# Playing the victim behavior: an experimental study

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**Abstract**

This paper experimentally explores playing the victim behavior, how prevalent it is, its determinants, and potential mechanisms to mitigate it with a subject pool from two regions (UAE and North America). The possibility of playing the victim is introduced by letting some participants receive a negative shock to their initial endowments, after which they can apply for extra compensation even when they do not receive the shock. We find that the majority of participants play the victim. We then test whether defaults and signing an honesty oath influence this behavior. We find, contrary to intuitions, that the omission treatments, where lying is a default, failed to increase misrepresentation, and if anything decreased it, while the oath substantially reduced it as expected. Moreover, the extent of pro-sociality and perceived social norms are found to be strongly related to playing the victim behavior. The findings are very similar across regions. Our findings offer some insights to design better policies to support victims, especially during crises such as the Covid-19 pandemic.

Keywords: Playing the victim, dishonesty, defaults, honesty pledges
JEL Classification: C9

## 1 Introduction

During crisis times, allocating resources efficiently -e.g., compensating people who are in need the most- can be very difficult. Correctly identifying and/or verifying the victims is one of the most

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critical factors in improving this efficiency. Imperfect verifiability and identifiability of victims open the door for manipulation of the allocation system. Some agents may take advantage of this inability to trick the system and make a profit out of it by playing the victim. In this paper, we investigate the prevalence, potential determinants, and some mitigation mechanisms of this behavior.

Consider Covid-19, one of the largest global crises the world has faced. Governments announced huge fiscal packages and bailout/support programs that targeted the firms and individuals who were potentially victimized by the pandemic. For example, the US government introduced a $4 trillion bailout plan that is considered to be necessary and timely, and due to the sensitivity and urgency of the situation, correctly identifying and/or verifying the victims was not a top priority. However, there is overwhelming evidence that it produced a lot of fraud. More than half of the funds went to businesses ($2.3 trillion), most of which were not required to prove that they need funds or they kept paying employees. More than $0.6 trillion went to firms as tax breaks that laid-off workers. Only about a fifth was spent on individuals who are assumed to be negatively affected by the pandemic (The Washington Post, 2020). Specifically, about 15% of the loans representing $76 billion distributed under the Paycheck Protection Program (PPP) -the $800 billion small-business rescue plan- were found questionable (Griffin et al., 2022; NYT, 2021). Some alleged fraudsters purchased Lamborghinis and yachts with PPP money according to the Justice Department (Project on Government Oversight, 2020). It has been estimated that about 55.000 businesses were ineligible for the plan and hundreds of individuals were accused of gaming the program (ProPublica, 2021). There were many similar incidences during and after the 2008 global financial crisis, for example, firms that received taxpayer bailouts paid incredible amounts of bonuses to their CEOs (NYT, 2013). These resource allocation failures were caused not only by the flawed identification/verification mechanisms of the real victims but also by malevolent people/organizations who try to take advantage of these flawed mechanisms by playing the victim. These examples, many anecdotal evidence, and the estimated large extent of it vividly show the importance of understanding playing the victim behavior, its determinants, and possible mitigation mechanisms.

In this paper, we examine whether and to what extent we observe playing the victim behavior with an online experiment. By collecting data from two different regions (United Arab Emirates-UAE vs. United States/Canada), we investigate the determinants of this behavior and employ different potential efficiency-enhancing mechanisms.[1] We aim to contribute to the limited social fraud literature. Our paper's contribution is three-fold: first, while there are some empirical and experimental studies on other types of fraudulent behavior, playing the victim behavior is not scrutinized enough in the literature. Second, we investigate whether the mechanisms used in real life are effective (commission and omission) and whether an honesty pledge that is shown to be

[1]Here, efficiency means that the ones who are negatively affected apply for compensation and get it, and the ones who are not affected negatively do not apply for it or even if they apply, they are not compensated.

effective in other contexts mitigates this behavior. Finally, we conduct a cross-cultural study by comparing data collected from the UAE and US/Canada, which is also missing in the literature.

To this end, we conducted an online experiment to mimic real-life situations where some people are affected by a negative shock, and there is a mechanism to compensate supposedly the real "victims". However, there might be people who may want to take advantage of the situation and claim undeserved benefits although they are not affected by the shock and are not the target of the compensation scheme. In the experiment, first, participants are randomly assigned an initial endowment (high or low, with unequal probabilities). Then, a limited number of participants randomly get a negative shock to their initial endowments, after which they can apply for extra compensation. By design, all can apply for the compensation, creating the possibility of playing the victim, by applying for extra compensation despite not getting the negative shock. Our treatment variable is how the application page is presented. In the first case, an active response is needed where subjects actively choose whether to apply or not. This is called "*commission*" in which there is no suggested default option in either way (towards an honest or dishonest act). Another treatment involves a passive response where subjects are automatically counted as applied for compensation if they do nothing. This is called "*omission*" in which there is a default option for the dishonest act and we have two versions of the omission case. Finally, the mechanism introduced to potentially reduce this type of behavior is taking "*an honesty pledge/oath*" just before making the application decision.

Our analysis clearly shows that playing the victim behavior is very common and the majority of the participants play the victim. The omission treatments unexpectedly failed to increase playing the victim behavior, and if anything decreased it. The honesty pledge considerably reduced playing the victim behavior as observed in other contexts. The degree of prosociality (measured by a dictator game) and beliefs about social norms (what percentage of others applied for compensation) emerged as important factors influencing playing the victim behavior. Finally, the behavior of participants in our samples turned out to be very similar across regions.

The paper proceeds as follows. Section 2 provides a framework and summarizes the related literature and hypotheses. Section 3 provides the details of the experimental design and procedures. Section 4 presents the results. Section 5 concludes with a discussion of the results.

## 2 Framework, related literature and hypotheses

Fraud -a deliberate act of one party attempting to deceive another to gain undeserved benefits- is a ubiquitous and significant problem. Although it is a multi-faceted and complex phenomenon, its emergence can be attributed to four elements that are common to almost all of its forms ("fraud diamond" by Wolfe and Hermanson, 2014). The first element is *incentives* which are the motives behind committing fraud. It can be not only pure-profit seeking, but also a sense of entitlement, prosociality, resentment, etc. The other element that goes hand in hand with incentives

is *rationalization (justification)*. People have advanced abilities to justify their wrongdoings such as rational choice (e.g., "*It is not risky, rational to do it.*"), social proof (e.g., "*Everybody is doing it.*"), deserving (e.g., "*This is my right, I deserve it.*"), vengeance (e.g., "*They deserve it, so, we get even.*"), being needy (e.g., "*I need to do this.*"), pressure (e.g., "*I am forced to do this.*"), etc. The third element that paves the way to commit fraud is *opportunity*. No system is perfect and there are inherent weaknesses that can be exploited. Weak controls and oversight increase the obviousness of existing opportunities but some opportunities can be created too. Without necessary traits and abilities, an opportunity cannot be recognized or turned into reality, which refers to the fourth element, *capability*. An obvious enough opportunity opens the door for fraud, incentives and rationalization can draw the person toward it, and the ones with enough capability commit it. However, even if opportunities are not obvious, a capable person can recognize and navigate his/her way to exploit them.

There are many different forms of fraudulent behavior such as *insurance claim fraud* (Tennyson, 1997; 2008), *unemployment benefits fraud* (Meyer et al., 1995; Hessing et al., 1993), *social security benefits fraud* (Dean and Melrose, 1996), *credence good fraud* (Balafoutas and Kerschbamer, 2020), *embezzlement* (Attanasi et al., 2019), *corporate fraud* (Dyck et al., 2021), *charity fraud* (Cordery et al., 2011), *mortgage fraud* (Jiang et al., 2014) etc. Some types of fraud arise in the existence of contracts that define the terms of compensation of losses in some circumstances. These can either be formal (e.g., unemployment insurance and state benefits) or informal (e.g., emergency aid programs or charitable aids) contracts between individuals/firms and firms/NGOs/government. These contracts are triggered by individual (e.g., car accident, work accident, dismission, etc.) or social crises (e.g., Covid-19, earthquake, etc.) in which people who are victimized by the crises are compensated. However, when these crises occur, they create not only their victims but also opportunities that can be exploited. Playing the victim behavior is a form of fraud that is observed when these opportunities created by personal or social crises are exploited by "fake victims".

Playing the victim behavior is somewhat different from other types of fraud both situationally and psychologically. First and foremost, the opportunity is exogenously created by a personal or social crisis situation. As opposed to other cases such as corporate fraud, procurement fraud or embezzlement that are usually observed as a result of deliberate planning or even without any crisis situation, the opportunities are considerably obvious, which essentially influence the other elements of fraudulent behavior. Profit seekers do not necessarily have to be very capable to be able to exploit the situation. They can also rationalize their fraudulent activity relatively easily: the emergency of the situation or relatively small size of the transgression may lead to low risk that makes it rational; even people who are unaffected by the crises can easily argue that they are indirectly victimized by the crises; the ones who once were victims can argue that they are still victims (e.g., unemployment insurance); depending on how the benefactor (e.g., government, insurance company etc.) is perceived, one can consider own transgression as benefiting from a public resource that

will not impose a large negative externality on the benefactor or other beneficiaries. However, since people are less willing to hurt known and real individuals than unknown and probabilistic individuals (Small and Loewenstein, 2003), the existence of real (not abstract or probabilistic) victims who are easily noticeable and clear target of the resources (e.g., Covid19/earthquake victim funds rather than general government fund) can impede justification.

Pretending to be a victim may involve a spectrum of behavior from the complete fabrication of losses to small exaggerations, breaches, or ignorances. For example, one may create a ghost firm and apply for the state aid targeting Covid-19 victims or a real business truly affected by Covid-19 may exaggerate the losses a little. One may try to get a welfare benefit with a fake ID or an unemployed person may not mention his cash-paying casual job to the benefit office only once. Since playing the victim is a surreptitious activity, its true extent is not trivial to estimate. Only big fraudsters are traced and caught, and only they are recognized due to high sensationalization in the media, but small fraudulent activities are reasonably more prevalent and more likely to go unnoticed. The well-established finding in the dishonesty literature, which shows that most people lie when they have a chance but not to the full extent (Mazar et al., 2008) also supports this contention.

In the fraud literature, playing the victim behavior -in general- is not scrutinized enough although there are some empirical and experimental studies specifically about insurance, unemployment benefits and social security benefits fraud that can be considered as playing the victim behavior. Now, the existing research is summarized and hypotheses are formulated. This paper ties into several strands of the literature, the first and foremost of which is the dishonesty and claiming undeserved benefits literature because playing the victim behavior is a dishonest act and involves undeserved benefits claims. Second, it is related to the default setting in the behavioral economics literature that investigates whether and how presented default options affect people's behavior. Third, the honesty pledge is proved to be effective in other contexts in the literature, and we use it as a potential mechanism to mitigate this behavior. Last but not least, our paper investigates this behavior in a cross-cultural context that is scarce in the literature.

***Dishonesty and claiming undeserved benefits:*** Economists have argued for a long time that engaging in unethical acts is a matter of marginal analysis (Becker, 1968) where expected marginal benefits from dishonesty are compared to its expected marginal costs (e.g., punishment). Although this approach is helpful, it has proven to be inadequate in fully explaining the empirical and experimental observations (Irlenbusch and Villeval, 2015). Evidently, *"dishonesty stems from a complex interplay of motivations and circumstances, moderated by morality, social norms, and institutional context."* (Tenysson, 2008). Since fraud -a deliberate deception to gain benefits undeservingly- is a dishonest act, the same factors driving dishonesty can be expected to drive fraudulent behavior as well.

There is overwhelming anecdotal evidence implying that playing the victim behavior exists in all types of formal and informal contractual relationships. However, its prevalence is hard to measure

due to its clandestine nature, and existing estimates substantially vary. Insurance and medicare fraud are estimated to cost at least $80 and $60 billion every year in the US, respectively. Fraud occurs in about 10% of property-casualty insurance losses (insurancefraud.org). One estimate of the direct costs of fraudulent property and liability insurance claims is 8% of the annual premium revenue of the industry. Caron and Dionne (1997) estimate that 10% of claims in the Quebec automobile insurance market are fraudulent. Regarding social security benefits, it is estimated that approximately 10% of the claims are fraudulent (Dean and Melrose, 1996). One of the most serious countries in measuring unemployment insurance fraud is the UK and the formal figure for fraudulent claims is only 0.7%. However, for similar EU countries, surveys suggest this figure from 20% to 30% (Van Stolk et al., 2006). Meyer et al. (1995) find that an increase in unemployment benefits in two US states increased the duration of workers' compensation claims by 20%. Figures about fraud in Covid-19 benefits were mentioned in the previous part.

Many factors are found to be related to fraudulent behavior and playing the victim behavior. Social norms are one of the main determinants of behavior in general. Dishonest/fraudulent behavior is not an exception. Many studies conclude that dishonesty is heavily affected by others' behavior and is somewhat contagious (Akin, 2019; Gino et al., 2009; Innes and Mitra, 2013; Lefebvre et al., 2015). Although it is different from playing the victim behavior, tax evasion behavior being heavily affected by the associated reference group (peers, neighborhood, state, etc.) is a well-established finding (Alm et al., 1999; Myles and Naylor, 1996). The social and cultural environment is also an important determinant of individuals' attitudes toward insurance fraud. Cummins and Tennyson (1996), Tennyson (1997), and Tennyson (2008) empirically show that auto insurance claim fraud significantly depends on attitudes toward claim exaggerations of others. In the context of playing the victim behavior, the probability of playing the victim behavior can be expected to be correlated with perceived social norms about dishonesty in the reference group.

