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Contract Design for Adoption of Agrienvironmental Practices: A Meta-analysis of Discrete Choice Experiments

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ABSTRACT

This study presents a meta-analysis of contract attributes for the adoption of agrienvironmental practices. We use a data set of 290 observations drawn from 79 studies reported in empirical studies using the Discrete Choice Experiment (DCE) method. The present meta-analysis explores the impact of methodological choices related to the timing of the DCE (design stage, implementation stage, and analysis stage) on farmers' contract preferences regarding the adoption of agrienvironmental practices. We first highlight the possibility of simplifying the contract attributes to propose two representative clauses: commitments and incentives. Several biases were found related to designing, implementing, and analyzing a DCE. We show that these results are relevant for two specific clauses (Duration and Quality). Finally, our contribution provides guidance for mitigating potential biases that can affect the results when DCE is implemented.

1. Introduction

To face climate change and food crisis challenges, agriculture must not only ensure food security but also contribute to ecosystem services to mitigate environmental impacts. This implies providing safe and healthy agricultural products to cope with the negative externalities generated by conventional agriculture and setting up practices and technologies respectful of the environment (Godfray, 2015; Bernard and Lux, 2017; Garbach et al., 2017; Garibaldi et al., 2017; Padilla-Bernal et al., 2018). To implement agroecological transition (Duru et al., 2015), several instruments and incentive mechanisms to adopt agrienvironmental practices have been designed by institutions at different scales (Bachev, 2009; Tittonell, 2014; Meek, 2015; Loconto and Vicovaro, 2016). Direct subsidies or indirect support of agrienvironmental practices through eco-conditionality measures is one of the main incentive mechanisms. However, direct subsidies may create distortions in the input allocation and become an important budgetary burden for governments without ensuring sustainability in the adoption of these practices (Van Mansvelt and Van der Lubbe, 1998; Tiwari and Dinar, 2002; FAO, 2013).

From a theoretical and empirical point of view, the incentive to adopt agrienvironmental practices involves different economic, social, and environmental dimensions. Ex post evaluation of motivations for adopting agrienvironmental practices have been used for a long time to support policymakers in their environmental management programs (Green, 2006; Ninan, 2014; OCDE, 2014, 2018). During the last two decades, a new approach, namely *Discrete Choice Experiment* (DCE), borrowed from behavioral sciences and marketing has emerged to understand the preferences of farmers for the adoption of agrienvironmental practices. The DCE method consists of ex ante evaluation of farmers' Willingness to Adopt (WTA) (and Willingness to Pay, WTP, for) agrienvironmental practices under different contract arrangements (Adamowicz et al., 1998a, 1998b; Louviere et al., 2000a, 2000b). The last two decades have seen the rapid development of experiment-based studies to analyze farmers' preferences for public policy instruments. A simple Web of Science Core Collection report , entering the two keywords “choice experiment” in Topic 1 and “Farmer” in Topic 2, applied in December 2019 confirms the rapid evolution of the number of papers dealing with this topic (Fig. 1).

Despite the growing number of academic publications based on the DCE method and its success as a policy design mechanism, its use in the design of agrienvironmental policies remains subject to many biases (status quo effect, choice availability biases, protest effects, behavioral biases of participants, and more.) Although the DCE is important for hypothetically testing theories or driving public policy decisions, the process must be carefully designed to acquire reliable results (Murphy et al., 2005; Rolfe and Brouwer, 2012; Carson and Czajkowski, 2014; Watson et al., 2017; Alemu and Olsen, 2018; Quaife et al., 2018). While

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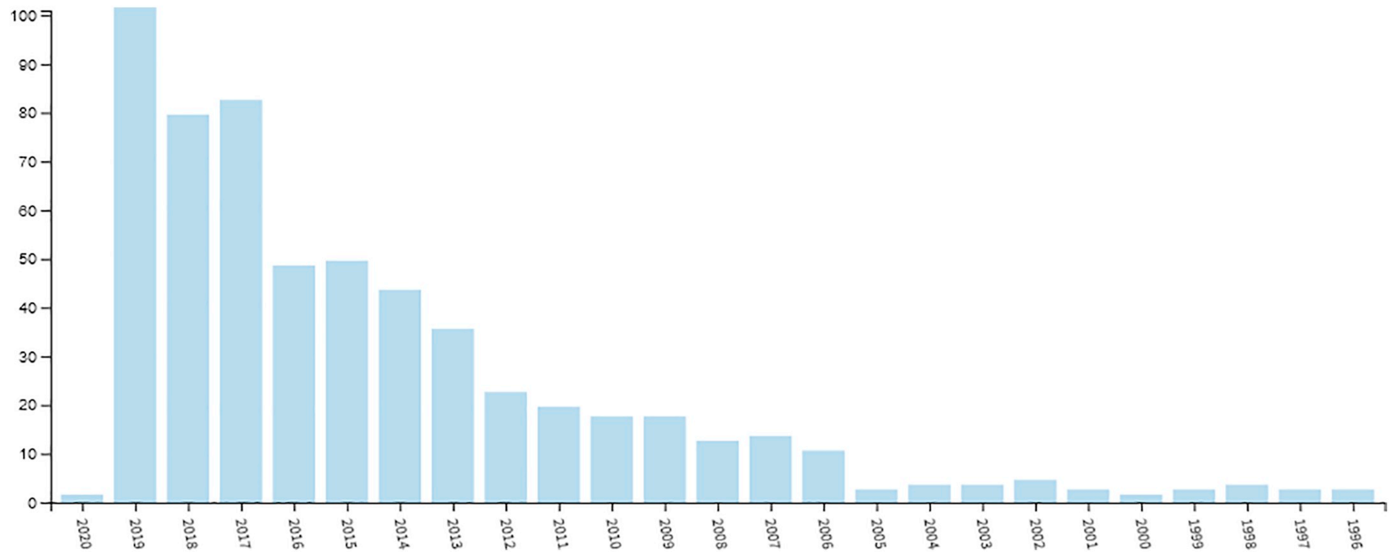

Fig. 1. Evolution of papers appearing on Web of Science for TOPIC: (choice experiment) AND TOPIC: (farmer).

there are few previous systematic reviews of papers about DCE (Villanueva et al., 2017a, 2017b; Barreiro-Hurle et al., 2018), our contribution is the first to carry out a meta-analysis of DCE applied to the design of agrienvironmental policies.

We conducted a meta-analysis of 79 studies that used the DCE method to address farmers' preferences for contract terms that may foster WTA agrienvironmental practices. Our objective is to investigate factors affecting estimates regarding the influence of contract clauses (hereinafter, “attributes”) on the WTA. To do so, we analyzed the impact of different explanatory variables related to the different stages of the DCE process (design, implementation, and analysis). Based on the results of the meta-analysis, we discuss and make recommendations to enhance the reliability of DCE use on incentive contracts and agrienvironmental policy.

