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Trait-Specificity Versus Global Positivity: A Critical Test of Alternative Sources of Assumed Similarity in Personality Judgments

Isabel Thielmann[1], Richard Rau[2], and Kenneth D. Locke[3]
[1] Department of Criminology, Max Planck Institute for the Study of Crime, Security and Law
[2] Department of Psychology, University of Münster
[3] Department of Psychology, University of Idaho

For decades, a recurring question in person perception research has been whether people's perceptions of others' personality traits are related to how they see themselves on these traits. Indeed, evidence for such "assumed similarity" effects has been found repeatedly, at least for certain characteristics. However, recent research suggests that these findings may be an artifact of individual differences in how positively or negatively perceivers see others in general, irrespective of trait-specific content. Overcoming the limitations of prior studies, the present work provides a critical test of trait-specificity versus global positivity as sources of assumed similarity in personality judgments. In two large studies (*N*s = 2,287 and 3,563) with preregistered hypotheses and analyses, perceivers rated 10 targets (strangers) each on the honesty–humility, emotionality, extraversion, agreeableness, conscientiousness, and openness to experience; HEXACO (Study 1) and Big Five (Study 2) dimensions to capture their perceptions of the "average other" (i.e., perceiver effects). We then computed "positivity-corrected" assumed similarity effects using trait-based and profile-based approaches. Although controlling for global positivity considerably reduced the strength of assumed similarity, perceiver effects were still positively related to self-reports. As predicted, these assumed similarity effects occurred foremostly for traits strongly linked to values. Specifically, in Study 1, positivity-corrected assumed similarity was observed only for honesty–humility and openness to experience, albeit meaningful effects merely occurred on one of the two self-report measures. In Study 2, traits' value-relatedness remained a unique moderator of assumed similarity after accounting for traits' positivity (i.e., social desirability). These findings demonstrate that assumed similarity is indeed, to some extent, trait-specific.

Keywords: assumed similarity, social projection, perceiver effects, social desirability, values

In our everyday social interactions, we are regularly required to judge what others, including strangers, are like. Such judgments about others influence whether we trust someone and cooperate with them (Balliet & Van Lange, 2013; Thielmann & Hilbig, 2015), whether we select someone as a friend or mating partner (Laner & Russell, 1998; Lippa, 2007), or whether we consider someone suitable for a job (Mathison, 1988). Historically, one of the oldest observations in research on individual differences in person perception is that individuals' perceptions of others show some overlap with how they see themselves—a phenomenon known as *assumed similarity* (Cronbach, 1955; see also Kenny & West, 2010; Robbins & Krueger, 2005). To illustrate, imagine two personnel selectors disagreeing about a job applicant's level of creativity, or two psychotherapists disagreeing about a patient's level of modesty. Could this disagreement be due to differences in the judges' *own* levels of creativity and modesty, respectively? Here, we present two large-scale studies to critically test whether (a) people actually tend to see (unknown) others as similar to themselves in terms of trait-specific content, that is, after accounting for individual differences in the overall positivity of their judgments and (b) whether this tendency to genuinely assume similarity with strangers occurs foremostly for traits that are strongly linked to values.

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Isabel Thielmann https://orcid.org/0000-0002-9071-5709
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We embrace the values of transparency and openness in science (Schönbrodt et al., 2015). Therefore, we followed the 21-word solution provided by Simmons et al. (2012).

Correspondence concerning this article should be addressed to Isabel Thielmann, Department of Criminology, Max Planck Institute for the Study of Crime, Security and Law, Günterstalstraße 73, 79100 Freiburg im Breisgau, Germany. Email: i.thielmann@csl.mpg.de

Assumed Similarity

Assumed similarity denotes a positive association between an individual's (i.e., *perceiver*'s) self-perception of a given characteristic and their perception of another person (i.e., *target*) with regard to that same characteristic. For example, assumed similarity may be evident if Paula sees herself as high on modesty and, at the same time, sees Tina as high on modesty. Obviously, however, if Tina is actually just as modest as Paula, *actual* similarity rather than *assumed* similarity would more adequately describe the observed association. As such, the concept of assumed similarity implies that one's own self-view is positively linked to the judgment of someone else's characteristics above and beyond actual similarity (Human & Biesanz, 2011; Thielmann & Hilbig, 2022).

Assumed similarity has been studied for decades by both personality and social psychologists, with a variety of methods and under a variety of labels.[1] Today, cumulative evidence suggests that assumed similarity is a reliable phenomenon (see, e.g., Kenny & West, 2010; Krueger, 2007; Robbins & Krueger, 2005; Thielmann, Hilbig, et al., 2020, for overviews). This begs the question *why* people may see others as similar to the self. Research suggests that seeing others as sharing one's own characteristics is functional to the perceiver. For example, studies have shown that the tendency to assume similarity with others is positively related to self-esteem (Human & Biesanz, 2011; Locke et al., 2012), and that perceiving a future interaction partner as similar to oneself increases perceivers' confidence that they will be liked by the interaction partner (Hampton et al., 2019). Conversely, perceivers experiencing chronic negative affect tend to show lower assumed similarity on positive characteristics (Lane & Gibbons, 2007; Moss et al., 2007), but higher assumed similarity on negative characteristics (i.e., negative emotions; Papp et al., 2010). Moreover, assumed similarity may satisfy the motive of felt belongingness: Perceivers with a higher communal orientation show stronger assumed similarity, particularly when judging liked others (Locke et al., 2012, 2021; Locke & Christensen, 2007). In general, assumed similarity is pronounced when perceivers rate others they like and/or feel attracted to (e.g., Collisson & Howell, 2014; Davis, 2017; Human & Biesanz, 2011; Locke et al., 2012; Weller & Watson, 2009), and it increases with greater relationship closeness and quality (Huelsnitz et al., 2020; Kenny, 1994; Lee et al., 2009; Selfhout et al., 2009; but see Kenny & West, 2010).

Overall, these findings suggest that perceivers particularly emphasize their commonalities with valued others, which may serve the function to maintain cognitive balance (Heider, 1958) or to self-enhance (Marks & Miller, 1987). This supports the notion that assumed similarity is a motivated bias that manifests in other-perceptions being distorted toward the perceivers' own trait levels. However, most existing studies have not explicitly tested whether assumed similarity is indeed attributable to trait-specific content that perceivers see as being shared by others (vs. due to differences in perceivers' overall positivity in other-perceptions), and/or studies have insufficiently accounted for potential influences of actual similarity between perceiver and target. In the present research, we address both these issues. To illustrate the role of these potential confounding influences on assumed similarity, we next turn to different operationalizations of assumed similarity that have been used in prior work.

Operationalizing Assumed Similarity

In the example of Paula judging Tina's modesty, it is not possible to tell whether or not the association between ratings is actually due to assumed similarity (or actual similarity for that matter) because there is insufficient information about how other perceivers see Tina on modesty or how Paula sees Tina on traits other than modesty. Once this information is available, there are two approaches to index assumed similarity, a *trait-based approach* and a *profile-based approach* (Back & Nestler, 2016). A trait-based approach would use the information of additional perceivers by correlating every perceiver's self-report, for instance, on modesty, with their perception of the target's modesty. Here, a positive correlation would indicate that perceivers who see themselves as more modest also tend to see the target as more modest. A profile-based approach would, in turn, use a perceiver's judgments on additional traits, for instance, by correlating Paula's self-perceptions on modesty, orderliness, and outgoingness with her perceptions of Tina's modesty, orderliness, and outgoingness. Here, a positive correlation would suggest that Paula sees Tina as higher on those traits on which she also sees herself as particularly high. Of note, researchers who pursue a trait-based approach usually study assumed similarity for not just one but for several traits. Likewise, researchers who pursue a profile-based approach usually study not just one but many perceivers.[2]

Although both the trait-based and the profile-based approach provided evidence for assumed similarity in the past, distinct strengths and weaknesses of the two approaches have become apparent. Specifically, one strength of the trait-based approach is that effects of actual similarity can easily be ruled out by design, for example, by having all perceivers judge *the same* target. In this case, variation in the perceivers' judgments of the target cannot be due to the target's actual trait level, given that it is constant. In a profile-based approach, however, actual similarity may conflate assumed similarity effects even when a constant target is used. This is because similarity refers to the configuration of multiple trait levels within a given perceiver–target dyad. To nonetheless account for actual similarity, profile-based analyses can include the target's self-reported personality profile as a predictor (e.g., Huelsnitz et al., 2020; Hughes et al., 2021; Human & Biesanz, 2012; Locke et al., 2014). However, this opportunity has often been missed in previous work (Paunonen & Hong, 2013), and even when done, the results are limited to the degree that targets' self-reports are reliable and valid (Westfall & Yarkoni, 2016).

In turn, a strength of the profile-based approach is that global judgment tendencies unrelated to trait-content—such as

[1] Alternative labels include *social projection* (Krueger, 2007), *attributive projection* (Holmes, 1968), *false consensus effect* (Ross et al., 1977), *self-based heuristic* (Ready et al., 2000), and *self-anchoring* (Cadinu & Rothbart, 1996).

[2] Traditionally, researchers using a profile-based approach have mostly been interested in the general occurrence of assumed similarity across traits (e.g., Human & Biesanz, 2011; Selfhout et al., 2009), whereas researchers using a trait-based approach have often focused on a trait-by-trait interpretation of findings (Thielmann, Hilbig, et al., 2020). However, averaging assumed similarity coefficients from a trait-based approach yields equivalent results as a profile-based approach, and using dummy variables to indicate trait-relatedness in a profile-based analysis can precisely reproduce the trait-based assumed similarity estimates. As such, the two approaches are complementary rather than reflecting competing analytic strategies.

acquiescence (i.e., perceivers preferring positive response options on a scale) and positivity (i.e., perceivers differing in benevolence when judging others)—can be readily accounted for. Specifically, acquiescence is automatically controlled for once centering judgments within perceivers, whereas positivity can be controlled for by including information on the judged characteristics' desirability as a predictor. In contrast, in a trait-based approach—where assumed similarity effects are considered for each trait separately—there is no straightforward way to control for these global judgment tendencies unless using balanced scales containing equal numbers of positively and negatively keyed items (Primi et al., 2019). In the present work, we combine the strengths of the trait-based and the profile-based approaches to provide the most critical test of assumed similarity in personality judgments of strangers to date.

To this point, we have focused on a case where perceivers provide judgments about a single target, such as a random stranger (Thielmann, Hilbig, et al., 2020). In most applied contexts, however, assumed similarity is a relevant phenomenon because it might reflect a tendency of perceivers to assume that others *generally* share their characteristics. In other words, what interests us about modest Paula seeing Tina as modest is the contention that Paula would also see Tom, Ted, and Tory as modest. Thus, one may actually have perceivers rate *multiple* targets and use perceivers' average judgments for a given trait computed across targets, that is, their *perceiver effects* as described in the Social Relations Model (SRM; Kenny, 1994; Kenny & La Voie, 1984). In the present work, we followed this strategy to index assumed similarity and correlated perceivers' self-ratings with their perceiver effects rather than with their perceptions of a specific target. Crucially, this not only serves to boost the generality of perceivers' judgments; it is also an effective means to diminish the influence of actual similarity, simply because it is unlikely that perceivers share a certain characteristic with *all* targets. Of note, it is also possible that some people are more similar to the average target than others, which might produce "spurious" assumed similarity effects when perceivers use normative knowledge to rate targets (Paunonen & Hong, 2013). However, given that the personality profile of the average person is highly desirable (Borkenau & Zaltauskas, 2009; Wood et al., 2007), this type of confound is effectively avoided once including information on trait desirability when modeling assumed similarity.

