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When Nudges Backfire: Evidence from a Randomized Field Experiment to Boost Biological Pest Control

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Abstract

Nudges are increasingly used to alter the behavior of economic agents as an alternative to monetary incentives. However, little is known as to whether nudges can backfire, that is, how and when they may generate effects opposite to those they intend to achieve. We provide the first field evidence of a nudge that is designed to encourage pro-environmental behavior, which instead backfires. We randomly allocate a social comparison nudge inviting winegrowers to adopt biological pest control as an alternative to chemical pesticide use. We find that our nudge decreases by half the adoption of biological pest control among the largest vineyards, where the bulk of adoption occurs. We show that this result can be rationalized in an economic model where winegrowers and winegrower-cooperative managers bargain over future rents generated by the adoption of biological pest control. This study highlights the importance of experimenting on a small scale with nudges aimed at encouraging adoption of virtuous behaviors in order to detect unexpected adverse effects, particularly in contexts where negotiations on the sharing of the costs of adoption are likely to occur.

Keywords: Nudges, Behavioral Economics, Pesticides, Government Policy.
JEL Classifications: D90, Q25, Q58.

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1 Introduction

Nudges aim at modifying agents' behavior by tweaking their context of choice without resorting to monetary incentives (Thaler and Sunstein, 2009). Nudges are increasingly used by policy-makers worldwide; for instance, nudge units are embedded within both the British and US governments. Social comparison nudges, where agents receive information about peer behavior, are extensively used to encourage environmental conservation (Schubert, 2017; Croson and Treich, 2014) and have been shown to decrease electricity use (Allcott and Rogers, 2014; Delmas et al., 2013) and water consumption (Ferraro et al., 2011; Chabe-Ferret et al., 2019). An important outstanding question about the effectiveness of nudges, however, is whether they can backfire and prompt recipients to adopt harmful behavior, and if so, in what context and why.

In this paper, we provide evidence of a social comparison nudge backfiring in a field experiment, along with a novel explanation for why it did. We conducted a randomized experiment with a French-winegrowers cooperative to test whether social comparison nudges can improve farmers' adoption of biological pest control as an alternative to chemical pesticide use. A randomly selected group of winegrowers received a flyer and text messages containing a social comparison message informing them of the widespread adoption of biological pest control in the neighboring cooperative. Winegrowers in the control group received the same mailing and text messages, but without the social comparison component. The alternative to pesticide use that our nudge seeks to promote is mating disruption, an efficient and expanding method of biological pest control used in European vineyards against grape berry moths (Hoffmann and Thiery, 2010; Delbac et al., 2013). Mating disruption uses pheromone dispensers to disrupt the mating of pests and is able to drastically reduce their offspring. Our nudge aimed at informing winegrowers that farmers similar to them had successfully adopted biological pest control on a large scale, hinting that expected costs were probably over-estimated and could be revised downwards.

We find that our nudge decreased the adoption of biological pest control by half among the largest farms (for whom adoption reached 40% in the control group). We show that this result can be rationalized in an economic model where cooperative managers and winegrowers bargain over future rents generated by the adoption of this new technology. In our model, the nudge signals to winegrowers that cooperative managers value the adoption of biological pest control more than the winegrowers initially believed. As a consequence, winegrowers withhold adoption in order to extract a larger cut of the gains.

Several pieces of information from the field tend to support this model. First, in interviews carried out with the winegrowers after the results of the experiment were known, two primary explanations were put forward. The first was based on a psychological reactance to social comparisons. The other, more in line with our theoretical model, suggests that the nudge backfired because it was interpreted as implying that the winegrowers would receive no direct reward from the cooperative for adopting biological pest control. Second, treated winegrowers ended up adopting the practice three years after the experiment at the same rate as winegrowers from the control group, in line with the idea that the nudge triggered a long-lasting bargaining procedure.

Our results thus bring a note of caution regarding the increasing number of nudges implemented within organizations or involving professional economic agents (Gosnell et al., 2016; Chabe-Ferret et al., 2019; Earnhart and Ferraro, 2021). Nudges may indeed backfire in economic contexts in which agents feel they can bargain to increase their share of the gains brought about by adopting the nudged behavior.

A number of empirical studies have already reported that nudges can backfire. In particular, social comparison nudges have been shown to decrease savings behavior (Beshears et al., 2015), organ donation (Behavioral Insights Team, 2013), and claims of social benefits (Bhargava and Manoli, 2015).[1] We add to this literature by providing evidence of a social comparison nudge backfiring in the context of environmental conservation, an important area of application of social comparison nudges. Conservation nudges that rely on social comparison have already been shown to have boomerang effects, where the most virtuous agents slack off when they receive information about the average behavior among their peers. Nevertheless, the magnitude of this type of boomerang effect, as reported in the literature, is generally not great enough to dominate conservation effects in the least virtuous agents (Schultz et al., 2007; Fischer, 2008; Chabe-Ferret et al., 2019; Ayres et al., 2013). The only exception is when the advertised behavior is so rare and extreme that the social comparison encourages most agents to slack off (Richter et al., 2018). In the present study, the social comparison nudge backfires despite most members of the comparison group having adopted the advertised technique.