Neither all lies (small vs. big) nor all liars are the same. People have different levels of attachment to their *moral principles*, and their *general attitudes towards dishonesty* vary largely (Jacquemet et al., 2020). They have an intrinsic commitment to doing the right thing, a desire for a positive self-image, and internal consistency (Mazar et al., 2008). Thus, one's own attitude toward dishonesty is potentially another determinant of playing the victim behavior. Tennyson (1997) shows that one's attitude toward tax evasion (used as a proxy for preference for honesty in general) is positively and significantly related to attitudes toward filing exaggerated claims. We also used participants' attitudes toward tax evasion as a proxy for the preference for honesty in our study.

Some studies find that *perception of institutions* that people engage in contractual relationships with influences people's attitudes toward fraud (Christodoulou et al., 2022; Tennyson, 1997). People holding beliefs that they are treated unfairly by the authorities are more likely to evade taxes to restore equity (Spicer and Becker, 1980). Justified disobedience based on beliefs about the

system being unfair and the government being corrupt is one of the main reasons that people express for their fraudulent behavior (Dean and Melrose, 1996; Spicer and Becker, 1980). Moreover, experiencing a norm violation that leads to a feeling of unfair treatment is sufficient to justify the violation of another norm at the expense of a third party (Houser et al. 2012). These potentially imply that an individual trusting in procedural and distributional fairness of the institution in which he/she engages would be less likely to play the victim. Thus, we analyze the participants' perceptions towards both the researcher and the government through survey questions and control for them in our analysis.

Since playing the victim behavior is a dishonest act at the expense of others, people's *social value orientation* can be expected to influence its extent. It has been shown that people not only care about their benefit from lying but also are sensitive to how harmful it is to others and are less prone to lie when it hurts others (Christodoulou et al., 2022; Gneezy, 2005; Hurkens and Kartik, 2009). In our context, we repeatedly emphasize that the target of the compensation scheme is the real victims and over-application by lying creates a negative externality on others, thereby hurting others (selfish lying or black lies, Jacobsen et al., 2018). Due to this relationship between social preferences and lying behavior (Irlenbusch and Villeval, 2015; Maggian and Villeval, 2016), we measure participants' prosocial behavior by using a classical dictator game to be able to relate it to their playing the victim behavior.[2]

The closest study to ours is Lefebvre et al. (2013) which experimentally investigates the determinants of both tax evasion and social fraud in the Netherlands, France, and the two main regions of Belgium. They design the social fraud as an undeclared activity while receiving unemployment benefits and find that the social fraud rate (app. 45%) is higher than the tax fraud rate (app. 39%). The social fraud rate is not significantly different across the four regions. Our contribution is different from Lefebvre et al. (2013) in at least two dimensions. We test playing the victim behavior with a larger sample in a more general context, not restricted to unemployment benefits, and we recruited participants from two quite different cultures.

Based on the mentioned findings in the existing studies about dishonesty and claiming undeserved benefits, we expect to observe playing the victim behavior in our data, but we do not have a specific hypothesis about its prevalence. We also expect the mentioned factors to be influential on playing the victim behavior in a similar way but this analysis is essentially exploratory.

***Default setting:*** Setting default options is powerful in affecting people's behavior. Governments, companies, and other organizations set default options to influence/nudge behavior towards the desired outcomes. The effect of default settings is shown in many contexts such as energy consumption (Liebe et al., 2021), organ donations (Johnson and Goldstein, 2003), savings (Choi et al., 2003), and charitable donations (Altmann et al., 2019). However, there are few studies

[2]There are other factors that are shown to influence dishonesty such as *inequity aversion* (Gino and Pierce, 2009), *religiosity* (Shalvi and Leiser, 2013), *educational background* (Jacobsen et al., 2018), and *gender* (Lefebvre et al., 2013).

investigating the effects of defaults in influencing people's dishonesty.

Mazar and Hawkins (2015) manipulate whether participants are presented with correct or incorrect answers as default and show that passively accepting an incorrect default answer (omission) leads to a higher degree of cheating than the cases where the default has to be overridden to cheat. Moreover, individuals cheat less when it requires overriding a default correct answer (super-commission) than when simply giving an incorrect answer (commission) despite the equivalent physical effort. Fosgaard (2019) presents high, low, no, or expected mean defaults by using a representative sample and found that high defaults increase cheating, but low defaults do not lead to any difference relative to no default or the expected mean default. However, students reacted differently and did not increase their answers with high defaults, but they did when the mean as a default answer is presented.[3]

By using similar default settings, we contribute to the literature on the effects of defaults on people's dishonesty (omission vs. commission), specifically, playing the victim behavior. Based on these findings, we test the following hypothesis:

**H1.** Individuals are more likely to play the victim by omission than commission because of no effort requirement.

***Honesty pledges:*** Oath-taking or honesty pledges are used commonly in many situations involving contractual relationships. For example, governments ask applicants to declare that all provided information is accurate, and private firms (for example, insurance companies) also ask for taking oaths to provide honest declarations (and, of course, the testimony mechanism that is a very important part of all judicial systems relies on taking an oath before testifying).

There is extensive literature on whether and how honesty pledges work in mitigating dishonest acts. It has been repeatedly shown that they are effective in reducing lying and cheating behavior. Jacquemet et al. (2019) show that oath decreases lying significantly in a loaded environment (explicitly calling lies). Jacquemet et al. (2020) show that only partial liars react positively to oaths, but Heinicke et al. (2019) show that the effect is strongest for extreme liars. Beck (2021) finds that subjects who take decisions fast tend to tell the truth more under oath. Peer and Feldman (2021) show that honesty pledges reduce dishonesty significantly and when it is combined with fines, it is more effective. One neutral result is that oaths do not reduce dishonesty on a group level (Krüger and Van Geen, 2016).

Since playing the victim is a dishonest act and oath-taking is shown to be overwhelmingly successful in reducing dishonesty, we have the following hypothesis:

**H2.** Honesty pledge will reduce playing the victim behavior.

***Cross-cultural studies in the dishonesty context:*** Cross-cultural studies are important to be able to make healthy generalizations of insights learned from specific subject pools, and to see the effects of cultural dynamics on the relevant behavior. To the best of our knowledge, there

[3]See Van Dijk et al. (2020) and Fochmann et al. (2021) for the effect of defaults on tax compliance.

is no cross-cultural study in regards to playing the victim behavior except Lefebvre et al. (2013) which investigates unemployment benefits fraud in countries with similar cultures. However, there are studies in the dishonesty context done in different countries with different subject pools such as academic dishonesty (Rawwas et al., 2004) and tax compliance (Alm et al., 1995; Cummings et al., 2009) showing pervasive dishonesty and substantial diversity around the world.

Pascual-Ezama et al. (2015), by using a coin-flip task, investigate the prevalence of dishonesty among university students in 16 different countries and find that despite some variation, behavior is quite similar across countries and a limited degree of dishonesty is observed. They also unexpectedly find that countries' corruption indices and cheating levels are not correlated. On the contrary, Gächter and Schulz (2016) find a correlation between high levels of rule violations in society and more dishonest behavior in 23 countries. Regarding the mediating factors driving differences across cultures, Mazar and Aggarwal (2011) suggest that collectivist values promote bribery.

There are a number of studies done in the United Arab Emirates on academic dishonesty (for example, Aljurf et al., 2020). However, there is no study investigating dishonesty in other contexts. Also, there is no study comparing behavior across western countries and UAE (except Williams et al., 2014). In this sense, our study contributes to the literature on cross-cultural comparison in the context of dishonesty.

# 3 Experimental Design and Procedures

## 3.1 Experimental Design

Table 1 summarizes the design that includes three main stages (2, 3, and 4) out of the total of five stages. In the first stage, participants play the dictator game in the role of the dictator and decide how much out of 10 (UAE dirhams or US dollars)[4] to give to another randomly selected participant. The initial endowment is determined in the second stage.[5] They toss a coin four times on the screen and count how many heads they see (the order of heads does not matter). If they get 0 (zero), 1 (one), 2 (two), or 3 (three) heads, their initial endowment becomes 25 dollars (with 0.9375 probability). If they get 4 (four) heads, their initial endowment becomes 40 dollars (with 0.0625 probability). The subjects are shown a table to clearly explain the initial endowment determination.

The third stage involves a possible negative shock to the initial endowment. Participants toss a coin four times again on the screen and based on the number of heads they get, their initial endowment is either reduced by 15 Dollars (negative change) or is not changed (no change). If they

[4]We make the payments in Dirhams in the sessions including only university students in UAE and US Dollars in the sessions including only university students from US and Canada. From now on, we use Dollars not to repeat both Dirhams and Dollars every time and drop the money unit sometimes for brevity.

[5]The reason for introducing randomization in the second stage, instead of assigning everyone the same endowment, is to create heterogeneity among subjects to allow them an opportunity to mimic higher endowment owners.

get 0 (zero), 1 (one), 2 (two), or 3 (three) heads, this means that they do not receive any shock *(no change in initial endowment)*. If they get 4 (four) heads, their initial endowment is reduced by 15 dollars *(negative change in initial endowment)*. The subjects are shown a table to explain how the shock to the initial endowment works.

Table 1: Experimental design

| STAGE 1 | STAGE 2 | | STAGE 3 | | | STAGE 4 | | | STAGE 5 |
|---|---|---|---|---|---|---|---|---|---|
| DIVISION GAME | RANDOMIZATION1 # OF HEADS FROM 4 COIN TOSSES | INITIAL ENDOWMENT | RANDOMIZATION2 # OF HEADS FROM 4 COIN TOSSES | SHOCK | ACTUAL FINAL ENDOWMENT | REPORTED FINAL ENDOWMENT** | APPLICATION *(for extra compensation)* YES / TYPE* | NO / TYPE* | SURVEY |
| Participants play the dictator game in the role of dictator and decide how much out of 10 (AED/$) to give to another randomly selected participant. | 0, 1, 2, 3 (%93.75) | 25 | 4 (%6.25) | YES | 10 (%5.75) | 10 | HONEST | HONESTALTRUIST | Participants fill out a survey including demographics and some other questions. |
| | | | | | | 25 | ENDOWMENTLIAR | SHOCKLIAR | |
| | | | | | | 40 | SHOCKLIAR and ENDOWMENTLIAR | | |
| | | | 0, 1, 2, 3 (%93.75) | NO | 25 (%88) | 25 | PLAYINGVICTIM | HONEST | |
| | | | | | | 40 | ENDOWMENTLIAR | | |
| | 4 (%6.25)*** | 40 | 4 (%6.25) | YES | 25 (%0.25) | 25 | HONEST | HONESTALTRUIST | |
| | | | | | | 40 | SHOCKLIAR | | |
| | | | 0, 1, 2, 3 (%93.75) | NO | 40 (%6) | 40 | HONEST | | |

* Possible Types: 1. Endowmentliar: Person who over-reports his initial endowment (endowment is 25, but the report is 40). 2. Shockliar: Person who gets the shock but reports no shock to his/her advantage. 3. Playingvictim: Person who does not receive the shock but reports the shock. This person is playing the victim. 4. Honest: Person who reports the final endowment and shock truthfully. 5. Honestaltruist: Person who gets the shock but reports no shock to his/her disadvantage. This person can be considered as honest and altruist.

** When 40 is reported, application is not possible. So, to classify the ones who report 40, we take the final endowment as the comparison point.

*** All percentages represent theoretical distributions.

Stage four includes participants reporting their final endowment (as a result of the second and the third stage) and their application decision for compensation. Since their initial endowments can be either 25 or 40 and they either receive a negative shock or they do not, the reported final endowments can be either 10 (initial endowment is 25 and it is reduced), or 25 (initial endowment is 25 and it remains the same or initial endowment is 40 and it is reduced), or 40 (initial endowment is 40 and it remains the same). We expect that no one would report lower than their actual final endowment (but some did). There is an opportunity to lie in this part by over-reporting the final endowment. After they report their final endowments, they can apply for extra compensation due to possible negative shock in the second stage only if they report 10 or 25. If they report 40, they cannot apply because this report means that their initial endowment is 40 and they got no shock. The following is mentioned a couple of times before they make their application decisions: *"Target of this compensation is the ones whose endowments were reduced in the third stage (negative change), but all participants (except the ones who reported their final endowments as $40) can apply and get a chance to earn extra compensation amount that is the same as that of the negative change, $15. If only the participants whose endowments were reduced apply, they will be compensated for sure. If there is an over-application, we will randomly choose whom to compensate without any verification. This means that in case of over-application, some participants whose endowments are actually reduced may not be compensated."* This is reminded again after they report their final endowment and before their application decision. Based on the reported final endowment and application decision, subjects are categorized into five different types based on their dishonesty as

shown in Table 1.[6]

Our treatment variable is how the application decision page is presented (see appendix E for the screenshots). We have the following conditions: Commission, omission (x2), and honesty pledge.

