Personality Testing and the Public Goods Game

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Daniel Woods*

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Abstract

Personality tests are commonly used to hire suitable employees but this process is susceptible to strategic misrepresentation by job-seekers. This paper uses a lab experiment as an analogy of such a hiring process by using the Public Goods Game (PGG) as a proxy for a cooperative work environment. Subjects first complete a Big Five personality test, focusing on the trait of "Agreeableness", which previous studies have linked to pro-social cooperation in the PGG. Two groups are formed: a high Agreeableness group and a low Agreeableness group. The high Agreeableness group should contribute more to the public good. The experiment manipulates the timing of revealing the group formation rule, as knowing the rule before the personality test allows for misrepresentation of Agreeableness. I find no evidence of misrepresentation when the group formation rule is revealed before the personality test. I also find that Agreeableness group formation increases contributions for both high and low groups, but only when it is described to subjects before the PGG. Contrary to the existing literature, I find no evidence that Agreeableness influences contributions in the PGG.

*Department of Economics, University of Innsbruck, Universitaetsstrasse 15, 6020 Innsbruck, Austria. Email: daniel.j.woods@uibk.ac.at. Funding from the IFREE Small Grants Program is gratefully acknowledged. In the spirit of transparent and credible science, an exhaustive OSF pre-registration is publicly available at http://doi.org/10.17605/OSF.IO/MDB7A.

1 Introduction

Psychometric tests, designed to measure a person's personality or other latent aspects that cannot be directly observed, are an established standard in many firms' hiring procedures. Firm hiring processes are so intertwined with psychometric testing, it even pervades its dictionary definition.[1] Psychometric tests are used on approximately 60 to 70% of US job-seekers (Weber & Dwoskin, 2014), and 75% of international firms either use or plan to use them in the future (Kantrowitz, Tuzinski, & Raines, 2018). Psychometric tests are multi-billion dollar industry globally, with expenditure reaching 12.32 billion USD in 2021 and forecast to hit 23.28 billion in 2030 (Emergen Research, 2022). It remains an open question whether this expenditure is justified. Finally, it has been suggested that psychometric testing is unfair or discriminatory against minorities or those with disabilities (Weber & Dwoskin, 2014; Hawkins & Monroe, 2021; McGee & McGee, 2022a). All of these elements illustrate the economic importance of psychometric testing, and why it is crucial to understand their efficacy, impacts, and any unintended consequences.

The problem with using psychometric personality tests for hiring is that potential employees have a material incentive to misrepresent their responses on the tests in order to more closely align towards what they perceive an employer is looking for. When completing a job application, job-seekers know they are being evaluated at every step. Job-seekers tailor their cover letters, embellish their CVs, and curate their references, all to paint themselves in the best light possible. It is no stretch of the imagination that they also adjust their responses to personality questions. After all, no one would willingly state they 'insult people' or 'get irritated easily' during a job interview, so why would they in a job personality test? Therefore, using personality tests during the hiring process may be uninformative of the potential employee's true personality. Such tests introduce further frictions and inefficiencies into the labor market, which must function smoothly for the overall well-being of society and the economy. Therefore, it is important to study whether personality testing maintains its effectiveness given the incentive to misrepresent.

[1] *'Psychometric test: A test that is designed to show someone's personality, mental ability, opinions, etc., often used by companies when they are deciding whether or not to employ someone.'* (Cambridge Business English Dictionary, 2023)

There is some evidence that using psychometric personality testing is effective in job hiring processes (Autor & Scarborough, 2008; Hoffman, Kahn, & Li, 2018), but it is not clear why. One proposed channel is that it puts more weight on objective measures rather than subjective opinions, but that would still require job seekers to represent themselves honestly. Another channel is that misrepresentation on personality tests could be attenuated by a preference for honesty, meaning the signal-to-noise ratio may be good enough for it to remain a useful metric. The challenge of accurately identifying what an employer is seeking would also decrease the likelihood of misrepresentation. On this note, it is possible that testing might instead be capturing some measure of intelligence (Borghans, Duckworth, Heckman, & Weel, 2008). Those that are smart enough to identify what the employer wants could also be more effective workers based on that intelligence. More negatively, those that are cunning enough to shape their personality responses may be ruthless enough to achieve results. Such workers would not value the firm's reputation or sustainability, and potentially drive out more genuine workers. Therefore, selecting workers in this fashion may be beneficial in the short-term but problematic in the longer-term, something that previous studies have not considered. The effects of personality testing in job hiring are multifaceted and warrant further study. There are still open empirical questions on whether such tests are effective, why they are effective, and if there are unintended consequences arising from their use.

In this paper, I design a laboratory experiment to evaluate under what conditions personality testing is effective. Lab experiments are becoming a common method to help inform firm personnel and hiring processes. Experiments are a cost-effective tool to evaluate potential firm policies and why they work, without confounds like employee self-selection and while still retaining good external validity (Villeval, 2016). I design the experiment to closely mirror the important elements of hiring using personality testing, and the subsequent work output. The experiment consists of two main parts, a personality test followed by a cooperation task. For the personality test, I elicit the 'Big Five' personality traits, which are widely employed in both hiring procedures and academic research in economics.[2] For the

[2]The psychometric testing firms Big Five Assessments, Hogan Assessments, and SHL, among others, incorporate elements of the Big Five as part of their battery of psychometric testing services that they offer to firms. For a variety of examples of the Big Five in economics research, see (Bartling, Fehr, Maréchal, & Schunk, 2009; Fréchette, Schotter, & Trevino, 2017; Donato, Miller, Mohanan, Truskinovsky, & Vera-Hernández, 2017; Holmén, Holzmeister, Kirchler, Stefan, & Wengström, 2021).

cooperation task, I use the Public Goods Game (PGG) as an representation of a cooperative work environment. In the PGG subjects can make socially-optimal contributions to a public good, but face a personal incentive to free-ride and contribute less. I interpret contributions to the public good as effort at work, which is something an employer would like to encourage. I focus on the Big Five personality trait of 'Agreeableness', the tendency to act in a cooperative, unselfish manner, as research finds it positively impacts contributions in the PGG and other similar social dilemmas (Perugini, Tan, & Zizzo, 2010; Volk, Thöni, & Ruigrok, 2012; Kagel & McGee, 2014; Thielmann, Spadaro, & Balliet, 2020). I sort subjects into groups for the PGG based on their Agreeableness score, to mimic the role of an employer hiring based on personality tests in an attempt to maximize their firm's success.

The crucial treatment dimension in the experiment is the timing of information about the purpose of the initial personality questionnaire, i.e., the group formation rule for the PGG. There are three treatments on the time dimension, *Before* the personality test, *After* the personality test (but before the PGG), and *Never*. In the *Before* treatment, subjects have an incentive to misrepresent their personality in order to try and get into a more cooperative group. This situation is similar to the current status quo, where job seekers are aware they are being evaluated for the job by the test. The compression of Agreeableness scores, alongside any mistrust that might arise due to the potential for strategic misrepresentation, makes this a challenging environment for personality testing to be effective in increasing PGG contributions. Whereas in the *Never* Treatment, subjects are never informed about how groups are formed, and therefore have no material incentive to misrepresent their personality. Without strategic misrepresentation, forming groups by the elicited Agreeableness scores is more likely to be effective in increasing PGG contributions in high Agreeableness groups. Finally, in the *After* treatment, subjects also have no material incentive to misrepresent their personality as the group formation rule is only revealed directly after the personality test. If subjects know that they are in a group with similarly cooperative people, then they can be more confident of current and future cooperation. Combined with the absence of strategic misrepresentation, this scenario is the most favorable environment for personality testing to be effective. The situations represented by *After* and *Never* are not particularly realistic, but instead they address the question of what conditions are required for personality testing

to be effective. They highlight a strength of economic experiments, in that it allows for an exploration of counterfactuals that would otherwise not occur.

The second treatment dimension is the group formation rule itself. Groups are typically randomly assigned in economics experiments, which makes a *Random* treatment a natural baseline for the *Agreeableness* group formation rule. The experiment is a 3x2 design, so subjects in the *Random* treatment also have the group formation rule revealed to them either *Before* or *After* the personality test, or *Never*. With this battery of treatments, I aim to address the following research questions:

Question 1 *To what extent do individuals misrepresent their personality when they have strategic reasons to do so?*

Question 2 *Under what conditions are personality tests effective in encouraging cooperative behavior?*

Question 3 *Does using personality tests in an unexpected way influence responses in subsequent tests?*

I address Question 1 by comparing the responses to the personality test between the treatment with *Agreeableness* group formation rule that is revealed *Before* to all other treatments, as strategic misrepresentation can only be present in the former. I find no evidence of misrepresentation of any personality trait in the *Before* treatment. A preference for honesty is the most likely explanation for this result.

I address Question 2 by comparing the impact of each treatment dimension on contributions in the PGG while holding the other dimension fixed. This approach allows me to isolate and identify the most significant empirical factors influencing behavior. I find that the *Agreeableness* group formation rule increases contribution rates in the *Before* and *After* treatments. However, I also find that contributions increase regardless of whether the group is of high or low Agreeableness, and I find no evidence that the *Agreeableness* group formation treatment is effective in the *Never* treatment. Therefore, the *Agreeableness* group formation rule by itself is ineffective. Rather, it is the knowledge that groups will be formed

by Agreeableness and subjects' belief that it will be effective that drives increased contributions. These results suggest that personality testing may cause a placebo effect, and that the knowledge that it is used for hiring may enhance rather than diminish its effectiveness.

I answer Question 3 by conducting another personality test after the PGG, and focus on subject responses in the *Agreeableness* group formation rule that is revealed *After*, as these subjects have had their personality responses used in an unannounced way. Question 3 addresses an important methodological question in experimental economics: whether the unexpected use of previous responses changes how subjects behave in the future. I find no evidence that withholding information about the group formation rule until *After* the personality test affects responses to subsequent personality tests. Unless contradicted by future evidence, this design feature remains an appropriate option for experimental economists when their research question requires it.

Overall, my findings suggest that personality testing is not substantially threatened by the existence of misrepresentation. Firstly, subjects appear to be honest enough that misrepresentation is not a major issue. Secondly, knowing how the personality test is used can be useful instead of harmful if subjects believe it is effective.