Several mechanisms have been put forward to explain why nudges might backfire. Psychological phenomena such as reactance (Brehm, 1966; Clee and Wicklund, 1980; Osman, 2020), discouragement (Beshears et al., 2015), and seeing the behavior of the reference group as unattractive (Bhargava and Manoli, 2015) have been cited as possible explanations. More recently, Bolton et al. (2020) propose a model to explain why making actions observable might backfire, based on social image motives. We contribute to this ongoing investigation by studying the effect of sending a nudge when a principal and an agent rationally play a strategic bargaining game.

The rest of the paper is structured as follows. Section 2 presents the context of the experiment. Section 3 describes the experimental design. Section 4 presents the results. Section 5 introduces the theoretical model. Section 6 concludes.

2 Context of the experiment

In many French regions, pesticide use is a major source of water pollution. This problem is particularly acute in the wine-producing regions. In order to control the development of the grapevine moth (*Lobesia botrana*), whose larvae inflict great losses in grape production and quality, French winegrowers make use of a toxic chemical insecticide that is believed to have harmful effects on environmental ecosystems and human health. In the South of France, pesticides used on vineyards have been detected in more than 92% of rivers.

Mating disruption is an alternative to pesticides that uses dispensers to saturate the air above and between the grapevines with female sexual pheromones used by female moths to

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[1] Publication bias and file drawer effects may explain why reports of nudges backfiring are rare in the published literature (Nemati and Penn, 2020; DellaVigna and Linos, 2022; Maier et al., 2022).

call males for mating. The ubiquitous presence of pheromones leads to the disorientation of the males and suppresses the calling behavior of the females. The overall effect is to reduce the number of offspring produced by the pest and thus curb the damage to the grapes. The technique is mainly used against two pests: the tortricid moth species *Lobesia botrana* and *Eupoecilia ambiguella*, commonly called the European grapevine and grape berry moths, respectively. Compared to the spraying of insecticides, which cannot be done after heavy rain, the control of grape berry moths is less dependent on weather conditions during the vegetation period. Moreover, it is easier to manage than insecticide treatments because it does not require the monitoring of oviposition periods (Hoffmann and Thiery, 2010).

Although this technique has been shown to be effective in a number of contexts, adoption rates in France are still very low: the proportion of the French vineyard area protected by biological pest control is currently only 3%, while it reaches 65% of the vineyards in Germany and 43% in Switzerland. There are at least three reasons for this. First, in the absence of subsidies, mating disruption is more expensive than insecticide use. French winegrowers apply an average of two pesticide treatments per year to control the moth,[2] which cost approximately 35 euros per hectare each. The cost of the pheromone diffusers is around 110 euros per hectare, plus the time which must be devoted by the farmer to setting-up the diffusers in the vineyards, which can add another 200 euros per hectare. Second, to be effective, mating disruption requires plots or contiguous blocks of plots covering at least 10 hectares. In many parts of Europe, it is unusual to find a single farm with such large blocks of vineyard, which often requires that winegrowers spatially coordinate their efforts in order to simultaneously adopt the bio-control technique on adjacent plots. Third, for many years, the use of chemical pesticides has been the norm in the French wine-growing sector, involving two chemical insect treatments per year on average. Habits and routines are hard to change. French farmers also doubt the efficacy of biological pest control.

We conducted an experiment in collaboration with a winegrowers cooperative,“Les Vignerons du Pays d'Ensérune” (hereafter the VPE cooperative). One of the cooperative's goals is to reduce pesticide use in order to improve the reputation of the wines and respond better to consumer demand, prevent criticism from neighbors increasingly concerned about pesticide-related health issues, and contribute to water quality and biodiversity improvement. Sustainable practices are a crucial marketing point that the cooperative seeks to leverage. the VPE cooperative has 650 members accounting for 3,200 hectares of vineyards in the South of France's Occitanie region. At the time we initiated this partnership for the implementation of an RCT, the cooperative's technical staff had repeatedly invited winegrowers to information meetings on mating disruption, but attendance remained low and the alternative practice was only used on a very limited area.

Several features of the context support the idea that a social comparison nudge can have a positive impact on the adoption of mating disruption. First, monetary concerns are no longer a major reason for non-adoption. Indeed, as part of the Common Agricultural Policy of the European Union, agri-environmental measures are available to winegrowers with an annual

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[2] According to the statistics of the French Ministry of Agriculture, accessible here: `http://agreste.agriculture.gouv.fr/IMG/ods/pratiquesviticulture2015T2bsva.ods`.

payment of about 310 euros per hectare in exchange for replacing pesticide use with bio-control techniques. Second, social comparison nudges can help alleviate the two main psychological costs detrimental to the adoption of a new technology: *(i)* the psychological costs of deviating from the norm of pesticide use; *(ii)* overestimation of coordination costs between neighbors for setting up the alternative practice. Our social comparison nudge therefore intended to reduce these two psychological costs by informing winegrowers that the alternative technique had already been largely adopted by neighboring farmers. Our nudge aimed to convince farmers that the dominant norm on pesticide use was evolving, that turning to biological control was becoming a more readily accepted technique, and that many winegrowers had successfully achieved coordination, hinting that coordination costs might not be as high as winegrowers anticipated.

3 Experimental Design

Treatment and outcomes

Our nudge includes two social comparisons: information on the total area at the provincial level (Herault *département*) which is already protected with mating disruption (5,500 ha); and information on the high rate of adoption of biological pest control in the neighboring winegrowers cooperative of Puicheric.

