European Economic Review 120 (2019) 103310

Contents lists available at ScienceDirect

European Economic Review

journal homepage: www.elsevier.com/locate/euroecorev

Truth-telling and the regulator. Experimental evidence from commercial fishermen

Moritz A. Drupp[a,*], Menusch Khadjavi[b,c,d], Martin F. Quaas[b,e,f]

[a] Department of Economics, University of Hamburg, Von-Melle-Park 5, 20146 Hamburg, Germany
[b] Kiel Institute of the World Economy, Kiellinie 66, 24105 Kiel, Germany
[c] Tinbergen Institute, the Netherlands
[d] Department of Spatial Economics, VU Amsterdam, the Netherlands
[e] German Centre for Integrative Biodiversity Research (iDiv) Halle-Jena-Leipzig, Deutscher Platz 5e, 04103 Leipzig, Germany
[f] Department of Economics, Leipzig University, Germany

ARTICLE INFO

Article history:
Received 25 April 2018
Accepted 18 July 2019
Available online 7 September 2019

JEL codes:
D63
K32
L51

Keywords:
Truth-telling
Lying
Field experiment
Fishermen

ABSTRACT

Understanding what determines the truth-telling of economic agents towards their regulator is of major economic importance from banking to the management of common-pool resources such as European fisheries. By enacting a discard-ban on unwanted fish-catches without increasing monitoring activities, the European Union (EU) depends on fishermen's truth-telling. Using a coin-tossing task in an artefactual mail field experiment with 120 German commercial fishermen, we test whether truth-telling in a baseline setting differs from behavior in two treatments that exploit fishermen's widespread ill-regard of their regulator, the EU. We find, first, that fishermen misreport coin tosses more strongly to their advantage in a treatment where they are faced with the EU flag, and, second, that misreporting is consistent with behavior in other hidden tasks. We also find some supportive evidence for our first result in a conceptual replication with 1200 UK citizens who voted 'leave' in the Brexit referendum. Our findings imply that lying is more extensive towards an ill-regarded regulator and that policy needs to account for this endogenously eroding honesty base.

"*fishermen hold an almost entirely negative view of the EU*"
McAngus (2016: 4) reporting survey results for UK fishermen

1. Introduction

Although honesty is regarded as a virtue or even a moral duty (Kant, 1785), lying and deception permeate economic life (Gneezy, 2005). Studying truth-telling has accordingly become a focus of inquiry for economics.[1] An area of particular public economic importance is the truth-telling of economic agents towards their regulating authorities—from the banking

* Corresponding author.
E-mail addresses: Moritz.Drupp@uni-hamburg.de (M.A. Drupp), Menusch.Khadjavi@ifw-kiel.de (M. Khadjavi), martin.quaas@idiv.de (M.F. Quaas).

[1] See, e.g., Abeler et al. (2014, 2019), Cappelen et al. (2013), Cohn et al. (2014, 2015), Fischbacher and Föllmi-Heusi (2013), Gächter and Schulz (2016), Gibson et al. (2013), Gneezy (2005), Gneezy et al. (2013, 2018), Houser et al. (2016), Mazar et al. (2008), Pasqual-Ezama et al. (2015), Potters and Stoop (2016), Rosenbaum et al. (2014) Houser et al. (2012) and Sutter (2009).

https://doi.org/10.1016/j.euroecorev.2019.103310

industry (Cohn et al., 2014), and tax reporting (Jacobsen and Piovesan, 2016; Kleven et al., 2011) to environmental regulation (Duflo et al., 2013). The case where the German car manufacturer Volkswagen systematically lied about cars' emissions is but one prominent example. Faced with uncertainty about how honest economic agents are, regulators need to decide how much to invest in monitoring and how to devise appropriate sanctioning schemes for misbehavior.

Appropriate monitoring and sanctioning mechanisms are especially crucial for the management of common pool resources (Ostrom et al., 1992; Rustagi et al., 2010), with the fishery as a prime example (Wilen, 2000; Stavins, 2011). Fishery management comes in many different forms around the globe. It ranges from stringent restrictions on fish catches using individual transferable quotas—as in New Zealand (Newell et al., 2005) or Iceland (Arnason, 2005)—to largely unregulated open-access fishing, as it is still the case for most high-seas fisheries. The costs of illegal, unreported and unregulated fishing are substantial and amount to US$ 10 to 23 billion per year (Global Ocean Commission, 2013). Due to its economic importance and the heterogeneity of its regulatory structures, the fishery has gained substantial interest in experimental economic work.[2]

This paper extends the scope of previous studies and investigates to what extent regulator framing affects truth-telling. Our study therefore adds a new dimension to effective regulatory policy. We present evidence from an artefactual mail field experiment that examines truth-telling of German commercial fishermen. German commercial fishing is regulated by the European Union (EU), which is the world's fourth largest producer of fish, under the European Common Fisheries Policy. The EU has recently enacted a ban on returning unwanted fish catches to the sea (also called "discard ban" or "landing obligation"), as the practice of discarding ensues substantial costs to the public.[3] The change in legislation has, as of yet, not been combined with more stringent monitoring. The regulator, and scientists assessing the status of fish stocks upon which recommendations for fishery management are based, thus depend on fishermen's truth-telling. Continuing to discard unwanted fish catches remains the individually optimal choice for fishermen in the present regulatory regime unless the regulator enforces the new policy. This, however, would require costly monitoring and sanctioning mechanisms.[4] This trade-off for the regulator between more costly monitoring and reliance on regulatee's honesty is not only relevant in the fishery for the newly enacted European "discard ban" or compliance with fishing quotas, but is present more generally, including the previously discussed cases of banking, tax reporting and environmental regulation.

For studying to what extent fishermen might tell the truth towards their regulator, we conduct a coin-tossing game in a mail field experiment targeting all commercial fishermen in Germany. Adapting the 4-coin toss game of Abeler et al. (2014), we ask fishermen to toss a coin 4 times and report back their number of tail tosses. For each reported tail toss, they receive five Euros. In a between-subjects design, we test whether truth-telling in a baseline setting differs from truth-telling in two further treatments with different EU framings, where, first, the EU flag is made salient on the instruction sheet, and, second, a framing that states additionally that the European Commission has funded the research. Based on a simple model of reporting behavior of fishermen that considers internal Nash bargaining among a pay-off maximizing 'selfish self' and a 'moral self', we hypothesize that the salience of the EU regulator may increase the bargaining power of the 'selfish self' vis-à-vis the 'moral self' and thus decrease overall lying costs if the EU is ill-regarded.

The fishery is an ideal test case for studying how truth-telling behavior may be affected by regulatory framing, as there is well-documented and wide-spread contempt among fishermen concerning stricter EU fishing regulation. We confirm the almost entirely negative view of the EU prevalent among European fishermen (documented for UK fishers by McAngus, 2016) for our field experimental setting in Germany: Besides ample anecdotal evidence, our survey results indicate that the vast majority of participating fishermen have a low trust in the EU, while this is only the case for about a third of a student control group. If regulator framing impacts truth-telling, we will therefore expect an almost uniform direction of the effect. To study the robustness of our findings, we conduct a similar experiment with a population that is similar to the fishermen with respect to the (negative) stance towards the EU: Brexiteers. Using a large sample of UK citizens we conduct the experiment with 1200 individuals who reported to have voted 'leave' in the Brexit referendum in previous questionnaires.

We find that fishermen misreport coin tosses to their advantage, albeit to a lesser extent than standard theory predicts. As hypothesized, misreporting is larger among fishermen who are faced with the EU flag. Our main effect is supported by further regression analyses. We also find some support for our main result in the conceptual replication with Brexiteers. Furthermore, we find that misreporting by fishermen is consistent with behavior in other hidden tasks involving a sent-along coin and the possibility to cheat in a competition task, suggesting some more general validity of the coin toss findings. Overall, our results imply that lying is more extensive towards an ill-regarded regulator. We close by discussing further policy relevance of our results.

[2] Among others, previous studies scrutinize cooperativeness, competitiveness and impatience among fishermen in Brazil (Fehr and Leibbrandt, 2011; Leibbrandt et al., 2013; Gneezy et al., 2015). Stoop et al. (2012) examine cooperation among recreational Dutch anglers, while Jang and Lynham (2015) investigate social preferences among lake fishermen in Kenya.

[3] Unused catches imply opportunity costs for fishermen and society. Patrick and Benaka (2013) estimate that bycatch discards represent a loss of $4.2 billion in potential sales in the US alone.

[4] More stringent monitoring could come in different forms, such as more frequent patrolling of sea police, sending observers on-board or installing video cameras on ships to monitor whether fishermen comply with the law. Associated cost estimates are substantial, ranging from $8,000 to $13,000 per ship annually for remote camera monitoring in Canada and Denmark (Mangi et al., 2015), to 200,000 € for on-board observers in Denmark (Kindt-Larsen et al., 2011). FAO estimates that discard-related spending by regulating authorities worldwide totals annual costs of $4.5 billion (Alverson, 1994).

