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When the cat's away, some mice will play: A basic trait account of dishonest behavior

Benjamin E. Hilbig [a,*], Ingo Zettler [b]

[a] University of Koblenz-Landau and Max Planck Institute for Research on Collective Goods, Germany
[b] University of Copenhagen, Denmark

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ABSTRACT

In line with every-day observation, research has established substantial individual differences in ethical behavior, especially dishonesty and cheating. However, these individual differences have remained mostly unexplained, especially in terms of traits as specified in models of basic personality structure. Theoretically, a prime candidate to account for these differences is the Honesty–Humility factor proposed as the sixth basic personality dimension within the HEXACO Model of Personality. Despite clear theoretical links, corresponding behavioral evidence is scarce and limited due to methodological caveats. In a series of six behavioral experiments we thus bridge the gap between behavioral ethics and personality research – critically testing whether individual differences in dishonest behavior can be accounted for by basic traits in general, and Honesty–Humility in particular. We implement different cheating paradigms, tasks, incentive structures, samples, and sets of covariates to evaluate the robustness and generality of results. Overall, variance in dishonest behavior was indeed accounted for by Honesty–Humility which was the only consistent predictor of cheating across the various experimental setups and beyond relevant covariates including other personality factors. The results thus corroborate that individual differences in ethical behavior can be accommodated by comprehensive models of personality structure in general and the Honesty–Humility factor in particular.

1. Introduction

Dishonesty, deceit, and fraud are among the most severe social and economic challenges societies face (Mazar & Ariely, 2006). Indeed, few would fail to name at least one athlete recently convicted of doping, one company violating antitrust laws, or one politician accused of corruption or involved in some scandal. Correspondingly, the study of behavioral ethics at the intersection of economics, psychology, and other social sciences has seen an upsurge of interest in recent years (e.g. Bryan, Adams, & Monin, 2013; Gino & Ariely, 2012; Mazar, Amir, & Ariely, 2008; Peer, Acquisti, & Shalvi, 2014; Shalvi, Eldar, & Bereby-Meyer, 2012). So far, many important determinants of deceitful behavior have been uncovered (for an overview, see Bazerman & Gino, 2012) and research has consistently revealed that most people are willing to cheat at least a little – though mostly engaging in relatively "minor" transgressions that preserve a positive self-view (Hilbig & Hessler, 2013; Mazar et al., 2008; Shalvi, Handgraaf, & De Dreu, 2011). Nonetheless, the sum of these small transgressions incurs societal costs in the billions annually. For example, the U.S. Internal Revenue Service estimates that close to 200 billion USD are lost annually to individual income tax evasion, that is, underreporting of tax owed (Mazur & Plumley, 2007).

Importantly, empirical findings also confirm the everyday intuition that there are noteworthy individual differences in the extent of dishonest behavior: For example, in a large-scale study ($N = 746$) by Fischbacher and Heusi (2008), about 39% of participants were completely honest, that is, they were unwilling to misreport the outcome of a hidden dice-roll to their advantage. By contrast, up to 22% of participants were completely dishonest, misreporting to the very maximum (larger reported outcomes were associated with larger payoffs). All others showed some intermediate degree of cheating, thus misreporting to some extent while avoiding extreme maximization. However, in most of the behavioral ethics literature, such individual differences are merely acknowledged, but rarely linked to theoretically well-established dispositional factors or personality traits.

The latter, in turn, is arguably one of the primary tasks for personality research: To account for variation in relevant "actual" behavior (King, 2010) – the imperative criterion for all

* Corresponding author at: Cognitive Psychology Lab, Department of Psychology, University of Koblenz-Landau, Fortstraße 7, 76829 Landau, Germany.
E-mail address: hilbig@uni-landau.de (B.E. Hilbig).

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psychological research (Funder, 2009a). Over a decade ago, Funder (2001) noted that "the catalog of basic facts concerning the relationships between personality and behavior remains thin" (p. 213) and more recent samples of personality research turned out no different (Baumeister, Vohs, & Funder, 2007; Furr, 2009). Provocatively speaking, one might conclude that actual behavior has remained an elusive criterion in personality research. At the same time, there are important to-be-explained individual differences in relevant behavior. In particular, personality traits have been largely ignored – and individual differences thus remained unaccounted for – in the behavioral ethics field. It is thus straightforward to attempt to bridge the two disciplines.

So far, the few existing approaches considering individual differences in dishonesty have focused on rather specific or narrow constructs with little theoretical overlap such as moral identity (Aquino, Freeman, Reed, Lim, & Felps, 2009; Aquino & Reed, 2002), regulatory focus (Gino & Margolis, 2011), or creativity (Gino & Ariely, 2012). Thus, individual differences in ethical behavior have not been systematically related to or explained through broad, basic traits as conceptualized in models of personality structure. In turn, to avoid construct inflation in personality theory, models of basic personality structure – that specify the "latitudes and longitudes" for the study of individual differences (e.g. Ozer & Reise, 1994, p. 361) – should be among the first to be considered. Indeed, the study of personality has greatly profited from the development of such models (Funder, 2001) – given that they are sufficiently broad to cover the many ways in which individuals differ while subsuming these many variants in a few basic traits or factors to achieve theoretical parsimony.

Most strikingly, recent advances in theories of personality structure actually imply that individual differences in honesty, morality, and prosociality represent a core dimension of personality: These aspects are subsumed in the Honesty–Humility factor (e.g. Ashton et al., 2004; Lee & Ashton, 2008) as conceptualized within the HEXACO Model of Personality (Ashton & Lee, 2007; Ashton, Lee, & De Vries, 2014). Stated briefly, the HEXACO model originated from lexical studies across several languages and cultures (Ashton et al., 2004; Lee & Ashton, 2008) the results of which suggested an extension and slight variation of the more widely known Big Five approach (e.g. McCrae & John, 1992). Several more fine-grained alterations notwithstanding (Lee & Ashton, 2004; Lee, Ashton, Ogunfowora, Bourdage, & Shin, 2010), addition of the Honesty–Humility factor represents the most striking difference whereas the other five factors of the HEXACO model either resemble (Emotionality, Agreeableness) or exactly match (eXtraversion, Conscientiousness, and Openness to Experience) the classical five factors in terms of factor content. In what follows, we will describe the proposed sixth personality factor, Honesty–Humility, in more detail and argue that it is the quintessential basic trait to account for individual differences in (un)ethical behavior. Next, we briefly describe the scarce evidence on this association and finally present a series of experiments that aim to fill this gap and overcome limitations of prior work. As such, we aim to demonstrate that individual differences in dishonest behavior can indeed be accounted for by a broad, basic trait as specified within a general model of personality structure.

1.1. Honesty–Humility and dishonest behavior

Honesty–Humility can be understood to subsume individual differences in morality, covering socially desirable attributes such as being sincere, faithful, and honest versus sly, deceitful, and greedy (Ashton & Lee, 2008a; Lee & Ashton, 2012). Generally speaking, Honesty–Humility has been defined as "the tendency to be fair and genuine in dealing with others" (Ashton & Lee, 2007, p. 156), thus representing people's willingness to refrain from exploiting others or bending rules and norms – even if such actions would be individually beneficial and bear little risk of retaliation or sanctions (Hilbig & Zettler, 2009). Indeed, various studies have demonstrated that this sixth basic factor accounts for variance in socially desirable outcomes and behavior – often beyond the influence of the remaining five factors within the HEXACO model and/or the classic Big Five (De Vries, De Vries, De Hoogh, & Feij, 2009; Lee & Ashton, 2005; Lee, Ogunfowora, & Ashton, 2005). Specifically, Honesty–Humility has been associated with more prosocial behavior and cooperativeness (Hilbig, Glöckner, & Zettler, 2014; Hilbig, Thielmann, Hepp, Klein, & Zettler, 2015; Zettler, Hilbig, & Heydasch, 2013), less socio-sexuality and fewer sexual quid pro quos (Ashton & Lee, 2008b; Lee et al., 2013), more moral behavior and honest responding (Hilbig, Moshagen, & Zettler, 2015) as well as higher integrity, less counterproductive work behavior, and other related criteria (Lee, Ashton, & De Vries, 2005; Marcus, Lee, & Ashton, 2007; Zettler & Hilbig, 2010). However, the extant evidence is rather indirect due to predominant reliance on self- and observer-report data and none of the above studies have specifically considered dishonest *behavior* as the criterion.

The only exception is a study by Hershfield, Cohen, and Thompson (2012, Study 4) which – as a sidelined aspect – found that Honesty–Humility negatively predicts self-scored performance. That is, participants were asked to solve eight anagrams within 15 min, losing part of their monetary endowment for each unsolved anagram. Importantly, anagrams were to be solved in the provided order and both the second and seventh anagrams were very difficult (though not impossible) to solve, i.e. very uncommon words. Participants self-scored their performance and were paid correspondingly. An unrealistically high level of self-scored performance – i.e. exceeding one solved anagram – was considered to be indicative of dishonesty. Results showed a medium-sized negative effect of Honesty–Humility on this performance measure.

Despite this encouraging piece of evidence, it must be noted that Hershfield et al. did not design their study with the intent to provide a basic trait account of individual differences in dishonesty. As such, three limitations remain that need to be addressed – apart from the vital and often dangerously neglected necessity of replication per se (Asendorpf et al., 2012; Johnson, 2013; Pashler & Wagenmakers, 2012). First, it is difficult to interpret performance on Hershfield et al.'s anagram task, primarily because it is unknown how many participants actually (thought they) solved the difficult anagrams. In turn, in line with many experimental designs used in the behavioral ethics field (Gino, Ayal, & Ariely, 2009; Mazar et al., 2008), a paradigm is needed in which (alleged) performance can be compared against some well-defined baseline, i.e. a performance level to be conclusively expected if no cheating occurred. Second, cheating was self-incriminating: Even and especially if participants did take the instructions seriously and realize they had not solved more than one anagram, claiming to have done so was tantamount to a barefaced lie that is obvious to the experimenter. As is well established in indirect questioning research (Lensvelt-Mulders, Hox, Van der Heijden, & Maas, 2005; Moshagen, Hilbig, & Musch, 2011), the mere fact that a response is deterministically self-incriminating is sufficient to produce substantial effects of social desirability. In turn, a paradigm is needed in which the responses of any one individual can never be conclusively linked to (dis)honesty although the degree of dishonesty can still be estimated on the aggregate (so long as the average extent of random noise is conclusively known, e.g. Moshagen, Hilbig, Erdfelder, & Moritz, 2014). Third, participants in Hershfield et al.'s study cheated only to avoid losses (and essentially had to cheat at least once to avoid loosing 88% their endowment). Given that gains and losses are neither perceived nor treated equally

(Kahneman & Tversky, 1979, 1984), it is unclear whether the pattern extends to the common cases in which dishonesty is resorted to in an attempt to increase one's gains.