In October 2016, all the winegrowers of the VPE cooperative with a valid mailing address (a total of 532 winegrowers) received a letter from the cooperative advisory staff inviting them to a technical information meeting on biological pest control. Of these 532 winegrowers, about half of them (the treatment group) received an additional flyer with the following information: "*Using mating disruption to fight the grapevine moth works! Already 5,500 ha protected in the Herault province. In 2017, the Puicheric cooperative will use it on its entire vineyard area.*" The other half (the control group) did not receive the flyer, only the invitation letter. The first impact of the nudge was then measured by the attendance rate of the technical meeting held on November 7th.

In November 2016, less than a month later, all winegrowers with a valid cell phone number (a total of 413 of the initial 532 winegrowers) received a text message: "*If you'd like to know more about grapevine moth control with mating disruption techniques, reply "OK" to this number and the technician will call you back shortly.*" Winegrowers in the treatment group received an additional sentence in their text message: "*Already 5,500 ha protected in the Herault province and soon the entire area of the Puicheric vineyards.*" A second measure of the impact of the nudge was thus whether a winegrower replied "OK" to the technician. Note that all the winegrowers who received the additional sentence in the text message had also received the same message via the flyer.

Finally, eight months after the end of the experiment, in July 2017, we measured the adoption of mating disruption in order to check whether our social comparison nudge had encouraged winegrowers to go beyond the simple decision to attend information meetings and indeed induced them to change their pest control technique. We also measured adoption of biocontrol three years after the experiment, in July 2020.

Our nudge displays five important features. First, we used the comparison with the Puicheric cooperative to show that adoption of biological pest control by similar winegrowers is possible. The VPE and Puicheric cooperatives, besides being geographically close, are both affiliated with the Union Foncalieu, which markets the wine produced by the two cooperatives. Puicheric is a smaller cooperative (it has about 100 members) but it produces similar grape varieties and has the same objectives in terms of wine quality, since just like the VPE, it produces mainly wine covered by a Protected Geographical Indication (PGI) targeted for the export market. Second, we sent messages to the control group to isolate the impact of the social comparison nudge from the impact of the cooperative's standard promotion of biological control. Both treatment and control winegrowers thus had their attention directed towards biological control. The only difference between the two groups was the social comparison message. Third, we combined both postal mailings and text messages to increase the chances that winegrowers would pay attention to the social comparison message. Previous experiments with electricity and water consumers relied exclusively on postal mail messages, and their effectiveness may have been curbed by the limited attention that consumers pay to their electricity or water bills. Fourth, there was only a short time gap between the nudge and some of the measured outcomes (two weeks elapsed between the date the information meeting invitation was sent and the date the meeting was held, and winegrowers could reply instantly to the text message for information on biological pest control). This short time frame is an important feature of the experimental protocol, since it has been shown that the effect of nudges may fade over time. This timing thus allows us to capture the effect of the nudge when it is expected to be the greatest. Fifth, a number of studies have already shown that French winegrowers tend to be sensitive to the behavior and opinions of their peers when deciding whether or not to adopt a technology (Le Coent et al., 2018; Kuhfuss et al., 2016). A nudge leveraging social comparison thus seemed likely to trigger changes in farmers' technology adoption choices.

Randomization, stratification and inference

We tested our nudge using a stratified RCT. The strata were defined based on two variables: the surface of the vineyard (3 classes: 1=missing value; $2 \leq 7$ ha; $3 > 7$ ha) and the geographic area (4 classes coded from 1 to 4 corresponding to the different zones that the cooperative covers). We chose these variables and classes with the help of the cooperative's technicians to reflect the diversity of the vineyards. We ended up with 11 strata (one of the 12 strata was empty), each containing at least four winegrowers. The treatment was randomly allocated at the individual level in each of the strata. Inference was conducted using an OLS regression including strata fixed effects, as suggested by Canay et al. (2017). Although the outcomes are potentially spatially autocorrelated, we did not cluster the standard errors, since the treatment was randomly allocated at the winegrower level, and thus not spatially autocorrelated (Abadie et al., 2017).

Table 1 presents descriptive statistics for the main sample.[3] Pre-treatment variables are adequately balanced between the treatment and control samples. If anything, a slightly larger proportion of treated farms had adopted mating disruption before the experiment, but this

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[3] For expositional purposes, we merge the first two surface classes into one for the remainder of the paper.

difference is not statistically significant.

Qualitative survey

During the summer of 2020, we returned to the field and conducted a qualitative survey with a sample of winegrowers who had been involved in the experiment. The interviews were conducted by phone and recorded (with the agreement of the participants). We surveyed only large vineyards with a registered phone number. We aimed to interview at least one winegrower in each of nine strata, defined by whether or not they had received the nudge, whether they had adopted mating disruption and whether they belonged to one of the three largest municipalities. We randomly ordered winegrowers within these strata and stopped collecting data once we had interviewed one winegrower in each strata. In the end, we interviewed nine winegrowers. The survey was mostly composed of closed questions in which we first asked the winegrowers background check questions, then about their opinion on our nudge and finally about its expected impact. We then revealed the actual results of the RCT experiment to each interviewee (none of the survey participants had the means to know the results of the RCT in advance) and asked their opinion about what mechanisms might explain the results.