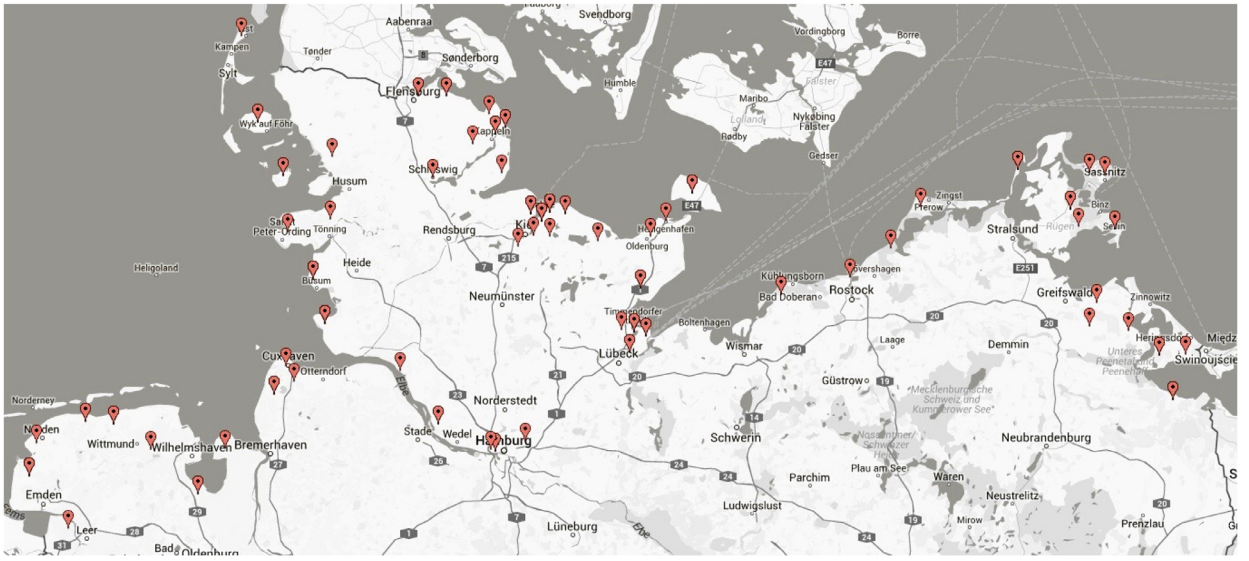

Fig. 1. Map of North Germany. The (red) balloons represent the zip-codes of participating fishermen. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

2. Field setting, experimental design and hypotheses

The fishery has economic relevance in the German coastal regions at both the North Sea and Baltic Sea. According to the European Union's Common Fisheries Policy (CFP), the Council of Ministers of the European Union and the European Parliament set fishing quotas for the German fisheries. The German Federal Office for Agriculture and Food distributes the national catch quotas to fishing organizations or individual fishermen. Monitoring and enforcement of compliance are the duty of EU member states, and ultimately of the federal states in the case of Germany. A total of 896 commercial fishermen, owning 1465 fishing vessels (German Fishery Association, 2015), are registered at the German Federal Office for Agriculture and Food as holders of catch permits for the North Sea or Baltic Sea. Cutter type trawlers and coastal vessels constitute the core of the fleet with 300 boats. Small coastal fishing with passive gear such as gill nets and fish traps on vessels of less than 12 m length, composed of 1139 vessels, is predominantly operated at the Baltic coast. The German fishing fleet also includes seven deep-sea trawlers and two special vessels for pelagic fishing that operate in long distant waters, and 46 shell- and other special boats. Fig. 1 depicts a map of Germany's coastal regions, where the red balloons indicate the zip codes of fishermen who have participated in the experiment.

The recent economic literature on honesty and lying has substantially advanced our understanding on what determines when and to what extent individuals lie. Abeler et al. (2019) conduct a large-scale meta-analysis of studies using coin-tossing and die-rolling tasks. This meta-analysis shows that, on average, individuals lie to some, but not to an exhaustive, extent and that the extent of lying does not seem to increase with the stakes.

This paper contributes a new dimension to the analysis of truth-telling behavior: by studying how the salience of the regulator, who depends on truth-telling behavior in the policy context, affects the behavior of those being regulated. To this end, we adapt the 4-coin-tossing game of Abeler et al. (2014) for our mail field experiment. The fishermens' task was to toss a one Euro coin exactly 4 times, and report their result in a table printed on the instructions sheet. For each instance they reported that the winning toss "tails" (in German "Zahl", meaning "number") laid on top, they received 5 €. A key feature of this task is that lying can be detected on aggregate when examining the distribution of decisions, but not on the individual level. Thus, depending on luck and honesty, each fisherman received between 0 and 20 € for this task. Besides the different sample pool, a major difference to the previous study is that Abeler et al. (2014) conducted their 4-coin experiments via telephone or in the lab and the decision whether to report truthfully or to cheat was immediate, while our subjects had several weeks to decide on whether to report honestly or to lie.

In absence of a possibility to detect individual lying, a fisherman i is assumed to face a trade-off between monetary incentives and moral costs of lying (Akerlof and Kranton, 2000, 2005; Cohn et al., 2015; Levitt and List, 2007).[5] Here we propose the following extension of the standard model where an individual maximizes a utility function that describes this trade-off. We assume that an individual fisherman faces an internal bargain between two 'selves', one being a purely pay-off

[5] Based on different schools of ethics, it is not trivial to assume an optimization problem of truth-telling. There may be some individuals who behave in line with Kantian deontological ethics and do not lie, out of a duty to tell the truth independent of the consequences. While studies like Gneezy (2005) and Gibson et al. (2013) find that many participants of their studies appear to be consequentialists, most studies also report at least some fraction of participants who never lie. Our model is based on the assumption that a sizeable fraction of fishermen are behaving as consequentialists.

maximizing 'selfish self', the other one being a 'moral self' purely interested in compliance with the moral standard to tell the truth. While the 'selfish self' derives utility only from its payoff proportional to the reported number r_i of coin tosses, the 'moral self' suffers a disutility from reporting a number r_i that deviates from the true number of tail tosses, r_{it}. To keep the model simple, we specify utility functions that satisfy standard assumptions and have only one parameter each. Specifically, for the 'selfish self' we assume the following one-parameter utility function that is increasing and concave in the payoff

$$u^s(r_i) = -e^{-\beta_i r_i}, \tag{1}$$

and for the 'moral self' we assume the following one-parameter utility function that is decreasing and convex in the squared deviation of the reported from the actual number of tail tosses,

$$u^m(r_i) = -e^{\frac{\gamma_i}{2}(r_{it}-r_i)^2}. \tag{2}$$

Here, $\beta_i > 0$ is a parameter scaling the marginal utility of income from reported tail tosses. The larger β_i the more the individual enjoys increases in the monetary payoff. The parameter $\gamma_i > 0$ can be interpreted as the *misreporting aversion* of the 'moral self'. The larger γ_i, the more the individual suffers from dishonest reporting. These two selves engage in a standard Nash bargaining (Binmore et al., 1986), i.e. they 'agree' on the reported number r_i of tail tosses that solves

$$\min_{r_i} (\bar{u}^s - u^s(r_i))^{\alpha_i} (\bar{u}^m - u^m(r_i, r_{it}))^{1-\alpha_i}. \tag{3}$$

That is, the resulting number r_i of reported tail tosses minimizes the weighted geometric mean of the deviation of utilities from respective upper reference levels $\bar{u}^s$and $\bar{u}^m$,[6] where the parameter $\alpha_i$captures the bargaining power of the 'selfish self' relative to the 'moral self'. To facilitate the analysis, we set $\bar{u}^s = 0 \geq \sup_{r_i} u^s(r_i)$ and $\bar{u}^m = 0 \geq \sup_{r_i} u^m(r_i)$in the following.

The first-order condition for the bargaining problem (3) is given by

$$\alpha_i\beta_i\left(e^{\frac{\gamma_i}{2}(r_{it}-r_i)^2}\right)^{1-\alpha_i}\left(e^{-\beta_i r_i}\right)^{\alpha_i} + (1-\alpha_i)\gamma_i(r_{it}-r_i)\left(e^{\frac{\gamma_i}{2}(r_{it}-r_i)^2}\right)^{1-\alpha_i}\left(e^{-\beta_i r_i}\right)^{\alpha_i} = 0. \tag{4}$$

Solving for r_i yields the optimal tail toss reporting of an individual:

$$r_i^* = r_{it} + \frac{1}{\lambda_i} \tag{5}$$

with

$$\lambda_i = \frac{(1-\alpha_i)}{\alpha_i}\frac{\gamma_i}{\beta_i}, \tag{6}$$

which can be interpreted as an aggregated lying cost parameter (cf. Cohn et al., 2015). The number of reported tail tosses monotonically decreases in λ_i towards the actual number of tail tosses r_{it}. An array of factors may impact lying costs, including an individual's gender, religion, and moral framing (Abeler et al., 2019; Arbel et al., 2014; Bucciol and Piovesan, 2011; Rosenbaum et al., 2014; Utikal and Fischbacher, 2013).[7] Our model captures some of these effects. In line with intuition, our theory predicts that lying costs increase with the coefficient γ_i of 'misreporting aversion' of the 'moral self', and decrease with the relative bargaining power of the 'selfish self' α_i and with the marginal utility β_i of income of the 'selfish self'. The relative bargaining power of the 'selfish self' is a parameter that is contingent on the particular decision situation. In the following we derive hypotheses on how the treatments affect the relative bargaining power and thus lying costs.

