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Counteracting dishonesty strategies: A field experiment in life insurance underwriting

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Abstract

Individuals often face financial incentives that challenge their desire to behave honestly. Strategically making excuses to justify dishonesty allows them to give in to the temptation of financial benefit and retain their moral self-image. In the context of insurance underwriting, the stakes are high, as providing false information or redacting information allows customers to reduce premiums. This is particularly true for smoking disclosures that carry great weight in life insurance. We conduct a field study with a large insurance company with the aim of neutralizing justification strategies that individuals deploy for reducing the costs of dishonest smoking disclosures to insurers. First, we raise awareness of the negative consequences dishonesty could have on other policy holders to counteract that individuals could attenuate or ignore such adverse consequences. Second, we make salient the pro-social efforts of the insurer to work against a potentially negative perception of the insurance industry that may feed the excuse of insurance companies being deserving of harm. The study presents field evidence that messages containing information about the social consequences of one's actions or the pro-social behavior of a second party can influence normative behavior, particularly honesty.

KEYWORDS

disclosure, field experiment, honesty, life insurance, smoking

1 | INTRODUCTION

Behaving honestly is crucial for resolving uncertainty in environments with high degree of information asymmetry like insurance underwriting (Akerlof, 1970), even though dishonesty is a rational decision weighing on the potential rewards against the probability of detection and its consequences (Becker, 1968). Yet we frequently observe low levels of dishonesty even when there is material financial gain and there is absence of detection and adverse consequences. This “economically irrational” finding is ascribed to the psychological costs that individuals incur when behaving immorally, which stems from their desire to maintain their self-concept of being honest (e.g., Abeler

et al., 2014, 2019; Mazar et al., 2008). There is even a sense of “pure lie aversion” as people appear to suffer a utility cost when they tell a lie that goes against their innate values or established social norms (Ellingsen & Johannesson, 2004; Kartik, 2009; López-Pérez & Spiegelman, 2013). Others argue that in the absence of detection and consequences, individuals would rationalize dishonesty as a gain-maximizing strategy (Abeler et al., 2014; Mir Djawadi & Fahr, 2015).

The context of our study is a field experiment on counteracting dishonesty strategies on life insurance applications, specifically customers lying about their true smoking status. Indeed, there is substantial evidence to suggest that people behave dishonestly by redacting risk-relevant information on life insurance applications. In Oceania, the

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region of our investigation, customer disclosures during underwriting point to an impossibly healthier, both physically and mentally, and generally low risk insured population (Christodoulou & Samuelli, 2020). We examine the question of dishonesty in reporting smoking status as we also observe an unlikely low rate of smoking disclosures. In the period leading to the experiment, only 4.5% insurance applicants disclose at the quotation stage that they smoke or have smoked during the last twelve months (see Table 2). This is in stark contrast to the population prevalence rates, with 13.8% daily smokers in Australia (Australian Bureau of Statistics, 2018) and 13.0% in New Zealand (New Zealand Ministry of Health, 2018) with another 2.5% of occasional smokers.

In this context, the probability of detecting dishonest disclosures is very low for lifestyle risk factors with diagnostic difficulties, like smoking. The only way to confirm smoking status is via an intrusive medical examination, which is not practiced by this insurer. The financial rewards from redacting smoking disclosures are indeed great and take effect immediately as, depending on the product, the admission of smoking can even double the premiums charged and may also lead to exclusions making the offer unaffordable.¹ The consequences of detection have the potential of being great (e.g., cancellation of contract or refusal to pay a claim), but this rarely materializes as insurers must retrospectively demonstrate intent to mislead or misrepresent material fact at the point of the insurance application using admissible medical evidence, and the pursuit of a legal battle also risks reputational damage.

By testing whether interventions neutralize specific strategies to reduce the moral costs of behaving dishonestly, the literature not only examines mechanisms that can be used to mitigate dishonesty but also provides insights into the underlying strategies that people may use to justify dishonest behavior. Our study contributes to the literature by examining novel interventions to counteract two strategies hypothesized to be used to justify dishonesty in the context of insurance underwriting: Consequence Attenuation (CA) and Victim Deservingness (VD).²

CA captures the denial of any (considerable) negative externalities from being dishonest (Köneke et al., 2015, p. 166; Sykes & Matza, 1957). Individuals may convince themselves that the negative economic impact of reporting false information to keep their own premiums low is negligibly small for an insurance company—a large, possibly wealthy, institution—and thus will not hurt the business. The customers could underestimate the severity of the direct impact their dishonesty may have on other customers through increased premiums being passed on to them, or they may even completely lack awareness that other customers could be affected. These considerations allow individuals to convince themselves that their dishonesty is not a crime because there is no actual victim harmed. Making salient the negative

externalities and indicating how the other customers (concrete victims) are likely harmed by one's dishonesty is shown to have potential to increase truth-telling (see also the meta-analyses by Gerlach et al., 2019; Köbis et al., 2019; Leib et al., 2021).