2. Brief Literature Review on DCE

2.1. DCE Foundations

The DCE is one of the most popular methods for identifying and evaluating the preferences of economic agents. Recently, this method has been widely used in agriculture, agri-food, and environmental sectors (Hoyos, 2010; Louviere et al., 2010; Whittington, 2010; Rakotonarivo et al., 2016). The DCE is a quantitative method for assessing the relative importance of various attributes that influence the choices of respondents (Adamowicz et al., 1998a, 1998b; Louviere et al., 2000a, 2000b). The method's popularity lies in its ability to include attributes or attribute options that are sometimes difficult to observe in real life situations, mainly in innovation situations (McFadden, 1986; Louviere and Hout, 1988; Louviere, 1994; Carson et al., 1994; Jaeger and Rose, 2008). The DCE originated in Thurstone's (1927) theory of random utility and Lancaster's (1966) consumer choice model. These theoretical foundations were developed by McFadden (1974) in the theoretical model of random utility, wherein utility is divided into observable and unobservable components. The random utility model has been used in multiple disciplines, including agriculture (Hanemann, 1984; Lancsar and Louviere, 2008).

The basic assumption of the DCE method is that any product or service can be described by its attributes and its valuation depends on the level of the attributes (Street et al., 2005; Bush et al., 2012; Ribeiro et al., 2017). Thus, the first stage of the method is to select the relevant attributes to describe the practice or the technology. This stage is considered to be one of the most critical steps in study design (Bridges, 2003). Several methods are used to define attributes of DCE choices, such as a literature review, focus groups involving stakeholders, interviews, panels of experts, or a combination of these methods (Kløjgaard et al., 2012; Ryan et al., 2012). Qualitative methods are recommended to extract the comments and views of stakeholders that may be helpful in determining the attributes (Coast and Horrocks, 2007; Coast et al., 2012). The next step is the design and compilation of choices. So-called choice cards are generally used in orthogonal plans for uncorrelated estimation of all main effects, assuming that all interactions are negligible (Addelman, 1962). The design stage can be assisted by computer programming (Hahn and Shapiro, 1966) used in various statistical computing software (Street et al., 2005).

The optimal design of choice cards can be complex (Louviere et al., 2000a, 2000b). Behavioral biases related to the number of attributes and choice options have been observed in the design of DCEs (Ribeiro et al., 2017). Therefore, card design should include only the attributes considered as the most relevant (Street and Burgess, 2007) without limiting the choices of respondents in hypothetical scenarios. Otherwise, it may create a bias of choice availability (Louviere et al., 2000a, 2000b; Hensher et al., 2005a, 2005b). That is why the choice cards should imply also the status quo, which represents the absence of hypothetical choice situations.

However, if the choice of status quo is frequent, a statistical reliability bias can be generated (Adamowicz et al., 1998a; Scarpa et al., 2005a, 2005b, 2007; Boxall et al., 2009; Barreiro-Hurle et al., 2018). Studies by Meyerhoff and Liebe (2008) and Villanueva et al. (2017a, 2017b) show that inclusion or exclusion of such protest responses can have a significant impact on the reliability of estimates. In addition, it is necessary to describe the choices clearly so that the people surveyed know how to make a trade-off between the “menu” of attributes (Bateman et al., 2002; Lancsar and Louviere, 2006, 2008). The representation of attribute levels is important to allow respondents to have an unambiguous description of the meaning of each of the choices (Hensher et al., 2005a, 2005b).

The implementation of the DCE requires a preliminary test of the choice cards to improve its efficiency (De Bekker-Grob et al., 2012). Moreover, the interviewer must do a first test (pre-test) to better assess the relevant questionnaire (Hall et al., 2004; Bateman et al., 2002). It is crucial to have relevant information on basic attributes that allow farmers to build their perception and preferences of the experiment attributes.

Utility theory is a primary tool to analyze the DCE data by estimating the parameters and the level of their random components.

Empirical models used are probit model, logistic regression including multinomial models (Zwerina, 2013), logistics conditional regression (Greene, 2003), and mixed logit (also called error components logit or random parameters logit). Also, a latent class model accounts for the heterogeneity of preferences in the sample population because error components may enter the expected utility for each contract choice (Greene and Hensher, 2003).

2.2. DCE in Agrienvironmental Practices

The DCE method has become an important method to help understand and design new agricultural markets (Lusk and Hudson, 2004; Windle and Rolfe, 2005). Unlike the ex post evaluation approach, which has the drawback of taking into account factors that influence farmers' decisions to participate in agrienvironmental practices after their design (Siebert et al., 2006), the DCE method offers the possibility of taking into account farmers' WTA according to their preferences related to the design of the practices and the definition of their outlines and their consequences.

The DCE method can help explore the potential provision of an environmental service by farmers when an environmental management program is implemented. Therefore, the DCE allows sufficient variation in the patterns of agrienvironmental practices to analyze the impact of agrienvironmental contract design (Espinosa-Goded et al., 2010).

In addition, the DCE method is adapted to the substantial diversity of the contract schemes proposed for the payment of environmental services. It can be adapted to different ecological, socio-economic contexts, or it may have a poor design due to a mistake or the need to accommodate political pressures (Wunder et al., 2008; Engel et al., 2008). Studying farmers' ex ante preferences regarding key dimensions of the proposed contract schemes (enrolled area, technical and administrative assistance, sharing of investment costs) can help to find a design with trade-offs that combine the effectiveness of public allowances and the success of proposed agrienvironmental practices (Espinosa-Goded et al., 2010).

The DCE method can be used to produce information about cost-benefits of public programs employment (species conservation, nature preservation, etc.). It can inform efficient and effective design of agrienvironmental practices and public regulation by providing to policy makers and stockholders information about farmer's preferences for various social, environmental and cultural features in return for financial incentives (Birol and Koundouri, 2008).

Methodological studies in DCE approach have, however, highlighted the importance of investigating heteroskedasticity. A Few studies of DCE method applied on agrienvironmental practices have investigated the impact of this concern on error terms and treat them (Swait, 2007; Birol and Koundouri, 2008).

Villanueva et al. (2017a, 2017b) explain the heterogeneity of farmers' preferences in agrienvironmental practices by (i) ecosystem services produced on the farm and how they are produced in conjunction with the farm's usual products; (ii) socio-economic factors of the farm and farmer; and (iii) extrinsic factors related to the agricultural market, public policies, and social norms in this field.

Dimal and Jetten (2018) found that the heterogeneity of farmers' preferences was influenced by spatial dimensions such as environmental risks and proximity to amenities, and respondents' socio-demographic characteristics such as their level of education, income level and environmental awareness. Respondents can be expected to be very familiar with the attributes of the contract offered, which is not always the case and therefore generates a bias in studies on revealed preferences (Schläpfer and Fischhoff, 2012).

These factors that lead to the heterogeneity of farmers' preferences for adopting agrienvironmental practices are generally interrelated and may be specific to individuals, groups, systems or regions (Villanueva et al., 2017a, 2017b). Hensher et al. (2005a, 2005b), Mariel et al. (2013), and Bell et al. (2014) found that heterogeneous effects of farmers' preferences induced by their own heterogeneity in the DCE method can be mitigated by the use of specific analytical models such as random parameters logit or mixed logit model. Heteroskedasticity is another concern in the DCE application in agrienvironmental practices (Tesfaye and Brouwer, 2012).

