Who knows best what the next year will hold for you?**The validity of direct and personality-based predictions of future life experiences
across different perceivers**

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Abstract

This study explored the validity of person judgments by targets and their acquaintances (“informants”) in longitudinally predicting a broad range of psychologically meaningful life experiences. Judgments were gathered from four sources (targets: $N = 189$, and three types of informants: $N = 1,352$), and their relative predictive validity was compared for three types of judgment: direct predictions of future life experiences (e.g., number of new friendships), broad (Big Five) domains (e.g., extraversion), and narrower personality nuances (e.g., sociable). Approximately one year later, the targets’ actual life experiences were retrospectively assessed by targets, and informants nominated by the targets (TNI). Overall, we found evidence for predictive validity across predictor sources and types. Direct predictions by targets were by far the most valid, followed by TNI. Personality-based predictions by targets and TNI had substantial, but lower validity. Domain-based predictions were less valid than nuance-based predictions. Overall, informants with lower “liking” and “knowing” towards targets made less valid predictions. Person-centered multilevel analyses showed both considerable validity of direct predictions (which increased with knowing), and positivity bias (which increased with liking). Taken together, given the relatively high methodological rigor of the study, these results provide an especially realistic picture of the (rather moderate) predictive power of person judgments regarding future life experiences, and corroborate the common practice of obtaining such judgments from targets and their close acquaintances.

Keywords: Outcomes, Accuracy, Relationship, Predictive Validity, Personality

Nuances

Who knows best what the next year will hold for you? The validity of direct and personality-based predictions of future life experiences across different perceivers

A core assumption in personality research is that people judge their own and others' personalities in order to ultimately predict the respective target persons' future behavior, experiences, and life course. For example, when judging a person as "industrious", one implies that the target person may be counted on to invest effort and diligence into tasks and is likely to deliver satisfactory results. In contrast, when judging a person as "impulsive" one implies that the person may have a hard time controlling themselves, which may lead to all sorts of undesirable future life experiences (e.g., speeding tickets, arrests). In the present study, we investigate to what extent such personality judgments (both on the level of Big Five domains and on the level of personality "nuances") enable valid predictions of actual future life experiences (e.g., number of new friendships). We compare this validity to the predictive validity of "direct" predictions of the same life experiences (e.g., the *expected* number of new friendships). In addition, we examine *whose* judgments are most valid: Do the targets know best what to expect from their own future? Or are other people who know the targets ("informants") able to make better predictions? Finally, we ask how the respective perceivers' relationship with their target persons – in terms of how well they know and how much they like the target – influences the validity of the perceivers' judgments. To answer all of these questions, we ask targets to judge their own personality, and to make direct predictions regarding important experiences in the next year of their lives. We also ask different types of informants to do the same for the targets. Approximately one year later, we then compare all of these judgments with retrospective reports of the important experiences that actually took place in the targets' lives since the judgments were provided.

What are Relevant Consequences of Personality?

There is vast body of literature examining how personality is associated with - or predictive of - all sorts of variables. For example, studies show that personality predicts academic achievement and job performance (Connelly & Ones, 2010; Lüdtke, Roberts, Trautwein, & Nagy, 2011; Ozer & Benet-Martínez, 2006; Paunonen & Ashton, 2013; Poropat, 2014; Roberts, Kuncel, Shiner, Caspi, & Goldberg, 2007), various aspects of social relationships (Back, 2015), subjective well-being (Little, 2015; McCrae & Costa, 1991), and mortality (Jokela et al., 2019; Roberts et al., 2007; Turiano, Chapman, Gruenewald, & Mroczek, 2015), to name just a few.

In the present study, we examine the validity of direct and personality-based predictions with regard to a large variety of actual life experiences. Our list of relevant life experiences is based on – and thus overlaps considerably with – previous research in the field (e.g., Hammen, 1991, 2005; Sarason, Johnson, & Siegel, 1978; Vaidya, Gray, Haig, & Watson, 2002; Wessels, Zimmermann, & Leising, 2016). We focus on experiences that are (1) most relevant for our student population and time frame of 1 year, and (2) relatively “factual” in nature (i.e., assessing them requires little inference or interpretation). Consistent with most previous research, in addition to variables that may be considered “outcomes” or “consequences” in the strictest sense (e.g., having a child) (Wessels et al., 2016), we also assess many more variables that may better be called “responses” or “behaviors” (e.g., smoking) but are still very useful as they may be viewed as easily available *proxy* measures of outcomes that are important but much harder to assess (e.g., actual health risks). Therefore, instead of “consequences” or “outcomes” we use the somewhat broader term “life experiences” here (Sarason et al., 1978; Vaidya et al., 2002). To provide some measure of order for the large number of life experiences that we investigate, we classify them in

accordance with three broad life domains suggested by Ozer and Benet-Martínez (2006): individual, interpersonal, and social/institutional.

Direct vs. Personality-Based Predictions

We assess the predictive validity of two different kinds of person judgments: (1) direct ones, in which perceivers try to explicitly judge the likelihood of those very life experiences occurring in a target's life within the next year (i.e., "direct predictions"), and (2) personality-based ones, in which perceivers judge a target's personality, with no explicit link to any *particular* future life experience (i.e., "personality-based predictions"). As for personality judgments, we compare judgments on different levels of abstractness: (1) broader ones, on the level of Big Five domains (i.e., "domain-based predictions"), and (2) narrower ones, on the level of personality items representing markers of personality "nuances" (i.e., "nuance-based predictions") (McCrae, 2015; Möttus, Kandler, Bleidorn, Riemann, & McCrae, 2017). Big Five domains have repeatedly been shown to predict actual behavior (e.g., Back, Schmukle, & Egloff, 2009) and rather specific future life outcomes (e.g., Ozer & Benet-Martínez, 2006; Roberts et al., 2007; Soto, 2019). Interestingly, more recent research on personality nuances (Möttus et al., 2017; Möttus, Bates, Condon, Mroczek, & Revelle, 2019; Möttus, Sinick et al., 2019; Seeboth, Möttus, & Kandler, 2018) has shown that item level personality judgments seem to have considerable (incremental) predictive power over Big Five personality judgments in predictions of diverse life outcomes (Möttus, Bates et al., 2019; Seeboth et al., 2018). In comparing these different kinds of predictions with one another, one needs to consider the so-called "bandwidth-fidelity trade-off" (Cronbach & Gleser, 1957). The term denotes the inevitable trade-off between broadness and validity in person judgments: Broader person judgments (e.g., of Extraversion or Agreeableness) are likely to predict a whole range of future life experiences, but the predictive validity for each individual life experience will not be particularly high. In contrast, more specific predictions may be higher in validity, but

the downside of such predictions tends to be that they are much more limited in scope. In the present study, we compare the predictive validity of broader and narrower personality judgments regarding specific life experiences with the validity of direct predictions of the same life experiences. Given that the match in bandwidth is perfect in the latter case, the validity estimates obtained this way may function as a kind of benchmark.

Longitudinal Predictions Using Different Sources

In their literature review of some of the more recent literature investigating the consequences of personality, Wessels et al. (2016) identified several problematic features of typical studies in this field. First, only a relatively small number of studies actually used longitudinal designs, which would be necessary to draw somewhat firmer conclusions regarding the *direction* of effects. In the present study, we do predict the targets' future life experiences longitudinally, over the course of approximately one year. Second, in the vast majority of previous studies the personality variables that were used as predictors, as well as the life experiences that were to be predicted, were assessed by the same source (usually the targets themselves). This may be problematic for various reasons. For example, in single-source studies "shared method effects" (Campbell & Fiske, 1959; Podsakoff, MacKenzie, & Podsakoff, 2012) may be misinterpreted as systematic associations between personality and life experiences. One such type of shared method effect may be due to the perceivers' *formal response styles*, such as their differential tendencies to consistently prefer a certain location (e.g., a value in the middle) over others on the available response continuum (Wetzel, Lüdtke, Zettler, & Böhnke, 2016). Another shared method effect may be due to the perceivers' *evaluative attitudes* toward the targets, making some perceivers consistently describe their targets in more positive terms than others (Anusic, Schimmack, Pinkus, & Lockwood, 2009; Leising, Erbs, & Fritz, 2010; Leising, Scherbaum, Locke, & Zimmermann, 2015). Each of these effects by itself would be sufficient to create correlations between measures that would

have to be disentangled from the actual “substantive” correlations of interest. In the present study, we therefore do not only ask the target persons to describe themselves, but also have them recruit a number of so-called “informants” who know them well enough to describe them, make predictions regarding their future life experiences, and retrospectively report on those experiences later on.

Predictive Validity of Different Perceivers

Of course, the present study is not the first to use informant-ratings as an additional source of information besides the targets’ self-ratings (e.g., Connelly & Ones, 2010; Poropat, 2014; Vazire, 2006). There are many reasons to suspect that such ratings may have some validity over target-ratings (Vazire, 2010): For example, the visual perspective of others (as compared to the targets’) may be advantageous for making judgments of highly visible traits (e.g., of emotional expressivity). Also, the targets may be distracted by their own inner experiences when tasked with forming generalized judgments of their own behavioral inclinations. On the other hand, the targets have “privileged access” to certain types of information about themselves (especially their own preferences, thoughts, interests and so on), and are thus likely to provide the most valid judgments regarding more internal traits. Indeed, research has shown that personality ratings obtained from informants can be as valid or even *more* valid than targets’ self-ratings (e.g., Connelly & Ones, 2010; Poropat, 2014; Vazire, 2010; Vazire & Mehl, 2008), including longitudinal predictions of behaviors (e.g., Connelly & Ones, 2010; Thielmann, Zimmermann, Leising, & Hilbig, 2017) and outcomes (e.g., Jackson, Connolly, Garrison, Leveille, & Connolly, 2015; Luan et al., 2019).

In most previous studies, however, informants were recruited by the targets themselves. Such informants will hereafter be called “target-nominated informants” (TNI; Leising, Erbs & Fritz, 2010). Research has shown that target-nominated informants tend to be people who are very close to the targets (e.g., spouses, friends). This may pose advantages as

well as disadvantages in terms of data quality: On the positive side, close acquaintances may be expected to *know* the targets best, given that they (1) probably spend more time with them than do most other people, (2) may have more opportunities to learn about the targets' inner experiences (e.g., by talking to them), and (3) may be able to observe the targets' behavior in more private situations. In fact, a large body of empirical evidence suggests that such acquaintance *does* promote the validity of person judgments (e.g., Bernieri, Zuckerman, Koestner, & Rosenthal, 1994; Biesanz & Human, 2010; Biesanz, West, & Millevoi, 2007; Blackman & Funder, 1998; Borkenau, Mauer, Riemann, Spinath, & Angleitner, 2004; Connelly & Ones, 2010; Funder & Colvin, 1988; Letzring, Wells, & Funder, 2006; Paunonen, 1989). In the present study, we also expect to find evidence for such an effect. In addition to this likely advantage of personality ratings by target-nominated informants, such ratings can also be obtained with relative ease (Vazire, 2006), which is probably one of the main reasons why using this kind of data has become relatively common.