VD acknowledges that there is a victim and captures the strategic justification that the victim deserves to be harmed (e.g., Köneke et al., 2015, p. 167; Sykes & Matza, 1957). This point of view may be based on bad media coverage about a firm or an industry as a whole or unpleasant information about the particular firm that the individual interacts with. After all, people not only care about what they can gain from lying but also care about how much the other side deserving harm would lose (Gneezy, 2005). Thus, dishonest behavior can be justified as an act of negative reciprocity or even used for retaliation.

We test two interventions that are designed to mitigate dishonesty due to the CA and VD strategies.³ If people are not reporting their smoking behavior due to either of these reasons, then we expect that our interventions will get more smokers to admit that they smoke or have smoked. We study a life insurer where customers initially complete applications with the help of an independent adviser and are offered a non-binding quotation. The information provided by applicants at this initial stage includes a binary response to whether they smoke or have smoked during the last 12 months. If the customer accepts the quotation, then a follow-up interview over the telephone with an insurer representative occurs, whereby the customer is asked again the same binary smoking question. Our intervention occurs during this phone call.⁴

To counteract CA, our first intervention makes salient the fact that dishonest disclosures may harm other customers to the extent that the most vulnerable may no longer afford insurance. Making the harm to others salient is designed to counteract the argument that being dishonest in the quotation and screening process is a crime without a true victim, and therefore increases the moral costs of lying. To counteract VD, our second intervention promotes the insurer's pro-social efforts. Providing a positive image of the insurance company is designed to mitigate customers' potential belief that the insurance company may be deserving of harm.

We find that both interventions result in a higher number of people revising their status from non-smoker in the quotation stage to smoker in the screening stage. In the control group, that is absent of any intervention and reflects the insurer's standard efforts to elicit more honest disclosures, 2.97% of customers revise their status from non-smoker to smoker. In the CA intervention, the revision rate increases to 3.77%, and in the VD intervention the revision rate reaches 4.35% (i.e., a 46% increase in comparison to the control group). As we explain, we find these results to be a remarkable achievement because our treatments compete with significant

¹The actual smoking premiums vary greatly depending on other customer characteristics, such as age, sex, profession, and the various medical and lifestyle risk disclosures. *Finder* estimates that the effect of smoking premiums in life term insurance varies from 63% to 93% across providers (<https://www.finder.com.au/regular-smokers-and-life-insurance>). *Comparing Expert* estimates a more dramatic smoking effect by elevating premiums from about 124% up to 144% (<https://www.comparingexpert.com.au/life-insurance/term-life-insurance-smoking-non-smokers/>). Web pages accessed as at 31/7/2022.

²An extensive overview of potential justification strategies in the context of insurance is provided in Köneke et al. (2015), who also provide suggestions about how dishonesty can be avoided, even going beyond counteracting justification strategies.

³Our choice of testing the CA and VD treatments follows our belief that these are most appropriate for our field setting. Also, there is lack of work testing their effectiveness in the field hence we could not form any a priori predictions about which one would be most effective. We should also note we were asked to restrict our intervention to only two treatments, and we had to watered down the language of the treatments to be consistent with internal and external guidelines of underwriting rules. This rewording may have reduced the treatments effectiveness.

⁴It is important to note that telephone screening by the insurer representative is as thorough, and perhaps even more so than the screening incurred by an advisor.

financial incentives for redacting smoking disclosures, the revised smoking disclosures can be perceived as an admission of lying, and our experiment coincided with the release of two damning federal reports on the misconduct of insurance companies followed by considerable negative publicity.

The rest of the paper is organized as follows. Section 2 discusses the relevant literature review. Section 3 describes the field experiment setup. Section 4 discusses the data. Section 5 summarizes the results. Section 6 concludes.

2 | LITERATURE REVIEW

When individuals behave dishonestly and perceive themselves as doing so, they need to update their moral self-image. In the trade-off between the benefits of honesty and the psychological costs associated with it, they tend to apply a range of self-serving strategies to justify their misbehavior (e.g., Ayal et al., 2015; Bandura, 1999; Köneke et al., 2015; Mazar et al., 2008; Shalvi et al., 2015). These strategies allow them to maintain a positive self-image despite behaving immorally by “neutralizing” the immoral act (Sykes & Matza, 1957) and thus reducing “ethical dissonance” (Ayal et al., 2015). Consequently, the moral costs of lying decreases, which makes it easier to lie. In addition to the internal rationalizations, inattention may also allow individuals to bypass their internally held values (Mazar et al., 2008).