**Commission (Com):** Participants have to choose between applying and not applying for compensation. Applying, in this case, implicitly means that these applicants got a negative shock and want to be compensated. But they can potentially lie and apply even if they did not receive the negative shock. Participants must make an active choice in this condition (either "I **want to apply** for extra compensation" or "I **do not want to apply** for extra compensation").

**Omission1 (Omi1):** In this condition, application is the default option and requires passive acceptance. If participants do not want to apply for compensation, they have to check a box. They read the following script: *"If you do not do anything, you will be automatically counted as applying for the compensation. Check the box below only if you do not want to apply for the compensation."* Then, they either click the next button directly (apply) or check the box and click the next button (do not apply). Thus, they do not have to do anything to apply but we make it salient what "doing nothing" implies.

**Omission2 (Omi2):** The second omission condition[7] is the same as the commission condition except that from the two options ("I **want to apply** for extra compensation" and "I **do not want to apply** for extra compensation"), the application option is chosen by default and the page automatically advances in 5 seconds.[8] One should just wait if he/she gets a negative shock and wants to apply (honest) or gets no shock and wants to take advantage of the application (playing the victim). Being truthful requires overriding the default (if one does not want to apply, he/she needs to change the default answer). For participants who got no shock but want to take advantage of the application, this omission condition facilitates turning their intention to action because this requires omission (passively accepting the default) and it also makes it much easier to rationalize their dishonest behavior (e.g., "it is not me who chose it, it is the computer; or I did not have enough time to change the default").

**Honesty pledge (Oath):** This condition is the same as the commission condition, but before subjects make their choices between applying and not applying, they read and sign a pledge with their initials in which they promise to be honest in their application decision. They made their application decision after they read and sign the following script with their initials:[9]

[6] See Pascual-Ezama et al. (2020) for classification of dishonest behavior.

[7] This treatment was run on Prolific only. All other treatments were run both on Prolific and online in the UAE.

[8] We determined the 5 seconds based on the behavior of subjects in the commission treatment where mean and median page submission times were 4.6 and 2.8 seconds. Thus, 5 seconds was enough for the vast majority of the subjects to decide. Moreover, in the omission2 condition, subjects had the chance to go back to the decision page after it advances in 5 seconds. We observed that nobody returned to the decision page, the vast majority submitted page themselves before 5 seconds, and the average page submission time was 3.11 seconds. These clearly show that the observed behavior in the omission2 condition were not likely due to participants having inadequate time to respond.

[9] If they leave the signature box empty, they cannot proceed. The program did not check what subjects wrote but

*"**I promise that I will only apply if I get a negative change (my endowment is reduced) and that I will not apply if I get no change (my endowment remains the same).** I know that the compensation will be added to my earnings based on my application decision and hence I will take it very seriously to be accurate in my application decision."*

*I declare that I agree with the above statement.*

*(Below, write your initials -the first letter of your name and the first letter of your surname. Do not write your name or surname.)"*

In the last stage, we ask some demographic questions and some other survey questions, and present some scenarios (Appendix B).

There are some important points about the design. There is no deception during the experiment. The coin tosses are implemented by Qualtrics and the initial endowments and shocks are determined based on the actual coin toss results. That is why the number of participants based on their initial endowment and whether they receive the shock are not the same as what we theoretically expect (see table 3 for the theoretical and actual distributions). Since we can trace the coin tosses, we can trace subjects and know their initial endowments and whether they receive the negative shock or not (we could not and did not trace and match participants' identities and their decisions). We follow them anonymously which is necessary to match their behavior and other information about them (demographics, survey, etc.).

We asked participants to report their final endowments instead of their initial endowments and they get paid their final reported endowments for sure if randomly chosen. Knowing this, they decide to apply for extra compensation, and the compensation is added to their earnings if they are randomly chosen. This is to mimic the real-life situations where playing the victim behavior is observed because when the potential victims are compensated, their true states (whether they are real victims or not) is usually not known (imperfect identifiability). However, this leads to the possibility that they can lie about both their initial endowment and whether they receive a shock (i.e, even if one's initial endowment is (10) 25, she can report (25 or 40) 40, or even if one does not receive a shock, she can apply for it, or both). Table 1 explains the possible types that can be observed. This leads to some attrition in the sampling procedure but the number of these participants who inflated their final endowment is not high, only about 8-10% (e.g., their real final endowment were either 10 or 25, but they overreported).

We also mentioned in the instructions that we will randomly choose whom to pay the extra compensation among the ones who applied for it without any verification (no verifiability). In real-life cases, there is usually a possibility of audit and hence a fine for misdeclaration. Since we have neither verification nor fine in our design, we can say that the level of dishonesty we observe

all entries were 2-3 letters, supposedly their initials.

is an overestimation. This implies that subjects can get away with their dishonesty for sure. We asked in the last stage whether they felt observed at any point in time during the experiment and the majority mentioned that they did not. Even if they felt observed, they know that there are no negative consequences associated with their decisions.

### 3.2 Procedures

The experiment was conducted online through Qualtrics. We recruited subjects from Prolific for the US/Canada sample. We recruited subjects from the UAE through email lists. The study was not interactive in the sense that participants could join the study whenever they wanted after it was announced (the study was active for approximately two weeks and two days in the UAE and in Prolific, respectively). We collected data from 597 subjects (236 from UAE) who were all university students. We only recruited university students to be able to make comparisons easier between the samples. Table 2 includes the conditions and sample sizes. The online study was expected to last approximately 20 minutes (the average time spent was 17.5 minutes). It could be done on laptops, mobiles, and tablets. Participants consented that they are university students, they do the study in one sitting, they do not communicate with anyone else during the study, and their participation is voluntary.

In multicultural studies, some risks involved are the difficulties of comparing units of account, implementing researchers, languages, and samples. We address the first concern at the end of this section, where we provide details about the earnings. Since this was an online study, the second concern was irrelevant. The experiment was completed in English for all subjects. The native language of some UAE participants is not English as opposed to US/Canadian participants. However, since they all attend universities where the medium of instruction is English, this should not be considered a problem. Finally, in appendix A, we provide summary statistics for the whole sample, by region, by treatment, and by region and treatment. In addition to demographic variables, we include some other variables to see whether the randomization process is done properly. UAE participants seem to be younger (21.9 vs. 24.7) but the difference is minor. Moreover, all the participants are university students (graduate student proportions are about 20% and the same across regions). The gender is balanced in all comparisons. The duration of the study is a little higher in UAE (19 vs. 16.6 minutes), but this is not a major difference and is also close to the expected duration of 20 minutes. This could be due to the relative language or experience advantage of US/Canada participants, which might have also caused the small difference between quiz scores (3.3 vs. 3.9 out of 5). Income is significantly higher in UAE possibly due to the sample chosen from only private universities. The business major percentage in UAE is also higher (%50 vs. %16) because more business students are reached and joined the study. We include these and some other variables as controls in the regression models. In all other dimensions, there are no differences between treatments and/or regions, which shows that randomization is done properly.

Table 2: Experimental conditions and sample sizes

| **US/Canada** *(Total N = 361)* | | | |
|---|---|---|---|
| Commission | Ommission1 | Honesty Pledge | Ommission2 |
| N = 100 | N = 80 | N = 80 | N = 101 |
| **United Arab Emirates (UAE)** *(Total N = 236)* | | | |
| Commission | Ommission1 | Honesty Pledge | |
| N = 82 | N = 77 | N = 77 | |

After consenting, the participants read the instructions on the screen that explain the flow of the study in detail. Then, how the earnings are determined was explained. It was also mentioned that they do not have to remember all the details but they need to understand overall how the study proceeds and how the earnings are determined because they will be asked some comprehension questions about them. Then, they answered quiz questions about the earnings and the study (see appendix C for the quiz). After their answers, the correct answers were shown and some crucial information was provided again. The results of the quiz were not used to screen participants (the average score from the quiz was 73%). The aim was to reemphasize the main points of the study.

After the quiz, subjects played the dictator game in the role of the dictator (we called the game a division game and players as divider and receiver. In general, we used neutral language throughout the study. See Appendix D for the dictator game instructions). After they submitted their allocation decision, the initial endowment determination stage began. They were explained how their initial endowment is determined. Then, they were shown a trial page on how the coin toss procedure is implemented. When they proceeded, they saw a coin toss outcome. If they wanted to see the coin toss again, they could see the trial page up to three times. Then, the actual four coin tosses were shown one by one. They were asked to remember how many heads they observed in the actual four coin tosses and to determine their initial endowment based on the rule they were given (40 if four heads, 25 otherwise). Then, the possible shock stage was explained. There was no trial period in this stage because the coin toss process was the same. They saw four coin tosses one by one and this determined whether they receive a negative shock or not (shock if four heads, no shock otherwise).

At the end of the fourth stage, they reported their final endowment. They chose either 10, 25, or 40 based on the initial endowment and the shock. They freely chose what they wanted and they were promised to be paid this final endowment if randomly chosen. If they chose 40, they automatically moved to the last survey stage. If they reported either 10 or 25, they were randomly directed to one of the extra compensation application pages (treatment variable). After making their application decision, they moved on to the last survey stage.

In the last stage, they answered some demographic questions, brief big-five personality questions, and some scenarios (see appendix B for all the questions). Scenarios were about their likelihood of taking some actions related to undeserved benefits. They were asked questions about risk, trust,

religiosity, corruption perceptions, norms about dishonesty, and perception of government that potentially influence playing the victim behavior.

The total earnings of the participants were determined based on their reported final endowments, dictator game choices, and extra compensation application decision. First, reported final endowments (10, 25, or 40) were added to the earnings. Second, at the end of the data collection process, some pairs of participants were randomly chosen (approximately 10% chance) either as dictators or receivers and the dictator's choice was implemented and added to the earnings of these participants. Third, since there was over-application, among the ones who applied for the extra compensation, some of them were randomly chosen (approximately 6% that is implied by the theoretical distribution, see table 1) and the extra compensation (15) was added to their earnings. Thus, the total earnings of a subject was equal to *the reported final endowment (10, 25, or 40) + amount between 0 and 10 from the division game (if chosen) + extra compensation (15, if applied and chosen).*

Payments were made as follows. All the subjects from the researcher's university in the UAE were paid their earnings by inviting them to collect their earnings on campus. Subjects from the other UAE universities who completed the study received their earnings as Amazon gift cards (average earnings was 27 Dirhams). The subjects from Prolific were paid a fixed amount of 2 Pounds (approximately 2.6 US Dollars) for their time which translates into 6 Pounds per hour. Moreover, some randomly chosen Prolific participants receive their total earnings as a bonus (the average total earning for these participants was $26). All information regarding earnings (minimum and maximum amounts that they can earn) and payments were clearly communicated to the subjects before they consent and all the above-mentioned details were explained in the instructions. The critique about the difficulty of matching payments for experiments conducted internationally is also inevitable for this study. However, we essentially made at least the minimum wage equivalent payments to all subjects and we believe that the incentives were equally enough to complete the study for participants in both regions.

# 4 Results

We expect different types of behavior -mentioned in table 1- to emerge as a result of our design. Table 3 summarizes the expected and actual frequencies of different types for our sample of N=597. Among all 559 subjects -excluding 38 not classified subjects[10]- *236 (42%) subjects were completely*

[10]There are 38 subjects whom we did not identify as possible emerging types. Their actual final endowments are higher than their reported endowments to their disadvantage. One possible reason might be that they misunderstood the instructions because they could have earned more by reporting truthfully. Another reason might be that they are extremely altruists. There is some statistical evidence -quiz results and pro-sociality- for these two reasons. One other reason for the ones who reported 25 although their final endowments were 40 may be that they might have wanted to appear honest by reporting less than the rare case of having an initial endowment of 40 (Choshen-Hillel et

*honest (honest+honestaltruist).* The remaining 58% engaged in some type of dishonesty.[11] 50 of them lied about their final endowments (endowmentliar), 3 of them lied about the shock they got (shockliar), and 2 of them lied about both of these (endowmentliar and shockliar). *The remaining (268 subjects) are classified as "playing the victim"* because even though they did not receive a shock, they applied for the compensation that is repeatedly emphasized to be only for the ones who got a negative shock.

Table 3: Categorization of subjects based on their types

| | INITIAL ENDOWMENT | # of Subjects | ACTUAL FINAL ENDOWMENT | # of Subjects | REPORTED FINAL ENDOWMENT | # of Subjects | APPLICATION *(for extra compensation)* | |
|---|---|---|---|---|---|---|---|---|
| | | | | | | | YES | NO |
| | | | | | | | TYPE | |
| N=597 | $25 | **553** *(560)* | $10 | **33** *(35)* | $10 | **22** | HONEST **19** | HONESTALTRUIST **3** |
| | | | | | $25 | **9** | ENDOWMENTLIAR **6** | SHOCKLIAR **3** |
| | | | | | $40 | **2** | SHOCKLIAR and ENDOWMENTLIAR **2** | |
| | | | $25 | **520** *(525)* | $10 | **27** | **20*** | **7*** |
| | | | | | $25 | **449** | PLAYINGVICTIM **268** | HONEST **181** |
| | | | | | $40 | **44** | ENDOWMENTLIAR **44** | |
| | $40 | **44** *(37)* | $25 | **0** *(3)* | $25 | **0** | HONEST **0** | HONESTALTRUIST **0** |
| | | | | | $40 | **0** | SHOCKLIAR **0** | |
| | | | $40 | **44** *(35)* | $10 | **3** | **2*** | **1*** |
| | | | | | $25 | **8** | **6*** | **2*** |
| | | | | | $40 | **33** | HONEST **33** | |

Note: Italic numbers represent the expected (theoretical) distributions; bold numbers represent actual distributions.
* These 38 -unclassified- subjects do not appear in our categorization in Table 1 because we did not expect them to emerge.