In the survey, four possible explanations were put forward as to why the nudge backfired: (i) the mention that 5,500 ha already equipped with biocontrol devices in the region was disheartening to the winegrowers; (ii) mentioning that almost all winegrowers in the Puicheric cooperative had already adopted biocontrol techniques was experienced as unfair and/or irrelevant by the winegrowers; (iii) the fact that almost all of the growers in the Puicheric cooperative had already adopted biocontrol techniques was seen as a signal that the cooperative expected winegrowers to do the same without additional incentives; (iv) the fact that almost all winegrowers in the Puicheric cooperative had already adopted biocontrol techniques was interpreted as a sign that additional efforts to decrease chemical pesticide use and improve water quality locally were not necessary.

4 Results

Figure 1 and Table 2 present the results of the analysis on the sample of 532 winegrowers included in the experiment. Figure 1 shows that the proportion of winegrowers who adopted biocontrol techniques increased over time in all groups, from 2% in 2016 to 20% in 2020. Large farms (area >7 ha) seem to lead the adoption dynamics, with an adoption rate increasing from around 10% in 2016 to over 40% in the control group in 2017.

Among large vineyards, the adoption rate of biological pest control in 2017 (the year following the sending of the nudge) is around 20% in the treated group and around 40% in the control group, thus revealing an almost 20 percentage point (p.p.) or 50% decline of adoption due to the nudge. Table 2 shows that the estimated decrease is actually 18 p.p. among all large winegrowers and 20 p.p. in the sub-sample of large winegrowers equipped with a mobile phone.[4] In 2020, there is no statistically significant difference in adoption rates between the

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[4] The analysis on the group of 413 winegrowers with a valid mailing address and cell phone number yields very similar results and are available upon request.

treated and control groups: the adoption rate in the treated group eventually caught up with that of the control group. We do not find statistically significant differences between the treated and control groups, whatever the outcome considered, in the overall sample or in the stratum of small vineyards. This seems consistent with the fact that adoption rates are lagging among small winegrowers.

Figure 2 plots the number of winegrowers interviewed who accepted one or more of the explanations put forward for why our nudge backfired. Only three of the nine winegrowers thought that the mention of 5,500 ha already using mating disruption would explain the result via a discouragement effect. Similarly, only two of them thought that winegrowers could have interpreted the mention that almost all winegrowers in the Puicheric cooperative had already adopted mating disruption as a sign that no further efforts were needed to decrease pesticide use in the area. The two leading explanations from six of the nine winegrowers were: *(i)* winegrowers reacted negatively to the mention of the Puicheric cooperative by feeling unfairly compared with the *do-gooders*; *(ii)* winegrowers felt that mentioning Puicheric meant that the VPE cooperative had no intention of giving any additional incentives to the farmers.

These two explanations are obviously not mutually exclusive and we have no way of choosing between them with our data. The first explanation, which is related to reactance, has already been studied in the literature. In what follows, we therefore further explore the second explanation with a model in which a nudge can trigger a bargaining game between the winegrower and the management of the cooperative.

5 When nudges backfire: a signalling game

In this section, we offer an economic explanation for why nudges may backfire, using the framework of a signalling game played by two rational agents, namely, the cooperative's top management (embodying the interests of the overall cooperative and its members as a group, henceforth the cooperative), who proposes biological pest control, and the individual winegrower (also a member of the cooperative), who decides whether to accept this proposal. In a nutshell, more communication efforts on the part of the cooperative (via a nudge rather than a simple email/letter) may lead the winegrower to suspect that the cooperative has private information on the collective benefits that could be gained from the winegrower's adoption of biological pest control and intends not to share these benefits with him. The winegrower thus reacts by delaying adoption, in the hope that the cooperative may later offer incentives for the adoption of biological pest control. It could be, for example, a higher price for grapes paid to cooperative members who have adopted biocontrol. This reasoning might explain why our nudge led to a smaller adoption rate of biological pest control in the treated group than in the control group.

Let us begin by describing the agents' payoffs. The bargaining game between the two agents takes place under asymmetric information on both sides. We can safely assume that the winegrower (hereafter he) has private information about his payoff u from biocontrol adoption. u aggregates the private benefits minus the private costs of biological pest control, plus perhaps some "warm glow" value for behaving in an environmentally-friendly manner. Note that overall u may be positive or negative.

Similarly, we can consider that the cooperative top management (hereafter she) better knows her own benefits $v > 0$ from the adoption of biological pest control, as this value may include gains in reputation or profit increases from increased sales or increased prices of a more ecological wine. Additionally, the success of the adoption of biological pest control may depend on the surface area of vineyards on which it is used; thus, it is in the cooperative's interest to enroll large farms. The larger the winegrower's vineyard, the higher v.

We now propose a simple timing sequence for this game. In the first stage, the cooperative privately learns the value of v, and then chooses a design x for her proposal; x may be a simple email or may include more sophisticated nudges. Design x is thus chosen in some set X, with some cost $c(x)$. In the second stage, the winegrower observes this proposal x, and decides whether or not to accept it. If he accepts the proposal, adoption takes place and the payoffs are u and $v - c(x)$. If they were to stop here, the problem would be easily solved: in all Perfect Bayesian Equilibria, there would be adoption if, and only if, it were in the winegrower's interest ($u > 0$), and if the cooperative's top management chose the least costly design, that which minimizes $c(x)$.