Outside the lab, there are only a few field studies that experimentally test interventions in their natural environment, which are designed to reduce the scope for justifying dishonesty. We could only locate three such field studies to date, involving financial rewards.⁵ Pruckner and Sausgruber (2013) tested the effect of a legal reminder (that stealing is illegal) and a moral prime (that honesty is socially rewarding) on paying for newspapers taken at newspaper stands. Overall, only about one third of people would actually pay for the newspaper, and generally not the full amount. However, the moral prime had an effect in increasing honesty by boosting payments from 10% to 25% of their value, whereas the legal reminder was entirely ineffective. Azar and Bar-Eli (2013) tested the honesty of restaurant patrons in returning excessive change to their cash payments. The majority of customers did not return the excess change, but regular customers were more likely to return the cash compared to the one-time customers. They also found that women were more likely to return the excess cash than men, and dining in pairs made no difference than dining alone. Perhaps the most interesting finding of all was that receiving a greater amount of excess change resulted in more people returning the cash. This is a surprising economic result as people seem to be morally content to cheat for making small financial gains, but feel less comfortable cheating for making more substantial gains.

The financial rewards of the above field experiments are very small, no more than a couple of dollars worth. The only field study on

honesty that we could find performed in the natural environment with substantial financial rewards is by Bott et al. (2019) on tax evasion. They hypothesize that one reason people may justify not complying with tax remittances is that they strategically believe that others are also not complying, and thus see a lower moral dilemma in paying taxes if others are perceived to also not paying taxes. They ran a field experiment among households that under-report their taxes and randomly told some households that most citizens in fact do correctly report their taxes. As a result, they find that providing information that most citizens correctly report their taxes increases tax remittances. Our study appears to be the only other field experiment on lying with high ecological validity involving substantial financial rewards that is specifically designed to counteract dishonesty strategies.

Our field experimental setting has important similarities with the lab experimental setting of Fischbacher and Föllmi-Heusi (2013), whereby lying cannot be detected at the individual level but the underlying distribution of the truthful outcome under full honesty is well understood. In the Fischbacher and Föllmi-Heusi (2013) setting, the underlying distribution is that of a random die roll. In our setting, the underlying distribution is approximated by the lifetime prevalence rates of smoking that are well documented. The die roll experiment allows us to identify private lying costs at the group level, which is very much alike the insurance underwriting setting. That is, we cannot know for sure if a specific customer lies in the insurance application, but we have a very good idea of whether the disclosures of the pool of customers is truthful.

3 | FIELD EXPERIMENT

The context of our field experiment is in retail applications for life insurance. The experiment investigates the effectiveness of two treatment interventions aiming to reduce dishonesty in self-disclosures of smoking status. The risk-relevant information for life insurance applications is collected in two stages.

At the quotation stage, a baseline price for life insurance is quoted to customers by a financial adviser operating independently of the insurer. The financial adviser is tasked with explaining the product's coverage, features, and suitability to the customer's needs. The baseline price is calculated on the basis of the following information that is collected at the quotation stage: age, sex, employment status, occupation, income, location, and smoking status. Using this information, the adviser presents to the customer a list of competitive quotes with similar coverage from different suppliers. It is important to know that the disclosures made at the quotation stage cannot lead to a decline of insurance, with the only exception of extreme high age lying outside the actuarial rule book range.

The information carrying the greatest weight for calculating fair prices is indeed collected at the quotation stage, where older is riskier, males are considered riskier than females, and there are premiums for occupational hazards (e.g., blue collar riskier than white collar) and smoking status. Smoking disclosures are particularly critical in determining the predictive accuracy of mortality risk and the calculation of

⁵For a comprehensive review of the literature on individual dishonesty, see Jacobsen et al. (2018), and for a review of experimental studies studying honesty, see Rosenbaum et al. (2014).

fair premiums.⁶ Collectively, these factors are strongly predictive of the likelihood of cancer, heart disease, cerebrovascular disease, and respiratory disease.