However, research has also shown that target-nominated informants tend to like their targets very much (Leising et al., 2010), and that they often produce descriptions of their targets that are even more positive than the targets' self-descriptions (Leising, Gallrein, & Dufner, 2014). Although validity and bias are not necessarily related to one another (West & Kenny, 2011), it may be asked whether such extremely positive attitudes can be detrimental to the validity of judgments. Some evidence for this was uncovered in previous studies (e.g., Wessels, Zimmermann, Biesanz, & Leising, 2020). Such an effect seems possible for various reasons: For example, perceivers who like their targets may either be aware of their targets' more negative characteristics but simply not report them out of loyalty. Another reason is that these perceivers may manage to systematically overlook the more negative aspects of their targets' personalities, which would also result in overly positive images of the targets that lack validity, despite the perceivers actually believing in them (e.g., Sackeim & Gur, 1978, 1979).

In the present study, we investigate this issue by examining the relative predictive validity of judgments that are provided by different types of informants whose relationships with the targets are characterized by varying degrees of closeness. More precisely, in addition to TNI, we recruit a number of informants per target (with the targets' explicit consent) who are not nominated by the targets. As these latter informants are recruited from groups to which the targets also belong (e.g., university seminars), we will hereafter call them “group informants” (“GI”). Based on what the group informants say about how much they like the targets they judge, we assigned the group informants to one of two sub-groups: the “high-liking group informants” (“GI+”) and the “low-liking group informants” (“GI-”).

The Present Study

After briefly addressing the issue of inter-rater agreement in judgments of life experiences (which, according to our knowledge, has not been looked at before), we present two sets of analyses addressing our main research questions. The first set of analyses we present is variable-centered. It addresses the question of how strong the validity of predictions of the targets' life experiences is overall, and how it varies across predictor types (domain-based, nuance-based, direct) and across predictor sources (targets, TNI, GI+, GI-). The second set of analyses we present is person-centered. It addresses the question of how validity and positivity bias in direct predictions of life experiences vary with how well a perceiver knows a target, and how much a perceiver likes a target.¹

¹ As design and analyses presented in the present paper are fairly complex, we give an overview of terminology in the Supplemental Note S1 in our OSF-project in order to make the presentation as unambiguous and easy-to-follow as possible.

Methods

The present study uses unique longitudinal data on psychologically meaningful life experiences which have not been published before. They are part of a larger dataset with Wave 2 personality ratings previously published in Study 2 in Wessels et al. (2020). Before consenting to participate, participants were fully informed about purpose and procedures of the study. No experimental manipulation took place, and there was no risk involved of being harmed. The German Research Foundation (DFG) granted the study without an additional ethics review. In total, there were four waves of assessment. In the present study, we focus on waves one to three, and report only those measures that are relevant to our research questions laid out above. The data is publicly available on the Open Science Framework (<https://osf.io/35mej/>). A complete list of all variables that were assessed may be obtained from the first author upon request. We report all measures relevant to the research questions that are addressed in the present study. We also report how we determined our sample size, and when and why we excluded which data from the analyses.

Recruitment

Participants were recruited in pre-existing groups of students, namely seminars and student councils from different faculties at two German universities. We did not perform a specific a-priori power analysis to estimate the sample size needed for the (complex) statistical models used in the present study. However, as a very rough approximation, we originally aimed for 350 targets in order to be able to statistically corroborate an effect size of $r = .15$ for a simple correlation between two variables with α (two-tailed) = .05 and $1-\beta = .80$. Recruitment turned out to be more difficult and time consuming than anticipated, however, so we (1) adapted our remuneration strategy in between to make participation more attractive and (2) had to stop recruiting before reaching the originally planned sample size as resources and time were exhausted. A group of subjects was eligible for the study when a minimum of

six persons were willing to participate, which was true for 355 groups with a total of 3575 students. Group size ranged between 6 and 22.

Wave 1

Procedure and Sample

The purpose of the first round of assessments was to recruit target persons and informants, with the latter knowing the targets but not being selected by them. Members of eligible groups were sent a link and a personalized code to access a round-robin online questionnaire in which participants were asked to report how much they liked and how well they knew each of the other group members (see below). The personalized code was established to match data from the different waves and to identify members of the same group. This first questionnaire was completed by 2801 participants, constituting a response rate of 78%. While initially participants were not compensated for completing the first questionnaire, participants who were recruited later received 5 € for their participation, in order to make participation more attractive (see above).

Based on the data from this questionnaire, we determined one target and up to six group informants for each group. This was done using an algorithm aiming at maximizing the range of liking and knowing of the informants towards the targets. Participants qualified as potential targets when there were at least two others in their group who reported not liking them too much (GI-; mean liking ≤ 3.5 on a 1-5 rating scale) and two more others who reported liking them (GI+; mean liking > 3.5) (Wessels et al., 2020). Seventy-seven groups were excluded because no suitable target could be identified based on the algorithm. Overall, the procedure led to the inclusion of 278 groups whose members were asked to complete the second online questionnaire (see Wave 2).

Measures

Initial Recognition. Participants were presented each of the other group member's full name and asked whether they knew who the respective person was (*yes/no*).

Liking and Knowing. For any group member that participants said they knew who the person was, we asked them to report how much they liked and how well they knew that person with four items each which were presented in randomized order. For both scales, two items had a negative valence, and two had a positive valence and internal consistencies were high (see Supplemental Table S2 in our OSF-project). We calculated liking and knowing means by rescaling the two negative items and then averaging the reported values on all four items. The response options ranged from 1 (*does not apply at all*) to 5 (*applies exactly*).

Wave 2

Procedure and Sample

The second questionnaire followed shortly after the first questionnaire. Its purpose was to collect target- and informant-ratings of the targets' personalities, baseline measures for some of the life experience variables, and explicit expectations of all perceivers as to what behavior frequencies and life events they would expect to occur in the targets' lives over the course of the next year. It is important to note that, for practical reasons, for many life experiences we assessed behavioral proxies rather than actual outcomes in the strictest sense (Wessels et al., 2016). For example, we assessed the number of cigarettes smoked as a proxy for negative consequences that are associated with smoking, but do not necessarily occur within one year (e.g., lung cancer). The questionnaire was administered in a one-with-many fashion (Kenny, Kashy, & Cook, 2006), that is, all members of each group (including the target) described the same person (the target). In addition, the targets were asked to provide email-addresses of up to three more informants (TNI) who had agreed to also describe them.

This way, we obtained more typical informant ratings in addition to the above-described group informant ratings, giving us the opportunity to compare the predictive validity of judgments by the different types of informants.

For statistical analyses, we included those groups in which at least one target and at least two group informants had completed this second online questionnaire (the maximum number of group informants per target was six). Out of the 278 groups whom we had asked to participate in this phase of the study, 189 ultimately met the requirements: The sample at this stage of the study thus consisted of 189 targets (female = 124; age: $M = 22.49$, $SD = 3.48$). 158 targets had recruited at least one TNI who completed the second questionnaire, resulting in a total of 409 TNI (female = 264; age: $M = 30.01$, $SD = 13.17$; range: 1-3; 2.59 per target who had nominated at least 1 informant). Overall, there were 943 group informants (female = 643; age: $M = 22.62$, $SD = 3.16$) with an average of 4.99 GI per target (range: 2-6). Out of these, 534 informants reported low average liking levels and 409 informants reported high liking levels towards the target (cutoff: 3.5). Group informants with high and low liking levels did not differ with regard to education, $\chi^2(6, N = 943)$, $p = .33$, age, $t(941) = -1.11$, $p = .27$, and gender, $p = .72$ (Fisher's exact test).

As for remuneration, targets initially received 20 €, and group informants entered a lottery with 500 prizes of 20 €. However, when it became increasingly difficult to recruit participants (see above), we decided to adapt the remuneration scheme in order to make participation more attractive. Now, all group informants and targets received 10 € for completing the second online-questionnaire, and targets received another 10 € when providing contact details of potential TNI who had agreed to participate. As an additional incentive, all participants in Wave 2 also entered a lottery of 1000 € and two times 500 €.

Measures

Liking and Knowing. As in Wave 1, we assessed the informants' liking and knowing towards the target with four items each (e.g., "I do not like this person", see Supplemental Table S2 in our OSF-project) and computed mean values across the four items, respectively, with very high internal consistencies for both the liking (ICC [2, 4] = .89) and the knowing (ICC [2, 4] = .93) scale (Wessels et al., 2020). Response scales ranged from 1 (*does not apply at all*) to 5 (*applies exactly*). Across the different informant groups, the two variables were positively associated with one another, $r_{(1352)} = .62, p < .01, 95\% \text{ CI } [.59, .65]$ (Wessels et al., 2020). As expected, the three informant groups differed with regard to their average liking *and* knowing levels towards the target (see Table 1): GI- reported significantly lower liking ($t_{Welch}(904.64) = -45.35, p < .001, \Delta = -1.50, 95\% \text{ CI } [-1.56, -1.43], d = 2.89$) than GI+ and also significantly lower knowing ($t_{Welch}(804.14) = -10.49, p < .001, \Delta = -0.63, 95\% \text{ CI } [-0.75, -0.51], d = 0.70$). In addition, GI+ reported significantly lower liking ($t_{Welch}(798.71) = -12.90, p < .001, \Delta = -0.41, 95\% \text{ CI } [-0.46, -0.35], d = 0.97$) and knowing ($t_{Welch}(620.16) = -27.70, p < .001, \Delta = -1.51, 95\% \text{ CI } [-1.61, -1.40], d = 1.94$) than did TNI. In the following analyses, we report liking and knowing values from Wave 2.

Predicted Life Experiences (PEX). We compiled a list of 73 items to assess direct predictions of different aspects of the targets' (future) life experiences (see Supplemental Table S3 in our OSF-project).² Based on existing lists of life events (Lüdtke et al., 2011; Sarason et al., 1978; Vaidya et al., 2002), we selected those events from the questionnaires that were most relevant to our student participant group and to the given time frame of one

² Note that we retrospectively assessed a number of additional life experiences at T3, which did not have direct predictions at T2, and are therefore not part of the present study.

year and formulated respective questions fitting our study design (e.g., “What do you expect: Within the next year, how many new romantic relationships are you going to have that will last at least one month?”). In addition, we added various items going beyond specific life events representing life change, but covering certain behaviors and outcomes occurring on a more regular basis. Items were answered on different scales: some were dichotomous (*yes/no*) and some were open numeric questions (e.g. “__ days a week”). We asked all participants (targets, GI and TNI) to make predictions about the developments of the targets’ life by means of these life experience items, which were only slightly reformulated to adapt them to the different perspectives (self- and informant-reports).

However, as the extent to which informants knew the targets was very diverse (due to our study design) and especially low liking group informants tended not to know the targets very well, we applied a special technique in order to allow all informants to also make valid predictions for life experiences that were conditional upon a baseline (e.g., “Are you currently in a relationship?”). For these life experiences, we provided all informants (GI and TNI) with the different possible baseline scenarios and asked them to make predictions based on each of those scenarios, *independent* of whether they knew about the target’s current situation (e.g., “Imagine this person is currently in a relationship. What do you expect: In one year, will this person still be in the same relationship?” *and* “Imagine this person is currently single. What do you expect: In one year, will this person still be single?”). For the analyses, we then selected those informant predictions that were in line with the actual target-reported baseline.