In addition to previously studied effects, we hypothesize that the salience of the regulator affects individual lying costs. Salience of the regulator, in this case the EU, may decrease (increase) the 'selfish self's' bargaining power α_i if the EU is well (ill) regarded. In our experiment we take advantage of the well-documented and wide-spread contempt among fishermen concerning stricter EU fishing regulation over the past decade.[8] That is, we unambiguously predict an increase in the 'selfish self's' bargaining power α_i if the salience of the regulator matters for truth-telling.[9]

In order to test our prediction, we sent out three versions of the instructions in a between-subjects design: (i) a baseline setting ('Baseline') in which only the logos of our two institutions present on the letterhead, (ii) a version where the EU flag is made salient in the letterhead of the instruction sheet ('EU_Flag'), and (iii) an additional treatment where the framing

[6] We assume that there always has to be an agreement, thus we consider the problem to minimize the deviation from some 'ideal' reference point, as opposed to the more often considered problem to maximize the improvement compared to some minimum utility levels of respective outside options.

[7] Lying costs may also be affected by identity priming (Cohn et al., 2014, 2015; Cohn and Maréchal, 2016). In our setting, fishermen were targeted in their identity as German fishermen. Thus, professional identity considerations may increase lying costs due to reputational concerns inflicted on the profession, reducing the level of reported tail tosses across all treatments.

[8] This is confirmed by fishermen's self-reported trust in the EU concerning fishery policy in our survey. First, trust in the EU, at 3.11, was substantially lower as compared to the German Federal Government, at 3.62, and the German Fishery Association, at 5.75 (paired t-tests: $p < 0.000$ and $p < 0.000$). Second, we find that trust in the EU among all fishermen, at 2.39, is substantially lower as compared to a student control group, at 5.05 (t-test: $p < 0.000$). For further visual anecdotal evidence, see Appendix B. This antipathy towards the EU is not unique for German fishermen and may even be stronger in other countries. Indeed, UK fishermen played a key role in the 'Brexit' campaign, and they overwhelmingly have a very negative view of the EU (McAngus, 2016).

[9] Note that our model describes the potential effects of our treatments on the intensive margin, not the extensive margin. We discuss how the treatments might impact the extensive margin below.

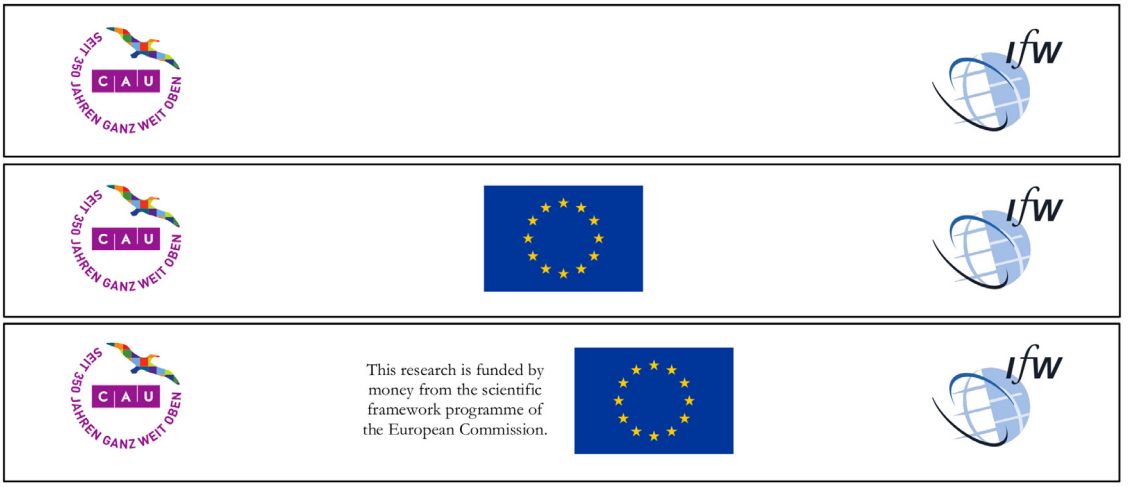

Fig. 2. Letterheads of the three treatments (from top to bottom: Baseline, EU_Flag and EU_Flag_Funding).

states that this research has been funded by the European Commission ('EU_Flag_Funding'). These framings were included on all three experimental sheets.[10] Fig. 2 depicts the three letterheads and Appendix A includes the experimental instructions.

Based on the insights from previous studies on lying behavior summarized in Abeler et al. (2019) and our treatments regarding the new regulatory dimension, we test three main hypotheses:

Hypothesis 1. *Fishermen report greater tail-tosses than the truthful distribution, but do not fully misreport in the Baseline treatment.*

The standard economic hypothesis of pure selfishness is that fishermen report their own payoff-maximizing option, i.e. every fisherman would report 4 times tails. This hypothesis has been called into question by recent empirical evidence on various lying costs (e.g. Fischbacher and Föllmi-Heusi, 2013; Gneezy et al., 2018; Abeler et al., 2019). We therefore expect that fishermen, on average, report coin toss results in between the expected outcome of 2 times tails if all fishermen reported truthfully and the payoff-maximizing outcome of 4 times tails. Explanations for not reporting four winning tail tosses may include individual lying costs and internalized reputational costs for the profession. It may also mirror fishermen's professional behavior of misreporting somewhat instead of lying to the full extent, for example declaring some part but not all of their bycatch.

Hypothesis 2. *Fishermen report less truthfully in the EU_Flag treatment compared to the Baseline treatment.*

As documented above, there is evidence for a widespread antipathy towards the EU among German fishermen, as most of new regulations by the EU have been regarded as burdensome for the fishermen. This makes the context of our study very useful to test Hypothesis 2, compared to cases in which the attitude towards the regulator is ambiguous. We therefore hypothesize that the presence of the EU flag will increase the bargaining power of the 'selfish self' relative to the 'moral self' thus decreasing lying costs and that fishermen in this treatment will thus report less truthfully out of ill-regard towards their regulator.

Fishermen may also perceive the difference in the Baseline and the EU_Flag treatment as a difference in wealth of the specific institutions and the research institutions being backed by the EU. This may affect truth-telling, as previous research has shown that costs to others matter for lying behavior (e.g. Gneezy, 2005). In an attempt to disentangle this effect from the direct effect of a particular attitude towards their regulator, we include the third EU_Flag_Funding treatment.

Hypothesis 3. *Fishermen report even less truthfully in EU_Flag_Funding compared to the EU_Flag treatment.*

We hypothesize that fishermen may regard the additional informational cue as an indication that there is plenty of funding available to those conducting the study. This may reduce the moral cost of lying, reducing the 'misreporting aversion' of the 'moral self', and lead fishermen to report less truthfully. Fishermen may also regard the provided information as an opportunity to acquire some of the EU's funds to compensate for the regulatory burdens imposed on them, thus giving more bargaining power to the 'selfish self', and leading fishermen to report less truthfully as well. We show below that the experiment rejects hypothesis 3. We come back to this issue and possible hind-sight explanations in the discussion in Section 5.

[10] Note that the EU funding information is true and is also mentioned in the acknowledgements. We also stated this EU funding on the invitation page to the mail survey in the two EU treatments.

Table 1
Descriptive statistics for the number of reported tail tosses in the fishermen experiment.

Treatment	*N*	Mean number of tails *R*	Relative frequencies		
			0/4 tails tosses	3/4 tails tosses	4/4 tails tosses
Fisher_All	120	2.46	0.03	0.43	0.11
Fisher_Baseline	42	2.38	0.02	0.45	0.05
Fisher_EU_Flag	36	2.64	0.00	0.39	0.17
Fisher_EU_Flag_Funding	42	2.38	0.05	0.43	0.12
Fisher_Coin_Back	22	2.23	0.00	0.41	0.00
Fisher_Coin_Kept	98	2.51	0.03	0.43	0.13

To examine truth-telling of fishermen towards their regulator, we targeted all commercial fishermen in Germany in a mail field experiment. Due to rigorous data protection by the German Federal Office for Agriculture and Food, the address data of fishermen were not available to us. For the purpose of our study, the Thünen Institute of Sea Fisheries, the national fishery research institute responsible for carrying out fishery surveys, sent out the study documents to all 896 fishermen on our behalf. We prepared the envelopes with the survey materials, including stamped return-envelopes, at the University of Kiel. We then delivered the envelopes to the Thünen Institute and were present when the address data was added. The envelopes were sent out on Friday, December 4, 2015, and the closing date for the experiment was January 31, 2016. We assigned anonymous ID numbers to 1200 prepared surveys, which were numbered according to their treatment cell. After having randomly shuffled all envelopes, 896 of these envelopes were sent out to fishermen by the Thünen Institute.[11]

The experiment material consisted of 7 pages, including a cover letter, three experimental tasks with one page each, a two-page questionnaire and a sheet for payment information. Appendix A contains an English translation of the material. Besides the coin-tossing task, it includes an experimental task to elicit fishermen's risk preferences, and an experimental task on competitiveness.[12] Fishermen were told that the payment for participating in the study was limited to 100 €, with an expected payoff of 50 € for around 30 min of work. Payment was made via bank transfer or by check via regular mail. To ensure availability of a coin to toss, we enclosed a 1 € coin that we stuck on the page of the task (see Appendix B).

We complement our main study with three additional data collection exercises to examine its relationship to the economic literature on lying and its robustness. First, regarding the relationship to the literature, we examine how our subject pool of interest—professional fishermen—relate to the conventional subject pool of university students. We therefore ran the Baseline treatment of our mail experiment also with 50 business and economics undergraduate students at the University of Kiel at the same time, 44 of whom participated.[13]

Second, to examine the robustness of our results regarding possible attrition issues we ran an additional online experiment with 717 student subjects from the University of Kiel subsequent to the main study including two treatments: positive and negative framing regarding the fishery policy of the European Union and its effect on trust towards the EU. The reason is that mail field experiments may suffer from substantial attrition and we are not able to rule out by design that attrition in our fishermen study may depend on the treatment. The additional online experiment allows us to investigate the potential importance of attrition for the treatments in our main study and offers secondary causal insights into the effect of positive and negative framing of the policy on trust in the European Union.