## 4.1 Existence and extent of playing the victim behavior

Although we repeatedly emphasize that target of the compensation is only the ones who got a negative shock (the real "victims"), many of the subjects who did not receive a shock applied for the compensation. Among 597 participants, 449 (75%) of them had an initial endowment of 25, did not receive the shock, and reported 25. Among these 449 participants who had the opportunity to play the victim by applying, 268 of them applied. That is, *59.7% (268/449) of the subjects played the victim. 181 subjects (40.3%) behaved honestly and did not apply.* The number of real

al., 2020). However, these interpretations are speculative and we cannot be sure about the actual reason.

[11] Although the design is different, Fischbacher & Föllmi-Heusi (2013) similarly found that 61% of subjects engaged in lying including partial liars. They also found that 20% lied to the fullest extent possible. In our case, the corresponding figure is 8.2%.

victims was only 33 (5.5%). In line with our expectations, this finding shows that *playing the victim behavior exists and it is substantially prevalent in our sample.*

### 4.2 Treatment effects on playing the victim behavior

Our treatment variable is how we present the application page to the subjects. In this subsection, we analyze and compare the commission and two omission treatments, and we also analyze the effect of the honesty pledge. We run the commission and omission1 treatment in both regions, but we run the omission2 treatment only in the US/Canada. We run the omission2 treatment as an additional treatment after all other treatments were finished because we obtained an unexpected result and wanted to be sure about the robustness of this result. We give more details about regional comparisons in the next subsection. Table 4 summarizes the distribution of subjects based on their final endowment reports and application decisions across treatments (we did not report the details of the subjects who reported 10 as their final endowment because there are only 11 dishonest subjects, see table 3).

Table 4: Distribution of subjects based on their choices across treatments

| | | **Commission** | **Omission1** | **Omission2** | **Oath** | **Total** |
|---|---|---|---|---|---|---|
| Reported final endowment | ***N*** | **182** | **157** | **101** | **157** | **597** |
| **10** | | **13** | **17** | **2** | **20** | **52** |
| | | | | | | |
| **25** | | **145** | **127** | **85** | **109** | **466** |
| Dishonest* | | 4 | 3 | 0 | 10 | 17 |
| Honest | | 141 | 124 | 85 | 99 | 449 |
| | Did not apply | 39 (28%) | 60 (48%) | 20 (23.5%) | 62 (62.6%) | 181 (40.3%) |
| | Applied *(played the victim)* | 102 (72%) | 64 (52%) | 65 (76.5%) | 37 (37.4%) | 268 (59.7%) |
| | | | | | | |
| **40** | | **24** | **13** | **14** | **28** | **79** |
| Dishonest *(Extreme Liar)* | | 14 | 9 | 8 | 15 | 46 |
| Honest | | 10 | 4 | 6 | 13 | 33 |

* This includes both overreporters and underreporters.

In the aggregate *commission* treatment (US/Canada + UAE) where subjects see the options of applying and not applying and then actively decide which one to choose, there were a total of 182 subjects. 24 of these subjects reported the maximum final endowment of 40, 10 out of 24 were honest. *141 subjects had the opportunity to play the victim and 72% (102/141) of them played the victim* (applied for the compensation). In the aggregate *ommission1* treatment (US/Canada + UAE) where subjects do not have to do anything to apply, passively accepting the default of applying, there were a total of 157 subjects. 13 of these subjects reported the maximum final endowment of 40, 4 out of 13 were honest. *124 subjects had the opportunity to play the victim and*

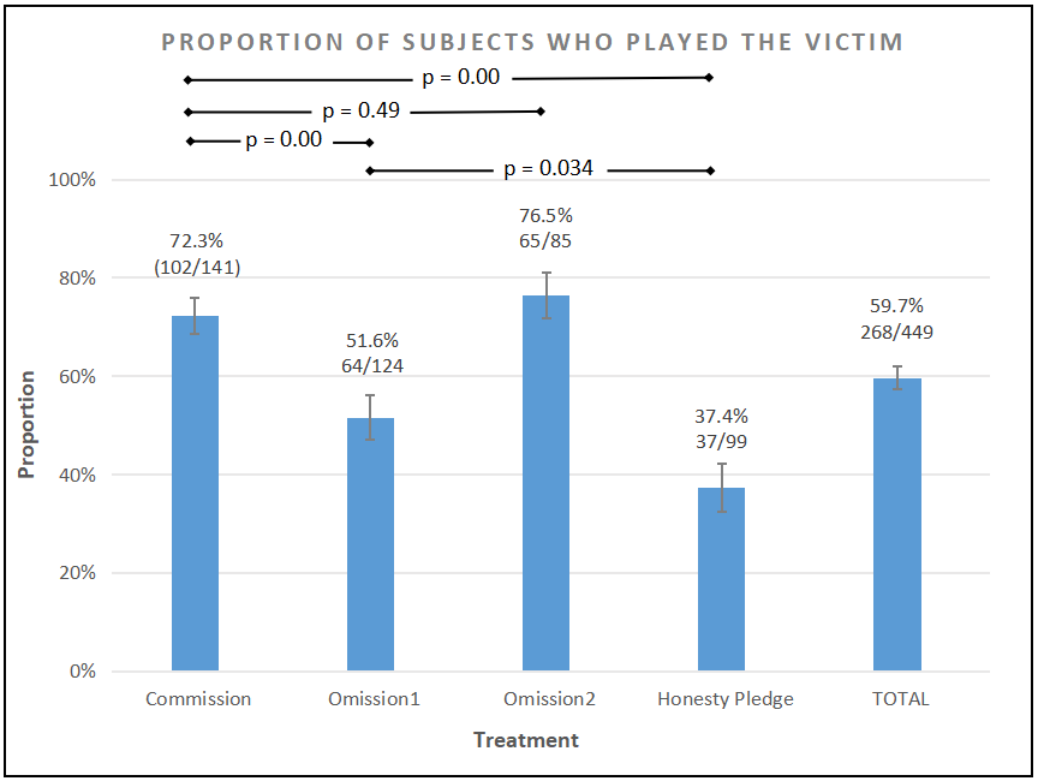


**Figure 1: Proportion of Subjects Who Played the Victim (Whole Sample)**
Note: Error bars display standard errors.

*52% (64/124) of them played the victim.* In the *ommission2* treatment (only US/Canada) where the default of applying was chosen and the decision page advanced in 5 seconds, there were a total of 101 subjects. 14 of these subjects reported the maximum final endowment of 40, 6 out of 14 were honest. *85 subjects had the opportunity to play the victim and 76.5% (65/85) of them played the victim.* In the aggregate *honesty pledge* treatment (US/Canada + UAE) where subjects have to sign an honesty pledge and actively choose to apply or not to apply, there were a total of 157 subjects. 28 of these subjects reported the maximum final endowment of 40, 13 out of 28 were honest. *99 subjects had the chance to play the victim and 37.4% (37/99) of them played the victim.*

Figure 1 summarizes the proportions of subjects who played the victim among all those who had the opportunity to play the victim and shows $p-$values of testing whether proportions are equal between relevant conditions. The lowest proportion is observed in the honesty pledge treatment (37.4%), while the highest one is observed in omission2 (76.5%) treatment.

***Commission vs. ommission1.*** Our first hypothesis asserts that playing the victim behavior would be more prevalent in the omission treatment(s) than in the commission treatment but this hypothesis is not supported. On the contrary, *the omission1 condition significantly reduced playing*

*the victim behavior* (72.3% vs. 51.6%, z = 3.4803).

Since we observed this outcome in the whole sample and in all the sub-samples (see next section for the details), it is unlikely that it is an experimental artifact. The reason for this observation is not clear, but one possible explanation is that subjects who did not get a shock were reminded that doing nothing would lead to an obviously untruthful outcome, which made the untruthful outcome of passive acceptance salient. This, in turn, made it more difficult to passively accept this untruthful option. An alternative explanation can be that explicitly mentioning the decision-making ability taken away from the subjects might have triggered a negative emotion that leads them to override the default and choose not to apply (Altmann et al., 2017). This implies that although it was beneficial in our context since the default option was an unethical act, an enhanced saliency of taking away the ability of decision-making may backfire and create a reverse effect on decision-makers.

Another possibility is the heterogeneous effects of this treatment on different groups. We checked gender and prosociality as the moderators of the treatment effect. We found that males reduced their playing the victim behavior more than females in the omission1 condition relative to omission2 condition. We compared the ratios of female fake victims across these treatments and found no significant difference (Fischer's exact test, p = 0.22), but this ratio was significantly different for males (p = 0.001). However, across omission1 and commission conditions, both ratios are significantly different although there is more statistical evidence in ratios for males. We repeated the same analysis for prosociality and found that only prosocial subjects reduced their playing the victim behavior significantly in omission1 compared to commission treatment (but no difference across omission1 and omission2). However, when we run a regression by including other possible determinants and controls, the significance of gender and prosociality vanish. Thus, the heterogeneous effect explanation is only weakly supported by the data.

Decision time heterogeneity is another possibility for this result. Since different interventions can affect subjects' allocation of attention differently, we compared the decision times of fake victims and honests. In the pooled data, the mean decision time for the ones who applied -fake victims- is significantly less than the time for the ones who did not apply -honests- (7.53 vs. 12.66 sec., $t_{273}$=3.51). Thus, being honest takes more time than being dishonest as the literature suggests (Shalvi et al., 2012). This is valid within each treatment except omission1 although the differences are not statistically significant. In omission1, it took more time to decide for the fake victims than for the honests (marginally significant difference, 9.66 vs. 7.55 sec., $t_{58}$=1.46). This can be seen as a supporting argument for the above-mentioned first reason of why omission1 condition reduced playing the victim behavior. This treatment might have triggered subjects to put more attention on the application decision by increasing saliency.

***Commission vs. ommission2.*** For robustness purposes, we run another omission session (omission2, only in US/Canada due to logistical reasons). This session is very similar to the ones

in the literature in the sense that the application option (playing the victim) is chosen by default and the page advances automatically (in 5 seconds). Thus, one does not have to do anything unless he/she does not want to apply for the compensation (playing the victim does not require any physical action, they do not have to even click the next button). In this treatment, *playing the victim behavior turns out to be more prevalent than in the commission treatment but the difference is not significant* (72.3% vs. 76.5%, z = 0.6848).

Two reasons can be mentioned why this difference turned out to be insignificant. One is purely statistical. This result might be a false negative due to the sample size that is not able to capture small effect sizes (insufficient power). However, in related studies such as Mazar and Hawkins (2015), the negative significant effect of the omission condition emerges with similar sample sizes. The other reason for the null effect can be that even with the active choice, playing the victim behavior is very pervasive (despite emphasizing that compensation is for "real victims"), so introducing passive acceptance does not significantly increase its prevalence. We address possible reasons for the observed high prevalence next.

From the well-established literature on dishonesty, we know that most people lie when they have a chance but not to the full extent (possibly due to self-image concerns, Mazar et al., 2008). We also expected participants to lie in our experimental design. But in the literature, playing the victim behavior is not very pervasive (Tennyson, 2008). One reason for the high level of playing the victim behavior in this study can be the absence of audits and fines that make cheating very easy and essentially free in our study.[12] Nonexistence of shame or fear of condemnation that are very strong deterrents of this behavior in real-life cases and the fact that the offered compensation can be perceived as a public good that is open to be overused are other possible reasons for high prevalence. The salience of the possibility of a higher initial endowment can be another reason because some people might think that they got unlucky and so might have felt treated unfairly although the probabilities make it obvious that very few people get a high initial endowment. This is related to the possible repercussions of the random determination of endowment and shock on immoral behavior. The ones who got 'unlucky' might have used this as an excuse to act immorally (Houser et al. 2012).[13] Moreover, social upward comparisons (low vs. high initial endowment) may also lead to more cheating (John et al., 2014). Another reason can be the finding of Gneezy (2005), who shows that the degree that people care how much the other side loses with their lie diminishes with the size of the gains to the decision makers themselves. Since in our experiment, the size of the loss of others due to their lies is small, they might have decided to lie to get the extra benefit. Last but not least, belief about the norms in this type of situation can be another driver of the high prevalence of this behavior. On the one hand, those who believe that most others

[12] As one of the participants mentioned: *"I feel like, if given the opportunity to lie in a vacuum, like in this study, I should lie. It has the least amount of consequences here. So yeah, I lied about my endowment to see if I could."*

[13] Although we incorporated this fairness aspect into our regression analysis and showed that it is not a significant factor, it might have affected some of the participants' decisions.

behave dishonestly can use this to justify their dishonesty. On the other hand, those who believe that dishonesty is not prevalent may want to free ride and take advantage of this situation.