Note that none of these limitations should be taken as general refutations of Hershfield et al.'s (2012) findings – especially since their study was not primarily designed to link personality traits with dishonest behavior. Nonetheless, the question of whether individual differences in dishonest behavior can be accounted for through a broad trait specified in a model of basic personality structure remains an open one. Our overall aim was thus to provide a series of critical experimental tests – across different paradigms, methods, and samples. More generally speaking, we aimed to bridge the relatively large gap between behavioral ethics and personality research.

2. Experiment 1

The first experiment was designed as an extended replication of Hershfield et al.'s (2012) study, implementing a paradigm in which (a) a randomized baseline condition provided an assessment of performance against which to compare self-reported performance from the cheating condition, (b) cheating could not be deterministically inferred on the individual level (thus increasing anonymity), and (c) gains could also be increased (rather than only losses avoided) through cheating. To this end, we used a variant of paradigms comparing actual to alleged performance that have been used successfully both in previous personality research (e.g., Hilbig, Heydasch, & Zettler, 2014; Paulhus & Williams, 2002) and the behavioral ethics field (e.g., Gino & Margolis, 2011; Mead, Baumeister, Gino, Schweitzer, & Ariely, 2009). Specifically, participants were given the task to judge the truth of trivia statements in each of two conditions (manipulated within participants). In the *cheating condition*, participants were asked to decide whether each statement was factually correct or false. However, they were not asked to provide this judgment but instead instructed to press a button labeled "I have made my decision – display solution now". Upon pressing the button, the factual solution ("true" or "false") was displayed and participants were then asked to report whether or not their original judgment had been correct. For each (allegedly) correct judgment they earned €0.20 (approx. $0.30 at the time of data collection), whereas €0.20 were subtracted from their winnings if they reported that their original judgment had been incorrect. Participants thus had the possibility to falsely claim that their original judgment had been correct and thus increase their winnings through over-reporting (for a similar paradigm, see Schurr, Ritov, Kareev, & Avrahami, 2012).

By contrast, in the subsequent *baseline condition*, participants were asked to provide their actual judgments for another set of statements by pressing the respective button (labeled "true" or "false") for each statement. For every factually correct judgment, they again earned €0.20, whereas €0.20 were subtracted from their winnings for every incorrect one. Thus, in this baseline condition, participants had no possibility to cheat. Importantly, statements were randomly assigned to the cheating and the baseline condition, respectively, with a new random assignment per participant. So, across participants, any differences in statement difficulty between the conditions will approach zero. As a result, on the aggregate level, a higher performance in the cheating condition must be attributed to over-reporting and thus cheating (Schurr et al., 2012). For example, if *all* participants were to perform twice as well in the cheating as compared to the baseline condition (say, 20 versus 10 correct judgments), we could deduce conclusively that cheating occurred and estimate its exact extent (namely, 50% illegitimate responses in the cheating condition). At the same time, there was no deterministic link between an individual's performance in the two conditions and cheating: Any single individual could have received easier statements in the cheating condition and thus performed better as compared to the baseline condition. An advantageous difference between the conditions therefore does not indicate cheating deterministically (that any one participant must have cheated), but only probabilistically – thereby protecting potential cheaters by rendering high performance non-incriminating.

2.1. Participants, materials, and procedure

Based on the findings of Hershfield et al.'s (2012) study, a medium-sized effect of Honesty–Humility on cheating seemed plausible. However, given the probabilistic link between observable behavior and cheating as implemented herein, a smaller effect size must be expected. Thus, we computed the sample size required to detect a small-to-medium-sized effect ($f^2 = .08$) with high statistical power ($1 - \beta = .95$) using G*power (Faul, Erdfelder, Buchner, & Lang, 2009). The required sample size was 165. Thus, a total of 168 participants (106 female, aged 18 to 40 years, $M = 22$, SD = 3.5 years) taking part in otherwise unrelated batteries of experimental tasks were recruited from the campus of a German university and a local participant pool. Practically all (95%) were students from diverse fields, mostly from the social sciences (40%) or business/economics (25%).

After providing consent and demographical information, participants were first asked to complete the German version of the HEXACO Personality Inventory-Revised (Lee & Ashton, 2004; Lee & Ashton, 2006) assessing each factor with 16 items (e.g., Zettler, Hilbig, & Haubrich, 2011), displayed on the computer screen (the items can be found at hexaco.org). Next, they worked on a set of different filler-tasks (not pertinent to this investigation), lasting between 20 and 30 min in total. None of these tasks provided any opportunity to cheat. Finally, participants completed the critical trivia-statements judgment task as described above. In each condition, they were shown a total of 20 trivia statements one at a time and in random order. For each condition and participant, statements were randomly drawn from a larger set of 80 statements to avoid effects of specific items. To further increase comparability across the conditions, it was ensured that there were 25% easy statements (e.g. "The Vatican is in Rome") and 75% difficult ones (e.g. "Nolan Bushnell founded the company ATARI") in each final set (based on average performance scores from prior studies having used these materials in similar populations, Hilbig, 2012; Unkelbach, 2007). Next, participants were paid contingent on their performance. Importantly, participants' overall performance across *both* conditions was automatically computed by the experimental software and only this overall result was displayed to the experimenter who paid participants, thus further protecting anonymity. It was clear to participants that their payment was based on their summary performance (across conditions). Participants earned €3.00 (approx. $4.00) on average in the total task which lasted about 15 min. Finally, participants were thanked and debriefed.

2.2. Results and discussion

Descriptive statistics of and correlations between the six HEXACO factors and participants' performance (i.e. proportion of factually correct judgments in the baseline, and proportion of *allegedly* correct judgments in the cheating condition) are reported in Table 1. As can be seen, participants performed better in the cheating as compared to the baseline condition (MD = .12, $SE_{MD} = .01$, $t(167) = 9.9$, $p < .001$), resembling a large effect size (Cohen's $d = .77$). Given the random assignment of statements to the two conditions, this difference clearly implies that cheating occurred, though to a limited degree (over-reporting by about 2–3

Table 1
Experiment 1: Means (standard deviations in parenthesis) and bivariate correlations of all variables with internal consistency reliabilities (Cronbach's alpha) in the diagonal.

	M (SD)	1	2	3	4	5	6
1. Honesty–Humility	3.37 (.56)	.80					
2. Emotionality	3.34 (.56)	.16*	.83				
3. Extraversion	3.63 (.58)	−.05	−.02	.88			
4. Agreeableness	2.95 (.50)	.34***	−.28***	.11	.81		
5. Conscientiousness	3.40 (.50)	.05	.10	.16*	.04	.80	
6. Openness	3.58 (.54)	.11	.01	.18*	.09	.14	.81
Performance[a] (cheating)	.75 (.13)	−.20**	−.19*	.08	−.13	.05	−.03
Performance[b] (baseline)	.63 (.10)	.04	−.12	−.16*	.07	.03	.04

Note that cheating is probabilistically indicated by the *difference* between the two conditions.
N = 168.
* $p < .05$ (two-sided).
** $p < .01$ (two-sided).
*** $p < .001$ (two-sided).
[a] Proportion of (reported) correct judgments in the cheating condition.
[b] Proportion of (factually) correct judgments in the baseline condition.

statements on average) which is well-aligned with much previous research indicating a prevalence of relatively minor lies (for an overview, see Hilbig & Hessler, 2013; Shalvi et al., 2011).

Table 1 further provides confirmation of the main hypothesis that variance in dishonest behavior can indeed be accounted for: There was a significant negative correlation ($r = -.20$, $p = .01$) between Honesty–Humility scores and performance in the cheating condition, whereas Honesty–Humility was unrelated ($r = .04$, $p = .63$) to performance in the baseline condition. In other words, there are no Honesty–Humility-related differences in actual ability or knowledge (as the baseline condition clearly shows), but in alleged performance (in the cheating condition). For clarity, this pattern is displayed in Fig. 1 which reveals that participants high in Honesty–Humility performed comparably across the two conditions (implying honest behavior given that the two conditions must, on average, be equivalent in task difficulty), whereas their counterparts low in Honesty–Humility performed substantially better in the cheating condition than in the baseline condition. To statistically test this pattern which is tantamount to an interaction between Honesty–Humility and condition, we regressed individuals' difference in performance between the within-subjects conditions (cheating vs. baseline) on Honesty–Humility (Judd, Kenny, & McClelland, 2001) and indeed found the latter to be a significant predictor ($\beta_{HH} = -.18$, $p = .02$). This finding held when controlling for participants' sex and age ($\beta_{HH} = -.16$, $p = .04$). However, it did not hold when controlling for the remaining five factors of the HEXACO model ($\beta_{HH} = -.11$, $p = .21$).

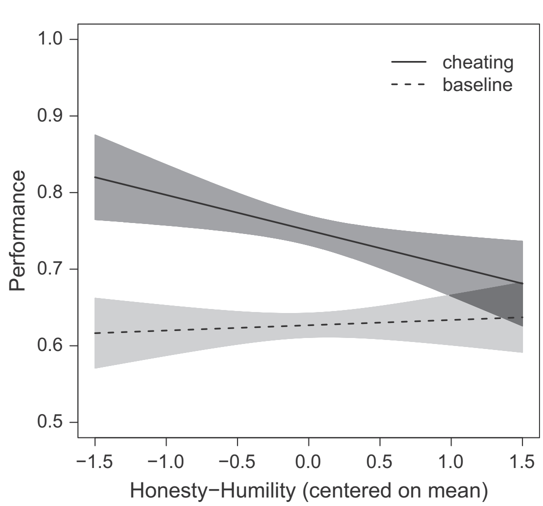

Fig. 1. Predicted performance from linear regressions on Honesty–Humility for each of the within-subjects conditions (cheating vs. baseline) in Experiment 1. Confidence bands represent the 95% confidence interval.

In summary, the results confirmed a rather typical degree of cheating on average and that Honesty–Humility scores were negatively related to the probability of cheating – defined as the performance difference between a self-scored and a baseline condition. As such, we replicated Hershfield et al.'s (2012) results and extended them in several ways: Our setup ensured that performance was interpretable on the aggregate by implementing a baseline condition. Thereby, findings could not be alternatively explained through differences in actual ability or honest errors. At the same time, we implemented a probabilistic rather than a deterministic relation between observed performance and cheating. In other words, participants knew that their *individual* responses and performance could never be interpreted as cheating or honesty. Finally, the association between Honesty–Humility and cheating was confirmed in a task in which cheating also increased participants' gains rather than merely avoiding losses.

However, it must also be stressed that the current data failed to replicate Hershfield et al.'s (2012) finding that Honesty–Humility accounts for unique variance in cheating beyond the remaining five HEXACO factors. This is most likely due to the relatively small effect size of Honesty–Humility as compared to Hershfield et al.'s (2012) findings. Thus, with one result speaking for the incremental predictive validity of Honesty–Humility (Hershfield et al., 2012) and the current results failing to confirm this, further replication seemed necessary.