1.1 Contribution to the Literature

I contribute to the voluminous literature on the PGG. When I refer to the PGG in this paper, I am using this as a shorthand reference for the commonly studied Linear Voluntary Contribution Mechanism, although it is worth noting other formulations exist (Ledyard, 1995). A typical pattern of behavior in the PGG starts out with average contributions to the public good of around 50%, which decays steadily over time (Ledyard, 1995; Chaudhuri, 2011; Villeval, 2020). The socially optimum contribution level is 100%, but subjects face an individual incentive to free-ride off the contributions of others. One specific focus has centered on mechanisms or interventions aimed at enhancing contributions in the PGG. Examples include allowing for punishment (Fehr & Gächter, 2000) or facilitating endogenous group formation (Ahn, Isaac, & Salmon, 2009). I contribute to this strand of the PGG literature by considering exogenous group formation through personality sorting. Prior studies on exogenous group formation have sorted subjects based on their previous contribution

behavior in a PGG, and found that this type of sorting is effective (Burlando & Guala, 2005; Gächter & Thöni, 2005; Gunnthorsdottir, Houser, & McCabe, 2007; Ones & Putterman, 2007). Typically in these experiments the sorting rule is withheld from subjects, and in all cases the information given on the sorting rule is constant by treatment. I contribute to this literature by examining how knowledge of the sorting rule affects contributions in the PGG. Additionally, I contribute to this line of literature by exploring whether it is possible to effectively sort subjects by indirect measures of their contribution rate, namely their personality traits.

Another strand of the PGG literature considers the effects that individual characteristics have on contribution behavior in the PGG. Of particular interest to the current paper are studies that elicit Big Five personality characteristics.[3] The Big Five personality trait of Agreeableness has been found to be a significant predictor of contribution behavior in the PGG (Perugini et al., 2010; Volk et al., 2012). I aim to tackle the logical next question in this line of research: Given our understanding that Agreeableness influences contributions, how can we leverage this insight? Creating PGG groups by Agreeableness in order to improve contributions is a natural next step, and is analogous to role of employers using personality testing to select well-suited employees.

Naturally, I also contribute to the literature on psychometric personality testing. Misrepresentation, regardless of the motivation behind it, has been a longstanding concern in psychology due to the threat it poses to the validity of psychometric testing.[4] The main limitation of psychology studies on misrepresentation is the absence of monetary incentives. Subjects are typically explicitly instructed to misrepresent themselves in a particular way, which effectively gives permission to lie. As a result, this type of research fails to capture the significant trade-off between honest representation and material gain. Furthermore, it is cognitively costly to determine which questions coincide with a specific personality trait. Without the incentive to do so, individuals will put less effort into this task. The use of incentives is a key difference between the fields of experimental psychology and experimental economics.

[3]Some other relevant papers on individual characteristics and the PGG are (Anderson, Mellor, & Milyo, 2004; Carpenter, Daniere, & Takahashi, 2004; Catola, D'Alessandro, Guarnieri, & Pizziol, 2021).

[4]For select examples see (Braun & Gomez, 1966; Velicer & Weiner, 1975; Kroger & Wood, 1993)

The most closely related paper in economics on personality testing using incentivized experimental methods is by McGee and McGee (2022b) (henceforth MM). In their experiment, they first elicit subjects' Big Five personality traits in an initial baseline session. In a follow-up session a week later, subjects complete a second Big Five assessment. Before taking the second personality test, subjects are informed that they will receive an extra payment if they are 'hired' for a hypothetical job. The hiring process is based in part on their Big Five characteristics as elicited in the second personality test. Subjects are given a job description that is designed to indicate that Big 5 personality trait of Extroversion would be ideal.[5] MM find that subjects misrepresent their personality in the presence of incentives.

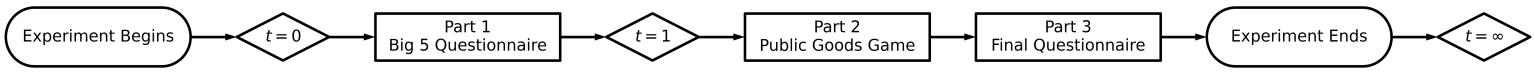
I take a different approach from MM which makes a complementary but distinct contribution to the literature. Firstly, my focus is how misrepresentation impacts subsequent work behavior. An employee is likely to behave differently when it comes to cooperative team decisions if they suspect their colleagues are manipulative, dishonest, and ill-suited for their roles due to misrepresentation. MM focus on the magnitude of misrepresentation given the incentive of being hired for a job that is never undertaken. Whereas, I extend the hiring process analogy to include the ensuing job effort decisions, in order to focus on the effects of misrepresentation. My main goal is to uncover whether personality testing is effective in fostering cooperative environments, and under what conditions. Secondly, I consider the misrepresentation of personality traits in a between-subject design rather than within-subjects as in MM. In this regard, I follow the experimental literature on dishonesty, which emphasizes that dishonest behavior is difficult to observe at the individual level (Fischbacher & Föllmi-Heusi, 2013). Comparing individual responses across two personality tests introduces a potential confounding factor. Subjects might be concerned that any substantial misrepresentation will be detected by comparing the two tests, leading them to be more honest than they would otherwise be. Finally, this paper contributes by proving a conceptual replication of some of the elements in MM. Replication is not the primary purpose of this study, but the externality is a welcome one given the current credibility crisis in the social sciences (Butera, Grossman, Houser, List, & Villeval, 2020). Independent replications can greatly increase the likelihood that any detected effect is actually true (Maniadis, Tufano, & List, 2014).

[5]MM also use a job description aimed at Introversion as well as a neutral description as robustness checks.

2 Experimental Design

I first briefly describe the experiment and its treatments, so that the necessity of some of the finer design elements are more apparent. The experiment consists of three parts that are common to all treatments. Part 1 is a Big Five questionnaire, Part 2 is a PGG, and Part 3 is a short questionnaire that elicits four other personality traits. The first treatment dimension is how groups are formed in Part 2, the PGG. In the *Random* (R) treatments, groups of three are formed randomly from all subjects in the session. In the *Agreeableness* (A) treatments, subjects are first randomly shuffled into silos of six. Within each silo, the three subjects with the highest Agreeableness scores (as elicited in Part 1) are assigned to one group, while the remaining three are assigned to another group. The second treatment dimension is the timing of when information about the group formation rule is provided. This is either *Before* Part 1 ($t = 0$), *After* Part 1 but before Part 2 ($t = 1$), or *Never* ($t = \infty$). An illustration of the timing of the experiment is presented in Figure 1. The experiment is a 3x2 design, meaning all combinations of the two treatment dimensions are considered, as summarized in Table 1. Henceforth, I denote each treatment with two characters as in Table 1, with the letter representing either the A(greeableness) or R(andom) group formation rule, and the number representing $t = 0$, $t = 1$, or $t = \infty$.

Figure 1: Timeline of Experiment



A diamond ($\diamond$) represents a possible treatment point at which the group assignment rule for Part 2 is revealed.

Table 1: Treatments

	$t = 0$ (*Before*)	$t = 1$ (*After*)	$t = \infty$ (*Never*)
Agreeableness (A)	$A0$	$A1$	$A\infty$
Random (R)	$R0$	$R1$	$R\infty$

2.1 Part 1 - Big Five Elicitation

Part 1 consists of 50 questions designed to elicit the Big Five personality traits (McCrae & John, 1992). These traits are Openness, Conscientiousness, Extraversion, Agreeableness, and Neuroticism. As the experiment is conducted in Austria, the questions (and all of the experiment) are in German. Each Big Five characteristic is elicited using the 30 question 'BFI-2-S Inventory' (Soto & John, 2017; Rammstedt, Danner, Soto, & John, 2020). The remaining 20 questions are all on Agreeableness, and sourced from the International Personality Item Pool's (IPIP) '100-Item Lexical Big-Five Factor Markers' (Goldberg, 2002; Goldberg et al., 2006; Streib & Wiedmaier, 2001). The Agreeableness trait is disproportionately weighted (26/50) as it is of primary interest and used for group formation in Part 2 in the A treatments. Subjects are asked to answer the personality questions accurately, and enter their responses using a 5-point Likert scale (Likert, 1932). Agreeableness is calculated based on each subject's numerical responses on the relevant questions.[6]

In all treatments, subjects are given a short overview of the PGG in Part 2 before completing the Part 1 questions. If information about the group formation rule is provided (i.e. $t \leq 1$), it is provided either directly before or directly after subjects complete the 50 questions. The *Agreeableness* treatments have an in-depth description of the group formation rule, which outlines the Agreeableness trait, its positive relationship to cooperative decisions (with academic references), and that the three subjects with the highest Agreeableness scores from a random silo of six will be grouped together.[7] A high level of detail is provided to help subjects understand the specific personality trait that is being used in the group formation rule, and why it could be beneficial or desirable to be in the high Agreeableness group.

2.1.1 Predictions: Misrepresentation

When it comes to strategic misrepresentation in the Big Five questionnaire of Part 1, there are three treatment groups of interest. The first are those that know in advance that their Part 1 responses will be used to form groups in Part 2 ($A0$). The second are those that know in advance that their Part 1 responses will not be used to form groups in Part 2 ($R0$). The

[6] See Appendix E for details on the personality trait questions and their scoring.

[7] The instructions for Parts 1 and 3 are presented in Appendix D.

final group are those that do not know in advance about the group formation rule in Part 2 ($t > 0$). The first two groups are aware of how their Part 1 responses affect Part 2 while answering Part 1, while the third group is unaware of this while answering Part 1.

I propose two behavioral channels that could influence Part 1 responses: the incentive to misrepresent Agreeableness, and the suspicion that Part 1 answers may be used in some way for Part 2. An incentive to misrepresent Agreeableness exists when it is known groups will be formed based on this trait. Suspicion occurs only when subjects are not aware of the purpose of the questionnaire. Subjects may believe (sometimes correctly) that the questionnaire will be used in some relevant way in the future, as they know there will be a following Part 2. Each of the comparisons between the relevant groups and the differences in operative channels between them are summarized in Table 2.