Personality Ratings. We assessed the Big Five personality traits with 30 adjectives compiled by Borkenau and Ostendorf (1998). Each factor is covered by six adjectives (e.g., “clever”), out of which three have a positive and three have a negative valence. Targets’ self-ratings and informant-ratings were assessed with the same set of items, which we rephrased somewhat to adapt them to the different perspectives (self- and other-reports). The response

scale ranged from 1 (*does not apply at all*) to 5 (*applies exactly*). Values for each of the Big Five personality traits were computed as means across the six items, after rescaling the three items with a negative valence. Internal consistencies of the five scales were high both per predictor source and across predictor sources (see Supplemental Table S4 in our OSF-project for more details), with an overall *Mdn* $\alpha = .77$ for Agreeableness, *Mdn* $\alpha = .78$ for Conscientiousness, *Mdn* $\alpha = .85$ for Extraversion, *Mdn* $\alpha = .76$ for Emotional Stability, and *Mdn* $\alpha = .73$ for Openness. The Big Five personality traits (= domain-based) as well as the 30 adjectives (= nuance-based) rated by the different perceivers were used for personality-based predictions of actual life experiences.

Wave 3

Procedure and Sample

As had been clearly stated in the beginning, approximately one year later, targets and target-nominated informants were contacted again via e-mail, provided with their personal code and invited to log in to another online questionnaire. The Wave 3 questionnaire assessed retrospectively whether or how often all of the life experiences had occurred in the target's life in the course of the last year. On average, the interval between Wave 2 and Wave 3 was 369.91 days for targets (*SD* = 11.97) and 354.90 days for target-nominated informants (*SD* = 25.87).

The Wave 3 questionnaire was originally completed by 155 targets, who received another 25 € for their participation, and by 345 target-nominated informants. Again, we excluded informants from statistical analyses when more than three TNI per target completed the questionnaire, which was true in two cases. One group with only one TNI was excluded because it was excluded from analyses in Wave 2 due to a failure to meet the criteria. We therefore included the data of 155 targets (response rate: 82%; female = 103; age: *M* = 23.46, *SD* = 3.47) and 342 target-nominated informants (response rate: 84%; female = 224; age: *M* =

31.55, $SD = 13.36$) in the statistical analyses. These were distributed across 162 groups. More specifically, there were 144 groups with both target- and 1-3 TNI-ratings, 12 groups with only target-ratings, and 6 groups with only TNI-ratings. Of the 342 target-nominated informants, 330 had also participated in Wave 2, while there were 12 participants who only completed the Wave 3 questionnaire. Therefore, participant numbers may vary depending on the analysis at hand.

We examined possible differences between participants who participated in Wave 3 and those who did not with two separate multiple logistic regressions. In the model predicting target dropouts, we included gender, age, education, and self-reported Big Five traits. In the model predicting target-nominated informant dropouts, we included gender, age and education, as well their liking and knowing towards the targets. Overall, there were basically no differences between targets and target-nominated informants who completed the Wave 3 questionnaire as opposed to participants who did not. Neither gender (T: $OR = 0.65, p = .37$; TNI: $OR = 0.84, p = .51$), nor education (T: $OR = 0.94, p = .84$; TNI: $OR = 1.13, p = .18$) significantly predicted dropouts in Wave 3. There were slightly higher odds for older target-nominated informants to participate in Wave 3 as opposed to younger TNI ($OR = 1.02, p < .05$), while there were no significant age differences for targets (T: $OR = 0.99, p = .81$). For targets, there were also no differences in terms of self-reported Big Five traits (Agreeableness: $OR = 0.66, p = .25$; Conscientiousness: $OR = 1.91, p = .12$; Extraversion: $OR = 0.82, p = .48$, Emotional Stability: $OR = 1.27, p = .39$; Openness: $OR = 1.01, p = .98$). For target-nominated informants, there were also no differences with regard to Wave 2 liking ($OR = 0.90, p = .74$) and knowing ($OR = 1.20, p = .49$) towards the target.

Measures

Actual life experiences (AEX). The list of 73 life experience items was now used by the targets (= target-rated actual life experiences, AEX_t) and the target-nominated

informants (TNI-rated actual life experiences, AEX_tni) to retrospectively assess the actual level of the life experience variables. The items were only adapted in terms of tense (e.g., “Last year, how many exams did you pass?”). Target-nominated informants were provided with an alternative response option when they thought they were not able to answer the question properly (“*I do not know*”). These were treated as missing values in the analyses.

Characteristics of Actual Life Experiences.

Distribution of Life Experience Variables. The 73 life experiences had different response scales, including dichotomous variables and a number of count and numeric variables. We dichotomized these variables for mainly two reasons: First and most important, we wanted to make analyses directly comparable with each other, as our main aim was not to show specific effects for specific life experiences but to explore the “big picture”. Almost all of the non-dichotomous variables were extremely skewed calling for a number of different statistical computing strategies and leading to major convergence problems. The dichotomization allows us to directly compare the analyses with different experiences with each other and report aggregate values across life experiences. Second, the dichotomization enables multilevel profile analyses across experiences (see below).

After an initial exclusion of extreme outliers (e.g., 88900 pregnancies during the last year) and outliers based on logical reasoning (e.g., more than 7 days per week of feeling hurt with a maximum of 7 days per week), we dichotomized the respective variables by recoding them into 0 representing “no” (= 0) and > 0 representing “yes” (= 1), where a meaningful 0 was present. In the course of this procedure, we excluded 19 life experiences where the value zero was implausible (also mirrored in no to very few zero ratings) (e.g., body weight), four life experiences due to constant values (e.g., number of days spent in prison) and three life experiences with extremely restricted distributions for either target-ratings, TNI-ratings, or both (e.g., number of abortions) (see Supplemental Table S3 in our OSF-project for more

details as to which life experience was excluded for which reason). The final analyses are thus based on 47 dichotomous life experience variables (see Table 2). The biserial correlations between the original and the respective dichotomous variables were high, with a median of $r_{Mdn} = .92$ for direct predictions of life experiences (range: .48 to 1), $r_{Mdn} = .98$ for target-rated actual life experiences (range: .45 to 1), and $r_{Mdn} = 1$ for TNI-rated actual life experiences (range: .46 to 1) (see Table 2).

Next to target-rated actual life experiences, we used the consensual impressions of close others (AEX_tni) as a second validation criterion for judgmental validity. The values for the TNI-rated actual life experiences therefore represent the *consensus* among target-nominated informants per target. They were computed as the mode of 1-3 target-nominated informants. Where there was no agreement among informants, that is, where no mode could be determined (e.g., when there was 1 TNI saying “yes” and 1 TNI saying “no”), the value for the respective life experience was set to missing. This was true for 5.1% of valid cases.

Content ratings. Life experiences were independently classified by two of the authors with regard to three broad life domains suggested by Ozer and Benet-Martínez (2006) in their literature review: (1) individual, (2) interpersonal, and (3) social/institutional. We use these three life domains as a scheme for organizing the variables with regard to content. Agreement among the two raters across all 47 variables was very high ($\kappa = .97$). In cases of disagreement ($n = 1$) the authors agreed on a category after discussion. Overall, 31.9% ($n = 15$) of the life experiences in the present study can be classified as “individual”, 17.0% ($n = 8$) as interpersonal and 51.1% ($n = 24$) as social/institutional. We arrange the results of our analyses in some of the tables and figures along these three domains.

Social desirability ratings. All 47 life experience variables were rated with regard to social desirability (“To what extent does the description of a target with this statement imply a

positive or negative evaluation?”). Ratings were obtained from a separate group of eighteen psychology students (16 females) between the age of 20 and 24 ($M = 21.72$, $SD = 0.96$). They were recruited at a university in a mid-sized German city and were remunerated with 5 euros for their participation. Social desirability was rated on a 10-point scale ranging from 1 (*very negative*) to 10 (*very positive*) with almost perfect reliability ($ICC [2, 18] = .98$). Social desirability ratings of actual life experiences ranged from 1.33 to 9.67 (on a 1-10 point scale) with a mean of 5.69 ($SD = 2.26$) (see Supplemental Figure S5 in our OSF-project displaying results per life experience). That is, they basically covered the full spectrum of the scale and were almost normally distributed. The items' average social desirability ratings were used to account for positivity bias in the person-centered analyses.

Statistical Analyses

Data preparation and descriptive statistics were done in SPSS Statistics Version 26.0, and analyses were conducted in R 3.5.3 (R Development Core Team, 2018; 2019) using the R packages *brms* (Bürkner, 2017), *lme4* (Bates, Mächler, Bolker, & Walker, 2015), *psych* (Revelle, 2018) and *tidyverse* (Wickham et al., 2019). The respective R-scripts needed to reproduce our results, as well as additional tables can be retrieved from our OSF project (<https://osf.io/35mej/>). Please note that most of the following analyses were exploratory in nature and we did not pre-register any specific hypothesis.

Variable-Centered Analyses

The following analyses are based on a “variable-centered” approach. In variable-centered analyses, the relationship between two variables is examined, with the individual perceiver-target-dyads constituting the cases.

Self-other and other-other agreement. We computed the average pairwise agreement between targets and target-nominated informants (i.e., self-other agreement), as

well as between target-nominated informants (i.e., other-other agreement) per (dichotomized) life experience using Cohen's Kappa. That is, for each life experience, we first computed Cohen's Kappa for individual pairs of perceivers (e.g., targets with their respective first informants, targets with their respective second informants, etc.), and then derived the median of those Kappas. We also report the average of these median Kappas across all 47 life experiences.

Validity of Predictions. One core issue addressed by the present study concerned the predictive validity of judgments by different groups of perceivers (i.e., "predictor sources"), namely targets' self-ratings ("T") compared to target-nominated informant ratings ("TNI"), compared to ratings of informants who were recruited independent of the targets, with higher ("GI+") or lower ("GI-") liking towards the target. By comparing these different sources with one another, we were able to determine whose ratings were more valid in predicting actual life experiences. We also compared the predictive validity of these different predictor sources with regard to three predictor types: direct predictions of life experiences, domain-based predictions (based on Big Five domain scores) and nuance-based predictions (based on item ratings as markers of nuances). For both types of personality-based predictions, we tested omnibus personality models including all of the respective personality ratings (5 for the Big Five, and 30 for the nuances) simultaneously in each of the models. This is in line with research suggesting that, especially with small sample sizes, individual effect sizes may be too small to be detected, but combining the effects in one model may yield significant predictions of outcomes (see Möttus et al. (2017) for a more detailed discussion of this reasoning). Also, we were mainly interested in the overall predictive power of personality ratings compared to direct predictions and not in specific effects of individual domains or nuances. To answer the research questions outlined above, *separately* for each of the four predictor sources (T, TNI, GI+ and GI-) we conducted several sets of (multiple) logistic regressions using (1) target-

rated actual life experiences (AEX_t) and (2) TNI-rated actual life experiences (AEX_tni) at Wave 3 as two distinct validation measures (i.e., criterion variables) to be predicted from (1) *direct predictions* of these life experiences, (2) *all five Big Five domain scores*, and (3) *all 30 Big Five items* (6 per domain) measured at Wave 2.

For each target, we randomly drew one informant of each type (GI-, GI+ and TNI) out of all informants who had described that target at Wave 2. Then we used the different ratings, including the targets' self-ratings, to predict the targets' actual life experience levels at Wave 3. The random selection of informants was repeated 1000 times, to obtain more reliable results. We report the median of the respective coefficients across all 1000 iterations.