In addition, the setup of Experiment 1 also yields some limitations that should be overcome: First, cheating was associated with both increasing one's gains and avoiding losses. Taking Hershfield et al.'s (2012) study into account, it is still an open question whether the main pattern holds for situations in which cheating is linked to gains only. Second, the (within-subjects) control condition providing a performance baseline was arguably obvious to participants and may have influenced their behavior differentially. Specifically, even though the link between performance and cheating was only probabilistic, a particularly large difference in performance (between conditions) did imply a larger probability of cheating. Although this cannot have influenced behavior in the cheating condition (as participants did not know the baseline condition would follow), it may have affected performance in the baseline condition negatively (e.g. participants who cheated may have feared that their performance would be identified as suspicious and *therefore* performed worse in the baseline condition), thus increasing the difference between conditions. Finally, incentives for each single instance in which one could have cheated were relatively small (± €0.20). An alternative explanation of our findings could thus be that some individuals only refrained from cheating because it did not seem profitable or would have required many lies (which in turn would produce a more "suspicious" performance pattern) to be profitable. To remedy these shortcomings, a second experiment was conducted.

3. Experiment 2

The second experiment was designed to remedy the shortcomings sketched above and to extend the hypothesized result (that Honesty–Humility should account for variation in dishonest behavior) to another setup. In light of the caveats inherent in performance baselines (i.e. non-cheating control conditions), we herein relied on a paradigm with a *stochastic baseline*. Specifically, we resorted to a type of task that does not require any comparison condition or suppositions about the difficulty of certain items. That is, we implemented a variant of the dice-rolling-paradigm that has been widely used in the behavioral ethics field (Fischbacher & Heusi, 2008; Hilbig & Hessler, 2013; Shalvi et al., 2011; Shalvi et al., 2012): Therein, participants roll a die in secret and merely report the outcome. As such, they can cheat and claim to have rolled whatever outcome is associated with gains. However, the outcome distribution on the aggregate is conclusively known (a fair, six-sided die will produce each of the six possible outcomes with $p = 1/6$), thus serving as a baseline.

To give an example, assume that claiming to have rolled a 6 incurs some gain. If the proportion of observed "winners" exceeds the expected value of 1/6 to a statistically significant extent, we know that cheating occurred – but can nonetheless never determine whether any one individual actually cheated (or simply got lucky), thus protecting anonymity. Say, 30 out of 100 individuals claim to have rolled a 6. Then, a one-sample z-test for proportions defines the test-statistic as

$$z = \frac{\hat{p} - p_0}{\sqrt{\frac{p_0(1-p_0)}{n}}} \tag{1}$$

with p_0 denoting the expected proportion of winners (in our example 1/6), $\hat{p}$ denoting the observed proportion of (alleged) winners (in our example 30 out of 100) and n denoting the sample size. In the example, we obtain $z = 3.58$ which is statistically significant with $p < .001$. Thus, we can conclude that cheating occurred (i.e. some of the observed wins are very likely illegitimate). Also, we know that about 1/6 of wins are legitimate, thus 100/6 = 17 in our example. The remaining wins (30 − 17 = 13) are most likely to be illegitimate. So, the maximum likelihood estimate of the proportion of illegitimate wins is 13/30 = 43%.

Nonetheless, we do not know which of the 30 alleged winners cheated. In other words, it is impossible to infer anything on the individual level, thus fully protecting potential cheaters. Stated differently, the procedure adds random noise to responses (thus maximizing anonymity) the degree of which (the factual probability of winning) is conclusively known on the aggregate – thus providing a fully defined baseline and thereby allowing for the approximation of how many illegitimate wins occurred and how many dishonest individuals were (most likely) in the total sample (Moshagen, Hilbig, Erdfelder, et al., 2014).

Additionally, we implemented the task with only one single consequential trial and a relatively large incentive for cheating (€5.00, approx. $7.00). Thereby, unlike in our previous experiment, it was no longer the case that profitable gains required more lies. Rather, one single lie sufficed to make substantial profit, while the baseline probability of the gain was entirely constant – so, in comparison to Hershfield et al.'s study and our own Experiment 1, higher gains were no longer more incriminating.

3.1. Participants, materials, and procedure

Given the small-to-medium sized effect found in Experiment 1, we computed the required sample size for an equivalent effect size in a logistic regression (see analysis below), that is, an odds-ratio of .50 for predicting alleged wins from Honesty–Humility. For a good power of $1 - \beta = .90$ (and assuming that 40% of participants report a win, cf. Hilbig & Hessler, 2013), a total of 88 participants was required and thus recruited via a local participant pool (54 female, aged 18–45 years, $M = 21.4$, SD = .40 years). Practically all (97%) participants were students from diverse fields, again mostly from business/economics (30%) and the social sciences (26%).

After providing consent and demographical information, participants completed the German version (Moshagen et al., 2014) of the 60-item HEXACO Personality Inventory-Revised (Ashton & Lee, 2009). Next, to separate the assessment of personality and the criterion in time (thus reducing the danger of consistent responding), a set of unrelated filler tasks which lasted about 20 min and provided no possibility to cheat were performed. Finally, participants were thoroughly introduced to the dice-task: They were told their "target number" (randomly drawn from the possible outcomes 1–6) and given a fair six-sided die as well as an opaque cup. It was guaranteed to participants (in a document signed by the principal investigator) that the die was fair[1] and that their roll would not be observed, recorded, or otherwise identified in any way. Participants were instructed to roll the die once privately and to subsequently check one of two answer options on a questionnaire, namely "Yes, I rolled the target number. I will therefore additionally gain €5.00" or "No, I did not roll the target number. I will therefore receive no additional payment". Finally, participants were paid correspondingly, thanked, and debriefed.

3.2. Results and discussion

Out of the 88 participants, 40 (45%) allegedly won, i.e. claimed to have rolled their target number and thus gained €5.00. The statistical probability of observing 40 wins in 88 independent dice-rolls by chance is 2.1677E−10. Correspondingly, the observed proportion of wins was well above the statistical baseline probability of 16.7% (as confirmed by one-sample z-test for proportions with $z = 7.2$, $p < .001$), implying that cheating indeed occurred. The maximum-likelihood estimate of illegitimate wins (i.e. the proportion of individuals falsely claiming to have won) was 63% and the maximum-likelihood estimate of the proportion of dishonest individuals in the total sample was 35%.[2]

[1] We strictly tested this by rolling each die used in this and all following experiments a total of 1998 times and expecting each outcome to occur 333 times (16.67%). This total number of rolls provides optimal power ($1 - \beta > .95$) to detect even small ($w = .1$) deviations from an equal distribution. As confirmed by non-significant χ^2-tests, all dice used in the following experiments were indeed fair.

[2] Here and in the following the maximum likelihood estimates are based on straightforward probability calculations (see Fischbacher & Heusi, 2008). The only vital assumption is that people do not lie to their disadvantage (i.e. never report a blank despite actually winning). Then, we have an unknown proportion of honest individuals (denoted h) who always report their true outcome and an unknown, complementary proportion of dishonest individuals $(1 - h)$ who report their true outcome if they actually won and lie (claim to have won) otherwise. We have a known probability of actually winning (denoted p), e.g. 1/6 in Experiment 2. Hence, an observed fail/blank is produced only by honest people who did not actually win $(h \times (1 - p))$. The maximum likelihood estimate of honest individuals in the total sample is thus h = proportion(blank)/$(1 - p)$. The proportion of illegitimate wins (i.e. actual cheaters among those who claimed to have won) is simply (proportion(wins) − (p))/proportion(wins).

Table 2
Experiment 2: Means (standard deviations in parenthesis) and bivariate correlations of all HEXACO variables with internal consistency reliabilities (Cronbach's alpha) in the diagonal.

	M (SD)	1	2	3	4	5	6
1. Honesty–Humility	3.24 (.62)	.75					
2. Emotionality	3.19 (.65)	.18	.83				
3. Extraversion	3.57 (.66)	−.01	−.31	.86			
4. Agreeableness	3.01 (.55)	.36**	−.05	.16	.76		
5. Conscientiousness	3.46 (.65)	.03	.04	.22*	.06	.85	
6. Openness	3.59 (.65)	.14	−.08	.29**	.05	.16	.78
Probability of winning[a]	–	.42*	.64	2.8**	1.2	1.3	1.4

N = 88.
* $p < .05$ (two-sided).
** $p < .01$ (two-sided).
[a] Values are odds-ratios from separate logistic regressions of the probability of winning on each factor.

To test the main hypothesis that variance in dishonest behavior can be accounted for through a broad, basic trait, we computed a logistic regression predicting whether participants claimed to have won (coded 1, otherwise 0) from Honesty–Humility (descriptives, reliabilities, and intercorrelations of all HEXACO factors are reported in Table 2). Indeed, Honesty–Humility was negatively associated with the probability of allegedly winning (*odds-ratio* = .42, $\chi^2(1) = 5.4$, $p = .02$), corresponding to a medium effect size (Rosenthal, 1996).[3] For illustration, the relationship is displayed in Fig. 2 which shows that individuals high in Honesty–Humility claimed to have won with a probability comparable to the statistical baseline (implying honesty) – whereas their counterparts low in Honesty–Humility made this claim much more often (and clearly more often than could be expected by chance).

Finally, we repeated the above logistic regression to test whether Honesty–Humility accounts for unique variance in the probability of winning. Adding sex and age as covariates did not change the coefficient for Honesty–Humility (*odds-ratio* = .41, $\chi^2(1) = 5.1$, $p = .02$). More importantly, Honesty–Humility also remained a significant predictor when controlling for the remaining five HEXACO factors. Indeed, the relationship between Honesty–Humility and the criterion was even stronger in this analysis (*odds-ratio* = .32, $\chi^2(1) = 6.6$, $p = .01$), corresponding to a moderate-to-large effect size.

Overall, the results of Experiment 2 can be considered relatively strong support for the hypothesis that (dis)honest behavior can be accounted for by means of Honesty–Humility. Importantly, the current findings extend those from Experiment 1 and Hershfield et al.'s (2012) conclusions in several ways: In particular, we implemented a distinct cheating paradigm in which there was the same (probabilistic) link between success and cheating for all participants. At the same time, we avoided creating a situation in which more cheating is inherently more incriminating but necessary to obtain larger gains. Also, the situation made cheating quite profitable and thus comprised a larger temptation, while providing gains only (rather than incurring potential losses). As such, the current findings show that (only) individuals high in Honesty–Humility will refrain from cheating even if the latter is profitable and bears no differential implications for how (dis)honest one may appear. Finally, unlike in Experiment 1, we herein replicated Hershfield et al.'s (2012) result that Honesty–Humility accounts for incremental variance in cheating beyond the remaining five HEXACO factors.

Despite these promising findings, two limitations deserve attention: First, all available evidence so far is based on lab-settings. In light of the unfortunately still widespread use of deception in psychological labs and its consequences on subsequent behavior (Hertwig & Ortmann, 2008; Ortmann & Hertwig, 2002), one could argue that participants in the lab may have feared that they were being secretly observed, that the dice were not actually fair, or the like – in other words, the findings could be alternatively explained by individual differences in suspiciousness or effects of social desirability due to insufficient subjective anonymity (Moshagen et al., 2011). As such, replication in a more (and more obviously) anonymous setting seemed in order. As a second drawback, all evidence available so far refers to monetary incentives for cheating. However, many "real life" instances of dishonesty do not necessary entail monetary gains but other non-monetary benefits (such as avoiding unpleasant experiences, e.g. DePaulo, Kashy, Kirkendol, Wyer, & Epstein, 1996). Thus, to test for the generality of the hypothesized effects, it would seem necessary to show that dishonest behavior can be accounted for whenever the incentives for cheating are non-monetary.