Table 2: Misrepresentation of Agreeableness - Treatment Comparisons

Treatment Comparison	Incentive	Suspicion
$A0$ to $R0$	$-$	0
$R0$ to $t > 0$	0	$+$
$A0$ to $t > 0$	$-$	$+$

Going from the first treatment to the second, $+$ indicates that channel has been added, 0 indicates no change, and $-$ indicates that channel has been taken away. The $t > 0$ grouping includes all treatments except for $A0$ and $R0$.

The experimental design permits a clean test of both incentives and suspicion, i.e. the two comparisons in Table 2 where only one channel is added or removed by moving between them. Both channels have the potential to influence Agreeableness. Incentives should increase the reported Agreeableness scores, as subjects will prefer to be in H groups (or avoid L groups). I propose that suspicion would push responses towards being more socially-acceptable, and therefore increase reported Agreeableness scores. There are many possibilities of what a subject might be suspicious of, but the obvious candidates of group formation or having answers revealed to others in Part 2 would both suggest a tendency towards more socially-acceptable responses. Hypotheses 1 and 2 formalizes the prediction that the reported Agreeableness scores in Part 1 in the presence of incentives or suspicion respectively.

Hypothesis 1 *Agreeableness scores are higher in* $A0$ *than in* $R0$

Hypothesis 2 *Agreeableness scores are higher in $t > 0$ treatments than in $R0$*

2.2 Part 2 - Public Goods Game

Part 2 consists of a PGG adapted from the version used by Lugovskyy, Puzzello, Sorensen, Walker, and Williams (2017). Groups of three are assigned from silos of six subjects by the group formation rule (i.e. randomly or by Agreeableness). Each group of three remains together for 15 'group cooperation decisions'. In each decision, each subject has 25 tokens they can allocate to either a Private account or a 'Cooperation' account.[8] Each token a subject allocates to the Private account earns that subject 10 points. Each token a subject allocates to the Cooperation account earns each of the three group members (i.e. including the subject in question) 4 points each. In other words, one token allocated to the Cooperation account earns the group 12 points overall. I refer to tokens allocated to the Cooperation account as 'contributions'. The marginal per-capita return (the ratio of the private benefit of one token to the Cooperation account to that of the opportunity cost of that token) is $MPCR = \frac{4}{10} = 0.4$. For $MPCR = \frac{4}{10} = 0.4$, it is a well-replicated result that groups' average contribution rates typically start at around 50% and then decline steadily over time (Ledyard, 1995; Chaudhuri, 2011). As I do not anticipate full contributions in the baseline $R0$ treatment, there is plenty of room for a intervention to increase contributions without censoring. Subjects make their decision by deciding how many tokens to allocate to the Cooperation account, with the remainder being allocated to their Private account. After making their decision, subjects are reminded of their own contribution, and also given information on the total group contribution in that round. These two pieces of information are also available at any time during Part 2 in a history table that is displayed at the bottom of the screen.

2.2.1 Predictions: Efficiency

I define efficiency as the number of tokens allocated to the Cooperation account, as full contributions are the first-best social optimum (i.e. socially efficient). One important dis-

[8] Framing the PGG in terms of group cooperation is likely to increase contributions (Dufwenberg, Gächter, & Hennig-Schmidt, 2011). This is not an issue as all treatments are framed in the same way.

tinction to make is that in any treatment that sorts by Agreeableness (i.e. A treatments), one group will have higher Agreeableness than the other. The high group is likely to have higher contributions than the low group. I therefore consider these two types of groups separately, as I would like to observe the positive effects of personality sorting.[9] I denote the two types of groups H and L for high and low Agreeableness respectively. In the following discussion, I take the viewpoint of the H group when describing potential effects.

I conjecture that there are three main factors at play here: the group formation rule itself, strategic misrepresentation of Agreeableness, and knowledge of the group formation rule. The Agreeableness (A) group formation rule should be effective in increasing contributions, as this personality trait is linked with cooperation and generosity. Hypothesis 3 tests this conjecture under each timing ($t = i$) condition .

Hypothesis 3 *The number of tokens contributed in AiH is greater than in Ri.*

The number of tokens contributed in Ri is greater than in AiL.

The number of tokens contributed in AiH is greater than in AiL.

However, the effectiveness of the Agreeableness group formation rule will differ depending on when information about the rule is revealed. Consider comparing $t = 0$ to $t = 1$, two treatments where subjects know the group formation rule before the PGG. In $t = 0$ the group formation rule is known prior to when Agreeableness is measured. Subjects have an incentive to misrepresent themselves in the Agreeableness elicitation to try and be placed in the H group (or to avoid the L group). Agreeableness scores will be compressed and the end result would be more similar to random group formation in terms of each group's true level of Agreeableness. Whereas in $t = 1$, the group formation rule is only revealed after the Agreeableness elicitation, precluding strategic misrepresentation. The Agreeableness group formation rule should be more effective in the absence of strategic misrepresentation. In terms of the Random group formation rule, I posit that t has no effect. Hypothesis 4 formalizes these conjectures.

Hypothesis 4 *The number of tokens contributed in $A0H$ is lower than in $A1H$.*

[9] In the employment framing of this environment, the low group would simply not be hired. However, given the expectations of lab subjects this is not practical to implement.

The number of tokens contributed in $R0$ is the same as in $R1$.

The number of tokens contributed in $A0L$ is higher than in $A1L$.

Now consider comparing $t = 1$ to $t = \infty$, two treatments that do not have strategic misrepresentation but differ in whether subjects know the group formation rule prior to the PGG. Knowing that the Agreeableness group formation rule is in effect means that subjects are aware they are grouped with similarly cooperative people. Such confidence will increase initial contributions if subjects are concerned about being taken advantage of by lower contributors. Higher initial contributions will have a flow-on effect if subjects are conditional cooperators. Therefore, Agreeableness group formation should be more effective when the rule is known in the absence of strategic misrepresentation. Hypothesis 5 summarizes these conjectures.

Hypothesis 5 *The number of tokens contributed in $A1H$ is higher than in $A\infty H$*

The number of tokens contributed in $R1$ is the same as in $R\infty$

The number of tokens contributed in $A1L$ is lower than in $A\infty L$

Table 3 presents especially interesting treatment comparisons that isolate the impact of a particular effect while holding other factors constant. This is under the assumption that effects are additively separable, but potential interactions means a full factorial design is prudent.

Table 3: Efficiency - Selected Treatment Comparisons

Treatment Comparison	Incentive to Misrepresent	Knowledge of group formation rule	Agreeableness group formation
$A0$ to $A1$	$-$	0	0
$A1$ to $A\infty$	0	$-$	0
$A0$ to $R0$	$-$	0	$-$
$A1$ to $R1$	0	0	$-$
$A\infty$ to $R\infty$	0	0	$-$

Going from the first treatment to the second, + indicates that channel has been added, 0 indicates no change, and $-$ indicates that channel has been taken away.

2.3 Part 3 - Final Questionnaire

In Part 3, subjects are first informed that they are to complete a final survey, and that their final earnings for the experiment have already been set. Subjects then answer 16 personality questions using the same 5-point Likert scale format as the questions in Part 1, and a standard demographic questionnaire. The 16 questions elicit the three elements of the 'Dark Triad' (Paulhus & Williams, 2002), and the 'Honesty-Humility' trait from 'HEXACO' (Ashton & Lee, 2009). The three Dark Triad measures are 'Machiavellianism' (Christie & Geis, 1970), 'Narcissism' (Raskin & Hall, 1979), and 'Psychopathy' (Hare, 1985). Machiavellianism is marked by a calculating, manipulative, and deceitful nature towards other people. Narcissism is defined as being egotistic and prideful with limited empathy for others. Psychopathy is characterized by selfishness, impulsiveness and a lack of remorse for ones actions. Honesty-Humility is a personality trait where people avoid manipulating others for personal gain, and feel little temptation to break rules.

Part 3 investigates an important methodological issue in experimental economics: whether omission of information leads to a loss of control over subjects' beliefs and expectations. There is a strong norm against using deception in economics experiments, which has existed from the inception of the field (Svorenčík, 2016). If a subject becomes aware they were deceived in an economics experiment, then they should not believe all of what they are told in experiments after that point in time. Subjects would adjust their responses to account for the fact that the underlying rules may suddenly change in a way that may be detrimental to them. Therefore, they would not reveal what they would actually do if the situation were exactly as described, resulting in a loss of experimental control. The current experiment does not use deception (it cannot - it is an economics experiment). Every piece of information provided to subjects, whether in the instructions or elsewhere, is literally correct. However, there are 'gray-areas' where full consensus among researchers about their acceptability has not yet been reached (Cason & Wu, 2019; Charness, Samek, & van de Ven, 2022). A relevant scenario is 'unexpected data use', when responses are used in a way not described or revealed to subjects. Charness et al. (2022) find that this technique is generally regarded by researchers as non-deceptive and is assessed as appropriate and useful. However, they

also find that of the scenarios they consider, student subjects state that unexpected data use is the most likely to influence their future responses and perceive it as more deceptive than researchers do. If subjects do change their future responses based on unexpected data use, this is a methodological problem on a similar scale as outright deception, despite what researchers may believe.

Part 3 provides a very conservative test of whether unexpected data use affects subjects' subsequent responses. It is conservative as subjects are explicitly informed that Part 3 is the last part of the experiment and that their final payments are already set. If this statement is taken seriously, then subjects have no material incentive to misrepresent their personality in their Part 3 responses. However in the $A1$ treatment, information about how the earlier Part 1 responses would be used in Part 2 was initially withheld and later disclosed to subjects. The unexpected data use from Part 1 may cause subjects to change their Part 3 responses in anticipation of additional unexpected data use, despite explicit statements to the contrary. It would be concerning if subjects in the $A1$ treatment responded in a different fashion than those in the other treatments, as it would imply a loss of experimental control. Such a finding would raise strong objections about using unexpected data use as a design feature in economics experiments going forward.