Before conducting the final analyses, we had to exclude several life experiences per validation measure causing estimation problems. More precisely, we excluded any life experience item with (1) extremely low or high occurrence rates ($<7.5\%$ or $>.92.5\%$) for both validation measures or (2) more than 60% missing values. This led to the exclusion of 14 target-rated and 16 TNI-rated actual life experiences due to (1) and two target-rated actual life experiences due to (2). In addition, there was one more life experience item creating estimation problems beyond these criteria for target-rated actual life experiences, which we also excluded (see Table 2 for information on which item was excluded for which reason). The core results we report are thus based on 3 (types of predictions) $\times$ 4 (sources) $\times$ 30 (life experiences) = 360 separate analyses for target-rated actual life experiences, and 3 $\times$ 4 $\times$ 31 = 372 separate analyses for TNI-rated actual life experiences. When we answer our research questions, we sometimes average the results of these analyses across predictor types, or across predictor sources (see Tables 3-6).

To derive p -values for individual models and relevant combinations of models, we applied a permutation strategy. That is, we first repeated each analysis 1,000 times, each time

randomly rearranging the values of the dependent variable.³ This way we created distributions of model coefficients under the null hypothesis for each model. We also created distributions of aggregated model coefficients (e.g., median across sources and types of predictions) as well as of differences between those aggregated coefficients. We then compared our observed values with these distributions to derive *p*-values, and statistical significance was determined by an alpha level of 1%. Models were tested one-tailed yielding significant results when the observed coefficient was larger than the 99% quantile of the distribution under the null hypothesis. Differences between models were tested two-tailed, yielding significant results when the observed value was smaller than the 0.5% quantile or larger than the 99.5% quantile of the distribution under the null hypothesis.

We selected the adjusted entropy-based R^2 ($R^2_{E,adj}$) proposed by Mittlböck and Schemper (1996) as our focal coefficient of predictive validity for individual models. $R^2_{E,adj}$ measures the relative reduction in maximized log-likelihood:

$$R^2_{E,adj} = 1 - \frac{LL_M - \frac{(k+1)}{2}}{LL_0 - \frac{1}{2}},$$

with LL_m representing the log-likelihood of the model, LL_0 the log-likelihood of the null (“intercept only”) model, and k the number of predictors in the model. $R^2_{E,adj}$ can be understood as a compromise between the unadjusted and the adjusted McFadden’s pseudo R^2 : While the unadjusted McFadden’s pseudo R^2 does not correct for the number of predictors

³ When conducting the 1,000 permutations, we reduced the number of random selections of informants from 1,000 to 20 for models involving informant ratings as predictors. This was done due to constraints regarding computational power and time (analyses with 1,000 random selections would have taken almost a year to compute on our computers).

and is upwardly biased in small samples with multiple predictors, the correction of the adjusted McFadden's pseudo R^2 is very strict and may lead to downwardly biased estimates. Indeed, when examining the expected values of these two pseudo R^2 statistics under the null hypothesis (using the median across all models and permutations), the expectation for the unadjusted McFadden's pseudo R^2 was much greater than zero, $E(R^2) = .0323$, and the expectation for the adjusted McFadden's pseudo R^2 was considerably smaller than zero, $E(R^2) = -.0543$. In contrast, $R^2_{E,adj}$ performed much better, with $E(R^2_{E,adj}) = -.0004$. However, there was some bias for $R^2_{E,adj}$ under specific conditions (e.g., the expected value was $> .01$ in 34.6 % of the simulated conditions). This remaining bias was almost completely driven by models for nuance-based ($k = 30$) predictions (in 96.4 % of the cases), while models for both direct ($k = 1$) and domain-based ($k = 5$) predictions of future life experiences were basically unbiased. In order to control for this remaining artificial inflation of predictive validity due to overfitting and to make the results more comparable with each other, we corrected each observed $R^2_{E,adj}$ value by subtracting the respective expected value under the null hypothesis (i.e., the median $R^2_{E,adj}$ across the 1,000 permutations). In the following, we consistently use these "bias-corrected" R^2 values.

As the interpretation of pseudo R^2 values is not so straight forward, we use the common interpretation of correlation coefficients ($r = .1$ for small, $r = .2$ for moderate and $r = .3$ for large) in personality psychology as a comparison standard (Funder & Ozer, 2019; Gignac & Szodorai, 2016). When transforming these into R^2 values, the resulting equivalent values would be $R^2 = .01$ for small, $R^2 = .04$ for moderate and $R^2 = .09$ for large. Note however, that in logistic regressions these values do not represent the amount of explained variance, but the relative extent to which the maximized log likelihood is reduced. To account

for this, we will mainly focus on the *relative* effects by comparing the effects for different predictor sources and types.

Person-Centered Analyses: Differential Effects of Liking and Knowing on Validity and Positivity Bias in Direct Predictions of Actual Life Experiences

Another aim of the present study was to investigate whether and how the perceivers' liking and knowing of their targets *differentially* affected their ability to make accurate predictions regarding those targets' life experiences in the course of the next year. We applied multilevel modeling to examine these effects on the informants' direct predictions of future life experiences. The analyses are based on the social accuracy model (Biesanz, 2010) and essentially represent an applied case of the Truth and Bias model (T&B model, West & Kenny, 2011), extended to multilevel profile analyses. That is, the core unit of analysis is the person ("person-centered approach") (Biesanz, 2010; Furr, 2008), and for each perceiver-target dyad, we predicted associations between individual profiles of actual and predicted life experiences from two relationship variables (i.e., liking and knowing). The analyses were applied in a similar fashion in previous research, where a detailed description of the approach is available (Wessels et al., 2020; Zimmermann, Schindler, Klaus, & Leising, 2018).

In the T&B model, the accuracy of a certain judgment is examined by predicting it from (1) truth variable(s) and (2) bias variable(s). In the present research, we performed two multilevel profile analyses. In both, the bias variable was the profile of the individual experiences' social desirability ratings. In Model 1, we used the complete profile of all 47 target-rated actual life experiences, and in Model 2 we used the profile of the 47 TNI-rated actual life experiences as truth variables. In the first analysis using AEX_t, the predictions were provided by all informants (regardless of informant group). In the second analysis using AEX_tni, predictions by the TNI were excluded to avoid shared method effects. The life experience ratings (Level 1) were nested in perceivers (Level 2), which were nested in targets

(Level 3), both for target-rated (Model 1) and TNI-rated (Model 2) actual life experiences. In the T&B model, the extent to which judgements are accurate is called “truth force”, while the extent to which judgments are biased is called “bias force”. We examined the effects of liking and knowing as moderators of the truth and bias forces. The models are based on Bayesian estimations using “Stan” for full Bayesian inference via the package “brms” (Bürkner, 2017) in R 3.5.3 (R Development Core Team, 2018; 2019). The model can be expressed by the following equations:

Model

Level 1:

$$\ln\left(\frac{P(PEX_{ipt} = 1)}{1 - P(PEX_{ipt} = 1)}\right) = \pi_{0pt} + \pi_{1pt}AEX_{ipt} + \pi_{2pt}DES_i$$

Level 2:

$$\begin{aligned}\pi_{0pt} &= \beta_{00t} + \beta_{01t}Liking_{pt} + \beta_{02t}Knowing_{pt} + r_{0pt} \\ \pi_{1pt} &= \beta_{10t} + \beta_{11}Liking_{pt} + \beta_{12t}Knowing_{pt} + r_{1pt} \\ \pi_{2pt} &= \beta_{20t} + \beta_{21t}Liking_{pt} + \beta_{22t}Knowing_{pt} + r_{2pt}\end{aligned}$$

Level 3:

$$\begin{aligned}\beta_{00t} &= \gamma_{000} + u_{00t} \\ \beta_{01t} &= \gamma_{010} \\ \beta_{02t} &= \gamma_{020} \\ \beta_{10} &= \gamma_{100} + u_{10t} \\ \beta_{11} &= \gamma_{110} \\ \beta_{12t} &= \gamma_{120} \\ \beta_{20t} &= \gamma_{200} + u_{20} \\ \beta_{21t} &= \gamma_{210} \\ \beta_{22t} &= \gamma_{220}\end{aligned}$$

The dependent variable represents the log odds of informant (perceiver) p on life experience (item) i to report expecting the respective life experience (PEX) for target t to occur (0 = no, 1 = yes). It was predicted by the actual life experience for target t (AEX_t) as rated by the targets themselves (AEX_t , Model 1; or by the TNI: AEX_tni , Model 2) and the items’ social desirability (DES_i) on Level 1, and perceiver p ’s liking ($Liking_{pt}$) and knowing ($Knowing_{pt}$) towards target t on Level 2. In order to enhance interpretability, we centered $Liking_{pt}$ (Model 1: $M = 3.75$; Model 2: $M = 3.37$), $Knowing_{pt}$ (Model 1: $M = 3.22$; Model 2: $M = 2.66$) and DES_i ($M = 5.69$) around their respective grand mean.

The fixed effect γ_{000} contains the log odds for the average informant expecting that a life experience with average desirability will occur (PEX) for his or her target when the actual life experience did not occur ($AEX = 0$). The fixed effects γ_{010} and γ_{020} represent the expected increase in the log odds of reporting that a life experience with average desirability is expected to occur for every 1 unit increase in liking and knowing, respectively, irrespective of whether the actual life experience occurred. That is, they reflect how much the informants' predictions of future experiences for their targets varied depending on how well they said they knew their targets, and how much they said they liked their targets. The expected changes in these values due to the actual life experience level (AEX ; 0 = no, 1 = yes) are represented by the fixed effects γ_{100} , γ_{110} , and γ_{120} . More precisely, the fixed effect γ_{100} represents the expected increase in the log odds of reporting that a life experience with average desirability is expected to occur when the actual life experience was rated to have occurred, holding liking and knowing constant at their respective means. It is thus a measure of the average informant's validity in predicting the actual life experience. The fixed effects γ_{110} and γ_{120} represent the extent to which the associations between the log odds of reporting that a life experience with average desirability is expected to occur (PEX) and the rated occurrence of the actual life experience change due to liking and knowing, respectively. That is, they reflect how the validity of the informants' predictions changed with how much they said they liked their targets, and with how well they said they knew their targets. The fixed effect γ_{200} represents the average positivity bias in predictions of future life experiences across targets and perceivers. The expected change in positivity bias due to variations in liking and knowing, respectively, are represented by the fixed effects γ_{210} and γ_{220} . Please note that, in order to account for any other possible differences between GI and TNI (e.g., due to the different recruitment strategies), in Model 1 we included an additional dummy-coded variable (*Type*) representing informant type (0 = GI, 1 = TNI) as another predictor in each of the three

equations at Level 2, which is not depicted in the algebraic model above: The respective fixed effects γ_{130} and γ_{230} represent the expected change in accuracy and positivity bias that are due to the respective perceiver belonging to a specific informant group (GI vs. TNI), while holding liking and knowing constant at their means.

The random effects r_{0pt} , r_{1pt} , and r_{2pt} as well as u_{00t} , u_{10t} and u_{20t} represent deviations in the log odds for the predicted experience as well as positivity bias and accuracy of the predictions that are due to perceivers p and targets t , respectively. Random effects were assumed to be normally distributed and allowed to correlate within levels. Because of the selection algorithm, targets t did not differ in terms of liking and knowing, so we did not include the respective random effects for the sake of model simplicity.

Results

When referring to specific life experiences, we provide the respective life experience number (see Table 2) in brackets.