To mitigate these biases, Scarpa et al. (2005a, 2005b) recommend the use of error-component models when comparing less known hypothetical alternatives with more known alternatives (status quo). The models are also recommended to reduce effect of discontinuous, continuous and conditional preferences, depending on whether the respondent takes into account each attribute when evaluating alternatives (Campbell et al., 2008; Hoyos, 2010; Villanueva et al., 2017b; Rodríguez-Entrena et al., 2019). The heterogeneity of institutional contexts adds to the previous ones and makes it difficult to generalize the results of the DCE from one study site to another institutional site, the cost-effectiveness and usefulness of this method remains one of its limits in providing information and drivers for environmental policy (Birol and Koundouri, 2008).

3. Methodology and Data

3.1. Data

79 studies published between 2006 and 2019 were selected for the meta-analysis according to two crucial criteria. First, we chose papers using DCE approach on contracts fostering agrienvironmental practices. That is why papers using DCE but with a focus on the design of other types of contracts (e.g. adoption of technology, farming contracts, …) are excluded from our "targeted population". Second, we included only papers estimating WTA. That is, studies applying DCE only to willingness to pay (WTP) were not included in our meta-analysis.

The studies were collected between August 2018 and December 2019 from a systematic review of empirical DCE literature applied to the adoption of agrienvironmental practices. The collection of studied papers was conducted through several scientific databases ("Google Scholar," "Web of Science," and "IDEAS") as well as the search engine "Google" to find especially unpublished studies (conference communications, Ph.D. theses, working papers) to reduce publication bias in our meta-analysis (Begg and Mazumdar, 1994; Egger et al., 1997; Egger and Smith, 1998; Duval and Tweedie, 2000) and to capture review publication effect as an explanatory variable in our meta-analysis (see sub-section 3.3). Among a total of 290 observations,[1] our database includes 69 observations from unpublished studies. The selected studies cover 34 countries from four regions of the world: Europe (33 studies), America (16 studies), African and Asian developing countries (24 studies), and Oceania (6 studies). Appendix 1 describes the case studies and their bibliographical references.

3.2. Method of Analysis

From each paper of our 79 papers population , we extracted information on contract clauses (attributes) estimates. Each attribute estimate represents one observation (with sign positive, negative, or nonsignificant) and therefore we may get more than one observation per paper. Finally, we get 290 observations in our database. Because the DCE method measures the impact (sign and statistical significance) of different clauses embedded in the choice cards, the 290 observations were split into two categories (sub-samples): a "commitment" category (173 observations) which contains all clauses (attributes) in the database related to commitment of parties to develop agrienvironmental

[1] In papers where there are several sub-sample estimates and one global sample estimates, only the estimates of the global samples are included in our database. In the case where the initial study uses only split-samples modelling approach, we include the estimates of the sub-samples.

Table 1
The different contract clauses analyzed in the 79 studies.

Clause		Definition	Obs	Significantly positive	Significantly negative	Null
Commitments			173	77	80	16
	Duration	Duration of the contract	34	9	20	5
	Harvest cost sharing	Buyer's commitment on harvest cost sharing	2	2	0	0
	Training and advice provision	Buyer's commitment on providing training and advice	16	10	4	2
	Management of the contract	Buyer's commitment to take charge over the administrative costs of the contract and its management	5	1	3	1
	Commitment on quantity to deliver and take	Commitment from both parties on minimum (and max) quantities to deliver and take	6	4	2	0
	Commitment on quality	Farmer's commitment on quality of the product	59	27	28	4
	Commitment on area	Farmer's commitment on area dedicated to the contract	20	10	8	2
	Collective commitment from farmers	Collective commitment from a "pool" of farmers on the same contract	11	3	8	0
	Commitment to renegotiate	Commitment from partners to renegotiate the initial clauses instead of breaching the contract	13	11	1	1
	Supervision	Commitment from the buyer (private or public) to ensure supervision of production	7	0	6	1
Incentives			117	80	30	7
	Premium	Annual premium paid by the buyer according to the performance of the contract	33	29	2	2
	Labelling premium	Market Premium when partners are committed in a Eco-labelling strategy	2	2	0	0
	Bonus/malus for quality	Specific payment when there is a commitment on quality	3	2	1	0
	Collective bonus	Bonus from a common contract, when a there a collective commitment from a pool of farmers (see above)	1	0	1	0
	Up-front Payment	Payment from the buyer when the farmer agrees to participate to the contract	15	12	3	0
	Sharing the production cost	Sharing of the production cost between contract partners	17	5	11	1
	Sharing of the specific asset costs	Payment form the buyer to the farmer for specific asset implemented in the contract	11	8	1	2
	Insurance	Climate Insurance against damages	3	3	0	0
	Income volatility reduction	Indexation mechanism to reduce farmer's income volatility	9	1	7	1
	Expected Income	Expected income that the farmer may get from the contract	18	15	3	0
	Price at the farm gate	Minimum price paid to the farmer at the farm gate	5	3	1	1

practices, and an "incentive" category (117 observations) grouping the whole incentive clauses (attributes) in the database (see Table 1). Our choice to group the observations in two dependent variables (commitments on the one side and incentives on the other side), may be referred to contract theory, where it is assumed that the commitment of one of the parties to make an effort, action or task is not necessarily credible because her/his effort, action or task: (i) is not (or is difficult to) observable by a third party; and (ii) is costly for the party who do it (Bolton and Dewatripont, 2005; Salanié, 2005; Laffont and Martimort, 2009). To make this commitment credible, it is necessary that the contract defines sufficient incentives to compensate for the costly effort, otherwise he/she will not do so even if he/she has signed the contract (moral hazard). This problem can be generalized to the situation where both parties must commit to making efforts and the contract must therefore encourage both parties (double moral hazard). The main purpose of the contract is therefore to balance the parties' commitments against the incentives necessary for the implementation of these commitments.

In Table 1, we can see that the significantly positive attribute estimates represent 54.83% of the 290 observations. On the other hand, the significantly negative attribute estimates represent 36.55% and those that are nonsignificant represent only 8.62% of the coefficients. In "Commitment" clauses, we can see that 45.09% estimates are significantly positive and 46.24% are significantly negative, while 8.67% are nonsignificant. In the "Incentive" clauses, we have 69.23% effects estimated as significantly positive, 22.22% of effects estimated as significantly negative, and 8.55% of nonsignificant estimated effects. Moreover, all studies include at least one clause in the "Commitment" subsample and one clause in the "Incentive" subsample (see Table 2).

3.3. Dependent and Explanatory Variables

To make a meta-regression for the 290 observations identified (173 for Commitments and 117 for Incentives), four binary-dependent variables have been defined.