The true age, sex, and occupation are difficult to conceal and can be easily confirmed, but verifying smoking status requires expensive diagnostic tests that are generally avoided as they risk sales in a highly competitive market with little product differentiation. Even post-contract there is very little capacity to retrospectively discover false smoking disclosures. To demonstrate dishonesty, the insurer must present reliable evidence that the customer redacted the true smoker status. Given the high friction costs, there is no systematic attempt to obtain such private and highly confidential medical records (unless there is litigation action). Hence, why life insurers largely rely on the honesty of customers in self-disclosing their true smoking status.

In the quotation stage, the financial advisor records the smoking status of the customer, by entering a Yes/No answer to the following statement that is read out loud:

If in the last 12 months the life insured has smoked or used any nicotine replacement products, including patches, gum, e-cigarettes or champix, please select smoker rates

The financial advisor is unlikely to discourage smoking disclosure. On the contrary, the advisor has the incentive to encourage the revelation of smoking status because it would increase premiums without risking the sale, which in turn boosts the sale commissions paid to the advisor that are calculated as a percentage of the sale (Christodoulou & Samuelli, 2020). Therefore, any redacted smoking disclosures should be ascribed to customer dishonesty.

Upon the acceptance of the baseline quote, the customer proceeds to the screening stage to be interviewed by the insurer's telephone operator, that is, an employee and representative of the insurer. In this stage, the customer is asked to reveal any additional disclosures of pre-existing medical conditions and risky lifestyle choices for underwriting purposes. The telephone operators do not see any of the disclosures that were provided at the quotation stage, other than the customer's name and date of birth that are used for identify confirmation purposes. But the customer does not know that the operator does not have access to the quotation disclosures, hence a revision of smoking rate may be perceived as an admission of lying. The operator begins the screening interview by seeking customer consent for recording the phone call, as follows:

Please note that your call today will be recorded and the recording may be used for verification and quality assurance purposes at a later time. Do you consent to having this call recorded?

This consent statement acts as a powerful reminder that any information disclosed as part of the interview can be used to verify disclosures and, if needed, to be used as evidence in future claims. Upon consent for recording the phone call, the telephone operator then plays the following duty of disclosure statement (DDS):

[here enter treatment statement if any] When we assess a claim we will rely on any information you have disclosed to us as part of your application. Where we have not verified information (for example your income) at the time of application, we reserve the right to verify it at the time of the claim. Before we continue, I need to remind you of your duty to answer the questions accurately and that you have a duty to disclose to us every matter that you know, or could reasonably be expected to know, is relevant to our assessment of your application, including any medical consultations or changes up until your application has been accepted or declined. Failure to comply with the duty may result in the policy being varied or voided. When you applied for cover, you declared that you had read and understood your duty of disclosure. Do you acknowledge your duty of disclosure?

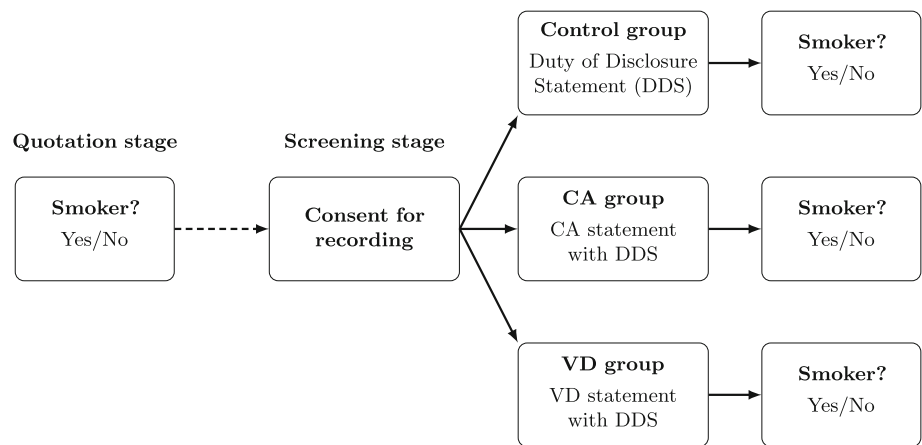
Our experimental intervention is recorded as part of this DDS, at its beginning. That is to say, during the experiment period, we play three pre-recorded DDSs. For the control group, the customers listen *only* to the default DDS as any customer would before the commencement of the experiment without any added treatment narrative. The two treatment groups listened to one of the two intervention statements followed by the DDS. The intervention statements and the DDS are pre-recorded together as a seamless statement. Furthermore, all three statements have been pre-recorded using the voice of the same young woman. The standardized nature of the recordings ensures that the effect of the interventions is not confounded by a telephone operator effect.