Variable-Centered Analyses

Self-Other and Other-Other Agreement in Judging Actual Life Experiences

Agreement between targets and target-nominated informants (i.e., self-other agreement), as well as between target-nominated informants (i.e., other-other agreement) varied greatly depending on the outcome being measured (see Table 2). It ranged from basically no agreement for, e.g., “drunk driving” [47] (self-other agreement $\kappa = .00$; other-other agreement $\kappa = .00$) and “debts” [13] (self-other agreement $\kappa = .00$; other-other agreement $\kappa = .00$) to very good agreement for, e.g., “continuing the same studies” [25] (self-other agreement $\kappa = .81$; other-other agreement $\kappa = .72$) and “pregnant” [21] (self-other agreement $\kappa = .90$; other-other agreement $\kappa = 1.00$). For most life experiences, the two forms of agreement were of very similar magnitude, $r = .82$, $p < .01$. The largest difference was

found for “fare dodging” [44] (self-other agreement $\kappa = .18$; other-other agreement $\kappa = .64$), followed by “cheated on partner” [20] (self-other agreement $\kappa = .26$; other-other agreement $\kappa = -.08$). Across all 47 life experiences, there was considerable self-other agreement, with a median of $\kappa = .44$, and other-other agreement, with a median of $\kappa = .43$.

Validity of predictions

As explained in the Methods section, we used a bias-corrected R^2 as our core measure of predictive validity. This validity measure was first computed separately for each individual combination of predictor type (3), predictor source (4), and criterion variable (30/31). In the following, we focus on results averaged across criterion variables, as we are particularly interested in differences between predictor types and sources that would hold across the two validation measures. Note that most of these analyses should be considered exploratory, as we had no particular hypotheses regarding many of the respective differences between predictor types and between predictor sources. Therefore, in our discussion of the individual effects in the next few sections we focus on relative effect sizes rather than p-values (see above). Figure 1 illustrates the overall pattern of results regarding the findings reported next. Overall, we found clear evidence for validity across predictor types and sources, but with small to moderate effect sizes. This is probably attributable to the relatively high methodological rigor in the present study, in particular the longitudinal prediction of life experience one year later, and probably partially also to differences in the “bandwidth” between predictors and criteria.

Validity of predictions for different predictor types. Results pertaining to the predictive validity for different predictor types *across* predictor sources are displayed in Table 3. Across criterion variables and predictor sources, all three predictor types afforded significant predictions of target-rated and TNI-rated actual life experiences. The greatest validity in this regard was found for direct predictions (AEX_t: *Mdn* $R^2 = .020$, $p < .001$;

AEX_tni: $Mdn R^2 = .024, p < .001$) and nuance-based predictions (AEX_t: $Mdn R^2 = .019, p < .001$; AEX_tni: $Mdn R^2 = .018, p < .001$). Domain-based predictions (AEX_t: $Mdn R^2 = .009, p < .001$; AEX_tni: $Mdn R^2 = .008, p < .001$) had significantly lower predictive validity than the other two types of predictions ($p < .001$).

Validity of predictions for different predictor sources. Results pertaining to the predictive validity for different predictor sources *across* predictor types are displayed in Table 4. Across criterion variables and predictor types, all four predictor sources made valid predictions regarding TNI-rated actual life experiences, whereas only TNI-rated and target-rated predictors made valid predictions regarding target-rated actual life experiences.

When predicting target-rated actual life experiences, targets ($Mdn R^2 = .030, p < .001$) and TNI ($Mdn R^2 = .021, p < .001$) provided the most valid ratings, while notably targets ($Mdn R^2 = .028, p < .001$) significantly outperformed ($p < .001$) TNI ($Mdn R^2 = .025, p < .001$) when predicting TNI-rated actual life experiences. Both of these findings, but particularly the latter, suggest that there were no substantive shared method effects. Predictions by high-liking group informants (AEX_t: $Mdn R^2 = .002, p = .021$; AEX_tni: $Mdn R^2 = .008, p < .001$) and low-liking group informants (AEX_t: $Mdn R^2 = .003, p = .010$; AEX_tni: $Mdn R^2 = .007, p < .001$) were only significant for TNI-rated actual experiences, and overall significantly less valid than predictions by targets and TNI ($p < .001$). They did not significantly differ from each other in that regard.

Predictive validity for different predictor sources, given predictor type. Results pertaining to the predictive validity for different predictor sources given a specific predictor type are displayed in Table 5. Overall, direct predictions were valid for all predictor sources, but targets vastly outperformed other sources of information. TNI also had substantial (moderate) predictive validity, while GI-rated direct predictions had only small predictive validity. The pattern of findings was similar for personality-based predictions: however,

targets and TNI made about equally valid predictions, and clearly outperformed GI-rated personality-based predictions.

More specifically, across validation measures, there was significant validity for *direct predictions* of actual life experiences by all four sources. For both target- and TNI-rated actual life experiences, targets provided the best direct predictions (AEX_t: $Mdn R^2 = .087, p < .001$; AEX_tni: $Mdn R^2 = .079, p < .001$), followed by TNI (AEX_t: $Mdn R^2 = .038, p < .001$; AEX_tni: $Mdn R^2 = .035, p < .001$), followed by GI+ (AEX_t: $Mdn R^2 = .007, p < .001$; AEX_tni: $Mdn R^2 = .011, p < .001$) and by GI- (AEX_t: $Mdn R^2 = .004, p < .001$; AEX_tni: $Mdn R^2 = .004, p < .01$). All differences between these sources were significant ($p < .01$ or $p < .001$).

Personality nuances rated by the targets had no validity on the $p < .01$ level (AEX_t: $Mdn R^2 = .025, p < .05$; AEX_tni: $Mdn R^2 = .024, p = .08$), but the effects were of similar size as and did not significantly differ from TNI-rated nuances, which significantly predicted actual life experiences across the two validation measures (AEX_t: $Mdn R^2 = .027, p < .001$; AEX_tni: $Mdn R^2 = .021, p < .001$). To a significantly lower degree ($p < .01$), GI+ ratings ($Mdn R^2 = .009, p < .01$) and GI- ratings ($Mdn R^2 = .012, p < .01$) of personality nuances had significant validity when predicting target-rated actual life experiences and did not significantly differ from each other in that regard. When predicting TNI-rated actual experiences, GI+ rated nuances ($Mdn R^2 = .009, p = .043$) and GI- rated nuances ($Mdn R^2 = .011, p = .010$) did not have any significant predictive validity on the $p < .01$.

Across the two validation measures, *personality domains* rated by targets (AEX_t: $Mdn R^2 = .017, p < .001$; AEX_tni: $Mdn R^2 = .014, p = .010$) and by TNI (AEX_t: $Mdn R^2 = .017, p < .001$; $Mdn R^2 = .012, p < .001$) were the most valid predictions. Differences in validity between target- and TNI-ratings of personality domains were not significant. In

addition, TNI-rated actual life experiences ($Mdn R^2 = .007, p < .001$) but not target-rated actual life experiences ($Mdn R^2 = .002, p = .174$) were significantly predicted by GI- rated personality domains. For neither of the two validation measures did GI+ rated personality domains have significant predictive validity (AEX_t: $Mdn R^2 = -.001, p = .797$; AEX_tni: $Mdn R^2 = -.002, p = .854$).

Predictive validity for different predictor types, given predictor source. Results pertaining to the predictive validity for different predictor types given a specific predictor source are displayed in Table 6. Overall, targets' direct predictions had by far the greatest predictive validity, followed by nuance- and domain-based predictions, which were about equally valid. The same pattern was found for predictions by TNI, which also had much stronger predictive validity for direct compared to personality-based predictions, yet the difference between the two was not as big. Predictions by GI overall had very little predictive validity and (almost) no predictive validity for domain-based predictions.

More specifically, when comparing the validity of different types of predictions for *targets*, direct predictions performed by far the best across the two validation measures (AEX_t: $Mdn R^2 = .087, p < .001$; AEX_tni: $Mdn R^2 = .079, p < .001$). Personality-based predictions were also somewhat valid, but to a significantly lower degree ($p < .001$). Nuance-based predictions (AEX_t: $Mdn R^2 = .025, p = .030$; AEX_tni: $Mdn R^2 = .024, p = .076$), were about equally valid as domain-based predictions (AEX_t: $Mdn R^2 = .017, p < .001$; AEX_tni: $Mdn R^2 = .014, p = .010$), but missed the threshold of significance.

Target-nominated informant (TNI) ratings had significant predictive validity across all three types of predictors, and across criterion variables. For both types of validation measures, direct predictions by TNI (AEX_t: $Mdn R^2 = .038, p < .001$; AEX_tni: $Mdn R^2 = .035, p < .001$) had significantly higher validity ($p < .01$ or $p < .001$) than personality-based predictions

both for domains (AEX_t $Mdn R^2 = .017, p < .001$; AEX_tni: $Mdn R^2 = .012, p < .001$) and nuances (AEX_t: $Mdn R^2 = .027, p < .001$; AEX_tni: $Mdn R^2 = .021, p < .001$).

For *high-liking group informants* (GI+) only direct predictions had significant validity across the two validation measures (AEX_t: $Mdn R^2 = .007, p < .001$; AEX_tni: $Mdn R^2 = .011, p < .001$). Nuance-based predictions by the GI+ performed equally well as direct predictions. They were significantly valid with regard to target-rated actual life experiences ($Mdn R^2 = .009, p < .01$) but missed the threshold of $p < .01$ with regard to TNI-rated actual life experiences ($Mdn R^2 = .009, p = .043$). Domain-based predictions by the GI+ did not have any significant predictive validity (AEX_t: $Mdn R^2 = -.001, p = .797$; AEX_tni: $Mdn R^2 = -.002, p = .854$).

Direct predictions by *low-liking group informants* (GI-) had some significant validity regarding both target- ($Mdn R^2 = .004, p < .001$) and TNI-rated ($Mdn R^2 = .004, p < .01$) actual life experiences. Nuance-based predictions by the GI- also had some significant validity (AEX_t: $Mdn R^2 = .012, p < .001$; AEX_tni: $Mdn R^2 = .011, p = .010$), and outperformed ($p < .01$) domain-based predictions with regard to target-rated actual experiences, which were not significant ($Mdn R^2 = .002, p = .174$). In contrast, when using TNI-rated actual life experiences as a validation measure, domain-based predictions by the GI- did show some validity ($Mdn R^2 = .007, p < .001$).

Summary of predictive validity in variable-centered analyses. To summarize, across predictor sources, direct and nuance-based predictions were the most valid types of predictors. Across predictor types, targets and TNI were the most valid sources of information. Targets' direct predictions had *by far* the best predictive validity. These judgments probably benefit from both the privileged access to (inner) information in combination with a perfect match in bandwidth between predictor and criterion. Direct

predictions by TNI had the second highest validity. Again, in these predictions there is a perfect match in bandwidth, but probably the TNI have less information about the targets compared to the targets themselves. Personality-based predictions by targets and TNI were also somewhat valid, but GI predictions had overall very little (to no) predictive validity.

To provide some more details on results regarding individual life experiences, we display exploratory results *per* life experience in Figure 2 (see also Supplemental Table S6 from our OSF-project for the five life experiences with the most valid predictions for each combination of predictor type and predictor source).

Person-Centered Analyses: Differential Associations of Liking and Knowing with Validity and Positivity Bias in Direct Predictions of Life Experiences

In Table 7, we display the results of multilevel profile analyses investigating associations between liking and knowing on the one hand, and validity and positivity bias on the other hand. Person-centered analyses were required in this regard, in order to be able to disentangle the unique effects of the two predictors (liking and knowing), which tend to be highly correlated. Please note that these analyses pertain to direct predictions only, as they were not possible for personality-based predictions due to the data structure.

