z - \frac{\left(1 - F(b^*)\right)^2}{f(b^*)p_z}$$

A7: Instructions – Experiment (in English).

Welcome and Thank You for Your participation!

Please read the instructions carefully. The amount of money you earn during the experiment depends on your own decisions and those of the other participants. The experiment and the entire interaction between you and the other participants takes place via computer terminals and will take about 60 minutes.
You are not allowed to communicate with other participants. If you violate this rule you will be expelled from the experiment without payment. Please note that neither the academic staff nor the other participants will be able to link you to your anonymous decisions. If you have any questions during the experiment, please raise your hand. We will then come to you and answer your question in a subdued voice. The question is subsequently answered aloud so that other participants are also informed.

Basic Procedure of the Experiment

The experiment consists of several parts. During each part you are asked to make one or more decisions. Please note that your decisions in the different parts of the experiment are completely independent of each other and that the result of your decisions will be communicated to you at the end of the experiment.

Payment

Your payout at the end of the experiment is calculated as follows: You will receive 3 Euro, regardless of your decisions in the experiment. You will also receive a payment depending on the decisions you made in a randomly selected part of the experiment. During the experiment, your earnings are calculated in points (currency unit of the experiment). At the end of the experiment, your earnings will be converted into Euro using the following exchange rate:

1 Point = € 0.10 (10 Eurocents)

You will receive your payment in cash at the end of the experiment upon presentation of your participation code.

Instructions: Part A

In this first part of the experiment you have to make 10 decisions. Each of your decisions consists of the choice between a LEFT and a RIGHT alternative.
In each of the 10 decision situations, the LEFT alternative provides you with guaranteed earnings. The earnings for the RIGHT Alternative, however, depends on chance.

Example: You might be asked if you prefer the LEFT alterative, which will definitely give you 70 points, or whether you want to choose the RIGHT alternative, where the probability of earning 100 points is 50% and the probability of earning 0 points is 50%. You would then have to choose one of the two alternatives. This decision-making problem would be displayed on your screen as follows:

Alternative LEFT	Your choice		Alternative RIGHT
You receive			You receive
70 points with 100% certainty	LEFT ◯	◯ RIGHT	**50% probability of 100 points and a 50% probability of earning 0 points**

In this first part of the experiment you will make a total of 10 such decisions. Your earnings in this section shall be determined as follows:

At the end of the experiment, one of the 10 decision situations is randomly selected for each participant and the merit is determined according to the choice made. By way of illustration: If the above-mentioned decision situation were chosen at random and if you choose the RIGHT alternative here, you would receive 100 points with a probability of 50% and 0 points with a probability of 50%. Whether you actually receive 100 or 0 points is randomly determined by the central computer.

Please raise your hand if you have a question. We will then come to you and answer your questions individually.

When you have understood the instructions, please click on "continue".

Instructions: Part B

In this part of the experiment you have to make 10 decisions. In each of the decision situations, you form a pair with one or another participant. This person is henceforth referred to as the "other person". This "other person" is randomly and anonymously assigned to you by the central computer.

Each of your decisions is again a choice between the alternatives LEFT and RIGHT. Each alternative has consequences for you and the other person.

Example: You might be asked if you prefer the LEFT alternative, which will give you 110 points and the "other person" 130 points, or whether you want to choose the RIGHT alternative, where you get 100 points and the "other person" 100 points. You then have to choose one of the two alternatives. This decision-making problem would be displayed on your screen as follows:

LEFT		Your Selection		RIGHT	
You receive	The other person receives			You receive	The other person receives
110 Points	**130 Points**	LEFT ◯	◯ RIGHT	**100 Points**	**100 Points**

In this second part of the experiment you will make a total of 10 such decisions. Your earnings from this part are determined as follows:

At the end of the experiment, the central computer will randomly determine whose decisions - yours or the "other person's" decisions - are used. Next, the computer will randomly select one of this person's 10 decisions and execute that decision. For example, if the above-mentioned decision situation was randomly selected and your decision was taken and you had chosen the LEFT alternative, you would receive 110 points and the "other person" 130 points.

Please raise your hand if you have a question. We will then come to you and answer your questions individually.
When you have understood the instructions, please click on "continue".

Instructions: Part C

Part C of the experiment consists of 5 sections. The result of section 1 is relevant for section 2 and the result of section 3 is relevant for section 4, while the result of section 5 stands alone. So at the end of part C there will be 3 results. If part C is used as a payout-relevant part, one of these 3 results is randomly selected to determine your payout.