Trait Differences in Assumed Similarity

Assumed similarity has been demonstrated for a diverse range of characteristics—including, for example, attitudes (Marks & Miller, 1982), goals (Dunlop et al., 2018), motives (Huelsnitz et al., 2020), vocational interests (Holtrop et al., 2018), political preferences (Locke et al., 2012), and affect (Thomas et al., 1997; Watson et al., 2000). However, when applying the trait-based approach to basic personality models, it becomes apparent that only a few trait dimensions show consistent assumed similarity effects. Among the Big Five, these are agreeableness and openness to experience, yielding meta-analytic assumed similarity correlations of $\bar{r} = .25$ and $\bar{r} = .23$, respectively (vs. $.09 \leq \bar{r} \leq .13$ for the remaining dimensions; Thielmann, Hilbig, et al., 2020). Among the honesty–humility, emotionality, extraversion, agreeableness, conscientiousness, and openness to experience (HEXACO) dimensions, in turn, these are honesty–humility[3] and openness to experience, yielding meta-analytic correlations of $\bar{r} = .42$ and $\bar{r} = .29$, respectively (vs. $.01 \leq \bar{r} \leq .12$ for the remaining dimensions; Thielmann, Hilbig, et al., 2020).[4] Notably, however, the effects for openness to experience are more fragile than those for Big Five agreeableness and HEXACO honesty–humility, especially in judgments of strangers (i.e., meta-analytic $\bar{r} = .14$ for HEXACO openness to experience).

So why is assumed similarity apparent for some traits and not for others? One prominent account argues that assumed similarity represents a "lack of information" effect (Kenny & West, 2010; Ready et al., 2000; Watson et al., 2000). According to this account, perceivers rely on their own personality when judging others whenever there is insufficient external information about the target available that they can use to make an informed judgment. This implies that traits characterized by low visibility—that is, traits that are less strongly expressed in overt behavior (Funder & Dobroth, 1987)—should be particularly prone to produce assumed similarity effects. The lack-of-information account thus suggests that assuming similarity primarily serves to "fill in the blanks" rather than, for example, to satisfy one's needs for cognitive balance, self-enhancement, or belongingness, as suggested by a motivational account of assumed similarity. However, contrary to this reasoning, Big Five neuroticism does not reveal consistent evidence for assumed similarity, even though this trait dimension is marked by low visibility (Connelly & Ones, 2010). Moreover, even in the absence of any trait-relevant information about a target, assumed similarity is still only apparent for some trait dimensions (Thielmann, Hilbig, et al., 2020), whereas the lack-of-information account would predict that assumed similarity should occur for all traits under such circumstances.

An alternative explanation for differences in assumed similarity between traits that has received considerable empirical support is that certain traits are more closely linked to values (Lee et al., 2009). Values denote "moral, social, or aesthetic principle(s) accepted by an individual or society as a guide to what is good, desirable, or important" (APA Dictionary of Psychology; VandenBos, 2007). Among the Big Five, agreeableness and openness to experience have been shown to be most strongly linked to values (Fischer & Boer, 2015), whereas in the HEXACO model this holds for honesty–humility and openness to experience (Anglim et al., 2017; Lee et al., 2010). Given that these traits are also the ones yielding the strongest and most consistent assumed similarity effects, it has been proposed that assumed similarity reflects a common desire for one's values to be shared by others (Lee et al., 2009)—and that this is because traits linked to values are particularly important to how individuals conceptualize themselves as persons (Thielmann, Hilbig, et al., 2020). As such, the value account fits well with the above notion that assumed similarity may be motivational in nature.

Trait-Specificity Versus Global Positivity

Contrary to existing theoretical accounts of assumed similarity, recent evidence on the factorial structure of perceiver effects has

[3] Honesty–humility has some conceptual overlap with Big Five agreeableness, but it adds content that is not well accommodated by the Big Five (Ashton et al., 2014; Ashton & Lee, 2020). Openness to experience, in turn, is defined in much the same way across personality models (Thielmann et al., 2021).

[4] To obtain these meta-analytic estimates, we integrated both meta-analytic estimates provided in Thielmann, Hilbig, et al. (2020), that is, those from prior studies and those from their newly conducted studies.

fundamentally challenged the view that assumed similarity is specific to the content of certain traits. Specifically, it has been shown that perceiver effects are, in large part, driven by unspecific judgment tendencies such as acquiescence and, more importantly, individual differences in global positivity in other-perceptions (Heynicke et al., 2021; Rau, Carlson, et al., 2021; Srivastava et al., 2010). Some individuals generally tend to see others in favorable ways across traits, whereas other individuals generally tend to see others in less favorable ways. These individual differences in global positivity are, in turn, related to certain (benevolent) characteristics in perceivers (Rau, Nestler, et al., 2021; Wood et al., 2010). A benevolent judgment style—as, for instance, implied by high levels of Big Five agreeableness—is thus more likely to manifest in higher ratings for socially desirable characteristics in others (e.g., Big Five agreeableness or openness to experience) than for more evaluatively neutral characteristics (e.g., extraversion; John & Robins, 1993). Thus, differences in assumed similarity between traits may largely, or even entirely, result from differences in the perceivers' benevolence rather than from trait-specific differences.

Whether assumed similarity is indeed attributable to seeing others as similar to the self with regard to trait-specific content (e.g., value-relatedness) or, instead, global positivity not only has vital implications for the theoretical understanding of accuracy and bias in person perception, but also for applied settings. For instance, most companies are likely aware that different personnel selectors need to be similarly benevolent in judging job candidates to warrant a fair selection process. But should they also have similar personality profiles? And if so, on which traits would they need to be similar? Currently, these questions are difficult to answer because most existing research has neglected global positivity as an alternative explanation for differences in certain traits' susceptibility to produce assumed similarity effects.

In turn, the two existing studies that did overcome this limitation to a certain degree yielded mixed findings. Wood et al. (2010) asked their participants to rate different types of acquainted others (i.e., friends, residents of the same floor, or members from the same organization) on a variety of adjectives. First, the authors found that these judgments were best accounted for by a single positivity factor. Second, self-reports of Big Five agreeableness were not only positively linked to seeing others as more agreeable—which indicates assumed similarity—but also, for example, to seeing others as more conscientious and open to experience. This suggests that high levels in Big Five agreeableness are related to more positive perceptions of others *in general*. The authors thus concluded that "a single factor concerning how positively others are perceived is sufficient to capture most of the covariation in how individuals tend to see others across a broad range of traits" (Wood et al., 2010, p. 183), although they did not test whether assumed similarity effects would remain after removing the variation in this factor from perceivers' perceptions of others.

A somewhat more optimistic stance on trait-specific assumed similarity above and beyond global positivity is implied by the findings of Srivastava et al. (2010). In their round-robin studies, previously unacquainted individuals were assigned to small groups and asked to rate each other on the Big Five after a getting-acquainted stage in the form of an interactive group task. First, the authors found that perceiver effects reflected not only global positivity in other-perceptions but also trait-specific influences. Second, and even more importantly for the issue at hand, assumed similarity effects remained significant after controlling for global positivity in other-perceptions. Even though these effects emerged on different Big Five dimensions across studies and measurement occasions, the authors generally concluded that "assumed similarity reflects more than just broad evaluative tone" (Srivastava et al., 2010, p. 526).

Apart from the apparent inconsistency in findings, both of these previous projects might have suffered from having perceivers' rate targets they had personally interacted with, which could lead assumed similarity correlations to be inflated by similarity evoked by the perceiver's own behavior during those interactions (i.e., elicited similarity; Hughes et al., 2021). For example, a charming and friendly individual may elicit positive behavior in their interaction partners and thus likely reveal a positive perceiver effect not (only) because of their benevolent judgment style but (also) because interaction partners did, in fact, behave friendlier toward them. Overall, given the scarcity, inconsistency, and methodological limitations of existing research, it essentially remains unanswered whether trait-specific assumed similarity is a reliable phenomenon and whether assumed similarity effects—in case they exist—are compatible with prior theoretical accounts emphasizing the role of the traits' relatedness to values.

The Present Investigation

The present investigation sought to provide a critical test of whether (a) people tend to assume that others are similar to themselves in terms of trait-specific content, that is, after considering the overall positivity of their personality judgments and (b) whether this tendency occurs foremostly for traits that are strongly linked to values. To tackle these questions, we present two large-scale studies, the first pursuing a trait-based approach and the second emphasizing a profile-based approach. Moving beyond common analytic strategies, in both approaches, we controlled for potential influences of global positivity in other-perceptions. To this end, in the trait-based approach in Study 1, we modeled differences in perceivers' benevolence using structural equation modeling. In the profile-based approach in Study 2, we modeled differences in items' characteristics (e.g., value-relatedness) as a moderator of assumed similarity while accounting for differences in items' positivity. Given that the resulting "positivity-corrected" assumed similarity effects measure the component of assumed similarity that is attributable to the traits' specific content, they provide unique information on the trait-specificity of assumed similarity.

Beyond this, the present work extends existing research on assumed similarity in additional ways. Specifically, in both our studies, participants judged the personality of 10 unknown targets, which boosts the generality of the assumed similarity effects and diminishes conflating influences of actual similarity. Further, targets were presented on photos (Study 1) or videos (Study 2) to preclude effects of elicited similarity that personal interactions between perceivers and targets might induce. Finally, we collected personality judgments from two large samples of perceivers that were heterogenous in terms of age, gender, and education and relied on two sets of targets that were heterogenous in terms of age and gender. Thus, the present studies also provide vital information on the generalizability of assumed similarity effects beyond demographically homogenous groups such as college students.

Overall, our investigation provides a strong basis to put trait-specificity versus global positivity to the test as alternative sources of assumed similarity in personality judgments. As such, the present

work can clarify whether self-views and other-perceptions are linked due to assuming similarity of specific—arguably value-related—characteristics or merely due to nonspecific individual differences in a benevolent judgment style.

Study 1

Study 1 presented participants with a standardized set of target stimuli showing screenshots of social media profiles as implemented in the Online-Tool for Assessing Perceiver Effects (O-TAPE; Rau, Nestler, et al., 2021). Unlike in most prior research, perceivers judged the targets on the HEXACO rather than the Big Five dimensions. The HEXACO model includes honesty–humility as a sixth basic trait dimension which is (a) strongly linked to values (Anglim et al., 2017; Lee et al., 2010), even more so than any Big Five dimension, and (b) highly evaluative in nature (De Vries et al., 2016), subsuming tendencies such as being honest, fair, and modest. Prior research suggests that the influence of global positivity on other-perceptions is particularly pronounced for highly evaluative traits (Rau, Carlson, et al., 2021), which suggests that assumed similarity for honesty–humility in particular may vanish once accounting for global positivity. Nonetheless, and in line with existing theoretical accounts proposing that assumed similarity should be pronounced for value-related traits (Lee et al., 2009; Thielmann, Hilbig, et al., 2020), we hypothesized that assumed similarity—particularly for honesty–humility and openness to experience (Thielmann, Hilbig, et al., 2020)—is specific to trait-content and will thus remain even after controlling for differences in global positivity of other-perceptions.