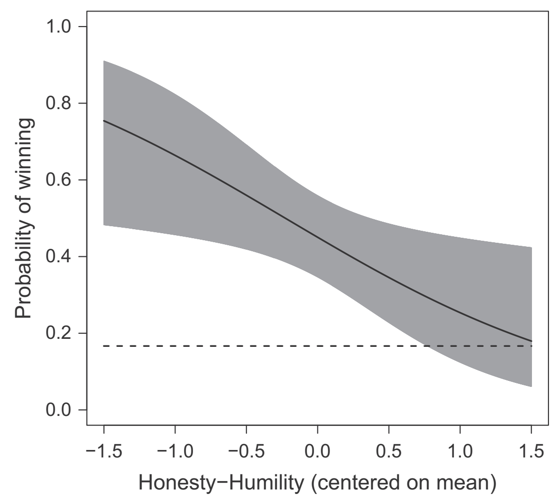

Fig. 2. Predicted probability of winning (claiming to have rolled one's target number) from a logistic regression on Honesty–Humility in Experiment 2. The confidence band represents the 95% confidence interval. The dashed line depicts the baseline probability of winning (16.7%).

4. Experiment 3

Aiming to overcome the limitations of prior studies as sketched above, the third experiment was implemented as a web-based experiment (cf. Gosling, Sandy, John, & Potter, 2010). Personality assessment via the internet has been judged unproblematic

[3] Note that Rosenthal's (1996) qualitative descriptors of effect sizes for odds ratios correspond almost exactly to what would result from Chinn's (2000) approximation of Cohen's *d* from an odds ratio (i.e. $d = \ln(OR)/1.81$).

(Chuah, Drasgow, & Roberts, 2006; Cronk & West, 2002), while affording the unique advantages of a particularly anonymous setting (Joinson, 1999) and the possibility to recruit more diverse samples (Gosling, Vazire, Srivastava, & John, 2004).

Also, in contrast to previous experiments, the incentive for cheating was non-monetary. Specifically, participants were introduced to a set of particularly boring, intellectually trivial yet resource-demanding, and essentially pointless tasks that they were going to be asked to perform – unless if they (reported to have) won in a short coin-tossing task (see below). If they won, they were allowed to move on without completing the dreary tasks. These tasks involved counting how often the letter "y" appeared in a set of 100 English pseudowords, how many out of another set of 100 English pseudowords contained both the letters "o" and "m", and how many out of a third set of 100 English pseudowords were more than eight characters long. All pseudowords were drawn from the database provided by Rastle, Harrington, and Coltheart (2002). All three sets of 100 pseudowords were present on the screen so participants could immediately grasp how unpleasant it would be to have to perform these tasks.

As hinted above, the cheating paradigm was a coin-toss task (for a similar approach, see Peer et al., 2014; Shalvi, 2012). Specifically, participants were instructed to toss a coin exactly ten times and record the number of successes. A success was defined as a toss that turned up on the target side of the coin (heads or tails) to which participants were randomly assigned. It was explained to participants that reporting exactly 5 successes was equivalent to "winning", thus allowing them to move on without completing the unpleasant tasks (see above). Any other number of successes meant that they were required to actually complete the unpleasant tasks. As in the dice paradigm used in Experiment 2, participants could thus cheat by reporting the required number of successes rather than the actual one (indeed, rather than tossing a coin at all). On the individual level, cheating cannot be determined, thus maximizing anonymity. Nonetheless, on the aggregate, the probability of 5 successes under the assumption of complete honesty can easily be calculated, namely 24.6% – thus providing a fully defined statistical baseline. As in Experiment 2, a proportion of alleged wins that exceeds 24.6% with statistical significance can be conclusively interpreted as cheating, whereas any individual win can always stem from honesty (and getting lucky) or dishonesty – thus ensuring that no response is self-incriminating.

4.1. Participants, materials, and procedure

Given that the experiment did not involve monetary incentives, we considered it plausible that the effect size might turn out substantially smaller than in the previous experiments. To be able to detect a small effect (*odds ratio* = .66) in a logistic regression with at least satisfactory power ($1 - \beta = .80$), a sample size of 161 was required. Oversampling slightly to counteract dropout, we recruited a community sample[4] of 185 participants (87 female), aged between 18 and 65 years with $M = 28$ (SD = 10) years from email lists and via social networks. A total of 54% were students from various fields, though less than 10% were students or former students of psychology or related behavioral sciences. Participation was completely voluntary and no incentives were offered.

After providing consent and demographical information, participants again completed the German version of the 60-item HEXACO-PI-R (see Experiment 2). Following an unrelated filler-task (lasting about 5–10 min), the above coin-toss paradigm and unpleasant pseudoword tasks were thoroughly explained to participants. Participants were instructed to complete the coin-toss and then check one of two response options, namely either "I have obtained exactly 5 successes (out of exactly 10 coin-tosses) and will therefore proceed without working on the pseudoword tasks" or "I have not obtained exactly 5 successes (out of exactly 10 coin-tosses) and will therefore complete the pseudoword tasks". In the latter case, input fields were displayed for participants to type in the correct solutions to the three pseudoword tasks. Out of those participants who worked on the pseudoword tasks, 85% provided correct answers to two or all three tasks, whereas less than 15% provided correct answers to only one or none of the tasks. Thus, most participants clearly took the pseudoword tasks seriously. Finally, participants were thanked and given the opportunity to sign up for general feedback about the purpose and results of the study.

4.2. Results and discussion

Out of the 185 participants, 66 (36%) claimed to have won (obtained exactly 5 successes in 10 tosses) which was significantly above the statistical baseline probability of winning ($z = 3.5$, $p < .001$). The observed proportion of wins implied 31% illegitimate wins and a proportion of 15% dishonest individuals in the total sample, calculated as described in Experiment 2. Again testing the main hypothesis that Honesty–Humility can account for variance in cheating, we computed a logistic regression predicting whether participants claimed to have won and thus skipped the dreary tasks (coded 1, otherwise 0) from Honesty–Humility (descriptives, reliabilities, and intercorrelations of all HEXACO factors are reported in Table 3). Indeed, Honesty–Humility negatively predicted the probability of allegedly winning (*odds-ratio* = .60, $\chi^2(1) = 3.9$, $p < .05$), corresponding to a small to medium sized effect (Rosenthal, 1996). Fig. 3 illustrates this relationship, showing that individuals high in Honesty–Humility claimed to have won to the extent that would be expected by chance (implying honesty) – whereas those low in Honesty–Humility won more frequently than their counterparts high in Honesty–Humility and more often than can be attributed to (honestly) getting lucky.

As Table 3 further reveals, no other HEXACO factor predicted the probability of winning. Correspondingly, adding the remaining five HEXACO factors to the above logistic regression left the effect of Honesty–Humility intact and actually lead to a slightly larger effect size (*odds-ratio* = .52, $\chi^2(1) = 5.1$, $p = .02$). The probability of winning was neither influenced by participants' sex (*odds-ratio* = .87, $\chi^2(1) = .88$, $p = .75$) nor age (*odds-ratio* = .98, $\chi^2(1) = 2.2$, $p = .14$). However, the effect of Honesty–Humility marginally failed to reach a conventional level of significance (*odds-ratio* = .57, $\chi^2(1) = 3.4$, $p = .06$) once the two were also added

Table 3
Experiment 3: Means (standard deviations in parenthesis) and bivariate correlations of all HEXACO variables with internal consistency reliabilities (Cronbach's alpha) in the diagonal.

	M (SD)	1	2	3	4	5	6
1. Honesty–Humility	3.35 (.61)	.75					
2. Emotionality	3.00 (.58)	.05	.76				
3. Extraversion	3.59 (.55)	.00	−.18*	.78			
4. Agreeableness	3.15 (.47)	.33**	−.05	.06	.67		
5. Conscientiousness	3.44 (.58)	.22**	−.04	.14	.01	.80	
6. Openness	3.46 (.52)	.08	.06	.24**	.01	.02	.67
Probability of winning[a]	–	.60*	.96	.87	1.1	1.0	.90

$N = 185$.
* $p < .05$ (two-sided).
** $p < .01$ (two-sided).
[a] Values are odds-ratios from separate logistic regressions of the probability of winning (claiming to have obtained exactly 5 successes in 10 coin tosses) on each factor.

[4] We thank Ellen Schlassa for recruiting the sample.

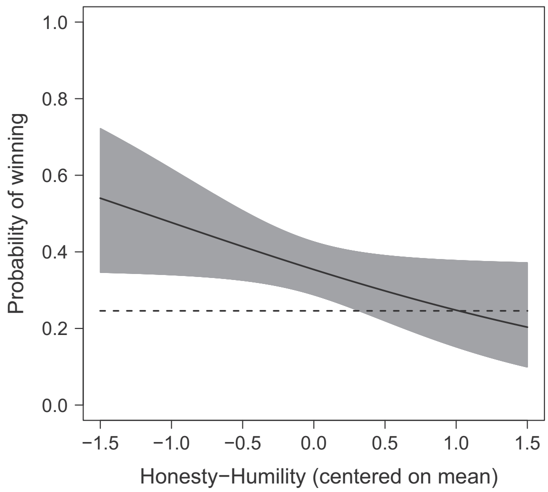

Fig. 3. Predicted probability of winning (claiming to have obtained exactly 5 successes out of 10 coin tosses) from a logistic regression on Honesty–Humility in Experiment 3. The confidence band represents the 95% confidence interval. The dashed line depicts the baseline probability of winning (24.6%).

as covariates. Nonetheless, the effect size of Honesty–Humility remained essentially unaltered.

In summary, the current results further corroborate the main hypothesis that dishonest behavior can be accounted for by means of the basic Honesty–Humility trait – extending the previously reported findings in two ways: First, the setting was still more anonymous given the web-based form of data collection, thus further reducing the danger that participants may have been suspicious or fearful that cheating may somehow be detectable. Second, we once more implemented a different cheating paradigm (coin tosses) and, more importantly, different incentives. Specifically, cheating did not incur higher gains (or smaller losses) in monetary terms as in all previous experiments, but rather gave participants the opportunity to skip a set of cumbersome, monotonous, and arguably pointless tasks. As such, the current findings show that the trait impact on dishonesty is not limited to monetary outcomes. Furthermore, as in Experiment 2, we again replicated Hershfield et al. (2012) in showing that Honesty–Humility accounts for unique variance in cheating beyond the remaining five HEXACO factors.