The CA treatment statement highlights the negative effects that individuals' dishonesty may have on other insurance customers through increased premiums in case of incorrectly provided information.⁷ This is meant to counteract the CA strategy, justifying dishonesty based on the argument that indicating false information in the screening process is not a crime since there is no true victim. Increasing identification with a victim, for example, by making a case study out of a hypothetical victim, can increase reluctance to impose harm (e.g., Gino et al., 2010; Yam & Reynolds, 2016). In a similar attempt to increase identification with a victim, the statement focuses on the harm to other policy holders instead of the faceless cooperation and

⁶In addition to lung cancer, the leading cause of cancer deaths, smoking is known to cause "age-related macular degeneration, diabetes, colorectal cancer, liver cancer, adverse health outcomes in cancer patients and survivors, tuberculosis, erectile dysfunction, orofacial clefts in infants, ectopic pregnancy, rheumatoid arthritis, inflammation, and impaired immune function" (HHS, 2014, p. iii).

⁷Compare to Köneke et al. ((2015), p. 311ff) who suggest to raise awareness of the solidary nature of insurance and the negative impact of unethical behavior on premiums of other policy holders, as well as on the insurer's employees through possible salary reductions or layoffs.

FIGURE 1 Experimental design: smoking disclosures pre and post intervention



emphasizes that the applicant and the potential victims are alike to trigger a feeling of group belonging. The exact wording of the CA statement is the following:

Please understand that we rely on you to provide us with correct information. Incorrect information may harm other people like you; it could lead to higher insurance prices for many, and some vulnerable people may no longer be able to afford insurance. To avoid this happening, we ask everyone to provide accurate and honest answers for all of the questions we will ask, including questions about your medical history, current medical conditions, and your habits. [followed by the DDS]

The VD treatment statement highlights the pro-social efforts of the insurer. It is intended to prevent individuals from applying the VD justification by providing information making the insurer appear in a more positive light, and thus being perceived as less deserving of harm.⁸ The exact wording of the VD statement is the following, where “---” indicates redacted information to protect the anonymity of the insurer:

--- is always trying to improve the lives of people in our community by helping the environment and caring for vulnerable people. We support the mentoring of young people through the --- foundation as well as --- and have embarked on a program with community-based --- to improve mental health outcomes. To keep up our good work we ask everyone to provide accurate and honest answers to our questions, including questions about your medical history, current medical conditions and your habits. [followed by the DDS]

Immediately after listening one of the three pre-recorded statements, the telephone operator asks the customer to declare the smoking status, using the following Yes/No question:

Nicotine products may include products that are smoked, but also those that are not smoked such as patches, gum or chewing tobacco. Have you smoked tobacco, e-cigarettes (vaping) or any other substance, or used nicotine products within the last 12 months?

Figure 1 summarizes the experimental design process and the points of data collection. The focus of our analysis is the net revision of smoking status. The revision rates are calculated as the difference between the binary responses on smoking status from the baseline quotation stage and the binary disclosures at the screening stage. We then compared the revision rates between the control group and the two treatment groups. Note that the standard DDS is purposefully designed by the insurer as a strongly worded intervention; hence, it is natural to expect that just by itself will elicit more honest revisions of smoking disclosures in the control group. The key research question is whether our treatment statements will increase honest revisions beyond the effect from the recording consent statement and the DDS.

Customers were not aware that they were participating in a research study because this could bias responses.⁹ If customers were made aware, then this could potentially lead to more socially desirable response biases, such as wanting to appear more honest.

4 | DATA AND RANDOMIZATION PROCEDURE

Our population of interest is adults (18+) who applied for life insurance coverage with a large insurer operating in the Oceania region.

⁸This is comparable to (Köneke et al., 2015, p. 314ff) who mention the importance of improving the image of the insurance industry, for example, through the explicit mention of exclusion clauses to increase understanding for payment refusals.

⁹The field experiment has been approved by the University of Sydney Human Research Ethics Committee (2017/822). Insurance applicants are legally required to be honest in their disclosures. Increasing disclosures amounts to helping customers meet their legally binding requirements.