The traits elicited in Part 3 all have a clear direction in terms of social desirability. Narcissism, Machiavellianism, and Psychopathy are clearly negative traits from the perspective of society, while Honesty/Humility is considered a positive trait. I propose that if a subject anticipates unexpected data use, then they would misrepresent themselves towards what is more socially acceptable. I propose two channels that would influence a subject's beliefs that their Part 3 responses will be used to affect something in the experiment. The first channel is whether subjects are aware that the data from personality questions have been used for something in the experiment. These are subjects in the $A0$ and $A1$ treatments, as they know the group formation rule in Part 2 was based on their Agreeableness score from Part 1. The subjects in the other treatments remain *Unaware* that personality responses could be used in other parts of the experiment. Subjects that know their personality questions in Part 1 were used in Part 2 could suspect that their personality responses in Part 3 are also used in some fashion, and misrepresent themselves accordingly. The second channel is

whether the use of the personality data was unexpected. Subjects in $A0$ expected this data use when completing Part 1, as they were told of the Agreeableness group formation rule in advance. Whereas, subjects in $A1$ did not expect it, but found out about it after completing Part 1. Subjects in the $A1$ treatment have a justified belief that their Part 3 responses may be used in some way that has not yet been revealed, and thus would be the most likely to misrepresent themselves in Part 3. Table 4 describes which channels are present between each group of treatments.

Table 4: Misrepresentation in Part 3 - Treatment Comparisons

Treatment Comparison	Unexpected Data Use Revealed	Knowledge of Personality Data Use
$A0$ to $A1$	+	0
$A0$ to $Unaware$	0	−
$A1$ to $Unaware$	−	−

Going from the first treatment to the second, + indicates that channel has been added, 0 indicates no change, and − indicates that channel has been taken away. The $Unaware$ grouping includes all treatments except for $A0$ and $A1$.

I aggregate each individual into one measure of 'Positive Perception', which positively weights Honesty/Humility and negatively weights the Dark Triad traits. Based on my previous reasoning, I posit the following Hypotheses about Positive Perception:

Hypothesis 6 *Reported Positive Perception is higher in* $A1$ *than in* $A0$

Hypothesis 7 *Reported Positive Perception is higher in* $A1$ *than in Unaware treatments*

Hypothesis 8 *Reported Positive Perception is higher in* $A0$ *than in Unaware treatments*

2.4 Procedures

The data collection is currently underway at the EconLab at the University of Innsbruck.[10] I will collect observations from 432 subjects, i.e., 144 groups of three. Each R treatment will

[10]The plan is to complete the entire data collection in the Winter Semester 2023/2024 at UIBK (02.10.2023 - 03.02.2024). The UIBK EconLab subject pool is starting to become a little thin. As a result, I am currently in discussions with the Vienna Center for Experimental Economics at the University of Vienna to conduct some sessions there.

have 16 groups, and each A treatment will have 32 groups. I collect a different number of groups as the A treatment is split between L and H groups. The current statistical analysis in Section 4 is based on the data collected thus far, which consists of 276 subjects (63.9% of the total planned number of observations).

Subjects are recruited using the online database hroot (Bock, Baetge, & Nicklisch, 2014), where UIBK students who are interested in participating in economics experiments can sign up. The experiment is computerized using oTree (Chen, Schonger, & Wickens, 2016). A session consists of 6, 12, 18, or 24 subjects (depending on how many show up for a session), as multiples of six are required for the A treatments.[11] All subjects within a session face the same treatment. Treatments were randomly assigned to sessions by randomly shuffling a list of the treatments and then sampling without replacement. The list of treatments included two entries for each A treatment as twice as many groups are required for this treatment. I sought to avoid the A treatments being disproportionately represented in the latter part of the data collection.

3 Simulations

As the PGG is finitely repeated, the Nash equilibrium can be solved by backwards induction, and results in all subjects contributing zero to the public good. The Nash equilibrium in the PGG has been resoundingly refuted by a large body of evidence. Therefore, I adapt the utility function used in Arifovic and Ledyard (2012), which incorporates other-regarding preferences alongside a desire to not be taken advantage of. Their learning model is able to capture various empirical regularities of the PGG while remaining relatively simple. The simulation exercise that I undertake is not designed to accurately predict the magnitude of contributions, rather it is to explore how the treatment effects described in Section 2 could operate. I also use it to simulate a sample data-set on which the statistical analysis in Section 4 is applied, in order to demonstrate the functionality of that code prior to data collection.

The base utility function is $U_i(c) = \pi_i(c) + \beta\bar{\pi}(c) - \gamma_i max\{0, \bar{\pi}(c) - \pi_i(c)\}$, where c denotes the total contribution of the group, $\pi_i(c) = 4c + 10(25 - c_i)$, $\bar{\pi}(c) = \frac{1}{3}\sum_{i=1,2,3}\pi_i(c)$,

[11] R sessions also use multiples of six for consistency even though only multiples of three are needed.

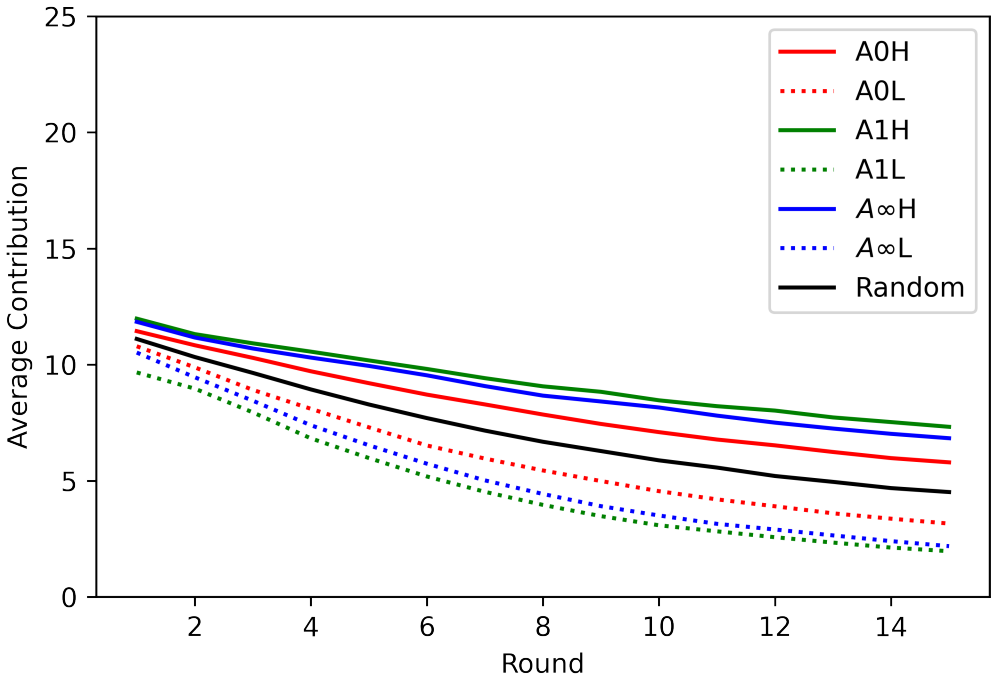
and β_i and γ_i are individual weights on the total average payoff and being taken advantage of respectively. I incorporate the Agreeableness of an individual $A_i \in [0,1]$, as well as the the Agreeableness of each group member $A_{j\neq i}$ by adjusting the weights on each element present in the utility function: $U_i(c) = (1-A_i)\pi_i(c) + \frac{A_1+A_2+A_3}{3}\bar{\pi}(c) - (1-A_i)max\{0, \bar{\pi}(c) - \pi_i(c)\}$. I propose that a higher level of Agreeableness reduces the weight on an individual's own payoff as well as their envy disutility. A higher level of Agreeableness also increases the weight placed on the group's average payoffs, but to a lesser extent as it also depends on the Agreeableness of the other two group members.

I conduct simulations based on this utility function that incorporates Agreeableness. I assume that $A_i \sim U[0,1]$. In all treatments, I assume that individuals have an estimate of A_j as reported in Part 1.[12] However, this estimate is distorted depending on the treatment. In the $A0$ treatment, the reported Agreeableness and true Agreeableness diverge due to misrepresentation. I model this misrepresentation as $\tilde{A}_i = max\{A_i + U[0,1], 1\}$, as the ability to misrepresent is likely heterogeneous across individuals. However, as a result of this misrepresentation, the reported $\tilde{A}_i$ are not believed, and the estimate is weighted down towards 0.50, the average expected A_i given the uniform prior of $U[0,1]$. The weighting in $A0$ is 0.75 on the uniform prior and 0.25 on the Part 1 report. In the $A1$ treatment, I assume that individuals do not misrepresent their personality, and therefore they believe the reports from Part 1 are accurate (i.e. there is zero weight on the uniform prior). In the Random R treatments and in $A\infty$, I assume Agreeableness is somewhat obfuscated as it is not known that it is important. In particular, I assume that equal weights are given to the reported A_i and the uniform prior.

I assume that individuals estimate $\bar{\pi}(c)$ from the other group member's contributions in the previous round. In round 1, I assume they expect $c = c_i + \sum_{i\neq j}(25A_j)$. Finally, I assume that decisions are made probabilistically using a logistic / quantal response decision rule with parameter λ that increases round on round to reflect the effect of experience. I present the results from 10,000 simulations in Figure 2. Figure 2 suggests comparative statics in the directions posited in Section 2, providing an example of how treatment differences could emerge.

[12]This is only for the simulation, subjects in the experiment are never told Agreeableness scores.

Figure 2: Simulation Results by Round



4 Statistical Analysis

The statistical analysis is conducted using Python and Stata in a Jupyter Notebook. Something that should be noted is that the analysis in this version of the paper is only on the data collected thus far. Currently, that is observations from 276 subjects, while 432 subjects are planned in total. Therefore, these results are preliminary, low-powered, and come with all the appropriate caveats that entails.