Part C, Section 1

At the beginning of Part C, the central computer will group all participants into groups of 10 people. You will remain in this group of 10 persons during part C.

In this part, each of the 10 members of your group (you included) will receive a chip that you can offer to a buyer - the computer. Each chip costs a certain amount. The cost of a chip is randomly chosen between 5 points and 264 points. Each of the numbers between 5 and 264 is equally likely to be selected. All members of your group are equally likely to own a chip at a specific cost. The cost of your chip will be shown on your computer screen. For the remainder of Part C, the cost of your chip will remain the same.

Your task

You can offer your chip to the buyer by indicating a price - the same applies to the other 9 members of your group. Please note: The price you specify cannot be lower than the cost of your chip.

Selection of the Winner

Rank order: The computer will then rank all 10 bids according to their price from low to high. Starting with the lowest bid, he or she accepts bids in the order of ranking until his or her budget of 975 points is used up.

Please note that you will never know the costs of the chips of the other participants in your group. Conversely, the other participants do not receive any information about the cost of your chip.

That means:

- You compete with the other 9 participants in your group to ensure that your bid is accepted.
- The lower your bid, the more likely it is to be accepted.

Your earnings

Your earnings in this section are calculated as follows:

- If your price is accepted, you will receive the price minus the cost of your chip.
- If the price you specify is not accepted, you will receive 0 points

Example: Suppose your chip costs 200 points and you make a bid of 300 points. Your earnings in this section are therefore 100 points (therefore 300 - 200), but only if your offer has been accepted. Otherwise you will receive 0 points in this section.

Summary

- Your group consists of 10 participants (you and 9 others)
- Each participant receives 1 chip.
- Each chip is associated with costs. The actual cost is determined randomly in intervals between 5 and 264 points.
- To sell the chip to the buyer, you have the option of submitting an offer to the buyer by indicating to him/her a price for the chip.
- The buyer will sort all offers according to their price from the lowest to the highest. Starting again with the lowest bid, the computer will accept bids until its budget of 975 points is exhausted.
- If your offer is accepted, you will receive the price of your chip minus the cost of your chip.
- If your offer is rejected, you will receive 0 points.

Please raise your hand if you have a question. We will then come to you and answer your questions individually.

Once you have understood the instructions, please click on "continue.

Part C, Section 2

The cost of your chip and that of your group members will remain the same in this section as in the previous section.

This section uses one of two rules that determine how winners are selected:

- Ranking (with the bids submitted by you and your group members in section 1) or
- Random (by chance)

Rank order: The computer will rank all 10 bids according to their price from low to high. Starting with the lowest bid, it accepts bids in the order of ranking until the budget of 2120 points is used up.

Random: The computer randomly selects winners until its budget of 975 points is exhausted. If this rule is applied, each individual offer is priced at 265 points.

- Please note that 3 out of 10 participants in your group will be selected as winners.

<u>Your task</u>

You and each of the other 9 participants in your group will be asked to vote for one of the two rules: Ranking or random. The rule with the most votes (simple majority) is then used. In the event of a tie vote, the rule to be used is selected at random.

Following this the selection of the winners and the calculation of the earnings are explained in detail: first for the 'Ranking' rule and then for the ‘Random' rule.

<u>Selection of winner (*Ranking)*</u>

- If the ranking rule is applied, the price you selected (and the prices given by the other 9 participants in your group) in section 1 will be used.
- The buyer (computer) will then sort all 10 offers according to their price from low to high. Starting with the lowest bid, it accepts bids in the order of ranking until the budget

of 975 points is used up. You therefore compete with the other 9 participants in your group to ensure that your offer is accepted.

Your earnings (ranking)

If the Ranking rule is applied, your earnings in this section will be calculated as follows:

- If your offer is accepted, you will receive the specified price minus the cost of your chip.
- If your offer is rejected, you will receive 0 points.

Selection of winner and earnings (Random)

If the random rule is used to determine the winners, your earnings will be calculated as follows:

- The buyer (computer) randomly draws winners until his budget of 975 points is exhausted. In this case, the price of each offer is fixed at 265 points.
- So 3 out of 10 participants in your group will be selected as winners.
- If you are randomly selected, you will receive 265 points minus the cost of your chip.
- If you are not selected, you will receive 0 points.

Please raise your hand if you have a question. We will then come to you and answer your questions individually.

When you have understood the instructions, please click on "continue".