TABLE 1 Randomization profile statistical summary

	Control group	Consequence Attenuation	Victim Deservingness	Pre-experiment sample
Monday <i>N</i>	248	280	271	476
Tuesday <i>N</i>	299	306	296	473
Wednesday <i>N</i>	291	289	294	512
Thursday <i>N</i>	231	290	272	566
Friday <i>N</i>	242	268	242	482
Total <i>N</i>	1311	1433	1357	2510
Female applicants	0.377 (0.013)	0.375 (0.013)	0.357 (0.013)	0.360 (0.010)
<i>p</i> -value		.91	.2804	.299
Occupational class	5.17 (0.085)	5.24 (0.082)	5.34 (0.085)	5.03 (0.062)
<i>p</i> -value		.601	.169	.162
Age insured	42.83 (0.256)	43.18 (0.250)	42.70 (0.255)	42.41 (0.180)
<i>p</i> -value		.337	.701	.175
Body mass index	26.58 (0.119)	26.73 (0.114)	26.73 (0.125)	26.56 (0.084)
<i>p</i> -value		.353	.362	.894
Log of sum insured	13.82 (0.040)	13.78 (0.039)	13.81 (0.040)	13.85 (0.030)
<i>p</i> -value		.445	.948	.569
Decile average				
Age insured	5.37 (0.080)	5.33 (0.076)	5.30 (0.079)	5.38 (0.057)
<i>p</i> -value		.711	.506	.952
Body mass index	5.45 (0.079)	5.45 (0.076)	5.46 (0.078)	5.46 (0.058)
<i>p</i> -value		.990	.962	.908
Log of sum insured	5.49 (0.080)	5.50 (0.076)	5.50 (0.078)	5.48 (0.058)
<i>p</i> -value		.948	.955	.951

Note: *N* is sample size. Risk factors are summarized using arithmetic means. Female applicants indicate proportions. Occupational class ranges from 1 to 11 and indicates the level of occupational hazard. Standard errors are reported in parentheses. *p*-values indicate two-sided two-sample proportion *z*-test or two-sample means *t* test between the control group and every other group (Consequence Attenuation, Victim Deservingness, and Pre-experiment sample); the null hypothesis is equality of proportions or equality of means. Decile average presents the means from the aggregate data that is published online; the aggregation from continuous to categorical data is necessary to eliminate the human ethics risk of re-identification.

The experiment was conducted during March-September 2019. We also have access to observational data before the commencement of the experiment from October 2018 to February 2019. We report the analysis of this data and the comparison between the within-experiment control group and the pre-experiment sample adds confidence that the characteristics of our population of interest remained the same during the experiment period. We make sure that during the experiment period, the scope and form of the smoking questions and the DDS remained unaltered.

Table 1 gives a statistical summary of the randomization profile. In the experiment period, we observed 1311 applicants in the control group, 1433 applicants in the CA experiment group, and 1357 applicants in the VD experiment group. In addition, we observed the revision rates of 2510 applicants before the commencement date of the field experiment, which we use to validate the stability of population characteristics in our experiment sample.

During the experiment period, we varied the two treatment interventions and the control group over cycles of 3 days. For example, the VA treatment was first run on Monday, the VD experiment on Tuesday, and the control group standard procedure on Wednesday, and then, we started the cycle again on Thursday. By the end of the experiment, there was an equal number of days across the three groups. As shown in Table 1, each group was represented by roughly the same weekday distribution of number of applications.

The sex proportions are about the same across the four groups, with about 36% to 37% female applicants. On average, age is about 42–43 years old, the body mass index is about 26.5, and the mean of log sum insured is around AU\$1,000,000 ($e^{13.82}$) across all groups. Occupational hazard is ranked from 1 to 11, with 1 being the safest, 10 the most hazardous, and 11 containing unclassified cases that require individual consideration but still highly risky. The average

TABLE 2 Smoking disclosures from quotation stage to screening stage

	Control group	Consequence Attenuation	Victim Deservingness	Pre-experiment sample
Quotation stage smoking rates	0.0496	0.0405	0.0545	0.0450
Binomial <i>p</i> -values		.954	.216	.865
Screening stage smoking revisions				
+ Non-smoker to smoker	0.0328	0.0391	0.0442	0.0359
– Smoker to non-smoker	–0.0031	–0.0021	–0.0007	–0.0056
Net revision smoking rate	0.0297	0.0370	0.0435	0.0303
Binomial <i>p</i> -values		.066	.003	.454
Screening stage smoking rates	0.0793	0.0775	0.0980	0.0753

Note: The quotation stage baseline disclosure rates are revealed to an independent advisor. The screening stage revised disclosure rates are revealed to the insurer's telephone operator. Some customers change their disclosure from non-smoker to smoker and some from smoker to non-smoker. Binomial test *p* values indicate binomial probability tests, with the null hypothesis that the revision rate on one of the treatments or in the pre-experiment sample is not greater than the revision rate in the control group.

occupational hazard rank is about 5 in all groups. To test for balance, we report two-sided tests of proportion equality and mean equality between the control group and every other group. The *p* values suggest that there is no statistically significant difference between customer characteristics and that the randomization process is statistically balanced.