Note, however, that the cooperative has control over its budget and can implement transfers to its members. A rejection at the second stage may thus be interpreted as an element of a bargaining strategy: the winegrower expects the cooperative to make a more favorable offer. To take this possibility into account, we enrich the game as follows: if the winegrower rejects the proposal x made at the second stage with some probability $\delta < 1$, we go to a third stage in which, in addition, the cooperative offers the winegrower a transfer, $t \geq 0$. This transfer may occur, for example, inside the cooperative, in the form of a better price paid for the grapes harvested under biological pest control, or of a cost subsidy for the installation of diffusers. Once more, the winegrower may accept or reject this new proposal, and then the game ends. Notice that δ may also be interpreted as a discount factor due to a delay between the second and the third stages. The game thus allows for both immediate and delayed adoption of biocontrol.

For technical reasons, we assume that v can take only two values $v_2 > v_1 \geq 0$ with probabilities m_2 and m_1 ($m_1 + m_2 = 1$). The distribution of u is characterized by a cumulative distribution function F and a density function f, strictly positive on the set of real numbers. We assume that the ratio $(1 - F(u))/f(u) - u$ is decreasing with respect to u.[5] A key assumption is that these values u and v are privately known and independent.

We are looking for Perfect Bayesian Equilibria (PBE) of this game, i.e., strategies that are best-responses to each other and beliefs revised at each stage from prior beliefs (m_1, m_2), using Bayes' rule whenever possible. A first striking result is that all such equilibria are pooling (a formal proof is given in the Appendix):

Proposition 1. *Whatever her private value v for the adoption of biological pest control, in equilibrium, the cooperative chooses the same design x.*

The intuition for this result can be sketched by proceeding by contradiction. Suppose that in equilibrium, a cooperative with a high private value chooses a more costly design. By observing this costly design, the winegrower is thus able to infer that the cooperative's payoff

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[5] This "hazard rate" property is standard in game theory and is satisfied by many distributions, including normal, log-normal and uniform distributions. Moreover, for our purposes it needs only hold when u is negative.

is high. Accordingly, the winegrower knows that if the last stage takes place, the cooperative will offer a more generous proposal. This in turn makes the winegrower less willing to accept the initial proposal. Overall, by transmitting the information that v is high, the manager only delays adoption and makes adoption more costly to her budget, since adoption is more often accompanied by a transfer. This is the contradiction we needed. It is now only necessary to verify that the game admits a simple equilibrium:

Proposition 2. *There exists a Pareto-dominating equilibrium, in which the cooperative always chooses the least costly communication design (i.e., without resorting to any nudge), whatever her private value for biocontrol adoption. Deviations to a costlier proposal are unprofitable, because they are interpreted by the winegrower as proving that the manager has a high private value.*

Overall, as in many bargaining games under asymmetric information, it is better not to signal a strong interest for the object to be awarded. In the proposed equilibrium, adoption takes place immediately if the winegrower's payoff is above a positive threshold u^*, because delaying is more costly for winegrowers with higher payoffs. Adoption occurs after a delay for winegrowers with $u \in [-t^*, u^*]$, where t^* is the transfer proposed in equilibrium at the last stage.

These results may help us understand why our nudge backfired. Including a nudge in the design of the proposal was interpreted by winegrowers as a deviation from the pooling equilibrium and as evidence that the cooperative's payoff from adoption was high. The consequence is that large winegrowers delayed adoption, as observed in Figure 1, in the hope that they would extract some additional benefits from the cooperative later on. We did not observe the last stage of the game, but Figure 1 also shows that delayed adoption took place after the initial proposal, and this is consistent with our model's predictions. Finally, the observation that large farms delay adoption more often may be due to the fact that larger vineyards are more indispensable to the success of the project, and thus are more tempted to exploit their stronger bargaining position.

6 Conclusion

We carried out a field experiment to test the effectiveness of a nudge initially designed to encourage the adoption of biological pest control in French viticulture. We bring an additional brick to the edifice of the literature that highlights the unexpected and possibly harmful effects of nudges, by focusing here on a field still little-explored, that of environmental and natural resources conservation. To better understand why our nudge backfired, we carried out a qualitative survey and further examined one of the two paths suggested by the answers in the survey, namely, that of a bargaining game between winegrowers and their cooperative's top management. Our model shows that a nudge can lead to a situation in which some winegrowers knowingly delay the adoption of the technique encouraged by the nudge if they interpret the nudge as a signal that the cooperative values the technique more than they initially believed. In such a case, they may withhold adoption to force the cooperative to share some of the gains of adoption with them.

This backfire effect may, however, only be short-lived. Indeed, if the winegrower and the cooperative's top management are able to come to an agreement for sharing the benefits of the nudged behavior, adoption can resume, as shown in both our model and in the results of our experiment. Nevertheless, the delay imposed by the nudge backfiring may be sizable. In our experiment, the nudge decreased adoption of biocontrol by 50%, from a baseline adoption rate of 40%, and adoption in the treated group caught up with that in the control group only three years after the experiment.