Method

The study was part of a larger project (the Prosocial Personality Project) containing several independent measurement occasions (for details, see https://osf.io/m2abp/). Here, we only used data from two measurement occasions: The first measurement occasion (*T*1) that involved the assessment of personality self-reports via a comprehensive personality inventory and a later measurement occasion (*T*2; termed "follow-up study 2020-05d" in the Prosocial Personality Project) that involved the assessment of other-perceptions along with self-reports on a personality short scale.

Transparency and Openness

All materials (in English and German, the original language of the study), data, and analysis scripts are provided on the Open Science Framework (OSF; https://osf.io/ntwjz/). We report how we determined our sample size, all data exclusions, and all measures used in the study. We did not receive ethical approval for the study, given that this is not required in Germany. However, data were collected in a manner consistent with ethical standards for the treatment of human subjects. Our hypotheses and analysis plan were preregistered prior to the *T*2 data collection (https://aspredicted.org/cw92q.pdf).

Materials

Self-reported personality traits were measured in two ways. First, at *T*1, the German version (Moshagen et al., 2014) of the Self-Report Form of the 60-item HEXACO Personality Inventory-Revised (HEXACO-60; Ashton & Lee, 2009) was administered. The inventory contains 10 items for each of the six HEXACO dimensions. Responses were collected on a 5-point Likert-type scale ranging from 1 = *strongly disagree* to 5 = *strongly agree*. Cronbach's α was satisfactory for all scales (Table 1).

Second, we developed a six-item bipolar adjective scale to capture personality self-reports at *T*2 on items that were also suited to the collection of other-perceptions (see below). Specifically, for each of the six HEXACO dimensions, we created a pair of adjectives that described the essence of the high and low pole of a dimension, using a similar format as common Big Five short scales (e.g., Gosling et al., 2003). Adjectives were selected in consultation with the developers of the HEXACO model to make sure that the dimensions were adequately reflected.[5] The resulting items read as follows: *Greedy*, *dishonest* versus *modest*, *sincere* (honesty–humility), *anxious*, *sensitive* versus *relaxed*, *emotionally stable* (emotionality), *reserved*, *quiet* versus *sociable*, *lively* (extraversion), *critical*, *quick-tempered* versus *sympathetic*, *gentle* (agreeableness), *disorganized*, *careless* versus *reliable*, *self-disciplined* (conscientiousness), and *conventional*, *uncreative* versus *open to new experiences*, *artistic* (openness to experience). Notably, the poles for emotionality were reversed such that the more socially desirable pole, which reflects emotional stability, was aligned with the more socially desirable poles of the other dimensions. Thus, we will refer to *emotional stability* rather than emotionality in what follows. Participants rated the degree to which each item described themselves on an 8-point scale ranging from adjectives characterizing the low pole to adjectives characterizing the high pole of a dimension.

Other-perceptions of the targets' personalities were collected via the same bipolar adjective scale used for the self-report at T2. Items were embedded in the O-TAPE (Rau, Nestler, et al., 2021), an online tool specifically designed to assess perceiver effects in first impressions. The O-TAPE presents perceivers with screenshots of 10 unknown targets' Facebook profiles showing the person's name, date of birth, and place of residence, along with a profile picture (depicting the person in a naturalistic pose and environment) and a cover photo (a background picture that is chosen by the profile owner and that may, for instance, show a landscape). The Facebook profiles were designed so as to appear as realistic as possible and pretested accordingly (see Rau, Nestler, et al., 2021, for details). Further, the 10 O-TAPE targets are balanced in terms of gender, aged between 21 and 38 years, and heterogenous in terms of attractiveness and expressiveness. The stimuli are provided on the OSF. Perceiver effect variables were calculated for each HEXACO dimension by averaging ratings across the 10 targets.

Procedure

Data were collected online using SoSci Survey (Leiner, 2019a). A professional panel provider in Germany recruited the participants and sought a representative sample of the German population with regard to age, gender, and education. Participants' responses were matched across the two measurement occasions using anonymous codes provided by the panel agency. All participants who had completed *T*1 (N = 4,585) were reinvited to *T*2, 166 days after

[5] We thank Michael C. Ashton and Kibeom Lee for their helpful comments in selecting suitable adjectives.

Table 1
Descriptive Statistics for Self-Reports of Personality Traits (Study 1; N = 2,287)

Dimension	Bipolar adjective scale		HEXACO-60			Convergence correlation
	M	*SD*	*M*	*SD*	α	
Honesty–humility	6.65	1.04	3.64	0.62	.76	.36
Emotional stability	3.64	1.90	3.19	0.60	.73	.54
Extraversion	4.36	1.94	3.23	0.61	.73	.61
Agreeableness	5.44	1.59	3.23	0.51	.80	.54
Conscientiousness	6.41	1.44	3.71	0.50	.78	.51
Openness to experience	5.46	1.70	3.26	0.67	.75	.43

Note. Ratings on the bipolar adjective scale were collected on a scale from 1 (*low level*) to 8 (*high level*); ratings on the HEXACO-60 were collected on a scale from 1 (*strongly disagree*) to 5 (*strongly agree*). HEXACO-60 = 60-item HEXACO Personality Inventory-Revised.

completing *T*1 on average (*SD* = 5.5 days). In turn, participants completed *T*2 169 days (i.e., 5.5 months) after *T*1 (*SD* = 6.4 days).

On both measurement occasions, participants first provided informed consent and demographic information. At *T*1, participants were then presented with different self-report questionnaires in random order, including the HEXACO-60 and other personality scales not pertinent to the current investigation. Within the HEXACO-60, we embedded an instructed attention check item reading "Please choose 'strongly agree' here (this serves to check your attention)" to foster careful responding. Participants were aware that their attention would be checked repeatedly throughout the survey and that they would be redirected to the panel provider without being able to complete the survey if they failed any of these checks. At *T*2, participants were first asked to provide a self-rating of the HEXACO dimensions using the bipolar adjective scale described above. Afterward, they were presented with the 10 O-TAPE targets (in random order) and asked to judge each target using the same bipolar adjective scale (i.e., "please rate the extent to which the characteristics provided below apply to this person"). No attention check was embedded in the *T*2 survey, but careful responding was tested based on response times and response variability (see details below). Participants were financially compensated for participation according to the panel provider's regulations.

Participants

A total of 2,677 participants started the study at *T*2, providing at least informed consent and demographic information. Of these, *n* = 72 dropped out voluntarily during participation and one participant participated twice, which is why we excluded their second participation. Of the remaining 2,604 participants completing *T*2, we further excluded *n* = 211 participants whose ratings for self- and other-perceptions (i.e., bipolar adjective scale) indicated careless responding. First, we computed an index of relative completion speed according to Leiner (2019b). To this end, we determined a "speed factor" per relevant survey page by dividing the sample's median completion time per page by the individual completion time on that page. After trimming these values to a maximum of three to exclude outliers, we computed an index of relative completion speed by averaging the speed factors across pages for each participant. An index of 2 indicates that a participant completed the survey pages in question in half the time on average as the typical respondent. Accordingly, participants with a relative speed index >2 were excluded (*n* = 190).[6] Second, we determined the variation in other-perceptions and excluded all participants (*n* = 21) showing no variation in ratings across targets at all (i.e., *SD* = 0). Moreover, after inspecting the final data set, we realized that additional participants should be excluded even though they did not meet any of the preregistered exclusion criteria. Specifically, we excluded three participants who indicated considerable deviations in demographic information between *T*1 and *T*2, reporting both a different gender and a difference in age of more than 2 years. Further, we excluded 27 participants who consistently alternated their ratings between just two response options throughout the entire O-TAPE. Finally, we identified several participants who provided plausible ratings in the beginning of the O-TAPE but then stopped varying their responses at some point. We excluded these participants in case they did not vary their responses for at least half of the targets (*n* = 86). Although applying these additional exclusion criteria likely enhanced data quality, we also confirmed that it did not alter the substantive conclusions reached with the preregistered criteria.

The final sample comprised *N* = 2,287 individuals. As intended, participants were almost balanced in terms of gender (i.e., 1,073 females, 1,212 males, two diverse), and they spanned a broad age range from 18 to 70 years (*M* = 44.5, *SD* = 12.4). They were also diverse with regard to educational background, with 35% holding a general certificate of secondary education (German: Mittlere Reife), 28% holding a vocational diploma or university-entrance diploma (German: Fachabitur or Abitur), and 35% holding a university/college degree. The majority of participants were employed (63%); only 7% were students.

Analytic Strategy

In the O-TAPE fraction of the data set, each row reflected a different participant (i.e., perceiver) and had a separate entry for every trait*target combination, such that there were 60 columns (six traits rated for each of the 10 targets). Before condensing these columns down to 6 (one perceiver effect variable for each trait) by averaging across targets, we sought to establish that ratings were

[6] Because of a mistake in the preregistration, we specified another speed criterion according to which we were planning to identify careless respondents. However, this criterion was not readily applicable (actually, it is not defined in Leiner, 2019b, in the way we preregistered it), which is why we ultimately decided to use the relative speed index as recommended by Leiner (2019b).

indeed driven by systematic differences between participants, that is, perceiver effects. This is a standard procedure in the SRM tradition and involves quantifying the amounts of variance going back to perceivers (rows) and targets (columns) for a selected trait. To this end, we estimated six crossed-random effects models, one for each HEXACO dimension, which used the respective trait rating as the criterion and regressed it on two random intercepts (one for perceivers and one for targets) without including any predictor variables, using the lme4 package (version 1.1-21; Bates et al., 2015) in R (version 3.5.1; R Core Team, 2019). From these models, we computed perceiver intraclass correlations (ICCs) as a descriptive statistic indexing the percentage of variance in judgments that is attributable to perceiver effects.

Second, after averaging across targets, we sought to establish a measurement model for the six HEXACO perceiver effect variables using confirmatory factor analysis. As described above, there is evidence for a strong positivity factor in perceiver effects, and we therefore considered it a reasonable starting point to use a unidimensional model with a general factor on which the six HEXACO perceiver effect variables loaded. Given the lack of prior experience with the six-item bipolar adjective scale used to assess perceiver effects, we anticipated that data-dependent modifications might be necessary to find a model that meets the conventional criteria for model fit, such as a comparative fit index (CFI) > .95 and a root mean square error of approximation (RMSEA) and standardized root mean residual (SRMR) < .08 (Hu & Bentler, 1999). To avoid overfitting, we used only half of the data when we first fitted and modified the unidimensional perceiver effects measurement model and then validated this model based on the data that were previously withheld.

Finally, we used structural equation modeling to determine the assumed similarity for each HEXACO dimension. Specifically, we entered personality self-reports as manifest covariates in the perceiver effects measurement model (separately for each HEXACO dimension) and estimated their correlation with the corresponding latent perceiver effect variable of that dimension. Crucially, these latent perceiver effect variables are trait-specific residuals (i.e., they are controlled for the general positivity factor). Thus, the estimated assumed similarity correlations are inherently corrected for individual differences in global positivity. All latent variable models were estimated using the lavaan package (version 0.6-3; Rosseel, 2012) in R.