The first binary-dependent variable, namely "*Commitment Positive,*" is equal to one for significantly positive estimates of Commitment clauses and equal to zero for significantly negative or nonsignificant estimates. The second binary-dependent variable, namely "*Commitment Negative*," is equal to one for significantly negative effects of Commitment clauses and equal to zero for significantly positive or nonsignificant effects. The third binary-dependent variable, namely "*Incentive Positive*," is equal to one for significantly positive effects of incentive clauses and equal to zero for significantly negative or nonsignificant effects. The last binary-dependent variable, namely "*Incentive Negative*," is equal to one for significantly negative effects of incentive clauses and equal to zero for significantly positive or nonsignificant effects. We use also clustered univariate probit to estimate four other variables related to commitment *Duration* clause[2] (*Positive*

[2] We grouped all the results of the coefficients estimated on the duration clause, regardless of the term of the duration clause. Mainly because we have very few observations for the duration estimates.

Table 2
Definition and descriptive statistics of studied variables.[a]

Description		Sub-sample for commitment		Sub-sample for incentive		Full sample		Sub-sample for duration		Sub-sample for quality	
		N = 173		*N* = 117		*N* = 290		*N* = 34		*N* = 59	
		Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Dependent variables											
Commitment positive	= 1 if significantly positive effects of Commitment clauses, 0 otherwise	0.45	0.03	/	/	0.45	0.03	/	/	/	/
Commitment negative	= 1 if significantly negative effects of Commitment clauses, 0 otherwise	0.46	0.03	/	/	0.46	0.03	/	/	/	/
Incentive positive	= 1 if significantly positive effects of Incentive clauses, 0 otherwise	/	/	0.69	0.04	0.69	0.04	/	/	/	/
Incentive negative	= 1 if significantly negative effects of Incentive clauses, 0 otherwise	/	/	0.22	0.03	0.22	0.22	/	/	/	/
Duration positive	= 1 if significantly positive effects of Duration clause, 0 otherwise	/	/	/	/	/	/	0.26	0.07	/	/
Duration negative	= 1 if significantly negative effects of Duration clause, 0 otherwise	/	/	/	/	/	/	0.58	0.08	/	/
Quality positive	= 1 if significantly positive effects of Quality clause, 0 otherwise	/	/	/	/	/	/	/	/	0.47	0.06
Quality negative	= 1 if significantly negative effects of Quality clause, 0 otherwise	/	/	/	/	/	/	/	/	0.47	0.06
Explanatory variables											
Number of alternatives	Number of contractual alternatives per choice card	3.07	0.04	3.11	0.05	3.08	0.03	3.11	0.11	3.13	0.07
Number of attributes	Number of clauses per contractual alternative	5.04	0.08	5.11	0.11	5.07	0.06	4.76	0.16	5.15	0.15
SQ	= 1 for choice card with status quo or opt-out, 0 otherwise	0.87	0.02	0.86	0.03	0.87	0.01	0.94	0.04	0.86	0.04
ICard	= 1 for choice card with illustration, 0 otherwise	0.31	0.03	0.34	0.04	0.32	0.02	0.29	0.07	0.37	0.06
Number of cards	Number of choice cards presented to respondent	11.0	0.58	10.8	0.69	10.9	0.44	11.8	1.40	10.0	0.90
Sample size	Sample size (respondents' number)	372	31.6	413	41.1	388	25.1	352	70.8	408	57.5
Face-to-face survey	= 1 for face-to-face survey, 0 otherwise	0.72	0.03	0.71	0.04	0.72	0.02	0.61	0.08	0.74	0.05
Web survey	= 1 for web survey, 0 otherwise	0.22	0.03	0.21	0.03	0.22	0.02	0.26	0.07	0.22	0.05
Year	year of publication of the study	2015	0.21	2015	0.25	2015	0.16	2015	0.45	2015	0.38
Europe	= 1 for studies done in Europe, 0 otherwise	0.47	0.03	0.40	0.04	0.44	0.02	0.41	0.08	0.40	0.06
North America	= 1 for studies done in North America, 0 otherwise	0.10	0.02	0.15	0.03	0.12	0.01	0.17	0.06	0.10	0.03
Developing Countries	= 1 for studies done in developing countries, 0 otherwise	0.36	0.03	0.38	0.04	0.37	0.02	0.32	0.08	0.45	0.06
Modelling	= 1 for RPL or MLM or ECL, 0 otherwise	0.91	0.02	0.94	0.02	0.92	0.01	0.94	0.04	0.88	0.04
ASC	= 1 for analysis with Alternative-Specific Constants, 0 otherwise	0.72	0.03	0.73	0.04	0.73	0.02	0.73	0.07	0.72	0.05
Published	= 1 for studies published in reviews, 0 otherwise	0.76	0.03	0.75	0.04	0.76	0.02	0.76	0.07	0.79	0.05
Number of authors	Number of authors	3.25	0.11	3.30	0.14	3.26	0.09	3.23	0.29	3.44	0.21
Innovative practices	= 1 for innovative practices crop, 0 otherwise	0.12	0.02	0.14	0.03	0.13	0.01	0.17	0.06	0.10	0.03

[a] A correlation test (similarity) was performed to test for correlation between explanatory variables (attributes) in our estimates (see Appendix 2).

and *Negative*) and *Quality* clause (*Positive* and *Negative*) the same way as the previous dependent variables.[3]

There is a large difference in the codification methods of the duration clause in the papers. For example, some papers do not take the first modality (shortest duration) as a reference in the estimates (Christensen et al. 2011) because they do not want to capture the effect of a long term contract, but rather its opposite, i.e. a shorter-term contract effect. In this case, we had to be careful about the sign of the effect to be put in the database. Some other papers put dummy variables for each of the duration modalities. There are also different modalities for short, medium and long durations (e.g. 5 years; 10 years; 20 years; 40 years for Greiner (2016); 15 years; 25 years; 35 years for Arifin et al. (2009); or a multi-year contract without fixed duration for Gramig and Widmar (2017)). Some papers specified a time-span instead of a fixed duration (Cranford and Mourato, 2014; Khanna et al. 2017).[4]

[3] We did this specific analysis on Duration and quality clauses because for these two clauses we have enough observations from the estimates of our primary studies. This also allows us to show how to proceed in the ideal situation where we would have enough studies from the literature to make a clause-by-clause meta-analysis.

[4] Some papers also estimate interaction effects of the duration clause with other clauses (See Appendix 1). Do to the homogeneity constraint with other estimates, we decided to not take them into account in our codification of the signs of the duration variable.

To tackle the great heterogeneity of the duration clause, we have chosen a simple rule to homogenize all these definitions of duration. We have decided to define for all the estimates of our database, the effect of duration as the effect of longer-term contract commitment. We coded all the estimation results according to this definition.

To regress on our dependent variables, following Barreiro-Hurle et al. (2018), we had recourse to explanatory variables related to the timing of the DCE: design stage, implementation stage, and analysis stage (Table 2).