Our study highlights that nudges may backfire when agents can bargain to increase their share of the gains brought about by adopting the nudged behavior. We view our results as reinforcing the importance of experimenting on a small scale with nudges aimed at encouraging adoption of virtuous behaviors in order to detect unexpected adverse effects, particularly in contexts where negotiations on the sharing of the costs of adoption are likely to occur.

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A Figures

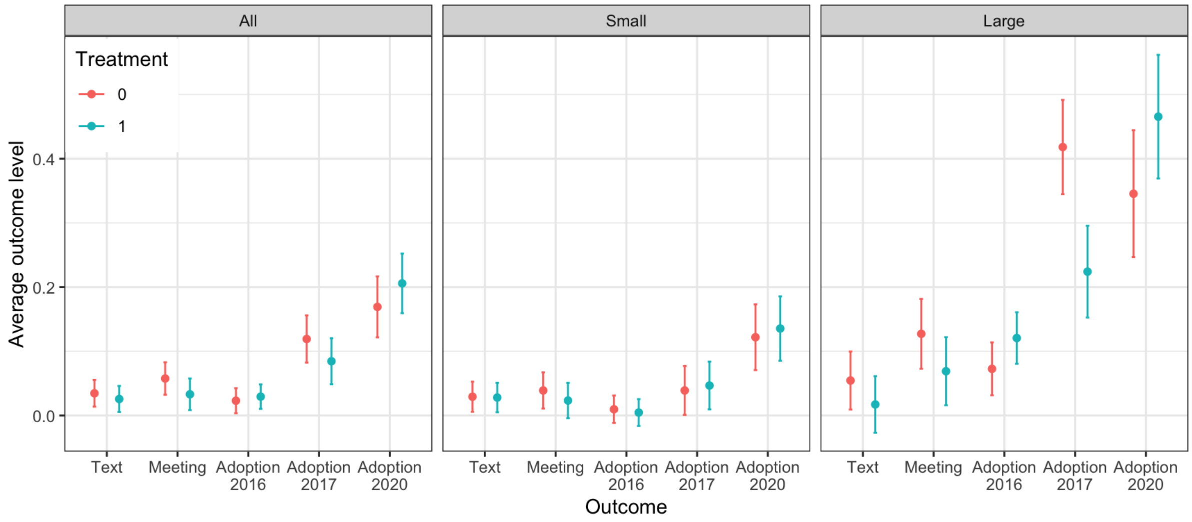

Figure 1: Results of the field experiment

Note: This figure presents the main results of our experiment. *All* refers to all farms in the sample (532 winegrowers). *Small* refers to the 419 farms with either a missing area or an area lower than 7 ha. *Large* refers to the 113 farms with a size larger than 7 ha. *Treatment* takes value 1 when a farm is assigned to receiving our nudge and 0 otherwise. *Text* measures the proportion of winegrowers who have replied to the text message sent by the caseworker asking them whether they were interested in receiving more information about mating disruption. *Meeting* measures the proportion of winegrowers who have participated in a meeting presenting mating disruption, which took place after our experiment. *Adoption 2016* measures the proportion of winegrowers who had adopted mating disruption in 2016, prior to our experiment. *Adoption 2017* measures the proportion of winegrowers who adopted mating disruption in 2017, after our experiment. *Adoption 2020* measures the proportion of winegrowers who adopted mating disruption in 2020, three years after our experiment. Error bars are 95% confidence intervals.

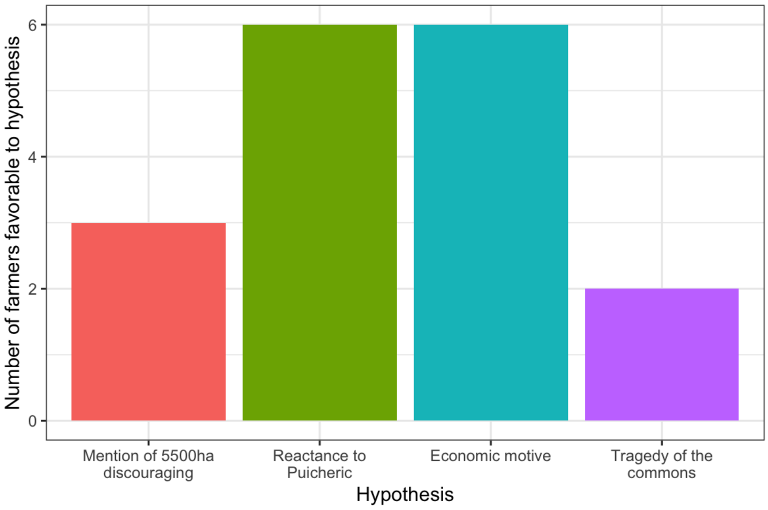

Figure 2: Explanations chosen by interviewed winegrowers as to why the nudge backfired

Note: This figure presents the number of winegrowers (among the nine who took part in our post-experimental survey) who agreed with a given response as likely to explain why our nudge backfired. *Mention of 5,500 ha discouraging* refers to winegrowers believing that their peers found the mention of 5,500 ha already practicing mating disruption in the region as discouraging. *Reactance to Puicheric* refers to winegrowers believing that their peers reacted negatively to the mention of the fact that almost all winegrowers in the Puicheric cooperative had already adopted mating disruption. *Economic motive* refers to winegrowers believing that their peers interpreted the mention of the fact that almost all winegrowers in the Puicheric cooperative had already adopted mating disruption as a sign that the cooperative expected them to adopt mating disruption without providing any additional benefits. *Tragedy of the commons* refers to winegrowers believing that their peers interpreted the mention of the fact that almost all winegrowers in the Puicheric cooperative had already adopted mating disruption as a sign that additional efforts to decrease pesticide use were not necessary.