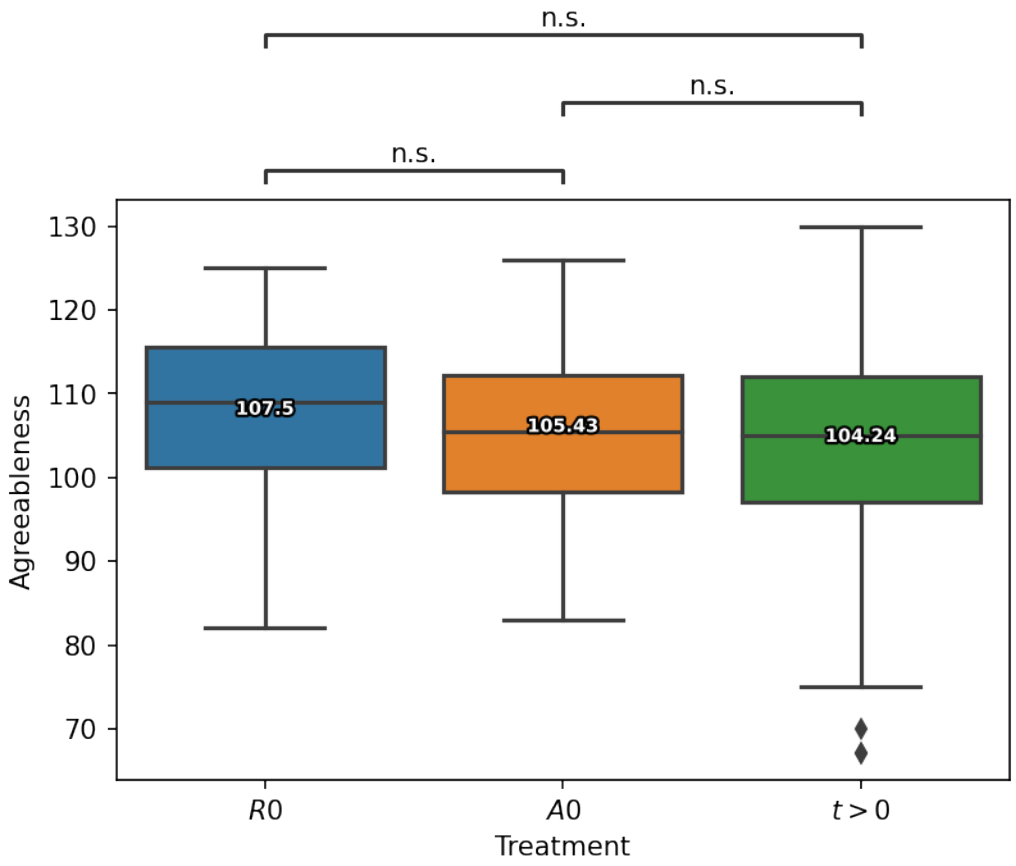
I report all statistical results using conservative two-sided tests regardless of whether the associated hypothesis is directional or not. I describe statistical results as strongly statistically significant when $p < 0.01$ (***), statistically significant when $p < 0.05$ (**), and weakly statistically significant when $p < 0.10$ (*).

4.1 Primary Analysis

4.1.1 Part 1: Strategic Misrepresentation of Agreeableness

As described in Section 2.1.1, there are three major groups of treatments: $A0$, $R0$, and all $t > 0$ treatments, because treatment differences can only impact Part 1 responses if they occur before Part 1. The outcome of interest is each subject's Agreeableness score, calculated from their responses to the Part 1 questions. Each of the three groups of treatments is tested using a Mann-Whitney test, with each subject being an independent observation. The comparison between $R0$ and $A0$ tests Hypothesis 1, and the comparison between $R0$ and all $t > 0$ treatments tests Hypothesis 2. I report these results alongside summary statistics in Figure 3.

Figure 3: Agreeableness by Treatment



Mean Agreeableness overlaid. Statistical results are based on a two-sided Mann Whitney test. ***=$p < 0.01$, **=$p < 0.05$, and *=$p < 0.10$.

Figure 3 shows that Agreeableness scores do not differ between treatments. This is evidence against Hypothesis 1, which suggests that strategic misrepresentation is not a major factor in this environment. This is also evidence against Hypothesis 2 and suggests that any suspicion from the omission of the Part 2 group formation rule has a minimal impact in Part 1.

I also consider whether misrepresentation is sophisticated or not. If misrepresentation is sophisticated then subjects only misrepresent the relevant trait of Agreeableness. However, if misrepresentation is unsophisticated, then responses could also change for other Big 5 characteristics. Table 5 reports the analysis for all Big 5 characteristics, and shows no real evidence of misrepresentation of any of the Big 5 characteristics.

Table 5: All Big 5 characteristics by Treatment

Characteristic	$R0$	$A0$	$t > 0$	p-values
Agreeableness	107.50	105.43	104.24	0.39, 0.20, 0.73
Open Mindedness	20.46	21.07	21.86	0.77, 0.25, 0.38
Negative Emotionality	15.54	16.50	15.76	0.54, 0.67, 0.57
Extraversion	19.88	19.57	20.55	1.00, 0.36, 0.40
Conscientiousness	22.83	21.67	20.97	0.22, 0.05, 0.56

The treatment columns report the average score of the given personality trait. Agreeableness $\in [26, 130]$ and all other personality traits $\in [6, 30]$. The p-values column reports the results from Mann-Whitney tests on the pairs: $R0$ to $A0$; $R0$ to $t > 0$; and $A0$ to $t > 0$ respectively.

4.1.2 Part 2: PGG Contributions

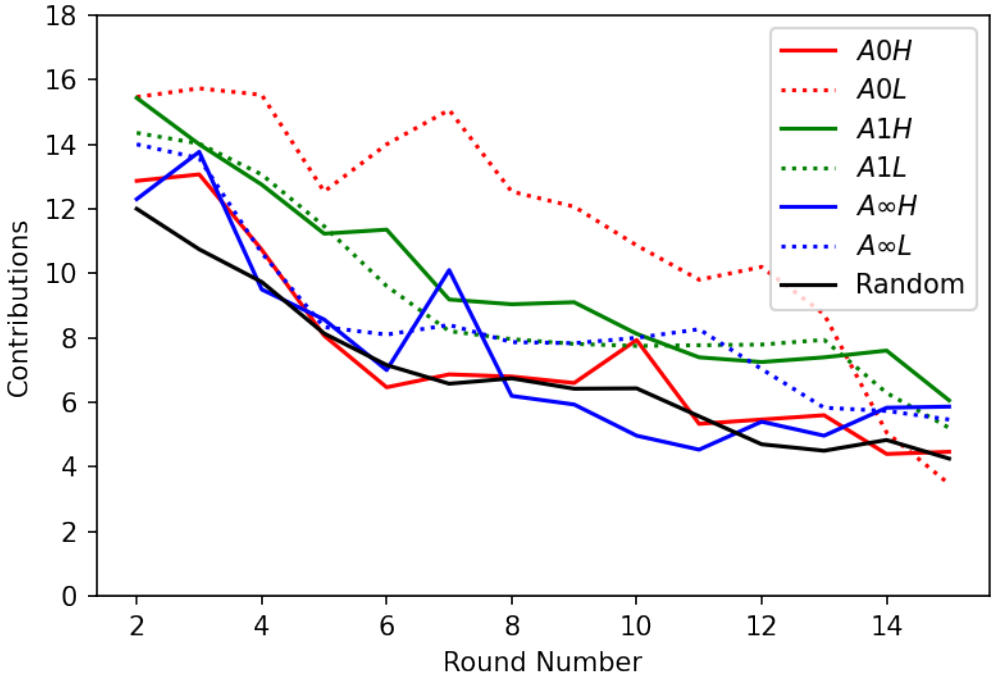
Figure 4 summarizes average contributions over time by treatment and group type. It shows that average contributions exhibit a similar pattern for the R and $A\infty$ treatments, and the $A0H$ groups. This is suggestive that forming groups by Agreeableness is ineffective in increasing contributions in the PGG. Rather, it appears as if simply mentioning Agreeableness makes the group formation rule effective, as there is a positive level shift in the both of the $A1$ treatments, as well as the $A0L$ groups.

Table 6 reports the average contribution by each group member to the public good by treatment and group type. Table 6 suggests that $A > R$, but that there are not major differences between H and L groups (or at least not in the direction one would expect).

Table 6: Average Contributions by Treatment

	$t = 0$	$t = 1$	$t = \infty$
AH	7.17	9.37	7.19
R	5.13	8.72	5.85
AL	10.94	8.79	8.19

Figure 4: Average Contributions by Round



Instead of using an ultra conservative test where each group is a single independent observation and their contributions are averaged over all periods (Clark, 2002; Harrison, 2007), I conduct a more sophisticated statistical analysis that uses a panel data approach in order to utilize more of the data while accounting for the underlying dependencies. I use the group's average contribution in a period as the dependent variable, and a treatment dummy alongside the period for the independent variables. I use a panel-data Tobit regression for the possible censoring that occurs at 0 and 25 tokens for upper and lower limits respectively. For each relevant comparison between two treatments (or group types within a treatment), I run the regression using only data from the pair that is being considered. Table 7 summarizes the results from the comparisons that are relevant for testing the proposed Hypotheses.

Table 7 provides no support for any of tested hypotheses. The only statistically significant result is in the opposite direction than the hypothesis predicts. However, Figure 4 does suggest treatment effects that may become statistically significant after more observations are collected.