The analyses were performed twice: once across 47 target-rated actual life experiences (Model 1, left half) and once across 47 TNI-rated actual life experiences (Model 2, right half). Overall, effects were very consistent across the two validation measures. Both of the analyses suggested that knowing and liking did not have an effect on overall levels of predicted life experiences. The latter might have been expected, given that the set of life experiences we studied was fairly balanced in terms of social desirability (see above). We did find a negative association for predictor type (OR_{030}) representing further differences between GI and TNI in

Model 1: The expected odds of reporting that a certain life experiences was expected to occur were 0.82 lower for TNI than for GI.

For both validation measures, we found considerable validity overall, as indicated by large odds ratios. Specifically, the expected odds of reporting that a certain life experience was expected to occur were eight to nine times greater when that life experience actually did occur later on, as compared to when it did not (Model 1: $OR_{100} = 7.93$; Model 2: $OR_{100} = 8.94$). Neither analysis suggested a moderating influence of liking or perceiver type on validity, but in line with our expectations, we did find a significant positive influence of knowing on validity (across validation measures), with the expected odds of making accurate predictions being about 1.1 times greater with every one unit increase in knowing (see Table 7). Also, there was a significant positivity bias across both validation measures (Model 1: $OR_{200} = 1.56$; Model 2: $OR_{200} = 1.21$), implying that life experiences were generally expected to be more likely to occur the more positive they were rated to be. In line with our expectations, liking significantly increased this positivity bias, with the expected odds of making more positive predictions being about 1.1 times greater with every unit of increase in liking (see Table 7). Thus, irrespective of whether or not certain life experiences did occur later on, perceivers expected better things to happen for targets they liked. We found a similar effect for perceiver type in Model 1 ($OR_{230} = 1.16$): TNI expected significantly more positive things to happen to their targets compared to GI, above and beyond their liking towards the targets. Knowing was associated with a slightly lower positivity bias across the two models (Model 1: $OR_{220} = 0.96$; Model 2: $OR_{220} = 0.97$), suggesting that perceivers expected slightly less positive things to happen for targets they knew better.

Discussion

In the present study, we investigated the predictive validity of various kinds of person judgments in regard to a broad range of psychologically meaningful life experiences. In contrast to most prior research in this field (cf., Wessels et al., 2016), we used a relatively rigorous methodology. Specifically, we predicted the targets' future life experiences longitudinally, over an interval of approximately one year, and we had both the predictor and the criterion variables in these predictions assessed by multiple sources: The target persons and the informants they nominated provided data on predictor and criterion variables. In addition, we also obtained predictor ratings from a new type of ("group") informants who were also somewhat familiar with the targets, but over whose recruitment the targets had no control. We did this in order to be able to investigate how validity and bias in person judgments vary across a broader spectrum of knowing and liking. We were also able to compare the predictive validity of three different predictor types varying in bandwidth (direct, personality nuance, and personality domains) with one another, and – using multilevel modelling – to determine the unique associations between perceiver liking and knowing on the one hand, and validity and positivity bias on the other hand. In the following, we will briefly discuss the most noteworthy finding from our research in more detail.

Based on a systematic overview of prior research into possible "consequences of personality", we compiled a relatively comprehensive list of 73 such consequences, concentrating on the ones being relatively "factual" in nature (i.e., assessing them required little inference or interpretation). This list may in itself be seen as one useful result of the present study, as it may inform the selection of criterion variables in other, future studies. As it integrates many of the diverse lists of life experiences that had been used in previous studies (e.g., Lüdtke et al., 2011; Sarason et al., 1978; Vaidya et al., 2002), we think that it may claim a certain level of representativeness. Our analyses of the data led to the insight that potential

problems with extreme base rates (sometimes resulting in convergence problems) need to be anticipated and accounted for in this type of research. In our study, only a subsample (30 - 47) of the life experiences we had assessed could actually be used for statistical analyses.

Variable-Centered Analyses

Our analyses of which types and sources of predictions were most valid yielded the following results: First, direct predictions of future life experiences by the targets themselves had by far the greatest validity (cf. Table 5). This may be explained (1) in terms of the perfect match between predictor and criterion in terms of bandwidth in this case, as well as (2) privileged knowledge that only the targets have about their own life circumstances, resources, plans, obstacles, and so on (e.g., Vazire, 2010). Second, direct predictions by the target-nominated informants were substantially less valid than those by the targets themselves, but still more valid than all other predictions (cf. Table 5). This may also be explained in terms of (1) bandwidth-match, and (2) superior knowledge that the target-nominated informants but not the group informants, had about the targets. In line with previous research (e.g., Connelly & Ones, 2010; Jackson et al., 2015; Luan et al., 2019; Thielmann et al., 2017), we also found evidence for a substantial validity of personality-based predictions by the targets and target-nominated informants. The level of validity for such predictions was fairly similar when comparing domains and nuances (cf. Table 6), and when comparing predictions by targets and TNI (cf. Table 5). Generally speaking, predictions based on information that was provided by the “group informants” often had some – significant validity as well, but this validity was fairly small (cf. Table 6). We conclude that the highly effortful recruitment of this relatively uncommon type of informant did *not* pay off in terms of validity (cf. Table 4). This, however, may be seen as “good news” in terms of how personality data tends to be collected in contemporary personality research: Judgments by targets and target-nominated informants are relatively easy to obtain (Vazire, 2010) and have substantial validity, even when making

actual predictions of future life experience, and even when predicting narrow experiences from broader personality nuances and domains. Among the four different sources of information that we studied here, the two that are most commonly used turned out to be by far the most valid (cf. Table 4).

Notably, we did not find much evidence in favour of shared method effects (Campbell & Fiske, 1959; Podsakoff et al., 2012) that inflate correlations between predictors and criteria. This conclusion may be drawn from a comparison of within-source (e.g., predictor and criterion rated by TNI) and between-source (e.g., predictor rated by target, criterion rated by TNI) predictions (e.g., in Table 6), which yielded very similar results. We assume that this finding is rooted in the relatively “factual” nature of the life experiences that we assessed as criterion variables. Even though these experiences differed from one another in terms of social desirability, and even though we did find both a general positivity bias in predictions and an increase of that bias with liking (see below), it seems still plausible that the participants’ retrospective reports regarding the actual occurrence of these experiences were relatively immune to shared method bias.

Interestingly, *across* perceivers and validation measures, the validity of nuance-based predictions was much stronger than that of domain-based predictions (cf. Table 3). These findings are in line with recent research showing that narrower traits (i.e., facets) sometimes outperform broader traits (e.g., Ashton, Paunonen, & Lee, 2014; McAbee, Oswald, & Connelly, 2014; Paunonen & Ashton, 2013) and, more specifically, nuances may outperform Big Five personality traits in predicting life outcomes to a substantial degree (Möttus, Bates et al., 2019; Seeboth et al., 2018). This can be interpreted as a rather unsurprising instantiation of the Bandwidth-Fidelity-Dilemma (Cronbach & Gleser, 1957). In the context of the present study, overfitting may have played a role as well, even though we attempted to diminish this influence to the extent possible by applying both a theoretical and an empirical bias correction

of the results. Firm conclusions in this regard would require proper cross-validation, however, which was not possible with the current dataset due to the relatively small sample size. Future studies with larger sample sizes could also explore the nature of direct predictions as compared to personality-based predictions further. For example, it would be interesting to examine whether there are differences in the information that is used to form the respective judgments, or whether direct predictions have incremental validity over personality-based predictions, particularly nuance-based ones (or vice versa).

The overall level of validity that was achieved in our study may be regarded as somewhat sobering, given the closer associations between personality judgments and “consequences” that were reported in many previous studies (e.g., Connelly & Ones, 2010; Roberts et al., 2007). In interpreting these findings, three things need to be considered: First, our study was more methodologically rigorous than many other studies in this field (e.g., Möttus, 2016; Wessels et al., 2016), as we used a longitudinal design with a relatively long interval between assessments of predictor and criterion variables. Therefore, in contrast to studies based on cross-sectional designs, associations between these two types of variables in the present study are likely not to have been inflated by shared effects such as the participants’ mood or the current life situation. In a way, our more rigorous research design enabled more realistic, and thus more modest estimations of the relevant effect sizes. Second, measurement error may have played a role as well, as each life experience in our study was measured with one item only and average interrater reliability across actual life experiences was only about .4. While this can be interpreted as considerable average interrater agreement for one-item measures, this rather low reliability (in absolute terms) might have attenuated associations between predictor and criterion variables. Third, whereas we did not find any evidence for *inflation* of correlations due to shared method effects (see above), we cannot rule out the possibility that non-shared method effects had an attenuating effect on correlations.

Specifically, if it is true that our criterion variables (life experiences) did not strongly reflect bias factors such as the perceivers' attitudes toward the targets (Leising, Scherbaum, Locke & Zimmermann, 2015), then correlations between these criterion variables and any predictors that reflect such biases more strongly may be diminished. Research has shown that most person-descriptive terms, from which the items of most personality measures are composed, are highly evaluative and thus do strongly reflect the perceivers' attitude toward the targets (Anderson, 1968; Dumas, Johnson, & Lynch, 2002; Leising, Ostrovski, & Borkenau, 2012).

Person-Centered Analyses

We also conducted person-centered multilevel analyses in which we predicted associations between whole sets of actual and predicted life experiences for each perceiver-target dyad (Level 1) from variables (liking and knowing) that characterize the relationship between perceiver and target (Level 2) for each target (Level 3). This was necessary in order to be able to determine the unique contributions of the two Level 2 predictors, which tend to be strongly correlated (Leising et al., 2010), also in the present study. Due to the data structure, these analyses were only possible for direct predictions of life experiences. Overall, we again found evidence for strong validity of such predictions, with Odds Ratios lying between 8 and 9 (cf. Table 7). In line with previous studies (e.g., Bernieri et al., 1994; Biesanz et al., 2007), we also found that validity increased with knowing. Furthermore, we did find evidence for an overall positivity bias (i.e., regardless of validity, informants regarded life experiences as more likely the more positive they were rated to be) which increased with liking. That is, the more perceivers liked someone, the more they overestimated the extent to which their targets would experience positive things. The latter effect aligns well with previous findings (e.g., Human & Biesanz, 2011; Leising et al., 2015; Leising, Ostrovski, & Zimmermann, 2013; Zimmermann et al., 2018). However, to our knowledge the present study is the first to corroborate these effects with regard to direct predictions of narrowly defined

life experiences. In addition, the additional predictor “perceiver type” covering any further possible differences between TNI and GI showed that TNI also expected more positive things to happen to their targets than GI, independent of what the targets actually experience, and above and beyond liking. While TNI are typically family members or close friends (Leising et al., 2010), group informants in this study were members of the same university courses. Therefore, it is conceivable that these effects might be due to the TNIs’ higher emotional investment (Vazire, 2010), which may not be fully covered by our operationalization of liking.

We did *not* find any evidence for detrimental effects of liking on validity. Again, this may be seen as “good news” regarding the common use of target-nominated informants in personality research. Given that such informants tend to have extremely positive attitudes toward the targets who recruited them (Leising, Erbs & Fritz, 2010), it was conceivable that their ability to accurately predict what is going to happen to these targets in the future is somewhat compromised. According to the present study, this is not the case, while, at the same time, the relatively high extent to which such informants *know* their targets clearly constitutes an advantage in terms of predictive validity.