For the design stage of the DCE, where the attributes and the cards are designed, we chose four explanatory variables. First, we have the *Number of alternatives* in the experiment, which is a continuous variable that measures the number of contract alternatives per choice. Second, we have the *Number of attributes*, which is a continuous variable for the number of clauses per contract alternative. Third, we include *SQ*, a binary variable that indicates the possibility for the farmer choosing the status quo (or opt-out) situation as an alternative in the choice cards. This critical variable measures the choice availability effect (Borgers et al., 1999; Kontoleon and Yabe, 2003; Costanigro et al., 2017a, 2017b; Barreiro-Hurle et al., 2018). Finally, we add *ICards*, a binary variable to indicate that the cards are illustrated. This aims to catch attention control effects (Orquin et al., 2013; Bagger, 2016).

For the implementation stage of the experiment, we had recourse to five explanatory variables. The first one is *Number of Cards*, a continuous variable that measures the number of choice cards presented to each respondent. It catches the fatigue effect on the quality and reliability of choice made by respondents (Bradley and Daly, 1994; Caussade et al., 2005; Savage and Waldman, 2008; Hess et al., 2012).

The second variable is the *Sample size*, a continuous variable that measures the number of responses to the survey. A priori, a higher number of observations in the database makes the estimates more reliable (Greene, 2003).

Third, for the survey mode bias, we created two binary variables: *Face-to-face Survey*, when the survey is addressed face-to-face, and *Web Survey*, when the survey is managed through the Internet). The survey mode can influence results because of different respondents' profiles, thus eliciting different responses (Olsen, 2007; Jäckle et al., 2010; Lindhjem and Navrud, 2011a, 2011b).

Fourth, we added a *Year* variable, a continuous variable that indicates the year of publication of the study. The variable may have an impact on the efficiency of DCE implementation because, as the years pass, researchers learn more about DCE implementation and thus the experience bias should decline with each year of study (Hedges and Olkin, 2014).

Five, we used four binary regional variables (*Europe North America; Developing Countries in Central & South America, Africa and Asia; Oceania*) to control for the institutional bias because public policy and institutional arrangements may constrain the adoption of formal contracts (North, 1990; Williamson, 1996; Ostrom, 2005). The Oceania region is used as the reference in our estimates.

For the analysis stage of the experiment, we used four variables. First, we introduced *Modelling*, a binary variable that indicates the econometric model used to estimate parameters of the DCE. The selection of modalities of this variable was justified by the fact that different terminologies are used in the literature to designate the Mixed logit models (Revelt and Train, 1998), which are widely used in DCE analysis. Indeed, the Mixed logit models (MLM) are also called random-parameters logit (RPL) or error-components logit (ECL). These models generalized the standard logit model in the sense that they do not exhibit the IIA (independence from irrelevant alternatives) restrictive property (Revelt and Train, 1998).

Second, we added ASC (Alternative-Specific Constants), a binary variable that indicates that the chosen modelling has an (alternative-specific) constant. Indeed, the lack of constant may influence the reliability of parameter estimations (Klaiber and von Haefen, 2011).

Third, we introduced a *Published* binary variable to indicate if studies are published in journals, reviews, or only in "gray literature" (conference communications, theses, working papers). This can allow us to measure publication bias related to the theoretical and methodological quality of the study (Rothstein et al., 2006).

Fourth, we added the *Number* of authors, a continuous variable to measure the number of authors that may influence the quality of the study and its acceptance rate into publication (McGillivray and De Ranieri, 2018). Smith and Newman (2014) report a strong bias when we have multiple authors, while Barlow et al., 2018report a positive effect.

To consider the heterogeneity between the different agrienvironmental practices to be adopted, we introduce *Innovative Practice*, a binary variable to indicate that the practice studied is an innovative one. Indeed, farmers are more reluctant to adopt innovative practices than standard ones (Rogers, 2003).

3.4. Empirical Model

Discrete outcome models were used to investigate factors affecting research results regarding the influence of contract attributes on the WTA agrienvironmental practices. Our baseline model is a cluster-robust probit model (Pustejovsky and Tipton, 2018). An appealing feature of this model is that it allows controlling for intra-study autocorrelations arising from multiple observations having been drawn from a given paper. In this approach, which is commonly used in meta-regressions (e.g., Barrio and Loureiro, 2010; Choumert et al., 2013; Ugur, 2014; Havranek et al., 2016; Minviel and Latruffe, 2017), the standard errors are clustered by each primary study. The cluster-robust probit model has been applied to four outcomes reported in the existing literature regarding the influence of contract attributes on the adoption of agrienvironmental practices. The outcomes concern the effect of Commitment and Incentive clauses on the adoption of agrienvironmental contracts. The probability of an outcome (y), given the moderator (explanatory) variables (x), can be shown as:

$$\begin{aligned} \mathrm{Prob}(y = 1|\ x, \beta) &= \mathrm{Prob}(x\beta + \varepsilon > 0) \\ &= 1 - \mathrm{F}(-x\beta) \end{aligned} \tag{1}$$

where $F(.)$ is an unknown function that depends on the distribution of the error term. In a binary probit model, $F(.)$ is specified as a cumulative density function of the standard normal distribution $\phi(.)$. Although the probit model is widely used, its validity relies on the normality assumption. However, as discussed in the literature (e.g., Gallant and Nychka, 1987; Klein and Spady, 1993), if the error term does not follow a normal distribution, the probit model, may lead to inconsistent estimates. Accordingly, and for comparison purposes, we also used more flexible semi-nonparametric[5] (SNP) binary outcome models (Gallant and Nychka, 1987; Gabler et al., 1993; Melenberg and Van Soest, 1996; De Luca, 2008). More precisely, we use SNP univariate (binary-choice) models, where the normality assumption is used indirectly.

For a univariate SNP model, the unknown density of the random error term is approximated by the Hermite polynomial expansion method, as follows (De Luca, 2008):

$$\mathrm{F}(-x\beta) = \Phi(-x\beta) - \frac{1}{\mathrm{H}}\phi(-x\beta) \times \left[\sum_{l=0}^{L} \alpha_l^{*} l^{*}(-x\beta)^{l}\right] \tag{2}$$

where $H = \int_{-\infty}^{+\infty} \lambda_K(\varepsilon)^2 \phi(\varepsilon)d\varepsilon$, $\varepsilon = -x\beta$, and λ_K is a polynomial of order K; $\Phi(.)$ is the standardized Gaussian distribution function, $\phi(.)$ is the probability density function of the standard normal distribution. H is a function that ensures the unknown function $F(.)$ integrates to a value of

[5] An alternative approach is the semi-parametric maximum likelihood (SML) estimator (Klein and Spady, 1993; Lee, 1995). However, Monte Carlo evidence in De Luca (2008) suggests that the SNP estimator has better finite sample performance than the SML estimator.

one. This expression shows that the polynomial approximation generalizes the binary probit model by adding several polynomial terms. If the parameter of the polynomial order (l) is equal to 0, the SNP model is equivalent to the binary probit model. That is, the univariate probit model is nested in the univariate SNP model. As such, a likelihood-ratio test of the probit model against the univariate SNP model can be easily implemented.[6]

4. Results and Discussion

We first present and discuss the results of the two different categories of the clause, that is, Commitment and Incentives. Then, we provide some comments on two specific clauses, namely Duration and Quality, for which we have enough observations to run robust estimates (34 and 59 observations for Duration and Quality, respectively).