Table 7: Efficiency - Regressions

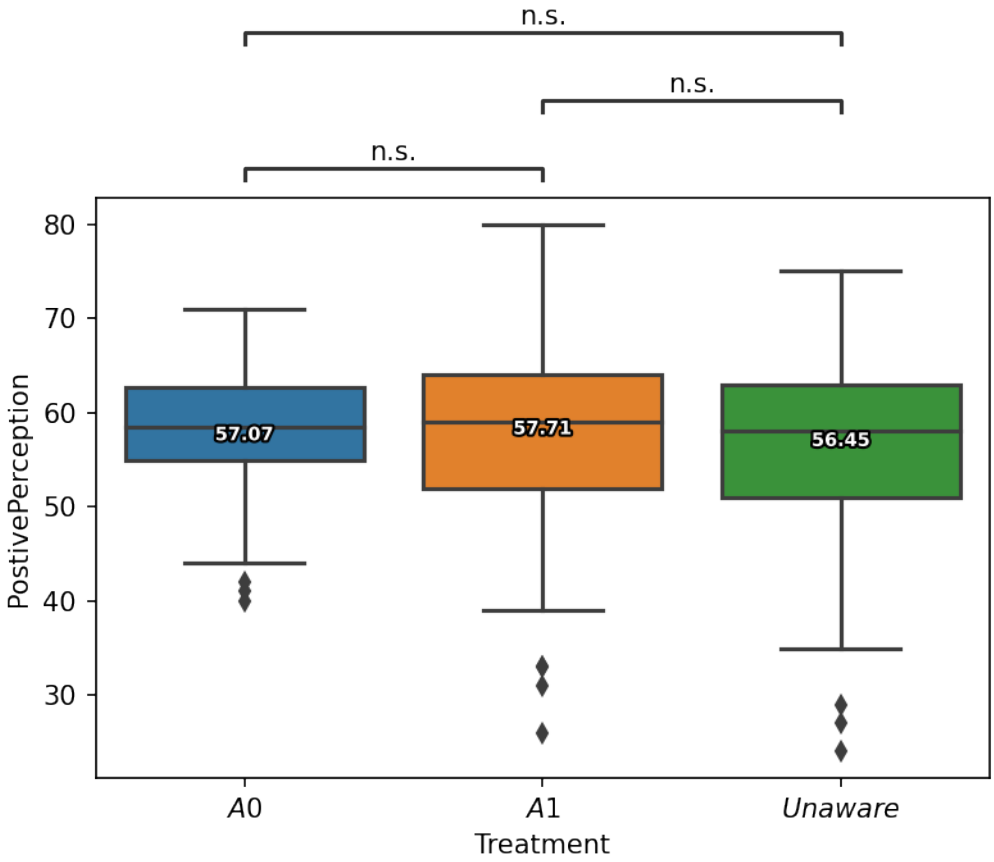
Pairwise Comparison	Coefficient	Hypothesis
Within $t = 0$		
$A0H$ - $R0$	2.73	H3 +
$A0L$ - $R0$	6.94*	H3 −
$A0H$ - $A0L$	-3.91	H3 +
Within $t = 1$		
$A1H$ - $R1$	1.51	H3 +
$A1L$ - $R1$	0.71	H3 −
$A1H$ - $A1L$	0.85	H3 +
Within $t = \infty$		
$A\infty H$ - $R\infty$	1.89	H3 +
$A\infty L$ - $R\infty$	2.57	H3 −
$A\infty H$ - $A\infty L$	-0.56	H3 +
Within A		
$A0H$ - $A1H$	-2.17	H4 −
$A0L$ - $A1L$	2.69	H4 +
$A1H$ - $A\infty H$	2.31	H5 +
$A1L$ - $A\infty L$	0.97	H5 ∼
Within R		
$R0$ - $R1$	-3.52	H4 ∼
$R1$ - $R\infty$	2.89	H5 ∼

Second group in the pair is the omitted dummy. *Within* X are the groups of hypotheses holding X fixed. +, −, and ∼ indicate a positive, negative, or neutral predicted effect respectively. ***=$p < 0.01$, **=$p < 0.05$, and *=$p < 0.10$.

4.1.3 Part 3: Other Personality Measures

The main test in Part 3 is to detect whether 'Data Use' of questionnaires to sort groups affects future responses. There are three groups, *'Expected'* Data Use (of their Part 1 personality responses) ($A0$), *'Unexpected'* Data Use ($A1$), and all treatments where subjects are *Unaware* of Data Use. I combine all of the characteristics elicited in Part 3 into one measure based on how likely it is they would be positively perceived by an observer. That is, I reverse

Figure 5: Positive Perception by Treatment



Positive Perception mean overlaid. Statistical results are based on a two-sided Mann Whitney test. ***=$p <$ 0.01, **=$p < 0.05$, and *=$p < 0.10$.

code the Dark Triad as these traits are negative, and leave the coding for Honest-Humility as it is. I call this combined measure 'Positive Perception'. As there are 16 questions elicited on a 5-point Likert scale, it can take a minimum value of 16, and a maximum of 80. I use a Mann-Whitney test to test for differences between the three relevant subject groups. Figure 5 summarizes the results of these comparisons. I find no evidence for Hypotheses 6, and 7, and 8, which suggests any unexpected data use of Part 1 responses does not contaminate later responses.

4.2 Secondary Analysis

It appears as though forming groups with high levels of Agreeableness is not effective in increasing contributions in the PGG. A natural question is why this is the case, given that previous studies have found a positive relationship between Agreeableness and contributions in the PGG.

To address this question, I regress an individual's Agreeableness on their contributions in the PGG, alongside other personality traits and controls. This analysis was pre-registered, but was in a lower 'Conceptual Replication' section, as it addresses the more broader research question of which individual characteristics affect pro-social contribution behavior. As it proves useful for the exposition of the paper, I have brought this statistical further up.

I use a mixed-effects panel tobit regression (censored at 0 and 25) clustered at the individual and group levels. The regression includes all of the elicited personality characteristics and demographics alongside treatment dummies and the average group contribution by others in the previous period.[13] I exclude the $A0$ treatment data from this regression, as I anticipated misrepresentation in Agreeableness in this treatment.[14] Table 8 lists the relevant coefficients from this regression, and suggests that an individual's Agreeableness score has essentially no impact on their contribution behavior. This is a surprising result as it is in contrast to previous studies that find Agreeableness is positively related to pro-social actions, including in the PGG. However, it is unsurprising then that the Agreeableness group formation rule by itself proved ineffective in increasing group contributions.

In terms of the other personality traits, Table 8 suggests that Open Mindedness and Extroversion increase an individual's contributions. Table 8 also suggests that Conscientiousness and Honestly Humility decrease contributions. In terms of demographics, those who have been at university for longer contribute less, and those in the Education or Engineering degrees contribute more. The other personality traits and demographics do not appear to substantially impact an individual's contribution rate. These results on individual characteristics should be viewed with an appropriate amount of skepticism given I did not

[13] Following Bardsley and Moffatt (2007), the initial lagged contribution in period 1 is found using a grid search.

[14] I still do this despite the fact that misrepresentation did not eventuate, as it was what I pre-registered.

form hypotheses for them in the pre-registration document.

Table 8: Individual Characteristics on Contributions

Ind. Variable	Coefficient
Lagged Avg. Group Cont.	0.61***
Agreeableness	0.02
Open Mindedness	0.30***
Negative Emotionality	0.11
Extroversion	0.21**
Conscientiousness	-0.25**
Honesty Humility	-0.29*
Machiavellianism	-0.23
Narcissism	-0.20
Psychopathy	0.08
Female	-0.14
2nd Year at Uni.	-1.86
3rd Year at Uni.	-3.84**
4th+ Year at Uni.	-3.73**
Grad. Student	-3.76*
GPA	-0.17
Economics	-0.71
Arts and Humanities	0.08
Natural Sciences	2.88
Education	4.87*
Engineering	6.30**
Law	-0.10
Social Sciences	-0.36
Medicine	1.18
Other	0.57

An individual's contribution to the public good is the dependent variable. Results are from a mixed-effects panel tobit regression (censored at 0 and 25) clustered at the individual and group levels. Controls for Treatment and Period are included in the regression but not listed. Observations from the $A0$ treatment are excluded. ***=$p < 0.01$, **=$p < 0.05$, and *=$p < 0.10$.

4.3 Exploratory Analysis

Naturally, the empirical findings of the experiment suggests additional analysis that is unanticipated and thus not in the pre-registered section above. It would be remiss to not follow the data where interesting results lie. In particular, I would like to establish the robustness of Agreeableness not having an effect on contributions, as this result stands in stark contrast

to the rest of the literature. To that end, I have run a variety of robustness checks on the analysis reported in Table 8.

Robustness Check	Coefficient (Std. err.)
All Data	0.04 (0.05)
No $A0$ or $A1$	0.02 (0.06)
No other Ind. Charact.	0.03 (0.05)
xtreg instead of metobit	0.03 (0.04)
Only Agreeableness	0.03 (0.04)
Simple Regression	0.00 (0.01)
Simple Regression, All Data	-0.01 (0.01)

Table 9: Robustness checks on Agreeableness and Contributions

An individual's contribution to the public good is the dependent variable. The coefficient of Agreeableness as an independent variable is displayed. The analysis is as specified in Table 8 except for the change described in the Robustness Check column. 'All Data' includes $A0$ observations. 'No $A0$ or $A1$' drops $A1$ observations. 'No other Ind. Charact.' drops all other personality and demographic measures as independent variables. 'xtreg instead of metobit' does not control for censoring or the nested subject/group/session relationship. 'Only Agreeableness' has Agreeableness as the only independent variable. 'Simple Regression' uses 'reg' in Stata, only has the independent variable of Agreeableness, and does not control for the panel nature of the data, censoring, or the nested subject/group/session relationship. 'Simple Regression, All Data' is the same as 'Simple Regression' but includes $A0$ observations. ***=$p < 0.01$, **=$p < 0.05$, and *=$p < 0.10$.

Table 9 reports the coefficient on Agreeableness as an independent variable on the dependent variable of the individual's contribution to the public good. It uses the same analysis as described in Section 4.2 as a base, but changes various factors as robustness checks. Table 9 shows the result of Agreeableness having no effect on contributions is very robust. There are very many robustness checks that could be conducted, however, the lack of result here looks like a strongly robust finding.

5 Conclusion

Using psychometric personality testing in the context of job hiring is a complex and sometimes controversial topic. These tests have become integral to modern hiring processes, helping firms to evaluate potential employees. However, a challenge is the incentive for job-seekers to tailor their responses to align with their beliefs of the employers' expectations. The incentive to strategically misrepresent one's preferences undermines the validity of such tests and their usefulness for job hiring decisions.

To shed light on this issue, I design and conduct an incentivized laboratory experiment that mirrors real-world hiring scenarios. I first elicit Big 5 characteristics through a questionnaire, much like what job-seekers have to fill out at some stage during the hiring process. I then use a standard PGG to represent a cooperative work environment. The Big 5 characteristic of Agreeableness has been found to positively impact contributions in previous studies, so sorting (or hiring) based on this trait makes sense. By changing the timing of the revelation of the sorting rule to before or after the initial questionnaire, I am able to quantify the level of misrepresentation and evaluate its subsequent impact on cooperative behavior.