Part C, Section 3

The cost of your chip and that of your group members will remain the same in this section as in the previous section.

Your task:

The cost of your chip and that of your group members will remain the same in this section as in the previous section.

Selection of winner

Rank order: The computer will sort all 10 bids according to their price from low to high. Starting with the lowest bid, it accepts bids in order of priority until the budget of 2120 points is exhausted. Please note: The budget of the computer is now higher than in sections 1 and 2.

As before, this means:

- You compete with the other 9 participants in your group to ensure that your offer is accepted.
- The lower your bid, the more likely it is to be accepted.

Your Earnings

Your earnings in this section are calculated as follows:

- If your offer is accepted, you will receive the specified price minus the cost of your chip.
- If your offer is rejected, you will receive 0 points.

Summary

- There are 10 participants in your group (you and 9 others) and each of them has received a chip with a certain cost - the costs are the same as in section 1.
- To sell the chip to the buyer, you have the option of submitting an offer to the buyer by offering him/her a price for the chip.
- The buyer will sort all offers according to their price from the lowest to the highest. Starting again with the lowest bid, the computer will accept bids until its budget of 2120 points is exhausted.
- The budget of the computer is higher than in section 1.
- If your offer is accepted, you will receive the price of your chip minus the cost of your chip.
- If your offer is rejected, you will receive 0 points.

Please raise your hand if you have a question. We will then come to you and answer your questions individually.

When you have understood the instructions, please click on "continue".

Part C, Section 4

The cost of your chip and that of your group members will remain the same in this section as in the previous section.

This section uses one of two rules that determine how winners are selected:

- Ranking (with the bids submitted by you and your group members in section 1), or
- Random (by chance)

Ranking order: The computer will rank all 10 bids according to their price from low to high. Starting with the lowest bid, it accepts bids in the order of ranking until the budget of 2120 points is exhausted.

Random: The computer randomly selects winners until its budget of 2120 points is exhausted. If this rule is applied, each individual offer is fixed at a price of 265 points.

Please note that 8 out of 10 participants in your group will be selected as winners.

Your task

You and each of the other 9 participants in your group will be asked to vote for one of the two rules: Ranking or random. The rule with the most votes (simple majority) is then used. In the event of a tie vote, the rule to be used is selected at random.

Following this the selection of the winners and the calculation of the earnings are explained in detail: first for the 'Ranking' rule and then for the ‘Random' rule.

Selection of the winners *(ranking order)*

- If the ranking rule is applied, the price you (and the prices given by the other 9 participants in your group) in section 3 will be used.
- The buyer (computer) will arrange all 10 offers according to their price from low to high. Starting with the lowest bid, it accepts bids in the order of ranking until the budget of 2120 points is exhausted. You therefore compete with the other 9 participants in your group to ensure that your offer is accepted.

Your earnings *(ranking order)*

If the Ranking rule is applied, your earnings in this section will be calculated as follows:

- If your offer is accepted, you will receive the specified price minus the cost of your chip.
- If your offer is rejected, you will receive 0 points.

Selection of Winners and their Earnings (Random):

If the random rule is used to determine the winners, your earnings will be calculated as follows:

- The buyer (computer) randomly draws winners until its budget of 2120 points is exhausted. In this case, the price of each offer is fixed at 265 points.
- This means that 8 out of 10 participants in your group will be selected as winners.
- If you are randomly selected, you will receive 265 points minus the cost of your chip.
- If you are not selected, you will receive 0 points.

Please raise your hand if you have a question. We will then come to you and answer your questions individually.

When you have understood the instructions, please click on "continue".

Part C, Section 5

Section 5 and section 1 are identical. The cost of your chip and that of your group members will remain the same in this section as in the previous section.

Your Task

You can offer your chip to the buyer by indicating a price - the same applies to the other 9 members of your group. Please note: The price you specify cannot be lower than the cost of your chip.

Selection of the Winner

Rank order: The computer will then rank all 10 bids according to their price from low to high. Starting with the lowest bid, it accepts bids in the order of ranking until the budget of 975 points is exhausted.

As before, this means:

- you compete with the other 9 participants in your group to ensure that your offer is accepted;
- the lower your bid the more likely it is to be accepted.

Your earnings

- If your offer is accepted, you will receive the specified price minus the cost of your chip.
- If your offer is rejected, you will receive 0 points.

Example: Assuming your chip costs 200 points, you make an offer for a price of 300 points. Your earnings from this section are therefore 100 points (i.e. 300-200), but only if your offer has been accepted. Otherwise, you will receive 0 points from this section.