4.1. Results of Commitment and Incentive Clauses

The estimates for the univariate (clustered) probit model (Eq. (1)) and the univariate SNP probit model (Eq. (2)) are reported in Table 3. The likelihood-ratio test of the univariate probit model against the univariate SNP model for the four dependent variables (Commitments $+/-$ and Incentives $+/-$) shows that the normality assumption may be violated; thus, the SNP model may provide more consistent estimates. That is, in terms of significance and uncertainty about the estimated parameters, the semi-nonparametric estimator outperforms the probit model.

If we consider the DCE design stage, regarding the number of alternatives, the SNP estimator indicates that the likelihood of a positive effect of Commitment on the WTA increases while the parameter estimates for univariate probit are not statistically significant. For the incentives clauses, both estimators show that the effect of the number of alternatives is nonsignificant.

For the number of attributes, the univariate probit models indicate that their effects on the Commitment clauses are not statistically significant. However, under the SNP estimator, the number of attributes also decreases the likelihood of a negative effect of Commitment clauses on the WTA, even if it does not increase the likelihood of a positive effect. In contrast, the number of alternatives is negatively associated with the effects of Incentive clauses. The optimization of the number of attributes by alternative is a major challenge in the DCE since our results suggest that a trade-off must be made between commitment and incentive clauses. Previous research has concluded that a large number of alternatives and attributes increase the cognitive load of respondents, resulting in inconsistent responses (Mazzotta and Opaluch, 1995; Swait and Adamowicz, 2001; DeShazo and Fermo, 2002; Bech et al., 2011). Watson et al. (2017) meta-analysis on the DCE also shows that these parameters can affect even the response rate to the survey. A reasonable number of attributes must be maintained while also capturing enough information to estimate stable utility at the individual level and avoid creating interaction effects by many attributes (Orme et al., 1997; Carson et al., 2009).

Regarding the choice availability effect, the results indicate that the presence of the status quo (*SQ*) option does not impact the positive or negative effect of Commitment or Incentive clauses. In this sense, the meta-analysis conducted by Barreiro-Hurle et al. (2018) shows that the presence of the Status Quo as an option in the choice cards decreases to the extent that the respondents' cognitive burden is lightened. If Illustrated cards (*ICards*) increase the likelihood of getting a negative effect of Commitment clauses, this variable has no significant effect on Incentives clauses. However, Bennet and Birol (2010) recommend in their guide to good practices for DCE that illustrated cards be used to help respondents make better choices.

For the implementation stage of the experiment, the SNP estimates show that number of cards increases the likelihood of having a negative effect for Commitment and Incentive clauses and at the same time reduces the probability of getting the positive effect of Commitment clauses. For the sample size variable, while the parametric probit estimates are not statistically significant, those of the SNP estimator indicates that the likelihood of a positive effect of Commitment clauses on the adoption of agrienvironmental contract increases, while the negative effect also decreases. In contrast, the impact on Incentive clauses is nonsignificant. This suggests that, for the latter clauses, the sample size must be carefully supervised to avoid bias. It seems that the sample size in the DCE may follow a rule of thumb related to the planned analysis methodology. Orme (2010) gives a formula for calculating the minimum sample size (N) for the DCE:

$$N > 500 * \frac{l}{J * S}$$

where l is the largest number of levels among the proposed attributes, J is the number of alternatives proposed by cards, and S is the number of cards inserted for each participant. Orme (2010) suggests a minimum sample size of 200 respondents per group. If this condition was not always fulfilled in the primary studies of our meta-analysis, the "McFadden (1984) rule", which states that sample size which yields more than 30 responses per alternative produces estimators that can be analyzed reliably by an asymptotic method, applies.

For survey methods used in the primary studies, the results indicate that when surveys are made face-to-face only or using Internet only, they decrease the likelihood of obtaining a positive effect of Commitment clauses or a negative effect of Incentive clauses on the adoption of agrienvironmental practices. Both methods, taken in isolation, may create biases (Ellis and Krosnick, 1999) therefore, a mixed survey approach using both face-to-face and internet surveys may be more efficient.

The year of publication (*Year*) seems also to have an effect because it negatively impacts the likelihood of having a positive sign for commitments ("Commitment Positive") and incentives ("Incentive Positive"). Similarly, it has a negative impact on the likelihood of having negative sign for Commitment and Incentive clauses. This may suggest that the more researchers are experienced in DCE methodology, the lower the likelihood of finding a biased impact, either positive or negative, of contract clauses on the WTA. Similar results can be found in the meta-analysis conducted by Barrio and Loureiro (2010) on the Contingent Valuation method.

Regarding geographic area, we find contrasting results on Commitment and Incentive clauses in North America and developing countrieson the one hand, and in European countries on the other hand (Oceania region is the reference). Our results indicate first that it is more likely to find a positive effect of Commitment clauses on the adoption of agrienvironmental contracts in developing countries (coefficient significant at 1% level), then in North America (coefficient significant at 5% level), and finally in Europe (coefficient significant at 10% level) . Regarding incentives, the estimates for "Incentive Positive" in Europe are significantly positive while the estimate for "Incentive Negative" is significantly negative. That is, the likelihood of a positive effect of Incentive clauses on the adoption of agrienvironmental contracts increases, while that of a negative effect decreases. This suggests that, in contrast to North America and developing countries , incentives are especially needed in Europe to foster the adoption of agrienvironmental practices.

[6] We also tested for multivariate probit models. First, for the bivariate probit models we note that the conditional tetrachoric correlation (ρ) between Commitment and Incentive clauses is not statistically significant. This suggests that the decisions to adopt Commitment and Incentive clauses are not dependent. That is, the univariate models are sufficient for investigating factors affecting research results regarding the influence of contractual attributes on the adoption of agrienvironmental practices. Second, we tested for multinomial and ordered probit models, but estimates are plagued by convergence issues. The results can be obtained from authors upon request.

Table 3
Estimated parameters for the univariate probit and the univariate SNP models.