However, although the findings of our experiments are in line with the theoretical notion that Honesty–Humility represents individuals' (un)willingness to bend rules for personal benefits, an alternative account must be considered. Specifically, Weller and colleagues (Weller & Thulin, 2012; Weller & Tikir, 2011) recently showed that individuals low in Honesty–Humility are generally more risk-seeking. That is, they seem to be more willing to engage in risky behaviors, independent of how tempting or attractive the potential outcomes are. Real-life dishonesty, in turn, clearly stands for a class of behaviors that are usually associated with risk – most obviously, the risk of being caught and facing sanctions. Indeed, the negative consequences of being caught are typically larger than the losses (or gains forgone) if one had refrained from this risky behavior in the first place. As an example, consider tax fraud: As one choice option, one can pay the appropriate amount of taxes; in terms of the risky choice framework (e.g. Savage, 1954; von Neumann & Morgenstern, 1947), this is equivalent to a sure loss without any risk. The alternative option, tax fraud, implies that one may avoid the sure loss (and pay lower taxes than would be appropriate), but this entails the risk of being caught in which case one must at least pay a fine on top of the loss. Consequently, the link between Honesty–Humility and cheating could be driven by a general tendency of those low in Honesty–Humility to be more risk-seeking, rather than their tendency to be more greedy or willing to lie. To tease these explanations apart, we conducted a fourth experiment.

5. Experiment 4

As reasoned above, observing that individuals low in Honesty–Humility are more likely to cheat could also be explained by their inclination to be more risk-seeking. Thus, an experimental paradigm is needed in which behavior driven by risk-seeking and behavior driven by greed are unconfounded. In other words, honesty should represent the risky option whereas dishonesty should represent the safe option (in terms of risky choice). To this end, we used the following *temptation paradigm*: Participants are given the simple choice between two dice-games and decide which one they want to engage in. In the *open game*, they roll a fair six-sided die once openly in front of the experimenter and gain €2.00 (approx. $3.00) if the result matches a pre-specified target number (for illustration, say 6). By contrast, in the *concealed game*, participants roll a fair six-sided die once privately and only report the outcome (exactly as in Experiment 2), gaining €1.00 (approx. $1.50) if they claim to have rolled the pre-specified target number. Clearly, the latter game allows participants to cheat, whereas the former does not.

Given this setup, the open game yields a chance of exactly 1/6 to win €2.00 (or nothing with $p = 5/6$), whereas the concealed game yields a chance of $1/6 + x$ to win €1.00 (or nothing with $p = 5/6 - x$) with x standing for the probability of dishonestly claiming to have won. Consequently, the attractiveness and riskiness[5] of the games depends on one's dishonesty (i.e. x): If one is completely honest ($x = 0$), the games are equally risky and the open game is more attractive (larger expected value). With any value of x greater than approximately 1%, the concealed game is less risky than the open game. That is, if one's chance of winning is increased to 17.4% or more due to dishonesty, the concealed game is the safer option. Finally, for $x \geqslant 1/6$ the concealed game additionally becomes more attractive in terms of expected value on top of being safer. Thus, the option that is more attractive to those willing to cheat (i.e., the concealed game) is actually the safer option in terms of risk.

Considering choice of game (open vs. concealed) as the dependent variable, clear-cut predictions can be made: If individuals low in Honesty–Humility are more likely (than their counterparts high in Honesty–Humility) to prefer the concealed over the open game, this indicates that they are indeed more willing to cheat and not merely more risk-seeking (since the open game is the risky option). This constitutes our hypothesis, that is, we expected to find individuals low in Honesty–Humility prefer the concealed game, thus ruling out the alternative account through risk-preferences. Vice versa, if individuals low in Honesty–Humility are more likely to prefer the open game, this would indicate that they are not necessarily more inclined to cheat (since the concealed game is more attractive if one is willing to cheat), but actually more risk-seeking. So, if a tendency to be risk-seeking motivated individuals low in Honesty–Humility to cheat in the previous experiments, they should select the game offering the higher, more risky prospects in the current temptation paradigm, that is, the open game.

[5] *Attractiveness* refers to a game's expected value, i.e. the sum of that game's possible outcomes multiplied by their respective probabilities of occurrence. *Riskiness* can be expressed by the "coefficient of variation", a widely-accepted measure of risk in decision making which is computed as the standard deviation of all possible outcomes divided by the expected value (Weber, Shafir, & Blais, 2004)

5.1. Participants, materials, and procedure

Given the alternative and novel paradigm implemented, it was not clear a priori what effect size to expect. Thus, we assumed the modal effect observed so far (small-to-medium sized) and aimed for very good power ($1 - \beta = .95$). We missed the required sample size of 109 slightly, recruiting a total of 106 participants (66 female, aged 18–35 years, $M = 21.2$, SD = 3.2 years) via a local participant pool. Most (89%) were students from diverse fields, with the social sciences including psychology (29%) and business/economics (26%) representing the largest groups. Participation was part of an experimental battery of other, unrelated tasks, lasting between 45 and 60 min.

Demographic information and the German version of the 60-item HEXACO-PI-R (see Experiment 2) were assessed online prior to participation in the lab. Specifically, all participants completed the HEXACO-PI-R at least 24 h before coming to their lab-session. Data matching proceeded through pseudonymous codes generated by the participants themselves and preserving anonymity. As part of the lab session, participants were introduced thoroughly to the two options, viz. the open game and the concealed game. It was explained to them that they were going to choose which of the two games (open vs. concealed) they wanted to play. In a room in which only one participant and the experimenter were present, participants first determined their "target number" by rolling a fair six-sided die openly. As in Experiment 2, it was guaranteed to participants that the die was fair and that their roll would not be observed, recorded, or otherwise identified in any way if they chose to play the concealed game. Participants then chose one of the two games and it was played out exactly as explained. Finally, participants were paid correspondingly, thanked, and debriefed.

5.2. Results and discussion

Out of the 106 participants, 56 (53%) chose the open game, whereas 50 (47%) chose the concealed game. Out of those who opted for the open game, 9 (16%) actually won (rolled the pre-specified target number) which almost perfectly matches the baseline probability of 16.7%. By contrast, every single participant who opted for the concealed game allegedly won (claimed to have rolled the pre-specified target number), thus clearly exceeding the statistical baseline probability of 16.7% ($z > 15.0$, $p < .001$). The proportion of observed wins in the concealed game implied 83% illegitimate wins and 47% dishonest individuals in the total sample (all of whom opted for the concealed game). As is apparent from these numbers, the probability of winning differed significantly between the two game variants (despite use of the exact same fair dice), $\chi^2(1) = 75.4$, $p < .001$.

To test whether and how Honesty–Humility would relate to the choice of game, we computed a logistic regression predicting whether participants chose the concealed game (coded 1, otherwise 0) from Honesty–Humility (descriptives, reliabilities, and intercorrelations of all HEXACO factors are reported in Table 4). Honesty–Humility negatively predicted the probability of choosing the concealed game (*odds-ratio* = .35, $\chi^2(1) = 7.7$, $p < .01$), corresponding to a medium to large effect size (Rosenthal, 1996). That is, individuals low in Honesty–Humility were substantially more likely to opt for the concealed game. In line with this finding and the above association between choice of game and the observed frequency of (allegedly) winning, Honesty–Humility also negatively predicted whether participants received a payoff (coded 1) or not (*odds-ratio* = .32, $\chi^2(1) = 8.2$, $p < .01$).

Testing for the robustness and incremental power of the effect of Honesty–Humility on choice of game, we added covariates to the above logistic regression. Neither adding sex and age as covariates nor adding the five remaining HEXACO factors substantially altered the coefficient for Honesty–Humility (*odds-ratio* = .31, $\chi^2(1) = 8.8$, $p < .01$ and *odds-ratio* = .33, $\chi^2(1) = 6.8$, $p < .01$, respectively). Indeed, when considered separately (see last row of Table 4) or jointly, none of the remaining HEXACO factors predicted choice of game (all $\chi^2(1) < 2.3$, all $p > .10$).

Table 4
Experiment 4: Means (standard deviations in parenthesis) and bivariate correlations of all HEXACO variables with internal consistency reliabilities (Cronbach's alpha) in the diagonal.

	M (SD)	1	2	3	4	5	6
1. Honesty–Humility	3.3 (.60)	.75					
2. Emotionality	3.1 (.64)	.10	.80				
3. Extraversion	3.6 (.55)	−.09	−.04	.79			
4. Agreeableness	3.1 (.50)	.32**	.17	.13	.69		
5. Conscientiousness	3.7 (.47)	.04	.12	.05	−.10	.71	
6. Openness	3.5 (.62)	.20*	−.01	.07	.06	−.02	.74
Game preference[a]	–	.35**	.85	1.1	.87	.53	.76

$N = 106$.
* $p < .05$ (two-sided).
** $p < .01$ (two-sided).
[a] Values are odds-ratios from separate logistic regressions of the preference for the concealed game (coded 1) over the open game (coded 0) on each factor.

The reported findings show that Honesty–Humility accounts for game preference in the temptation paradigm. Specifically, individuals low in Honesty–Humility were substantially more likely to opt for the game that produces a safer, albeit lower payoff – if and only if one is willing to cheat. Given that all participants who opted for this game variant allegedly won, this willingness to cheat was substantial. In turn, the findings rule out that Honesty–Humility may previously have been linked to cheating due to the confounding effect of risk-preferences: If a tendency to be risk-seeking had motivated individuals low in Honesty–Humility to cheat in prior experiments, they should have chosen the open game which offered the larger and more risky prospects in the current temptation paradigm. However, the opposite was the case as these individuals preferred the smaller gain that could be safely attained through cheating. These findings also further specify the conclusions of Weller and colleagues (Weller & Thulin, 2012; Weller & Tikir, 2011) who showed that individuals high in Honesty–Humility are more risk-averse in gambling paradigms. As shown in the current experiment, this pattern may reverse once risky behavior is the option compatible with "lead us not into temptation" – in such situations, individuals high in Honesty–Humility will actually act in a more risk-seeking way than their counterparts low in Honesty–Humility.

However, although the previously reported findings consistently speak for the hypothesis that a basic trait, Honesty–Humility, can account for individual differences in dishonesty, two limitations remained: First, in all studies linking Honesty–Humility to dishonest behavior, the sample was primarily (if not entirely) comprised of students. Given that students arguably represent a rather specific subset of the general population (especially in terms of age and education), one may question the generality of the current findings (for related arguments, see Henrich, Heine, & Norenzayan, 2010). In turn, replication of the role of personality traits for (dis)honest behavior in a more diverse sample seemed necessary – including a test of whether Honesty–Humility would then still account for unique variance in cheating beyond variables such as age and education, given substantial heterogeneity on both these variables. Second, whereas the findings from Hershfield et al. (2012) and our Experiments 2–4 lend support for the unique contribution of Honesty–Humility beyond the classic five factors of personality, this support bears a caveat: The more classic five factors of personality were assessed via the HEXACO-PI-R which is essentially designed to provide a measure of Honesty–Humility distinct from and independent of the other five factors. It is thus an open question whether any of the five classic factors may also account for dishonest behavior and whether Honesty–Humility actually accounts for unique variance beyond these classic Big Five. A conclusive answer requires assessment of the latter via an independent five-factor inventory. We addressed both of these limitations in a fifth experiment in which we also tested for additional moderators to further evaluate the robustness of the main hypothesis.