Summary

- To sell the chip to the buyer, you have the option of submitting an offer to the buyer by giving him/her a price for the chip.

- The buyer will sort all offers according to their price from lowest to highest. Starting again with the lowest bid, the computer will accept bids until its budget of 975 points is exhausted.
- If your offer is accepted, you will receive the price of your chip minus the cost of your chip.
- If your offer is rejected, you will receive 0 points.

Please raise your hand if you have a question. We will then come to you and answer your questions individually.
When you have understood the instructions, please click on "continue".

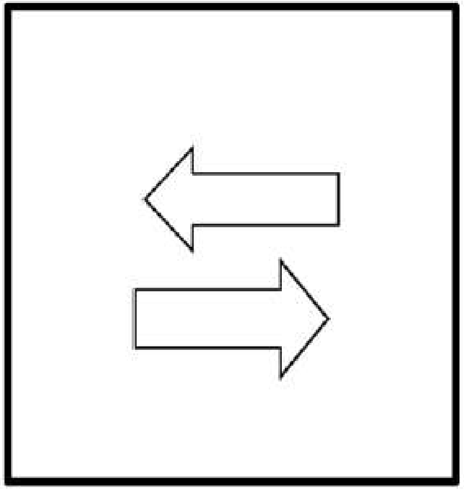
Appendix B – Appendix to chapter 3

B1: Design of the field experiment.

Part 1- Entry survey			Part 2 - Main experiment			Part 3 - Exit survey	
Stage 1.1	**Stage 1.2**	**Stage 1.3**	**Stage 2.1**	**Stage 2.2**	**Stage 2.3**	**Stage 3.1**	**Stage 3.2**
Socioeconomic characteristics	Risk preferences	Social preferences	Mock auction	Conservation auction	Choice of PES allocation mechanism	Fairness perceptions of PES allocation mechanisms	Perceptions of agrobiodiversity loss
Questions on farmers' household and agricultural profiles	Ordered lottery design (Binswanger, 1980)	Four binary choices (Bartling et al., 2009; Kerschbamer, 2015)	Asking participants to place bids on the minimum amount of money they would accept to sell bananas.	Bidding for the requested payment for replacing the conventional crop variety with the traditional one	Auction versus Fixed-price scheme.	Perceptions of distributive and procedural fairness of PES allocation mechanisms.	Perceptions of agrobiodiversity loss (Wale, 2011).

B2: Farmer's voting paper.

RANKING



INDIFFERENT

RANDOM

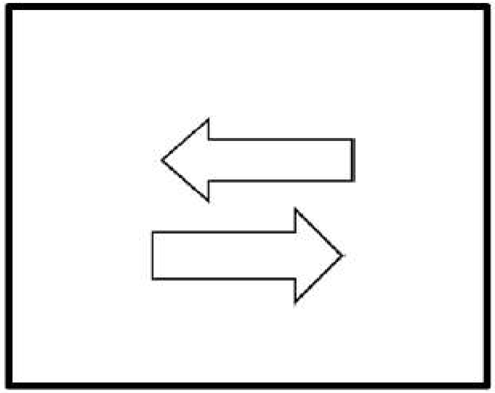
B3: Group’s voting result paper.

Village ID:

Group:

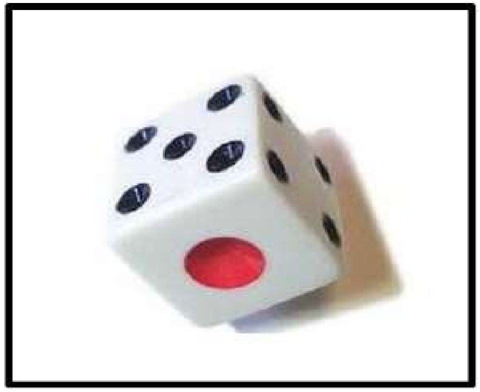
RANKING

1	2	3	4	5	6	7	8	9	10



INDIFFERENT

1	2	3	4	5	6	7	8	9	10



RANDOM

1	2	3	4	5	6	7	8	9	10

B4: Summary statistics of conservation auctions and fixed-price schemes.