Third, to conceptually replicate and further investigate our main hypotheses and potential confounds, we conducted another online experiment with 1200 Brexit voters. We describe the details of this additional experiment in Section 4.

3. Results

We received 136 responses by fishermen, amounting to an overall response rate of 15%.[14] Of those, 120 responses included results for the coin-tossing task (see Table 1 for descriptive statistics),[15] which were provided by fishermen located in 58 different ports.[16]

[11] Additionally, fishermen could contact us directly by responding to advertisements in the journal of the German Fishery Association. If a fisherman contacted us, we cast a 6-sided die to determine which of the three treatments he would receive. Casting numbers 1 and 4 (2 and 5) [3 and 6] resulted in the Baseline (EU_Flag) [EU_Flag_Funding] treatment. We also randomly distributed envelopes to 34 junior fishermen. Five junior fishermen and three fishermen that contacted us directly participated in the study.

[12] We do not find any significant correlations of truth-telling and risk or competitive choices and therefore do not discuss these other tasks in more detail here.

[13] One of the authors distributed 50 envelopes to students in the lecture "Cost- and Performance Accounting" on December 4, 2015, and the closing date for the survey was also January 31, 2016.

[14] The response rate is comparable to Fischbacher et al. (2015), greater than in typical charity solicitation mail experiments (Gneezy et al., 2014; List and Lucking-Reiley, 2002) and, depending on the reward, similar to mail experiments with 'hot list' store customers (Gneezy and Rey-Biel, 2014).

[15] We follow standard procedures to test for response-bias and find no indication that observable characteristics or time of response drive the reporting behavior of fishermen (see Appendix D).

[16] Fishermen had several weeks to complete the study and while fishermen are spread out considerably across the German coast, as Fig. 1 shows, and there was no major fishery meeting during this time, it is possible that they discussed the study among themselves. We test for how this possibility might

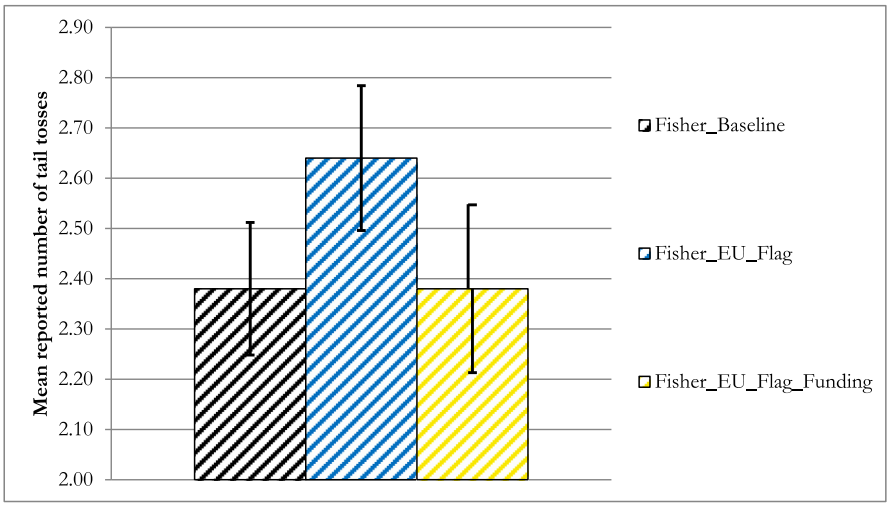

Fig. 3. Mean reported number of tail tosses per treatment, ±1 SE.

Aggregating all of our three treatments, we find that overall reporting by fishermen differs significantly from the truthful distribution as well as from payoff-maximization: fishermen report to have tossed 2.46 winning tails on average. This indicates substantial lying costs in line with the previous literature. A fraction of 10.83% of fishermen report that they have obtained four times tails, and 42.50% report three times tails. The distribution of reported outcomes is statistically highly distinguishable from both the payoff-maximizing outcome as well as from the truthful distribution. Two-sided binomial tests of the expected truthful against the observed frequency for 3 tails and for the payoff maximizing decision of 4 reported tails yield $p = 0.000$ and $p = 0.055$, respectively. In particular, we find reporting of 3 tail tosses at the expense of reporting 0 or 1 coin toss. The latter differs from the truthful distribution significantly (two-sided chi-squared test on combined 0 and 1 reports: $p = 0.000$). We therefore confirm Hypothesis 1 and previous findings in the literature. Next, we analyze the effects of our treatments on truth-telling.[17]

Fig. 3 shows the mean reported tail tosses across the three treatments. In the Baseline treatment, as well as in the EU_Flag_Funding, fishermen report an average coin toss result of 2.38 winning tails. In the EU_Flag treatment the average coin toss result was 2.64 tails. While this qualitatively suggests that fishermen over-report tail tosses in the EU_Flag treatment as compared to the Baseline, this is not statistically significant (two-sided t-test: $p = 0.191$). There is virtually no difference in mean reported tail tosses between Baseline and EU_Flag_Funding. Next, we examine tail toss response across treatments more closely.

Fig. 4 shows the theoretical binomial distribution for four tosses of a fair coin (blue dots connected by the dashed line), which is the distribution that we would expect if all fishermen truthfully report the outcome of their four coin tosses. The probability that four times tossing a coin results in $r_{it} = 0$ or 4 (1 or 3) [2] times tails is 6.25% (25%) [37.5%]. We refer to this distribution as the "truthful distribution", where the mean truthful response is $R_t = \frac{1}{N}\sum_{i=1}^{N} r_{it} = 2$ tail tosses, for $N \to \infty$. The payoff-maximizing choice would be the reporting of $r_p = 4$ times tails, with its mean denoted by R_p. Standard economic theory in the absence of lying costs predicts a distribution with 100% of reported coin tosses being tails. The colored bars in Fig. 4 show actual reporting behavior of fishermen. Fishermen's aggregate actual mean response is given by R.

impact reports in two ways: First, we compare fishermen that are members of a fishery association with those that are not. We find no indication for imbalance across treatments (two-sided chi-squared test: $p = 0.529$) and being a member has no significant effect on coin toss reporting (two-sided t-test: $p = 0.722$). Second, we compare fishermen from ports in which they are the only fisherman that participated with fishermen from ports with more than one respondent. Again, we find no indication for imbalance across treatments (two-sided chi-squared test: $p = 0.190$) and coin toss reports do not differ significantly across these two groups (two-sided t-test: $p = 0.324$). Thus, while we cannot exclude the possibility of treatment contamination, we find no indication of it.

[17] We find that response rates are roughly equally distributed across treatments, with response numbers of 45 (43) [48] in the Baseline (EU_Flag) [EU_Flag_Fund] treatment. Non-response concerning coin toss reporting is somewhat higher in the two EU treatments, with 7% (16%) [13%] in Baseline (EU_Flag) [EU_Flag_Fund]. For questionnaire responses that are significantly correlated with truth-telling, we have no indication of bias across treatments for those 16 fishermen that did not report coin tosses. Concerning the two major covariates of lying (year of birth and how often a fishermen has moved) go in opposite directions for the EU_Flag treatment. While fishermen in the EU_Flag treatment that did not report their coin-toss have only moved once in their lifetime on average, as compared to 3.5 [3.3] in the Baseline [EU_Flag_Fund] treatment, their mean birth year is 1952, as compared to 1960 [1957] in the Baseline [EU_Flag_Fund] treatment. Overall, we find that eight covariates are correlated with lying behavior in univariate regressions, but none remains significant in a multivariate regression of lying with these five controls. Covariates are balanced across all treatments, but in pairwise treatment comparisons we find that the five fishermen who receive a base salary are only represented in the EU_Flag and EU_Flag_Funding treatments (cf. Table C.2). Excluding these five observations reduces the p-value for the comparison of EU_Flag versus Baseline for 4 tail tosses to $p = 0.125$ but keeps all other findings qualitatively unchanged. We include salary along with the other significant co-variates in a regression analysis below.

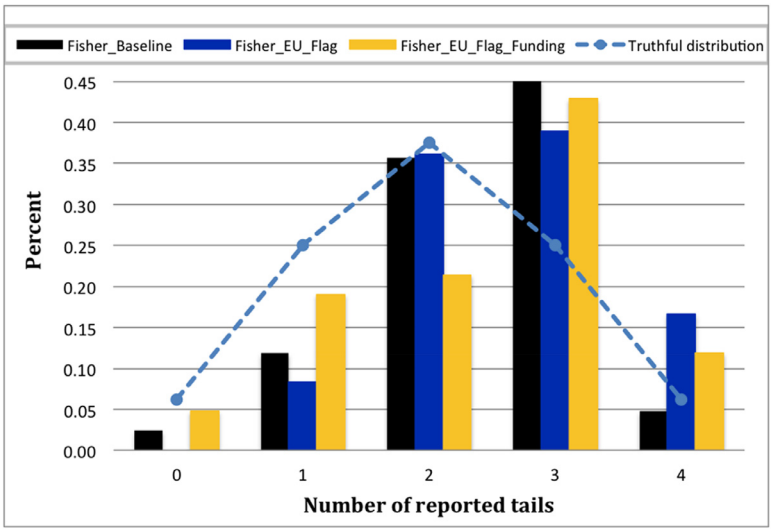

Fig. 4. Tail toss reporting behavior of fishermen in the Baseline (black bars, left), the EU_Flag (blue bars, middle) and the EU_Flag_Funding treatments (yellow bars, right). The (blue) dots connected by the dashed line represent the expected distribution if all report coin toss outcomes truthfully ('truthful distribution'). (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

Table 2
Regression analysis: Tail toss reporting, EU treatments and controls.