6. Experiment 5

Aiming to overcome the sample-inherent limitations of prior results, the fifth experiment was again implemented as a web-based study (cf. Gosling et al., 2010). To obtain a truly diverse sample, we turned to a professionally managed panel with specific requirements concerning the composition of the target sample (aiming for heterogeneity in age and education). In addition, we used both the HEXACO-PI-R and a well-established measure of the classic five personality factors. Finally, we again varied the cheating paradigm, though retaining the core idea of a probabilistic link between observable success and cheating with a statistical baseline.

The cheating paradigm was a coin-toss task (cf. Experiment 3). In the current variant, participants were asked to toss a coin exactly ten times and record the number of successes (defined as coin-tosses that turned up on the target side, heads or tails). If participants reported exactly k successes, they received a monetary gain. To obtain a more fine-grained measure of cheating than in the previous experiments, we implemented two rounds of this task, representing two conditions: In the *winning-unlikely* condition, reporting exactly 3 or 8 successes out of the ten coin-tosses incurred a gain. The cumulative probability of exactly 3 or 8 successes is 16.11%. In the *winning-likely* condition, exactly 4 or 7 successful coin-tosses incurred the gain (which will occur with a cumulative probability of 32.2%).

In addition, to overcome the inherent limits of cross-experimental comparisons, we sought to test whether the size of the incentives might moderate the link between personality and cheating. Thus, we manipulated between participants the gain associated with reporting the required number of successes. Specifically, participants either gained €2.00 or €4.00 when reporting the required number of successes per round/condition. So, by reporting the required number of successes in both rounds/conditions, they could gain a maximum of either €4.00 (approx. $5.50) or €8.00 (approx. $11.00) on top of the flat-fee they received for participation. Importantly, payment was handled entirely by the independent panel provider; thus, participants' anonymity was perfectly preserved.

6.1. Participants, materials, and procedure

Given the similarity in paradigm, we computed the required sample size based on the findings in Experiment 3. This resulted in a required sample of 114 (*odds ratio* = .60 with satisfactory power: $1-\beta = .80$). However, since we had no prior experience with samples strongly heterogeneous in education, we decided to oversample by about 30% and therefore a total of 147 participants (78 female) completed the study. Participants' ages ranged from 18 to 72 years with $M = 39.9$ (SD = 14), thus implying substantial heterogeneity on this dimension and a higher average age than is typical for research primarily relying on students samples. Indeed, only 14% were students, whereas most (60%) were in employment or already retired (11%). Finally, there was noteworthy diversity in educational backgrounds, with 25% holding a certificate of secondary education (German: Hauptschulabschluss), 25% a general certificate of secondary education (German: mittlere Reife), 31% a vocational diploma or university-entrance diploma (German: Fachabitur or Abitur), and 19% an university/college degree.

After providing consent and demographical information, participants again completed the German version of the 60-item HEXACO-PI-R (see Experiment 2) as well as the German 60-item NEO-Five-Factor-Inventory (Borkenau & Ostendorf, 1994) which assesses the classic five personality factors (Costa & McCrae, 1992) with 12 items each. To avoid specific carry-over effects, the order of these two questionnaires was counterbalanced across participants. Next, the coin-toss task was thoroughly explained to participants and they were informed about the incentive for reporting the required number of successes (€2.00 vs. €4.00, random assignment). Then, the two rounds of this task (representing the two conditions: winning-unlikely vs. winning-likely) were completed – again with order counterbalanced across participants. In each round, participants were asked to (i) set their target-side (heads or tails), then (ii) toss the coin exactly ten times recording the number of successes, and finally (iii) report whether or not they had obtained the required number of successes (cf. Experiment 3). Finally, all were thanked and debriefed.

6.2. Results and discussion

Out of the 147 participants, 55 (37%) claimed to have won (obtained the required number of successes) in the winning-unlikely condition and 68 (46%) did so in the winning-likely condition. Both proportions differed significantly from the respective statistical baseline probabilities of winning ($z = 7.1$, $p < .001$ and $z = 3.6$, $p < .001$, respectively), indicating that cheating occurred in both conditions. The proportions of observed wins implied 37% and 46% illegitimate wins (25% and 21% dishonest individuals) in the winning-unlikely and winning-likely condition, respectively. Thus, there was only a small and non-significant difference between the two rounds/conditions, as confirmed by a McNemar-test for dependent proportions (McNemar, 1947), $\chi^2(1) = 2.3$, $p = .13$. Moreover, the between-subjects incentive condition did not influence the proportion of alleged wins in the winning-unlikely ($\chi^2(1) = .05$, $p = .81$) or the winning-likely condition ($\chi^2(1) = .06$, $p = .80$).

To test the overall pattern and, more importantly, the influence of personality (for descriptives, reliabilities, and intercorrelations see Table 5), we used a generalized linear model (Nelder & Wedderburn, 1972) with a binomial distribution of the dependent variable (alleged wins, coded 1, 0 otherwise) and logit as the link function (thus essentially implementing a logistic regression with the condition – i.e. winning likely vs. unlikely – as the within-subjects factor). First, we entered the design factors, i.e. the incentive-condition (between subjects), the two coin-toss rounds/conditions (within subjects), and the order of the latter (between subjects) into the model (all between effects dummy coded). Confirming the above findings, neither the incentive condition ($\chi^2(1) = .02$, $p = .90$) nor the coin-toss round/condition ($\chi^2(1) = 2.3$, $p = .13$) predicted the probability of winning. The same held for the order of the coin-toss rounds/conditions ($\chi^2(1) = .4$, $p = .54$) and there were no two- or three-way interactions between any of these factors (all $\chi^2(1) < 1.7$, all $p > .20$).

Much more importantly, adding Honesty–Humility as a covariate to the model revealed that it negatively predicted the probability of winning (*odds-ratio* = .58, $\chi^2(1) = 7.3$, $p = .01$), corresponding to a small-to-medium-sized effect (Chinn, 2000; Rosenthal, 1996). Honesty–Humility did not interact with any of the design factors (all $\chi^2(1) < 1.1$, all $p > .31$) and thus the influence of Honesty–

Table 5
Experiment 5: Means (standard deviations in parenthesis) and bivariate correlations of all HEXACO and NEO-FFI variables with internal consistency reliabilities (Cronbach's alpha) in the diagonal.

	M (SD)	1	2	3	4	5	6	7	8	9	10	11
1. HEX Honesty–Humility	3.5 (.59)	.71										
2. HEX Emotionality	3.1 (.53)	−.07	.71									
3. HEX Extraversion	3.3 (.57)	.02	−.16	.80								
4. HEX Agreeableness	3.1 (.45)	.18*	−.11	.35***	.65							
5. HEX Conscientiousness	3.5 (.50)	.23**	.03	.18*	−.05	.70						
6. HEX Openness	3.3 (.66)	.33***	−.05	.23**	.19*	.22**	.79					
7. NEO Openness	3.4 (.52)	.32***	.01	.19*	.11	.20*	.78***	.72				
8. NEO Conscientiousness	3.7 (.54)	.17*	.05	.31***	.04	.68***	.24**	.16	.83			
9. NEO Extraversion	3.2 (.58)	−.08	−.11	.80***	.30***	.13	.07	.10	.32***	.82		
10. NEO Agreeableness	3.6 (.48)	.35***	.08	.37***	.57***	.24**	.32***	.30***	.31***	.36***	.75	
11. NEO Neuroticism	2.7 (.68)	−.10	.48***	−.60***	−.31***	−.18*	−.13	−.12	−.23**	−.49***	−.22**	.87
Probability of winning[a]	–	.58**	1.0	.91	1.0	.66	.93	1.2	.65*	1.2	0.81	1.1

$N = 147$.
* $p < .05$ (two-sided).
** $p < .01$ (two-sided).
*** $p < .001$ (two-sided).
[a] Values are odds-ratios from separate within-subjects logistic regressions (accounting for all design factors, i.e. round, order, and incentives) of the probability of winning on each personality factor (i.e. the main effect of each factor).

Humility was also independent of the incentives for cheating. For clarity, the relationship between Honesty–Humility and the observed proportion of (alleged) wins in each of the two coin-toss rounds is depicted in Fig. 4 (across incentive conditions). As can been seen, individuals high in Honesty–Humility tended to win with a probability matching the baseline in each of the rounds/conditions, whereas their counterparts low in Honesty–Humility were substantially more likely to win in both situations.

Finally, to test whether Honesty–Humility explains unique variance in cheating, we controlled for different sets of additional covariates in the above model. Controlling for sex, age, and education left the influence of Honesty–Humility virtually unchanged (*odds-ratio* = .60, $\chi^2(1) = 5.5$, $p = .02$). Similarly, when controlling for the remaining five factors of the HEXACO model, the influence of Honesty–Humility was unaltered (*odds-ratio* = .56, $\chi^2(1) = 7.1$, $p = .01$). Finally, controlling for the five NEO factors also did not affect the influence of Honesty–Humility on the criterion (*odds-ratio* = .58, $\chi^2(1) = 5.8$, $p = .02$).

In summary, the results of Experiment 5 lend further support to the main hypothesis that Honesty–Humility indeed accounts for individual differences in (dis)honest behavior. Extending prior findings to a diverse (non-student) sample, we again replicated the influence of Honesty–Humility and found that it was robust against experimental variation of the baseline-probability for winning and the size of the (monetary) incentives. Indeed, the size of the incentives did not influence cheating behavior per se which is actually compatible with prior findings in the behavioral ethics literature (Mazar et al., 2008). The effect of Honesty–Humility held after controlling for socio-demographic variables, the five remaining HEXACO factors, and the classical five personality factors as assessed via a corresponding inventory. Beyond showing that a basic personality trait can consistently account for variance in dishonest behavior, the findings thus also support the notion that Honesty–Humility is a relevant and useful addition to the classical five factors (Ashton & Lee, 2008a; Ashton & Lee, 2008b).