We ran these assumed similarity models twice, once using the self-reports on the bipolar adjective scale and once using the self-reports on the HEXACO-60 (i.e., mean scores for each dimension) as manifest covariates. The correlations involving ratings on the bipolar adjective scale reflect the common approach to determining assumed similarity (i.e., using the same scale to elicit self- and other-perceptions), whereas the correlations involving the HEXACO-60 self-reports can be considered more conservative for at least two reasons. First, although we did our best to cover all aspects of each HEXACO dimension in the bipolar adjective scale, some imperfections in terms of content overlap with a more comprehensive personality inventory such as the HEXACO-60 is inevitable given the breadth of the personality constructs under investigation. Second, there was a considerable time lag between the administration of the HEXACO-60 and the O-TAPE (i.e., 169 days on average), such that imperfect temporal stability of the HEXACO dimensions further diminishes the size of assumed similarity correlations that can possibly be observed for the HEXACO-60 self-reports.

To provide a point of reference for the trait-specific (i.e., positivity-corrected) assumed similarity correlations, we also computed uncorrected ("raw") assumed similarity correlations that involve the manifest (rather than latent) perceiver effect variables. Moreover, we report how personality traits were related to global positivity in other-perceptions. Given the large sample size, we do not rely on statistical significance when discussing results but instead interpret correlations of $r \geq .10$ as meaningful effects. As with the remaining analytical details, this approach was preregistered (https://aspredicted.org/cw92q.pdf).

Results and Discussion

Descriptive Statistics

Table 1 summarizes the descriptive statistics of the personality self-reports, including the convergence between responses on the HEXACO-60 and the newly developed bipolar adjective scale. As is apparent, there was moderate convergence between measures for most dimensions ($r > .50$) and adequate convergence for honesty–humility ($r = .36$) and openness to experience ($r = .43$).

Table 2 presents descriptive statistics for other-perceptions as captured by the O-TAPE. ICCs were larger than zero for all HEXACO dimensions, ranging between .08 for extraversion and .20 for honesty–humility. As such, with the exception of extraversion, variance proportions exceeded the commonly applied threshold of 10% indicating a considerable contribution of perceiver effects to other-perceptions (Kenny, 1994, 2019). Across items, 15% of the variance in ratings was attributable to target effects, whereas 69% of the variance was specific to the respective perceiver*target combination (i.e., relationship effects + error).

Table 3 summarizes the intercorrelations among the personality self-reports as well as among the manifest perceiver effect variables. The self-reports based on the bipolar adjective scale showed

Table 2
Descriptive Statistics for Perceiver Effects Measured via the Online-Tool for Assessing Perceiver Effects (O-TAPE; Study 1)

Dimension	ICC	*M*	*SD*	α
Honesty–humility (*greedy, dishonesty* vs. *modest, sincere*)	.20	5.42	0.78	.74
Emotional stability (*anxious, sensitive* vs. *relaxed, emotionally stable*)	.12	5.35	0.77	.63
Extraversion (*reserved, quiet* vs. *sociable, lively*)	.08	4.69	0.78	.57
Agreeableness (*critical, quick-tempered* vs. *sympathetic, gentle*)	.14	5.35	0.73	.66
Conscientiousness (*disorganized, careless* vs. *reliable, self-disciplined*)	.16	5.38	0.76	.69
Openness to experience (*conventional, uncreative* vs. *open to new experiences, artistic*)	.13	5.57	0.75	.63

Note. *M* and *SD* refer to individual ratings (i.e., per perceiver per target). Ratings were collected on the bipolar adjective scale (see adjectives in parentheses) and provided on a scale ranging from 1 (*low level*; printed before the "vs.") to 8 (*high level*; printed after the "vs."). ICC = intraclass correlations for perceivers.

somewhat stronger interrelations than the self-reports based on the HEXACO-60. More importantly, however, perceiver effects showed substantial interrelations (i.e., $.12 \leq r \leq .73$, with a median of $r = .48$), supporting the notion of a common (positivity) factor underlying perceiver effects across trait dimensions.

Perceiver Effects Measurement Model

A unidimensional model in which all perceiver effect variables loaded on a common positivity factor with any residual correlations disallowed did not reach an acceptable level of fit when estimated with the first half of the data, $\chi^2(9) = 446.5$, CFI = .855, RMSEA = .206, SRMR = .097. Stepwise exploration of modification indices revealed that allowing for residual correlations between honesty–humility and agreeableness on the one hand, and emotional stability, extraversion, and openness to experience on the other hand, resulted in a well-fitting model, $\chi^2(5) = 20.4$, CFI = .995, RMSEA = .052, SRMR = .016. Given that honesty–humility and agreeableness both tap into the broad content-domain of *communion*, whereas emotional stability, extraversion, and openness to experience tap into the broad content-domain of *agency* (Abele & Wojciszke, 2007; Bakan, 1966), these residual correlations are substantively plausible. This interpretation is supported by the observation that correlations of the same items also stood out as relatively large in the self-reports of the bipolar adjective scale (see upper panel in Table 3). In turn, the model yielded good fit when estimated with the second half of the data that were previously withheld, $\chi^2(5) = 27.8$, CFI = .992, RMSEA = .063, SRMR = .020. Figure 1 displays the standardized solution of the final model when fitted to the full sample, $\chi^2(5) = 45.1$, CFI = .993, RMSEA = .059, SRMR = .017.

The interpretation that the general factor reflects global positivity is supported by two observations. First, other-perceptions on extraversion—a dimension that is known as the most evaluatively neutral trait in basic personality frameworks (John & Robins, 1993)—showed a markedly lower (albeit positive) loading on the general factor compared to the remaining HEXACO dimensions. Second, based on prior studies using the O-TAPE (Rau, Nestler, et al., 2021), we expected (and predicted) that perceivers' self-reports on honesty–humility and agreeableness would be related to a more benevolent judgment style, which was confirmed by correlations of $r = .10$, 95% CI [.06, .14] and $r = .20$, 95% CI [.16, .24], respectively, of the HEXACO-60 self-reports with the general factor. In contrast, self-reports of emotional stability, conscientiousness, and openness to experience were not meaningfully correlated with the general factor (*r*s between .04 and .08; see Table O1 on the OSF for details; https://osf.io/ntwjz/). Of note, an unexpectedly large correlation was observed for extraversion, $r = .23$, 95% CI [.18, .27]. This suggests that previous work using the O-TAPE (Rau, Nestler, et al., 2021), which had considerably lower statistical power than the present study, might have underestimated the role of extraversion in qualifying the "benevolent personality." As such, high positivity does not appear to be found primarily among prosocial perceivers (indicated by high honesty–humility and high agreeableness) but rather among perceivers with a social approach tendency more broadly (indicated by high honesty–humility, high agreeableness, and high extraversion).

Assumed Similarity

Table 4 summarizes the assumed similarity correlations for all combinations of self-report measures (bipolar adjective scale and HEXACO-60) and perceiver effect variables (manifest and latent). The full correlation table, including cross-trait correlations of all self-report measures and perceiver effects, is available on the OSF (Table O2). Considering the raw correlations (i.e., correlations involving manifest perceiver effects; left and middle-left panels in Table 4), a "meaningful" level of assumed similarity (i.e., $r \geq .10$) was present for all dimensions except extraversion when self- and other-ratings were both measured with the bipolar adjective scale, with the highest correlation emerging for honesty–humility ($r = .19$). In turn, for the HEXACO-60 self-reports, we only observed meaningful assumed similarity correlations for agreeableness and openness to experience (both $r = .10$). As such, even though some findings were in line with prior evidence (e.g., honesty–humility yielding the strongest assumed similarity effect when self- and other-ratings were assessed on the same scale), the pattern of raw

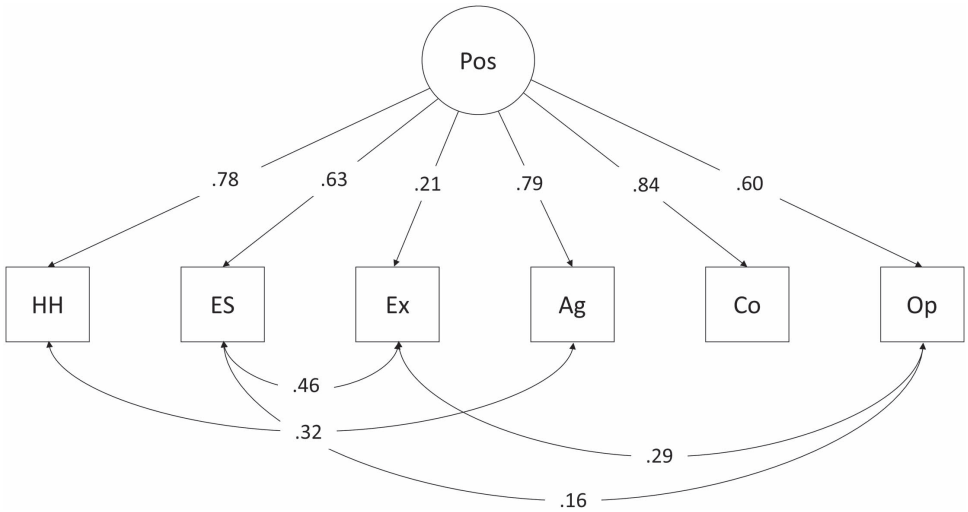
Table 3

Intercorrelations Among HEXACO Self-Reports and Perceiver Effect Variables (Study 1)

		Correlations					
Rating	Dimension	1	2	3	4	5	6
SR	1 Honesty–humility	—	.12	−.04	.35	.23	.09
	2 Emotional stability	.00	—	.34	.22	.23	.17
	3 Extraversion	.12	.25	—	.05	.13	.25
	4 Agreeableness	.26	.04	.23	—	.21	.19
	5 Conscientiousness	.17	.04	.17	.01	—	.10
	6 Openness to experience	.08	−.03	.17	.02	.13	—
PE	1 Honesty–humility	—	.49	.12	.73	.64	.45
	2 Emotional stability		—	.48	.47	.54	.48
	3 Extraversion			—	.12	.22	.35
	4 Agreeableness				—	.67	.47
	5 Conscientiousness					—	.51
	6 Openness to experience						—

Note. Self-reports based on the HEXACO-60 below the diagonal in the upper panel; self-reports based on the bipolar adjective scale above the diagonal in the upper panel. All $r \geq |.07|$ were significant at $p < .01$. HEXACO = honesty–humility, emotionality, extraversion, agreeableness, conscientiousness, and openness to experience; SR = self-reports; PE = perceiver effects.

Figure 1
Perceiver Effects Measurement Model Featuring a Global Positivity Factor (Study 1)



Note. HH = honesty–humility; ES = emotional stability; Ex = extraversion; Ag = agreeableness; Co = conscientiousness; Op = openness to experience; Pos = global positivity. Single-headed arrows reflect factor loadings; double-headed arrows reflect residual correlations. All parameters are standardized.

correlations also showed some deviations compared to previous (meta-analytic) findings on assumed similarity of the HEXACO dimensions. Notably, however, these prior findings were exclusively based on correlations between self- and other-perceptions collected via versions of the HEXACO Personality Inventory-Revised (Lee & Ashton, 2004, 2006), whereas here, for the first time, we used single-item bipolar adjective scales to measure perceiver effects (as well as self-reports). Moreover, it should again be noted that the convergence between the bipolar adjective scale and the HEXACO-60 was lowest for self-reports on honesty–humility (r = .36; Table 1), which likely explains the relatively small assumed similarity correlation for this dimension in particular when self-reports were based on the HEXACO-60.