Independent variables	Dependent variable: tail toss reports		
	(1) model with univariately significant covariates	(2) model (1) plus key demographics	(3) model (2) without 'income increase' variable
EU_Flag	1.696** (0.738)	2.290*** (0.817)	0.822* (0.459)
EU_Flag_Funding	0.169 (0.695)	0.442 (0.726)	0.124 (0.452)
Year of birth	0.024 (0.029)	0.033 (0.031)	0.020 (0.018)
Number of times fishermen moved	−0.135 (0.104)	−0.122 (0.109)	−0.099 (0.075)
Do they receive a salary	1.696 (1.787)	1.629 (1.843)	1.117 (1.060)
Probability of income increase	0.012 (0.012)	0.013 (0.013)	
Planned years in fishery	0.005 (0.015)	0.005 (0.016)	0.013 (0.013)
Fishing alone	0.300 (0.567)	0.276 (0.625)	0.062 (0.439)
Trust-worthiness of European Commission	0.016 (0.216)	−0.139 (0.224)	0.069 (0.155)
Trust-worthiness of German government	0.161 (0.236)	0.341 (0.246)	0.001 (0.154)
Education level		−0.173 (0.213)	−0.003 (0.101)
Income relative to other fishermen		−0.093 (0.170)	0.008 (0.104)
Observations	54	52	106

Note: The table presents coefficients from ordered Logit regressions, with standard errors reported in parentheses. The observations from the Baseline treatment are the baseline of the estimations. Statistical significance: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

As Fig. 4 shows, no fisherman in the EU_Flag treatment reported 0 tail tosses, fewer fishermen reported 1 tail tosses compared to the Baseline treatment (8.33% vs. 11.90%) and more fishermen reported 4 tail tosses (16.67% vs. 4.76%). We find that fishermen in EU_Flag over-report tail tosses that yield the highest payoff (two-sided chi-squared test: $p=0.084$). This finding provides some confirmation for Hypothesis 2: The salience of the regulator does seem to play a role for truth-telling and the wide-spread ill-regard for the EU seems to translate into stronger over-reporting of tail tosses.

We find no material and significant differences between the EU_Flag and the EU_Flag_Funding treatments in terms of 4 tails reporting (two-sided chi-squared test: $p=0.547$). However, we find that fishermen in the EU_Flag_Funding treatment report significantly more 0 and 1 tail tosses (combined: 23.81% in the EU_Flag_Funding vs. 8.33% in the EU_Flag treatment; two-sided chi-squared test: $p=0.067$). These findings reject Hypothesis 3. Specifically, we do not find support for the 'wealth-of-funding-institutions' or 'taking-back from the EU' hypotheses as fishermen in the EU_Flag_Funding do not report more 4 or combined 3 and 4 tail tosses. We discuss this finding in Section 5.

To further scrutinize hypotheses 2 and 3, we run a regression analysis of overall tail toss reporting with the two treatments and additional controls (see Table 2). Specification (1) includes all eight covariates that are significantly correlated with tail toss reporting in univariate regressions. Specification (2) also includes the standard demographic variables income and education level as additional controls. Since many fishermen did not respond the question on the probability of their income increasing over the next five years, which causes the low number of observations in specifications (1) and (2), we run an additional regression specification (3) dropping this variable.

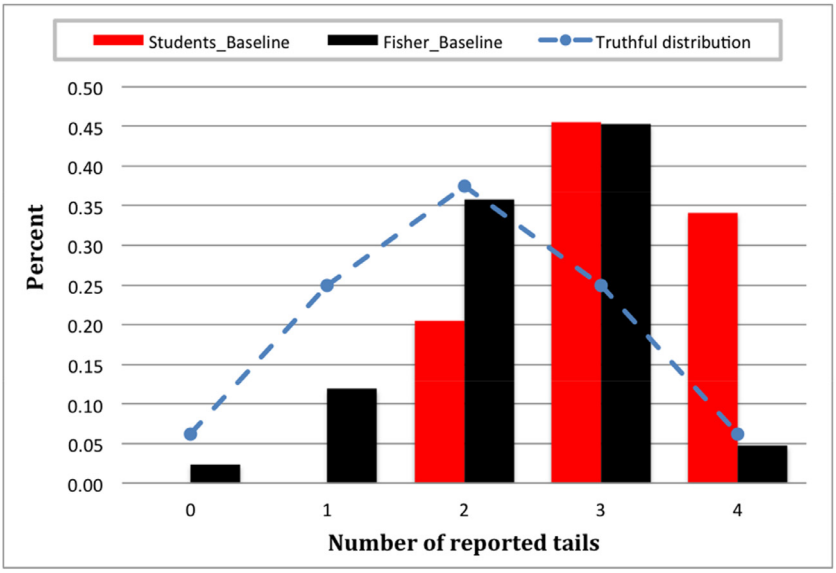

Fig. 5. Aggregate reporting behavior of fishermen in the 4-coin-toss task (dark black bars) versus the student sample (lighter red bars), both in the Baseline version. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

The regression analysis shows that across the three specifications EU_Flag_Funding changes sign. Indeed, we find that it is very far from significantly explaning different overall tail toss reporting compared to the baseline, with $p = 0.808$, $p = 0.543$ and $p = 0.784$, respectively. We find that the only consistently significant explanatory of variable of tail toss reporting is the EU_Flag, with a p-value in specification (1) of $p = 0.022$, in specification (2) of $p = 0.005$, and in specification (3) of $p = 0.074$. This provides further support for our Hypothesis 2. We summarize

Result 1. *Fishermen over-report more severely when facing the EU flag.*

Our study also included two novel 'hidden' tasks that offer the possibility to underscore truth-telling or lying behavior. First, we deliberately left the ownership about the one Euro coin that we included on the coin tossing decision page unclear. A related aspect of fishermen's fidelity is thus whether they sent back the coin with their decision sheets. We find that the 22 fishermen who sent back the coin report a coin toss result of 2.23 tails on average, compared to 2.51 tails for those who did not send back the coin (see Fig. C.1 in Appendix C). This difference is not significant (two-sided t-test: $p = 0.205$), yet tentatively suggests consistent behavior between the coin-tossing task and this hidden measure and therefore some external validity. Second, we conducted a separate task to measure fishermen's competitiveness using a real production task where fishermen have to produce paper shreds by hand from an A7-sized (74×105 mm) piece of paper. Fishermen decided on whether they want to be paid 0.05 € per piece, or whether they want to play competitively and receive 0.15 € per piece if they perform better than a randomly drawn other participant. As the A7-sized paper we sent the fishermen was of standard white format, dishonest fishermen could add additional alien paper shreds to increase their payoffs. To control for this possibility to cheat, we measured the weight of the returned paper shreds on an analytical scale from the physical chemistry lab. We find that the 10 heaviest envelopes with paper shreds, i.e. those where paper shreds have been added most likely to unduly increase payoff, report a mean coin toss result of 3.00 tails, compared to 2.41 tails for the rest (t-test: $p = 0.062$).[18] We summarize these two indicative findings as:

Result 2. *(Mis-)Reporting in the coin-tossing task is consistent with behavior in hidden truth-telling tasks.*

These findings are in line with growing and distinct evidence on the external validity of experimental lab measures of truth-telling in the literature (Cohn and Maréchal, 2018; Cohn et al., 2015; Dai et al., 2018; Gächter and Schulz, 2016; Potters and Stoop, 2016). Our indicative finding, together with the mounting evidence in the literature, therefore suggests that the coin-toss truth-telling measure is informative of fishermen's honesty behavior in the field.

Next, we compare fishermen in the Baseline treatment with our student sample that faced the exactly same study design as the fishermen (see Fig. 5). As outlined in the previous section, we collected this data to see how our fishermen subject

[18] We find a similar pattern for the students: The eight students sending back the 1 € coin report 3.00 tails on average, compared to 3.17 tails for those who did not send it back (two-sided t-test: $p = 0.566$); The four students with the heaviest envelopes report 3.75 tails as compared to 3.08 tails for the rest (two-sided t-test: $p = 0.079$).