Nonetheless, all of the experiments reported above bear two remaining caveats. The first is a methodological one, namely that personality traits and cheating were assessed within the same study or session with only a short time lag between the two (24 h, cf. Experiment 4). Consequently, part of the association between Honesty–Humility and cheating may have been driven by participants trying to respond consistently. Second, all cheating tasks implemented so far represent one particular type of situation, namely one in which an abstract agent – the experimenter – bears

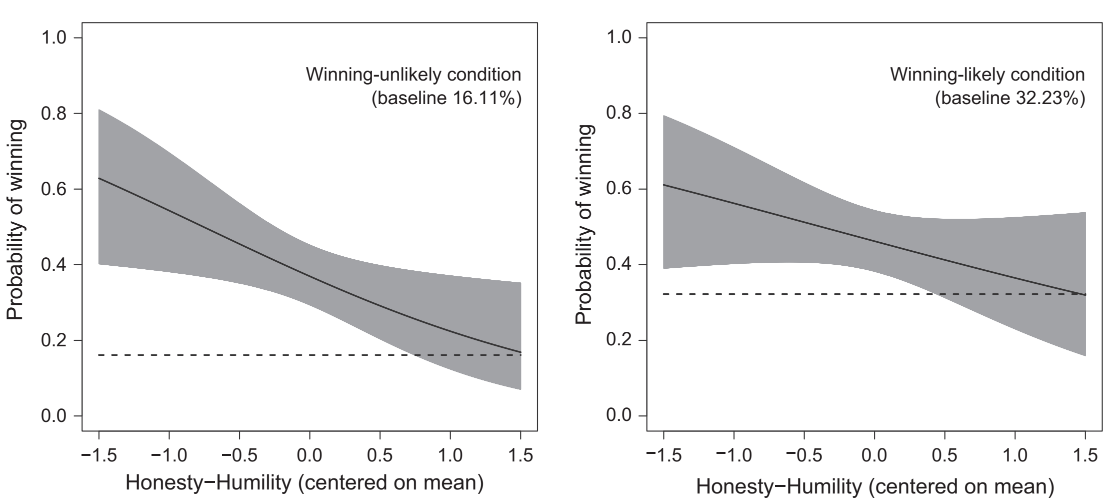

Fig. 4. Predicted probability of winning (reporting the required number of successful coin-tosses) from a logistic regression on Honesty–Humility, separately for the two coin-toss rounds/conditions in Experiment 5. Confidence bands represent the 95% confidence interval. The dashed lines represent the respective baseline probabilities of winning in each of the two conditions (16.1% and 32.2%, respectively).

the full costs of unethical behavior whereas the perpetrator and other individuals are unaffected. Arguably, many real-life situations are different in that one's unethical acts may negatively affect oneself or similar others in the long run – especially if many individuals act unethically, thus mirroring a typical social dilemma structure (Dawes & Messick, 2000; Kollock, 1998). For example, if many choose to free-ride on public transport, the perpetrators themselves may eventually face the costs of the service being discontinued. Possibly, the effect of Honesty–Humility is limited to relatively artificial situations in which cheating cannot incur (social) costs. In turn, we intended to test whether individuals low in Honesty–Humility might refrain from cheating in the face of potential costs – essentially mirroring their tendency to cooperate strategically if defection is risky (Hilbig & Zettler, 2009; Hilbig, Zettler, & Heydasch, 2012; Zettler et al., 2013).

7. Experiment 6

In line with the design of the previous experiment, we again implemented a web-based study and turned to a professionally managed panel to recruit a diverse sample. Also, we once again included the HEXACO-PI-R and the NEO-FFI so as to replicate the comparison from Experiment 5. However, the current experiment separated assessment of personality and cheating behavior in time – by an average of 137 days (SD = 3 days) and thus about 4.5 months. Additionally, we once more varied the cheating paradigm to design a situation involving potential (social) costs of unethical behavior.

Again, we used a coin-toss task (cf. Experiments 3 and 5), this time with two conditions. The *standard condition* mirrored those implemented previously: Participants were asked to toss a coin exactly twice and were told they would receive a monetary gain of 5.00€ (approx. $7.00) if they reported exactly two successes (defined as coin-tosses that turned up on the target side, heads or tails). The baseline probability of winning was thus 25%. In the *common goods condition*, instructions so far were the same, but participants were additionally told the following:

> "However, if more than 40% of all individuals participating in this study claim to have obtained two successes, we will have to assume that some individuals must have cheated. So, if this actually occurs, no-one will receive a monetary gain (regardless of their answer and whether or not it was honest). Since the statistical baseline probability of winning is 25%, it is almost impossible to observe 40% legitimate wins in the number of responses we are collecting (about 100 individuals). The probability of this actually occurring is smaller than 0.01%."

Thus, we implemented a situation in which cheating could have severe long-term consequences, namely eliminating all gains for perpetrators and honest individuals alike. As in the previous experiments, all participants were asked to select one of two response options and all were reminded that their additional payoff depended only on their response (and, in the common goods condition, the aggregate response frequencies). Once again, payment was handled entirely by the independent panel provider as was data matching over time; thus, participants' anonymity was perfectly preserved.

7.1. Participants, materials, and procedure

The required sample size was computed based on the findings of Experiment 5 (*odds-ratio* = .58). Aiming for very good statistical power ($1 - \beta = .95$) resulted in a required sample size of 163. Once again oversampling by 30%, a total of 208 participants (91 female) completed both parts of the study and were randomly assigned to the standard ($n = 107$) and the common goods conditions ($n = 101$), respectively. As intended, the sample was diverse as participants were aged between 18 and 65 years ($M = 42.9$, SD = 13), and only 8% were students whereas most (58%) were in employment or already retired (12%). Also, there was the intended variance in educational backgrounds, with 26% holding a certificate of secondary education (German: Hauptschulabschluss), 20% a general certificate of secondary education (German: mittlere Reife), 25% a vocational diploma or university-entrance diploma (German: Fachabitur or Abitur), and 30% an university/college degree.

At time point 1, participants provided consent and demographical information along with their responses to the German version of the 60-item HEXACO-PI-R and the German 60-item NEO-Five-Factor-Inventory (see Experiment 5), followed by independent tasks not pertinent to the current research question. All received monetary payment for this first part of the study. Approximately four months later, participants were invited to complete a follow-up study consisting of several unrelated tasks the first of which was the coin-toss task outlined above. It was thoroughly explained and participants were then instructed to (i) set their target-side (heads or tails), then (ii) toss the coin exactly twice, and finally (iii) report whether or not they had obtained exactly two successes (cf. Experiments 3 and 5). Finally, all were thanked and debriefed.

7.2. Results and discussion

In the standard condition, 54 participants (51%) claimed to have won (obtained the required number of successes) as compared to 44 (44%) in the common goods condition. Both proportions differed significantly from the 25% baseline probabilities of winning ($z = 4.0$, $p < .001$ and $z = 2.4$, $p < .01$, respectively), indicating that cheating occurred in both conditions. Plausibly, the implied proportion of illegitimate wins was smaller in the common goods condition (26%) than in the standard condition (36%), implying 27% and 17% dishonest individuals, respectively. However this difference between conditions was not significant ($\chi^2(1) = 1.0$, $p = .32$). As these numbers also indicate, the proportion of alleged wins in the common goods condition was above the 40%-threshold. Consequently, none of the participants in this condition received a payoff (regardless of what their answer had been).

To test the influence of personality (for descriptives, reliabilities, and intercorrelations see Table 6), we conducted logistic regression analyses with participants' responses as dependent variable (alleged wins, coded 1, 0 otherwise), always accounting for the main effect of the between-subjects condition (standard vs. common goods, dummy coded) which did not turn out significant as indicated above. As hypothesized, Honesty–Humility negatively predicted the probability of winning (*odds-ratio* = .37, $\chi^2(1) = 13.8$, $p < .001$), corresponding to a medium-to-large effect (Chinn, 2000; Rosenthal, 1996). At the same time, Honesty–Humility did not interact with the between-subjects condition (*odds-ratio* = 1.1, $\chi^2(1) = .02$, $p = .88$). The full pattern relationships is depicted in Fig. 5 which shows that the (noteworthy) degree to which individuals low in Honesty–Humility were more likely to cheat did not differ between the standard and the common goods condition. Specifically, individuals high in Honesty–Humility once more tended to win with a probability matching the baseline in each of the conditions, whereas their counterparts low in Honesty–Humility were substantially more likely to win in both situations and thus more willing to cheat even to an extent that incurrent substantial social costs (namely, no payoffs for anyone in the common goods condition).

Again, we tested whether Honesty–Humility explains unique variance in cheating by controlling for different sets of additional covariates in the above model. Although neither sex, nor age, nor

Table 6
Experiment 6: Means (standard deviations in parenthesis) and bivariate correlations of all HEXACO and NEO-FFI variables with internal consistency reliabilities (Cronbach's alpha) in the diagonal.

	M (SD)	1	2	3	4	5	6	7	8	9	10	11
1. HEX Honesty–Humility	3.5 (.63)	.75										
2. HEX Emotionality	3.1 (.61)	−.02	.79									
3. HEX Extraversion	3.3 (.60)	−.02	−.33***	.81								
4. HEX Agreeableness	3.1 (.47)	.32***	−.20**	.28***	.65							
5. HEX Conscientiousness	3.6 (.48)	.10	−.12	.27***	.10	.70						
6. HEX Openness	3.2 (.66)	−.07	−.08	.35***	.08	.23**	.79					
7. NEO Openness	3.3 (.51)	−.07	.07	.28***	.10	.21**	.80***	.72				
8. NEO Conscientiousness	3.8 (.55)	.18**	−.22**	.38***	.24**	.47***	.10	.08	.85			
9. NEO Extraversion	3.2 (.53)	−.07	−.18**	.79***	.22**	.23**	.30***	.28***	.28***	.80		
10. NEO Agreeableness	3.6 (.47)	.44***	.05	.32***	.56***	.06	.10	.11	.22**	.29***	.74	
11. NEO Neuroticism	2.7 (.74)	−.18**	.64***	−.65***	−.35***	−.31***	−.19**	−.07	−.42***	−.52***	−.28***	.90
Probability of winning[a]	–	.37***	.93	.91	.75	.64	.96	.90	.65	.97	.44**	1.4

$N = 208$.
** $p < .01$ (two-sided).
*** $p < .001$ (two-sided).
[a] Values are odds-ratios from separate within-subjects logistic regressions (accounting for the between-subjects condition) of the probability of winning on each personality factor (i.e. the main effect of each factor).

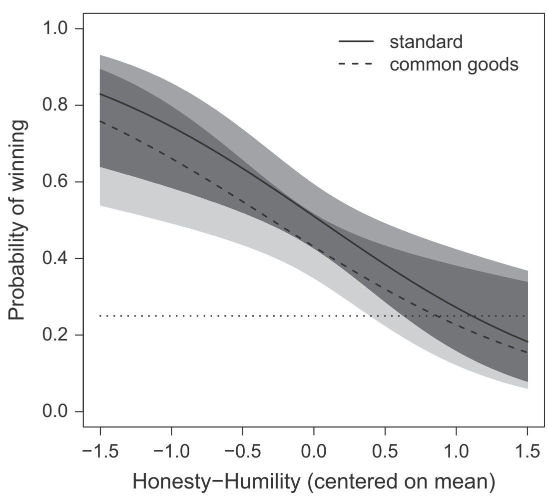

Fig. 5. Predicted probability of winning (claiming to have obtained exactly two successes out of two coin tosses) from a logistic regression on Honesty–Humility and the between-subjects condition (separate curves) in Experiment 6. The confidence bands represent the 95% confidence interval. The dotted line depicts the baseline probability of winning (25%).

education influenced cheating significantly, there was a trend for the latter such that more educated individuals were less likely to report a win (*odds-ratio* = .82, $\chi^2(1) = 2.9$, $p = .09$). Nonetheless, the effect of Honesty–Humility was almost exactly the same when controlling for these demographic variables (*odds-ratio* = .35, $\chi^2(1) = 14.9$, $p < .001$). Similarly, controlling for the remaining five factors of the HEXACO model (neither of which explained unique variance) did not alter the influence of Honesty–Humility (*odds-ratio* = .37, $\chi^2(1) = 12.7$, $p < .001$). The same results were found when controlling for the five NEO factors: Neither of these accounted for unique variance, whereas Honesty–Humility continued to do so (*odds-ratio* = .44, $\chi^2(1) = 7.6$, $p < .01$). It should be noted that, contrary to the previous experiment, NEO Agreeableness did predict cheating if it was the only predictor (see Table 6). However, a direct comparison of NEO Agreeableness and Honesty–Humility revealed that the latter accounted for unique variance (*odds-ratio* = .42, $\chi^2(1) = 9.2$, $p < .01$) whereas the former did not (*odds-ratio* = .69, $\chi^2(1) = 1.1$, $p = .29$). In other words, Honesty–Humility covers the content in NEO Agreeableness that predicted cheating but not vice versa (cf. Hilbig, Glöckner, & Zettler, 2014).