The decile average reported at the end of Table 1 presents the means from the aggregate data in centile form for age insured, BMI, and sum insured. The aggregation from continuous to categorical form for these three variables is necessary for reporting purposes to eliminate the human ethics risk of re-identification. This form of centile data, together with the categories of occupational class and sex, is available from the authors.

5 | EXPERIMENT RESULTS

Table 2 reports our main results, with the smoking disclosure rates revealed at the quotation stage and the revised rates at the screening stage.¹⁰ In the control group, 2.97% of the customers who declare themselves as non-smokers in the quotation stage revise their disclosure to a smoking status in the screening stage. This revision rate is a net outcome from revising non-smoker status to smoker status (3.28%) and from smoker to non-smoker (0.31%).¹¹

We attribute the increase in disclosures from non-smoker to smoker status in the Control group to the success of the screening stage context. There are important differences that set apart the

quotation stage context from the screening stage context. In the quotation stage, the disclosures are made with the help of a friendly advisor, in a relaxed environment and no formal records of the conversation are held. In the screening stage, the customer is formally interviewed by an unknown representative of a corporate entity and is asked to provide explicit consent for recording and verifying the interview, plus to acknowledge the duty of disclosure that makes salient the potential consequences from redacting risk-relevant information. Indeed, the formality of the interview process at the screening stage, including the consent to record the phone call and consent to the DDS statement, is purposefully designed to elicit more disclosures.

The control group disclosure rates and revision rates are inline to those observed in the pre-experiment sample, with the binomial test and the proportion equality test confirming no statistical difference between the revision rates of the two groups. This is a reassuring result, which suggests that the effectiveness of the default screening stage design in eliciting more honest disclosures has not changed during the experiment period.

The CA treatment achieves a net 3.77% revision in smoking disclosures (27% increase in comparison to the control group) and the VD treatment a 4.35% increase (46% increase by comparison to the control group). The binomial tests and proportion equality tests suggest a significantly greater proportion for the VD treatment.¹² The effectiveness of the CA treatment is inconclusive. The VD treatment appears quite successful in eliciting even more honest disclosures in addition to the standard efforts of the insurer.

We attribute the lower effectiveness of the CA intervention compared to the VD intervention to two important reasons. First, justifying dishonesty with a large insurer's perceived own misbehavior may

¹⁰The choice of net revision rate as the target variable followed a detail exposition on the insurer's underwriting process, where we noted that the telephone operators performing the screening would always ask about the smoking status right after verifying the identity of the customer and receiving customer consent for the duty of disclosure (where our treatments were inserted). We also knew that smoking was disclosed in the quotation stage and that smoking is generally under-reported. This made the revision of smoking as the natural candidate target variable. This is the sole analysis that we considered that was directly inline with our research design methodology.

¹¹We are told that the handful of revisions from smoker to non-smoker may be attributed to transcription errors at the quotation stage, but one cannot exclude the possibility of some customers deciding to redact their smoking status in the screening stage.

¹²The binomial test is formulated as $Pr(k < k_{obs}) = \sum_{m=0}^{k_{obs}} \binom{N}{m} p^m (1-p)^{N-m}$, where N is the number of observations, k_{obs} the observed number of successes, and p the assumed probability of success on a randomized trial as reflected in the control group. The proportion equality test is asymptotically normally distributed and is calculate as $z = (\hat{p}_1 - \hat{p}_2) / s_{d_0}$, where $s_{d_0} = \sqrt{\hat{p}_1 \hat{q}_1 (1/n_1 + 1/n_2)}$ is the standard error of $\hat{p}_1 - \hat{p}_2$ under the null hypothesis of $p_1 = p_2$.

have been the more prevalent justification strategy at that time, thus lending more scope for dishonesty to be counteracted by our VD intervention. Second, the CA intervention has an implicit emphasis on mistrust lowering cooperation with implications on unkind reciprocity (Falk & Fischbacher, 2006). The VD intervention focused more on the good deeds of the insurer and worked towards improving the public image. Honesty was requested as a means of keeping up the insurer's good work. The CA intervention instead highlighted the bad consequences for others of incorrectly provided information and requested honesty to prevent those consequences from materializing. While the implicit signal of mistrust may have triggered dishonesty by itself, the combination with the standard compliance statement which also refers to the negative consequences of being dishonest (for the applicant) may have reinforced that signal.