	Auction	Fixed-price scheme
Rules		
Selection rule (procedure)	Competition	Random
Payment rule	Winner receive as they bid and losers conserve their endowment	Winners receive the same payment level and losers conserve their endowment
Tie deciding rule	Random	NA
Budget size per group of 10 farmers	FCFA 3,300	FCFA 3,300
Number of participants	250	250
Bids		
Minimum bid	FCFA 500	
Maximum bid	FCFA 9,000	
Mean bid	FCFA 1,202.7	
Median bid	FCFA 1,000	
Standard deviation	FCFA 1,005.25	
Outcomes		
Number of winners	94	75
Payment per contract	FCFA 718.883	FCFA 1,100
Fairness perception		
The allocation mechanism is fair (1-10)	6.924 (2.3691)	5.932 (2.7359)
Distributional fairness perception		
Arg 1: The fact that the fund is not equally distributed among winners /equally distributed among winners (1-10)	6.648 (2.452)	6.712 (2.464)
Arg 2: The fact that winners receive as they bid /the same payment (1-10)	7.704 (2.1678)	6.956 (2.4318)
Procedural fairness perception		
Arg 3: The fact that winners are competitively (randomly) selected (1-10)	6.96 (2.3675)	6.168 (2.8390)
Preferences over PES allocation mechanisms	55.6%	40%

Note: Standard deviations are in parentheses

B5: Pearson correlation test between fairness perceptions and arguments in favor of fairness perceptions.

	Fairness perception of auction	Arg 1: the fact that the fund is not equal distributed	Arg 2: The fact that winners receive as they bid	Arg 3: The fact winners are competitively selected	Fairness perception of fixed-price scheme	Arg 1: the fact the fund is equally distributed	Arg 2: the fact that winners receive the same payment	Arg 3: The fact that winners are randomly selected
Fairness perceptions of auction	1.000							
Arg 1: the fact that the fund is not equal distributed	0.255*	1.000						
Arg 2: The fact that winners receive as they bid	0.339*	0.373*	1.000					
Arg 3: The fact winners are competitively selected	0.522*	0.231*	0.440*	1.000				
Fairness perception of fixed-price scheme	0.027	0.286*	0.259*	0.036	1.000			
Arg 1: the fact the fund is equally distributed	0.146*	0.164*	0.255*	0.146*	0.384*	1.000		
Arg 2: the fact that winners receive the same payment	0.143*	0.157*	0.209*	0.2*	0.211*	0.582*	1.000	
Arg 3: The fact that winners are randomly selected	0.029	0.301*	0.173*	-0.009	0.644*	0.343*	0.305*	1.000

B6: Spearman correlation test between fairness perceptions and arguments in favor of fairness perceptions.

	Fairness perception of auction	Arg 1: the fact that the fund is not equal distributed	Arg 2: The fact that winners receive as they bid	Arg 3: The fact winners are competitively selected	Fairness perception of fixed-price scheme	Arg 1: the fact the fund is equally distributed	Arg 2: the fact that winners receive the same payment	Arg 3: The fact that winners are randomly selected
Fairness perceptions of auction	1.000							
Arg 1: the fact that the fund is not equal distributed	0.269*	1.000						
Arg 2: The fact that winners receive as they bid	0.292*	0.387*	1.000					
Arg 3: The fact winners are competitively selected	0.530*	0.256*	0.384*	1.000				
Fairness perception of fixed-price scheme	0.032	0.301*	0.218*	0.032	1.000			
Arg 1: the fact the fund is equally distributed	0.1513*	0.190*	0.250*	0.134*	0.380*	1.000		
Arg 2: the fact that winners receive the same payment	0.155*	0.209*	0.246*	0.216*	0.204*	0.534*	1.000	
Arg 3: The fact that winners are randomly selected	0.038	0.304*	0.159*	-0.018	0.636*	0.337*	0.284*	1.000

B7: Exit survey - fairness perceptions of PES allocation mechanisms (in English).

[Please administer the following questionnaire to each farmer. Other farmers should not hear the interviewee's answers. Please do not forget to check farmers' codes before recording their answers.]