More importantly, most of the assumed similarity correlations became considerably smaller once controlling for global positivity in other-perceptions (right and middle-right panels in Table 4). Specifically, for the self-reports based on the bipolar adjective scale, assumed similarity only remained meaningful for honesty–humility, yielding r = .14. For the self-reports based on the HEXACO-60, in turn, assumed similarity only remained meaningful for openness to experience, yielding r = .12. In contrast, there was no evidence for meaningful (i.e., $r \geq .10$) assumed similarity above and beyond global positivity for any of the remaining HEXACO dimensions. The most striking difference between the raw and positivity-corrected assumed similarity correlations emerged for agreeableness: Whereas there was evidence for meaningful levels of assumed similarity on agreeableness for both self-report measures when considering the raw correlations (i.e., r = .17 and r = .10, respectively), these effects vanished entirely (and even became slightly negative) once controlling for global positivity (i.e., r = −.03 and r = −.07, respectively).

Taken together, assumed similarity effects were considerably reduced once we accounted for global positivity in other-perceptions. Yet, there remained evidence of meaningful levels of positivity-corrected assumed similarity for honesty–humility and openness to experience—the two HEXACO dimensions arguably most strongly linked to individuals' value system. However, that evidence was inconclusive as it was inconsistent across the two types of self-report measures. Thus, although Study 1 provides some support for trait-specific assumed similarity above and beyond

Table 4
Raw and Positivity-Corrected Assumed Similarity Correlations (Study 1)

Dimension	Raw correlations		Positivity-corrected correlations	
	Bipolar adjective scale	HEXACO-60	Bipolar adjective scale	HEXACO-60
Honesty–humility	**.19 [.15, .23]**	.08 [.04, .12]	**.14 [.10, .18]**	.05 [.00, .09]
Emotional stability	**.12 [.08, .16]**	.04 [−.00, .08]	.02 [−.02, .06]	.04 [−.00, .07]
Extraversion	.06 [.02, .10]	.05 [.01, .09]	.03 [−.01, .07]	−.01 [−.04, .03]
Agreeableness	**.17 [.13, .21]**	**.10 [.06, .14]**	−.03 [−.07, .01]	−.07 [−.11, −.03]
Conscientiousness	**.11 [.07, .15]**	.03 [−.01, .07]	−.06 [−.11, −.01]	−.08 [−.13, −.03]
Openness to experience	**.16 [.12, .20]**	**.10 [.06, .14]**	.07 [.03, .11]	**.12 [.08, .16]**

Note. Correlations printed bold-faced indicate $r \geq .10$, which was our preregistered criterion for a "meaningful" effect size. Values in brackets denote 95% CIs. HEXACO-60 = 60-item HEXACO Personality Inventory-Revised.

global positivity for value-related traits in particular, we sought additional evidence in a second study.

Study 2

The goal of Study 2 was to address the following limitations of Study 1. First, other-perceptions were assessed using a single item (i.e., pair of adjectives) for each HEXACO dimension in Study 1. Although these items were carefully constructed so as to broadly capture the essence of each dimension, a single item may not suffice to reflect these broad personality traits in their entirety. In line with this reasoning, self-reports collected on a more comprehensive and well-validated personality inventory (i.e., HEXACO-60) showed relatively weak convergence with the newly developed bipolar adjective scale for some HEXACO dimensions, questioning the new scale's construct validity. Critically, convergence was lowest for honesty–humility, which potentially explains why assumed similarity for honesty–humility was only meaningful for self-reports collected on the bipolar adjective scale but not for self-reports collected on the HEXACO-60. In general, the findings were inconsistent across the two self-report measures used, thus yielding only partial support for our hypotheses and calling for additional evidence. Closely related, it is unclear to what extent the adjectives describing honesty–humility and openness to experience in the bipolar adjective scale captured the association of these traits with values. Potentially, the relatively small assumed similarity correlations observed in Study 1 may indicate a weak coverage of value-related content.

Another limitation pertaining to our bipolar adjective scale is that all items were keyed in the same direction. As a consequence, the general factor labeled "positivity" also tapped acquiescence variance, that is, differential tendencies to prefer positive or negative response options irrespective of an item's content. Whereas prior research has shown that positivity differences contribute more to perceiver effects than do acquiescence differences (Heynicke et al., 2021; Rau, Carlson, et al., 2021), we were not able to separately test the influence of either tendency when computing assumed similarity correlations.

Finally, although the use of a highly standardized target set arguably has several advantages, the Facebook profiles included in the O-TAPE measure are plainer than typical Facebook profiles, which, for example, also show status updates, liked or followed pages, and information about friends. In addition, whereas the 10 targets in the O-TAPE are fairly representative of the German Facebook community in terms of sociodemographic characteristics, the size of the target sample is still relatively small. Thus, the generalizability to perceptions of strangers in more naturalistic contexts is limited.

Study 2 aimed at addressing these limitations. Specifically, we used an existing data set (Wiedenroth & Leising, 2020) in which (a) self- and other-perceptions were measured with a validated item set containing 30 trait-descriptive adjectives (Borkenau & Ostendorf, 1998), half of which were negatively keyed; (b) targets (again strangers to perceivers) were presented on videos that were much richer in content than standardized Facebook profiles; and (c) perceivers rated 10 randomly selected targets taken from a much larger stimulus set counting 200 targets in total. This latter aspect of the study rendered systematic influences of certain targets on perceivers' ratings—and, thus, of actual similarity—highly unlikely.

Another advantage of Study 2 is that we could more specifically test the influence of value-relatedness on assumed similarity. Whereas our reasoning in Study 1 rested on the assumption that honesty–humility and openness to experience should be more closely tied to values, in Study 2 we collected ratings of the value-relatedness of the 30 trait-descriptive adjectives. We could thus examine whether this item characteristic in particular can account for the strength of assumed similarity over and above global positivity. Moreover, we explored the moderating influence of other item characteristics that have been considered relevant for assumed similarity (i.e., visibility, identity-relevance). Overall, Study 2 offered a unique testbed to critically evaluate different theoretical accounts of assumed similarity.

On the basis of the pilot tests (see below), the results from Study 1, and prior theorizing, we derived the following hypotheses. First, we expected to find evidence for assumed similarity across the rated characteristics (H1). Second, we predicted assumed similarity to remain robust once accounting for global positivity in other-perceptions, as operationalized by the relation between perceiver effects and the items' social desirability (H2). Third, following the value account, we predicted trait-specific (i.e., positivity-corrected) assumed similarity to be pronounced for characteristics with stronger relations to values (H3). Finally, we predicted this moderating effect of value-relatedness on assumed similarity to remain robust once controlling for items' identity-relevance (H4). Although it has been proposed that the high relevance of value-related traits for individuals' identity has a unique moderating influence on assumed similarity (Lee et al., 2009; Thielmann, Hilbig, et al., 2020), direct empirical evidence for this has been found neither in previous work (Thielmann, Hilbig, et al., 2020) nor in our pilot test. Thus, we did not predict (or preregister) that identity-relevance would moderate assumed similarity beyond value-relatedness. Nonetheless, we aimed to provide a direct test of this idea in a highly powered design. Moreover, we sought to scrutinize the lack-of-information account further by investigating the moderating influence of trait visibility on assumed similarity (Kenny & West, 2010; Ready et al., 2000; Watson et al., 2000).

Method

The data were collected in the context of a large-scale project on person perception (LE2151/6-1; funded by the German Research Foundation). None of the present authors were involved in data collection. Parts of the data used here have already been included in other publications on interpersonal perception (Heynicke et al., 2021; Wiedenroth & Leising, 2020; Wiedenroth, Unkelbach, et al., 2021; Wiedenroth, Wessels, et al., 2021). Crucially, none of these previous articles and analyses used the perceivers' self-reports. A detailed summary of the overarching project is provided in Wiedenroth and Leising (2020) as well as in the project's OSF folder (https://osf.io/q6xdn/). Here, we will only describe those parts of the study that are relevant for the present investigation. Note that the design of the study was specifically tailored to the goals of that original project.

Transparency and Openness

The materials and data used in the current investigation as well as the analysis scripts are available on the OSF (https://osf.io/ntwjz/).

Prior to data analyses (and before having access to the full data set), we preregistered our hypotheses and analysis plan (https://osf.io/kc5fy) based on a subsample of 400 perceivers (out of the total sample of $N = 3{,}963$ perceivers). This subsample was only used for the pilot tests as reported in the preregistration; thus, all analyses reported below are based on the remaining $N = 3{,}563$ perceivers. All study procedures were consistent with ethical standards for the treatment of human subjects and approved by the German Research Foundation (Grant number: LE2151/6-1). Formal approval by an ethics committee was not obtained, given that this is not a requirement in Germany.

Materials

Self- and other-perceptions were measured using 30 German trait-descriptive adjectives (see Table 5), capturing the Big Five personality traits with six items each (Borkenau & Ostendorf, 1998). Half of the items were negatively keyed. Ratings were collected on 5-point Likert-type scales ranging from 1 = *does not apply at all* to 5 = *applies exactly*. We conducted all analyses at the item level, meaning that we did not compute sum scores for the Big Five dimensions (see details on the analytic approach below).

The stimulus set used to measure other-perceptions contained 200 target persons in total, all of which were videotaped while engaging in 20 different, standardized tasks (i.e., "situations"). For example, targets were asked to tell a joke of their choice, to talk about an embarrassing experience, and to answer general knowledge questions. The complete list of the 20 situations is available in Wiedenroth and Leising (2020). Situations were selected so as to make different personality traits visible in targets, and each video lasted only a few minutes. To recruit a diverse target sample, targets were contacted through various channels, including advertisements in local newspapers and on the internet. The target sample comprised 98 males and 102 females aged between 17 and 80 years ($M = 33.3$, $SD = 14.5$).

Procedure

Data were collected online. After registering for the study, participants (i.e., perceivers) received a one-use-only link to access the survey. They viewed video clips of 10 different targets acting in the same, randomly selected situation; immediately after watching a video, participants provided other-ratings of the target.[7] Next, participants provided self-reports on the same list of 30 trait-descriptive adjectives used for the other-ratings as well as demographic information. As compensation for participation, participants received 10€.

Participants

Participants were recruited from all over Germany using various channels (e.g., advertisements in newspapers, on websites, at universities). To be eligible for participation, participants had to be 18 years or older. The sample size was determined a priori based on design considerations, with the goal that each of the 200 targets is rated in each of the 20 situations by 10 different perceivers, resulting in a planned sample size of 4,000. Overall, 4,212 participants completed the study, several of which had to be excluded because (a) they reported being under the age of 18 ($n = 35$); (b) they indicated knowing any of the 10 targets they rated ($n = 82$); (c) they reported significant problems with video or audio quality ($n = 51$); (d) their responses were indicative of careless responding, as reflected in inconsistent ratings of positively and negatively keyed items for a given Big Five dimension, or zero variance in ratings of a given video ($n = 55$); or (e) they participated twice ($n = 26$; only the first assessment of these participants was retained). These exclusion criteria were specified a priori by the original authors, and applying them resulted in a final sample of 3,963 perceivers (see Wiedenroth & Leising, 2020, for further details). Of these, we used a random subset of $n = 400$ for the pilot test, which formed the basis for our preregistration. Thus, the final sample used for the current analyses comprised the remaining $N = 3{,}563$ perceivers (61.5% female, aged 18–82 years, $M = 28.5$, $SD = 10.8$).