Limitations and Outlook

Despite the relatively high methodological rigor of the present study, a few limitations need to be acknowledged as well: First, the present study had a relatively small sample size, yielding potential overfitting problems for multiple regression models with several interrelated predictors. While we used adjusted pseudo- R^2 measures as our focal estimate of effect sizes and additionally freed these estimates from their empirically observed bias under the null hypothesis, some remaining over-fitting issues cannot completely be ruled out for our analyses involving personality nuances. Therefore, their results need to be interpreted with caution, and future studies should aim for considerably larger sample sizes which allow for

cross-validation and alternative statistical methods such as penalized regressions (e.g., LASSO).

Second, the present analyses focus on linear relationships between predictors and actual life experiences. While this is a common approach in psychological research, it is conceivable that our predictors may have non-linear and/or interactive effects on life experiences. One reason for our focus on linear relationships was the rather low interrater reliability (i.e., self-other and other-other agreement) of the life experiences examined, as poor reliability prevents the detection of non-linear relationships using (e.g.) machine learning (Jacobucci & Grimm, 2020). Future studies should therefore not only aim for larger sample sizes, but also for additional and more reliable forms of measuring real life experiences (e.g., behavioral observation, more objective measures).

Third, we used an open answer format for some of the life experiences that we assessed, which resulted in some extremely skewed distributions. To control for that, and in order to enable our person-centered multilevel analyses, we decided to dichotomize these variables. Doing so resulted in a loss of information, which is why the estimates we obtained from those analyses should be regarded as somewhat conservative. Future studies may benefit from consistently assessing life experiences with closed-ended questions, to make such post hoc dichotomization unnecessary. To preserve some of the “hard”, factual nature of these assessments, it may be helpful to first gather some representative information about the distributions of the variables using open-ended questions and then apply some reasonable categorization scheme.

Fourth, our strategy for recruiting alternative (“group”) informants was supposed to increase the range of liking and knowing towards the targets, which largely succeeded. However, all of these informants (as well as the targets) were recruited in a university context,

making them rather uniform in terms of age, education level, etc. A more diverse group of participants would yield higher levels of generalizability for the present findings.

Fifth, for the same reason, some of the (more consequential) life experiences (e.g., days spent in prison) yielded extremely low or zero occurrence rates and thus had to be excluded from the analyses. Aiming for a more diverse sample of targets would yield greater variation in this regard, and enable more generalizable conclusions regarding such “grave” experiences as well.

Sixth,

Overall, given the relatively high methodological rigor of the present study the results cast a more realistic – maybe somewhat sobering – light on the rather modest predictive power of personality judgments for a broad range of relatively specific meaningful life experiences. At the same time, the present study showed that people do indeed know – to certain extents – what the next year will hold for them when asked to make *direct* predictions: the target persons themselves made by far the best direct predictions, vastly outperforming all other kinds of predictions, followed by their close acquaintances. Hence, instead of relying on inferences about broader or (rather) narrower personality traits, sometimes it might be much more informative to ask perceivers directly when we want to predict a target persons’ future behavior, experiences, and life course. Interestingly, while perceivers do expect more positive experiences to occur for their targets when they like these targets more, our findings suggest that this does not affect the validity of these perceiver’s predictions. Taken together, across different types of judgments, the targets themselves, as well as their close acquaintances seem to be the best judges of the targets’ future life experiences. This may be interpreted as good news for personality research, as it corroborates the common practice of examining person judgements provided by targets and their close acquaintances.

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Figures

Figure 1
The Predictive Validity of the Four Different Sources and the Three Predictor Types

Note. Median bias-corrected R^2 values for the four different sources and the three predictor types across 31 TNI-rated actual life experiences (left half) and 30 target-rated actual life experiences (right half).

Figure 2*Predictive Validity for the Different Predictor Sources and Types per Life Experience*

Note. Bias-corrected R^2 for 732 (multiple) logistic regression models. Results are stratified according to 33 actual life experiences (y-axis) rated by the target-nominated informants (“TNI”) (left) or targets (right), the three predictor types (colors) and the four different sources (shape). The box and whiskers plots summarize the individual results per predictor across sources. Two extreme R^2 values ($> .40$) were rescaled as .405 in order to improve the figure’s interpretability. Life experiences are grouped along three broad life domains suggested by Ozer and Benet-Martínez (2006) (displayed on the right): individual, interpersonal, social-institutional.

Tables

Table 1*Means and Standard Deviations for Liking and Knowing in different informant groups*

Group	<i>N</i>	Variable	<i>Min</i>	<i>Max</i>	<i>M</i>	95% CI		<i>SD</i>
						<i>LL</i>	<i>UL</i>	
GI-	534	Knowing	1.00	4.75	2.39	2.32	2.46	0.84
		Liking	1.00	3.50	2.72	2.67	2.78	0.62
GI+	409	Knowing	1.00	5.00	3.02	2.92	3.11	0.97
		Liking	3.75	5.00	4.22	4.19	4.26	0.39
TNI	409	Knowing	1.75	5.00	4.53	4.47	4.57	0.51
		Liking	1.75	5.00	4.63	4.59	4.67	0.45

Note. GI- = group informants with liking values ≤ 3.5 ; GI+ = group informants with liking values > 3.5 ; TNI = target-nominated informants; *CI* = confidence interval; *LL* = lower limit; *UL* = upper limit. TNI values are also reported in Wessels et al. (2020) Table 3, p. 159.

Table 2
Means of Dichotomous Predicted and Actual Life Experiences, Biserial Correlations between Original and Dichotomized Variables and Agreements between Perceivers on Actual Life Experiences

			PEX				AEX_t		AEX_tni		AEX		
	no	Life Experience	T	GI-	GI+	TNI	biserial		biserial		biserial		
			<i>M</i>	<i>M</i>	<i>M</i>	<i>M</i>	<i>r</i>	<i>M</i>	<i>r</i>	<i>M</i>	<i>r</i>	s.o	o.o
individual	1	signed sick*	0.349	0.788	0.804	0.641	0.780	0.384	0.580	0.408	0.562	.396	.229
	2	visited doctor* ^{1a}	0.942	0.978	0.961	0.939	0.494	0.949	0.452	0.952	0.530	.148	-.032
	3	hospital stay*	0.042	0.191	0.115	0.066	1.000	0.089	1.000	0.111	1.000	.499	.642
	4	accident*	0.095	0.339	0.333	0.232	1.000	0.203	0.960	0.234	1.000	.516	.475
	5	psychological disease ^{1a}	0.079	0.054	0.020	0.054		0.040		0.026		.431	.368
	6	psychotherapy* ^{1a}	0.074	0.056	0.029	0.064	1.000	0.031	1.000	0.059	1.000	.382	.482
	7	smoking*	0.095	0.191	0.142	0.139	1.000	0.147	1.000	0.131	1.000	.737	.651
	8	drug use*	0.132	0.176	0.169	0.152	1.000	0.145	0.675	0.118	0.781	.267	.118
	9	weekly alcohol consumption*	0.794	0.886	0.905	0.778	0.922	0.740	0.854	0.803	0.883	.450	.400
	10	amount of alcohol per consumption*	0.852	0.927	0.939	0.826	0.781	0.828	0.726	0.839	0.798	.569	.453
	11	moved	0.370	0.240	0.237	0.235		0.316		0.316		.622	.721
	12	changed type of accommodation	0.228	0.281	0.318	0.301		0.243		0.214		.612	.571
	13	debts* ^a	0.042	0.084	0.078	0.051	0.873	0.099	1.000	0.074	1.000	.000	.000
	14	lost something important*	0.323	0.601	0.619	0.491	0.840	0.170	1.000	0.187	1.000	.306	.167
	15	spent time abroad*	0.857	0.867	0.897	0.845	0.478	0.727	0.504	0.756	0.459	.711	.497
interpersonal	16	weekly social activity* ^{1a}	1.000	0.985	0.998	0.990	0.858	0.943	0.787	0.986	0.839	-.011	.000
	17	ended friendship*	0.291	0.713	0.626	0.457	0.699	0.333	1.000	0.323	1.000	.313	.400
	18	new friendship* ^{3a}	0.952	0.961	0.985	0.975	0.517	0.879	0.553	0.935	0.650	.143	.000
	19	stability in relationship status	0.709	0.680	0.677	0.694		0.769		0.781		.767	.616
	20	cheated on partner*	0.164	0.219	0.141	0.091	1.000	0.131	1.000	0.201	1.000	.256	-.077
	21	was pregnant/caused pregnancy* ^{1a}	0.021	0.047	0.037	0.060	1.000	0.017	1.000	0.012	1.000	.898	1.000
	22	new relationship of at least 1 month*	0.333	0.403	0.423	0.297	1.000	0.251	1.000	0.244	1.000	.781	.653

<i>social/institutional</i>	23	sexual intercourse*	0.778	0.831	0.878	0.868	0.645	0.793	0.663	0.839	0.746	.496	.346
	24	stability in job	0.667	0.557	0.550	0.580		0.562		0.632		.350	.318
	25	continuing the same studies	0.866	0.897	0.929	0.932		0.907		0.920		.806	.718
	26	finished studies ¹	0.274	0.468	0.472	0.479		0.061		0.081		.572	.453
	27	finished studies and started work ^{1a}	0.118	0.657	0.687	0.709		0.032		0.018		.424	.371
	28	finished studies and started postgraduate studies ^{1a}	0.102	0.272	0.296	0.374		0.043		0.035		.515	.554
	29	finished studies and took a break ^{1a}	0.113	0.320	0.364	0.347		0.034		0.013		.483	.549
	30	got award ^{1a}	0.027	0.079	0.090	0.183		0.069		0.027		.314	.487
	31	changed subject of study ^{1a}	0.043	0.045	0.032	0.042		0.023		0.022		.792	.652
	32	changed university ^{1a}	0.059	0.051	0.054	0.066		0.036		0.025		.586	.387
	33	stopped with job ²	0.080	0.170	0.109	0.070		0.250		0.236		.435	.382
	34	changed job ²	0.379	0.372	0.347	0.328		0.285		0.247		.476	.272
	35	job for at least 1 month	0.677	0.554	0.671	0.625		0.491		0.425		.494	.333
	36	failed exam*	0.296	0.346	0.301	0.267	0.979	0.205	1.000	0.205	1.000	.681	.628
	37	passed exam* ^{1a}	0.978	0.977	0.976	0.982	0.692	0.992	0.739	0.967	0.764	.646	.500
	38	canceled exam*	0.199	0.313	0.301	0.194	1.000	0.310	1.000	0.172	1.000	.382	.336
	39	community activity for at least 1 month	0.317	0.361	0.443	0.333		0.204		0.209		.493	.582
	40	doing sports	0.914	0.699	0.770	0.804		0.691		0.716		.432	.414
	41	doing musical activity	0.579	0.489	0.457	0.489		0.432		0.381		.424	.599
	42	doing arts	0.275	0.228	0.264	0.208		0.203		0.169		.382	.426
	43	doing other activities	0.814	0.796	0.851	0.822		0.699		0.851		.157	-.138
	44	fare dodging*	0.259	0.258	0.215	0.235	0.917	0.309	0.780	0.126	1.000	.177	.638
	45	speeding ticket*	0.275	0.348	0.311	0.306	0.985	0.156	1.000	0.153	1.000	.419	.629
	46	drunk cycling* ^a	0.169	0.170	0.147	0.123	0.922	0.141	0.946	0.052	1.000	.038	.000
	47	drunk driving* ^{1a}	0.011	0.054	0.049	0.025	1.000	0.024	1.000	0.008	1.000	-.005	.000

Note. The table shows the means of direct predictions of life experiences (PEX) per rater group (no = life experience number; T = Targets, GI- = group informants with liking values ≤ 3.5 ; GI+ = group informants with liking values > 3.5 ; TNI = target-nominated informants) and means of target-rated (AEX_t) and TNI-rated (AEX_tni) actual life experiences per item. Life experiences were classified along three broad life domains suggested by Ozer &

Benet-Martínez (2006). to Self-other agreement (s.o) and other-other agreement (o.o) on ratings of actual life experience were computed based on averaged Cohen's Kappas.