B Tables

Table 1: Descriptive statistics

Cell phone	Large farm	Treatment	Age (in years)	Area (in ha)	Adoption 2016	N
0	0	0	71.44	0.94	0.00	55
0	0	1	70.40	1.10	0.00	58
0	1	0	55.50	10.49	0.00	2
0	1	1	60.25	11.22	0.00	4
1	0	0	59.60	2.16	0.01	150
1	0	1	57.71	2.03	0.01	156
1	1	0	50.52	18.28	0.08	53
1	1	1	48.83	19.09	0.13	54

Note: This table presents descriptive statistics for our main experimental sample. *Cell phone* takes value 1 when a winegrower has a valid cell-phone number in our database and 0 otherwise. *Large farm* takes value 1 when farm size is larger than 7 ha and 0 otherwise. When farm size is missing, we assign it a size of 0 ha. Farms with missing size belong to a separate strata in our experiment. We merge them with small farms to simplify exposition. *Treatment* takes value 1 when a farm is assigned to receiving our nudge and 0 otherwise. *N* is the size of the corresponding strata. *Adoption 2016* measures the proportion of winegrowers who had adopted mating disruption in 2016, prior to our experiment.

Table 2: Average treatment effects, by farm size and sample composition

Outcome	Sample	Farm size		
		All	Small	Large
Meeting	All	-0.02	-0.02	-0.04
		(0.02)	(0.02)	(0.05)
Text	All	-0.01	-0.00	-0.04
		(0.01)	(0.02)	(0.03)
Adoption 2016	All	0.01	-0.01	0.04
		(0.01)	(0.01)	(0.06)
Adoption 2017	All	-0.03	0.01	-0.18
		(0.02)	(0.02)	(0.09)
Adoption 2020	All	0.04	0.01	0.13
		(0.03)	(0.03)	(0.09)
Meeting	Cell	-0.03	-0.02	-0.04
		(0.02)	(0.02)	(0.06)
Text	Cell	-0.01	-0.00	-0.04
		(0.02)	(0.02)	(0.04)
Adoption 2016	Cell	0.01	-0.01	0.05
		(0.02)	(0.01)	(0.06)
Adoption 2017	Cell	-0.03	0.02	-0.20
		(0.03)	(0.03)	(0.09)
Adoption 2020	Cell	0.06	0.03	0.14
		(0.04)	(0.04)	(0.10)

Note: This table presents the estimated average treatment effect of our experiment, by farm size and sample composition. *All* refers to all farms in the sample. *Small* refers to farms with either a missing area or an area of less than 7 ha. *Large* refers to farms larger than 7 ha.

Meeting refers to the meeting presenting the information on mating disruption, which took place after our experiment. *Text* refers to the text sent by the caseworker asking winegrowers whether they were interested in receiving more information about mating disruption. *Adoption 2016* measures the proportion of winegrowers who had adopted mating disruption in 2016, prior to our experiment. *Adoption 2017* measures the proportion of winegrowers who adopted mating disruption in 2017, after our experiment. *Adoption 2020* measures the proportion of winegrowers who adopted mating disruption in 2020, three years after our experiment. Estimates are obtained using OLS regressions with strata fixed effects. Standard errors are in parentheses.

C Proofs

Proof of Proposition 1: The manager's (hereafter she, or C) strategy is $(x,\ t)$, where $x(v)$ is the design of the proposal chosen by a cooperative with type v, and $t(v,x)$ is the transfer the cooperative offers at the last stage if the design is x, and the winegrower has rejected the proposal. The winegrower's (hereafter he, or W) strategy is $(\alpha,\ \beta)$, where $\alpha(u,x) \in \{0,1\}$ specifies whether W accepts or rejects the proposal when his type is u and the design is x, while $\beta(u,x,t) \in \{0,1\}$ specifies whether W accepts or rejects the transfer t at the last stage of the game. Moreover, W revises his prior beliefs (m_1,m_2) into $(\mu(x), 1-\mu(x))$ upon observing x. A Perfect Bayesian Equilibrium requires that these beliefs are obtained from a/the Bayes rule whenever possible and that each player plays a/their best response, given these beliefs and the other player's strategy.