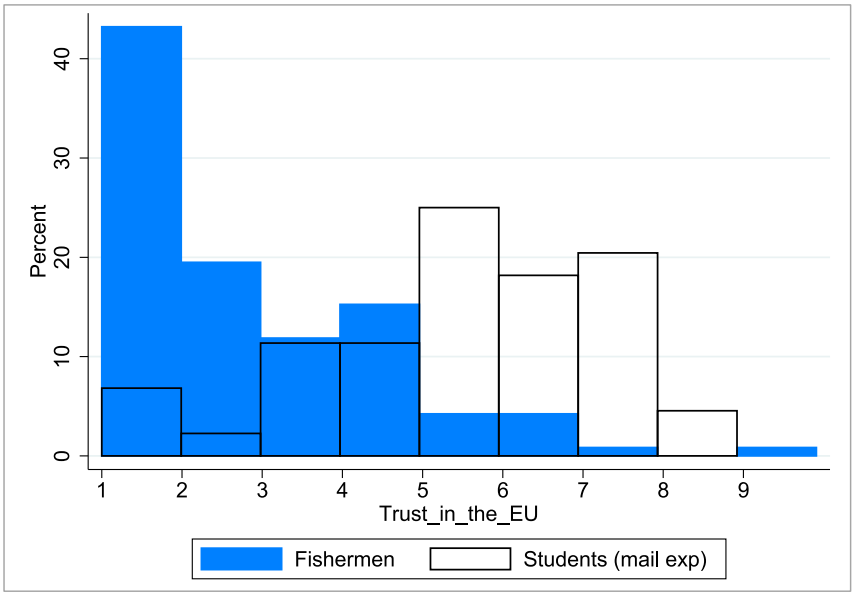

Fig. 6. Distributions of trust in the European Union, by subject pool.

pool relates to the conventional student subject pool. While fishermen reported to have tossed 2.38 tails on average, students report 3.14 tails on average, which is significantly higher (two-sided t- test: $p = 0.000$).[19]

This result points to the importance of using our subject pool of fishermen to answer our research questions regarding fishery policy. Inspired by the quote of McAngus (2016) at the beginning of our paper, we were also interested to see how the reported trust in the European Union (specifically the European Commission), measured on a Likert scale from 1 to 9, compares between our fishermen and students. Our findings reflect the spirit of McAngus' quote: the average trust in the Baseline treatment of the fishermen in the EU is 2.29 compared to 5.05 of the students. We find no significant differences in the reported trust levels among fishermen across the three treatments (pairwise t-test: $p > 0.60$ for all cases). Fig. 6 depicts the distribution of trust towards the EU for all fishermen versus the students. We see that almost half of the fishermen report a trust level of '1', the lowest possible answer on the scale. The two distributions are highly different from each other (two-sided Kolmogorov–Smirnov: $p = 0.000$).

From the distributions in Fig. 6, it is evident that there is heterogeneity of students' trust in the EU (while the fishermen's trust is skewed). We took advantage of this observation to run an additional online experiment with students from the same university (as outlined in Section 2). This online experiment serves the purpose to investigate the impact of trust in the EU on attrition. As noted in the experimental design, mail field experiments may suffer from attrition. As we cannot rule out effects of our treatments on the extensive margin by design, i.e. that participants selected on responding not independently from the random treatment assignment, an alternative explanation for our findings would be that there is a fixed proportion of honest and dishonest fishermen and that the honest participants were less likely to send in the study when being confronted with the EU flag.

To study whether the EU framing may induce attrition, we collected additional data from 717 student subjects in the online experiment. We randomly assigned the subjects into two treatments: positive EU framing and negative EU framing. In both treatments we provided subjects with true information regarding the EU fishing policy. The difference was that in the positive EU framing treatment the regulatory efforts of the EU fishery policy were framed as a success and in the negative EU framing treatment the regulatory efforts of the EU fishery policy were framed as a failure.[20] To reinforce the strength of the framing, we subsequently asked subjects to report positive (negative) own experiences with EU regulations. Thereafter we asked subjects to report their trust (a) in the European Commission, (b) the federal German government and (c) the government of their German state.[21]

[19] This level of cheating by students is close to what has been found in the meta-study by Abeler et al. (2019).

[20] The positive framing states: "In recent years, EU fisheries management has become increasingly successful: the share of sustainably fished stocks has been significantly improved." The negative framing states: "The EU's fisheries management is still largely considered a failure: around half of the stocks are considered overfished."

[21] All subjects came from the same state (German: 'Bundesland').

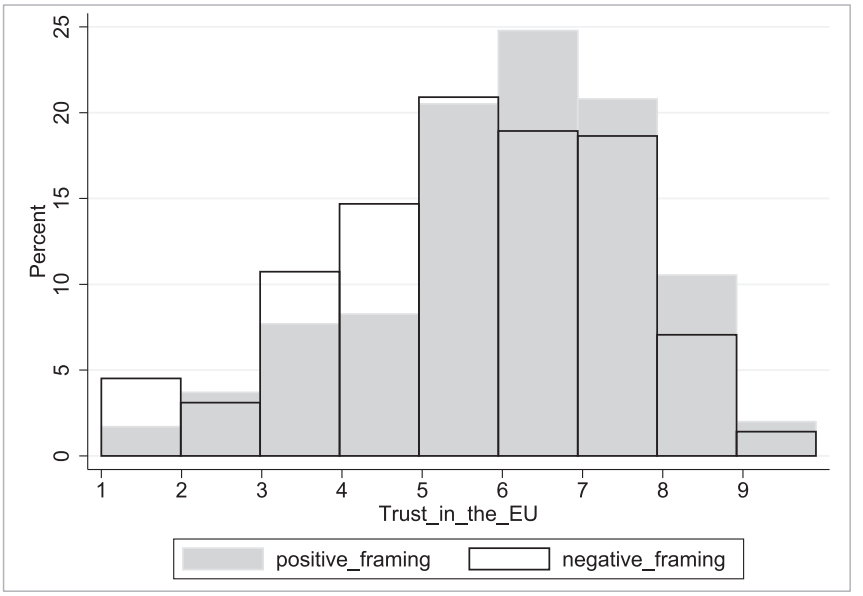

Fig. 7. Distributions of trust in the EU by framing treatment.

This exercise yields two key insights. First, we find that subjects in the positive EU framing treatment report to trust the EU significantly more than subjects in the negative EU framing treatment (two-sided Kolmogorov-Smirnov test: $p = 0.017$).[22] Fig. 7 depicts these results. Thus, the framing seems to have worked and we can examine how it may impact attrition. Second, we found overall attrition of only 1.25% (from 717 to 708 participants). Crucially, there is no significant difference between the attrition in the two treatments (two-sided chi-squared test: $p > 0.1$). While this finding provides suggestive evidence that attrition does not depend on the sort of framing (positive or negative) regarding the EU, it does not preclude the possibility that attrition in our main experiment with fishermen has been treatment-specific.

4. Conceptual replication with Brexiteers

To study the robustness of our results and possible mechanisms for regulator framing, we conducted a second online experiment. As it is difficult to get access to addresses of fishermen for studies and as we had already contacted all commercial fishermen in Germany for our main experiment described above, the aim was to find a group of individuals who, like fishermen in Germany, are known for not being fond of the EU as a regulator. We identified Brexiteers as such a candidate and accessible population, who may not have the same close regulatory experience with the EU as fishermen but are certainly know for not being fond of the EU. The platform Prolific (www.prolific.ac) offers the opportunity to run online studies with a fairly large and diverse UK population (N>20,000) and the platform pre-collected information on subjects' self-reported Brexit votes ('leave', 'remain' or no vote). This gave us the opportunity to run a conceptual replication and extension of our initial study with up to 4064 potential subjects who had previously stated that they voted 'leave' in the Brexit referendum. The subsections below describe the experimental design and the results of our conceptual replication.

4.1. Design

In our conceptual replication with Brexit voters we used the same 4-coin-tossing task of Abeler et al. (2014) that we also used in our main experiment with fishermen. For each instance they reported that the winning toss "tails" laid on top, they received 1 GBP. Thus, each subject received between 0 and 4 GBP for this task, in addition to a 2 GBP participation fee.

Following a few introductory questions, we used similar headers for the screen of the coin tossing task on the website in the new Brexiteers_Baseline and Brexiteers_EU_Flag treatments as in the Fisher_Baseline and Fisher_EU_Flag treatments with the fishermen, except that the headers were slightly larger in relation to increase the visibility on the computer screen. Based on inconclusive evidence regarding the Fisher_EU_Flag_Funding treatment in our main study, we included a third Brexiteers_Funding treatment, in which we displayed a text informing the participants that the study is funded by public research funds – yet the Brexiteers_Funding treatment does neither include any mentioning of the EU nor any EU flag. The

[22] Trust levels for the federal and state governments do not differ across the two treatments (two-sided Kolmogorov–Smirnov tests: $p > 0.1$ for both tests).

Brexiteers_Funding treatment aims at disentangling the effects of providing information on research funding from the EU flag effect. Figure E.1 in Appendix E displays the three screen headers of the coin tossing task.

We included further follow-up questions to investigate potential mechanisms that might drive treatment effects. First, our replication includes three variables on negative reciprocity adapted from Falk et al. (2018). We included these variables to reveal possible mechanisms for why individuals who dislike or mistrust the EU are reporting higher tail tosses in the EU_Flag treatment (and are therefore more likely to lie in this treatment). Second, as a potential confound that might drive treatment differences we investigate experimenter demand effects (Zizzo, 2010; De Quidt et al., 2018). To this end, we included the following question in as a follow-up: "How strongly do you feel that the researchers of this study wanted you to report in a particular way in the coin task?" and elicited responses on a 9-point Likert scale from 1 (not at all) to 9 (very strongly). Finally, even though Prolific allowed us to pre-screen for Brexit voters, we asked participants of our replication study whether they voted 'leave' in the referendum and whether they would still vote for some form of 'leave' (Hard Brexit or PM Theresa May's Leave Deal).