***Effect of honesty pledge.*** In the other treatment, participants read an honesty pledge specific to the current situation and enter their initials just before they choose to apply on the same screen. The results clearly show that *the honesty pledge worked and significantly reduced the undeserved application* (72.3% vs. 37.4%, z = 5.40), which supports our second hypothesis. The effect is very significant both statistically and economically in the sense that playing the victim behavior is reduced by almost half. Honesty pledge reduced undeserved application ratio compared to omission 1 condition too (51.6% vs. 37.4%, z = 2.12).

***Compensating real vs. fake victims.*** One way to evaluate the success of different methods of compensating the victims (commission, omission and honesty pledge) in our case is the probability of compensating the real victims. In our sample, there were a total of 287 applications for the compensation (excluding unclassified, endowment liar and shock liar types), only 19 of which were real victims (remaining 268 played the victim). Since the ones who will be compensated were determined randomly, this implies only a 6.6% chance of compensating the real victims (if we consider all applications, the chance becomes 19/321 ˜ 5.9%). This chance is 4.7%, 10%, 1.5%, and 14% in the commission, omission1, omission2, and honesty pledge treatments, respectively. This implies that *the chance of compensating the real victims is tripled in the honesty pledge treatment relative to the commission treatment.*

***Direct lying vs. playing the victim.*** In our design, subjects had the chance to lie twice: when they report their final endowment (direct lying) and when they decide to apply for compensation (playing the victim). Interestingly, direct lying (10% - 55/553) was much less prevalent than playing the victim behavior (59.7% - 268/449). Proportions of subjects who lied about their final endowments across treatments were not significantly different whereas proportions of playing the victim behavior were significantly different (figure 1), which indicates proper randomization of subjects across treatments because this stage was the same across all treatments without any intervention.

Possible explanations of this significant difference in lying behavior could be the followings. First, subjects can convince themselves that an application is morally less questionable than a straight lie, which is very salient because one directly lies for his/her (sure) benefit independent of what others do. Second, the random component of the application process (with a probable benefit) could have created a moral wiggle room facilitating lying. Last, we observed that the ones who believe that more of others applied for the compensation (perception on social norms about application) are more likely to apply. Although we do not have a question on the social norms about endowment lying, we suspect that beliefs about social norms about lying and playing the victim behavior might be different. First two explanations are compatible with the self-concept maintanence theory (Mazar et al., 2008) because direct lying threatens self-image more strongly

whereas probabilistic nature of application process helps maintain a positive self-view.

### 4.3 Regional comparison of playing the victim behavior

When we repeat the analysis in the previous subsection for different regions and compare them, we observe that, for US/Canada, among all 348 subjects -excluding 13 not classified subjects- 153 (44%) subjects were completely honest. The number of subjects who played the victim was 176 (60.9%, 176/289 subjects) and the number of real victims was 18 (5%). For UAE, among the 211 subjects -excluding 25 not classified- 83 (40%) subjects were completely honest. The number of subjects who played the victim was 92 (57.5%, 92/160 subjects) and the number of real victims was 15 (6.4%). Tables 3a and 3b in Appendix A that are replications of table 3 summarize the frequencies of different types for different regions. When we separately analyze the regions (US/Canada vs. UAE), the conclusion of highly prevalent playing the victim behavior in the whole sample still holds and *there are no significant differences between regions in terms of the prevalence of playing the victim behavior* (60.9% vs. 57.5%, z = 0.70).

When we analyze the treatment effects on playing the victim behavior across regions, the qualitative patterns are the same. Figure 2 shows the proportions of subjects who played the victim by treatment and region (it also shows $p-$values of testing whether proportions are equal between regions for each condition). For each treatment, the proportions are very close to each other across regions (72.2% vs. 72.6%, z = -0.05; 38.6% vs. 35.7%, z = 0.29).[14] The proportion seems to be higher for the omission treatment in UAE relative to US/Canada but the difference is not significant (47% vs. 57%, z = -1.11). For each region, the same conclusions hold in terms of the treatment effects (72.2% vs. 47%, z = 3.1 for US; 72.6% vs. 57%, z = 1.76 for UAE; and 72.2% vs. 38.6%, z = 3.91 for US; 72.6% vs. 35.7%, z = 3.73 for UAE). Thus, *the first hypothesis is not supported and the second hypothesis is supported for both regions as in the whole sample.*

In all studies of dishonesty, some extreme behavior is observed. Ours is not an exception. In our case, if one reports 40 when his/her final endowment is not 40, we call this person an extreme liar. We observed 46 extreme liars among all 597 subjects in our study (7.7%). There were 2 and 44 subjects who report 40 when their final endowments were 10 and 25, respectively. When we analyzed this behavior across regions, we observed that there were 16 (out of 361, 4.4%) and 30 (out of 236, 12.7%) extreme liars in the US/Canada and UAE, respectively. These proportions are significantly different from each other (z = -3.7).

### 4.4 Determinants of playing the victim behavior

We now investigate whether the observed effects of the treatments sustain statistical scrutiny and are robust to adding other potential determinants and controls to the analysis. The binary response

[14] Peer and Feldman (2021) also found that pledges are significantly effective across individual differences.

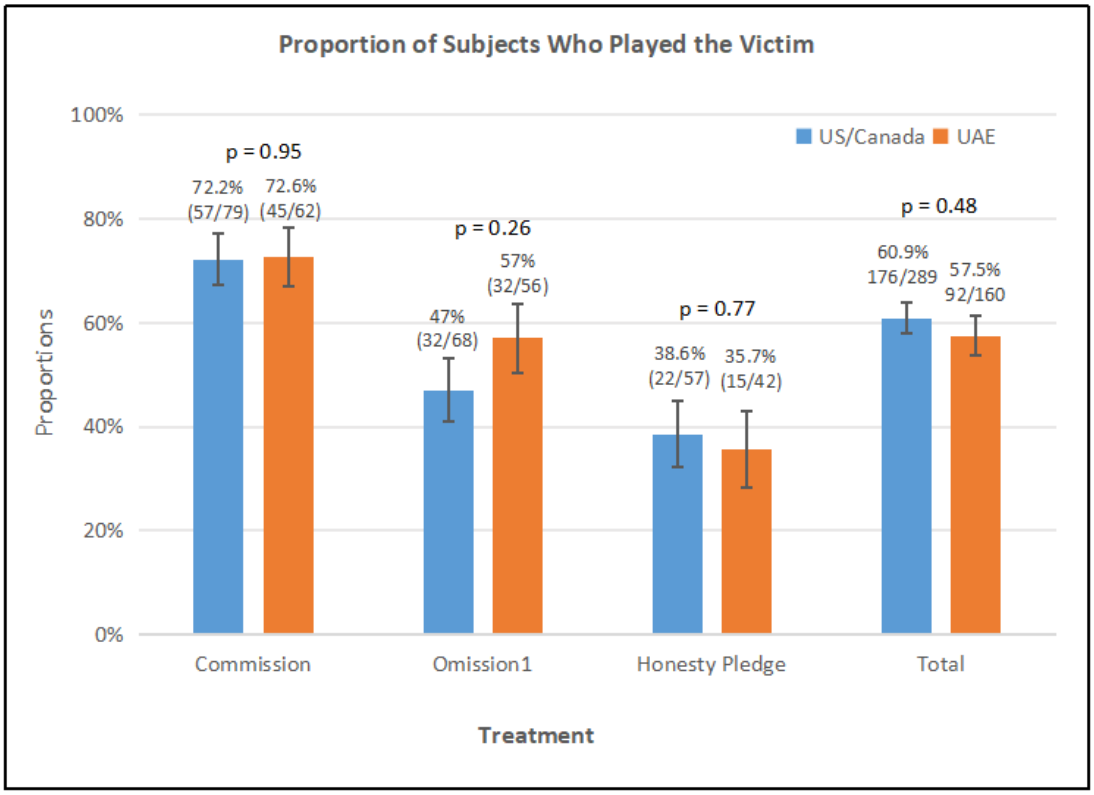


**Figure 2: Proportion of Subjects Who Played the Victim (Across regions)**
Note: Error bars display standard errors.

variable in the probit model takes a value of 1 if the subject played the victim and zero otherwise. We only include participants who had the opportunity to play the victim behavior in the analysis (449 out of 597 subjects). As independent variables, a dummy variable for each treatment (omi1, omi2, and oath) is added by keeping the commission as the base condition. We introduce a binary variable that takes the value 1 if the subject is from US/Canada.[15]

Due to the relationship between social preferences and lying behavior, we measured it with the dictator game, which is the most commonly used method to measure social preferences (List, 2007). In our study, each participant played this game in the role of the dictator (divider) and decided how much to allocate (0, 1, 2,. . . , 10) to another anonymous participant. We used the amount that a participant keeps for her/himself as a measure of how pro-self s/he is. We expect a positive and significant relationship between the likelihood of playing the victim behavior and the extent of

[15]We use this binary variable in the aggregate model. In the regional models, we used UAE local and US/Canada local binary variables because the cultural differences might be more salient within each region based on whether the subject is local to the region or not (especially in UAE where there is a huge diversity).

being pro-self.

Since dishonesty is heavily affected by (beliefs about) others' behavior and social norms, we asked the participants *"what percentage of the participants do you think applied for the compensation in this study you participated?"* (7-point Likert scale; 1: 0%-15%, 2: 15%-30%,..., 7: 90%-100%). We used this as a proxy for participants' beliefs about what others did in terms of compensation application (mean: 5.13 referring to %60-%75, st. dev: 1.45). We expect that as people believe that more people applied for the compensation (i.e., more of other participants played the victim), the likelihood of their application (playing the victim behavior) would increase.

In addition, we asked subjects how acceptable some statements related to tax evasion and corruption are:

- *"It is all right not to report some of your income to the tax office to lower the amount of taxes you would otherwise pay." (tax evasion)*
- *"A private citizen offering a bribe to a public official to speed up administrative procedures." (Corruption 1)*
- *"A public officer asking for a bribe to speed up administrative procedures." (Corruption 2)*
- *"A public officer being recruited based on family ties and friendship/networks." (Corruption 3)*

We also asked how likely they would engage in fraudulent behavior through the following scenarios (Cullis et al., 2015):

- *"An unemployed person on state/government benefit takes a casual job for a month and is paid $500 in cash. He/she does not report this income to the benefits office and keeps the $500." (Undeserved benefit 1)*
- *"A person got into a small accident with his/her insured car. When he/she sends the insurance claim to the insurance company, he/she increases the claim by a small amount to make up for the deductible." (Undeserved benefit 2)*
- *"A person has been receiving $500 extra state/government benefit per month since a back injury stopped him/her working. Even though in the past month, he/she has been healthy enough to do various types of full-time work, he/she does not inform the benefits office and keeps the $500." (Undeserved benefit 3)*

By using the answers to the above statements, we create a corruption tolerance index (Cronbach's alpha: 0.72; mean: 1.77, st. dev: 0.86) and undeserved benefit/social fraud tolerance index (Cronbach's alpha: 0.76; mean: 2.5, st. dev: 1.08) for each subject and control for these in our analysis (Lefebvre et al., 2013). We expect a positive relationship between these variables and playing the

victim behavior. We also control for one's own attitude toward dishonesty in another setting (tax evasion, Tennyson, 1997), which can be expected to have a positive relationship with playing the victim behavior (mean: 2.13, st. dev: 1.26).

Since people's perception of the institution that they engage in the contractual relationship influences their dishonest behavior, this is possibly relevant in playing the victim behavior as well. In our experiment, if people felt they were treated unfairly (although there was nothing unfair), this might lead them to behave more unethically. To control for this, we asked the participants how strongly they agree with the following statement: *"All procedures were fair in this study."* We used the answer as a proxy for fairness perception and included it in the analysis (the scores for this question were 4.04 and 4.13 out of 5 for UAE and US/Canada, respectively.). We expect a negative relationship between fairness perception and playing the victim behavior. Moreover, although the decision-making context is not directly related, how much people trust the government and their general attitude towards it might influence their behavior. Thus, we created an index for the government perception/trust (Cronbach's alpha: 0.6; mean: 2.77, st. dev: 0.98) for each subject through the following statements, and included it in the analysis as well (we expect a negative relationship between this index and playing the victim behavior):

- *"I believe that the government would help me and my family meet our daily needs in terms of income, food, and shelter, in case of a crisis such as COVID-19, earthquake, financial crisis, etc." (Government perception 1)*
- *"I trust that the figures, numbers, and reports the government announces reflect the reality in case of a crisis such as COVID-19, earthquake, financial crisis, etc." (Government perception 2)*
- *"I believe that corruption is not a problem at all in the government." (Government perception 3)*

All the indices we created are measured by 5-point Likert scale, 1 being the lowest acceptability/agreement/likelihood. Finally, we include controls for other individual characteristics: gender (binary), graduate student (binary), business major (binary), parental income level (5 levels, 1 being less than the median income in each region), religiosity (5-point Likert scale, 1 being not religious), attitude towards risk (5-point Likert scale, 5 being a risk taker), and brief big-five personality traits (score out of 10).