Item Characteristics Survey

To investigate the influence of different item characteristics—including positivity (i.e., social desirability) and value-relatedness—on assumed similarity, we asked 27 trained raters (i.e., student research assistants; 74.1% female, aged 19–29 years, $M = 22.9$, $SD = 2.4$; all native German speakers) to judge the 30 trait-descriptive adjectives with regard to four item characteristics that have been considered relevant for assumed similarity in prior research: (a) *social desirability* as a proxy of positivity (i.e., "To what extent does the use of this term cast a more positive or negative light on the person that is being described?"), rated on a scale from 1 = *very negative* to 9 = *very positive*; (b) *visibility* (i.e., "How easy is it for others to judge 'from the outside' whether a person is accurately described by this term?"), rated on a scale from 1 = *very difficult* to 9 = *very easy*; (c) *value-relatedness* (i.e., "To what extent is this trait related to values?") rated on a scale from 1 = *no value-relatedness* to 9 = *strong value-relatedness*; and (*d*) *identity-relevance* (i.e., "How important (or relevant) is this trait for a person's identity in general?"), rated on a scale from 1 = *very unimportant* to 9 = *very important*. The order of the four ratings was randomized. For exploratory purposes, raters also evaluated the items' desired similarity (i.e., "How similar to themselves do most people desire others to be on this trait?"), rated on a scale from 1 = *not at all similar* to 9 = *very similar*, and their expectations of whether an item would yield assumed similarity (i.e., "How strongly do most people assume that others are similar to themselves on this trait?"), rated on a scale from 1 = *not at all similar* to 9 = *very similar*. Because these latter two item characteristics are beyond the scope of the current investigation, we will not discuss them further. Materials and data from the item characteristics survey are available on the OSF.

Table 6 shows the descriptive statistics from the item characteristics survey including interrater reliabilities. As can be seen in the two rightmost columns, reliabilities of individual ratings (i.e., ICC[3, 1]; Shrout & Fleiss, 1979) were unsatisfactory for some item characteristics but averaging across the 27 raters yielded highly reliable scores for all item characteristics (i.e., ICC[3, 27]; Shrout & Fleiss, 1979). Thus, we used these mean ratings in all analyses. Intercorrelations between item characteristics were mostly moderate

[7] The study also comprised a second (so-called "within-subjects") condition, where perceivers rated the same target in the 20 different situations. Data from this condition are not considered here.

Table 5
Descriptive Statistics for Self-Reports, Perceiver Effects, and Focal Item Characteristics (Study 2)

Item	Self-reports *M* (*SD*)	Perceiver effects *M* (*SD*)	Perceiver ICC	Social desirability *M* (*SD*)	Value-relatedness *M* (*SD*)
Lively	3.06 (1.07)	2.53 (0.51)	.14	4.74 (0.76)	3.44 (1.80)
Bad-tempered	2.82 (1.04)	2.55 (0.54)	.20	2.30 (0.61)	3.30 (1.64)
Considerate	4.05 (0.78)	3.45 (0.42)	.12	8.52 (0.58)	8.26 (0.86)
Emotionally stable	3.08 (1.02)	3.21 (0.41)	.12	7.59 (0.80)	3.26 (1.53)
Knowledgeable	3.82 (0.69)	3.24 (0.44)	.14	7.81 (0.79)	4.70 (2.30)
Shy	2.65 (1.07)	2.72 (0.46)	.09	3.52 (0.94)	2.74 (1.48)
Selfish	2.40 (0.96)	2.36 (0.52)	.19	1.52 (0.64)	7.44 (1.74)
Sociable	3.52 (1.04)	3.30 (0.41)	.08	7.59 (1.12)	5.33 (2.00)
Persistent	3.44 (0.96)	3.19 (0.45)	.11	6.59 (1.15)	6.44 (1.91)
Prudent	3.98 (0.66)	3.39 (0.45)	.16	8.37 (0.69)	4.89 (2.64)
Silent	2.74 (1.11)	2.64 (0.45)	.10	4.00 (0.78)	3.22 (1.80)
Unresourceful	2.06 (0.87)	2.63 (0.49)	.12	2.70 (1.10)	2.74 (1.72)
Good-natured	4.03 (0.79)	3.60 (0.42)	.14	7.78 (1.31)	7.63 (1.36)
Changeable	2.46 (0.98)	2.55 (0.47)	.15	2.78 (0.93)	3.93 (2.11)
Vulnerable	3.65 (0.96)	3.15 (0.49)	.15	4.19 (1.04)	3.37 (1.86)
Dynamic	3.44 (0.89)	2.81 (0.48)	.11	6.48 (1.16)	3.41 (1.67)
Uninformed	2.10 (0.73)	2.49 (0.46)	.16	2.48 (0.80)	3.52 (2.24)
Obstinate	2.98 (1.07)	2.49 (0.57)	.19	1.85 (0.72)	5.56 (2.26)
Industrious	3.66 (0.98)	3.37 (0.43)	.12	7.78 (1.15)	8.04 (0.98)
Irritable	3.50 (0.99)	3.05 (0.47)	.13	3.22 (1.09)	3.30 (1.46)
Responsible	4.22 (0.79)	3.54 (0.44)	.14	8.33 (0.68)	8.30 (0.91)
Lazy	2.17 (1.00)	2.30 (0.47)	.18	2.15 (0.91)	6.56 (2.24)
Reckless	2.35 (1.05)	2.31 (0.50)	.17	3.19 (1.11)	4.15 (2.14)
Helpful	4.25 (0.72)	3.60 (0.42)	.15	8.70 (0.67)	8.44 (0.70)
Witty	3.74 (0.74)	3.10 (0.47)	.14	7.74 (0.76)	4.48 (1.76)
Calm	3.33 (1.02)	3.32 (0.47)	.11	6.96 (0.81)	4.96 (2.19)
Resistant	2.38 (0.98)	2.71 (0.45)	.12	5.48 (1.22)	3.37 (1.50)
Unimaginative	2.05 (0.98)	2.68 (0.50)	.13	3.33 (1.14)	3.15 (1.46)
Domineering	2.07 (1.03)	2.06 (0.56)	.22	1.59 (0.84)	6.56 (2.14)
Reserved	3.07 (1.09)	2.98 (0.42)	.06	4.56 (0.97)	3.52 (1.95)

Note. Self-reports and perceiver effects (scale: 1–5): $N = 3{,}563$; social desirability and value-relatedness ratings (scale: 1–9): $N = 27$ trained raters. ICC = intraclass correlations.

in size (i.e., $r \leq .40$), with the exception of relations involving identity-relevance, which correlated $r = .70$ with social desirability and value-relatedness and $r = .47$ with visibility, respectively (see Table O3 on the OSF for details).

Analytic Strategy

Similar to Study 1, we analyzed perceiver effects, which we computed by averaging the perceivers' ratings across all 10 targets. As items were nested within perceivers who, in turn, were nested within 20 different situations, we first sought to determine whether a three-level model was necessary to account for the hierarchical data structure. Specifically, we partitioned variance in perceiver effects (PEs) into item-level (ε_{ips}), perceiver-level (u_{0ps}), and situation-level (r_{00s}) variance by fitting the following "null" (intercept-only) three-level model:

$$PE_{ips} = \beta_{000} + r_{00s} + u_{0ps} + \varepsilon_{ips}. \quad (1)$$

PE_{ips} is the item i's PE for perceiver p and situation s; β_{000} is the mean PE across all items, perceivers, and situations; r_{00s} is the residual between-situation variation in PE; u_{0ps} is the residual within-situation between-perceiver variation in PE; and ε_{ips} is the residual within-situation within-perceiver between-item variation in PE.

Intraclass correlations (ICCs) indicate that 2.4% of the total variance in PE_{ips} was between-perceiver variance (i.e., variance in u_{0ps} reflecting variation in acquiescence), but less than 0.1% of the total variance was between-situation variance (i.e., variance in r_{00s}). As specified in the preregistration, since less than 1% of the variance in perceiver effects was attributable to the situation in which the targets were observed, we did not include situation as a factor in our models. However, we did include perceiver as a random factor. Thus, the multilevel models below are two-level (items nested within perceivers) models.

In these models, we center all item-level predictors (except for interaction terms) within perceivers for both conceptual and practical reasons. First, the present study does not seek to explain why assumed similarity varies between perceivers; instead, the present study seeks to explain variance in assumed similarity within

Table 6
Descriptive Statistics and Intraclass Correlations of Item Characteristics Ratings (Study 2)

Item characteristic	*M*	*SD*	ICC(3, 1)	ICC(3, 27)
Social desirability	5.13	2.49	.88	.99
Visibility	5.66	1.31	.44	.96
Value-relatedness	4.93	1.90	.58	.97
Identity-relevance	5.72	1.04	.28	.91

Note. $N = 27$ trained raters. All ratings were provided on 9-point scales ranging from 1 (*low scores*) to 9 (*high scores*). ICC = intraclass correlations as proposed by Shrout and Fleiss (1979), indicating interrater reliability for a single rating [ICC(3, 1)] and the average of the 27 ratings [ICC(3, 27)].

perceivers as a function of the trait being judged, and centering predictors within perceivers allows us to decouple within-person sources from between-person sources of variance. Second, centering item-level predictors within-person enhances the interpretability of the intercepts and interaction terms.

Results

Table 5 provides the focal descriptive statistics for self-reports and perceiver effects on the 30 trait-descriptive adjectives (intercorrelations of all measures are available in Table O4 on the OSF). As in Study 1, the ICCs index the percentage of variance in raw other-ratings (i.e., *not* averaged across targets) that was attributable to perceiver effects. The majority of items exceeded the benchmark of 10% perceiver variance, which is indicative of a substantial contribution of perceiver effects to other-perceptions (Kenny, 1994, 2019). Mirroring Study 1, the only exception again occurred for items tapping extraversion (i.e., *shy*, *sociable*, and *reserved* showed only 9%, 8%, and 6% perceiver variance, respectively). Across items, 16% of the variance in ratings was attributable to target effects, whereas 70% of the variance was specific to the respective perceiver*target combination (i.e., relationship effects + error).