We based the number of observations (N) in this conceptual replication with Brexit voters on a power calculation that is informed by the effect size of the EU_Flag versus Baseline treatment on coin toss results in our main experiment with fishermen. The power calculation informed us that it would be necessary to collect 173 observations per treatment in order to replicate the fishermen's effect.[23] With three treatments, this would yield $N = 519$. Due to the differences between our fishermen experiment and this replication, we decided to increase N by spending up to our budget constraint of 8000 Euro, and to pre-specify hypotheses and use one-sided tests for those pre-specified hypotheses (we use two-sided test for analyses that we did not pre-specify). In particular, we expected that participants in such an online experiment participate in studies more often and may take our study less seriously than the commercial fishermen. For this reason, there are likely attention differences between our print-out mail experiment, for which fishermen had a long time to make decisions, and the online screen experiment for which participation times are usually very short and inattention may be an issue. Thus, in our pre-analysis plan we set the number of observations to be collected to $N = 1200$ complete responses and set this total number on the Prolific platform. The data collection started on March 6, 2019. Subjects' average completion time was 3.7 min (222 s) and they received an average payment of 4.32 GBP, amounting to an hourly-equivalent payment of 70.14 GBP. For the collection of our data, we employed the platform Social Science Survey (www.soscisurvey.de).

4.2. Results

Randomization, which was computerized through the Social Science Survey platform, yielded almost exactly 400 observations for each of our three treatments (399, 401 and 400, respectively). Kruskal–Wallis tests and chi-squared tests over all three treatments do not report significant differences for any of the collected variables.[24] This evidence suggests that the randomization process worked well.

We do not find significant differences between our three treatments for the reported number of tail tosses using all data (see Fig. 8). The average reported number of tail tosses is higher in Brexiteers_EU_Flag than in Brexiteers_Baseline, and is therefore qualitatively as expected and formulated in our pre-analysis plan. Yet, this difference is not significant ($p = 0.267$, one-sided t-test, as formulated in our pre-analysis plan). As stated in our pre-analysis plan, we anticipated that the attention and involvement of participants of our online experiment may be heterogeneous and that completion time can be a proxy for (in)attention or fast-clicking. As attention and involvement may be crucial for regulator framing to work, we stated in our pre-analysis plan that we will analyze the data for a sub-sample that suggests a relative high level of attention and involvement. We expected that the sample of Prolific may contain participants who just fill out items and finish studies as fast as they can. Likewise, there may be participants who leave the screen to do other things for some time. These participants may not have paid the required attention to the screens and added noise to our dataset. For this reason, we proceed to analyze the treatment effects for a sample that excludes the 10% fastest and the 10% slowest participants, which we denote as 'inattention truncation'.[25] Fig. 8 depicts the mean reported tail tosses between treatments both for the full sample and the inattention truncation sample.

In the next step, we conduct the (one-sided) t-tests for effects across treatments. We find marginally significant evidence for a higher mean number of reported tail tosses in Brexiteers_EU_Flag compared to Brexiteers_Baseline ($p = 0.093$). We therefore find suggestive evidence that points into the same direction and is consistent with Result 1 in our main experiment with fishermen.

Our hypothesis in the pre-analysis plan regarding the difference in the mean number of reported tail tosses between Brexiteers_Funding and Brexiteers_Baseline is that there is a lower mean number in Brexiteers_Funding. The one-sided t-test clearly rejects our hypothesis ($p = 0.959$). Rather, there is evidence pointing into the opposite direction ($p = 0.083$, for a two-sided t-test). As such, this does not support the tentative conclusion from the main fishery experiment that the funding

[23] For the power calculation we used the 'power' command for two-sample means with an alpha of 0.05 and a beta of 0.8 in Stata 15.1. The used input was m1=2.38, m2=2.64, sd1=0.85 and sd2=0.87. Of course, the mail experiment with fishermen in Germany and the online experiment with Brexit voters in the United Kingdom differ in many dimensions – yet our fishermen experiment was the best guess we could base our power calculation on.

[24] Table E.1 in Appendix E reports descriptive statistics of our experiment with Brexiteers.

[25] The 10% cut-off levels correspond to excluding participants with response times of less than 2.3 min (137 s) and more than 5.3 min (318 s). This gets rid of the very fast clickers and also those who spent a considerably longer time in responding, which may be due to inattention.

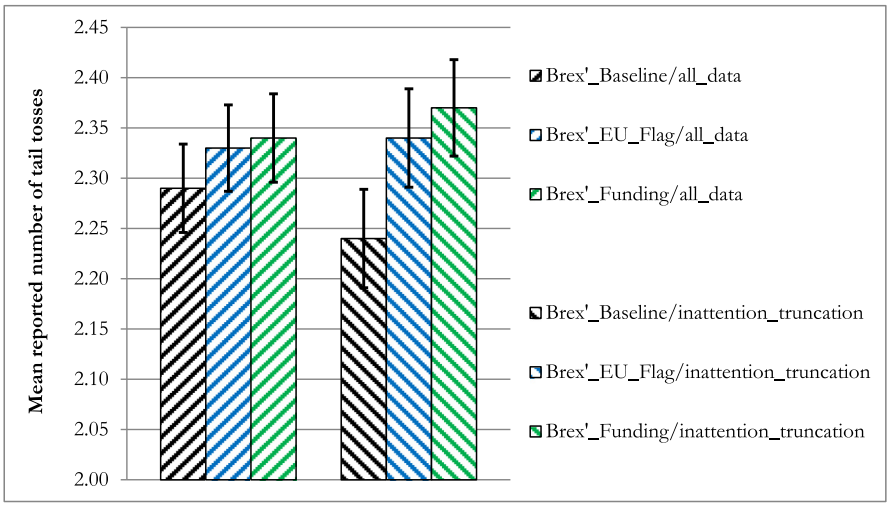

Fig. 8. Mean reported number of tail tosses per treatment, ±1 SE, for all data and for the inattention truncation sample that does not include the 10% fastest and the 10% slowest participants.

Table 3
Regression analysis: tail toss reporting, treatments and controls for Brexiteers.

Independent variables	Dependent variable: tail toss reports	
	(i) model with univariately significant covariates	(ii) full model with all covariates
Brexiteers_EU_Flag	0.237 (0.146)	0.269* (0.147)
Brexiteers_Funding	0.245* (0.146)	0.290* (0.148)
Age (in years)	−0.013** (0.005)	−0.014*** (0.005)
Female		−0.073 (0.129)
Relative income (scale 1–9)	0.075** (0.037)	0.075** (0.037)
Number of accommodation moves	−0.009 (0.012)	−0.009 (0.012)
Trust in the EU (scale 1–9)		−0.039 (0.048)
Trust in the UK gov't (scale 1–9)		−0.031 (0.047)
Trust in the local gov't (scale 1–9)		0.027 (0.046)
Trust in the Royal Academy (scale 1–9)		−0.004 (0.043)
Perceived experimenter demand (scale 1–9)		−0.024 (0.025)
Seconds to fetch the coin		0.000 (0.002)
Willingness to take revenge (scale 1–9)		−0.023 (0.049)
Willingness to punish for me (scale 1–9)		0.035 (0.052)
Willingness to punish for others (scale 1–9)		−0.057 (0.039)
Voted for 'leave'		0.176 (0.229)
Hard Brexit supporter		0.136 (0.131)
Observations	963	962

Note: The table presents coefficients from ordered Logit regressions for the inattention truncation sample, with standard errors reported in parentheses. The observations from the Brexiteers_Baseline treatment are the baseline of the estimations. Statistical significance: *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

information might curb over-reporting. A third hypothesis of ours in the pre-analysis plan was that the mean number of reported tail tosses is higher in Brexiteers_EU_Flag than in Brexiteers_Funding. A *t*-test rejects this hypothesis ($p=0.656$).

To examine the robustness of these results and to examine the potential mechanisms discussed above (trust in the EU, experimenter demand effects and negative reciprocity), we run a regression analysis of overall tail toss reporting including control variables. Table 3 reports the estimation coefficients. Specification (i) includes the treatment dummies and the three covariates ('age', 'relative income' and 'number of moves') that are pairwise significantly correlated with tail toss reporting over all three treatments. Specification (ii), the full model, also includes all other variables collected from the participants. The estimations show that the effect size in the multivariate regressions for the EU_Flag treatment is much smaller in the Brexiteers sample as compared to the fishermen. For the full model – including all variables collected from participants in our online experiment – we find additional marginally significant evidence pointing again into the direction that the average number of reported tail tosses is higher in Brexiteers_EU_Flag than in Brexiteers_Baseline and also higher in Brexiteers_Funding than in Brexiteers_Baseline.[26]

[26] Fig. E.2 in Appendix E provides a histogram of the distribution of reported tail tosses in the three Brexiteers treatments. In line with our pre-analysis plan, we tested differences between the treatments using chi-squared tests. These tests reject the hypotheses for differences in tail toss reporting.

Overall, our replication using an online experiment with Brexiteers provides some supportive evidence for Result 1 from our mail experiment with fishermen. Our replication study did, however, not provide the expected explanation for the treatment effect of Brexiteers_Funding compared to Brexiteers_Baseline. We come back to this result in Section 5.

The replication with Brexiteers also allows us to include additional questions before and after the coin tossing task and to examine possible mechanisms explaining mean coin toss reporting in our three treatments. Before the Brexiteers encountered the coin-tossing task, in all treatments they were asked for their trust in the EU. After the coin tossing task, they were further asked to state to what extent they felt they were pushed to report the coin tossing in a certain manner (to study experimenter demand effects) and, also after the coin tossing task, the Brexiteers were asked to answer questions on negative reciprocity (inspired by Falk et al., 2018). For the purpose of examining possible mechanism of our treatment effects, we dissect the mean number of tail tosses by reporting pairwise correlation matrices for the relevant five variables by treatment (see Tables E.2a–c in Appendix E). For Brexiteers_Baseline we do not find any significant pairwise correlations of the five variables with the reported number of tail tosses (Table E.2a). In our Brexiteers_EU_Flag treatment we find that one variable correlates (marginally) positively with the number of reported tail tosses: the answer to "Willingness to punish someone who treats me unfairly". Hence, we get a weak hint that negative reciprocity might be a motivation for lying in Brexiteers_EU_Flag. Finally, in Brexiteers_Funding, we observe that all three negative reciprocity answers correlate (marginally) negatively with tail toss reporting. A preference for reciprocity in combination with a generally careful view on public funds might curb some lying. This exercise neither yields any correlation between the number of reported tail tosses and trust in the EU nor between tail tosses and perceived experimenter demand.