I find that subjects do not misrepresent their personality in order to be placed into groups with higher levels of Agreeableness. This likely indicates that the preference for honesty outweighs the indirect benefits of being in a high-Agreeableness group. Without misrepresentation, the effectiveness of the Agreeableness group formation rule in improving contributions should be similar in the $A0$ and $A1$ treatments. My findings confirm this for both treatments. However, the effectiveness is similar in both H and L groups, and is ineffective in the $A\infty$ treatment. Therefore, in conjunction with the result that Agreeableness does not correlate with individual contributions, the higher contributions in $A0$ and $A1$ cannot be attributed to the formation of groups with higher Agreeableness. Instead, the operative factor is the provision of information about the Agreeableness trait and its positive relationship with contributions in the PGG. Lastly, I find that using personality tests in an unannounced way does not affect subsequent personality tests. Therefore, 'unexpected data use' remains a valid methodological tool for economics experiments when required by the

study design.

My paper highlights the importance of pre-registration, power analysis, and replication. My design is built upon the previous findings that Agreeableness is related to individual contributions in a PGG, and pro-social behavior more generally. This relationship has a handful of (unintentional) conceptual replications, however, they were conducted before the necessity of pre-registration and power analysis was widely known. In a pre-registered and well-powered study, I fail to replicate any relationship between Agreeableness and contributions in a PGG. Had this been established earlier, the rationale for forming groups based on Agreeableness would not exist, making an alternative design more suitable. In this project I have gone to great lengths to create an exhaustive pre-registration that has been public from the first day of data collection. I hope this serves as an example for future studies to try to emulate and improve upon. My findings raise a challenge to the broader literature on personality traits and economic behavior, as well as any other established literature that was published prior to the proliferation of pre-registration. We need to be sure that 'classical' results that are generally accepted have been independently replicated by more than one pre-registered study.

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A Deviations from the Pre-registration Document

Any deviations from what was pre-registered will be documented here.

B Conceptual Replication Results

The current experimental design permits a conceptual replication of some elements of McGee and McGee (2022b). In particular, Research Questions 1 and 2 from that paper can be partially answered.

MM Research Question 1: *How important are incentives when measuring personality?*

The incentives in McGee and McGee (2022b) were a direct lump-sum payment if selected for a job. Whereas in the current study the incentive is indirect, as it is membership in the more cooperative H group that could increase earnings in the PGG. Research Question 1 is addressed by Hypothesis 1 and the comparisons in Figure 3. As there is no evidence for Hypothesis 1, I conclude that indirect incentives are not very important when measuring personality.

MM Research Question 2: *Are incentivized personality measures influenced by traits other than personality?*

McGee and McGee (2022b) posit that traits such as intelligence, Machiavellianism, self-deception, optimism, acceptability of lying, risk aversion, and locus of control could be correlated with misrepresentation. They find that most of these characteristics are uncorrelated with misrepresentation in all treatments of their experiment.

In particular, McGee and McGee (2022b) find no evidence that Machiavellianism is correlated with misrepresentation in any of their treatments. This is an interesting result, given that people high in Machiavellianism tend to be manipulative and strategically self-serving in their words and actions. In this paper, Part 3 elicits Machiavellianism using a different set of questions, and its relationship to misrepresentation of Agreeableness in Part 1 can be explored. I test this relationship with a Tobit regression censored at 26 and 130[15] of the following form $Agreeableness_i = \beta_0 + \beta_1 Machiavellianism + \beta_2 Machiavellianism \times A0 + \epsilon_i$. The coefficient β_1 represents the correlation between Agreeableness and Machiavellianism, and β_2 represents the increase (if > 0) in reported Agreeableness when there is an incentive to misrepresent (i.e. in $A0$).[16]

Table A1 summarizes and shows that Agreeableness is increasing in Honesty Humility

[15] The minimum and maximum value that Agreeableness can take in this experiment.

[16] A simulation-based power analysis suggests a minimum detectable effect size of around 1 unit.

Table A1: Personality Traits and Agreeableness Misrepresentation

Trait	β_1	β_2
Honesty Humility	0.57**	0.07
Machiavellianism	-1.10***	0.21
Narcissism	-0.32	0.17
Psychopathy	-2.14***	0.14

β_1 represents the correlation between the trait and Agreeableness, and β_2 represents the correlation between the trait and misrepresentation of Agreeableness.***=$p < 0.01$, **=$p < 0.05$, and *=$p < 0.10$.

and decreasing in the Dark Triad traits, as expected. However, I find no evidence that any of these personality traits affect misrepresentation of Agreeableness.

C Power Analysis

For each statistical test, I conduct a simulation-based power analysis in order to determine the minimum detectable effect size that attains 80% power given a $\alpha = 0.05$ rejection threshold. The simulations are conducted in the same code as the statistical analysis that was attached to the pre-registration. I report the minimum effect size for each test below.

C.1 Part 1

The outcome of interest is each subject's Agreeableness score, calculated from their responses to the Part 1 questions. Each of the three groups of treatments is tested using a Mann-Whitney test, with each subject being an independent observation. The power analysis for the $R0$ to $A0$ comparison suggests a minimum detectable effect size of 7.7 units, and for the $A0$ *Others* comparison it is 5.1. These minimum detectable effect sizes are reasonable given they represent an average change of one or two out of the 26 Agreeableness questions being flipped from 1 to 5.

C.2 Part 2

I conduct a more sophisticated statistical analysis that uses a panel data approach in order to utilize more of the data while accounting for the underlying dependencies. I use the group's average contribution in a period as the dependent variable, and a treatment dummy alongside the period for the independent variables. I use a panel-data Tobit regression for the possible censoring that occurs at 0 and 25 tokens for upper and lower limits respectively. For each relevant comparison between two treatments (or group types within a treatment), I run the regression using only data from the pair that is being considered. The power analysis suggests a minimum detectable treatment effect size of 1.7 tokens.

C.3 Part 3

I combine all of the characteristics elicited in Part 3 into one measure based on how likely it is they would be positively perceived by an observer. That is, I reverse code the Dark

Triad as these traits are negative, and leave the coding for Honest-Humility as it is. I call this combined measure 'Positive Perception'. As there are 16 questions elicited on a 5-point Likert scale, it can take a minimum value of 16, and a maximum of 80. I use a Mann-Whitney test to test for differences between the three relevant subject groups. A power analysis suggests minimum detectable effect sizes of 6.3 and 5.2 respectively.

D Instructions

Full Instructions are provided in the OSF project and/or replication packet - either in the oTree code or a .docx file for the paper instructions. Selected parts of the Instructions that are particularly relevant for the understanding of the experiment are presented below.

D.1 Part 1

D.1.1 First Screen

This experiment will have two parts.

Part 1 will be a set of questions about yourself. We ask that you answer these questions accurately.

Part 2 has 15 decision rounds. A brief summary of one decision round follows:

- Subjects are in groups of 3
- Each subject has 25 tokens that they divide between their Private Account or a Cooperation Account
- Each token placed in their Private Account earns that subject 10 points.
- Each token placed in the Cooperation Account earns the entire group 12 points.
- Everyone in the group receives an equal portion of the earnings from the Cooperation account, that is, they earn 12*1/3=4 points per token in the Cooperation Account.

This is only a basic outline of Part 2. More instructions will be provided before starting Part 2.

[$t > 0$ Treatments:]

We will now start with Part 1 - the set of questions about yourself.

D.1.2 Second Screen

[Second Screen only in $t = 0$ Treatments]

[*R*andom Treatments:] For Part 2, you will be assigned to a group of three **randomly**.

[*A*greeableness Treatments:] For Part 2, you will be assigned to a group of three **based on your 'Agreeableness' score. Your Agreeableness score is determined by your responses to particular questions in Part 1.**

Agreeableness is a personality trait where people high in Agreeableness are often described as *selfless, trusting, good-natured, generous, and forgiving.* (Costa, McCrae, & Dembroski, 1989)

In scientific studies, **a high level of Agreeableness has been found to have a positive effect on group cooperation decisions** similar to the type in Part 2.

[References button with pop-up window that states:

Perugini, Tan, & Zizzo in Economic Issues, Volume 15, Part 1, 2010.

Volk, Thöni, & Ruigrok in the Journal of Economic Behavior & Organization, Volume 81, Issue 2, 2012.

Kagel & McGee in Economics Letters, Volume 124, Issue 2, 2014.

Thielmann, Spadaro, & Balliet in Psychological Bulletin, Volume 146, Issue 1, 2020.]

Each group of three is formed from six randomly selected subjects. **The three subjects with the highest Agreeableness scores will be assigned to one group, and the remaining three subjects to the other group.**

[All $t = 0$ treatments:] Each group of three will remain together for all 15 decisions in Part 2.

We will now proceed with Part 1 - the set of questions about yourself.

D.2 Part 2

[$t \leq 1$ Treatments:]

[*A*greeableness treatments:] For Part 2, you will be assigned to a group of three **based on your 'Agreeableness' score. Your Agreeableness score is determined by your responses to particular questions in Part 1.** Agreeableness is a personality trait where people high in Agreeableness are often described as *selfless, trusting, good-natured, generous, and forgiving.* (Costa, McCrae, & Dembroski, 1989) In scientific studies, **a high level of Agreeableness has been found to have a positive effect on group cooperation decisions** similar to the type in Part 2.

Each group of three is formed from six randomly selected subjects. **The three subjects with the highest Agreeableness scores will be assigned to one group, and the remaining three subjects to the other group.**

[*R*andom treatments:] For Part 2, you will be assigned to a group of three **randomly**.

D.3 Part 3

Parts 1 and 2 of the experiment are now complete.

We ask you to fill out a final short survey, before your final earnings are displayed. Your final earnings have already been calculated and set.

There are no further parts to the experiment after this final survey.

E Personality Questions

The 50 questions in Part 1 are taken from the 30 question 'BFI-2-S Inventory' (Soto & John, 2017), and the 20 questions on Agreeableness from the International Personality Item Pool's (IPIP) '100-Item Lexical Big-Five Factor Markers' (Goldberg, 2002; Goldberg et al., 2006). The 16 questions in Part 3 are taken from the 'Dirty Dozen' (Jonason & Webster, 2010) and four Honesty-Humility questions from HEXACO's 60-item version (Lee & Ashton, 2009). Subjects are asked how much they agree each statement applies to them using a 5-point Likert scale (Likert, 1932). The 5 points are labeled: 1 = Disagree strongly, 2 = Disagree a little, 3 = Neither agree nor disagree, 4 = Agree a little, and 5 = Agree strongly. They are presented using horizontal radio buttons. Subjects face blocks of five questions on a page, and all questions are presented in a random order that differs across subjects.[17] Personality traits are scored based on each subject's numerical (i.e. 1-5) responses by the following formula: $Trait = \sum_{i \in Q}(LikertValue_{+veKey} + (6 - LikertValue_{-veKey}))$, where Q is the set of relevant questions to that trait. Appendices E.1 and E.2 report which questions are related to each trait and whether the questions are positively or negatively keyed.