Testing Hypothesis 1

According to our first hypothesis, there should be assumed similarity—defined as a positive association between self-ratings and perceiver effects—across the rated characteristics on average. To test H1, we fit the following mixed model:

$$PE_{pi} = \beta_{00} + \beta_{01} * mSR_p + \beta_{10} * cSR_{pi} + u_{0p} + u_{1p} * cSR_{pi} + \varepsilon_{pi}. \quad (2)$$

PE_{pi} is the perceiver p's item i's average target rating. β_{00} is the average perceiver effect. mSR_p is the perceiver p's mean self-rating (SR) and β_{01} is the typical degree to which acquiescence tendencies in self-ratings predict acquiescence tendencies in PE. cSR_{pi} is the perceiver p's item i's self-rating (after subtracting that perceiver's mSR_p or acquiescence tendency). β_{10} is the key coefficient—the typical "assumed similarity" effect (i.e., for the typical perceiver, the association between within-perceiver between-item variation in self-ratings and within-perceiver between-item variation in PE). Finally, u_{0p} and u_{1p} are the residual between-perceiver variations in the within-person intercept and slope, and ε_{pi} is the residual between-item variation in PE_{pi}. The results showed a powerful positive assumed similarity effect, $\beta_{01} = .247$, $SE = .003$, 95% CI [.241, .254], $t = 74.6$, $p < .001$ (for remaining parameter estimates from the model, see Table O5 on the OSF). Thus, the results were consistent with H1. Nonetheless, there were also noteworthy individual differences in assumed similarity. Specifically, the variance for u_{1p} was .031 ($SE = .001$), which means that perceivers whose assumed similarity coefficients were between 1 *SD* below versus above the average effect varied between $\beta_{1p} = .070$ and $\beta_{1p} = .424$.

Testing Hypothesis 2

Our second hypothesis stated that there would remain a significant level of assumed similarity after accounting for global positivity in other-perceptions. To test H2, we regressed perceiver effects on social desirability and self-ratings simultaneously using the following model:

$$PE_{pi} = \beta_{00} + \beta_{01} * mSR_p + \beta_{10} * cSR_{pi} + \beta_{20} * cDesirability_i + u_{0p} + u_{1p} * cSR_{pi} + u_{2p} * cDesirability_i + \varepsilon_{pi}. \quad (3)$$

$cDesirability_i$ is the item i's mean social desirability rating. Like the profile of self-ratings, the profile of desirability ratings is centered within-perceiver; but unlike the profile of self-ratings, the profile of desirability ratings is the same for every perceiver. As in the previous model, β_{10} is the degree to which the perceivers' self-ratings on a given trait predict their PE for that trait (i.e., assumed similarity). However, given the presence of $Desirability_i$ as a second predictor, β_{10} now reflects *trait-specific* (i.e., positivity-corrected) assumed similarity. This correction for positivity happens because β_{20} captures the degree to which an item's desirability (rather than its trait-specific content) predicts PE. u_{1p} and u_{2p} are the residual between-perceiver variation in trait-specific assumed similarity and perceiver effect positivity, respectively.[8]

As expected, PE was strongly positively associated with trait desirability, $\beta_{20} = .135$, $SE = .002$, 95% CI [.131, .138], $t = 84.1$, $p < .001$. There was also considerable between-perceiver variance in this effect ($u_{2p} = .008$, $SE < .001$), such that the estimated b_{2p}s for individuals 1 *SD* below and above average in perceiver effect positivity were .047 and .223, respectively. This wide variation aligns well with previous research showing that perceiver effects are to a considerable extent driven by differential tendencies to see others as globally positive versus negative (Rau, Carlson, et al., 2021). After controlling for this PE–desirability association, the assumed similarity effect was considerably weakened but nonetheless robust: $\beta_{10} = .093$, $SE = .002$, 95% CI [.088, .097], $t = 39.5$, $p < .001$. Thus, consistent with H2, assumed similarity was not solely attributable to global positivity influences. In short, there remained *trait-specific* assumed similarity.[9]

[8] For simplicity, we only provide definitions of model components that were not defined in the previous section or whose interpretation has changed. Results for all model parameters are provided in Table O6 on the OSF (https://osf.io/ntwjz/).

[9] Although not directly related to our hypotheses, we extended this model to explore which personality traits could explain between-perceiver variability in the tendency to assume similarity. For this, we entered the perceivers' self-reported Big Five traits, one by one, as Level-2 predictors of the random assumed similarity slopes. Perceivers scoring high on agreeableness and conscientiousness showed a greater tendency to assume similarity than perceivers scoring lower on these traits ($\beta_{11} = .028$, $t = 15.53$, $p < .001$ and $\beta_{11} = .017$, $t = 9.41$, $p < .001$, respectively). The remaining Big Five dimensions also revealed statistically significant effects but effects were considerably smaller in size (βs ranging from −.009 to .011; all ps $< .001$; for detailed results, see Table O7).

Moreover, we computed positivity-corrected assumed similarity correlations for each of the Big Five traits separately, to allow for a better comparison with the findings from Study 1. Summarized briefly, these analyses showed meaningful (i.e., $r \geq .10$) assumed similarity correlations for all Big Five dimensions except extraversion, with the strongest correlation occurring for agreeableness ($r = .31$). However, once accounting for global positivity, effect sizes decreased considerably and only the correlations for emotional stability and agreeableness remained meaningful in size (both $r = .16$). Of note, particularly the robust assumed similarity effect for Big Five agreeableness is in line with our reasoning and prior evidence given that agreeableness is among the Big Five dimensions most strongly linked to values (Fischer & Boer, 2015), and it also has considerable overlap with HEXACO honesty–humility (Thielmann et al., 2021). However, there were also noteworthy discrepancies in comparison to our Study 1's findings, which we present and discuss further in detail in the section "Study 2 Trait-Based Analyses" in our supplemental analyses on the OSF.

Testing Hypothesis 3

Our third hypothesis stated that items' value-relatedness would moderate trait-specific assumed similarity. To test H3, we expanded the preceding model as follows:

$$\begin{aligned} PE_{pi} = {} & \beta_{00} + \beta_{01} * mSR_p + \beta_{10} * cSR_{pi} \\ & + u_{1p} * cSR_{pi} + \beta_{20} * cDesirability_i \\ & + u_{2p} * cDesirability_i + \beta_{30} * cValueRel_i \\ & + u_{3p} * cValueRel_i + \beta_{40} * cValueRel_i * cSR_{pi} \\ & + u_{4p} * cValueRel_i * cSR_{pi} + u_{0p} + \varepsilon_{pi}. \end{aligned} \quad (4)$$

ValueRel*i* is the item *i*'s mean value-relatedness rating; like the profile of desirability ratings, the profile of value-relatedness ratings was centered within-perceivers and was the same for every perceiver. β_{30} and β_{40} are the typical effects of value-relatedness on PE and the trait-specific (positivity-corrected) assumed similarity slope, respectively. u_{3p} and u_{4p} are residual between-perceiver variation in the typical effect of value-relatedness on PE and assumed similarity.

Although our preregistration focused on value-relatedness, we also explored the moderating effects of visibility and identity-relevance by replacing value-relatedness with either visibility or identity-relevance in the above model. Table 7 shows the key moderating effect (β_{40}) from each analysis. Consistent with H3, trait-specific assumed similarity was stronger for traits that were more strongly linked to values (see Table O8 on the OSF for respective parameter estimates from the full model). Trait-specific assumed similarity was also stronger for traits that tend to be viewed as central to one's identity. In contrast, visibility did not moderate assumed similarity.

To provide a more interpretable metric of the size of the moderating effect of value-relatedness, we fit the Equation 4 model after first standardizing within-perceivers the relevant predictors (PE, SR, and value-relatedness). The resulting standardized coefficients were $\beta_{10} = .174$ for trait-specific assumed similarity and $\beta_{40} = .062$ for the moderating effect of value-relatedness. Thus, the estimated trait-specific assumed similarity effects for a trait 1 *SD* below average versus a trait 1 *SD* above average in value-relatedness were $\beta_{1i} = .112$ versus $\beta_{1i} = .240$. In other words, the positivity-corrected assumed similarity effect size was weak for traits low in value-relatedness but was (what is generally considered) moderate for traits high in value-relatedness.

Table 7
Effect of the Interaction Between Self-Ratings and Item Characteristics on Perceiver Effects (Study 2)

Interaction term	β_{40}	*SE*	95% CI	*t*	*p*
SR × Visibility	.000	.001	[−.003, .002]	0.27	.990
SR × Identity-relevance	.021	.001	[.019, .023]	16.74	<.001
SR × Value-relatedness	.018	.001	[.016, .020]	20.89	<.001

Note. $N = 3{,}563$ perceivers. SR = self-ratings.

Testing Hypothesis 4

Our fourth and final hypothesis was that the moderating effect of value-relatedness on trait-specific assumed similarity would remain robust after controlling for identity-relevance. Crucially, given that value-relatedness and identity-relevance moderate assumed similarity (Table 7) and are highly correlated ($r = .70$), the moderating effect of value-relatedness may not be independent of the moderating effect of identity-relevance. To test H4, we first computed the residual value-relatedness rating after removing variance shared with identity-relevance and the residual identity-relevance rating after removing variance shared with value-relatedness. Then, we repeated the analyses from the preceding section (see Equation 4) but replaced value-relatedness with residual value-relatedness and identity-relevance with residual identity-relevance.

Table 8 shows the relevant interaction effect from each analysis (for remaining parameter estimates, see Table O9 on the OSF). Consistent with H4, residual value-relatedness (controlling for identity-relevance) moderated trait-specific assumed similarity as strongly as unresidualized value-relatedness. In contrast, residual identity-relevance (controlling for value-relatedness) showed a *negative* association with trait-specific assumed similarity. By implication, value-relatedness predicts trait-specific assumed similarity independent of its association with identity-relevance, whereas identity-relevance predicts trait-specific assumed similarity only by virtue of its association with value-relatedness.

General Discussion

The question of how the self influences accuracy and bias in person perception is a long-standing issue in psychology that has received considerable attention in research (e.g., Funder, 1995; Holmes, 1968; Kenny & West, 2010; Shrauger & Altrocchi, 1964; Taft, 1955; West & Kenny, 2011). One of the oldest observations in this regard is that individuals' views of others converge with their self-views to some extent (Cronbach, 1955), at least on some traits (Thielmann, Hilbig, et al., 2020). These assumed similarity effects are commonly interpreted in terms of individuals' tendency to project their own characteristics onto others (e.g., Hoch, 1987; Holmes, 1968; Kenny & West, 2010; Robbins & Krueger, 2005). In contrast to this understanding, however, recent evidence has seriously questioned whether assumed similarity can be traced back to a trait-specific self-reference. Instead, findings had suggested that assumed similarity may be an artifact of individual differences in how favorably a person sees others in general, meaning that assumed similarity may almost entirely be driven by differences in perceivers' benevolence when judging others, independent of the specific content of the qualities being judged (Rau, Carlson, et al., 2021; Wood et al., 2010).

Across two large studies (total $N = 5{,}850$), we put this idea to a critical test. In Study 1, participants rated targets from 10 standardized Facebook profiles using six bipolar adjective scales representing the HEXACO dimensions. Study 2 sought to overcome several limitations of Study 1, relying on more naturalistic, more diverse, and richer stimuli (i.e., videos of 10 targets randomly selected from a set of 200 targets) that participants rated on a

Table 8
Effect of the Interaction Between Self-Ratings and Residualized Item Characteristics on Perceiver Effects (Study 2)

Interaction term	β40	*SE*	95% CI	*t*	*p*
SR × Residual value-relatedness	.022	.001	[.020, .024]	19.79	<.001
SR × Residual identity-relevance	−.012	.002	[−.015, −.008]	6.96	<.001

Note. N = 3,563 perceivers. SR = self-ratings.

considerably larger item set containing 30 trait-descriptive adjectives representing the Big Five. Both studies—by using standardized target stimuli instead of real interactions between judges and targets—forestalled influences of actual and elicited similarity on assumed similarity. More crucially still, we used tailored trait-based (Study 1) and profile-based (Study 2) analyses to control for the influence of global positivity in other-perceptions on assumed similarity. Although results of Study 1 remained somewhat inconclusive, Study 2 provided clear support that assumed similarity is not simply driven by global positivity in other-perceptions but also by trait-specific content that perceivers judge to be shared by others. Our findings thus align with the conclusion that "global evaluation is part of the story but not all of it" (Srivastava et al., 2010, p. 530).