On a scale from 1 to 10, where 1 is not at all and 10 is very much, how fair do you think it is to use the ranking rule (ranking farmers' claims) to select winners and determine earnings?	v61	
On a scale from 1 to 10, where 1 is not at all and 10 is very much, when you think about how fair the ranking rule is, how important is it in your view that the fund is not equally distributed among winners?	v62	
On a scale from 1 to 10, where 1 is not at all and 10 is very much, when you think about how fair the ranking rule is, how important is it in your view that winners receive as they bid?	v63	
On a scale from 1 to 10, where 1 is not at all and 10 is very much, when you think about how fair the ranking rule is, how important is it in your view that winners are selected by competition?	v64	

On a scale from 1 to 10, where 1 is not at all and 10 is very much, how fair do you think it is to use the random rule to select winners and determine earnings?	v65	
On a scale from 1 to 10, where 1 is not at all and 10 is very much, when you think about how fair the random rule is, how important is it in your view that the fund is equally distributed among winners?	v66	
On a scale from 1 to 10, where 1 is not at all and 10 is very much, when you think about how fair the random rule is, how important is it in your view that winners receive the same payment?	v67	
On a scale from 1 to 10, where 1 is not at all and 10 is very much, when you think about how fair the random rule is, how important is it in your view that winners are randomly selected?	v68	

B8: Exit survey - perceptions on replacement and loss of traditional varieties (in English).

[Please administer the following questionnaire to each farmer. Other farmers should not hear the interviewee's answers. Please do not forget to check farmers' codes before recording their answers.]

On a scale from 1 to 10, where 1 is not at all and 10 is very much, how important is it in your view that it is becoming more and more difficult to find local/traditional varieties of crops in the village and on farmers' fields?	v69	
On a scale from 1 to 10, where 1 is not at all and 10 is very much, how important is it in your view that replacement of local/traditional varieties by the improved ones is happening and leading to disappearance of local/traditional varieties?	v70	
On a scale from 1 to 10, where 1 is not at all and 10 is very much, how important is it in your view that growing local/traditional varieties of crops is inevitable for better adaptation to harsh local conditions?	v71	
On a scale from 1 to 10, where 1 is not at all and 10 is very much, how important is it in your view that loss of local/traditional varieties of crops influencing your livelihoods?	v72	

On a scale from 1 to 10, where 1 is not at all and 10 is very much how important in your views are the following arguments for maintaining local/traditional varieties in your village:		
The traditional varieties have been replaced by the improved ones	v73	
The local/traditional seeds are well adapted to the harsh local environment	v74	
It is better to use our own seeds than getting exposed to unforeseen problems	v75	
We are used to the local varieties; we will still keep on using them	v76	
The local seeds maintain the quality of land	v77	
It is better to face more predictable outcome than getting indebted to buy improved seeds on credit and face unpredictable outcome	v78	
Others	v79	

Appendix C – Appendix to chapter 4

C1: Random-effects panel regression of subjects‘ bids on treatments.

	Model 1		Model 2	
	Coefficients	Standard errors	Coefficients	Standard errors
Opportunity cost	0.624***	0.066	0.587***	0.070
T1	10.315	7.22	-11.898*	6.524
T2	10.097	9.814	-23.811***	8.465
Constant	127.154***	9.621	19.342	72.312
Individual controls	NO		YES	
Group dummies	NO		YES	
Round	NO		YES	
N	1,680		1,680	
Prob > Chi2	0.000		0.000	

Note: The table reports random-effects estimates (cluster-robust standard errors at the group level) . The dependent variable is subject's bid. Opportunity cost refers to subject's opportunity cost.T1 is a binary variable that takes the value 1 if the subject belongs to treatment 1 and 0 otherwise. T2 is also a binary variable that takes the value 1 if the subject belongs to treatment 2 and 0 otherwise.Coefficients reflect differences as compared to the control treatment (no pre-existing price before conservation auctions). Individual controls include age, gender, income, social preference traits, risk index, and fairness perception of the auction. Group dummies are variables that takes the value 1 if the subject belongs to a specific group and 0 otherwise. Round indicates the round in which the bid has been observed. ***Significant at the 1 percent level. **Significant at the 5 percent level. *Significant at the 10 percent level.

C2: Instructions – Experiment – Treatment T1 (in English)

Welcome and thank you for participating!

Please read the instructions carefully. The amount of money you earn during the experiment depends on your own decisions and those of the other participants. The experiment and the entire interaction between you and the other participants takes place via computer terminals and will take about 60 minutes.

You are not allowed to communicate with other participants. If you violate this rule, you will be expelled from the experiment without payment. Please note that neither the academic staff nor the other participants will be able to link you to your anonymous decisions. If you have any questions during the experiment, please raise your hand. We will then come to you and answer your question in a subdued voice. The question is subsequently answered aloud so that other participants are also informed.

Basic procedure of the experiment

The experiment consists of several *parts*. During each part you are asked to make one or more decisions. Please note that your decisions in the different parts of the experiment are completely independent of each other and that the result of your decisions will be communicated to you at the end of the experiment.