* Items were dichotomized in order to make results comparable with one another and to allow for multilevel profile analyses. For these life experiences, we report biserial correlations between the original variables and the respective dichotomous variables for PEX, AEX_t and AEX_tni.

¹ AEX_t excluded from variable-centered predictions due extreme baserates (< 7.5% and > 92.5%) creating estimation problems; ² AEX_t excluded variable-centered predictions due to > 60% missing values; ³ AEX_t excluded from variable-centered predictions due to additional estimation problems.

^a AEX_tni excluded from variable-centered predictions due extreme baserates (< 7.5% and > 92.5%) creating estimation problems. No AEX_tni had to be excluded due to the other two exclusion criteria (see ² and ³).

Table 3
Validity of Predictions for Different Predictor Types

predictor types	Median bias-corrected R^2				R^2 Differences in predictor types							
					direct				domain-based			
	AEX_t		AEX_tni		AEX_t		AEX_tni		AEX_t		AEX_tni	
	R^2	p	R^2	p	ΔR^2	p	ΔR^2	p	ΔR^2	p	ΔR^2	p
direct	.020	.000	.024	.000								
domain-based	.009	.000	.008	.000	.011	.000	.016	.000				
nuance-based	.019	.000	.018	.000	.001	.798	.006	.040	-.010	.000	-.010	.000

Note. The table displays median bias-corrected R^2 and respective p -values per predictor type *across* the different predictor sources and 30 target-rated (AEX_t) and 31 TNI-rated (AEX_tni) life experiences, respectively, as well as differences in median R^2 (ΔR^2) between predictor types. P -Values are based on permutation tests.

Table 4
Validity of Predictions for Different Predictor Sources

predictor source	Median bias-corrected R^2		R^2 Differences in predictor sources											
			GI-				GI+				TNI			
	AEX_t	AEX_tni	AEX_t	AEX_tni	AEX_t	AEX_tni	AEX_t	AEX_tni	AEX_t	AEX_tni	AEX_t	AEX_tni	AEX_t	AEX_tni
	R^2	p	R^2	p	ΔR^2	p	ΔR^2	p	ΔR^2	p	ΔR^2	p	ΔR^2	p
GI-	.003	.010	.007	.000										
GI+	.002	.021	.008	.000	.001	.786	-.001	.736						
TNI	.025	.000	.021	.000	-.023	.000	-.014	.000	-.023	.000	-.014	.000		
Targets	.028	.000	.030	.000	-.026	.000	-.023	.000	-.026	.000	-.022	.000	-.003	.178
													-.009	.000

Note. The table displays median bias-corrected R^2 and respective p -values per predictor source *across* the different predictor types and 30 target-rated (AEX_t) and 31 TNI-rated (AEX_tni) life experiences, respectively, as well as differences in median R^2 (ΔR^2) between predictor sources. P -Values are based on permutation tests. GI- = group informants with liking values ≤ 3.5 ; GI+ = group informants with liking values > 3.5 ; TNI = target-nominated informants.

Table 5*Predictive Validity for Different Predictor Sources, given Predictor Type*

predictor type	predictor source	Median bias-corrected R^2				R^2 Differences in predictor sources within types											
		AEX_t		AEX_tni		GI-				GI+				TNI			
		R^2	p	R^2	p	ΔR^2	p	ΔR^2	p	ΔR^2	p	ΔR^2	p	ΔR^2	p	ΔR^2	p
direct	GI-	.004	.000	.004	.003												
	GI+	.007	.000	.011	.000	-.004	.008	-.007	.000								
	TNI	.038	.000	.035	.000	-.034	.000	-.032	.000	-.031	.000	-.025	.000				
	Targets	.087	.000	.079	.000	-.083	.000	-.075	.000	-.080	.000	-.068	.000	-.049	.000	-.043	.000
domain-based	GI-	.002	.174	.007	.000												
	GI+	-.001	.797	-.002	.854	.003	.178	.009	.000								
	TNI	.017	.000	.012	.000	-.015	.000	-.005	.066	-.018	.000	-.014	.000				
	Targets	.017	.000	.014	.010	-.015	.002	-.007	.210	-.018	.000	-.016	.008	.000	.948	-.002	.678
nuance-based	GI-	.012	.000	.011	.010												
	GI+	.009	.005	.009	.043	.003	.652	.003	.718								
	TNI	.027	.000	.021	.000	-.015	.004	-.010	.126	-.018	.002	-.013	.074				
	Targets	.025	.030	.024	.076	-.014	.320	-.013	.424	-.016	.240	-.015	.380	.002	.912	-.003	.886

Note. The table displays median bias-corrected R^2 and respective p -values per predictor source *given* a specific predictor type and *across* 30 target-rated (AEX_t) and 31 TNI-rated (AEX_tni) life experiences, respectively, as well as differences in median R^2 (ΔR^2) between predictor sources. P -Values are based on permutation tests. GI- = group informants with liking values ≤ 3.5 ; GI+ = group informants with liking values > 3.5 ; TNI = target-nominated informants.

Table 6
Predictive Validity for Different Predictor Types, given Predictor Source

predictor source	predictor type	Median bias-corrected R^2				R^2 Differences in predictor types within sources							
						direct				domain-based			
		AEX_t	AEX_tni	AEX_t	AEX_tni	AEX_t	AEX_tni	AEX_t	AEX_tni	AEX_t	AEX_tni	AEX_t	AEX_tni
		R^2	p	R^2	p	ΔR^2	p	ΔR^2	p	ΔR^2	p	ΔR^2	p
GI-	direct	.004	.000	.004	.003								
	domain-based	.002	.174	.007	.000	.002	.142	-.003	.090				
	nuance-based	.012	.000	.011	.010	-.008	.020	-.007	.124	-.010	.008	-.004	.398
GI+	direct	.007	.000	.011	.000								
	domain-based	-.001	.797	-.002	.854	.009	.000	.013	.000				
	nuance-based	.009	.005	.009	.043	-.002	.582	.002	.626	-.011	.000	-.010	.036
TNI	direct	.038	.000	.035	.000								
	domain-based	.017	.000	.012	.000	.021	.000	.024	.000				
	nuance-based	.027	.000	.021	.000	.011	.004	.014	.000	-.010	.014	-.010	.044
Target	direct	.087	.000	.079	.000								
	domain-based	.017	.000	.014	.010	.070	.000	.065	.000				
	nuance-based	.025	.030	.024	.076	.062	.000	.055	.000	-.009	.496	-.010	.486

Note. The table displays median bias-corrected R^2 and respective p -values per predictor type *given* a specific predictor source and *across* 30 target-rated (AEX_t) and 31 TNI-rated (AEX_tni) life experiences, respectively, as well as differences in median R^2 (ΔR^2) between predictor types. P -Values are based on permutation tests. GI- = group informants with liking values ≤ 3.5 ; GI+ = group informants with liking values > 3.5 ; TNI = target-nominated informants.

Table 7*Effects of Liking and Knowing on the Validity and Positivity Bias of Direct Predictions of Actual Life Experiences*

		Model 1				Model 2			
		Direct predictions of target-rated actual life experiences				Direct predictions of TNI-rated actual life experiences			
<i>Fixed Effects</i>		<i>Estimate (SE)</i>	<i>95% CI</i>		<i>OR</i>	<i>Estimate (SE)</i>	<i>95% CI</i>		<i>OR</i>
			<i>LL</i>	<i>UL</i>			<i>LL</i>	<i>UL</i>	
Intercept	γ_{000}	-0.94 (0.03)	-0.99	-0.89	0.39	-1.07 (0.03)	-1.13	-1.01	0.34
AEX	γ_{100}	2.07 (0.05)	1.98	2.17	7.93	2.19 (0.05)	2.10	2.28	8.94
Des	γ_{200}	0.44 (0.02)	0.40	0.49	1.56	0.19 (0.01)	0.17	0.21	1.21
liking	γ_{010}	0.00 (0.02)	-0.04	0.04	1.00	-0.00 (0.02)	-0.05	0.05	1.00
knowing	γ_{020}	-0.03 (0.02)	-0.07	0.00	0.97	-0.01 (0.02)	-0.06	0.03	0.99
type	γ_{030}	-0.20 (0.05)	-0.30	-0.10	0.82				
AEX*liking	γ_{110}	-0.00 (0.03)	-0.07	0.06	0.99	0.02 (0.04)	-0.06	0.09	1.02
AEX*knowing	γ_{120}	0.10 (0.03)	0.04	0.17	1.11	0.10 (0.04)	0.03	0.18	1.11
AEX*type	γ_{130}	0.09 (0.07)	-0.05	0.23	1.09				
Des *liking	γ_{210}	0.10 (0.02)	0.07	0.13	1.11	0.05 (0.01)	0.03	0.06	1.05
Des*knowing	γ_{220}	-0.04 (0.02)	-0.07	-0.01	0.96	-0.02 (0.01)	-0.04	-0.01	0.97
Des*type	γ_{230}	0.15 (0.04)	0.08	0.22	1.16				
Random Effects									
<i>Perceiver variability</i>									
Intercept	$SD(r_{0pt})$	0.25 (0.02)	0.21	0.28		0.23 (0.02)	0.02	0.19	
AEX	$SD(r_{1pt})$	0.04 (0.03)	0.00	0.10		0.05 (0.04)	0.04	0.00	
Des	$SD(r_{2pt})$	0.18 (0.01)	0.15	0.21		0.08 (0.01)	0.01	0.06	
<i>Target variability</i>									
Intercept	$SD(u_{00t})$	0.22 (0.02)	0.18	0.27		-0.23 (0.47)	0.47	-0.93	
AEX	$SD(u_{10t})$	0.44 (0.04)	0.38	0.52		-0.95 (0.04)	0.04	-1.00	
Des	$SD(u_{20t})$	0.21 (0.02)	0.17	0.24		0.17 (0.46)	0.46	-0.76	

Note. Estimates are based on a multilevel logistic regression including all 47 experiences. Estimates in Model 1 are based on 155 targets and 1166 perceivers (including both TNI and GI). Estimates in Model 2 are based on 161 targets and 807 perceivers (only GI). Samples were drawn using sampling (NUTS). AEX = actual life experiences as reported by targets (Model 1) and target-nominated informants (Model 2); Des = social desirability; type = perceiver type (GI = 0, TNI = 1); OR = odds ratio; CI = confidence interval; LL = lower limit; UL = upper limit. Bold-face indicates CI does not contain 0. Correlations between random effects were freely estimated but omitted in this table.