5.1 | Context and significance

We faced a challenging context in our attempt to elicit greater honesty in disclosures. Our interventions compete with large financial incentives that incentivize dishonesty, as the redaction of smoking disclosures increases premiums that could make the product unaffordable for some, and also risks exclusion of cover for claims related to smoking. Indeed, the final tally of smoking disclosures remains far lower than the expected population prevalence rates of 15% to 16%, even without accounting for adverse selection (see Table 2).

Another major obstacle is that our intervention aimed to encourage life insurance customers to revise their smoking status that had previously been falsely disclosed. It is important to note that the customer was not made aware that the telephone operator was not informed of the disclosures made at the quotation stage. Therefore, a revision of the anchored information may have been perceived as an admission of dishonesty, which some individuals would be reluctant to make irrespective of the type of intervention applied.

Moreover, the experiment happened to take place at an unpropitious time, immediately following the conclusion of the Australian Financial Services Royal Commission (2019), and the release of the Financial Markets Authority and Reserve Bank of New Zealand (2019)

joint report. Both inquiries presented a damning case about the misconduct of insurance companies, with considerable evidence on products designed and sold without consideration of good customer outcomes in mind, with systemic complacency in identifying and dealing with vulnerable customers. The negative news surrounding the release of these reports may have acted as further justification for dishonest strategies. We believe that this also explains the lower effectiveness of the CA treatment, which points at the potentially negative effects that dishonesty may have for some vulnerable people, which could have been perceived as hypocritical. On the other hand, the VD treatment restored some pro-social goodwill on behalf of the insurer.

Finally, the treatment effects that we seek to measure are small. Figure 2 presents the power analysis for testing the difference between the smoking revision rate of each treatment group and the control group. Although for lab-based experimental studies our sample size would be considered large, here compared to the base rate 2.97% of the control group, we would need roughly a 2.1 percentage point increase to detect significance at the 5% level with 80% power. In relative terms, this means that we would need almost a 70% relative increase (2.1/2.97) which is a lot to ask from a field intervention in such a challenging setting. Another way of looking at this problem is the following. To significantly detect the effect sizes of 0.008 (0.0377–0.0297) in the CA intervention and 0.0138 (0.0435–0.0297) in the VD intervention (the differences in the revision rates relative to the control group), we would need to observe 15,974 and 5812 customer applications for 80% statistical power, respectively.

As explained by DellaVigna and Linos (2020), it is not unusual for field experiments to have a small effect. To make this point, they demonstrate how this is the norm in field RCTs conducted by government nudge units. The main difference between academic publications and a nudge unit is that the latter has the opportunity of conducting interventions in a much larger scale, thus achieving statistical power in detecting small effects. We were not afforded the luxury of a large-scale RCT by the commercial partner, who limited our involvement in their business processes to testing only two interventions for a limited period of time.



FIGURE 2 Power curves for a two-sample proportion test. Note: The test is for Pearson's χ^2 two-sample proportion test at the significance level $\alpha = .05$. The sample sizes and observed proportions are reported in Tables 1 and 2

6 | CONCLUSION

We experimentally investigated the effect of two interventions aiming to counteract the strategies individuals apply to justify dishonesty in disclosures for life insurance applications. Our treatments were simple information statements included as part of the DDS in the insurance screening process. This allowed for verification and potential adjustment of the information provided in the quotation stage. One statement focuses on the negative externalities of dishonesty in disclosures on other insurance holders through increased premiums, while the other statement highlights the pro-social deeds of the insurer to counteract negative reciprocity or trigger positive reciprocity, respectively.

Both interventions lead to a nominal decrease in dishonesty; however, the effectiveness of the CA treatment is inconclusive. Only the VD treatment produces a statistically significant effect. Given the scale of the published negative information about insurers shortly before the experiment was conducted, it is encouraging to see that a pro-social message seems to be able to help restore customer trust. Our interventions however have an important financial impact, resulting in a fairer distribution of insurance premiums among policy holders.

For future research it would be interesting to dig deeper into whether and how the effectiveness of the interventions differs across certain types of individuals. If the application of particular justification strategies is dependent on individual characteristics, interventions counteracting the strategy that is most likely applied may be more effective in reducing dishonesty. Ultimately, the insurer could personalize the intervention statements and thus maximize their effectiveness. Furthermore, while we assumed that mentioning the negative impact of dishonesty on others would be more powerful than highlighting the positive impact in case of honesty, the presumably negative effect of the concomitant signal of mistrust may be of more importance. A follow-up study could investigate how the effectiveness of a CA intervention focusing on everyone being able to afford insurance if correct information was provided would compare to the statement we tested.

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DATA AVAILABILITY STATEMENT

Summary data are available on request due to privacy/ethical restrictions.

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