Payment

Your payout at the end of the experiment is calculated as follows: You will receive 5 Euro, regardless of your decisions in the experiment. Furthermore, you will receive a payment depending on the decisions you have made in *one* randomly selected part of the experiment. During the experiment your earnings will be calculated in "points" (the currency unit of the experiment). At the end of the experiment, your earnings will be converted into euros according to the following exchange rate:

1 Point = € 0.10 (10 Eurocents).

You will receive your payment in cash at the end of the experiment upon presentation of your participation code.

Instructions: Part A

At the beginning of part A, the central computer randomly assigns participants to 2 groups of 10 subjects: Group 1 and Group 2. The groups are independent and you will stay in the same group during the experiment.

In this first part of the experiment you have to make **10 decisions**. Each of your decisions consists of **the choice between a LEFT and a RIGHT alternative**.

In each of the 10 decision situations, the LEFT alternative provides you with guaranteed earnings. The earnings for the RIGHT alternative, however, depends on chance.

Example: You might be asked if you prefer the LEFT alterative, which will definitely give you 70 points, or whether you want to choose the RIGHT alternative, where the probability of earning 100 points is 50% and the probability of earning 0 points is 50%. You would then have to choose one of the two alternatives. This decision-making problem would be displayed on your screen as follows:

LEFT Alternative	Your choice		RIGHT Alternative
You receive			You receive
70 Points with certainty	LEFT ◯	◯ RIGHT	**50% chance of 100 Points** and **50% chance of 0 Points**

In this first part of the experiment you will make **a total of 10 such decisions**. Your earnings in this section shall be determined as follows:

At the end of the experiment, one of the 10 decision situations is randomly selected for each participant and the merit is determined according to the choice made. By way of illustration: If the above-mentioned decision situation were chosen at random and if you choose the RIGHT alternative here, you would receive 100 points with a probability of 50% and 0 points with a probability of 50%. Whether you actually receive 100 or 0 points is randomly determined by the central computer.

Please raise your hand if you have a question. We will then come to you and answer your questions individually. When you have understood the instructions, please click on "continue".

Instructions: Part B

In this part of the experiment you have to make **10 decisions**. In each of the decision situations, you pair up with another participant from your group. This person is henceforth referred to as *"the other person*". This "other person" is randomly and anonymously assigned to you by the central computer.
Each of your decisions is again **a choice between the LEFT and RIGHT alternatives**. Each alternative has consequences for you and "the other person".

Example: You might be asked if you prefer the LEFT alternative, which will give you 110 points and the "other person" 130 points, or whether you want to choose the RIGHT alternative, where you get 100 points and the "other person" also gets 100 points. You then have to choose one of the two alternatives. This decision-making problem would be displayed on your screen as follows:

LEFT		Your choice		RIGHT	
You receive	Other person receives			You receive	Other person receives
110 points	**130 points**	LEFT ○	○ RIGHT	**100 points**	**100 points**

In this second part of the experiment you will make **a total of 10 such decisions**. Your earnings from this part are determined as follows:

At the end of the experiment, **the central computer will randomly determine whose decisions - yours or the "other person's" decisions - are used.** Next, the computer will randomly select **one of this person's 10 decisions** and execute that decision. For example, if the above-mentioned decision situation was randomly selected and your decision was taken and you had chosen the LEFT alternative, you would receive 110 points and the "other person" 130 points.

Please raise your hand if you have a question. We will then come to you and answer your questions individually. When you have understood the instructions, please click on "continue".

Instructions: Part C

At the beginning of Part C, the central computer will divide all participants into groups of 10. You will always remain in this group of 10 people throughout Part C.
In this part, each of the 10 members of your group (including you) will receive a chip that you can offer to a buyer - the computer. Each chip costs a certain amount. The cost of a chip is randomly chosen between **10 points** and **300 points**. Each of the numbers between 10 and 300 is equally likely to be selected. All members of your group are equally likely to own a chip at a specific cost. The cost of your chips will be shown on your computer screen. For the remainder of Part C of the the experiment, the cost of your chip will remain the same.

.

Selection of winners:

Random: The computer randomly selects winners until its budget of **1200 points** is exhausted. If this rule is applied, **each individual offer is priced at 300 points.**

That means:

- Whether you are actually selected is randomly determined by the central computer.

Your earnings:

Your earnings in this section are calculated as follows:

- If you are randomly selected, you will receive 300 points minus the cost of your chip.
- If you are not selected, you will receive 0 points.