	Binary probit				SNP model			
	Commitment positive	Commitment negative	Incentive positive	Incentive negative	Commitment positive	Commitment negative	Incentive positive	Incentive negative
Number of alternatives	0.11 (0.19)	0.13 (0.22)	0.17 (0.25)	0.14 (0.32)	0.66*** (0.17)	0.05 (0.16)	0.16 (0.20)	0.12 (0.17)
Number of attributes	5.44E-04 (0.11)	0.06 (0.12)	0.25** (0.12)	0.28** (0.12)	0.03 (0.09)	0.41*** (0.11)	0.49*** (0.18)	0.49*** (0.15)
SQ	0.37 (0.29)	0.32 (0.32)	0.31 (0.41)	0.19 (0.45)	0.09 (1.27)	0.22 (0.38)	0.39 (0.43)	0.29 (0.29)
ICard	0.17 (0.21)	0.35* (0.19)	0.49 (0.29)*	0.06 (0.33)	0.31 (0.27)	0.37*** (0.28)	0.36 (0.33)	0.03 (0.29)
Number of cards	0.03 (0.01)	0.03** (0.01)	0.01 (0.02)	4.36E-04 (0.03)	0.05* (0.03)	0.10*** (0.02)	0.02 (0.02)	0.04** (0.02)
Sample size	5.27E-04 (3.69E-04)	2.18E-04 (3.47E-04)	7.21E-06 (2.94E-06)	3.22E-05 (3.22E-05)	1.65E-03*** (4.17E-03)	9.385E-04*** (1.72E-04)	2.15E-07 (2.74E-04)	1.35E-04 (3.96E-04)
Face-to-face survey	0.22 (0.37)	0.48 (0.41)	0.34 (0.43)	0.41 (0.69)	0.76* (0.45)	0.11 (0.52)	0.14 (0.51)	1.41** (0.62)
Web survey	0.21 (0.42)	0.56 (0.45)	0.27 (0.48)	0.10 (0.65)	1.22*** (0.45)	0.81 (0.56)	0.16 (0.55)	1.23** (0.61)
Year	0.03 (0.05)	0.03 (0.05)	0.06 (0.05)	5.57E-04 (0.06)	0.03*** (5.29E-04)	0.03*** (4E-04)	0.06*** (5.49E-04)	7.01E-03*** (4.54E-04)
Europe	0.39 (0.33)	0.48 (0.30)	0.04 (0.39)	0.22 (0.64)	0.72* (0.38)	1.13* (0.61)	0.39 (0.57)	1.07 ** (0.49)
North America	0.71* (0.41)	1.01*** (0.39)	1.07** (0.53)	0.79 (0.75)	1.90** (0.75)	2.71 *** (0.87)	2.53*** (0.66)	2.74 *** (0.53)
Developing countries	0.66** (0.33)	0.71** (0.30)	0.55 (0.48)	0.61 (0.76)	1.62*** (0.50)	1.76*** (0.61)	0.88 (0.64)	1.29*** (0.43)
Modelling	0.05 (0.37)	0.06 (0.44)	0.29 (0.67)	/	0.57 (0.49)	1.45** (0.34)	0.28 (0.48)	2.53 *** (0.69)
ASC	0.08 (0.31)	0.09 (0.31)	0.01 (0.30)	0.46 (0.33)	0.13 (0.38)	0.85*** (0.31)	0.08 (0.36)	0.75 *** (0.28)
Published	5.59E-03 (0.25)	0.281 (0.24)	0.42 (0.33)	0.47 (0.38)	0.18 (0.26)	1.02 (0.31)	0.70*** (0.23)	1.11*** (0.24)
Number of authors	0.09 (0.07)	0.12** (0.06)	0.04 (0.07)	0.05 (0.07)	0.09 (0.06)	0.29 *** (0.08)	0.09 (0.09)	0.01 (0.07)
Innovative practices	0.38 (0.31)	0.37 (0.26)	1.58*** (0.56)	1.22* (0.69)	0.85*** (0.32)	0.75 (0.54)	2.66*** (0.64)	0.73** (0.33)
Intercept	62.85 (99.87)	53.59 (100.45)	133.309 (98.09)	8.82 (115.01)	/	/	/	/
N	173	173	117	117	173	173	117	117
LR test of Probit model against SNP model (Chi2)					11.36***	12.12***	10.72***	13.99***

NB: *, **, ***: successively significant at $p \leq .10$, $p \leq .05$, $p \leq .01$.; standard errors in parenthesis.

Finally, if we consider the analysis stage, the SNP model shows that the regression models used in the primary studies have a significant effect on the impact of clauses. For instance, our results show that the use of regression models that do not exhibit the IIA restrictive property (MLM, RPL, or ECL) increases the likelihood of a positive effect of Commitment and Incentive clauses on the adoption of agrienvironmental practices. In his review of stated choice methods like DCE, Shen (2006) emphasizes the importance of avoiding the IIA hypothesis in the analytical models; otherwise, the estimation results will be biased. In addition, it appears that the integration of an ASC in the primary estimation decreases the likelihood of a negative effect of Incentive clauses on the adoption of practices. According to the latter author, the inclusion of ASC in the estimates captures the heterogeneity of respondent characteristics. These results suggest that empirical models should be scrutinized before deriving policies from the estimates.

Regarding publication effect, the results indicate that it is more likely to find a positive effect of Incentive clauses on the adoption of agrienvironmental contracts in published studies than in non-published ones. That is to say, the theoretical and methodological quality of the primary studies reduces the likelihood of obtaining negative effects of contract clauses on the WTA. This could also be understood in the sense that it is more likely to publish papers with positive effects of contract clauses on the WTA. Regarding the number of authors, we observed a limited effect because the SNP estimator indicates that the number of authors decreases the likelihood of having a negative sign for Commitment clauses, while it has a nonsignificant impact on the sign of Incentive clauses.

Regarding the nature of the practice to be adopted, our results indicate that an innovative practice has a negative impact on the likelihood of getting a "positive Commitment" clause, while it increases the probability of having an "Incentive positive" clause. This implies that when an innovative practice is at stake, the Incentive clauses have a positive effect on the WTA, while the Commitment clauses have a negative one. These results suggest that agri-environemntal contracts designed to encourage the adoption of innovative practices must introduce incentives clauses.

4.2. Results for Duration and Quality Clauses

Regarding the sign of the duration clause, our meta-analysis confirms that in average duration has a negative effect on the adoption of agri-environmental contracts (20 over 34 estimates reported). But this is not true in general because we found 9 observations from 6 papers in our database for which estimates show a positive effect of duration. Furthermore, there are 5 estimates in our database with a non-significant effect. Evidence on the effect of the duration is thus still inconclusive (Vorlaufer et al., 2017).[7]

In the clustered univariate probit estimates of *Duration* and *Quality* clauses, our estimates on these two specific clauses (*Duration* and *Quality*) in the Table 4 mainly confirm our previous results for the two categories of clauses, *Commitments* and *Incentives*.[8]

For example, the number of attributes decreases the likelihood of getting a negative effect on duration. This is very similar to the previous results for Commitment clauses. Similarly, the number of alternatives variable reduces the probability of having a positive sign for the duration. In contrast, both variables have a nonsignificant impact on the quality clause.

The regional variables have the same impact for duration and quality clauses than for commitment clauses. However, other variables exhibit different results than those found in Table 3. For example, the number of cards and the introduction of an innovative practice increase the likelihood of having a positive sign for the duration clause but have a nonsignificant impact on the quality clause.