Assume such an equilibrium exists. Then β is easily determined: in an equilibrium, it must be that W accepts the transfer if and only if $t+u \geq 0$ (whether this inequality is strict or not does not matter, since the distribution of the winegrower's types is continuous.) We can also determine α easily. Indeed, W accepts the proposal by comparing his payoff u to the expected payoff $Z(u,x)$ associated to waiting, in the hope that with probability δ a sufficiently high transfer will be offered:

$$Z(u,x) \equiv \delta E[\max(t(\tilde{v},x)+u,0)|x]. \tag{1}$$

Note that the expectation is taken with respect to the revised distribution $\mu(x)$ of v, since the winegrower has already observed x. Moreover, $\delta < 1$ implies that the difference $u - Z(u,x)$ is strictly increasing in u; and since Z is at least zero, when $u < 0$ this difference is strictly negative. Therefore, there exists a unique threshold value $u^*(x) \geq 0$ such that W rejects the offer if $u < u^*(x)$, and accepts it otherwise. Finally, since $u^*(x) \geq 0$ and transfers are at least zero, using (1) the equality $u^*(x) = Z(u^*(x),x)$ becomes

$$u^*(x)(1-\delta) = \delta E[t(\tilde{v},x)|x]. \tag{2}$$

Let us now turn to the transfer $t(v,x)$ chosen by C at the last stage. Since C knows that the remaining types are those with $u \leq u^*(x)$, when her payoff is v she chooses t to maximize:

$$b(t,u^*,v) \equiv (F(u^*)-F(-t))(v-t).$$

The "hazard rate" assumption ensures that this problem has a unique solution.[6] We also learn that this solution is a function of x only through $u^*(x)$. Therefore, the equilibrium strategy can be written $t(v,x) = T(v,u^*(x))$, for some function T. Finally, since the derivative of b with respect to t is increasing with respect to v (the cross-derivative is $f(-t) > 0$), and decreasing with respect to u^* (the cross-derivative is $f(u^*) > 0$), the solution $T(v,u^*)$ must be increasing with respect to v, and decreasing with respect to u^*.

We can now turn to the first stage, at which the manager chooses x to maximize:

$$-c(x) + (1-F(u^*(x)))v + \delta \max_t\ b(t,u^*(x),v).$$

Thanks to the envelope theorem, the derivative of this expression with respect to u^* is:

$$-f(u^*)v + \delta f(u^*)(v - T(v,u^*)), \tag{3}$$

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[6] Indeed, when solving, we can focus on the interval $-u^* < t < v$, so that the manager gets a positive payoff. The derivative of this payoff with respect to t has the same sign as $-(F(u^*)-F(-t))/f(-t)+v-t$. The derivative of this expression is $-2-(F(u^*)-F(-t))f'(-t)/f^2(-t)$. It is negative if $f' \geq 0$, and otherwise, it is less than $-2+(F(-t)-1)f'(-t)/f^2(-t)$, which is itself negative by the hazard rate assumption. This shows that the derivative of the payoff is positive, then negative, so that the maximum is unique.

which is negative since $\delta < 1$, and decreases in v since $\delta < 1$ and T increases with v. Therefore, a manager with a higher v must choose a design associated to a lower acceptation threshold $u^*(x)$.

Suppose now that there exists a separating equilibrium in which v_1 chooses a design x_1, and $v_2 > v_1$ chooses a design $x_2 \neq x_1$. From what we just said, it must be that $u^*(x_1) \geq u^*(x_2)$. Since $x_1 \neq x_2$, the winegrower learns v by observing x, so that his revised beliefs are characterized by $\mu(x_1) = 1$ and $\mu(x_2) = 0$. We then apply (2) to get

$$u^*(x_1)(1-\delta) = \delta t(v_1, x_1) = \delta T(v_1, u^*(x_1)) \geq u^*(x_2)(1-\delta) = \delta t(v_2, x_2) = \delta T(v_2, u^*(x_2)).$$

But we have already shown that T increases with v and decreases with u^*. Thus, we have reached a contradiction: a separating equilibrium cannot exist.[7]

Proof of Proposition 2: We now know that all equilibria are pooling, in the sense that all types of the cooperative choose the same design x^*. Let us build one such equilibrium. On the equilibrium path, W does not learn anything from the proposal x^*, and the threshold $u^*(x^*)$ can be computed from (2):

$$u^*(x^*)(1-\delta) = \delta(m_1 T(v_1, u^*(x^*)) + m_2 T(v_2, u^*(x^*))).$$

In equilibrium, when her type is v the manager C thus gets the payoff:

$$B^*(v) = -c(x^*) + (1 - F(u^*(x^*))v + \delta \max_t b(t, u^*(x^*), v).$$

If C deviates to a different design $x \neq x^*$, we impose that the winegrower interprets this deviation as signalling that the manager is of type v_2. We obtain a new threshold $u^*(x)$, and (2) implies:

$$u^*(x)(1-\delta) = \delta T(v_2, u^*(x)).$$

Because T is increasing in v and decreasing in u^*, we obtain $u^*(x) \geq u^*(x^*)$: the deviation thus makes immediate adoption less probable. Moreover, the manager's payoff becomes:

$$B(x, v) = -c(x) + (1 - F(u^*(x))v + \delta \max_t b(t, u^*(x), v).$$

As already observed (see (3)), this expression is decreasing with respect to u^*, so that:

$$B(x, v) \leq -c(x) + (1 - F(u^*(x^*))v + \delta \max_t b(t, u^*(x^*), v) = -c(x') + c(x) + B^*(v),$$

and therefore the deviation to x' is unprofitable as soon as x minimizes the cost $c(x)$, whatever the type v of the manager. This concludes the proof.

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[7] Note that in the case $\delta = 0$, this inequality decreases to $u^*(x_1) = u^*(x_2) = 0$. Then one can only conclude that the manager must choose a design that minimizes the cost $c(x)$.

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