Table 5: Probit regression for playing the victim behavior

| | Aggregate | | UAE | | US/Canada | |
|---|---|---|---|---|---|---|
| | Model 1 | Model 2 | Model 3 | Model 4 | Model 5 | Model 6 |
| Oath | -0.319*** | -0.339*** | -0.350*** | -0.347*** | -0.329*** | -0.374*** |
| | (0.069) | (0.072) | (0.104) | (0.107) | (0.094) | (0.097) |
| Omission 1 | -0.243*** | -0.237*** | -0.211** | -0.224** | -0.275*** | -0.283*** |
| | (0.066) | (0.068) | (0.102) | (0.107) | (0.092) | (0.093) |
| Omission 2 | 0.024 | 0.004 | | | 0.015 | -0.004 |
| | (0.077) | (0.079) | | | (0.088) | (0.088) |
| US/Canada | -0.031 | -0.072 | | | | |
| | (0.060) | (0.079) | | | | |
| UAE local | | | 0.163* | 0.168* | | |
| | | | (0.089) | (0.091) | | |
| US/Canada local | | | | | -0.080 | -0.130 |
| | | | | | (0.085) | (0.087) |
| Pro-self | 0.056*** | 0.055*** | 0.036* | 0.036* | 0.073*** | 0.069*** |
| | (0.014) | (0.014) | (0.021) | (0.020) | (0.019) | (0.020) |
| Belief about norms | 0.133*** | 0.140*** | 0.094*** | 0.104*** | 0.168*** | 0.182*** |
| | (0.020) | (0.022) | (0.029) | (0.032) | (0.028) | (0.030) |
| Corruption index | 0.070** | 0.062* | 0.019 | 0.004 | 0.100** | 0.117** |
| | (0.034) | (0.035) | (0.045) | (0.053) | (0.052) | (0.053) |
| Fairness perception | -0.017 | -0.004 | 0.018 | 0.014 | -0.032 | -0.008 |
| | (0.035) | (0.027) | (0.045) | (0.049) | (0.032) | (0.035) |
| Government trust index | -0.010 | -0.003 | -0.073* | -0.057 | 0.035 | 0.060 |
| | (0.027) | (0.030) | (0.042) | (0.044) | (0.036) | (0.043) |
| Controls | No | Yes | No | Yes | No | Yes |
| Log-likelihood | -235.95 | -224.38 | -86.54 | -81.55 | -142.27 | -132.21 |
| # observations | 441 | 429 | 152 | 151 | 289 | 278 |

Notes: Commission treatment is the base condition. Average marginal effects reported. Robust standard errors in parentheses.

* p<0.10, ** p<0.05, *** p<0.01

Table 5 shows the estimated marginal effect of each variable in the probit models.[16] Model 1, 3, and 5 were run by using data sets from the whole, UAE, and US/Canada samples, respectively. These models included only the main potential determinants, while even-numbered models included the controls too. Observations with missing values for any of the explanatory variables leave 429 observations in the aggregate sample which originally had 449. The estimation results are highly supportive of the observed treatment effects. Namely, relative to the commission treatment, taking

[16]Since the tax evasion and undeserved benefit variables are highly correlated with corruption and government perception indices (and with each other too), we dropped them from the analysis.

an honesty oath reduces the likelihood of playing the victim behavior by at least 31.9%. This is a large substantive effect, especially when the negligible cost of this intervention is considered. The unexpected negative effect of omission 1 treatment is also confirmed, such that it reduces the likelihood of playing the victim behavior by at least 21%. The other omission treatment does not make any significant difference in playing the victim behavior. Finally, there is no significant difference between the UAE and US/Canada samples. However, we also included a binary variable in the regional models representing citizens of that region (117/236 -50%- and 316/361 -%87- subjects were locals in UAE and US/Canada samples, respectively). Being local has an estimated marginal impact of 0.16 which suggests that the probability that a subject plays the victim is increased by 16% if s/he is a UAE citizen relative to other subjects from UAE. This effect is negative but insignificant for the US/Canadian citizens, which is not reliable due to very low variation.

The estimation results regarding the other potential determinants are also in line with the expectations. Both the degree of being pro-self (measured by how much the subject kept for him/herself out of 10 in the dictator game) and the perceived -descriptive- social norm (measured by the belief about the percentage of other participants who applied for compensation) are positively and significantly related to the subject's playing the victim behavior in all models. A one-unit increase in the kept amount for the self increases the probability of playing the victim by about 5.5% (smaller in UAE, larger in US/Canada). This result is robust to defining this variable as binary (pro-self, more than 5 vs. pro-social, 5 or less; or extremely pro-self, 9 or 10 vs. not extremely pro-self, 8 or less). As one believes that more of others played the victim, s/he is more likely to play the victim as well (about at least 0.09 increase in probability for each 15% increase in belief). These together provide convincing evidence that the degree of pro-sociality and beliefs about descriptive norms influence playing the victim behavior.

We conjectured that individuals' attitudes towards corruption would be positively related to playing the victim behavior. This is confirmed in the US/Canada sample but not in the UAE sample. One unit increase in the approval of corruption leads to a 0.1 increase in the probability in the US (it is also significant in the whole sample but this is apparently driven by the US/Canada sample). It seems that participants from US/Canada perceive playing the victim behavior in the presented context as a corruption-related activity. Neither fairness perception nor government trust seems to affect playing the victim behavior in our sample (except for an expected but weak effect in the UAE sample that vanishes with the inclusion of controls). Finally, although some of the previous studies find that gender, risk-taking, and religiosity are significant factors that influence dishonest behavior, neither these variables nor any other control variable included in the models turned out to be significant.[17]

[17]The coefficients of only conscientiousness and agreeableness from the big-five personality traits were negative and marginally significant in the whole sample, the latter was negative and marginally significant in the US/Canada sample, and none was significant in the UAE sample. We also created and included a dummy variable in the regression

### 4.5 Other regional comparisons

We now look at whether there are differences in attitudes/behavior/beliefs between UAE and US/Canada samples in different dimensions. We made the following analysis based on the whole UAE and US/Canada samples and then repeated it by restricting the samples to only UAE and US/Canada citizens. Unless mentioned, they give the same conclusions.

We have already established that there was no difference between the two regions in terms of the prevalence of playing the victim behavior. We also showed that the proportion of extreme liars (who chose 40 as their final endowment although their actual final endowments were less) in UAE is significantly higher than in US/Canada (12.7% vs. 4.4%, z = 3.7).

When we look at pro-sociality, the average amount taken for the self is 6.28 and 6.55 out of 10 in UAE and US/Canada, respectively (the pooled average is 6.44). Comparison via the Kolmogorov–Smirnov (KS) test reveals an insignificant difference in the distributions between the regions. However, in the restricted data set, the null hypothesis of equality of distributions is rejected (6.03 and 6.52; p=0.029), which implies that UAE locals are more prosocial (although the difference is not economically very significant). Regarding fairness, participants overall perceived the procedures followed in the study fair as the average score for this question was 4.06/5.00, and there is no difference between regions (3.99 vs. 4.11, p=0.33).[18]

Participants from UAE reported themselves as significantly more risk taker and more religious relative to US/Canada participants (3.76 vs. 2.96, p<0.001; 3.58 vs. 2.22, p<0.001). US/Canada participants trust others significantly more than UAE participants (2.94 vs. 2.33, p<0.001). Indicators related to attitudes towards corruption and tax evasion showed that UAE participants consider these activities more acceptable. The corruption index (and each of its sub-measures) and tax evasion score are significantly higher in the UAE sample (2.01 vs. 1.60, p<0.001; 2.33 vs. 1.99, p<0.001).

The likelihood of engagement in activities involving undeserved benefits is not significantly different across regions. These activities involved taking undeserved unemployment benefits, undeserved injury benefits, and exaggerating insurance claims. We did not find a significant difference between regions in any of these dimensions (undeserved benefit index, 2.51 vs. 2.50, p=0.91). In the restricted sample, the likelihood of taking undeserved unemployment benefits is significantly higher in US/Canada sample (2.73 vs. 2.37, p=0.046). Regarding the trust in government, data shows that government perception is significantly higher in the UAE sample (government perception/trust index, 3.08 vs. 2.56, p<0.001). Their beliefs about the government helping its citizens during crisis times and not being corrupt are relatively stronger than the beliefs of the US/Canada sample. There is no significant difference in perception between regions only for the question about

model, "observed" that takes value 1 if a subject believes that he/she agrees or somewhat agrees to the statement "during this study, I felt observed at any point in time." Its coefficient is insignificant in all regressions.

[18] KS test is used for all the comparisons in this section unless mentioned otherwise.

the government telling the truth about the figures in crisis times (3.23 vs. 3.20, p=0.28). However, in the restricted samples, there is a significant difference in this dimension as well (3.52 vs. 3.22, p=0.021).

We also compared the personality traits of the participants in the two regions. While there is no difference in the dimensions of agreeableness and conscientiousness, UAE participants see themselves as significantly more extravert (6.24 vs. 5.44 out of 10, p<0.001), and US/Canada participants see themselves as more neurotic -marginally significant- and open to experiences (6.39 vs. 5.74, p=0.06; 7.35 vs. 6.43, p<0.001). In the restricted sample, the difference in neuroticism disappears (6.37 vs. 6.01, p=0.54).

Last but not least, since each participant in this study answered questions about how likely they are to engage in or how acceptable different dishonest acts are, we can compare and rank their acceptability. People are usually more condemnatory of social fraud than tax evasion, but they indicate that they are more likely to commit social fraud than tax evasion (Cullis et al., 2015). Lefebvre et al. (2013) showed in a lab setting that social fraud is more frequent than tax fraud. We measured attitudes toward corruption, undeserved benefits, and tax evasion through survey questions and compared scores of these at the individual level. In line with the mentioned studies, the acceptability of taking undeserved benefits is higher than that of tax evasion, and corruption-related acts are the most unacceptable for the whole sample and for all sub-samples (paired t-test, p<0.01 in all comparisons).[19]

# 5 Discussion and Conclusion

Crises are prevalent from the individual level (e.g., car accidents) to the societal level (e.g., Covid-19, financial crises). These crises create their own victims who are supposed to be protected or helped under formal (e.g., car insurance) or informal (e.g., relief programs, charities) contracts. In all these situations, compensating true victims is a critical but challenging task. However, many anecdotal evidence and empirical studies show that playing the victim behavior, i.e., faking to be a victim to take advantage of the arised opportunity, is substantially prevalent and leads to considerable inefficiencies. In this paper, we investigate this behavior in an online experiment where a limited number of participants randomly receive a negative shock, and then, a compensation mechanism targeting real "victims" is offered to all that gives participants the opportunity to play the victim.

The findings show that playing the victim behavior is very common. Although passively accepting a default of automatic application (omission) is expected to increase playing the victim behavior relative to actively applying for the compensation (commission), one omission treatment increased it insignificantly, and the other one conversely reduced it significantly. We confirm the

[19]There is only one exception where acceptability of undeserved benefits and tax evasion are not significantly different from each other for UAE locals.

effectiveness of pledges in this context because those who took the honesty pledge lied significantly less about the shock they got than the ones who did not.[20] We also find that as people are more pro-self and as they believe that more of others applied for compensation, they are more likely to play the victim. Last but not least, the behavior of subjects turns out to be very similar across UAE and US/Canada samples, which implies that the propensity for playing the victim across different regions are similar.

One criticism about the lab experiments with the possibility of lying is external validity. It is not clear whether participants who lied in the lab behave in the same way when the stakes are high or not necessarily monetary. As mentioned, the current design abstracts from important factors such as audit/fines and shame/fear of condemnation. Future studies can incorporate these as treatment variables and investigate their effects. Furthermore, one can argue that the positive association found between prosociality measured using a dictator game and playing the victim behavior can be an experimental artifact that might be driven by the subjects who want to maximize their payoff in the lab setting and whether this continues to hold in real-world settings is not clear. Although this is a valid concern and needs to be addressed with field data to check its robustness, we do not think that it is a serious limitation because of the following observations: In our context, all subjects had the chance to maximize their payoff by reporting the highest possible endowment (extreme liars). However, we observed very few extreme liars. Furthermore, there is no positive correlation between dictator game choices and being an extreme liar. In a separate probit regression model where being an extreme liar is the dependent variable, dictator game choice turned out to be insignificant.

Another possible methodological criticism would be conducting the study with the university student population who are usually not one of the main groups directly affected by crises. Although replicating our study in the field or with samples representing the population would certainly be a worthwhile extension, we believe that using student samples is not a serious limitation for our study. The main purpose of this study is to show the existence of this behavior, possible international variations in its prevalence, and the effectiveness of different mechanisms to mitigate it rather than measuring its prevalence and extrapolating it to the entire population.

We deliberately utilized only the simplest institutional and incentive structures in the study. However, given the multi-dimensionality of the issue, many extensions are possible. One is *to make the option of not applying the default,* which is the correct choice for most of the subjects.[21]

[20]Subjects were free to sign the pledge or not but almost all subjects signed it in Jacquemet et al. (2020). Peer and Feldman (2021) made the honesty pledge mandatory where ach subject had to rewrite the pledge manually, and virtually everyone did so correctly. In our case, it was mandatory to write the initials. In this sense, it is an extension of these studies and in all cases, pledges worked effectively.