Finally, we briefly address attrition in our online experiment with Brexiteers. Randomization took place when entering the treatment page and prolific only counted participants that finished the study and came back to prolific afterwards. Of the 66 incomplete responses that are included in the dataset, 29 dropped out immediately, 31 looked at the introduction page but did not proceed to the next page with preliminary questions, which only six individuals entered and four of them completed. Two of the four individuals proceeded to the treatment page. They were allocated to the EU_Flag and Funding treatments, respectively. These two individuals did not report a coin toss result and stopped the survey at this point. We therefore find that treatment induced attrition might be relevant for less than 0.2 percent of the sample, and based on the small sample we find no indication for it. This corroborates the findings from the additional student sample on the likely absence of treatment-specific attrition in the setting of online experiments. However, this does not imply that treatment-specific attrition is no concern in the main fishermen mail field experiment, as both the populations and response times differ substantially. Indeed, we cannot rule out the possibility that some treatment differences can be driven by treatment-specific attrition. The potential magnitude of such attrition biases would have to be tested in future mail field experiments.

5. Discussion and conclusion

This paper presents field experimental evidence on truth-telling of German commercial fishermen who are regulated by the European Union (EU). To our knowledge, this is the first artefactual field experiment with professional common-pool resource users on truth-telling.[27] Examining truth-telling of German fishermen is of direct relevance, as the member states of the European Union stand to decide on how much costs to incur to monitor a recently enacted ban on discarding unwanted fish catches to the sea. The regulator thus currently depends on fishermen's honesty, while standard economic theory predicts substantial lying behavior. This paper not only studies fishermen's overall degree of dishonesty but extends the scope of previous studies by asking how regulator framing affects truth-telling—a dimension that is relevant for the effective and efficient design monitoring and sanctioning mechanisms. Our results are therefore not only relevant for the specific fishery context, but crucial for a broader understanding of truth-telling, the management of common pool resources around the world, and for regulatory policy more generally. To examine the more general applicability of our findings, we also conducted a conceptual replication and extension of our initial study with 1200 UK citizens who voted 'leave' in the Brexit referendum.

Adapting an established coin-tossing game (Abeler et al., 2014), where subjects have to toss a coin 4 times and receive 5 € for each of the 0 to 4 reported tail tosses, we test whether truth-telling in a baseline setting differs from behavior in two treatments with different EU framings. The fishery is an ideal test case for studying how truth-telling behavior may be affected by regulatory framing, as there is almost uniform contempt among fishermen concerning stricter EU fishing regulation. We therefore hypothesized that if regulatory framing affects truth-telling, it would lower lying costs and thus result in higher misreporting among the treated fishermen.

We find overall that fishermen misreport coin tosses to their advantage, albeit to a significantly lesser extent than standard theory would predict. Specifically, we find an average reported tail toss result of 2.46, while the expected truthful distribution would result in 2 and the payoff-maximizing choice in 4 reported tail tosses. Fishermen thus do not lie to their maximum advantage, but partial misreporting is prevalent among fishermen, in line with recent evidence by Abeler et al. (2019) and Gneezy et al. (2018). Qualitatively, we find the same to be true for Brexiteers.

[27] Previous studies examining social behavior among common pool resources users have either reported cooperativeness in standard public goods games, common pool resources or ultimatum games (e.g. Gneezy et al., 2015; Jang and Lynham, 2015; Velez et al., 2009) or more severe forms of anti-social behavior (e.g. Prediger et al., 2014). Akpalu (2011) and Akpalu and Normanyo (2014) use surveys to investigate illegal fishing behavior.

Crucially, we find that misreporting is larger among fishermen who are faced with the EU flag compared to the control sample without the EU flag on the instruction sheet. Furthermore, the replication with Brexiteers provides some supporting evidence for this main finding. This result confirms our hypothesis according to which many fishermen (and Brexiteers) have lower moral lying costs towards the EU, which they dislike. This indicates that previously elicited degrees of truth-telling may not be appropriate for principal-agent relationships, where the principal or regulator is ill-regarded by the economic agents.

Although we have found consistent evidence across our main experiment and the conceptual replication, our central result is only borderline significant. The main study with fishermen may be underpowered, and the replication study with Brexiteers may have suffered from lack of attention. Future research should therefore be devoted to confirm the generalizability of this result. In particular it would be important and interesting to test whether the results also extend to other settings.

The experiment with fishermen did not confirm our initial hypothesis 3 that fishermen report less truthfully in EU_Flag_Funding compared to the EU_Flag treatment. Our expectation was that fishermen may regard the additional informational cue as an indication that there is plenty of funding available to those conducting the study, and that this may reduce the moral cost of lying, such that fishermen would report less truthfully. This effect is not apparent in the experiment with fishermen. In the regression analysis, the EU_Flag_Funding treatment variable does not explain any difference in reported tail tosses compared to the baseline treatment.

The conceptual replication with Brexiteers includes a treatment where we show the information that this study is funded by public funds, but without the EU flag, i.e. omitting reference to the regulator. This treatment leads to increased misreporting in a similar way as the EU_Flag treatment. Indeed, it might be that the additional information box about funding may make the wealth of the funding institution more salient and taking money may appear permissible to the Brexiteers. This would be in line with our initial hypothesis 3 formulated for the experiment with fishermen.

However, the conceptual replication did not provide an explanation why fishermen were more honest in the EU_Flag_Funding treatment than in the EU_Flag treatment. One mechanism that may seem plausible is that fishermen may have considered the joint information on research funding and the EU flag as information that the EU is using money to survey fishermen. As this was a mail experiment, fishermen may even have taken the opportunity to discuss these issues with relatives or family members, which may have reinforced such a view. This may lead to more support for the regulator, rendering the responses statistically indistinguishable from the baseline treatment. However, this is just one possible explanation and there may be others. Our findings show that the ill-regard of the regulator is not the only effect present, and that other mechanisms may offset the dislike effect. It is an interesting question for future research if and how the regulator can approach regulated individuals in such a different kind of way to offset the negative attitude and thus increase truth-telling behavior.

Moreover, we find evidence suggesting some consistency of behavior between the coin-tossing task and two other measures of truth-telling or lying behavior. This finding is based on two hidden tasks in the experiment—leaving the ownership of a coin to flip ambiguous and using an additional task in which it was possible to provide more material than was supplied—that may be of use for experimental methodology beyond our specific context to investigate the external validity of standard lying tasks.

Overall, our findings imply that regulators not only have to consider some exogenous degree of dishonesty among the regulated, but also take into account that truth-telling may erode in reaction to the regulatory policy. Faced with a variable degree of dishonesty, the regulator can act strategically in adopting its regulatory approach, such as shifting part of the regulatory work to bodies that are closer to the regulated, thus considering how the regulated will adapt their behavior.

Whereas the substantial number of fishermen who likely report honestly might suggest that softer monitoring approaches could be sufficient, the strategic aspect of regulatory experience calls for a more deliberate approach. One possible solution to coping with this strategic dimension of dishonesty would be to choose the 'corner solution' and comprehensive control.[28] In practice, this would mean a monitoring scheme relying on-board observers or camera systems. However, instead of directly incurring the high costs to the regulator and fishermen of comprehensive control, our recommended approach would be to introduce monitoring of different degrees of stringency selectively to study the effects of monitoring on honesty. Overall, our findings imply that lying is more extensive towards an ill-regarded regulator and that policy needs to account for this endogenously eroding honesty base. Studying this new dimension of truth-telling in further detail is a promising avenue for future research.

Acknowledgments

We are grateful to two anonymous reviewers and an associate editor for very helpful comments that greatly improved this manuscript. Furthermore, we thank Peter Breckling, Alain Cohn, Uri Gneezy, John List, Michel Maréchal, Frikk Nesje, Linda Nøstbakken, Andries Richter, Arno Riedl, Jörn Schmidt, Benjamin Schmöde, Simeon Schudy, Claudia Schwirplies, Daan van Soest, Jan Stoop, Bertil Tungodden, and Rudi Voss as well as seminar audiences at CREED Amsterdam, BIOECON2016

[28] For determining optimal fishery regulation and enforcement, the regulator must also consider the cost of enforcement (Nøstbakken, 2008; Sutinen and Andersen, 1985).

Cambridge, EAERE2016 Zurich, EAERE-FEEM-VIU2016 Venice, Freiburg and ICES2016 Riga for helpful discussions. We thank the participating fishermen, Jörg Berkenhagen and Gerd Kraus for providing address data, Gernot Friedrichs for access to an analytical balance as well as Jan Deller, Theresa Dorn, Natalia Grinberg, Christina Martini, Clara Paczkowski, and Gui Pinto for research assistance. This work was supported by the German Ministry of Education and Research [grant 01UT1410] and the European Commission [grant 289257].

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.euroecorev.2019.103310.

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