Further, we tested whether the trait-specificity of assumed similarity can be traced back to individuals' tendency to see others as similar to the self on value-related traits in particular—as has been suggested by prior research and theorizing (Lee et al., 2009; Thielmann, Hilbig, et al., 2020). Supporting this reasoning, the trait-based approach used in Study 1 showed that assumed similarity correlations only remained robust after accounting for global positivity for those two HEXACO dimensions that are most strongly linked to values, namely, honesty–humility and openness to experience (Anglim et al., 2017; Lee et al., 2010). By contrast, for the remaining HEXACO dimensions, there was no evidence for assumed similarity over and above global positivity. Of note, however, results were inconsistent across the bipolar adjective scale versus the HEXACO-60 self-report measures. Providing stronger evidence, in turn, results from the profile-based approach in Study 2 revealed value-relatedness as the most consistent moderator of trait-specific assumed similarity. Specifically, those trait adjectives with stronger links to values (as evaluated by trained raters) yielded greater assumed similarity effects over and above global positivity than those trait adjectives with weaker (or no) links to values. Crucially, though, in both studies, effect sizes were reduced when accounting for global positivity. Nonetheless, on the whole, the results corroborate the trait-specificity of assumed similarity, which can arguably be attributed to the traits' value-relatedness.

Implications and Directions for Future Research

The current work has important implications for theory and research on person perception. By providing the most critical test of the trait-specificity of assumed similarity to date, our findings support prior theoretical accounts suggesting that assumed similarity emerges from trait-specific content. This is an important insight given that it refutes recent claims that assumed similarity might entirely result from traits being saturated with desirability information and perceivers' differential tendencies to see others in desirable ways as a function of their personality self-views (Rau, Carlson, et al., 2021). Instead, the present findings are compatible with a value account of assumed similarity, proposing that assumed similarity is particularly apparent "on those personality characteristics whose relevance to values gives them central importance to one's identity" (Lee et al., 2009, p. 460). In fact, our results suggest that value-relatedness is a unique feature of traits that influences whether or not people will assume that strangers are similar to them. By contrast, this does not seem to hold for identity-relevance, which failed to explain assumed similarity once we accounted for shared variance with value-relatedness. Thus, identity-relevance itself does not seem to render traits particularly susceptible to assumed similarity—as one of us had proposed before (Thielmann, Hilbig, et al., 2020)—but value-relatedness does.

Moreover, the results of Study 2 contradict the idea that assumed similarity is attributable to a lack-of-information effect (Kenny & West, 2010; Ready et al., 2000; Watson et al., 2000), as trait visibility did not moderate the strength of assumed similarity. Thus, along with prior evidence on the theoretical underpinnings of assumed similarity in personality judgments (Thielmann, Hilbig, et al., 2020), the present findings suggest that trait-specific assumed similarity cannot be considered an artifact of "superficial" characteristics of specific traits, such as visibility. Instead, assumed similarity seems to be best described by a desire to see others as similar to oneself in regard to those traits that anchor the axes of individuals' value system. This is also compatible with other evidence suggesting that assumed similarity is motivational in nature, in the sense of serving the function to satisfy one's needs for cognitive balance, self-enhancement, or belongingness (e.g., Human & Biesanz, 2011; Locke et al., 2012, 2021; Locke & Christensen, 2007).

Another implication of the present findings is that future research should account for the influence of global positivity on other-perceptions in the study of assumed similarity whenever possible. In both our studies, effect sizes decreased substantially once statistically controlling for global positivity. In general, the finding that there is specific bias in other-perceptions that can be accounted for by a combination of (a) perceivers' benevolence in judging others and (b) their personality traits indicative of values implies that researchers should consider both of these perceiver characteristics when interpreting ratings about others' personalities. In fact, the influence of a benevolent judgment style may play an even greater role in everyday social interactions compared to when personality judgments are made about strangers "from a distance," as was the case in the present studies. Evidence suggests that a perceiver's overall tendency toward positivity is more likely to overshadow trait perceptions in contexts characterized by greater personal involvement (Rau, Carlson, et al., 2021), implying that

assumed similarity is less likely in face-to-face contexts. At the same time, however, other findings suggest that members of peer groups perceive each other as increasingly similar on specific traits upon interacting more over time (Srivastava et al., 2010). Arguably, the latter finding is to some extent a consequence of elicited similarity (Hughes et al., 2021); to test this, future research on real-life encounters that disentangles trait- and state-perceptions would be valuable. Specifically, this type of research could quantify the extent to which perceived similarity in everyday social interactions is attributable to stable, potentially unwarranted assumptions about others versus actual situational elicitation of trait-specific behaviors.

In addition to the broad dimensions reflected in models of basic personality structure (i.e., the HEXACO and Big Five), future research should also investigate positivity-corrected assumed similarity for other characteristics, such as personality nuances, attitudes, and emotions. For example, Wood et al. (2010) found that the global positivity factor extracted from other-perceptions correlated most strongly with perceivers' characteristics indicative of positive affectivity. Correspondingly, the consistently observed assumed similarity effects for trait affect (e.g., Beer et al., 2013; Papp et al., 2010; Watson et al., 2000; Weller & Watson, 2009) may potentially be attributable to individual differences in global positivity. Conversely, assumed similarity about specific attitudes (e.g., concerning controversial topics such as abortion or environmental protection) may be less prone to influences of global positivity because there rarely is a normatively prescribed position that may be picked up on by a benevolent judgment style (e.g., Marks & Miller, 1982). Further research is needed to address these questions and thereby inform the generalizability of our findings to other person perception domains.

Moreover, another fruitful avenue for future research is the investigation of the consequences of trait-specific assumed similarity for behavior. For example, positive beliefs about others' prosociality are a consistent driver of prosocial behavior (Balliet & Van Lange, 2013; Pletzer et al., 2018). Such beliefs may, in turn, be an expression either of viewing one's own prosociality—as, for instance, captured by individual differences in honesty–humility (Thielmann, Spadaro, et al., 2020)—to be shared by others or of benevolent attitudes toward others in general (i.e., global positivity). Given the vital significance of prosocial behaviors for well-functioning interpersonal relationships and society at large, future research is required to understand the fine-grained processes underlying prosocial behavior that is contingent on beliefs about others. Even though initial evidence linking perceiver effects to cooperation in social dilemmas is ambiguous (Rau et al., 2020), further studies are necessary to tackle this issue.

Likewise, additional research into the behavioral consequences of assumed similarity on values and attitudes related to openness to experience is warranted. For example, openness to experience shows consistent positive associations with pro-environmental attitudes (Soutter et al., 2020; Soutter & Mõttus, 2021; Zettler et al., 2020). Thus, it is conceivable that trait-specific assumed similarity also drives higher pro-environmental behavior among individuals high in openness to experience. This is because pro-environmental behavior reflects a real-life social dilemma (Hardin, 1968; Joireman, 2005; Van Lange et al., 2013), suggesting that individuals should only engage in this individually costly behavior to the degree that they expect others to do the same. Assumed similarity may thus be one mechanism through which openness to experience is linked to pro-environmental behavior.

Finally, it is important to note that the present findings should not be taken to imply that assuming similarity with a target will necessarily decrease the accuracy of personality judgments. In the present studies, we chose a design in which it was essentially impossible that actual similarity with targets could influence other-perceptions. However, individuals' social environments may select for certain personality features (e.g., with regard to moral values or aesthetic preferences; e.g., Liu et al., 2018) and, as such, assuming similarity with others in everyday life may lead to accurate judgments more often than to inaccurate ones (Hoch, 1987; Paunonen & Hong, 2013).

Limitations

Although our research can advance the understanding of individual differences in person perception in important ways, some limitations ought to be acknowledged. First, the trait-specific (i.e., positivity-corrected) assumed similarity effects were relatively weak. In Study 1, for example, the relevant effects of honesty–humility and openness to experience hovered around $r = .10$. However, as has recently been emphasized, "an effect-size r of .10 indicates an effect that is still *small* at the level of single events but potentially more ultimately consequential" (Funder & Ozer, 2019, p. 166). We concur with this assessment, noting that individuals are constantly required to assess what others are like in day-to-day interactions. This renders it likely that even a slight tendency to assume similarity with others will have an impact on individuals' real-life experiences in the long run. Further, one reason for the relatively small observed effect sizes may be that we analyzed items from personality inventories, which are not only related to values but also saturated with both value-unrelated trait content and mere evaluativeness (Leising et al., 2015). By implication, stronger (trait-specific) assumed similarity effects should emerge for items that almost exclusively tap values (e.g., *conservative* or *liberal*) than for items that tap a mixture of (partly) value-related trait-content and evaluativeness (e.g., *conventional* or *open to new experiences*). Research using a more heterogenous set of items is needed to test this proposition.

Second, we exclusively focused on judgments of strangers, whereas in their everyday experiences people regularly judge others they know. Indeed, assumed similarity effects tend to become stronger with increasing relationship closeness between perceivers and targets (Kenny, 1994; Lee et al., 2009; Selfhout et al., 2009). Whether this pattern of greater assumed similarity in closer relationships holds once accounting for global positivity in other-perceptions is an open question that needs to be addressed in future research. Moreover, in our studies perceivers could not interact with targets given that targets were only presented on photos or videos. Although we deliberately decided to avoid any social interaction between perceivers and targets to prevent influences of elicited similarity, future research is needed to test whether our findings can extend to situations involving face-to-face encounters.

Finally, our studies do not address the cognitive processes underlying assumed similarity. Although the results suggest that the association of traits with values is a unique feature that renders traits more or less susceptible to assumed similarity, our studies are

mute on whether assumed similarity results from *projection* of one's own characteristics onto others or from any other mechanism leading to a convergence between self- and other-perceptions. For example, assumed similarity may be attributable to a reciprocity or "tit-for-tat" mechanism in the sense that people infer (or justify) their own characteristics based on their perceptions of others (e.g., "others are dishonest, so I am dishonest back"). Although we maintain that a self-reference mechanism, such as projection, is more likely in light of the cumulating evidence on assumed similarity, more direct evidence on the underlying cognitive processes of assumed similarity is needed.

Conclusion

Assumed similarity in personality judgments cannot be fully accounted for by individual differences in how favorably people see others in general. On some traits, perceivers do indeed judge others to be similar to themselves, in the sense that certain aspects of their self-concepts reoccur in their perceptions of strangers. By implication, professionals who are required to make fair and objective personality assessments of strangers—for example, in personnel selection or psychotherapy settings—are well advised to be sensitive to this potential source of bias, especially when judging traits that are linked to their value system. In conclusion, "true" assumed similarity in personality judgments is a reliable phenomenon that can be well accounted for by individuals' desire to view others as sharing their values.

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