[21]This is a trivial extension similar to the super-commission condition in Mazar and Hawkins (2015). The reason why it is not conducted in this paper is that omission condition where the default option is "apply" is directly mimicking the real life case where people automatically get the aid/compensation and they do not have to do anything for it (as in the case of cheques sent under the Covid relief packages). "Do not apply" default in our context does not directly fit into a realistic case.

This would decrease lying by making it more psychologically costly since it requires overriding the correct choice. Another is, instead of determining the endowment randomly, to give participants a performance-based task and let them *earn their endowments*, which is more realistic and possibly influential on their playing the victim behavior (Gërxhani and Schram, 2006; Lefebvre et al., 2015). Since most of the aid is made by governments as transfers that are financed by tax revenue, instead of separating the resources for endowment and compensation, we can *"tax" participants* to create the compensation fund and make the allocation from this tax-funded resource. This is more realistic, but it is not trivial how this may affect people's motivation to play the victim. Another treatment variable would be *the size of the shock (and compensation)* to see whether and how the extent of playing the victim is driven by them. Last but not least, *moral reminders* might be another mitigation mechanism by either turning the focus on others or on the self (Frydman and Rangel, 2014). We have already used the former by emphasizing the negative externality that the application decision creates on real victims, but this can be mentioned when they just make their decision to increase the saliency. The latter can be achieved by framing the alternative choices as follows: *"I got a negative shock that lowered my endowment, so I apply"* or *"I got no negative shock and my endowment did not change, so I do not apply."* Both of these moral cues have the potential to reduce this behavior.

This study highlights that in the context of playing the victim behavior, careful use of default choices is necessary, almost costless honesty pledges can be very effective, and descriptive norms are critical. These findings are of practical relevance to policymakers and contract designers. For example, in order to nudge applicants towards honesty and reduce false claims, the programs that necessitate an active application for compensation can incorporate an honesty pledge and the programs that do not require an active application (passive acceptance) can add reminders to increase the saliency of the (potentially) untruthful outcome of passive acceptance. Furthermore, some informational interventions can be incorporated when claim decisions are made to influence people's beliefs about what the norms are in the relevant context. This study can be viewed as the first step of a broader agenda to better understand playing the victim behavior and effective mechanisms to mitigate it. Further studies both in the lab and in the field on the default setting and other interventions are certainly needed to address this problem comprehensively.

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## APPENDIX A

### Summary statistics

| | Aggregate | Regions | | | Treatments | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | | *US/Canada* | *UAE* | *p-value* | *Com* | *Omi1* | *Oath* | *p-value (Com-Omi1)* | *p-value (Com-Oath)* |
| *Age* | 23.6 | 24.7 | 21.9 | <0.01 | 23.19 | 23.99 | 22.98 | 0.26 | 0.74 |
| | (6.36) | (6.79) | (5.21) | | (6.03) | (7.0) | (6.07) | | |
| *N* | 592 | 361 | 231 | | 180 | 155 | 156 | | |
| *Female* | 0.507 | 0.495 | 0.53 | 0.45 | 0.512 | 0.54 | 0.484 | 0.69 | 0.61 |
| | (0.5) | (0.5) | (0.5) | | (0.5) | (0.5) | (0.51) | | |
| *N* | 574 | 344 | 230 | | 178 | 150 | 149 | | |
| *Grad student* | 0.21 | 0.21 | 0.208 | 0.95 | 0.2 | 0.22 | 0.2 | 0.66 | 0.88 |
| | (0.4) | (0.4) | (0.4) | | (0.4) | (0.42) | (0.4) | | |
| *N* | 591 | 361 | 230 | | 180 | 155 | 155 | | |
| *Business major* | 0.3 | 0.16 | 0.5 | <0.01 | 0.297 | 0.37 | 0.32 | 0.16 | 0.57 |
| | (0.46) | (0.37) | (0.5) | | (0.46) | (0.48) | (0.47) | | |
| *N* | 597 | 361 | 236 | | 182 | 157 | 157 | | |
| *Income* | 2.86 | 2.39 | 3.6 | <0.01 | 2.93 | 3.01 | 3.01 | 0.61 | 0.57 |
| | (1.5) | (0.37) | (1.4) | | (1.51) | (1.54) | (1.47) | | |
| *N* | 588 | 361 | 227 | | 179 | 154 | 154 | | |
| *No type* | 0.063 | 0.036 | 0.1 | <0.01 | 0.05 | 0.07 | 0.1 | 0.42 | 0.042 |
| | (0.24) | (0.18) | (0.3) | | (0.22) | (0.25) | (0.31) | | |
| *N* | 597 | 361 | 236 | | 182 | 157 | 157 | | |
| *Quiz score* | 3.66 | 3.9 | 3.28 | <0.01 | 3.7 | 3.47 | 3.6 | 0.045 | 0.39 |
| | (1.07) | (1.02) | (1.03) | | (1.01) | (1.12) | (1.09) | | |
| *N* | 597 | 361 | 236 | | 182 | 157 | 157 | | |
| *Duration (min.)* | 17.53 | 16.56 | 18.99 | 0.048 | 17.02 | 17.32 | 18.28 | 0.82 | 0.4 |
| | (14.7) | (11.95) | (18.07) | | (10.46) | (14.1) | (16.75) | | |
| *N* | 597 | 361 | 236 | | 182 | 157 | 157 | | |
| *Real Endowment* | 26.1 | 26.28 | 25.82 | 0.16 | 25.99 | 25.57 | 26.9 | 0.25 | 0.054 |
| | (3.92) | (4.2) | (3.42) | | (3.73) | (2.88) | (5.02) | | |
| *N* | 597 | 361 | 236 | | 182 | 157 | 157 | | |
| *Shock* | 0.055 | 0.049 | 0.063 | 0.47 | 0.043 | 0.063 | 0.083 | 0.42 | 0.14 |
| | (0.228) | (0.217) | (0.244) | | (0.2) | (0.24) | (0.27) | | |
| *N* | 597 | 361 | 236 | | 182 | 157 | 157 | | |

## Summary statistics *(Continue)*

| | Regions and Treatments | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | UAE | | | | | US/Canada | | | | | | |
| | *Com* | *Omi1* | *Oath* | *p-value (Com-Omi1)* | *p-value (Com-Oath)* | *Com* | *Omi1* | *Oath* | *Omi2* | *p-value (Com-Omi1)* | *p-value (Com-Oath)* | *p-value (Com-Omi2)* |
| *Age* | 21.91 | 22.24 | 21.6 | 0.72 | 0.64 | 24.2 | 25.6 | 24.3 | 24.7 | 0.17 | 0.9 | 0.6 |
| | (5.1) | (6.2) | (4.2) | | | (6.5) | (7.3) | (7.2) | (6.3) | | | |
| *N* | 80 | 75 | 76 | | | 100 | 80 | 80 | 101 | | | |
| *Female* | 0.57 | 0.55 | 0.45 | 0.72 | 0.13 | 0.46 | 0.52 | 0.52 | 0.5 | 0.43 | 0.48 | 0.62 |
| | (0.5) | (0.5) | (0.5) | | | (0.5) | (0.5) | (0.5) | (0.5) | | | |
| *N* | 80 | 75 | 75 | | | 98 | 75 | 74 | 97 | | | |
| *Grad student* | 0.19 | 0.21 | 0.23 | 0.69 | 0.55 | 0.21 | 0.23 | 0.19 | 0.22 | 0.8 | 0.7 | 0.89 |
| | (0.4) | (0.4) | (0.4) | | | (0.4) | (0.4) | (0.4) | (0.4) | | | |
| *N* | 80 | 75 | 75 | | | 100 | 80 | 80 | 101 | | | |
| *Business major* | 0.45 | 0.57 | 0.5 | 0.13 | 0.49 | 0.17 | 0.18 | 0.15 | 0.16 | 0.93 | 0.71 | 0.82 |
| | (0.5) | (0.5) | (0.5) | | | (0.38 | (0.4) | (0.36) | (0.37) | | | |
| *N* | 82 | 77 | 77 | | | 100 | 80 | 80 | 101 | | | |
| *Income* | 3.61 | 3.75 | 3.44 | 0.5 | 0.48 | 2.39 | 2.33 | 2.63 | 2.28 | 0.75 | 0.26 | 0.57 |
| | (1.4) | (1.4) | (1.5) | | | (1.4) | (1.3) | (1.4) | (1.4) | | | |
| *N* | 79 | 74 | 74 | | | 100 | 80 | 80 | 101 | | | |
| *No type* | 0.05 | 0.1 | 0.17 | 0.19 | 0.014 | 0.05 | 0.04 | 0.05 | 0.01 | 0.68 | 0.99 | 0.1 |
| | (0.2) | (0.3) | (0.37) | | | (0.2) | (0.2) | (0.22) | (0.1) | | | |
| *N* | 82 | 77 | 77 | | | 100 | 80 | 80 | 101 | | | |
| *Quiz score* | 3.35 | 3.17 | 3.3 | 0.25 | 0.73 | 3.99 | 3.76 | 3.9 | 3.95 | 0.13 | 0.54 | 0.77 |
| | (1.0) | (1.0) | (1.0) | | | (0.9) | (1.1) | (1.0) | (1.0) | | | |
| *N* | 82 | 77 | 77 | | | 100 | 80 | 80 | 101 | | | |
| *Duration (min.)* | 18.13 | 18.2 | 20.7 | 0.96 | 0.37 | 16.1 | 16.4 | 16 | 17.6 | 0.79 | 0.91 | 0.46 |
| | (13) | (18) | (22.3) | | | (7.6) | (8.9) | (8) | (18) | | | |
| *N* | 82 | 77 | 77 | | | 100 | 80 | 80 | 101 | | | |
| *Real Endowment* | 25.73 | 25.39 | 26.4 | 0.45 | 0.3 | 26.2 | 25.75 | 27.4 | 25.9 | 0.42 | 0.09 | 0.56 |
| | (3.3) | (2.4) | (4.3) | | | (4.1) | (3.3) | (5.6) | (3.6) | | | |
| *N* | 82 | 77 | 77 | | | 100 | 80 | 80 | 101 | | | |
| *Shock* | 0.025 | 0.078 | 0.09 | 0.12 | 0.07 | 0.06 | 0.05 | 0.075 | 0.02 | 0.77 | 0.69 | 0.69 |
| | (0.2) | (0.27) | (0.3) | | | (0.2 | (0.2) | (0.27) | (0.14) | | | |
| *N* | 82 | 77 | 77 | | | 100 | 80 | 80 | 101 | | | |

*Note: Income ranges from 1 (representing less than median monthly income 10K AED in UAE and $5K in US) to 5 (more than 40K AED and $25K). No type takes value 1 if the participant's type is unidentified and 0 otherwise. Shock takes value 1 if the participant got a negative shock and 0 otherwise. Quiz score is out of 5. Standard deviation in parenthesis. P-values are from t-tests that test the equality of means.*

Table 3a: Categorization of subjects based on their types (US/Canada)

| | INITIAL ENDOWMENT | # of Subjects | ACTUAL FINAL ENDOWMENT | # of Subjects | REPORTED FINAL ENDOWMENT | # of Subjects | APPLICATION *(for extra compensation)* | |
|---|---|---|---|---|---|---|---|---|
| | | | | | | | YES | NO |
| | | | | | | | TYPE | |
| N=361 | $25 | **330** *(338)* | $10 | **18** *(21)* | $10 | **14** | HONEST **13** | HONESTALTRUIST **1** |
| | | | | | $25 | **3** | ENDOWMENTLIAR **2** | SHOCKLIAR **1** |
| | | | | | $40 | **1** | SHOCKLIAR and ENDOWMENTLIAR **1** | |
| | | | $25 | **312** *(309)* | $10 | **8** | **7*** | **1*** |
| | | | | | $25 | **289** | PLAYINGVICTIM **176** | HONEST **113** |
| | | | | | $40 | **15** | ENDOWMENTLIAR **15** | |
| | $40 | **31** *(23)* | $25 | **0** *(2)* | $25 | **0** | HONEST **0** | HONESTALTRUIST **0** |
| | | | | | $40 | **0** | SHOCKLIAR **0** | |
| | | | $40 | **31** *(29)* | $10 | **0** | **0** | **0** |
| | | | | | $25 | **5** | **4*** | **1*** |
| | | | | | $40 | **26** | HONEST **26** | |

Note: Italic numbers represent the expected (theoretical) distributions; bold numbers represent actual distributions.
* These 13 -unclassified- subjects do not appear in our categorization in Table 1 because we did not expect them to emerge.

Table 3b: Categorization of subjects based on their types (UAE)

| | INITIAL ENDOWMENT | # of Subjects | ACTUAL FINAL ENDOWMENT | # of Subjects | REPORTED FINAL ENDOWMENT | # of Subjects | APPLICATION *(for extra compensation)* | |
|---|---|---|---|---|---|---|---|---|
| | | | | | | | YES | NO |
| | | | | | | | TYPE | |
| N=236 | $25 | **223** *(221)* | $10 | **15** *(14)* | $10 | **8** | HONEST **6** | HONESTALTRUIST **2** |
| | | | | | $25 | **6** | ENDOWMENTLIAR **4** | SHOCKLIAR **2** |
| | | | | | $40 | **1** | SHOCKLIAR and ENDOWMENTLIAR **1** | |
| | | | $25 | **208** *(209)* | $10 | **19** | **13*** | **6*** |
| | | | | | $25 | **160** | PLAYINGVICTIM **92** | HONEST **68** |
| | | | | | $40 | **29** | ENDOWMENTLIAR **29** | |
| | $40 | **13** *(15)* | $25 | **0** *(1)* | $25 | **0** | HONEST **0** | HONESTALTRUIST **0** |
| | | | | | $40 | **0** | SHOCKLIAR **0** | |
| | | | $40 | **13** *(12)* | $10 | **3** | **2*** | **1*** |
| | | | | | $25 | **3** | **2*** | **1*** |
| | | | | | $40 | **7** | HONEST **7** | |

* These 25 -unclassified- subjects do not appear in our categorization in Table 1 because we did not expect them to emerge.