Example: Suppose that your Chip costs 50 points; the computer posts a price of 300 points. Your earnings in this section are therefore 250 points (i.e., 300 – 50) if you are randomly selected. Otherwise you will receive 0 points in this section of the experiment.

Summary:

- Your group consists of 10 participants (you and 9 others)
- Each participant receives 1 chip.
- Each chip is associated with a cost. The actual cost is determined randomly in intervals between 10 and 300 points.
- You can sell your chip to the buyer.
- The buyer will randomly select winners until its budget of 1200 points is exhausted. Each individual offer is priced at 300 points.
- If you are randomly selected, you will receive 300 points minus the cost of your chip.
- If you are not selected, you will receive 0 points.

Please raise your hand if you have a question. We will then come to you and answer your questions individually. Once you have understood the instructions, please click on "continue".

Instructions: Part D

Part D of the experiment consists of multiple rounds. If part D is used as the payout-relevant part, one of these rounds will be randomly selected to determine your payout.
The cost of your chip and that of your group members remains the same as in the previous section.

<u>Part D, Round 1:</u>

<u>Your task:</u>

You can offer your chip to the buyer by indicating a price - the same applies to the other 9 members of your group. Please note: The price you specify cannot be lower than the cost of your chip.

<u>Selection of winners:</u>

<u>Rank order</u>: The computer will then rank all 10 bids according to their price from low to high. Starting with the lowest bid, it accepts bids in the order of ranking until its budget of **1200 points** is used up.

Please note that you will never know the chip cost of the other participants who belong to your group. Conversely, the other participants will not receive any information about your cost of your chip.
This means:

- You **compete** with the other 9 participants in your group to ensure that your bid is accepted.
- The lower your bid, the more likely it is to be accepted.

<u>Your earnings:</u>

Your earnings in this section are calculated as follows:

- If your price is accepted, you will receive the price minus the cost of your chip.
- If the price you specify is not accepted, you will receive 0 points

Example: Suppose your chip costs 50 points and you offer it for 200 points. Your earnings in this section are therefore 150 points (i.e., 200 - 50), but only if your offer has been accepted. Otherwise, you will receive 0 points in this section.

Summary:

- Your group consists of 10 participants (you and 9 others)
- Each participant receives 1 chip.
- Each chip is associated with costs. The actual cost is determined randomly in intervals between **10 and 300 points**.
- The buyer will sort all offers according to their price from the lowest to the highest. Starting with the lowest bid, the computer will accept bids until its budget of **1200 points** is exhausted.
- If your offer is accepted, you will receive the price of your chip minus the cost of your chip.
- If your offer is rejected, you will receive 0 points.

Please raise your hand if you have a question. We will then come to you and answer your questions individually. Once you have understood the instructions, please click on "continue”.

Part D, Round 2:

Round 2 is identical to Round 1. Please raise your hand if you have a question. We will then come to you and answer your questions individually. Once you have understood the instructions, please click on “continue”.

Part D, Round 3:

Round 3 is identical to Round 2. Please raise your hand if you have a question. We will then come to you and answer your questions individually. Once you have understood the instructions, please click on “continue”.

Part D, Round 4:

Round 4 is identical to Round 3. Please raise your hand if you have a question. We will then come to you and answer your questions individually. Once you have understood the instructions, please click on “continue”.

Part D, Round 5:

Round 5 is identical to Round 4. Please raise your hand if you have a question. We will then come to you and answer your questions individually. Once you have understood the instructions, please click on “continue”.

Part D, Round 6:

Round 6 is identical to Round 5. Please raise your hand if you have a question. We will then come to you and answer your questions individually. Once you have understood the instructions, please click on “continue”.

Part D, Round 7:

Round 7 is identical to Round 6. Please raise your hand if you have a question. We will then come to you and answer your questions individually. Once you have understood the instructions, please click on “continue”.

Part D, Round 8:

Round 8 is identical to Round 7. Please raise your hand if you have a question. We will then come to you and answer your questions individually. Once you have understood the instructions, please click on “continue”.

Part D, Round 9:

Round 9 is identical to Round 8. Please raise your hand if you have a question. We will then come to you and answer your questions individually. Once you have understood the instructions, please click on “continue”.

Part D, Round 10:

Round 10 is identical to Round 9. Please raise your hand if you have a question. We will then come to you and answer your questions individually. Once you have understood the instructions, please click on “continue”.