[7] "Several choice experiments included the contract duration as an attribute in their experimental design. Overall empirical evidence is inconclusive. Some studies found a preference for shorter contracts (5 vs 9 vs 17 years) (Balderas-Torres et al., 2013), while others found preferences for longer contracts (15 vs 25 vs 35 years) (Arifin et al., 2009) and (3 vs 10 years) (Zabel and Engel, 2010)" (p. 98).

[8] Some moderators (explanatory variables) have been omitted because of collinearity, for example, *Modelling* and *Web Survey* variables in the Duration estimates (see Table 2).

Table 4
Estimated parameters for the univariate probit Duration and Quality.

	Duration positive	Duration negative	Quality positive	Quality negative
Number of alternatives	4.27*** (1.40)	2.80*** (1.48)	0.32 (0.35)	0.001 (0.33)
Number of attributes	0.85* (0.53)	0.44 (0.47)	0.28 (0.22)	0.30 (0.23)
SQ	1.30 (1.24)	1.55 (1.61)	0.52 (0.60)	0.77 (0.69)
ICards	2.13* (1.21)	0.92 (0.80)	0.009 (0.42)	0.13 (0.48)
Number of cards	0.27*** (0.10)	0.14 (0.10)	0.02 (0.03)	0.03 (0.03)
Sample size	0.001* (0.001)	0.002* (0.01)	0.001*** (0.001)	0.002** (0.001)
Face-to-face survey	3.04*** (1.25)	0.27 (1.24)	5.20*** (0.76)	4.37*** (0.92)
Web survey	/	/	5.52*** (0.85)	4.86*** (0.78)
Year	0.21 (0.24)	0.24 (0.19)	0.15** (0.07)	0.27*** (0.80)
Europe	4.63*** (1.50)	6.25*** (1.28)	5.119*** (0.67)	5.03*** (0.67)
North America	1.64 (2.75)	9.77*** (3.15)	5.35*** (0.92)	5.25*** (0.90)
Developing countries	6.61*** (1.76)	6.88*** (1.33)	5.25*** (0.86)	5.39*** (0.88)
Modelling	/	/	0.98 (0.79)	1.70* (0.99)
ASC	1.66*** (0.70)	2.34 (1.71)	0.29 (0.54)	0.46 (0.62)
Published	_0.47 (0.80)	1.26 (0.85)	0.11 (0.51)	0.24 (0.51)
Number of authors	0.56** (0.28)	0.21 (0.23)	0.18 (0.13)	0.22 (0.14)
Innovative practices	2.08** (1.18)	0.38 (0.75)	0.23 (0.73)	0.69 (0.85)
Intercept	445 (493)	_497 (402)	308** (147)	424*** (163)
Pseudo R^2	0.50	0.45	0.25	0.33
Log pseudo likelihood	9.73	12.5	30.5	27.1
N	34		59	

NB: *, **, ***: successivement significatifs at $p \leq .10$, $p \leq .05$, $p \leq .01$.

Our estimates from the last two sub-samples have a limitation related to their size, particularly for the *Duration* clause where there is little variability in the observations. Moreover, there are problems with the original data sets and the lack of consistency between primary papers in the definition of clauses, e.g. the duration clause. Our meta-analysis, therefore, makes a recommendation to encourage reviews to promote a more standardized codification of the main contract clauses.

5. Conclusion

In our paper, we conducted a meta-analysis of a set of academic studies that employ the same DCE methodological approach to focus on the design of contracts that may increase a farmer's Willingness to Adopt (WTA) agrienvironmental practices. In the set of studies, a variety of data collection and analysis methods were implemented that generated significant differences in estimated values. Our objective was to analyze the impact of methodological choices related to the timing of the DCE process (design stage, implementation stage, and analysis stage) on the estimates. Our meta-analysis provides guidance for mitigating potential biases that can the results of this assessment method. Based on our results, the present meta-analysis can also be used to make recommendations on how to increase the reliability of public policy instruments for agrienvironmental practices adoption.

More precisely, our targeted population of studies was defined according to two criteria. First, we chose papers using DCE approach on contracts fostering agrienvironmental practices. Papers using DCE yet focusing on the design of other types of contracts (e.g., adoption of technology, farming contracts, and more) were excluded from our target population. Second, we included only papers estimating WTA. Papers estimating WTA before WTP (Willingness to Pay) were also considered, but those estimating only WTP were excluded.

We first highlighted the possibility of simplifying contract attributes to propose two representative clauses (commitments and incentives), which were regressed using the univariate clustered probit and the more flexible univariate semi-nonparametric (SNP) model (Gallant and Nychka, 1987; De Luca, 2008).

When we consider the DCE design stage, increasing the number of alternatives may have a positive impact on commitment clauses. Regarding the number of attributes, our meta-analysis suggests that if increasing the number of attributes has a negative impact on incentive clauses, it also reduces the likelihood of a negative effect of commitment clauses. Hence, optimizing this critical parameter in the initial design of DCE is necessary to improve the reliability of the data collected and their interpretation. Regarding the attention control effect, illustrated cards increase the probability of getting a significant impact from commitment clauses. In contrast, the availability effect (status quo option) has no significant impact on clauses.

For the implementation stage of the DCE, our estimates show that the number of cards is associated with a negative impact on contract clauses. In contrast, the sample size variable has a positive effect on commitment clauses only. Furthermore, regarding the survey methods used in the primary studies, the results indicate that compared to face-to-face surveys on the one hand and Internet surveys on the other hand, a mixed survey approach using both methods can be more efficient. The year of publication also has an effect, since it negatively impacts the likelihood of having a positive sign for contract clauses. This may suggest that the more researchers are experienced in DCE methodology, the lower the likelihood of finding a positively biased impact of contract clauses on WTA. Regarding geographic area, we observed that in contrast to developing countries and North America, incentives are especially needed in Europe to foster WTA.

When we consider the analysis stage of the DCE, our meta-analysis indicates that the regression models used in the primary studies have a significant effect on clauses. Moreover, the integration of an ASC in the primary estimation has also a significant effect, which suggests that empirical modelling should be carefully scrutinized before deriving policy recommendations. Regarding publication effect, published papers are more likely associated with the positive impact of incentive clauses than non-published ones. This suggests that "academic quality" status of the primary studies reduces the likelihood of negative effect of contract clauses on WTA. In contrast, the number of authors has a nonsignifiant effect. Regarding the nature of the practice to be adopted, when an innovative practice is at stake, the incentive clauses have a positive effect on WTA. This suggests a clear recommendation for public policy to include incentive clauses in agri-environmental contracts to foster the adoption of innovative practices.

Our meta-analysis on two specific, *Duration* and *Quality*, mainly confirms our previous results. We also have contrasting results regarding the number of cards or the introduction of innovative practice.

Finally, our approach and results could also be relevant for the application of DCE to other domains than agrienvironmental practices, as well as for applications focusing on other land or resource managers (foresters, fishermen, etc.) or other types of contracts, for example, resource use (Guerrero-Baena et al., 2019), adoption of technology

(Windle and Rolfe, 2005), or farming contracts (Abebe et al., 2013).

Declaration of competing interest

None.

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