In summary, Experiment 6 further confirmed the main hypothesis that Honesty–Humility drives (dis)honest behavior. First off, the findings from Experiment 5 were replicated, demonstrating the influence of Honesty–Humility in a diverse, (non-student) sample and showing that it accounts for unique variance beyond the classical five personality factors as assessed via a corresponding inventory. More importantly, the current experiment extended prior findings by showing that the effect of Honesty–Humility cannot be attributed to consistent responding: Separating the assessment of personality and cheating in time actually led to the second largest effect size of Honesty–Humility across all experiments. Furthermore, the current experiment entailed a common goods condition in which noteworthy (social) costs were associated with a level of cheating beyond a certain (explicit) threshold. This design mirrors a more realistic type of situation in which unethical behavior jeopardizes a common resource and may thus incur costs for the perpetrator and others alike. Strikingly, the effect of Honesty–Humility was exactly the same as in a standard condition. Indeed, cheating occurred so frequently in the common goods condition that participants lost all additional payoffs (including those earned honestly). In other words, individuals low in Honesty–Humility did not appear to shy away from or even notably reduce cheating even though they themselves and – worse yet – innocent others had to bear the costs.

8. General discussion

Discouraging though it may be, the well-publicized cases of fraud, corruption, and dishonesty that surface almost daily are probably little more than the tip of the iceberg. Clearly rejecting the "only a few bad apples" idea, research on behavioral ethics has established that many individuals are indeed willing to cheat when given an opportunity (for an overview, see Bazerman & Gino, 2012). Fortunately, and contrary to the expectations of standard economic theory, most individuals avoid major lies and transgressions (Hilbig & Hessler, 2013; Mazar et al., 2008; Shalvi et al., 2011). Nonetheless, there are noteworthy individual differences in the extent of dishonest behavior and quite a number of individuals willing to maximize their profits even if this requires a large degree of dishonesty (Fischbacher & Heusi, 2008). Thus, both everyday observation and empirical findings confirm that individuals differ in their willingness to manipulate or exploit others, bend rules to their own advantage, or impose costs on others and society at large. At the same time, these substantial individual differences observed in the behavioral ethics literature have not been consistently accounted for through personality traits, especially broad traits as specified in models of basic personality structure.

Strikingly, recent developments in basic theories of personality highlight exactly such individual differences in honesty and morality. In particular, the Honesty–Humility factor (Lee & Ashton, 2012) established as part of the HEXACO Model of Personality (Ashton & Lee, 2007) represents basic tendencies toward manipulative, exploitative, and dishonest behavior. This extension of the classic Five-Factor Model of Personality (McCrae & Costa, 1999; McCrae & John, 1992) has shown promise in recent work (e.g. Ashton & Lee, 2008a; Hilbig, Zettler, Leist, & Heydasch, 2013; Lee & Ashton, 2014) and it is theoretically a primary candidate to explain individual differences in ethical behavior. Indeed, as a side-lines aspect, one prior study has indicated that Honesty–Humility may indeed account for variance in dishonesty (Hershfield et al., 2012, Study 4). However, due to the experimental design, it remains open whether and to what extent cheating actually occurred in said study. Apart from this one instance, we are not aware of any study linking basic traits to dishonest behavior – despite some scattered work on other, rather secluded and narrow trait-like constructs (e.g. regulatory focus, Gino & Margolis, 2011).

In the current work, we thus set out to systematically bridge the gap between research on behavioral ethics and recent developments in models of personality structure. Our broader aim was to address a question that Baumeister et al. (2007) have identified as one of the seriously understudied ones in personality research as a whole, namely: "How do people with different degrees of a personality trait *behave* differently?" (p. 401, emphasis added). Specifically, in a series of six experiments, we critically tested whether (self-reported) Honesty–Humility can account for individual differences in dishonest behavior (i.e. cheating), making use of an array of paradigms provided by the behavioral ethics literature (e.g. Fischbacher & Heusi, 2008; Hilbig & Hessler, 2013; Lewis et al., 2012; Schurr et al., 2012; Shalvi et al., 2011) and new variants of these.

The results of the six experiments reported herein are summarized in Table 7. As can be seen, Honesty–Humility consistently accounted for dishonest behavior across various paradigms, incentive structures, modes of data collection, and samples. Overall, the effect was about medium-sized and typically held when controlling for sex, age, and educational background. Of note, no other personality factor showed any consistent links with cheating across the experiments (or even a subset of experiments): In Experiment 1, HEXACO Agreeableness showed a weakly negative association with cheating and in Experiment 2 HEXACO Extraversion positively predicted said criterion. In Experiments 5 and 6, Big Five Conscientiousness and Agreeableness, respectively, were negatively associated with the probability of winning. Thus, there is little indication that any of the other basic personality traits may consistently account for individual differences in unethical behavior. Correspondingly, Honesty–Humility accounted for unique variance beyond the remaining five factors of the HEXACO model (in Experiments 2–6) and the classic five factors of personality as assessed through a corresponding inventory (in Experiments 5 and 6). These findings support the notion that Honesty–Humility is the quintessential basic trait to account for individual differences in ethical behavior. This conclusion, in turn, is compatible with previous findings on the role of Honesty–Humility for pro-social and cooperative behavior in general (Ashton et al., 2014; Hilbig, Glöckner, et al., 2014).

The different experiments summarized in Table 7 were also designed to rule out a number of alternative explanations and remedy several drawbacks. Specifically, Experiment 1 rules out the alternative explanation that actual performance differences may have driven prior findings reported by Hershfield et al. (2012). More importantly, in line with most experimental designs in the behavioral ethics field (Fischbacher & Heusi, 2008; Shalvi et al., 2011), Experiments 2–6 relied on a probabilistic link between observable behavior and cheating: In statistical terms, the paradigms add random noise – the extent of which is conclusively known on the aggregate – to observed responses. Any observed win could have been produced by both honesty (and getting lucky) or dishonesty – thus counteracting effects of social desirability. Thereby, each individual response was perfectly anonymous (allegedly winning was not automatically self-incriminating) while retaining a fully incentive-compatible setup without any deception of participants (Ortmann & Hertwig, 2002). At the same time, the underlying probabilities (of observing a win) are conclusively known; so, one can determine how often and to what extent cheating occurred on the aggregate. Experiments 3, 5, and 6 further used a paradigm implemented via the web, thus ensuring that participants can rely on complete secrecy. Experiment 3 was designed to show that the effect of Honesty–Humility is not limited to monetary incentives. Experiment 4 rules out the alternative explanation that the link between Honesty–Humility and cheating may be due to trait-specific risk-preferences rather than a willingness to break rules to increase one's gains. Of note, Experiment 4 primarily measured intentional or "planned" cheating (whereas the other experiments entail a mixture of planned and spontaneous cheating) and it is thus interesting that this experiment revealed

Table 7
Summary of results.

Experiment	Paradigm	Incentives	Mode of data collection (sample)	Effect size of HH	Incremental effect of HH over: Other HEXACO factors	Incremental effect of HH over: Classic five factors
1	Judging statements	Monetary gains and losses	Lab (N = 168, students)	Small-to-medium	No	
2	Dice-task	Monetary gains	Lab (N = 88, students)	Medium	Yes	
3	Coin-toss task	Avoiding tedious work	Web (N = 185, community sample)	Small-to-medium	Yes	
4	Temptation paradigm[a]	Monetary gains	Lab (N = 106, students)	Medium-to-large	Yes	
5	Coin-toss task	Monetary gains (size varied between participants)	Web (N = 147, general population)	Small-to-medium	Yes	Yes
6	Coin-toss task	Monetary gains (common resource)	Web, longitudinal (N = 208, general population)	Medium-to-large	Yes	Yes

HH: Honesty–Humility.
[a] Note that the temptation paradigm measures cheating indirectly by giving participants the choice between a game yielding a larger risky gain and a game yielding a smaller gain that is safer if (and only if) one is willing to cheat.

the largest effect size for Honesty–Humility. Experiment 5 provided evidence that the effect of Honesty–Humility is robust across different baseline probabilities and incentive sizes. Finally, Experiment 6 demonstrated the effect of Honesty–Humility in a longitudinal setup and extended it to a common goods condition in which cheating potentially bears costs even for the perpetrator. Also, Experiments 5 and 6 show that the effect in question holds for a diverse, non-student sample (in line with recent criticism of the almost exclusive reliance on young, educated samples in psychological research, cf. Henrich et al., 2010).

Overall, the current findings provide a consistent account of individual differences in unethical behavior – in terms of a comprehensive theory of personality structure in general and the Honesty–Humility personality factor in particular. Concurrently, our results substantiate the importance of Honesty–Humility as a basic personality trait, the "reality" of this personality factor, and the validity of the corresponding (self-report) scale in the HEXACAO-PI-R. Given how rarely actual behavior is assessed as a criterion in personality research and beyond (Baumeister et al., 2007; Funder, 2001), the consistent association of Honesty–Humility with unethical behavior can be considered strong support. Also, the moderate effect sizes of Honesty–Humility for predicting cheating must be viewed in light of the current methodology: The addition of random noise (used to render responses non-incriminating) necessarily implies that the true effects of personality traits such as Honesty–Humility on cheating must have been underestimated in our results. The experimental setup thus comprised non-trivial hurdles for the personality traits to overcome.

From a broader perspective, it is our hope that the current set of investigations can help bridge existing gaps between the behavioral ethics field and personality research. Indeed, both fields stand to gain from such attempts: Behavioral ethics research will profit from a theoretically sound foundation for observable individual differences – as specified within a general model of personality structure – and thus a more comprehensive understanding of the determinants of deceit, dishonesty, and the like (Gino & Ariely, 2012). Observable variation in behavior is more than noise and accounting for this variation by means of personality theory will allow for more powerful future studies, the generation of novel hypotheses, and the integration of trait effects with the various situational influences documented in the behavioral ethics literature (Bazerman & Gino, 2012). Personality research, on the other hand, gains a rich set of well-devised paradigms to study a particular type of behavior and one that bears noteworthy societal relevance. Through such paradigms, the reality and real-life consequentiality of personality traits can be demonstrated vividly – exactly in line with the notion that "the relevance of these [personality] measures for consequential social behavior will have to be demonstrated rather than assumed" (Funder, 2009b, p. 121).

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