As the experiment is conducted in Austria the experiment is conducted in German. While the majority of the university students that make up the subject pool are fluent in English, it is important to conduct personality tests in their native language. Firstly, there will be heterogeneity in subjects' confidence or ability in using English. Secondly, there is a literature that suggests that elicited personality traits are different in bilingual speakers depending on what language is being used (see Dylman and Zakrisson (2023) for examples). I would rather observe a subject's 'regular' personality rather than one that is shaped by a foreign language. Strategic misrepresentation of personality is already likely to be difficult enough as it is, let alone with an additional levels of complexity on top of that. The question sets used in Parts 1 and 3 all have pre-existing German translations. Rammstedt et al. (2020) translate the BFI-2-S. Streib and Wiedmaier (2001) translate the 100-Item IPIP. Küfner, Dufner, and Back (2015) translate the Dirty Dozen. A translation for HEXACO is provided by Lee and Ashton (2009). A list of the questions and their translations are

[17] Technically it is possible that two subjects face exactly the same ordering, however this is unlikely as the probability of that occurring in Part 1 is $p = \frac{1}{50!}$ and in Part 3 $p = \frac{1}{16!}$.

provided in Appendices E.1 and E.2.

Some questions were changed or removed. In Part 1, a question was changed slightly to avoid excessive repetition, from *'I am compassionate and soft-hearted'* to *'I am compassionate'*, as another question is *'I have a soft heart'*. Two less relevant questions from Honesty-Humility were removed in order to maintain an equal number of questions between each trait in Part 3. The removed questions are *'I'd be tempted to use counterfeit money, if I were sure I could get away with it.'* and *'If I knew that I could never get caught, I would be willing to steal a million dollars.'*

Some of the pre-existing translations were changed based on feedback from native German speakers. The question *'I have a soft heart'* was changed from *'Ich habe ein weiches Herz'* to *'Ich bin gutherzig'*. The question *'I have a good word for everyone'* was changed from *'Ich habe ein gutes Wort für jeden'* to *'Ich rede gut über andere'*. These two changes were implemented as the original translations were considered ambiguous and a little too literal. The question *'I make people feel at ease'* was changed from *'Ich mache andere Leute ungezwungen'* to *'Ich kann andere beruhigen'*. Ungezwungen can be interpreted as being unhinged rather than calm, and may also be grammatically incorrect. The question *'Ich habe getäuscht oder gelogen, um meinen Willen durchzusetzen'* was changed to *'Ich neige dazu, zu täuschen oder zu lügen, um meinen Willen durchzusetzen'*, and similarly the question *'Ich habe Schmeicheleien genutzt, um meinen Willen durchzusetzen'* to *'Ich neige dazu, Schmeicheleien zu benutzen, um meinen Willen durchzusetzen'*. The other questions in the Dirty Dozen all have *'Ich neige dazu'* (I have the tendency to), and I was concerned that the question about lying could be interpreted as whether they have been deceitful in the current experiment, rather than a tendency in general.

E.1 Part 1 Questions and Translations

English Questions	German Questions
Agreeableness Positively Keyed	
I am interested in people.	Ich interessiere mich für Leute.
I sympathize with other's feelings.	Ich kann die Gefühle anderer nachempfinden.
I have a soft heart.	Ich bin gutherzig.
I take time out for others.	Ich nehme mir Zeit für andere.
I feel other's emotions	Ich kann die Gefühle anderer nachfühlen.
I make people feel at ease.	Ich mache andere Leute ungezwungen.
I inquire about other's well-being.	Ich erkundige mich nach dem Wohlbefinden anderer.
I know how to comfort others.	Ich weiß wie ich andere trösten kann.
I love children.	Ich liebe Kinder.
I am on good terms with nearly everyone.	Ich komme mit fast jedem gut aus.
I have a good word for everyone.	Ich rede gut über andere.
I show my gratitude.	Ich zeige meine Dankbarkeit.
I think of others first.	Ich denke zuerst an andere.
I love to help others.	Ich liebe es anderen zu helfen.
I am compassionate.	Ich bin einfühlsam.
I assume the best about people.	Ich schenke anderen leicht Vertrauen, glaube an das Gute im Menschen.
I am respectful and treat others with respect.	Ich begegne anderen mit Respekt.
Agreeableness Negatively Keyed	
I insult people.	Ich beleidige Leute.
I am not interested in other people's problems.	Ich interessiere mich nicht für die Probleme anderer Leute.
I feel little concern for others.	Andere Menschen kümmern mich wenig.
I am not really interested in others.	Ich interessiere mich nicht wirklich für andere.
I am hard to get to know.	Mich kennenzulernen ist schwer.
I am indifferent to the feelings of others.	Ich bin den Gefühlen anderer gegenüber gleichgültig.
I am sometimes rude to others.	Ich bin manchmal unhöflich und schroff.
I can be cold and uncaring.	Andere sind mir eher gleichgültig, egal.
I tend to find fault with others.	Ich neige dazu, andere zu kritisieren.
Extraversion Positively Keyed	
I am dominant and act as a leader.	Ich neige dazu, die Führung zu übernehmen.
I am full of energy.	Ich bin voller Energie und Tatendrang.
I am outgoing and sociable.	Ich gehe aus mir heraus, bin gesellig.
Extraversion Negatively Keyed	
I tend to be quiet.	Ich bin eher ruhig.
I prefer to have others take charge.	In einer Gruppe überlasse ich lieber anderen die Entscheidung.
I am less active than other people.	Ich bin weniger aktiv und unternehmungslustig als andere.

Table A2: Part 1 Questions 1-32

English Questions	German Questions
Conscientiousness Positively Keyed	
I am reliable and can always be counted on.	Ich bin verlässlich, auf mich kann man zählen.
I keep things neat and tidy.	Ich mag es sauber und aufgeräumt.
I am persistent and work until a task is finished.	Ich bleibe an einer Aufgabe dran, bis sie erledigt ist.
Conscientiousness Negatively Keyed	
I tend to be disorganized.	Ich bin eher unordentlich.
I have difficulty getting started on tasks.	Ich neige dazu, Aufgaben vor mir herzuschieben.
I can be somewhat careless.	Ich bin manchmal ziemlich nachlässig.
Negative Emotionality Positively Keyed	
I worry a lot.	Ich mache mir oft Sorgen.
I tend to feel depressed and blue.	Ich bin oft deprimiert, niedergeschlagen.
I am temperamental and get emotional easily.	Ich reagiere schnell gereizt oder genervt.
Negative Emotionality Negatively Keyed	
I am emotionally stable and not easily upset.	Ich bin ausgeglichen, nicht leicht aus der Ruhe zu bringen.
I am relaxed and handle stress well.	Ich bleibe auch in stressigen Situationen gelassen.
I feel secure and comfortable with myself.	Ich bin selbstsicher, mit mir zufrieden.
Open-mindedness Positively Keyed	
I am fascinated by art, music, or literature.	Ich kann mich für Kunst, Musik und Literatur begeistern.
I am original and come up with new ideas.	Ich bin originell, entwickle neue Ideen.
I am complex and a deep thinker.	Es macht mir Spaß, gründlich über komplexe Dinge nachzudenken und sie zu verstehen.
Open-mindedness Negatively Keyed	
I have little interest in abstract ideas.	Mich interessieren abstrakte Überlegungen wenig.
I have few artistic interests.	Ich bin nicht sonderlich kunstinteressiert.
I have little creativity.	Ich bin nicht besonders einfallsreich.

Table A3: Part 2 Questions 33-50

E.2 Part 3 Questions and Translations

English Questions	**German Questions**
Narcissism Positively Keyed	
I tend to want others to admire me.	Ich neige dazu, von anderen bewundert werden zu wollen.
I tend to want others to pay attention to me.	Ich neige dazu, von anderen beachtet werden zu wollen.
I tend to expect special favors from others.	Ich neige dazu, besondere Gefälligkeiten von anderen zu erwarten.
I tend to seek prestige or status.	Ich neige dazu, nach Ansehen oder Status zu streben.
Psychopathy Positively Keyed	
I tend to lack remorse.	Ich neige dazu, keine Gewissensbisse zu haben.
I tend to be callous or insensitive.	Ich neige dazu, gefühllos oder unsensibel zu sein
I tend to not be too concerned with morality or the morality of my actions.	Ich neige dazu, mich nicht um die Moral meiner Handlungen zu kümmern.
I tend to be cynical.	Ich neige dazu, zynisch zu sein.
Machiavellianism Positively Keyed	
I have used deceit or lied to get my way.	Ich neige dazu, zu täuschen oder zu lügen, um meinen Willen durchzusetzen.
I tend to manipulate others to get my way.	Ich neige dazu, andere zu manipulieren, um meinen Willen durchzusetzen.
I have used flattery to get my way.	Ich neige dazu, Schmeicheleien zu benutzen, um meinen Willen durchzusetzen.
I tend to exploit others towards my own end.	Ich neige dazu, andere für meine Zwecke auszunutzen.
Honesty-Humility Positively Keyed	
I wouldn't use flattery to get a raise or promotion at work, even if I thought it would succeed.	Ich würde keine Schmeicheleien benutzen, um eine Gehaltserhöhung zu bekommen oder befördert zu werden, auch wenn ich wüsste, dass es erfolgreich wäre.
I wouldn't pretend to like someone just to get that person to do favors for me.	Ich würde nicht vortäuschen, jemanden zu mögen, nur um diese Person dazu zu bringen, mir Gefälligkeiten zu erweisen.
I would never accept a bribe, even if it were very large.	Ich würde niemals Bestechungsgeld annehmen, auch wenn es sehr viel wäre.
Honesty-Humility Negatively Keyed	
If I want something from someone, I will laugh at that person's worst jokes.	Wenn ich von jemandem etwas will, lache ich auch noch über dessen schlechteste Witze.

Table A4: Part 3 Questions