## APPENDIX - B
## SURVEY and SCENARIOS

**Do not write your name, ID, or anything related to your identity to anywhere.**

1. What is the (monthly) income level of your family? *(In UAE, Dirham is used instead of Dollar.)*

- ☐ <5000 Dollars
- ☐ 5000-10000 Dollars
- ☐ 10000-15000 Dollars
- ☐ 15000-20000 Dollars
- ☐ >20000 Dollars

2. How strongly do you agree or disagree with the following statement?

***"I am a risk taker."***

| 1 | 2 | 3 | 4 | 5 |
|---|---|---|---|---|
| Disagree | Somewhat disagree | Neither agree nor disagree | Somewhat disagree | Agree |

3. How strongly do you agree or disagree with the following statement?

***"Most people can be trusted."***

| 1 | 2 | 3 | 4 | 5 |
|---|---|---|---|---|
| Disagree | Somewhat disagree | Neither agree nor disagree | Somewhat disagree | Agree |

4. To what level, do you consider yourself to be religious?

| 1 | 2 | 3 | 4 | 5 |
|---|---|---|---|---|
| Not religious | Somewhat not religious | Neutral | Somewhat religious | Very religious |

5. To what extent do you consider the behavior in the following statement as acceptable?

***"A public officer being recruited on the basis of family ties and friendship networks."***

| 1 | 2 | 3 | 4 | 5 |
|---|---|---|---|---|
| Unacceptable | Somewhat unacceptable | Neither acceptable nor unacceptable | Somewhat acceptable | Acceptable |

6. To what extent do you consider the behavior in the following statement as acceptable?

***"A public officer asking for a bribe to speed up administrative procedures."***

| 1 | 2 | 3 | 4 | 5 |
|---|---|---|---|---|
| Unacceptable | Somewhat unacceptable | Neither acceptable nor unacceptable | Somewhat acceptable | Acceptable |

7. To what extent do you consider the behavior in the following statement as acceptable?

***"A private citizen offering a bribe to a public official to speed up administrative procedures."***

| 1 | 2 | 3 | 4 | 5 |
|---|---|---|---|---|
| Unacceptable | Somewhat unacceptable | Neither acceptable nor unacceptable | Somewhat acceptable | Acceptable |

8. How strongly do you agree or disagree with the following statement?

***"During this experiment, I felt observed at any point in time."***

| 1 | 2 | 3 | 4 | 5 |
|---|---|---|---|---|
| Disagree | Somewhat disagree | Neither agree nor disagree | Somewhat disagree | Agree |

9. How strongly do you agree or disagree with the following statement?

***"All procedures were fair in this study."***

| 1 | 2 | 3 | 4 | 5 |
|---|---|---|---|---|
| Disagree | Somewhat disagree | Neither agree nor disagree | Somewhat disagree | Agree |

10. What percentage of the participants do you think applied for the compensation in this study you participated?

| | | | | | | |
|---|---|---|---|---|---|---|
| 0% -15% | 15% -30% | 30% -45% | 45% -60% | 60% -75% | 75% -90% | 90% -100% |

11. Consider the government of the country where you have lived most of your life. How strongly do you agree or disagree with the following statement?

***"I believe that the government would help me and my family meet our daily needs in terms of income, food, and shelter, in case of a crisis such as COVID-19, earthquake, financial crisis, etc."***

| 1 | 2 | 3 | 4 | 5 |
|---|---|---|---|---|
| Disagree | Somewhat disagree | Neither agree nor disagree | Somewhat disagree | Agree |

12. Consider the government of the country where you have lived most of your life. How strongly do you agree or disagree with the following statement?

***"I trust that the figures, numbers, and reports the government announces reflects the reality in case of a crisis such as COVID-19, earthquake, financial crisis, etc."***

| 1 | 2 | 3 | 4 | 5 |
|---|---|---|---|---|
| Disagree | Somewhat disagree | Neither agree nor disagree | Somewhat disagree | Agree |

13. Consider the government of the country where you have lived most of your life. How strongly do you agree or disagree with the following statement?

***"I believe that corruption is not a problem at all in the government."***

| 1 | 2 | 3 | 4 | 5 |
|---|---|---|---|---|
| Disagree | Somewhat disagree | Neither agree nor disagree | Somewhat disagree | Agree |

14. How strongly do you agree or disagree with the following statement?

***"it is all right not to report some of your income to the tax office to lower the amount of taxes you would otherwise pay."***

| 1 | 2 | 3 | 4 | 5 |
|---|---|---|---|---|
| Disagree | Somewhat disagree | Neither agree nor disagree | Somewhat disagree | Agree |

Instructions: How well do the following statements describe your personality?

| I see myself as someone who … | Disagree | | Neither agree nor disagree | | Agree |
|---|---|---|---|---|---|
| 1. … is reserved/shy | 1 | 2 | 3 | 4 | 5 |
| 2. … is generally trusting (others) | 1 | 2 | 3 | 4 | 5 |
| 3. … tends to be lazy | 1 | 2 | 3 | 4 | 5 |
| 4. … is relaxed, handles stress well | 1 | 2 | 3 | 4 | 5 |
| 5. … has few artistic interests | 1 | 2 | 3 | 4 | 5 |
| 6. … is outgoing, sociable | 1 | 2 | 3 | 4 | 5 |
| 7. … tends to find fault with others | 1 | 2 | 3 | 4 | 5 |
| 8. … does a thorough (complete) job | 1 | 2 | 3 | 4 | 5 |
| 9. … gets nervous easily | 1 | 2 | 3 | 4 | 5 |
| 10. … has an active imagination | 1 | 2 | 3 | 4 | 5 |

**SCENARIOS:**

1. Read the following scenario and evaluate how likely you would engage in the mentioned activity/behavior.

***"An unemployed person on state/government benefit takes a casual job for a month and is paid in cash. He/she does not report it to the benefit office and keeps the $500."***

| 1 | 2 | 3 | 4 | 5 |
|---|---|---|---|---|
| Unlikely | Somewhat unlikely | Neither likely nor unlikely | Somewhat likely | Likely |

2. Read the following scenario and evaluate how likely you would engage in the mentioned activity/behavior.

***"A person got into a small accident with his/her insured car. When he/she sends the insurance claim to the insurance company, he/she increases the claim by a small amount to make up for the deductible*** *(deductible is the amount you must pay out-of-pocket for each claim before the insurance company begins paying).***"**

| 1 | 2 | 3 | 4 | 5 |
|---|---|---|---|---|
| Unlikely | Somewhat unlikely | Neither likely nor unlikely | Somewhat likely | Likely |

3. Read the following scenario and evaluate how likely you would engage in the mentioned activity/behavior.

***"A person has been receiving $500 extra state/government benefit per month since a back injury stopped him/her working. Even though in the past month he/she has been well enough to do some types of full-time work he/she does not tell the benefit office and keeps the $500."***

| 1 | 2 | 3 | 4 | 5 |
|---|---|---|---|---|
| Unlikely | Somewhat unlikely | Neither likely nor unlikely | Somewhat likely | Likely |

Do you have any concerns/comments/suggestions regarding this experiment?

_____
_____
_____

## Appendix C - Quiz

Q1. Suppose that your initial endowment becomes 25 and then your endowment is reduced (negative change). Your final endowment becomes 25-15=10 Dollars. In this case, you can apply for compensation and have a chance to get an extra 15 Dollars.

THIS IS TRUE!

Q2. Suppose that your initial endowment becomes 40 and your endowment is not reduced (no change). Your final endowment is 40-0=40 Dollars. In this case, you can still apply for compensation and have a chance to get an extra 15 Dollars.

THIS IS FALSE! If you report your final endowment as 40, you cannot apply for extra compensation.

Q3. Suppose that your initial endowment becomes 25 and then your endowment is not reduced (no change). Your final endowment becomes 25-0=25 Dollars. In this case, you can still apply for compensation and have a chance to get an extra 15 Dollars.

THIS IS TRUE! **BUT, target of the compensation is the ones whose endowments are reduced (negative change).** However, all participants (unless they report their final endowment as 40 Dollars) can apply and get (a chance to earn) 15 Dollars. Since there will be a limited number of people whose endowments will be reduced (negative change), there will be a limited amount of compensation as well. If only the participants whose endowments were reduced apply, the compensation is added to all of their accounts. If there is over-application, we will randomly choose whom to add the compensation without any verification. **That means, in case of over-application, some participants whose endowments are actually reduced may not be compensated.** At the end of the whole study, we will separate the ones who applied for extra compensation, **we will randomly choose some participants among them (approximately equal to the number of participants whose endowments are reduced), and add the compensation amount (15 Dollars) to their accounts.**

Q4. All participants can apply for extra compensation.

THIS IS FALSE. **Target of the extra compensation is the ones whose endowments are reduced.** Only the ones who report their final endowments as 10 or 25 can apply for extra compensation. **If you report your endowment as 40 (that means you got 40 as initial endowment and your endowment is not reduced -no negative change), you cannot apply for it.**

Q5. The reported final endowments will be added to all participants' earnings, but these earnings will be paid as bonus only to some randomly chosen participants.

THIS IS TRUE!

## Appendix D - Dictator game

In this game, you, as divider, must choose how to split 10 Dollars between you and the participant who will be paired with you (receiver). You may choose any allocation that sums to 10 and that consists of whole numbers. In other words, you may choose any of the following allocations:

| How much you want to keep for yourself | 10 | 9 | 8 | 7 | 6 | 5 | 4 | 3 | 2 | 1 | 0 |
|---|---|---|---|---|---|---|---|---|---|---|---|
| How much you want to give to the other participant | 0 | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 |

**Payment**: At the end of the whole study (data collection), we will randomly choose some pairs (dividers and receivers) and we will add the amounts based on the divider's split to these selected participants' earnings. For example, suppose you are randomly chosen as divider, then, if you choose the column with (10, 0), 10 Dollars will be added to your account, 0 Dollars will be added to the other participant's account; if you choose the column with (5, 5), 5 Dollars will be added to your account, 5 Dollars will be added to the other participant's account, etc...

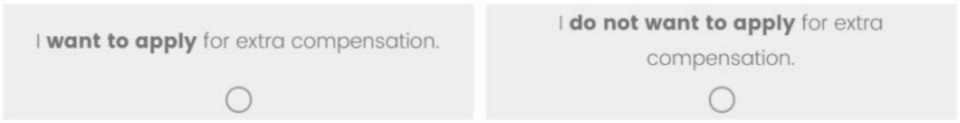
Now, please make your allocation choice below (they should add up to 10).

How much you want to keep for yourself : ____

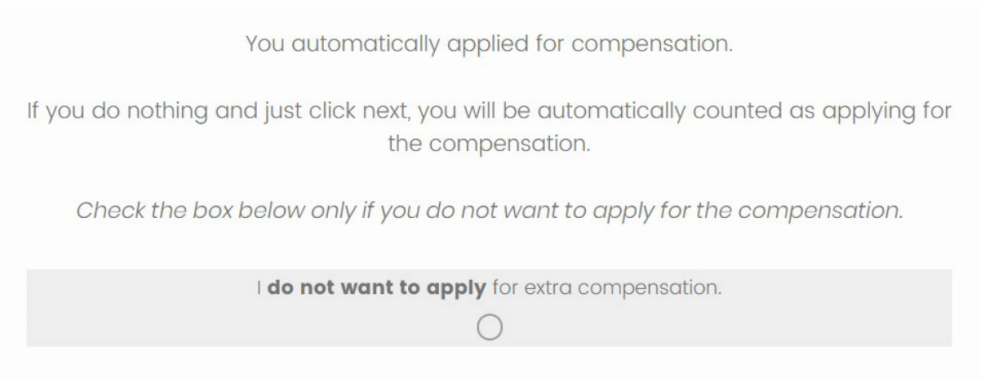
How much you want to give to the other participant : ____

## APPENDIX - E
## TREATMENTS - Screenshots

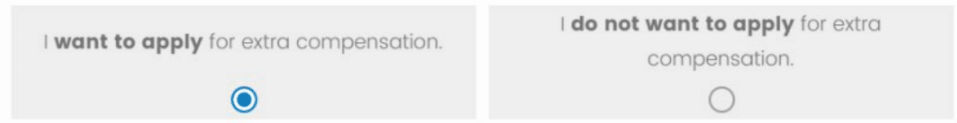
**CONTROL (Commission)**




**OMISSION - 1**




**OMISSION - 2**




*(First option is selected by default and after five seconds this page automatically advances.)*

**HONESTY PLEDGE**